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# ***Interactive comment on “Ice melt, sea level rise and superstorms: evidence from paleoclimate data, climate modeling, and modern observations that 2 °C global warming is highly dangerous” by J. Hansen et al.***

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## OVERVIEW

The authors use a mixture of palaeoclimate data evidence, direct observations, and climate modeling runs to make inferences about past and then potential future changes in atmospheric circulation, sea level and storminess under a business as usual emissions scenario. They argue that 2 degrees should not be seen as safe. In many senses the study should be uncontentious and does not challenge in a fundamental manner

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several pre-existing paradigms.

It is very well understood that during stage 5e global sea levels were several metres higher than present and that the surface temperatures globally were somewhat warmer than today (although almost certainly no more than 2oC above Pre-Industrial). It is therefore all but certain that we are committed to several metres eventual sea-rise unless surface temperatures are stabilised rapidly. The question really is how this sea level increase will occur, with what atmospheric and oceanic consequences, and over what timescale.

It is also well known that, especially during glacial to interglacial transitions, there are relatively short-lived events of rapid change in climate and sea-level relating to rapid freshwater discharge into the northern Oceans from land-bound ice masses. The question here is whether with an interglacial ice-sheet configuration as at present day such event types can occur either from Greenland or Antarctica or whether they are solely a result of inherent instabilities in those great ice-sheets that are transient features between glacials and interglacials. These presently absent ice sheets are obviously much more prone to collapse given their repeated complete appearance and then disappearance over the past 2 million or so years.

Hence, where the potential contention arises is in the assertion that we may lie close to or already have passed effectively a tipping point in the present-day cryospheric components of the climate system which presages a period of large scale and rapid changes in sea-levels, ocean and atmospheric circulation and storminess. To make this conclusion relies to an uncomfortable extent upon a causal chain of the nature given a then b and because b then c and c means that d shall occur etc. Each link in this chain is certainly plausible based upon the relatively scant evidence to hand, but is not by any stretch determinant. At each link there is a finite probability that that link will not actually be realized. Given the length of the causal chain and the reliance upon in particular:

- i) very sparse palaeo-records which may have multiple plausible interpretations and;
- ii) a single coarse resolution model with effectively hosing experiments which have

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been variously criticized elsewhere for lack of realism and which the authors at various points recognize as being potentially biased / oversimplified, it is far from certain that the results contended shall match what will happen in the real-world.

At the same time, however, it would be foolish in extremis to discount this out of hand as a possibility of what shall occur. I fully concur that the 2 degrees limit should in no sense be seen as safe. We do not know enough about the earth system as a whole to make such a determination and I share many of the authors' manifestly obvious concerns about its use as a target; and have done since it was agreed and adopted. The precautionary principal should sensibly be applied aggressively without 2 degrees seen as some safe handrail that we can walk up to. Human civilization has flourished during a brief climatic period that in the geologic context appears remarkably stable and we risk deliberately moving away from that state through our historical and current actions.

In that context there is certainly a place for exploration of potential high impact event outcomes. Sadly, the only tools to hand that enable any meaningful insights are palaeo records and models no matter how imperfect each of these tools are.

However, as Carl Sagan popularized "Extraordinary claims require extraordinary evidence". It is not clear to me that the findings described herein raise to the status of extraordinary evidence. In my view further analyses are required to reach such a point.

I therefore find myself conflicted over a recommendation as to whether to publish in the full ACP journal or not. Hence I make no explicit recommendation at this time. I may, based upon the multitude of comments received, come back with a firmer recommendation nearer the conclusion of the review period. I do, however, make a number of comments herein.

#### PUBLICITY PRIOR TO PUBLICATION

Before going on I will declare my personal discomfort at the paper being openly and

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actively publicized before the discussion period is complete. In my view this has caused issues over a fair and open review.

First, it has solicited a large number of informal reviews, the majority of which frankly are off-topic and will serve to distract the editor and authors in coming to a determination on the paper. Arguments of the existence of the greenhouse effect in the long thread initiated by Nabil Swedan clearly are off-topic and should, in my view, not require response. There are better places for such descriptions such as basic textbooks and more germane places for such discussion than an EGU journal paper review. Addressing these in the redraft would be a significant diversion and adversely affect accessibility of the paper.

Second, if the review process yields large-scale changes the version on record shall, inevitably, be at variance to the version that was publicized. This represents to my mind risks both to the authors and the journal.

I have undertaken the review outside this context but it would be remiss not to declare these concerns. These are concerns which I am on the record as expressing on social media anyway. I declare this here in the interests of full transparency.

## SUITABILITY FOR JOURNAL

Clearly suitability for the journal is a determination that is in the end purely editorial in nature. However, I would note a couple of interlinked points that the editor may wish to note in making a determination on this aspect from my perspective as a reviewer.

### 1. Paper length.

The paper is of inordinate length closer to a thesis than a scientific paper in nature. To some extent in the EGU journals this doesn't matter. It is also a welcome antidote to the short letters journal where 90% of the content is shoved to obscurity in SI. So this is far from a criticism per se. However, the paper takes a long time to read even cursorily and much longer to cover in the depth required to even start to fully grasp it.

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Even after multiple readings it is far from certain that I have grasped all aspects and I strongly suspect additional reviews will highlight points I have thus far missed as a consequence. This is a serious issue if the intent is for the paper to make sense to the readership.

The paper length could be addressed by aggressively editing to avoid repetition. I found myself with a distinct feeling of déjà vu on a number of occasions as topics were repeated often saying pretty much the same thing. Reassessing the paper structure in such a way that each topic is ideally raised once and repetition avoided to the extent possible would undoubtedly help to create a more concise and readable paper. Section cross-referencing should be used liberally in lieu of repetition of points to reduce the paper length and make it more readable and accessible. In particular I find the two substantive segments on the palaeo-record an odd. There is substantive repetition therein and I wonder whether an alternative structuring is possible to avoid this.

## 2. Fit to journal remit

I applaud the choice of an Open Access review and publication ethos and am a strong champion for the EGU's publication model. Obviously, however, it is in both the journal's and the authors' interests that the paper appear in a suitable journal.

For the journal publishing something somewhat out of scope risks a perception of mission creep with all that entails in future viz. submissions and editorial workload.

For the authors in terms of long-term readership and use it is important it appears in the most relevant journal where it will accrue most reads and subsequent challenge / confirmation studies to build confidence in what the authors conclude.

The ACP journal remit is described thus: "The main subject areas comprise atmospheric modelling, field measurements, remote sensing, and laboratory studies of gases, aerosols, clouds and precipitation, isotopes, radiation, dynamics, biosphere interactions, and hydrosphere interactions (for details see journal subject areas). The

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journal scope is focused on studies with general implications for atmospheric science rather than investigations that are primarily of local or technical interest. The manuscript types considered for peer-reviewed publication are research articles, review articles, technical notes, and commentaries/replies.”

At the same time Climate of the Past remit is described thus: “The main subject areas are the following:

- \* reconstructions of past climate based on instrumental and historical data as well as proxy data from marine and terrestrial (including ice) archives;
- \* development and validation of new proxies, improvements of the precision and accuracy of proxy data;
- \* theoretical and empirical studies of processes in and feedback mechanisms between all climate system components in relation to past climate change on all space scales and timescales;
- \* simulation of past climate and model-based interpretation of palaeoclimate data for a better understanding of present and future climate variability and climate change.”

Given this I am not convinced that ACP is the correct place for this paper within the EGU stable of journals and it may be better suited to consideration at Climate of the Past where the palaeo components of the paper may also logically receive a more in-depth expert review which would help to ensure the scientific verity of the piece. Alternatively, the paper could be split across the two journals with the palaeo-evidence in a paper in Climate of Past and the modelling and dynamical aspects remaining here.

However, overall I am minded to recommend that the final bullet for Climate of the Past remit suggests that that journal would be a substantively better fit for this paper than ACP. Perhaps the paper could be transferred? Or perhaps Climate of the Past editors could be asked to nominate a couple of reviewers to ensure an in-depth expert assessment of the palaeo-evidence sections?

At a minimum the authors should explain why in their view this paper should be considered here and not there.

## THE 'WHAT NEXT?' QUESTION

The authors present an undoubtedly contentious possible future based upon a relatively small set of palaeo evidence, emerging direct observational evidence, and a single coarse resolution model using a fairly simplistic hosing experiment. That leaves a glaringly obvious question as to what next steps by the scientific community may reasonably be expected to improve our knowledge and either confirm or refute the authors' findings. In my view it is beholden on an investigator who suggests such a contentious position to at least address this aspect in some depth in their manuscript.

That said, clearly the authors are not in a possession of a crystal ball and cannot prescribe exactly what is required / should be done. However, I would expect a substantive discussion section in a paper that raised such a potential paradigm shift. I would expect that discussion to highlight remaining gaps in knowledge and capabilities and in a broad brush sense what sorts of studies / advances / modeling innovations / ice sheet modeling innovations etc. should be pursued so as to address the issue more fully. The lack of such a discussion section sits uneasily. Especially so given the considerable author cast who could, easily, provide a substantive and useful discussion section that really tried to give a sense for what 'what next?' may actually entail. I find that without such a section the paper feels incomplete and not as useful as it should be.

There are in places in the text segments that would logically sit in such a discussion. These should be collated and augmented in a stand-alone section just before the conclusions.

## PRESENTATIONAL STYLE

Overall I found the writing style quite accessible. However, there was a tendency in several places to editorialize. By that I mean that in places the paper tends to read

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somewhat more as a blog post or advocacy piece than a scholarly paper. I fully recognize that this is perhaps a matter of primarily personal taste. However, I would prefer the passages that read in the manner more akin to a blog or advocacy to be tightened up and written in a manner more akin to a traditional paper. I believe that this would better suit the journal as well as leaving less obvious points at which critics can raise (quasi-)legitimate concerns regarding the paper contents. In particular I see the current summary section as being unduly about policy rather than recapping and substantiating the principal findings. That section in my view is out of scope as written. That is not to say the points it raises are unimportant. Similarly, some aspects of the abstract tend towards this issue.

## SCIENTIFIC QUERIES / CONCERNS

### 1. Sea-level and storminess indicators in the palaeo-record

The authors use evidence of high stands, large wave deposits, and apparently marine mediated boulder placement high above present sea-level (Bahamas only) in two Atlantic Ocean locations Bermuda and the Bahamas during 5e as the observational basis for their assertion regarding sea levels and storminess. I have several queries here.

I concur with the authors that in both the Bahamas and Bermuda isostatic effects are either non-existent or sufficiently small to be ignored. However, eustatic change is the summation of both global volume and basin-scale dynamical effects. If, as the authors suggest, the 5e climate was typified by a movement of the Bermuda high to the north or north east then I would expect the local dynamical eustatic component to yield an increase in sea-level at these locations that may substantively enhance the global-mean volume change mediated component. Presumably in locations in the East Atlantic the opposite would be true and the sea-level change reduced compared to the global mean. The effect should be calculable at least to an order of magnitude estimate and I would like to see in the redraft an attempt made to ascertain the possible dynamical

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component of the local eustatic sea level change in these locations. Firstly, this will help to better elucidate the global implications and secondly in terms of planning were such a scenario to eventuate it would highlight the need for local actions that are more nuanced than would be implied by a single global sea-level rise number.

The authors present strong evidence that both the chevrons and the boulders' movement and deposition are marine mediated. Indeed, it is hard to envisage a non-marine mediated deposition mechanism in this location. However, I am unconvinced that the sole possible source of either feature is very substantial storms. It is entirely plausible that the rocks and chevrons could have been deposited by one or more tsunamis. It is well known that several Atlantic islands are prone to large landslides that may mediate tsunamis with a fetch from the north east at the location. I disagree with reviewers who have pointed to an ice sheet calving mediated tsunami as the direction is likely wrong and also the Greenland ice-sheet at present day (and therefore presumably 5e ) is unlikely to collapse directly into the ocean in sufficiently large chunks as it is largely land-bound with glacial outflow.

Perhaps there is evidence in the Azores, Cape Verde or Canaries for substantial landslips during 5e that could have caused tsunamis? This is an area where recognized expert input would be beneficial to either rule-in or rule-out the possibility. It is not an area where I have the requisite in-depth knowledge of the available palaeo-evidence.

Regardless, given the size of the boulders, the typical density of the material, and the possible nominal sea-level it should be possible to calculate the mechanical energy that would have been required to move the boulders from the local 5e sea-level stand to their current location and elevation. Furthermore it should then be possible to calculate the relevant significant wave height given the embayment characteristics that would have been required and from that the wind speed / fetch combination that would have been required. Clearly, if that windspeed is beyond a plausible windspeed of the strongest possible hurricane then it points to a tsunami-mediated deposition.

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The bottom line here is that I believe based upon simple physical calculations and recourse to experts in palaeo-tsunamis in the Atlantic it is possible to much better elucidate firstly whether these boulders were deposited by storms or tsunamis and then if they were storm deposited to make a quantitative estimate of the storm strength. So, further work here would in my view yield undoubted benefits. I am unconvinced that the absence of evidence of tsunamis on the US Atlantic seaboard rules out the tsunami mechanism. Absence of evidence is not the same thing as evidence of absence.

## 2. Use of a single coarse resolution model and a single set of hosing experiments

I find the use of a single coarse resolution model and single hosing experiment approach, even if repeated with varying and invariant GHGs, a substantial overall scientific weakness.

The paper would be substantially stronger if the model results were able to be replicated with at least one additional independent model. At the coarse resolution used so much is dependent upon sub-grid scale parameterizations. Furthermore as the authors themselves state in several places the model has recognized inadequacies in both the ocean and atmospheric domains. These may or may not affect the adequacy of the model to simulate the relevant processes.

Because of gross inadequacies in the cryospheric components and assumptions of the model, hosing experiments are required. These hosing experiments spread freshwater over large domains of the North Atlantic and the Southern Ocean in the model. It is unclear how representative of what would happen in the real-world such experiments are and I am not convinced that this aspect has been adequately covered in the submitted draft. In the real-world freshwater inputs will tend to occur as almost point sources from outlet glaciers or at worst regional inputs from floating ice shelves. It is unclear to what extent the real-world oceanic mixing processes would then result in a freshwater surface plume spreading over the oceans' polar gyres leading to a widespread and persistent local cold SST anomaly. Given the import of the SST anomaly fields

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and sea-ice response in driving the atmospheric circulation and storminess response posited in the analysis I see this as a potential weak link in the causal chain.

If the model is instead forced with more local fresh water hosing does it yield a distinct response? Is this response more or less consistent with the available palaeo-evidence? These and other questions could and possibly should be answered both by further experimentation with the current model and the use of at least one additional model. Such further experimentation would help to build confidence in the authors' findings.

### 3. The physical basis for ice sheet melt doubling rates

The authors make some simplified assumptions that the rate of ice sheet mass loss can be approximated by a doubling every  $n$  years. While this may indeed be possible there is no robust underlying physical basis given at the outset (the justification is left to a few lines in Section 3.2 with no forward reference to Section 7.3). Specifically it would be useful to know from where on the great ice sheets and how such a melt could, plausibly, occur at this point to justify the assertion. The stopping melt at a certain point is also unrealistic as the authors themselves admit.

It is not in my mind sufficient to assert that recent behavior can be extrapolated forward more than a very finite time as a predictor for the future. If the rate is really likely to increase quasi-exponentially some more physically based rationale would be warranted as to how this will be realized. There is recent evidence that at least some of WAIS may now have passed a tipping point with the question being when and not whether it melts out. What other evidence is there that can build confidence in the plausibility of ice sheet loss rates increasing quasi-exponentially at least in the short term and therefore the realism of the applied freshwater fluxes? I see a nice discussion in Section 7.3 but it does not directly address the realism of the model prescribed fluxes to the extent I would expect. Personally I would place the evidence basis before describing the modeling approach so it is justified a priori.

### 4. Modern instrumental evidence

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There is certainly robust evidence for an increase in mass loss from the Greenland and Antarctic ice sheets. This aspect of the discussion of modern era records is almost certain to be correct.

Evidence, however, becomes more ambiguous with respect to locally decreased SST and expanding Antarctic sea-ice and arguably sub-surface ocean measurement is sparse and should be treated with caution.

Since the paper was submitted Antarctic sea-ice anomalies have turned negative after a period of four years around +1 million square kilometres extent anomalies. This may simply be temporary. However, it does yield questions as to whether the recently observed increase in Antarctic sea-ice coverage has been indicative of a long-term trend process or simply natural variability. At the very least the record is too short and arguably the mechanisms too poorly understood to place much credence in it. At a minimum therefore the discussion needs to be recast so as to be less certain of the observational support for the posited sea-ice mechanism until it is much clearer whether there is in fact a long-term trend to increase underway in the real world climate system.

Similarly, the seas south of Greenland are anomalously cool relative to a mid-to-late 20th Century climatology. However, as shown in the recent ERSSTv4 analysis by Huang et al (caveat emptor applies) virtually the whole instrumental record is anomalously cold relative to this period. Certainly the recent anomalies are not unusual or unprecedented in the context of the record as a whole. Rather it is the climatology period which is unusual in the longer-term context. I therefore see little observational support that the SSTs south of Greenland are truly anomalous relative to 1880 to date.

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 20059, 2015.

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