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Climate of the Past Discussions

Interactive comment on "Climatic and insolation control on the high-resolution total air content in the NGRIP ice core" *by* O. Eicher et al.

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We copied the remarks by the reviewer and added our answers in *italics*

General comments

This paper considerably contributes to our understanding of the complicated nature of the variability of the air content of polar ice by providing and interpreting the first high-resolution air content record obtained along the deep Greenland ice core from the NGRIP drilling site. The local insolation effect on the air content is for the first time confirmed for the Northern Hemisphere and, what may be even more important - for a site with considerably (5-8 times) higher snow accumulation than at the Antarctic sites (Dome C, Vostok, Dome Fuji) where this effect was initially discovered. This

finding may help to improve our (still very poor and only qualitative) understanding of the mechanisms by which the insolation signal is imprinted in the air content of ice (At present it is assumed that local integrated summer insolation (ISI), by controlling temperature gradients in the near-surface snow during summertime, significantly affects the microstructure of snow. This implies that at sites with higher accumulation and therefore a shorter residence time of the snow near the surface, the probability that an insolation signal might be found in the properties of the ice should be lower. However the Greenland data shows that a longer and warmer summertime may well compensate for the negative effect of high accumulation.) In addition, the authors attempt to give the first possible (and quite realistic) explanation for the DO event-related variations of air content that have also been observed in the other Greenland ice cores (similar data from the GRIP ice core, as far as I remember, was presented at the PIRE meeting in Grenoble this year). However the section entitled 'Transient firnification model experiment' in which they explain how a sharp increase in accumulation rate may cause an observed decrease in the air content of ice is rather short and somewhat unsubstantial. I would suggest that the authors devote a bit more space and consideration to this section, which contains a novel approach to air content interpretation, in order to make it more convincing to the reader and to improve the overall presentation of the work (see my specific comments below). I also think that the manuscript could still gain from more careful and precise writing.

We address these questions in the specific comments below. We do agree that we have to discuss this transient response of the firn in more detail.

P5510, L23-24. The empirical relationship between pore volume at close-off and snow temperature was for the first time discovered by Raynaud and Lebel (Nature, 1979, 281(5729), 289-291).

Included in the text as follows:

Raynaud et al. (79) discovered an empirical relationship of pore volume at bubble close-off and snow temperature in the Camp Century (Greenland) ice core, owing to changes in the densification process in equilibrium conditions. Martinerie et al. (92) confirmed this positive correlation, mainly in Antarctic, but also alpine and Greenland ice cores in late Holocene snow.

P5513 and Fig. 1. Descriptions of the apparatus and error estimate are not sufficient to judge whether the declared (very high!) accuracy of the measurements is correct. What are the volumes V1 and V2? How was the volume of the system measured? (The declared accuracy for V1 and V2 seems very high!) Where is the pressure gauge located in Fig. 1? What is the accuracy of pressure (p exp) measurements and what is the p exp typical value during the measurements? Do you correct p exp for the partial pressure of water vapor, and how do you estimate the latter? My feeling is that the overall (absolute?) accuracy of the method involving the inhomogeneous temperature of the system is overestimated, but I cannot judge this based on the limited data available from the ms.

And:

P5515, L4-6. From my point of view these systematic shifts between different sets of measurements give us the right impression about the real absolute accuracy of the method (i.e., of the order of 5%)

Due to your comments and those by the second referee, this section will undergo some changes for better understanding and be moved to the end of the manuscript. We will make it more clear how we come up with the claimed precision and how we dealt with the inhomogeneous temperature in our system. The pressure gauge will be

included in Fig. 1, and the figure itself changed for better understanding.

P5519, L8-9. where Ts is the snow temperature in Kelvin, here assumed to be the same as the temperature at bubble close-off depth. I don't understand the meaning of this misleading sentence here. Then, in equation (8) for Vcr you distinguish between Ts and Tc, and this is correct. (Indeed, at present-day (stationary) conditions, Ts=Tc within uncertainties not exceeding 1.5℃. This approximate equality was used by Martinerie et al.(1992, 1994) to derive empirical relationships Vc(Ts) (7) from data on air content in recent ice at different drilling sites using both Ts and Tc whatever was available), but this information is not important for your consideration).

We included both Ts and Tc since in the Martinerie (92) paper Ts is used, while we later use Tc, referring to Kindler et al.(2013). So we change the sentence: where T_s is the snow temperature in Kelvin, here assumed to be the same as the temperature at bubble close-off depth.

To:

where T_s is the snow temperature in Kelvin, here assumed to be the same as the temperature at bubble close-off depth, T_c when the firn column is in thermal equilibrium.

(But, we could also just use T_c, despite Martinerie's notation, if the reviewer wishes)

P5527, L21-24. Please explain (or provide reference) how did you estimate changes in temperature and accumulation rate during DO-event that you used in the modelling.

We will. (And refer to Kindler et al.)

P5527, L24-27. These 4 lines of the text do not explain how was the resulting modelled TAC evolution shown by solid blue curve in Fig 11 actually obtained. Please give more details explaining the calculations, which will allow the reader to judge whether the proposed scenario is realistic or not.

We modelled the firn density during the transition into a DO-event with a transient firnification model (Schwander et al.,1997).

Replaced by:

We have used a standard dynamic firn densification model (Schwander et al., 1997) to calculate this upper TAC limit for a typical DO-event. In addition to computing the time and depth where the steady state close-off density is reached (as in the normal usage of the model), this model also outputs the density that a firn layer reaches after a certain number of years. As a first order estimate we assume that after the sudden onset of a DO-event, the same number of years as under the preceding stadial conditions is needed to reach close-off because the temperature in the firn increases only slowly at the bubble close-off depth. Accordingly, we calculate the density reached by the model after this number of years, however, with the new accumulation conditions at the surface, which changes the pressure hence deformation in the firn. This density reflects the true close-off density and corresponding TAC better than values obtained for steady state interstadial conditions.

And deleted the sentence:

We have calculated the density of the layer with the age corresponding to the bubble close-off age under steady state interstadial conditions. This density is assumed to be the bubble close-off density during the first part of the event.

Also we changed, due to a mistake, the sentence:

Interstadial temperature is set to –46 ℃ and ice accumulation rate to 0.05 $m\,a^{-1}$.

To:

Stadial temperature is set to –46 ℃ and ice accumulation rate to 0.05 $m\,a^{-1}$.

P5529, L7-11. I agree with the authors' conclusion, but I myself would not refrain from an attempt to calculate, e.g. using the CWT technique, the time delay between the filtered air content record on the AICC2012 ice age scale and the ISI curve on the astronomical scale. Such an exercise could help to quantify the effect of the climate-related variations on the precision of the air content-based chronology for the Greenland ice core.

We still refrain from calculating a new timescale based on TAC, since we consider the uncertainties to be too large. Such a new time scale would be based on the correlation of ISI and TAC, however, the measurement noise as well as the DO signals are so large, that such a correlation will not give a precise result. Moreover, it is not clear per se which ISI forcing parameter to use. Note that Raynaud et al. used an ISI parameter that maximizes the correlation, thus introduced another degree of freedom. In summary, we do not see how such a new age scale would improve upon the existing AICC2012 age scale. Moreover, we feel that such a new less precise age scale than

AICC22012 or GICC05 would actually lead to confusion in the literature. We will add, however, some more discussion about the lowest part of the ice core TAC data (older than 110 kyr) and the ISI, where the phasing of TAC and ISI differ substantially indicating that AICC2012 and an air content based chronology would differ here. We will discuss the consequences on the climate record if we matched TAC to ISI for this interval.

Finally, I suggest that the authors mention in this paper, where it is appropriate, the work of Suwa and Bender, 2008 ('O2/N2 ratios of occluded air in the GISP2 ice core') in which both the local insolation signal and the millennial scale signals that are in phase with the local temperature record of rapid climate change (DO events) are discussed in application to the O2/N2 record from the GISP2 core. Provided the variations in the air content and the O2/N2 ratio are both related to variations in the close-off porosity, as proposed in Lipenkov et al. (2011), it is relevant to compare the findings of the reviewed work with those of Suwa and Bender.

Good point, will be included in the revision.

Technical comments

Please check and correct if needed the use of symbols in equations and their definitions. I give only few examples where corrections are needed: 1. To denote air content you use V in eqs. (1) and (8), but TAC in eqs. (2), (4), and (9). Why do you use different symbols for the same thing? 2. In eq. (2) you use for the first time R (gas constant) which is defined only after eq. (9). 3. In the middle part of eq. (9) Ts in the denominator should be replaced by Tc, Vc(T) in the numerator should be written as Vc(Ts), and Pa

likely represent Pc (? the definition for this is not found).

Those remarks will be addressed.

Interactive comment on Clim. Past Discuss., 11, 5509, 2015.