General Comments:

This manuscript uses satellite laser and radar altimetry to identify two previously unidentified subglacial lakes on Kamb Ice Stream, and discusses the implications of this discovery for ice dynamics.

This is my second time reviewing this manuscript and I found it much improved. The quantitative treatment of the CryoSat-2 data is significantly better, both in how it was explained and in how it was actually executed. My remaining comments are much more minor. My primary scientific issues are concerning interpretation, both the calculations concerning R-channel size and stability as well as the discussion of the existence of the channel in the subglacial estuary. If these areas can be improved, I believe this manuscript will become a nice addition to the Cryosphere.

I list more specific scientific, stylistic, and specific comments below.

Scientific Issues:

1. Uncertainty could still be clarified in some cases. I noted particular issues in the specific comments sections.

2. What were the length and duration of time windows tests? I am just concerned about spurious effects from irregular sampling and/or elimination of small elevation changes?

3. The discussion of R-channels under the grounded ice stream is a confusing. The first part is fine, but I am not actual sure the pressure differential supplied by the lake is sufficient to sustain a channel. I also agree with the authors that other mechanisms are likely active under ice sheets.

4. The arguments for/against a subglacial channel in the estuary are conflicting. I think the arguments for a subglacial are more convincing and suggest the authors revisit that section to make it consistent reasoning throughout.

Style/Organizational Comments:

1. When describing the CryoSat products, it would probably be best to start with a description of the different modes and then proceed to discuss them. Right now the text jumps backwards and forwards too much between these, which I found confusing as a reader. Probably best to start with geographic coverage, then discuss precise imaging characteristics, and then finally to proceed to uncertainty. This was better done for LRM than for SARin mode.

2. The authors occasionally switch from present to past tense. It's best to just use one convention. I'm not particularly partial to one or the other, as long as the use is consistent.

Specific Comments:

Page 1

12: What is the timescale for rapid? Over a few months?

13: Add "…subglacial drainage network…"

14: I worry about using "clearly links the lakes" here when it was not directly observed. Replace with "likely"?

21: What does "subdued" mean? Fewer lakes or longer fill–drain cycles?

Page 2

10: change "margin" to "marginal"

12–13: Does work by either Le Brocq et al. (2013) or Alley et al. (2016) indicate the existence of the channel? If I remember correctly both do indicate a channel and it might be good to cite some observational evidence too.

21: Something is missing in this sentence. Perhaps "report the existence of" or "report the presence of"

31–32: Does the range in uncertainty you quote here include the slope error? If not, how much does that increase local uncertainty?

Page 3

10–11: What are uncertainties of LRM L2 products?

15: What sorts of errors does these height error flags indicate?

16–17: Perhaps state what data were used to make the DEM product?

24–25: What are the errors precisely? RMS or residuals to linear fit, standard error, or something else?

28–29: Use "surface elevation change rates" instead of "large change rates" for clarity?

Page 4

4: How many time windows did you test? Was there a standard procedure for this?

23–24: Citation for these figures?

25–26: Citation for ICESat filtering algorithm applied here?

Page 5

20: Is there unit issue here? Perhaps volume variations instead of elevation variations?

26-27: It should be mentioned that SARin mode samples high points and can thus bias observations when this mode is initially described back in section 2.1. That would make understanding the origin of the biases mentioned here easier.

Page 6

1: Refer to the section where this is discussed in parenthesis.

6–7: Reverse the clause ordering, so that "According to…" begins the sentence so that the reader knows this is a based on a previous modeling result.

22: Perhaps note that this is similar to the estuary documented by Horgan et al. (2013).

26: Is this radar penetration into the snow similar to what is found by other studies? Or is this purely an empirical conclusion?

30: Again, possibly cite Le Brocq et al. (2013) and Alley et al. (2016) papers if they also indicate a subglacial channel in this location.

32: I'm not sure retreat rate is the right word here. The feature is extending inland but perhaps not retreating? In any case, some precision with language would be useful here so that the reader knows that you are not referring to grounding-line retreat.

Page 7

4: "Posit" instead of "suppose".

11: Capitalize "lake" in "lake Conway" and "lake Mercer" for consistency.

15–21: Some care may be needed with language to stress that these are hypothesized linkages and relationships between lakes.

17–18: Reword: "The water drained from KT2 that passes directly through KT3…"

19–21: Reword: "In comparison to the behaviors of K1 and K34, which show more typical connectivity for subglacial lakes…"

Page 8

1–2: This discussion is a bit muddled. The first part is reasonable clear. However, the discussion of effective pressure is not. The perturbation in the effective pressure necessary to generate the tunnel is the important quantity, so that when the elevation is high sufficient pressure is present whereas when it is not present, the effective pressure is too low to maintain the channel. Furthermore, a more complex consideration of effective pressure over a long flow path is probably needed if considering more than a local effect right at the lake boundary.

10–11: Inflow into WIS is transient too, so I don't see an issue with this. There's no reason to suppose that an N-channel into the sediment cannot be closed and opened by repeated lake drainage, likely in approximately the same location (as dictated by the basal hydropotential) but not precisely the same flowpath.

13–15: I'm not particularly convinced by this, especially as I think others have observed channel-like features in MODIS imagery here.

29–30: I like this hypothesis better than suggesting only channelized flow.

33–34: This is really just an application of the Jenkins (1991) model and that should be cited too.

Page 9

4–6: I think this reasoning for the existence of a large subglacial channel is more consistent with the observations, but it contradicts earlier statements. I suggest revising the wording in that section.

20: Bridging "stresses" instead of "forces".

Page 10

2–3: Perhaps say that both mechanisms could be active and are not necessarily mutually exclusive. Less water overall, but also more channelized where there is water such that little lubrication can be supplied by the subglacial water.

Figure 3: In caption, state that red rectangles are at the center of the hydropotential lows.

Figure 4 (and 5b and 5c): Could volume change be plotted on the right y-axis as the shapes of the curve are identical?

References

Alley, K. E., T. A. Scambos, M. R. Siegfried, and H. A. Fricker (2016), Impacts of warm water on Antarctic ice shelf stability through basal channel formation, Nature Geosci. 9, 290–293, doi: 10.1039/ngeo2675.

Horgan, H. J., R. B. Alley, K. Christianson, R. W. Jacobel, S. Anandakrishnan, A. Muto, L. H. Beem, and M. R. Siegfried (2013), Estuaries beneath ice sheets, Geology, 1159–1162, doi:10.1130/G34654.1.

Jenkins, A. (1991), A One-Dimensional Model of Ice Shelf–Ocean Interaction, Journal of Geophysical Research 96(C11), 20671–20677.

Le Brocq, A. M., N. Ross, J. A. Griggs, R. G. Bingham, H. F. J. Corr, F. Ferraccioli, A. Jenkins, T. A. Jordan, A. J. Payne, D. M. Rippin, and M. J. Siegert (2013), Evidence from ice shelves for channelized meltwater flow beneath the Antarctic Ice Sheet, Nature Geosci. 6, 945–948, doi: 10.1038/ngeo1977.