Response to Anonymous Referee #1

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. I appreciate that for 4 out of the 5 authors, English is not a first language, and al- though the language is fairly good (much better than I could do in Korean), there are still many instances in the manuscript that need to be edited for English grammar/language – I would recommend the native English speaking author to edit the manuscript accordingly.
⇒ The revised manuscript would be improved. We hope that it can be read easily now.

2. I found the structure of the paper to be a little confused – methods where in the results, discussion in the results, no conclusion section. This needs to be rectified with careful attention as it makes the manuscript difficult to follow.
⇒ We changed the arrangement of the manuscript to conventional form.

3. I am confused as to why you present so many methods for deriving elevation change from CryoSat-2 data – I think there are three – one of which was introduced in the results section (see note above). Is this necessary? And if you believe so, please be clear why it is that you are doing this and which results are linked to which method.
⇒ To clarify, we moved all of the method in Result section to Method section.

4. You introduce ICESat results in the results section but there is no mention of this in the methods sec- tion. You need to include a description of this data and the processing methodology in you methods section.
⇒ We added the processing of ICESat data.

5. Throughout your text you report numbers of surface lowering from altimetry but you do not report associated errors – this makes it difficult to discern whether or not the changes in rates that you report are significant or not. I suspect in some instances they might not be. This must be rectified.
⇒ It was corrected all.

6. Also regarding errors, you report in you methods that you output and error statistic for the CryoSat elevation change signals from the regression error but later, in figures, use uncertainty measures from standard deviations of measurements and also from the signals from (assumed) constantly changing regions. I think it would be better/clearer/less confusing to stick to just one measure of uncertainty for any given parameter.
⇒ We clarified where the errors come from. Please read Method section.

Specific points (given by Page number and Line Number – e.g. P1 L1):

P1 L11-12 – change "inferred from CryoSat-2 altimetry, indicating that" to " inferred from surface height changes using CryoSat-2 altimetry and indicate that. . ."
⇒ It was corrected (P1 L12 -13)

P1 L13 – change "The orientation of the drainage network. . ." to 'The structure of the subglacial drainage network. . ."
⇒ We changed it as "The subglacial drainage network structure". (P1 L13)

P1 Introduction – general comment: I think that in the introduction you need to explain more about the previous work done into understanding the stagnation of the KIS. There is more than one theory for why this ice stream stagnated, and so I think that you need to acknowledge this.
⇒ It was corrected (P2 L1-5).

P1 L20 – this is a bit of an empty statement – explain why or to what extent the stagnation is significant for the mass balance of the WAIS – i.e. there is a mass gain.
⇒ We changed Introduction section so that reflect you and Reviewer2's comments (Your recommendation is reflected in P1 L26-27)

P2 L5 – you outline the limitations of the ICESat data, but what about the limitations of interferometry (which you mention in Line 3) for detecting SGLs ?
⇒ We added that contents. (P2 L15-16)

P2 L5 is this lake in the Northern Corner highlighted in any figure? If not, I would do so and include a reference here.
⇒ We refer a figure of Fried et al. (2014). (P2 L9-11)

P2 L10 – it would be helpful if you clearly stated the aims and objectives of your study here.
⇒ The last paragraph of Introduction section is changed. (P2 L20-24)

P2 L 29 – why not just use a simple threshold at this stage?
⇒ The variances of Cryosat-2 elevations are too variety in this area and, moreover, the uncertainty of Cryosat-2 elevations is dependent on the surface slope. Therefore, the determination of a simple threshold over wide area for data editing is so difficult.

P3 L3 – Why not just use a smaller grid cell size? It is likely that there would be sufficient data within the grid cells at ~2 km to provide a robust solution.

⇒ We already have tried smaller (or bigger) grid cells. But in the case of smaller grid size, the noisy signals around subglacial lake were increased. Since our data processing is conducted in the time window of 2-year, the size of 2 km by 2 km is too small to gather enough measurements for stable linear fitting.

P3 L5 – regression of what? Do you mean the quadratic fit?

⇒ It was corrected. (P3 L17)

P3 L6 – is your selection of lake areas subjective or quantifiable? You need to explicitly state what criteria you use to determine if an area is lake or not.

⇒ It was corrected. (P4 L7-8)

P3 L9 -10 – these are results not methods

⇒ We corrected these sentences. (P2 L20-24)

P3 L10 – I would be interested to know if there are any potential lakes where you don't see any.

⇒ We also could find unknown subglacial lake in Whillans Ice Stream region. But we didn't stated in this paper because this study is only focused on KIS region.

P3 L11 – I don't understand why you have also use the plane fit method if this provides more detailed information on the boundaries. Also, I don't really understand how this can provide more detailed information on the boundaries, because the same data should be included in both methods.

⇒ The plain fit method was conducted by 5*5km grid cell overlapped 1 km interval and we just utilized it to finding candidates of subglacial lake. We could not use it to derive time series or lake boundary of the lake region, because it shows very low temporal and spatial resolutions (2-year window/ 5*5km grid cell). After we identify the possible existence of subglacial lake, we figured out the specific information of lake by a method of DEM difference with finer resolution (100 m). The structure of Data&Method section is changed.

P3 L12 state the resolution of these DEMs. Also what filtering criteria, if any, do you use to remove unreliable height estimates.

⇒ The resolution of DEM (100 m) was added (P4 L2). We applied a 3-sigma filter as mentioned in manuscript. In addition, the semivariogram modeling in kriging method includes nugget effect so that generates a smooth surface geostatistically.

P3 L18 – why do you think this is? Provide a physical explanation.

⇒ I think we need a more sophisticated statistical approach to the Cryosat-2 measurements, the ice surface morphology, and etc, but beyond the scope of this study. Therefore, a criterion for boundary selection was empirically chosen by comparison with ICESat measurement.

P3 L18-19 – lake areas should be in results.

⇒ It was corrected. (P5 L14-15)

P3 L19-20 – more detail needed for the methodology of deriving the hydraulic potential – at least a reference to the paper(s) from which you derived the method.
⇒ It was added in Method section. (P4 L34 – P5 L8)

P3 L25-29 – this is methods.
⇒ It was moved to Method section (P4 L10-20)

P3 L27 – state the magnitude and standard deviation of the trend removed? How much spatial variability was there in this trend?
⇒ Now more specific method is presented in Method section. (P4 L10-20)

P3 L 32- include uncertainty – this needs to be done for all your results (numbers) reported in the text.
⇒ It was corrected all.

P4 L 4 “descend completely yet” – replace with “ returned to the previous level by the end of the study period.”
⇒ It was corrected. (P5 L25-26)

P4 L7 – L17 – this is all discussion
⇒ It was moved. (P7 L12-22)

P4 L 20 – what are the values of head differences when the lakes are fully filled? Include numbers.
⇒ It was added. (P6 L3)

P4 L28 – you need to include your methods for using the LRM data in your methods section.
⇒ It was added. (P3 L5-11)

P4 L 35 replace “present” with 2015.
⇒ It was corrected (P6 L15)

P5 L 9 significant effect on what?
⇒ Reviewer 2 also point out that this sentence should be in Conclusion. We deleted this sentence in this section.

P5 L 9 – what exactly do you mean by the estuary? What characteristics does it have? In Figure 6 you show the area in a box – on what basis have you defined this area?
⇒ We added more specific statement and discussion in P8 L17~ P9 L3. In this area, the hydraulic potential lows are widening and flattening and the persistent elevation lowering are observed. Therefore, we think this area is an estuary where the subglacial flow enters into the ice shelf cavity.

P5 L11 – where has this ICESat data come from? Again you need to include this in your methods.
⇒ It was added (P4 L22).

P5 L 12 – How have you accounted for mission offsets in the elevations measured by the two missions? If you haven't, I don't think that you can meaningfully present the two as a 'continuous' time series on the same plot. Suggest you either show on separate plots, or show one plot of elevation changes (dhdt).
⇒ It was mentioned in Figure 6. The offsets of Crysat-2 elevations compared to the ICESat elevations due to mostly the penetration of radio wave into surface snow pack were estimated through the comparison between the ICESat and Cryosat-2 elevations at nearby stationary ice. The estimated offset (1.03 m) was subtracted from the ICESat elevation change time series for a continuous time series. We added it in the manuscript once again (P6 L26-27).

P6 L1 – explain why the feature is too concave for CryoSat-2 to detect.
⇒ It was added. (P7 L2)

P6 L8 – Change "If this method is applicable to the KIS, the strong. . ." to " If this method is applicable to the KIS, it is probable that the strong. . .."
⇒ This sentence was changed. (P9 L1)

P 7 – what about the conclusion section?
⇒ We changed the structure of this paper.

Figure 1 – I presume the yellow line is the Grounding Line? Include in figure caption.
⇒ It was included.

Figure 2 – There are two regions of surface lowering and 1 region of surface increase in the north of this figure? Why aren't these identified as lakes?
⇒ The two of the signals you pointed have large uncertainties. The small lowering signal between of them has low uncertainty, so it is another candidate of subglacial lake. However, it is ambiguous a bit because the elevation rate change of this region rapidly oscillate and also the location of this signal is slightly changes. The signal from this is distinct from the signals from KT2 and KT3, so it was excluded from our study.

Figure 3 – why not show continuous fields of the ice surface elevations and elevation anomalies?
⇒ In order to consider the locality of surface elevation, we made the local DEMs centered on each lakes using the kriging method based on local semi-variograms estimated in each areas.

Figure 4: why do you show the standard deviations here and not the uncertainty on the elevation change measurements that you derived in the processing?
⇒ We wanted to show the measurement error, not a regression error in elevation change time series. The measurement error from radar altimeter has been suggested in Wingham et al.,(2006b) and we chose their method. (P4 L16-19)