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Atmospheric Chemistry and Physics Discussions

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Interactive comment

Interactive comment on "Assessment of pre-industrial to present-day anthropogenic climate forcing in UKESM1" *by* Fiona M. O'Connor et al.

Anonymous Referee #1

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This paper is an important documentation of effective radiative forcing in a leading climate model, and should be published following modifications.

For reasons that I understand and appreciate, the paper has a rushed and inconsistent feel to it and is poorly written in some places. I have a large number of comments, some of them rather important for clarification of what has been done, and what the results indicate. Hence, I have recommended the paper needs major modifications before I believe it will be suitable for publication.

Major

There is a lack of clarity about some of the experiments which led to significant confu-

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sion. Some of it was dissipated by the text later in the paper but, for some, "mystery" persisted. Although I suspect these labels follow the various MIP protocols, they are unhelpful to a reader less immersed in the world of MIPs and clearer information, early in the paper, of what is and what is not included in various experiments is necessary. While Table 3 goes some way to doing this, it leaves various issues unclear; it, or an accompanying table, needs to be much more specific about what is, and what isn't included in different integrations

1. L33 starts the confusion about what NTCF is and whether it includes (following AR5) methane and the shorter-lived halocarbons. So a clear definition early in the paper is needed (or as indicated below, NTCF is not used at all, as it is incorrectly used as a shorthand for aerosol and ozone precursors).

2. L33: Even after reading the paper, I did not really understand what "tropospheric ozone precursors" means and whether the impact that these have on lower stratospheric ozone (e.g. following what was shown in the Sovde et al. papers) was included or excluded from the analysis. Was ozone change above the tropopause prevented in piClim-O3 runs (and the associated individual runs such as piClim-NOx)? If not, I would class it as quite misleading to call this "tropospheric ozone" and would suggest an alternate name (such as "ozone reactive precursor gases" to make it clearer). If it was prevented, then it needs to be made clear what the criteria were for preventing ozone change in the stratosphere. There is the parallel issue for pi-Clim-HC, and whether it allows tropospheric ozone change resulting from the ODSs. An additional confusion is whether CH4 is classed as an ozone precursor. Again, it took some time before this became clear (or more precisely, less unclear) and Table 3 didn't help in this regard.

3. L160: piClim-HC gave me even more trouble. I actually thought HC stood for "hydrocarbon" until quite late in the paper. I then found it meant "halocarbon" but it was being used interchangeably with ODS, which was strange until I learnt that non-ODS halocarbons were excluded, for reasons that are not clear to me given the name of the

experiment. And it wasn't until later in the paper that it became clear that the impact of HCs on ozone was included in these experiments, but still whether this influence remains stratospheric is unclear. This needed more detail in Table 3 or a new table making clear what is what.

4. L160: To find that piClim-VOC really meant VOC+CO was also confusing.

5. L160: Added clarity would be given if Table 3 could make clear the distinction between experiments that impose changes in emissions and those that apply changes in concentrations (or in lower boundary conditions). Even after reading the paper, I am not entirely clear on some of these (e.g. methane). For this table, HC should be spelt out (or replaced by ODS) and it should be explicit what gases are included in the Trop O3 precursors. The authors should also reconsider whether "aerO3" is a better descriptor than NTCF, or removed entirely.

6. An additional and quite prevalent issue is that many of the important references are to papers that are submitted (but unavailable) and in some cases only "in preparation" – some key results was in these papers and I felt a bit teased by the allusion to these, without being given either sufficient information or the underlying punchline. I am particularly concerned about the overlap between this paper and Andrews et al. (2019 - submitted) which, from the title of that paper includes forcings in UKESM1, and the Morgenstern et al (2019 - in preparation) paper that contains a discussion of a key result (HC-driven net forcing is strongly negative) when no details at all are presented here, beyond a cursory (4 line) section. This also leads to a very mixed feeling to the paper. Tropospheric ozone is handled in depth, while stratospheric ozone is dealt with in a cursory way, with a reference to the in preparation paper.

7. I am concerned that it is hard to understand what features in the geographical plots are signal and which result from unforced variability. This needs to be discussed in more detail, especially for forcings such as N2O that are inherently small. This is one clear disadvantage of the ERF and partly the reason for my comment at 104/127,

below, about ERF being the metric of choice. I do not believe it is the metric of choice in all circumstances, especially for small forcings.

Other (but note that these are not exhaustive)

25: “altering the sign” – it would help readers to know what the sign has been altered from and to.

49: I would say the change of concentrations resulting from changing emissions is the first part of the cause-effect change

65-94: The intro, with its focus on LLGHGs and tropospheric ozone, feels unbalanced. Why are other forcings not discussed here (or is this detail inappropriate here)? The split between tropospheric and stratospheric ozone forcing is to some extent artificial and a little dated (see e.g. Sovde et al.), because the mapping of the drivers of ozone change does not map exclusively on to ozone change in the troposphere and stratosphere, and that could be clearly acknowledged

104 and 127: “preferred” and “metric of choice” – by who?

117: Xia et al – although it would be tiresome to say “found in their model” on all occasions in this paper, I think it is important in cases, as it is a result of a single model study

119-121: This needs clarification. For RF it is a requirement that tropopause and TOA forcings are identical (not “nearly identical”) following SARF, so in a sense the TOA/trop distinction doesn’t matter. However, where it does matter is that RF requires the specification of the tropopause, which is always to some extent arbitrary. whereas ERF does not – the model just does its own thing. This is one advantage of ERF.

154: “addressed the strong negative” – the nuance here (and in other places in the paper) is that it was too strong negative (i.e. unrealistic). Is that the intention?

268: “from energy budget constraints” – I have no idea what this means and hence whether the Andrews and Forster (2019 - submitted) paper is in some ways a superior assessment to that given here. Should we (or Andrews and Forster) be concerned that the UKESM forcing falls outside their stated range? This is another problem with the heavy reliance on unpublished papers.

282: "some evidence" – I did not know what this meant. It either is, or it isn't? Or is this meant to imply that the derived forcing is no larger than the unforced variability in this region, and hence is not a robust signal? There is no indication of statistical significance in any of the plots, or discussion of it and this should be rectified in some way. See Major Comment 7

282: This paragraph is the first place where it is made clear that the HC calculations include the ozone forcing from ODSs (and indeed that HC=ODS) and leaves the reader unclear whether this is the case for (e.g.) methane and CO2.

287: The GHG forcing is rather inhomogeneous. Is this statement that the aerosol forcing is more so based on an objective measure of inhomogeneity or a subjective assessment?

288-290: Is this conjecture or the result of model analysis?

292: Here it is unclear whether ozone change from methane is in the GHG or the trop O3 plot

296-297: Is the land use forcing over the ocean real (resulting from downstream rapid adjustments) or just indicating unforced variability in the model?

298: This negative forcing over the mid-lat continents is a striking result. Is this the first paper to indicate it? Also, presumably the Land Use forcing in these areas is mostly "historical". This paper understandably does not address the time dimension of the evolution of the forcing, but perhaps some added text could be added to indicate when this forcing would have been most active.

303: I wonder if adding an additional frame with the zonal-mean forcings of each component would be useful for bringing out the overall larger-scale structures of the forcing.

303: It is striking that the geographical plots in each figure differ, presumably because different coauthors were responsible for them, with different conventions in the headers, and in the case of Figure 8, a different projection. Could these be made more consistent? Personally, I find the addition of global mean values on each frame, as in Figure 4, quite helpful.

323: Here and elsewhere it should be made clear if the concentrations are surface values or mass-weighted means over the whole atmosphere (and whether a single global mean values is used everywhere in the model for each LLGHGs).

328: 121 ppm. Presumably the expressions in Etminan et al. could be used to derive their forcing for the same CO2 change?

331: Some of the CO2 SW forcing likely comes from the SW bands of CO2, but this is implicitly discounted here. Or are these bands not included in the radiation code?

352: These two estimates overlap within the uncertainties.

358: “noise” – here and throughout, it is important to guide the reader as to which features are noise and which are robust. Could this be done via masking or stippling? For example, is the marked negative feature in clear-sky longwave in Figure 4c over Russia and its surroundings, robust?

363-365 and Figure 5: 364 talks about “increased ozone” (in the UTLS) but no hint is given as to what drives this feature. Is it a “self-healing” effect of the overlying depletion? The caption to Fig 5 needs to make clear this is for the N2O experiment.

396-398: There is no hint of what is going on here, except a reference to a paper that is “in preparation”. This is very frustrating and leads me to question whether the HC/ODS results should be shown here at all, if there is no accompanying analysis of the striking result. It became a bit unclear to me why, for example, the tropospheric ozone is discussed in such detail in this paper, but not stratospheric ozone. Also note that the assumption that HC=ODS is made here without elaboration, although later there is some hint at this.

414-415: Again, this allusion to a different submitted paper, with no hint of what is found, is frustrating

423: The frames in Figure 6 are in a different order to those in Figure 4, and there are a different number. Commonality of presentation would help the reader. Again, the caption to this Figure does not make it clear it is for methane. (All captions should be checked for this – e.g. Figure 12 suffers the same lack of specificity.)

440: This is the first place the reader learns that the non-ODS halocarbons are not included in HC.

444: "warrant further investigation" – I agree. this difference is interesting and one factor not considered (spectral overlap) would likely push the difference in the wrong direction (i.e. make GHG even lower than the sum of individual components)

473: There is a minor ambiguity in using "sum", "all" and "total". As I understand the Total=All? This also applies to the text from 513-521.

484: "many other models" – but aren't versions of HadGEM included in the Stjern et al. 2017 paper?

494-496: This sentence seems out of place, especially without any supporting reference.

550: I had a lot of comments on Section 4.4 as it was particularly unclear in my view. One underlying issue, raised above, is whether the tropospheric precursor gases are allowed to change lower stratospheric ozone, and whether that change is included here. Without a clear statement, it is very hard to understand this section and place it in the context of prior work. Similarly, the lack of clarity on whether CH4 is included or not, as an emitted species, is unhelpful.

559: This figure purports to show “tropospheric column” but the panels are labelled in ppbv. Is this the mean (mass weighted) ppbv over the depth of the tropophere? How is the troposphere defined for this purpose? Would Dobson Units be a better measure? Figure 5a included a latitude-height plot of ozone change due to N2O, and a similar plot would be helpful here.

565-566: I wasn't sure why the “South Pacific” was highlighted and here and when “Western Pacific” is mentioned, I presume in both cases it means tropical Pacific?

588: By applying this mask, how much forcing (due to the precursors' impact on stratospheric ozone) is excluded from the analysis?

594: I found this paragraph hard to follow, and in particular whether differences in the literature were due to radiation code differences (or the effect of applying a kernel) or process level differences in determining the ozone change. I ended up confused as to whether the new result was higher or lower than Stevenson et al. and for what (dominant) reason it was so.

607: “Section 3.2.3” – I presume this means Section 4.2.3 but, as noted above, there is essentially no detail in 4.2.3, and certainly nothing on the tropospheric ozone change in piClim-HC. This is frustrating when trying to make sense of this section.

616: It would be useful to have the global-mean values, perhaps in the same way that they are shown on Figure 4 in each frame. Also, some comment about how different Fig 10d and Fig 3c are (i.e. RF versus ERF) would seem useful.

666: Are the methane concentrations, the change (i.e. NOx – control, as indicated in Column 1) or the absolute values for NOx. It might be better to show the change?

680: Make clear that this is the OH at 1 km.

686: These do not appear large (e.g. compared to NOx)

695: “eastern Pacific” – does this mean the eastern tropical Pacific, as I see changes

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of both signs in the E Pacific?

699: AOD is meaningless without specifying a wavelength. I'm curious why the VOC/CO run has an increase in AOD but a decrease in CDNC (e.g. over the W Pacific). Is this because one compares column integrated amounts and the other is at 1 km?

712: "negligible" – is this because of cancellation of regions of positive and negative, but the local values can be large?

726: Are the NOx sources just from the surface, or are aviation emissions also included? I know it is beyond the scope of this work, but it is of interest whether this net negative applies to all sources or likely just surface sources.

739: Again, a reference to an "in preparation" paper is unhelpful here.

750: I think this would be better labelled as Aerosol/Ozone, rather than NTCF since it clearly isn't all NTCFs (which would avoid the "excluding methane" repetition). Since both aerosol and ozone are treated separately anyway, I am not quite sure of the need for this section, especially as they, together, only form a subset of NTCFs.

769-771: These lines seem to contradict each other about whether it is cloud fraction or cloud optical depth, or maybe I misunderstand the point being made.

776: Units are missing for the temperature and cloud parameters. Are temperature changes really in K?

810-811 "too strong" and "bias still exists" – I haven't read the Robertson paper, but could it be said what the too strong albedo response is relative to (other models? observational constraints?), and whether this indicates that UKESM is definitely incorrect (as this text implies) rather than being a plausible outlier?

Typos etc (but note that these are not exhaustive)

25: If "forcing" means "ERF" it should say this.

83-89: Long sentence

64 and elsewhere: “it’s . . . doesn’t . . . weren’t” – it doesn’t worry me much, but such contractions are not usually used in formal scientific writing

216: machine’s

236: These equations use the subscripts cs and clear – do they mean the same thing?

552: refer to Figure 3 here?

592: “Garcia”?

608: I think it important to also refer to the Sovde corrigendum https://doi.org/10.5194/acp-12-7725-2012. .

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