

Interactive comment on “Observational constraints on ozone radiative forcing from the Atmospheric Chemistry Climate Model Intercomparison Project (ACCMIP)” by K. Bowman et al.

Anonymous Referee #1

Received and published: 12 October 2012

I find this manuscript to be rather unpolished and in need of significant revision, but I believe it should be suitable for publication following such revision as it contains some very useful science on ozone radiative forcing. I think there are two dimensions to the current problems in the paper, one being on the science and one being on the presentation and terminology. While some of these are in the details below, I believe the authors should be more circumspect in their presentation of the observationally-constrained radiative forcing. The observational constraint is, in my opinion, quite weak, and it is a conjecture as to whether the constrained forcing is any better than the pure modelled

C8080

one. I suggest some reduction of the emphasis on this. Indeed, perhaps “constrained” is the wrong word, and something like “modified” would be better as it avoids the connotation that it is necessarily better? The real strength and novelty of this paper for me is the methodology by which the model ozone fields are assessed for their significance from a radiative forcing view, rather than a purely composition point of view, and I would suggest this aspect is stressed more.

SPECIFIC COMMENTS

23605, 7-10: (MAJOR) Parts of the abstract are difficult to understand as quite specific terms (in some cases misleadingly, in my view) are used in the main text. Here “instantaneous radiative forcing” is misleading as it is really referring to the change in OLR as a result of using ACCMIP rather than TES ozone. I don’t believe this is a radiative forcing, as commonly defined, as it doesn’t represent the effect of some change in composition over a given time period, but rather represents a possible model bias.

24606, 10-11: The use of parentheses in this type of sentence construction has been ranted about several times in the literature. Such sentences (especially in this case with a triple set) are horrible to read. I refer to Robock (2010, doi:10.1029/2010EO450004) and rest my case.

23606, 16-17 and 18-19: “uncertainty in these processes” – I don’t think the uncertainties in the previous few sentences are responsible for the uncertainty range in the AR4 ozone forcing (line 1 of this page). Maybe I am wrong.

23606,24: “lived”

23607, 21: (MAJOR) There is an important general point here, which may explain some of the TES-model differences. The 9.6 micron band is not the only contributor to ozone radiative forcing. For stratospheric ozone, the 14 micron band contributes almost 30% of the total forcing (see section 8.2.2 of the 1994 WMO Ozone Assessment). Although the contribution to tropospheric ozone as a whole is stated to be much smaller, pre-

C8081

sumably the 14 micron band would start to become relatively more important in the upper troposphere. Some of the radiation codes probably have this band in, but others may not.

23610, 19: There ought to be some terminological tidying in the paper which is highlighted here. Sometimes different words are used for the same quantity, and OLR, flux, forcing and irradiance get used interchangeably. I would suggest OLR is fine when truly looking at the irradiance at the top of the atmosphere, forcing is fine when looking at the effect of changes in composition over two time periods, and all usages of flux could be replaced by irradiance (or even vice versa). At this particular place in the text, L is referred to as OLR (which it isn't, in its normal definition, and normal units), whereas one line later it is correctly referred to as radiance.

23611, 5-10: (MAJOR) I don't feel this text and Equation (2) is best placed here and I suggest it is moved until later (perhaps near equation 10) in the text when the context is clear. As noted above, I also felt iRF is not good terminology as it is in no sense a radiative forcing but rather some offset in the OLR. Perhaps Δ_{OLR} would be a much better term and this could then be interpreted later as possibly a radiative forcing, but it seems cleaner to separate out what the quantity is (i.e. a change in OLR), and what it might be interpreted to be (i.e. a change in forcing).

23611,15-18: I became confused here as these two sentences appear to contradict each other – one says it is for a 100% change and the next says it is NOT for a complete absence of ozone.

23612, 26: “radiative equilibrium” – this is only true in a global average sense. In the models I presume that stratospheric adjustment is achieved using fixed dynamical heating (i.e. the dynamical heating is not zero, as required in the case of pure radiative equilibrium).

23614 (MAJOR) I have a lot of problems with this page and I think the work, while an interesting conjecture, requires major caveats. First, to repeat, I do not believe equation

C8082

(8) should be referred to as (change in) RF, as it is an OLR (or similar) offset. Calling it (and applying it as) an RF carries with it the assumption that the models are wrong for the present day but right for the pre-industrial case, which feels absurd. It would seem an equally valid conjecture that if models are wrong for the present day, they will be as wrong for the pre-industrial times. So applying a correction only for the present day could arguably be an unjustified bias. I have no objection to the authors applying their conjecture, but they should start by making clear that Equation (8) is not a forcing, but under a limited range of assumptions it is possible to conjecture that it is.

23615, 1: I did not understand this sentence (“inherently more robust”)

23615, 20, and thereabouts: (MAJOR) This is a rather major comment. I do not think there is any robust justification for reducing the TES TOA flux by 20%. TES is observing the OLR and if there has been any temperature adjustment as a result of the ozone difference, such an adjustment will be represented in the observed OLR. Hence, I disagree with the statement that (lines 16-17) that TES directly observes instantaneous OLR, and indeed struggle to understand what “instantaneous OLR” means from an observational point of view. The adjustment only happens because of a change in concentration and TES does not observe any such change. Perhaps the 20% change can be justified, but it certainly isn't justified very well in the present manuscript, in my view. Note also that the stratospherically-adjusted forcing is, by definition, the same at the tropopause and TOA.

23617, 23 (MAJOR) I felt I learnt almost nothing useful from this section, except that two radiative transfer codes gave different answers for unknown reasons. At 23618:14, I think the “well” could be deleted, as I didn't feel any useful analysis was presented to help the reader. If, as I understand from 23618:22, the two calculations used different background atmospheres (clouds etc) then this would be a very plausible cause. Unless the authors can do something more insightful here (would it be possible, for example, to compare clear sky fluxes?) I would suggest removing this section, especially if the effect, on the global mean is small as stated at 23618:12.

C8083

23618, 3: I didn't understand what "area-weighted" meant in the context of zonal means.

23622, 25 (MAJOR) As noted above, I do not think the 20% adjustment is well justified in this case.

23624, 1: I feel a bit is missing from this paragraph. First, given the uncertainties, the three different estimates are not really "significantly" different. But another important aspect of the findings seems to be that the AR4 Forster et al. value of 350 mW m⁻² is actually rather good, given current understanding, but this statement is left implicit, rather than explicit. If there is big news in this paper it is not that the size of the forcing is any different to what has previously been assumed, but rather the uncertainty in that forcing (presuming that the combination of ACCMIP and this TES analysis sample that uncertainty) is rather strongly reduced. But this in itself depends on the untested, and maybe untestable, assumption that we know present day and pre-industrial pre-cursor emissions sufficiently well, and even knowing these emissions, we do not have really useful pre-industrial measurements. Hence, the question is whether the reader should assume that the quoted uncertainties are robust to all sources of uncertainty. I would also question whether it is appropriate to present the uncertainties as 1-sigma values, rather than 2-sigma values, and I also suggest that the choice of 1-sigma is made clear in the abstract and conclusions, as the IPCC values are 90% uncertainty limits.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 23603, 2012.