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The Cryosphere
Discussions

Interactive comment on "Brief communication: Glacier thickness reconstruction on Mt. Kilimanjaro" *by* Catrin Stadelmann

Anonymous Referee #1

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A) Summary

The manuscript presents estimates for the ice thickness distribution for the glaciers on Mount Kilimanjaro. The estimates refer to the years 2000 and 2011, and are based on a combination of in-situ observations, past ice thickness reconstructions derived for areas that are now ice free and a numerical, ice-flux based approach. The paper seems to have two main points. For one, the available global-scale ice thickness estimates seem to have overestimated the ice thickness for one of the two investigated glaciers. For another, the idea of using a combination of past and present digital elevation models (DEMs) to derive ice thickness observations that can be passed to ice-flux estimation approaches is suggested to hold promise for future applications. The paper has a general good quality, and the findings are certainly worth conveying to the larger

audience. Slight improvements seem necessary in the way that individual details are presented. The discussion section could benefit from a somewhat more substantial revision.

B) Major comments

1) In various instances, I had difficulty in following the details of the presentation. Often, I believe that it comes down to slightly revising the wording. In other occasions, I felt that some methodological information was missing. I understand that a "brief communication" has not the same amount of space available as regular papers, but still I think that some improvement can be done. I hope that the line-by-line comments can be useful to address the various cases.

2) Somehow, I was left in doubt on how the available Ground Penetrating Radar (GPR) measurement enter the game. They are briefly mentioned in the Data section (L. 48), do not show up in the Methods, and re-appear in the Discussion (L. 145). In particular, clarification is required for what the mentioned "assimilation" (L. 33 and 145) actually entails. As the manuscript stands now, no information is provided, and that should be rectified.

3) I had some reasonably hard time in following the discussion. I found it particularly hard to keep track of the many comparisons done for the two glaciers, targeting at the three Experiments performed in the work itself, the two (or three?) models used in the consensus estimate, and the two available sources of in-situ observations (boreholes and GPR data). To me, it would seem natural to show a figure depicting the various model results along the available GPR transects. Since both surface DEM and thickness are available for any of the various results, all information required to generate such a plot seems available. Most likely, this would help the readers to better grasp the main outcome of the discussion which, as far as I understand, rather focuses on the performance of the consensus estimate than on the results of the manuscript itself?

4) An important point of discussion that seems to have been missed is that ice thick-

ness estimation approaches as used in this study require the investigated glaciers to have some ice flux. Otherwise, the main idea behind the approaches somewhat breaks down. This point is skimmed in the Conclusions & Outlook section (L. 183) but would probably deserve some space in the Discussion section as well. May it help to explain some of the discrepancies noted between model results and observations?

5) The last few sentences of the Conclusions & Outlook (L. 190-198) seem the paper's strongest and most valuable point. Shouldn't these implications be highlighted in the abstract as well?

C) Minor comments

1) There are several undefined acronyms, including, amongst other, SRTM at L. 35, MB at L. 61, TDX at L. 66.

2) I could not follow the logics exposed at L. 61-63. According to the sentence, the surface mass balance model applied in the study was "slightly altered" because (sic) "it was never tested for Kersten Glacier before". I imagine that the model was actually tested by the authors before altering it, and that the matter is only one of wording?

3) At L. 69-72 the authors state that they removed all positive elevation differences from the analysis because such positive changes are "unlikely" to happen. The issue is that this removal apparently affects some 15% of the area of the Northern Icefield, which calls for some more detail. For example: What is the spatial distribution of these removed cells? Is it completely scattered, suggesting random noise, or is it clustered, indicating that the signal might be real after all? What is the confidence in the individual DEMs? Etc.

4) I was not able to follow L. 90-94. A "coupling length parameter" is introduced without further explanation (I assume the definition is found in Fuerst et al. 2017, which is ok) and, as far as I understand the wording, is first said to control how the surface DEM is "imprinted in the thickness field" (I'm not entirely sure what this means) and later said

to control the "smoothness" on not further specified "flux streamlines". I don't want to exclude that the wording makes perfect sense to a reader familiar with the details of Fuerst et al (2017) but I think that some additional words of explanation will help the majority of the readership.

D) Line-by-line comments

A (rather long, I apologise) set of line-by-line comments is found in the annotated document, attached to this review. The comments provided above are contained therein as well.

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2019-310/tc-2019-310-RC1-supplement.pdf

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