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The Cryosphere
Discussions

Interactive comment on "Spatially extensive estimates in annual accumulation in the dry zone of the Greenland Ice Sheet inferred from radar altimetry" *by* S. de la Peña et al.

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We would like to thank the three anonymous reviewers for their helpful comments and suggestions. Following the comments made by reviewers #1 and #2 regarding the data presented in this paper and concerns about similarities of the data presented by *Hawley et al*. (GRL, 2006, hereafter referred as HA06), the first section of this response is an overview of the objectives of our paper and an explanation of the differences between the two papers. We continue with a more detailed explanation of how our snow/firn densities were derived, including a table with the values used in the paper (which will be included in the final version of the paper). We then provide specific responses to

the individual comments made by each reviewer.

Data used and differences between our paper and HA06

Our paper aims specifically to investigate the spatial and temporal trends in snow accumulation across a 200 km transect of the dry zone of the GrIS. We find a clear gradient in the thickness of the annual snow layers observed which is not apparent in the shorter ∼27 km profile presented in HA06. We then use snow density data in conjunction with thickness of the layers to derive accumulation rate estimates across a large part of the dry snow zone of the GrIS where observations have been very limited. We are therefore using a similar methodology (with differences – see below) to HA06 but applying it to a much larger area with the specific intention of deriving spatial and temporal trends in annual accumulation. The comparison with model results show that by means of radar altimetry we are able to quantify spatial and temporal trends over a large region where limited *in-situ* observations exist and that a model calibrated with historical records does not represent accurately. To our knowledge, there are no measurements of snow accumulation made for this period over these spatial scales in the dry snow region of the GrIS. While previous work with the ASIRAS radar system, such as HA06, has focused on testing the accuracy of the system by detailed comparisons with *in-situ* field measurements, our objective is to prove the capacity that new radar altimeters have to accurately estimate accumulation rates over very large areas with unprecedented resolution by making simple but reasonable assumptions of snow density. The results shown, in conjunction with new accumulation estimates from future ASIRAS campaigns and more extensive CryoSat-2 data, are essential for investigating trends in mass balance in the dry snow zone of the GrIS and for improving the data that is incorporated into models predicting the future evolution of the ice sheet.

Although the data shown here and in HA06 were collected during the same campaign (CryoVex 2004), they represent two separate surveys made on different dates and across different elevation ranges. The profile shown in HA06 was taken on the 14th of September, 2004. It is a ∼27 km survey ranging in elevation from 2650m to 2750m made around the field site area on the EGIG line known as T21 (70.54^{o}N, 43.03^{o}W). Spatial variability is presented and briefly discussed, but there is no significant trend in accumulation along the profile. The survey presented in our paper was taken on the 17th of September, 2004, and it extends for more than 200 km to the ice divide, ranging in elevation from 2750m to 3150m, thereby covering along the transect most of the elevation range of the dry zone of the western slope in this section of the GrIS.

While both papers use similar methodologies, there are several differences and additions that justify the extended analysis presented. Without knowing the precise details of the data processing used in HA06, the following comments indicate the key differences regarding the methodology we used.

- Equation (1) in HA06 sets the system operational parameters (bandwidth, sampling frequency and pulse length) according to a desired two-way travel time. It should be used only for system design purposes, and therefore we do not use it for data processing in our paper. The two-way travel time between samples depends on the sampling frequency of the radar (which in turn also restricts bandwidth and frequency step used by the system).
- To obtain electrical permittivity from snow density, we use a model based on empirical observations presented in *Hallikainen et al.* (1986), while HA06 use a model based on *Looyenga* (1965). We did not verify the accuracy of *Looyenga* (1965). *Hallikainen et al.* (1986) is based on experiments and observations carried out in the laboratory, yielding very accurate values of electrical permittivity of snow for microwave signals.
- HA06 estimate accumulation down to 1995 representing eight years of accumulation (their Fig 4). However, as stated in HA06, they only have continuous layers along their transect down to the 5th layer. To obtain accumulation below this 5th layer, they take a shorter section of the transect to obtain accumulation. In our

paper, we can see deeper layers than we present but we only include accumulation data for those annual layers which are continuous throughout the whole of the transect. At our highest elevations, the deepest layer that can be discerned represents accumulation for the period 1992-93 (Figure 2) but these can only be seen above ~2900m, where snow density is sufficiently low for the radar signal to penetrate. With decreasing elevation, the signals seen below the 1998 layer become increasingly weak, making it harder to identify their depth with the desired accuracy hence we do not incorporate them in the paper.

Derivation of our snow densities

Referees 1 and 2 asked for clarification of the methodology used to infer snow densities along our 200 km transect. We have therefore added a more detailed explanation in the "Data and Methodology" section concerning the derivation and use of snow density estimates. The following text was added to the paper (starting line 13, page 772):

"Two density profiles obtained from shallow firn cores and neutron probe measurements at the lowermost and the highest points of the survey are used. Our lower density profile from T21 (2650m) was obtained from neutron probe measurements made in May 2004 (*Hawley et al*, 2006). Our upper density profile, from Summit (3200m), was obtained by averaging the density estimates derived from both a shallow firn core and from neutron probe observations made in June 2004 (*Hawley et al.*, 2008). For each layer (year), at both the upper and lower sites, a mean annual snow density was estimated. The annual layer density values at each of our nine sites was then determined by linear interpolation between the upper and lower observed values according to elevation. We use the estimated densities at each point along the transect to derive the refractive index (from equation 1) and thus accumulation. (Table 1)."

One of the objectives of the paper is to obtain accumulation estimates in regions with very limited snow/firn density data by using simple assumptions about the variability of the density. Snow/firn densities change very slowly with elevation in the dry snow zone because conditions remain similar along the transect surveyed and there is no melting and refreezing, hence it is reasonable to assume a linear decrease in average annual snow density with altitude. A table with both the observed and estimated density values along our transect is now included in the paper for clarification (shown below).

[Table 1]

Note: In the originally submitted version, we assumed that the density at our highest transect position (3150m) was the same as at Summit (3200). We have rectified this to improve the accuracy of our density estimates at each site, as explained above and in the new submitted manuscript. The resulting new estimates of annual accumulation do not differ significantly from the previous values and do not modify our conclusions. The comparison with historical records now suggests no significant change in accumulation below 3000 m and an increase in accumulation of ∼15-20% above 3000 m.

Response to Anonymous Referee #1 individual remarks:

Comment: pg770, line 9: The authors forgot to mention the impact of snow drift in the factors that cause elevation changes.

Response: The text now reads as follows:

". . . and elevation changes may be caused by factors other than accumulation and melt such as snow compaction and densification processes and by the redistribution of drifting snow (*Parry et al.*, 2007)."

Comment: pg 770, line 20: What is the snow density used here? Is it a constant value over1998-2003? A table showing measurements made at Summit and at 2650m is required here.

Response: As described above, we now include in our methods section a more detailed explanation of the derivation of our snow densities including a new Table (see previous section).

Comment: pg 775, line 6: A comparison year by year with real measurements (in a table) should be more interesting here to see if the interannual variability from the ASIRAS-based accumulation is reliable.

Response: To our knowledge, there are no published measurements of accumulation for the years 1998-2003 across our transect. The method we propose for determining accumulation is relevant specifically because of the lack of observations across the bulk of the dry zone of the GrIS. We do however now include a more detailed comparison with previous accumulation estimates by Anklin and Stauffer (1994) and present these in the text and Table 3.

Comment: pg 775, line 9: The reference to Table 2 is missing.

Response: Reference to Table 2 is now included (which has now become Table 3).

Comment: pg 775, line 10. The comparison with measurements at 2750 shows that the methodology used here overestimates the accumulation rates of 4 cm. This explains likely why their estimates are 5 cm higher than the previously recorded observations. The main uncertainty in this methodology is the snow density. What should be the snow density value needed at 2950 m for having a full agreement with measurements? Are the snow density measurements reliable?

Response: It is not clear what the overestimate in "the accumulation rate of 4 cm" is actually referring to here. At 2750 m, we estimate an average accumulation rate of 0.435 m.w.e. for 1998-2003, compared to 0.439 m.w.e. (1988-1989, *Fischer et al.*, 1995) and 0.437 m.w.e. (1977-89, *Anklin and Stauffer.*, 1994) from historical records, 0.486 m.w.e. from the model, and 0.47 m.w.e. reported in HA06. We expect to obtain lower accumulation estimates than in HA06 since their estimates were made at a lower elevation (2650 m). There is an uncertainty of 3 cm in estimating the location of each layer from the radar data and snow density variability within each layer may affect the estimates as is reflected in the error bars in Figure 3.

Comment: pg 775, line 17. What is the value of the constant snow density used here?

Response: When estimating the error in accumulation, we use a constant density for each annual layer derived from the densities at T21 and which are now included in a table. The text has been amended as follows:

"To emphasize that the assumption of a linear decreasing density from 2650m to Summit does not introduce large errors, we made a second estimate of the accumulation rates using a constant density for each annual layer derived from the observations at T21 (Table 1). At our highest point (3150 m), where the snow density is the lowest and thus the error would be greatest, the estimate for mean annual accumulation is 0.0298 m.w.e. $\pm$ 0.014 higher than observed, equivalent to just a $\sim$10% increase."

Comment: pg 776, line 23. A reference is needed here.

Response: The comparison with historical records refers to the accumulation rates shown in the references Fischer et al. (1995) and Anklin and Stauffer (1994). Since Fischer et al. (2005) is a subset of Anklin and Stauffer (1994), we now compare only with the later, and an extra column in the table is added to indicate the increase. The sentence now reads:

"Comparison with historical records (*Anklin and Stauffer*, 1994) suggests that the accumulation patterns have increased by 15-20% at sites above 3000 m over the last 20-25 years."

Comment: pg 777, line 3. The absolute value of the simulated SMB of the dry snow zone should be given here and not only the error.

Response: The value provided is the difference between the modelled and the observed values extrapolated to the total area of the dry snow region, not actually an estimate of SMB. We however included estimates for mass gain due to accumulation assuming the same accumulation pattern across the dry snow zone of the GrIS, and consider a dry snow zone that covers 55% of the GrIS.

The text now reads:

"Assuming similar accumulation patterns, the difference would mean an uncertainty in the annual mass gain due to accumulation of approximately 44.5 km^3 (275 km^3 for ASIRAS and 320 km^{3}for the model) if applied to a dry snow area of 7.7 $\times$ 10^5 km^2 (approximately 55% of the GrIS), significant if compared to the 1979-2006 estimated total surface mass balance for the whole GrIS ranging from 170 to 308 km^3 yr^{-1} (*Fettweis*, 2007)."

Response to Anonymous Referee #2 individual remarks:

Comment: First, the paper seems to be a remake of the Hawley et al. (2006); it is the same data and the same objectives. The authors should better explain the differences between both studies and pointed out their own results. For instance, why the average accumulation is so different? What is the difference between both methodologies? I think paper cannot be accepted without such a clear discussion.

Response: A detailed explanation of the differences in the aims of the two papers and between the data sets and methodologies used can be found in the first section of our response – see above. The average accumulation estimates made by HA06 and ourselves appear to be different, but only when comparing the average estimates for the *whole* transect made by HA06 (0.47 ma^{-1}) with ours (0.37 ma^{-1}). The estimates represent averages across different elevation ranges, and the data set we present extends to the ice divide, where accumulation is significantly lower. The average estimate at the lowermost point of our survey (2750m) is 0.44 m a^{-1} compared with 0.47 m a^{-1} estimated by HA06 at 2650m and as noted above, we would expect accumulation to be lower at the higher elevations in our transect.

Comment: Second, the derivation of annual accumulation rates from the observations should be better explained. Indeed, the speed of light in snow only depends of the permittivity or on the density and we understand well how derive the layers thickness. However the derivation of annual accumulation rates from the internal layers thickness needs several external observations such as snow density profile. The paper should have a discussion of the used snow profile and on the used densification law. Snow density is highly variable in time and space, so that few in situ measurements are not enough without justification. The authors only refer to papers of Hawley et al. (2006 and 2008), they must give more details about this.

Response: We have included a table with density estimates and explained more fully how we interpolated the density profiles between the two *in-situ* density measurements made at either ends of the transect. One of the objectives of the paper is to obtain accumulation estimates using limited snow density information, not least because this situation is commonplace across large areas of the ice sheets. Since climatic conditions are similar across the area covering our survey, we believe it is a reasonable assumption that density will steadily decrease with altitude and that the method of interpolation is justified.

Comment: Cryosat-2 is launched since few months and few profiles above Greenland are processed (some was shown during the Cryosat-2 session during the Living planet symposium of ESA at Bergen at the end of June). The authors can show such a profile and discuss about the capacity of Cryosat-2 to retrieve snow accumulation.

Response: A recent CryoSat-2 power profile over the dry zone of Greenland is shown below (Figure 1). In the waveform, the surface signal is clearly seen at range bin 53, and 2 distinct signals related to accumulation periods can be identified at range bins 59 and 64.. Weaker signals are also observed up to a depth of around 10 meters below the surface. The CryoSat-2 profile is therefore encouraging regarding the capacity of the satellite to retrieve snow accumulation but we are not yet at a stage where we are ready to analyse this signal in full.

[Figure 1]

Comment: Pg. 774, line 2. Penetration depth in Ku-band depends of a lot of parameters, snow temperature, dielectric loss due to scattering (then ice grain size) or absorp-

tion, dielectric loss due to internal stratification, roughness internal layers... not only of snow density. Additional layers observed with increasing elevation could probably be explained by decreasing temperature and loss.

Response: We agree that there are several variables which control the radar penetration depth in the microwave region and changes in a number of these may aid enhanced radar penetration at higher elevations. However, other work on the EGIG line emphasises the importance of decreasing density in aiding radar penetration, which is why we specifically refer to it here (with an appropriate reference: *Scott et al.* (2006)).

Response to Anonymous Referee #3 individual remarks:

Comment: Were other transects measured during the campaign? And if yes, why they were discarded from this analysis?

Response: Several transects were made over different regions of Greenland by ASIRAS for the 2004 CryoVex campaign. A long transect taken in spring, flown over the percolation zone, will be presented in another paper, primarily because the seasonal melt-signal ensures that a different interpretation of the data is required and the paper has different aims. A short transect flown over the lower section of the dry snow zone and presented in HA06 is the only other ASIRAS survey from this zone during the Autumn 2004 CryoVex campaign..

Comment: The authors mention Cryosat-2 as a possible space-borne equivalent instrument of the one used in the present study, and evasively mention a possible issue with the vertical resolution. I think a clearer conclusion should be given concerning the ability of Cryosat-2 to measure annual accumulation on the ice-sheets.

Response: Due to CryoSat-2's larger vertical resolution, it may be difficult to identify annual accumulation layers in areas with very low accumulation rates, e.g. in certain regions in Antarctica. However, in areas with accumulation greater than several tens of cm such as in the dry snow zone of the GrIS, CryoSat-2 should be able to resolve these annual layers. We have modified our text and now put it in the conclusion of the paper to clarify the above.

Comment: Pg 771 line 5. Add value of vertical resolution and of the footprint size.

Response: The sentence now reads:

"Due to CryoSat-2's larger vertical resolution compared with ASIRAS (47 cm in free space (*Wingham et al.*, 2006)); approximately 32 cm in snow with a density of ~0.4 g cm^3 (Scott et al, 2006)), it may be difficult to identify annual accumulation layers in areas with very low accumulation rates, e.g. in certain regions of Antarctica. However, in dry snow zone areas with accumulation greater than several tens of cm such as in the GrIS, CryoSat-2 should be able to resolve these annual layers."

Comment: pg 771 line 22. Replace "as are" with "are".

Response: We have kept "as are" to retain our intended meaning but added commas to improve the sentence structure which now reads:

"Thus stratigraphically distinct layers in the snowpack, as are commonly formed early in the fall in the dry snow region of the GrIS (Autumn hoar), make it possible to identify widespread internal layers reflecting annual accumulation (Hawley et al., 2006)."

Comment: pg 772 lines 5-l13. The use of the term permittivity is incorrect. Use "refraction index" instead.

Response: The correction has been made with the caveat that we use the term *refractive index*.

Comment: pg 772 line 15. Give quantitative details about the snow density profiles (mean value, std, . . .) and why not a plot of these profiles.

Response: We now discuss in more detail the derivation of the snow density profiles (see above) and a table has been included showing the density values used.

Comment: pg 774 line 7-l8. I don't understand these two sentences.

Response: The text now reads:

"Un-compacted snow close to the surface may present high spatial density variability introducing further uncertainties in our estimates. For this reason, only accumulation estimates for layers below the 2004 layer are made."

Comment: pg 775 line 25. Replace "clbrated" with "calibrated".

Response: The correction has been made.

Comment: pg 776 lines l6-l7: I don't understand what should be concluded from this statement.

Response: We agree that this statement is not adding any clarity to the discussion and have therefore removed it.

Comment: pg 776 line 23: Replace "30 years" with "20 years".

Response: We have amended our line to say over the last "20-25 years" since we are referring to historical averages spanning 1977-89 and our data span 1998-2003. The text now reads:

"Comparison with historical records (*Anklin and Stauffer*, 1994) suggests that the accumulation patterns have increased by 15-20% at sites above 3000 m over the last 20-25 years."

Comment: Fig 1. Precise what the contour lines represent.

Response: Contour lines represent 250 m elevation. This is now included in Figure 1 and in the legend.

Comment: Fig 2. The intensity of the surface echo should be reduced to highlight the others which are the only ones discussed in the paper. Maybe a log-scale of the intensity.

Response: Figure 2 in the paper illustrates the snowpack structure as seen by ASIRAS along the surveyed transect. The problem with using a logarithmic scale for plotting this figure in order to enhance the signals is that the background noise gets amplified as well (see Figure 2). The power from each waveform is normalized by the system and the power received from each signal cannot be reconstructed properly because we lack knowledge of the actual power being transmitted.

[Figure 2]

Comment: Fig 2. Labeling the different layers with the year they were deposited (on the right or left of the picture) would greatly help the reader to follow the results discussion.

Response: The suggested labels have now been added to Figure 2 and the caption amended.

Comment: Fig 2. I don't understand 10.5 ns in the legend.

Response: The legend has been corrected. The y-axis shows the time delay from the surface, so the estimated surface is located at 0 not at 10 ns.

Comment: Fig. 3. "For scaling purpose". This reason is unclear and is different from the reason given in the text.

Response: We have removed this from the caption as it is not necessary.

Interactive comment on The Cryosphere Discuss., 4, 767, 2010.

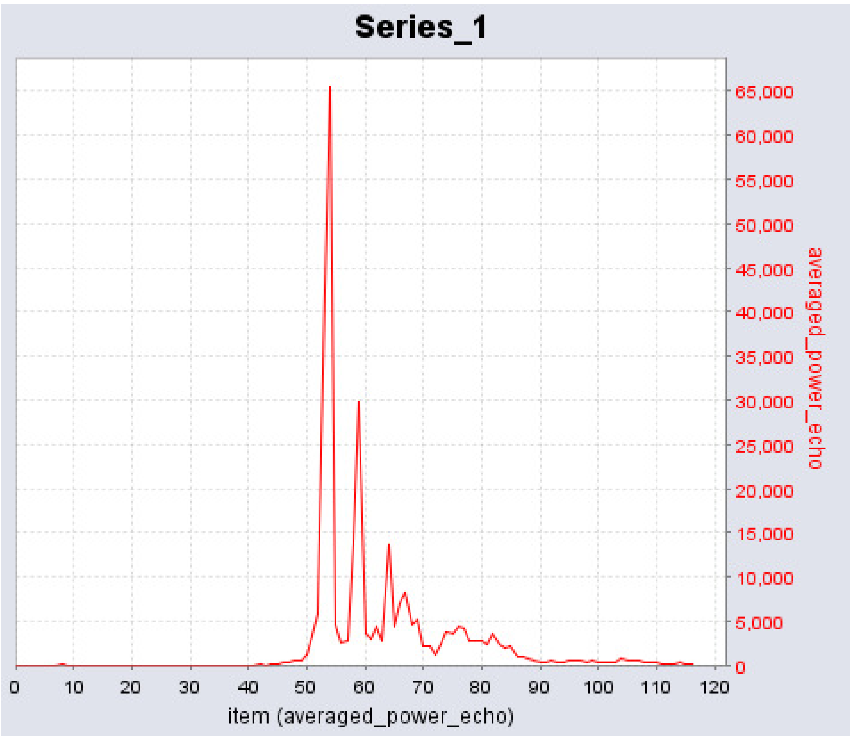

Fig. 1.

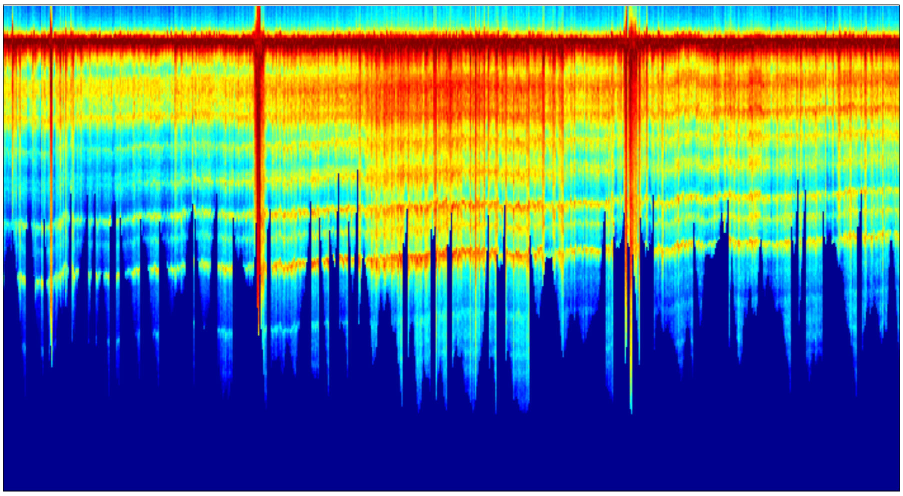

Fig. 2.

Year\Height(m)	2650 T21	2750	2810	2870	2930	2990	3050	3090	3110	3150	3200 Summit
2003	0.410	0.404	0.401	0.397	0.395	0.392	0.389	0.387	0.386	0.382	0.380
2002	0.435	0.427	0.422	0.417	0.412	0.407	0.402	0.398	0.395	0.392	0.390
2001	0.440	0.433	0.430	0.427	0.424	0.421	0.418	0.416	0.415	0.412	0.410
2000	0.450	0.446	0.443	0.441	0.438	0.435	0.432	0.430	0.429	0.426	0.425
1999	0.465	0.465	0.464	0.459	0.455	0.450	0.445	0.444	0.443	0.441	0.440
1998	0.485	0.477	0.472	0.468	0.464	0.459	0.455	0.452	0.451	0.447	0.445

Fig. 3. Table 1