

Review of the Preprint

Long-range impacts of biomass burning on PM_{2.5}: a case study of the UK with a globally nested model

Dear Damaris Y. T. Tan and colleagues,

This work is quite important, well-structured and has great scientific quality. I would most likely use it as a reference. The following is my review of this paper, which I tried to read and comment as well as I could. I think it should be published. However I also think that some minor revisions would benefit it greatly and make the work more robust and accessible to everyone. Thank you for your contribution.

Best regards,

Tobias Osswald

General Comments

This paper contributes to our understanding of the transport of BB emissions across the world, focusing on a country that has very few forest fires, but that still seems to be impacted by them. It is significant for the society at large, and points out that even countries like the UK, are unexpectedly affected by wildfire smoke, coming not only from Europe, but also Asia and North America. As such I consider the topic to be relevant. The authors focused on PM_{2.5}, a valid choice, as this pollutant is well known to be a risk to human health and is associated with BB.

The method relies on a chemistry transport model (CTM) called EMEP4UK, which was run for a total of 2 years in a nested configuration, from global to national level, that represents a notable computational effort (How many cells in the horizontal directions were used for each of the domains? I find this information useful for the reader to get an idea, please include this in the text). Several runs were carried out where each one considered the BB emissions from different regions of the world. A base run without BB emissions was also carried out. The differences in concentrations between the different runs allowed the authors to assess which regions of the world contributed the most to the air quality degradation in the UK. This is a smart and thorough method and a great advantage of CTMs in studying these

phenomena. It allows the authors to consider higher-order effects taking part in the dispersion and chemical transformations of smoke in the atmosphere.

The methodology section often points to appendix B, where a deeper description of the model and its comparison to air quality stations is available. The EMEP4UK global model version used here had not been used elsewhere before, and so **should in my opinion have a more prominent place, not in the appendix but in the methodology section**. Its comparison to real world data is also very relevant for the paper.

There is one possible flaw in this methodology however, which was not pointed out. Plume rise is not being modeled at all, as is done for example in CHIMERE or other CTMs, where a plume rise height is calculated and a plume rise profile applied. Instead the smoke is assumed to be homogeneously mixed in the ABL as soon as it is released. **I wonder if the authors can defend their methodology in this regard in their reply to this review? Weather this is possible or not I think that some considerations about this topic should be done in the paper.**

In the discussion the authors went into a higher detail and assessed the secondary production of PM_{2.5}, its chemical composition, the population-weighted average concentrations among others. I would prefer a more focused and direct approach, discussing only the results that are actually relevant for the main conclusions of the paper. Particularly in the discussion on sections 4.2 and 4.3 I struggled in understanding the concepts and the plots presented by the authors. When the authors mention area-weighted average, they mean what is the most intuitive average (on the other hand, an average value that was presented as such with no further description and that was not area-weighted would be misleading, would it not?), and so I would **remove the term area-weighted**. I would also **remove most of the data and discussion about the population-weighted average**. Though this can be mentioned once, as it may be interesting as a first-estimate of the impact on human health, it seems to me that this is not the focus of the authors, which is why there is no mention of this in the conclusions. If the authors do not agree with me on this then I would ask them to explain to me in their reply why this is relevant in this paper, and to what degree.

Also in the discussion a particular episode in April was pointed out. During that month there was an abnormally high concentration not only of PM_{2.5} (BB), but also from other sources. I am curious to know what was the reason for this. What caused the episode of bad air quality from other sources and where did the BB emissions come from? I do not think that more than a sentence should be added to the text, as this is

not the main point of the paper, however, being an abnormal peak, I think that a concise (but still deeper than the current one) explanation of it would be better.

In line 229 I could not understand how the authors could see from the plots the tendency they mention. Could I have this explanation in the reply to this review, and if necessary also in the paper?

In section 4.3 an interesting extrapolation of the data to other years is carried out. However I would not put too much emphasis on it as it may have significant errors due to the presented assumptions and may also propagate errors inherent to the main methodology for 2019. For this reason I think that the text here should be more concise (about half the characters). Also in this section one of the assumptions that is correctly identified is that the proportions of the emitted pollutants stay constant (I believe the issue here is because other pollutants can impact the PM_{2.5} production or destruction, correct?), and then in parentheses “there is no reason why these proportions should vary substantially”. Possible reasons would be changes in fire intensity and fuel load. I would say that there are reasons for why this could change substantially. However, the authors’ assumption is also reasonable, since they are accepting that the extrapolation has its limits, but is still as good of an estimate as possible. I would therefore remove the sentence within parentheses.

In the conclusion the authors point out how relevant it is to mitigate anthropogenic emissions, as these will have an impact in PM_{2.5} (BB), the latter being a significant driver of air quality in the UK. This section once again points out the relevance of this work. In line 324 it is pointed out that the importance of PM_{2.5} (BB) is likely to increase in regard to total PM_{2.5}. This is because there will be more burning and less or equal anthropogenic emissions. However, could the decrease in SIA counterbalance the increase in BB emissions? Could this effect be so strong that the relative impact of BB emissions to anthropogenic emissions on air quality remains the same? I don't expect that this can be answered without further calculations, but I would like to know the opinion of the authors in reply to this review.

Technical Aspects

There were other, more technical aspects of the paper where I would like to point out some issues I found and that I think should be addressed:

Line 123: The phrase that begins here is ambiguous, hard to understand. Could it be that the authors are trying to fit two sentences into one? Could you explain what is meant here on your reply and rephrase in the text.

Line 125: The population-weighted means seem out of place here, and indeed they are not really part of the sensitivity study. However, as previously noted, this variable should probably be omitted altogether.

Figure 2: This way of showing the numbers of the regions is not the most intuitive, but it does its job. A legend box with colored squares next to the numbers would be more natural.

Line 142: Areas outside... This is already in the figure description, and is not relevant for the main text in my opinion.

Figure 4: I found this figure confusing, I may be misinterpreting some things. However I would like to see a new version that I can understand more easily. Perhaps changing labels and the description of the figure will suffice.

Figure 4a: What is meant by Total in the y-axis? How is this different from the concept of Mean in the y-axis of figure 4b? I found overall the usage of the word “total” confusing in figure 4. I also fail to understand where I can see the non-BB PM_{2.5}. Are the blue and gray areas stacked upon each other? Also in the text line 144 is confusing for the reasons mentioned above.

Figure 4b: What do the percentiles represent? How were they calculated? Are they calculated from the daily means of each cell? Is the mean for the domain done at each time-step and the percentiles calculated from those values? Is it something else? I would also remove the red curve.

Figure 4f: the chosen gray and purple are hard to distinguish at first glance

Line 181: remove the sentence that starts with “To the author’s knowledge...”

Line 186: Why 31/42? Is this relevant? The uncertainty of this value is probably more than 1% and so I think that 74% is an adequate enough representation of this fraction, and easier to grasp.

Line 280: “Error bar” is a term more widely used for the graphical representation. perhaps “margin of error”, or some other term would be more appropriate.

