Dear Prof. Phipps,

We are submitting our revised manuscript entitled "How changing the height of the Antarctic ice sheet affects global climate: A mid-Pliocene case study" by *Huang et al.* to you for consideration for publication in *Climate of the Past*.

We are very grateful to the reviewers for the thoughtful suggestions, which have been incorporated into the revised paper (track changes). They are detailed below.

Comments of Reviewer 1:

Huang et al. addressed most of my comments well. However, a few of the comments were not sufficiently addressed, and there are certain points where the text remains confusing or logically inconsistent.

I think this paper could be published with minor revisions to address these issues.

Specific comments

Line 64, "In this scenario, Antarctica's melting ice sheets would raise sea level 20 meters in coming centuries (Grant et al., 2019).": This is not necessarily inaccurate, but it's confusing given that the aspect of AIS reduction of concern in this study is not the sea level rise but the change in the volume of the ice sheet. As you describe, could you make a statement here about the volume of ice lost, rather than the resulting sea level rise?

Done. We have changed the magnitude of sea level rise to the volume of ice lost, following the good suggestion (see lines 65-66).

Line 65, "we use the Pliocene as an idealized test case to investigate how large changes in the East AIS (EAIS) height affect the climate.": This is a minor tweak, but I think it would be clearer to say something like, "we use a model of the Pliocene to investigate how large, hypothetical changes in East AIS (EAIS) height would affect the climate." I suggest this change because the Pliocene itself is not the test-case;

rather, the test-cases are the hypothetical scenarios which are perturbations on the Pliocene case.

Thanks. We have improved the sentence following the suggestion (see lines 67-70).

Lines 123-127, “All these sensitivity experiments are hypothetical scenarios, because changes in surface albedo due to ice sheet removal have not been accounted explicitly in the present study through increasing the sea level.”: My previous comment was concerned with ice sheet volume, not surface albedo. In the mid-Pliocene warm period, the climate had time to adjust to near modern levels of CO_2. Thus, the ice sheet volume in the PRISM4 reconstruction is meant to represent a longer-term adjustment than we have thus far experienced in the present (as you mention in your introduction). The 0%, 25%, 50%, and 75% scenarios therefore represent a somewhat arbitrary further reduction against mid-Pliocene ice sheet volume. Would these ice sheet volumes correspond to any projected future scenarios, and if so which scenarios? If you make clear that these experiments are hypothetical, I don’t think you necessarily need to discuss surface albedo here, since you already mention it elsewhere.

Yes, our experiments are hypothetical. We have deleted the discussion on the surface albedo (see lines 126-132).

Line 136-138, “The results are presented as anomalies from the control for the sensitivity experiments, thereby estimating the EAIS height effect during the mid-Pliocene warm period.”: I suggest you remove “thereby estimating the EAIS height effect during the mid-Pliocene warm period.” Again, unless I’m missing some important information, the 0%, 25%, 50%, and 75% scenarios were not Pliocene scenarios. Your control in this study is the mid-Pliocene warm period, and the anomaly plots you show are hypothetical effects.

Many thanks for the suggestion. We have deleted the sentence "thereby estimating the EAIS height effect during the mid-Pliocene warm period" (see lines 147-148).

Comments of Reviewer 2:

The authors have made several useful adjustments and additions to the manuscript to address some of the issues pointed out. In contrast to what was suggested by the editor, the revisions are mostly minor and hardly any additional analysis and/or discussion was provided where requested. Therefore, a solid basis explaining the relevance of the study is still missing, as well as the elements needed for a proper physical understanding of the results shown.

In their responses, the authors seem to evade most of the issues raised and simply repeat what was already in the text, or state some trivial explanations that are often not related to the questions asked.

Many of the results and discussion focus on the obvious result, which is a lapse-rate induced temperature change and global temperature/pressure/precipitation response by redistribution of mass. The more interesting and far less intuitive responses beyond this first order effect are in my opinion left mostly untouched. I feel a more substantial effort can and should be made before publication.

Main comments:

L112: you mention specifically that you use dynamic vegetation here, yet you answer that all the boundary conditions are the same except for the EAIS height. I assume between your simulations? This still means that there is a difference between your MPcontrol and the original PlioMIP simulation. Please clarify.

Yes, there is a difference between our MPcontrol and the original PlioMIP simulation, namely the MPcontrol uses dynamic vegetation while the original PlioMIP simulation uses fixed vegetation. Our sensitivity experiments only changed the height of the EAIS on the basis of the MPcontrol experiment. All the results showed in the manuscript are anomalies between the sensitivity experiments and the

MPcontrol experiment, rather than those between the sensitivity experiments and the original PlioMIP simulation. The effect of the dynamic vegetation can be assessed by comparing the results between the MPcontrol experiment and the original PlioMIP simulation, and is certainly interesting, but should be a focus of another paper. To make it clearer, we have improved some sentences in the Methods section (see lines 115-120).

Temperature and precipitation responses outside of the EAIS are clearly not all linear between the different experiments. Are the linear responses you mention globally averaged, or over the EAIS only? Especially with the larger reductions, both temperature and precipitation patterns become interesting and are likely related to circulation changes in both the atmosphere and ocean. These patterns as well as their dependency to the EAIS height are not fully explained.

Yes, temperature and precipitation responses outside of the EAIS are clearly not all linear between the different experiments. In our paper, we claimed linear temperature and precipitation responses in regions over the EAIS. We actually stated "Compared with the MPControl experiment, the East Antarctic annual mean surface temperature increases by about 5 °C, 10 °C, 15 °C, and 18 °C with the height reduction of 25%, 50%, 75%, and 100%, respectively (Figure 2 in the manuscript)". These linear responses are also confirmed by the change in temperature with height (Figure S1 in supplementary material) and the energy balance plots (Figure 8 in the manuscript). The former is similar in all of the experiments. So, at a given location temperature changes quasi-linearly with height. The later show that all factors (topography, heat transport etc.) make up a similar fraction of total temperature change in all the figures.

We agree that the temperature and precipitation changes are likely related to circulation changes in both the atmosphere and ocean. For this, we added the changes of the annual water vapor flux over Antarctica (Figure 7 in the manuscript). The results show that with the height reduction of the EAIS, the easterly flow encircle the East Antarctic continent, extending from ~60°S to the continental periphery. This means that the water vapor flux decreases over that the continental periphery upon successive reduction of the EAIS height, which indicates that the circulation in the atmosphere and ocean are both weakened.

Improvements made to the figures are very minor and most of the issues regarding readability as well as relevance to the results and conclusions remain.

The figures in the manuscript have been further improved based on this and previous comments. First, the changes of the EAIS between the specific experiments have been added (see Figures 1b-d in the manuscript). Second, the projection and latitudinal extent of Figure 6 (Figure 7 in previous version) have been changed, in order to make it consistent with those of Figures 2 and 5 (see Figure 6 in the manuscript). Third, the energy balance between other sensitivity experiments and MPControl has been added into Figure 8 (Figure 10 in previous version) to make the explain more logic. In addition, we added one more Figure (Figure 7 in the manuscript) to make the results and conclusions clearer and another Figure to the supplementary material (Figure S1) to support the quantitative calculation.

Regardless of the changes made, section 4 is a mix of discussion and model results making this part confusing and messy. None of the analyses are presented in the methods section, and new results now seem to appear out of the blue past the main results section. The minor additions made to the methods section currently fail to resolve the overall unclear structure.

Many thanks for the suggestion. We moved the experimental design of the new sensitivity experiment, which is similar to the -100%EAIS experiment, except artificially raising the sea level by reducing the land level (away from Antarctica) by 60m, from the Discussion to Method section (lines 133-138). Moreover, the "4.4 Energy balance" section was merged into the "4.2 Causes of global temperature changes" section, to make section 4 more structured (lines 248-295).

Specific comments:

Correlation does not imply causation. Indeed, the redistribution of air causes global changes in pressure when changing the height of the EAIS. Using the ideal gas law, one can argue that warmer air will increase surface pressure. In reality, thermal heat lows would claim the opposite relation, as the atmospheric circulation also responds to the density anomalies. This is probably just a poor example in comparison to the study, but explaining the temperature response purely from the ideal gas law at least needs some more explaining. You could at least check whether the temperature and pressure anomalies and their spatial patterns are consistent with your hypothesis.

Over Antarctica, we show that heat transport is the main cause for the temperature changes over this region. To make it clearer, the "4.4 Energy balance" section was merged into the "4.2 Causes of global temperature changes" section (see lines 248-295). Moreover, we added the Energy balance Figure of other experiments into Figure 8 (see Figures 8b-d) and calculated the contribution of each energy balance component to the temperature changes over the East Antarctica and the rest of globe. The results show that heat transport is the primary factor and Topography (the ideal gas law) is the secondary factor for the temperature changes over Antarctica, while over the rest of the globe, Topography (the ideal gas law) is the primary factor and heat transport is the secondary factor for the temperature changes. These results have been added to the "4.2 Causes of global temperature changes" section (see lines 258-262, 264-266)

In fact, the spatial pattern of temperature and pressure anomalies have already been presented in our paper (Figures 3 and 9 in the manuscript). Evidently, the surface air pressure increases over Antarctica and decreases over elsewhere, which is similar to the spatial pattern of the air temperature changes.

Anyway, we replace "well explains" with "may explain" to tone down the argument (line 289).

The lapse rates found as a result of changing the EAIS height should be explained better in the text as well. I am also not sure whether this calculation is correct; in the 50% reduction case there is a >18C temperature increase over the highest region of the ice sheet, which is reduced by about 2km in elevation. This would correspond to a 9C/km lapse rate rather than 5C/km. This may be completely different from how the lapse rates were calculated, but it is impossible to tell without a proper explanation.

It is true that the lapse rates are higher over the summit regions than other areas of the EAIS. However, the lapse rate we presented is the average value over the EAIS area, not for the summit area of the EAIS. To make it clearer, we calculated the lapse rates and added the Figure to the supplementary material (Figure S1) and the detailed description also has been added to the manuscript (lines 154-160).

The text still mentions that 'precipitation changes are consistent with decreased temperatures', without explaining any of the regional patterns, inconsistencies or non-linear responses. The -100% precipitation anomaly clearly is not twice the -50% one, for example, this is neither mentioned, nor explained. Another example is a slight precipitation increase over West Antarctica, where we see strong cooling. It is also unclear to me how the 5% precipitation increase per degree C was obtained, is this global average precipitation vs temperature?

We aim to address the changes over the East Antarctica versus the rest of the globe. Therefore, the regional patterns, inconsistencies or non-linear responses are not the focus of our paper, and should be topics of other papers. To make it clearer, we have improved some sentences in the introduction/abstract (see lines 23, 79).

The precipitation enhancement vs temperature is estimated only for the East Antarctica, rather than global average. Our results clearly show that the -100% precipitation anomaly clearly is twice the -50% one for East Antarctica, but not for the rest of the globe. It was obtained from the average precipitation increase dividing by the average temperature increase (see lines 184-187).

We do not agree that there is a slight precipitation increase over West Antarctica with a strong cooling. Figures 2 and 4 in the manuscript clearly show that, with the successive reduction of the EAIS height, the precipitation and temperature both decrease over West Antarctica, in terms of the spatial resolution of the data.

I am still missing any mechanical explanation as to why the ITCZ/SPCZ would respond to EAIS changes. A thermal imbalance between hemispheres can indeed be a cause (but should then be quantified) of an ITCZ shift, but it is still unclear why the effect ramps up beyond the -50% reduction.

The ITCZ/SPCZ is very sensitive to convective mixing, which is closely related to the intensity of trade winds. As shown in Figure 3 in the manuscript, beyond the -50% reduction, the meridional gradient of surface air temperature decreases over the low to mid-latitude Western Pacific, leading to a weakening of the trade winds and decreased water vapor flux (Figure 1 below). This may explain the decreased precipitation over most of the tropical areas.

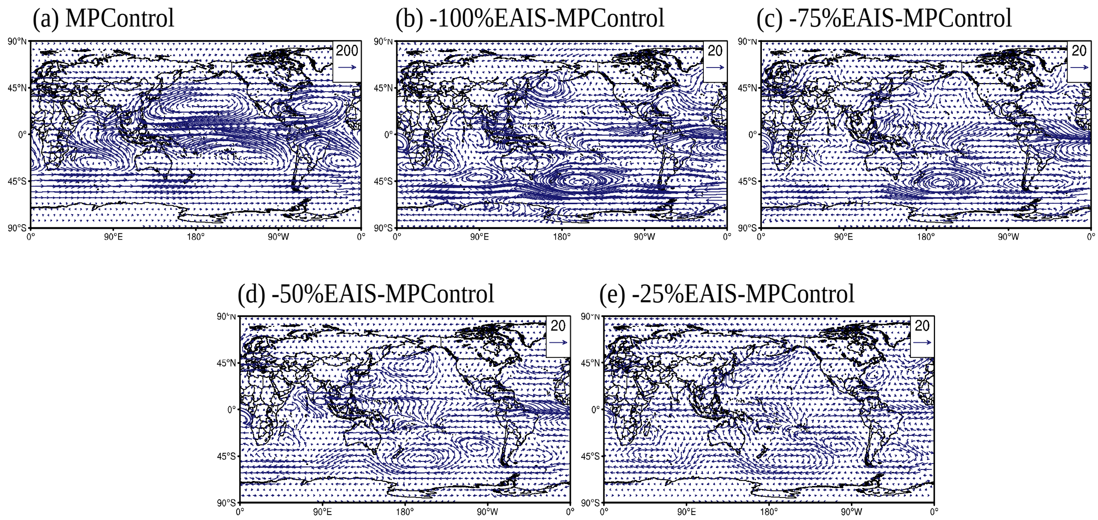

Figure 1. Vertically integrated water vapor flux (arrows, units: kg m^{-1} s^{-1}) for (a) the MPControl, and the anomalies (arrows, units: kg m^{-1} s^{-1}) for (b) the -100%EAIS relative to the MPControl, (c) the -75%EAIS relative to the MPControl, (d) the -50%EAIS relative to the MPControl, and (e) the -25%EAIS relative to the MPControl.

The Zonally averaged temperature changes (Figure 2 below) show that the temperature increases significantly over southern hemisphere while changes very slightly over northern hemisphere, indicating a clearly thermal imbalance between hemispheres. ITCZ shift may play an additional role, but factors influencing it remains unclear, including changes in the balance of heat between the hemispheres, the wind-driven ocean circulation, and convective mixing and so on (Donohoe et al. 2014; Adam et al. 2016; Green and Marshall, 2017; Talib et al., 2020), which requires systematic investigation in future studies.

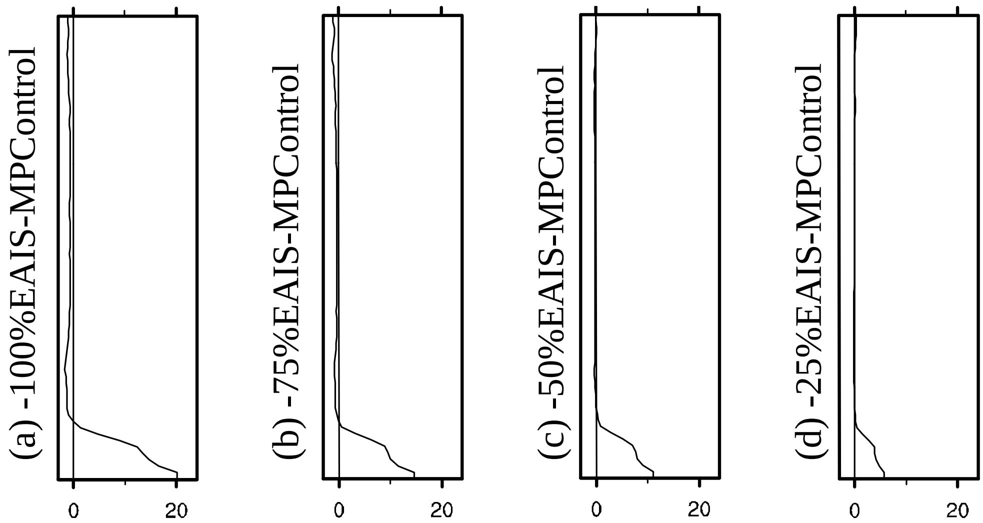

Figure 2. Zonally averaged temperature changes (units: ℃) for (a) the -100%EAIS relative to the MPControl, (b) the -75%EAIS relative to the MPControl, (c) the -50%EAIS relative to the MPControl, and (d) the -25%EAIS relative to the MPControl.

It is pretty much impossible to see the change in katabatic winds from figure 7. Although it may be straightforward, the wind field alone is not enough to explain moisture transports without knowing the actual moisture field. Even in the current climate, katabatic winds are confined to the area very near the ice sheet's edge, making it tough to explain moisture transports over a large latitudinal range from this effect only.

We have added water vapor flux figures to further support the explanation (Figure 7 in the manuscript and lines 235-247).

The statement that responses to the EAIS are linear are still not substantiated by any clear figure or clear quantitative assessment. Without such, it is a claim that cannot be validated nor explained.

Do not agree. As mentioned earlier, the precipitation and temperature both increase over East Antarctica with the successive reduction of the EAIS height, which are clearly presented in Figures 2 and 4 in the manuscript. We added the calculation in the text (lines 156-160, 184-187). Moreover, these linear responses are also confirmed by the change in temperature with height (Figure S1 in supplementary material) and the energy balance plots (Figure 8 in the manuscript). The former is similar in all of the experiments. So, at a given location temperature changes quasi-linearly with height. The later show that all factors (topography, heat transport etc.) make up a similar fraction of total temperature change in all the figures.

If the study focuses only on the effects over Antarctica versus the rest of the globe, this is currently unclear from the abstract/introduction. As you show in the energy balance analysis, heat transports are the primary contribution to much of the changes. Yet, you claim that the ideal gas law and temperature changes are mostly responsible. This is at least partly contradictory, as the heat transport suggest that circulation changes should have at least a comparable contribution. While mentioning this contradiction yourself, the ideal gas law explanation is still presented as the main mechanism in section 4.2.

Yes, we aim to address the effects over the East Antarctica versus the rest of the globe. In fact, we have already mentioned this in the introduction, which is further clarified in both the introduction (line 79) and abstract (line 23) in the revised manuscript.

Our results show that over the Antarctica, heat transport is the primary factor influencing temperature, and the topography (which represents the ideal gas law) and GHG play a secondary role (turquoise line in Figure 8 in the manuscript), while over the rest of globe, the topography (which represents the ideal gas law) and GHG are the primary factor influencing temperature, and heat transport plays a secondary role. In Section 4.2, we did not say that the ideal gas law is the ‘primary’ factor. To make the expression clear and logical, the “4.4 Energy balance” section was improved and merged into Section 4.2 (lines 248-295).

The experiment in which the land surface is decreased by 60m does not act to support the direct link between temperature and pressure. It merely shows that the mass loss of the AIS that was previously unaccounted for does not substantially alter the results. If anything, Figure 9 shows that outside of the AIS, where lapse rate effects dominate, hardly any spatial correlation remains between the temperature and pressure responses.

The experiment (-60 m land surface) is not designed to act to support the direct link between temperature and pressure. In fact, this experiment is designed to provide a more realistic -100%EAIS experiment by lowering the land surface (artificially raising the sea level), and acts to verify the cooling effect of EAIS height change over extra-Antarctic regions.

Our paper focus on the comparison of the EAIS height effect between East Antarctica and the rest of the globe. The temperature contrast between them is well explained by heat transport and surface air pressure change, as evidenced by the energy balance results (lines 248-295). The temperature and pressure anomalies are much smaller over extra-Antarctic regions compared to those over Antarctica, which may result from complex interactions between ocean and atmosphere. This requires further study.

Figures:

Figure 1: this is a nice addition, but does not show any new information compared to what can be found in previous PlioMIP publications. The aim of such a figure would be to show how the EAIS was changed between the specific experiments.

Done. The changes of the EAIS between the specific experiments have been added (see Figures 1b-d in the manuscript).

Figures 2-4: as figures 2 and 3 show the same field over a different region, while figure 4 shows a very similar field over the same region, at least one of them is redundant in the current set-up. Of course, SAT and SST effects are closely related over the ocean, as they are both at or near the surface and therefore nearly the same.

Done. The SST effects have been moved to supplementary materials.

Figure 7: The projection and latitudinal extent used here is not at all consistent with Figures 2 and 5, so I fail to see what kind of consistency is meant here. Regardless of consistency, the figure remains near impossible to read and interpret. Cylindrical versus stereographic projection will not make much of a difference when looking only at the pole, but the former becomes very unrealistic when showing an entire hemisphere.

Thanks. We have changed the projection and latitudinal extent of Figure 6 in the manuscript (Figure 7 in previous version), in order to make them consistent with those of Figures 2 and 4 (Figure 5 in previous version).

References

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