

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 #2

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This paper presents an extensive study of high-resolution satellite DEMs for determining elevation changes and ice-shelf basal melting of Pine Island Glacier in unprecedented detail. The amount of data and complexity of the analysis are impressive and well justified. A lot of care has been taken to find optimum methodologies and complementary ways of representing basal melt rates in a spatial domain that is constantly evolving. The high technical quality of the paper will make it a benchmark for related studies in the future, and the detailed results of basal melting will be highly useful for modellers.

The glaciological part of the paper is somewhat drowning in all the technicalities, but has a high potential for follow-up papers concerning links with ice-ocean interactions. It is therefore still well suited for a journal like the Cryosphere. The main things I would

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like to see improved before publication are the overly fragmented sections and the figures which are sometimes too excessive and not as informative as they could have been. More details on that are provided below, first on the figures and then on the manuscript text in line-by-line comments and edits which are mostly of minor nature.

Figure comments and suggestions:

Fig. 1: Although an impressive data set, I do not see the purpose of these regional DEM figures when the paper is all about PIG. Figure 2d would be a better opening figure, potentially with Fig. 1A or similar as an inset to show the location of PIG within West Antarctica. The present Fig. 1 could still be included in the Supplement, and in that case grounded ice and ocean areas could be masked out in panel b to highlight the ice shelves.

Fig. 2: The upper three panels could be split into a separate figure to better highlight the detailed and nice ice shelf one. The ice shelf outline is hardly visible in panels b and c, which should be improved for context, preferentially with bedrock and velocity close-ups of the ice shelves like the upper third panel of Fig. S1. That would be a good opportunity to highlight the “transverse seabed ridge” in the main paper, as well as velocity variations on the main ice shelf which are totally saturated in Fig. 2c. In that case, a new multi-panel figure would definitely be needed and could replace the current Fig. 1.

Fig. 2 caption: I assume that it is over Bedmap2 surface hillshade, not “Bedmap2 DEM”. For panel B; “...anisotropic interpolation of available data” (or be more specific). Unless grids or coordinates are included in any of the figures, the caption should say something about the location and map projection.

Fig. 3: Could be moved to the supplement, but since this paper focuses so much on methodology it is okay to keep here. Please define in the caption what the dashed lines (mean or median of all errors?) and error bars represent (16-84% spread?). The small text at the top is somewhat cryptic and should be removed or included in the caption.

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Fig. 4: This is a powerful illustration of the importance of careful co-registration for studies like this. To be improved: Make panel numbers as in preceding figures. Define what “Count” is. Include the PIG grounding line in white since that partly coincides with where the DEM variability is higher.

Fig. 5: Panel numbers missing.

Fig. 6: This figure and the elaborate caption give a good explanation for a very complex methodology, but repeats and overlaps with the manuscript text which is unnecessarily fragmented into a series of sub-sections. These parts need to be better harmonized, potentially by moving much of the caption into the main text and removing repeated information from there.

Fig. 7: A shaded relief would be a nicer background than black in panels a and c. Ice shelf outlines (in white) would also be helpful in all panels to see relations with dh/dt patterns.

Fig. 8: Switch order of panels similar to the order of discussion in the text? I am not sure if the color scale is optimum, a lot of the ice shelf appear dark with no visual contrast.

Fig. 9: It is a very good idea to separate channels and keels in the figure, but in the end it is actually difficult to see clear differences between panels and that also makes some of the main conclusions of the paper weaker. Have you tried other color bars or different scaling to see if differences may come out more clearly? Panel numbers are also missing.

Fig. S1: Include panel numbers. The header “anisotropic interpolation” is confusing unless more details are provided (interpolation of what?).

Fig. S2: Something wrong with the reference here. I assume you want to refer to the similar Fig. 3 in the main manuscript. See also my comment to that figure.

Fig. S4: See comment to Fig. 8. Although different colorbar stretches here are helpful

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for interpretation of Fig. 8, I still feel that a lot of area appear dark without visible contrast.

Line-by-line comments and edits:

62-66: All the information and parentheses make this sentence hard to read. Split/rewrite or remove some content. This is also good to keep in mind elsewhere in the manuscript during revisions.

67: To be clear; "... across the grounding line of the South ice shelf"

82: Fig. 2C is not a good reference here since it saturates at 1 km/yr. Remove.

83: The "South shelf" has already been introduced at L67, but more clearly here. Perhaps the order of the text should be switched or L67 can be simplified/removed.

92: This is an important feature and therefore it is somewhat unfortunate that the reader is only referred to the supplement. However, it is also visible in Fig. 2B if the ice shelf outline is made thicker to highlight the relevant area, i.e. reference (Fig. 2B and S1).

198: Four levels of sections is quite elaborate for a single research paper. I think that subdivision into 2.2.2.1 and 2.2.1.2 is more confusing than helpful.

228: I assume that this applies to the ice-shelf side of the grounding line. Suggestion for rewrite: "To provide a smooth transition from fully grounded ice to freely floating ice, we defined the coefficient ... downstream of the grounding line."

Eq. 3: t is not defined

257-259: It is not clear what the "tilt tolerance" is and how it was used.

267: Define or remove CE90/LE90

335: Mention "anisotropic" here to make the link with Fig. S1 obvious.

345: How was the finer interpolation of the coarse RACMO data done?

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352: How was firn air content removed? As a constant or time-variable?

353: Here you talk about surface accumulation, earlier SMB. Be consistent.

365: What do you mean by “dropping the constant d”? It is probably reasonable to treat it as a constant, but that should be briefly discussed/justified, e.g. in section 2.7.

418: I think the word “product” is misleading here since it’s more a description of how the calculation is done, whereas composites/mosaics are described later.

443: Is a separate section needed? It is all almost about the initial-pixel approach, and the last sentence can be moved to line 417.

482: The simplified assumption of a constant firn air content in space and time is probably a minor issue for PIG, but not for most other ice shelves in Antarctica. A brief discussion or recapture of the cited reference would therefore be appropriate.

559: In this section you discuss characteristics of channel and keel melting shown in Fig. 9, but refer to arrows in Fig. 8. That is confusing, and even more so when some features like the “prominent longitudinal keel” is generally visible in Fig. 8, but not in Fig. 9 where keels and channels are separated.

593: It is very hard to actually see this in Fig. 9. Is it possible to find a better color scheme that makes this key point more convincing?

656: FDM is not used in the manuscript as far as I can see (constant firn air content)

Table 3: Define “n”

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