The Cryosphere Discuss., 5, C51–C57, 2011
www.the-cryosphere-discuss.net/5/C51/2011/

The Cryosphere Discussions

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

O. Eisen (Referee)

olaf.eisen@awi.de

Received and published: 16 February 2011

General comments

The authors report on application of deconvolution to GPR data to improve imaging of internal reflections in firn. Subsequently, they analyse the layering and its change over a one-year time span to derive values for accumulation and firn compaction at different sites near the Ross Island.

The paper is well written, the results important and the whole theme well presented. Nevertheless, I list below some comments which might still improve the paper and might make it a standard reference for deconvolution applied in radioglaciology.

The deconvolution seems to produce reasonable results, which is not something one

would expect in advance. Several authors used a convolution or related approach to produce synthetic radargrams from known or assumed profiles of physical properties (e.g. Miners, 1998, 2002, Kohler et al., 2003, Eisen et al., 2003, Arcone et al., 2005) with different degrees of success. One reason often considered for only limited reproduction of measured radar signals by forward modelling with convolution is the lateral inhomogeneity of the firn pack. Therefore I find it highly interesting that the deconvolution is apparently working so well, at least to remove a considerable part of the source signature.

Specific suggestions

(page.line)

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.

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.

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.

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.

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.

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

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?

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.

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.

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?
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.

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

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.
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.

Minor comments

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.

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.

7.24 "... snow layers WITH DIFFERENT ε_r' can ..."

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.

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.

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

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

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.

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

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

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.

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

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

14.20 Are the ± 22 spatial standard deviation or true uncertainty/error? See specific suggestions above.

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.

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

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).

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

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

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

21.19 Use "deepest" rather that "last"

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.

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

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