Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "Can we use modelling methodologies to assess airborne benzo[*a*]pyrene from biomonitors? A comprehensive evaluation approach" *by* N. Ratola and P. Jiménez-Guerrero

Anonymous Referee #2

Received and published: 25 November 2015

1. The paper addresses the question of whether pine needles can be used as a proxy for measurement of atmospheric BaP. This is an interesting topic, and one relevant to the field of atmospheric chemistry and the study of atmospheric PAHs in particular.

2. While the method of biomonitoring of BaP is not novel, this detailed assessment of biomonitoring campaigns against modeled and measured atmospheric BaP is an important conceptual step to take.

3. The conclusion reached, that biomonitoring is effective for detecting the presence and spatial distribution of BaP, is significant and quantitatively investigated. The spatial distribution of BaP being reflected by biomonitoring is indeed supported by the results,

though at varying degrees depending on the season.

4. The scientific methods are presented in a clear manner. Specific comments on the methodology: a) A model was used as pseudo-reality, after being calibrated to measurements. This use of the calibrated model presents no problem provided that the model values are independent of the biomonitor-calculated values that they are compared to. The authors acknowledge that the pine needle-based estimates that rely on deposition velocities are therefore connected to the model estimates because the model also uses a deposition velocity value in the process of calculating concentrations. The authors argue that since the model does well in comparison to EMEP air measurements, that this model's deposition velocity is appropriate for the Iberian Peninsula. I believe that the comparison's reliance on this argument, and the independence of the model calculations and vegetation-based estimates, should be discussed further.

b) There are many sources of uncertainty in the EMEP measurements, the modeled concentrations, the biomonitored concentrations, and the methods of intercomparison. I wonder how these uncertainties limit the conclusions of the evaluation of the biomonitoring. The authors should at least comment on how the uncertainties involved qualitatively affect the evaluation, if not quantitatively estimate the effect of the uncertainties.

5. Overall, I believe that the results support the conclusions, but I believe that there is one point that must be further discussed/explored: In the comparison of calibrated model to biomonitors, it is found that the chosen deposition velocity strongly affects the quality of the fit. With such widely varying deposition velocities from the literature (the ones used in this study varied over orders of magnitude), there is much room for the selection of the deposition velocity to match a given set of measurements. I worry that this opens the possibility for overfitting in the model-biomonitoring comparison. Would choosing the same deposition velocity for this region over a different time period yield results that are as good? Some independent reasons to choose the deposition velocity 1d for the comparison domain would strengthen the choice, but the authors write

"none of the studies where the available approaches were reported used needles from the same pine species of the current study nor was located in areas of similar climatic or geographical conditions." (p26496 l27) Is the chosen method for biomonitoring (1d) robust? Does this deposition velocity make sense over the others for a reason other than the fit with the model? As the authors note, many factors describing the atmosphere, surface, pine needles, etc. contribute to the deposition velocity. I believe that a physical argument that the deposition velocity used in method 1d is at least reasonable in an order of magnitude sense for this situation would greatly strengthen the results and conclusions.

6. Experiments and calculations: the authors have clearly described their methodology in a way that I believe allows reproduction.

7. I believe that the authors have clearly described what is new to this work and given appropriate credit to previous studies.

8. It is unclear to me that the title reflects paper. The question that the paper is attempting to answer seems to be more along the lines of: "Can biomonitors effectively detect airborne benzo[a]pyrene? An evaluation approach using modelling"

9. The abstract accurately reflects the work, but needs some clarification of the final sentence. Do the authors mean to say that the model can be an effective predictor of air concentrations and values in vegetation, or something else?

10. The overall presentation of the paper is well structured and clear.

11. The language used in the paper is clear and concise, but requires some editing for english phrasing: e.g. p26497 l13: "This supposes a climatic viewpoint to the problematic of BaP..." 'supposes' should probably be replaced with 'highlights', 'displays', or similar, and 'problematic' should be replaced with 'problem'.

12. The use of formulae is effective, and units are clearly denoted where applicable.

13. Tables: - Table 1. Are the +/- values given with the means the variances? - Table

2. Same question.

14. The number and quality of references is appropriate for this work.

15. The supplemental information provided is appropriately detailed and clear.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 26481, 2015.