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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling future changes in surface ozone: a parameterized approach" *by* O. Wild et al.

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Review of the ms "Modelling future changes in surface ozone: a parameterized approach" by Wild et al.

The authors investigate the impact of future emission scenarios on surface ozone. They use a multi model approach to derive a response function relating emission changes to changes in surface ozone and chose one model to investigate further sensitivities of this approach. I think the approach is valid and can be applied for a number of questions. The findings are relevant and I propose publication with minor comments - if point 4 (see below) can be settled easily.

My comments basically concentrate on some aspects on the presentation.

1) On a first glance it sounds weird to use many simulations to derive a simplified response model and then to apply it on a limited number of emission scenarios. I think it would be worthwhile to include a deeper discussion on the advantages and disadvantages of such an approach in the introduction.

In addition a delimitation to other approaches would be helpful. That is on the one hand side tagging approaches like early work from Dentener, Lamarque et al., 2005 (Snapshots in transient simulations), Grewe, 2007 (tagged transient), and on the other side other linearisation approaches like Köhler et al., 2008 (JGR) or Grewe and Stenke 2008 (ACP).

I also would suggst to put a little bit more emphasis on the nature of linearity. What people often confuses is the difference between a local linear approximation and a global linear system. What the authors show is that the chemical system can locally linearised. That does not imply that chemistry is linear! In fact it is highly non-linear. I would appraciate if this could be pointed out more clearly, because if the reader understand that the approach suggest that tropospheric chemistry is linear, this study would be non-credible.

2) The authors have very carefully described how the approach has to be understood. E.g. 27446 "For this reason the sensitivity approach used in the HTAP studies is unsuitable for deriving a full source apportionment for O3.", or 27558 "regional source attribution for the O3 changes". I like that!

3) Formula (1) hides the emissions. It would be better to have an f, which is only dependent from the base simulation, in this case 1/0.2 and the emission change expicitely in the formula. Moreover, it is unclear which formula is used in the end. This should be stated clearly including the formulas (g replaced by f?).

4) Fig. 3 The derivative seems to be 0 for Fig 3 top left. Hence the relative relative error is approximately 100% ! This should be noted. It would be helpful to include the results from formula (1) (with f and g in Fig. 3.) Moreover, I am not yet sure whether

this is in fact major short-coming of the approach.

5) What is the temporal resolution of the emission data?

6) It would be helpful to explicitely write the error formulas in section 27554! It would be helpful to understand it! I am not quite sure what kind of errors the authors are actually referring to.

7) 27560/15 I am not quite sure whether I understood correctly what simulation was performed and why. The base simulations for the response model are year 2000 simulations. Was this simulation taken into account and emission changes between 1960 and 2000 were investigated? The differences then compared with the response model? 27564/21 than->that?

Fig: 1 a and b not marked explicitely Fig. 3 include results from formula (1) with f and g

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