Review of:

"Investigating the relationship between Total Air Content (TAC) variations in polar ice cores and local surface climate conditions"

By Guilluy et al.

Summary:

This paper investigates relationships between total air content (TAC) measured in ice cores and parameters that may affect the TAC signal on different temporal and spatial scales. To do so, the authors present new measurements of (TAC) from several ice cores from Antarctica. With these new measurements and previously published data, they use regression analyses to investigate TAC signals in relationship to local insolation, surface temperature, and accumulation rate.

My opinion is that this is a promising work to help better understand TAC and what it can tell us about paleoclimate. The new data are a valuable addition to the existing archive of TAC data, and the authors provide a thorough analysis using these new data and existing data. The topic and scope of the paper are appropriate for publication in The Cryosphere. However, before publication, there are a number of issues I believe should be addressed, which are organized into general and line-by-line comments.

General comments:

1. From a structural standpoint, this is a rather long, verbose paper. This isn't necessarily a bad thing, but given its length the paper lacks an organizational structure that stays focused on a specific scientific question or hypothesis. This is especially true in the discussion. For example, Section 4.1 seems to be much more of a results section than a discussion section. Again, it. isn't necessarily a bad thing to mix, discussion and results, but in this paper, I found it hard to follow the scientific arguments as it seemed that the paragraphs bounced between discussing time scales and spatial scales. My suggestion would be: (1) to add a more specific hypothesis and/or scientific question at the end of the introduction; (2) consider restructuring so that there are separate sections for spatial scale, analysis, and temporal scale analysis, each of which include germane results and discussion.
2. From a scientific standpoint, the new data and methodology presented in this paper is sound. However, some of the conclusions drawn from these data seem speculative and/or lacking specificity. For example, TAC correlations with insolation are attributed to grain size and grain metamorphism, but the paper does not provide any data to back up that assertion. From there, inadequate explanation is given as to why different grain structures result in different TAC. Broadly, grain size differences are attributed to HYSI,

but there are many more factors besides insolation that affect initial grain size/structure and subsequent metamorphism. Likewise, the authors vaguely point to "high" and "low" HYSI, but don't provide detail on what is considered high and low, nor do they provide an adequate explanation of why it is considered a threshold system rather than a continuum. (For example, see paragraph starting on line 460). In short, while the correlations the authors found appeared to be robust the explanations for the physical causation are lacking.

3. This is related to the above point: the introduction lacks discussion about the relationship between grain size and pore volume (which is the same thing as density) – it seems to me that the grain size affects the poor structure, but you can have the same poor volume with different grain sizes. In reading this, it occasionally seemed that you were equating grain size with pore volume (e.g., line 91: "increase the size of snow grains in the first few meters of firn, which then decreases pore volume" – what is the mechanism for that?). Is the relationship between poor volume and TAC as simple as: lower density (higher pore volume) at BCO means higher TAC and vice versa, or is it more complicated than that with pore geometry (e.g., fewer large pores or more small pores can yield the same pore volume, but perhaps have different characteristics for trapping air?) Adding description of the role of pore structure and clarifying the language around this in the introduction would likely help support the discussion later in the paper.
4. For the multiple linear regressions featured in figure for and described online 355: Aren't your predictors in this case themselves correlated? I understand that HYSI and surface temperature are not the same, however, I would expect them to be highly correlated. (And figure 8 seems to show that they are). You state in the paper that HYSI and summer temperatures are correlated, so I don't understand why an annual temperature would be independent when summer temperature comprises a substantial part of a mean annual temperature. On Line 432 you seem to indicate that HYSI is related to temperature ("thermal driver"). Figure 5 clearly shows that using the two predictors yields better results, but is still not clear to me why you don't consider these correlated, which would skew a multiple linear regression.
5. I got a bit lost at times keeping track of relevant timescales for analysis throughout the paper. For example, you stated that you used a 10 K year filter on the TAC data. But, then you used yearly temperature as a predictor. Are the yearly temperatures are filtered with the low pass filter? In which case I would expect an annual temperature data to be more correlated with half year solar insolation? Apologies that this is not a well formulated comment, but I wanted to note it to give you the opportunity to perhaps clarify some language around different relevant timescales with different variables for the different analysis.

6. The manuscript does not contain a data availability statement, which is required by Copernicus publications: https://www.the-cryosphere.net/policies/data_policy.html
7. There are numerous typos and grammatical errors throughout the paper. Please do a careful read through to correct these.

Line by line comments:

(note that many of these comments may be related to the general points above)

77: add more specifics about "increase TAC through pore volume" – what is the physical mechanism by which higher temperatures increase poor volume at close off?

90: This sentence appears to be verbatim of a sentence from Epifiano et a., (2023; third paragraph of introduction), which states: *"The proposed mechanism for this relationship requires that higher local summer insolation increases the size of snow grains in the first few meters of firn, which then decreases the pore volume in these same layers as they reach bubble close-off".* While I recognize the process of obtaining information from sources, I think it is inappropriate to copy a sentence this directly. I found this on a whim of looking into the background science, but please ensure that all of the text is your original writing. (also, your citation is of the preprint of the Epifiano paper rather than the final published edition)

102: "surface-driven": be more specific

Figure 1 and line 226: where do the present day mean annual surface temperatures come from?

Table 1:" unpublished": estimates for those values are easily obtained from DEMs and climate models.

Table 2: the formatting for the stable is not clear – eventually, I figured it out, but I suggest either reformatting it in a friendly format or adding text in the caption to aid the reader in interpreting it.

258: remove "$T_S = T_C$" – redundant

320: "interesting": what is interesting, and why?

Figure 4: I figured this out eventually, but it is not immediately clear what +1 and what +2 mean in panel B: (I assume it is a multiple linear regression with HYSI and accumulation for 1 and temperature for 2?)

Figure 4: Why is there no HYST for DF and TALDICE?

General: Be consistent with regression formatting throughout paper: lowercase r/r^2 is typically used for a single linear regression, and uppercase R/R^2 is for a multiple linear regression. (e.g., line 340 uses lowercase r, and table A2 uses uppercase R.)

Figure 4: Related to general comment above: The low R^2 shown in figure 4 in general suggest that these variables are not explaining most of the variability in total air content, but there is little data-supported discussion about what explains the remainder of the variability.

Figure 5: I am confused about the filter. First, it says you use a 10 kyr filter, then later the caption states a 16.7 kyr pass filter. Please clarify.

Figure 5: the caption says yellow boxes are where visually there's a better correlation, but the numbering and text in the caption indicates that these are the marine isotope stages. Please clarify.

Figure 5: caption states that "Yellow boxes highlight significant visual improvements": visual improvement is not an objective metric for comparison, and if you want to investigate correlation specifically during those periods, there are statistical tests to do so. I suggest first setting some threshold that is considered "improved" and running a statistical test to see where that occurs.

Figure 6, panel B: the colors for the residuals, and the predicted appeared to be the same. Please use different colors to highlight which is which.

382/Figure 6: the yellow interval does not appear to be centered on the total air content peak, which appears to be more at 250 ka and at the edge of the yellow highlighted region. Please clarify specifically what the yellow line is showing. The figure caption gives a bit more information, but it's still not clear how you're determining the width and center of the yellow highlighted region. For example, what is a "strong" millennial scale, TAC variation?

413: "This suggests that HYSI influences TAC values at spatial scale." – this is a broad statement. Can you be more specific?

415: "These results suggest a marked regional heterogeneity in Vc - climate relationships" – here is another example of a statement lacking support. Why is this the case? What is the physical explanation? An explanation may be given later, but if so, this relates to my general comment above about the organizational structure of the paper.

428: *"In line with the observations in Fig. 7.e., we do not observe a correlation with HYSI and Vcr for the EAIS sites (Fig. A5.b.)"*: Fig. A5.b. appears to show the non-EAIS sites - please clarify and correct this sentence.

435: “These geographical discrepancies point to different firn densification regimes”: again, this is an intriguing hypothesis, but lacks supporting data.

443: the claim “below a HYSI threshold, accumulation-driven processes

could dominate” seems counter to results from figure 4, which showed low correlation between accumulation and TAC for Greenland sites. If accumulation driven processes are driving V_c, shouldn’t TAC have a stronger correlation with accumulation?

446: “dual-threshold”: Can you explain further what you mean here? What are the two thresholds?

450/Figure 8: I’m wary of this analysis, as the regressions for Greenland and WAIS have only six and five data points, respectively. Although there is no accepted minimum number of points for a robust OLS regression, I think five or six is quite low. I don’t doubt that regional scale climate differences will make a difference in the relationship between accumulation and temperature, but the small sample size prevents you from fully understanding that on the ice sheet scale that you’re claiming.

Figure 8, panel B: I would much more expect temperature to be a function of insolation rather than insolation as a function of temperature as you have it plotted (i.e., dependent variable on the y axis). The R^2 will be the same, but it is probably more accurate way to think about the regression.

Figure 8: It would be helpful in this figure to use color coding for each point consistent with the ice sheet it represents. E.g. since you’re using a blue line for EAIS, all EIAS sites should be colored with a blue dot or consistent shape or something similar so that the reader can see which points are corresponding to which regression.