Dear Editors,

Suggestions of both reviewers were very helpful and have been addressed as described below.

The largest task was a major reorganization of the paper. Both reviewers (in the following reviewer 1 refers to David Archer and reviewer 2 to Peter Thorne) found the paper to be difficult to follow, including division of the paleoclimate discussion into two different sections. That original flow followed the temporal development of our study over an eight year period. I have reorganized the paper with the climate model study first, then modern observations, then paleoclimate. Co-authors reading the new version agree that it is now much clearer.

Despite the fact that the reorganization allowed removal of some redundancy, the paper is not shorter, mainly because of requested clarifications, as described below. A crucial point is that the strength of the paper depends substantially on the consistency of conclusions derived from these three different parts (model, modern data, paleo data and interpretation). I hope that the paper now makes clear that the whole is greater than the sum of the parts, so it would be unproductive to break the paper into more than one paper, which would make it more difficult for readers who want to understand the full implications. Fortunately the reviewers are not insisting that the paper be broken up. I also agree with the second reviewer that moving figures to the Supplement, even though it would shorten the paper, would be disadvantageous.

Reviewer 1 notes that carbon cycle models cannot yet satisfactorily reproduce paleo CO_2 variations, that our discussion paper may have left the impression that the mechanisms by which Southern Ocean ventilation of the deep ocean tends to control atmospheric CO_2 are understood, and that it is not necessary for our purposes to get into the details of the ocean carbon cycle. These points are all well taken and we have revised the text accordingly, which allowed us to omit a few references, instead adding reference to a review by Ridgwell & Arndt (which includes a 4th ocean carbon pump involving dissolved microbial carbon). We do not need to get into such carbon cycle detail to make important points about the role of the Southern Ocean in atmospheric CO_2 and climate.

Back to the question of possible streamlining or breaking up of the paper: could the role of the Southern Ocean in the carbon cycle and evidence for storminess on the North Atlantic be handled in separate papers, instead focusing this paper on what is probably our main conclusion, the feedback effect of melt water on ocean stratification? There are several reasons why that is not a good idea, including: (1) the story about what happened late in the Eemian is interesting, important, and relevant to what might happen in the 21st century, and our interpretation of the end-Eemian would not be possible without the Southern Ocean/carbon cycle connection and the North Atlantic/AMOC shutdown/storminess story; (2) our conclusions about the sensitivity of the climate system and the fact that 2°C warming is dangerous are made stronger by evidence from the Eemian case. A modeling study showing that we are close to significant amplifications may be interesting by itself, but it is much less persuasive.

Reviewer 1 asks for more explanation for why we suspect that Greenland is not subject to the same nonlinearity as Antarctica. One of the Short Comments on our paper (by Jason Box) also gave several reasons (amplifying feedbacks) why Greenland may be vulnerable to rapid change.

Box is right that there was a misleading phrase in our discussion, and that may be part of what reviewer 1 was referring to. Greenland seems to be a more complicated case than Antarctica, as there are both additional amplifying feedbacks and diminishing feedbacks not associated with the Antarctic case. We have tried to clarify that in a way that should satisfy the concerns; to avoid adding too much to the paper length we also refer to our Author Response on the ACPD website.

Reviewer 1 ends by noting our paper's chaotic presentation – hopefully our reorganization of the paper has addressed this matter well.

Reviewer 2 asks whether the conclusions of the paper depend on causal chain of logic among the three components of the paper: modeling, modern data, and paleo data. I would rather argue that these three areas are mutually supportive and make interpretation of events clearer and more persuasive. For example, reviewer 2 notes that sparse paleo records have multiple potential interpretations – that is true, but when we present these three pieces together, interpretation of the paleo data becomes clearer and logical.

Criticism of our "hosing" experiments as unrealistic is countered by the fact that the model was tested on the reasonably well quantified 8.2 ky event (the freshwater event associated with demise of the Hudson Bay ice dome). That published test was not described in the submitted version of our paper, so we have noted that the model yielded a North Atlantic cooling and AMOC shutdown comparable to that indicated by the paleo record.

Reviewer 2 states that we used a single set of hosing experiments and it may have yielded very different results with more local hosing experiments. However, in fact we did both experiments: spreading the freshwater uniformly over the relevant ocean (e.g., over the Southern Ocean) or introducing the freshwater only at the coastal gridboxes and letting the model distribute it.

One topic on which we strongly disagree with reviewer 2 concerns the statement "Extraordinary claims require extraordinary evidence", which seems to be used to assert that we should do more analyses before we conclude that 2°C is dangerous. This seems to us to turn things on their head. Given all the evidence that we have presented, it seems to us that a claim that 2°C is a safe guardrail is the extraordinary claim that requires extraordinary evidence.

Our paper culminates an investigation that began years ago and has accumulated substantial evidence that warrants presentation. Furthermore, to go back and start the modeling program again is not really an option in the principal investigator's current situation.

Reviewer 2 suggests that the publicity received by the ACPD submission has caused issues over a fair and open review of the paper. Actually it seems to us that it was beneficial in generating many useful comments and suggestions (as well as some that were off-topic, but these latter comments require only minimal response). The openness of the review process seems very beneficial, and we note that reviewer 2, given his reservations about the paper, should be praised for his willingness to openly record his opinions on these matters and separate them from his scientific evaluation.

Reviewer 2 properly criticizes the final section, which was scattershot. He asks instead for a summary of "what next". We have entirely rewritten that final section, which I trust you will now find to be clear, informative about the principal conclusions, and much more useful.

In response to reviewer 2's suggestion we did look at model implications for dynamical contributions to local eustatic sea level, but found these to be at least an order of magnitude smaller than the global mean changes, so to avoid making the paper still longer and introduce delays we did not investigate these further.

We agree that we need to make clearer the case for unusually strong storms in the North Atlantic, which is made more important by recent evidence that the Atlantic Meridional Overturning Circulation may already be beginning to slow. If we do not adequately defend our assertion that storm-driven waves can toss large boulders, a natural tendency to be skeptical may cause some readers to question other conclusions in the paper. Thus we have clarified the geologic evidence, adding two geologic colleagues as co-authors (which I am assuming is allowable). We also added a reference (Cox et al.) to eye witness and photographic evidence of 80 ton boulders recently tossed by the stormy sea on islands off the coast of Ireland

Use of a coarse resolution model with sub-grid scale parameterizations. These parameterizations affect all model resolutions. Actually we show that our model successfully simulates some of the most essential processes for our purposes (such as forming Antarctic Bottom Water along the coast of Antarctica, rather than in the middle of the Southern Ocean, as many "state-of-the-art" models do). Parameterizations of sub-grid mixing may affect the key ocean stratification feedback in climate models, and we now draw attention to this issue in hopes of obtaining better model inter-comparisons, including publication of the fundamental response function of models.

As for our assumption that multi-meter sea level rise can occur on a century time scale, we have referred to evidence for such rapid change in paleoclimate records, which is one of the merits of combining modeling with paleo studies.

We agree with referee 2 that the discussion and figures for modern observations were not as clear as they could be. It was worth making the data figures up to date and making the model projections clear, because these are fundamental matters on which our model/conclusions clearly oppose those of the standard models that do not include ice melt.

I draw your attention to the final two paragraphs. The topic of this paper is unusual in its scope and potential implications for humanity. We feel that it is appropriate to draw attention to normative implications and hope that you find it permissible. A few days ago I met with a wise old man (98-year old Walter Munk) on the science topic, and, unprompted, he strongly advised that we needed to register such a clear statement as a conscience for the scientific community. Be that as it may, we have set the two paragraphs off distinctly, so it is clear that they are separate from the science per se.

James Hansen