The Cryosphere Discuss.,
doi:10.5194/tc-2017-30-RC2, 2017

The Cryosphere
Discussions
Open Access

Interactive comment on "Application of a two-step approach for mapping ice thickness to various glacier types on Svalbard" *by* Johannes Jakob Fürst et al.

F. Maussion (Referee)

fabien.maussion@uibk.ac.at

Received and published: 13 April 2017

General comments

In their manuscript, the authors present a new method to estimate the ice thickness of glaciers and ice-caps. They rely on an established theoretical background, but the paper presents innovative ways to deal with limited or inconsistent data input. The study is solid, comprehensive, and I am confident that many readers will find the paper useful for their research.

I agree with most (if not all) of the issues raised by D. Brinkerhoff, and there is no point

Printer-friendly version

Discussion paper

in repeating them here. However, I will modestly try to offer a different perspective, driven by my personal interests (large scale glaciology and reproducible science).

Generalisation of the method to other glaciers/regions

The authors state two times (in the abstract and in the conclusion) that their method has "*data requirements which are comparable to other approaches that have already been applied world-wide*". I have to disagree with this statement, which unnecessarily raises the reader's expectations. To my knowledge, we are still far away from a global dataset of surface mass-balance and $\partial h/\partial t$. The most promising method to estimate geodetic mass-balance (DEM differencing) is rarely applied to regions larger than a catchment or mountain chain, and the global methods (GRACE, Icesat) suffer from considerable drawbacks (coarse spatial resolution and high uncertainties). Therefore, I suggest to remove this statement from the abstract.

My understanding of the study is that it presents a way to deal with uncertainties in the boundary conditions and in the observations to which the model is tuned. Most efforts, it seems, are spent into correcting $\dot{a}$ to avoid singularities and in defining a formal way to propagate observational uncertainties. In the end, I feel like the paper would benefit from a more thorough discussion about the benefits and drawbacks of their method for large scale experiments, i.e. without any observation and/or without an observational dataset for $\dot{a}$.

Structure of the paper

Like D. Brinkerhoff, I notice that the paper could gain in readability. I am however unsure how to proceed. One the one hand, I truly appreciate the authors' thoroughness, and I am sure that the interested reader will find most of the information s/he needs to reproduce the steps listed here. On the other hand, the paper is long and some-

Printer-friendly version

Discussion paper

times difficult to follow. A change along the lines proposed by D. Brinkerhoff will surely improve the paper's readability, but I would refrain from cutting too much text out of the paper: instead, move some details to the appendix or the suppl. material. (take the paragraph about the slope angle threshold for example: a specialist will probably be interested in this information, but a more general audience would rather skip these details).

Case study?

To test a new method, one should rely on the best possible data input for calibration/validation. When looking at the fields in Fig. 02 I can't really imagine that this is the case in Svalbard. It is too late for this study, but in the future I would suggest to look at more appropriate benchmark glaciers (right at our doorstep?), where data denial experiments can be realised much more easily and with much more confidence in the boundary conditions.

Specific comments

P2, L2, "virtually complete coverage" none of the cited studies (apart maybe from Paul et al, which is rather a methodological review paper) states that surface elevation change products have reached complete coverage. This is related to my general comment: the method presented in this paper is promising, but still belongs to the "demanding ones" in terms of data availability.

P2 L27, "apparent flux divergence" I also think that this new terminology makes no sense. One can argue about whether "apparent mass-balance" is the best term or not, but "apparent flux divergence" is definitely more confusing than helping.

Figure 5 Obviously, both the observations and the ice divides (zero flux locations) are way too visible on the bedrock topography. This calls for a more constrained tuning, either by changing the way B is allowed to vary or by changing the way the model is dealing with small slopes?

Figure 8 I understand the reason for using normalized values here, but from a volume estimation perspective (e.g. sea level rise), other metrics are much more important: bias and absolute error (i.e. small relative errors for large thickness values can be more relevant than large relative errors for small thickness values). Have you considered looking at absolute values, too?

Editorial comments

P3 L15 dot is missing

Fig. 02 intuitively, I associate blue values with positive mass-balance / surface change and red with the opposite. Consider reversing your colortable.

All figures consider damping the topographical shading, which is currently very strong without a clear added value.

All figures consider using another colormap. Rainbow (or "jet" in python) is now considered by many as being misleading (e.g. https://www.climate-lab-book.ac.uk/2014/end-of-the-rainbow/ and many further refs online)

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2017-30, 2017.