



***Interactive comment on* “Development of the high-order decoupled direct method in three dimensions for particulate matter: enabling advanced sensitivity analysis in air quality models” by W. Zhang et al.**

Anonymous Referee #2

Received and published: 13 December 2011

This paper describes the development of the high order decoupled direct method to enable advanced sensitivity analysis in air quality models. The use of techniques could enable much more efficient analysis of the key uncertainties associated with existing prognostic models, helping to refine where focus should be directed. There are caveats associated with generalizing conclusions from such techniques, especially when only one host model is used. However the focus of this paper fits well within the remit of this journal and i would recommend publication after some minor concerns are addressed.

General comments: Whilst the paper seems to focus on an example application of the

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



DDM, I am not sure how the conclusions might be generalized if the detailed inorganic composition remains thermodynamically uncoupled to a 'bulk' organic representation. It appears at least that DDM could be applied to a model that would account for this coupling. Can the authors comment on any potential changes to their conclusions based on this? Is this a basic limitation of models that try to simplify the complex organic fraction? After all, a very important question surrounds the ability to predict SOA mass loading/composition and its relationship with existing inorganic compounds. Whilst this may be difficult to answer, the caveats associated with the host model should at least be relatively clear alongside any potential future implementations of the technique used.

A brief summary of the study should be written at the end with particular care given to the broader significance of the results.

The authors make broad statements that the techniques can be applied to uncertainty analysis of air quality modeling. I would imagine that since results are dependent on the host model, that such sensitivity studies should include a wide range of models before 'true' sensitivities can be derived to inform future model developments. Are there any plans to include this in other models as a broader study? It may be that the work required to implement this technique is not entirely clear.

I would like to see more work on the use of the Taylor Series Expansions as this would be rather appealing to many readers of this work. I would suggest another, perhaps more challenging, example.

Minor comments

page 2608, line 11. It would be useful to have quantitative information on the difference in numerical cost. One example is all that is required.

Page 2612:line 11. It would be helpful to list some examples of processes that are linear. line12: The 'algorithmic treatment of secondary inorganic aerosol interactions with surrounding gases' is a vague statement. I presume this refers to the effect of

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

non-ideality on the equilibrium vapour pressure of a condensing compound?

Page 2614: I would be slightly concerned about the applicability of Bromleys formula and the effect this would have on the overall conclusions. Given the availability of potentially more accurate formalisms, do the authors believe their sensitivities would change with choice of activity method?

There appears to be some inconsistencies between formula formatting in the text and tables, please check through these carefully.

Interactive comment on Geosci. Model Dev. Discuss., 4, 2605, 2011.

GMDD

4, C1176–C1178, 2011

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C1178

