Response to Referee#1

We would like to thank Referee#1 for their extremely thoughtful and useful review. In this document, the referee's comments are repeated; our responses are in red.

Comments from Referee#1

The article by Stevenson et al. constitutes a major undertaking and manages to formulate clear and concise statements concerning the current state-of-the-art in terms of assessing the radiative forcing from tropospheric reactive gases for both the historic period as well as future scenarios. The authors analyze a set of chemistry climate model experiments run by up to 17 global chemistry climate models, and they perform a careful analysis of the uncertainties due to varying model responses, different radiative schemes, and other effects. The results presented in this study are destined to set a benchmark for years to come. There is only one concern I have with this study: the carefulness and level of detail that is spent on assessing tangible uncertainties may trick some readers to believe that these are indeed the final uncertainties of the radiative forcing estimates. Here, I would disagree, because the important issue how tropospheric ozone biases in a model affect the radiative forcing calculations for this model is insufficiently treated. There is some discussion in the literature concerning the validity of early tropospheric ozone measurements (e.g. Montsouris), and it is generally found that "pre-industrial" model runs overestimate these early measurements by a substantial fraction. As it remains unclear at present who is right, it would be justified to incorporate this potential model bias in the RF uncertainty estimates. This issue is touched upon in the discussion but would deserve a more thorough treatment, especially in light of the potential impact that this study may have.

We have added some discussion of pre-industrial (PI) O3 observations and differences with model simulations (see also response to Referee #3).

I also don't fully agree with the "extrapolation" of the RF estimate from 1850-2000 to 1750-2010 which is largely based on the Skeie et al., 2011 study. These authors assumed (without further justification) that 1750 biomass burning emissions were only half of 1850 biomass burning emissions. In light of newer studies dealing with this subject, this appears exaggerated, particularly for tropical latitude burning. Eventually, this second "flaw" balances the first one to some extent, because it will tend to drive down 1750 ozone concentrations and thus bring the model results closer to the observed values. Nevertheless, these aspects should be discussed more carefully.

Our study is based on model results for 1850-2000, but 1750-2000 is what is needed for IPCC AR5. We cannot contribute further to the debate about O3 changes between 1750-1850 with our results. We just used the Skeie et al (2011) study to adjust our results. This is further discussed in Lamarque et al (2011). We have added some discussion on the uncertainties in this area.

Detailed comments:

Abstract: the abstract should be improved. Currently, the essential information of the main findings is spread over several sentences. There are three key points which the reader should learn from the first sentence: multi-model study, total RF estimate (1750-2010) of 0.4 W/m2, and uncertainty of 30%.

The Abstract has been revised.

Introduction: This is a weird beginning for this paper, because it focuses on observed changes in tropospheric ozone and other trace gases, while exactly this aspect is treated rather peripherally in the remainder of the text. I suggest to start with the 2 sentences on p. 26051, l.17 ff, describe the state-of-the-art in modeling, and only then introduce the history of tropospheric trace gas observations.

The Introduction has been revised.

The authors should also cite more recent work on tropospheric ozone trends, e.g. Logan et al., 2012, Parrish et al., 2011, and Tilmes et al., 2012.

The Introduction has been updated to include these references.

I cannot quite agree with the sentence "Despite the paucity of observations, tropospheric ozone is thought to have increased..." - this finding is based primarily on the few available observations, even though these admittedly don't tell much about the magnitude - it is the quantification, not the qualitative finding where the paucity of data matters.

This statement has been revised.

"Although increases in anthropogenic emisisons..." this statement deserves some references! – this is targeted on p. 26052 l. 11 ff.; as that paragraph doesn't fit there, I suggest to incorporate it into the "Although" paragraph above.

There is a reference to the Lamarque et al (2010) paper on emissions in the preceding sentence. The text has been re-organised in the light of these comments.

p. 26054 l.9: duplicated "simulated" in one sentence

Deleted.

p. 26055 l. 7ff: this is a critical point! While homogenized emissions indeed narrow the model spread, they may also lead to false perceptions of total uncertainty. I don't want to dispute the strategy that was adopted in this study, but this is nevertheless an important argument which should be brought out more clearly.

We have clarified that our modelling approach (with everyone using the same emissions from Lamarque et al., 2010) does not allow us to assess uncertainties associated with the emissions.

p. 26059 l.4ff: what about changes in tropopause height with climate? Wouldn't this introduce another uncertainty in the RF estimates?

We have not considered this. We have included it as another potential uncertainty. A change in the tropopause height would affect the partitioning of RF between tropospheric and stratospheric ozone. The choice of tropopause definition and its influence on RF is considered in the paper (i.e. calculations with MASKZMT and MASK150 in Table 3). Changes in tropopause height, and their influence on RF, will probably be dependent on tropopause definition.

p. 26095 l.9: while there is a statement here about the model evaluation for present-day, a similar statement is missing concerning the 1850 scenario. (see main comment above)

There is some evaluation of the 1850s O3 in Young et al. (2012). We have added some further material to this paper, including a comparison of the 1850s simulations with available (highly uncertain and sparse) observations. (Also see response to Referee #3). Evaluation of

1850s reactive nitrogen deposition (i.e. related to O3) is included in Lamarque et al (submitted).

p. 26060 l.2: "Rockies" is jargon

We will replace with The Rocky Mountains.

p. 26062 l. 2&3: why "~" in front of "791" and "1751" ppb? These concentrations seem to be exact values.

We have deleted.

p. 26065 l.15: how do the inter-model differences concerning climate change effects impact the uncertainties (or rather biases) in RF calculations? This is discussed later (mainly by arguing that these changes are small compared to the forcing-driven changes), but this discussion should take place here.

Climate change impacts on O3 RF are generally relative small, but are part of the reason for the spread in 2100 (e.g., Figure 7). We have not fully quantified climate change impacts, just qualitatively. A more complete analysis is beyond the scope of this paper, but some more discussion will be included.

p. 26067 l.24: as stated in my general comment above, the Skeie et al., 2011 estimate of 1750 to 1850 changes should be treated with some uncertainty attached.

We have added some discussion of the uncertainty in the 1750-1850 changes.

Table 8: readability could be improved by inserting a small spacing between blocks of 3 rows. The caption talks about "upper box" and "lower box" while you probably mean to say "upper row" and "lower row".

Table 8 has been revised.

Figure 5 (and a few others, particularly in S): what do the white colored patches mean?

These are off the scale. We have revised these figures/captions.

Figure 6: figures too small and colorbars and labels not readable. Suggest to follow the concept of the rest of this paper and show only mean changes (perhaps together with standard deviation?) in the main text and place individual model results in S. – would it be interesting to try and relate the ozone changes to "climate" changes, for example by plotting delta-O3/delta-T here?

We agree and we have revised the figures. We are exploring the possibility of including delta-O3/delta-T plots.

Figure 7: this figure could be made more appealing by translating it into an "IPCC style" graphics where you show the mean results as lines and the uncertainty as shaded areas, all on one time axis. The individual simulation results (present figure) could be kept for the supplementary material.

We have revised the figure and re-organised the paper with this comment in mind.

References

Lamarque, J.-F., Kyle, G. P., Meinshausen, M., Riahi, K., Smith, S. J., van Vuuren, D. P., Conley, A., and Vitt, F.: Global and regional evolution of short-lived radiatively-active gases and aerosols in the representative concentration pathways, Clim. Change, 109, 191–212, doi:10.1007/s10584-011-0155-0, 2011

Lamarque, J-F., et al. (2013) Multi-model mean nitrogen and sulfur deposition from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP): evaluation and historical and projected changes, submitted to Atmos. Chem. Phys. Discuss.