Response to Reviewer 2

This study uses well-established radiative kernel and APRP technices to diagnose anthropogenic aerosol radiative cooling and its cloud-mediated components in the TaiESM1 model, primarily in the context of historical minus pre-industrial conditions. It is a very readable paper, and I appreciated learning about a fairly unique model that has not yet featured prominently in aerosol forcing diagnosis work of the past. This paper will be relevant to ACP readers as the community prepares for similar analysis expected in CMIP7, and will be useful as a reference for future TaiESM developers. That being said, at times the paper lacked sufficient motivation for the methodological choices and analysis provided, and at times a lack of methodological description left me confused. At points I also felt the analysis was a bit surface level given that this is a single-model paper. My comments below reflect that.

We thank the reviewer for the positive assessment and the constructive comments. The main changes in the revised manuscript are:

(1) fixed-SST piClim-control and piClim-aer simulations with TaiESM1, which follow the protocol of Zelinka et al. (2023) for the APRP-based ERF components and provide a benchmark for the residual kernel method;

(2) the historical − piNTCF contrast in the kernel analysis (revised Fig. 4), with piNTCF − piControl moved to the Supplement (Fig. S1);

(3) maps of the changes in liquid water path and cloud droplet number (revised Fig. 2) and a model-specific discussion of why ERFaci is strong in TaiESM1 (Sect. 4.1);

(4) a comparison with the fixed-SST results of Zelinka et al. (2023), with ERFaer isolines and a 1:1 line replacing the regression lines of the original Figs. 6 and 7 (revised Figs. 7 and 8);

(5) a more detailed description of the methods in Sect. 2 (cloud radiative effect versus cloud radiative response, the definition of α, histSST and the experiment protocols);

(6) a shorter Sect. 4.2, refocused on the SST-mediated cloud response and the effect of air–sea coupling.

With the fixed-SST protocol, TaiESM1's ERFaci (−1.98 W $m^{-2}$) is almost unchanged from the original estimate (−2.00 W $m^{-2}$), so the main conclusion of a strong cloud-mediated aerosol cooling is robust. Items (1), (3) and (5) respond to the reviewer's main suggestions. Point-by-point responses follow.

General: The kernel method used in this study is useful for a multi-model evaluation, because it uses very common CMIP experiments. But this is a single-model study, so

the authors could have just run fixed-SST timeslice experiments designed for radiative forcing analysis, like the piClim RFMIP protocol. This is a more direct way to diagnose aerosol foricng and the simulations are cheaper. Why then did the authors rely on the kernel method and the accompanying coupled simulations? Some motivation should be provided. If the motivation for this choice was to also be able to capture the corresponding temperature-driven responses, it would be useful to say so in the introduction as a motivator. For instance, was the lack of warming in this model an impetus for this analysis?

Reply: We thank the reviewer for this important suggestion. We agree that fixed-SST time-slice experiments provide a more direct framework for diagnosing aerosol effective radiative forcing in a single-model study. In the revised manuscript, we have added the piClim-control and piClim-aer experiments and now use the piClim-aer − piClim-control contrast as the primary basis for the APRP decomposition of aerosol effective radiative forcing. The revised manuscript now makes this distinction explicit in the Introduction and Methods. The two approaches are therefore used for complementary purposes: the kernel analysis evaluates how this aerosol-mediated cloud response manifests in the coupled historical climate system, whereas the fixed-SST APRP analysis identifies the dominant aerosol forcing pathway. The kernel analysis was therefore motivated by the need to separate aerosol-mediated and temperature-related cloud responses in the coupled historical climate, which also helps to explain the weak historical warming of TaiESM1 (Sect. 4.1).

Line 95-103: These lines make the argument for why we can't just look at AOD to understand ACI forcings. I agree, but I don't think anyone views ACI so narrowly in that way anymore. I wonder if there is another way to frame this? Maybe pointing out that ACI is driven by many factors and there is a benefit to decomposing the relevant radiation changes?

Reply: We agree. The paragraph has been rewritten to motivate the decomposition of the aerosol radiative effects rather than to argue against relying on AOD.

Section 2.1: Some additional, relevant detail on the TaiESM1 aerosol and cloud schemes would be helpful here. The authors could describe the activation scheme, how aerosol size distribution is handled (bulk? Modal?) Have the cloud microphysics been modified since CESM1?

Reply: Thank you. Section 2.1 now includes a paragraph on the aerosol and cloud schemes as "The parameterizations relevant to aerosol-cloud interactions include the two-moment stratiform cloud microphysics of Morrison and Gettelman (2008), the University of Washington moist turbulence scheme (Bretherton and Park, 2009) and the shallow convection scheme (Park and Bretherton, 2009) from CAM5, whereas the cloud macrophysics (cloud fraction) is replaced by the GTS scheme. Aerosols are represented by SNAP, which adopts a modal representation of the aerosol size distribution and prognostically tracks its zeroth, second, and third moments, corresponding to particle number, surface area, and mass, respectively. Droplet activation follows Abdul-Razzak and Ghan (2000), so the cloud droplet number responds directly to the simulated aerosol population, and aerosols are removed by rainout and washout. The deep convection scheme is not coupled to the cloud microphysics, so aerosol effects on cloud droplet number are represented in stratiform clouds but not in deep convective clouds."

Section 2.1 and Table 1: piNTCF is an "all but one" experiment where everything is set to historical while NTCF is returned back to pre-industrial conditions, while in contrast AerosolOnly is a "one but all" experiment where the aerosols are perturbed historically and everything else is set back to pre-industrial. Does this protocol discrepancy matter for TaiESM and thus complicate the combined analysis of these experiments? Some comment on this in the text would be helpful. Simpson et al. (doi.org/10.1175/JCLI-D-22-0666.1) suggest it can matter, but not for all models.

Reply: We thank the reviewer for pointing us to Simpson et al. (2023). TaiESM1 now provides three estimates of the aerosol-related cloud radiative response: an all-but-one estimate from historical − piNTCF (−1.21 W $m^{-2}$), a single-forcing estimate from AerosolOnly − piControl (−1.15 W $m^{-2}$) and a fixed-SST estimate from piClim-aer − piClim-control (−1.45 W $m^{-2}$). Because TaiESM1 has no interactive chemistry, piNTCF differs from historical only in the anthropogenic aerosol and aerosol-precursor emissions, so the two coupled estimates perturb the same forcing agent. Their total cloud radiative responses (−1.88 and −1.77 W $m^{-2}$), surface cooling (−1.27 and −1.17 K) and aerosol-mediated responses are similar, so the protocol has little effect on the coupled estimate in TaiESM1, although it can matter in other models (Simpson et al., 2023). Both coupled estimates are weaker than the fixed-SST estimate, mainly because

the α·ΔTs term overcorrects for the cloud response to the aerosol-induced cooling (Sect. 4.2). This comparison and the reference to Simpson et al. (2023) have been added to Sect. 3.2.

Line 153: The authors should provide more information about histSST. Are time-evolving SST's prescribed? If so, over what time period? Or is a climatology of SST's prescribed? Where do the SST's come from?

Reply: The histSST experiment prescribes monthly-mean, time-varying SST and sea-ice concentration for 1850–2014 taken from the TaiESM1 historical simulation, following the AerChemMIP protocol (Collins et al., 2017). This information has been added to Sect. 2.1 and Table 1.

Equation 2 and related text: An important point with this method is that because alpha is derived from 1pctCO2, it is not just a sensitivity of cloud feedback, but also include the contributions of CO2 cloud and other rapid adjustments, which can be large. This is why removing the alpha_1pctCO2 * dT term isolates aerosol adjustments so well. Without including the CO2-induced rapid adjustments (i.e. if alpha from abrupt4xCO2 was used), the residual calculation would still include potentially large CO2 adjustment terms. This should be explained.

Reply: We fully agree and thank the reviewer for this clear explanation. Because α is derived from the transient 1pct$CO_2$ experiment, in which the $CO_2$ forcing and ΔTs increase together, it includes the rapid cloud adjustment to $CO_2$ as well as the temperature-mediated feedback. Removing α·ΔTs therefore also removes the $CO_2$-induced cloud adjustment, whereas a regression slope from abrupt-4x$CO_2$ would leave this adjustment in the residual. This explanation has been added to Sect. 2.2. The text now also distinguishes α from the abrupt-4xCO2 cloud feedback shown in Fig. 4e.

Line 176-182: I see mention of good agreement with aerosol cooling from fixed-SST experiments. I presume this means the author's "aerosol-mediated cloud radiative response" calculation is essentially an estimate of aerosol forcing "cloud rapid adjustment"? If so, it would be helpful to mention the connection to the term "rapid adjustment", since it is widely used in the radiative forcing literature now.

Reply: Thank you for this suggestion. The aerosol-mediated cloud radiative response is related to, but broader than, the cloud rapid adjustment. In a coupled simulation, it

contains the instantaneous cloud radiative forcing from aerosol–cloud interactions (the Twomey effect), the rapid adjustments of cloud amount, liquid water path and semi-direct effects, and the cloud response to aerosol-induced changes in SST and circulation. Only the first two parts belong to the aerosol ERF, and the new fixed-SST simulations estimate them directly (−1.45 W $m^{-2}$; revised Fig. 4d). This distinction has been added to Sect. 2.2.

Line 166: The authors should clarify whether "cloud radiative response" differs from "cloud radiative effect" and how. For instance, is cloud masking removed?

Reply: The two quantities do differ, and both are now defined in Sect. 2.2. The cloud radiative effect (CRE) is the difference between all-sky and clear-sky TOA fluxes, and ΔCRE includes cloud-masking effects from changes in temperature, water vapor and surface albedo. By contrast, the cloud radiative response used in the radiative-kernel analysis follows Soden et al. (2008) and C. Wang et al. (2021), in which the change in CRE is corrected for these non-cloud cloud-masking effects using the corresponding all-sky and clear-sky radiative kernels. Thus, cloud masking is removed from the kernel-derived cloud radiative response, whereas the SWCRE and LWCRE diagnostics shown separately in the manuscript represent the uncorrected cloud radiative effects. We have revised Section 2.2 to define these quantities explicitly.

Lines 215-218 and Figure 1a: I am curious why the AOD change is positive in the southern ocean for historical-piControl when the aerosol-dominated and greenhouse gas-dominated AOD changes in that region appear to be equal and opposite in sign. Is there another component contributing AOD? Nonlinearities? Or am I mistaken about the magnitudes? If the latter, it may be because the colorbar is too broad and thus hard to decipher the magitude of the background AOD changes.

Reply: Thank you for pointing this out. The apparent inconsistency was primarily caused by the broad color scale in the original figure, which made the relatively small AOD changes over the Southern Ocean difficult to distinguish. We have revised Fig. 1 using a narrower color scale to better resolve these regional differences. The updated figure shows that the apparent discrepancy results from the visualization rather than an additional large-scale AOD signal.

Line 222-234: Similar to two comments above, this is the first mention of cloud

radiative effect, but the authors should define it in the methods section and clearly state how it differs from cloud radiative response. Also, I would prefer they use the abbreviations LWCRE and SWCRE instead of LWCF and SWCF, because they call it cloud radiative effect and not cloud forcing.

Reply: Thank you. The CRE is now defined in Sect. 2.2 together with the cloud radiative response. The abbreviations LWCF and SWCF have been replaced by LWCRE and SWCRE throughout the text and figures.

Line 233-235: The authors argue the LW cloud forcing in the aerosol-related case is not spatially coherent, but I'm not sure what they mean by spatially coherent. I see a clear tropical Pacific signature that appears ENSO-like and what one may expect from a cloud feedback/circulation process. It is important to clarify this, because they further argue in the next sentence that this lack of coherency suggests aerosol-cloud changes is dominated by SW not LW cooling. I agree that's true in a global-mean sense, but not necessarily regionally.

Reply: Thank you for pointing this out. We agree that the original wording understated the regional structure of the LWCRE response. In particular, the aerosol-related contrast exhibits a pronounced ENSO-like LWCRE pattern over the tropical Pacific. We have therefore revised the text to acknowledge that longwave cloud radiative changes can be regionally important. At the global scale, however, positive and negative LWCRE anomalies partly offset each other, and the global-mean LWCRE contribution remains smaller than the SWCRE contribution. The revised text now distinguishes the regional importance of the longwave response from the dominance of shortwave cooling in the global mean.

Figure 3 Caption: I think there is some confusion here about "row" vs. "column". The text description of figure position does not align with that is actually shown.

Reply: Thank you for noticing this. The caption of revised Fig. 4 (former Fig. 3) has been corrected and matches the new layout. Rows (a)–(d) show the contrasts historical − piControl, AerosolOnly−piControl, historical−piNTCF, and piClim-aer−piClim-control, the left column shows the total cloud radiative response (W $m^{-2}$), and the right column shows the aerosol-mediated cloud radiative response (W $m^{-2}$). Panel (e) shows the cloud feedback map (W $m^{-2}$ $K^{-1}$ ), which is derived once from abrupt-4xCO2 and

is common to all contrasts.

Figure 3: It would be helpful to print the global-mean values for each subplot like has been done for other figures in the paper.

Reply: Global-mean values are now shown in each panel of revised Fig. 4.

Line 259-260: I am surprised the global-mean total cloud response and aerosol-mediative cloud response are so similar. Likewise, the spatial maps of these terms in Fig 3a look almost identical. Even the AerosolOnly-piControl has a larger difference between total and aerosol-mediated response. Does it suggest the historical simulation has very little contribution from temperature-driven radiative changes or from GHG radiative forcing? Is warming (seems yes, as shown later) and greenhouse gas radiative forcing small in this model? Some context would be helpful.

Reply: We thank the reviewer for this comment. The difference between the total ($-2.08$ W m$^{-2}$) and aerosol-mediated ($-2.24$ W m$^{-2}$) responses ($+0.16$ W m$^{-2}$) is the temperature-related term $\alpha \cdot \Delta Ts$, and it is small because the historical − piControl warming of TaiESM1 is small ($\Delta Ts = 0.30$ K ). The cloud feedback itself is not weak ($+0.84$ W m$^{-2}$ K$^{-1}$; revised Fig. 4e). The weak warming instead reflects the strong aerosol cooling of TaiESM1, as discussed in Sect. 4.1 (revised Fig. 8). A sentence stating this has been added to Sect. 3.2.

Line 271-273: It's not clear why the authors have started using the AerosolOnly – piControl contrast to diagnose aerosol response after using historical-piNCTF previously. Is it because they want to highlight the effects of the different base states? If so, that should be stated at the beginning of this paragraph.

Reply: We thank the reviewer for this comment. AerosolOnly − piControl was included as a second coupled estimate with a different design. Historical − piNTCF removes the anthropogenic aerosol emissions from the full historical forcing (all-but-one), whereas AerosolOnly − piControl adds them to pre-industrial conditions (single-forcing). Both contrasts are now introduced together at the beginning of Sect. 3.2 and compared in revised Fig. 4 (rows b and c). Their aerosol-mediated responses are similar ($-1.21$ and $-1.15$ W m$^{-2}$), so the larger historical − piControl residual arises mainly from the non-aerosol historical forcings (see also the reply on Simpson et al., 2023).

Line 287-291: Since this is a single-model study, there is opportuntity to provide more detail about the aerosol-mediated cloud response from piNCTF-piControl. Could dust or sea-salt changes be playing a role? Does this model have interactive chemistry so that interplay between $CH_4$, $O_3$, and OH could be having an impact on background sulfur dioxide or organic aerosols? Without more detail, the evaluation of piNCTF-piControl doesn't add much to the story, as we know anthropogenic aerosol changes are not the main culprit based on experimental design.

Reply: Thank you for this suggestion. TaiESM1 does not include interactive tropospheric ozone chemistry, and both $CH_4$ and ozone concentrations are prescribed. Therefore, the $CH_4$–$O_3$–OH chemical feedback suggested by the reviewer is not represented in these simulations. The piNTCF − piControl contrast includes the response to historical non-NTCF forcing, which may also affect natural aerosols, including dust and sea salt, through changes in meteorological conditions. However, the present diagnostics do not allow us to quantify these contributions. Given its limited relevance to the primary focus of this study, the kernel diagnostics for piNTCF − piControl (former Fig. 3c) have been moved to the Supplement (Fig. S1), and Sect. 3.2 now states that this residual ($-1.03$ W m$^{-2}$) is not attributable to anthropogenic aerosols.

Line 367: Define TS here and again in the relevant figure captions.

Reply: Thank you for pointing this out. $\Delta$Ts is now defined in Sect. 4.1 and in the caption of revised Fig. 8 as the change in global-mean surface temperature between historical and piControl, averaged over 1985–2014.

Section 4.1 generally: I appreciate the authors put the TaiESM1 results into the context of the CMIP ensemble. That was helpful. But since this is a single model study, I feel the authors could go deeper in their discussion of the results. Why is ACI scattering so large, for example? Is it related to model tuning choices made? Is there something unique about the TaiESM1 aerosol activation or microphysics schemes that may be the culprit? I don't expect the authors to find all of the answers, but a more model-specific discussion of the causes would really elevate this paper and its novelty relative to the many multi-model papers these methods are usually used for.

Reply: We thank the reviewer for this suggestion. We have expanded Section 4.1 to provide a more model-specific interpretation of the strong aerosol–cloud interactions

in TaiESM1, incorporating additional diagnostics of vertically integrated cloud droplet number (CDNUMC) and liquid water path (LWP; revised Fig. 2). The results indicate that the strong cloud-scattering response is associated with substantial increases in CDNUMC from relatively low pre-industrial values, particularly over South Asia and the northwestern Pacific. Increased LWP over parts of East Asia may further enhance cloud optical thickness, potentially reflecting adjustments in warm-rain microphysics. These findings suggest that changes in droplet number and cloud water contribute to the strong negative ERFaci in TaiESM1. However, the relative contributions of aerosol activation, cloud microphysics, and model tuning cannot be established from these diagnostics alone. We have therefore discussed these processes as potential mechanisms rather than attributing the strong response to a particular parameterization or tuning choice.

Line 378-385: This paragraph is written as if the aerosol-cloud coupling is wrong or deserve additional development. Is it? Some evidence (e.g. citation of observational constraints) would be useful.

Reply: We agree with the reviewer that the original wording was too strong and could be interpreted as implying that the aerosol–cloud coupling in TaiESM1 is necessarily biased. Our diagnostics identify a strong cloud-mediated aerosol response and indicate which cloud processes contribute to that response, but they do not by themselves demonstrate that these processes are incorrectly represented. We have therefore revised the paragraph to avoid implying a demonstrated model deficiency. We also added references to recent observational constraints on aerosol–cloud interactions to provide context for the range of plausible cloud responses, while noting that a direct observational evaluation of these processes in TaiESM1 is beyond the scope of the present study.

Section 4.2 generally: I don't really understand the point of this analysis. Some additional motivation context would be helpful. Anthropogenic aerosol forcing is largely unrelated to ocean dynamics or SSTs, and since the SST fields are the same in the historical and histSST experiments (are they?) isn't it obvious the temperature-mediated cloud radiative changes would be extremely similar too? The authors point to some regional differences in cloud fraction between historical and histSST, but could it just be noise? In summary, I can appreciate that ocean coupling dynamics, separate

from SST change, can have an impact on the climate, but it just doesn't seem particularly relevant to a study about aerosol radiative forcing and I think the results here prove that.

Reply: We agree with the reviewer. Because histSST prescribes the SST and sea ice of the TaiESM1 historical simulation, the global-mean cloud radiative responses of historical and histSST are expected to be very similar, and the results confirm this (−2.08 and −2.00 W $m^{-2}$). Section 4.2 has therefore been shortened and refocused. It now shows that the ocean affects the aerosol-mediated cloud response mainly through the aerosol-induced SST change, which accounts for the 0.30 W $m^{-2}$ difference between the coupled AerosolOnly and fixed-SST estimates (revised Fig. 9c), rather than through air–sea coupling for a given SST evolution. The original Figs. 9 and 10 have been moved to the Supplement (Figs. S3 and S4), and the remaining regional differences between historical and histSST are small and mostly not statistically significant at the 95 % level (Fig. S4).