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Interactive Comment

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

Anonymous Referee #1

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Evaluation of "InMAP: a new model for air pollution Interventions" by C. W. Tessum, J. D. Hill, and J. D. Marshal

In the manuscript by Tessum et al., the authors present the development of a new modeling approach called InMAP and compare the results against WRF-CHEM. InMAP is a "expedited" approach for providing how longer term (here, annual average) pollutant concentrations will change in response to emissions changes (interventions) using an air quality modeling approach that can account for aspects of atmospheric chemistry and transport. Inputs in to InMAP come from an initial run of a more traditional air quality model, in this case WRF-Chem. The main advantages of InMAP appear to be the ability to have a variable resolution grid and more rapid execution to explore a suite of emission interventions. They evaluate the model, primarily for it's ability to recreate the results of WRF-Chem. They also use InMAP to simulate year 2005 concentrations, but that is not nearly so informative as the model is based on running WRF-Chem, first,

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extracting properties from there, and then assessing changes. Thus, the performance of InMAP against the observed concentrations is likely most determined by the base WRF-Chem simulation, and they state that "comparing InMAP against observed values represents a use of the model that is beyond what that model was designed for." (as such, maybe this part should be removed... it is actually misleading.) . General Comment: The method is rather clever, though the authors need to do a more thorough job of explaining how it is implemented before the community could become comfortable with the approach. Further, model evaluation should be more comprehensive given the identified potential uses (health studies associated with interventions).

In terms of more detail on model implementation, the authors do not provide enough detail on how the time integration is conducted. What is the order of process integration? Does it matter?

In terms of model evaluation, it would be interesting to do traditional advection solver tests, e.g., the rotating cone, except that the advection field is reversed after a rotation (See Walcek and Aleksik, Atm. Env 1998 for tests). The current model evaluation also should provide more information on if there are regional/temporal differences in the errors, as those can be important in heath studies. Further, how well does the model do at capturing the impact of changing point source impacts, e.g., from power plants. Is the behaviour of InMAP the same as WRF-Chem. It would be of interest if the sulphur dioxide and sulphate changes were the same. They should include a table of their assessment of how InMAP recreates the concentration changes from WRF-Chem for each of the eleven scenarios they have conducted. How this should be given is a plot of Delta(C)InMAP vs. Delta(C)WRF-Chem, for each of the major species of interest (ozone, sulphate, nitrate, black carbon, organic carbon, nitrogen dioxide), along with the regression information (slope, correlation). That would appear to be the most telling approach to assess how well their approach works. Their evaluation also does not link to the recent air quality model evaluation in Europe being conducted as part of the AQMEII initiative.

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Specific Comments:

Equation 1 should be set up to account for the multipollutant mixtures in the atmosphere. R should explicitly include gas-particle partitioning, and given that they are including aerosols, something about aerosol growth should be included/mentioned. Dry deposition is a boundary condition.

They present an interesting comment "InMAP's advection scheme accounts for variability in wind direction. For instance, for a location where wind travels West at 5 m/s and East at 5 m/s the other half of the time, InMAP's advection calculation in each time-step would include wind traveling both West and East at 2.5 m/s." Thus, it would seem that this would act as diffusion in the end. Now, let's think of an example that the wind coms from the NW at 5 m/s half the time and from the SE at 5 m/s the other half. If there was a point source in a grid, the impact of that point source should be along a NW-SE line. However, it would seem that in the current implementation, the point source would be diffused, and it is not apparent to me that you would retain the directionality. For the simple case they mention, what is the equivalent diffusivity (I calculate it to be U(dx): if this is true, this should be discussed in the text)?

It is a bit discomforting to have an empirical factor (FA in eqn 3) that, so far as I can tell, has no fundamental basis. In essence, doesn't this just make up for dividing the velocities by 2 because, on average, flows are 50% from each direction? Would this have to be re-determined for each application or is there some more general foundation for the choice.

In their appendix A, they should include OSAT and PSAT that have been implemented in CAMx. They also make an interesting comment about reduced form models "For this reason, these methods generally are not amenable to use by non-experts." Is this approach amenable to use by non-experts (particularly since it doe use an empirical adjustment factor)?

In the end, the authors present a new and potentially clever and interesting approach.

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There is still much to be done to provide the community comfort on whether the results are reasonable and could be used to assess interventions similar to those proposed. They need to further explore and communicate the limitations. The minimum that needs to be done before it can be reconsidered for acceptance:

1. In their test case, show what happens to the ozone, SO2 and sulphate plumes due to an individual power plant in the WRF-Chem and InMAP cases. 2. Do a more detailed comparison of the WRF-Chem and InMAP 12 km applications to assess the ability of InMAP to capture the impacts of changing mobile source emissions, e.g., plot and provide performance data on the change of concentrations on a grid-by-grid basis. 3. Follow the behaviour of the emissions of a point source in a windfield that is diagonal to the grid, but like they discuss, goes in one direction 50% of the time and in the other direction 50% of the time.

Without those it should not be accepted. Other issues are above, and the results of those tests may indicate other issues to address.

In response to specific review questions:

1. Does the paper address relevant scientific modelling questions within the scope of GMD? Does the paper present a model, advances in modelling science, or a modelling protocol that is suitable for addressing relevant scientific questions within the scope of EGU? Yes 2. Does the paper present novel concepts, ideas, tools, or data? Yes 3. Does the paper represent a sufficiently substantial advance in modelling science? Unclear at this time. More evaluation is required. 4. Are the methods and assumptions valid and clearly outlined? No... see the review. 5. Are the results sufficient to support the interpretations and conclusions? No... See the review. 6. Is the description sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? In the case of model description papers, it should in theory be possible for an independent scientist to construct a model that, while not necessarily numerically identical, will produce scientifically equivalent results. Model development

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papers should be similarly reproducible. For MIP and benchmarking papers, it should be possible for the protocol to be precisely reproduced for an independent model. Descriptions of numerical advances should be precisely reproducible. Very close. Could be better. 7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes 8. Does the title clearly reflect the contents of the paper? The model name and number should be included in papers that deal with only one model. Yes 9. Does the abstract provide a concise and complete summary? Too much time is spent on unnecessary material (background) without getting to the important/novel aspects of the model and the results. 10. Is the overall presentation well structured and clear? Reasonable. For exceptions see the review. 11. Is the language fluent and precise? Yes 12. Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Not totally, see the review. 13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? Reasonable. See the review. 14. Are the number and quality of references appropriate? Yes 15. Is the amount and quality of supplementary material appropriate? For model description papers, authors are strongly encouraged to submit supplementary material containing the model code and a user manual. For development, technical, and benchmarking papers, the submission of code to perform calculations described in the text is strongly encouraged. The supplementary material could be extended to provide additional performance information.

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

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