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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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We are very grateful to the reviewer for their encouraging comments and for their time and effort in pointing out where improvements could be made. We have addressed each of the points raised, and have altered the text to address the concerns appropriately.

Response to Comments:

My only major comment relates to the description of the reproduction of observed ozone trends (section). While I agree that the approach discussed here is only applicable for regions, not specific stations, there is room for expansion. In Particular, the

works of Cooper et al. and Parrish et al. have identified the United States West coast as an area of rapid surface ozone increase. This is not reproduced in Lamarque et al. (ACP, 2010) and it would be interesting to see 1) if the simple approach provides asimilar timeline and 2) if the approach can be used to sample the parameter space to identify the potential sources of the discrepancy.

We have applied the approach to particular locations to explore the reproduction of observed trends (Section 4 and Fig 7). We examined trends at Trinidad Head, but find very little signal in the 1988–2000 time frame. There are increases of more than 0.03 ppb/yr from Asia and 0.02 ppb/yr from higher global CH_4, but these are matched by O_3 decreases from North American and European sources. Between 2000 and 2005 there is an increase of about 0.07 ppb/yr from Asian sources (emission changes from RCP8.5 scenario), but this is the largest we find and remains far below the observed trends of 0.34 ppb/yr described by Parrish et al. (2009). This suggests that increases in other source contributions are required (e.g., shipping emissions?) but more focussed model studies will be required to reconcile these differences with the high trends observed. We have amended the final paragraph of section 5 to show that the approach can be used to identify the contributions to modelled trends, but given that the behaviour seen over the US west coast is very similar to that over Mace Head we have not included an additional detailed comparison here.

P 27549, line 8: as is mentioned later, this is not quite uncertainty. I would refrain to use this term and possibly use range or spread.

The different models produce a spread of results, and while this spread does not represent the formal uncertainty in the results, it does provide a reasonable measure of confidence in them given the wide range of model algorithms and inputs applied. We are careful not to claim that the spread represents the uncertainty directly, but are keen to emphasise that it does constitute an important component of the uncertainty (this is discussed in more detail in the body of the paper in Section 6). We therefore choose

to retain the word uncertainty in the abstract, but alter the wording to provide a clear statement of what we mean by it to avoid misinterpretation.

P 27552, line 15: it is best to use the references from the RCP special edition. All paper are now published in Climatic Change.

Thank you for this suggestion; these references have now been updated.

P27552, line 20: it would be good to include here a discussion of methane since it is so important for the long-term horizon in the RCPs.

We have added a further comment here in Section 2 on the widely differing methane pathways in the RCPs, highlighting the importance of this for tropospheric ozone.

P27553, lines 1-10: are those perturbations done on a monthly basis (i.e. one simulation per perturbed month) or the full year? How long are the simulations? Based on the first author previous paper, a discussion of the impact of resolution should be included.

The perturbations are done on an annual basis, and are run for a full year (plus a spin-up period) as described at the start of Section 2 and also in more depth in the publications cited (HTAP, 2007; Fiore et al., 2009). We have added "annual" to the description of these perturbation experiments to emphasise this, and have included an additional sentence stating that the runs were each one year (plus additional time for spin-up). Model resolution will certainly influence surface ozone on a local basis, but no systematic differences over the continental-scale regions considered here have been observed with the (albeit limited) range of resolutions considered here ($5 \times 5°$–$1 \times 1°$, see HTAP, 2010). We have now included a sentence to highlight this in section 6.

P27556, line 20: it is probably important to relate the size (20 or 60%) of those changes to historical changes, i.e. how far back is a 60% decrease.

It is clearly helpful to relate these changes to historical regional emission changes to

get a feeling for the timescales over which the approach can realistically be used. However, there is substantial regional dependence in these changes, and it differs widely from precursor to precursor. To give some context, we now state in the text that a 60% decrease for NO_x emissions is roughly equivalent to a return to 1950 emissions over Europe and North America and 1970 emissions over Southern and Eastern Asia. Note that we already address the magnitude of emission changes clearly in the historical trends section (Section 5) where regional emission changes for 1960 are quantified explicitly, and that changes for one of the 2050 emission scenarios are presented in Table 5.

P27557, line 10: why not use 20 and 60% instead of 10 and 20?

This sentence provides a simple explanation of the quadratic expression presented in Equation 4 by noting that there is a 10% smaller response for successive 20% emission increases (and 10% larger response for 20% emission decreases), matching the curvature shown in the upper panels of Fig 3.

P27557, line 15-20: the discussion of the different parameters (f and g) for NOx emissions needs to be expanded. This all seems somewhat ad hoc (unlike Eqs (1)-(3)).

The inclusion of the quadratic term is based on the curvature that is clearly seen in the upper right-hand panels of Fig 3. To avoid the impression that this is ad hoc, we have now restated this clearly. The corrections for titration conditions have been applied based on the source region responses seen in the upper left-hand panel of Fig 3, and we have now amended the text to make this clearer. In addition, we have summarised the conditions applied in a new equation so that the reader can see the final expressions used.

P27559, lines 8-20: since the differences encompasses the variations in PD emissions, this discussion should be removed, unless the authors can estimate the size of impact of these variations.

There are differences in present day emissions between the models, and for regional NO_x emissions these are typically 10–15% (Fiore et al., 2009). This reflects the uncertainty in regional emission estimates, and contributes to the differences in ozone response between models. However, the discussion here (and the associated Fig 5) demonstrates clearly that the parameterization reproduces the full model results relatively well for each model independently, and this is important to emphasise. While it would be valuable to normalise the model responses to specified regional emissions, there is no self-consistent way of doing this without repeating the HTAP experiment with all models using the same emissions. Previous model intercomparisons have adopted this approach (e.g., the ACCENT study, Dentener et al., 2006), and we note that the resultant ozone changes between 2000 and 2030 they present are nevertheless almost identical to those derived with the present parameterization, as described at the end of Section 6.

P27563, line 25: it is probably good to remind the readers that this is without climate change or change in circulation, including STE. The latter one seems to be of quite strong significance in RCP8.5 as discussed in Kawase et al (2011) http://www.agu.org/pubs/crossref/2011/2010GL046402.shtml and Lamarque et al. (2011).

We note at the end of the historical section (Section 5) that changes in meteorology and STE are neglected, and again at the end of the future section (Section 6) that changes in climate are neglected. However, we have now included an additional sentence in Section 6 noting the possible future increases in STE suggested by Kawase et al.

P27565, lines 18-29: this section should include a reference the Jacob and Winner paper.

This paper is cited elsewhere, but the results are also clearly relevant here, and a citation has been added.

Section 8: if possible, it would be interesting if the authors could indicate what, in their views, is the potential for such parameterized approach for other quantities than surface 2011 ozone and/or different measures than monthly mean ozone (AOT40 for example).

We have explored the possibility of using the same approach for tropospheric ozone burden, ozone column changes and ozone radiative forcing, and the results look promising. However, further tests are required to determine the degree of non-linearity for these variables. The approach could certainly be extended to other precursors and to their impacts such as nitrogen deposition, and we have now added a sentence in the conclusions to acknowledge this. The approach is unlikely to work as well for threshold metrics such as AOT40, where responses may be very sensitive to proximity to the threshold value.

Tables 1 and 2: Table 1 is supposed to show the number of models participating in the simulations. However, Table 2 only lists 14 models. Can you clarify?

Table 1 shows the total number of models or model/meteorology combinations that contributed results for each HTAP simulation. Table 2 lists only the models which contributed a sufficiently complete set of simulations to allow use with the parameterization. This is explained at the start of Section 5, but unfortunately the table reference was omitted from the text; this has now been corrected, and the opening sentence of the section has been rephrased to make it clear why these 14 models were used.

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