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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Tropospheric ozone changes, radiative forcing and attribution to emissions in the Atmospheric Chemistry and Climate Model Inter-comparison Project (ACCMIP)" *by* D. S. Stevenson et al.

Anonymous Referee #3

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This is a useful and well written study of tropospheric ozone changes and associated radiative forcings resulting from an analysis using ACCMIP global model simulations for past present and future conditions. The study updates previous studies on the same theme- used in previous IPCC assessments. Obviously this study intends to inform the IPCC AR5 process.

I have little concern on the scientific credibility of the methods used in this paper, I have however some minor suggestions regarding sharpening some of the conclusions, and especially on the resulting uncertainties of tropospheric ozone radiative forcing.

Specifically in the abstract and conclusion I would like to see clearer statements on the uncertainties of our ability to calculate RF from ozone and its precursors, based on quantified and unquantified uncertainties. The paper is mentioning a +/- 1 sigma uncertainty of 30 %; this however implies that we a sufficient confidence in the pre-industrial values of ozone. Unfortunately, current models systematically seem to overestimate the (few) pre-industrial measurements available. If the measurements are true- this would probably imply some missing process in the models that would lead to a greater RF and hence the uncertainty would at least double. I would like to see this issue somehow taken into account.

Compared to earlier works, does this mean a major improvement or is the answer essentially unchanged given the uncertainties?

Finally with regard to RFs used in the prescribed RF experiments for IPCC (AMIP5), is there anything to say on the accuracy of these RFs with regard to the parts coming from ozone and CH4?

I therefore recommend publication of this paper when addressing the minor revision outlined above and below in the detailed comments.

Detailed comments:

p. 26049- line 5 mention that prescribed boundaries were used, most importantly common anthropogenic emission inventories- which are better constrained for the present day then for the past. The RCPs also give a limited view on the envelope of future air pollution emissions.

p. 26049- line 20 I would for the sake of the abstract just talk about preindustrial numbers- explain elsewhere that this was derived by adding 0.04 Wm-2 to 1850 numbers. It is a bit confusing right now.

p. 26049 line 20 An important piece of information that should be in the abstract is the relative contribution of methane relative to other air pollutants- which will be very different in the scenarios. I propose to make two sentences -separating 2030 and 2100, and include in brackets the amount attributable to O3 from CH4.

p. 26050: The forthcoming IPCC-AR5 WG1 report contains an updated evaluation of tropospheric ozone trends during the last decades- updated from the earlier HTAP (2010) report- based on work by Parrish and Cooper.

p. 26051 l. 24 methodology used in/recommendations issued by IPCC: can you be more specific about the IPCC reference what is being assumed. Refer here to the later section which explains this in more detail.

p. 26052l. 17 O. Wild did an attribution of O3 to NOx, CO, VOC on the one hand and CH4 on the other hand. Surprisingly that work showed a large uncertainty associated with O3 from CH4.

p. 26502 this is appr. 50 % higher; is somewhere in this paper discussed what this could mean for the IPCC scenario work?

p. 26503 Model specific results in a supplement is necessary. Nevertheless, it would sometimes be good to discuss more specific model results, if they are outliers and determining the signal. Are these outliers for good reasons- or do the outliers point to model bugs- and should not be included in the paper.

p. 26504 Here or earlier an exact definition of RF is needed (perhaps again following IPCC: "The definition of RF from the TAR and earlier IPCC assessment reports is retained. Ramaswamy et al. (2001) define it as 'the change in net (down minus up) irradiance (solar plus longwave; in W m–2) at the tropopause after allowing for stratospheric temperatures to readjust to radiative equilibrium, but with surface and tropospheric temperatures and state held fixed at the unperturbed values"

Probably described also later, but it may be confusing to realize that each model's meteo is changing- while probably for the calculations a fixed p meteo was used.

p. 26504 l. 22: do you analyse what is the impact of this on ozone and resulting RF?

Some of the attribution experiments may help.

p. 26505: Model K: how different was methane from observed values = and will the impact of this be evaluated?

p. 26506 I think this 'climate' change effect is perhaps one of the newer aspects of these simulations. However, I didn't find it in the abstract-even if the conclusion is that it is not a major factor.

p. 26056 l 23: Here it said that meteo fields were constant? Meteo from one single model, or from each individual model. It is explained on p. 26057 that indeed one model was used: is there any sense of what uncertainty could arise from this (meteo not necessarily consistent with the indidual models). As mentioned before, I think the concept should be introduced earlier.

p. 26058 A recent analysis of tropospheric ozone forcing using HTAP models and GFDL code was recently described in Fry et al (JGR, 2012).It would be good to have some qualitative evaluation of differences with that work as well.

p. 26059 l. 7 To me the two different tropopause definitions give pretty similar results. There is a quite extensive discussion on the use of various other assumption, but I think the section should end with a bottom-line point of how the authors summarize the various effect (and not postpone to a later section).

p. 26059 l. 15 This points to the fact that it is not so clear what has been used for pre-industrial stratospheric ozone (perhaps I missed it earlier).

p. 26060 clouds). We only use a single representation of cloud distributions (from the 64-level HadAM3 model) in the E-S calculations; cloud fields from individual models were not used: this information should be more upfront and the reason for doing so explained. Earlier you say that the uncertainty in clouds is being explored but I didn't find it-only that you did calculations with and without clouds and varying tropopause conditions, but that is not quite the same.

p. 26062 l. 14 for spin-up times of 6 months of more, it probably was. Is that true for this analysis?

p. 26062 l. 21: why do you consider use of single factor better than from the individual models. Models are known to have fairly different feedback factors. Would that induce an additional uncertainty?

p. 26063 :However, the sum of the indirect effects on methane must sum to zero, but actually sum to -98mWm-2: I didn't get this statement why should this sum be zero? Can you elaborate?

p. 26064: l. 4 Why is this? Because the direct effects of NOx on O3 and OH are in absolute sense much higher? Where is the threshold for this method to be accurate?

p. 26064:l. 5 is this referring to the -98 mW number?

p. 26064: l. 12 these numbers can probably be compared to those at p. 26063 l. 21? On the global average the corrections seem to be relevant but not determinant ....

I found the discussion above rather hard to follow- please try to further improve the description.

p. 26065 l. 15 This section would need a quantitative statement on the resulting RF- or mention that a robust number could not be determined.

p. 26068 Model P: any explanation why results are so different?

p. p. 26068 l.14 Since Shindell is part of this study, and contributed to this paper, it seems not impossible to find out what is the difference. Given the importance of that earlier paper, I would expect a somewhat more quantitative attempt to explain this. Was there possibly a bug in the earlier study? In support of the current paper, it was indeed argued in a comment to the Shindell paper that additional studies should confirm the Nature paper. http://www.nature.com/news/2009/091029/full/news.2009.1049.html If indeed the current study downplays the importance of CH4 in this conclusion would

merit a more prominent place in the conclusions.

p. 26069 On the role of climate change I would say the abstract should mention that no robust conclusion could be drawn?

p. 26070 As introduced earlier- some discussion on what we don't know (e.g. the per-industrial to industrial change) and how that would change the conclusion would be helpful.

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