Response to Anonymous Referee #2

We really appreciate for taking the time to review our paper and the comments valuable to improve our paper greatly. We address the comments in the order of the review as bellows. We noted the pages and line numbers indicating the corrections in revised manuscript corresponding to the comments. In addition, we would like to apologize a mistake in the analysis of the thinning in the estuary region. Owing to the referees' comments, we carefully validated the elevation time series from Cryosat-2 especially in the estuary region and found that the time series indicating the acceleration of thinning since 2013 were contaminated by the background elevation change (e.g. change of surface mass balance), because we could identify such changes in elevation on nearby stationary ice (e.g. even on the Siple Dome). We removed the background elevation changes around the estuary (modified Figure 6) and the statements related to the acceleration of thinning by subglacial flood in 2013. However, we believe it does not affect the main suggestion and conclusion of this paper.

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.

⇒ We changed the structure of the manuscript.

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?

⇒ We modified the method section. The standard deviation of residuals over non-changing portions is to show the uncertainty of elevation change 'time series' as Figure 2 in Wingham et al. (2006b). For the uncertainty of elevation change 'rate', we used linear regression error. It is included now.

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.

⇒ We added labels in Figure 7. We also added annotations in Figure 1.

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.

⇒ The explanation of ICESat data was included.

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.

⇒ We added about the calculation of hydropotential. You might feel confuse about the hydropotential in Figure 3. In this figure, the mean of hydropotential was removed for better visualization as stated in the caption.

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.

⇒ We modified the method section for clearer description.

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.

⇒ We changed the structure of method.

Style/Lanuage/Grammar comments

1) Hyphenation use is inconsistent – For example, "repeat track method" vs. topographyfree elevation" and "sub-glacial" vs. "subglacial". I tend towards hyphenation, i.e. repeat track and topography-free, but use should be consistent.

⇒ It was corrected all.

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.

⇒ It was corrected all.

Specific Comments

P1 L11: "We have identified two previously unknown active subglacial lakes…"

⇒ It was corrected (P1 L11)

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.

⇒ A streamline from the regional hydropotential also indicates the connectivity lakes (P1 L13-14)

P1 L13: “lakes area” seems redundant. Perhaps just “lakes.”
⇒ It was corrected (P1 L14)

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

P1 L15: “subglacial lakes”.
⇒ We are sorry that we couldn’t understand what this comment exactly means.

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.
⇒ It was corrected (P1 L18-21)

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.
⇒ All of the contents about rapid thinning is withdrawn now.

P1 L17: “sub-glacial” to “subglacial”.
⇒ We choose “subglacial”, and all of inconsistent uses are corrected.

P1 L19: Change “rapid ice flow” to “streaming ice flow”.
⇒ It was corrected (P1 L25)

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.
⇒ The arrangement of first paragraph in Introduction is changed (P1 L23~ P2 L5)

P1 L21: Change “indicated” to “posited”, “hypothesized”, “suggested”, or similar to indicate hypothetical nature of this conclusion (even though it is likely correct).
⇒ It was corrected (P1 L28)

P1 L21: Change “Siple-coast” to “Siple Coast”.
⇒ It was corrected (P1 L28)

P1 L22: Change “Ice Stream” to “ice streams”.
⇒ It made the capitalization consistent (P1 L28)

P1 L22: Delete “of these cycles”.
⇒ It was deleted.

P1 L24: Change “long term” to “long-term”.
⇒ It was corrected (P1 L31)

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?
⇒ It was corrected (P1 L31-32)

P1 L25: “Therefore, it has been suggested…”
⇒ We rearranged the introduction section and this expression was deleted.

P1 L28: Change “predict its dynamics” to “understand the ice dynamics” or similar.
⇒ This expression was deleted.

P1 L30: Change “while” to “although”.
⇒ It was corrected (P2 L2)

P2 L1: Delete “contributing the basal hydrology”.
⇒ It was deleted.

P2 L3–4: “the hydrological connections between adjacent lakes”.
⇒ It was corrected (P2 L8-9)

P2 L4: “by the sparse coverage of the ground tracks”.
⇒ It was corrected (P2 L13-14)

P2 L5–7: Is this a supposition of the authors or are there other studies that posit this?
⇒ I was corrected. This supposition is posited in Fried et al., (2014) (P2 L9-11)

P2 L11–16: This paragraph mixes methods and introductory materials.
⇒ We changed this paragraph so that only contain an introductory material. (P2 L18-24)

P2 L11–12: Inconsistent capitalization: Antarctic Ice Sheet here vs. Antarctic ice sheet previously.
⇒ We use “Antarctic Ice Sheet” (P2 L18-19).

P2 L13: “two previously unknown subglacial lakes”
⇒ It was corrected (P2 L21)

P2 L14: What sort of activity? Downstream lakes fill as the upstream lakes drain?
⇒ We changed this sentence. (P2 L22-23)

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

⇒ It was removed.

P2 L13–15: This sentence is awfully general. Perhaps change to something more specific to the results presented in this manuscript.
⇒ We changed this paragraph. (P2 L18-24)

P2 L17: Seems like there should be a more extensive methods section.
⇒ We reconstructed the data and methodology section (P2 L25 – P5 L8)

P2 L21: “geophysical” should probably be “geographic”.
⇒ We changed this sentence. (P2 L32 – P3 L1)

P2 L23: Which version of the L2 product? If I remember correctly, there are multiple processing baselines.
⇒ As you noted, we used both of baseline B and baseline C products. Our results shown in manuscript are already corrected the bias between those products. We added this statement in P3 L2-4.

P2 L24: What does “interior of ice” mean? Ice-sheet interior? Why the range of uncertainty?
⇒ It was corrected (P2 L31). Wang et al. (2015) suggests that the uncertainty is proportional to the slope of surface (P2 L30-32)

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.
⇒ Wang et al. (2015) computed the uncertainty of Cryosat with respect to ICESat measurement, and the uncertainties depended on the slope of ice surface. The uncertainty of Cryosat measurement on KT region is also estimated in this study, and presented in Figure 4, 5 and revised method section.

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.
⇒ The methodology section is changed (Section 2.2 - 2.4).

P2 L28: What do height error flags signify?
⇒ This sentence was changed (P3 L1-2).

P2 L30: Does this method follow Helm et al. (2014) or is it distinct in some way from that processing?
⇒ Helm et al. (2014) is a reference of the DEM product used for 3-sigma filtering.

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.
⇒ We changed Data & Method section (Section 2.2 - 2.4).

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

⇒ It was added (P3 L19-20).

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

⇒ We added those values (P3 L28-29).

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.

⇒ We described in details (P3 L20). The detection of lake was performed using an overlapping window scheme of a constant time length (2 year). However, in order to highlight the elevation change of lake and clearly select the lake boundary, we use the time windows with different lengths for generating DEMs as described in section 2.3.

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

⇒ We added it in Method section. (P4 L34 - P5 L8)

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

⇒ It was corrected and moved to result section (P5 L11-12)

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

⇒ It was removed.

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?

⇒ We added it in Method section. (Section 2.4)

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

⇒ We added their uncertainties. (P5 L22)

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?

⇒ One possible scenario has been stated in P7 L12-22. The high-amplitude fluctuations observed when the lakes were filled is probably due to spatially irregular sampling of the elevation change pattern like a dome within the lake boundary. This speculation was added (P5 L26-28).

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.

⇒ We added more discussion in P7 L22 – P8 L11. In the assumption of the R-channel, the energy analysis similar to Wingham et al. (2006b) shows the energy released by the subglacial flood between KT1 and KT2 is enough to maintain the semi-circular conduit against creep closure. However, the R-channel theory may be not adequate in the environment of Antarctica, especially Siple Coast. Therefore, we referred a recent study investigating the possibility of a canal flow in the Whillans/Mercer Ice Stream.

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.

⇒ We clarified it in Figure 4 and in P6 L2-4

P4 L23: "role of the hydraulic barrier" or "role of hydraulic barriers".

⇒ It was corrected (P7 L18-19)

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

⇒ It was corrected (P6 L8)

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.

⇒ We added the description about LRM products in Section 2.1 (P3 L5-11).

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.

⇒ A sentence was added in discussion section (P7 L19-21)

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

⇒ It was removed.

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.
⇒ It has been suggested in the caption of Figure 6. We added it in the manuscript once again (P6 L26-27)

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).
⇒ Although not shown in this paper, the ICESat elevations within KT1 from our processing are continuously and slowly lowering in a rate of ~ -0.3 m/yr in consistent with Smith et al. (2009). However, we cannot find a significant elevation change in the Cryosat-2 elevations from 2010 to 2012. We suppose the slow draining of KT1 observed by ICESat was stopped around 2009. After the filling event in 2013, the KT1 seems to be slowly lowering similar to the observation during ICESat era but we need to monitor it for a few more years in order to figure out the long-term draining or its periodicity.

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.
⇒ We added more discussions that you recommended. (P8 L26- P9 L3).

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.
⇒ This argument is deleted now.

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.
⇒ The features of channel described in Marsh et al. (2016) is mainly from MOA image. I MOA image, we only can see the dark and flat features from KIS cavity and we think MOA is not proper to find out the specific morphology. Since the undulating feature is from Landsat imagery, we cannot simply compare the observation of Marsh et al. (2016) with our observation directly and discuss although we have some speculations.

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?

⇒ Although not shown in this paper, the differencing of Landsat images (using Landsat 7 and Landsat 8 scenes) gives us the retreat rates of 30 - 50 m/yr along the grounding line of KIS in consistent with Thomas et al. (1988), so we referred it instead of including the result.

P6 L6: "to the oceans".

⇒ This sentence was changed (P8 L32-34).

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.

⇒ We referred Jenkins (2011). Based on the result in Jenkins (2011), the steep ice base near the grounding line of KIS trunk estuary observed in BEDMAP2 may support the strong basal melting by meltwater plume. (P9 L4-6)

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

⇒ The location of observed channel is nearly similar with the modeled one in previous studies (see Figure 3 in Carter et al.,2012 and Figure 2 in Goeller et al., 2015).

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.

⇒ We corrected them all that you recommended.

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.

⇒ We cannot decide the kind of flow in current observation. We corrected its expression so that it could be opened to other possibilities. (P7 L22-P8 L11)

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.

⇒ It was corrected (P9 L14-15)

P7 L5: "definitely" should perhaps be "definitively"?

⇒ It was corrected (P10 L2)

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.
⇒ It was changed by the similar sentence (P10 L2-4)

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 1: It was corrected that you recommended.

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.
⇒ We are sorry for that we didn't state the mean of hydropotential is removed in figure (c). we added this statement.

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.
⇒ The color scale made you feel confused about it. Note that the color axis was cut at 500kPa although the hydropotential actually go to nearly zero around the grounding line, because we want to highlight the potential differences of KT2, KT3 and the estuary near grounding line. The hydropotential rapidly changes near the grounding line because the ice base is very steep. The BEDMAP2 uses GL04C geoid as a datum for elevations.

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.
⇒ We added labels in this figure and also added that the linear fit is performed.