Answers to the Review by S. Carter.

1 General Comments

In this paper the authors assimilate a suite of elevation data derived from satellite remote sensing, primarily ICESat, Envisat, and SPOT5, to better characterize a highly unusual subglacial drainage event (the so-called "CookE2 Flood" first described by Smith et al., (2009)) and infer the fate of the water released from this lake. This follows upon work by Carter et al., (2011) and Fricker et al., (2010) showing that a greatly improved understanding of the large scale subglacial hydrologic system can be revealed through the careful analysis of complimentary lines of remote sensing data analysis and the application of water budget modeling to account for water released from upstream. Overall the analysis part of the study appears to be based on solid analyses and logic, and is potentially quite interesting.

While this manuscript holds promise, in its current form it suffers from confusing organization, and poor and sloppy use of the English language. The result is that some important concepts are not explained very clearly and other pieces of logic cannot be located without a certain amount of hunting. Furthermore, there are some surprisingly naïve statements made about subglacial hydrology to introduce the study, that really have no place in a paper on subglacial hydrology, for example "It might seem surprising that liquid water can exist under the ice of the coldest continent on Earth." Given the 3rd author's publication record on sub ice sheet hydrology (e.g. Remy et al., 2003), it is surprising that the paper does not convey a fuller sense of immersion in the extensive literature that has preceded it. The paper requires major revision to bring it up to standard. Although I have compiled an extensive list of fixes and edits, the list should be thought of as a representative subset of larger structural problems. I recommend that the authors engage a (preferably native English) proof-reader to assist them with the significant editing task that lies ahead. It is above and beyond the task of any Editor or Reviewer to improve the English and grammar in the manuscript to the level that would be acceptable for TCD. Overall the combination of poor organization, awkward wording, lack of perspective with regards to related literature, and the sloppy figures is consistent with authors who have rushed their submission of the manuscript. While there can be many motivations for doing so, none of them justify such an abuse of the TCD open access system.

Overall, the illustrations are interesting and show a great deal of logical progression, but unfortunately they fail to communicate some concepts that I believe are essential for a study of this nature. The most importantly omission is a map of the regional hydropotential. To assist here, I have included a hydropotential map based on Bedmap2 (Fretwell et al., 2013) and a velocity map (based on Rignot et al., 2011) with my review. A figure of this nature would be a critical addition to the manuscript. Secondly the layout and inconsistent labeling between figures makes them appear as if they were imported directly from the analytical software into the manuscript without any attention given to making them presentable to a non-specialist. This is particularly frustrating because the authors have plenty of experience making presentable figures (e.g. Flament and Remy, 2012) and many of the fixes necessary would not have been particularly time consuming (I was able to create improved versions of Figures 4 and 5 in less than an hour (see figures R1 and R2 (supplemental file))).

We would like to apologize if the submitted paper gave an impression of rushed submission. We first detected the presence of this cascade in Envisat elevation time series in 2011 (1.5 years before submission) and took time to acquire/process more datasets (e.g., SPOT5 DEM acquired after a special request to the French Space Agency, CNES), wait for BEDMAP2 to be released and go further in the understanding of this massive flood. We hope the revised paper better fits the standards.

2 Specific Comments:
Introduction

P842 L21: This sentence is kind of awkward.
and
L23: The presence of liquid water at the base of the ice sheet is not surprising at all, we have known about this for decades (e.g. Oswald and Robin, 1973). You are trying to say that over 300 lakes have now been identified beneath the Antarctic Ice Sheet, the coldest place on earth at the surface. Liquid water persists at the base due a combination of geothermal/deformational heat and the insulation provided by the overlying ice. This is well established! In fact you can probably shorten this whole paragraph and make it sound a lot better (here is where the proof-reader comes in!).
and
P843 L4: This is an awkward sentence that requires rewriting and proofreading. You're saying that the ability of various methods of satellite monitoring of surface elevation change have allowed us to infer the filling and drainage of subglacial lakes, but that their influence on the flow of the overlying ice is uncertain.
Response to all previous comments: The introduction has been rewritten, shortened and its “naive” tone removed.

L14: Here you want to cite Smith et al., 2009 as the first continent-wide inventory of lakes.
We added “continent-wide”.

L15. This is not the correct use of the phrase “In particular”. It's an important fact for your paper, but was a small part of Smith and others, 2009.
This sentence has been rewritten

L23: I recommend the term “leverage” instead of “take advantage of.” It should be stated more clearly that the CookE2 flood was a pretty exceptional event when compared to any of the other lake drainage events in Antarctica. While the volume lost was only 1.5 - 2 times that for any other lake, the vertical change was nearly an order of magnitude greater than what is observed anywhere else on the continent. Indeed it is closer in magnitude to the floods from subglacial lakes under the Vatnajokull Icecap (e.g. Björnsson et al., 1998).
We added references to jökulhlaups and added a sentence on how the reported event compares to other known Antarctic lake drainages.

Section 2
A. This section requires significant editing and would benefit from some restructuring. I recommend adopting the first person, active voice for methods, i.e. “We processed the data”, rather than “the data were processed”. Let me suggest the following sub-section headings:
2.1 Data
2.2 Data processing (where you talk about observing drawdown / uplift)
2.2.1. Error sources
2.3 Water Modeling
The paper was thoroughly re-organised.

B. I recommend a chart or table of each of the remote sensing data products used that includes certain technical information, like footprint, approximate track spacing, alongtrack sample density, duration of mission and possibly corrections applied. Not every TCD reader is familiar with these instruments and their products.

This table has been added.

C. ICESat. There is standard nomenclature when working with ICESat data. The laser campaigns are called "Laser 2a", "Laser 2b, etc". The timings of the campaigns are generally given by months rather than seasons, due to confusion with NH and SH seasons. The release of the data you use is not the latest release - Release 531 (not 31) is about 1.5 or 2 years old. You should be using Release R633 for your work. Also, there has now been a Gaussian-Centroid offset identified (see NSIDC website), and this might have some impact on your results. What inter-campaign bias do you apply, if any? It probably doesn't matter for the drainage of CookE2 (since the signal is so large), but I suspect it might be more of an issue with observing uplift downstream.

ICESat measurements were only used for the vertical adjustment of the stereo DEMs around the lake area and, through comparison with SPOT5, for the estimate of the change in volume of the surface depression since the drainage. For that task, decimetre precision is sufficient. ICESat data, due to its limited temporal resolution, were not used for the detection of the sometime faint signal due to presence of active subglacial lake downstream of Lake $Cook_{E2}$. We used release 531 because release 633 was not available when we started to work on the topic. We do not think that the small changes (order of magnitude of centimetres) between the two releases will alter our results and our conclusions and justify re-performing the whole analysis. We did not apply any inter-campaign bias. Again, this would also affect the results by a few centimetres only (Borsa et al., TCD, 2013, http://www.the-cryosphere-discuss.net/7/4287/2013/tcd-7-4287-2013.html) which does not matter compared to the decametric signal observed in the Lake $Cook_{E2}$ area.

D. While it is nice (and useful) to see a comparison between ICESat laser and Envisat radar altimetry, there are some important points that appear to be overlooked. There are substantial differences between radar and laser altimeters, in particular with regards to sub-surface penetration depth, yet this important fact is not even discussed. What about the differences in footprint sizes? This information may appear at other places in the manuscript, but this Data section is the place to discuss the characteristics of the data you are using for the study.

We never directly compare ICESat and Envisat measurements. Envisat elevation data is always used separately which is why we did not discuss penetration depth in the submitted paper. This issue of penetration, although not affecting our analysis, is now briefly touched upon in the data section (2.4).

E. Can you provide a hypothesis as to why there was a "tilt" between the Aster and SPOT5 DEM's?

The tilt was measured only in the SPOT5 DEM and estimated using ICESat (not ASTER) data. We now state that this tilt is probably due to an error in the measurement of the attitude of the SPOT5 satellite combined with the fact that the DEM is derived without any ground control points.

Also could you describe precisely the domain over over-which you are comparing the SPOT5 DEM against ICESat data and the ASTER DEM?

The new figure 1, study area, now precisely locate the region covered by the complete SPOT5 DEM (where comparison against ICESat data has been performed) and the region of the lake where it has been compared to the ASTER DEM.

P844 L3: Passive voice. The verb should not be the last word in the sentence. Indeed you could probably shorten this paragraph significantly.

We changed to active voice.

P844 L4: These sentences really do not tell us very much, except that the logic of the techniques applied is apparently well established in the literature.

We moved these to the method section.

The technique was presented in Flament and Rémy (2012) only from the perspective of dh/dt. Here, we use the full time series (not only the temporal trend) and this part had not been presented. We think a rapid presentation of the whole process is necessary.

P844 L24: Please provide some examples of rugged terrain.

We added some examples

P844 L24: In this sentence "In the following, time series . . ." there should be another word between "following" and the comma

We changed to "in what follows".

P846 L23: I assume you mean ICESat? Other readers might not assume this. Also what do you mean by standard deviation?

We precised "between SPOT5 DEM and ICESat measurements" after "elevation differences".

P846 L29: I am not sure what you are trying to say here. I understand that you are using ICESat, ASTER and SPOT-5, but it's not clear how you are accounting for the differences between the various data products.

This description seemed unclear to both reviewers, we rewrote it and we hope that our procedure is now understandable.

Section 3

Is this now Results? The Methods sentences at the start of this section should go to Methods.

This sentence is here, at the start of the 'Results' section, to remind to the readers very briefly how we specifically study the drainage of Lake Cook$_{E2}$. We decided to retain it.

A. At first it seems as though you assume a 1:1 relationship between surface displacement and volume change, when there are many reasons this might not be the case.

While you do mention possibilities of water flow into the depression, ice flow into the depression and snow blowing into the depression, there are several other issues that may be considered (see point B of this section). Moreover, you discuss these sources of uncertainty present many paragraphs after describing the initial assumption, and then go on to perform your calculations for volume change without any further consideration of the errors. This discussion of possible sources of error really needs to be moved up to methods and given more consideration in subsequent calculations.

We stated more clearly that the volume differences we measure might not reflect the exact change in volume of water underneath the ice layer but that as many others we choose this hypothesis because of the lack of alternatives. We clearly state that this is a limit of our analysis.

B. The assumption from Smith et al., (2009) is probably less valid for lake CookE2 than it would be for any other known subglacial lake in Antarctica. The assumption works fine for a lake in a relatively shallow basin surrounded by slippery sediments undergoing a few m of elevation change. The assumption becomes more problematic when you're dealing with a lake undergoing 10's of meters of elevation change, surrounded by steep bedrock topography. Sergienko et al., (2008) has a decent discussion about surface change versus lake volume change. For this situation however, I wonder if ice flowing into the depression following the lake drainage, as described in Pattyn (2008) might explain the apparent refilling? In the end there are a number of hypotheses for the apparent filling during the later part of the observation cycle, all of which seem testable with the current datasets.

Refilling: we tested some of these assumptions for the apparent filling of the lake. This is part of the discussion (not the 'results') because we do not really bring new data/method on this topic.

C. They authors should include some material on the initiation of lake drainage and subsequent evolution. I suggest starting with reading Fowler (1999) and Evatt et al, 2007), which deal with flood initiation and surface deformation associated with a subglacial flood.

We think this is a subject in itself and deserves more than one paragraph. We studied the evolution of the slope over the lake area before the drainage. We could not extract a significant slope change.

P847 L8: should probably be written like: "we use a combination of . . . . "
P847 L10: Should read something like "the intersection of ICESat tracks XXXX and XXXX appears to coincide with the location of maximum surface drawdown"

We edited both sentences.

P847 L12: Is it a second lake or just another part of the lake missed by the Smith et al., 2009 inventory? I suspect the latter.

Smith et al. 2009, did not have data on the secondary lobe. McMillan et al. 2013 provide a discussion on this subject.

P847 L21: For perspective: 3m is about the order of magnitude of total elevation change for some lakes.

This is why we stated in the discussion of the paper that DEM differencing (given the current level of precision of DEMs) is only appropriate for exceptional events not for inventory/comprehensive active lake detection.

P848 L1: You seem to have a number of sources of error here. That is fine, but an introduction sentence or list might help the reader follow your logic. Also I am surprised how low the number is relative to the total volume change.

The error bar for the differencing of DEM has been revisited and is now much more conservative. Our initial error bar was purely formal and probably did not account for some of the sources of uncertainties (spatially varying biases in the DEM and arbitrary manual interpolation of the contour line in the regions where there are data voids in the ASTER DEM).

P848 L14: I am not following here. Are you saying that water was flowing out of the lake while it was increasing in volume, or are you trying to account for water flowing into the lake while it was draining? I suspect the bit about ice flow into the lake basin versus water flow might apply here.

Because there was an uplift of the surface of the depression between the end of the drainage in October 2008 and the date of acquisition of the SPOT5 DEM in February 2012 (whatever his origin: water inflow or ice inflow), our estimate of the volume changes between 2006 and 2012 is a lower bound to the lake drainage. The maximum amplitude of the drainage happened earlier.

Section 4

A. It would be good to include a few sentences on data quality, and interpolation issues. In this case much of the region around CookE1 and CookE2 is pretty well surveyed.

We added the map of bed elevation uncertainty from BEDMAP2 in figure 2 and a few words about BEDMAP2 data density at the end of section 3.2.

B. There's a little bit of a logic issue here: You may want to refer to Carter and Fricker (2012) Ann. Glaciol. paper discussion on tuning. I appreciate that you have a different method with the probabilistic route finding and that the method is highly reproducible, but it appears resolution limited. LeBrocq et al., 2006 has some other routing methods that may get more water into CookE1, and a close look at the hydropotential from Bedmap2 (Figures R1 and R2 (supplemental file)) suggests that it is possible.

We changed the radius of the disc fitted to the hydrologic potential. $Cook_{E1}$ is now resolved and the buffer zone no longer needed.

To insure that we do not miss other lakes because of our routing algorithm, we made a sensitivity test by adding a buffer zone of varying width around our best "search" area for active lakes. Few points are detected in the expanded flowpaths. The two figures C1 and C2 give the details of this sensitivity study.

The increase in water volume with a 40-km wide buffer zone is only explained by the increase in the considered area, not in the amplitude of elevation change. Few lakes are detected past the grounding line and can be considered as false detection (due to the much steeper slopes and higher noise level in the radar altimetry time series).

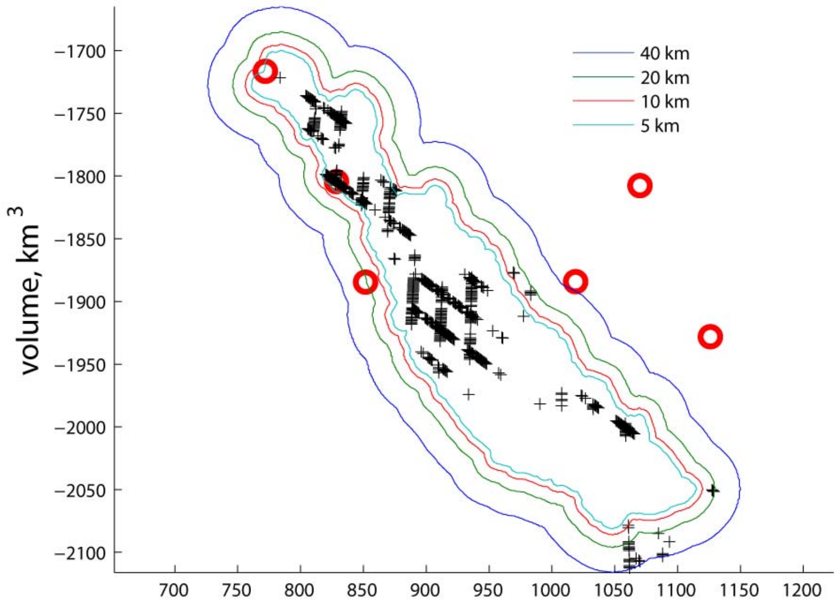

Figure C1: Flowpath area enlarged by a “buffer zone” of varying width (5, 10, 20 and 40 km width). Lake Points are plotted as black crosses, lakes from Wright and Siegert’s “Four inventory” are plotted as red circles.

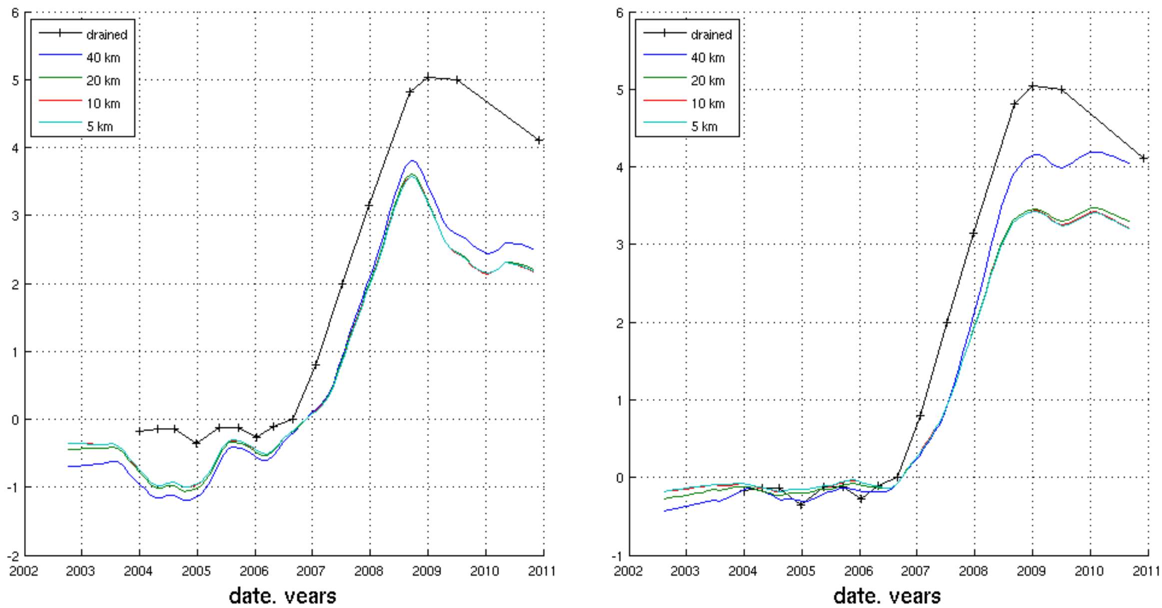

Figure C2: Uncorrected and corrected times series in downstream lakes (similar to figure 8 in the revised paper) taking into account a buffer zone of varying width. The correlation threshold used here is 0.8.

C. I appreciate the work on automatic lake detection, however a review of Carter et al., (2007) and / or Fricker et al., (2010) would indicate that you need a hydropotential low as well. The fact that you can account for the water volume lost from CookE2 from your integration of surface uplift downstream holds substantial promise. I am just not sure how much effort was made to tie surface change to actual lake boundaries. As it stands now each point representing 12 km2 without considerations to shape is a bit suspect.

The locations of the lakes do not precisely match the hydropotential lows but we found that lakes were concentrated in regions where there was a large density of such lows. The fact that some of our active lakes do not correspond to hydropotential lows does not mean that these lakes should be discarded. Indeed, the identification of hydropotential lows is dependent on the accuracy of the BEDMAP2 which, as shown in Fig 2d, is highly variable.

We better justified the use of the 12 km² area in the text. This is a statistic approach; we consider that the sampling confined to satellite tracks is a representative random sampling of the elevation changes of the area. This allows us to take into account all the lakes in the area, even the ones not directly surveyed by altimetry.

P849 L20: Although Carter et al., 2011 discusses water routing and water conservation in a manner that is relevant to this paper, Shreve (1972), is actually the better reference for calculating hydropotential.

We changed the reference.

P849 L22: you may want to introduce hydropotential a little earlier. We need a figure of this, as I stated earlier.

We added Figure 2 to give a better overview of the glaciological context in the area.

P850 L20: I'd be curious to see a sentence or two on the distribution of lakes versus bed topography data quality (See Fretwell et al., 2013). Also you should ask yourselves: are the lakes you find located in hydropotential lows? They should be (See Figures R1 and R2 (supplemental file)).

Our lake detection is independent of the quality of the BEDMAP2 but it can be seen from Figure 2d of the revised paper that some lakes (e.g. $Cook_{E1}$) are in areas of poorly resolved bed. It can be expected that small lakes in deep troughs will be badly resolved by ground penetrating radar.

See answer above regarding hydropotential lows.

P851 L16: There are many databases for surface mass balance (e.g. Lenearts et al., 2012). I suspect using data from one of these will make your argument stronger.
P851 L22: Could some of this inter-annual variation be essentially "noise? (e.g. Lenearts et al., 2012).

For these last two points, we asked and used the data from Ligtenberg et al., 2011 to obtain the firn depth variation and remove it from the time series in fig. 8. The result was not convincing (Figure C3). The fluctuations of the series prior to the drainage are not improved.
Comparison of firn depth correction with altimetry results shows some differences that are not fully explained (Ligtenberg et al., 2012). The magnitude of the elevation change signal varies sometimes with a factor 3 but the sign of the trends are in good accordance.

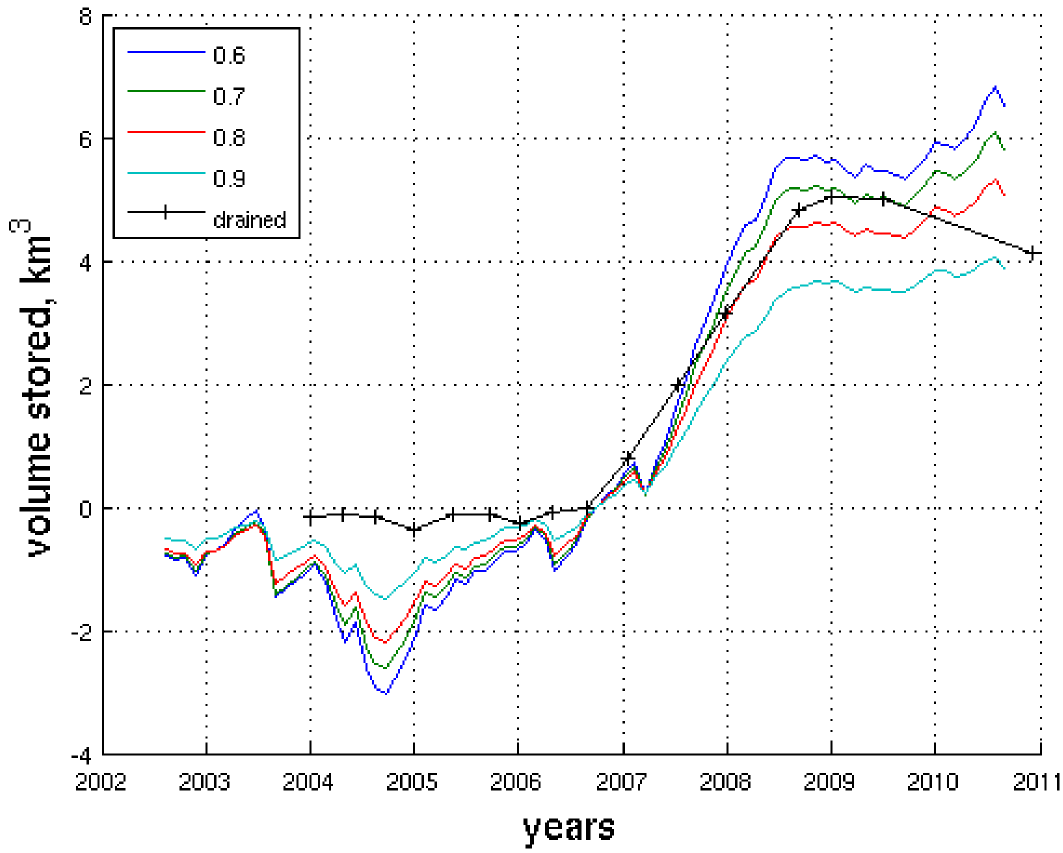

Figure C3: Time series of water volume in downstream lakes, similar to Figure 8 in the revised paper, but with a correction based on the firn model of Ligtenberg et al., 2011.

Although originally not successful, this suggestion to use SMB data to correct our altimetry time series led us to propose a correction based on "non-lake points" within the flowpath area defined above. We use the altimetry time series at locations that are not identified as active lakes and remove this average "non-lake elevation changes" to correct the lakes time series. This procedure significantly reduces the pre-drainage elevation changes. RMS of the volume changes before the drainage got down from 0.79 to 0.67 (with an 0.8 correlation threshold). This gives confidence in the quality of the correction.
This correction of the regional elevation change also significantly modifies our interpretation of where subglacial water goes. During the last months of the observation period (mid 2008 till end 2010), the surface of the ice sheet (outside of the lakes) fell by over 20 cm in average over the Wilkes Land, probably due to a regional negative surface mass balance anomaly. When this regional elevation change anomaly is not taken into account over active lakes (as in the discussion paper), it leads to a decrease of the amount of water cumulated into lakes after mid-2008, i.e. it gives the impression that part of the water escaped out of the subglacial system and reached the ocean. With the new correction, the amount of water stored into active lakes remains constant toward the end of our observation period which we believe is an indication that most of the water is probably trapped in the subglacial drainage system.

Section 5

Section 5.1. The limitation of relying solely on ICESat for lake shape is not a new concept (Smith et al., 2009; Fricker et al., 2010).
P852 L17: You may want to mention this a bit earlier. Also you have mistated Smith et al., (2009); it was not a "natural assumption" it just making the best out of a limited number of data points.

We just wanted to state these limitations and explain the difference with our result, we added references.

Section 5.2. I would remove the word "classic". Should this sub-section be renamed "Inability of satellite radar altimetry to..." Also please note that Fricker et al., 2010 described the limitations of satellite radar altimetry for monitoring active lakes under ice streams, noted that it is not possible to retrieve an elevation measurement from inside the surface trough of a lake whose diameter is similar to the PLF of the radar altimeter, and showed that the ICESat time series and Envisat time series for the same period for the same lake were not correlated. In light of that work, I think it's more significant that radar altimetry works so well in other parts of the ice sheet.

We employed the word « classic » by opposition to the new Delay Doppler Altimeters and interferometric capability of Cryosat-2's SIRAL. We prefer to keep it.

Radar altimetry has some inherent limitations but with appropriate care, some signals can be retrieved, even over fast ice streams. In this case, we agree with the "inability" and changed the section title.

Section 6

The new material mentioned in the Conclusions really does belong earlier in the manuscript and should be inserted elsewhere first if you are to use it here.
P856 LL23: It's nice that you make the Iceland comparison, however the Conclusions is not the place to bring up new material (nothing new should appear in the Conclusions).

We removed any new material from the conclusion. But still thought it was relevant to include some perspectives on ice dynamics.

P857 L9: Try rewriting this as "The dense temporal sampling of Envisat allows a more precise constraint for the onset of lake drainage."

Suggested text included.

P857 L9: What kind of surface features?

We assume the reviewers meant "P856 L23" or 'P855 L1-2". Both "features" refer to the same pattern of shading over the lake area on MODIS mosaics. We detailed what "features" we meant in section 5.4.

P857 L16: "That outlet glaciers that flow into Cook Ice Shelf drain part of the deep Wilkes Subglacial Basin, where much of the bedrock lies below sea level."

We changed this sentence.

Tables

Table 1 caption

If you are instructing readers to "(see text)" you may want to be more specific as to which part of the text is relevant to the table.

Rather than referring to the text, the caption has been detailed

Figures

Figure 1. A color bar would be helpful. I can barely make out the little boxes. I suggest combining this with figure 8 and comparing the estimated areas.

If our paper is accepted, we will ask to the publisher that this figure be printed as a full two-column figure so that the color scale is readable. No comparison is made with Figure 8 because the outline shown in figure 8 of the submitted MS (now figure 10) is extracted from this figure (now figure 4).

Figure 2. Why do you put the values on each data point? We can all read a graph.

We believed this was a good mean to avoid an additional table and thus, an interested reader could pick the exact values he was interested. According to the referee comment, it was not a good idea... So the revised figure now includes the name of the ICESat laser periods and the labels of the horizontal axis all start on 1 January of each year.

Figure 3a-c. The white arrows don't show up against a white background. Also label the red and blue lines.

We replaced arrows by labels on the location map. We added the description of the curves in the "series" panels.

Figure 3d. This needs axis labels and longitude latitude lines. Also the time interval is not clear here.

We restored the axis labels and gave the period for elevation trends.

Figure 4. make the map units in km
Add lines of longitude / latitude
This is probably the best place to add hydropotential (Figure R1 (supplemental file)).
Figure 5. I like the concept behind this, however I have several suggestions for improvements:
Use a larger font on the color bar, and perhaps make it months and year (i.e. Jan 2007, Mar 2007 etc . . .)
For the background contour map use hydropotential rather than surface elevation.
Outline your "lakes"?
Make the map units in km.
Add lines of longitude / latitude.
For your caption you may want to specify where in the text.
This might also be a good place to include surface velocity as well as hydropotential.

We merged former figures 4 and 5 in a single one. Comments are addressed either through new figure 2 or 7.

Figure 6. I appreciate that by changing the sign on the volume change for CookE2 to from volume gained to volume drained you get the curves to match. The danger in the line you use for CookE2 drainage is that you are implying that it subsequently inflated after drainage. I recommend making the volume change for CookE2 and all downstream lakes all the same sign, otherwise it is confusing to the reader.

We understand the concern of the reviewer. However we thought it would be a good representation as we want to compare the output of Lake $Cook_{E2}$ to the volume stored in other lakes. Our convention is now well explained in the legend.

Figure 7. It would look much better if you could make the x axis the actual year, rather than years from 01 / 01 / 2000.
Also the "a" and "b" should be in the upper right of each subplot

4 digits date takes more room in the axes labels but we agree the figure is better with the full date. We changed that.

Figure 8. Why not compare the area of the lake from MOA with the other area estimates you have?

We did not use the MOA to estimate the contour of the lake. The contour plotted on this figure (figure 10 of the revised manuscript), as explained in the legend, comes from the DEM differencing (so from figure 4 of the revised MS).

Our lake area estimates compares well with that of McMillan et al. 2013.