

Summary

This manuscript documents the discovery of two new subglacial lakes beneath the currently stagnant portion of Kamb Ice Stream, West Antarctica, from CryoSat-2 satellite radar altimetry and ICESat satellite laser altimetry. This is a novel observation and extends our knowledge of subglacial hydrology and ice dynamics in this region. Additionally, it provides context for understanding centennial-scale ice-stream stagnation and reactivation cycles, which are currently an important control on ice-sheet mass balance in this sector of West Antarctica. Finally, this work provides an example of using decades of observations of altimetry data – data which should be used more often – to understand subglacial hydrology and dynamics. This work has the potential to substantially add to our knowledge of subglacial hydrology and presents new techniques for analyzing long time series of satellite altimetry data. However, as currently written, there are organizational and scientific issues that must be addressed before publication.

General Points:

- 1) **Organization/structure** – This paper at times lacks a clear organizational structure with introductions, methods, results, interpretation/discussion, and conclusions. I think that following this traditional structure could be useful in this case, as many different methods are being used and it is often difficult for the reader to determine the reason for selection of the method or analysis technique applied.
- 2) **Methods and uncertainty** – ICESat laser altimetry, surface elevation, and bed elevation data are used throughout the paper with clear explanation or citation. A thorough methodology and uncertainty analysis should be added to the manuscript. Regression error and standard deviation of residuals over non-changing portions are both used as measures of uncertainty. What are the reasons for including both uncertainty methods?
- 3) **Labeling** – Labeling in figures is often inconsistent. Sometimes panels are labeled; other times they are not. I'd prefer to err on the side of caution and label. Similarly, features in the figures are also often unlabeled or annotated. For example, grounding lines are not labeled or cited, but are plotted in the figures. Lakes outlines are also not consistently labeled, so it's unclear to me sometimes whether they are from Smith et al. (2009) or Wright and Siegert (2012) or this study.

Scientific Points:

- 1) **ICESat** – ICESat laser altimetry data are extensively used but are not discussed. This is especially crucial as these data are quantitatively compared to CryoSat-2 data, and the amplitudes of the elevation change derived are similar to the uncertainty of these methods. More discussion of the ICESat data and comparison between the ICESat and CryoSat-2 data would be useful to ascertain the significance of the signals analyzed.

- 2) **Hydropotential** – It is not clear what reference datum is used. It also seems as if two different datums are used as the hydropotential values differ significantly in the two figures shown. Finally, I assume that glaciostatic hydropotential (subglacial water pressure equal to overburden pressure) is calculated, but this is never stated.
- 3) **Subglacial lake detection algorithm** – I would expect a more precise outline of the steps used, similar to the algorithm presented in Smith et al. (2009). For example, it's unclear to me what “visually inspect” means.
- 4) **CryoSat-2 data** – Several methods are presented for analysis of the elevation data (quadrature curvature surface fitting, differences from reference DEMs, etc.). It's a little difficult to determine the exact sequence of methods being used and why. A more stepwise description and possibly flowchart figure would add clarity.

Style/Language/Grammar comments

- 1) Hyphenation use is inconsistent – For example, “repeat track method” vs. “topography-free elevation” and “sub-glacial” vs. “subglacial”. I tend towards hyphenation, i.e. repeat-track and topography-free, but use should be consistent.
- 2) Capitalization use is inconsistent – For example, Antarctic ice sheet vs. Siple Coast Ice Streams. I would tend to lean towards Siple Coast ice streams and Antarctic ice sheet, but capitalization should be consistent regardless of convention.

Specific Comments

P1 L11: “We have identified two previously unknown active subglacial lakes...”

P1 L12: Rapid fill-drain events do not necessarily indicate lake connectivity via a drainage network though they do indicate a subglacial drainage network exists.

P1 L13: “lakes area” seems redundant. Perhaps just “lakes.”

P1 L14–16: This sentence should be rewritten to highlight the evidence that links subglacial lake drainages to the acceleration of thinning.

P1 L15: “subglacial lakes”.

P1 L16–17: It seems unlikely to me that this conclusion can be justified from data presented here, which is too short a time series to suggest regions for the shutdown. It could be consistent with such a shutdown mechanism. This sentence should be reworded accordingly.

P1 L16: Figure 2a does not seem to indicate rapid thinning. If anything, thickening seems the dominant signal. Perhaps the background elevation-change rate has been removed? Hopefully stated later.

P1 L17: “sub-glacial” to “subglacial”.

P1 L19: Change “rapid ice flow” to “streaming ice flow”.

P1 L20–21: This sentence reads awkwardly. This paragraph could perhaps be restructured to facilitate reading. I would suggest the following general order:

- 1) Introduce ice-stream stagnation/reactivation cycles and their effects on ice-sheet mass balance.
- 2) Note that KIS stagnated ~160 years ago.
- 3) Note that changes in basal hydrology (rather via water piracy or change in dominant subglacial hydrology system structure) are likely a cause of this stagnation.

P1 L21: Change “indicated” to “posited”, “hypothesized”, “suggested”, or similar to indicate hypothetical nature of this conclusion (even though it is likely correct).

P1 L21: Change “Siple-coast” to “Siple Coast”.

P1 L22: Change “Ice Stream” to “ice streams”.

P1 L22: Delete “of these cycles”.

P1 L24: Change “long term” to “long-term”.

P1 L24–25: Presumably it is associated with changes in basal-melt rate and upstream subglacial water supply, but how are these related? Are the authors suggesting a Tulaczyk et al. (2000) thermodynamic till mechanism or purely water piracy?

P1 L25: “Therefore, it has been suggested...”

P1 L28: Change “predict its dynamics” to “understand the ice dynamics” or similar.

P1 L30: Change “while” to “although”.

P2 L1: Delete “contributing the basal hydrology”.

P2 L3–4: “the hydrological connections between adjacent lakes”.

P2 L4: “by the sparse coverage of the ground tracks”.

P2 L5–7: Is this a supposition of the authors or are there other studies that posit this?

P2 L11–16: This paragraph mixes methods and introductory materials.

P2 L11–12: Inconsistent capitalization: Antarctic Ice Sheet here vs. Antarctic ice sheet previously.

P2 L13: “two previously unknown subglacial lakes”

P2 L14: What sort of activity? Downstream lakes fill as the upstream lakes drain?

P2 L15: Change “evidences” to “evidence”.

P2 L13–15: This sentence is awfully general. Perhaps change to something more specific to the results presented in this manuscript.

P2 L17: Seems like there should be a more extensive methods section.

P2 L21: “geophysical” should probably be “geographic”.

P2 L23: Which version of the L2 product? If I remember correctly, there are multiple processing baselines.

P2 L24: What does “interior of ice” mean? Ice-sheet interior? Why the range of uncertainty?

P2 L24–26: I am confused by this sentence. There seem to be multiple numbers for uncertainty and their dependence on physical values varies. A more complete uncertainty approach is probably needed, or at least a more complete discussion of the approach adopted here.

P2 L27–P3 L10: I think a step-by-step description and/or processing flow chart is needed here. As is currently written, the description of the processing steps is somewhat unclear.

P2 L28: What do height error flags signify?

P2 L30: Does this method follow Helm et al. (2014) or is it distinct in some way from that processing?

P3 L3–5: What does visually inspect actually mean? To be robust, I think quantitative metrics are required to determine reliability of subglacial lake detection. A protocol like that described by Smith et al. (2009) seems to be needed here.

P3 L5: What are the uncertainties estimated from the regression? It is hard to ascertain what the numbers actually are from Figure 2b.

P3 L6: What is sufficiently lower? A more rigorously quantified uncertainty analysis is needed.

P3 L12: In various time windows? Different windows seem to represent different lengths of time? I would have thought an overlapping window scheme of a constant time length would be used? If these irregular windows can be easily justified, I'd like to know why.

P3 L19: I assume this is glaciostatic hydropotential following Shreve (1972), but it would be nice to have this verified at least once.

P3 L19–20: Why not say directly that the “lakes are located in hydropotential lows.”

P3 L24–25: This sentence does not seem necessary.

P3 L27–28: “The background temporal elevation changes outside the lake boundaries are removed to examine only the elevation changes associated with the SGLs activity.” How was this done precisely?

P4 L1: Are the elevation change numbers sufficiently robust to actually derive this balance flow rate? What is the associated uncertainty?

P4 L1–4: This reverse sequential drainage seems odds compared to the sequential filling. If KT1 is supplying the other two lakes, why does it not continue filling if the water is no longer flowing to the other lakes? Does it go someplace else? Or has the inflow rate just slowed? In Figure 4, the fluctuations seem large relative to the uncertainty. Is the uncertainty here truly representative of that in these data? Can these high-amplitude fluctuations be interpreted?

P4 L5–15: Much of this seems speculative and just descriptive of the results of other studies. Are the flux rates from these lakes sufficient to maintain a connected network of Röthlisberger channels as the authors seem to suggest here? Alternate theories also seem possible. The simplest explanation may be that these lakes, separated by ~100 km, simply are not hydraulically connected and fill and drain separately. I am hesitant to put much faith in hydropotential maps on this level and much of the inflow from KT3 appears to come from a separate upstream hydraulic catchment anyway. If the authors are suggesting that a channelized hydrology system can be maintained against creep closure via this water flux, I’d like to see some calculations and type of channel suggested (R- or N-channel, canal network, etc.). I note that the one real-time observation of transient water flow under an ice stream (i.e., not a subglacial lake filling or draining, but subglacial water in motion; see Winberry et al. (2009)) indicates that the channels are probably not maintained for more than a few weeks, not the many months suggested here.

P4 L19: It is hard to verify the hydraulic head difference cited here from the figures. Looking glibly at Figure 4, the hydraulic head changes seem like they should be somewhat less than the numbers cited here. However, it’s difficult to tell if the authors are including flow focusing or some other effect in their calculation. More details are needed.

P4 L23: “role of the hydraulic barrier” or “role of hydraulic barriers”.

P4 L25–28: This sentence could be split into two or a comma should be added to separate clauses: “...KIS area, but the...”.

P4 L28: How are the LRM products used? This should be added to the methodology, or at least there should be a citation if following an established method. LRM mode differs significantly from SARin, and thus differences in uncertainty, reliability, resolution, etc., would be expected; clarification of these differences is needed.

P4 L32 – P5 L1: This is more like the patter of draining I would expect. I wonder if an extended discussion of the difference between the upstream and downstream lakes would be useful.

P5 L8–9: This sentence seems like a conclusion before the data/interpretation/discussion are presented.

P5 L12: Methods on how the ICESat and CryoSat-2 time series are combined are necessary. To do this properly, error and biases should be clearly presented.

P5 L15–17: Can you really assess when precisely KT1 stopped draining with the elevation amplitude presented in Smith et al. (2009). I would think the ICESat data would allow more precise determination of subglacial lake drainage timing. A drainage lasting this long seems unlikely compared to other subglacial lake drainages documented (c.f., Siegfried et al., 2016).

P5 L19–22: How does this estuary compare to the one documented on Whillans Ice Stream (see Horgan et al. (2013a,b) and Christianson et al. (2013)). A discussion of similarities and differences between the estuaries would be important, as progradational till deltas were directly observed in that estuary and it seems as the authors are suggesting a similar depositional structure here, with sheet (distributed) flow and channelized flow coexisting.

P5 L23–24: Enhanced lubrication isn't a necessary condition for this. Tensile forces must inherently exist at the junction of an ice stream and ice shelf (see Weertman (1974) and Schoof (2007) among others). Although additional basal lubrication could result in increased longitudinal stress. The timing of the lake drainage does seem nicely correlated with the increase thinning, but it could have but both events could have been triggered as a result of regional grounding line retreat, and thus the lake drainage could have been an effect and not a cause. Some discussion of the nuances and limitations of the data would be helpful, as well as connecting more directly to known background thinning rates and ice bottom/bedrock geometry.

P5 L30: These feature looks distinct from the channel described in Marsh et al. (2016), which does not have undulating topographic features in the along flow direction. Some discussion of why this might be would be useful.

P5 L33: I am suspicious of the 30 m/yr retreat rate. Grounding-line location was not well known along the Siple Coast in the late 1980s. Is there no newer result?

P6 L6: “to the oceans”.

P6: L4–8: There should probably be a citation to the buoyant meltwater plume circulation that drives this circulation in the channel – I'd suggest Jenkins (1991) and Jenkins (2011). Ruling out other possible channel creation mechanisms is probably needed too, i.e., highly variable bed topography, suture zones, etc.

P6 L12–13: Is the “observed” channel in the same location as the modeled ones?

P6 L15: Here and throughout the manuscript I wonder if the distributed rather than sheet flow might be what the authors are describing. Sheet flow implies a thin water film a few millimeters thick. Distributed flow would allow more generality.

P6 L19–21: Much of this seems like conjecture. Other drainage systems besides a strictly channelized system could lead to relatively rapid connectivity between lakes. Small outburst floods with transient channelization (see Winberry et al. (2016)) seem more likely to me than a long-lived R-channel connecting lakes. The flow of water along paths down the hydropotential gradient does not necessarily imply channelized flow either.

P6 L30: Perhaps “Comparison of our results to Siegfried et al. (2016)” to avoid confusing the data presented here with those derived from studies of Whillans Ice Stream subglacial hydrology.

P7 L5: “definitely” should perhaps be “definitively”?

P7 L5–8: Perhaps reword as: “At present, our results cannot definitively determine whether KIS stagnation occurred via basal water channelization, water piracy, or some combination of these. Further studies of basal water flow in the KIS trunk would be necessary to make this determination.” I don’t see the need for channelization or water piracy to be mutually exclusive.

Figure 1: Smith et al. (2009) or other sources should be cited for location of previously known subglacial lakes. Grounding line (yellow) and appropriate citations should be given in the caption. What is the date of this grounding line? Citations for MEaSURES velocity data and MOA imagery are similarly needed (I cannot tell which version of each was used). Some reference should be made to lake outlines shown for KT1, KT2, and KT3 (even if “as discussed in the text.”). Some demarcation should be shown for Figure 5–7 (or shown sequentially if not marked here). This is especially crucial for Figure 5, as there are not good references to scale for that figure.

Figure 3: What datum is used in hydropotential calculations. Although the gradients are the amplitude I’d expect, the value of hydropotential itself seems unlikely to be slow. Theoretically hydropotential should go to 0 at the grounding line. Although some values of hydropotential may drop below 0 on grounded ice, this seems to be widespread. I suspect this is a result of the datum being used.

Figure 6: Once again, hydropotential values seem off. They should go to near 0 near the grounding line. Even using a standard datum (WGS84 ellipsoid; EGM2008 geoid) would get relatively close. These seem too high. These values are also inconsistent from those shown in Figure 3. Grounding line (in yellow) should be noted in caption. How was the estuarine area shown in red determined? Why is there no uncertainty on the elevation change plot (d) when there are uncertainties on similar plots in other Figures? Label ICESat tracks shown in Figure b.

Figure 7: Though not particularly important, I am surprised gap filling did perform better for the November 2011 Landsat 7 scene. I assume the fit in the final panel is linear, but it would be good to note this. Uncertainty values in elevation-change panel would also be desirable. I suspect that labeling the panels (a–e) would be useful here too.

References

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