Geoscientific
Model Development
Discussions

Interactive comment on "InMAP: a new model for air pollution interventions" *by* C. W. Tessum et al.

C. W. Tessum et al.

tess0050@umn.edu

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Comment:

The paper discuss the formalization and implementation of a new, simplified model, to be used for the fast evaluation of emission scenario impact over a certain domain. The work is of great interest for the air quality fields, and the presented paper show the great effort performed by the authors in order to explain their idea and results.

Response:

We thank the reviewer for this comment.

Comment:

The main issues to be solved before publication that I noticed are related to the pre-

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sentation of the results (see specific comments).

Response:

We have made improvements to the presentation of results as described below.

Comment: The work is well structured but some issues have to be addressed before the publication in order (1) to help the reader to better understand the formalization and (2) to show a more exhaustive evaluation of the presented model. In particular: 1. In the introduction the authors should mention other model/approaches used to allow fast simulation of the scenario impacts on AQ. There are a number of work (and also European project) related to this, in to physical/nonphysical model. See fairmode.jrc.ec.europa.eu, www.appraisal-fp7.eu as examples.

Response:

We thank the reviewer for pointing out these resources. The manuscript contains a discussion of other available model types in Appendix A. Many of the model types discussed in the websites above are already discussed in Appendix A, but as a response to this comment we have added discussion of two additional model types: neural network models and chemical mass balance models.

Changes:

We add the following text to Appendix A in the manuscript: "...models based on neural networks or neuro-fuzzy systems (Carnevale et al., 2009)..."; and "Chemical mass balance models (e.g., CMB (US EPA, 2004)) estimate the contribution of different emissions source types to ambient pollution concentrations by analyzing the relative contributions of different chemical tracers and matching them to tracer profiles of known sources. This method is useful for estimating the contribution specific source types, but requires detailed location-specific measurements and can only track contributions from sources with known tracer profiles. Additionally, chemical mass balance models cannot directly predict how changes in emissions would impact concentrations."

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Comment:

2. The methodology is well explained but probably a table presenting what are the data computed pre-processing wrf-chem models for each of the section 2.1 will help the reader and the possible interaction of the authors with other group in the reproduction of the approach/test on other domain and with other models.

Reponse:

We thank the reviewer for this suggestion. We have added such a table.

Changes:

We have added a table describing how WRF-Chem data is used.

Comment:

3. The results are the part needing the major effort by the authors. (a) Even if the scenario are presented in other works, a resuming table presenting the emission change with respect a base case or the emission levels have to be presented here, in order to allow a better interpretation of the results.

Response:

In response to this comment and others, we have added a series of appendix figures showing the spatial patterns in concentrations for each scenario as predicted by In-MAP and WRF-Chem, the spatial patterns in differences between the two models, a description of the major emissions sources in each scenario, and scenario-specific performance statistics. Although this comment specifically asks for a table, we feel that more information can be gotten from the figures that we have included than could be gotten from a table with similar information.

Changes:

We have added a series of appendix figures showing the spatial patterns in concentra-

tions for each scenario as predicted by InMAP and WRF-Chem, the spatial patterns in differences between the two models, a description of the major emissions sources in each scenario, and scenario-specific performance statistics.

Comment:

(b) I suggest to start from the evaluation with respect the measurement (3.3) and then starting the larger discussion on emission scenario reproduction.

Response:

The presentation order suggested by the reviewer (first compare against measurements, then compare against other models) is a common approach, in part because typically the comparison against measurements is the main comparison point of a model and the comparison against other models is more of a side-comparison. Our paper describes an intervention model, which reverses the relative importance: the model-model comparison is the main point of our model and the main comparison we wish to make, whereas the comparison to measurements is more of a side-comparison. (The comparison to measurements is a use of the model that deviates from the main goals of the model. We feel it is an important comparison to include, but, again, it an aside rather than the central comparison.) As we describe in the manuscript, direct model-measurement comparison is not able to evaluate performance in predicting the changes in concentrations caused by changes in emissions. We considered swapping the order to meet this comment from the reviewer, but ultimately decided that the current order better reflects the importance and emphasis we wish to give to the two comparisons.

Comment:

(c) Figure 2: how the scenario selection has been performed? I suggest to add a differences map (i.e. with respect wrf-chem maps) allowing a better presentation of the differences in the results.

Response:

One of the scenarios was selected randomly for inclusion in Figure 2. In Figure 2, we elected not to include maps of differences because the figure contains four different models or model configurations and we were not able to find a way to include difference maps for all combinations without creating visual confusion. However, in response to this comment we have added a series of appendix figures which include maps of differences between WRF-Chem and InMAP-12km for each scenario. We have also added an explanation to the caption for Figure 2 that similar figures for the rest of the scenarios are available in an appendix.

Changes:

We added a series of appendix figures including maps of concentrations for all of the scenarios, including maps of differences between WRF-Chem and InMAP, as well as a corresponding discussion. We also added the text: "Figures S1–S12 provide similar information for the rest of the scenarios." to the caption for Figure 2.

Comment:

(d) I think the best way to evaluate this kind of model is comparing the responses with respect emission change. If you have a base case (used in 3.3?) you can show not the values of the index itself (population- or areaweighted) but how this change. In this way you can appreciate if/when the model shows a completely different from wrf-chem and/or cobra. For example, is the InMAP model usually more/less sensitive to the emission change of some PM2.5 precursors?

Response:

We thank the reviewer for this comment. Figures 3 and 4 in the manuscript shows the changes in population- and area-weighted concentrations for each scenario for total PM2.5 (Figure 3) and for PM2.5 concentrations resulting from each precursor (Figure 4). Figure 5 shows how these changes vary by region. We have additionally added

a series of appendix figures that show spatial patterns in concentration changes and scenario-specific performance metrics.

Changes:

We have added a series of appendix figures that show spatial patterns in concentration changes and include scenario-specific performance metrics.

Comment:

(e) A good way to evaluate models to be used for scenario analysis could be find in the frame of the fairmode planning working group. I suggest to at least cite this possibility, showing the differences in your evaluation approach, its limitations and strengths.

Response:

We thank the reviewer for bringing this to our attention, and we now cite the fairmode metric in the paper. We employ seven commonly used metrics for model performance evaluation (MFE, MFB, ME, MB, R2, MR, and S) and compare InMAP performance against performance criteria published by Boylan and Russel. These criteria and metrics are among the most commonly used in the US, and since the current implementation of the model is for the US only, we feel that these methods are appropriate for this manuscript. As the fairmode methods are presumably commonly used in Europe, we would plan to include them in evaluations of future versions of InMAP with a European or global extent.

Changes:

We added the text: "There exist other criteria for determining model suitability (e.g., those proposed by Thunis et al. (2012)) which could be explored in future research."

Comment:

(f) The impact of the correction factor could be investigated, for example adding a section on sensitivity of response to F and KNH (optional, on the basis of the length of

the paper resulting after revisions)

Response:

In response to this comment and others, we have redesigned the advection scheme and the ammonia chemistry algorithm to no longer include empirical coefficients. After these changes, the model no longer includes any empirical coefficients.

Changes:

We have changed them model so that it no longer contains empirical coefficients and have updated the manuscript accordingly.

Interactive comment on Geosci. Model Dev. Discuss., 8, 9281, 2015.