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.

General comments:

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.

2. About N_d decrease in EC-Earth-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 N_d 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).

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.

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

5. Some statements in the manuscript do not have proper citations. Please see my specific comments for details.

Specific comments:

L82-83: Please give references (i.e., which models do not include dust as CCN). Also please note that many ESMs actually do include dust as CCN, e.g., CESM and E3SM.

L88-89: "Dust readily … at warmer temperatures". Please give references about that dust can work as INPs and K-feldspar is more efficient.

L90-93: Please give references for the impact of dust on cirrus clouds.

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.

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

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?

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.

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

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

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?

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

L462-463: 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.

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

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

Table 1: (1) Consider add 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.