Atmos. Chem. Phys. Discuss., 11, C14422–C14426, 2012
www.atmos-chem-phys-discuss.net/11/C14422/2012/

Atmospheric Chemistry and Physics Discussions

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

O. Wild et al.

o.wild@lancaster.ac.uk

Received and published: 18 January 2012

We are very grateful to Volker Grewe for his encouraging comments and for his careful reading of the paper. We have addressed each of the points raised, and have altered the text, figures and captions to address the concerns appropriately. We believe that this has improved the paper, and thank Dr. Grewe for his time and effort in pointing out where improvements could be made.

Response to Comments:

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.

The concept and benefits of the approach are briefly introduced in the introduction, but we feel it is clearer to leave a more detailed discussion of advantages and disadvantages until after the approach itself has been described (section 3). We have already included a paragraph on the merits of the approach in the final paragraph of this section, but have adapted this slightly. The main disadvantage of the approach is in its applicability, and this is explored in detail throughout the whole of section 4. We have summarised the benefits and limitations of the approach again in the conclusions.

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).

Tagging and sensitivity approaches are acknowledged here in section 3 as methods for source apportionment, but there are already a number of good reviews of these approaches (in the papers mentioned, and also in Grewe et al., 2010), and attribution of changes is only one focus of this paper. We have therefore amended the final paragraph in section 3 which refers to attribution, citing previous work, but have aimed to avoid more in-depth discussion of topics that are already covered well elsewhere.

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.

We agree that there is often confusion regarding the concept of linearity in chemical systems, and that there is a temporal/spatial scale dependence here that is frequently overlooked. We accept that this point needs to made clearly, and we have amended the text to emphasise that the mild non-linearities seen over the monthly continental scales used here do not contradict the fact that there are strong non-linearities in the chemistry that can be observed clearly over smaller spatial and temporal scales (e.g., urban, street or plume scales).

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!

Thank you. We have tried to be clear that as the method is based on sensitivities around current conditions it is appropriate for assessing moderate emission changes but not for source attribution which requires other modelling approaches.

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?).

While we could include the emission terms directly in Equation 1, we have aimed to keep this expression as simple as possible so that it is immediately clear that the net ozone response is a simple sum of the constituent ozone changes by source region and precursor. The scale factors, f, are thus dimensionless and are simply the ratio of the emission change to the 20% change applied under HTAP, and in fact this is particularly useful given that we are applying fractional emission changes rather than absolute emission changes here. However, we appreciate that readers might be unclear about the final formula used and have therefore restated the conditions explicitly in this section in a new equation.

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.

The derivative of the annual data mean ozone change is close to zero for this model. However, the parameterization is applied to monthly data so that we can generate the seasonality in ozone responses, and the annual mean response is calculated from these and not from the annual mean ozone changes directly. This is why monthly changes are presented in Fig. 3, and is also why the nonlinear scaling ('g') is important to include. Note that the derivatives are positive in summer and negative in winter in this case, but that the position of these curves is likely to differ from model to model. Given the large range in monthly derivatives, an annual mean derivative of zero does not present a major problem for the approach.

The results from Eqn. 1 are already included in Fig. 3; for clarity we present these as signed differences on a larger scale in the lower panels. The solid lines show the error due to a linear scaling (f) and the dashed line shows the effect of using the quadratic scaling (g). To make this clearer, we have now added the labels 'f' and 'g' in the figure caption.

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

The historical and future emission changes applied here are based on decadal emissions totals for years 1960, 1970, 1980... 2050, with the addition of 2005 available from the RCP data set. Emission changes are applied on a monthly basis (as noted in Sections 3 and 5), but as different assumptions about the seasonality of emissions are used in different models we apply annual mean fractional emission changes so that scale factors f and g are independent of month.

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.

This is a good suggestion, and although we have aimed to keep the mathematical content of the paper reasonably low to ensure that it is accessible to as wide an audience as possible, it is relatively easy to add these formula without introduction of much additional notation.

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?

The model was run with emissions representative of 1960 conditions (but still with year 2001 meteorology) so that the parameterized estimate of surface ozone changes between 1960 and 2000 could be compared with the difference between full model simulations with 1960 and 2000 emissions. The phrase 'run with 1960 emission changes' in the text is unclear, and this has now been rephrased.

27564/21 than->that?

Thank you, this has now been corrected.

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

The left and right hand panels in Fig 2 have now been labelled as suggested. As noted in the response to point 3 above, f and g are already presented in the lower panels of Fig 3 which show the error due to a linear scaling (f) and the improvement from using a quadratic scaling (g), and the caption has been altered to highlight this.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27547, 2011.