First of all, we would like to thank both reviewer's for their careful reading of the manuscript and their helpful and constructive comments. Their suggestions are really appreciated and helped a lot to improve the manuscript.

General remarks:

1) We fully agree with the reviewer's, that the manuscript lacks in the presentation and description of errors (e.g. area and volume). We will take care of this in a revised version (to be submitted after this response).

2) As the Reviewers has several comments concerning the presentation of the data coverage, and hence questions, we will provide an improved figure showing the used data sets. Therefore, we will separate the data sets and plot in an updated Fig. 2a) ice surface topography profiles (DGPS) and Fig. 2b) ice thickness/bedrock topography profiles (GPR). Additionally, the data sets from the different campaigns are will be color coded to show the temporal coverage We hope that clarifies the data coverage and, in particular in which parts the DEM's are based on data and where we used extrapolation.

3) The specific comments have been addressed and in almost all cases we followed the reviewer's recommendation.

4) The English spelling of the manuscript will be corrected by a native speaker.

In the following, we quote the reviewer's comments followed by our replies in bold typesetting.

'Done.' denotes that this point will be solved. This could be that it has been either done directly, or that due to other changes the point does not arise any more, or that the point has been answered at another place in this text already.

Referee #1 (Ola Brandt)

This is a nice presentation of an important data set – good job! I only have a few comments/ideas/clarifications that I would like to see and hear more about (sorry, comments are not in importance order, neither in chronological, just random):

1. There are stuff in the section "Results" that should go under the heading "Methods". I am particularly thinking of: "Furthermore we added the coast line (taken from Ruckamp et al., 2011) with values of 0m for the ice thickness except for the airborne surveyed areas. The spatially unstructured data set along the profiles was then grided using the kriging algorithm on a 250 m grid for the ice surface topography zs and ice thickness H. Subtracting the ice thickness grid from the ice surface grid, we obtain a grid for the bedrock topography zb. With these data sets we constructed digital elevation models (DEM) for the ice surface and bedrock topography as well as a map of the ice thickness distribution." This is not results! That is methods and data... Please move to appropriate heading.
Makes sense, the section will be moved accordingly.

2. In "Results" section "3.1 Ice thickness map". Ice volume and area... Could you give some error estimates on the area/volume?
Done. Cf comment of Referee #2 (F. Navarro) on the same subject.

3. In "Results" section "3.1 Ice thickness map". "The smooth contours e.g. near the Admiralty Bay coast, are an extrapolation artefact." Could you mark this area in fig? and areas where the data density is poor and the resulting grids is mostly an extrapolation construction? Maybe fade the area out a bit? Quasi transparent? Just so people like me, not familiar with the area, quickly can see

where the good data is and where is it may be a bit more questionable. I also think that is important for modelers -imagine you have some really detailed areas where you reveal rough bed topography well and then you have extrapolated smooth areas... that makes a difference.
We will provide an improved Figure 2 showing the data coverage. In the new Fig. 2b) we shaded the area which is based on extrapolation.

4. I would find it very useful if you would have plotted the sounded lines on top of the grids. I know it could look a bit messy -but I do think it would help when getting an idea of what is extra/interpolation constructions. Maybe show the gridded area (like a polygon) as well as the taken surface topography area in fig 2?
As mentioned above, we will provide an improved figure showing the data coverage. We hope that clarifies which parts are based on extrapolation.

5. “Results”, “After merging the data sets, we included an already existing ice surface topography data set for the Admiralty Bay available via SCAR KGIS (Braun et al., 2001).” Is there any way you could show where this is in relation to your profiles? Sort of a map showing all the data sources? Fig 2?
As mentioned above, we will provide an improved figure showing the data coverage. Regarding this point, we plot in the new Fig 2a) the ice surface topography data from the SCAR KGIS database.

6. What is actually the blue line in all the grid figures (4-6)? Coastline? Maybe also change color?
Yes, it is the coastline. The choice of the blue color is bad; therefore we will plot the coastline in black with a dashed style. Additionally we will refer to in the legend/caption.

7. “The radar consists of a shielded broadband antenna system with integrated electronics for downward transmission of the 30 MHz wavelet and reception of upgoing reflected waves.” (bottom of p 125). Later (p 127 line7) you write “frequency domain Butterworth-Bandpass filter from 5 to 30 MHz” -Sounds a bit strange to me to apply a bandpass filter where the upper “limit” is set equal to the center frequency. If it is broadband you cut away loads of the energy? What is the reason for putting the upper limit to 30 MHz -equal to the center frequency?
Yes, the reviewer is right regarding this confusing information. The BGR-P30 system was developed to have a center frequency around 30 MHz. However, the radargrams (frequency spectrum) shows that the center frequency is around 25 MHz. Therefore, the upper limit of the Bandpass filter of 30 MHz equals not the center frequency. We will add a clarifying sentence in section 2.1 “GPR system”: The actual spectral maximum of the BGR-P30 system is at 25 MHz (corresponding to wavelengths of 6.7 m in ice and 12 m in air), the bandwidth is approx. 20 MHz. The spectral content of thin-layer reflections and point diffractions is slightly shifted towards higher frequencies. To suppress these and to emphasize the bedrock reflections a filter setting of 5 to 30 MHz is appropriate.

8. Page 127, Line 14, “The ice surface elevation zs was obtained by subtracting the thickness of the air layer derived from the airborne GPR measurements from the measured DGPS height at the GPR antenna”. How accurate do you get the ground elevation? Did you do the pick manually or automatic? What is the crossover accuracy over snow? What difference do you get depending on surface type (powder snow, wet snow, ice, stone)? Any risk for bias? i.e. you pick the right point in the returned waveform. For instance if you have a surface return mixing with a near surface return, the wave form you get will differ compared to a return from a single surface return. How do you account for that? Have you flown over different surfaces with known elevation to see what accuracy you get? Not that I think it really matters here just curious …
Compared to the DGPS height (with an accuracy of a few centimetres) the GPR accuracy is rather low (ca. Lambda/4 in air). Therefore, the given mean accuracy of approx. +/- 1m (Page 129, Line 2) is the accuracy of the picked surface reflection. For picking the reflections, we used the REFLEX automatic picker. However, if the automatic picker looses the phase of the reflection, we have to correct manually. As we had limited flight hours, we were not able to test the accuracy over different surface types.

9. P 128 line 26. “The estimated vertical accuracy of the ice surface topography is about #6 cm for the groundbased measurements (a few thousands of crossover check points).” I know I am picky now... but is the “surface elevation” relative the “ice” surface? Or relative the “snow” surface? Or relative the “firn”? or what is it? I assume it is the snow surface? Or have you subtracted the winter snow?
Your assumption is correct: it is the snow surface. However, in the ablation area (below ~250m a.s.l.) it is the ice surface, because all the winter snow was melted. Therefore, we refer the data set as to be valid for the 2008/09 austral summer (Line 128, 18/19). As the section describing the error estimates will be rewritten (cf. Comments Referee #2 (F. Navarro)), we will also add a clarifying sentence.

10. There are some error estimates/ideas given -But I would like to have a bit more ..., especially in the results/conclusion sections. I know it is tricky, and I am not sure there are any good robust methods to really give it on the final interpolated results, but nevertheless a rough idea would be nice. I would guess you have an idea – don’t think at this stage it needs to be much more than that.
That is a good point, thanks! In our manuscript, we provide only an error estimate for the original data, but not for the DEM's. Therefore, we will calculate the vertical differences from the grid to the original data (soundings) using a bi-linear interpolation. That results, for instance, in a mean difference of +/- 0.53 cm. In the revised version we will provide errors for the different areas (i.e. Bellingshausen Dome, Arctowski Icefield, Central part, and the airborne survey in the coastal parts). We will add a paragraph in the revised version.

11. Further ... you could make a “heavily crevassed zone” map as well? Do you clearly see where there are lots of crevasses in the data?
However, one has to define “heavily crevassed”? We used this wording to emphasize the risks with groundbased measurements. Therefore, we need the helicopter in these regions. However, an idealized map would show crevassed zones in the airborne surveyed regions with increasing “crevasse density” towards the coast. This crevasse distribution corroborates in general with our field observations. It fails, for instance, in the area around x=412.5km; y=3122.5 km as there are no crevasses observed. However, that is not very sophisticated and, in particular, not the answer to your question. Yes, I think we could create such a map as we see crevasses in the radargrams. But I think, such an analysis goes beyond the scope of this paper, in particular as I would include remote sensing data (e.g. TerraSAR-X imagery) as validation parameters. However, it is a nice idea for a further analysis of the KGI data. Additionally, an optical camera will be added to the BGR-P30 system to document surface structures.

Again, I think it is a nice piece of work, and I can imagine all the data processing... It doesn’t really come through in the paper... Most be loads! Ola Brandt

Referee #2 (Francisco Navarro)

This is an interesting paper describing the geometry of King George Island (KGI) ice cap (ice surface, ice thickness and, from them, subglacial topography) with a level of detail that is rarely found for ice thickness data. The detailed geometry provides an excellent input for numerical models of glacier dynamics. Moreover, the location of KGI in a region of Antarctica that has undergone a marked warming during the recent decades gives this dataset an added value. I recommend the publication of this paper, subject to some changes detailed below.

As the other referee (Ola Brandt) has already reviewed the paper and made many suggestions (that I have read, and I agree with most of them), I will try to avoid repeating suggestions, except in a couple of cases in which I wish to emphasize that the change should be done or I wish to provide some further comment on the same subject. I will state my comments/suggestions by order of appearance (not in order of importance), and will include at the end a list of minor typos/corrections.

Upon implementation of the changes, I would recommend the authors to send the paper to a native English speaker for review of the writing style/grammar. Though written in a rather good English, it would clearly benefit from such an additional review.

I have also reviewed the accompanying dataset, and found the data properly described and structured.

COMMENTS/SUGGESTIONS:

Page 124, line 23: State approximate area of KGI (1250 km^2), with reference (e.g. Simões et al., 1999, that is already in your reference list).
Done.

Page 124, lines 25-26: You mention that "the ice caps of the SSI are regarded as temperate", without giving any reference. Though it is true that some authors have made such kind of statements, it is also true that there are evidences of an upper cold ice layer in the ablation area of some glaciers ending on land and of cold ice patches under the firn in the accumulation area (Molina et al., 2007; Navarro et al., 2009). Even the authors, in their Blindow et al. (2010) paper, point out that "Diffraction features are abundant in areas of surface elevation below 400 m a.s.l. but are scarce or absent in areas above that. We used the abundance of diffractions (high backscatter) as an indication for temperate ice caused by liquid water content" and that "radar backscatter vanishes in the few profiles below the equilibrium line (at 160 m a.s.l. in 1991/92) which are cold according to Wen and others (1998)". Consequently, I suggest changing the original sentence to something like "the ice caps of the SSI are mostly temperate, though a polythermal structure has been suggested for the ablation areas of some of their glaciers and ice caps (Wen et al., 1998; Navarro et al., 2009; Blindow et al., 2010)".
Done. We followed the reviewers' suggestion by changing the sentence to: "Due to their small size and and geographical location in maritime climate conditions, the ice caps of the SSI are regarded as mostly temperate (ice temperatures at or close to pressure melting point conditions); meltwater may be present within the ice body (Paterson, 1994). A polythermal structure has been suggested for the ablation areas of some of their glaciers and ice caps (Wen et al., 1998; Navarro et al., 2009; Blindow et al., 2010)."

Page 125, beginning: Though "Blindow et al. (2010) provided the most detailed picture so far of the ice surface and the bedrock topography as well as the ice thickness distribution of the KGI ice cap." is a true statement, and it is also true that in Blindow et al. (2010) the authors acknowledge earlier echo sounding efforts in KGI, it would be nice recognizing here such previous efforts by other authors, as done in Blindow et al. (2010). If you do not wish to overload the paper with too many references, add at least a sentence such as "There were some earlier echo-sounding works on KGI, though with limited coverage and sometimes ambiguous results. References to these earlier works can be found in Blindow et al. (2010)".
Done. We will follow the reviewers' suggestion and add the suggested sentence as we do not want to overload the paper with too many references.

Page 125, lines 8 & 22: I suggest that you delete the word "monopulse" in line 8 (leaving just "The GPR equipment used ..") and that, in line 22, you change "This 30 MHz monopulse system" "This 30 MHz impulse system" (or, if you prefer, to "This 30 MHz GPR system". As discussed in Navarro and Eisen (2010): "An impulse radar is one whose waveform is a short individual pulse (typically, a single-cycle sine wave or a Ricker wavelet). Its most distinctive characteristic is its very wide bandwidth related to the short pulse, as opposed to the very narrow bandwidth of conventional pulsed radar systems.... The fact that impulse radars are mono-cyclic (i.e. transmit a single cycle) has implied that some authors in the glaciological literature have used the term monopulse radars for them. We do not recommend such use, as the term monopulse radar has a different meaning (referring to an adaptation of conical scanning) in the more general radar literature.... GPR uses essentially the same technique as impulse radars. Perhaps the main difference is that GPRs achieve higher frequencies than classical impulse radars, as present GPRs cover a range of

frequencies between 12.5 MHz and 2.3 GHz." Thus, a GPR is a modern term to refer to a type of glacier that, in the general radar literature, is referred to as impulse radar, and monopulse should not be used for a GPR to avoid confusion with such term as used in the general radar literature.
Done. We will use "This 30 MHz GPR system" in almost all cases. At first appearance, we will use "This 30 MHz GPR impulse system".

Page 125, line 12, and page 133, line 8: State the reference as "Smith and Evans (1972)", that is a classical one, and change reference list entry to "Smith, B.M.E.". I am aware that, in the title page of their J. Glaciol. paper, the author name appears as "B.M. Ewen Smith", but Ewen is a first name (of Scottish/Gaelic origin), and even themselves –Smith and Evans (1972)– refer to Smith's papers, in their reference list, as B.M.E. Smith.
Done.

Page 126, lines 13-15: The grids are orthogonal. Consequently, I wonder whether saying "orientated in northwest-southeast direction" and "arranged in north-south direction" is correct or it would be better to state "orientated in NW-SE and NE-SW intersecting directions" and "arranged in N-S and E-W intersecting directions", respectively.
Done.

Page 127, line 8: I fully agree with reviewer Ola Brandt in that using a bandpass filter 5-30 MHz for filtering radar records from a radar with central frequency of 30 MHz is rather unusual and should at least be justified.
Yes, both reviewers are right regarding this confusing information. The BGR-P30 system was developed to have a center frequency around 30 MHz. However, the radargrams (frequency spectrum) shows that the center frequency is around 25 MHz. Therefore, the upper limit of the Bandpass filter of 30 MHz equals not the center frequency. We will add a clarifying sentence in section 2.1 "GPR system": The actual spectral maximum of the BGR-P30 system is at 25 MHz (corresponding to wavelengths of 6.7 m in ice and 12 m in air), the bandwidth is approx. 20 MHz. The spectral content of thin-layer reflections and point diffractions is slightly shifted towards higher frequencies. To suppress these and to emphasize the bedrock reflections a filter setting of 5 to 30 MHz is appropriate.

Page 127, lines 17-19: As it is written, it is not clear whether you did CMP measurements additional to those reported in Blindow et al. (2010) or you just use the latter. I believe that you are using the same values reported in Blindow et al. (2010). If so, please change the sentence to "We used the common mid point (CMP) measurements reported in Blindow et al. (2010) to determine the velocities of the radar signals in the subsurface." (otherwise, i.e. if you did additional CMP measurements, please give their results). This sentence could be even extended to give the values (0.194 and 0.168 m/ns) obtained for the two-layer (firn-ice) model, which would justify eq. (1). You could also mention that these values are consistent with those found by Travassos and Simões (2004) on KGI (0.194 and 0.168 m/ns) and by Navarro et al. (2009) (ca. 0.190 m/ns for a firn layer 20 m thick) on Hurd Peninsula Ice Cap, Livingston Island (by two independent means: CMP measurement and by applying the relationship between RWV and ice density by Macheret and Glazovsky (2000) to the density data retrieved from a neighbouring borehole).
We used the same CMP measurements reported in Blindow et al. (2010). Therefore, we changed the sentence as suggested by the reviewer: "We used the common mid point (CMP) measurements reported in Blindow et al. (2010) to determine the velocities of the radar signals in the subsurface. These measurements revealed a two-layer model with a firn velocity of 0.194 m/ns and an ice velocity of 0.168 m/ns. These values are consistent with earlier measurements performed by Travassos and Simões (2004) on KGI and by Navarro et al. (2009) on Hurd Peninsula Ice Cap, Livingston Island."

Page 127, line 21: There is an erratum (parenthesis misplaced) in eq. (1): it should be: $H = 0.194 * t_{firn} / 2 + 0.168 + (t_b – t_{firn}) / 2$.
Done.

Page 127, line 22 to page 128, line 4: Your firn correction (eq. 2) overestimates the thickness of the

firn layer at the upper elevations (ca. 500-700 m a.s.l.), especially at the uppermost ones (up to 50%, for z=700 m a.s.l.), and slightly underestimates its thickness at the middle elevations (ca. 250-500 m a.s.l.), resulting in a slight overestimate/underestimate of the thickness of the entire firn-ice column. You mention 3 "typical" firn thickness at 3 different elevations: 0 m at 250 m a.s.l., 25 m at 400 m a.s.l. and 40 m at 700 m a.s.l. While eq. (2) indeed produces t_firn=0 ns (and thus H_firn=0 m) for z=250 m, it gives t_firn=210 ns (H_firn=20 m, using v_firn=0.194 m/ns; underestimated) for z=400 m and t_firn=630 ns (H_firn=61 m; overestimated) for z=700 m. Are these numbers significant? Let us analyze the largest discrepancy: for z=700 m, eq. (2) gives t_firn=630 ns, while it should be 412 ns for the typical thickness of 40 m. This implies that you are assuming that a larger portion (than the real) of the TWT corresponds to firn. By eq. (1), this implies an overestimate of the ice thickness by an amount Delta_H=(0.194-0.168)*(620-412)/2 = 2.7 m, which is nearly coincident with the vertical (range) resolution of a 30 MHz GPR, which is 2.8 m (assuming that the vertical resolution is about one half the wavelength in ice). Consequently, such error is "acceptable". However, it should be noted that it is not a random error, but rather a systematic one. A more elegant solution would have been to adjust a parabola passing through all 3 data points of typical firn thickness. In this way, the systematic error would have been removed. Though this may seem picky (especially considering that the "data" points are just "typical" values), you should recognize that an estimate (by eq. 2) of the firn thickness as 61 m at 700 m a.s.l. is a clear overestimate. The main point is that systematic errors should be corrected for whenever possible, and thus the remaining errors will be expected to be random fluctuations around the "true" value.
Yes, you are right. Our argument was, that these overestimates are negligible. However, we agree that systematic error should be corrected. We will use your recommended fit and update the ice thickness map (also the PANGAEA data set). Errors (crosspoint analysis) also have to be checked, but we are sure, they are not largely influenced.

Page 128, lines 16-18: The sentence "Measurements in areas above 250 m a.s.l. do not require corrections in case of multiple readings in different years. In these areas ice thickness and hence bedrock topography do not show significant changes within a decade." Requires a justification. It is enough if you just refer to Rückamp et al. (2011), since it is clearly seen there that the thickness changes (at Bellingshausen Dome) approach zero above elevations of ca. 250 m a.s.l. (for a decade, I would rather say above 280 m). But perhaps the justification of your statement is the one given in the next paragraph of your manuscript (page 128, lines 20-24). In such case, both paragraphs should be better linked, to remark that the second one gives the justification for the first one.
We will follow the reviewer's suggestion and reorganize and rewrite the section. We agree with him, that the description is unclear. However, we think the main points, which have to clarify are:

1) The assumption and justification that the ice cap above 250 m a.s.l. is in balance comes from comparative DGPS heights collected in several years. The analysis in Rückamp et al. (2011) compares all DGPS data collected from 1997 – 2009. Only the DGPS profiles have crosspoints form 1997-2009, which allows such an estimation. (To emphasize the temporal coverage and overlap of the different data sets, we show the profiles in Fig 2a,b color coded.)

2) The crosspoint analysis of the GPR measurements allows no assumption of changing ice thickness over the time period from 1997- 2009. The GPR surveys are performed in three different campaigns with a poor overlap (groundbased: 1997 at Arctowski Icefield and 2007 and the central part; airborne: 2008/09 at the coastal parts).

3) Here, comparative ice surface heights and ice thickness heights are calculated of each individual campaign to get a value of the accuracy of the grids.

Page 128, lines 17-18: Additionally, the part of the sentence stating that "In these areas ice thickness and hence bedrock topography do not show significant changes within a decade." is

awkward (the bedrock topography changing). Please change to “In these areas ice thickness does not show significant changes within a decade.”
Done.

Page 128, lines 20-24: Does the “meter range or less” vertical accuracy yielded by the crossover analysis include the entire dataset (both ground-based and airborne profiles, at all elevations)? I assume that the answer is yes, and that this means that the crossover analysis similar to that in Rückamp et al. (2011) gave straight lines with Delta_H below 1 m, for all z values, even for elevations below 250 m a.s.l. In any case, as said above, I think that the text in lines 16-24 should be rewritten in order to better link both paragraphs.
As suggested, we will rewrite the section. Cf response to your comment above, in particular point 3.

Page 128, line 24: Mention (if not done earlier) that lambda/2 gives roughly the vertical (range) resolution of the GPR and that, in this case (30 MHz GPR), is ca. 2.8 m (considering a RWV in ice of 0.168 m/ns).
Done. Note that the frequency spectrum shows a center frequency around 25 MHz, therefore lambda/2 is 3.4 m (considering a RWV in ice of 0.168 m/ns).

Page 128, line 25: After saying that “The crosspoint analysis yields a vertical accuracy in meter range or less for the ice thickness (lower than half the wavelength in ice lambda/2)” you just insert a “magic %” for the relative error in ice thickness. Please explain where does the 2.3% come from. Does it arise from the estimated error (or a bound of it) in the radio-wave velocity (RWV)? (such a number would be something “admissible” for it). Some detail in the RWV expected errors should be given. In Blindow et al. (2010) you state that “The variability of firn thickness with altitude introduces uncertainty in the estimated velocities, and we estimate the best accuracy to be +/- 1.7%”. However, not only firn thickness with altitude plays a role. Space (and temporal) variations of water content in temperate ice can account for changes of several percent units in the RWV. As reported in Navarro et al. (2009), in the neighbouring Livingston Island, CMP measurement (in December) at 8 different points yielded RWV values differing by up to 5% (excluding outliers), and associated water contents ranging between 0 and 1.6%.
Yes, you are right. It is not understandable, where the 2.3% error value comes from. Our error estimates are based on the crosspoint analysis. From this analysis we calculated a mean vertical accuracy, delta_z=+/-5.5m. With the calculated mean ice thickness of 238m (P. 129 L.23) we found the relative error of +/-2.3%. As we rewrite the section we will take care of this.

Page 128, line 25 (continued): In addition to the differences yielded by the crossover analysis, and the error estimated for the ice thickness data at the individual data points (2.3%) nothing is said about the ice thickness interpolation error (interpolation of the ice thickness in the grid points of the thickness DEM -250 m grid size-from the GPR data points, collected along profiles with distances between profiles of 1000 m (groundbased, Arctowski), 500 m (ground-based, Central Part) and 700 m (airborne). Any estimate (even if rough) about it?
As already mentioned above (Answers to Referee #1) we provide only an error estimate for the original data, but not for the DEM. Therefore, we calculate the vertical differences from the grid to the original data (soundings) using a bi-linear interpolation. That results, for instance, in a mean difference of +/- 0.53cm. In the revised version we will provide errors for the different areas (i.e. Bellingshausen Dome, Arctowski Icefield, Central part, and the airborne survey in the coastal parts). We will add a paragraph in the revised version.

Page 129, lines 19-23 and page 130, line 1: It would be convenient to accompany all given ice thickness values by the corresponding (+/-) errors determined from your 2.3% error estimate.
Done. All quoted thickness values in the text will accompanied by the corresponding error estimates.

Page 130, line 3: As suggested by reviewer Ola Brandt, area and volume should be accompanied by some error estimates, even if rough. The error in area will depend on the pixel size from the

satellite images used for the ice cap boundary delimitation and the grid size of the DEM. For the error in volume estimate, if something more sophisticated is not available, use sqrt[(Aˆ2*E_Hˆ2)+(H_meanˆ2*E_A)], with A=area, H_mean=average ice thickness and E_A and E_H the estimated errors in average thickness and area. If the estimated error in area is negligible, use just A*E_H as a rough estimate of the error in volume.
Good point! We will follow the Reviewer's suggestion and calculate some error estimates.

Page 130, line 10 (and Figure 5): are z_s values WGS84 ellipsoidal heights or orthometric heights a.s.l.? A clarifying comment such as that in p. 131, lines 3-6 would be convenient.
These are WGS84 ellipsoidal heights. However, at first appearance of a height value in the manuscript (P. 125 L. 2) we will add the following sentence: "All heights are given in WGS84 ellipsoidal heights, if not stated otherwise." and check the whole text for consistency.

Page 130, line 19 (and Figure 6): are z_b values WGS84 ellipsoidal heights or orthometric heights a.s.l.? A clarifying comment such as that in p. 131, lines 3-6 would be convenient.
See comment above.

FIGURES:

Figure 4: Contours shown in figure should be smoothed/filtered to avoid so many "small islands".
Done. We will add a sentence in the caption, that the data are smoothed for presentation.

Figures 5 and 6: Are z values given WGS84 ellipsoidal heights or orthometric heights a.s.l. Obtained using EGM 2008 model as described in page 131? In Fig. 6 caption, change "bordered" to "surrounds".
See comment above.
"bordered" will be changed to "surrounds".

Figures 4, 5 and 6: Contrary to fig. 1 and 2, they do not show the (UTM) coordinates (really, they are not needed in Figs. 4-5-6 (though a scale bar indicating 5 km would be convenient). However, they include an inset stating "Coordinate system WGS84, Zone 21S" that is unnecesary.
Done. We will include the scale bar and dropping the statement "Coordinate system WGS84, Zone 21S"

TYPOS/CORRECTIONS:

Page 124, line 5: Change "newly" to "new".
Done.

Page 124, line 9: Change "They successfully operated" to "It was successfully operated".
Done.

Page 124, line 26: remove duplicated "and" at end of line.
Done.

Page 125, line 7: Change "summer" to "summers".
Done.

Page 125, line 10: Change "with regard to detection" to "with the aim of detecting".
Done.

Page 126, line 4: Change "stacked 256 times" to "256-fold stacked".
Done.

Page 126, line 14: Change "between the profiles" to "between neighbouring profiles".
Done.

Page 126, line 18: Change “length is 250 km” to “length was 250 km”.
Done.

Page 126, line 19: Change “profiles is 700 m” to “profiles was 700 m”.
Done.

Page 127, line 1: Change “there are” to “there were”.
Done.

Page 128, line 8: Change “coincide” to “coincides”.
Done.

Page 129, line 19: Remove “composed”.
Done.

Page 130, line 3: Change “calculated to” to “calculated to be”.
Done.

Page 130, line 3: Change “on an area” to “over an area”.
Done.

Page 130, line 3: Change “characteristic” to “characteristics”.
Done.

Page 130, line 10: Remove “composed”.
Done.

Page 131, line 8: Change “these” to “this”.
Done.

Page 131, line 9: Change “depth of -91 m” to “depth of 91 m” or “height of “-91 m”.
Done.

Page 131, line 14: Change “lacks in measurements” to “lacks measurements”.
Done.

Page 131, line 17: Change “these part” to “this part”.
Done.

Page 131, line 22: Change “glacial” to “glacier”.
Done.

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Referee #2 (Francisco Navarro) Addenda

I forgot to add to my review that I missed, in the database where the GPR+DGPS data is stored, a boundary file including the (X,Y) coordinates of the boundary nodes (the outer limit of the glacier). This is needed for any work with the data. An alternative to the boundary file would be to identify such nodes in the data file itself, in a column with a marker in case the node is a boundary node, but I haven't seen such marker in the file (in any case, I think it is simpler and more effective a separate boundary file).

The reviewer is absolutely right! We will upload a separate boundary file to PANGAEA indicating the boundary type (e.g. ice cliff).