Response to Reviewer Comments

We thank the Editor and reviewers for their additional comments. Below are our responses (in regular font) to their comments (in ***bolded italic*** font).

Editor:
Dear Authors,
Both reviewers report that the manuscript has been improved and has the potential to become a valuable contribution in documenting the dataset.
Both reviewers have a number of specific revisions that they suggest implementing.
I would like to thank the reviewers for their efforts.

All addressed.

Report #1

Suggestions for revision

Review of Radiative sensitivity quantified by a new set of radiation flux kernels based on the ERA5 reanalysis

By Huang and Huang

essd-2022-474

Summary

The authors have improved the paper relative to its previous version and I am mostly satisfied with the changes they have made in response to my and other reviewers' comments. There remain a few places where further revisions are needed, which I detail below.

Specific Comments

When making use of these kernels, I initially struggled to get good closure at the surface. The issue is that the surface temperature and humidity kernels peak at the lowest atmospheric level, but in many models this level is below ground. Thus even if the kernel is nonzero and large, the radiative impact is zero because that level has no change in temperature or humidity (because it is underground). The solution I found was to set the atmospheric temperature and humidity values equal to their surface values anywhere that they were zero or undefined at the lowest levels of the atmosphere. This correction ensures that there is something nonzero to multiply the kernel by at the near-surface level where the kernel peaks. I think you may need to provide this methodological detail somewhere in the paper in order for people to correctly implement these kernels.

We thank the reviewer for this suggestion and added on Line 969 the following texts:

"To illustrate this issue in an example, consider a location (latitude-longitude grid point) where the surface pressure is 960 hPa in a GCM and the lowermost level of non-zero value of ERA5 air temperature kernel is located at 975 hPa. Had the air temperature change been set to zero or NaN value due to the GCM ground level being above 975 hPa, the contribution to the surface radiation change from the air temperature change in the bottom layer of the atmosphere would not be included, which may lead to a biased quantification of the feedback. We recommend interpolating the air temperature changes from the GCM vertical profile to the kernel vertical profile, using surface values to replace the missing levels (e.g., the 975 hPa level in the above example) before multiplying with the kernel values, when computing the feedbacks of air temperature and water vapor."

Figure 5: I don't feel as though the results shown in the right column of this figure are adequately explained. I think the statement on L370 is incorrect: Rather, the figure indicates that the negative surface temperature kernel has strengthened in ERA5. Why has this happened? I cannot rationalize this from looking at the changes in Figure 5g,h,i. I would have thought the moister atmosphere might weaken the surface temperature kernel (the opposite of what happens). I also am not sure what is being referred to on L373 regarding the

linkage between the discrepancy noted in Figure 4i and the SW WV kernel results. Please elaborate on this.

To clarify these results, we added the following text at Line 383:

"Although the total column water vapor and total cloud cover are higher in the ERA5 (Figure 5h and i), their differences are complex and vertically non-uniform (Figure S8 d and e), which leads to a slight strengthening of surface temperature kernels compared with ERAi (Figure 5j)."

Figure 6: This is also not explained particularly well. It is stated on L391-392 that "the reduction of sea ice in the Arctic region leads to a significant decrease of radiative sensitivity to surface albedo". If this statement is taken at face value, one would expect panels d and f to look like mirror images of panel a, but there is little correspondence at all. But I don't think there is any reason to expect the albedo kernel to depend on surface albedo since it is defined as the SW impact of a 1% increase in albedo. This kernel mainly varies with insolation and cloud cover. So the change in total cloud cover (panel b) actually explains most of the geographical structure in the change in surface albedo kernel (rather than "also contributing" as the authors state).

We agree that clouds also explain the surface albedo kernel value differences. However, we would like to point out that surface albedo kernel value does depend on the surface albedo value, as discussed by Huang et al. (2021, c.f. the univariate nonlinearity discussion there, e.g., Fig 3 and 6), due to the multiple-scattering between the surface and the atmosphere.

Huang, Y., Huang, H., & Shakirova, A. (2021). The Nonlinear Radiative Feedback Effects in the Arctic Warming. *Frontiers in Earth Science*, *9*, 693779.

To clarify these results, we revised the relevant texts at Line 408:
"In the sea ice loss case, the reduction of sea ice in the Arctic region (Figure 6a) leads to a significant decrease of radiative sensitivity to surface albedo in the areas with noticeable sea ice retreats (Figure 6d and f), with the maximum difference exceeding 30% of the radiative kernel value, because of the nonlinear dependency of the reflected solar radiation on the surface albedo (e.g., see Huang et al., 2021b, Fig. 3 and Fig. 6). The cloud cover changes also contribute to changes in surface albedo kernel values due to the coupling effect between cloud and surface albedo (see Huang et al., 2021b), which for example is seen in the Siberia and to the west coastline of Europe."

Figures 8: I find it very hard to reconcile the very small SW residuals in Figure 8 with the quite substantial zonal mean residuals shown in the bottom row of Figure 7. Please double check this calculation. I also suggest removing the substantial white space at the top and bottom of this figure, since no values extend above about 2 W/m2/K or below about -3.5 W/m2/K. In the caption, "list" should be "listed", and "pentagrams" should probably be "stars."

We verified the results are correct. This is because the area-weighting limits the effect of the high latitude biases on the global mean.

We revised the figure according to the reviewer's suggestions.

Abstract: There is no mention of the analysis regarding dependence of the kernel on mean state (as examined in Section 3.3)

Added.

L22: seems odd to not mention climate change in addition to variability here.
L65: should be "approximate"
L81: you may also consider citing Figure S2 of Zelinka et al (2020)
L87: I don't think "calls into question" is the right phrase here. Perhaps "...this warrants investigating whether..."
L108: should be "set" (singular)

All corrected.

L147-156: This methodological description is still awkward. You are not performing separate simulations, right? There is one control simulation, and within that simulation you perform multiple radiative calculations, each time with a small perturbation in a field. I think "simulations" should be replaced with "calculations" in most cases.

Revised.

L152: should be "calculate the radiative"

Corrected.

L172-173: Many analyses in the paper use monthly resolved kernels, but I don't think this is stated when it occurs.

Added.

L180: I think you should explain explicitly what atmospheric kernels are here, since it may not be obvious what they are or how they are computed

Clarified.

Figure 1 and others: Need to notify the reader that the colorbar ranges vary among panels

Added.

L258: I understand why the authors refer to the Kramer et al (2019) kernels as CloudSat/CALIPSO, but this could possibly confuse readers who might think they are cloud radiative kernels. My understanding is that these make use of thermodynamic fields from

ECMWF, so they actually use similar inputs as the ERA kernels developed here. I think a brief clarification of what these kernels are is warranted to avoid confusion.

Clarified.

Table 1: Suggest renaming the third column as "Vertical levels" or something, since the resolution is not shown
L289: should be "fractional"

Both corrected.

L299,308,404 and elsewhere: The word "biases" still shows up in the revision even though these are not biases.

All corrected and we double-checked other "biases"/"biased" in the manuscript and they all now represent the proper meaning.

L323: 10% of what? Please specify

Specified.

L426-427: commas are not needed after the experiment names, and "and" should be inserted after "1850,"

Corrected.

L432: Rather than "following the previous studies" this methodology deviates in a fairly significant way from Zelinka et al and Smith et al (and most studies that use abrupt-4xCO2 experiments). Namely, the conventional way is to difference piControl and abrupt over the duration of the abrupt run, compute annual mean values, and regress on annual mean surface temperature anomalies (the Gregory method). The method used here is quite different and needs to be explained and motivated better. Note – I am not criticizing the method. I just want you to explain and motivate it better, and to delete the phrase about it following previous studies. One nice motivation is that it obviates the need to worry about rapid adjustments.

The following texts are added at Line 453 to clarify this, following the reviewer's suggestion:

"To exclude the effect of rapid adjustments, the radiative feedbacks in this study are measured using the difference of feedback variables between the abrupt4xCO2 and piClim-4xCO2 experiments and vertically integrated from the surface to model top. Note that these treatments are different from some other studies, e.g., Zelinka et al., 2020, which used piControl simulation as the climatology baseline and vertical integration from the surface to the tropopause, although the quantitative differences in the diagnosed global mean feedback values are small."

L441: "notre" should be "note"
L447: delete "atmospheric" since it includes surface temperature and albedo

Both corrected.

L449: Somewhere in this section you need to note that the kernels and the climate fields they are multiplied with are at monthly resolution

Added.

L460-471: For any casual reader, this description of how to compute cloud feedbacks is probably inadequate and bewildering. As another reviewer noted, the math ends up the same, but the physical connection of the equations to how it relates to clouds is lost. Suggest re-doing this (or appending discussion onto it), perhaps adhering more closely to Eqs. 22-25 in Soden et al (2008).

The following explanations are added on Line 491 to clarify our formulation:

"It is worth noting that ΔR_c measured according to Eq. (6) is essentially the part of total radiation change not explained by the non-cloud feedbacks and is equivalent to the other formulations of the adjusted cloud radiative effect method (e.g., Shell et al. 2008; Soden et al., 2008). Interested readers can refer to, for example, Huang (2013) for a detailed formulation and explanations of the method."

Figure 7: The figure panels are too small, partly because there is so much redundant information that is repeated. All colorbars are identical, so there is no need to show them near each panel – this would clear up a lot of space. You could also label each row once and each column once rather than putting a title on each panel.

We reorganize the panels and now use only one common colorbar in each column, but keep the title on each panel as the RMS has to be shown there.

Figure 10: The ERA5 kernels produce anomalously large Ts and Ta feedbacks relative to the other kernels, but I don't think this is discussed at all. Please discuss.

The following texts are added in Line 584 and Line 900 to discuss this result:

"The sum of air temperature and surface temperature feedbacks shows better consistency compared with the respective components (except for the HadGEM3 kernel), and the respective air temperature and surface temperature feedbacks quantified by the ERA5 kernel are stronger than the results from the other kernels. These discrepancies are due to the reason discussed in the Appendix – a possibly wrong quantification of surface temperature effect."

"As a result, in Figure 10, we see stronger air temperature and surface temperature feedbacks quantified from ERA5 kernels than those from other kernels and in Table S4, we can only report the sum of surface and air temperature feedbacks."

L579: should be "containing." Also I would suggest noting in this section (rather than earlier in the text) that the multi-kernel dataset is also provided at this link. Thank you for providing this.

Added.

L587: I don't think "including the kernel values" is needed here as this is obvious
L594: should be "Antarctic" or "over Antarctica"

Both corrected.

L598, L617: 30%/10% of what?

Added.

L600: suggest pointing the reader to the Appendix here.

Added.

L605: I think you should say this "might explain" the discrepancies, since you have not established this across kernels (which also differ in other ways including radiative transfer codes)

Corrected.

L618 and elsewhere: The word "affirm" appears 10 times in the manuscript; suggest using a synonym occasionally.

Modified.

L807: "the multiply of" is not the correct phrasing

Corrected.

References

Kramer, R. J., Matus, A. V., Soden, B. J., & L'Ecuyer, T. S. (2019). Observation-Based Radiative Kernels From CloudSat/CALIPSO. Journal of Geophysical Research: Atmospheres, 124(10), 5431– 5444. https://doi.org/10.1029/2018JD029021

Soden, B. J., Held, I. M., Colman, R., Shell, K. M., Kiehl, J. T., & Shields, C. A. (2008). Quantifying Climate Feedbacks Using Radiative Kernels. J. Climate, 21, 3504–3520. https://doi.org/10.1175/2007JCLI2110.1

Zelinka, M. D., Myers, T. A., McCoy, D. T., Po-Chedley, S., Caldwell, P. M., Ceppi, P., et al. (2020). Causes of Higher Climate Sensitivity in CMIP6 Models. Geophysical Research Letters, 47(1), e2019GL085782. https://doi.org/10.1029/2019GL085782

Reviewer #2:
Suggestions for revision

The authors have thoroughly addressed my comments, revising and adding valuable analysis to the sections I had concerns about. I recommend the manuscript for publication after the authors address a few minor comments below:

Line 104-105: In their reviewer response the authors note that many studies have shown the superiority of ERA5, including over ERAi. It would be helpful to cite those papers after "…which demonstrates superior 105 accuracy in the quantification of various atmospheric states"

Added (Line 110).

Line 405-407: What if someone wants to use radiative kernels specifically to diagnose radiative feedbacks associated with ENSO? Does this recommendation still apply? Or should they use a kernel that was derived from an El Nino/La Nina year? I wonder if this recommendation is specifically geared towards users who want to compute global feedbacks for long-term climate change?

We agree the complexity of the situations should be considered when choosing kernels. Here, we mean to advise on how to capture the "mean" radiative sensitivity. This is clarified with the following texts (Line 424):

"If only one year's atmospheric profiles are used to generate radiative kernels, we recommend selecting a year without significant anomalies in atmospheric states, e.g., due to El Nino or severe sea ice loss, so that the computed kernel values better represent the radiative sensitivity climatology."

Line 516-519: The authors refer to e.g. air temperature and water vapor feedbacks as the source of inter-kernel spread in cloud LW feedback (and albedo for cloud SW feedback). To avoid confusion, they should be a bit more specific mention they are referring to the "cloud masking" terms in the cloud feedback calculation i.e., the difference between all-sky and clear-sky temperature feedback, water vapor feedback, etc.

Clarified following the reviewer's suggestion (Line 542).

Figure 9 caption should say similar to figure 7 not figure 8

Corrected.