General comments:

The submitted manuscript by Goelzer et al. investigates a new transient simulation of the last interglacial (LIG) period with a bi-directionally coupled climate-ice sheet model. More precisely, the authors use LOVECLIM1.3, an earth system model of intermediate complexity including interactive components for Greenland and Antarctica, i.e., the solely remaining ice sheets during the LIG. Consequently, the focus of the paper lies on climate and ice sheet changes in Greenland and Antarctica and the resulting sea level evolution throughout the LIG. The simulation is compared to previous experiments which exclude ice sheet changes or use a one-way coupling approach. Furthermore, they analyze different sensitivity experiments where specific climate processes are modified or omitted in the experimental setup. The main result of the paper is that the evolution of the Greenland ice sheet (GrIS) is dominated by changes in the surface mass balance whereas the Antarctic ice sheet (AIS) is mainly governed by melting of the shelf area driven by sea-level rise and reduced ice shelf viscosity in a warming climate. A valuable outcome of the model effort is also the temporal and spatial evolution of both the GrIS and the AIS generated within the same climate-ice sheet simulation and thus a consistent experimental setting.

The baseline of the paper is the remarkable technical effort to produce a fully-coupled climate-ice sheet model simulation for the LIG, i.e., a simulation which allows feedbacks between all components of the climate system and hence somewhat represents the “best possible estimate” of the LIG climate with a modeling approach. To my knowledge, the simulation is the first of its kind for the LIG and certainly a valuable contribution for the paleoclimate science community. However, in the present manuscript I am missing a comparison of the simulated climate with proxy records, at least for the two key regions Greenland and Antarctica, as this comparison would have the role of an evaluation of the novel model setup. Moreover I expect a more critical discussion of the chosen model approach regarding remaining improvements and challenges.

Concerning the formal aspects, I think the manuscript needs to be improved in several aspects. Whereas Sections 1-4 are mostly well-written, the results (Section 5) are sometimes hard to follow and need a revision to become more complete and comprehensive. Some figures are only partly described and very poorly referenced in the text (Table 1 is not mentioned a single time in the text). As you will see, I have many minor comments where I feel the language could be more precise to make the manuscript more reader-friendly.

Please find below the full list of major and minor issues.

Major issues:

1. Critical discussion of experimental setup

As stated above I expect a section which critically reflects on the quality of your model setup. I think as much as your reference simulation deserves credit for having a pioneering role as a fully-coupled simulation of the LIG it asks for a discussion of its strengths and weaknesses as well as of remaining challenges and possible improvements. This additional section could be in form of a “discussion” or an “outlook” section which both are non-existent at the moment. The discussion should also include a comparison to Helsen et al. 2013 CP, who previously assessed the GrIS retreat during the LIG with a bi-directionally coupled model approach.

Regarding modeling the climate in Greenland I further wonder if your setup includes the relevant feedbacks on temperature and precipitation found in response to a retreating Greenland ice sheet (Merz et al. 2014 CP, Merz et al, 2014 JGR, Hakuba et al. 2012 JGR). I suspect that the limited spatial resolution of the EMIC in the atmosphere (T21) might be a problem here. Furthermore, the authors should address the use of the positive-degree-day

method (PDD) for the ice sheets as this is a serious issue for the LIG as shown by van de Berg et. al. 2011 NatGeo.

2. Scaling factor

You use the scaling factor (described on lines 192-203) as a necessary tuning factor to avoid a complete loss of the Greenland ice sheet during the LIG. I wonder if the scaling factor is necessary due to the simplified representation of the climate in LOVECLIM over Greenland as I guess that the climate-ice sheet feedbacks previously mentioned in major issue 1 (described in Merz et al. 2014 CP, Merz et al, 2014 JGR, Hakuba et al. 2012 JGR) are probably not included.

How do you feel that this artificial control affects your result concerning Greenland ice sheet evolution and consequently its contribution to the LIG sea level?

Do I understand it correctly that no scaling factor is applied for the Antarctic ice sheet?

Line 274: I think you should state clearly here that the choice of the scaling factor crucially affects the contribution of the GrIS to the sea-level high stand of the LIG.

3. Additional part describing all (sensitivity) experiments

Currently, the manuscript presents results from various sensitivity experiments at different occasions, which makes it hard for the reader to keep the overview. Therefore, it would be much more reader-friendly to add a subsection to Section 4 describing all (sensitivity) experiments and their purpose. I think this subsection could be complemented with a respective list in a table.

I further advise to clearly state in the text that you define the two-way coupled simulation as “reference”. Similar definitions might be worth for the stand-alone experiments etc. Make sure that you use these terms consistently in all text and figures.

4. Extended analysis/description of results

I think the manuscript would greatly profit from an extended analysis and some additional figures in order to present a complete picture of your two-way coupled simulation rather than just showing selected aspects.

Specifically I request:

As I like Figure 3 showing the gained value of the two-way coupling, I think a similar figure for temperature in Greenland and Antarctica would be highly appreciated as these two regions are the main areas of interest in your paper.

Moreover, I am missing a clear statement in the text regarding the results shown in Fig. 3: (i) the simulation with two-way coupling only marginally differentiates from the simulation with one-way coupling with respect to global mean temperature throughout the LIG. (ii) Excluding ice sheet changes and freshwater forcing as done in the noIS simulation leads to a decreased glacial-interglacial temperature contrast and an earlier warming going into the LIG. However, there is only a small difference to the one-way and reference simulation after ca. 128ka! I wonder whether the latter result also applies for temperatures in Greenland and Antarctica?

In order to evaluate your two-way simulation against data I strongly suggest a comparison of the simulated Greenland and Antarctic temperature evolution with respective ice cores (e.g., NEEM, EPICA). As the NEEM delta18O-based temperature reconstruction likely assumes an

overestimated delta18O-temperature relationship you could also include the NEEM temperature curve based on the recent delta18O-temperature relationship presented in Masson-Delmotte et. al. 2015 Cryosphere.

It might also be worth to show the evolution of the freshwater fluxes throughout the LIG to complement your findings presented at Line 353pp and Line 429pp.

5. Mass balance of Greenland and Antarctic ice sheet

I think a proper definition of the (surface) mass balances for the Greenland and the Antarctic ice sheet is required. Please clearly state what you refer to as accumulation, ablation, runoff, (surface) melting, calving flux and how they combine to the mass balance. Please use the same terms in the text as in the axis labels of Figs. 4 & 7.

I think it would be a valuable addition to show the net mass balance as a further panel in Figs. 4 & 7 so the reader can reconstruct the evolution of the ice volume shown in Figs. 4e and 7e. Whenever possible use the same scales for the different terms of the mass balance in Figs. 4 and 7.

6. References to figures in text

Throughout the paper I miss many references to the corresponding figures, which would substantially help the reader to understand the descriptions in text-form. Please be more precise when discussing panel plots, e.g. put reference to Fig. 4a rather than just to Fig. 4.

Some examples of missing/imprecise figure references:

Line 365: Fig. 7b
Line 375: Fig. 7a,d
Line 382: Fig. 7d
Line 393: Fig. 9b after “experiments”
Line 402: Fig. 7a and 2c
Line 427: Fig. 3 after “evolution”
Line 445: Fig. 10c
Line 446: Fig. 10b

Minor issues:

Lines 23: Please be more specific than “surface mass balance changes”

Lines 24-25: “Our results indicate” could be replaced with “The comparison of fully-coupled with stand-alone Greenland ice sheet simulations emphasizes”

Line 68: change “lower bound of 5.5m” to “lower bound of Eemian sea level rise of 5.5 m above present-day levels”

Lines 75-78: This sentence is misleading as it implies that any Southern Ocean warming is induced by the interhemispheric seesaw effect.

Line 104: Wrong reference: Robinson et al. 2011’s ice sheet model uses output of a transient EMIC simulation as input but does not give feedback to the climate model. Helsen et al. 2013 CP would be a more appropriate reference here.

Line 111: “climate and oceanic conditions over the ice sheets and in their proximity” seems not to be a correct/precise statement.

Lines 109-118: Whereas I like the rest of the introduction, this last paragraph should be improved to better stress the focus and strategy of the paper. I think you should highlight here that you generate the first transient simulation of the LIG with a bi-directional coupling of climate and GrIS/AIS model components. Furthermore, please clarify that you study key mechanisms and feedback processes with the aid of sensitivity experiments and with the comparison to one-way coupled and stand-alone ice sheet simulations. I would also state here that you focus on climate and ice sheet changes in Greenland and Antarctica and the resulting sea-level evolution throughout the LIG.

Lines 170-171: specify “ice loading changes” e.g., with “ice loading changes coming out of the penultimate glacial period”.

Line 184-191: You use the sea-level reconstruction by Grant et al. 2012 as boundary condition for your simulations. Wouldn’t it be more consistent to use the “internal” sea level corresponding to the simulated global ice sheet changes? What would be the consequences for the melting of the AIS which apparently most strongly responds to sea level changes? What are the reasons for driving the model with a respective sea-level reconstruction instead?

Line 205: introduce the abbreviation “(SA)” here.

Line 215: The title of this subsection could be more specific, e.g., “Initialisation of the reference simulation”

Line 241: might be more precise to replace “importance of ice sheet changes” with “importance of two-way coupling between the climate model and the ice sheet models for the GrIS and the AIS”

Line 247-251: This finding is somewhat hard to understand. May be it would help if you show the freshwater fluxes in a figure (as also requested in major issue 4) and put a respective reference.

Line 264: Rather put the reference to Fig. 4e here.

Lines 334-336: I think you could add here that the ice-albedo feedback is a positive feedback.

Line 351pp: “The warming necessary….” This sentence is not easy to comprehend. Please revise.

Line 365: show the freshwater fluxes in a figure or add (not shown) after “hemispheres”.

Line 367: You speak of “ablation” but in Figure 7 you name it runoff – is this the same? Please be consistent with all terms describing the mass balance of the GrIS and the AIS (see also major comment 5)

Line 376: add (not shown) after “130 kyr BP”.

Line 380: add (not shown) at end of sentence or put a reference to Goelzer et. al. 2015.

Line 418-419: Is “their model” equal to the simulation you termed “one-way” at other occasions in the script?

Line 470-472: I think this sentence should be rephrased to state more clearly that you artificially limit the melting of the GrIS to conform to existing ice core constraints.

Line 477-478: Please be more specific. I think “ice-climate feedback” is a too general term for a take-home message in the conclusions.

Line 482: I think it should also be stated here that an unconstrained fully-coupled climate-ice sheet simulation does not fully agree with data, e.g., the GrIS would melt away completely during the LIG. This implies deficiencies in the model physics or unknown/excluded processes. It also emphasizes the NEEM paradox of strong warming coinciding with limited GrIS melting that can hardly be understood in a model perspective.

Table 1: Needs to be discussed in the text or should be removed.

Figure 1: The references (Opsteegh et al. 1998, Brovkin et al. 1997 and Goosse and Fichefet, 1999) mentioned in Fig. 1 should also be added to the reference list.

Fig. 4b,c,d: Does the horizontal stippled line represents the pre-industrial level? Please clarify in figure caption.

Fig. 6b: This schematic is somewhat difficult to comprehend and it is only mentioned once in the text. Should be revised or removed.

Figure 7d and text: Is there a difference between shelf melting and sub-shelf melting? Please be consistent in text and figures

Figure 9b: Does the blue curve represent the experiment with excluded surface AND sub-shelf melting or just the latter? In line 392 you mention both. Please revise to be consistent in text and figures.

Figure 10: please number the panels with a,b,c. Furthermore, the figure caption should include additional information, e.g., the meaning of the stippled lines.

Technical corrections:

Line 89: remain**s**

Line 96: van **d**e Berg

Line 256: “is retreating” rather than “has retreated”

Line 379: **a** weakening

Caption of Fig. 8: move listing of (a), (b), (c) in front of description as done in all other figure captions.