Reviewer's report

Title: A novel method to estimate exposure reduction in hospitality workers after a smoking ban

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Reviewer: Anke Huss

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Re: A novel method to estimate exposure reduction in hospitality workers after a smoking ban

In their article, Rajkumar et al describe a large measurement series to assess exposure to second hand smoke (SHS) in Switzerland. They applied stationary as well as personal measurements, and took samples before and after the introduction of a smoking ban. The data are interesting especially given the change in legislation and its impact on exposure levels that occurred in that time frame. While I have several comments, they all pertain to how the data were presented and discussed, not the quality of the study itself.

Major comments

1) The focus of the paper is on both the comparison of different measurement methods as well as on the change in SHS exposure levels as a result of the smoking ban. One could assume that saliva measurements are closer to a “gold-standard”, given that this method would be expected to more closely reflect the actual dose received by exposed workers. However, in this case, measurements were not taken within a time frame that made these measurements informative regarding the impact of work-related exposure to SHS. Most of the exposure levels presented in the paper are therefore not comparable and it is unclear why the comparison of the methods as such was turned into the focus of the paper. The authors could instead emphasis more the change in exposure levels over time, which will still make it an informative paper.

2) Table 1 should be restructured, as it is partly not informative, and partly confusing. Why were intervention group data compared to “all”? This makes it very hard to compare and understand the results. The data should be split into intervention group and the different control groups separately. Also, control group II are the non-smoking venues of which there was no follow-up. Where these measurements used in the baseline calculation? If yes, why was this done, given that the table highlights the impact of smoking legislation on SHS exposure in exposed (=smoking venues) at baseline?

The numbers do not seem to add up. The authors describe they sampled 167 venues at baseline, but only data of 50 are shown in the Table, what happened to the remaining 117? Did the data get censored if there were no corresponding personal measurements? If yes, this should be better explained. It is also unclear
why this was done (if this is what happened here).
Conversely, at follow-up I, data of 40 work place badges were evaluated for the intervention group and 42 for “all”. However, according to Figure 1, there were only 37 intervention venues with measurements for follow-up I. Did the duplicate badges that were performed in some of the venues erroneously end up in this calculation or were workplace badges counted double if they corresponded to several workers? Also, in this case, did only 2 additional venues contribute data for the non-intervention group? Then these numbers are not informative. It would be more interesting to see the summary data of all venues as well as of all personal measurements. The authors could also do an analysis to check if venues without personal measurements differed from those with personal measurements, which would also be interesting to learn.

3) On p13, the authors write that there were strong differences in levels of SHS exposure depending on the type of smoking regulation in the venue as well as according to the type of venue. Did the authors consider analyzing these factors in a regression analysis to see if they were still relevant if adjusted for each other?

Minor

4) Please explain abbreviations the first time they occur in the full text (MoNIC, liq-liq, GC-NPD, SRNT).

5) If the MoNIC badge was validated, it would be good to give a reference. If not, it needs to be discussed what that means.

6) The authors could list the eligibility criteria for venues and workers.

7) On p10 it says “Initially eligible study participants” – who was only initially eligible and not any more at a later time point?

8) On p11 it says that “if two badges were placed in a venue, we used the values from the smoking section for our calculations as waiting staff spends more of their time there”. Did the authors assess time spent in different compartments or how did they get to this conclusion?

9) P11, last paragraph, describes that a time-weighted average was used. Was this time-weighting also applied for the number of hours that a venue was open on each day? It is also unclear how the correction factor of 1.75 came about: What is this factor based on? It would be good to give somewhat more explanation here.

10) On p12 it says that “the type of smoking legislation in place clearly influenced SHS exposure” – did the authors mean “regulation”, given that this refers to baseline?

11) The last paragraph on p13 repeats the table and could be shortened or omitted.
12) On p16 it says that the salivary measurements “would have been more reliable if participants were sampled immediately after leaving the workplace”. The measurements would not have been more reliable, though, they would have just more likely reflected work-related SHS exposure. This wording should be adapted.

13) On p16, last paragraph, the authors state that they calculated a time-weighted average of the workplace badge and that, for this reason, workplace badges likely provided the most accurate measure of changes in exposure. However, accuracy as such was not evaluated and the wording should be adapted accordingly.

14) On p17 the authors say that their longitudinal study design minimized potential confounding, resulting in a higher study power when compared to a cross-sectional study. It would be good to learn what kind of confounding was minimized here. It is also unclear in how far the minimized confounding led to a higher study power.

15) On p17, the authors state that they sampled more restaurants than bars and that restaurants had lower levels of SHS so that, consequently, an underestimation of the true SHS levels was highly likely. It is somewhat unclear what the authors want to say here. Do they want to express that type of venue biased the comparison of CE/day between smoking regulations (this could be easily addressed in a regression analysis, see comment above). Or do the authors want to express a kind of average exposure when working in a restaurant or in a bar? Because there likely ARE more restaurants than bars and the authors could probably weigh their values by the ratio of restaurants/bars if this is what they want?

16) In the conclusions the authors state that the “time-weighted average of the workplace badge turned out to be the most reliable method to determine changes in personal SHS exposure”. This is an overstatement, given that reliability as such was not assessed.

17) Reference number 26 is corrupted.

**Level of interest:** An article of importance in its field

**Quality of written English:** Acceptable

**Statistical review:** No, the manuscript does not need to be seen by a statistician.

**Declaration of competing interests:**
I declare that I have no competing interests.