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Interactive comment on "The HadGEM3-GA7.1 radiative kernel: the importance of a well-resolved stratosphere" *by* Christopher J. Smith et al.

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This paper has two objectives: 1) to present a new set of radiation kernels with high top, and 2) to intercompare this and a few other sets of radiation kernels, specifically with respect to the estimate of the radiative impact of stratospheric temperature adjustment in response to CO2. These are both potentially important contributions to make and warrant the efforts here. While I find the first objective well done and generally welcome a new kernel set to enrich the feedback analysis toolbox, I find the second objective relatively poorly executed. I'd suggest the authors take into consideration of the following comments and questions in revising this paper.

There lacks a solid basis for the "recommendation" of the three kernel datasets con-

cluded by the paper. For making such an important and strong statement as to which kernels are better, a principle (criterion) needs to be explicitly stated and justified for comparing them – this is currently missing in the paper. Note that a high-top kernel does not guarantee a higher accuracy in its assessment of radiative impact because the atmosphere which the kernel is based on can be biased – one should be especially cautious if the atmosphere is from a GCM – or because the radiation code used for the kernel computation is biased against the radiation code used in the target GCM simulation. On the other hand, a lower-top kernel also does not necessary lead to a poorer assessment, as shown by the GFDL kernel included here, due to fortuitous compensation of errors or due to some technical details of radiative transfer. Fore instance, some kernels may have used high-top atmospheric profiles in their computation but then truncated to lower top when applied to computing feedback. Moreover, computing and applying kernels at lower vertical resolution may be less subject to the nonlinear coupling between different vertical layers – one can test the non-radiation closure due to this issue, for example, by comparing the sum of vertical kernels to the true radiation change computed using the same radiation model from a vertically uniform 1-K temperature change.

To make a more objective and informative assessment, I suggest adding: 1) the comparisons of a) the global mean radiation change (Ax) due to layers above 1hPa, 10 hPa, tropopause and surface (whole column), respectively, assuming a uniform 1 K change of atmospheric temperature - this would disclose how the different kernels differ with respect to the radiative sensitivity to different portions of the atmosphere and whether there may be compensation of errors from different vertical portions; and b), like a), but using the atmospheric temperature adjustment to CO2 forcing as simulated by one representative GCM or the multiple model mean.

2) additional kernels, especially those observation-based kernels, such as the kernels of Huang et al. (2017) based on ERA-interim and of Yue et al. (2016) based on satellite. The former one (available from https://huanggroup.wordpress.com/research/) was

computed with a high-top atmospheric profile using RRTMG and provides kernel values up to 1hPa, which would provide a good comparison to ECMWF kernel here based on another radiation model (Oslo) – e.g., for assessing radiation code dependency noted above.

Additional comments: Line 17. It is recommended to include Zhang & Huang (2014) here, as this is one of the earliest that quantified CO2 forcing, including both instantaneous forcing and the adjustment components, using kernels. The quantification of adjustment in multiple models reported by this work would make good comparisons to the results reported here, e.g., Table 2, 3.

Line 108. It is not obvious to me that cloud masking has a lesser impact on the surface flux. Please clarify and be more quantitative here.

Line 148-150. Can you illustrate the biases of these low-top kernels mentioned here?

Line 201. Again, the first that applied this residual method was Zhang&Huang [2014].

Line 10, 206. Can't approve such a "recommendation" for the reasons above. And such a recommendation could lead to wrongly denial of the use of the other kernels - both the lower top ones like the GFDL one that can achieve similar quantitative results and those the authors failed to include for comparison here.

References Huang, Y., Y. Xia and X. Tan, (2017), On the pattern of CO2 radiative forcing and poleward energy transport, J. Geophys. Res.-Atmosphere, 122, 10,578–10,593, doi: 10.1002/2017JD027221. Kernel data: https://huanggroup.wordpress.com/research/

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