The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-68-RC2, 2020

The Cryosphere Discussions
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Interactive comment on "Subglacial lakes and hydrology across the Ellsworth Subglacial Highlands, West Antarctica" *by* Felipe Napoleoni et al.

Anonymous Referee #2

Received and published: 23 April 2020

This paper identifies 37 new subglacial lakes in West Antarctica from ice-penetrating radar data. Radiometric properties were used to classify the confidence of these lakes. A volume estimate was made for these lakes. New topography measurements were used to make an updated DEM of the Ellsworth region so that a water routing model could be generated to investigate the potential for drainage.

This work is an important contribution to lake inventories and hydrological understanding, though some areas of this paper require clarification or further discussion. The volume estimates do not seem particularly meaningful given the assumptions made in the methods and the uncertainty of the results (see comments below). Given that the

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water routing model is the primary evidence for connected drainage, it would be useful to include more information on the topography data (e.g. survey spacing, data density). Also, improved topography is an important contribution, and the impact could be enhanced by providing quantitative information on the improvement or showing comparisons to Bedmap2.

There are some statements that seem to conflate active and stable lakes (see comments on lines 37-40), and I believe there could be more discussion on which category the newly discovered lakes fall into. Generally speaking, active lakes identified with satellite observations do not have a clear radar signature, and RES-detected lakes are not observed to have surface elevation changes. The authors hypothesize that these lakes are part of a dynamic drainage system and speculate about cascade-type drainage. It is fine to suggest this, but the fact that many of the lakes in this study are "definite" RES-detected lakes indicates that they could very well fall into the inactive RES lake category. So far, no active drainage has been observed in this region. Previous investigations of SLC and SLE have concluded that these lakes are stable. Perhaps there is a more nuanced stance where RES lakes can be part of a drainage system without the dramatic ice surface drop of active lakes, and the authors do acknowledge that satellite observations of change would be required to confirm drainage. But I think it is important that the authors discuss these contradictory pieces of evidence.

Line 16: "reported acceleration of ice velocity"

Reported an acceleration of ice velocity?

Line 37-40: "These active subglacial lakes have been identified using a range of techniques including satellite measurements of ice surface elevation changes (e.g., Wingham et al., 2006; Smith et al., 2009), characterisation of the subglacial topography from ice surface data (e.g., Bell et al., 2007; Bell, 2008; Jamieson et al., 2016); airborne radio echo sounding (RES) (e.g., Robin et al., 1970; Popov and Masolov, 2003); and/or

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ground-based RES (e.g., Rivera et al., 2015)."

It is unclear what is meant by the identification of lakes through the "characterisation of the subglacial topography from ice surface data." Bell et al. (2007) detected active lakes using satellite data, similarly to Wingham et al. (2006) and Smith et al. (2009). Bell (2008) reviews subglacial lakes gathered from a variety of different sources and surveys, including active lakes detected from satellite data, and non-active lakes detected with radar. The Jamieson et al. (2016) study does not identify lakes. Rather, they hypothesize about potential lake locations by running a water routing model on estimated bed topography.

Was this intended to be a statement about active lakes, or subglacial lakes in general? To the best of my knowledge, neither Robin et al. (1970) or Popov and Masolov (2003) have identified active lakes; the lakes they found are considered stable. The Rivera et al. (2015) study also concluded that their lake was stable. The only study that I am aware of that has seen any radiometric evidence for active lakes is Langley et al. (2011):

Langley, K., Kohler, J., Matsuoka, K., Sinisalo, A., Scambos, T., Neumann, T., ... & Albert, M. (2011). Recovery Lakes, East Antarctica: Radar assessment of subâĂŘglacial water extent. Geophysical Research Letters, 38(5).

Line 47-49: "Given the fact that this region is located up-ice of the fastest-changing ice streams in the world (e.g., Pine Island Glacier and Thwaites Glacier), and that they are some of the most vulnerable glaciers to ongoing climate change (Martin et al., 2019)..."

This statement could be better cited. Some options:

Rignot, E., Mouginot, J., Morlighem, M., Seroussi, H., & Scheuchl, B. (2014). Widespread, rapid grounding line retreat of Pine Island, Thwaites, Smith, and Kohler glaciers, West Antarctica, from 1992 to 2011. Geophysical Research Letters, 41(10), 3502-3509.

Rignot, E., Mouginot, J., Scheuchl, B., van den Broeke, M., van Wessem, M. J., & Morlighem, M. (2019). Four decades of Antarctic Ice Sheet mass balance from 1979–2017. Proceedings of the National Academy of Sciences, 116(4), 1095-1103.

Joughin, I., Smith, B. E., & Medley, B. (2014). Marine ice sheet collapse potentially under way for the Thwaites Glacier Basin, West Antarctica. Science, 344(6185), 735-738.

Line 85: The BBAS acronym is not defined

Section 2.3: There are two different definitions of specularity. It might be helpful to explain that the method in this manuscript is different from the specularity calculation method in Schroeder et al. (2014) to avoid confusion.

Line 139: “in a GIS”

In a GIS software?

Section 2.5: The methods for surface area and volume calculation are not convincing. The dimensions of two lakes (SLC and SLE) do not provide a statistically robust or representative basis for the dimensions of other lakes. The lake geometry outlined in Figure 3 seems unrealistic, especially if a lake does not happen to fall within a trough or if there is sediment infill. And since the data is not SAR processed, isn’t it possible that the hyperbola slopes are different from the actual topography?

Section 2.6: Given that the water routing model is the primary piece of evidence for the connected drainage hypothesis, I think you can elaborate on the topography. It would be helpful to include information on radar grid spacing, changes from Bedmap2, or percentage of grid cells within 5km of a data point.

Lines 255-256: Is the order of magnitude uncertainty in cumulative lake volume really make this a meaningful result, especially given the assumptions in volume calculation?

Line 302: “the range of length notably smaller”

The range of length is notably smaller?

Line 310: “poorly constrained”

Are poorly constrained?

Section 4.2: Is it being hypothesized that these lakes are active lakes? It should be discussed that radar-detected lakes do not have drainage that can be observed from the surface. Or maybe radar-detected lakes are more dynamic than previously expected, but not active enough to be seen from the surface? If you are hypothesizing a more active regime, it might be helpful to cite the MacKie et al. (2020) study which predicts that there are active lakes in the Ellsworth region.

MacKie, E. J., Schroeder, D. M., Caers, J., Siegfried, M. R., & Scheidt, C. (2020). Antarctic Topographic Realizations and Geostatistical Modeling Used to Map Subglacial Lakes. Journal of Geophysical Research: Earth Surface.

Line 335-335: “The identification of any such episodic draining (e.g., Wingham et al., 2006) would require analysis of ice elevation changes to capture water infilling/drainage through time.”

Following on the comment above, there’ll have to be a thoughtful statement about why you think these lakes could be detected by satellite, when so far all other radar-detected lakes (with a possible few exceptions) have not.

Line 337: “down-ice”

Downstream?

423-424: Why is it assumed that the observed lake lengths are the minimum length?

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