05 February 2016

Regarding acp-2015-432   Submitted on 11 June 2015

Revised version submitted 05 February 2016 in response to four reviews and editor's review

1. Introduction

We appreciate the unusual effort required to review such a long multidisciplinary paper, so we would like to express our special gratitude to the editor and four referees for their thoughtful reviews. We believe that these reviews have led to a much clearer presentation.

Our explanation for changes made in response to the reviews is organized as follows:

2. Response to Editor's Review
3. Expanded Responses to Several Short Comments (SCs) in ACPD
4. General Issues Related to Multiple Reviews
5. Response to Reviewers R1, R2, R3 and R4

2. Response to Editor's Review

<u>Additional response to several Short Comments (SCs) identified in R2 lines 529-756</u>

We very much appreciated the efforts of the large number of people providing SCs, which totaled more than 100! These had a substantial helpful impact on our rewriting of the manuscript. The best response to some of the issues raised in the SCs requires information or insights relating to different parts of the paper – modeling, paleoclimate or modern observations; so in some cases we felt that accounting for the comments in the rewrite, i.e., making some specific points clearer, was the most useful response we could make and accounts for the brevity of our published response to some SCs. However, in Section 3 below we expand upon our responses to several of the SCs.

<u>Paper Title</u>

You mention that referees R3 and R4 question the title of the paper. The issues raised about the title concern the word "Dangerous" in the title, and they are important because they get at the very heart of our paper and the overall topic of human-made climate change. I think that the discussions raised are pertinent and I am glad that you give us the chance to propose a title and show that it is well motivated.

I believe that you may have misread the relevant comment of R4. R4 notes that a major goal of our paper is to define "dangerous anthropogenic interference", and he then quotes the 1992 United Nations Framework Convention on Climate Change (UNFCCC), as follows

> "…to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system."

R4 then says that our paper: "…significantly advances this quest for a more quantified definition of such human impact. Very few serious efforts have been made to arrive at a useful definition of 'dangerous anthropogenic interference'. Previous efforts focused on sea level rise have been less rigorous, I believe, with less analysis of the coupling of ice meltwater with oceanic dynamics."

R4 does not mention the paper's title or criticize it, but explicitly recommends publication of the paper.

R3, on the other hand, does criticize the title and does so by referring to the UNFCCC, but his reference is not correct. The word "dangerous" appears once and only once in the UNFCCC, namely in the most fundamental phrase of the Convention, which is given in the inset phrase above. R3 says that "…in the climate change debate, the term 'dangerous' has been given a rather precise definition in Art 2 of the UNFCCC, namely a change that doesn't allow ecosystems to adapt, that threatens the food production and that prevents economic development in a sustainable manner". This is a rephrasing of Article 2 that seems to slightly change its meaning. Let us look at Article 2 in its entirety:

> The ultimate objective of this Convention and any related legal instruments that the Conference of the Parties may adopt is to achieve, in accordance with the relevant provisions of the Convention, stabilization of greenhouse gas concentrations in the atmosphere at a level that would prevent dangerous anthropogenic interference with the climate system. Such a level should be achieved within a time frame sufficient to allow ecosystems to adapt naturally to climate change, to ensure that food production is not threatened and to enable economic development to proceed in a sustainable manner.

Article 2 thus uses the word "dangerous" with regard to the level of greenhouse gases in the atmosphere. The ecosystems/food/economics sentence refers to timeframe in which that level should be obtained. The word "dangerous" is not further defined, perhaps because it is assumed to be well understood.

R2 suggested "potentially dangerous" (probably you meant to refer to R2, rather than R4), but that is too weak. That conclusion already could have been reached without any of the research in our paper.

There is an important issue at play here: overall, it seems to me that the relevant scientific community has been exercising self-censorship in its warning to the public about the danger of human-made climate change. It would be difficult to overstate the threat of increasing human-made climate change, which we suggest threatens to bring about some of the greatest injustices in the history of the planet: of current adult generations to young people and future generations, and of people of the industrialized North to people of the South, as climate change is due mainly to emissions from nations at middle and high latitudes.

My preference would be to just remove the word "highly" from the title, i.e., replace "highly dangerous" with "dangerous", thus making the title slightly shorter and less "journalistic', which was a concern of at least one referee. However, I understand that some scientists consider that title to be too definitive, so in hopes of avoiding delay in publication we have chosen "Ice Melt, Sea Level Rise and Superstorms: Evidence from Paleoclimate Data, Climate Modeling, and Modern Observations Implies that 2°C Global Warming Above the Preindustrial Level Would Be Dangerous", which has been suggested as a possible compromise. I hope that you agree that our proposed alternative phrasing for the title is well motivated.

Reflection on Uncertainties & How to Corroborate/Falsify the Paper's Thesis

We concur about the need to properly caveat our conclusions; we especially like your connection of that issue with the need to discuss the follow-up work needed to corroborate/falsify the paper's hypothesis.

We also want to stress the need for finding the right balance in caveats, because there is a danger involving the issue of self-censorship. It is easy to lard a manuscript with caveats, and that might make it easier to get a paper through scientific review; a scientist seems authoritative if he lists many caveats. However, we must also bear in mind the need to communicate when we conclude that we can make some conclusions with a reasonable degree of confidence. Here is a specific example:

Orbital parameters. Some reviewers suggest that we emphasize that the Eemian may not be a good example for 21st century climate, because Earth orbital parameters differed in the two periods, thus the geographical and seasonal distribution of insolation were not identical. We are aware of that and provide what we think is an appropriate caveat. However, if we keep repeating the caveat it becomes pedantic and even harmful, because it leaves the impression that we think that caveat has special relevance to our interpretation, thus confusing the reader and making it harder to understand the relevant physics.

Based on observational data we showed that during the End-Eemian period the tropical Atlantic was warmer than during the Holocene, but humans are now causing warming bringing tropical temperatures close to Eemian levels. We also showed evidence that strong cooling occurred in the North Atlantic at the end of the Eemian, associated with shutdown or substantial slowdown of the Atlantic Meridional Overturning Circulation (AMOC). The resulting horizontal ocean surface and atmospheric temperature gradients are the important conditions affecting North Atlantic storms, not the local effect of changes of orbital parameters. This warm tropics/cool subpolar Atlantic is very similar to the situation we will get in the Atlantic this century if increased Greenland meltwater allows shutdown or major slowdown of the AMOC, a process that we argue is already beginning. It would not be helpful to focus on additional more extensive and largely irrelevant caveats about orbital parameters being different during the Eemian.

We do not say that orbital parameters are unimportant. On the contrary, we show why Eemian orbital parameters imply that the ice melt causing late Eemian sea level rise was from Antarctica, not the Northern Hemisphere. The Northern Hemisphere was into the phase with hemispheric ice beginning to increase, while conditions in the Southern Ocean and Antarctica were optimum for ice loss. We also show that these conclusions are consistent with other data, including ocean core data and Greenland ice core data, which show that the size of the Greenland ice sheet changed little in the late Eemian.

The Editor, in linking the issue of uncertainties with the topic of how to corroborate or falsify the paper's hypothesis, seems to conclude that one important way to address uncertainties is to discuss what work is needed (observational and modeling) to resolve the uncertainties. We agree. Here is an example:

SMOC & AMOC shutdowns. Other than the threat of large sea level rise, the most startling conclusion of our paper is that the SMOC and AMOC are on the verge of shutdown, and slowdowns are already underway. The SMOC (Southern Ocean Meridional Overturning Circulation) slowdown and then shutdown is the source of the amplifying ice melt feedback, which we have emphasized, but there are other major consequences of altering the two great ocean circulation systems. AMOC shutdown was hypothesized by Broecker a quarter century ago, but he was criticized by climate modelers, who generally could find only moderate slowdown rather and that was very far off in the future. We get a different answer: slowdown of SMOC and AMOC is already occurring with observed melt rates and shutdowns occur by mid-century with current ice melt growth rates. Further, we present evidence that many ocean models, ours included, are too insensitive to freshwater forcing, so shutdown is likely to occur earlier than in our present model runs. This is a very different picture than the picture one obtains from CMIP climate simulations and the IPCC reports that rely on the CMIP simulations. There are two main reasons that we get a different conclusion: (1) we include effects of ice melt in our simulations, with a hypothesis that ice melt will continue to grow (backed up by empirical data for the present and by paleo data from Earth's climate history, and supported by our finding of amplifying feedbacks in our modeling), (2) we argue that some ocean models, ours included, are too diffusive and thus less sensitive to freshwater forcing than the real world, and we note observations suggesting that changes of the nature predicted are already starting.

We agree that we should do a better job of defining the work (modeling and observations) that needs to be done to corroborate or falsify our hypothesis and conclusions, as discussed two paragraphs below.

Policy Discussion

We accept the instruction to minimize policy discussion. Therefore, after we present our conclusions about expected sea level changes, storms, fundamental changes in ocean circulation, and likelihood that, if fossil fuel emissions continue to increase, we will soon be handing young people a climate system that is out of their control, we end with just two sentences about policy: "We conclude that the message our climate science delivers to society, policymakers and the public alike is this: we have a global emergency. Fossil fuel CO_2 emissions should be reduced as rapidly as practical." To say less would seem to be science self-censorship.

Expansion of "identifying critical modeling and observations to corroborate work"

As noted above, we agree that it is important to identify ways that answers can be found to some of the issues that we raise sooner than by waiting until the real world makes the answers clear. So we have taken the suggestion, of referees as well as the editor, to discuss what might be done (in modeling and observations) to gain more confidence, without simply waiting to see whether the climate changes materialize (Sec. 6.7 Modeling Priorities, Sec. 6.8 Measurement Priorities).

Anthropocene

Our comments about the early Anthropocene (which do not take much space, and present a conclusion that differs from those of Ruddiman or his detractors) seem to be warranted because they follow naturally after the CO_2 control knob and paleoclimate discussion, and lead into the current global climate situation in which the human forcing has become very dominant.

Boulders

We agree with the Editor's implication that too much space for the "boulders" may detract from more important parts of the paper. We moved part of that section to the Supplement (discussion of sea level history, evidence of late Eemian sea level rise to +6-9 m), because, unlike when we started working on the paper, agreement is more widespread that sea level did reach that high level in the Eemian.

However, others have suggested that we might be better off by dropping the "boulder" part of the paper. It is surprising to many people that waves could throw a 1000-ton boulder onto a ridge more than 15 m above current sea level. Given that the boulder story is somewhat tangential to the main conclusions of our paper, would it be better to omit that part of the story, or say that we are not sure whether the boulders were thrown by a storm or by a tsunami, while emphasizing the other evidence for strong end-Eemian storm? No, in part for a special reason explained below, we think it is better to note that the simpler interpretation is that all the features – boulders, chevron ridges, runup deposits – are more concisely and logically explained as storm-produced., while also noting that it is possible that the combination of two phenomena (storms and an independent tsunami for the boulders) would also be consistent with the observed facts, even though this dual explanation is more tortuous and even though there is no evidence elsewhere in the Bahamas or on the U.S. East Coast supporting the occurrence of an end-Eemian tsunami.

The special reason is that the boulder story draws attention to an important characteristic of our analysis: we use models for large scale phenomena for which the ability of global models is relatively well-proven (even though we will argue that many ocean models are too diffusive, so meltwater effects may be even stronger and more immediate than we model). We do <u>not</u> use ice sheet models, hydrodynamic models for boulder throwing, or global models to simulate tropical storms. Global general circulation models, based on conservation of energy, mass, and momentum and other fundamental equations such as the ideal gas law, have been developed over more than half a century and shown to do a good job of simulating atmosphere and ocean circulation. In contrast, the models for other phenomena mentioned above are still at an early stage of development and in some ways still fundamentally inadequate.

The inadequacy of ice sheet models was shown by Pollard et al. (2015) when they found that simulated sea level rise in response to a 2°C ocean temperature rise changed from 2 m to 17 m if they added into their model parameterizations of hydrofracturing and ice cliff failure, processes that are known to occur but which are very difficult to model well. The Pollard et al. study also shows how two feedbacks that individually are moderate can feed off each other to produce a large effect: in addition to increasing the sea level rise, the two amplifying feedbacks combine to reduce the time scale for large change from several centuries to several decades. Furthermore, the amplifying feedbacks that we identify in our paper will combine with those in the ice sheet model to reduce the response time further, helping account for how sea level could change rapidly in the paleoclimate record despite the weakness of paleo forcings.

The inadequacy of hydrodynamic modeling of boulder movement is shown by observations of large storm-tossed boulders. North Atlantic storms threw boulders as large as 80 tons to a height 11 m AHWM (above high water mark) on Ireland's Aran Islands, this specific storm on 5 January 1991 being driven by a low pressure system that recorded a minimum 946 mb (equivalent to a category 3 hurricane). Winds gusted to 80 knots and the closest weather station to the Aran Islands recorded gale force winds for 23 hours and sustained winds of 40 knots for five hours [Cox et al., Boulder ridges on the Aran Islands (Ireland): Recent movements caused by storm waves, not tsunamis, J. Geology, 20, 249-272, 2012]. The storm waves built onto swell that was developed by strong winds during the prior two weeks.

Cox et al. (2012) note that existing hydrodynamic modeling equations would not lift the boulders, and they cite two reasons to disregard the equations. First, wave height measurements reveal that waves twice the SWH (significant wave height) of models frequently occur. Second, existing wave equations do not include effects of reflection from cliff and shoreline, and attendant wave amplification. Cox et al. note that wave heights at shoreline cliffs can be much greater than the equilibrium height of approaching deep-water waves. The waves steepen as they shoal, impact the coast, reflect back, meet advancing wave crests causing a mixture of constructive and destructive interference, with intermittent production of very large individual waves capable of quarrying and transporting large blocks and boulders.

These considerations also help explain why megaboulders (~1000 tons) on Eleuthera are only found just south of Glass Window Bridge at the apex of an embayment that funnels waves before they encounter a steep shoreline cliff (Figs. 1-3 of Hearty, P.J., Quatern. Sci. Rev., 17, 333-355, 1998; also Hearty, P.J., Quatern. Res. 48, 326-338, 1997). The special effect of that apex is shown in a photo (Fig. 1) taken on Halloween 1991. Despite relatively calm conditions on Eleuthera, as indicated by the waters in the photo, just southwest from the narrow Eleuthera island, the northeast side of the island was being battered by large waves generated in the North Atlantic by the 1991 "Perfect Storm". The Perfect Storm originated as an extratropical low east of Nova Scotia that tracked first toward the southeast and then west, sweeping up remnants of Hurricane Grace, which deepened the low. The storm at peak intensity had sustained winds of 75 mph (120 km/h), a category 1 hurricane, making landfall on Nova Scotia on 2 November. The shoreline cliffs just south of the Glass Window Bridge, facing slightly east of due north (Fig. 3 in 1st Hearty paper above), were battered by the deep long-period waves generated by the North Atlantic storm.

An unsuspecting bread truck driver, seduced by the relative calm and fair weather (Fig. 1), was swept off the road by one of the bursts (Fig. 1) as the water swept across the road. The truck was thrown/washed well into the shallow waters on the Caribbean-facing side of the island – the driver escaped in these relatively calm waters to the southwest, but his now rusted-out truck frame remains there today.

Fig. 1. The "Rage", as Bahamians term it. Photo was taken on Halloween 1991 from a few hundred meters offshore from southern protected bank-side near the Glass Window Bridge, looking northeast. Telephone pole on left and the 15-20 m cliff provide scale, the splash height exceeding the height of a 10-story building.

Cox et al. (2012) conclude that the equations used to model storm transport of boulders are inadequate, especially for waves reaching a cliffed coastline. Thus the feature about the boulders most puzzling to the lay person, the fact that they rest atop a steep cliff, is likely a key part of the explanation of how they could get to such height. Note that in the current expert discussion about the origin of the boulders there is no disagreement about the fact that they were wave deposited, quarried from the nearby sea cliffs both above and below sea level by deep long-period waves. The only major issue is whether the waves were caused by a tsunami or a powerful storm that was sufficiently long-lasting to generate deep long-period waves. Note also that, although the boulders placement upon Eemian substrate on the ridge can only be directly dated as either late Eemian or within the following few tens of millennia, it almost certainly had to be late Eemian when sea level was 6-9 m higher than today. Sea level fell rapidly after end-Eemian, which would make lifting of the boulders implausible by any water waves, whether storm or tsunami.

Further confirmation of the ability of storm waves to lift large boulders was provided recently by May et al. (Block and boulder transport in Eastern Samar (Philippines) during Supertyphoon Haiyan, Earth Surf. Dynam., 3, 543-558, 2015). Despite the fact that this storm did not have the "advantage" of being stationary for the long period required to develop deep powerful waves, the typhoon produced longshore transport of a 180 ton block and lifted boulders of up to ~24 tons to elevations as high as 10 m. May et al. (2015) conclude that these observed facts "…demand a careful re-evaluation of storm-related transport where it, based on the boulder's sheer size, has previously been ascribed to tsunamis."

One referee suggested that we calculate the energy needed to lift a megaboulder, though it is unclear what that proves. A cubic meter of water weighs a ton, so powerful deep ocean waves have a lot of energy. It is easy to calculate the velocity that the wave would need to impart to the boulder if the wave were like a little boy at the bottom of the cliff throwing a baseball up. If sea level was 6-9 m higher and there was a storm tide, the height for the boulder to be lifted may not have been so great, but for calculation let's say 10 m. Then, setting $mgh = ½ mv^2$ shows that it does not depend on m. g = 9.8 N/kg, Newton in SI base units kg m/s^2 yields v = 14 m/s ~ 50 km/hr ~ 30 mph. However, the wave does not really "throw" the boulder – it carries the boulder. As Fig. 1 shows, even waves generated thousands of km away can generate a big vertical "splash" at a cliff, especially a cliff at the apex of this unique horseshoe-shaped bay. The driver of the bread-truck that was thrown into the bay must have been duly impressed!

We don't know of modern storms lifting Eemian-sized boulders, but that is our point: Eemian conditions, with unusually warm tropics and unusually cold North Atlantic due to AMOC shutdown, were more extreme than today. However, similar conditions could be achieved today if high fossil fuel emissions continue and lead to AMOC shutdown. It is worth noting that the end-Eemian rage included more than boulder lifting. Storms created the chevron ridges, with multimeter thick sand deposits stretching several kilometers across the island. It is probably fair to term the conditions producing those deposits, in descriptive vernacular, as all hell breaking loose in the North Atlantic region.

Response to Editor's direction concerning specific suggestions of reviewers R1, R2, R3, R4

R1: We have used the suggestion of R1 to clarify the strategy of how we investigate an amplifying feedback without modeling ice sheet physics, at the point in the manuscript that he suggested (on page 3, the 4th paragraph in Section 2). As noted above, we also reordered the manuscript sections as he suggested, so the storm and boulder sections are immediately following the climate simulations.

R3: We have addressed all or almost all of the suggestions of R3, as delineated in the full response to the R3 review below. Some of R3's points are already discussed above.

R4: We have made changes in response to these suggestions, as delineated below.

R2: It is not practical and we suggest not desirable to change the color scheme in our figures. The red-blue scheme that we use has been developed so as to allow the viewer to identify the specific numerical intervals of each color. In most cases the color-blind person can usually figure out which color is positive and which is negative. As for using brown and green for the hydrologic cycle, we have done that in the past, but found the color-to-color distinction was not as sharp as when we use the red and blue scales.

We are glad to make the distinction between IPCC and CMIP, and have now done that at the several relevant points in the paper.

In cases where we refer to a specific chapter in an IPCC report, we have changed the reference to the authors of that chapter, as suggested by R2.

R2 seems to have the impression that we are using the GISS-ER model that was submitted to CMIP. The atmosphere model is the same as in the CMIP version of the GISS model, but we made fundamental improvements to the ocean model physics, achieving major improvements in the ocean circulation such as location of Antarctic Bottom Water Formation, transport through the Drake Passage and AMOC strength, as shown by the model diagnostics that we include in the paper. The improvements in the ocean physics, described in Section 3.1, are fundamental to the model performance and its use for our purposes.

R2 has done an enormous amount of work in reviewing the paper down to fine details, for which we are grateful. We have used a majority of the suggestions. We have indicated in the appropriate section below the changes that were made. Some changes were not made, e.g., where it was simply a matter of preference or would have lengthened the paper unduly.

We delineated one-by-one the changes made in response to the first 75 of the 128 items numbered by R2. Some of these required substantial work, e.g., making new figures, updating data through 2015 to include data that accumulated during the lengthy review process, and in one case calculating changes from a new base period. After reaching item 75, and concerned about increasing publication delay, we realized that the Editor required that we explain each change only up to item 65. We used about one-third of items 76-128 without discussing each one, but we thank the referee for his extensive discussion and suggestions.

3. Additional response to several Short Comments (SCs)

M. de Rougemont comment (SC C5401).

De Rougemont is right that we did not include all data of Church and White for the period 1900-present, specifically we omitted Church and White data for the period of satellite data, because we considered the data of Nerem et al. for the satellite era to be more accurate. In our response on the ACP web site (http://www.atmos-chem-phys-discuss.net/15/C7961/2015/acpd-15-C7961-2015-supplement.pdf) we provided a graph that added the later data of Church and White. Since then, an analysis of bias effects in satellite data has been published by Watson et al. (Unabated global mean sea-level rise over the satellite altimeter era, Nature Clim. Change, 5, 565-568, 2015). Watson et al. make a persuasive case that the rate of sea level rise in the early part of the satellite era may have been overestimated. I understand that Nerem et al. and other researchers working on satellite-era sea level are working on improved analyses. Based on details of the Watson et al. analysis, it seems likely that new analyses are likely to show some acceleration of the rate of sea level rise during the period 1993-2015, which would not be surprising, given the evidence that Greenland and Antarctic ice mass loss increased over that period. Watson et al. estimate sea level rise of 2.6-2.9 mm/year for the entire satellite record, which compares with ~3.3 mm/year in other analyses. Given that Watson et al. calibrate their rate based on tide gauge data, their lower rate is not surprising.

The point we make with our graph is that the near zero rate of change of sea level in recent millennia increased to 1-1.5 mm/yr in the 1900s, and ~3 mm/year in the satellite era (1993-present). We do not try to judge among alternative analyses in the satellite era, as the difference does not affect our conclusions. We have made a new graph for our paper, showing the full range of estimates for the satellite era.

We do not make any issue about exact change points for the rate of sea level rise, so it does not seem worthwhile to calculate the significance of any perceived change point, as R2 suggests. Rather than search for change points in the rate of sea level rise during the 1900s, it would be more useful to look for changes in the rate of sea level rise during the past decade or two and in the near future, when a contribution from melting ice sheets should begin to be visible. For that reason the recent paper of Watson et al. is important, even though others may challenge their lower average rate for the satellite era, because of the way it is calibrated with tide gauge data. The important point is that data in the satellite era should soon be capable of detecting with confidence acceleration of the rate of sea level rise – that is a task for the experts in analyses of satellite data, but not for our paper.

Dr. Colgan comment (SC C5493).

Dr. Colgan is correct that there is some very useful discussion of nonlinear processes in IPCC (2013) Section 4.4.4 (Causes of Changes in Ice Sheets), indeed in the entire Vaughan et al. chapter. We now acknowledge that early in Section 5.1. Later in that section we discuss some of the specific processes that Dr. Colgan brings up.

<u>M. Pelto comment (SC C5538)</u>.

M. Pelto provides a very useful discussion of details of ice melt, mainly focusing on Antarctic ice streams, as is appropriate. Although, this is valuable information for readers, it does not seem appropriate for us to add to our paper detailed discussion about why each Antarctic basin is behaving as it is. Nor do we agree that nonlinear growth of ice melt up to sea level rise of several meters is not plausible. The ice resting on retrograde beds far below sea level is vulnerable to rapid accelerating mass loss up to sea level rise of several meters. Multimeter sea level rise per century has occurred a number of times in Earth's history, despite the fact that the rates of change of climate forcing were much smaller than in the 21st century. Arguments that current ice streams are already moving as fast as they can are refuted by high rates of sea level rise during Earth's history.

The examples provided of some areas on Antarctica having increasing mass are no surprise. It is expected that a warming planet will produce greater snowfall over Antarctica. However, we have shown that there is an important feedback that will likely reduce if not eliminate future increases of snowfall over Antarctica: cooling of the Southern Ocean, which will tend to cause more of the increased snowfall to occur before the air masses with increased water vapor make it to the continent.

This comment underlines the reason why a multidisciplinary analysis is essential. Most ice sheet models tell us that ice sheets are very stiff and change only slowly. Paleoclimate data wakes us up and tells us that there must be something missing in the ice sheet models. Sea level can change by several meters in a century, even with weak paleoclimate forcings. Paleoclimate data also presents no evidence of large hysteresis is ice sheet size, i.e., sea level goes up and down with little lag behind global temperature change, contrary to the behavior in most ice sheet models.

<u>G. Flato comment (SC C5878)</u>.

R2 suggests that we did not respond sufficiently to Flato's discussion, suggesting that we should include a section on the global distribution of precipitation change and temperature change. We do in fact briefly note the precipitation changes, providing maps for several scenarios. However, to add more discussion of these would be to lengthen an already very long paper in areas that are not our main focus. We must focus in this paper on the mechanisms and the sense of the major climate changes, and as these are verified finer detailed assessments can be made.

Regarding the comment of R2 about the use of freshwater injection at −15°C: this is discussed below (under Berner comments, SC C5966), where we note that this is a conservative cooling due to the fact that some of the freshwater injection is in the form of icebergs and we must account for the heat of fusion. This matter, especially heat of fusion effects, seems to be important and not well appreciated, so we include this topic in Section 6.7 Modeling priorities.

<u>M. Whipple (SC 5284) & R2 comment re proportion of Eemian sea level from Greenland</u>.

This is a case where it seems much better to point toward the discussion in our revised paper as providing a more persuasive analysis (especially 4.2.4 End-Eemian climate and sea level change). Evidence that most of the late Eemian sea level rise was from Antarctica, not from Greenland, comes from the overall analysis. The Greenland ice core data show that Greenland was not losing mass at that time, so the only other plausible place to find several meters of sea level is Antarctica. We cannot prove which part of Antarctica the ice came from, but we do point toward analyses that may help unravel details, e.g.:

(From 4.2.4 End-Eemian climate and sea level change.) We suggest that the Southern Hemisphere was the source for brief late-Eemian sea level rise. The positive warm-season insolation anomaly on the Southern Ocean and AMOC slowdown due to C26 added to Southern Ocean heat, causing ice shelf melt, ice sheet discharge, and sea level rise. Rapid Antarctica ice loss would cool the Southern Ocean and increase sea ice cover, which may have left telltale evidence in ice cores. Indeed, Masson-Delmotte et al. (2011) suggest that abrupt changes of $\delta^{18}O$ in the EDML and TALDICE ice cores (those most proximal to the coast) indicate a change in moisture origin, likely due to increased sea ice.

<u>Drijfhout et al comment</u>.

I can find no record of the SC by Drijfhout et al. among the >100 SCs that I have, but I can respond here using R2's discussion of the presumed Drijfhout comment. [Note: the Editor has pointed out to us that the Drijfhout et al. comment is SC C6867 filed under J.E. Williams. The comments below address that SC, but we have added a summary statement beginning on line 406 below.]

1. R2 says that the Eemian cannot be directly compared to any future climate eventuality. We do not disagree, nor have we suggested equivalence. However, as discussed above (lines 91-112), the North Atlantic situation in the late Eemian when an AMOC shutdown or substantial slowdown caused strong North Atlantic cooling while the tropics were unusually warm is relevant for comparison with what we model for later this century if Greenland ice melt increases. The tropics will be as warm or warmer than the Eemian, and the shutdown of AMOC that we model produces cooling comparable to end-Eemian.

2. Multi-stage sea level issue. We discuss evidence indicating that there was a sea level minimum during the Eemian and a large sea level rise late in the Eemian. Detail about sea level change within the Eemian would be interesting, but we do not need to assert that such occurred for our purposes, as our study concerns the End-Eemian climate events. However, we include in the Supplement some additional information about Eemian sea level.

3. Our simulations are for two cases: 1 m sea level rise this century and 5 m, which seems to be a good set. Presumably almost everyone will agree that a 1 m sea level rise this century is possible. Sea level rose 130 m between the last glacial and the current interglacial period, an <u>average</u> of more than 1 m per century. Rohling et al. argue for <u>average</u> rates of sea level change of that order within the Eemian period. Given that there is ice corresponding to more than 5 m of sea level sitting on retrograde beds below sea level, given that the paleoclimate record includes cases of sea level rise of the order of 5 m in a century, and given that the human-made climate forcing dwarfs the natural climate forcings that led to such documented rates of sea level rise, it seems reasonable to also consider the 5 m case.

4. I do have the comment of Wehner (SC C5522) that R2 refers to. Wehner refers to a climate model that tries to explicitly model hurricanes with a model resolution of 25 km. Does this model have strong cooling in the North Atlantic and AMOC shutdown or substantial slowdown? Presuming that it does not, the simulations do not seem to be relevant to our study. Also germane: does this tropical storm model produce ocean waves that can throw boulders as heavy as 180 tons, as occurred during tropical storm Haiyan on 8 November 2013 (May et al., Block and boulder transport in Eastern Samar (Philippines) during supertyphoon Haiyan, Earth Surf. Dynam., 3, 543-558, 2015)? Those boulders were thrown by storm waves, not by a tsunami. We should not be held accountable for the failure of other models that attempt to model small scale phenomena; they may or may not do an adequate job of simulating the small scales. However, the point is this: we do not go the route of modeling small scale phenomena, and we should not be required to evaluate deficiencies in such models.

As explained in our paper, the unique geometry at the apex of the bay where larger boulders were thrown in Eleuthera would cause a funneling of a storm's energy. And the sheer cliffs at the apex where the boulders are located are likely an important contributor via constructive interference of incoming and reflected waves, as Cox et al. (2012) discuss. However, it is unreasonable that we should be required to take on the task of evaluating the role of the constructive interference phenomenon, or evaluating the suggestion of infrared gravity waves made by May et al. (2015) based on a prior suggestion of Munk (Origin and generation of waves, Coastal Engineering Proceedings, 1, 1-4, 1950) or other possible explanations for the failure of wave models to duplicate observations. To directly answer Wehner's question we would need to assess the validity of and figure out the problems of both hurricane models and wave models. Our analysis intentionally avoids use of such models.

Addendum: SC C6867 by J E Williams raises several issues that are largely responded to above, but we have one additional general comment and clarify a specific response. The general comment is that one must bear in mind that the forcings and climate change implied by business-as-usual GHG gas emissions dwarf any rates that have ever occurred in Earth's history, with CO_2 rising to levels as great as 900 ppm by the end of the present century. This extraordinary forcing and rate of change must be borne in mind when considering what changes are possible in a century time scale. Paleoclimate helps reveal how processes work, but it does not provide a comparable example for rate of change. With regard to a possible mid-Eemian sea level minimum (which is not required for any of our major conclusions), a late Eemian collapse of West Antarctica likely would yield a preceding minimum, because the Northern Hemisphere had entered a cooling phase and thus likely ice sheet growth. With regard to where 5 m of sea level rise in the future would come from, we did not mean to imply that it would be entirely from West Antarctica. Surely Greenland, East Antarctica, small ice caps and mountain glaciers, and thermal expansion of the ocean would contribute to sea level rise. The simulations in the latter part of the paper had two-thirds of the freshwater from Antarctica and one-third from Greenland, so even if we neglect East Antarctica, small ice caps, mountain glaciers, and thermal expansion of ocean water, the required contribution from West Antarctica would be only 3.3 m.

Berner comments (SC C5966).

Berner makes many very interesting comments and cites many articles in the literature. Discussion of all these could easily double the length of our paper and is simply not practical and attempting to do so here or within our paper could prevent us from finishing the revised version of our paper! Instead, we have read these comments carefully as a prelude to modestly expanding the section of our paper that recommends needed observations (as suggested by the referees). In addition, in looking through these comments again, we note two matters that need clarification.

First, regarding use of 5, 10 and 20 year doubling time for freshwater input in our numerical experiments: we do not say that we believe ice melt growth at a 5-year doubling is likely to occur. We and others (including IPCC) are interested most in the 21st century, and freshwater injection with a 40-year doubling time would yield little response in the 21st century while increasing our computing requirements. Even though 5-year doubling may be unrealistic it is useful because it lets us bracket the empirical ~10 year doubling time and lets us show that much of the simulated response is not sensitive to this rate – instead it depends more on total freshwater amount (1 or 5 m of sea level) not on the 5, 10 or 20 year doubling rate.

Second, we may not have made it clear enough in the paper how and why we used meltwater at −15°C, which, as explained here, is a very conservative estimate of the immediate cooling effect of the meltwater. Of course there is never any water at −15°C in the model. The injected freshwater is mixed as a first step into the upper three ocean layers, so the −15°C water only slightly reduces the temperature of those layers. The reason for using a low temperature for the injected water is that, in the real world, part of the injection is in the form of icebergs. Prior to the simulations in the present paper, with a model that did not yet include the corrections to the ocean model described in section 3.1 of our paper, we did experiments with the injected water being much colder, so as to account for the heat of fusion of ice, i.e., the fact that melting 1 g of ice requires about 80 cal ~ 335 J of energy. However, we found that the larger effect of freshwater injection, even on ocean temperature, was caused by the density decrease of the ocean mixed layer due to the freshwater, i.e., the main effect of the freshwater was caused by its lower density not its lower temperature. Future detailed studies should include this direct cooling due to ice melt (heat of fusion) but proper modeling will require estimating the fraction of freshwater that enters the ocean as icebergs and either tracking the iceberg movement or estimating the area where iceberg melting occurs.

4. General Issues, Some Related to Multiple Reviews

Segregation of topics. We appreciate the concern that the paper is long and complex as it draws on disparate sources of information from different fields of study. This characteristic is essential to our analysis, but we can see that it made the paper harder to follow. Now the paper is reorganized such that, after short sections for (1) Introduction & (2) Organization of Paper, we have the main sections (3) Modeling, (4) Paleoclimate, and (5) Modern Observations, and the final section (6) Summary & Implications. The two previous separate paleoclimate sections have been put together into one section.

We have not gone to the extreme of removing every mention of paleoclimate from the other two main sections, because that would defeat our purpose of exposing insight into how the climate system works.

Degree of caveating and avoiding unnecessary repetition. We appreciate the admonitions to (1) avoid repetition and (2) include relevant, significant caveats. Sometimes these desires conflict, and we must compromise. Above we discussed a specific example, Earth orbital parameters, to explain why we think multiple repetition of the same caveat is harmful, not only in lengthening the paper, but in misleading the reader about the importance of the caveat.

We found the SCs (more than 100!) to be very helpful. In a few cases, an SC raised so many questions that full response would depend on information in a substantial part of the paper. In that case, the most efficient way to clarify the matter, both for us and for the person wanting to understand the response, was for us to take account of the SC in the rewrite. However, we provided additional discussion in response to several SCs above (lines 291-443).

Mischaracterization of IPCC processes. We have been careful in the rewrite to describe the modeling as having been done by CMIP, and the IPCC reports as using CMIP model results. Also we would like to clarify that we do not mean to be critical of the IPCC reports – on the contrary we have the highest respect for the generally authoritative IPCC treatises, which are invaluable reference volumes.

Uncertainties, corroboration/falsification of result, use of models. These topics are related. We agree that we should take care in wordings about uncertainties, and we have tried to do that, but not overdo it.

Uncertainties are inherently difficult to quantify. I would argue that our approach, which involves gaining insight from a variety of sources, specifically modeling, Earth's history (paleoclimate), and modern observations that provides hints of what is already beginning to happen, is actually a good approach that yields a relatively high degree of confidence. There is a lot of solid physical reasoning and there are checks from rather independent sources of information.

We agree with the reviewer(s) who suggested that we need to identify critical modeling and observational work that will help to corroborate (or falsify) our conclusions by expanding discussion of those topics in the final section. This seems particularly appropriate for the question about how soon the overturning circulations will be substantially shut down – is it already too late to avoid that – so our discussion in sections 6.7 (Modeling Priorities) and 6.8 (Measurement Priorities) emphasize that.

5. Response to Specific Referee Suggestions

Referee #1.

Line 89. We have used the Referee's suggestion (see paragraph 4 of Section 2), which is helpful in clarifying the strategy of the paper.

Referee suggested moving storm section and boulders further up front. Storms are now at the end of the modeling section (Section 3) and boulders start the paleoclimate Section 4. Modern data is now Section 5.

Referee #3 (response to Referee #2 is given last, as it is much longer).

Change title: we changed the title, as discussed above.

Leave out ethical, juridical, policy considerations from the conclusions: we have minimized discussion of practical implications of our conclusions. We have not totally eliminated mention of policy implications, as in our opinion that would amount to irresponsible self-censorship, as discussed above. However, we eliminated the two long policy paragraphs (the final two paragraphs of the prior version of the paper) in interests of avoiding further publication delay.

Further efforts to make the paper shorter and more readable: we have trimmed in several places and put part of the sea level discussion in the Supplement, but requests for clarifications have offset the shortening, while making the paper easier to understand.

Conclusions of the paper:

(1) regarding definition of "dangerous", see discussion above (lines 23-82).

(2) as it was suggested by R2 as well as R3, we have added a sentence at the end of the abstract drawing attention to the predicted cooling. However, this cooling is highly time dependent and transitory. In the Southern Hemisphere, the SMOC recovers quickly if ice melt stops, while in the Northern Hemisphere AMOC has hysteresis effects that depend on whether it is slowed down or shut down.

We do not get into a discussion of the human health and agricultural impacts of this (very regionally dependent as well as time dependent) cooling -- that would take substantial space. Furthermore we are not experts in these climate impacts – and use of a single model for such impact studies has been shown to yield unreliable results.

(3) Referee 3 is correct that the Copenhagen Accord does not use the phrase "guard rail" for the 2°C target. In the interests of not delaying publication – and reducing the length of the paper – we have reduced the paragraph that had the "guard rail" phrase to our essential conclusion that a global warming target of 2°C would not provide safety, and we have removed the following two paragraphs on policy.

Circular reasoning?: R1 made a suggestion that we make clear our strategy of imposing accelerating ice sheet melting and then examining whether it generates feedbacks that would support such acceleration, which we have done on p. 3 as noted above. As for the question of whether accelerating ice melt can occur, current observations and paleoclimate data each provide ample support for that conclusion.

Background explanation of stratification, thermohaline circulation, −15°C (heat of fusion), etc.: Several questions raised by R3 are useful in revealing that we did not do a good job in explaining several aspects of our investigation. Now, in Section 2 (Background information and organization of paper) we added the paragraph to explain our strategy re hypothesizing nonlinear melt and then examining whether there are feedbacks that would support it. Immediately following that paragraph we add a paragraph discussing the basic effect that we are investigating, the stratification tendency in the polar oceans caused by adding freshwater, i.e., we explain what we mean by stratification and its effect on vertical mixing. We also improve our discussion of ocean circulation in connection with the ocean diagram (Fig. 22), as requested by R3. It helps us in explaining why we think ocean mixing is so important, why we suspect that ocean mixing is not well represented in many ocean models, and the possible implications of that, especially early shutdown of SMOC and AMOC with all the implications that would have.

Radiative forcing, Climate forcing, Net forcing, Energy imbalance: R3 notes that we should be more precise in using these terms. We should have defined these clearly, which we now do at the beginning of Section 3 (Section 3.1). Radiative/climate forcing and energy imbalance are both measured in W/m^2, but they are not equivalent, indeed very different. This is a fundamental matter that is important to clarify. Their relationship can be explained succinctly, using one simple equation. This also clarifies a major point that we raise about what seems to be a basic deficiency of many climate models, including ours. In an earlier (2011) paper in ACP on Earth's energy imbalance we noted in passing that most atmosphere-ocean climate models (including four that we tested: GISS, GFDL, NCAR and a British model) seem to mix quantities into the deeper ocean too efficiently, which has significant implications. The present paper brings that matter to the fore, because of the effect of excessive ocean mixing on high-latitude ocean stratification caused by injection of freshwater in polar regions. Clarifying the relation between forcing and energy imbalance is a necessary step in raising the basic question about the effect of freshwater on ocean mixing and stratification. We will present evidence later in the paper that stratification effects are proceeding faster in the real world than in models, a result that we interpret as probably being a consequence of excessive ocean mixing in the models. In Section 6 we discuss observations and modeling needed to understand these matters.

Global cooling due to ice melt: R2 and R3 both note the need to draw more attention to and elaborate on the global cooling caused by ice melt. We added a sentence at the end of the abstract. We draw attention to the complex spatial and temporal nature of the cooling in Section 3 mainly in conjunction with Figure 6. That cooling is temporary (if net ice melt stops) in comparison with the longevity of CO_2 in the climate system, but the time scales are long enough for practical importance, especially if AMOC shuts down. Discussing human health and agricultural impacts is beyond the scope of our paper.

Most of the additional points at the end of R3's review, re things that make the manuscript hard to read, are addressed above. We have added a sentence explaining why our model runs are for 5, 10 and 20 year doubling times, even though the range 10, 20 and 40 years may be more realistic.

Referee #2.

Issues raised in R2's preface to his review of the resubmitted manuscript are mentioned in the above pages and handled by clarifications in our revised manuscript. Numbering below follows R2's re-review.

1. We have added a sentence in the abstract about the temperature response, but not precipitation, for reasons mentioned above.

2. It would be a glaring omission not to state this fact (that Hearty's papers were not referenced by IPCC). It is stated in a non-critical neutral way. Hearty's papers, many of them in the period 10-20 years ago are meticulously documented, of exceptional scholarship and clarity, and are of great import for IPCC topics.

I was stunned by Hearty's papers when I came upon them in 2007. His persuasive documentation of a late-Eemian sea level rise to +6-9 m, based on field work at 15 different locations around the world came when most estimates were +2-4 m, and increased to +4-6 m in the 2007 IPCC report. Of equal import was the substantial well-documented evidence of strong late-Eemian Atlantic storms.

When I queried Hearty as to why his papers were not mentioned by IPCC he had no answer and he made no accusations. Even in the 2013 IPCC report there is only one reference to any of Hearty's papers, and it is only a marginally relevant reference.

A principal reason why we should note that the IPCC reports do not mention the Hearty findings is that Hearty's papers give a different impression (than the IPCC report) regarding the danger posed by the modest level of warming in the Eemian relative to today.

A second reason is that this information is relevant to science of a different sort, specifically to issues about possible "scientific reticence" and self-censorship. Richard Feynman emphasized the slowness at which scientific investigators were willing to move away from authoritative positions. He gave the example of the value for the electron charge that Millikan had established in his famous oil drop experiment. New investigations moved only slowly, bit-by-bit, away from Millikan's value until they finally achieved an accurate different value. Reticence to question authority is a valid topic, especially for a topic such as sea level rise, because of the delayed response of the climate system to human-made forcing, which makes sea level rise a problem that is very difficult to handle, especially if there is reticence in describing the threat.

3. I think the detailed information about climate oscillations that paleoclimate scientists have been able to squeeze from geologic records is remarkable. Nevertheless, I replaced "remarkable" with "intricate".

4. No, the statement is fine as is and this is not a place to be larding with unnecessary caveats.

5. No. Paleoclimate data makes it clear that they are realizable in nature. We have plenty of good discussion of this at appropriate places.

6 and 7. Agreed. These are good points – we have changed to CMIP where appropriate.

8. Correctly is the right word. Oceanographers agree that mixing occurs mainly on isopycnal surfaces, and in any case that is what the Gent-McWilliams parameterization is meant to do.

9. The condition is correctly written, but admittedly it is cryptic. For clarification we have added a footnote: "Where ocean depth exceeds 1000 m, these conditions yield D = 1000 m, thus excluding any first-order abyssal bathymetric imprint on upper ocean eddy energy, consistent with theory and observations. The other objective of the stated condition is to limit release of potential energy in the few ocean gridboxes with ocean depth less than 400 m, because shallow depths limit the ability of baroclinic eddies to release potential energy via vertical motion." As indicated in the text, John Marshall suggested these criteria.

10. We have now added the units into the caption (they were already in the figure title, but it is possible that people do not expect to find them there). We cannot add them to the color bar without expanding the figures vertically, which would increase the number of pages of an already long paper.

11. As discussed above, we have not changed the color scheme, which would have disadvantages as well as perhaps advantages, depending on preferences – we think our color scheme is good and very clear.

12. We have added reference to an appropriate paper.

13. The colors are distinct and the color scale is clear – changing it likely could make it less good.

14. The figure is useful for showing the long-term stability and multidecadal variability.

15. We do not need to show that the change is significant, because we do not claim that it is significant. The issue that we raised in our 2011 paper is that ocean models, the several that we examined, have a response time substantially slower than what is suggested by the analysis in our paper on Earth's energy imbalance. The change in our ocean model as a result of the changes in the physics are in the correct direction to make the response time more realistic, but they are not nearly as large as we argued in the 2011 paper is likely in the real world. In fact the change probably is significant, but we have no motivation to waste our time proving that it is, because it is not the large change that we argue is needed!

16. We do not agree. Paleo data reveal numerous cases of rapid 1-5 m sea level rise, within a century. Thus the arguments that ice streams only allow such and such a flux and will then slow down don't seem to hold much water. In any case, as R1 has suggested, we make clear at the beginning that this is our hypothesis and then we look at what the consequences would be for such a melt rate.

17. The climate forcings that we define in that paragraph are from a peer-reviewed, highly-referenced paper. The −15°C issue is dealt with in detail elsewhere in our revisions, as noted above. Specifically it is related to the heat of fusion matter.

18. Those two sentences, occupying only two lines, are useful at that point because that is the point at which the reader wants to know what is in the section, and it is helpful to know how it is organized. If we moved them way back to the beginning of section 3, they would be forgotten by the time the reader gets to this section.

19. O.K., we agree that injected "into" may be better – we changed it.

20. This is not the place for the caveat – it goes earlier. In fact, lots of feedbacks are allowed. Feedbacks are what we are interested in investigating! What is not allowed to change is the growth rate of freshwater input because we set that in our original hypothesis – which we make very clear.

21&22. We experimented with many color scales, for years, choosing one that allows quantitative clarity. Many people think that our figures are very clear. No doubt some people do not, but the color scales are well thought out and we do not want to revisit the horse in the middle of the stream. It is true that some people may not look in the figure title for the units, so we have added those to the caption. We cannot put them on the color scale itself without adding a line, making all figures taller and the paper longer, which is the last thing we want to do. However, the units are now very clear.

23&24. The present location of these paragraphs is best for the sake of understanding this complex topic.

25. Some people might suggest that this distinction is splitting hairs, as the CMIP studies are designed to feed into the IPCC conclusions. However, as noted above, we have now changed our description so that we directly credit the appropriate CMIP simulations.

26. See response to 21&22.

27. We would indeed like to encourage other modeling groups to make analogous simulations, and the way for us to do so is to publish this paper. It took us years to complete this study, and to suggest that we start over and do it again with another model is an unreasonable requirement. We are presenting results that make physical sense, that predict effects that seem to be corroborated by paleoclimate data, and which make predictions for the present and near future that can be judged against observations.

28. Units have been added into the caption. The rationale for limiting the number of modeling figures in the main text is that there should be some balance in number of figures among (1) modeling, (2) paleoclimate, and (3) modern data. We have already pushed it in the modeling direction as far as we think is reasonable. We do recognize that many people may not get into the Supplement, but the main paper is already very long, and we doubt that most people are interested in this figure.

29&30. Yes, the figure numbers in the caption were mis-numbered – thank you for catching that.

31. The −15°C is actually a very conservative estimate for the heat of fusion effect of ice. If the "freshwater" is 100% icebergs the correct temperature to use is near −80°C!

32. The long NADW recovery time is well understood, achieved by all the better models, well documented by numerous shutdowns documented in the paleoclimate literature, and discussed elsewhere in our paper. The quick recovery time of the SMOC is the simple response to the change in vertical stratification. R3 suggested that we add a brief discussion of the stratification effect, which we have done on page 3; we also clarified the discussion with the diagram of ocean circulation (Fig. 18).

33. Agree that the forward reference is not essential, so we dropped those lines for the sake of brevity.

34. Some foreshadowing is useful here, but we have reduced the three sentences to one sentence and incorporated it into the prior paragraph.

35. The section describes modeling with more realistic forcings than were employed in the prior section and the title needs to make that clear.

36. We have changed "on" to "into" as suggested.

37. "remarkable" is an appropriate adjective, given the precision which sea level change must be measured to achieve it, but we have omitted the adjective

38. Thanks for catching that. The intention was to refer to the climate forcing figure in the Supplement. That has been corrected – it is Fig. S16.

39. This hypothesis, as discussed above, is well justified in the manuscript.

40. Yes, one of the section numbers was incorrect, but has now been corrected. We have changed "…modern observations show that these feedbacks are already underway" to "…modern observations suggest that these feedbacks are already underway."

41. The figure was drawn by the first author and requires no citation.

42. "Stimulate" is a correct adjective, but to make it clearer we have substituted "engender".

43. Assuming that the meaning of that statement might be unclear to other readers we have redrafted it to the following. "Frieler et al. (2015) note that 35 climate models are consistent in showing that warming climate yields increasing snow accumulation in accord with paleo data for warmer climates, but the paleo data refer to slowly changing climate in quasi-equilibrium with ocean boundary conditions."

44. These two sentences require only two lines and are important to help guide the reader as to what we are doing.

45. Agreed. We have rewritten the paragraphs accordingly.

46. See above discussion of color bars and locations of units.

47. The underlining is for emphasis. Exposure of this feedback is a principal result of our paper, so if the data suggest that the feedback is underestimated (compared to observations) that is worth drawing attention to. We have changed to italics, but of course will go with the journal style guidelines.

48. Such a project is beyond the scope of this paper and would substantially delay it. Any reader who is interested can compare the figure in our Supplement with the published figure of Marshall, for which we provide the reference and the figure number.

49. A good alternative word is “wrings”, which we have substituted.

50. This is a crucial conclusion from our analysis, usefully foreshadowed here.

51. The suggested meaning is what we intended, but we have reworded to make it even clearer. We have also added a sentence to that paragraph noting the IPCC chapter on cryosphere observations (Vaughan et al., 2013) contains valuable discussion on nonlinear ice sheet processes that could accelerate ice sheet mass loss, but which are not included in current ice sheet models. The latter sentence is in response to the useful suggestion SC C5493 (Dr. Colgan)

52. This discussion has been modified for clarity, as suggested by another referee, including the information from the new paper of Watson et al. (2015). We make clear that we are not suggesting any change points, so there is no need to test such for significance.

53. The bias corrections to satellite data by Watson et al. (2015) remove any significant differences during the satellite-era between the satellite data and the tide gauge analyses, thus eliminating any need for discussion of that difference. So we have removed Fig. S19 and the sentence in the text referring to it. Whether the bias correction of Watson et al. is accurate probably will be debated by other experts in analysis of satellite data, as Watson et al. are essentially calibrating satellite data with the tide gauges. However, the differences between the Watson et al. and Univ. Colorado analyses are not large enough to materially affect our paper, so for the sake of reducing our paper length we stay out of that discussion.

54. In fact that is what the sentence is intended to say, but since such was not clear, we have reworded it.

55. That is correct. We have eliminated the sentence here, because the same topic comes up again (e.g., see #57), where we are careful to say that the data are consistent with a broad range of time scales.

56. The circum-Antarctic cooling is suggested by data, as well as the model, so that is clarified. Mentioning implications of modeling is not inappropriate anyhow, because strict segregation of modeling and observations to separate sections is not helpful for increasing our own and the reader’s understanding.

57. Yes, the data record is too short and “noisy” to infer a time scale for change. We only meant to say that it was not inconsistent with a decadal rate of change, but it is right that we should say that it also allows longer time scales of change, so we now say that explicitly.

58. We mean both Greenland and Antarctica, so we now say that.

59. Same as #57 above, i.e., we agree and have clarified.

60. Greenland comments have been consolidated into earlier paragraphs on Greenland. We have eliminated the other sentence here, noting elsewhere that longer response times are not inconsistent with the data.

61. This section involves a comparison of models and observations, not simply observations.

62&63&67. IPCC has been changed to CMIP where appropriate.

64. No, this model was not involved in CMIP. Ocean circulation is fundamentally improved in our model, including specifically in areas important to our paper. For example, the model you refer to had AABW formation in the middle of the Southern Ocean (like most of the models). Our model forms deepwater along the Antarctic coast, as in the real world – we include the relevant model diagnostics in the paper and in the Supplement.

65&66. Low variance before 1980 is not very obvious in the figure. Nevertheless we undertook the work to update the data through 2015 and to calculate results using a new base period starting in 1979. This has the merit of making the base period the same for SST and sea ice, which is recompense for the work of doing this. It turns out that the updates and change of base period do not alter the nature of the model and data comparison or the interpretation, but in the text we have noted the reason for the choice of period, with reference to Huang et al.

68. model → simulations

69. There are no appropriate observations that we are aware and it is not a project that we can undertake.

70. That is not correct – Southern Hemisphere sea ice area decreased in 2015, but only briefly hit the long-term mean. La Nina occurrences are not unusual – we showed the relation of sea ice cover to El Nino/La Nina variability in response to an SC. There is a trend of increasing sea ice with some indication of an El Nino/La Nina variability superposed on the increasing trend.

71. We do not have time for projects. The question of how well the model's AMOC strength agrees with observations is noted elsewhere. Here we are dealing with the question of future AMOC shutdown or substantial slowdown, not questions of how well the model's basic AMOC strength matches the real world AMOC – we showed earlier in the paper that it does a good job.

72. Fig. 24 has been updated with 2015 temperatures

73. the discussion in lines 900-903 refers to the AMOC, not to the global warming hole temperature

74. "achieve" changed to "reach"

75. making these changes would make it harder for the reader to understand the climate system and our paper – a lot of insight follows from the cross-referencing with knowledge from paleoclimate

76-128. We examined all of these and made changes in response to about one-third of the suggestions. In general we did not agree that proposed moving around of sections or sentences, which, it seemed to us, would make it harder for the readers to understand the paper or gain insights about the climate system. However, many of the suggested changes were useful and were used.

Finally, we want to clarify that we are not in general critical of IPCC, on the contrary, we appreciate the huge amount of work that IPCC scientists have done to produce scholarly state-of-the-art volumes. We have the highest regard for the quality of the IPCC work, the most recent set of reports reaching such a high level of scholarship that researchers now include these tomes as primary references. Clearly an enormous amount of work went into these documents, and they could only have been produced by outstanding researchers who have earned everyone's gratitude.