

Interactive comment on “On the attribution of contributions of atmospheric trace gases to emissions in atmospheric model applications” *by* V. Grewe et al.

Anonymous Referee #2

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This paper discusses two methodologies to assess emission impacts on chemistry. To describe the combined effect of emissions from individual emission categories, chemistry models have been using tagging and sensitivity methods in the past. The authors introduce an improved tagging scheme and present analytical solutions for two idealised chemistry schemes. The paper is an interesting contribution to the discussion of how assessments of emission changes should be done and is appropriate for GMD. Nevertheless I feel that the balance between equations (too many) and explanations (too few) is not quite right yet.

Mayor concerns: Part of the motivation for the work comes from the discussion of

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Figure 1, yet the reader has to wait until section 3.3. for a mentioning of Figure 1 (the figure is actually quite busy and maybe the authors should consider to split the figure into two panels adding lines incrementally). I believe the authors would help the reader, by contrasting the ideas of tagging and sensitivity methods more clearly earlier, already using Figure 1 in the introduction. The basic point is obviously that a tangent is not always well constraint by two points on a graph and that tagging might be advantageous in quantifying some impacts better, with the extra numerical costs involved. (Even though I am not entirely sure that this is always true.)

The discussion of the error analysis in section 5 is very confusing. It would really help if the authors could spell out what kind of error they are referring to, and to remind the reader of the meaning of the different “Zs” in the equation. In addition some more detail about the construction of the PDF would not hurt. I think this section is a good example of authors being so immersed in their work, that they lack clarity in the communication and explanation of results.

On a related matter: The authors derive steady state solutions, but do not reflect on the relevance of the steady state assumption for the real atmosphere. Quite likely some chemical reactions will not be in steady state in the real/modelled atmosphere, taking into account varying temperatures and solar zenith angles (to name just a few complications). Does this matter for the error estimates? In addition, the rationale for equations R12 and R13 should be clearly spelled out (this being an example for not enough explanation).

Section 3.3. should more clearly distinguish in its introduction what the sensitivity method can and cannot provide, when emission impacts are assessed.

Summary: I believe the paper is a valuable contribution to an important discussion and should be published after some revisions to improve its readability. Hoping and assuming that the authors will take my criticism as a positive encouragement to provide the reader with more text and a slightly improved structure, I will not comment on word

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choices, grammar and spelling (only some small issues here and not worth to be listed,; apart reactors/reactants).

Interactive comment on Geosci. Model Dev. Discuss., 3, 819, 2010.

GMDD

3, C261–C263, 2010

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