Responses to Anonymous Referee #1 on "The effect of high dust amount on the surface temperature during the Last Glacial Maximum: A modelling study using MIROC-ESM" by Cp-2018-2 Ohgaito et al.

We wish to express our appreciation to the referee for the constructive and insightful 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

Overall, I didn't find this work is placed very well in the context of past studies. How does the dust-cloud scheme used differ from Takemura et al 2009, and Sagoo et al 2017? How comparable is the snow-ageing scheme to Krinner et al 2006, or Ganopolski et al, 2010? Please re-write the introduction to better place the current work in the context of past studies. What is different (or the same) as past work? What do you hope to find? What are main uncertainties etc?

Our aerosol scheme is identical to that of Takemura et al. (2009). Both Takemura et al. (2009) and Sagoo and Strelvmo (2017) implemented parameterizations of interaction between aerosols and ice crystals based on empirically derived formulations following laboratory experiments and observations (i.e., Lohman and Diehl (2006) and DeMott et al. (2015), respectively). The formulations are different but the schemes of Takemura and Sagoo do similar things; both formulate ice nucleation dependent on temperature and aerosol concentration. It should also be noted that the representations of the cloud water phase of climate models are uncertain and all failed to reproduce the amount and distribution of global observations (Komurcu et al. 2014).

Concerning the ageing scheme, Krinner et al. (2006) used an ageing scheme based on Warren and Wiscombe (1980) and Wiscombe and Warren (1980) and the MIROC-ESM used that of Yang et al. (1997) based on Warren and Wiscombe (1982). Ganopolski et al. (2010) used simple scaling of albedo reduction with dust flux relationship. This information has been added in the introduction and model description sections.

Our main research objective was to elucidate how glaciogenic dust might influence the global climate, especially surface temperature. This has been added in the introduction.

The manuscript has insufficient detail on the methods used, especially on how glaciogenic dust was included. Did you tune the fluxes to the LGM dust observations somehow?

In this work, as a first step, we forced additional dust emission constantly following the estimate of Mahowald et al. (2006). The source areas of glaciogenic dust in the MIROC-ESM are shown in Supplementary Fig. A. The source strengths for these areas are shown in Table 3 for the non-glaciogenic dust (LGM.a) and the non-glaciogenic and glaciogenic dust (LGMglac.a), following Mahowald et al. (2006a).

How well does your snow ageing model agree with other schemes (e.g Warren Wiscombe, 1980).

The snow ageing scheme of the MIROC-ESM is that of Warren and Wiscombe (1982). A suitable description has been added in the revised manuscript.

Are your LGM results comparable with e.g. Krinner et al 2006?

Krinner et al. (2006) suggest that the ageing effect of snow prevents formation of permanent snow over eastern Siberia, consistent with our results. An appropriate statement has been added in the revised text.

You do not include any discussion of potential uncertainties, which would seem to be quite large, especially for dust-cloud interactions. Perhaps summarise the approach in SPRINTARS compared to other models (e.g. Komurcu et al., 2014).

Yes, we agree the uncertainty of the aerosol–cloud interaction cannot be overlooked. Komurcu et al. (2014) provided an overview of the uncertainty among the major models and they reported wide ranges of uncertainty in both magnitude and spatial distribution; therefore, our results might differ from other schemes. Acknowledgement of this possibility has been added in the discussion section.

Are your dust cloud effects in agreement with those presented for e.g. 'high dust' by Sagoo et al 2017? If not, could you speculate as to why.

In terms of the global mean, the negative radiative effect of dust is consistent with Sagoo and Strelvmo (2017) and other studies. In the mid- to low latitudes, our results are also consistent with those previous works with regard to cooling. However, in the high latitudes, our results of warming via high dust deposition contrasted with their findings. Because Sagoo and Strelmvo (2017) did not conduct a standard LGM experiment (they changed only CO2 and dust from their control experiment), it is not possible to specify a reason for this. However, their "idealized high dust" means that their emission factor is about 3.4 times that of the control experiment, globally, whereas our glaciogenic dust sources are located in the high latitudes. Therefore, it is likely that the influence of regions of glaciogenic dust emission such as the Pampas of South America on surface temperature around Antarctica is more pronounced in our simulation results. This analysis has been added in the discussion section.

Please also could you explain why the dust-cloud effects are so important in the southern hemisphere, but not in the northern hemisphere, and also why the reverse is true for the snow-ageing. Could you expand figure 9 to compare the radiative perturbations from the 3 separate effects of dust that you have studied. Hence, I would recommend major revisions to the text before publication.

Snow ageing in the MIROC-ESM is tuned to fit the observations in Aoki et al. (2006). According to Aoki et al. (2006), it can be considered (approximately) that albedo starts to reduce with snow impurity of ≥10 ppmw. Dust deposition over the northern high latitudes is of the order of 100 g m^{-2} y^{-1}, which corresponds to the order of 1000 ppmw. Conversely, dust deposition near Antarctica is about 0.01 g m^{-2} y^{-1}, which corresponds to the order of 0.1 ppmw

Glaciogenic dust travels higher into the troposphere in the Southern Hemisphere and it promotes ice nucleation. Additionally, the dust deposition flux of the standard LGM.a is higher than PI.a in the Northern Hemisphere but lower in the Southern Hemisphere. Therefore, the impact of glaciogenic dust might be more efficient in the Southern Hemisphere. This has been explained in Sect. 3.3.

Specific comments

Page 3, lines 3 to page 4 line 2. This whole section could be summarised more succinctly for the reader. What is the main message from all previous work? What were the main steps? I would say, most studies simulate a cooling effect, but it is variable and that the

introduction of (i) vegetation feedback (Mahowald et al 1999), and (ii) glacio genic sources (Mahowald et al 2006) and (iii) dust-cloud interactions (Takemura et al 2009, Sagoo et al 2017) are the main developments.

The introduction has been rewritten more succinctly following your suggestions.

Page 4: Lines 3-11. I find it incomplete here to only list the inclusion of the ocean. You should also mention the dust-cloud interactions and the dust on snow effects and the inclusion of glaciogenic sources in this study.

The sentence has been modified according to your suggestions.

Page 5: lines 3-4: Did you reduce the imaginary part of the dust refractive index as done by Takemura et al 2009 (their page 3063)?

Our aerosol module (SPRINTARS) is identical to that of Takemura et al. (2009). The refractive index of dust aerosols was taken from Deepak and Gerber (1983), but its imaginary part was reduced for consistency with recent measurements of weaker shortwave absorption.

Page 6: Lines 5-6. More detail of the glaciogenic model setup is required. Did you optimise the fluxes from the emissions using the ice-core data, or marine data or both? What simulations did you use to calculate this? Or did you simply scale emissions in these regions to match the emissions simulated by Mahowald et al 2006?

Our method is simple. As a first step, to develop a more sophisticated method for obtaining a best fit to the proxy data archive, we specified the area of glaciogenic dust emission (Supplementary Fig. A) and allowed the emission of a constant dust flux following the estimate of Mahowald et al. (2006). The next step will be to introduce a more realistic method for the emission of glaciogenic dust. We intend to investigate this in subsequent research using an updated version of the MIROC model, which is now under preparation for the submission of experiments to PMIP4. Here, we acknowledge that we adopted a simple method but it was shown successful in obtaining better dust deposition distribution in comparison with the proxy data. Improvement of the scheme is certainly required; however, we think even if a difference in amplitude is derived, the main conclusion will still hold.

Page 9: lines 13-16: Isn't it more likely that this small 1 degree shift, is showing that the effect is small over North America? Your argument seems to be that a much higher resolution model would be more sensitive, but I can't see why this should necessarily be the case? Perhaps I have misunderstood.

We agree that the sentences were confusing and we have rewritten them.

Figure 8: Can I suggest you separate this plot out into several panels for clarity?

For clarity, the shading has been changed to be semi-transparent.

Figure 9: It would be nice to compare the dust-radiation, dust-cloud and dustcryosphere effects somehow?

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.

Table 2: Takemura et al 2009 quote -0.9 Wm-2 for the net dust-cloud effect at the LGM relative to the PI, but your LGM.a -PI.a difference is only -0.36 Wm-2. Could you comment on the differences with that older study?

The model of Takemura et al. (2009) and ours both use the SPRINTARS aerosol module. However, there are differences between the experimental setups for PI and LGM experiments and the model version.
The difference of the global mean value is derived mainly from the different boundary conditions for PI. The SST used by Takemura et al. (2009) (Ohgaito et al. 2009; Fig. 1) over the warm pool is about 1° warmer than the SST used in this study (Sueyoshi et al. 2013; Fig. 4). It suggests different convective activity, resulting in different amounts

of cloud ice and cloud water. This tropical difference influences the global mean value, suggesting that the SST bias of the control experiment could affect both regional and global mean values. This discussion has been added in Sect. 4.

Technical comments

Abstract Line 23: "for a first trial": I think you are referring to coupling with the ocean? It might make more sense to say "for testing the dust feedbacks in a fully coupled GCM for the first time" or similar?

Thank you for this observation. It has been changed accordingly.

Abstract Line 25: Perhaps change "interaction" to "coupling"?

This has been changed as suggested.

Page 2 line 17: "Although mineral dust aerosol is not the most significant cause of warming, its effect is not negligible because it is the most abundant aerosol." This makes it sound like mineral dust might have contributed to recent warming. Suggest to rephrase as "Mineral dust is the most abundant natural aerosol today."

This has been changed.

Page 3 Line 13: "where supposed to generate substantial amount of moraine debris during glacial periods" Change "where" to "were". Perhaps include some of the primary references on this topic.

The sentence has been changed.

Page 4: Line 4: "The feedback of the aerosol to the ocean and sea ice and back to the atmosphere was not taken into account". Technically, in a slab ocean model the sea-ice can respond, only the oceanic circulation is fixed.

The sentence has been rewritten.

Page 4: Line 19: So the vegetation is not fully dynamic?

The dynamic vegetation module simulates global vegetation dynamics and terrestrial carbon cycling (Sato et al., 2007) using the output of the physical module, but it returns only the LAI and amount of carbon back to the land and atmosphere, respectively. Thus, the dynamic vegetation model is loosely coupled with the MIROC-ESM.

Page 5 Line 6: "that control" not "correlated to the" Also, do these variables also control the glaciogenic dust flux?

This has been changed and explanation added regarding glaciogenic dust.

Page 6 line 10: "The emission area is also consistent between the experiments, with little deviation following the land-sea mask of MIROC-ESM" Sorry, I don't follow this.

Supplementary Fig. A has been added to clarify the source areas of glaciogenic dust used in our experiments and the sentence has been reworded.

Page 7, line1: Is it really drier over the Sahara? I would be less surprised if it was stronger winds?

Yes, you are correct. Stronger wind is the reason for more dust from desert areas. The sentence has been modified appropriately in the revised text.

Page 7: line 3: "is probably because of the increased soil moisture, resulting in an enhancement of precipitation" Shouldn't this be "resulting from"?

This has been changed accordingly.

Page 7 line 21: change "location" to "source".

This has been changed accordingly.

Page 8 line 10: "It represents the total effect of the glaciogenic dust on radiation towards the earth surface" Do you mean dust-radiation plus dust-cloud plus dust-cryosphere interactions?

We mean the total effect of the glaciogenic dust load in the atmosphere toward the

surface of the earth. The sentence has been rewritten to clarify this point.

Page 8 line 19: Repeated sentence.

Thank you. The duplicated text has been deleted.

Page 9 line 7: Refer to figure 6 here.

We have done as you suggested.

Page 9: 18-19: Please can you briefly summarise what these are?

An appropriate explanation has been added.

Page 10 line 16: i.e. it contributes to atmospheric heating.

The global mean radiative perturbation by glaciogenic dust is cooling (−0.19 W m^{-2}) However, glaciogenic dust behaves differently over the polar regions and it contributes to atmospheric heating. An appropriate explanation has been added in the revised manuscript.

Page 13 line 12: "draught" should be "drought".

Thank you for identifying this error; it has been changed accordingly.

Page 15 line 15-16: How strong is this snow bias in MIROC-ESM? Might be worth shoing

Supplementary Fig. H has been added to show that snow cover tends to remain in boreal spring over southern Siberia.

Figure 8: This caption doesn't completely make sense to me: "Green line denotes LGMglac.naging.a-LGM.naging.a, which means the change arose from non-aging effect of snow albedo." Does this mean that the snow albedo is affected by dust but not by ageing? Also change "Shades" to "Shading".

We wanted to say that the "LGMglac.naging.a–LGM.naging.a" shows "the change is

289 **not attributable to the ageing effect of snow". The caption for the figure has been**
290 **rewritten in the revised text.**