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The Cryosphere Discussions

Interactive comment on "Kinematic response of ice-rise divides to changes in oceanic and atmospheric forcing" *by* Clemens Schannwell et al.

Anonymous Referee #2

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1 Overview:

As the title states, this work attempts to understand the kinematic responses of ice-rise divides to changing oceanic and surface mass balance (SMB) forcing, with an aim to understanding the causes of past ice-rise migrations evident in observations of isochrone patterns in existing ice rises. The authors initialize their model to match present-day conditions for two East-Antarctica ice rises, and then perform a set of numerical experiments to examine the effects of surface-mass-balance (SMB) forcing and ocean forcing (via ice-shelf thinning). the experiments are well-conceived and the results show an interesting differentiation in the rates of response as seen in the ice-

divide positions. This is a nice piece of work which deserves publication after some issues have been resolved.

My biggest concern is that the simulations are under-resolved. In fairness, the issue is mentioned in the text, but mostly in passing, when in reality, under-resolution has the potential to call all of the results in this work into question. It's clear that the "regular"-mesh runs are under-resolved, given the major differences between the "regular" mesh and the "refined" 500m one. The 350m mesh looks promising, but you need another data point to demonstrate that you're in the convergent regime, since the regular→500m→350m runs don't appear to show any sort of consistent trending behavior (I'm specifically looking at the long-term behavior in figure 6a here – assuming I'm reading the results correctly, the trend from "regular"→500m is to reduce displacement, then the trend from 500m→350m is to increase displacement, so there's not much of a consistent convergence signal). Ideally, you'd run one demonstration run finer than 350 m which would reinforce the trend from 500m→350m and would demonstrate that the 500m mesh is sufficiently resolved to capture the same dynamics as the more-refined solutions. Otherwise, you really don't have a lot of confidence that you're entering the asymptotic regime. I do realize that might be computationally unattainable. A shorter test run may well be sufficient to make this case.

In all honesty, there doesn't seem to be much point in spending as much time and space as you do on the "regular" mesh results, since they are so clearly under-resolved as to be of dubious value.

I'm also concerned about trying to glean so much information from ice divide positions when much of the response is distances which are less than a single mesh cell. Is there perhaps another quantity which might be useful to reinforce your conclusions? In modeling, integrated quantities are often more useful for filtering out any mesh-dependent noise. Perhaps some sort of weighted moments of the ice thickness or patterns of changes in ice thickness would be useful here.

2 Specific points:

1. Figure 1: You should note in the caption that the inset figure (b) is rotated with respect to the full-continent figure (a).
2. p2, line 9; "ice-dynamic archive" should be "archives"
3. Figure 2: It would be helpful to point out that subfigure (e) doesn't necessarily correspond to subfigure (b) and subfigure (f) doesn't necessarily correspond to subfigure (c), although the layout encourages that assumption.
4. p3, lines 6-9. It would he helpful if these two sentences were re-ordered to match the ordering in figure 2 (e-f). (fast migration, then slow)
5. p 4, line 2: "relict" -> "relic"
6. p4, line 24: "km2" should be km^2 (2 should be an exponent)
7. p4, line 32: "... ice shelves receive..." – should it be "ice shelves provide"?
8. p5, line 5: (possibly too pedantic on my part), I'd suggest "a complete description" instead of "the most complete...". Also, I'd suggest changing "FS flow model" to ' "full-Stokes" (FS) flow model' for accessibility.
9. p7, line 1: I'd suggest changing "sea pressure is prescribed" -> "hydrostatic sea pressure is prescribed"
10. p7, line 21: you have "J_m" twice instead of "J_m" and "J_p". Likewise, line 23 has two λ_C's and two J_C^{reg}'s
11. p8, "Experimental design" –
    (a) Can the grounding lines move/retreat in your model, or are they held fixed?

    (b) How do you handle the 1km or so of remaining ice shelf once you remove the downstream shelf in the shelf removal experiments? For example, is the shelf thickness maintained at the original thickness profile? If it is allowed to change, what constraints on shelf thickness are maintained? (I could, for example, envision a response to downstream shelf removal in which velocities in the shelf remnant (and in the upstream grounded ice) increased, causing an increase in shelf flux, which could lead to thinning and grounding-line retreat.) Also, what forcing (subshelf melt + SMB + calving rules) do they see?
12. Figure 5(b): Caption above should be "FS friction coefficient", not "NS" (unless you're actually solving the Navier-Stokes equations)
13. Somewhere in the problem description, it would be useful to show the bedrock geometry you're using, to make it apparent that the ice rises seem to be on non-retrograde bed slopes, for example.
14. Somewhere in the text, clearly describe how ice divide positions are computed. You refer to the swath, and I think you're computing averages over the swath, but it's not clear how that's done. In particular, I'm more than a bit concerned that the changes in position that you're reporting are less that a single mesh spacing. How long is the swath? Do you see evidence that the ice divide could be rotating relative to the swath-normal direction?
15. Figure 6:
    (a) The figures are too small to be legible on a printed page – they're only usable by zooming in on the electronic version. Please make them larger (perhaps a 2x2 layout). thicker lines would help as well. Please ensure that the printed-page version of the figure is usable.

(b) In subfigure b, there is a jump in the displacement followed by a retreat around 350-400 years which occurs at the same *time* in the different-resolution runs (vs. at the same displacement location), which seems to indicate that it's being driven by something in the external flow. Can you comment on that? Do you have any idea what's causing it? It seems unlikely to be simple noise since it shows up in more than one experimental run.

(c) Subfigures c and d demonstrate conclusively that the "regular" mesh is under-resolved. Could you include the results from the 350m run on subfigure (d)? If they're similar enough to the refined 500m results, they would bolster the case that the 500m results are useful.

16. Figure 7: It's not clear how useful showing "regular" mesh results is, since they're demonstrably under-resolved.

17. Figure 8:

    (a) As with figure 6, these plots are unreadble on the printed page. A 2x2 layout would probably be more useful here as well.

    (b) Could you do a 350m finest-resolution run for the ocean-forcing (shelf removal) case as well?

18. Figure 9: As with Figure 7, it's not clear how useful showing the "regular mesh" results is.

19. Figure 10:

    (a) I think you've mislabled the two 90% lines? (the trends would make more sense if the East-90% and West-90% lines were swapped). If that's not the case, then it would be useful to swap them anyway and have line-color denote forcing amount, and solid vs. dashed represent east-west.

(b) Could you increase the vertical size of subfigure c? It's hard to discern what's going on after 5-6 years, particularly whether the lines stay above y=0. Additional stretching of the plot in the y-direction would definitely help here.

20. p19, line 8: "appears more less distinct" – presumably it's either more or less, unless you're aiming for a second career in politics.

21. Figure 12:

    (a) Any idea what's causing the Cartesian-mesh-like artifacts in the $|\nabla(aspect)|$ fields? I find them puzzling since you're using an unstructured mesh and they seem to be definitely some sort of Cartesian grid artifacts.

    (b) The jumps in subfigure f could be numerical noise on the order of the mesh spacing, couldn't they?

22. p 21, section 5.2. You do a very nice job here of discussing the resolution issue. I'd suggest again that there's not much point in discussing specifics of the "regular" mesh results, other than to reinforce that the 2km mesh is under-resolved.

23. p21, line 27: You spend some time here discussing rates of dynamic response. How do you choose the timesteps for the different runs? Is $\Delta t_{regular} > \Delta t_{refined}$? If you're reducing the timestep for the finer-resolution runs (which is reasonable), could faster dynamic response be a product of finer temporal resolution instead of the finer spatial resolution?

24. p22, line 4. I'd suggest replacing "first order convergence between the different mesh resolutions" with "numerical convergence with mesh resolution", because the issue here is a lack of any convergence at all, not just the inability to obtain first-order convergence (you can have (positive) convergence rates less than first order which are still at least converging)

25. p22, line 5: I think you can confidently replace "may be required" with something like "are likely required"

26. p22, line 23: should "same finite amount" be "same fraction" or "same percentage" or something similar? ("amount" implies a fixed value, like 100m)

27. p22, line 24: "the divide to migrate" should probably be "the divide migrating"...

28. Table 3:

    (a) What is the value in the second column? max GL flux? value at a certain time? Integrated flux over time (in which case the units are incorrect)?

    (b) Column 4 is labeled "GL Flux reduction", but all of the flux values in column 2 seem to represent flux *increases*?

    (c) In the first line of the table, when I subtract 23.65-9.553, I don't get 14.01 as in the table (I get 14.097, which would be 14.10). Am I misunderstanding what's being done here or is this typo?

    (d) Shouldn't the 4th column be relative to the reference flux? (change in flux)/(reference flux) instead of (change in flux)/(new flux)

29. p23, line 6. As mentioned before, if you're making the statement that things are controlled by the subglacial topography, you should show the subglacial topography at some point, preferably with a specific example.

30. p23, line 19: "factor two" -> "factor of two"

31. p24, line: 13: "similarly susceptible...than" -> "similarly susceptile... as"?

32. p24, line 26: "first-order convergence" -> "numerical convergence" or "convergence with mesh resolution"

33. p24, line 27: "that that"

34. p24, line 29: "While this does not affect the results of the paper..." – That's too strong of a statement to make without some proof. You could say something like "while we believe that the dynamic results in this work are still valid..."

35. p26, line 10: "proof" -> "prove"

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