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Interactive comment on "Simulating interactive ice sheets in the multi-resolution AWI-ESM 1.2: A case study using SCOPE 1.0" *by* Paul Gierz et al.

Anonymous Referee #2

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General comments:

The focus of the paper has not become clear to me and there are contradicting signals in title, abstract and main text. Some parts read like the paper should be a description paper of the SCOPE coupler. Other parts suggest that it is a case study on fully-coupled simulations of the Greenland ice sheet. At the same time, references to other ice sheets (NHISs, AIS) are present suggesting that the work could be considered as establishing a fully coupled system, ready to be used for any (paleo) configuration. I see considerable shortcomings for the two latter interpretations, which makes me lean to suggesting a specific focus as a SCOPE description paper. In any case, the paper should be considerably reworked to make it clear from title to abstract and introduction what the focus of the paper is.

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The manuscripts lacks important references to earlier and similar work on including interactive ice sheets in Earth System Models. At the same time, it seems that the authors do not fully appreciate the complexities associated with such undertaking (see e.g. Fyke et al., 2018). Considerable efforts have been made and are ongoing e.g. to improve the representation of the SMB over ice sheets (e.g. Vizcaíno et al., 2010; Sellevold et al, 2019) and to produce consistent coupled initial states for climate-ice sheet simulations (e.g. Fyke et al., 2014). Shortcomings of the current modelling approach should be critically discussed in view of these and other existing studies (e.g. Smith et al. 2020).

It is mentioned in P6 l147 that other domains are implemented. But why are they not analysed? I don't think they can be considered similar enough so that showing the model for a Greenland case only is sufficient. AIS and NHISs have considerably different characteristics. In particular the interaction with the ocean of these marine ice sheets is clearly a different case than what can be done with a predominantly land-based Greenland during warm periods. The PICO model e.g. has been specifically developed for the Antarctic case. It is unclear to me why it is tested in the Greenland context. The interaction of the ocean with Greenland outlet glaciers is clearly not adequately represented in this model setup, which is a severe shortcoming for this use case that should be discussed.

If the aim of the paper is to show that the coupling is functional beyond a purely technical nature, it is crucial to see some critical experiments that explore the model's capabilities. How does the SMB for the present day over Greenland compare to observations and other model results? Is the SMB anywhere close to adequate as boundary condition for an ice flow model? If not, how does that limit the predictive capability of the model as a tool to look into warm climates of the past and future? How could the SMB be further improved? How does the atmosphere respond to an ice sheet that is considerably lowered and for an ice sheet that is retreating over land? How does the ocean respond to freshwater input? All of these are questions that need to be ad-

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dressed should the model be used for simulations of the PD, LIG and future. And many more questions arise if the model should be employed for colder periods.

It should be noted somewhere prominently that the GrIS cannot be expected to be in a steady state neither for the LIG nor for the MH. We can of course use snapshot climate simulations to study the effect of a climate perturbation of a certain pattern and magnitude, but it should be clear that we are not looking at a real (transient) climate experiment. Comparing the GrIS sea-level contribution from these experiments with e.g. LIG reconstructions of sea-level is therefore problematic and requires some additional comments.

Title:

From the title, it is not clear what SCOPE 1.0 is. Maybe "A case study using the Standalone Coupler SCOPE 1.0"

The term "multi-resolution" in the title is not picked up in the manuscript. Suggest to remove it or add substance concerning this feature in the manuscript.

The title mentions "ice sheets", but the paper presents only results for one ice sheet (Greenland). Suggest to rephrase.

Abstract:

Important elements from the manuscript and title should be present in the abstract. The coupler SCOPE is not mentioned in the abstract, while it is an important part of the manuscript.

P1 l19 I think you mean "future ... studies" as in "upcoming". Try to avoid paring "future" and "paleoclimate" in this sentence.

Comments:

P2 l25 It seems counter-intuitive to measure the amount of freshwater stored in the ice sheets in SLE. Could give the percentage of the global freshwater supply instead. Also

not clear why the glaciers are included in this point.

P2 L28 While you discuss the AIS, GrIS and glaciers before, here is only information about recent changes of the GrIS. Not clear why. Should extend to the AIS.

p2 l30 Avoid repetition of "large-scale"

P2 l31 What is "interior ocean circulation"? Rephrase?

P2 l36 What are these shifts? Heinrich events, or DO events?

P2 l37 "trigger or response". Neither trigger nor response suggest the notion that you put forward earlier of a fully coupled system with feedbacks. This may be important to reformulate.

P2 l38 "Earth System models with the capability"

P2 L39 Add version number after AWI-ESM?

P2 L40 Remove "that is"

P2 L40 Reference year Shi et al missing.

P2 L42 "which is implemented"

P2 l45 "The simulations are based on"

P2 l48 "time slice simulations". Need to clarify how that relates transient coupled simulations.

P2 l50 "climate states". Why plural?

P2 l51 Ice sheets can always have runoff. Do you mean anomalous runoff.

P2 l53 There is no motivation given why the focus is now solely on the GrIS. Please motivate that choice and why that is a good test case or easier to handle than the AIS or NHISs.

P3 l76 This is a difficult sentence, consider introducing an abbreviation for ECMWF before and reformulate.

P3 l84 Consider introducing abbreviation ECMWF before. See also previous point.

P4 l116 Please describe what the implications of using dynamic vegetation are.

P5 l126 The PDD method is often assumed to be ill-fitted for paleo applications with different orbital parameters. This should be discussed as a possible caveat.

P5 l128 The units of the PDD factors are 3 orders of magnitude wrong. Typically 8 mm d-1 K-1 for ice.

P5 l130 "forcing" is repeated here. Not clear what "forcing adds white noise" means Reformulate.

P5 l132 Sea-proximal. For Greenland this concerns marine-terminating outlet-glaciers. Reformulate?

P5 l133 Are the three parameterisations employed simultaneously? Clarify.

P5 l146 The past sentence of the paragraph contradicts the sentence just before.

P6 l155 "shown in Figure 2"

P6 l162 Could you please explain why the information needs to be anonymized?

p6 l173 YAML needs a reference.

p6 l182 This sentence does not read well and contains too much information.

p6 l185 While the time spent for the coupler takes a relatively small percentage in the entire model, it must be much slower than an online coupling procedure within the model. It would be interesting to read some discussion about that and an estimate how much the coupling time could be reduced if a more efficient procedure would be employed.

P7 This is difficult to read (too small font) and does not provide critical information. Improve and move to the appendix?

P8 L189 It is not clear to me why and it sounds drastic to claim that running asynchronously means a violation of the laws of physics. Reformulate? I seem to understand that you do run the model in this way during initialisation, so this should be mentioned here as a case where it makes sense to employ the model in this way.

P8 l195 I miss a paragraph 3.0 about Atmosphere/Ice sheet coupling. How to produce an adequate SMB as boundary condition for an ice flow model is not at all obvious. The large difference in resolution between the atmosphere model and the ice sheet has to be bridged somehow. The fact that you use the PDD scheme in the ice sheet model does not make it easy to separate this problem, but should be discussed nevertheless. How is the atmospheric information interpolated/ downscaled. Does the PDD calculate in anomaly mode or with the absolute temperature. How do you deal with the fact that atmospheric grid cells can contain a mix of ice sheet, ice-free land and ocean?

P8 l197 Be precise and explicit about what information is passed as change/anomaly and what as absolute field. What is the reference elevation (in the climate model and in the ice sheet model) if changes are communicated? Is the extent really communicated as a change in extent?

P8 l198 Since you distinguish ablation and runoff, which field is used for what? WHat do you use ablation for?

P8 l200 Why is runoff routed by the atmosphere and not by the land surface model as mentioned on p3? Is this discussion not better placed in the interaction with the ocean? How do you distinguish runoff from subglacial discharge and from frontal/subshelf melt.

P8 l200 What is the hydrology scheme in AWI-ESM-1-1? Explain.

p8 l216 How does this removal from the hydrological scheme work. Is it globally dis-

tributed?

p9 l230 What are the exchanged quantities?

p9 l231 What is the three equation model for. Explain better.

p9 l233 Most of the Greenland glaciers don't have ice shelves at present. How does this scheme translate to the most common case of an outlet glacier ending in a vertical calving front? I think it doesn't. The models you discuss here have been developed for a typical Antarctic shelf geometry not for Greenland.

p9 l238 Remove "The first equation" and brackets around Eq 1 and similar for Eq 2 and 3 below.

p9-p10 I am confused about 3.2 as all of the discussed approaches are relevant for an Antarctic case but not for Greenland. Are we still in the Greenland use case, or is this about general model capability. If the latter, the paper needs to be restructured to discuss general modelling approaches aside from the concrete use case. See also general point on the question of paper scope.

p10 l258 Does "In our case" refer to the Greenland case? If so, the PICO model is not useful parameterisation for that case.

p10 l267 Why "not necessarily". I would say quite certainly not.

p10 l276 Start a new sentence between 'dynamics' and 'with'.

p10 l278 How would total ice sheet volume influence climate? Reformulate.

P11 l283 Where do the reconstructed ice sheet geometries originate from? Are these model states? Describe better.

P11 l286 Does the glacial cycle spinup use climate information from the same model? If not, any arguments about consistency? A glacial-interglacial ice sheet spin-up usually has the purpose to produce an internal ice rheology distribution in line with the history

of past forcing. Continuing with a steady forced ice sheet simulation destroys this information. How do you deal with this problem?

P11 l290 How do you examine the ice sheet volume, what is the criterium?

P11 l294 This asynchronous run was explained to violate physics. Modify that statement or explain why it is fine to do that here.

P11 l297 Translates to only 120 climate years, correct. Maybe add that as additional information.

P11 l302 Motivate your choice of 3 years. Why not more or less?

P11 l305 In my opinion it would be more interesting to see results of experiments 3 and 4 and not only in ice volume, but also e.g. in SMB components. Fig 4 seems to suggest that may outlet glaciers are thickening. Why is that?

P11 l306 It is good to see confirmed that the GrIS volume decreases with increasing boreal summer insolation, but also a pretty limited view of a complex coupled system. What else interesting is going on in these experiments? Are there any difference between different ice sheet sectors? What happens in with atmospheric circulation, the ocean and the sea-ice. How do outlet glaciers respond to those changes?

P12 l313 What are these assumptions and how are the results in line with those? Please also note my general point on the non-steady state behaviour of the Greenland ice sheet during the LIG.

P12 l316 "Figure 4"

P12 l316 I think it would make more sense and be more instructive to start the LIG and MH simulations from a fully coupled steady state PI and observe changes in all components as perturbations relative to that baseline.

P12 l323 Running an ice sheet model puts you in the position to identify the cause of a velocity change. Please confirm this statement from your model output.

p13 l369 Any discussion that could be added for figures 9 and 10? If not, I suggest to remove them from the manuscript.

p13 l372 What metrics is the model performance measured against?

p13 l371-376 Not clear what this comparison is supposed to show. I would believe the point is not to show that including a fully coupled ice sheet in a climate model has hardly any effect. I understand that it is good so see that including a dynamic Greenland ice sheet does not completely explode the climate. Nevertheless, there are other things worth exploring as I note here again: The standardised 4xCO2 experiment for example would be a great test to see if the ice sheet is retreating at a rate comparable to other models. Does the MOC respond at all to the additional freshwater input? How does the atmosphere see the changing ice sheet topography? Does the retreating ice sheet change the albedo? What are the feedbacks at play during ice sheet decay under strong atmospheric forcing? These are just the most basic questions that need to be addressed to convincingly show that the model is a useful tool for coupled simulations.

P14 l376 Is that seasonality specific for the model including the ice sheet, or is that a generic behaviour of the model? I would suggest to focus this section on aspects of the climate/ ice sheet system, that are different from the uncoupled climate model.

P14 l400 It seems strange that dEMB is mentioned for the first time in this manuscript in the conclusions.

p15 l412 Adapting coastlines seems like a long shot compared to all the other limitations of this model. Are there concrete ongoing works that address these issues?

Table 1. Why is this important? Is it discussed anywhere in the manuscript?

Figure 1. Explain better how topography (a) is an example of model resolution. If patches one sees are showing the resolution of the atmospheric grid, say so. This is not at all visible on a printout. Zooming in on the pdf until I see the patches, I see that they are at the image resolution limit. Suggest to enlarge and improve image

resolution.

Figure 2. Not clear what Runs 1 2 3 are. Are these years? Maybe indicate what SCOPE is in the upper figure, the green and blue arrows. It is not really clear where the separation is between the upper and the lower part.

Figure 3. 'Insolation anomalies for a the Mid Holocene and b the Last Interglacial compared to Pre-Industrial' Why not run MH and LIG from the PI ice sheet?

Figure 4. Why are many outlet glaciers thickening under LIG and MH climate? The PI ice sheet looks like filling the entire continent to the land-sea mask. That is typically the sign for an inadequate SMB boundary condition with way too little ablation. If this is the case, some critical statements are required here. The MH and LIG cases do not seem to show much if any retreat from the coast. This may be related to the point just before. No retreat during LIG. Red and black contours not visible on my printout nor on the pdf. Give units as colorbar labels. The ice Caption: The divide is not an area! The last sentence messes something.

Figure 5. The colour scale on the right panel suggests that there is no ablation area (all SMB is positive). Chose a better colour scale. The panels are too small on a printout. I don't see why data with global coverage has to lead to the odd rotation of the grid visible in the figure. Suggest to fix that for the sake of clarity of the figures.

Figure 6. Not sure what we should expect for the right hand panel given that the PICO model is clearly the wrong model for this purpose. But, if what we see is the only ocean forcing applied to this ice sheet, it is not very realistic, to say the least. I think at this point it is clear that this has to be discussed es a severe shortcoming of the model. With a more realistic representation of the interaction of Greenland outlet glaciers with the ocean, it would be interesting to see how far the ocean model grid extends, where ice is grounded and floating and what the extrapolated information is in between.

Figure 7. Can you give some explanation to what we see in this figure. What is the

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origin of the inter-decadal variability visible in some periods? Are we looking at oscillations between two states? What is the reason for the arbitrary offset along the time axis?

Figure 8. It would be interesting to see more details about the effect of the coupling other than global temperature evolution. What is happening with the ice sheet in these runs and with the ocean and atmosphere around it?

Figure 9 and 10. Can be removed in my opinion, unless a meaningful discussion is added.

References

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