## Authors response to the comments of referee #2

We thank the reviewer for the thorough and very fast evaluation of the manuscript. The reviewers comments concerning accuracy helped us to improve the wording in the manuscript and to include another dataset (IceBridge) to provide further confidence in our results. We would like to mention that the correction of a small inconsistency in our variance propagation of the GNSS profiles and the variable ICESat campaign precisions (recommended by the reviewer) slightly changed the results for the campaign biases. However, this does not change anything in the general messages of the manuscript.

We respectfully disagree with the main scope of the review to withdraw two thirds of the paper, which is also not supported by the other two reviews. We will justify our opinion in the following detailed comments.

**[RC] ICESat intercampaign biases:** *I am very concerned about the presentation of these ICESat intercampaign biases, which are a tricky thing to determine. My concern lies here: 1) the intercampaign biases presented here (Table 3) are determined based on kinematic, traverse–based GPS data, which generally has decimeter–scale accuracy; 2) the GPS traverse data are processed via DGPS methods, using 5 different base–station sites (Vostok, Mirny, Progress, Casey, and Davis), with baselines that can exceed 800 km, and therefore, I question the accuracy of these DGPS results, especially when considering that the troposphere (and ionospheric) corrections have to span that spatial scale; 3) the authors claim to assess the accuracy of the traverse data by comparing the DGPS results of the various baselines; while comparing various baselines is a good strategy for beating down the noise in the solution, this strategy represents an assessment of the spread of the result, or the PRECISION, not the accuracy, thus, the authors have no assessment of accuracy, or ground 'truth'; 4) to develop intercampaign biases, you have to make an assessment of ICESat vs 'truth', and given the comments I have made about accuracy/precision, I do not believe that the authors have done this adequately; 5) to develop intercampaign biases, you also need to do this using relatively coincident (with respect to time) data, or you have to show that the surface is not changing; from Figure 2a, the surface being used as ground 'truth' (which is a substantial percentage of the East Antarctic Ice Sheet) is changing at decimeter scales, yet the authors obtain cm–level bias corrections. Given that they are using GPS data from a changing surface, with a gap in time associated with the first half of the ICESat campaign (2003 – 2006), I again do not believe that the authors have adequately defended their set of intercampaign biases. Therefore, overall, this method does not represent the rigorous attention to detail needed to determine cm–level intercampaign biases. Yet casual users of ICESat data will take Table 3 as 'truth'.*

(1) The accuracy of a single epoch is not much better than a decimeter, but we have to consider that in the bias estimation we use more than 80,000 crossovers in the Lake Vostok region from 15 profiles spanning 14 years. Furthermore, the GNSS-processing was performed on a daily basis, i.e. each day can be considered as an independent estimate and each profile consists of several days, even in this region. This high number of individual results gives us the confidence that our biases are very well constrained.

(2) In the processing, the ionospheric effect is eliminated as we use the ionosphere-

free linear combination L3. The troposphere is fixed for the reference station in a preprocessing step and then estimated together with the coordinates for the kinematic sites. This means that for each single baseline solution, the noise includes the uncertainty of the tropospheric estimate, which is summarized as $\overline{RMS_{BL}}$.

(3) This is the most important misunderstanding we hope to resolve with this response. The terms accuracy and precision have indeed not been used with sufficient severity in the manuscript. This has been corrected now throughout the paper.

Apart from this, the reviewer's notion that we just compare multiple baseline results and claim that they reflect the accuracy is a misunderstanding. We assess our surface elevation data using crossovers between different profiles of one season, as practiced also in other peer-reviewed works (e.g. Siegfried et al., 2011; Kohler et al., 2013). These profiles are independent to a very high degree. We agree that an external independent dataset could help to further pinpoint their accuracy. Therefore, we now included crossovers with an IceBridge ATM track crossing Lake Vostok. There are relatively few crossovers (between 1 and 9 per profile) and surely some of them may be affected by local peculiarities but in sum, they show, that $\overline{RMS_X}$ is a realistic estimate. We conclude therefore, that within the given confidence intervals our data are indeed ground 'truth'.

(4) see (3).

(5) We do not determine our biases over the whole area. We only use the Lake Vostok region (100–108.5 E, 76–79 S). We now make this more clear in the manuscript. Elevation change is accounted for by the term $\dot{h}$ in Eq. (4).

**[RC] DEM assessments:** *A DEM represents a snapshot of the ice surface at some specific time. The DEMS that the authors are assessing are from about 2006 and 2009, while the validation data (from the traverses) generally spans the subsequent 6 to 8 years (Table 1). The result they find is that near the coast, where the surface is steeper, the DEM elevations deviate from the GPS data. However, steep–slope areas around the coast of Antarctica are also where the ice sheet is changing the most (Pritchard, et al 2009); their result is probably at least partially associated with real surface change. The DEM comparison, in my mind, is pointless.*

The elevation changes in the area of our profiles do hardly exceed 10 cm/yr, even in the margins. For the mentioned time span this affects the assessment by less than 1 m. In contrast, the differences of the elevation models are in the order of 20 m in this area. We agree that the mentioned effect exists, but we show in this paper that its magnitude is insignificant compared to the observed elevation differences.

*I suggest that the authors remove the intercampaign bias and DEM assessment sections of this manuscript.*

We respectfully disagree for the reasons mentioned above.

**Specific Comments:**

*– line numbers are needed*

They have existed in the submitted document (Copernicus Latex template, compiled with the 'manuscript' option), but disappeared in the type setting step for the discussion paper.

– *abstract lists accuracy of in situ data, but not precision. What is accuracy based on?*

As mentioned in (3), on intra-season crossover differences ($\overline{RMS_X}$), now on IceBridge profiles too.

– *"A crossover analysis with three different Envisat..." this sentence is so specific and doesn't allow the abstract to stand–alone. Consider edit.*

Edited.

– *Baseline B, to the best of my knowledge, is no longer available, thus, these results are not reproducible.*

Unfortunately this is true. However, as most of the recent publications (e.g. Armitage et al., 2014; McMillan et al., 2014; Simonsen and Sørensen, 2017) still use Baseline B, we think that these are very important results regarding the interpretation of those results.

– *give example(s) for 'systematic effects'*

Done.

– *"One crucial step in the processing of surface elevations from satellite radar altimetry... is the slope correction": this is not unique to radar; this was a big problem for ICESat, which had smaller, 70–m footprints. I believe what the authors are getting at is the large error in the radar. But this error is not negligible in the laser altimetry, when significant mass change in east Antarctica is associated with cm–level surface change. An edit is needed here.*

At this point in the introduction we are not talking about laser altimetry at all. The slope error is much more crucial for radar altimetry as for laser. For radar retracking, the retracking point in the leading edge corresponds to the point of closest approach (POCA). In contrast, the gaussian fit in the ICESat laser signal retracking refers to the middle of the return signal and thus the footprint average. Schutz et al. (2005) state, that 'an error of 1 arcsec in laser pointing knowledge, combined with a surface slope of 1°, will introduce an effect of 5 cm on the inferred spot elevation'. Thus we agree, that the slope has to be considered when it comes to accuracy but we think, there is no 'slope correction' as for SRA.

– *Author needs to verify proper mission naming conventions throughout (e.g., CryoSat–2, NOT Cryosat–2; Ice, Cloud, and land Elevation Satellite, NOT Ice Cloud and Land Elevation Satellite)*

Checked and changed.

– *"...(ICESat) mission these effects do not arise..." be more specific here. The slope issue does arise in steeper terrain. I believe you mean volume scattering, which is NOT necessarily mitigated by the use of laser altimetry (as opposed to radar altimetry); certain wavelengths of light could potentially volume scatter.*

See answer concerning slope above. We are aware that the slope causes an additional uncertainty. However, when calculating elevations at crossover locations, we believe the interpolation error is much larger than the slope error due to a non-uniform slope in the footprint (which would cause an asymmetric return waveform). This is discussed in Sect. 3.3.3. A comment on slope effects has been added.

*– write out GPS and GLONASS here in the last paragraph of the Intro. You write them out in section 2.2, but THIS is the first instance...*

This has been changed in the manuscript.

*– "This set of surface–elevation profiles..." Processed? Raw data?*

We do not see an ambiguity in our wording. The previous sentence states that the surface elevation profiles are derived from kinematic GNSS observations. We think when mentioning "surface–elevation profiles" it should be clear, that those are the final profiles, no raw data.

*– "The profiles acquired on snowmobiles provide accuracies of only a few centimeters..." Based on what? Are you comparing the snowmobile data to the static site?*

To detect possible biases between a static site and a kinematic rover, a very detailed knowledge of the topography (including microtopography) around the static site would be necessary. As stated by Richter et al. (2014), "Crossovers between the profiles acquired during the same field season (usually within a few days) are not affected by long-term surface height changes and are therefore used to assess the accuracy of the surface height determination." A similar comparisons between two profiles during the same day have been performed by Siegfried et al. (2011). They find a mean elevation bias of 9 mm. Also King et al. (2009) state that in their profiles "Uncertainties of the GPS-derived heights are 0.05 m with effectively zero bias". References have been added.

*– "...and are thus well suited for precise studies on local elevation and elevation changes..." Only if their precision (again, compared to what) is also small/good. Given that you don't use these data, I'm inclined to tell you to remove this text (and the accuracy text).*

This paragraph explains the differences between kinematic GNSS on lightweight snow mobiles and heavy tractors. We find it important to mention this to explain the reader the additional difficulties related to the correction of the offset between antenna and snow surface.

*– regional peculiarities: like what? Slope? Surface compaction? What else?*

Kohler et al. (2013) subdivide their area and relate different mean offsets to topographic peculiarities but furthermore to different driving constellations of the two vehicles as well. As we measured the antenna–snow-surface offset repeatedly, we expect such effects to be of minor importance. However, especially topographic features play an important role in the slope correction. Thus, only long-range profiles allow a statistically significant analysis involving larger areas.

*– Figure 1: a colorbar is not useful for capturing the date detail. Make a legend or label them in the figure.*

Details about the dates are given in Tab. 1. Figure 1 intends to give an overview and thus we think (together with the area column in Tab. 1) this figure gives a good overview over the times and locations.

*– Processing: 800 km baselines are long. Did you try looking at PPP solutions? Kohler et al 2013 used PPP specifically for this reason. You could use the DGPS method when close to the stations and compare your results.*

We added some sentences in the GNSS data processing section. Geng et al. (2010) compare the two processing methods and found quite comparable results even over longer baselines. Compared to the results of this work we think that our DGPS results are even more reliable as we use a combination of several baselines.
Kohler et al. (2013) use Precise Point Positioning (PPP) which does not require reference stations but instead need precise satellite clock information for every epoch. Usually these clock offset data have a rate of 30s. Prior to 2004, the rates were even more sparse (5min). Only after 2008, the Center for Orbit Determination in Europe (CODE) publishes a 5s high rate product but this would not be applicable for the early campaigns. Here, the significantly decreasing number of observations would lead to much larger errors in a PPP-solution.
However, comparing the crossover differences between profiles of the same season, our results show even smaller differences compared to Kohler et al. (2013). We suppose that this is a result of our improved antenna offset correction and thus assume, that both GNSS processing schemes are comparable (when applicable).

*– IGS08: is this appropriate for comparison with both ICESat and CryoSat–2? What frame are those data in?*

It was not mentioned in previous manuscript versions. This is not very well documented for most of the datasets but finally we found the information and included it in the manuscript.

*– Perhaps Table 1 could capture which kinematic traverses that included GLONASS*

Included in Table 1.

*– "Therefore, in this case we used the Melbourne–Wübbena and the Quasi–Ionosphere–Free Linear Combination only" this needs more description or references*

Reference added.

*– The last part of this paragraph needs more elaboration/clarity. This is important, given my previous statement about PPP. Your troposphere and ionosphere corrections won't hold up over these length scales. So what does this technique (with which I am not familiar) do to address this critical issue?*

As mentioned in (2), the tropospheric correction from a preprocessing step is fixed for the reference stations. Thus the correction for the kinematic receiver is independent from the baseline length. The ionospheric effect is eliminated as we use the ionosphere-free linear combination (see Dach et al., 2015) in the final step.

*– "Altimetric elevations, in contrast, refer to the "mean tide" system..." I believe*

*that ICESat has tide–free (WGS–84/ITRF) height as well as TPX 'mean–tide' heights. If so, this statement is not entirely accurate. How does your conversion compare to what's on the ICESat data product?*

The field "deltaEllip" in ICESat is "Surface Elevation(T/P ellipsoid) minus Surface Elevation (WGS84 ellipsoid)." To our understanding, this only refers to the ellipsoidal parameters a and f but does not correct for different conventions in the reference system such as the handling of the permanent tide.

– *"A more realistic measure is found by comparing multiple baseline solutions." What about comparing to PPP solutions?*

See comments above concerning PPP for older datasets.

– *Further, comparing GPS solutions from multiple baselines does not compare these GPS data to 'truth'. Without 'truth', you cannot get at an overall bias/accuracy assessment of your GPS data. Instead, your RMS_BL informs you about the reproducibility, or spread, of the results; this is the precision of the solution, not the accuracy/bias, which is the difference between the measurement and truth. RMS_BL may be a meaningful error assessment, but not as described.– Same of RMS_S*

– *Same for RMS_X. These are all spreads of the data.*

As mentioned above, we agree that $RMS_{BL}$ is precision, not accuracy. However, Shuman et al. (2006) call intra-campaign crossovers a "relative accuracy". Our crossovers between different profiles of one season ($RMS_X$) are independent measurements to a very high degree. The antenna/snow-surface offset is independent, satellite constellation is completely different, the equipment used is different. The only common thing among those profiles is the use of the GNSS technique. As GNSS measurements are used to define the IGS/ITRF reference system, we assume the technique itself to be free of offsets.

– *For RMS_X, a useful value would be the number of crossovers per traverse. How large is the dataset 'N'?*

This table is already very large and contains very much information. Besides K08C, the smallest amount of $RMS_X$ crossovers is 26 between K11A and K11B. Usually there are several hundreds up to several thousands (more than 21,000 between K12E/F) of crossovers. We do not believe, that this number of crossovers contains any significant information here.

– *"While crossover differences within one expedition are used for accuracy estimates, the elevation differences in crossovers between profiles of different years allow to assess temporal rates of surface elevation changes (h_dot)." The first part of this statement is not accurate: again, RMS_X is an assessment of precision, not accuracy. The second part of this statement is true, as a differential assessment (h_dot) does not require absolute 'truth'.*

As discussed above towards $RMS_X$, we believe, that this is a good measure for accuracy.

– *"are found on the traverse to Mirny. In the lower parts..." lower = elevation?*

Yes, changed.

*– "Our profiles shall be used nevertheless for the validation of SRA, which is..." You will be comparing this to ICESat as well, yes? Then perhaps SRA is not the best term to use. Perhaps 'SA'?*

The whole paragraph has been rewritten.

*– From section 2.4 to section 3, these are really results. And then other datasets are introduced in section 3... It might be good to reorganize the paper a bit to have all of the data introduced early (then perhaps questions associated with, e.g., IGS/WGS84 are answered immediately).*

We had many discussions about the structure of the manuscript and decided against introducing all datasets together due to the very different nature of these types of data and thus the methods applied.

*– "Above steeper terrain, the altimeter is switched to SARIn Mode..." 'Above'?*

The altimeter is "flying above" the terrain.

*– Fricker et al., 2005, Shuman et al., 2006, Kohler et al., 2013, Siegfried et al., 2011 should all be cited in the ICESat accuracy assessment section. Most of these were 'on–ice' or 'ice–like' surface assessments. Schutz et al, 2005 is an ICESat overview paper, not an assessment based on in situ data.*

Schutz et al. (2005) defined the mission requirements of "a series of points on the ice sheet with vertical accuracy at the decimetre level". We agree that a validation is even more convincing than mentioning the mission goals. Thus we changed the reference to Fricker et al.(2005) and Shuman et al.(2006). However, as they relate to very early releases of the ICESat data the results are not comparable any more and we did not relate to them in the further assessment. A section comparing the results of Kohler et al. (2013) and Siegfried et al. (2011) has been added.

*– "Our validation approach is the following: We assess how accurately the altimetry data reflects the actual surface elevation at the nominal positions of the altimetry data." This assumes that the in situ data are 'truth' and error–free, which is probably never the case. It's reasonable to make that assumption, it just has to be stated, with the caveats.*

See above. This "validation" is exactly the same as Siegfried et al. (2011) or Kohler et al. (2013) did. The much higher amount of independent profile makes our ground 'truth' even significantly more reliable.

*– "On the other hand, in this zone typically the largest elevation can be expected" this is not clear to me.*

This comment seems to refer to an older version of the manuscript where "changes" was missing. In the discussion paper it is "elevation changes".

*– "ICESat surface elevations are less sensitive..." 'Relatively' less sensitive. This is still an issue. For cm–level surface change, the effect of slope on ICESat data is still significant, especially in your 'zone 1'.*

We are writing "less sensitive", not "not sensitive", so we think, this is correct.

*– "Including the unbiased GNSS profiles..." The authors are trying to present a new set of ICESat intercampaign biases. The community may cite these widely.*

*My concern is that theauthors haven't truly provided and accuracy to their kinematic data (they instead provide precision). I am strongly against presenting a new set of bias corrections this way. See my comments above.*

See comments above. Even if all our datasets would have an offset (e.g. of 34 cm to match the average Zwally et al. (2015) biases), the relative biases would still be correct, which would still be sufficient for the detection of elevation change rates from ICESat. But nevertheless, now by comparing the ICEBrige data too, we are even more confident, that this is not the case.

*– "For the ICESat elevations, in contrast, we assume a homogeneous accuracy and adopt a standard deviation of 10 cm..." also not a good idea. The spread (standard deviation) of ICESat data increased (got worse) with extended laser life. It was not static.*

We completely agree with the reviewer. We have changed this towards the use of intra-campaign $RMS_X$ now, which slightly changed the resulting biases.

*– Figure 4: "4 ... 9" must mean 4 – 9?*

Was already changed in the final discussion paper.

*– Table 2: Are statistics with an 'N' of 2 (or 3 or 6) really meaningful? I know you acknowledge this in the text, but it jumps out at you in the table.*

We agree. This has been marked more obviously and mentioned in the caption, too.

*– Fig 5: What are the 'N's associated with each assessment?*

'N's added.

*– Prior to the validation of ICESat elevation data, we first determine the ICESat laser campaign biases as described in Sect. 3.2.2." Again, I express my concern on how this is being presented, given that a more rigorous accuracy assessment needs to be made for the ground–based data. Note that for many of the other assessments of the intercampaign bias, the timing of the in situ data and the ICESat data was taken into consideration (e.g., Fricker et al., 2005, Borsa et al., 2014, Siegfried et al., 2011). These GPS data have very little overlap with ICESat overpasses.*

We take the timing into consideration as we estimate $\dot{h}$ together with the biases.

*– "...not surprising that our biases are very similar to the set presented by Richter et al. (2014) for R33 including the Gaussian–Centroid (G–C) correction" What does this mean? Did you remove the G–C correction from the R33 data to make the comparison?*

Perhaps the wording was a bit ambiguous. We have changed this in the manuscript.

*– "we perform an absolute calibration..." I don't believe this to be true, given what I have said about the RMS method of determining 'accuracy'.*

This point has already been discussed towards $RMS_X$ above.

*– Fig 7b: what are the 'N's? Also, right–most panel shows that slope has an impact on ICESat (comments above)*

’N’s added. It was never said, that the slope has no influence. We just say, that there is no slope correction, as the elevations refer to the footprint average (gaussian), not the POCA. The increased standard deviation is discussed in the text.

*– Section 4: Why are we validating the 2007 and 2009 DEMs (which are snapshots of the ice surface at some specific time) with these GPS datasets, which generally (from Table 1) span the subsequent 6 to 8 years? This is not meaningful.*

See discussion towards "DEM assessments" above.

*– “However, with increasing slope the standard deviation of this DEM grows...” High slope areas around the coast of Antarctica are also where the ice sheet is changing the most (Pritchard, et al 2009). Thus, some of these differences are probably associated with real surface change. I ask again why are the authors validating a 2007 and 2009 DEMs with these GPS datasets, which generally span the subsequent 6 to 8 years?*

Yes, some part of the differences might be related to elevation changes, as discussed in the end of Sect. 3.3.3. However, those results also pinpoint the magnitude of those effects. In the "$> 0.5°$"-zone ICESat-Kin is -0.13$\pm$ 0.35 m. For the ICESat-DEM, this is -7.67$\pm$ 28.12 m. The significant difference shows, to what extent elevation changes might have played a role here.

*– “The comparison of the CryoSat–DEM with the ICESat–based models proves that SRA with advanced instrument design provides excellent elevation information over all zones” Note that the traverses are more coincident with the CS–2 time period. Again, this comparison ispointless.*

We do not think so. All evaluated DEMs rely mainly on ICESat, except the CryoSat-DEM. This comparison shows the influence of the interpolation error in ICESat-based DEMs. The CryoSat-DEM, even if not as accurate at the data locations itself, does not have this weak point.

*– “We resolved the challenges of the GNSS processing, such as the very long baselines and ...” I do not think that you have characterized the accuracy, therefore, I do not feel that this statement is valid.*

See discussion above.

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