Detailed responses to the comments from reviewer #2

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.

We thank Reviewer #2 for their thorough reading of our manuscript and for their detailed general and line-by-line comments, which have helped us to significantly improve our manuscript. Below, we provide detailed point-by-point responses (in blue) and changes in the text (in bold blue) to each comment (in black). The changes will also be visible in the track-change version of the manuscript.

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.

We thank the reviewer for this constructive feedback. We agree that the original structure made it difficult to follow the scientific arguments, as results and discussion were mixed across spatial and temporal scales. We have therefore reorganised the manuscript to address both suggestions, ensuring that spatial- and temporal-scale analyses are each treated in sections that combine results and discussion, avoiding back-and-forth between scales.

(1) We have added an explicit research question at the end of the introduction along with a quick description of the manuscript's structure. The revised paragraph now reads:

(122) Despite existing correlations with summer insolation and surface parameters (e.g., accumulation rates, temperatures), the mechanisms driving TAC remain poorly understood. In particular, the physical controls on pore volume changes require further investigation. **We therefore address the following research question: "To what extent do surface climate parameters : local summer insolation, mean annual temperature, and accumulation rate, control TAC and Vc across spatial and temporal scales?"** This study addresses these gaps by compiling published and unpublished TAC records from Antarctica and Greenland (Fig. 1). ~~Based on this new compilation, we examine TAC signals, and associated pore volume changes across spatial and temporal scales (from millennial to orbital) and investigate their relationship to local surface parameters, namely the local insolation, the surface temperature, and the accumulation rate~~. **We first present new TAC datasets (Section 3), before examining spatial-scale (Section 4) and temporal-scale (Section 5) relationships between TAC, Vc and surface climate parameters, and discussing implications for orbital dating (Section 5.4) and remaining limitations (Section 6).**

(2) We have restructured the manuscript to clearly separate spatial- and temporal-scale analyses. New TAC datasets are presented in Section 3 (3.1 The EDC TAC records; 3.2 TALDICE, EDML and shallow ice core records). Spatial-scale relationships between TAC, Vc and surface climate parameters are examined in Section 4 (4.1 Relationships between TAC, Vc and present-day surface parameters; 4.2 Regional patterns and regime-dependent controls on Vc). Temporal-scale analyses are gathered in Section 5 (5.1 Orbital-scale TAC changes; 5.2 Millennial-scale TAC changes in the EDC ice core; 5.3 Discussion: relative influence of surface climate parameters at orbital- and millennial scales on the TAC signal; 5.4 Implications for the use of TAC as an orbital dating tool). Remaining limitations and perspectives are addressed in Section 6.

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.

(1) 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.

We agree with the reviewer. In the original manuscript, we attributed the TAC-insolation correlations to grain size following the hypothesis proposed in previous studies, but our study does not provide microstructural data to test it. The link between local summer insolation and TAC through near-surface snow metamorphism was first proposed by Raynaud et al. (2007) and Lipenkov et al. (2011), and was recently supported by Raynaud et al. (2024), who showed that EDC TAC is strongly anti-correlated with simulated local summer temperature. An insolation imprint on TAC has also been documented in Greenland (Eicher et al., 2016) and at South Pole (Epifanio et al., 2023). However, how this near-surface signal is transferred to the close-off depth is still debated, and at least two mechanisms have been proposed. In the first, coarser grains formed under stronger summer insolation reduce the pore volume at close-off (Raynaud et al., 2007, 2024; Epifanio et al., 2023). In the second, based on microstructural measurements at Dome Fuji, stronger insolation enhances the mechanical contrast between summer and winter layers, which prolongs pore closure and increases air loss during bubble formation (Fujita et al., 2009). None of these studies considers the mechanism as demonstrated: the underlying physical processes are described as not yet understood (Eicher et al., 2016; Raynaud et al., 2024), the microstructure and physics behind the grain-size mechanism remain to be explored (Epifanio et al., 2023), and the grain-size hypothesis still needs to be tested with measurements of grain size, porosity and permeability in the close-off zone (Fujita et al., 2009). Direct microstructural observations of pore close-off remain limited to a few sites (Gregory et al., 2014).

Measuring firn microstructure is beyond the scope of this study, and our TAC data cannot discriminate between these mechanisms, which predict the same sign of the TAC response to insolation. We have therefore replaced "grain size" and "grain metamorphism" by the broader term "insolation-driven near-surface metamorphism" throughout the manuscript when discussing temporal-scale relationships. Where grain size is mentioned, it is now explicitly presented as a hypothesis proposed in the literature.

Importantly, the spatial Vc–HYSI correlation observed within the EAIS does not directly reflect this insolation mechanism, it was an extrapolation in the first manuscript. Our analysis shows that, once the temperature dependence of Vc is removed, no significant Vcr–HYSI relationship remains for EAIS sites (Section 4.2). The spatial Vc–HYSI correlation within the EAIS therefore results from the combination of the Vc–temperature relationship common to all sites and the strong temperature–HYSI covariation specific to the EAIS geography, and cannot be attributed to a direct insolation effect on firn densification. This is now stated explicitly in the abstract, in Section 4.2, and in the conclusions.

The corresponding changes in the manuscript are as follows.

In the abstract, the original sentence has been replaced by: *(14)* "**We evidence a strong correlation between the pore volume at close-off and local half year summer insolation for East Antarctic sites, reflecting the spatial-scale covariation between summer insolation and mean annual temperature across this region.**"

In the introduction, "**insolation-driven near-surface metamorphism**" is introduced as the proposed framework for temporal TAC–HYSI relationships, with grain size presented as one of the two hypothesised pathways (Section 1, addressed further in the response to general comment 3).

In Section 4.2, the spatial Vc–HYSI correlation is now explicitly interpreted as indirect. This particular section has undergone major changes as its conclusions were initially different: the positive spatial Vc–HYSI relationship therefore does not reflect the insolation mechanism proposed for the inverse TAC–HYSI relationship observed at individual sites over time.

In Section 5.3 and Table 3, the temporal insolation control is described as: "**Near-surface grain growth via summer temperature gradients, leading to lower D0 and lower Vc**", consistent with insolation-driven near-surface metamorphism at EAIS and Greenland sites (Eicher et al., 2016; Raynaud et al., 2024).

In the conclusions, the original sentence has been replaced by: *(842)* "**At cold, high-insolation sites (EAIS plateau and some coastal EAIS sites), the strong spatial-scale covariation between HYSI and mean annual temperature produces an empirical Vc–HYSI relationship that cannot be attributed to a direct insolation effect on firn densification.”**

(2) From there, inadequate explanation is given as to why different grain structures result in different TAC.

We have clarified the mechanism in the introduction by adding an explanation of the D0 pathway and noting that summer and annual temperatures act on D0 in opposite directions:

(85) “**The inverse relationship between local summer insolation and TAC has been documented at orbital timescales in East Antarctic ice cores (Raynaud et al., 2007; Lipenkov et al., 2011; Raynaud et al., 2024), in the Greenland NGRIP ice core (Eicher et al., 2016) and at South Pole (Epifanio et al., 2023). How the grain structure acquired near the surface is transferred to the close-off depth is however not established, and at least two mechanisms have been proposed. The first relies on D0 (the relative critical density of snow, ∼0.6, marking the transition from grain-boundary sliding to power-law creep): since larger grains slide less easily past each other, whereas power-law creep is insensitive to grain size (Alley, 1987; Arnaud, 1997; Arnaud et al., 2000), coarse-grained firn formed under strong summer insolation reaches this transition at a lower D0, resulting in fewer pores per grain and a lower Vc at close-off (Arnaud, 1997), and thus closes off at a higher density, yielding a lower TAC (Freitag et al., 2004; Raynaud et al., 2007; Lipenkov et al., 2011; Raynaud et al., 2024; Epifanio et al., 2023). Note that summer temperature therefore acts on D0 in the opposite direction to mean annual temperature: the former through the initial grain size, the latter through the densification rate at the transition (Raynaud et al., 2024). In the second mechanism, based on microstructural measurements at Dome Fuji, stronger insolation enhances the mechanical contrast between summer and winter firn layers, which prolongs pore closure in the harder summer layers and**

increases air loss during bubble formation (Fujita et al., 2009). Neither mechanism has yet been demonstrated by joint measurements of grain size, porosity and air content in the close-off zone (Freitag et al., 2004; Fujita et al., 2009; Raynaud et al., 2024), but both predict a decrease of TAC with increasing summer insolation, consistent with the observations in individual ice cores. A quantitative relationship between grain structure and Vc, independent of other firn properties, has however not yet been established (Gregory et al., 2014; Raynaud et al., 2024), and the spatial relationship between Vc and insolation across different sites in Antarctica and Greenland has not been explored yet."

(3) 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

We fully agree. Near-surface grain size and firn microstructure result from several interacting surface processes, of which summer insolation is only one. These include accumulation rate, which sets the time snow spends in the near-surface metamorphic zone and therefore the duration of insolation-driven grain growth (Courville et al., 2007; Gregory et al., 2014; Epifanio et al., 2023); wind, which fragments and compacts fresh snow, redistributes it heterogeneously at the surface, and thereby controls both the initial microstructure and the surface residence time of snow (Martinerie et al., 1994; Fujita et al., 2009; Libois et al., 2014; Picard et al., 2019); and mean annual temperature, which affects the densification rate at the snow-firn transition through D0 (Arnaud, 1997; Arnaud et al., 2000). These factors are now explicitly presented in the introduction: *(110)* "**Wind further modulates near-surface grain structure by fragmenting and compacting fresh snow and by redistributing it heterogeneously, so that the surface residence time of snow, and hence its exposure to summer insolation, can vary from 0 days to one year and more over a distance of just a few meters at low-accumulation sites (Martinerie et al., 1994; Fujita et al., 2009; Libois et al., 2014; Picard et al., 2019, Ekaykin et al., 2023)**", and in Section 5.3, where HYSI is presented as one of several controls on firn metamorphism whose relative importance varies between sites and timescales (Table 3). To characterise this, and extend it to all TAC control mechanisms, we refined Table 3 as the following:

Table 3. Summary of ~~V_c~~ TAC control mechanisms ~~across polar regions~~at temporal scale

Parameter	Proposed process	Reference	Timescale	Expression
Summer insolation (HYSI)	Near-surface grain growth via summer temperature gradients → lower D_0 → lower V_c	Raynaud et al. (2007, 2024); Lipenkov et al. (2011)	Orbital	TAC anti-correlated with HYSI at EAIS and Greenland sites
Mean annual temperature + elevation (P_c)	Glacial–interglacial changes in surface climate (annual temp. and acc.) affecting firn densification; possible contribution of elevation changes	Raynaud et al. (2007); Eicher et al. (2016)	Orbital + millennial	TAC decoupled from HYSI at 100-kyr timescale
Annual acc. rate + temp	Reduced surface exposure or overburden pressure; regime depends on site conditions	Eicher et al. (2016); Epifanio et al. (2023b)	Orbital + Millennial	TAC decoupled from HYSI: acc./temp. becomes dominant. Peaks during deglaciations.
Wind speed + post-depositional processes	Wind-driven compaction, redistribution and sublimation of surface snow, firn post-depositional processes	Martinerie et al. (1994); Khomyakova et al. (2025)	Millennial	Site-specific non-orbital TAC anomalies, not synchronous between nearby sites (not observed in this study)

(4) 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).

We agree that "high" and "low" HYSI and "threshold" were not sufficiently defined. We now base this discussion explicitly on the glacial-to-Holocene comparison presented in Table 4 in section 5.1, where all sites experience a similar relative HYSI increase (+1.5 to +1.8%), so that their contrasting TAC responses reflect differences in local conditions rather than different orbital forcings. All sites with a mean Holocene HYSI above ~358 W m^{-2} show a TAC decrease (−1.1 to −10.9%), across a wide range of accumulation rates (2 to 46 cm w.e. yr^{-1}), whereas the four sites below this value (WD, RICE, SIR and SP) show a TAC increase or minimal change. Notably, RECAP and SIR have similar elevations and accumulation rates but contrasting HYSI and opposite TAC responses. As deglacial warming alone would increase TAC through its effect on Vc (Martinerie et al., 1992), the decreases at high-HYSI sites could indicate that the insolation imprint dominates regardless of accumulation rate. However, we cannot rule out that the apparent separation reflects a continuum rather than a true threshold. It is important to note that this apparent separation can result from the influence of local climate factors beyond HYSI and accumulation, such as changes in cloudiness, wind regime or elevation, on the TAC responses at low-HYSI WAIS sites. More specifically, deglacial warming affects TAC through two opposing mechanisms that largely cancel (increasing Vc but decreasing air density at close-off; Martinerie et al., 1992), to what we add elevation changes via Pc (Eq. 1) that are not accounted for here. High-HYSI EAIS sites likely thickened during LGM while low-HYSI WAIS sites thinned (Buizert et al., 2021, Grieman et al., 2024), which could also contribute to the contrasting TAC trends. We have replaced "dual-threshold" throughout the

manuscript with "regime-dependent behaviour" (Section 4.2 title) or "threshold-like behaviour" (Section 5.3), given the HYSI values of all sites explicitly in Table 4. Temporal TAC records from high-HYSI, high-accumulation sites such as Law Dome and more contrasted sites in coastal Greenland would be valuable to further test whether the insolation imprint persists across a wider range of surface conditions.

We added these limitations in the manuscript:

(546) "However, these interpretations face site-specific uncertainties: **deglacial warming affects TAC through two opposing mechanisms (increasing Vc but decreasing air density at close-off) that largely cancel (Martinerie et al., 1992), and elevation changes via Pc (Equation 1) are not accounted for here, as high-HYSI EAIS sites likely thickened during deglaciation while low-HYSI WAIS sites thinned (Buizert et al., 2021; Grieman et al., 2024), which could also contribute to the contrasting TAC trends in Table 4**. Ice flow variations may also influence TAC responses differently across sites (Table 3). While the insolation control on grain metamorphism is documented at cold, low-accumulation East Antarctic sites (Raynaud et al., 2007; Lipenkov et al., 2011; Raynaud et al., 2024), the physical mechanisms by which this imprint is preserved at warmer, high-accumulation, high HYSI sites such as Greenland remain poorly understood (Eicher et al., 2016)."

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 pore structure, but you can have the same pore 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 pore 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.

We have clarified the introduction on three points.

First, the D0 mechanism by which grain size influences close-off density and ultimately Vc is now explained in the introduction, as detailed and cited in our response to general comment 2, point (2).

Second, the multiple factors controlling near-surface grain size beyond insolation are now presented in the introduction, as detailed and cited in our response to general comment 2, point (3).

Third, the relationship between Vc and TAC is not purely geometric. This is now addressed in the introduction *(86)*: "**In stratified firn, denser layers can seal underlying layers before complete closure, trapping more air per unit volume (Stauffer et al., 1985; Martinerie et al., 1992; Mitchell et al., 2015),**

although this effect remains small at low-accumulation sites (Lipenkov et al., 2011; Fourteau et al., 2019). Air can also escape from closed bubbles towards still-open pores during closure, so that a longer closure duration leads to greater air loss and lower TAC (Fujita et al., 2009; Lipenkov et al., 2011). The Vc derived from TAC measurements therefore integrates these processes, and firn densification processes vary with climate conditions, resulting in different pore volume signatures across sites and timescales."

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.

We thank the reviewer for raising this important methodological point. The key distinction is between spatial and temporal correlations. Fig. 6b shows a spatial correlation between HYSI and temperature across sites, but the multiple regressions are temporal, at individual sites. At the temporal scale, HYSI and annual temperature respond to different orbital components: HYSI lacks a significant ~100 kyr periodicity whereas annual temperature is dominated by the ~100 kyr glacial-interglacial cycles features, because the summer temperature signal is largely cancelled in the annual mean by winter temperature, which is in anti-phase on precession timescales (Raynaud et al., 2024). This is why we use annual temperature rather than simulated summer temperature as a second predictor.

The independence of the two predictors is supported by the variance partitioning: the multiple regression R^2 is close to the sum of the two individual r^2 values (R^2 = 0.55 at EDC, 0.63 at Vostok), indicating minimal shared variance between predictors, as now reported in Section 2.6. We agree that regression coefficients should not be over-interpreted in terms of physical mechanisms, but the improvement in explained variance when combining HYSI and annual temperature justifies their joint inclusion in the regression (Gregorich et al., 2021).

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.

To clarify: the 16.7-kyr low-pass FIR filter described in Section 2.6 is applied consistently to all variables used in the filtered regressions, including TAC, HYSI, annual temperature, and accumulation rate. The mention of a "10 kyr" filter in a figure caption was a typographical error that has been corrected in the revised manuscript. We also note that, as discussed in our response to general comment 4, HYSI and annual temperature remain largely independent at the temporal scale even after filtering. Common variations do occur during deglaciations but with different amplitudes, which is why combining both predictors improves the explained TAC variance during these transitions.

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

This has been added.

7. There are numerous typos and grammatical errors throughout the paper. Please do a careful read through to correct these.

The manuscript has been carefully proofread and corrected throughout.

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 pore volume at close off?

This is now addressed in the introduction through the D0 mechanism described in our response to general comment 2: warmer temperatures increase D0, leading to higher close-off porosity and larger Vc, and hence higher TAC (Martinerie et al., 1992; Arnaud, 1997; Arnaud et al., 2000).

(85) “**The inverse relationship between local summer insolation and TAC has been documented at orbital timescales in East Antarctic ice cores (Raynaud et al., 2007; Lipenkov et al., 2011; Raynaud et al., 2024), in the Greenland NGRIP ice core (Eicher et al., 2016) and at South Pole (Epifanio et al., 2023). How the grain structure acquired near the surface is transferred to the close-off depth is however not established, and at least two mechanisms have been proposed. The first relies on D0 (the relative critical density of snow, ~0.6, marking the transition from grain-boundary sliding to power-law creep): since larger grains slide less easily past each other, whereas power-law creep is insensitive to grain size (Alley, 1987; Arnaud, 1997; Arnaud et al., 2000), coarse-grained firn formed under strong summer insolation reaches this transition at a lower D0, resulting in fewer pores per grain and a lower Vc at close-off (Arnaud, 1997), and thus closes off at a higher density, yielding a**

lower TAC (Freitag et al., 2004; Raynaud et al., 2007; Lipenkov et al., 2011; Raynaud et al., 2024; Epifanio et al., 2023).

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-o>”. 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)

We thank the referee for pointing this out. We apologise for this oversight and agree that this sentence was too closely paraphrased from Epifanio et al. (2023). We have removed this sentence and the mechanism is described in the paragraph stated above (previous comment) and in part 2(2) of this review. We have also carefully checked the rest of the manuscript to ensure no other passages are closely paraphrased from the source. Additionally, we have corrected the citation to refer to the final published version of Epifanio et al. (2023) rather than the preprint.

102: “surface-driven”: be more specific

We have replaced "surface-driven metamorphism" with "insolation-driven near-surface metamorphism" at line 102.

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

Figure 1, as already cited, surface temperature comes from QAntarctica (Matsuoka et al., 2021) and Qgreenland (Moon et al., 2023; Fisher et al., 2023) mapping environments. As stated, at line 226, the data are available in Table 1. We made it more clear that references are available there too: **(see table Table 1 for data and references).**

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

We are aware that these values could be estimated from DEMs or climate models, but we have already compiled field-based pressure and accumulation measurements for these sites, which will be published in a separate study (as already noted in the Table 1 caption). We chose to keep these values as "unpublished" rather than substitute them with model-derived estimates, to remain consistent with the field-based approach used throughout Table 1.

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.

We have added explanatory text to the Table 2 caption.

258: remove “TS = TC” – redundant

We implemented this change.

320: “interesting”: what is interesting, and why?

"Interesting" was an unnecessary value judgement. We have removed it from the sentence in the revised manuscript.

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?)

We agree this cross-referencing between panels was not sufficiently clear. We have relabelled the columns in Fig. 4b as "HYSI+acc." and "HYSI+temp." to remove any ambiguity, and we have also clarified this in the figure caption.

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

The LOVECLIM simulations of half-year summer temperature (HYST) were prioritised for a subset of contrasting sites (GRIP, SP, NGRIP, EDC, Vostok, TALDICE and RECAP were initially targeted, following Raynaud et al., 2024), but simulations for DF and TALDICE could not be completed in time for this study.

General: Be consistent with regression formatting throughout paper: lowercase r/r2 is typically used for a single linear regression, and uppercase R/R2 is for a multiple linear regression. (e.g., line 340 uses lowercase r, and table A2 uses uppercase R.)

We implemented this change.

Figure 4: Related to general comment above: The low R2 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.

As already noted in the manuscript (line 350), low-resolution sites such as TALDICE and RECAP consistently show the weakest correlations across all parameters, because their temporal resolution is insufficient to resolve individual orbital cycles peaks. This is the primary source of unexplained variance in Fig. 4. for the very low r2.

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.

The mention of a "10 kyr filter" in the caption was a typo; a 16.7-kyr low-pass FIR filter is used consistently throughout, as stated elsewhere. MIS labels are now explicitly described as a separate element.

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.

Yellow boxes now indicate time intervals where, within a 15-ka sliding window, the Pearson correlation between measured TAC and the HYSI+Temperature model is at least 0.10 higher than that of the HYSI-only model. Green boxes indicate intervals coinciding with deglaciations.

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.

In the revised manuscript, the TAC-HYSI residuals are now shown in dark orange and the TAC-HYSI predicted signal is now in dark green.

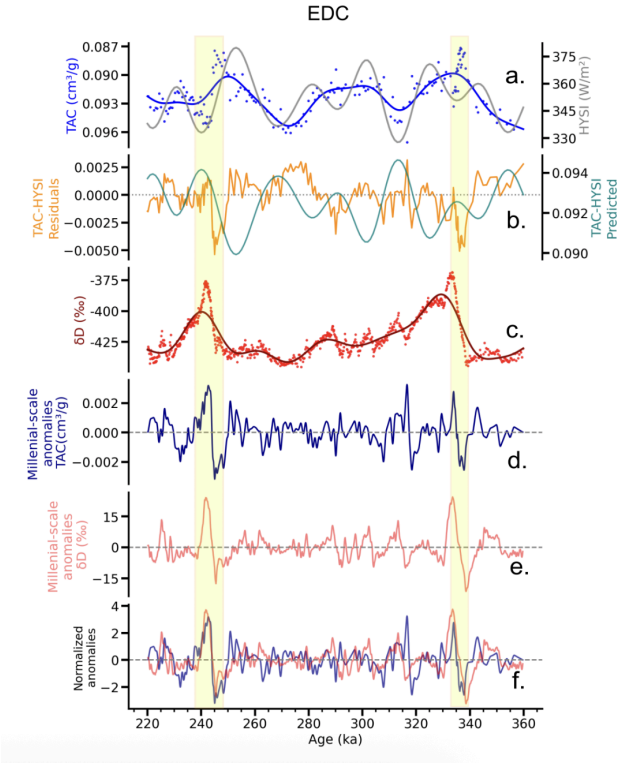


Fig. R1. Millennial-scale TAC changes. Implemented changes in panel b.

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?

In the revised manuscript, the yellow boxes now follow a data-driven criterion: they highlight time intervals where both millennial-scale TAC and δD anomalies simultaneously exceed ±1σ of their respective full-record distributions.

In the caption: **"Yellow boxes indicate time intervals where millennial-scale TAC and δD anomalies simultaneously exceed ±1σ of their respective full-record distributions, highlighting periods of coherent millennial-scale variability in both records."**

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

This sentence has been replaced in the revised manuscript. The spatial Vc–HYSI correlation within the EAIS is now explicitly interpreted as indirect, reflecting the strong temperature–HYSI covariation specific to the EAIS geography rather than a direct insolation effect on firn densification. The revised manuscript now reads (Section 4.2): *(409)* "**The Vc–HYSI relationship observed within the EAIS therefore results from the combination of the Vc–temperature relationship common to all sites and the temperature–HYSI relationship specific to the EAIS, and cannot be attributed to a direct insolation effect on firn densification.**"

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.

This is now stated in Section 4.2, addressed by the previous comment and in the concluding remarks:

(424) "**Together, these findings show that, at the spatial scale, Vc is dominated by mean annual temperature. Within the EAIS, Vc can also be parametrised empirically as a function of HYSI, but this relationship is indirect, as it reflects the strong temperature–HYSI coupling specific to this region, and cannot be extrapolated to Greenland or WAIS sites**."

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.

We thank the referee for catching this error. We have changed (Fig. A5.b.) to (Fig. A5.a.)

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

This sentence has been replaced in the revised manuscript at the beginning of section 4.2, to introduce the discussion on the geographical differences: (429) "**These geographical discrepancies could point to different firn densification regimes, suggesting that present-day controls on Vc operate through different physical pathways depending on local climatic conditions. Vc increases with mean annual temperature across all sites, with WAIS, EAIS, and Greenland sites following a consistent single relationship (Fig. 4.b), with a higher critical density D0 at warmer sites (Martinerie et al., 1992; Arnaud et al., 2000). But Vc correlates with HYSI within only the EAIS and Greenland (Fig. 4.e): WAIS sites do not follow this relationship and show higher Vc than EAIS sites with similar HYSI (Fig. 4.d). Once the temperature dependence is removed, no significant Vcr–HYSI relationship remains in any region (Fig. A5). This raises the question of why HYSI and temperature yield a common Vc relationship within the EAIS but not across regions. To understand these spatial differences, we examine how temperature, accumulation, and HYSI covary across our compiled sites (Fig. 5).**"

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 Vc, shouldn’t TAC have a stronger correlation with accumulation?

We agree and have removed the claim that accumulation dominates below a HYSI threshold. We note that the low correlation between TAC and accumulation at Greenland sites in Fig. 4 does not rule out an accumulation control: at NGRIP, the accumulation signal in TAC is transient, occurring during abrupt D-O events rather than continuously across the record (Eicher et al., 2016), which would not produce a strong overall correlation. The spatial and temporal analyses are also not directly comparable, as HYSI, temperature and accumulation all covary spatially. The contrasting TAC responses of sites with similar accumulation but different HYSI, such as RECAP and SIR (Table 4), suggest that high insolation may imprint firn grain structure even where snow burial is fast, though this requires further investigation (See section 4.2 in the revised paper).

(475) **"This separation is however not straightforward at all sites: temporal changes in surface elevation, accumulation or wind regime can modulate or override the insolation and temperature signals at times, as suggested by the different mechanisms proposed at SP and NGRIP (Eicher et al., 2016; Epifanio et al., 2023; Edwards and Epifanio, 2025)."**

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

We have removed the term "dual-threshold", which was insufficiently justified. See our response to general comment 2 (4) above for the revised interpretation.

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.

We agree that regional regression lines based on a few points should be interpreted with caution. The main result of Fig. 6 does not rely on these slopes but on the clear regional clustering visible in the data itself. We have modified the text in the revised manuscript to describe regional groupings rather than quantitative slopes, and added a note in the figure caption acknowledging the limited statistical robustness of the regional regressions. Note that the addition of NEEM and GISP2 increases the number of Greenland sites compared to the original manuscript.

(407) “**Greenland sites (n = 7) and WAIS sites (n = 4) do not follow the EAIS regression, but instead form distinct regional clusters, indicating that the Vc–HYSI relationship observed in East Antarctica does not extend to other polar regions.**"

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 R2 will be the same, but it is probably more accurate way to think about the regression.

We have put temperature as a function of insolation in the figure (Fig. R2).

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.

We have changed this in the figure (Fig. R2). Please note that we also changed this for the Vc spatial-scale analysis figure (Fig. R3).

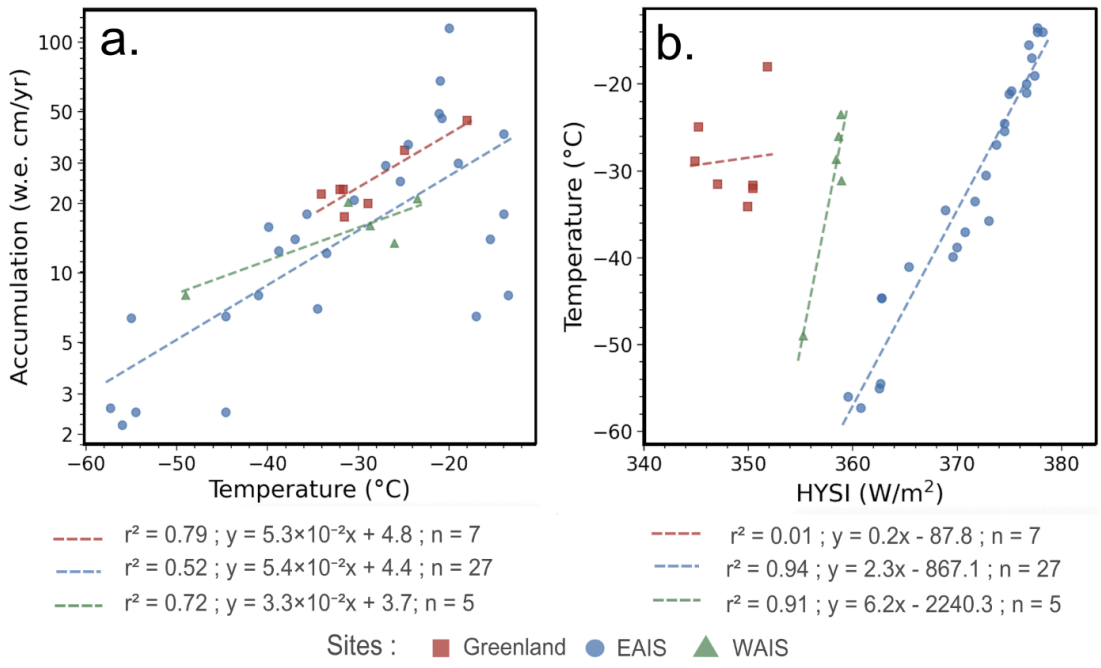


Fig. R2: Scatter plots showing present-day annual temperature, accumulation rates and HYSI relationships.

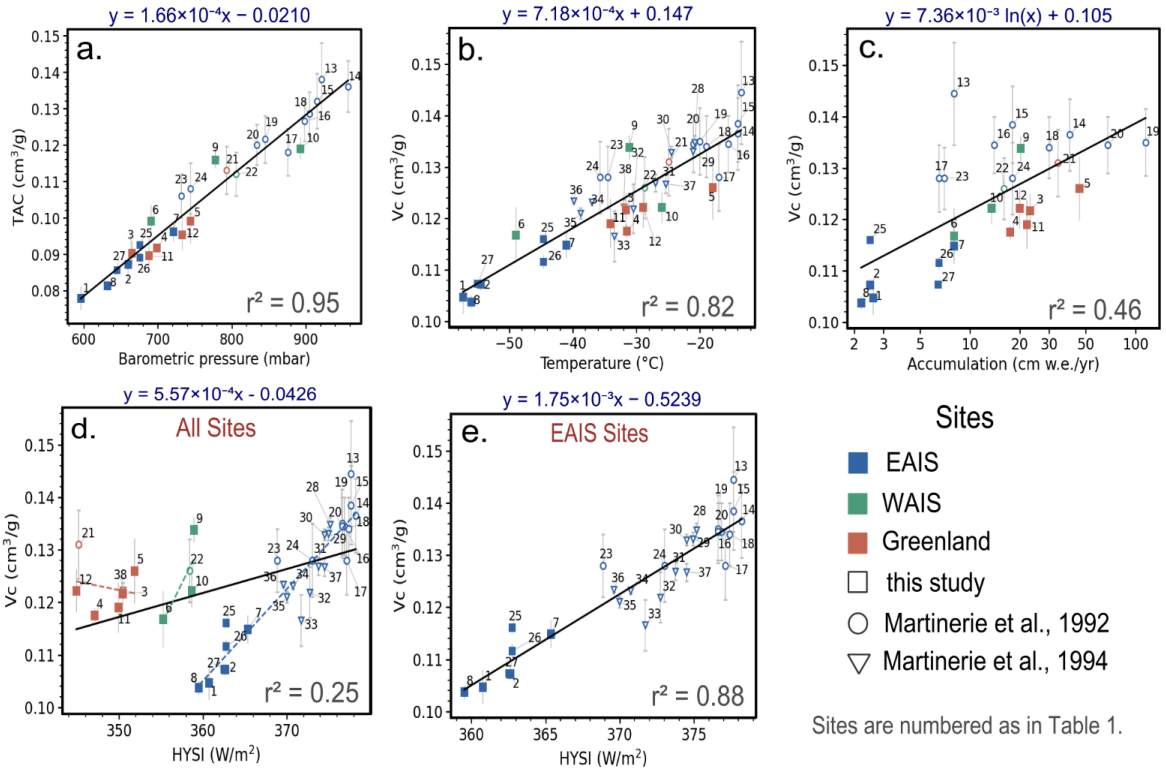


Fig. R3: Scatter plots showing Vc and surface climate parameters across sites.

Finally, please also note that, compared to the original manuscript, NEEM and GISP2 have been added to the spatial and temporal analyses (holocene analysis). This addition may result in minor differences in some regression values reported in the revised manuscript relative to those cited in our responses below. Additionally, a minor error in the insolation index computation was corrected, which affects absolute HYSI values and some linear regression (not significantly) but does not alter any of the conclusions drawn in the manuscript.

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