We copied the remarks and added the answers directly *in italics*

Reviewer I

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 and made appropriate changes in the manuscript.

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).

Changed to :

"Raynaud and Lebel (1979) 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 densication process at equilibrium conditions. Martinerie et al. (1992) conrmed 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.

Our measurement uncertainty estimate appears to be correct. The scatter of the data is only partly due to the measurement error, partly due to real (potentially seasonal) variations in TAC in the ice. As outlined in the manuscript, together they readily explain the variance in the data.

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 has undergone some changes for better understanding.

We made 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 is now included in Fig. 1, and the entire figure was improved for better understanding.

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

We included both T_s and T_c since in the Martinerie (92) paper T_s is used, while we later use T_c, referring to Kindler et al.(2013). So we changed 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."

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 added:
"we increase the temperature from –46° C to –36° C and the ice accumulation rate from to 0.05 $\mathrm{m\,a^{-1}}$ to 0.1 $\mathrm{m\,a^{-1}}$ within 100 years, based on model data by Kindler et al., (2014)."

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 rn densication model (Schwander et al., 1997) to calculate this upper 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) we obtain the density that a rn layer reaches after a certain number of years. This number of years was set to the duration needed to reach close-off under interstadial conditions. Under the above mentioned assumption of an initially constant duration to reach close-off, this density reects the true close-off density and corresponding TAC better than values obtained for steady-state stadial 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 corrected the sentence:

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

To:

"Stadial temperature is set to -46° C 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 (W/m^{-2}-threshold for integration) 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 added, 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 chronol-

ogy would differ here. We will discuss the consequences on the climate record if we matched TAC to ISI for this interval, considering new findings by Landais et al., (2016)

Finally, I suggest that the authors mention in this paper, where it is appropriate, the work of Suwa and Bender, 2008 ("O_2/N_2 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 O_2/N_2 record from the GISP2 core. Provided the variations in the air content and the O_2/N_2 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.

We included this in the revision.

In the introduction:
"This orbital synchronization is further supported by variations, in the O_2/N_2 ratio, which is also correlated with summer insolation (Bender et al., 2002). The latter relation was shown to hold for the Greenland record GISP2 as well (Suwa and Bender, 2008). Further, Suwa and Bender, (2008) showed increasing O_2/N_2 ratio for DO events in the GISP2 ice core."

In the "spectral analysis" part:

"Our results for NGRIP support the findings by Raynaud et al., (2007) and Lipenkov et al., (2011) on Antarctic TAC being related to ISI. Both records show coherence in the obliquity and precession bands. Antarctic TAC and O_2/N_2 ratio (Lipenkov et al., 2011) were known to contain ISI signal as was the O_2/N_2 2 ratio in Greenland (Suwa and Bender, 2008) as well. Now we show for the first time that Greenland TAC contains an ISI signature, similarly to Antarctic TAC records."

In the "relation to climate change during DO events" part:

"Apparently, TAC shows an anti-correlation not only to ISI but also to rapid DO warmings. Again a comparison to the O_2/N_2 ratios is of interest, since Suwa and Bender, (2008) found not only a correlation to insolation but as well a correlation of O_2/N_2 ratios with DO-warmings. We speculate that both proxies are inuenced by the same not yet fully understood firnication processes."

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?

We now use TAC only, but write in the text that sometimes V is used in the literature.

2. In eq. (2) you use for the first time R (gas constant) which is defined only after eq. (9).

changed to:
"So, with the ideal gas law and the gas constant R, we define."

3. In the middle part of eq. (9) T_s in the denominator should be replaced by T_c, $V_c(T)$ in the numerator should be written as $V_c(T_s)$, and P_a likely represent P_c (? the definition for this is not found).

Totally correct, we changed this accordingly.

Reviewer II

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 O_2/N_2 ratio measured on the air trapped in ice and the paper would highly benefit from comparing the NGRIP TAC record with the GISP2 O_2/N_2 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 NGRIP 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 changed the structure of the calibration section and carefully revised the manuscript. The differences are too large in number to discuss here in detail, please have a look at the revised manuscript.

The O_2/N_2 ratio measured on the air trapped in ice and the paper would highly benefit from comparing the NGRIP TAC record with the GISP2 O_2/N_2 record by Suwa and Bender as it has been already done for the Vostok Antarctic record.

See answers to the first reviewer.

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 has been rewritten and changed in structure. It is not moved to the back of the paper, as we believe it is written clearer now and hopefully easier to follow. The pressure gauge is included in the new Fig.1.

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.

A table with abbreviations is now 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 changed the text to:

"TAC was initially developed to provide robust information about the past surface elevation of ice sheets (Lorius et al. (1968), Raynaud and Lorius, 1973), due to its pressure and, thus, altitude dependence. However, consecutive studies showed that the densication and bubble close-off processes have an even larger inuence on the pore volume enclosed in polar ice and, thus on T AC."

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 were 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. (2000) show".

P. 5511, lines 6-8 It should be mentioned here that O_2/N_2 ratio are correlated with local summer insolation in Antarctica, but Suwa and Bender (2008) suggest that it is also the case in Greenland (GISP2).

".. imprint in T AC and used it to constrain the timescale of Antarctic ice core records. This orbital synchronization is further supported by variations in the O 2 /N 2 ratio, which is also correlated with summer insolation (Bender, 2002). The latter relation was shown to hold for the Greenland record GISP2 as well (Suwa and Bender, 2008). Further, Suwa and Bender (2008) showed increasing O 2 /N 2 ratios for DO events in the GISP2 ice core."

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 added specifications, as follows:
"Note that the insolation effects on pore volume represent a signal imprinted on the rn structure during densication and thus are a signal imprinted in the ice matrix. In contrast, variations in T AC due to direct temperature changes, as expected during DO events, reect changes in air density at bubble close-off and thus are imprinted in the gas record itself."

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.

changed to:
"So, with the ideal gas law and the gas constant R, we define."

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. The melt-refreeze step was part of all the measurements performed in the lab specialised for methane and nitrous oxide (eg publications

by Baumgartner, 2012 and 2014)
We later compare this data to data measured according to Schmitt et al., who are using a different procedure.

Therefore only two data sets would be compared in the table and we thus think such a table is not necessary. The whole section has been rewritten and restructured for, hopefully, better understanding

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 changed the text to:
"The measured TAC data show notable variability which is much larger than the derived analytical error of 1.3mL/kg (see Fig. 3)."

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.

Changed to

"There is a clear trend to lower scattering with depth, although not with a high correlation coefcient, indicating that part of the scatter is embedded in the ice itself. Martinerie et al. (1992) found seasonal peaks with up to 10-25 % amplitude in T AC, so we can assume most of the scattering to be caused by seasonal cycles or inter-annual variability. 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 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 average slighty lower, except 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.

We 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.

Changed to

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

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

V denoted TAC here, as it was used in papers we refer to, but to avoid confusion we clarified the nomenclature 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 sec-

tion, but as you comment correctly, we shouldnt write it as an already checked anti-correlation at this point of the paper. Therefore we changed in the text:

"Instead, the high-frequency variations in T AC seem to change in parallel with CH 4 and therefore on gas age scale, suggesting a direct inuence of temperature on the number of moles of air enclosed in the pore volume during bubble close-off. We will discuss this anti-correlation in the next section."

And in the caption of figure 8:

"Grey lines indicate the beginning of the DO events in CH_4. 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 large uncertainties since our data is rather noisy and would not serve the purpose of a more precise dating.

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 didnt provide this numbers ourselves, they appeared during the copy editing process of the discussion paper by Copernicus.

P. 5532, line 6 Check the names of the authors. They dont correspond to the cited paper.

Corrected!

Other changes/improvements since the discussion version:

In Fig. 8 the Ca^{2+} *concentration is now also included, since we discussed it in the manuscript.*

The onsets in CH_4 *for the stacking have been redefined, to be consistent with the Baumgartner et al., (2014) publication on the same samples. Therefore Fig. 10a looks slightly differently now.*

Since the AICC2012 gas age scale is inconsistent with its ice age scale, as stated in Baumgartner et al., (2014), at the DO onsets, we added a comparison of the phasing between TAC minima and $\delta^{18}O_{ice}$ *on the gas age scale based on ss09sea06bm published for 10 to 120 kyr by Kindler et al., (2014). This added also 3 more columns to the table 4.*

Table 4 and 5 have been merged to one table.

The nomenclature has changed for the ISI as well as for the beforehand so-called "inversed standardized TAC" and "inversed standardized V_{cr}*", since the terminology "inversed" has let to confusion by readers. We defined*
$TAC^* = -\frac{TAC-\overline{TAC}}{\sigma(TAC)}$ *and* $V_{\mathrm{cr}}^* = -\frac{V_{\mathrm{cr}}-\overline{V_{\mathrm{cr}}}}{\sigma(V_{\mathrm{cr}})}$,
for better understanding.