"The global distribution of unintentional acute pesticide poisoning: estimations based on a systematic review" – article published in BMC Public Health 2020 (https://doi.org/10.1186/s12889-020-09939-0)

Rebuttal by the authors of a retraction by the journal's editor

1 Background

We, the authors of the above-mentioned article, were informed by BMC Public Health on 24.04.2023 that they plan to retract the paper. The decision was reportedly taken based on one Editorial Board member's assessment. The retraction notice sent by BMC Public Health was as follows:

"The Editor has retracted this article because concerns were raised about the use of 'ever' prevalence of pesticide poisoning to represent annual frequency in the extrapolations by a reader and by Dunn et al. [1]. Expert assessment has confirmed the validity of this concern and also concluded that the assumption of annual exposure for countries where the time frame is not reported is unreliable. The Editor therefore no longer has confidence in the results and conclusions presented."

Following a request by the authors for additional information we were informed that the Editorial Board member and the reader had concerns not just about the data of France but also with respect to some other countries. Following a further request by the authors the names of these countries were provided. No analysis by the Editorial Board member or reader was provided for these additional countries.

The letter to the editor (LTE) by Dunn et al.[1] mentioned in the retraction note is by employees of Bayer and Croplife International. The LTE was published one and a half years ago and was responded to by us authors at that time.[2]

The concerns assume that generally an overestimation of poisonings cases occurs by using a reported "history of pesticide poisoning" or poisoning in an unspecified time frame for annual estimations. This assumption is wrong. Acute poisonings are by definition bound to a reasonably short time span (e.g. 24 h) after exposure. Acute poisonings can occur repeatedly when exposure occurs repeatedly, so many times in a year. Pesticides can be applied on a weekly basis by the same persons, for a number of crops. For example, Tomenson & Matthews (2009) – an industry-led international survey on pesticide poisoning – reported for Cameroon that within 12 months there were 1418 incidents by 154 users, so 9.2 cases per person per year. In general, there is little interest in studying lifetime incidence in surveys of acute intoxications and it is reasonable to assume that respondent's reports of acute poisonings refer to the repeating periods of pesticide application.

We discussed at length in our article the heterogenicity of the included studies as well as the consequences of low data-coverage of countries and provided results from sensitivity analysis. This was obviously to the complete satisfaction of the peer-reviewers of the paper.

In what follows, we first give detailed answers to the critique on using data on France and the other countries for which the Editorial Board member and reader had concerns. We show that this critique is unfounded and false and-- if true-- the effect on our results would be negligible. We furthermore rebut the newly forwarded critique by the said Board member and finally turn to the procedure by

[1] https://bmcpublichealth.biomedcentral.com/articles/10.1186/s12889-021-11940-0
[2] https://bmcpublichealth.biomedcentral.com/articles/10.1186/s12889-021-11941-z

which the journal managed the critique and point to the scientifically unsound exclusion of authors and expertise in this process. A rebuttal, including the information below, was sent to the journal. This version has been updated. Finally, we rebut the retraction also due to the fact that two of the three parts of our article consist of a state-of-the-art systematic review and the analysis of a routine database, neither of which are addressed by the critique at all.

2 The first critique in detail

We provide a detailed inspection of the critique on the inclusion of studies from France and several other countries, and our comments on them.

2.1.1 The "reader" critique

Critique taken from the reader's email to BMC Public Health as reported on 03.01.2023 to the authors	**Reply by authors**
"I was rather surprised by the very large number found, and decided to have a specific look at the estimate for my own country, France. Indeed, the paper states an estimate of 7 fatal poisoning every year. See Table 7 on… and 139,357 non fatal poisoning"	The figures are wrongly cited. The figures mentioned are not for France but for Western Europe.
"These numbers are way larger than current estimates generally agreed in France."	Where does this come from? Which estimates? Are there any documents supporting this claim? Neither the information nor the source of the information have been provided so the claim cannot be examined or included.
"The paper states 'A history of pesticide poisoning was reported by 845 individuals (6.1%) among the 13,900 who completed the information (89.7%). ' Thus, a few percent of the respondent have declared to have suffered UAPP once in their lifetime. It appears that the authors of the paper have used this percentage as if it was representative of a yearly frequency, thus increasing the frequency of poisoning (and therefore the number of yearly UAPP) by a factor of 30-40."	This is a wrong assumption, since a "history of pesticide poisoning" logically does not mean "once in a lifetime". A history of pesticide poisoning also includes a person with acute poisoning in every year-- or even several times per year. All such incidents would be counted just as one poisoned person, if a poisoning history was reported. In fact, the exact question of the study referred to, by Baldi et al., was "Have you ever been intoxicated by a pesticide?" with answer categories: "Never - Once - Several times. If yes, in which year(s)" 3 answers were possible. The said study reports the prevalence of poisoning with no indication that only the "once"-category was analysed. The enrollment phase was 2005-2007. The "factor of 30-40" mentioned by the critic is without references and therefore without validity.

Even if this criticism was justified, deletion of the French data in total would change our global estimations by 0.04 %! The critique is not just wrong but is overall negligible to the estimate.

2.1.2 Critique on other countries

After repeated inquiry, we received information on further countries of concern to the Editorial Board Member by email from the team Manager, BMC Series from 27.04.23: "The other countries on where concerns have been raised by the reader and Editorial Board Member include the UK and Cameroon where 'ever' prevalence has been used as an annual estimate. Furthermore, the concerns also flag that you have assumed annual exposure where the timeframe is not reported in other countries (e.g. Nigeria, Tanzania, Zimbabwe)."

We provide a detailed exploration of the studies with respect to the above-mentioned countries and highlight sections relevant for the time-frame of exposure.

(i) Countries with alleged "Ever" prevalence of poisonings

Cameroon:

For Cameroon our national estimations could be based on 5 surveys, including one strictly reporting an annual prevalence. We used the overall mean prevalence of 49 %, which is lower than that of the pesticide-industry study by Tomenson and Matthews (2009) reporting annual prevalence for Cameroon.

Achancho et al. 2019: 21% "… it was found that 21% of them said that they experience headache, after spraying".

Assokeng et al. 2017: 39 % "As far as discomforts of gardeners are concerned, various health problems were observed during handling: headache, transpiration, cold, burns and eye aches."

Pouokam et al. 2017: 40.3 % "Concerning themselves, 158 farmers interviewed declared to have experienced at least one case of pesticide accident during manipulation."

Tandi et al. 2014: 84.9% "Most farmers (85.0%) reported at least one symptom of acute pesticide poisoning following spraying."

Tomenson & Matthews 2009: 59% "… shows the percentages of users experiencing incidents in the last 12 months."

UK

For UK, our national estimations could be based on only 1 survey. Deleting UK data would reduce our global estimate by 211,580 non-fatal cases, which translates as 0.05 % of the estimate; and this in itself would provide an error as acute unintentional pesticide poisoning does occur in UK, as documented in this study and others that did not meet the systematic review criteria.

Solomon : "whether any of 12 listed symptoms had ever been experienced within 48 h of using such pesticides "

(ii) Countries with alleged not reported time frame

Nigeria:

For Nigeria (Africa Western) results could be based on 3 surveys. We used the overall mean prevalence of 60.9 %, which is lower than that of studies strictly reporting cases occurring during use of pesticides. In the following studies, the various surveys asked farmers what symptoms they experienced after using pesticides, and surveys were reported as being taken in a specific year, or reported the month and year.

Bassi et al 2016: 42% "Thus, clients present with multiple finding or symptoms. In this study most farmers experienced chest pain/tightness, cough, headache, dizziness, reddening of the eyes; sneezing and rheum more often".

Oluwole & Cheke 2009: 91.3% "For the human health effects, only acute symptoms that appeared within 48 hours of pesticide sprays were considered… Each interview took about 15–25 minutes to complete and all were conducted during March 2008." And: "By asking the farmers if they experienced any health weakness (discomfort) in their day-to-day handling of chemical pesticides. A majority (91.3 per cent) responded that they or someone in their family had suffered from pesticide-related health symptoms during or after application of pesticides."

Ugwu et al 2015: "One hundred and one (101) farmers corresponding to 74% of the sample reported having experienced at least one of the symptoms on occasion of pesticide handling." Data reported in this study was collected in 2014.

Tanzania

For Tanzania (Africa East) results could be based on 4 surveys including one reporting annual prevalence and one reporting 3-month prevalence. We used the overall mean prevalence of 76.4 % which is comparable to studies strictly reporting an annual prevalence or shorter.

Da Silva et al. 2016: 61 % "Pesticide users were asked if they had experienced the symptoms during or soon after direct contact with pesticides. To be counted as a pesticide-related symptom, the exposure had to be direct contact, and the symptoms had to occur on the same day or the next day. We also asked for the frequency of experienced acute health symptoms."

Lekei et al 2014: 93 % "Approximately 93% of respondents reported previous poisoning by pesticides in their lifetimes (past year inclusive) with frequency ranging from 1 to a maximum of 7 times; 76.4% of the poisoned respondents reported two or more poisonings and 63.5% reported 3 or more poisonings at some point in the past. The 112 farmers with past APP reported approximately 432 past poisonings in total."

Manyilizu et al: 76.6% "Every disease symptom out of 12 (symptoms) had occurred to an average of 51% (66/128) farm workers in the past three months."

Tomenson & Matthews 2009: 74.8 %. "… shows the percentages of users experiencing incidents in the last 12 months." Good example for multiple intoxications. 154 users experienced poisonings and reported 1418 incidents, so 9.2 per user and year."

Zimbabwe

For Zimbabwe (Africa East) results could be based on 1 survey. This adds approximately 2 million non-fatal poisonings to the Africa East estimation of 51 million.

Magauzi et al. 2011: 45.1 % "We assessed the health effects of agrochemicals in farm workers in commercial farms of Kwekwe District (Zimbabwe), in 2006… Forty-five percent of the participants stated that they had suffered some multiple symptoms at one point in time that they knew or suspected to have been caused by pesticide exposure".

3 The second critique in detail

We sent BMC Public Health the above detailed comments on the detailed critique pointing out that the reasons for the planned retraction are scientifically wrong, the procedure lacking transparency and excluding those most involved - the authors – until a late stage of the process.

The team manager of the journal informed us on 24.05.23 that they have "now received further advice from an Editorial Board Member on your rebuttal below in response to our announcement that we will be proceeding with the retraction of your study from *BMC Public Health.* The comments are pasted below." The following table details these comments and our replies.

Comments of an Editorial Board Member	Reply by authors
"The Tomenson & Matthews (2009) study cited in the response does indeed show that in Cameroon, there were an average of 9.2 events per person per year. However, this rate is among the highest of all presented (if not the highest). Across all countries, there was an average of 4.4 incidents per year in those users who experience symptoms. Regardless, I'm not sure of the significance of this argument, as this indicates that the same individuals are experiencing UAPP multiple times, and so each poisoning event is not a unique 'case' that can be extrapolated to the population."	The Board member rightly admits that there can be multiple poisoning cases per person per year. This means that a lifetime prevalence cannot just be divided by the number of years of exposure to arrive at an annual prevalence as the critique seems to assume. We did not take the frequency of cases for a prevalence and none of the studies included did. Our extrapolations were not based on the frequency of cases. The Board Member might wish to consult the methods and discussion section of our paper.
"It is difficult to find information on the ratio of 'ever' to 'annual' poisoning, which would more usefully inform this argument. I was able to find one study which was published some 35 years ago (Jeyaratnam et al 1987) which showed that 13.8% of Indonesian pesticide applicators had ever experienced poisoning, but only 0.3% in the past year. The ratio was smaller in other countries (e.g. Malaysia, 14.5% ever versus 7.3% in the past year). This seems to suggest that using an 'ever' prevalence to denote annual frequency may result in an overestimation."	The Board Member might wish to consult our paper where we provided more studies. The Jeyaratnam paper could not be included in our review as it is outdated analysing data 40 years old! However, the Board Member in citing the Indonesian data should have taken advice from the paper: "The low result from Indonesia (0.08 %) for poisoning in the preceding year is atypical, compared with the previous years, and is probably the result of an interview error." (Jeyaratnam et al 1987) However, we never stated that an "ever" prevalence could not lead to an overestimation of annual prevalence of poisoning, see again our discussion section. The question here is if such an overestimation has taken place. We detailed above that for most countries mentioned as candidates for overestimation that the prevalence used in our extrapolation are not higher than "true" annual ones and therefore no overestimation has taken place. For countries without a strict annual prevalence the effect on the extrapolations is negligible. Has the Board Member a comment on this?
"Further, another more recent study (Negatu et al, 2018, doi 10.1136/oemed-2017-104538) showed that of the 41 respondents who had ever experienced poisoning, 71% had experienced it once, 22% twice,	We fail to see how the Board Member came to this conclusion. It is not based on the paper which states "Our study reported similar APP prevalence (16%)

and 7% three times. Granted, this is a very small sample size, but it seems to suggest again that using 'ever' poisoned to represent 'annual' rates will overestimate (given that most applicators will presumably use pesticides for many years, and so three times ever does not translate into once a year)."	when compared with those reported in studies in low and middle-income countries in Asia (11.9% to 19.4% among pesticide users) **despite differences in APP case definitions (ie, 'ever suffering from APP'** …"). (Negatu et al. 2018)
"Related to my specific concerns around the Cameroon and Tanzania estimates, I'm unclear as to why the authors used an average of studies when they had an annual estimate available. Regardless of whether this was higher or lower than the average, would it not be more defensible to use the annual estimate?"	No, it would not. The Board Member again could have got the idea from our paper. Poisonings results from exposure, exposure results from pesticides and pesticide use which in turn result from the kind of crops and the way how they are grown. This has tremendously changed in the last decades leading e.g. to an increase in global pesticide use. In order to arrive at an up-to-date picture we could not rely on data 20 years old. Again, our averaged prevalences of pesticide poisoning in Cameroon and Tanzania were not higher than strictly annual ones.
"There seems to be an argument being made by the authors around the year of data collection, in particular in response to the Nigeria comments. However, the year in which data were collected is of little consequence to this issue; instead, the question used to gather the data and whether a timeframe was specified in that question is of interest."	We did not emphasize the year of data collection but that the symptoms reportedly showed up during or after pesticide exposure. This is the classic definition of acute poisoning. When poisoning followed exposure and exposure happens regularly - even several time per year - this is the basis for an annual prevalence. Again, for Nigeria we used the overall mean prevalence of 61%, which is lower than that of studies strictly reporting cases occurring during use of pesticides.

Our above given response was sent to the journal on 17.07.23. We received no answer to these points, but a new retraction note was announced on 19.10.23 now saying:

"The Editor has retracted this article because concerns were raised about the use of 'ever' prevalence of pesticide poisoning to represent annual frequency in the extrapolations. Expert assessment has confirmed the validity of this concern and also concluded that the assumption of annual exposure for countries where the time frame is not reported is unreliable. The Editor therefore no longer has confidence in the results and conclusions presented."

In contrast to the first retraction notice citing the critique by a reader and by Dunn et al., this notice no longer gives the original reason for retraction.

4 Response to the second critique

On 24-09-17, almost one year after the last announcement of a retraction note, we received the following email by BMC PH.

"Please accept my apologies for the delay in contacting you regarding the ongoing investigation on your above publication in *BMC Public Health*.

Having discussed your attached rebuttal letter with our Editorial Board Member as well as having sought the advice of an additional Editorial Board Member at this stage, I am sorry to inform you that we will be proceeding with the retraction of your article as we have lost confidence in the conclusions presented based on the original concerns raised in the Matters Arising article by Dunn et al. and on the assessment and advice received from our Editorial Board Members.

The Editorial Board Member who we consulted originally states that their concerns regarding the use of an 'ever' prevalence to denote annual prevalence for a number of countries in your analysis are still valid; that this is an assumption which was unfortunately not given enough attention within the manuscript.

Regarding the Editorial Board Member's previous comments which you have responded to in the attached letter:

- *"Further, another more recent study (Negatu et al, 2018, doi 10.1136/oemed-2017-104538) showed that of the 41 respondents who had ever experienced poisoning, 71% had experienced it once, 22% twice, and 7% three times. Granted, this is a very small sample size, but it seems to suggest again that using 'ever' poisoned to represent 'annual' rates will overestimate (given that most applicators will presumably use pesticides for many years, and so three times ever does not translate into once a year)."*
    - The Editorial Board Member states this comment was to illustrate that 'ever' does not equal 'annual', and the response you have provided unfortunately does not address this. While the Negatu paper does indeed state that "Our study reported similar APP prevalence (16%) … despite differences in APP case definitions", the Editorial Board Member feels that you have not attempted to address the timeframe of those prevalences (in both cases, the prevalence reported is an 'ever' prevalence)
- *"There seems to be an argument being made by the authors around the year of data collection, in particular in response to the Nigeria comments. However, the year in which data were collected is of little consequence to this issue; instead, the question used to gather the data and whether a timeframe was specified in that question is of interest."*
    - The Editorial Board Member states that their comment here may have been unclear. They are referring to a past rebuttal where you stated that "surveys were reported as being taken in a specific year, or reported the month and year" presumably to indicate that this meant the prevalences reported were annual. Their latest comment was to indicate that this is not necessarily so – just because a survey is taken at a particular time, this does not mean the participants responded with recent information – the survey could address 'ever'. They do not feel that your response at this time addresses this concern.

With regards to the original concerns raised by Dunn et al:

The paper states "A history of pesticide poisoning was reported by 845 individuals (6.1%) among the 13,900 who completed the information (89.7%). »

Thus, a few percent of the respondent have declared to have suffered AUPP once in their lifetime. It appears that the authors of the paper have used this percentage as if it was representative of a yearly frequency, thus increasing the frequency of poisoning (and therefore the number of yearly UAPP) by a factor of 30-40. Indeed, their methodology is simply to multiply the "prevalence for having ever been poisoned » by the « farmer population », with no correction for the period during which the poisoning events are recorded.

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; that this has significant limitations to be used as a prevalence for the nation and does not represent the true prevalence of occupational poisoning for the whole country.

In addition, our concerns still stand regarding the fact that some countries (eg: France where you have limited to those who have declared to be affected by asthma) were covered by data on small samples sizes of specific study populations, which are not representative of the whole population.

In light of the above, and as we have lost confidence on the conclusions presented, I'm afraid that we will be proceeding with the following retraction of your manuscript:

The Editor has retracted this article because concerns were raised about the use of 'ever' prevalence of pesticide poisoning to represent annual frequency in the extrapolations by a reader and by Dunn et al. [1]. Expert assessment has confirmed the validity of this concern and also concluded that the assumption of annual exposure for countries where the time frame is not reported is unreliable. The Editor therefore no longer has confidence in the results and conclusions presented. All authors disagree with this retraction."

Reply of authors to the email of 24-09-17 from 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."

This email was answered by BMC PH on 24-10-04:

"Thank you for your response. We acknowledge that there have been delays in the process of finalizing a decision regarding the retraction of your article and would like to assure you that there is no agenda with regards to the timing of our correspondence with you. The delays have arisen due to the fact that there have been multiple discussions on the scientific issues raised with your study and we have had multiple experts describe how your analysis is incorrect during this time. We therefore stand by our decision to issue a retraction in this case in order to correct the scientific literature.

We will therefore be proceeding in this regard."

Email reply by us authors:

"It has become a feature of PMC PH communication that we authors find out about important issues through incidental remarks in emails. While so far all comments have come from the notorious "Editorial Board Member" and - according to your last email - an "additional Editorial Board Member", we are now informed "The delays have arisen due to the fact that there have been multiple discussions on the scientific issues raised with your study and we have had multiple experts describe how your analysis is incorrect during this time". Unfortunately, again, no details are given of this process and the results. It is essential that the written statements of these "multiple experts" are made available to us authors. It is unacceptable for a journal to lead discussions on scientific issues of a published article without transparent objectives, methods and rules of such a peer review. This is all the more so as the various errors made in the refereeing of critics and critiques in your communication undermine the confidence in BMC PH."

On 24-10-09 the article was retracted with the above mentioned wrong retraction note.

5 Comment on scientifically unsound action and process of BMC Public Health

There was no information provided to the authors about any kind of investigation into our paper, nor about an additional critique, prior to being informed about the planned retraction. We, the authors, were piece-wise informed about the reasons, only after repeatedly asking for them. We are informed that a single Editorial Board Member was consulted, and now this evaluation is the basis of the retraction. We learned by email on 26.04.2023 "… that the details of the investigation remain confidential."

Editors in general have several ways of handling articles in dispute:

- inviting the critic to a correspondence making the dispute open to the scientific community,
- starting a new review process to make sure that expert's knowledge is involved,
- considering need for a correction of a paper.

None of these options have been used by BMC Public Health. The Editor assumes that the reader's critique has escaped the attention of several reviewers during the review process.

The response by the journal to our rebuttal against the plan to retract did not take up our arguments, even in these cases where the editor is proven to be wrong. Large parts of our rebuttal go unmentioned.

6 Rebuttal in general

The editor of BMC Public Health is obviously not aware of the overall structure of our article. The paper consists of 3 parts:

1. a systematic review of the literature, carried out and reported by PRISMA standards,
2. an analysis of data from WHO cause-of-death database for fatal poisonings,
3. a synopsis of surveys on non-fatal poisonings.

We know of no critique concerning the first two parts, still the retraction of the paper affects these parts and suppresses important results based on scientifically sound methods. We furthermore have shown that the mentioned critique to part 3 is unfounded. In the best case, this discussion should be open to the scientific community—an important exchange which is suppressed by a retraction.

Studies like ours, making use of the best available data, are standard practice in an effort to generate information that may allow for the appropriate directing of resources in the interest of public health and harm prevention. In global health, acting on the precautionary principle in order to save lives means that waiting for perfect estimates is not ethical, nor is it feasible. Withdrawing the paper is doing more harm than good, which is against the ethos of global health and undermines the integrity of the scientific process.

The retraction of our paper is an unacceptable result from an unacceptable process. We have shown that even if the critique was right, it would affect the results of our study negligibly. The decision to retract therefore is in contrast to the own policies of BMC Public Health, saying: "On rare occasions, when the interpretation or conclusion of an article is substantially undermined, it may be necessary for published articles to be retracted" and "Retractions are not usually appropriate if: … An editor has inconclusive evidence to support retraction…"

10.10.2024 Wolfgang Bödeker, Meriel Watts, Peter Clausing, Emily Marquez