Atmos. Chem. Phys. Discuss., 11, C12815–C12817, 2011
www.atmos-chem-phys-discuss.net/11/C12815/2011/

Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

Received and published: 6 December 2011

This paper discusses a parameterized approach to simulate the impact of emissions on surface ozone, based on HTAP simulations and additional simulations with the main author's chemistry-transport model. This paper is interesting and well written. I have comments and questions that the authors should address before publication.

Major comment

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 a

similar timeline and 2) if the approach can be used to sample the parameter space to identify the potential sources of the discrepancy.

Minor comments

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.

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

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.

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.

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.

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

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)).

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.

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).

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

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 ozone and/or different measures than monthly mean ozone (AOT40 for example).

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?

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