Review on the “Modeling impacts of dust mineralogy on Earth’s Radiation and Climate” by Song et al.

Recent studies suggest growing importance of mineral types in estimating the role of dust to the Earth’s climate. However, the impact of soil and dust mineral types is still highly uncertain, while most of global models assume a homogeneous mineral mixture. The present study reports a modeling study of the impact of the mineralogy map to climate system using the GFDL AM4 model. This work starts with the how mineralogy map by Claquin et al. (1999) is implemented to GFDL AM4, and how model experiments and optical properties are setup. The model simulations are evaluated with the observations by di Biagio et al. (2019) and AEROENT data. The present study further examines the impact of dust mineralogy to climate system mainly over North Africa with various parameters such as, radiative fluxes, precipitation, surface temperature, temperature profile, etc.

The method used in the present work is scientifically sounding and it presents several interesting results that might be a beneficial for the potential readers. The paper is well organized and reasonably well written. I would recommend the paper to “minor revision”, however I also have suggestions in many places which I would like to see in the revision.

Major comments:

One of the major questions I have is what is the reason for HD2.7% is used as a standard GFDL AM4 models if the hematite fraction is too high? The present study suggests that HD0.9% would be more realistic than HD2.7%. However, the reference study by Balkanski et al. (2007) suggested HD1.5% as the best hematite fraction in the mixture. I would like to see the result of HD1.5% even with a brief result only. Also, it would be good to provide more discussion on this. I would say the differences from 0.9 to 1.5 and to 2.7 % are not negligeable.

The present work needs more background study for the existing research. At least I am aware of a similar recent work by Balkanski et al. (2021, https://doi.org/10.5194/acp-21-11423-2021). How they are compared each other?

Analysis of dust and mineral mass distribution such as horizontal and vertical distribution, and size dependence of distribution is not covered at all in the manuscript, although these are an important step before analyzing climate impact.

The title includes "Earth", however the present paper is limited in North Africa only. There is a brief result in Supplementary Material. I would suggest to expand the work or change the title.

Method section needs more work to make it more clear and specific. I listed a few.

Other comments:

L24-26: It is unclear the reduction of what and is reduced from what?

L101: "Section 2 provides ...." Split the paragraph to a different on.

L110-111: Please be specific if the present work is participating to the AMIP projects.

L137, 138: Change soil map to soil mineralogy map.

L137: Change "to resolve dust (" to "to resolve dust mineralogy (".

L139-141: Please discuss more detail how BFT is implemented to GFDL AM4.0.

L139: Change "disperse" to "fully disperse".

L146-147: The sentence "Moreover, ..." needs to be further elaborated. Please include the difference of goethite and absorption in outside of visible spectrum.

L150-156: This section of internally and externally mixed hematite is confusing and this section needs more improvement in the revision. First, what is the base of partitioning hematite to internal and external mixture in clay? How much of hematite is in internal mixture and external mixture? Second, what is the base of 5% mass fraction threshold for internal and external mixture of the hematite in accretions? Please be specific how Goncalves et al. (2023) used that assumptions. Thirdly, it needs more clear description on how hematite from soil mineral map is used. If the method follows Goncalves et al. (2023), the assumption of 5% is not necessary. Also I wonder how the internal and external mixture assumption is used in other mineral type?

L162: Please specify the meaning of "clay433" here.

L171-174: Optical tables are important for the experiment, and it needs more description here. Or add a sentence that more details will be discussed in Section 4.

L183: SSA and CRI of AERONET is Inversion product. Please specify in the text.

Table 2 BM-RT 2) and 3): Please specify how much of hemitite and gypsum are removed. I am also curious why only LC, RH, and RG are considered among 9 mineral classes?

Figure 1: CRI in longwave length is indistinguishable. I would suggest it to separate to shortwave and longwave.

L269-270: "weighted by solar spectrum"? Please be more specific the meaning.

L280-282: Please discuss why the result in the present paper is different from Balkanski et al. (2007), which concludes that 1.5 % by volume of Hematite using MG mixing gives the best results.

L295-296: Please add discussion for longwave radiation.

L329-330: The sentence is misleading since SSA is affected by both dust size and refractive index (k).

L334-336: Figure S3 and text need more improvement. Please specifically explain why particle size distribution (or SSA) in HD27 is more sensitive than HD09.

L337-338: The sentence needs to be improved in revision too. The previous paragraph and Figure S3 needs to be more clearly described.

L348-351: The present paper concludes that HD09 shows the best agreement with AERONET SSA. The result is not consistent with Balkanski et al. (2007) who found that 1.5 % by volume best matches with AERONET.

L360-361: Spatial variation of iron oxides content would be one reason for the low spatial variation in modeled SSA. How about model emission, deposition and horizontal- and vertical-transport?

L379: Linear averaging is not a correct way. Please specify if SSA interpolated to 550nm with Angstrom Exponent.

L380-381: "Figure 2 shows..." is not necessary. Delete the sentence.

L381-382: The sentence is unclear. Why MB is not representative if MB and BM are similar each other?

L397-398: The sentence about EMIT would belong to discussion.

There are five figures (Figures 4-8) in Section 5.1. However there is no table. I would suggest to provide a table that summarizing DRF.

L422-423: North Africa is hotspot. However global mean estimation is also important to examine the impact of dust mineralogy.

L451-453: Please provide actual number for 25% and 10% reductions. Also it is unclear the change is same for three mineralogy resolved tests or they are given in range. The same applied to other figures 4, 5, 7.

L455: Global estimate is an important result of the present study even if the magnitude is not as large as North Africa. I would suggest including a table for global radiative forcing. How about other major dust sources such as Asia and Australia?

L470-472: The sentence about EMIT is not result of this work, it belongs to discussion.

L473-482: The study argues that HD09 is better than HD27 in the comparison with CERES radiation flux observations. However it is unclear if the better agreement in HD09 is the correction of the model or something else. (1) The CERES observation includes many factors including many aerosol types such as sea-salt, biomass, burning, anthropogenic, and volcano. (2) Many factors are involved in dust radiation flux calculation, e.g., dust amount, optics table, size distribution, etc. I think one can make better agreement with CERES by modify one these fields.

L481-482: Please be specific about "better".

L515: From Figure 9, Maybe it is worthy to point out that the difference between HD27 and HD09 is about +1 deg K.

L555-560: Please make the two sentence more clear.

L607-610: I wonder if the result is based on observation or speculation. If authors have seen during analysis it can be written more firmly.

Section 6: The authors argue that reducing mineral types do not have significant impact on radiative forcing. However it should be pointed the mineralogy also have importance on geochemical cycles and cloud nucleation.

L665: I still do not understanding what actually means 'clay433'. Could it be more human understandable?

L718-720: The finding that the dust in the standard AM4.0 (HD27) is too absorbing is quite surprising. I would like to see more this finding. What is the background of choosing HD27 other than lower iron-oxides. Is it based on studies or inherited from OPAC type of table?

The result is also soil mineral map dependent. The iron oxides content would be different from Claquine et al. to EMIT. This needs to be included.

L471-473: Again, I wonder if this recomendation is also consistent with Balkanski et al. (2007 and 2021).

L760-770: The present study needs to further esmitate or discuss the uncertainty by missing Goethite in ironoxides.