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Interactive comment on "Dynamic response of Antarctic Peninsula Ice Sheet to collapse of Larsen C and George VI ice shelves" *by* Clemens Schannwell et al.

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General comments

This paper from Clemens Schannwell and his colleagues investigates, in a timely manner, the response of the Antarctic Penisula glaciers to a collapse of the Larsen C and Georges VI ice shelves. They use three types of numerical models of different complexity applied to two main experiments, supplemented by secondary experiments. Experiment 1 starts without ice shelves, as if an ice shelf collapse had already occurred, Experiment 2 starts with the current geometry and use a calving law and potentially leads to ice shelf collapse, depending on future scenarios, which are either RCP4.5 or RCP8.5. The secondary experiments helps to quantify uncertainties, and are built upon Experiment 2 to which was added mild or strong sub-shelf melting, and a last experiment use another dataset for ice geometry (the one of Huss and Farinotti 2014) instead of the classical Bedmap2 dataset used elsewhere in the study. All those experiments are simulated from today to 2300.

I find this study very interesting and I think this can be published with minor revisions. The fact of applying different types of ice sheet models, having not only different physics for time evolutive simulations but also different approaches for building the initial spin-up (inversion + relaxation), to the same case of study is not easy but it makes the results more robust. The paper is globally well written apart from some details on which you will have specific comments below. The description of the methods is quite clear, even though it lacks the definition of Glen's flow law from which the authors could introduce the enhancing factor, which should be defined clearly.

I first have a couple of minor concerns:
- The Spin-up for BISICLES at a 1000m resolution looks odd from Figure3, the results doesn't seem to converge with the resolution (dvdt at 0 very rapidly for 4000, 2000 and 500m resolutions, but quite different for 1000m). Did you, by any chance, accidently shift, say, the colors for 4000m and 1000m ??? If not, could you make a few comments on that in the paper.
- I was not always sure about the type of SMB that you applied, for the spin-up but also for the experiments. You introduce the Albmap SMB but in the inversion process, not in the spin-up neither in the description of transients. Make it more clear in the text.
- Could you change the time origin in the evolutive plots to be 2000 instead of 0 ?
- Could you mention that sub-shelf melting is always 0 (if I understood correctly), apart from your two additional experiments
- I would be glad if you could indicate the position of the calving front in your maps of Experiment2 results, for 2100 and 2300 for instance, or maybe, if it's more easy, indicate the year of collapse in Figure7. This would strongly help the understanding of ice dynamics differences between Exp1 and Exp2.
- I would be in favor of adding a table to summarise all the experiments, including the main exp1 and Exp2 but also the three others.
- I would also be in favor of adding those results to Table2, and put the two tables in the supplementary, to help the reading and understanding of what has been done.
- Some assertions are not always correct in the reading of the results (see below)

The rest of my review is a series of specific comments and recommmendations, which

I would like to be followed as well.

Specific comments

Page1
l17: Could you mention those other mechanisms ?
l18: Could you add a word like "slightly" just before increased ? This is at least what I understand from the Jansen et al., 2015 study
Page2
Figure 1:
- The Y label should be "elevation [m a.s.l]"
- In the caption, "localities mentioned *in the text*"
- In the caption, remove "below sea level"
- In the caption, replace "Black polygons..." by something like "The grounded part of the ice sheet only is represented"
l8: "a tendency...": Here the way this instability works doesn't appear clearly. Nowhere there is written that you have deepening of the bedrock towards the interior, which is a necessary condition (at the necessary condition that bedrock is below sea level) to a MISI. Be more precise please.
Page3
l10: Why is the SIA not valid at the grounding line ? could you mention the reason or/and add a citation here ?
l21: The way this is said, it is not clear whether the condition is imposed at the grounding line, to me at least... Could you rephrase.
Page4
l11: Is there a reason to take 0.5 specifically ? Tsai et al., advises $f<0.6$, Brondex et al., takes 0.5. Could you make a few comments on that.

Page5:
l9: So the criterion in Bisicles is Height surface crevasse + Height basal crevasse = ice surface ? this is not clear to me
l12: Do you need a capital H in historical ?
l29: You should have defined this enhancement factor before reaching this part. It is worth to detail the Glen's flow law equation somewhere above, from which you can easily define the enhancement factor.
Page6
l7: I am glad you showed Figure A3 (Hum, this figure reminds me Belgium...)
l11: m=1 ? Something is wrong here, or is this a typo ?
l18: You used SMB for the spin-up only (and alos the transient for , could you be more accurate here
Page7
Figure 2: Is that the PSU3D grid that we can distinguish in Figure2 ? Why is that so ?
l4: " In the first simulation (hereafter Experiment 1)" -> in Experiment 1 ? l5: This is not clear what you took for SMB in Experiment 1 ?
Page8
Figure3:
- Isn't that curious that the 1000m resolution results for BISICLES are outlying compared to the others ? I would have expected the results to converge when getting closer to 500m resolution, but it is not the case here. Could you discuss that ?
- The solution of PSU3D is not really converging. The model can't be applied at a lower resolution ? Could you add few comments on that if you find it relevant. l1: "best available": I don't think this is relevant to say so, no offense...
Page9
l5: Results and discussion
l12: Sea-level rise by 2100 ?
l17: The differences in terms of sea level response may be due to those large differences that you have between friction coefficients fields ?

l18: Could you maybe detail the relevant specific differences ?
Page10
Figure4: Could you start your time scale at 2000 ?
l1: where do we see the dependence to grid resolution in Figure A4 ? This is rather observed in Figure A6. And according to Figure A6, there is also a sea level contribution dependency to grid resolution in PSU3D. Moreover, this is true that this dependency is small for Georges VI (not absent though) and comparable to BISICLES for Larsen C. Could you rephrase here.
l4: you definitely refer to Figure A6
Page11
Figure5: It seems that the 2000 grounding line is slightly different between PSU3D and BISICLES. The differences are difficult to quantify, so could you maybe write down somewhere the maximum difference between initial grounding lines for the two models?
l10: A necessary condition to have a MISI is a retrograde bed slope, insofar as you have a marine based basin as well. You thus need to replace "mostly marine-based outlet" by "retrograde bed slope something..."
l13: Could you discuss this a bit more. There is this paper from Gudmundsson et al., in 2012 and Gudmundsson in 2013 about the buttressing provided by an ice shelf to its upstream glacier as a function of the grounding line gate width...
Page12
l3: "dependent"
l7: Remove "fixed calving front simulation, immediate shelf-collapse scenario" and keep "Experiment 1"
l10: Remove "just"
l10: "the larger" -> "the largest"
Page13
Figure7: Time between 2000 and 2300
l1: "projections ... agree well": this is not really what I see in Figure7a, where the

two models may agree for the first 50 to 70 years, but not really for the rest of the simulations. Could you rephrase ?
l4: "BISICLES projects no slr for RCP4.5": more correct would be to say "small" or "limited" instead of "no", especially for Larsen C
Page14
l1: "Both sheet-shelf models project similar sea-level rise by the mid 22nd century in Experiment 2": Again, I don't agree with this sentence, after 250 years Larsen C ice loss is 0.25 and 0.6 for BISICLES4.5 and PSU3D4.5, and this is not the only example where I find differences where you write the opposite (see above). You should rewrite here.
l2: What do you mean by "forced back" ? Does it simply mean that the grounding line retreat ? Could you rephrase ?
l3: "because a fixed calving front": Here I dont 'understand, the fact of having a fixed calving front does not prevent the retreat of the grounding line. You need to rephrase.
l1 to l14: For this paragraph, this is not cristal clear to me if you talk about Georges VI or Larsen C ice shelf. Can you make the text more clear please.
l25: Don't understand this sentence. you say that your model show a strong dependence to what ? Calving criteria or sub-shelf melting ?
Page15
l3: Ok, this is another experiment. I recommend to add a table to summarize all the experiments
l8: five more drainage basins, means not the LarI to LarV and GeoI to GeoV ? could you indicate in a figure which are the supplementary basins that you accounted for ? Maybe put it into Figure8 ?
Page17
Table1: What did you put in parenthesis from the dGL and dGt/dt columns ? Does it correspond to the year you had the maximum speed up ? You need to write it down then.
Table1 and Table2: Would you like to move those 2 tables in the Supplementary ? I

have the feeling that it affects the reading of the paper...
Page19
l4: You definitely need to show your results with the Fuss and Farinotti geometry

Suplementary
Figure A3: Could you explain why you chose $\lambda_C = 10^{-1}$? It doesn't seem that obvious looking at A3c. Stupid question maybe, why is there a jump between 10^{-1} and 10^{1} (I mean no 10^{0} appearing) ?
FigureA6: Time origin should be 2000