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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Radiative forcing and climate metrics for ozone precursor emissions: the impact of multi-model averaging" *by* C. R. MacIntosh et al.

Anonymous Referee #2

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This discussion paper by Macintosh, Shine and Collins addresses an important and often overlooked problem in averaging model results, one that is becoming more and more embedded in our chemistry-climate assessments. There is some very interesting material in the paper, but it is so long and unfocused, the continuing strings of deltas or perturbations become confusing, and as well it misses some of the basics that the community has already been through. My view is that it needs to put this work in a better perspective of the known correlations and chemical reactivities of the atmosphere and then state clearly what is new here and what is important. Hopefully this can be achieved in fewer pages so that the less stout can find the important results. Further,

the use of un-normalized NOx perturbations makes the results not useful as some of the model spread (but not most) is spurious.

27197ff This intro spends much time on the values and issues of GWP/GTPs but that is not what the paper is about. It really is about averaging, how to average, and how correlated errors can cancel and reduce uncertainty (or at least the model spread). The paper seems to have missed already published discussion on this topic, even within the limited framework of NOx, O3, CH4 and climate forcing. For example, the Holmes et al 2011 PNAS paper ("Uncertainties in climate assessment for the case of aviation NO") clearly points out the correlation of model results for RF short-O3, long-O3 and long-CH4 and then shows how it affects the model spread is smaller than the sum of the components (a conclusion here). It would be better to start from that framework and build on it here with the self-consistent calculation of RF from the 3D fields (as is done).

27198ff While the Collins et al 2013 paper clearly showed the regional differences in emissions-to-impacts, the much earlier work of Wild et al 2001 GRL ("Indirect long-term global radiative cooling from NOx emissions") has a figure/table showing the clear cancellation of the RF of short-O3 with that of long-O3+CH4 as well the large differences in the absolute impacts according to the latitude of emissions. There is a clear disagreement between that paper and the results presented here (p. 27204 & Fig.3) that had me stumped until I read carefully and found that none of the results had been normalized to a standard perturbation (e.g., 1 Tg-N/yr) per region. Furthermore, the individual models perturbations were %s and not absolutes – all of these perturbations need to be renormalized to make sense, and further the perturbations for the 4 HTAP regions must also be rescaled. It makes no sense to argue that the SA impacts of NOx are small when the perturbation is much smaller – I could not find these key numbers in the tables.

27213-4 The discussion Section 6 is very good, and I began to realize the value of this work. It would be helpful if the authors focused from the beginning on what was new here, and how by using the HTAP runs, imperfect as they are, we can learn something

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about ‘ensembling’.

Various points. There are many confusing points in the paper as well as ambiguously defined calculations. I give a quick run-through of my notes below in the order they appear:

The use of +-(greek sigma) and SD are not clear. Is there a difference?

The definition of sigma/SD must also be clear as to what time/space series is being used (hourly, daily, monthly) and over what period the SD is computed. Resolution is also critical and finer resolution will always have greater variability.

I believe that there 13 scenarios plus 1 control? = 14?

You note that the 20% is not the same in all models, this should be rescaled.

The definition of tropopause when averaging is critical and I doubt you have hourly data, so you need to realize that a monthly or zonal mean tropopause height does not accurately separate the two regions. What was done here?

If you think about it, 4 points during the year hardly resolve the annual cycle, but they do reasonably sample it.

The term uncertainty (p.27201) keeps slipping in when you mean model spread.

Use of the abbreviation PM for primary-mode O3 or long-O3 is very odd and confusing. Finally in the conclusions you revert to the more standard short-O3 and long-O3 that is more standard. Primary mode is OK, but not PM. Because then the short-O3 should be Secondary Modes (plural).

The discussion about calculating the steady-state CH4 abundance from the feedback factor is based on some very careful definitions of lifetimes, time scales and feedbacks etc in the literature – see the recent WMO and IPCC sections on this. The lifetime of CH4 must be defined to include ALL losses, otherwise the method here does not work. It is unclear in Table 2 just how these “lifetimes” for CH4 are derived and thus how

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someone might usefully follow the chain of mapping the dln(lifetime)/dln(burden) onto a perturbation lifetime.

I suspect that each model's VOC emissions are alos very different, not only in quantity, but also in their makeup. This will greatly increase the model spread for a 20% perturbation. Thus the arguments here about short lifetime (which sound plausible) may not be the reason.

In the Climate Metric section, there are so many numbers as to be confusing – some do not even have units (p. 27210).

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27195, 2014.

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