Author comments: "Radio-echo sounding measurements and ice-core synchronization at Dome C, Antarctica"

A. Winter, et al.

5   Review by Tom Jordan, University of Bristol

Overall the scientific analysis is of a high quality and the manuscript is well written. I have, however, made a few suggestions where more detail and precision in the presentation is required; particularly in the methods section. The manuscript is well structured and referenced, with informative figures and tables. Regarding the novelty of the study, I think it needs to be
10   made more explicit as to what differentiates the manuscript from Cavitte et al. 2016 (which also considers IRH detection at Dome C for different radar systems). I appreciate that there are differences, (e.g. the use of the synthetic trace in this study), but this may not be obvious to the general reader. The electromagnetic modelling framework for the synthetic trace appears well established, and physically rigorous. However, given the overall emphasis on comparing how radar system characteristics influence the sounding data, one major area which requires explicit investigation is frequency dependence (see specific
15   comments, Sect. 2.2, Sect. 5.1). Whilst probably not directly impactful for Oldest Ice/synchronization, frequency dependence could be important for combining IRH data sets where reflection amplitudes are important, and thus is required to complete the overall compatibility aspect of this study. The manuscript is clearly of general interest to the readership of The Cryosphere. RES of IRHs can provide useful glaciological information that goes substantially beyond ice core chronologies; for example temperature (from attenuation) and ice dynamics (from IRH derived metrics such as the continuity index). Such a comprehen-
20   sive compatibility study between different radar systems is therefore of suitably high impact, and will no doubt be a central reference point for future studies which combine RES data sets. Whilst I appreciate that the glaciological application here is Oldest Ice, I have made a few specific comments regarding how the authors could broaden the scope of their introduction and discussion.

We thank Tom Jordan for his very detailed and constructive comments and readily follow his suggestions to broaden the in-
25   *troduction and discussion of our study. As suggested, we included further glaciological applications, emphasized the novelty of our study with respect to Cavitte et al.'s study, and included the frequency dependence. A comprehensive investigation of reflection amplitudes with respect to frequency dependence was not feasible, the reasons for which we discuss in our answers on the specific comments. We appreciate that some parts of our method were not described with sufficient precision and we have rewritten these sections substantially. Below we address the specific and minor comments one by one with the repeated*
30   *referee's comment, followed by our answer highlighted in blue italic font.*

Specific comments

1

A clear case needs to be made in the introduction what differentiates the study (both in terms of the questions addressed and the methods that are used) from Cavitte et al. 2016.
35   *We added a section to the introduction that emphasizes the novelty of our study, especially with respect to the Cavitte et al. (2016) study. We see the major differences in: Cavitte et al.'s major point is to trace the radar stratigraphy to extend the ice core chronology through the ice sheet and possibly connect two deep drill sites for timescale synchronization. To achieve this they combine data of the HiCARS system (new surveys focused on Dome C region) with those of the older UT/TUD survey focused on the Vostok region. Additionally they use one line of the CReSIS MCoRDS system as an alternate route for the*
40   *single 200 km long TUD connecting line. This separate radar system also serves as additional test of the accuracy of dating of HiCARS reflections at Dome C. The goal of our study is not to trace layers/extend the age scale as far as possible, but to compare the measurements, spatially confined to a small area, of the RES systems that measured a good part of Antarctic RES surveys for combined usage. We compare measurements of FIVE different systems, all within about 2 km. For assessment of the physical properties recorded by the RES systems and their relation to the radar data, and for depth conversion of IRHs we*
45   *model synthetic radar traces based on the EDC ice core record.*

As mentioned in the general comments, I think the emphasis in the introduction on Oldest Ice is too narrow. This is an excellent opportunity to communicate to the wider glaciological community what rich information is present within RES data (in particular derivable from IRHs), and therefore could be exploited on a broad scale if different data sets can be combined. Two relevant examples are: depth-averaged temperature (from the attenuation rate inferred from internal reflections), Matsuoka et al. 2010, Macgregor et al. 2015b, ice dynamics (from the IRH continuity index) Karlsson et al. 2012.

We included further glaciological applications of RES data, like inference of ice dynamics and attenuation rates/temperature, in the second half of the introduction. As we made substantial changes to this section, they can be seen easiest in the revised manuscript, where changes are highlighted in color. However, we would like to keep our focus on Oldest Ice and the internal reflection horizons to keep the study constrained. For the same reason we put less emphasis on the relative and absolute amplitudes (see also answer to comment 5.1.).

2.1

It would be helpful here to provide more information regarding the DEP method and the gamma absorption method. In particular; how some of the underlying assumptions/restrictions of these methods could impact the rest of the investigation. For example, in the case of the DEP method, I think that it is important to note explicitly that the reference frequency (100 kHz) is significantly less than the radar systems (100 MHz), which may be important regarding frequency dependence of dielectric conductivity/attenuation (see recent discussion in Macgregor et al. 2015b)

We propose to introduce the frequency dependence of RES reflections and conductivity by adding the following to in Sect. 1:

"The IRHs from conductivity changes, in contrast, can be found throughout the ice sheet. The reflection coefficients of those IRHs are related to changes in the imaginary part of the complex dielectric permittivity and are thus proportional to conductivity changes and inversely proportional to frequency. A change of crystal orientation fabric (COF) is the second reason for reflections in the deeper parts of the ice column, predominating in zones of high shear. In RES measurements the conductivity-based IRHs can be distinguished by the frequency dependence of their reflectivity from IRHs caused by COF and density, which have frequency independent reflection coefficients related to changes in the real part of the complex permittivity (Fujita et al., 1999, 2000). Conductivity itself was assumed frequency independent in the range of RES frequencies, but more recent work implies that its frequency dependence cannot be neglected for e.g., attenuation studies (MacGregor et al., 2015b)."

and extend the discussion on synthetic vs. RES data (Sect. 6.2.1) by the frequency dependence of conductivity:

"Furthermore, conductivity itself is frequency dependent (MacGregor et al., 2015b). This implies that also the different measuring frequency of the DEP compared to the source wavelet (100 kHz vs. 150 MHz) effects the reflection amplitudes of the synthetic trace. However, the uncalibrated conductivity profile of the Dome C core and the simple model itself do not allow to quantitatively analyze the reflection amplitudes, but only the signature of reflection patterns in our study."

However, we do not really see a strong point for taking the different frequencies into account, or the temperature for that matter. The bench used at Dome C and the calibration of the conductivity was not as good as for example at EDML, and the real part of the complex dielectric permittivity was not measured. So all that can be used is the conductivity changes. We cannot get the correct absolute conductivity at Dome C and even if we were to take the frequency into account, and the temperature, yet it still is only an uncalibrated relative conductivity. For that reason it is only the changes in conductivity that have some meaning. We do now emphasize in our methods section that we are only using the conductivity changes.
The gamma absorption method is described at length in Eisen et al. (2006) and the papers cited therein. We have now included this reference in the manuscript. However, we do not see any impact of underlying restrictions of this method in connection with radar measurements. More so, as we use the density only for calculating permittivities in the upper 100 m.

2.2

Given the range of radar system frequencies that are considered in this study (60-195 MHz) I think it is necessary to investigate the frequency dependence of the synthetic trace. Note; that this is in the context of thin-film interference (and how frequency/wavelength dependence affects the peaks/relative amplitudes of the synthetic trace) rather than the intrinsic frequency dependence of the dielectric conductivity as mentioned above. Specifically, it would be useful to repeat the synthetic trace analysis, for source wavelets at the CReSIS (195 MHz) and UTIG (60 MHz) centre frequencies. If pronounced sensitivity is demonstrated, then these repeat traces could be used to improve the comparison between the synthetic A-scope trace and the radar A-scope traces in Section 5 and the related discussion in Section 6. I hope, given the excellent EM simulation framework available to the authors, this is request is fairly straightforward to do, and would add a significant value to their investigation.

The issue with CReSIS and UTIG systems is that they are focusing radars and use extended chirps as source signals, not short pulses. The systems use different processing sequences to get to the A-scope presentation. Implementing that in the forward model is not straight forward and time consuming. We have run the model with short pulse wavelets of different carrier frequencies. This does not improve the horizon matching with any of the RES traces considerably, as, for several reasons, the model is not able to correctly reproduce the reflection amplitudes. The synthetic trace solely mirrors the signature of the conductivity IRHs, i.e. the pattern of reflections. Since it would give only small merit, we decided not to add even more (synthetic) traces to the figures. They already contain a lot of information and would become too confusing. For these reasons we chose one source wavelet that can be compared best with most of the traces at the same time, with respect to vertical resolution, and still be matched relatively easy with measured conductivity peaks to keep the depth determination simple. However, we do now include the frequency dependence and the different measuring frequencies, as well as the different source types in the discussion (Sect. 6.1.2 and 6.2.1).

On a related note, I assume that the EM simulations assume a monochromatic source? Since the chirped radar systems have both finite and differing bandwidths, this is clearly an important simplification. Finally, is it also correct that the EM simulation method is physically a closer representation of the pulse/burst systems rather than the chirped radar systems? Again this needs to be discussed, along with potential caveats for cross-comparison.

Yes, we used a monochromatic source with decreasing amplitudes towards the margins of the burst length, (which is, however, shorter as for the radar systems) which is a closer representation of the burst systems. It is a simplification, and there are other simplifications as well. For example the model is 1D (to be able to simulate several km of ice at cm resolution) compared to the 3D ice sheet. For these reasons we do not expect the synthetic trace to look exactly like one of the radar data. Nevertheless, the simulation shows distinct reflections and patterns of reflections at the same TWTs as the radar traces and thus is very useful to connect the radar data with the conductivity record.

As mentioned in the previous comment, the forward model does indeed mimic a pulsed radar and not a chirp. We extended the discussion of these limitations.

2.3

Given the overall emphasis on cross-comparison between different radar systems, it is desirable that the authors provide evidence for how robust the method used to determine the permittivity of ice is for other radar systems (only AWI is discussed here). The variance in the obtained values could then be discussed in relation to other components of their investigation.

It was not our aim to get the exact permittivity value or its uncertainties with our method, as we actually do not need the exact value. Our method of depth conversion with the sensitivity studies with the conductivity profile is independent of the permittivity. We just need some good "first guess" for the permittivity so the reflections in the synthetic trace occur at more or less the same TWT as in the measured ones to match the peaks. In fact, as neither system was exactly at the Dome C drill position nor has the core reached bedrock, there is no possibility to get the true permittivity right. However, the accuracy of the permittivity obtained with our method is high enough to warrant the comparison and conclusions. We chose the AWI data because they have comparably high vertical resolution and the burst system is better represented by the modeled data. Additionally, most RES traces are even further off the drill as the AWI profile. That increases the risk of mistakenly interpreting the spacial variations of reflector depths as uncertainties/differences in permittivity. Furthermore, this method of manually comparing various traces of 40000 ns is quite time-consuming. For that reason we would like to keep it as is, as the accuracy of the permittivity we use

is sufficient for our purpose. However, we appreciate from several comments that we did not make our aim of a first estimate of the permittivity clear enough. For that reason we revised the whole section 2.3, as shown in the revised manuscript with changes highlighted in red and blue color.

5.1

5 If possible I would like to see a discussion (and potentially an explanation) for the variation in the relative amplitude of the IRH peaks for the different radar systems. In particular, the relative amplitude of the peaks for the UTIG trace appears to be lower than the other systems of comparable vertical resolution (AWI,CReSIS), and this may potentially relate to frequency dependence. One reason why reflection amplitudes are important is that they are used to determine depth-averaged attenuation rates (and thus information about depth-averaged temperature). Subsequently, even if only preliminary conclusions be made

10 regarding differences in amplitude, this will be useful for future combination studies.
We appreciate that the differences in amplitude are a very interesting point, especially with respect to the attenuation and temperature in the ice. However, as our focus is on age structure of the ice sheet, we are concentrating on the depths of the IRHs, but not on the amplitudes or decline of amplitudes of the reflections (see answer to comment 1). We do not think we have the means to draw any quantitative conclusions regarding the amplitudes. Most of the data that were provided to us

15 *for this study were already processed in some way, including deconvolution, pulse shaping and gain adjustment. For some systems we even use differently processed data for the different depth regions (always the best visibility of IRHs in mind). That already has significant influence on the relative amplitudes. We cannot compare the relative amplitudes of differently processed data and separate the effects of measuring frequency and processing. To collect all the raw data and calculate the corrected relative decibel power for all the different radar systems would, in our opinion, be beyond the scope of this study. However, we*

20 *added to our discussion (Sect. 6.1.2) and conclusion, that the differences in relative amplitudes, possibly based on frequency dependence, should be closer examined when using RES data to investigate attenuation rates and the reasons for which we did not examine them.*
Since it is mentioned in the conclusions that AWI, CReSIS and UTIG are likely suitable for combined analysis, it would be useful to see some direct cross-over analysis of the traces. I appreciate from Fig 1 that this may only be possible for AWI and

25 UTIG, but this would act to strengthen the overall conclusions regarding combining data.
Good point. We included this figure (Fig. 1) as a direct comparison of AWI and UTIG radargrams at the cross-over point of the profiles. We think it is a great example that the data are indeed well combinable as all strong horizons can be traced smoothly across the intersection. But this figure also shows, that the strongest reflections in one profile are not necessarily the strongest ones in the other profile (e.g., H4 and horizon just below). This could be caused by the different source types, frequencies,

30 *and/or processing of the data. As you already pointed out, this cross-comparison is not possible for the other profiles, due to the lack of more cross-over points in the data available to us.*

6.1.1

I think it would be helpful here (or potentially in the caption of table 1) to provide relevant equations regarding the relationship between bandwidth and vertical resolution for the chirped systems (e.g. as supplied in the CReSIS reference).

35 *Thank you for this suggestion. We changed the caption of Table 1 and included: "the vertical resolution, determined as $\frac{kc_0}{2B\sqrt{3.17}}$ for the chirp systems. For the window widening factor k we use 1.53, as given in CReSIS (2016)." and changed the values for the vertical resolution, now including the windowing factor. Furthermore, we extended the table by references for the data and the characteristics of the synthetic data.*

6.2.1

40 Is it possible to provide a rough estimate of the radar footprint diameter in the Dome C region? My guess is that as we are dealing with comparatively thick ice this would be toward the upper end (or possibly exceed) the range that is stated.
You are right, there is comparably thick ice in this region and the systems' footprints at the ice-bed interface exceed the given range of 10–100m scale. E.g., the one for the CReSIS system is about 110 m. However, in this context we are not talking about
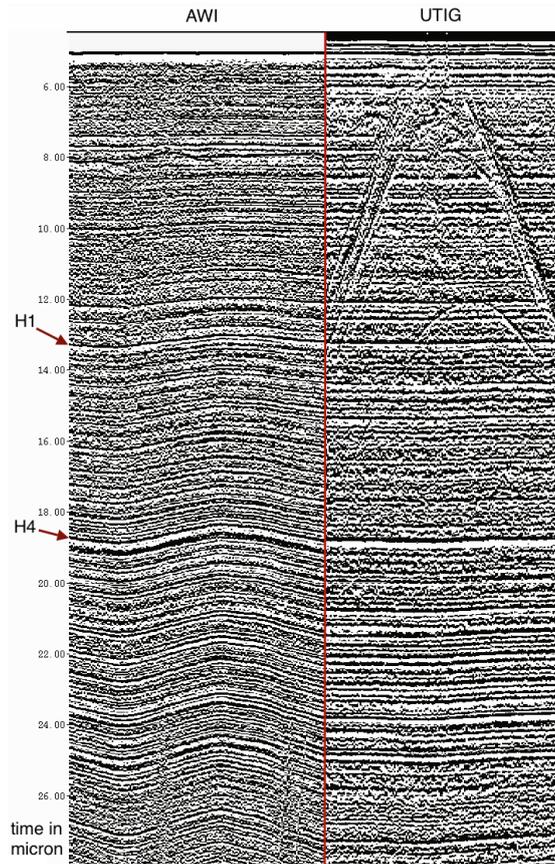
Figure 1. Intersection of AWI and UTIG profiles for direct visual comparison. Two examples of identified IRHs are indicated by red arrows. Note that the air-ice reflection is shifted to 5 μs. All strong horizons can be traced smoothly across the intersection.

the bed, but the IRHs in the ice column. Nevertheless, since we neglect the upper few hundred meters, the 10 m scale is quite understated and we changed it to 100 m scale.

7

As with the introduction, I think a clear case needs to be made what distinguishes the conclusions of this study from Cavitte et al. 2016.

I think it also needs to be discussed explicitly in the conclusion how variable vertical resolution (particularly how multiple peaks transition to single peaks as function of vertical resolution), pose challenges for combining data sets. This discussion will hopefully also address other glaciological information derivable from IRHs.

We checked the whole section in consideration of your suggestions. We included the possible challenges due to variable vertical resolution and the investigation of attenuation rates and temperature as additional glaciological information. Furthermore, we put more emphasis on the comparison of RES data and RES vs. synthetic data, which, in our opinion, distinguishes this study from Cavitte et al.'s.

Minor comments

There are quite a few examples where there is no white space preceding the SI units. (e.g. 0.2m). These should be corrected.
Be consistent with the hyphenation of ice-core/ice core.
There are many instances where use of 'vertical resolution' would be less ambiguous than 'resolution'.

5 *We added white space in front of all units, we now use "ice core" without hyphenation throughout the manuscript, and have added "vertical" in front of resolution, where it was ambiguous.*

1

Line 17: It would be helpful to add a reference here.
Augustin et al. (2004) was added as a reference.

10 For brevity, the final paragraph of the introduction could be dropped.
The paragraph was removed.

2.1

Line 1: ice: → ice (: should only be used for equation arrays/lists)
We could not find this phrase in this section and assume the comment was unintentionally copied from the 2.2 section.

15 Line 9: Reword 'we shortly discuss the input parameter permittivity of ice.
The sentence was reworded to "we describe how we derive the value for the permittivity of ice that we use in all further proceedings."

Line 11-13: If possible, please reference the core data (temperature, accumulation, etc.)
The EPICA Dome C 2001-02, science and drilling teams (2002) was added as a reference, additionally to Augustin et al.
20 *(2004).*

Lines 18,22: Give units for sigma and rho when they are introduced.
Units were included

Lines 24,27: Unit spacing for Xmm
Done

25 Line 24: measuring: → measurement
Changed

Line 25: comma before rho_ice
Done

2.2

30 Title: Consider changing to 'Electromagnetic modeling of radar traces'
Title was changed as suggested

Line 1: ice: → ice (: should only be used for equation arrays/lists)
Changed

Line 4/equation 2: The equation is correct, but the symbols (epsilon,epsilon',epsilon'',sigma, omega) must be introduced
35 correctly in text; see, for example, Eisen, et al. 2004, equation (1). Additionally, epsilon_0 is best described as the 'vacuum permittivity' (the use of ordinary is confusing since 'ordinary permittivity' is used in the context of anisotropic media).
We changed "ordinary permittivity" to "vacuum permittivity" and introduced the symbols.

Line 14: 'incorrect ordinary permittivities (the real part of the complex relative permittivity)' → 'incorrect real permittivities'.
Changed

40 Line 24: Measuring → measurement
We could not find this and assume the comment was copied from 2.1

Line 17: '1D-FD' → '1D-FD (One-Dimensional Finite Difference).'
Added

Line 19: Please be more specific about the boundary condition(s). Is it the lower or the side boundaries?

As the model is 1D the absorbing boundary is also implemented in the only space dimension, which is the depth direction. For clarity we added "in the depth direction".

Line 20: Please reference the Courant Criterion, and state what it tests for (convergence of the numerical solution).

5 *Courant et al. (1928) was added as a reference, and "that ensures the stability of the numerical calculations" was added*

Line 21: See my specific comment about the frequency dependence/sensitivity of the synthetic trace. It should be clearly started here that there are differing frequencies and bandwidths for the radar systems.

We changed the section to "... we use a source wavelet of two and a half 150 MHz cycles. It should be noted here, that, for simplicity this wavelet is based on the burst and pulse radar systems rather than the chirp systems, which require additional
10 *post processing like pulse compression (Sect. 3). However, this synthetic pulse is much shorter and the wavelet is not identical to any of the RES system ones. We chose it as trade-off between being long enough to..."*

Line 24: Please provide a reference for Hilbert magnitude transform.

Hilbert (1906) was added as a reference

15 **2.3**

Title: Consider changing to 'Determination of the relative permittivity of ice'

Since we rewrote the whole section, we decided to change the title to: "Assessing the permittivity of ice"

3.2

Line 17,22: Unit spacing for X microseconds.
20 *Done*

3.4

Line 27: Unit spacing for km.
Done

3.5

25 Line 27: Unit spacing for MHz.
Done

For completeness it would be helpful to list the distances of the profiles from Dome C for all radar systems (this is only provided for AWI and CReSIS).

The distance to drill site is given for all systems in the systems' sections. It just is not always in the last sentence of the section.

30 **4**

Line 15: No new paragraph?
Changed

5.1

Line 12: second → upper right
35 *Changed to upper middle*
Line 18: resolution → vertical resolution
Changed

5.2

Line 6: y → vertical
Changed
Line 8: about → an approximately
Changed
Line 17: is starting → starts?
Changed
Line 32: missing → missing from the synthetic trace
Added

5.3

Line 2: Is there a suitable reference for the 'sensitivity approach' used here?
Eisen et al. (2006) was added as a reference
Line 15: The advantage → The advantage of the sensitivity approach. . . ?
Added

6.1.1

Title: Consider changing to 'Vertical and horizontal resolution'
Title was changed
Line 14: UTIG systems the→ UTIG systems to be the?
Changed
Line 15: Obviously in → Due to their lower vertical resolution
Sentence was changed
Line 25: continue → continue with
Changed

6.1.2

Line 3: remove 'and others'
Removed
Line 8: accumulated → accumulates?
Changed
Line 10: it is → the slope is
Changed
Line 13: Urbini et al. needs a date.
We have removed this sentence in the revised manuscript.
Line 14/15: Reword sentence starting: 'That would...
We removed the sentence
Line 22: they → this
Changed

6.1.3

Line 8: comparing → comparative
Changed

6.2

Lines 12-14: 6.2.2. and 6.2.1 are introduced in the wrong order.
Order was changed

6.2.1

Lines 19,20,28: Unit spacing for Xm
Added
Line 23: examine → who examine
(Line 22?) This would lead to an incorrect sentence?
Line 28: find → found
Changed

6.2.2

Line 14: missing full stop after trace
Added
Line 15: remove 'used'?
Removed
Line 27: example: The → example, the
Changed

7

Line 9/10: It probably best to relate the well resolved IHRs for these systems directly to their better vertical resolution than the other systems (rather than implicitly through their bandwidth).
We changed the sentence to "If interested in well resolved IRHs at intermediate depths, the AWI, CReSIS, and UTIG systems, are the most suitable, due to their comparably high vertical resolution."
Line 13/14: Reword: The best quality in imaging the basal layer have the CReSIS, UTIG and BAS data, the latter, however, with ...
Changed to: "The CReSIS, UTIG and BAS systems have the largest penetration depth and are able to image some structures in the basal region. Over the comparably low vertical resolution of the BAS data, we attest the CReSIS and UTIG systems the best suitability in our comparison..."
Line 19: Remove 'profound' (it is best practice to avoid superlatives)
Removed
For the conclusions it is best practice to use past tense rather than present tense, and I would recommend carefully checking this section.
We changed the tense of the section to past tense and checked it again. Changes can best be seen in the revised manuscript with color-highlighted changes.

Figures

Table 1

Relabel 'resolution' as 'vertical resolution'. For completeness it would be desirable to provide an indication of how windowing/processing affects the vertical resolution (e.g. for the CReSIS system I believe that the post-windowed vertical resolution is 4.3 m).
"Vertical" was added. Furthermore we changed the caption and included the equation for the vertical resolution, including the windowing/processing and changed the values accordingly.

Fig 2.

Given that the reflections are ultimately caused by discontinuities in conductivity (rather than the peaks themselves), I think it would be useful to provide a plot of the vertical gradient of conductivity underneath the conductivity plot. I wouldn't be surprised if this gradient plot has a more 'immediate' correspondence with the synthetic trace reflections, and could be used to improve the analysis in Section 5 and 6.
We replaced the plot of the conductivity with the vertical gradient of the conductivity (for clarity we plotted the envelope). Like you anticipated, the synthetic trace is closer resembled by the gradient than by the conductivity profile
Following the terminology in Section 4, it should be added explicitly to the caption that Fig. 2 is an A-scope plot
Caption was changed to "A-scopes for traces of the five radar systems and synthetic trace. ..."

Fig 3.

The font size for the axes labels/numbers should be increased
The font size was increased.
of → for
Changed
x-axis → horizontal axis
Changed
closer →closely?
Changed

References

Augustin, L., Barbante, C., Barnes, P. R., Barnola, J. M., Bigler, M., Castellano, E., Cattani, O., Chappellaz, J., Dahl-Jensen, D., Delmonte, B., et al.: Eight glacial cycles from an Antarctic ice core, Nature, 429, 623–628, doi:10.1038/nature02599, 2004.

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