

Interactive comment on “The Radiative Forcing Model Intercomparison Project (RFMIP): Experimental Protocol for CMIP6” by Robert Pincus et al.

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Review of The Radiative Forcing Model Intercomparison Project by R. Pincus et al.
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I regard this particular MIP as having high importance and thank/congratulate the author team for their leadership.

My comments are mostly rather minor, and will be easy to deal with.

2-(6-7): I agree with the “normally interpreted” here, but there have been important indications that diversity in the forcing is also responsible, most notably Chung and Soden (2015 doi:10.1088/1748-9326/10/7/074004) in the recent literature.

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2-13: “Observational estimates of the radiative forcing ...” I didn’t quite get this sentence. The reference to Skeie et al. seems strange given that in their paper “for short lived climate forcers, detailed chemical transport modelling and radiative transfer modelling using historical emission inventories is performed”. I wondered if this was mixing up other work by the same author team on quasi-observationally based estimates of climate sensitivity, rather than forcing?

2-10: IRP – presumably this means “instantaneous radiative perturbation” but this isn’t spelled out, nor is IRP used in some of the following paragraphs (it is given in full). I’m also not quite sure why IRP is “more precise” than IRF, but perhaps I miss something. (Note typo “language” on same line.)

2-22 and 2(32-33): The masking effect of clouds is also normally included in the IRP (and stratosphere-adjusted forcing) – I wasn’t sure why it was just mentioned in the context of ERF. The difference with ERF is that the clouds can respond to the forcing.

6-5 “somewhat surprising” and 6-10 “might be expected to have some error” seem contradictory. I’m not surprised, by the way. We’ve even seen that different implementations of the same radiation code lead to different forcings (e.g Myhre et al. 2009, 10.1127/0941-2948/2009/0411). Perhaps this shows that codes should be tested in implementations as close to those used in the ESM as possible, and this is a point made by the Chung and Soden paper referred to above.

6-11 “current spectroscopic knowledge” – I am not sure what the evidence is that radiation parameterisation error is due, to a significant degree, to spectroscopic knowledge. For example, Kratz (10.1016/j.jqsrt.2007.10.010 – see especially his Table 6) shows rather small impacts of changing HITRAN database for any post-1990 data base, and indeed concludes “the line parameter updates to the HITRAN database are not a significant source for discrepancies in the radiative forcing calculations”, and that is also my overall experience.

6(17-21) I slightly lost the plot here – firstly I am not sure what evidence there is that

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cloud optical properties is “likely to have a larger impact” is (especially in the context of global mean forcing), and if it is the case, it seems to undermine the reason for focusing on clear skies.

6(25) “obscured” – I think the intermodel spread is pretty evident in Collins et al. (2006) where the tropopause standard deviation is almost as big as the mean; I think what has happened in more recent years is a realisation that this really matters for hydrologic(al) sensitivity.

7(2) I had a few comments on this table. One is that specification of “HFC”, rather than a specific HFC, seems too vague to me, and I am not sure what will be learnt unless there is a tighter specification. Second the +4K experiment is well motivated, but not really elaborated on – I guess the focus is on the temperature dependence of transmittance, but this experiment might also have some Planck function dependencies in the results, which will be harder to tease apart. Finally note a typo for the ozone experiment, where it says O2 not O3.

7-23: Not entirely sure why LBLRTM is singled out here – it is, of course, a very important resource, but not the only reference code. Perhaps the reference to Pincus et al. (2015) suffices?

7-30 Perhaps the most major of my comments. I naturally assumed that the shortwave GHG forcing would be included for the greenhouse gases, but I realised at this point that while the insolation conditions are specified for aerosols, they are not specified for greenhouse gases. I would say that one drawback with earlier shortwave GHG comparisons is the simple (single zenith angle etc) specification of the shortwave parameters, and it would be advantageous to have a proper day/global average even if it is for a single day, as in the aerosol case. Could the authors at least clarify the situation regarding the GHG SW forcing?

8-3 “parameterization error increases model diversity” – isn’t parameterization error a (and possibly the major) CAUSE of model diversity (unless all models have the same

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error!).

8-9 This feels like sloganeering, and I haven't really got a clue what "the 20th century belonged to sulphate" means (there are many contenders to the accolade of "the 20th century belonged to ..."! I might vote for the Beatles), especially given the claim that a strong negative aerosol forcing is implausible and the statement that greenhouse gas forcing dominates at 2-23. Incidentally, is the reference to Carslaw et al., in the following sentence, the right one? There seems little or nothing on sulphate global optical depths in the cited paper.

8-13 "Starting in the mid-1970s" – maybe better "Since the mid-1970's"? I presume the stated changes (five-fold, factor of two etc) refer to the present day compared to mid-1970s?

8-29 "less negative" – isn't this "more negative"?

9-3 typo "response"

9-18 This section was written in a different way to the equivalent in Section 3. I could not see a reference to Table 5, and the section is a bit cluttered by the acronyms of the experiments, which are a bit opaque (to me). Perhaps this section could be restructured (not a big job) to do as was done in Section 3, and leave the acronyms to the table, but ensure the underlying motivation of experiments is spelled out.

9-31 "warming" – sorry if I am getting confused, but as I understand, RFMIP only provides evidence of "forcing" rather than "warming". Perhaps it is the coupling with DAMIP that is being referred to here?

10-2 "warming hole" – same comment as above. In fact the whole of this sentence, seems to go a bit beyond what RFMIP results could achieve (although they give important clues), so I wondered if this was really referring to DAMIP.

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