Reviewer comment on 'How changing the height of the Antarctic ice sheet affects global climate: A mid- Pliocene case study' by Huang et al

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.
- 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.
- 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.
- 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.

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.

- 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.
- 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?
- 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.
- 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.
- 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.
- 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.
- 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.

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.
- 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.
- 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.