TCD

Interactive
comment

Interactive comment on "Ice shelf basal melt rates from a high-resolution DEM record for Pine Island Glacier, Antarctica" *by* David E. Shean et al.

Anonymous Referee #1

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GENERAL COMMENTS

This paper describes the creation of maps of height change rates and basal melt rates for Pine Island Glacier's ice shelf, at high spatial resolution and with time-dependence. The authors use a wide range of input data products to produce maps that can resolve the structure of basal melting at scales of individual channel features. This will provide the community with valuable validation for increasingly high-resolution ocean models, and insights into critical processes determining the stability of PIG.

Most of the manuscript is a detailed technical description of the processes used to integrate the different data sets and optimize the output products in terms of basal melt rate. I'd hope to see more interpretation of the results in future papers. However, the manuscript is well written, a valuable description of how to get these important prod-

ucts, and even the limited "results" will be of significant interest, so I hope to see the paper published fairly quickly. While there are lots of comments, my major problem in reading the manuscript was in Section 2.9, where I needed more information to really understand the value of the initial-pixel approach to plotting the results. At the same time, this raised the question of whether there was complementary information available in Eulerian dh/dt processing, that might help with the problem that the Lagrangian processing over two years gives a large spatial along-flow average (a few km; larger than the channel and keel cross-flow scale); perhaps Eulerian is better near the grounding line, even with all the extra noise from lateral advection of surface topography?

SPECIFIC COMMENTS

Fig. 1: Add labels "Amundsen Sea" and "Pine Island Bay", and add the grounding line.

54-55: The laser and radar elevation data sets have very different issues with regard to their value for trend detection, and probably should be described separately.

Fig. 2: Would be useful to plot the most- and least-advanced grounding line on this figure, then explicitly refer to it when discussing GL migration.

82, Fig. 2c: the color scale for velocity on Fig. 2c saturates at 1 km/yr. But velocity is critical to the interpretation of the spatial scales of basal melt rates, so we need to see the fully resolved velocity field. At a minimum, include it as a full figure in the Supplement, with a color scale that resolves up to 4 km/yr even if it is stretched to improve resolution of speeds <1 km/yr as in Fig. 2c.

92, and Fig. 2: The location of the transverse seabed ridge is sufficiently important in this paper that it should be marked on at least one Main Text figure.

140-142: This is *very* technical, and won't make sense to anyone who hasn't worked on this already. The Shean (2016) thesis probably contains this information, but one option is to move this and other very technical stuff in the Supplement where you can

give it enough space without bringing the Main Text readers to a complete stop.

196: What is a "DEM-point time offset" ?

196-197: The accuracy of control points is small, and won't affect anything reported here. But (a) How is this accuracy determined? and (b) Is it really "accuracy", or "precision" of a specific measurement over the control?

199-200: I was confused by "with DEM-altimetry time offsets". Understand now (I think), but it needs more introductory text. I think the problem arises in part because you have these level-4 headers (2.2.1.1 and 2.2.1.2). If you write 2.2.1 as a single sub-section, and just use paragraph breaks to separate WV from SPIRIT discussion, then edit as a single section, it'll be clearer.

202-203, 210: I would like to see some sort of explanation for the 3.1 m bias". It is laser-to-visible, so there are no snow/firn penetration issues. If it could be traced to, say, different geoids, then it might be a spatially-dependent bias. Or, it is close to just making a sign error on MDT correction.

208-211: This is another complex "explanation" that requires more familiarity with the topic than many readers will have.

224: While it won't change your results, Andersen and Knudsen (2009) is a very old citation for MDT: if you are really using that old a product, you might want to change for more recent versions.

295-300: I think I follow what you are doing, but (a) you don't explain WHY the reverse-ordering is a good approach, and (b) it suggests to me that the optimum time stamp for the resulting product may not be as centered on the central image time as I would have expected.

346 ff: Everything before this point is describing the input data sets. Starting here, you are describing what you get in terms of products you really want to get at. Overall, the current Section 2 is too piecemeal, as evidenced by getting into level-4 subheaders.

Perhaps there is a better organization where Section 2 is just about the input data, then Section 3 (made up of 2.8-2.10) could be "Deriving elevation changes and basal melt rates". That way you'd never get to level-4 subheaders, and the transition from "data setup" to exploitation would be clearer.

362 through eq. (9): (a) "then compute ice-equivalent freeboard thickness: H = . . .". This slowed me down for a while. H is "ice thickness assuming no firn air", right? Maybe this would be clearer if eq. (8) was expressed "h = . . ." or it might just need better lead-in words before eq. (8). (b) I also had to think a bit about eq. (9). You apply divergence to firn air (d). However, you don't allow for any time rate of change of local firn air content. I think you end up accounting for this later, but here you describe it as "dropping the CONSTANT d from . . ." which seems unjustified at this time.

373 ff: If you decide that section 2.8 should stay in the existing section 2 (see comment two above here), then consider maybe Sections 2.9 and 2.10 into a separate section ("3. Basal melt rate estimation"?). Whatever you do, restructure to avoid level-4 sub-headers.

405-410: This para needs to be revised. Start by explaining the concept of channels and keels. At the moment, you introduce the idea of "vertically variable" melt rate before I have any idea in what way a melt rate might vary "vertically" at a single site.

444-450, and more generally 419-451: I found this text very difficult to follow the first few times I read it, and maybe it is still a problem. Given that Lagrangian-derived melt rate is only calculated on 1-2 year time periods, how can you really get information at smaller scales than is associated with advection over this time? The initial-pixel product is more detailed (Fig. 8), but you don't really know anything about whether it varies on such small scales. The description of the differences and limitations of the two methods is honest, so it's okay as-is, but it raises the question of whether there is valuable complementary information in an EULERIAN dh/dt calculation, especially with regard to the region near the grounding line where melt rates change rapidly. Maybe

the extra noise in the Eulerian calculation is worth it in this case?

486: The velocity error derived from look angle and elevation uncertainty makes sense. However, you cited tide and IBE error as 0.1 m. Why do you need to use the full tide, rather than the error, in this calculation?

Figure 7: (a) Add grounding line(s) to these plots. (b) Maybe make the ICESat and ATM tracks a bit wider so it's easier to see the color shading of the tracks. (c) {In caption}: There's no such thing as "ICESat-1" is there? Just "ICESat". (d) A "North" arrow would be useful (on one panel).

Figure 8: I found the arrows harder to find than they should be. Perhaps use a white outline around the edge of the arrow, and/or add a shaft to the arrowhead, and/or use lighter shades of each color as the background is generally dark.

537-540: Even with the colorbar stretch in the lower panels of Fig. S4, it is hard to see detail of melt rate on the North ice shelf, and even much of the South. Maybe choose an even smaller range for the lower panels of Fig. S4, or even add a third set of panels at a lower range, designed specifically to highlight North and South ice shelves.

609-610: How can Payne et al. (2007) be evidence both for "significantly higher than past estimates" but also "more consistent with the Payne et al. (2007) estimates" ?

611-613: You can't really claim that your estimates are less than past estimates, when they "fall within reported uncertainty". It would be better to acknowledge first that they are the same within uncertainty, but then speculate on "maybe they really are smaller, which we'd explain as . . ."

625-628: The other advantage to Ross and FRIS is that theya re further south where ICESat orbits are closer together.

TECHNICAL CORRECTIONS

Given the oceanographic interest, the use of "shelf" instead of "ice shelf" can be con-

fusing. I'd recommend always saying "ice shelf".

When referring to a paper, I tend to go with "BY Smith et al." rather than "IN Smith et al." unless it refers to a specific items like a figure or an equation. Just a more direct credit to the authors.

The use of the dash between dates is variable and confusing. "from 2008-2015" might mean "from 2008 to 2015" or "during the period 2008-2015". Try to read it out, and see if just using "to" or "between XXXX and XXXX" would work better.

Present/past tense: Wanders a bit, e.g., line 168, probably should have "mask" => "masked" and "remove" => "removed". Also in Section 2.1.3, and several other places.

Try to avoid "Figure/Table X shows that". Usually it's possible to give the science, then the figure cite parenthetically.

556, 603, 630: Remove all "We note that"; not needed.

"ICESat-1": My understanding is that,e ven though there is now an ICESat-2, the original laser mapping mission is still called "ICESat", not "ICESat-1".

10: "directly and indirectly" presumably refers to direct loss from the ice SHELF, and indirect loss from the ice SHEET, but it immediately slows down the reader.

38-48: Break this sentence up; use separate sentences for each major methodology.

74-75: I expected to see a cite to Shepherd et al. 2018 (IMBIE) here.

82: "Two additional ice shelves". Not really. North and South ice shelves are part of PIG, and you count them as part of PIG.

83-86: Confusing construction, and I'm not sure it's even true. It is true that velocities of the N and S ice shelves are relatively small, but the quoted thickness ranges are similar to most of the main trunk. In fact, the S shelf is thicker, on average, than most of the Main trunk. (Fig. 2d treated as a thickness proxy).

87, Fig. 2A: Please add the catchment boundaries to this figure.

91-92: Probably don't need a paragraph break here.

109-110: Sentence starting "Water at . . ." needs a cite or two.

135-136: Fig. 1 shows the "ASE" coastline, not the "West Antarctic" coastline.

186: maybe "extend the . . . record BACK to 2003". When I first read it, sounded like 2003 was later than most data.

201: revise to "There is a . . . (Fig. 3, Table 2)."

242-243: Isn't there also a Ben Smith (co-author) explanation of this approach for processing of ICESat along-track data?

248: "the main shelf and fast-flowing trunk". Aren't these the same thing? Okay, no, I guess the "trunk" is the grounded ice, so say "main {ice} shelf and fast-flowing GROUNDED trunk"

263-264: probably just use "high-priority areas such as . . ." and delete the "i.e., and out parentheses.

312: "m/px" is a strange abbreviation. Maybe cleaner to use "m/pixel"

314: "over a ~11-189 day period" reads awkwardly.

317: I have no idea what "barycentric interpolation" is.

336: {FYI} Mueller et al. (2012; JGR) used bed interpolation along flow lines to make a sub-ice-shelf bathymetry grid for Larsen C.

379: "Lagrangian Dh/dt basal melt rates" doesn't make sense. Maybe "basal melt rates based on Lagrangian height-change measurements"

399: "from A single, fixed"

400: "step but, in practice, . . ." (move first comma).

401: "useD", "substituteD"

521: "downstream of THE grounding line"

555: "over keels" is ambiguous; "on keels" or something else would be clearer.

577: Why quotation marks around "resolution" ?

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Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-209, 2018.