Comment: This paper examines dust effective radiative forcing in nine CMIP6 Earth System Models using AerChemMIP experiments that double dust emissions under pre-industrial conditions. The total forcing was decomposed into direct and cloud (indirect) forcings. The intermodal differences were related to dust and cloud properties. The dust effective radiative forcing, especially the indirect forcing, is highly uncertain. Therefore, this is a meaningful work that helps to understand this uncertainty, and it is well within the scope of ACP. However, I find that some of the results are not supported by enough evidence and/or not fully explained/discussed. Therefore, I would recommend a major revision for this manuscript. Please find my general and specific comments below.

Reply: *We thank the reviewer for the insightful comments and suggestions. The reviewer's comments have meaningfully contributed to elevate the quality of the manuscript. We have added evidence and explanations where it was suggested. Line numbers refer to the tracked changes version of the revised manuscript. Blue text represents added new text in the manuscript.*

General comments

Comment: 1. About the dust impact on cirrus clouds in NorESM2-LM (L394-400). I think it is not plausible enough to say that the IWP and high cloud fraction increase are caused by cirrus cloud increase. First, high cloud may include some mixed-phase clouds, and IWP is a column integrated variable. In addition, the authors have not shown evidence supporting the statement that "in regions where heterogeneous ice nucleation predominates, additional INPs typically increase ice crystal concentrations, which appears to characterize NorESM2-LM". I suggest the authors to examine 3D zonal average variables (e.g., heterogeneous and homogeneous ice nucleation rate, ice water content, cloud fraction, ice number concentrations, temperature, etc.) to better support this statement. If this is not possible due to lacking output, I suggest the authors provide some discussion based on possible previous studies.

Reply: *We thank the reviewer for this question. The reviewer is of course right that the IWP is a column integrated quantity and therefore would include changes in cloud ice from both the cirrus and mixed-phase clouds. However, a bug in NorESM2-LM has the effect of disabling heterogeneous nucleation outside of the cirrus regime. The issue stems from a limiter in NorESM2 (CAM6-Nor) that was designed to prevent the tendencies of in-cloud ice number concentration from exceeding the maximum of what is possible based on the calculated number concentration of available ice nucleating particles (INPs). The problem with this limiter is that the tendencies for ice particle formation (immersion, contact, and deposition freezing) from the classical nucleation theory scheme, as described in Hoose et al. (2010), did not contribute to this limit. Therefore the Hoose scheme could not add to the ice number. This did not affect the parameterisation for heterogenous ice nucleation within cirrus clouds, which is activated based on a temperature threshold (below -37 C). Therefore, we are confident that the change in IWP is mainly driven by a cirrus cloud response. Figure S8, now added to the supplementary material, also confirms that the IWP response in the model is driven by changes in the cirrus cloud regime.*

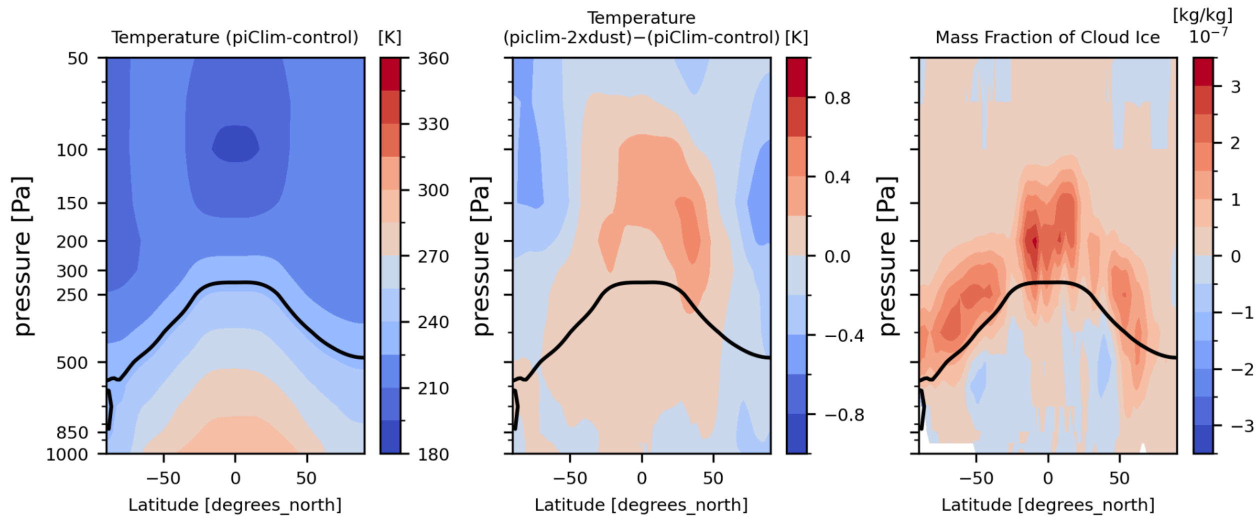

Figure S8: *Zonal plot of NorESM2-LM piClim-control temperature, change in temperature between piClim-control and piClim-2xdust and change in ice mass fraction (kg/kg).The black solid line highlights the −37°C isotherm, which indicates the transition between the mixed-phase and cirrus cloud regimes.*

Comment: 2. About Nd decrease in EC-Earth3-AerChem and MPI-ESM-1-2-HAM (L415-417) and other models. It is not clear to me why the increase in dust results in Nd decrease. If dust does not activate as CCN, please specify (this does not seem to be the case for MPI-ESM-1-2-HAM as written in L155-156?). Also, how do the two models treat condensation of other aerosols/gases on dust? Do they assume that no secondary aerosol that can be CCN is formed on dust so that the condensation is a pure sink for, e.g., SO 2 ? In addition, the CCN decrease (Figure S6) is not necessarily related to "reducing formation of secondary aerosols". The authors may consider directly show changes in secondary aerosols and their precursor gases (e.g., sulfate, nitrate, SO 2 , etc).

Reply: *We appreciate the reviewer's questions. We have now specified in Table 1 which models activate dust as CCN. The (small) decrease in Nd observed in EC-Earth3-AerChem and MPI-ESM1-2-HAM is consistent with several contributing factors, which we cannot disentangle based on the limited diagnostics available in CMIP6. Only those dust particles which get coated by soluble material become CCN. Note that the coating process is not necessarily increasing the number of CCN; the presence of more dust reduces likely the number of (fine) aerosol particles which can act as CCN, due to condensation of precursor gases and coagulation processes. Furthermore, the overall reduction in cloud fraction might also accompany a decrease in global mean Nd.*

Q1: Also, how do the two models treat condensation of other aerosols/gases on dust? Do they assume that no secondary aerosol that can be CCN is formed on dust so that the condensation is a pure sink for, e.g., SO 2 ?

Reply: *We have updated the description of how EC-Earth3-AerChem and MPI-ESM1-2-HAM treat condensation of aerosols on dust. In short, they represent coagulation of soluble modes on the dust particles and condensation of e.g. H2SO4, and these processes in the model can act to transfer the dust from the insoluble to soluble state. Dust in the* *soluble state is considered a CCN. Therefore, dust is not merely a sink for aerosol precursor gases; if sufficient mass condenses onto a dust particle, it will be transferred to the soluble mode and be counted as a CCN.*

Q2: In addition, the CCN decrease (Figure S6) is not necessarily related to “reducing formation of secondary aerosols”. The authors may consider directly show changes in secondary aerosols and their precursor gases (e.g., sulfate, nitrate, SO 2 , etc).

Reply: *Between the piClim-control and piClim-2xdust simulations, emissions of precursor gases remain unchanged. However, the increased dust does provide more surface area for condensation, resulting in a larger condensation sink for precursor gases, which subsequently reduces the new particle formation rate from these gases. Coagulation with dust can only decrease the number of cloud condensation nuclei (CCN), as soluble particles stick to initially insoluble dust particles, creating larger soluble particles. Furthermore, doubling the dust concentration would lead to a substantial reduction in radiation at the surface locally, resulting in less evaporation and cloudiness, which could also contribute to a decrease in Nd. Unfortunately, the diagnostics for changes in secondary aerosols and their precursor gases are quite limited and incomplete in the CMIP6 experiments. Therefore, we consider addressing these changes, as suggested by the reviewer, to be beyond the scope of this paper. However, we agree that adding such diagnostics would be very useful in future experiments. Given that only two models provided CCN diagnostics, we can offer little more than our hypothesis regarding the situation. We have revised the text to clarify that this is our hypothesis and that further information on CCN changes is needed to draw definitive conclusions. Karydis et al. (2017) did several modelling experiments investigating the impact of dust on Nd and found dust to decrease Nd in polluted regions, while increasing Nd over the deserts, overall they found dust to decrease Nd by 11% globally .*

Comment: 3. In section 3.1, the authors explain the variations in total DuERF mainly through dust properties related to direct forcing. How would indirect forcing and cloud properties impact DuERF? Also, in these models, DuERF is dominated by direct forcing, and the direct forcing is more uncertain than indirect forcing. This is different from the estimate by IPCC AR6, which shows that aerosol indirect forcing is larger and more uncertain. Please note this and add some discussion about this issue in the manuscript.

Reply: *We appreciate the reviewer's question and reply for the different questions separately:*

Q1: How would indirect forcing and cloud properties impact DuERF?

Reply: *The most recent Kok et al. (2023) assessment found that dust indirect effects would most likely have a slight positive effect. Therefore, if the ESMs made improvements on their representation of dust-cloud interactions we would expect the DuERF to be less negative, although uncertainties remain large.*

Q2: Also, in these models, DuERF is dominated by direct forcing, and the direct forcing is more uncertain than indirect forcing.

Reply: *The model spread is larger in terms of the direct DuERF than the cloud DuERF, however, this only tells us that the ESMs currently have larger diversity in how they represent dust direct radiative effects compared to indirect* *radiative effects. Furthermore the majority of the ESMs do not represent many of the important processes that are necessary to represent the full range of dust indirect effects, e.g. missing aerosol-aware INP representation, thus interpreting the inter-model spread in cloud DuERF as the uncertainty in it would be inaccurate.*

Q3: This is different from the estimate by IPCC AR6, which shows that aerosol indirect forcing is larger and more uncertain.

Reply: *The indirect effect related to soluble aerosols (not primarily dust) is represented within the microphysics in more or less all climate models; therefore intermodel spread for total aerosol effects gives a more accurate depiction of the uncertainty, as stated in AR6. As we show in Figure 3a, individual LW and SW flux changes due to dust can be larger in magnitude than the dust direct radiative effects. This is unlike the indirect forcing by anthropogenic aerosols, that is mainly connected to changes in liquid clouds and droplet number concentration, which is well known to mainly have a SW cooling effect. In contrast, dust influences both liquid and ice clouds and the balance between SW and LW radiative effects can pull in either direction, making the overall dust cloud radiative effect weaker than that of anthropogenic aerosols. We acknowledge that this is still very uncertain.*

We have added the following discussion to the manuscript see ***line 570-580****:* Contrasting direct DuERF (Figure 2a) and cloud DuERF (Figure 3 a), we see that the inter-model spread and magnitude of DuERF are dominated by direct DuERF. However, the larger spread in direct DuERF should not be interpreted as the cloud DuERF being less uncertain compared to direct DuERF, as current ESMs cannot be trusted to accurately depict the uncertainty in dust-cloud interactions. This only shows that the ESMs currently have larger diversity in how they represent direct radiative effects of dust compared to indirect radiative effects. Given that most ESMs lack crucial processes for depicting dust-cloud radiative effects, e.g. aerosol-aware INP representation, the apparent model consistency is due to a lack of representation and not lack of uncertainty. The DuERF is also different from the anthropogenic aerosol ERF (e.g., IPCC AR6 Forster et al., 2021), which shows that aerosol indirect forcing is the largest and most uncertain aspect of aerosol radiative forcing. However, the dust radiative effect is in several aspects different from the indirect effect of soluble aerosols; for example, dust influences both liquid and ice clouds, and the SW and LW radiative effects can pull in opposite directions (McGraw et al., 2020), making the overall dust cloud radiative effect appear weaker than that of anthropogenic aerosols.

Comment: 4. If possible, the authors may consider add the surface albedo forcing (Ghan, 2013) for completeness of the decomposition.

Reply: *We thank the reviewer for the suggestion and we have added maps of the surface albedo forcing to the supplement, together with maps of the Cloud DuERF, and Direct DuERF. See Figures S10 to S12. We also added some brief discussion around the Albedo DuERF,* ***See Lines 581-586:***

The Ghan (2013) decomposition includes a `residual' term that is attributed to changes in albedo (Figure S12). With respect to the global mean value, the albedo DuERF ranges from −0.01 to 0.14 Wm^{-2}, and except for NorESM2-LM and CNRM-ESM2-1, it is small below 0.05 Wm^{-2}. The spatial distribution of the albedo forcing is also not consistent between the ESMs. Consequently, we provide the albedo term for completeness of the decomposition in the supplement (Figure S12), but refrain from any further analysis of the albedo DuERF due to uncertainty related to distinguishing the signal from the noise. Maps of the forcing for each of the terms of the Ghan (2013) decomposition are provided in the supplement Figures S10 - S12.

Comment: 5. Some statements in the manuscript do not have proper citations. Please see my specific comments for details.
Reply: *We thank the reviewer for identifying these issues and we have addressed the specific comments below.*

Specific comments

Comment: L88-89: “Dust readily ... at warmer temperatures”. Please give references about that dust can work as INPs and K-feldspar is more efficient.
Reply: *In the revised introduction this sentence was removed to focus the introduction more towards how ESMs represent dust radiative effects.*

Comment: *L90-93: Please give references for the impact of dust on cirrus clouds.*
Reply: *Added references to the impact of cirrus clouds.*

Froyd, K. D., Yu, P., Schill, G. P., Brock, C. A., Kupc, A., Williamson, C. J., et al. (2022). Dominant role of mineral dust in cirrus cloud formation revealed by global-scale measurements. Nature Geoscience, 15(3), 177–183. https://doi.org/10.1038/s41561-022-00901-w

Comment: L108 and the following lines: it is not clear to me how DuERF is determined when first reading this paragraph. It is better to clarify here that piClim-2xdust is compared to piClim-control. Also, it may be necessary to briefly mention that dust is the only perturbing factor, and all the others were kept the same as piClim-control.

Reply: *We thank the reviewer for pointing this out and we have updated the text to make this more clear.*
Line 171-171. We define DuERF as the difference in the TOA imbalance between *piClim-2xdust* and *piClim-control*, with the dust emission perturbation being the only factor that separates the two simulations.

Comment: L115 Do Thornhill et al. (2021) only examine total DuERF and not separate direct and cloud DuERF? If so, please specify.

Reply: *We thank the reviewer for pointing this out, indeed Thornhill et al. (2021) do only examine the net DuERF; we have updated the text accordingly.*
Line 183-184: This article expands on the ~~outcome~~results of Thornhill et al. (2021), by quantifying the direct and cloud DuERF in the models, which was not shown in the Thornhill paper.

Comment: L170-174: It is not clear to me whether dust impact heterogeneous ice nucleation in mixed-phase clouds in NorESM2. Does the bug results in no heterogeneous ice nucleation in mixed-phase clouds?
Reply: *Yes this bug negates the ability of the Hoose scheme to modify the cloud ice numbers. See our reply to comment 1 for details.*

Comment: L207-208: Do you mean once other aerosols/precursor gases condense on dust, the hygroscopicity of dust does not change and no secondary aerosols are formed on dust (so dust is a pure sink for, e.g., sulfate and nitrate)? Please give references for this treatment.

Reply: *Dust aerosols are represented as internally mixed in the OMA scheme, however, dust is not included in the hygroscopic mass fraction of aerosols that can participate in the cloud nucleation processes (Schmidt et al., 2014). We have added a reference and rephrased the sentence.*

Line 287-289: Dust aerosols do not directly impact cloud droplet concentration ~~; however, their ability to be coated by other aerosolsallows~~because dust is not included in the hygroscopic mass fraction of aerosols that can participate in the cloud nucleation processes (Schmidt et al. 2014).

Comment: Eq (5)-(7): I think the original equations in Ghan (2013) (see their Section 3) is clearer and more widely used.

Reply: *We thank the reviewer for this comment. The original equation might indeed be better known. We chose to display the equations as written in the manuscript to make it clear, which CMIP6 CMOR variables were used. However, to accommodate readers that are familiar with the Ghan (2013) equation, we now show the original equation next to the equation containing the CMOR variable name.*

Comment: L279-281: please add the estimate from Thornhill et al. (2021) here for comparison.

Reply: *Thanks for pointing out this issue, we have added the range from Thornhill et al.*

Line 382-383:~~ ranging~~ from 0.09Wm^{-2} to −0.41Wm^{-2} compared to 0.09Wm^{-2} to −0.18Wm^{-2} reported in Thornhill et al. (2021).

Comment: L400-401: This seems to be different from what being said in Section 2.2. Does the bug fully deactivate heterogeneous ice nucleation in the model?

Reply: *We thank the reviewer for identifying this issue and we have revised the text accordingly.* ***See line 529- 530***

Comment: L457-458: how did you make the assessment? Also, please give the exact number for BC inhabitation.

Reply: *We appreciate the reviewer's question. We made this assessment by considering that the two mechanisms are related in that both BC and dust lead to a warming of the atmosphere. The assessment obviously compares the dust (anth. BC) impact on precipitation to a reference case without dust (anth. BC) (clarified in the text). We have added the exact number for the BC precipitation from Samset (2022) in the revised manuscript.* ***See Line 621-622***

Samset, B. H. (2022). Aerosol absorption has an underappreciated role in historical precipitation change. Communications Earth & Environment, 3(1), 1–8. https://doi.org/10.1038/s43247-022-00576-6

Comment: I would say the models do not agree on the change in precipitation over North Africa, because four of them show decrease. Also, please show spatial distribution of precipitation change to support that ITCZ has shifted.

Reply: *We thank the reviewer for this comment. We agree that the original statement is somewhat inaccurate, and we have now revised the text to indicate that the majority of the models exhibiting the largest increase in Dust Absorption Optical Depth (DAOD) over North Africa also show an increase in precipitation across the region. We have for completeness added a figure showing the spatial distribution of the precipitation change (see Figure S13).* ***See line 689-692*** *for text changes.*

Comment: L478-480 This is not consistent with previous statement. It was claimed that surface albedo is relatively consistent among models in L269-271.

Reply: *We thank the reviewer for pointing out this issue. The way the statement was written was indeed ambiguous. What we were trying to say is that the models are not internally consistent on how they represent the albedo of the surface versus the albedo from airborne dust. Thus the models have a too large contrast between the surface and the dust above, a scene that in reality should not be very contrasting. We have rephrased the sentence in the revised manuscript.* ***See Line 645-649***

Comment: Figure 4: please check the unit for ARC. Also, does c) show dust AAOD in absolute value or its change?

Reply: *We thank the reviewer for noticing this issue. The ARC is converted to equivalent precipitation units to make it easier to relate ARC back to precipitation change. This conversion is now explained in the supplement (see Section S1.1). The DAOD shown is the change between piClim-2xdust and piClim-control. We updated the figure caption accordingly.*

Comment: Table 1: (1) Consider adding a column showing if dust can be CCN or not. (2) Column MB95: does X mean N (no)? (3) Column Size char.: what are the numbers in parenthesis? (4) Please give reference for Ghan. (5) Please verify if GFDL-ESM4 has Ghan method or not.

Reply: *We appreciate the reviewers suggestions and have made the following changes to Table 1. We removed the MB95 column. Instead we added one column showing which ESMs include LW scattering and one column showing which models include dust as CCN. The GFDL-ESM4 model does have the Ghan diagnostic and we have updated Table 1 accordingly. We thank the reviewer for noticing this error. For the updated table please see the tracked changes version of the manuscript.*