

General comment

The authors propose an evaluation of the effect of the inclusion of a changing ice sheet geometry on the climate around Greenland and vice-versa. They do this in part 1 of the manuscript by comparing 2 simulations carried out with the Earth System Model CESM2 coupled to the ice sheet model CISM2, in a ramp-up scenario where major parts of the ice sheet are lost.

The authors mention that, despite that 2-way coupled climate and ice sheet simulations of the GrIS had already been carried out with CESM2-CISM2, there had never been an evaluation of what the effects of including a 2-way coupling in CESM2-CISM2 would be. They also mention that this is the first time the effects of GrIS topographic changes on blocking events was being investigated, despite it being mentioned in the literature for some time now. I found it really interesting that the effect of changes in topography on blocking and the other negative feedbacks outweigh the melt-elevation and melt-albedo feedbacks, as those are often mentioned first as a justification for using coupled climate and ice sheet models. In that way, this manuscript brings something new and much needed.

The manuscript is well written and generally easy to read and my comments mostly concern rewriting some sentences to make the reading a bit easier. The authors were also really thorough in their justifications.

Comment on the second part of the manuscript

I do however have an issue with the second part of the manuscript, the one where the ramp down scenario and the investigation of the effect of a cooling on the GrIS is introduced. I don't think it really belongs with the first part, as this one is really an evaluation of an "improvement" in a model. This, along with a few other things, made me think the manuscript was a (condensed version of) a student's thesis. A quick check online confirmed that it was a masters thesis.

In a thesis, there's a common thread where different sections are related to each other but are their own distinct piece of work. Putting them together in one paper doesn't really work in my opinion as, in its current form, the manuscript has 2 messages/questions: the first being "what effect does including a 2-way coupling has on future projections of the GrIS melt?" and the second "are there conditions under which the projected melt of the GrIS can be reversed in CESM?".

Ideally, if you had branched a 1-way ramp down simulation from the same point of the 2-way ramp up as the 2-way ramp down simulation, you could have done a similar comparison as in the first part of the manuscript where you'd have evaluated the effect of a 2-way coupling on the recovery of the ice sheet. But running this extra simulation, if you haven't already, is going to take a long time.

The results and the described processes (NAMOC overshooting, the existence of a smaller but stable ice sheet in the future and the comparison to the PI state at the same temperature ...) are interesting so I am not saying that they shouldn't be published at all. But I am also not entirely convinced that part 2 has enough material to be smaller paper or brief communication in its current form. As you say, "the timing and the rate of the CO2 ramp-up and ramp-down likely have a large influence on the mass balance evolution and influence whether mass loss can be halted or reversed" and you'd probably have to re-write extra bits to justify the work. Have there been other similar simulations done by other CESM2 users you could include as comparison? Or could this be part of a larger study involving, e.g an overshoot scenario study with CESM2-CISM2?

In conclusion, I think that, worse case scenario, part 1 is enough for a paper and turning a masters thesis into a peer reviewed manuscript of this quality is already an achievement. But I don't want to discourage a young researcher from publishing their work either so, if you can't find a way to publish the second part as a separate paper but really manage to justify its inclusion in the present manuscript more (including by modifying the title a bit to change the message maybe?), I will not

be opposed to its publication in its current form.

Minor comments

General comment on figures: some figures might benefit from being slightly wider (e.g. maps). I liked the consistent use of colours but the shades of blue, red and green were sometimes difficult to tell apart, especially in figure 2d and again in figures 7b and 9b. Could you keep the same main colours but change the tint/shade (i.e. make darker tones even darker and lighter ones lighter)?

Abstract

p1, line 10: I suppose the lapse rate you're mentioning is the lapse rate used to downscale temperature from the elevation tiles to the ice sheet grid. It would be useful to add that information.

p1, lines 12-13: add that it is for the 2-way coupled simulation

1. Introduction

p2, line 52: what do you mean by “such a period”? A certain length of time or simply that it's a warmer period?

2. Method

p4, lines 96-97: I'm not sure I understand these 2 sentences. Is the snowpack thickness reset to 10m at the beginning of every year? Meaning that if the thickness at the end of the year is 10+X m, X m is the positive SMB that is transferred to CISM2? And, in the second sentence, what do you mean by “further melt”?

p4, line 119: I don't think the definition of refreezing capacity is necessary here, as you only mention it much later in the manuscript (p19, end of section 5). Instead, I would just change line 376 (p19) in “The refreezing capacity (amount of refreezing divided by the amount of available water) peaks earlier...”

p5, line 126 (then p6, line 146 and p11, line 252): The way you wrote these different sentences, I am not sure whether the fixed lapse rate of -6K/km is used in both the 1- and 2-way simulations. From line 126, I think yes but then I was a bit puzzled when reading “*as is done in the 2-way coupled configuration*” in line 252. Finally, in lines 250 to 254, you mention a computed lapse rate that you compare to the fixed lapse rate of the 1-way simulation.

If I understood correctly, the lapse rate used for downscaling the temperature from CESM's to CISM's grid is fixed in both simulations. I'd add in line 126 that it is the case in both simulations, to make it clear the first time it's mentioned. And I'd remove “*as it is done in the 2-way coupled configuration*” entirely in line 126.

Then, in section 4, lines 250-254 I think the lapse rates that you mention you're computing are computed offline, in the same way as you computed the lapse rates for the ME feedback. I'd add here for clarity that those are computed offline I would then add, either in line 252 or 253, something along the line of “*compared them with the fixed applied lapse rate used to downscale temperature and LW during the simulations*”. I'd also remove the 1-way simulation mention if the 2-way simulation also uses a fixed lapse rate for temperature and LW. I think adding that would make the reader know immediately which lapse rates you are referring too and would make the reading easier.

p5, line 137: You're mentioning the fact that, since the coupling from POP2 to CISM2 is not implemented yet (presumably because you need a way to downscale ocean temperatures onto the ice sheet grid in order to be able to resolve the fjords), there is no direct influence of the ocean on the ice sheet via it's forcings on marine terminating glaciers. Could you add a few words about the potential biases this could lead to and the processes involved?

p6, line 165 to p7, line 193: Section 2.4 (metrics definitions)

I don't think this section is necessary in this form. Some of the concepts are useful to define in a thesis but are well known to the readers of scientific papers (e.g. the definition of ELA) and others are only used much later in the manuscript and should, in my opinion be moved there. I'd keep the definitions of lapse rates, GBI and the moving average and remove the ELA, NAO and IVT.

- Lapse rates: I am not entirely sure what the lapse rates refer to here. I think you're using them to isolate the melt-elevation feedback as this would be the only way to evaluate that the ME feedback leads to more melt in the 2-way simulation since the SMB doesn't decrease as much in the 2-way simulation and the snowfall doesn't differ much. I'd move the definition to section 4.1.
- GBI: I'd keep the definition in the manuscript as you're using the modified GBI proposed by Hanna et al. (2018), which they call GB2. I'd move the definition to section 4.2 and would add that what you're using is called GB2 in Hanna et al.
- Moving averages: I'd keep that one to remind the reader that the length of the moving average changes during the simulations but I'd put it at the end of section 2.3 (simulation design).

3. Simulated mass loss

p8, lines 216-218: Is there a numerical threshold in the NAMOC index for you to consider it to collapse or are you looking at increased rates of change?

4. Climate feedbacks

p12, line 262: I'd add *orange dashed line* after *Figure 5c* so the reader can spot the point without reading the caption.

p11, caption figure 4: the blue line shows the monthly mean surface temperature, not the red line.

6. Discussion

p21, line 425: *By considering the surface temperature* ? What does considering mean here?

Typos, spelling, punctuation

p4, line 103: ice thermodynamicS model instead of thermodynamic? Like in ice dynamics model?

p4, line 106: same

p6, line 140: simulationS design?

p6, line 146: -6 K km⁻¹ instead of K/km (as p5, line 126)

p6, line 148: "In contrast, the 2-way coupled run..". *to the 1-way coupled run* in not necessary here as you were just talking about the 1-way coupled run in the previous sentence.

p6, line 157: I'm not sure the last part of the sentence (after *which*) is grammatically correct. I would write "the 4xCO2 scenario is an extreme warming scenario and, after the year 140, has a similar radiative forcing to that of the SSP5-85 scenario at the end of the 21st century".

p7, lines 197&203+p8, line 206: 20-year centered moving average (with a hyphen)

p8, line 207: returns within one standard deviation OF the PI mean.

p10, line 244: no comma after *we account for this*

p10, line 245: comma after *while* if you put one after *1-way simulation*

p12, line 264: no comma after *simulation*

p15, line 311: *not significantly* instead of *not significant*

p16, line 335: here you express SLR in cm but in line 321 you express it in m. Can you check

throughout the manuscript and pick one?

p18, line 372: no comma between *forcing* and *are*

p21, line 395: *as* opposed to?

p21, line 399: comma before *although*

p21, line 403: comma after *cloud cover*

p21, line 404: the sentence is a bit long so I'd start a new one after *transmissivity* (This aligns...)

p22, line 433: *tens of thousands of years*

p22, line 448: no comma after *level*

p22, line 449: no comma after *small*

p22, line 450: *if we were extending* I think