Report 1

I am happy with the careful revisions the authors have made to this manuscript. Clarity and overall message of the study has been greatly improved. I only recommend one further revision (outside of technical corrections). Figure A2 is not necessary in my opinion. Not much can be seen as to the eye all three subfigures look identical. Initial state A is already depicted in Figure A1. Figure A3 is then much more useful for comparing the initial states.

We warmly thank the reviewer for their time and assistance in improving the manuscript. We have removed the figure as suggested.

Report 2

I would like to thank the authors for responding to my comments and acknowledge the efforts made to improve the manuscript. However, I still have a few comments on both the content and the style. Indeed, several sentences would benefit from stylistic revisions and careful language editing. The phrasing could be improved for clarity as several sentences are rather long and could be split for better readability. Moreover, there are still a number of typographical errors. I have listed a few of them below (see Other Comments), but this list is not exhaustive. I therefore recommend a careful proofreading and appropriate corrections, especially since at least one of the authors is a native speaker.

We warmly thank the reviewer for their times spent reviewing the manuscript and for providing suggestions for improving the clarity of the paper. We will improve the typographical and grammatical errors and standardize the text to American English.

Main Comments

1. One of the main points I raised in my first review concerned the need to better highlight the attribution of oscillations in the Humboldt-Petermann glacial system to chaotic variability. In appreciate that the authors added a figure (Fig. 6 in the revised manuscript) showing the final ice volume as a function of the ramping rate and the tipping time. However, I agree with their argument stating that Fig. 6 does not provide any evidence of a threshold ramping rate. This is why, following the author's response, I suggest that this figure could be removed from the main manuscript or moved to the Supplementary Material.
Moreover, the comments in this section may not have been fully updated. It seems that references to Figure 5 have been replaced by references to Figure 6, although the quantitative analysis appears to correspond to Figure 5 rather than Figure 6, in particular Figure 6b.

We have removed Figure 6 from the manuscript as suggested. We have also corrected the figure references.

2. Instead of the current Fig. 6, I suggest a panel to Figure 7 showing time series of the ice volume for a given temperature forcing (DT = 1.15 K or DT = 1.20 K) and different ramping

rates. Similar figures could also be included in the Supplementary Material for other temperature forcings (or other ramping rates).

We have added a panel to Fig. 7 as suggested.

3. After a first reading, I thought that Section 3.4 could be moved to the Discussion section. However, I have changed my view after taking a closer look at Figure 12. Indeed, the bedrock elevation is computed as the difference between surface elevation and ice thickness. Yet, in the figure, this difference appears to reach approximately 200 m, whereas the authors report about 40 m. I therefore suspect that there may be an error in this figure.

Figure 12 displays the value of the bedrock altitude, not the difference. So, while the bedrock in the area of the ice stream does reach about 200 m, this is only approximately 40 m above the minimum bedrock altitude. We will improve the text in section 3.4 to make it clear that the change in the bedrock altitude is not a large contributor to the change in the ice sheet surface altitude.

4. Section 4.1 I don't understand the comments of Fig. 14. What is meant by "a 2-period cycle as it does for a forcing of 1.0K"? I think the authors need to further elaborate on Fig. 14 to highlight the objective of this figure. I confess that I don't really understand why simulations performed with DT = 1.0 K and DT = 1.15 K appear to be anomalous compared to other simulations. The authors then refer to chaotic transients, which are discussed in Section 4.3. Is there any possibility to change the structure of this section to make things clearer?

We have removed Fig. 14 as it does not ease interpretation of the results. The goal was a visual representation of the chaotic variability through some discrete mapping similar to a Poincaré map or recurrence map, but it ultimately does not contribute to an understanding of the variability beyond what is observed in Fig. 7, for example. What may have been unclear was that all simulations at a given forcing magnitude were plotted in the same colour, such that a single simulation at a forcing value of 1.0 K and a single simulation at a forcing value of 1.15 K were anomalous compared to the rest of the simulations at those forcing magnitudes. By having them all be the same colour, this was not clear. Regardless, those anomalous simulations are discussed in Appendix E, so no information is lost by removing Fig. 14.

We have also swapped the order of sections 4.1 (Ice-stream oscillations) and 4.2 (R-tipping of the GrIS), as well as removed the mention of chaotic transients from the previous section 4.1. We still reference section 4.3 in this section, but believe the ordering the discussion to be r-tipping (the research question) to ice stream oscillations (which affect the tipping) to chaotic transients (which describes how the oscillations affect the tipping) flows better.

Other comments

L22: Avoid the use of “dynamics” in this context. The following sentence refers to surface processes rather than ice dynamics.

We have corrected this sentence as suggested.

L26: “Long time scales”: Can you be more specific?

We have clarified the uplift rates for Greenland as being on the order of millimeters per year, resulting in timescales of the response being millennia or longer.

L41: add “thereby” before “activating”, or split the sentence.

We have corrected this sentence as suggested.

L43: contributes significantly (or substantially)

We have corrected this sentence as suggested.

L60-62: The sentence is not grammatically correct.

We have corrected this sentence.

L70-72: replace “being” with “is”

We have corrected this sentence as suggested.

L93: The reference is (Le Meur and Huybrechts, 1996).

We have corrected this reference.

L101-102: Ice sheets gain or lose mass also through dynamic processes, and by sublimation and wind-drift (the latter two are surface processes).

We have corrected this sentence.

L119: Remove “and”

We have corrected this typo.

L147: add “a” before “clear”

We have corrected this typo.

L159: (Fig. 5).

We have corrected this typo.

L176: What about moving the last paragraph of Section 3.1 at the beginning of Section 3.2?

We have added a connecting sentence at the beginning of Section 3.2 to better flow from Section 3.1.

L190-191: Please clarify (maybe in the description of the model) and be more specific.

We have replaced “significant” with “nonzero” and moved this sentence to the beginning of the model description, along with some more clarifying sentences.

L215: The increase in the basal velocity

We have corrected this typo.

L217: I would say “resulting directly from the thermomechanical coupling”.

We have corrected this sentence as suggested.

L221: build-up

We have corrected this typo.

L227-229: The sentence is generally understandable, but it is very long, and not very well structured.

We have split this sentence in two to make it more understandable.

L231: configurations

We have corrected this typo.

L255: assess

We have corrected this typo.

L255: Remove “otherwise”

We have corrected this sentence as suggested.

L266: Replace “value” with “factor”

We have corrected this sentence as suggested.

L306: “can” instead of “may”?

We have removed this sentence in connection to removal of Fig. 14

L313: Please, provide references

We have included references.

L323: atmospheric model

We have corrected this typo.

L335: Can you explain why?

We have removed this sentence and re-framed the role of atmospheric versus oceanic forcing in terms of what is observed in the model simulations.

L356: Could you specify what you mean with “directions”. Please, refer to the ice sheet physical system rather than a theoretical framework

We have reworked the first sentences of this section which contained unexplained jargon. We have left the remainder of the section intact, however, as the theoretical framework is necessary to be able to discuss the results, which can not easily be translated into a physical understanding. For example, there is no physically realizable “saddle state” as it is unstable, so it is not feasible to describe what this would look like in terms of a real-life ice sheet configuration. (It may be approximated, as discussed through an edge-tracking algorithm, but that is beyond the scope of this study). Similarly, it is difficult to motivate a physical description of a “ghost attractor”. However, this theoretical saddle state must exist if the system is bistable, and it is important when discussing the two types of chaotic transients: either as r-tipping through a chaotic saddle, or as b-tipping through a ghost attractor, which in turn is required to discuss whether r-tipping is occurring in the model simulations. Thus the theoretical framework is used because it is the best tool available to analyze these chaotic transients.

L371: a system-specific

We believe this is in reference to the critical forcing value fo 1.28 K, so have added this caveat there.

Figures:

Increase the font size: Figures 1, 2, 3, 5, 8, 9, 10

The font sizes in all of these figures has been increased.

Panels (c), (e), (g) and (i) in Fig. 9 are identical to panels (b), (d), (f), (h) in Fig. 10. Please replace with the right panels

The left-hand panels in Fig. 10 have been removed.