

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

Anonymous Referee #2

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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

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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.

Specific comments 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. 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. English is not my mother language but on the whole I found the manuscript difficult to be read. I think that the text requires some language polishing. P. 5510, lines 1- 2: . . . by the atmospheric pressure and temperature, . . . P. 5510, lines 21-23: There is still hope that air content is providing robust information about past surface elevation od 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

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Century” by Raynaud and Lorius, *Nature* 243, 283 – 284, 1973. 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) . The paper by Martinerie et al. (1992) is confirming and specifying the empirical relationship between pore volume at close-off and temperature. 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. P. 5511, line 4. It seems that the reference to Parrenin et al. (2007) is inappropriate here. 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). P. 5511, line 11-12 Dependence of grain size on insolation in the first meters of firn. If personal communication is the reference, it would be necessary to give more details: what kind of experimental evidences, other evidences? P. 5511, line 13. Could you shortly define tTGM? P. 5511, line 18. The insolation effect has been already documented also in Greenland ice (through O₂/N₂ measurements, 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. 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. 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. 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

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TAC. This seems in contradiction with what is written p. 5514, lines 2 and 3: “the melt-refreeze step was part of all measurements “ 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. 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. 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. P. 5517, line 17. The GRIP air content is on the whole slightly 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 Smitt et al. (2014) data hard to understand. If it is a minor point I suggest to delete it, if not the text needs clarification. P.5518, line 17 TAC at EPICA DC is shown to be anti-correlated with ISI during approximately the last 400,000 years. P. 5119, equation . Please check the form of the equation. The dimensions should be relative to T_s and V_c. 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. p. 5522, line 24. Using the ideal gas law. . . 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 is 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. 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? P. 5532, line 6 Check the names of the authors. They

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don't correspond to the cited paper.

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

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