The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-25-RC1, 2020

The Cryosphere Discussions
Open Access
EGU

Interactive comment on "Revealing the former bed of Thwaites Glacier using sea-floor bathymetry" *by* Kelly A. Hogan et al.

Matteo Spagnolo (Referee)

m.spagnolo@abdn.ac.uk

Received and published: 21 February 2020

Review on: Revealing the former bed of Thwaites Glacier using sea-floor Bathymetry by Kelly Hogan et al.

Dear Editor and authors. Thank you for the opportunity to comment on this manuscript. This presents a new high resolution bathymetry dataset for one of the least accessible regions offshore Antarctica, proximal to the Thwaites Glacier, whose vulnerability and relevance to sea level could not and should not be underestimated.

Being able to collect, present and analyse data so proximal to the ice shelf of the glacier is exceptional and would warrant publication in its own right. However, the authors present a number of interesting observational and quantitative analyses that

lead to relevant implications relative to the dynamics of ice flow and ice shelf behaviour. All in all, I can only recommend publication of this manuscript, which is very well written and illustrated.

I have, however, some recommendations. Besides some 40+ comments that I attached directly to the pdf version of the manuscript, I would like to highlight, in no particular order:

1. The main conclusions that emerge from the discussion, and in particular the idea of soft and hard bedrock highs and their implication to ice dynamics, are very important and yet there is little mention to them in the abstract and no mention at all in the title. I would therefore recommend incorporating this and other highlighted points in the abstract and consider an alternative, more result-focused, title 2. Evidence of glacial erosion on the flat-topped highs does not necessarily implies that the fat top morphology is due to erosion. It could be simply related to the presence of harder horizontal layers in a sedimentary rock. Nonetheless, the role of a more or less thick blanket of glacial sediment that could be eroded and its importance for a potential feedback mechanism remains valid. 3. Generally speaking, there are a number of sentences that are a bit vague, and I have highlighted places where the authors should make an effort, if possible, to quantify mentioned differences, similarities, significant implications for, etc. This is especially important in terms of CDW. How much the refined topography of this new bathymetry redraws the estimates of CDW incursion towards Thwaites grounding zone? 4. The spectral analysis description (as the entire manuscript), is interesting and very well written but comes across as rather technical, and a departure from the rest of the manuscript. I recommend the authors to look into ways of making it more accessible to the wider glaciology community, perhaps by moving some its technicality to the supplementary materials and/or by taking greater advantage of an illustrative example. On the other hand, I had the impression that some of the key parameters used in the analyse are not fully explained, but this could all go into the supplementary material.

Otherwise, I am looking forward to seeing this paper published, Matteo

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2020-25/tc-2020-25-RC1-supplement.pdf

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-25, 2020.