Responses to Reviewer 2 for: "Subglacial lakes and hydrology across the Ellsworth Subglacial Highlands, West Antarctica" (MC No: tc-2020-68).

We are grateful to reviewer 2 for the helpful reviews of our manuscript and for the references provided. Below we respond (non-highlighted text) to the comments of reviewer 2 (*italic* and highlighted in grey).

Anonymous referee #2

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 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..

We appreciate all the elements and resources for improving this manuscript that the referee is offering in this review.

Specific comments:

Line 16: "reported acceleration of ice velocity" Reported an acceleration of ice velocity?

We will amend the sentence.

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 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 subglacial water extent. Geophysical Research Letters, 38(5).

We refer to subglacial lakes in general as opposed to just 'active' lakes. We will rewrite this paragraph to clarify how subglacial lakes in general are identified, and also will clarify the methods by which active lakes have been defined. We will check the specific papers as we do this.

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

We appreciate the suggested references, and we will include some, or all of them in the text.

Line 85: The BBAS acronym is not defined Section

BBAS is the name (not directly an acronym) used to refer to flight lines from the 2004/2005 PASIN survey over PIG (Vaughan et al., 2006). We will be explicit about this in the text.

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.

We will clarify in the text that we use Carter et al. (2007) definition for specularity.

Line 139: "in a GIS" In a GIS software?

We will change this in the text complementing GIS with the word 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?

Although, previous studies have made similar assumptions on the shape (i.e., circular shape) in calculating the area for their hypothesised subglacial lakes we do acknowledge the area and volume estimations are subject to very large uncertainties. The other reviewer also made this comment. As a consequence, we will remove this from the text – it will not significantly impact the overall findings of the paper.

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.

We will make sure we describe the generation for the new DEM fully and that we describe the features within it carefully with an eye on how they end up controlling the drainage and connections between lakes. We will make explicit in the text the references where details on the BBAS survey can be found and will show the unpublished radar survey grid from CECs in a figure. Also, we will show a figure with the differences in the new DEM model (this work) and Bedmap2.

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?

We do acknowledge these uncertainties in the different methods applied. Therefore, will remove this estimation from the text.

Line 302: "the range of length notably smaller" The range of length is notably smaller?

We will add the word "is".

Line 310: "poorly constrained" Are poorly constrained?

We will reword this to point out that Geothermal Heat Flux values are variable depending on the selected technique to model it; and also, that the resolution of the models may not show localized highs in the Heat flux.

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.

Thanks for your suggestion and for the reference. In this article we hypothesize that some of these subglacial lakes may be part of wider active subglacial hydrological drainage system without ice surface changes, provided that hydrological system is in steady state. As long as the rates and locations of flowing water at the base of the ice do not change, it would not affect the surface elevation or they may not be noticed on the surface. We will discuss this more fully in the text and will refer carefully to the MacKie et al (2020) study too.

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.

You are correct, very few subglacial lakes detected by radar have also been identified by satellite means. It may be the case that the lake is too small relative to the ice thickness or the recharge period is too long, and the modern satellite have not been able yet to observe one of the drainage events. We remove this statement.

Line 337: "down-ice" Downstream?

We will change it to downstream.

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

We made two different assumptions to produce two different ideas of lake size because the radar may pick up at most, the longest dimension of the lake, and at a minimum, it would pick up the shortest dimension of the lake – thus we'd produced two end member estimates (with very large uncertainties). However, we recognise these assumptions have a considerably imprecision (both reviewers commented on this) and therefore we will remove the section of lake dimensions because it does not significantly impact our key findings for the paper.