

Interactive comment on “Coupled regional climate–ice sheet simulation shows limited Greenland ice loss during the Eemian” by M. M. Helsen et al.

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Helsen et al. present in this paper the first modeling exercise of the last interglaciation (LIG) Greenland Ice Sheet (GIS) that is forced by GCM climate downscaled through a regional climate model with more precise modeling of the surface mass balance. They find that the GIS contributed ~ 2.1 m to LIG sea level rise (range 1.2–3.5 m). Given the >6 m highstand, an Antarctic contribution is also required.

I think this is a great paper that should be published, but it can also be improved by the authors. Most notable is the absence of a discussion of the implications of their study. Since this is the most “thorough” attempt at modeling the LIG GIS, I think

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including such implications would greatly improve the paper beyond what is at present a thorough modeling paper. I outline these below.

The authors should add a section on why they get different results than previous modeling experiments. They mention that previous attempts used a single forcing or PDD methods downscaled from a GCM, but I think further discussion is needed if the reader is to believe their approach is superior or “more right”. One could argue that layering models on top of models propagates uncertainty in each model and so such a discussion would improve the impact of the papers results.

The authors do not discuss the impact that GIS runoff would have on ocean circulation. What is the Sv discharge? What is the total runoff (i.e., not sea-level rise component but the total freshwater flux to the ocean, which would be larger)? How does this compare with climate simulations that suggest small amounts of LIG discharge from the GIS or the Arctic could cause local ocean cooling near the GIS (Cottet-Puinel et al., 2004; Born et al., 2010; Sanchez-Goni et al., 2012)? Indeed, a paper the authors should look at suggested cooling of the Labrador Sea through much of the LIG from GIS melt (and increased Arctic freshwater export) that also may have reduced deep overturning: Winsor et al. (2012, G3). How would their results be impacted by such a local feedback? Would this then make their retreat results a maximum estimate if this feedback was included? There is a hint of this with their varying the melt rate of marine margins, but this could be further developed in light of these studies.

Similarly, I think it would be great if the authors would include a figure or two of the regional climate model anomalies. Such high resolution simulations for an important region during the LIG would be a great addition to the paper. I note this because I think the authors could better discuss some of the complexities of LIG climate in this region. First, the CAPE project glossed over much of the LIG nuance and showed negative anomalies (e.g., work by Bauch and others) as no change. Similarly, they used many records that do not have chronology beyond being assumed to be from the LIG. Where they fell in terms of timing in the LIG was not considered. Records that

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had a chronology were also still lumped together into one time period. There are many regions of the North Atlantic that were no warmer than present/the Holocene during the LIG, including regions close to Greenland (Bauch et al., 1999; 2012; Van Nieuwenhove et al., 2011; Winsor et al., 2012). Likewise, the Baffin Island chironomid record the authors reference has been revised and shows temps during the LIG that were no warmer than peak Holocene temps (Axford et al., 2012). Such nuance beyond the CAPE reconstruction would benefit from the publishing of the regional climate model results.

The timing of their minimum GIS extent/maximum sea-level contribution could also be further developed. Although not well dated, the ~121 ka minimum agrees with the inferences of the minimum GIS extent from Eirik Drift sediment records (Carlson et al., 2008; Colville et al., 2011). How does this timing compare with estimates of the timing of peak LIG sea level (e.g., Dutton & Lambeck, 2012; Kopp et al., 2009)? Are there any variations in retreat that could explain sea-level volatility suggested by some records (e.g., Rohling et al., 2008; Thompson et al., 2011)?

Minor comments by page/line

1736/21: This 4-5 K warmer is highly selective, I would note the nuance discussed above. The CAPE project also didn't consider transient climate evolution so a 4 K peak at 128 ka was grouped with another 4 K peak at 118 ka, which is clearly not right.

1737/29: I wouldn't say "very well" as this is not quantifiable

1738/7: Change to "Eemian interglacial period". Interglacial is an adjective, not a noun.

1738/8-12: This is an odd sentence. I would reword: "With this approach we not only take advantage of the improved...., but also the two-way coupling, which ensures a and the climate forcing, yielding more confidence in the SMB reconstruction".

1739/1: I would use a ";" not a colon, this could be changed in a lot of places in the text.

1740: Somewhere such non-linear feedbacks of the GIS on ocean circulation/temperature needs to be mentioned, which aren't included in this simulation because the GCM is not coupled. For instance, even if an ice margin is land terminating, what effect does a cold surface ocean have on SAT?

1741/19-21: I would say “on land during our Eemian simulations”, since this is model result; actual data on whether ice retreated from the sw coast is lacking at present.

1743/11-14: Okay, so even starting at 129 ka, one still must include the Laurentide, which didn't fully disappear until 128-126 ka (Carlson, 2008; Carlson & Winsor, 2012). This ice sheet would definitely impact the Lab Sea (Carlson et al., 2008) and could influence Greenland climate (LeGrande & Schmidt, 2009). I think the authors need to further discuss this influence that they have not included in their simulations. What happens if one starts the LIG simulation at 126 ka, when the Laurentide is fully gone?

1744/16-18: Okay, so here would be an example of a place where the reader is left wondering why this simulation doesn't have more NE GIS retreat, which is found in other simulations. Can the authors discuss further?

1745/3-8: So, what happens when a cold Lab Sea would be included in response to GIS retreat? Would this reduce SW GIS retreat? Keep a calving margin?

1745/28: This timing could be further developed as suggested above.

Section 4.3.1: Okay, so here's where the effect of the Laurentide needs to be further considered. It was likely still quite large up until ~128 ka, when ice over Hudson Bay collapsed (Carlson, 2008), and disappeared shortly there after by ~126 ka. So this simulation sensitivity should be expanded to see what happens when the simulation is started at say 126 ka so as to have no Laurentide influence.

1748/15: I would say “indicating in these simulations that the Eemian...”

1748/25: Sanchez-Goni et al. (2012) also simulated cooling around Greenland from GIS meltwater runoff.

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1749/5: Okay, I wouldn't reference Francis et al. but rather their revised work in Axford et al. (2012), which shows <5 K of warming relative to the late Holocene over Baffin and Baffin LIG SAT that was no warmer than Holocene SAT.

1751-1752: In general, I think the authors are making too big of a deal over the NEEM misfit. First, the model is only outside of the NEEM SAT uncertainty window at ~127 ka, when the NEEM record could have issues with the Laurentide fore bulge migrating through. Otherwise, it agrees. The authors should consider the uncertainty in these ice-core records. So this would remove the problem about underestimating the temperature change. The model gets it right for much of the LIG, assuming NEEM is "right".

1754/7: The authors should note that this inference from their model that Antarctica contributed a significant amount of LIG sea level rise is in agreement with the conclusions of Colville et al. (2011) based on those authors' observations from GIS discharged sediment records.

1756/23: This publication name is incomplete.

1757/10: This publication has way more authors than listed.

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