Reply to email of 24-09-17 from Natalei Pafitis, BMC Public Health, regarding our publication "The global distribution of acute unintentional pesticide poisoning".

This latest announcement of the planned retraction reaches us almost two years after the discussion was started by an anonymous reader. It is clear to us that there is a member of the Editorial Board who, for some reason, is determined to pursue the retraction against all evidence. This persistent and prolonged effort is in addition to the previous violation of the COPE retraction policy, which states that publications should be retracted as soon as possible after the editor is certain that this is an appropriate action to take. We also note that this announcement is being made just prior to the meeting of the Persistent Organic Pollutants Review Committee of the Stockholm Convention, as was the case last year, when we received a reply to our counsel's letter. The unusual timing could be construed as an effort to influence the outcome of the meeting, where a highly hazardous pesticide is being considered for listing.

The envisaged retraction of our paper would be an unacceptable outcome of an unacceptable process. We have explained in detail in our rebuttals that the prevalences used in our extrapolations are not higher than annual prevalences and therefore no overestimation has taken place for this reason. We have repeatedly shown that even if the criticism were correct, it would have a negligible impact on the results of our study. A retraction would therefore be contrary to the journal's own policy, which states that retractions are not appropriate when there is inconclusive evidence to support a retraction.

Regarding the editorial board member's comments, we must reiterate that at no time did we argue that there was no difference between an "ever" and an "annual" prevalence of poisoning. In contrast, our publication devotes a full page to discussing the challenges of estimation, including different case definitions and at-risk times, apparently to the satisfaction of the reviewers. We also made this a major issue in our rebuttals. How can this lead the editor to feel that we have not addressed the time frame and "... not given enough attention within the manuscript"?

Your email says "With regard to the original concerns raised by Dunn et al:" but what follows is not from their letter. On the contrary, it is obviously taken from the "anonymous French" reader's email to BMC Public Health, which postulates an overestimation by "a factor of 30-40". The editor seems to have made this his opinion, as no quotation marks are used. We have pointed out from the beginning that there is no source or reference for this factor. It is mathematical nonsense, and neither the reader nor the editor has ever explained the idea on which it could be based. How can this be made a reason for retraction? It is on the contrary, the anonymous French reader's assertions that should be retracted.

Another comment says "The additional Editorial Board Member we have consulted agrees that one study based on self-reporting data does not represent the true prevalence as this varies year by year". This is a confusing statement. Does this argue against self-reported data, or against an annual prevalence or both? Depending on what it is thought to mean it might stand against about 100 years of epidemiological study science and practice. How can this be made a reason for retraction? A minimum requirement when seeking grounds for a retraction is to provide detailed arguments, supported by references. This comment appears to be in favor of making no effort to estimate, because the data are variable. That is not science.

The issues of small sample size and representativeness have been raised before. These were also addressed in detail in our publication. Please note that the majority of studies used a representative design and we made this part of our quality assessment. We also subjected countries with poor coverage to a sensitivity assessment. The results show that this did not change the extrapolations. However, we would have preferred to base our estimates on even better data. In general, the question is what to do with incomplete data. For France, for example, excluding the country because only 'ever' prevalences may be available would mean that UAPP would not be taken into account although it had occurred, otherwise there would have been no cases at all. Including the country could mean that UAPP might be overestimated, which we do not know for sure because a history of poisoning does not mean once in a lifetime. So, the trade-off is between a certain underestimation and a possible overestimation. We've chosen to do the latter and to be transparent about the approach and the limitations. This is the way science should work, and the peer reviewers supported this approach.

The envisaged retraction note has changed again. The mention of Dunn et al. was not in the last retraction note, and now it is there again. The retraction note is incorrect, because Dunn et al. do not raise the question of “ever” prevalences at all. The proposed retraction note has changed at least three times during this discussion, without any new arguments being put forward. The arguments that have been provided are weak, and do not further scientific inquiry in this area of study. Finally, it would be completely unacceptable to cite Dunn et al. but not our response to that letter, which addressed each point, and indicated that Dunn et al. are wrong on several points. To do so would be lacking in integrity. As we have previously requested, there can also be no retraction without a link to our rebuttals.

2024-09-23 Wolfgang Bödeker, Meriel Watt, Peter Clausing, Emily Marquez