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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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Like in our answer to reviewer 1, we copied the comments and added our answers in *italics*

General comments:

I appreciated very much the effort made in this paper to provide for the first time evidence of a local summer insolation in the air content record along a Greenland ice core. It should be mention that such insolation signature was already revealed on another ice core property: the O2/N2 ratio measured on the air trapped in ice and the paper would highly benefit from comparing the NGRIP TAC record with the

GISP2 O2/N2 record by Suwa and Bender as it has been already done for the Vostok Antarctic record. The most important application (and motivation) of the discovery of the correlation between TAC and local summer insolation is to establish an ice core chronology tunes on local insolation (see for instance Lipenkov et al.). Even when the method has still to be confirmed and since it is here shown for the first time that the TAC – local summer insolation is valid not only for low accumulation Antarctic sites but also at NGRIP in Greenland, it is frustrating to read that the authors refrained to give a TAC chronology and to compare it with the existing chronology. The second and most innovating contribution of the paper by Eicher et al. concerns the GRIP TAC response to DO-events. My comments are very close to those, made by reviewer 1. The challenge is to explain why TAC is decreasing at an onset of a DO-event and I found the explanation innovative and quite convincing (transient effect of changes in firnification induced by rapid increase of accumulation rate at the onset of a D-O event). I would not be surprised if in the near future such idea will inspire the ice core community. Unfortunately I regret that the manuscript is on the whole difficult to read. To my point of view it will need some major restructuration and polishing (I have the feeling that the manuscript has been written too quickly). For instance the part concerning the experimental procedure including the calibration is really complex, likely difficult to follow and sometime to understand for most of the readers. I suggest a restructuration and clarification of this part in the frame of an annex.

We will carefully revise the manuscript (see also comments to reviewer 1 above).

The O2/N2 ratio measured on the air trapped in ice and the paper would highly benefit from comparing the NGRIP TAC record with the GISP2 O2/N2 record by Suwa and Bender as it has been already done for the Vostok Antarctic record.

A good point and as answered to the first reviewer's remark, this will be included.

For instance the part concerning the experimental procedure including the calibration is really complex, likely difficult to follow and sometime to understand for most of the readers. I suggest a restructuration and clarification of this part in the frame of an annex.

The calibration part will be moved in the back of the paper and clarified, by the help of improved language and a new Fig. 1, with the pressure gauge included in the scheme.

You use the designation "Total Air Content (TAC)". You may note that in part of the literature the use is Air Content (V), probably because Total Air and Air are considered as a redundancy. No problem to use TAC or V but it would be good to mention for the reader that the 2 denominations indicate the same property.

See answer to reviewer 1.

The equations should be written homogeneously and because of the large number of abbreviations used to define properties or parameters in the equations, a complete list of abbreviations should be added to help the readers.

Can be provided.

P. 5510, lines 1- 2: by the atmospheric pressure and temperature.

Included.

P. 5510, lines 21-23: There is still hope that air content is providing robust information about past surface elevation of ice sheets. Lorius et al., (1968) mention this possibility based on measurements made on a coastal ice core from Adelie Land, but the first pioneering paper showing convincing results about past changes in surface elevation based on air content and ice isotope (temperature) records is to my knowledge: "Climatic implications of total gas content in ice et Camp Century" by Raynaud and Lorius, Nature 243, 283 – 284, 1973.

We will address this issue in the revision.

P. 5510, lines 23-24 The first empirical relationship of pore volume at close-off in Antarctica and Greenland for a wide range of temperature has been discovered by Raynaud and Lebel (Nature 281,289-291, 1979).

Remarks will be addressed. (see answers to the first reviewer).

P. 5511, line 1. You mention here “Krinner et al., (2000) and other studies”. Please cite the other studies. It would be appropriate to mention in this part of the text what kind of variability we observe along the air content records.

Sentence changed to “Krinner et al. show”.

P. 5511, lines 6-8 It should be mentioned here that O2/N2 ratio are correlated with local summer insolation in Antarctica, but Suwa and Bender (2008) suggest that it is also the case in Greenland (GISP2).

icluded as follows:

..imprint in TAC and used it to constrain an absolute timescale of Antarctic ice core records. This orbital synchronization is further supported by variations, in the O2/N2 ratio, which also depends on summer insolation (Bender, 2002). This relation is shown to hold for the Greenland record GISP2 as well by Suwa and Bender (2008).

P. 5511, lines 26 and following. Please clarify the different pore volume and temperature effects you are talking about. For pore volume you have at least two effects: temperature (near the surface? or all along the firnification column?) and insolation (at the surface). On the other hand the temperature will affect directly the air

content enclosed in the pore volume at the time and place of the close-off, according to ideal gas law.

We completely agree with the interpretation of the reviewer and will try to be more specific in the revisions.

P. 5513, equation 2: n and R should be defined or at least it should be said that equation 2 is obtained according to the ideal gas law.

“So” changed to “So, with the ideal gas law, we define”

P. 5513, line 3 to P. 5515 line 23 : the part concerning the experimental procedure including the calibration is really complex, likely difficult to follow and sometime to understand for most of the readers. I suggest a restructuration and clarification of this part in the frame of an annex. [. . .]Just keep in the main text the major conclusions and a discussion of the main uncertainties of the TAC record discussed in this paper, both about the absolute values and the observed relative variations.

The part with the description will undergo some polishing and be moved to the end of the Manuscript, as specified in the answer to the first referee.

It would be useful to include a comparative table summarizing the different measuring procedures between the 2002-2004 and the 2010-2012 data. For instance in Figure 2 caption (and also in figure 4) you mention 2 types of data – melt-refreeze data and vacuum-melt TAC. This seems in contradiction with what is written p. 5514, lines 2 and 3: “the melt-refreeze step was part of all measurements.

Sorry for this confusion. We changed the sentence to: "The melt-refreeze step was part of all the measurements in our lab."
We later compare our data to data from Schmitt et al, from another lab, with a different procedure.

Therefore only two data sets would be compared in the table and we thus think such a table is not necessary.

P. 5516, lines 4-5 and figure 2. It is difficult to infer from figure 2 that TAC variability is much larger than the analytical error. You may for instance give a few figures for range and mean of analytical errors as well as for range of TAC variability.

We will consider how we can show more clearly that the variability is larger than the analytical error at this point of the manuscript. In the figures later, eg 8 and 9, it is clearly visible, so we could tell the reader to look there for more details.

P. 5516, line 15: you assume here that the scattering could be caused by seasonal variations of air content. I suggest that you cite here previous works reporting on such

seasonal variations. I think for instance to a paper by Martinerie et al.. You could thus report on the observed range in air content seasonal fluctuations.

There is a clear trend to lower scattering with depth, although not with a high correlation coefficient, indicating that part of the scatter is embedded in the ice itself. If we assume most of the scattering to be caused by seasonal cycles, then we average over more cycles with increasing age difference in the 25 cm of the adjacent reproducibility samples.

Changed to

There is a clear trend to lower scattering with depth, although not with a high correlation coefficient, indicating that part of the scatter is embedded in the ice itself. Martinerie et al., (92) found seasonal peaks with up to 10-25 % amplitude in TAC, so we can assume most of the scattering to be caused by seasonal cycles. If we do so, we average over more cycles with increasing age difference in the 25 cm of the adjacent reproducibility samples. The measured variation..

P. 5517, line 17. The GRIP air content is on the whole slighty lower, except maybe during the last 8,000 years.

The data show good agreement, with the GRIP TAC being slightly lower.

Changed to:

The data show good agreement, the GRIP TAC air content is on the whole slighty lower, except maybe during the last 8,000 years.

P. 5518, lines 7 and following. I found the discussion about the intercalibration issue between Raynaud et al. (1997) and Schmitt et al. (2014) data hard to understand. If it is a minor point I suggest to delete it, if not the text needs clarification.

Deleted the section:
Some of the difference can potentially be explained by an intercalibration issue between the Raynaud et al.(1997) and Schmitt et al. (2014) data. This effect has been estimated to be smaller than 0.5 mL/kg^{-1}. However the intercalibration was based on recent measurements on the Antarctic EPICA Dome C ice core and methodological and calibration changes over the last 15 years since Raynaud et al. (1997) published the GRIP data cannot be ruled out. At the moment we are not able to quantitatively explain the difference between measured and theoretical TAC values at GRIP and NGRIP.

P.5518, line 17 TAC at EPICA DC is shown to be anti-correlated with ISI during approximately the last 400,000 years.

Sentence "TAC in Antarctica is known to show an anti-correlation with the integrated local summer insolation (ISI) as shown in Raynaud et al. (2007).

Changed to

TAC in Antarctica is known to show an anti-correlation with the integrated local summer insolation (ISI) as shown in Raynaud et al. (2007), for the EPICA Dome C record during approximately the last 400,000 years.

P. 5119, equation . Please check the form of the equation. The dimensions should be relative to Ts and Vc.

V denotes the TAC here, as it was used in papers we refer to, but to avoid confusion we will reconsider the naming of TAC/V in the revision.

P. 5522, line 14, Based on figure 8, it is not obvious that TAC changes more in parallel with methane. This should be statistically checked.

We discuss this relation later (eg. displayed In figure 10), in the next section, but as you comment correctly, we shouldn't write it as an already checked anti-correlation at this point of the paper. Therefore we changed in the text:

The high-frequency variations in TAC seem to change more in parallel with CH4 (anti-correlated) and therefore on gas age scale in the higher-frequency variations, suggesting a direct influence of gas temperature on the number of moles of air enclosed in the pore volume during bubble close-off. We investigate this proposed anti-correlation in the next section.

And in the caption of figure 8:

"Grey lines indicate the beginning of the DO-events in CH4. More on the timing of TAC changes in section 4.3.2 and in Fig. 9 and 10.

5522, line 24. Using the ideal gas law

Changed.

P. 5529, lines 7-11. The most important application (and motivation) of the discovery of the correlation between TAC and local summer insolation is to establish an ice core chronology tunes on local insolation (see for instance Lipenkov et al.). Even if the method has still to be confirmed and since it is here shown for the first time that the TAC – local summer insolation is valid not only for low accumulation Antarctic sites but also at NGRIP in Greenland, it is frustrating to see that the authors refrained to give a TAC chronology and to compare it with the existing chronology.

We understand that it is an interesting subject to develop a NGRIP chronology based on TAC. As outlined in detail in the reply to referee 1 we think that a chronology based

solely on the TAC would suffer from very large uncertainties since our data is rather noisy.

P. 5529-5533, references. All references have to be checked. The last number(s) of each reference indicate(s) the page(s) where the reference is cited. Is that a requirement of CP?

It may be a requirement for this stage, but we didn't provide this numbers ourselves, they appeared durng the copy editing process of the discussion paper by Copernicus.

P. 5532, line 6 Check the names of the authors. They don't correspond to the cited paper.

Strangely, in my bibtex-file this was wrong, thanks for noticing.

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