Clim. Past Discuss.,
doi:10.5194/cp-2015-175-RC5, 2016

Climate of the Past Discussions
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EGU

Interactive comment on "Last Interglacial climate and sea-level evolution from a coupled ice sheet-climate model" *by* H. Goelzer et al.

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Received and published: 23 February 2016

This paper does represent something of a technical achievement, succeeding in making a coupled run of climate and both Greenland and Antarctic ice sheets across the last interglacial (LIG). To demonstrate that ability, and highlight the steps that are needed to improve on it, I think the paper should eventually be published in CP. However it does need quite a lot of work to explain both details and its limitations correctly. I notice that the paper has already achieved several reviews, so I will not go into huge detail but just give some overall comments, with a little more emphasis on data aspects of the study.

The strength of the paper, as I have indicated, comes from the achievement of making such a study. However I think it is important that it is correctly labelled. It is really a

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demonstration simulation, not a testable prediction. The Greenland ice sheet coupling is achieved only after applying a randomly chosen scaling to the temperature data (it's a tuning in the sense of aiming at a Greenland SL contribution the authors think is sensible, but random in the sense that there is no reason at all to think that a linear tuning is correct). The Antarctic ice sheet apparently responds despite the ice dynamics processes that many glaciologists consider paramount for West Antarctica not being present (or at least I don't think they are). Given these two issues, the actual values that are achieved seem almost meaningless. I don't suggest they should not be explored, and the relative timing of the contributions is of interest for example, but the paper should make much clearer that it does not in any way represent a success in explaining LIG sea level, rather it is a demonstration of how one might start to assess that in a consistent manner.

Another significant issue I would like to see addressed concerns data. This is in two senses; firstly some critical data seem a little misquoted, and others seem to be ignored. But also there is an opportunity here to test different aspects of the model results rather than just the SL response. In particular the climate response in both polar regions could be well-tested using the recent Capron et al (2014, QSR) compilation; but in fact this paper is not even cited. I suspect for example that this paper would allow the authors less room to suggest that the Greenland temperature response is overestimated in the model, and force them instead to consider that the ice sheet may be too sensitive, which is quite a critical issue.

A final major issue I think the authors need to address concerns the mechanism by which they achieve a significant loss of WAIS – this seems to be global SL and ice shelf viscosity. This seems really surprising to me: global sea level is higher than today really only because of the loss of WAIS in these expts, so it is hard to see why this should be a part of provoking such a loss. That leaves us having to accept that Antarctic temperature in Fig 7a apparently provokes a change in viscosity and loss of ice just a few tenths of a degree above present: this would be a very alarming result,

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but seems quite at odds with the mechanisms that usually concern people about WAIS (they generally worry about dynamic loss through the major ice streams and glaciers on the Amundsen Sea side, which have little or no ice shelf restraint, rather than the ice flowing into the large ice shelves). Perhaps I have not understood your mechanism but this definitely needs exploring: either your model is way too sensitive to this process, or glaciologists are worrying about the wrong thing and should be very urgently concerned about ice shelf viscosity. I rather suspect the former as I can't see how there can be such a sharp breakpoint in ice shelf viscosity that a couple of degrees would drain the whole of WAIS and destroy the Ross and Ronne-Filchner Ice Shelves. In any case this certainly needs a discussion.

More detailed comments:

Line 47: Turney and Jones compiled data that were not contemporaneous, ie they combined the maximum temperature at each site over a long time slab. It is therefore impossible to deduce a global mean temperature anomaly from their paper. Probably better to acknowledge this.

Line 56. I think the most commonly cited numbers for LIG sea level are 5-10 m from IPCC AR5, and 6-9 m from the recent Dutton et al (2015, Science) review paper. There is not a great basis for emphasising 6 m in particular.

Page 4. Here is a first place one could mention the Capron et al compilation which could act as a check on your climate outputs or as a forcing in standalone experiments.

Line 186-188 is badly worded. The Grant et al paper uses an approach that doesn't use synchronisation to a mixed record of SL and deep sea temperatures but it doesn't assume anything about their independence or otherwise does it?

Line 192-203. While I understand your decision to scale I think it needs more discussion. From Fig 4a I read off that without forcing you would estimate a Greenland warming of about 3 degrees. This is not only below the NEEM estimate, it's below

other NEEM lower estimates (such as Masson-Delmotte et al 2015), and I am pretty sure it is already similar to other model estimates. Your preferred estimate allows only a one degree warming and this would be really hard to reconcile with NEEM data or with compiled SST data in Capron et al. So, for pragmatic reasons, Ok use the scaling, but I feel you should admit that this might be telling you that your Greenland model is too sensitive, and at least discussing your model in the context of others.

Line 277. While the elevation at NEEM is not perfectly constrained, I suspect its equally important that ice sheet elevation at NEEM is not a strong constraint on the size/area of GrIS. Perhaps re-word.

Line 284. I am not sure what point you want to make here about Cap Century. The same paper also suggests no ice older than 115 ka at Summit but this is clearly not taken to mean there was no Eemian ice there.

Line 314 and around. While we don't understand how an ice sheet at +8 degrees could survive, I still question whether your result illustrates a NEEM paradox or an oversensitive Greenland ice sheet model. You should at least discuss both options.

Line 353-359 and beyond is really confusing. Firstly you say that "Antarctic surface climate is isolated from millennial fluctuations". But then later you agree with previous authors in ascribing the warm Antarctic to the bipolar seesaw. Please make your text consistent. I assume in fact you do think it is the bipolar seesaw response to NH melting that is important in warming the Antarctic at a time when orbital forcing would cool it.

Fig 6b: I could not follow this figure, please explain it better.

Fig 10 is really not comprehensible. It needs a much better caption. In any case I am not sure it serves any purpose since the NHIS evolution dominates everything. This means that while the extent of the highstand above present is a prediction that can be aimed at, the shape of the deglacial rise is really dominated by your (prescribed) NHIS loss.

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