Reviewer 1

Overview

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 modeled 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.

We agree with you about where the real strength of the results are. However, it is still important to ask what the implications are of model bias in OLR on modeled ozone RF. The correlation between present day OLR and ozone RF is an encouraging result that suggests observations can be profitably used to inform ozone RF.

The terminology has been greatly simplified, which we hope would make the paper easy to read.

SPECIFIC COMMENTS

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3605, 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.

In the original text, we stated in 23608 L15 that "Critical to this approach is the assumption that differences between TES and ACCMIP ozone can be attributed to the changes in ozone from the preindustrial period. " To the extent that such differences are due to changes in ozone from anthropogenic processes that are not captured by the model, then it could properly be considered radiative forcing. This assumption is one extreme of attributing the ozone difference. The other extreme could be that all of these differences are associated with model bias. If it's the latter, then that model bias would be present in the preindustrial period as well. For the latter case, the differences are informative for ozone RF. However, we can not separate model bias from ozone changes, so we've removed these terms.

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.r

We stand admonished. The offending parentheses have been expunged.

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.

We agree as few if any AR4 models have interactive carbon-chemistry schemes. The "uncertainty in these processes" statement has been moved to follow the chemistry-climate paragraph and the carbon-chemistry interactions are now a separate paragraph.

23606,24: "lived"

Done

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, presumably 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.

We agree with the referee that the 14 micron band for O3 can have a significant contribution to stratospheric radiative forcing, however, as noted in the 1994 WMO Ozone Assessment, the contribution from this band is only 2% for tropospheric ozone forcing due to the O3 weaker absorption lines that are obscured by pressure-broadened CO2 absorption around 14 microns. Because of the weaker O3 absorption and the CO2, TES nadir-viewing Jacobians for tropospheric ozone in this band are close to zero. This can also be seen in the relative contributions for the 14 and 9.6 micron bands to ozone heating and cooling rates in Clough and Iacono, JGR, 1995. In the troposphere and lower stratosphere, the 9.6 micron band has features that are 2 or more orders of magnitude higher than the 14 micron band.

Since we are focused on tropospheric ozone forcing for this study, we do not evaluate stratospheric ozone forcing with the TES IRKs. However, this could be a direction for future work and we will consider the contribution from the 14 micron band. We will also investigate if this is a potential difference in the model results that may or may not include this band.

To avoid confusion about the contribution of the 14 micron band in this paper, we will modify the sentence:

"...9.6 micron band (where ozone is radiatively active)" to state:

"...9.6 micron band (where ozone has the strongest infrared absorption)"

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.

We have clarified that F is OLR and that L is a spectral radiance measurement. We use irradiance in the rest of the manuscript where applicable.

23611, 5-10: (MAJOR) I don't feels 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 delta_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).

We've removed the reference to iRF, which indeed is an interpretation of the delta OLR as a change in ozone rather than a bias. We now use delta OLR throughout. Consequently, the current placement we believe is still reasonable.

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.

This confusion is a consequence of the fact that the LWRE is the change in OLR to a logarithmic change in the ozone distribution. To the extent that a logarithmic change log dx ~ dx/x we can think of the change as fractional. However, we can not drive the difference to zero. We've tried clarify this point:

"Note that the LWRE is a logarithmic change referenced to TES ozone and consequently it can not be used to calculate the change to 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

Thanks for catching that. It's has been changed to "radiative-dynamical 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 (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.

Equation (8) is valid because we made the assumption (conjecture) that we could attribute all of the differences between observed and modeled ozone to a change in ozone rather than bias. We agree that this assumption is limited and those caveats were stated in the final paragraph of that section. We also agree and stated that differences not attributable to a change in ozone should inform the preindustrial ozone estimate. Those two extremes, all the ozone differences are due to ozone change and all the ozone differences are due to bias, represent a bounding of the ozone RF constraint.

Nevertheless, this section is removed.

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

We’ve removed this section.

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.

The point of reducing TES OLR was not a statement about the equilibrium of the actual atmosphere but rather how a model would respond if it’s ozone distribution were instantaneously changed to look like TES ozone as shown in Eq 8. That change would require a stratospheric adjustment.

The term “instantaneous” is meant relative to its application to climate/weather problems. The OLR at an observation point is measured over 4 seconds.

Nevertheless, that section has been removed.

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.

We agree that we can not as yet explain these differences. However, given that TES is observing the real atmosphere and using essentially a line-by-line radiative transfer code, the difference in sensitivity calculation between a GISS and TES is significant. This analysis will be pursued in greater detail in the future. In the mean time, we’ve removed this section.

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

For latitude-band zonal means, this accounts for the relative areas of the latitude bands, i.e., weighted higher near the equator for the larger area covered

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

This has been removed.

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.

The reference point for this paper is not the AR4 estimate but rather the companion Stevenson et al ACPD, 2012 manuscript. In that regard, the main finding is the reduction in standard deviation of these results. We agree that these results do not substantially differ from the AR4 estimates. Your point is well-taken concerning whether major sources of uncertainty have sampled. It is difficult to assess what preindustrial emission were. However, as you’ve pointed out earlier, a number of natural processes should be the same for the both the preindustrial and present day. So, reducing uncertainties in present day natural processes should improve estimates of preindustrial ozone even if it’s not possible to reduce all sources of uncertainty. The spatial patterns of OLR differences are informative in this regard and should permit more focused investigations on processes leading to those differences be it natural or anthropogenic. We’ve used 1-standard deviation to be consistent with Stevenson et al, 2012. We’ve updated to the text to read:

We have presented here an evaluation of ACCMIP ozone and OLR using TES data and the implications of those differences on ozone radiative forcing. We find a significant correlation R2 = 0.59 between δOLRm bias with TES and model ozone RF. Based upon that correlation, we estimate the ACCMIP radiative forcing to be 394±42 mWm−2 (1 standard deviation), which is close to the ACCMIP full ensemble mean radiative forcing in Stevenson et al. (2012) but with about 30% less standard deviation.

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