The Cryosphere

Discussions

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Interactive comment on "Basal control of supraglacial meltwater catchments on the Greenland Ice Sheet" *by* Josh Crozier et al.

Anonymous Referee #2

Summary

This paper explores the factors controlling the catchments of surface rivers on the western Greenland Ice Sheet. It focuses on the relationship between basal topography and these rivers, and concludes that certain geometric aspects (basal bumps and the basal slip ratio) control the organization of surface hydrology. From there, a a possible ice-flow feedback is hypothesized based on future projected changes in melt rate and slip ratio. The sign of the ice-flow feedback is unknown.

The study emphasizes the methods (Laplace-domain transfer functions) and most of the results presented (transfer function amplitudes) are a step away from reality, limiting the extent to which results are compared to data. Accordingly, the Results section is very brief (2 pages) compared to the rest of the manuscript (17 pages + Appendix) and

the Methods section (8 pages). Phrases like "as expected" of "consistent with previous work" appear frequently, highlighting that this study is light on novel contributions. Most of the 3-page Discussion is speculative and only loosely constrained by the results presented.

The study design is flawed in that the root data (Morlighem bed DEM) are **not independent** of the validation data (ArcticDEM for the ice-sheet surface) in the regions the authors chose to study (which are, incidentally, areas where Morlighem applied mass conservation). The authors also studied one area (R7) where mass conservation was not applied; results there are not shown, but I would expect their predicted surface to more poorly match the true (Arctic DEM) surface. This is hinted at in Figure 9, but never addressed. The techniques used to generate the bed DEM must be considered in this analysis; preferably, multiple regions with bed DEM constructed from mass conservation and with kriging should be analyzed and compared to one another.

The primary question of the study (see first sentence of this review) is interesting and potentially compelling over the next hundred years or so. However, the methods address it incompletely (surface processes, such as fluvial erosion, are only speculated on) and suffer from a considerable flaw in using a bed DEM informed by the surface topography. The paper is out of balance and difficult to follow. If the authors can restructure the manuscript, address the data issues, and either refocus the central question on basal control alone or add treatment of surface-based topographic controls, this would become a worthy contribution.

Specific comments

The best-fit value of C^{0*} = 11 reported here is, as pointed out by Reviewer 1, anomolously high compared to field observations. This is especially important because the authors identify C^{0*} as a parameter that IDC density is most sensitive to (Figure 12); thus, it would seem crucial to use a realistic value of C^{0*}. The authors' finding of C^{0*} = 11 by their techniques thus suggests either (1) that other techniques

should be used to find a more realistic C^{0*} before this analysis is continued, or (2) if the authors are confident in C^{0*} = 11, the meaning and implications should be explored, which could be an interesting result.

A good paper can demonstrate much of its message through its figures alone. In this case, it is hard to follow the meaning of the figures, which are too many in number (12) and too focused on the methods, which are already well established (Gudmundsson 2003 and other work since). However, this can be readily improved. Recommendations for the figures:

Figure 1: Adapt but keep. Zoom in better on Panel A. Add labels to Panel B – is this the ArcticDEM surface, or a predicted surface?

Figure 2: Unnecessary and repetitive from earlier work, remove. Phase is not a big part of the analysis, consider discarding or at least deemphasizing (no figures on phase).

Figure 3: Could adapt and keep. Is panel B correct: thin ice (H=500 m) will best express bedrock features of 100 km scale?

Figure 4: Unnecessary; remove.

Figure 5: Unnecessary; remove all except Panel E, which could be incorporated into Figure 3.

Figure 6: Potentially useful, but why is the misfit pattern so sensitive to η? How many values of η were tested, and why is the misfit so concentrated at 10^{15} Pa s? Not what I would expect.

Figure 7: Keep; make color scales the same on Panels E and F.

Figure 8: Panel B is not useful. Are the data shown in Panel A from this paper, or previous work? Consider deleting entirely.

Figure 9: Panel A is misleading because the Stream Free Region (RSF) looks different from all other regions, which may be intended to show the influence of streams. Yet

the cause is simply much different H, u, and α in this region compared to other regions (Table 1). For better fidelity, the authors should select a RSF with similar ice geometry to the stream regions.

Figure 10: Even after a lot of thought, I am still not sure what is being plotted here. I understand the meanings of $\%d$ and Λ, but cannot understand the choice on the y-axis (difference from maximum). In the text, a normalized framework (0 to 1) is discussed, but the data here are not shown that way. The text also highlights variability at small wavelengths (P13 L6-11), but the figure presentation makes this information uninterpretable (all curves are plotted too densely at low wavelengths). Regardless, I infer that the point of this figure is to show the natural variability in both $\%d$ and Λ, by showing the values across R1-R7, and then comparing to the flow networks. For Λ, the flow networks fall within the natural variability, but for $\%d$, it does not. This could suggest something about the control of fluvial erosion, or other surface processes, on surface topography, but this is not addressed.

Figure 11: Panel A is not necessary, but Panel B presents a comparison of inferred surface to actual surface, which is essential. Why were such comparisons not run on all 7 study regions? Yet, the text (P13 L16-20) declares the slope-area metric to be of limited utility, according to previous work and data from this study. Thus, any conclusions based on this data (P13 L21-27, P14 L1-2) should be de-emphasized or removed.

Figure 12: Keep.

I also suggest better distinguishing what is observationally based (e.g., stream networks) from what is computed here using transfer functions (e.g., flow networks).

The main conclusion, that "bed topography transfer alone can explain ~1-10 km scale ice sheet surface topography" (P13 L29-30), is not illustrated well in any one figure. It can perhaps be inferred from Figure 7E, but the spatial scale must be eyeballed, rather than shown as the independent variable like in the majority of the figures.

The Discussion section is largely uncoupled from the rest of the work. Speculation on changes in basal slipperiness on hourly to seasonal timescales (P14 L28, L33, P15 L1-2) is not relevant to the bed-to-surface propagation this paper addresses, as the stated timescale for adjustment is >3 years (P14 L14). Thus, the hypothesized feedback (increasing melt changes sliding, which changes IDC size, affects local melt water volumes at the bed, which again changes sliding), which operates on seasonal or shorter timescales, is not supported or constrained by the study. It would be an interesting concept if it could be shown, but that is not accomplished here.

The first paragraph of Section 4.1 reads like the main motivation for the study, and as such should appear in the Introduction.

Equations 7 and 8 appear in the Discussion, which is strange, and are not applied to further analysis. They should be removed.

The ideas on fluvial erosion (P16 L1-30) are potentially interesting, but again, are completely unexplored in the work. The statement "Our conformity metric calculations (Fig. 10) are consistent with an external control on supraglacial stream network geometry" (P16 L20-21) is not supported by the work. It may or may not be true, but the data were not shown to demonstrate it.

Overall, the base idea is worthy of exploration, but the paper is light on results and heavy on unsupported and speculative discussion, and does not fully consider the limitations of its primary dataset.