Author’s response to reviews

Title: Selective citation in the literature on swimming in chlorinated water and childhood asthma: a network analysis

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Author’s response to reviews:

Swimming in chlorinated water - Reviewer reports:

Reviewer #1: Thank you for the opportunity to review this manuscript. The authors have used citation networks to examine the potential for citation bias in studies on swimming in chlorinated water and childhood asthma, finding that authors were more likely to cite themselves and that articles confirming the association. The strengths of the research include the number of factors that were considered, the neat and clear structure, the reporting of a protocol, and the systematic identification of literature. The weaknesses include the small number of articles that were included, which may have affected the ability to draw conclusions about the differential effects of self-citation versus citation bias (selecting for result) for other reasons. In what follows, I have made comments and suggestions generally in chronological order but because they relate to sections of the manuscript I haven't provided line/page numbers.

Major comments:

1. Abstract: when stating odds ratios, always include 95% confidence intervals.

Reply:

We would like to thank the reviewer for his thorough appraisal and his many useful comments.
We agree with this comment. 95% confidence intervals are now included in the abstract and the results section (these changes can be found at line numbers 41-46 and 377-389 of the new manuscript with tracked changes).

2. Extracting articles only from Web of Science Core Collection for clinical articles may have been a problem. It would be important to expand the search to include at least PubMed and preferably Embase as well. I agree that reference checking may affect the validity of the study but there is a strong chance many articles will have been missed. Details of the systematic identification of the literature should be described in detail, and the search strategy, number of identified articles, and the search date should all be included (or replicated from the protocol in supplementary information).

Reply:

The search strategy has now been added to the Supplement (Table S1), and a reference to this table was added to the main document (line number 170). A reference to the search output flow chart (Figure S1) has also been added to the main document (line number 347). The search in Web of Science Core Collection has been updated and the exact date is now stated in the document (line number 181: “20 June 2017”).

We agree with the reviewer that an expansion of the search strategy to Medline and possibly Embase would improve the citation analysis. However, the software that we use to create a citation network does not currently support this. In reaction to this comment we contacted the manufacturer of this software to find out if there is a workaround to include records from Medline and/or Embase. Unfortunately, this is currently not possible but they are looking into ways to provide this option in the future.

We do not think that the citation dynamics or our results would change if Medline or Embase records had been added to the network. However, it is indeed a limitation of our citation analysis, and we added the following paragraph to address this important issue to the discussion section (line numbers 487-495):

“Our network consists of publications identified by the Web of Science Core Collection. This database offers the option to download reference lists as part of the search output. Other databases, such as Medline and Embase, do not provide this option yet. Because these reference lists are needed by the software we used (CitNetExplorer) to build a citation network, we did not include these other databases in our search strategy. It is therefore likely that we missed parts of the literature. However, we see no reason why the citation dynamics in the Web of Science Core Collection would be different from other databases. Nevertheless, it would be an improvement if reference lists were added to the search output of these other databases, so that this literature can be included in future citation networks.”
We also explained better the rationale for the use of Web of Science Core Collection in the Methods section (lines 291-293 in manuscript, see also reply to comment 3).

3. Methods: The details of how the citations were identified was not clear and should be the first part of the methods given its focus in the aim of the study. If this was done using CitNetExplorer and not manually validated (with proof that it correctly retrieves all articles correctly), then there is a major risk that citations were missed. Since there are only a small number of articles and potential citations, this could easily be done manually (we have previously done this for more than 10,000 citations in 152 articles: 10.1016/j.jclinepi.2014.09.014)

Reply:

We performed the manual check as suggested. We did not change the structure of the methods section, but we added an introductory paragraph (lines 160-164) to give an overview of the study method. In addition, we gave more details on how the citations were identified by CitNetExplorer and on the manual check that we performed:

“We used the built-in algorithm of CitNetExplorer to determine whether a citation had occurred (16). This algorithm makes use of reference lists that can be downloaded from the Web of Science Core Collection. The reference lists of all articles in the network were linked by the algorithm with the actual articles in the network. If possible, this linkage was done by DOI, a unique Digital Object Identifier assigned to most present-day articles; otherwise it was based on a combination of first author’s surname, first author’s first initial, publication year, volume number and first page number. Manual checking of the reference lists of the included articles showed that all classified citations were correct and that no citations were missed by the algorithm.” (lines 291-306)

4. Methods: Explain in more detail how differences were resolved when the publication date of a study was shortly before the publication date of an article and may have been published after the final submission date of the other article. Were submission dates examined? Because authors know their own articles are accepted before they are published, this may have skewed the potential for self-citation due to differences in timing.

5. Methods: Similarly, the explanation of the method for statistically analysing the selection of citations was brief and not clear. This could be expanded given that it is different from the rest of the analyses.

Reply:
We agree with comments 4 and 5 and thank the reviewer for the opportunity to explain these issues better, as they are both relevant for how our network is built. The selection of citation paths was based on the variable time to citation, so we explained this variable in more detail and cross-reference to the selection of citation paths:

“Time to citation was the number of years between the publication date of the cited article and the submission date of the citing article. This variable was also used to determine the dataset of potential citation paths (see Statistical Analysis below).

As publication date we used either the electronic publication date or the paper publication date, depending on which one was earlier. The average duration from submission to publication was seven months in this network. Submission date was not always given. If submission date was missing, it was estimated by subtracting seven months from an article’s publication date.” (lines 260-269)

Also, we now explain better how potential citation paths were selected, and cross-reference to the variable time to citation. Furthermore, we checked whether the underlying assumption for our selection of potential citation paths holds, namely that an article can only be cited if it is published after the submission date of the citing article. We found one exception which was indeed a case of self-citation. As the reviewer rightly pointed out, inclusion of this kind of citation paths might skew the potential for self-citation. So even though in this case there was an actual citation, it was not considered a potential citation and excluded from our analyses.

“Statistical analysis

The dataset consisted of all potential citation paths between citing and cited articles. A potential citation path means that the cited article is published before submission of the citing article (i.e. time to citation has a positive value). The underlying assumption is that articles can only cite up to their submission date, and can only be cited from their publication date onwards. This assumption was met for the entire network with one exception: one article had cited another article that was not yet published at the moment of submission of the citing article. The same authors were involved in both articles, which explains why they could be aware of the cited article before it was published. (This citation was not considered a potential citation and therefore excluded from our analyses.)” (lines 280-289)

6. Figure 1: A co-authorship network is fine given the results, but given the objective of the manuscript, why not include a figure for the citation paths? There are neat ways of representing potential citation paths that also include information about the publication date. These go back to Garfield's work, but an example of one that includes temporal information
and would be suitable for this size network is here: 10.1016/j.jclinepi.2013.11.015 (disclosure - I authored this).

Reply:

The visualization of (part of) the network including citation paths can be found in the Supplement, Figure S2. We now added a reference to this figure (line 356). We are happy to move the visualization to the main document if the editor considers this a better place.

7. Consider a measure in which the conclusion of the cited article matches the conclusion of the citing article. Citation bias makes more sense when the two are aligned and you could define a measure which indicates the over-representation of "positive" articles cited in articles with positive conclusions compared to the distribution of citations of those "positive" articles in articles that were "negative", each given as a proportion of the number of times an article *could* have been cited.

Reply:

We completely agree with this comment. It is a different but highly relevant way to look at citation bias. The proposed analyses can be found in the Supplement, Tables S4 and S5 (This is to maintain focus in the main document, and also because these results do not seem to add much information). In addition, we changed the last paragraph of the Statistical Analysis methods section:

“Where applicable, we also calculated whether the cited and the citing articles had the same characteristics (concordance). This would for instance be the case if positive articles would prefer to cite other positive articles, and if negative articles would prefer to cite other negative articles. If citation would be based on concordance of study outcome, it would be another sign of citation bias. To test if concordance on several characteristics has an impact on the likelihood of citation, univariate and adjusted fixed effects logistic regression analyses were applied.” (lines 313-319)

The results on this analysis are described as follows:

“In addition, we tested whether articles with similar characteristics were more likely to cite each other. The results showed that concordance between articles did not have much impact on the likelihood of being cited, except possibly for concordance of study quality (Tables S4 and S5).” (lines 390-398)
8. Using only 36 articles and a substantial number of characteristics is problematic because of multiple comparisons and this could have been determined at the protocol stage. First, state the number of different predictors next to the number of articles (which is the denominator of the analysis). Then decide if the 36 articles are able to say anything useful about these measures. This was a major limitation of the study. While there is not much that can be done about it now, it seems unfortunate that all the statistics are reported and then the limitations get a few sentences at the end. I have no problem with the way the conclusions are stated in relation to the study.

Reply:

We agree that the number of predictors is high relative to the size of the network. This is one of the reasons why we refrained from conducting multivariate analyses with multiple predictors, apart from the analyses in which we adjusted for 1 or 2 quality-related predictors. We agree that performing a multitude of univariate analyses may have led to a capitalization of chance. This would be particularly problematic if the number of multiple comparisons is unclear. We therefore pre-registered our planned analyses in a protocol, and reported all planned analyses regardless of the direction or significance of the outcome.

We changed the wording of the first limitation:

“Our citation analysis has several limitations. First of all, the network is small relative to the number of predictors, which makes it vulnerable for chance findings and chance capitalization.” (lines 386-388)

Minor comments:

1. In the abstract, it would be worthwhile stating the number and proportion of the 570 citation paths that were "realised" (i.e. the 191; 34%). The problem, of course, is that it is not always clear when the date of revised submissions and publication dates were incomplete or ambiguous.

Reply:

We made the adjustment in the abstract. (line 39)

2. In the background, try re-writing the definition for citation bias more precisely. Readers trying to understand the difference between the selective citation (affected by lots of factors) and citation bias (selecting studies based on their results or conclusions) may struggle.
Both reviewers have raised this important point. We thank them for pointing out the unclarity and for the opportunity to improve our definitions of selective citation and citation bias. We have done so in lines 65-69:

“If this selection is not representative, but instead associated with specific characteristics of the cited literature, we speak of selective citation. Citation bias is a special case of selective citation (2). It concerns the selective citation of studies based on their outcome.”

3. In the background I felt the description of a QRP did not add much to the manuscript and was a little out of place. To motivate the study, explain what the potential impact of citation bias can have and use examples where citation bias is known to have affected the research consensus.

Reply:
We now removed the paragraph on QRP and included some examples of the potential impact that citation bias can have:

“This can lead to a false consensus that is not evidence-based (3). For instance, it has been shown that biased citation of previous evidence shapes the conclusions of reviews (4). Citation bias can also lead to research waste by influencing funding decisions and guiding research in a wrong direction (4, 5).” (lines 71-78)

4. There are some other examples where citation networks have been used to evaluate bias in research more recently than Greenberg, though that is a seminal example.

Reply:
We thank the reviewer for this comment, but we believe that Greenberg’s citation analysis is indeed a seminal example of a claim-specific citation analysis, as compared to Fanelli’s non-specific approach.

5. Methods: I have never seen "in duplo" before and I'm not sure if it is a word.

Reply:
We changed it into: “by two authors” (line 194).
6. Methods: No need to repeat the definition of citation bias here. In addition, it should be defined rather than "spoken of".

Reply:

We removed the repeated definition, and instead wrote the following:

“We differentiated between two ways of looking at study outcome: data-based conclusion and authors’ conclusion. Selective citation based on either of these classifications of study outcome would signify citation bias.” (lines 199-201)

7. Methods: How was "gender" assessed? How was "country" assessed where authors had multiple affiliations in different countries?

Reply:

We assessed gender by the first name and/or title on a publication, or otherwise via ResearchGate or their institution. We added “(assessed by first name and/or salutation)” after “gender of corresponding author” (line 253). To our surprise this was very straightforward. In our network there were no authors with multiple affiliations in different countries.

8. State p-values exactly, not as "p<0.05" and especially when the data are not shown.

Reply:

Yes, of course, we agree with the reviewer. We changed the p value in line 413.

9. When referencing a study by an author's name, include the reference after the name so it is clear to the reader which reference is being discussed.

Reply:

We added references in lines 152, and in lines 473 and 475.

Reviewer #2: This article is an interesting and well conducted assessment of the relationship between article/study characteristics and citation in a small but contested field of healthcare research.
I have only two comments that should be quite easily resolved.

1. Page 13 (line 312). I don't understand the meaning of zero positive in this context. Can another term be used here or a definition be provided in brackets?

Reply:

We thank the reviewer for the kind words.

This sentence is indeed not clear. We modified it as follows: “while 0% of the positive results were interpreted as negative.” (line 412 in the modified manuscript with tracked changes)

2. More broadly I am unclear about the difference between selective citation and citation bias. This is the main conclusion and features in the title of the article. I have checked the protocol but cannot see a definition of either citation bias or selective citation. To me this is the equivalent of not defining an outcome you are interested in measuring before conducting an experiment. To overcome this please can both of these concepts be defined and explained in the Background section, or even in the methods? This is an important part of the take home message, and whilst I can see that there is some evidence for increased citation rate depending on the conclusions, I am not sure how this weakly supports the assertion of citation bias, when the evidence for selective citation is strong.

Reply:

Both reviewers have raised this important point. We thank them for pointing out the unclarity and for the opportunity to improve our definitions of selective citation and citation bias. We have done so in lines 65-69:

“If this selection is not representative, but instead associated with specific characteristics of the cited literature, we speak of selective citation. Citation bias is a special case of selective citation (2). It concerns the selective citation of studies based on their outcome.”