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Climate of the Past Discussions

Interactive comment on "Annual layering in the NGRIP ice core during the Eemian" *by* A. Svensson et al.

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We thank the reviewers for providing insightful comments and for pointing out weaknesses in the submitted manuscript. Please find below our point-to-point answers to the comments of the reviewers. Reviewer comments are shown in *italic*:

Anonymous Referee 1. Received and published: 29 April 2011

This paper presents the first high resolution profiles of sodium, ammonium, dust and conductivity over the bottom part of the NorthGRIP ice core corresponding to the last interglacial period. From these measurements (especially dust and conductivity), the authors are able to deduce the thickness of the annual layer at the very bottom of the NorthGRIP ice core. This is very important since it permits to constraint the age model

of this ice core for its deeper part.

In general, the paper is concise, well written and the method is clearly explained but I have several important concerns concerning the goal of the paper as well as some points in the discussion of the dating issue that are detailed below :

Part 4: I had difficulties in following the explanation for the profile smoothing from line 26 of the page 754. So that it would be nice to write this section again more clearly. More specifically, it seems to me (if I understood correctly the text) that between line 12 and 25, the authors try to explain the differences between Holocene and Eemian data (Dust, conductivity, Na, NH4) but do not manage in a simple way ; it seems that neither the effect of ice compression nor impurity diffusion explains the spectra. Then, they discuss separately the different data sets and draw different comparison (with glacial ice for dust for example). I had really difficulties in following the argumentation in this section as well as to see the conclusions of this section: what conclusions do the authors draw here?

This section has been rewritten and is hopefully clearer now. Still, this is a first attempt of looking at ice core CFA data in the frequency domain (as has long been done for water isotopes) and we are not able to come up with simple explanations for all that we see. We think this is a useful way to investigate the records, for example to learn about ion diffusion in the ice, and we do draw some basic conclusions concerning that subject, but certainly more investigations and more records from other depth intervals are needed to fully explore this topic.

Part 5: This part is more clearly explained but still, I have the feeling that the conclusions from this section is not clear. It seems to me that we do not learn new things from this part. We have information on different mean level of impurity or chemical species and 'so what'? We are left with questions for what to do with this information. What is it useful for?

In the manuscript, we present for the first time records of Greenland impurity levels from the last interglacial period and the glacial inception. Those are climatic periods currently of great scientific interest, but for which very limited data records from Greenland ice cores are available. As we mention in the manuscript, a thorough interpretation of the observed impurity levels would involve discussion of a wide range of topics, which is beyond the scope of our work. The one clear conclusion we do draw in this section is that 'the Eemian [impurity concentration] levels are very comparable to those of the Early Holocene and that dust and ammonium appear to express a climatic dependence in concentrations'. Although this is not a highly sophisticated interpretation, we believe that it is the most important first-order conclusion to draw, and we are convinced that our Greenland impurity dataset (i.e. table 2 in the manuscript) has value in itself, for example for the modeling community that simulates atmospheric transport and deposition in Greenland of dust, sea salt, and biogenic material over the last glacial cycle.

Part 7: In this part, I think that the authors should do an effort for discussing in more details the different ice core chronologies that are available for the NorthGRIP ice core as well as the uncertainties attached to that. In contrast with the authors' affirmation, there are large discrepancies (several kyrs) for MIS 5 between a chronology based on speleothem (Drysdale et al., 2007 ; Wang et al., 2008 ; Cheng et al., 2009) and a chronology based on the Antarctic EDC3 timescale (Capron et al., 2010). So it is not really possible to state that the NorthGRIP ss09sea is supported by these two very different chronologies. It would be nice if the authors could discuss more precisely the different chronologies, show a graph with the differences and try to conclude from their constraints if one timescales is better than the other. Probably, the constraints on the annual thickness is too low to choose between the two timescales but in this case, it should be stated more clearly and also recognize that ss09-sea is not in agreement with both speleothem constraints and EDC3 timescale.

We probably were a bit imprecise in the comparison of the existing Greenland time scales for the early glacial period and we have reformulated this paragraph and made

reference to a new interesting European speleothem record that has appeared meanwhile (Boch et al., 2011, submitted). Based on our data alone, we cannot evaluate the accuracy of the various time scales, but we can constrain the annual layer thicknesses within the investigated period.

Part 8 can probably be placed before part 7. Indeed, it seems to me that the most important potential conclusion of this paper is the dating issue.

In the paper, we are addressing different scientific communities in the different sections. For those very concerned about time scales part 7 is probably the most interesting section. For those interested in microbiological and molecular analyses of ice cores, it is of great importance to have conclusions about the possible existence of liquid veins between ice crystals in the ice sheets (Price, 2000; Miteva et al., 2009). Our study seems to exclude the existence of such veins in the deep Greenland ice and we have added a few references to make this application more evident.

Anonymous Referee 2. Received and published: 4 May 2011

This paper explores the possibility of annual layer counting of the deepest section of the NGRIP ice core. This is a continuation of the method developed to date the Holocene and Glacial period from this ice core and shows that some annual layers may still be preserved in ice from the last interglacial.

The paper is generally well written and presents some interesting data however I am sceptical that the dating approach is reliable enough to count individual years. Unfortunately I am not convinced by the methodology and therefore in its current form I do not think it is suitable for publication.

The authors show that ammonium has undergone significant diffusion over the 110kyr period making it too smoothed to detect annual layers. Sodium is also dampened and therefore the resulting annual layer counting, and subsequent conclusions about layer thickness, is based largely on two parameters. I am doubtful that dust and conductivity *are good enough to independently count annual layers. Looking back at some of the figures from Rassmussen et al., (2006) ECM and dust are not very convincing during the Holocene. The figures published in that paper show several multi-peaks in dust while ECM is really not very well suited for identifying annual layers. These early studies were aided by multi-parameters to constrain the selection of years however this is not possible during the Eemian ice.*

Concerning the feasibility of annual layer counting based on dust profiles in Greenland ice cores we find that dust is actually quite applicable for annual layer counting in the Holocene. It is true that in high-resolution dust profiles multiple annual peaks are frequent, but when the profiles are smoothed either by the analytical method or by subsequent data processing the annual layering becomes rather prominent. Sometimes the annual layering even gives rise to a peak in a power spectrum, such as the peak seen for Holocene dust at 75 mm in Fig. 4 of the submitted manuscript. This is because the main fraction of dust arrives to Greenland during spring time (in the Holocene) although it may be deposited in several events. Good examples of NGRIP early Holocene dust profiles of different measurement resolution and with clearly visible annual layering are shown in Bigler et al. (2011), Fig. 5. In the deepest part of NGRIP the dust profile is very much compressed as compared to the Holocene, and for this ice we are, therefore, much less concerned with over-counting caused by multiple annual dust peaks, because individual dust deposition events will not be resolved in those thin layers. Furthermore, compared to chemical impurities, the diffusion of dust particles in ice is very low, and, therefore, dust is probably the most well-suited parameter for annual layer counting in deep ice core ice.

In contrast, we agree that the liquid conductivity profile (as well the ECM profile) is not very applicable for layer counting in the Holocene. To our surprise, however, we find quite a good correspondence between the dust and the conductivity profiles in the deep NGRIP record that is unlike observations in the Holocene. This is the reason for arguing in the manuscript that most likely the conductivity profile has undergone signif-

icant post-depositional processes (diffusion) in the deep ice and does no longer reflect the same signal as does the Holocene profile. Because of the good correspondence between dust and conductivity in the sections of overlap, we find it justified to apply the conductivity profile for layer counting in the sections where the dust profile is absent.

Certainly, an annual layer counting based on just one or two profiles will never be comparable in precision to the annual layer counting we did in the Rasmussen et al. (2006) for dating of the early Holocene NGRIP section, which was based on a strong multi-parameter dataset that allowed for very precise dating and a very low counting error. In the present study, we are not fortunate to have a dataset of similar quality, but we are confident to see clear evidence of annual layering in the ice, and the reported errors will just have to reflect the poorer data quality (see discussion below).

I think it is possible that some annual layers can still be identified during the Eemian. Indeed the periodicity of the wiggles in some sections is convincing, however the errors presented in this paper are not appropriate for this dataset. There are years which have been assigned as definite where there is no data (eg Fig 5) and sections where dating is based purely on conductivity (Fig S1) (which we know from the Holocene sections is probably not reliable). If the authors wish to peruse this dating method then I would urge them to assign a far greater error estimate. Using only two (and in some cases just one) parameters has to have a far greater error associated with it than the previously published multi-proxy approaches.

We understand the concern of the reviewer and we agree that there are large uncertainties related to the applied counting approach. Indeed, the goal of the present work is not to push the counting error to low values or to provide a very precise dating of the earliest part of NGRIP. We agree that our data set is not sufficiently strong for that. What we do want to show is that annual layers are still present in the deepest section of NGRIP and that we are able to detect them by applying very high resolution measurements. At the same time, we think that we should provide some kind of error estimate of the annual layer thickness, otherwise comparison to other dating methods become meaningless, and for convenience we have applied the same error estimate method that has been applied for younger sections of the core.

In order to accommodate the concerns of error estimation we have doubled the error estimate of the counting in the revised manuscript (as compared to the reviewed version). That is, each 'uncertain' layer adds ±1 year to the uncertainty rather than ±1/2 year as applied in Rasmussen et al. (2006). Whereas this is a rather cruel approach, it is still based on the visual interpretation of the records and it is thus consistent with previously published approaches, and at the same time it allows for much greater uncertainty related to limited (and occasionally missing) data sections. The doubling of the error estimate does not influence the main conclusions of the dating effort that 1) overall there is good agreement between the counting and ss09sea, and 2) the counting suggests a somewhat longer duration of the MIS5e section of the ice than does the model.

There is value in publishing data from this time period. Presenting small sections where annual layers may still be identified, comparing concentrations with Holocene values and exploring the diffusion and ice crystal effects are important in helping us understand the climate of the last interglacial. However, I am not comfortable with using this method to date the ice core or make conclusions about the annual layer thickness. Additional parameters are needed and a more realistic estimation of the error involved.

Whereas we would certainly prefer to present additional ice core parameters in sub-annual resolution from the last interglacial period, it is quite likely that dust and conductivity are the only parameters that have preserved the annual signal over this long period and at the same time can be measured over longer sections of ice core in sufficiently high resolution to resolve annual layers. We, therefore, attempt to extract the maximum information from the available dataset and to present it with realistic error estimates.

We note that the estimated annual layer thicknesses are rather constant throughout

the investigated ice core sections. Therefore, a selection of smaller depth intervals with the most well-resolved records would not significantly influence the estimate of layer thicknesses, so in order to have the best possible statistics for the error estimate we apply the entire data set for this analysis.

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