

Interactive comment on “Ocean forced evolution of the Amundsen Sea catchment, West Antarctica, by 2100” by Alanna V. Alevropoulos-Borrill et al.

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In this work, the authors present projections of evolution of the Amundsen Sea Catchment of the Antarctic Ice Sheet (and resulting contributions to global sea levels) under a range of climate forcings provided by CMIP5 global circulation model simulations. The specific model results used are based on assessments of model skill in the Southern Ocean, and subshelf melt-rate forcing is computed based on model-computed ocean temperature anomalies. There is also a fairly careful study of the effects of uncertainties in the initial problem setup due to the parameter estimation (via inversion) required for the initial model state to match present-day observations.

This is a very nice paper, for the most part well-written and clear. With some exceptions

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(outlined below), the methods used are clearly documented, the results are presented clearly, and the discussion is thoughtful, careful and clear. I have only minor suggestions as listed below, and would support publication once these are addressed.

2 General Comments

My main high-level suggestion is to consider including the dependence on initialization that you demonstrate as a key point. As a modeler, I think this is a major takeaway from this article – you demonstrate how projections can be very sensitive to something which is almost always only mentioned in passing and can be something of a dark art. You even make it the final sentence of the paper, which indicates its significance. After reading the article a few times and also looking at Nias et al (2016), I admit that I'm confused as to whether you're running full-continent AIS simulations while forcing only in the ASE sector, or if you're running a regional model. I think it's the former, but that should be clarified.

Graphs in my preprint are too small to read easily when printed on paper. They should be larger in a final printed version. »» The font sizes of the figures have been increased and are included in the revised manuscript.

Specific Comments 1. line 55: Seroussi and Morlighem (2018) is an odd reference to use here, since that work focused primarily on evaluating approaches to discretizing subshelf melt in ice sheet models, not the effects of subshelf melting in general. »» Thank you for pointing this out. The reference has been amended to Schoof (2007).

2. line 219: I'd suggest the use of "finest" instead of "maximum" when discussing resolution. »» Done.

3. line 253: I don't think "accounted for" is the right way to say this. Perhaps "addressed by"? »» Done.

4. line 257: What is r? Did you mean b (bedrock)? (would it be simpler to simply say "grounded" and "floating"?) »» Thank you for noticing, this should be bedrock b and

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has been changed.

5. section 3.2: As I mentioned in the general comments above, I think you're running on a whole-continent AIS domain, but I don't think you explicitly say that anywhere (apologies if I missed it). In this section, it would be helpful if you can provide the following: (a) Explicitly state which domain you're solving on (a picture with your initial velocity field might be helpful here). (b) Are you modifying the state outside the ASE in any way? (For example, in other works, BISICLES has been run with high friction values imposed outside the catchment of interest to "turn off" the flow there, isolating the flow to the single catchment – are you doing anything like that here?) (c) What are you doing for marine forcing outside the ASE? »» We have clarified this in line 354 of the updated manuscript: "We perform regional simulations of the ASE sector on the domain defined in Cornford et al. (2015)."

6. line 356: You might also consider citing the recent paper by Gudmundsson, et al (2019) here. »» We have included this reference, thank you for the suggestion.

7. line 366: I'd suggest rephrasing "...grounding line retreat in 2100 relative to...", possibly as "grounding line positions in 2100 which are retreated relative to..." »» Done

8. line 367: I'd suggest rephrasing "The individual ice stream response...", possibly as "Response of the individual ice streams..." »» Done

9. lines 489-513: This paragraph doesn't read as well as the rest of the paper and could use some editing for clarity. »» This paragraph has been edited: "Existing modelling investigations exploring future ASE mass evolution indicate a range of SLE contributions by the end of the 21st century, due to the differences in model physics and experimental design. Cornford et al., (2015) found a 1.5 to 4.0 cm SLE in response to the A1B scenario from CMIP3, which is consistent with our findings, despite the A1B scenario being of a lower magnitude forcing than RCP8.5. Furthermore, a 16 member ice sheet model intercomparison study projecting the response to an RCP8.5 scenario by Levermann et al. (2019) gave a 90% likelihood upper bound SLE contribution of approximately 9

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cm relative to the year 2000, with a median of 2 cm. Whilst the uncertainty range in their investigation is derived from the differences between the ice sheet models, and thus their resolutions and model physics, the study does not account for uncertainty associated with individual model configuration which would result in a greater uncertainty range in SLE projections. Our projected 21st century sea level rise estimates are broadly consistent with existing investigations despite the use of alternative forcing scenarios and models."

10. line 492: This sentence is a bit convoluted. I think you meant to say "instead performing" instead of "instead of performing", perhaps? »» This sentence has been removed in the editing of the paragraph (as proposed in comment 9).

11. line 495: In fairness, our catchment-independence results in the 2019 GRL paper suggest that you're safe doing basin-scale models for O(100 years) or so. »» This sentence has been removed in the editing of the paragraph (as proposed in comment 9).

12. line 526: As I mentioned above, I think this is a key point of your work here (and should be consequently included in the "key points" at the beginning). »» We accept this suggestion and the sentence has been added to the key points.

13. Table 1: It's odd that the basal traction isn't monotone as you progress from low end through optimum, median, and high-end, the way all of the other parameters are. It might be helpful to comment a bit more on that. Are there other ensemble members which have similar "high-end" responses while also being monotone in all of the quantities, relative to the other cases? As you discuss in the text, this case is interesting because it helps set off the competing impacts of varying friction vs. viscosity, but on the other hand, it might obscure more basic relationships in these quantities. »» We have amended the following discussion paragraph to incorporate this suggestion and hope it sufficiently addresses this comment. "We find the uncertainty associated with the ice sheet model parameters, C , φ and M_b , obtained in the initialisation procedure

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alters the sensitivity of the ASE response to ocean forced basal melting. The sensitivity of projections to uncertainties associated with model parameters increases with increasing magnitude of ocean forcing, consistent with Bulthuis et al. (2019). Generally, increased (decreased) viscosity, basal traction and decreased (increased) initial basal melt act to suppress (amplify) the mass loss from the ASE ice streams and projected SLE estimates, which is illustrated by the results of the full N16 ensemble. However, the response of the region to the perturbed basal traction parameters is not consistent with the expected trend that has been illustrated through linear regression (Nias et al., 2016), instead perturbed parameters increase in the order of optimum, high-end, low-end, median while the mass loss increases from low to high. This relationship may arise partly because our experiments explore only a sample of the theoretical parameter space, whereas other, unmodelled, parameter combinations might show clearer dependencies. However, the lack of linearity between basal traction and mass loss may also indicate that the latter is more strongly influenced by variations in, for example, ice viscosity, than by basal friction. The range of SLE projections in response to varied ocean forcing is therefore dependent on the specific combination of these individual spatially varying parameters, and in our experiments, the range in SLE uncertainty attributable to parameter selection exceeds that from choice of AOGCM forcing.”

14. Figure 5: It would be useful if you labeled various ice streams (and other features) referred to in the text in at least one frame of this figure. »» Labels have been added.

Technical Comments 1. line 50: Formatting is odd here, with 2 words and the remainder empty space. Perhaps an errant line break? »» Amended.

2. line 199: missing period at the end of the sentence. »» Now added.

3. line 224: The formatting of this equation is off – the dot for the divergence appears to be a period. If you’re using latex, use the `\cdot` character; in MS Word, there is a dot-product dot in the math character set. »» Amended.

4. line 241 (eqn 3): More equation formatting. Along with the dot-product issue already

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mentioned above, I’m confused by the character above the viscosity (μ). I think it’s meant to be the overbar (indicating vertical averaging?). Finally, there is an accent over $\dot{\epsilon}$, rather than what’s generally a time-derivative dot. (in latex, `\dot{\epsilon}` produces $\dot{\epsilon}$. In MS-Word, there is a way to do that using equation formatting.) »» This has been changed now, thank you.

5. line 248: There should be a comma between "n=3" and "satisfies". »» Done

6. line 257: Formatting for this set of cases is a bit off (too crowded, vertically) – this is true for equations 6,7, and 8. »» These equations have now been modified.

7. line 269: Same issue with the dot-product formatting. »» Done.

8. line 272: "...above, BISICLES..." → "...above, the BISICLES..." »» Done.

9. line 276: "...results of initialization" → "...results of an initialization..." »» Done.

10. line 284: I’d suggest "consistent with present day observations" »» Done.

11. line 301: I don’t think you need (or want) a comma after "forcing" »» Removed.

12. line 368: "response... differs" »» Changed

13. line 378: I don’t think "denote" is the right word here; perhaps "indicate"? »» Changed

14. line 393: should it be "has a lesser", or "have a lesser"? »» Changed to have

15. line 400: I’d suggest commas after "Pope" and "Smith" »» Commas added

16. line 450: I’d suggest "response" instead of "behaviour" »» Changed

17. line 463: Do you mean fig 6b here? »» Yes, this has been changed

18. line 479-484: This is a pretty long sentence... »» Shortening of this sentence has been considered but the authors have decided to keep it unchanged.

19. line 482: "models... produce" »» Changed

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20. line 483: I think the semicolon should be a comma here, since what comes after isn't an independent clause... »» Changed to a comma.
21. line 485: "responses... illustrate" »» Changed
22. line 486: (same thing) "responses ... highlight" »» Changed to '...melt forcing, highlighting'
23. lines 498-499: I think this is a sentence fragment. ?? »» This has been considered and the authors have decided to keep this sentence unchanged.
24. line 508: "captured by the within model configuration"? »» This sentence has been changed in the editing of the paragraph (as proposed in comment 9).
25. line 512: "our range... is marginally..." »» Changed.
26. line 516: Do you mean fig 6b here? »» Changed
27. line 550: "cavity resolving" → "cavity-resolving", perhaps? »» Changed.
28. Figure 6 caption: Did you mean the "400-00m layer" as stated, or is there a typo? »» This is a typo, thank you for identifying it. It has been amended.

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-202>, 2019.