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Geoscientific Model Development Discussions
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Interactive comment on "A method to represent ozone response to large changes in precursor emissions using high-order sensitivity analysis in photochemical models" *by* G. Yarwood et al.

Anonymous Referee #2

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Yarwood et al. present a methodology for using HDDM sensitivities and ozone concentrations from model simulations at multiple emissions levels to estimate gridded ozone concentrations at any NOX and VOC level. They apply this methodology to ozone in 22 cities in 2006 and compare their hourly ozone estimates to two months of model predictions from simulations with different emissions inputs. Overall, the article is clear and well written and the methodology appears to generally sound. The method adds to the literature on applications of HDDM and will be of interest to GMD readers. I have some concerns about the robustness of the evaluation and the arbitrary nature of some methods choices. I recommend publication after the authors address the following comments.

Major comments:

1. The authors present this methodology as capable of predicting ozone concentrations at any combination of NOx and VOC emissions, yet their evaluation is limited to 3 cases in which VOC and NOX are modified by the same percentages (i.e. 25% NOx, 25% VOC). Since the method is developed based on two model simulations in which NOx and VOC emissions are scaled equally, equal percentage NOx and VOC emissions changes should be the most likely for the methodology to properly replicate. In order for the authors to make the claim that their methodology can really replicate any combination of NOx and VOC emissions, they must present evaluation of some "corner cases". For instance, the authors should test additional brute force simulations with NOx only and VOC only emissions changes and also unequal changes in NOx and VOC (i.e. 25% VOC, 75% NOx; 75% VOC, 25% NOx etc).

2. The choice of transition points in Equation 3c seems somewhat arbitrary. The authors state that they were based on results of performance tests but give no details and do not quantify these tests. Would the authors get better performance if they used 10% and 50% as their transition points instead of 15% and 25%? 10% and 50% seem like more natural choices. Were these tested? Was there some quantification method used to choose the specific transition points of 15% and 25% over other possible choices?

3. The authors should discuss in the text that the ozone estimates at varying NOx and VOC emissions levels are only valid for modeled time period. For instance, the authors modeled the year of 2006 so their results are not necessarily applicable to meteorology conditions that occurred in other years or that might occur in the future.

4. This technique, like most HDDM applications, is simply a way to recreate model predictions under a range of emissions conditions without rerunning the modeling system for every potential emissions level. It should be acknowledged in the text that the model itself has errors and unless the base conditions (100% NOx and 100% VOC) are explicitly evaluated against ambient data, the results are completely within the "model

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world" and may or may not accurately reflect how actual concentrations would respond.

5. This technique is evaluated for 22 cities, but no information is given for how cities were delineated. Are results based on model predictions for all grid cells within the city limits, all grid cells within the MSA, or by some other method? Please provide some description or map showing how the grid cells representing each city were delineated.

Minor comments:

1. Page 2590, line 10: References should be made to the actual articles which document the creation of the emissions (Pouliot et al) and meteorology (Vautard et al) inputs.

*George Pouliot, Thomas Pierce, Hugo Denier van der Gon, Martijn Schaap, Michael Moran, Uarporn Nopmongcol, Comparing emission inventories and model-ready emission datasets between Europe and North America for the AQMEII project, Atmospheric Environment, Volume 53, June 2012, Pages 4-14, ISSN 1352-2310, 10.1016/j.atmosenv.2011.12.041. (http://www.sciencedirect.com/science/article/pii/S1352231011013288)

*Robert Vautard, Michael D. Moran, Efisio Solazzo, Robert C. Gilliam, Volker Matthias, Roberto Bianconi, Charles Chemel, Joana Ferreira, Beate Geyer, Ayoe B. Hansen, Amela Jericevic, Marje Prank, Arjo Segers, Jeremy D. Silver, Johannes Werhahn, Ralf Wolke, S.T. Rao, Stefano Galmarini, Evaluation of the meteorological forcing used for the Air Quality Model Evaluation International Initiative (AQMEII) air quality simulations, Atmospheric Environment, Volume 53, June 2012, Pages 15-37, ISSN 1352-2310, 10.1016/j.atmosenv.2011.10.065. (http://www.sciencedirect.com/science/article/pii/S1352231011011605)

2. Page 2592, line 6: The first ME should be MB. The associated equation should be changed accordingly.

3. Figures 5-8: Increase the font size on these figures, the axes and regression line

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equations are unreadable at the current size.

4. Figure 6: Subplots b) and d) are unnecessary for this figure since they duplicate exactly Figure 5b and 5d.

5. Figure 8: Subplots b) and d) are unnecessary for this figure since they duplicate exactly Figure 7b and 7d.

6. Tables S1 and S2: Consider bringing these into the main paper.

Interactive comment on Geosci. Model Dev. Discuss., 6, 2585, 2013.