Clim. Past Discuss., 8, C1095–C1106, 2012
www.clim-past-discuss.net/8/C1095/2012/

Climate of the Past Discussions

Interactive comment on "Quantification of the Greenland ice sheet contribution to Last Interglacial sea-level rise" *by* E. J. Stone et al.

P. Applegate (Referee)

patrick.applegate@natgeo.su.se

Received and published: 13 August 2012

Review of Stone et al., submitted 2012, Quantification of the Greenland ice sheet contribution to Last Interglacial sea-level rise: for publication in Climate of the Past.

Review by Patrick Applegate, Pennsylvania State University

I have a potential conflict of interest in reviewing this manuscript, but feel I can still offer useful comments. Dr. Stone and I are co-authors on a recently-published paper in The Cryosphere (Applegate, P. J., Kirchner, N., Stone, E. J., Keller, K., and Greve, R., 2012, An assessment of key model parametric uncertainties in projections of Greenland Ice Sheet behavior, The Cryosphere 6, 589–606). However, I have not heard about the present paper before now, and was not involved with the study. The editor

should weight my comments accordingly in evaluating this manuscript. Nathan Urban (Los Alamos National Laboratory; nurban@lanl.gov) or David Pollard (Penn State; pollard@essc.psu.edu) would be excellent alternate reviewers.

Stone et al. use a combined climate-ice sheet modeling exercise to make a probabilistic estimate of Greenland Ice Sheet contribution to Eemian sea level rise (130-120 ka). They cull their ensemble based on the presence or absence of ice at central Greenland ice core sites, and by simulated modern ice geometry. The authors settle on a range of GIS Eemian sea level rise contributions of 0.6-3.5 m (10-90% range).

This manuscript combines two important branches of Greenland ice sheet modeling studies. Most past studies used just one simulation each, based on best-estimate model parameters, and the climate forcing came from pattern scaling of present-day climate fields, using ice core-inferred paleotemperatures to do the scaling. More recently, some studies have run perturbed-physics ensembles with ice sheet models, following long-established practice in the climate modeling community. At the same time, other studies have used global climate models to provide temperature and precipitation fields. This study includes both of these improvements.

I have a number of concerns with the present manuscript, and some of these concerns may require additional analyses to address. Thus, I recommend "accept after major revisions" for now. The authors are acknowledged experts on ice sheet and climate modeling, and may very well have already considered these issues. In that case, additions to the text may be sufficient to address these potential problems.

My concerns have to do with 1) the equilibration properties of the coupled atmosphere-ocean climate model simulations, 2) the technique used for spinup of the ice sheet model, 3) the statistical methods used to extract a probabilistic estimate of sea level rise contributions from the ice sheet, and 4) a possible neglect of important constraints from sea level fingerprinting studies. I also recommend 5) significant reorganization of the text and figures.

/— Climate model equilibration —/

The authors use a 200-yr period to spin up their GCM. The first 100 yr is based on orbital parameters, greenhouse gas concentrations, and Greenland ice sheet topography appropriate for the preindustrial period (perhaps 1850). The second century of each GCM run is based on conditions appropriate for different times during the Eemian warm period.

My concern about this spinup method stems from a recently-published study on GCM equilibration by Brandefelt and Otto-Bliesner (2009, Equilibration and variability in a Last Glacial Maximum climate simulation with CCSM3, Geophysical Research Letters 36, L19712). These authors discovered that an apparently-equilibrated Last Glacial Maximum simulation with the AOGCM CCSM3 was actually far from equilibrium. Their results suggest that about a thousand years of model evaluation was required to reach a "true" equilibrium. The long equilibration time was associated with the response time of the model ocean. The difference in Greenland summit temperatures between the apparently-equilibrated and fully-equilibrated model states is 6-10 deg C.

This earlier study suggests that a century of Eemian spinup might not be sufficient to achieve a good climate model state to feed into the ice sheet model, and that the temperature errors involved could be substantial. Naturally, the Eemian is a lot closer to the present-day state than the Last Glacial Maximum, so the errors are likely to be smaller than those noted by Brandefelt and Otto-Bliesner (2009). But, I think some demonstration that the GCM runs really are equilibrated, or that the errors associated with poor equilibration are likely small, is absolutely needed for the paper.

One way of partly addressing this concern would be to adjust the y-axes on Figure 3. This figure is clearly important, because it shows the temperature trajectories of the GCM runs, but it doesn't allow a reader to determine whether the runs have properly equilibrated. A lack of equilibration would be indicated by a consistent, nonzero slope in global mean temperature near the end of the run. Please rescale all the y-axes so

that they conform more closely to the y-extent of the temperature results in each panel. In column 1, I suggest using a y-extent of -23 to -20 deg C; in column 2, -22 to -17 deg C; and in column 3, -19 to -12 deg C.

Also in Figure 3, fit a straight line through the temperature values for the last 30 yr of each climate model run (the part of the run that is actually used for ice sheet model forcing). Plot these lines on the figure, and report the slopes in a table (or on the figure).

The authors should also look at the abyssal ocean temperature trajectories, as did Brandefelt and Otto-Bliesner (2009). Including these trajectories in Figure 3, in separate panels, would be most helpful.

/— Ice sheet model spinup —/

Rogozhina et al. (2011, On the long-term memory of the Greenland Ice Sheet, Journal of Geophysical Research 116, F01011) have recently published a study on different ways of spinning up ice sheet models. It is a bit difficult to establish from Rogozhina et al. (2011) what are "right" and "wrong" ways to spin up ice sheet models, but Stone et al. should provide some discussion of the paper and its possible implications for their own results. Figure 4 seems especially relevant in this context; here, Rogozhina et al. (2011) begin their simulation with an equilibration run under constant climate, then apply a time-dependent forcing. This method is at least broadly similar to that used in the present ms. One crucial difference is that Rogozhina et al. (2011) spin up to a glacial state, whereas Stone et al. spin up using interglacial conditions. From my own work, my sense is that ice sheet simulations tend to converge rather quickly during warm periods, regardless of initial state.

/— Statistical methods, bias, and overconfidence —/

This study has the laudable goal of making probabilistic estimates of Greenland contributions to Eemian sea level change. Many probabilistic estimates, of any quantity, neglect important factors that make the inferred pdf too narrow (that is, overconfident) and shift it to one side relative to the "true" answer (bias). This study likely has similar problems. The ms needs a fuller accounting of sources of bias and overconfidence in the results, particularly in the abstract, but also in the discussion.

Here are some potential sources of bias and uncertainty in the results.

– Eemian summer temperatures from the GCM. If I'm reading section 4 correctly, the simulated Greenland surface temperatures are about 1.5 deg C too cool. In that case, the simulated Greenland contributions will be biased toward too-small values.

– The model is naturally imperfect, leading to structural uncertainty. Stone et al. (2012) acknowledge some of these problems very briefly, especially in the discussion. One possibly serious problem is related to a lack of basal sliding in the ice sheet model. In a previous paper with some of the same authors (Stone et al., 2010, The Cryosphere), the ice sheet was "glued" to its bed. The authors don't mention basal sliding in the present paper, making it hard to determine whether this ensemble has the same problem. If present, this flaw will tend to lead to too-large ice volumes, and maybe to too-small Eemian sea level rise contributions. Please acknowledge the lack of basal sliding, and the fact that other models of the same class do include this process, in the text.

– The use of root-mean-squared error on gridded modern ice thicknesses to evaluate the fidelity of model runs to the modern ice sheet conditions (Eqn. 7) will cause bias and overconfidence, because of spatially autocorrelated residuals due to model structural error. Because of the lack of certain processes in the ice sheet model (e.g., ice streams, basal sliding), the differences between the observed and modeled ice thicknesses in adjacent grid cells will be highly correlated with one another. For example, the real ice sheet has one very large ice stream, but the simulated ones won't, and the ice thicknesses of grid cells over this ice stream will be consistently in error. If this problem isn't accounted for in the choice of objective function, you effectively think you have more information than you have, and the results will be biased (wrong) and

overconfident (you'll be very sure about your wrong answer). I would suggest using an aggregate metric, like ice volume, to assess the match to the modern ice sheet.

– I don't fully understand how the information from the modern ice volume is being taken into account. I think it would be more standard to define some uncertainty about this modern ice volume estimate and use that to determine which runs are most consistent with the modern ice volume, perhaps using a normal distribution as a weighting function. What is the absolute difference in volume between the estimated modern ice volume and the model realization that produces the smallest ice sheet that is consistent with the Summit/NorthGRIP Eemian ice presence constraint?

/— Sea level fingerprinting studies —/

The authors cite Bob Kopp's 2009 study that estimates Eemian sea level change. On p. 866, Kopp et al. write, "The posterior distribution suggests a 95% probability that both Northern Hemisphere ice sheets and Southern Hemisphere ice sheets reached minima at which they were at least 2.5m e.s.l. smaller than today, although not necessarily at the same point in time (Fig. 5, dotted line)." If applied to the present study, this constraint would wipe out about two-thirds of the allowed range of sea level rise contributions identified by Stone et al. At minimum, this point needs to be mentioned in the discussion, if the constraint is not actually included.

/— Organization of the paper —/

At present, the manuscript is divided into many sections that mix methods and results. This organization makes it difficult to find crucial details. I would prefer to see the classic introduction-methods-results-discussion, with the methods and results sections each divided into ice sheet modeling, climate modeling, and statistical methods subsections. As a side benefit, this reorganization might help make the paper shorter.

The number of figures in the paper is very large; could the authors consolidate some figures and cut others? Figures 8 and 11; 10 and 14; and 12 and 13 could be combined.

/— Comparison to other methods of ice sheet model forcing —/

Many other studies have used simple pattern scaling of modern climatology fields to estimate climate forcings for the past. I would be very interested in the authors' ideas on what errors we should expect to see in Eemian simulations driven by ice core forcings, instead of results from GCMs. This material could easily go into a subsection of the discussion.

/— Detailed comments follow —/

Page 2

Lines 1-18: Remove all inline (parenthetical) references from the abstract.

Lines 1-3: perhaps, "During the last interglacial period ($\sim$130-115 thousand years ago, ka), Arctic climate was warmer than today, and global mean sea level was likely >6 m higher."

Lines 4-6: perhaps, "However, there are large discrepancies in the estimated contributions to this sea level change from various sources (the Greenland and Antarctic ice sheets, and smaller ice caps)."

Line 9: what model(s) did you use?

Line 11: what is meant by "model uncertainty?" do you mean parametric uncertainty (as in Stone et al., 2010, The Cryosphere), model structural uncertainty (differences among models or between models and the real world), stochastic uncertainty in the climate model, or all three?

Lines 9-14: change to, "Here, we perform an ensemble... to determine the likely contribution... Our results suggest a 90% probability that this contribution exceeded 3.5 m sle..."

Lines 9-14: your probabilistic estimates are likely to be overconfident and/or biased for one reason or another; mention any important sources of uncertainty or bias that you

neglected here

Line 14: can you say something about *why* your results are different? is it because you used a coupled climate-ice sheet model, and other studies did not?

Line 20: avoid the use of "since" to mean "because," because "since" also means "after" and is sometimes ambiguous

Page 3

Line 9: the word "this" should always be followed by a specific noun; perhaps "this estimated temperature increase"

Line 14: should mention which of the Greenland ice cores contain Eemian ice and which do not (see Alley et al., Quaternary Science Reviews, 2010)

Line 18-24: based on your abstract, comparison to previous studies is a crucial aspect of your work – please make this a separate paragraph and expand, explaining that some studies are "straight" ice sheet modeling studies (Letreguilly et al., 1991; Ritz et al., 1997; Cuffey and Marshall, 2000; Huybrechts, 2002), others are constrained by additional data (Tarasov and Peltier, 2003; Lhomme et al., 2005), and others are coupled climate-ice sheet modeling studies (you cover this point)

Line 18-24: also mention the 2.5 m lower bound constraint from Kopp et al. (2009), which is independent of ice sheet modeling.

Line 25-29: this sentence is extremely long; please break it into several sentences. also provide some more details here: briefly describe why you chose this approach and what you hoped to learn from applying it

Line 29: "ice sheet climate interactions" should probably be hyphenated: "ice sheet-climate interactions"

Page 4

Section 2, 3...: please subsume the relevant sections into a Methods section, and lead this Methods section with a brief description of the relevant methods and how they help you solve your problem

Section 2.1: this section is made up of two very long paragraphs; please break them up and organize according to topic sentences that contain the most important points – a reader should be able to glean all the important points just from reading the first sentence of each paragraph

Section 2.1: much of this detail seems unnecessary; can you simply indicate how the model is different from earlier and later versions of the same model and provide a reference?

Page 5

Section 2.2: for a reader who is not familiar with ice sheet models, this description might be a bit too technical; can you reconfigure this text with a non-ice sheet modeler in mind, or refer to general texts on/introductions to ice sheet modeling?

A higher-order ice sheet modeler (I am not one) would probably insist on a boilerplate description of the disaadvantages of shallow-ice approximation models here. For their benefit, could you include some text on this issue here? Some explanation of why shallow-ice models are still useful would also help. Clearly, one cannot run a full-Stokes model 500 times over thousands of years.

In Stone et al. (2010), the ice sheet model did not include basal sliding; is this process represented in the model version used here? (The string "basal sliding" doesn't seem to be anywhere in the paper.)

Page 6

line 25: again, follow "these" with a specific noun

lines 27 and ff, "Here we generate...:" please move this key methodological detail to

the first sentence of a paragraph; also, this information seems much more likely to be unfamiliar to a reader than how a climate or ice sheet model works – I would suggest deemphasizing sections 2.1 and 2.2 in favor of providing more detail here

Page 7:

line 4: "130, 125, and 120 ka:" as you point out in the abstract, the Eemian lasted until about 115 ka; why did you choose not to do a set of runs at 115 ka? I don't insist that you do these runs, but some explanation of this choice would be helpful.

lines 13-14: see comments on climate model spinup above.

Was parametric uncertainty in the GCM considered, or was this source of uncertainty only treated in the ice sheet model?

line 19, "this:" "this procedure"

Page 8

19, what does "perihelion 2.6 (day of the year)" mean? does this phrase indicate that the perihelion is 2.6* the Julian date?

19, "range of climate:" perhaps "range of climate states" (missing a word here?)

Page 9

11-20: parts of this paragraph about isostatic rebound seem redundant with material on pp. 7 and 8; consider combining/rewriting

21: "appropriated:" delete the "d"

Page 10

21 and following: because this material explains *why* the GCM simulations were done in the way you describe, it should probably be moved to the beginning of this section

Page 12

20-24: if your simulated summer temperature anomalies are too small, won't that make your estimated GIS contributions to Eemian sea level change also too small?

Page 15

1-15: I looked for an answer to this question in the text, but did not find it easily. Where do your modern simulations come from? Did you run the simulations to equilibrium under a modern climatology, as in Stone et al. (2010)? Are the modern simulated ice volumes shown somewhere in the paper? The volumes in Stone et al. (2010) were all bigger than the "real" volume, if I remember correctly.

Page 16

17: "Probabilisitc" is misspelled

Page 22

24: Colville et al. (2011, Science) inferred contributions as low as 1.6 m

Figures

For each figure, please include one sentence at the end of the caption that tells a reader what they should take away from the figure.

Figure 1: I think this figure is a bit uninformative – it shows that the points are distributed through the five-dimensional space, but I can't easily see if there are any gaps in the Latin hypercube (which can occur, depending on the number of points). Please replot as 10 different x-y plots, similar to Figure 1 of Applegate et al. (2012).

Figure 3: see comments above.

Figure 5: Please show these results as a scatterplot of estimated LIG Arctic summer temperatures from paleo-data on the x-axis vs. your model-inferred temperatures on the y-axis. Show also the 1:1 line; if the points lie close to this line, then the model is behaving well relative to the data.

Figure 6: please stack the three panels one on top of another and make them wider; also scale the y-axes to be consistent with the data. The skill-score color scale probably only needs to be shown once.

Please show a histogram of the spinup ice volumes, with lines indicating the modern ice volume and ensemble mean.

In this figure, time runs from right to left, which isn't consistent with Figure 3. Please make the time values on the x-axis negative and switch the x-axes from left to right. This "switching" should also be done for the other figures with time dimensions.

Please show the ice volume trajectories during the spinup period in all the panels. Compressing them so that they don't take up the whole x extent of each panel is OK.

Figure 9: Please show another panel which is the same as this figure, but including only the runs shown in black on Figure 6.

The small star is hard to see, and may be even less visible on the final typeset manuscript. Please replace this star with a vertical red line.

Interactive comment on Clim. Past Discuss., 8, 2731, 2012.