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Earth System Science Data Discussions

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Interactive Comment

Interactive comment on "King George Island ice cap geometry updated with airborne GPR measurements" *by* M. Rückamp and N. Blindow

F. Navarro (Referee)

francisco.navarro@upm.es

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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 kmˆ2), with reference (e.g. Simões et al., 1999, that is already in your reference list).

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)". 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

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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)".

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.

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

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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.

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. 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.

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).

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.

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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.

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

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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.

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."

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.

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).

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%".

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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%.

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 (ground-based, Arctowski), 500 m (ground-based, Central Part) and 700 m (airborne). Any estimate (even if rough) about it?

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.

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.

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.

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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.

FIGURES:

Figure 4: Contours shown in figure should be smoothed/filtered to avoid so many "small islands".

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".

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.

TYPOS/CORRECTIONS:

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

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

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

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

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

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

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

Page 126, line 18: Change "length is 250 km" to "length was 250 km".

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

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

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

Page 129, line 19: Remove “composed”.

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

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

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

Page 130, line 10: Remove “composed”.

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

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

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

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

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

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