Response for the Anonymous Referee #2 on "The effect of high dust amount on the surface temperature during the Last Glacial Maximum: A modelling study using MIROC-ESM" by Rumi Ohgaito et al.

The manuscript by Ohgaito and colleagues presents results of a study on the impacts of dust on the Last Glacial Maximum climate, conducted with different configurations of the MIROC-ESM global model. Attention is given to the role of glaciogenic source of dust. Dust feedbacks on climate include direct effects, cloud effects, and snow darkening. Dust effects are discussed, in terms of perturbation to the atmospheric radiation budgets and surface temperatures. The study is an interesting contribution to both the dust community and the paleoclimate community. In my opinion the manuscript still need some improvement before publication.

We wish to express our appreciation to the referee for the positive and constructive comments and suggestions, which have helped us improve our manuscript considerably. In the following, the referee's comments are written in black and our replies are written in blue.

General comments

The abstract seems all focused on glaciogenic dust, whereas the title and the manuscript deal with both glaciogenic and non-glaciogenic dust. I suggest to make more clear in the abstract that both aspects are analyzed, and what are the relative contributions to the net dust effects.

The abstract has been modified to include mention of both non-glaciogenic and glaciogenic dust.

A more detailed description of what glaciogenic sources represent, and how glaciogenic sources are embedded in the model setup are strongly encouraged, given the relevant role they play in this manuscript.

An appropriate description has been added in Sect. 2.2 and the source areas of glaciogenic dust are shown in Supplementary Fig. A.

The discussion should be improved by comparing more extensively with existing results from the literature, and by enhancing the last section which is an original contribution.

We have improved the discussion section following your suggestion.
The final section of the manuscript is not simply an analysis of the original contribution but it also provides an evaluation of the effect of glaciogenic dust on surface temperature. We intended to leave detailed analysis of the oceanic response for subsequent study using ongoing PMIP4 model experiments. However, your suggestion made us realize the interest concerning the oceanic element. Therefore, the oceanic response to different dust fluxes under the conditions of the LGM is more discussed in Sect 3.4.

Specific comments

1/14: "the impact of glaciogenic dust". Do you mean "glacial climate dust"? In fact your study explores the effect of both glaciogenic and non-glaciogenic dust.

Our focus was on glaciogenic dust. Thus, the differences between scenarios with and without glaciogenic dust were analysed as a priority. However, analyses were also performed regarding scenarios with glaciogenic dust and non-glaciogenic dust. The sentence has been modified accordingly in the revised text.

1/18: "sources" rather than "provenances"

This has been changed as suggested.

1/21: one gets curious here: is the enhanced cloud cover caused by semi-direct or indirect effects?

According to the definition of the IPCC AR5 Chapter 7, the aerosol–cloud interaction does not include semi-direct effects. If semi-direct effects dominate, enhancement of cloud prevents shortwave radiation reaching the earth's surface, whereas the change in longwave radiation causes surface warming in this case.

1/22-23: It's not clear what you mean by "a first trial of glacial dust modelling" in the specific context of fully-coupled simulations, rather than the atmosphere-only ones.

This has been changed to "an initial examination of the effect of glaciogenic dust on an oceanic general circulation model"

2/7-8: Rather than “capturing past climate sensitivity”, I would say “estimating climate sensitivity by looking at past climates”, or perhaps more appropriate for the scope of this manuscript, “capture past climate conditions”.

This has been changed appropriately.

2/21-3/2: Repetition that higher dust fluxes are more pronounced at higher latitudes

Thank you. The repetition has been avoided in the revised text.

Pages 3-4: In this historical review section some recent, relevant papers are not cited, e.g. Albani et al. (2014), Sagoo and Storevlmo (2017). I would recommend to consider them along with other also studies in the discussion section, in terms of global dust budget and impacts.

Both in the historical review and the discussion sections, the works by Albani et al. (2014) and Sagoo and Storevlmo (2017) are now included.
The global dust budget of previous studies is summarized in Table 1 of Hopcroft et al. (2015). They highlighted that the dust amount is highly dependent on the model, not only for LGM experiments but also for PI experiments. Our emissions and loadings are listed in Table 3. Our values fall in the middle of the ranges determined by previous studies. However, they are close to those of Takemura et al. (2009) for PI and LGM, probably because the models adopted are from the same model family and use the same aerosol module. The emission of LGMglac is close to that of Mahowald et al. (2006a), most likely because we adopted their glaciogenic dust.

3/11-13: This sentence is not grammatically correct, please rephrase. Also, moraine debris does not appear to be itself a potential dust source type, but rather fine grained material would be. Please try to be more specific in your definition of glaciogenic sources (e.g. see Bullard et al. 2016).

The sentence has been rewritten and the term “moraine debris” has been changed to “glacial flour” (Bullard et al. 2016).

4/9-10: it’s not very clear what is the difference between Sections 3.2. and 3.3 in this brief

description.

Section 3.2 describes the effect of glaciogenic dust on surface temperature. The question of how glaciogenic dust might modulate the surface temperature, especially surrounding Antarctica, is discussed in Sect. 3.3. The text has been rewritten accordingly in the revised manuscript.

5/11-13: I do not understand this sentence, i.e. how this weighting occurs

The ageing of snow is implemented following Yang et al. (1997) and tuned to fit the observations by Aoki et al. (2003, 2006). The weighting parameters are defined according to the absorbing property of the material. However, this part has now been removed because soot is no longer discussed in this paper.

5/14-15: Are you using this kind of off-line model in this study? If not, it seems irrelevant yo mention this fact here.

Because we discuss this in Sect. 4, the sentence you have identified has now been deleted.

5/20: Maybe "specific" rather than "particular" would be more appropriate here?

This has been changed as suggested.

6/9-10: How is this implemented in the model? At the level of grid cells (do you have the same horizontal grid?)? Or rather you are redistributing total emissions on your own grid cells matching the spatial coverage of the same geographical area? Are the emission fluxes prescribed as a repeated monthly varying quantity, or some other way? Please provide more details on this central part of your methodology, and list the geographical location of these glaciogenic sources.

The glaciogenic source areas are defined by following Mahowald et al. (2006). Supplementary Fig. A has been added to clarify the source areas of glaciogenic dust. For each source area, we set a constant dust emission to match the flux in Mahowald et al. (2006). As a first trial, glaciogenic dust is emitted constantly. Once it emitted, the treatment of the dust is the same as any other dust, i.e., its transportation, advection,

and deposition processes. Although constant emission cannot happen in nature, this attempt was simply intended to emit the identical flux as in Mahowald et al. (2006) as a first step. Introducing temporal variation in emission and obtaining original glaciogenic dust flux that fits the updated proxy archive is the next research ambition. This has been outlined in Sect. 4.

Table 1: Does the integration length refer to the length of your simulations only, or does it also correspond to the period averaged to derive the diagnostic quantities discussed and compared in the manuscript? Please specify how long was our spin-up and how many years you averaged for analysis.

The listed integration lengths include the analyses periods. Now the ranges of the analyses are shown in Fig. 1.

6/18-19: Indeed Australia is the major missing dust source, but also South Africa and the SW North America would fall into this category. Can you comment on how the present day simulations with the same model perform in this respect?

Our PI.a (PI.e) has wet bias and relevant high LAI over South Africa and SW North America. The manuscript has been rewritten to include mention of these areas.

7/3-4: "enhancement" is repeated twice

Thank you for noticing this error; it has been corrected.

7/5-6: expressing these quantities in Tg/year would help the reader relating to the existing literature. Actually it would be very useful to report global budgets of dust emissions, load, and deposition in a table.

The unit has been changed and the additional information requested is now presented in Table 3.

7/11: What do you mean exactly by "higher uplift"? Transport to higher levels in the troposphere? Please clarify this aspect, as it may be confused with larger emissions (which should not be case, since glaciogenic sources appear to be prescribed to a fix emissions flux).

We apologize for the confusion. We meant to indicate greater dust concentration at higher levels of the troposphere. The wording has been changed appropriately in the revised manuscript.

Figure 4: please specify if the data reported from Albani et al. (2014) refer to the bulk or to the fine fraction in terms of particle size range. In the caption, please change to "Crosses represent terrestrial sediments, circles marine sediment cores, and diamonds ice core data" - terrestrial sediments are typically loess sections.

We used the bulk values of Albani et al. (2014); the caption has been changed accordingly.

7/21: Do you mean "the main source of dust deposited in this region"?

Yes, you are correct. We have clarified this in the revised text.

8/6: What do you mean by "glacial dust"? Glaciogenic dust or glacial climate dust? IF you mean the second one, it would be useful to explicitly clarify the distinction, better in earlier sections of the manuscript. If not, you should consistently use "glaciogenic" rather than "glacial" to avoid confusion, I think.

This was an error. The word has now been changed to "glaciogenic" and the entire manuscript has been checked to avoid other such occurrences.

8/11: Do you mean "Figure 7 shows a reduction in the shortwave radiation anomaly . . ."? Similarly, in the following lines, I would suggest referring to "-wave radiation anomaly".

This has been changed appropriately.

8/15-16: What do you mean by "radiative perturbation by the dust"? And how is that different from the analysis just carried out in the previous lines?

In this section, we discuss the surface radiation anomaly. In the following section, we discuss the causes of this anomaly. It is clear that the anomaly is caused by glaciogenic dust based on the experimental setting; however, we have separated the

effects of aerosol–radiation, aerosol–cloud interactions. A suitable explanation has been added in the text.

8/6-16: A comparison with Mahowald et al. (2006) seems in order here, being the only other study discussing directly the impacts of glaciogenic sources.

Comparison with Mahowald et al. (2006b) and further discussions have been added.

8/17-19: This paragraph is repeated twice.

Thank you for identifying this error. The duplicate text has been deleted.

9/6: Please indicate where we can see this effect, i.e. "the cooling effect of the dust loading in the atmosphere" - it is not self-evident.

The likely cooling effect of dust on the earth's surface is suggested in the IPCC AR5 Sec. 7 and references therein. However, the uncertainty ranges from negative to positive. Each of our experiments also resulted in a cooling effect of dust in the global mean (PI.a: −0.99 W m^{-2}, LGM.a: −1.50 W m^{-2}, and LGMglac.a −1.71 W m^{-2}) at the surface.

9/6-9: Again, where can the reader see these features?

Supplementary Fig. B has been added to show the albedo difference between LGMglac.a and LGM.a. A description of Supplementary Fig. B has also been added in the revised text.

9/6-16: It would seem appropriate to compare you results for this process at least with the study by Krinner et al. (2006).

Thank you for your suggestion. The result of Krinner et al. (2006) is consistent with ours and a sentence explaining this has been added in the manuscript.

9/20: A net cooling of . . . how much?

Quantification of the cooling has been added in the revised text, i.e., PI.a: −0.99 W m^{-2},

LGM.a: −1.50 W m^{-2}, and LGMglac.a: −1.71 W m^{-2}.

Table 2: Could you further split aerosol-radiation interactions between snow darkening and atmospheric impacts? Also, can you indicate the total dust radiative perturbation (from all types of feedback)?

We have created Supplementary Fig. C. It shows the LGMglac.a–LGM.a anomaly of aerosol–radiation and aerosol–cloud interactions for the TOA and the surface. Furthermore, it also shows the same format without the snow ageing effect. The panels clarify that the snow ageing effect on the radiative perturbation is minor. The figure also clarifies that the anomaly of aerosol–radiation interaction tends to be significant at the level of 0.1 W m^{-2}, whereas the significance of the aerosol–cloud interaction is difficult to determine. Nevertheless, the positive anomaly around Antarctica at the surface is significant.

10/1-18: There is ample space here to compare the results in terms of aerosol-radiation interactions with additional existing work, e.g. see Albani et al. (2014) and Hopcroft et al. (2015).

Comparison with the works of Albani and Hopcroft has now been included and appropriate discussion has been added.

10/16-18: Please rephrase, this sentence is not very clear to me.

The sentence has been rephrased appropriately.

10/19-20: The link between this statement an Figure 10 is not clear to me. Please review this passage.

The sentence explains the content of Fig. 10. It has been rephrased accordingly.

11/2-6: A comparison with Sagoo and Storelvmo (2017) would be appropriate here.

Comparison with Sagoo and Salmiento (2017) and appropriate discussions have been added at the end of paragraph.

11/7: A more precise title for this section could be “Influence of glaciogenic sources on the ocean SST”?

Because consideration of the effect of dust on oceans has been added, the section title has been left unchanged.

11/7-21: This section is potentially very interesting. Unfortunately in its present form the discussion is quite superficial in my opinion. I would recommend to expand the section and perhaps enhance

We intended to elucidate the oceanic response in our next study using LGM experiments for PMIP4. However, additional analyses have been performed and the findings are explained in the revised text.

Figure 12 with a scatterplot or some other representation that would allow the readers to appreciate the effects on SST and land temperature anomalies.

The temperature anomaly of the zonal mean over land and scatter plots of the anomaly of the proxy data and of the anomaly of the corresponding model grids are shown in Supplementary Fig. E. It illustrates the level of agreement between the model and the proxy archives. Pronounced discrepancy is evident in the northern high latitudes with some proxy data suggesting warmer temperatures than PI, whereas the model shows a negative anomaly. Compared with LGM.e, LGMglac.e generally exhibits slightly closer agreement with the proxy data.

13/1-3: As discussed in the manuscript, the mismatch is to be attributed to the lack of dust emissions in regions such as Australia in the model used for this study. I fail to see what’s the link with the prescribed glaciogenic sources.

We meant that Mahowald et al. (2006a) used the DIRTMAP dust deposition archive (Kohfeld and Harrison 2001) to fit the model deposition flux, which had no proxy points over the southern Pacific Ocean. This could also be one of the reasons for the underestimation. The manuscript has been rewritten to clarify this point.

14/6: see also Mahowald et al. (2014) or Albani et al. (2014)

Thank you for your suggestion. These studies have now been cited because discussion of their findings is appropriate in this section of our manuscript.