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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 #1

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*Summary

This paper describes aspects of the AWI-ESM-1.2 climate model, focussing specifically on the SCOPE system used to provide coupling to an interactive ice sheet component. It also gives some general information about the performance of the system in a set of case-studies. As the authors note, there is a pressing need to include ice sheet and shelf couplings to help answer many important questions in climate science, and this work addresses a timely and important topic. The SCOPE coupler at the heart of it all seems to be a flexible and useful tool likely to be of interest to workers in the field. Given the potential abilities of the model and coupler, and the range of simulations that have been conducted, I found overall that this manuscript provides a rather unsatisfying write-up which I think it could be significantly improved for readers by giving more detail

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in a number of areas.

*General Comments

Researchers in Earth System modelling are currently working out how best to implement ice sheet couplings in state-of-the-art models, and it's encouraging to see new models like this appearing. The SCOPE framework they describe seems like it is flexible enough to couple between various components, and also couple them at different levels of complexity - eg SMB provided by either PDD-relevant fields or the dEBM - and I think that's a really useful approach that might enable other groups to try plugging an ice sheet into their climate modelling systems. I really like what it looks like they can do, and I'm pleased to see that the authors have included some paleo simulations as well as the DECK in the case studies they've done to illustrate its performance. However, I don't think this manuscript really gives me the level of detail I'd want to see in order to assess how AWI-ESM with ice sheets behaves in practice, nor contains enough to convince other modelling groups that they might want to use the SCOPE coupler. It's really valuable for interested readers to be able to see what the modelling systems do well, and also where they don't, as part of assessing what scientific questions its appropriate to use the model for. As it stands, the paper implies that the coupling system can pretty much do it all, but has only had those couplings tested in a very limited way, and the results of those tests are not evaluated here in much detail.

This is a short text, and pretty much the first third is given over to summarising pre-existing model information. The coupling system is then described in terms that include the potential for a range of ice shelf-ocean interactions, and the Introduction has promised DECK and PMIP case-studies, so it's disappointing that the only details we get about how this all really plays out in practice concern Greenland-only runs in warm climates whose results are sketched out in the barest detail. As a GMD paper this isn't necessarily the place for a detailed description of simulation results for their own sake, but I think it's important that descriptions of a modelling technique come with enough illustration of how they work (and maybe how they don't) to be able to judge whether

the proposed technique/model is useful for a particular application.

I may be misreading it, but the paper feels confused as to whether its purpose is to document how SCOPE and the coupling work in principle, or to describe how AWI-ESM behaves once equipped with ice sheets. Depending on the authors' goal, I might recommend adding different types of information to the paper. In either case, it wouldn't hurt to have a more focussed statement of the scope and aims of the work up front. If SCOPE and the technical adjustments made to the models during coupling are the main focus - perhaps with a goal to advertise SCOPE to other groups - then I think they would improve the paper by providing more explicit sample configuration files that illustrate the flexibility in how it can be used and what transformations and parameterisations SCOPE can take care of and what would need to be altered inside the host atmosphere/land/ocean models. More illustrations of how SCOPE transforms input fields into information suitable for use in the target models for a range of case studies would be good here too. If instead the coupler performance really is inseparable from the AWI-ESM climate simulations it's embedded in, then it would be good to see a lot more /evaluation/ of what happens in those simulations, both to the ice and relevant climate fields - eg not just ice extents with respect to other work, but deep water formation in the ocean and how that affects large-scale heat transports. In an ideal world their case-studies would include some cold paleo climates and other ice sheets as well, to show a broader range of behaviour and sensitivity, but that is probably too much additional work to suggest at this point. Few details are shown in the case study simulations that are presented here, and there is almost no critical evaluation or comparison with other studies in what is presented, which is a little disappointing.

*Detailed comments

abstract: most of the abstract is concerned with motivating the inclusion of coupled ice sheets in climate models, rather than summarising the work that has been done here.

line 5 (and elsewhere): the term "comprehensive" is used in a few places in an abso-

lute sense, but no climate model includes representations of every possible - or even every relevant - physical process. This and "fully coupled" are simply inaccurate terms, unless used relatively to say that a model includes more couplings than others.

line 6 (and elsewhere): "cryosphere" is used in a few places in a way that makes most sense if taken to mean land-based ice, but the term is wider than that - snow cover and sea-ice are part of the cryosphere, for example, and their interactions are common features of most climate models

line 30: repeat of "large scale"

line 40, 618: citation of Shi et al has year 0

line 53: having motivated the need for global ice sheet modelling, this is the first suggestion, almost off-hand, that only Greenland will be looked at in this work. This limitation is only actually made explicit on line 145 at the end of section 2.2 - I think it needs to be said much more clearly, much earlier, in the descriptions of what has been done in the abstract and introduction. Given that the system is apparently technically capable of looking at other ice sheets too, at least that's how I interpret line 146, I think that looking at only Greenland is a disappointingly limited case study. If more ambitious runs had been undertaken I guess that they would have been used instead, but it would have been great to see.

line 58: by "AR4 IPCC scenarios" do they mean the SRES scenarios (Nakicenovic 2000) - that might be a more general and helpful term here?

line 130: Gregory et al 2012 found that the size of artificial variability added to PDD melt calculations can have a major impact on the results - can more detail be given here than simply "white"? Gregory et al, "Modelling large-scale ice-sheet–climate interactions following glacial inception", Clim. Past, 8, 1565–1580, 2012

line 161: "hinting" is a rather informal term. Why does the origin of the data have to be anonymised completely, it seems that having it potenitally retained in some form of

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metadata might be useful in some cases if SCOPE can be used to generate boundary condition files from different source models that could in principle be archived for later use?

line 186: I don't disagree with the conclusion that the ice sheets are likely not a major additional computational expense in the context of current CMIP ESMs, but it perhaps should be noted that the runs here only include Greenland at 5km. I imagine an Antarctic sheet/shelf system would cost significantly more, as would glaciated areas appropriate to a Last Glacial Maximum scenario, which I imagine is in scope since PMIP is an area of interest here. That thought raises another question - how would their system run separate GrIS and Antarctic ice sheets? In one global domain, or totally separate instances of PISM? Would that require separate SCOPE instances too? The timesteps and coupling intervals being used in the system have also not been cited, which might be useful.

line 188: "violate the laws of physics" sounds extreme. I think outlining specific conservation issues implied by the asynchroneity would be better.

section 3.1: I'm not really clear on a couple of issues related to melt and conservation that run through this section. Perhaps they could be explained more clearly. I appreciate this stuff is tricky to explain - my description of my confusion that follows is not the clearest either, but should gave a flavour of the aspects I think could be rephrased.

As ECHAM/JSBACH runs, it must make some estimate of the melt over the ice sheet, if only as part of the latent heat calculation and estimating surface temperature. Is melt actually produced, and routed to the ocean, or is there no explicit melt/runoff occurring at all under an assumption (line 220) that the ice sheet (without interactivity) is in steady state so ice sheet runoff in JSBACH is just whatever precipitation has fallen? Under a PDD scheme, PISM will be calculating melt in a wholly different manner, unconnected with what happened in JSBACH - so that must be why the PISM ablation (line 198 - but under a PDD SMB scheme how is surface ablation different from runoff, is there basal

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water too?) is handed back and (line 224) the "hydrological discharge is corrected". That year's discharge has already happened in JSBACH though, so is an adjustment made to the amount of the next year's precipitation that makes its way to the ocean? Is that adjustment spread through the year evenly, or with some seasonal pattern? Does it have a spatial pattern? Is any attempt made to reconcile the surface latent heat fluxes with the melting that PISM has seen?

line 205 says the orography changes are phased in over the next year of ECHAM run. Is the same true of the extent/mask changes, or are they instantaneous? In some models, the surface roughness (linked to the glaciated mask) would be related to the subgrid orography terms. Linking to my question above, are the various water fluxes (line 212) phased through the year of climate run too? The discharge correction is not noted here, but it does say that surface mass loss is part of what is transported to the ocean - is this term actually the (corrected) ECHAM precipitation, or really the PISM surface ablation?

Line 206 simply notes that the JSBACH glacial mask is updated. The PISM horizontal resolution is much finer than that in JSBACH - is there a threshold for the amount of a JSBACH box that is glaciated in PISM used for this, or can JSBACH cope with both fractional ice extent in surface type and soil representations? Soil water is buried by advancing ice, but vegetation is destroyed - does JSBACH track carbon or nutrient in the soil, and is this buried as well? Is the vegetation mass/carbon simply lost from the model completely as ice advances, or conserved somehow?

Line 215: calved mass would ideally not only be released over time, but over quite a large area as well - is all of this water placed at the surface, right at the coast?

section 3.2: given that the previous section is so specific about what is done in the ECHAM/JSBACH case, it feel incongruous that this section talks mostly about capabilities that aren't used in AWI-ESM. It also only deals with the provision of a melt rate to the ice shelf, with nothing on information that might be passed back to the ocean, such

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as shelf geometry or adjustments for heat or salinity conservation based on meltrates that might have been computed in PISM or PICO. Can this be done in their system - the land-surface equivalent is described in the previous section for ECHAM/JSBACH?

line 230: the first case described seems to be relevant for where the ocean model explicitly resolves shelf profile. Does SCOPE take 2d fields along the shelf surface, or a 3d fields from the basin for this? When regridding, is any adjustment made for the greater resolution of surface geometry detail on the PISM grid, equivalent to the lapse rate adjustment to the surface temperatures in the PDD melt calculation?

line 259: I'm not clear on where PICO sits in the framework here. Is it somehow built into SCOPE, or PISM? Does SCOPE prepare FESOM output for PICO, then get called again to process PICO output for PISM? If so, how are the separate parts called and coordinated - is coordination of sub-models a SCOPE function too?

line 267: further to the note on ocean vs ice sheet model resolution above, surely it's not just that FESOM isn't resolving areas beyond the grounding line, but for the Greenland case-study used for the rest of the paper, it's not resolving /anything/ of the fjord systems that lead up to the marine-ice interfaces that PISM wants boundary conditions for. This is where PICO comes in as not just an option but absolutely essential.

line 273: as noted before, it's a bit disappointing that only warm PMIP climates have been looked at. Could more comment be made on whether AWI-ESM could/will be used to look at colder paleo ice as well? The PI is also a bit tricky in terms of being their most modern case-study, it's much easier to provide an evaluation of the model performance for the present day for which there are actual observations.

line 274: this sentence is a good description of what I think should be in this section, but what is actually written in section 4 doesn't live up to this ambition yet.

line 302: I don't think it's said anywhere why a three year coupling period is chosen. I may have missed this detail, but line 130 notes that monthly data is saved from the

atmosphere model - does the coupling pass a 3 year timeseries of atmospheric forcing data to PISM, or does it construct a climatology by averaging over those three years somehow?

line 317: I think a "Fig" is missing from the number in brackets "(4)". This figure is useful, but it would help the reader to see a more detailed evaluation of the LIG, MH and PI GrIS states compared to other reconstructions / simulations

line 337: The FESOM resolution is still not sufficient to resolve the majority of the coastal system relevant to the ice marine boundary conditions for the outlet glaciers, so although the ice/ocean grid numbers match up better there's still an awful lot of real physics missing. This is inevitable in a global model, but I think it should be stated more clearly. I'm not clear on how exactly PICO is deployed here either - are there individual (and individually tuned) PICO boxes for each outlet glacier, or are the different areas aggregated somehow? How has PICO been tuned for use here? Again, some evaluation of how the melt rates achieved stack up against other evidence, or even simply what is observed in the present day, would be useful.

line 351: typo, "asyncronously"

line 354: are these simulations accelerated with the same protocol as used in part 3 of the "Spin-up Procedure", eg 3 climate years -> 25 ice years?

section 4.5: At less than 1 page, this section is simply too short to usefully illustrate or evaluate the model performance across two climate change scenarios and two paleoclimate simulations. Given that a coupled Greenland ice sheet is the main feature of the model in this paper, it is also a little bizarre that the section focusses on global, large-scale climate fields, with an evaluation limited to the fact that the coupled Greenland doesn't make much difference to the simulations. As it stands, it's little more than a statement of the simple fact that these climate setups can be run with AWI-ESM, it doesn't tell me anything about the ice sheet, how the coupling is working in the model or anything about the climate system in reality.

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line 411: I don't think "prognostically" is used correctly here. Not being able to change the coastlines mid-run is an important limitation, common to many models with coupled ice sheets, and should be mentioned earlier when the coupling system is first described.

fig 2: this schematic only shows atm -> ice coupling, and omits the ocean/PICO coupling processes?

fig 4: figure has no units

fig 5,6: caption doesn't note the source (eg simulation, time-averaging etc) for the coupling fields. It might be good to show these as block-fill plots rather than smooth contours, that would illustrate the resolutions that the different models are working at - 5a and b could show the ECHAM grid, and 5c the PISM one, for example.

fig 9,10: figure has no units, caption should note the reference climate for the anomalies (preindustrial)

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