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The Cryosphere Discussions

Interactive comment on "Snow accumulation and compaction derived from GPR data near Ross Island, Antarctica" *by* N. C. Kruetzmann et al.

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First of all we would like to thank both reviewers for their thorough comments and suggestions which helped improve the manuscript significantly. All of their points have been addressed and most suggested changes included in the revised manuscript. Individual responses to the points raised are given in the following in the order they appear in the reviewers' comments.

In order to avoid confusion between figures in the manuscript and in the present document, all figures in the latter will be referred to as "Response Figure #" (or Res. Fig. #). Italicised text in quotation marks represents added or changed text as it appears in the revised manuscript. The location of these excerpts is given in brackets after the text. A PDF of the full revised manuscript is attached as a supplement.

Responses to Steven Arcone:

Comment 1: Air waveform. In Figure 2 you show an air wave waveform and say that is the waveform transmitted into the snow. In GPR, an antenna on the ground surface is "loaded" by the reaction fields induced near its surface, the result of which is usually an attenuation of higher frequencies and a dominant frequency lower than that specified by the manufacturer. Fortunately, new dry fluffy snow provides almost no loading and so what you see in air is likely what is transmitted. I have seen this many times with the GSSI "400 MHz" antenna unit. In your case you have found a dominant frequency of 620 MHz for a Sensors and Software "500 MHz" unit. You should look within your data for isolated wavelets and compare with your air wavelet. They are probably similar, given your excellent results with deconvolution.

Response:
As stated by the reviewer, the effect of ground loading is usually to reduce the effective centre frequency of the emitted radar pulse. Response Figure 1 shows the spectrum of a distinct reflection found at about 12 m depth at L2 and the inset in the top right corner shows the reflection in the time domain.
The apparent centre frequency of this deep reflection is 650MHz, which is even slightly higher than the centre frequency we measured in air (620MHz), though this increase is of course relatively small ($\approx 5\%$) and within measurement uncertainties. Nevertheless, as far as we are aware there is no mechanism that can increase the centre frequency of a radar pulse relative to its "free space" value. Therefore, we conclude that there is no measurable reduction of the centre frequency due to ground loading, dispersion or absorption in the conditions prevailing during our measurements and the effects can be neglected.

Comment 2: The **Fourier spectrum** shown in Fig 2b needs to have the vertical axis labeled as to whether it is power (intensity) or amplitude. If it is intensity then your assessment of the (half power -3dB) bandwidth is correct.

Response:
The spectrum in Fig. 2b represents amplitude, not power (this is now explicitly stated in the figure caption) and accordingly the bandwidth given was a mistake. The actual bandwidth of the system is only 330 MHz.

Comment 3: The **theoretical resolution criterion** on p. 4 is nice but not practical. It is simpler to measure the time duration of the 3/2 cycle wavelet (about 2.25 ns), translate it into distance within the firn medium (pick a density) and then take $\frac{1}{2}$ to account for round trip propagation. Applying the Kovacs formula to 500 kg/m3 density, the resolution is 24 cm, not the 19 cm the formula gives. Practically, when looking at a GPR profile one can usually see horizons merging and follow the phase fronts to get even better resolution.

Response:
Thank you for this suggestion but we are not sure about the reasoning behind this practical resolution estimate and therefore prefer to stick to the theoretical one. Using the corrected value for the bandwidth, the resolution is 0.32 m. We agree that it is usually possible to get even better resolution than the one calculated in either fashion, which is why we are now using a different method of measuring the relative accuracy of the system in order to give an estimate of the error of the compaction measurements (see response to Eisen's Comment 5).

Comment 4: Fig 2. should show the resulting deconvolved waveform, which should show at least one half cycle removed, and the resulting Hilbert magnitude transformation.

Response:
An example of a reflection and its envelope before (blue) and after (red) deconvolution is shown in Res. Fig. 2. The narrowing (focussing) of the peak is clearly visible in the envelope (dashed), but we think that showing a single trace is quite selective and the improvement is more succinctly presented in the larger scale plots of Fig. 3. Thus, we prefer not to include this diagram for conciseness.

Comment 5: The reflectivity series is also known as the **impulse response**.

Response:
We prefer to use the term reflectivity series as we find it more descriptive of the physical process.

Comment 6: The paper states that **dispersion and absorption** are the causes of variation in waveform. Not always true, especially in dry firn. Trace by trace examination of any dry firn GPR profile will show strong variations, and dispersion and absorption cannot be the cause. I think it is mainly caused by interference but this is not clear because interference cannot shift a frequency spectrum lower, yet we see it all the time in Antarctic GPR profiles, from 3 MHz pulses to 400 MHz pulses. The profiles seen in Arcone (1996) were recorded on the McMurdo Ice Shelf in January, when there was much water, and show much waveform variation. I doubt there was any melt in November, when you recorded. Your success with deterministic deconvolution is statistical; you apparently made a good average choice of waveform.

Response:
Dispersion and absorption are the two main factors that are usually mentioned when arguing why deconvolution does not work very well on GPR data, and interference is of course also important for the success of deconvolution, but only on a trace-by-trace basis. Since interference cannot cause a general change of the radar waveform, we

do not think it is an argument against using a deconvolution approach.
The profiles discussed in Arcone (1996) were not recorded in the dry snow zone. Particularly the presence of water close to the transmitter is very likely to cause variations in the waveform in the radargrams. In fact, we observed similar effects in our data from L1 which is why we excluded this data from the analysis given here. From its geographical location we initially expected that there would be occasional melting at L2 as well, but since we did not observe any ice layers in a 3.3 m snow pit in 2008, we concluded that L2 is in the dry snow zone. At this site, and also at L3, we do not observe a shift to lower frequencies in the spectra of deeper reflections (see also response to Comment 1).
We think that the stability of the waveform as it travels through the snow is probably a product of a consistent system output, stable environmental factors (e.g., temperature), and relatively uniform snow properties in the horizontal plane. As the reviewer also states in Comment 1, this is probably an important reason why deconvolution works for our data.

Comment 7: There is much discussion regarding **causes of density variations**. A primary cause is hoar layers; read Alley's (1988) classic paper on firn stratification or your cited reference Arcone et al, 2004, and follow-on papers in Annals of Glaciology and Journal of Glaciology. Many of the wavelets you see may be thin layer responses to low density hoar. This is especially true on the West Antarctic plateau. Dust is likely not concentrated enough to make reflections, but it may have caused melt and subsequent freezing, or even metamorphosis. On the McMurdo Ice Shelf there are also melting and ice layers (see Arcone, Geophysics, 1996), so hoar is just one factor in this complex setting.

Response:
We have included hoar layers as a potential cause, but they are not discussed in much detail because, to our own surprise, we did not observe many significant hoar layers in

the snow pits and firn cores at L2 and L3. One of the few examples is the low-density layer found in the firn core at L2 in 2009 (see also Eisen's Minor Comment 12.10-11), though in the deeper parts of the firn cores some may have been overlooked:
"*Reflections seen in radar recordings can sometimes be associated with distinct accumulation or melt events, or depth hoar layers (Eisen et al., 2004; Arcone et al., 2005; Helm et al., 2007; Dunse et al., 2008).*" (Section 1, paragraph 4)
"*Alley (1988) and Arcone et al. (2004) suggest that a combination of thin layers and depth hoar are likely sources for GPR reflections in dry snow. While we did not observe many distinct hoar layers in the snow pit and core data, some may have been overlooked due to the coarseness of the recorded density profile.*" (Section 5, paragraph 4)

Comment 8: There is also discussion on **causes of horizons dipping**, as in Fig. 8. Generally the topographic effect is seen at a 1–10 km scale, with profiles oriented close to that of the katabatic wind (see Arcone et al, 2005, JG, Fig. 5) This is antidunal accumulation, whereby more snow accumulates on windward slopes than on leeward ones. In the dunefields of East Antarctica the differential accumulation is extreme. On the McMurdo ice shelf there is no significant undulating topography. Instead, there is compression, which becomes clear in long profiles of many km length, especially toward Williams Field and beyond, toward the Ross Ice Shelf. There is also shorter range compression, especially against Ross Island north of Scott Base, best exemplified by the buckling "rollers." See the Nobes et al reference below.

Response:
The topographic effect we observed at L3 was indeed on a smaller scale. The reference has been removed and the paragraph rewritten to explain the observed topography and how we believe this relates to the dipping of the horizons in the radargrams:
"*The elevation difference between the lowest (I1) and the highest point (A9) is almost*

20 m, with A9 located on a local crest and the terrain sloping down towards the north-west. The dip in the observed reflection horizons is on the leeward side of this crest where more drift snow accumulates." (Section 4.2, paragraph 4)

Comment 9: The highlights of the paper are **Figs. 10, 11**. I suggest adding a theoretical curve and some data of others (Arthern?) for comparison. In the Discussion, you try to reconcile your results with those of others. I think that that your results show the limitation of previous measurements.

Response:
We added a theoretical curve to Figs. 11a and 11b (shown here as Res. Fig. 3), based on Sorge's law and the average accumulation observed for each of the stake farms. Arthern et al. (2010) focused on temporal rather than vertical resolution with three vertical intervals: 0-5 m, 0-10 m, and 0-20 m. Subtracting the total compaction over one year in the first interval from that in the second interval leads to only one data point for each of their sites which can be compared to our diagrams. These are the numbers already mentioned in the manuscript and including them in the diagrams wouldn't add any information.
"*Assuming constant density profiles with time (Sorge's law) and using the measured average accumulation from the stake farms to determine the initial offsets, we can calculate expected compaction curves. The results are the dashed blue lines in Figure 11a and b. In both cases the general trend matches that of the compaction measurements, but above about 4 m measured compaction rates are higher than those predicted by the model, and lower at greater depths.*" (Section 4.3, last paragraph)
As the full caption for Res. Fig. 3 is too long for the online response form, it is reproduced here:
"*(Figure 11 in the revised manuscript): Snow compaction vs. depth for (a) L2 and (b) L3. (a) shows the compaction calculated from six lines at L2, while (b) includes all lines from L3 except one. The vertical axis represents depth before compaction has*

occurred. The horizontal error bars illustrate only the spatial variability along each of the lines, not measurement error. The vertical error bars show the thickness of the layers before compaction. The thick black line represents the average compaction for each horizon pair and the dashed blue line represents the expected compaction when assuming a constant density profile with time (Sorge's law)."

Non-technical Comment 1: Writing style. This paper is verbose. Constructions like, "is probably an indicator of," is better phrased as, "probably indicates." Paragraphs use "However" too often, and first person constructions need to be used more. Eliminate phrases such as, "note that," because providing the thought that follows implies that it is to be noted. And please don't start a sentence by saying, "Incidentally,' which suggests that what follows is not very important, or happened by chance. See my MMS.

Response:
Thank you for the detailed comments in the marked manuscript. Most of the suggestions have been included and the indicated phrases have been removed.

Non-technical Comment 2: Organization. The paper is well organized but the Discussion is too long. Some paragraphs in the Discussion should be in the Conclusions, and some can be eliminated. One section of paragraphs should be in an Appendix. See my MMS

Response:
Some parts of the discussion have been shortened, but since the second reviewer suggested the addition of some material rather than a shortening of the manuscript, the overall length has not changed substantially. The suggestion of moving a few paragraphs into an appendix seems to be a stylistic preference and we believe that this part of the discussion is important and should remain in the main body of the manuscript. The conclusion has also been rearranged and partially rewritten as a

result of the suggestions from both reviewers (see PDF-supplement).

Responses to Olaf Eisen:

Comment 1: To make the outlined procedure even more applicable to other studies in the future, I suggest to extend the presentation of deconvolution results in parts. Fig. 3 diplays the results of processing on one radargram in variable density mode. This would, however, be of more value if a subset of traces (10 or so) would be shown in variable wiggle display as well, for instance zoomed in in time on one or several reflectors. The reader could therefore more easily see how the deconvolution reduces the influence of the source signature in detail.

Response:
We find that the wiggle display of a few traces is very selective and does not add any extra information (see also response to Arcone's Comment 4). The utility of the deconvolution is most apparent in the large scale view when comparing Fig. 3b and 3c since both of them had the same processing applied with the exception of the deconvolution step in the case of Fig. 3b. The removal of the source signature on the other hand is most directly observable in the whitened spectrum which has now been included (see Comment 3).

Comment 2: I suggest to extend the display of the autocorrelation function to something like 10 ns. The 2 ns shown here seem somewhat short to evaluate the value of the deconvolution, as it merely comprises the duration of one cycle of 500 MHz, but a radar source signal is usually more than 2 cycles long, as seen in Fig. 2.

Response:
For the manuscript, we have extended the presentation of the autocorrelation to 5 ns, since this is approximately equal to the duration of the radar signal. At later

times the autocorrelation of the trace is relatively low. Minor peaks (before and after deconvolution) can be related to either deeper reflections or coherent noise in the system (ringing) and are therefore to be expected. Response Figure 4 shows the autocorrelation extended to 10 ns as suggested. The dashed and solid lines correspond to the trace before and after deconvolution, respectively. We do not think the last 5 ns add any relevant information.

Comment 3: In addition to compressing the autocorrelation function, successful deconvolution should whiten the spectrum. It would therefore be interesting and valuable to show the averaged spectrum, e.g. of the radargrams in Fig. 3. (Yilmaz has many examples of how this could be done.) It is not that I doubt the results presented. But as the authors stress that they present a new method, it would be nice to have the deconvolution part presented even more comprehensive for future references.

Response:
The spectra of the individual traces do show noticeable whitening and the spectra of a trace before and after deconvolution have now been included in the manuscript as Fig. 4b and c and are reproduced here as Res. Fig. 5.
"*As expected, the spectrum of the same trace (Fig. 4b) is also clearly whitened after decovolution (Fig. 4c). The ACFs and the spectra in Fig. 4 were computed from a truncated version of the trace that does not include the first 8 ns, for reasons mentioned above.*" (Section 3, third paragraph after the list of processing steps).

Comment 4: There seems to be an unresolved issue regarding the influence of the deconvolution on the depth of the actual impulse response for a mixed-phase source signal. This is important if it comes to linking core and pit data to radargrams. Depending on the way the convolution is implemented, the actual deconvolved output signal might be offset by some ns. Fig. 3 is too blurred to investigate this. Maybe the above suggestion to show wiggles already clarifies this point. Nevertheless, some additional comments in the text would be helpful.

Response:
The shift occurs because the emitted waveform is not minimum-phase. In our case the shift is equal to 1.8 ns or 18 data points. As one would expect in such a case, this time shift is equal to the time from the start of the emitted waveform used for deconvolution to its peak amplitude. The shift had been taken into account in all post-deconvolution diagrams, but hadn't been mentioned in the text in error and we have corrected this: "*...as the emitted waveform is not minimum-phase, the deconvolved data is offset by -1.8 ns (upwards). This corresponds to the time from the start of the waveform used for deconvolution to its peak amplitude (Yilmaz, 1987). Accordingly, the timezero for all data has to be adjusted by this amount after step (3).*" (Section 3, first paragraph after the list of processing steps)

Comment 5: Figure 10/11 and related discussion in the text:
At first sight, the negative value of compaction below 9.5 m has an "error bar" which is considerable smaller than the deviation from 0. One would assume that 1σ error bars are shown, but even then it seems that the provided errors (or uncertainties) miss some important component to be true estimates of accuracy. Actually, I have a problem with the authors' usage of the term "error". As stated on p.16.26 "The horizontal error bars are the standard errors of the respective reflections' depths". So the error bar is not an error bar but a measure of the spatial standard deviation of the horizons depths! The caption of Fig. 10 is silent on this and actually misleading, as it suggests that the shown error bars are the sum of the horizons depth "uncertainties", and not spatial variation. Overall, this seems to be misleading the readers. I therefore suggest to include a more thorough discussion of errors, separately from those of spatial variation, and including true error (or uncertainty) estimates in figures like Figures 10 and 11.

Response:
The term 'standard error' is used because we take the compaction values to be aver-

age estimates for the whole line and therefore divide the "spatial standard deviation" by the square root of the number of data points (approx. 1600 for each line). Using data acquired in a snow pit with metal stakes we can estimate the relative accuracy of the system to be about 11%.
"*...the relative resolution of the system was found to be considerably better. We tested the relative accuracy of the system by recording a GPR profile of a snow pit which had metal stakes inserted into one wall at 0.5 m intervals (not shown). From the apparent separations of the reflection hyperbolas we found that the average error of relative measurements within the snow is 11%, as long as the distance between the reflectors is greater than the theoretical resolution.*" (Section 2, paragraph 1)
This has now been included in the calculation of the error bars for Fig. 10 in the revised manuscript (shown here as Res. Fig. 6), giving a more representative picture of the uncertainties involved.
The full caption of Res. Fig. 6 is:
"*(Figure 10 in the revised manuscript): Snow compaction along the line from stake E1 to E9 at L2, calculated from the change in the average reflection horizon separations between Fig. 6c and d. Horizontal error bars are a combination of the relative accuracy of the radar system and spatial variability of the horizons along the line.*"
The error bars in Fig. 11 have not been changed as they are meant to illustrate the difference in spatial variability at the two sites, but the figure caption has been altered to clarify this point (see also response to Arcone's Comment 9).

Comment 6: Figures 1 (b, c), 7 and 9 need an indication of geographical direction, e.g. a north arrow.

Response:
Figures 1 (b), 7 and 9 now have north arrows. Figure 1(c) does not, since the site at L3 had a different orientation from the other two and the orientation of the sites can now be deduced from the arrows in Figure 7.

Comment 7: Figure 7: As the dust layer is undulating more than the other horizons, i.e. produces a rougher surface, wouldn't it be good to show the accumulation from another layer as well, or are they not too different if gridded to this scale?

Response:
While the results are not too different when using a different horizon, the dust layer horizon is the only one we can date. The dates of any of the other layers can only be guessed, and this would need to be based on their relative location with respect to the dust layer.

Comment 8: I do not like the term "carrier". This would be appropriate for something like FMCW radars, where a carrier signal is modulated, but not for pulsed radar systems. The antenna makes the signal from the pulse, there is no carrier. I suggest to rather talk about the source or transmitter signature.

Response:
We believe that the term carrier does not only apply to FMCW systems and thus continue to use this term.

Comment 6.10-12: For an introductory section on data acquisition this is too much interpretation already and should be moved to the actual interpretation/discussion.

Response:
We see the point, but since the interpretation is only there to explain why the data from L1 will be excluded from the discussion in the following sections, we think the sentence should stay where it is.

Comment 6.16: I can't see why the envelope should remove the source signature.

That is not its purpose. Rather, the envelope aims at showing the instantaneous magnitude over the whole source signature's length and removing the influence of the phase in the signal. It is of good value for stacking neighboring traces that have only partly coherent signals because of low SNR, so that the signals are not destacked. Is this intended here?

Response:
This has been rewritten: "*In many cases the processing of GPR data has been adapted from the processing of seismic recordings. One frequently used procedure is to calculate the envelope of the received signal via the Hilbert Transform (e.g. Taner et al., 1979) thereby removing the phase information. The resultant trace gives a picture of the instantaneous amplitude of the received signal, but is still strongly influenced by the source signature.*" (Section 3, paragraph 1)

Comment 9.20: I can't really see the necessity for the Hilbert transform, if the deconvolution works, as it should results in the impulse response. If the deconvolved signal still shows cycles, the deconvolution was not completely successful. Please rewrite both instances mentioning envelopes.

Response:
Ideally a deconvolution should turn each reflection of the source signature into a single (delta-) peak. Depending on the sign of the dielectric gradient causing the reflection, this peak can be positive or negative. In practice, no deconvolution is perfect and the resulting deconvolved reflection has, for example, a Mexican-hat type of shape of a certain width. We calculate the envelope to ensure that all peaks are positive and as distinct as possible. The sentence which may have suggested that all reflections were turned into single peaks by the deconvolution process alone has been removed.

Comment 16.14-25: This is somewhat unclear. "We calculate the correlation between each horizon..." What do you correlate? This passage requires clarification.

Response:
First we calculate the correlation of the path of a tracked horizon in the first year (in TWT) with the path in the second year. If we accidentally tracked a different reflection horizon in one year this would probably not show the same bumps and hollows at various positions along the profile and the correlation would be low. Secondly, since a large-scale trend, like the dip observed at L3, will dominate this direct method of correlation, we also calculate the separations between layer pairs (in terms of TWT) and correlate these "separation-profiles". This second step also removes disturbances such as the increased surface roughness observed at L2 in 2009 which can change the apparent path of an internal reflector. – The paragraph has been rewritten to clarify this point:
"*. . . we calculate the correlation between the TWT profile of each horizon in 2008 and its counterpart in 2009. Additionally, we calculate the distance between pairs of horizons in terms of TWT for each year and the correlation of these relative TWT profiles. Only those horizons that show a correlation greater than 0.5 in all cases are used for compaction calculations.*" (Section 4.3, paragraph 2)

Comment 21.1: A recent study by Hörhold et al. (JGR-F, 2011, doi:10.1029/2009JF001630) shows that the true picture of densification is much more complex than classically assumed and implied here.
and Comment 21.6: change in compaction mechanism": I doubt that the resolution of your densification profile of 1–2 m wide depth bins is sufficiently high to derive this conclusion! The observed "change in mechanism" could merely be an artifical result of this resolution and might look different, if one would be able to measure strain at e.g. 0.1 m depth resolution. I think it is more likely that the inter-annual variation of properties of a firn layer are sufficient to cause – or at least contribute significantly to – different compaction rates from year to year, and thus at different depth. This is

also a reason why I do not think that there are real discrepancies between your and Arthern et al's study. Compaction as a function of depth considerably depends on local conditions,as does density (e.g. Hörhold et al., Fig.3). So I would actually be surprised if one gets similar compaction rates at the same site over years or even at different sites in different years.

Response (to both comments):
Thank you for pointing out the Hörhold et al. (2011) study. This part has been rewritten:
"*According to the density profiles in Fig. 5, the 550 kg m^{-3} level lies around 6 m depth at both sites, which compares well with the model estimate by van den Broeke (2008), who gives a range of 5 to 8 m depth for the 550 kg m^{-3} level in this area. However, the depth at which we observe a change in compaction rate (4 m) is more shallow than this theoretical threshold density. A recent study by Hörhold et al. (2011) suggests that the 'classic' picture of snow compaction is too simple, but better resolved density measurements would be required to explain the origin of the observed step in compaction rate.*" (Section 5, paragraph 6)

Minor Comment 1: Usually, strain is given in nondimensional units, e.g. 10^{-2}. Here the authors use e.g. cm/m and talk about compaction rates, e.g. 1 cm / 1 m. I wonder if that is a good choice.

Response:
We find cm/m more descriptive and easier to visualise than just 10^{-2}.

Minor Comment 7.19: I doubt that at this site near Ross Island the conductivity is as low as on the plateau, which is suggested by the cited reference. Salt/aerosol fluxes by accumulation are much higher at this site. I don't know the numbers but assume the authors' assumption is right. Nevertheless, citing a study which indicates that local conductivities in this region are low enough to still treat the firn as a low-loss medium seems more appropriate than referring to a plateau study.

Response:
The reference has been changed to Kovacs et al. (1995).

Minor Comment 7.24: "... snow layers WITH DIFFERENT ϵ_r' can ..."

Response:
This has been rewritten accordingly.

Minor Comment 8.1-2: The authors do not mention noise here. I suggest to do so for completeness and dump the term later for the sake of practicality. See Yilmaz for a thorough discussion.

Response:
The noise term has been added.

Minor Comment 9.21: Do you mean horizontal stacking, i.e. along the line? Provide number of traces here. Stacking is usually applied as one of the first steps (after deco but before filtering) to remove non-coherent noise.

Response:
The word 'horizontal' has been added for clarification; the number of stacks is different for each year and is given in the paragraph immediately following the list of processing steps.

Minor Comment 10.19-20: "The tracking ..." awkward sentence, please rewrite.

Response:

This has been rewritten: "*We use the processing detailed above to identify and track internal reflections in the radargrams. The tracking was performed using the KINGDOM Suite 8.2 software.*" (Section 3, second to last paragraph)

Minor Comment 11.5: I can't see the estimate of ϵ_r' in equation (4). Please double-check or clarify. Would be better to use ϵ_r'(z) and v(z) in eq. (5).

Response:
This should have said Eq. (1) and has been corrected. We have also included the depth dependence.

Minor Comment 11.11: reflection-depth -> reflection depth You use kg/m2/a for the accumulation throughout. I do not understand why you convert depth to w.e. Are all the depths referred to later w.e. depths or is the compaction provided in w.e.? If so I don't think that is a good choice. Confusing.

Response:
This sentence was a remnant of an earlier version of the manuscript in which we used water equivalent and has been removed.

Minor Comment 12.10-11: Why should the dust layer be related to the coarse-grained low-density layer? Seems speculative. Clarify.

Response:
"*Approximately at the depth at which we expected to find the dust layer, the core contained an unusually coarse grained low-density layer (starting at 3.42 m). As we found a similar low-density layer right below the dust layer in the previous year, we believe this could be a depth hoar layer that formed either as surface hoar prior to the storm or underneath the wind crust afterwards.*" (Section 4.1, paragraph 2)

Minor Comment 12.20ff: You conclude that the higher value of 437 kg/m2/a is caused by high interannual variability, but you do not mention that a wrong date for the reference layer could be a cause as well. Please clarify.

Response:
This has been rewritten: "*This value is considerably higher than the one measured by the stake farm and could either indicate an error in the dating of the dust layer, or a high inter-annual variability in snow accumulation in this area.*" (Section 4.1, paragraph 3)

Minor Comment 12.26ff: This paragraph until 13.14 does not seem to fit in here. This is rather a presentation of a method/approach than a result.

Response:
We see the point, but if this paragraph was moved to the end of Sect. 3 there would be references to Fig. 5a and b before the figures are actually introduced. Alternately, only the first two sentences of the paragraph could be moved, but this would cause unnecessary fragmentation of the line-of-thought pursued here.

Minor Comment 13.18: Why is it more likely that a (more) undulating horizon is caused by a storm event? Explain or provide reference.

Response:
It is right that we have no evidence and this is speculation. We reworded this sentence. "*The yellow arrow corresponds to the dust layer depth and coincides with a horizon which is more undulating than other horizons in its vicinity. The particularly strong roughness might be related to buried sastrugis caused by the storm event in May 2004 (Steinhoff et al., 2008; Dunbar et al., 2009).*" (Section 4.2, paragraph 1)

Minor Comment 14.9: Measurement error: So, how large is it? See specific suggestions above.

Response:
Since the depth of the dust layer reflection is an absolute measurement, we use the theoretical resolution to estimate this error. At L2 the error "*is estimated by assuming that the actual depth of the tracked layer is 16 cm (half of the theoretical resolution) above or below the measured value, giving an error of* $\pm$*14 kg m*$^{-2}$ *a*$^{-1}$*.*" At L3 "*the measurement error due to the resolution of the system is approximately* $\pm$*13 kg m*$^{-2}$ *a*$^{-1}$*.*" (Section 4.2, paragraph 2 and 3, respectively)

Minor Comment 14.20: Are the $\pm$22 spatial standard deviation or true uncertainty/error? See specific suggestions above.

Response:
This is spatial variability. The changes to the text made due to the previous comment should make this clearer in the context.

Minor Comment 14.24: I think the relation of accumulation and surface slope is much more complicate that implied here. It depends on the region, the kind of accumulation, flow, etc. Eisen et al, Rev. Geoph, 2008, sec. 3.2 list a number of studies on this topic.

Response:
This has been changed (see also response to Arcone's Comment 8).

Minor Comment 15.25: Rewrite to: "... and Fig. 6d shows the same horizons ..." Stick to either Fig. or Figure, but do not mix.

Response:

The full word “Figure” is used because it is at the beginning of the sentence, which is the required style for this journal, see "Abbreviations and Accronyms" on http://www.the-cryosphere.net/submission/manuscript_preparation.html

Minor Comment 16.19: The usage of the double-arrow implies equivalence and is not accurate, as it depends on the wave speed chosen. Rather use something like "corresponds to ..." (same holds at 20.20 later in the text).

Response:
We now simply use the words “*corresponding to*” in both cases to avoid confusion.

Minor Comment 16.23: Rewrite to "... intervening time period, assuming a constant wave speed". Can you quantify possible effects of the different density profiles from the two years, i.e. provide a numerical upper limit for the effect of the (unknown) wave speed contributing to the compaction signal? See also 17.9-10

Response:
When assuming a constant depth-density profile (instead of constant density), the measured TWT difference corresponds to 2.2 cm/m. The increase (+0.6 cm/m) is approximately equal to the amount already mentioned in Section 4.3, paragraph 4.

Minor Comment 17.3-9: This is also a methodological description and should rather be moved to the methodological section.

Response:
While this is a methodological description, it would be quite difficult to understand earlier on in the document, i.e. before the Fig. 5 and 6 and the tracked horizons have been introduced. Therefore we prefer to leave these sentences where they are.

Minor Comment 17.4: "mean depth" over one profile or at the location of a single trace?

Response:
The mean depth for the whole profile is used – the sentence has been rewritten for clarification.
“*The TWT difference between two horizons is converted to a physical separation by calculating the mean depth between the two horizons for the whole profile in 2009,(...)*” (Section 4.3, paragraph 4)

Minor Comment 21.19: Use "deepest" rather that "last"

Response:
This has been changed accordingly.

Minor Comment 23.10: "for the first time": This is not quite true. Compaction is nothing else like vertical strain. To derive strain radar has been applied by Jenkins et al., JGlac., 2006. Moreover, Heilig et al. (2010, doi:10.1002/hyp.7749) used a comparable approach to derive compaction of a seasonal snow layer over the course a weeks. Please check other instances in the paper to make clear what your "new method" refers to.

Response:
We were not aware of the paper by Heilig et al. (2010), thank you. The sentence has been rewritten: “*By comparing vertical separations of internal reflection horizons from one year to the next, we were able to estimate compaction rates from GPR measurements down to 13 m depth.*” (Section 6, paragraph 3)

Minor Comment 25.3, 27.6: antarctic -> Antarctic (bibtex-problem? Check other instances of location names as well)

Response:
These are correct in the submitted document and appear to be changed by the processing done at TC.

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Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/5/C321/2011/tcd-5-C321-2011-supplement.pdf

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Interactive comment on The Cryosphere Discuss., 5, 1, 2011.

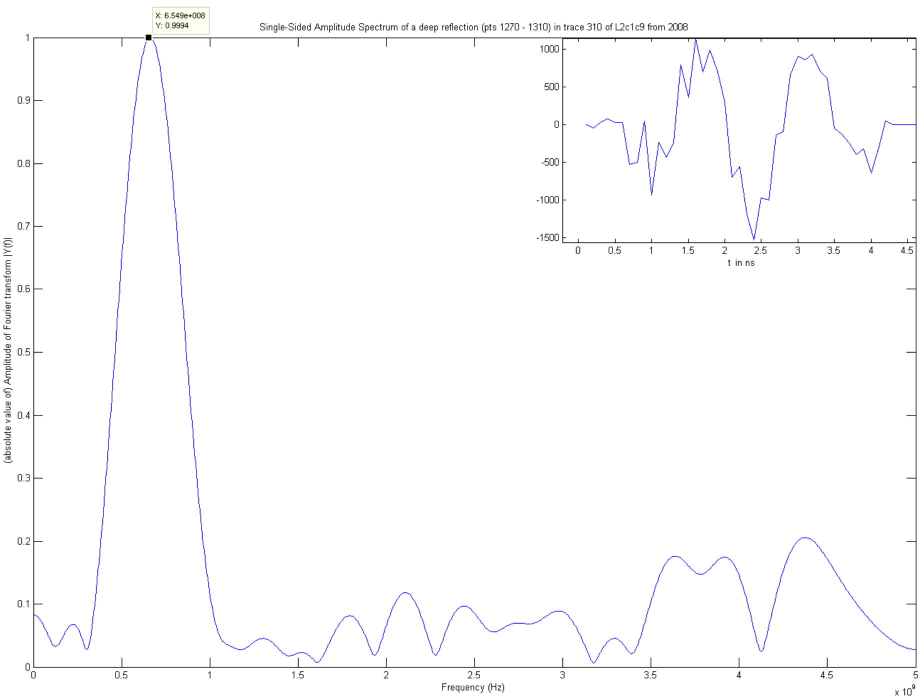

Fig. 1. The spectrum of a distinct reflection found at about 12 m depth at L2. The inset in the top right corner shows the reflection in the time domain.

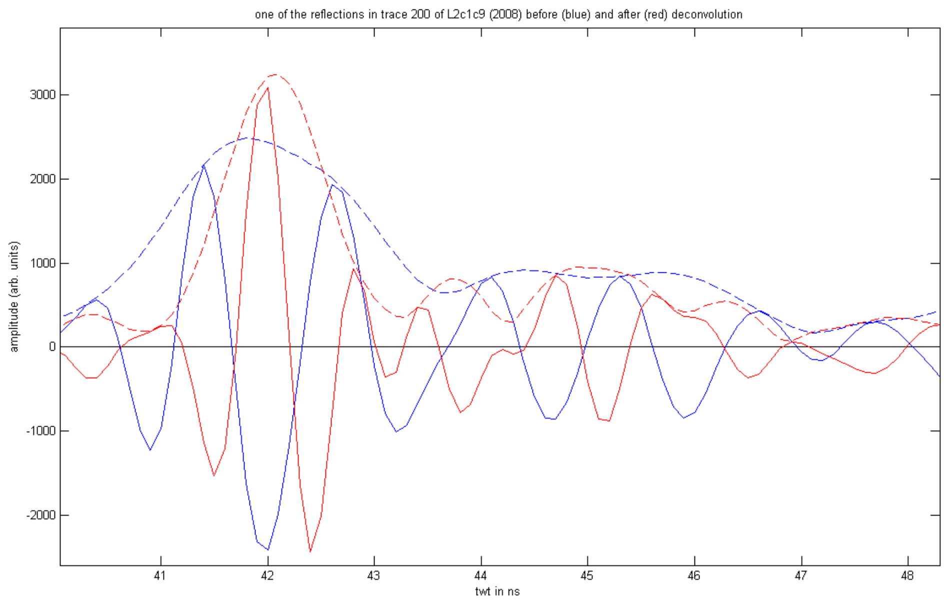

Fig. 2. Example of a reflection and its envelope before (blue) and after (red) deconvolution.

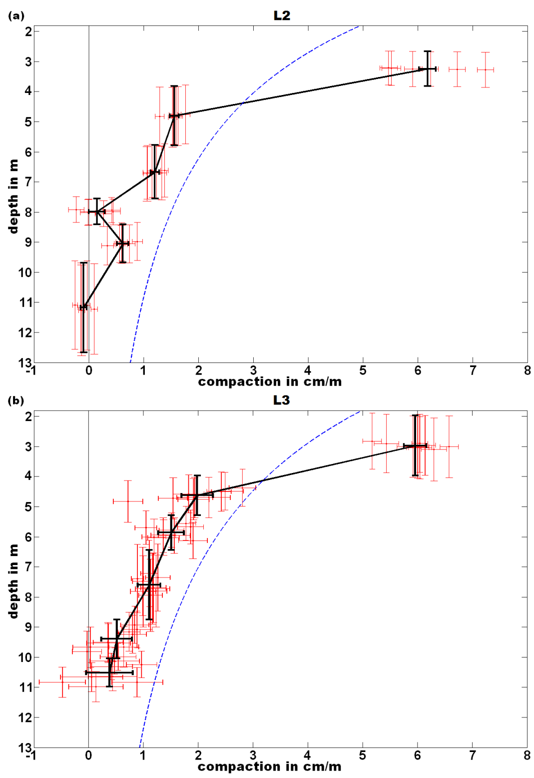

Fig. 3. (Figure 11 in the revised manuscript): Snow compaction vs. depth for (a) L2 and (b) L3 (...) - for full caption see response to Arcone's Comment 9

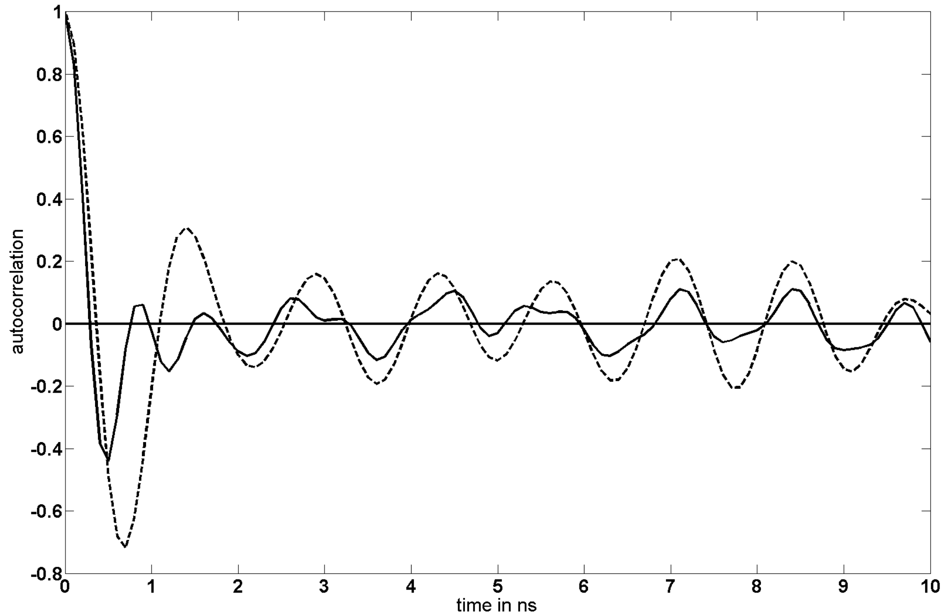

Fig. 4. The autocorrelation of a trace before (dashed) and after (solid) deconvolution. The first half (5 ns) corresponds to Figure 4a in the revised manuscript.

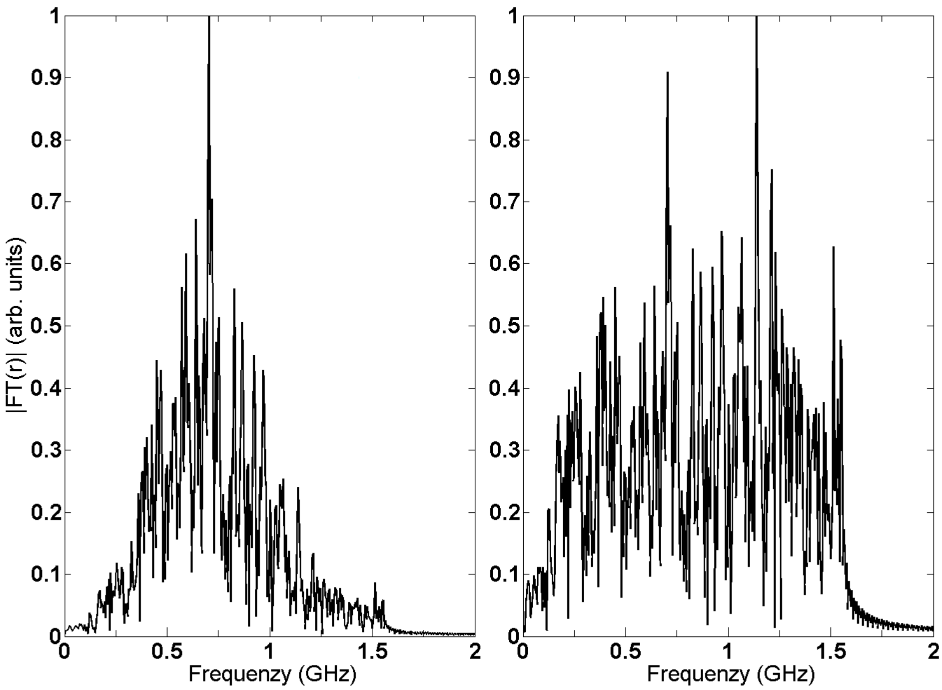

Fig. 5. (Figure 4b and c in the revised manuscript): Amplitude spectrum of a trace from L2 before (left panel) and after (right panel) deconvolution. The spectra are normalised to a maximum value of one.

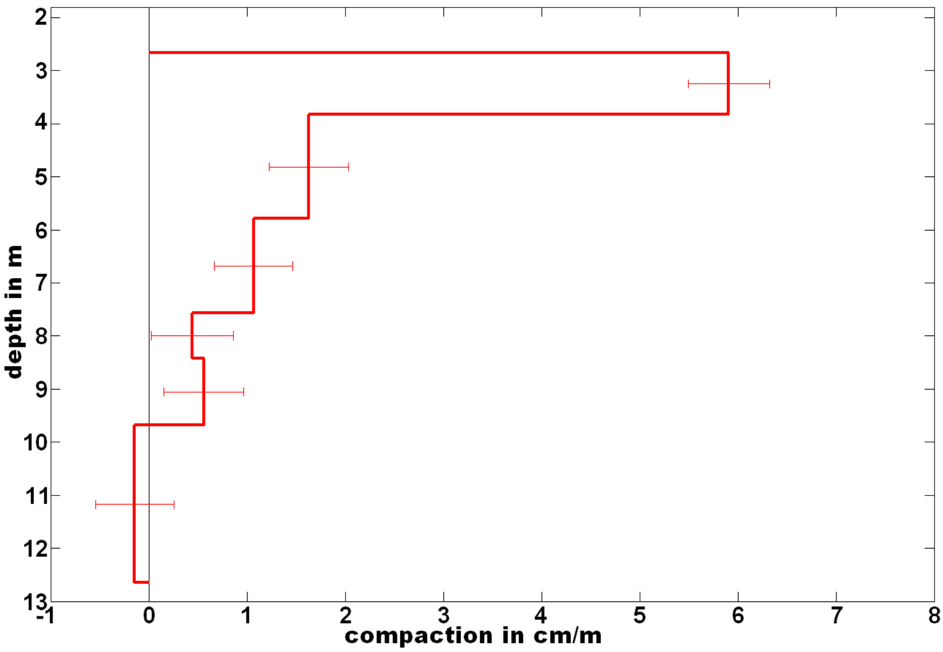

Fig. 6. (Figure 10 in the revised manuscript): Snow compaction along the line from stake E1 to E9 at L2 (...) – for full caption see response to Eisen's Comment 5