Reply to Review 1

We thank Reviewer 1 for the constructive and insightful comments. Here is our point-by-point reply. The review comments are in black and our replies are in red.

The authors use existing ice core data and new climate model simulations to provide a new interpretation of the ice core total air content (TAC) proxy. Based on a model-data correlation analysis, they suggest that the EDC TAC record reflects local summer temperature. The idea is interesting if corroborated, and the paper is overall well-written.

Main comments:

(1) After reading the paper, it was not exactly clear to me what the authors believe to be the mechanism for the TAC-summer temperature relationship. Which of these is it:

(a) Insolation controls summer temperature. Summer temperature controls TAC.
(b) Insolation controls summer temperature. Insolation controls TAC.
(c) We cannot distinguish between scenarios (a) and (b) based on the available evidence
(d) Something else altogether.

Please indicate which of these options describes your understanding best, and please clarify this in the manuscript as other readers may have the same confusion as I do.
We mean option (a). The link between insolation and TAC is through summer temperature. In the introduction (p.2, lines 59-62) of our manuscript we have explained that the anti-correlation between local summer insolation and TAC can be attributed to a mechanism where the local summer insolation is controlling the near-surface snow temperature and temperature gradients during summer time, which affects the near surface snow structure and hence TAC. In section 6 we discuss the possible mechanisms linking TAC and local summer temperature. We propose a mechanism based on snow/firn physics, which could explain the strong anti-correlation observed between TAC and the mean summer surface temperature. Nevertheless, a numerical model, which takes into- account the successive mechanisms involved between the surface snow and the closure of pores, is still required. Such model would explain that time periods with higher summer insolation and summer temperature will promote a coarser-grained snow structure, a lower critical density of snow and then a reduced TAC at pore closure.
We will stress more about this point in the revised manuscript.

(2) The analysis relies on correlation analysis between the EDC TAC record, the mean insolation over the local astronomical half-year summer, and the modeled summer temperature. The correlation between TAC and modeled summer T is stronger than the correlation between TAC and insolation alone. Could this be because both the TAC and summer temperature have 100ka power, whereas the insolation metric does not? Presumably the summer T has 100ka power because the GHG forcing that was applied.

I would request the authors include additional figures to clarify these questions better.
In Figure 1 could you please add the time series and spectrum also for the (i) mean insolation over the local astronomical half-year summer, (ii) modeled summer temperature, (iii) modeled mean-annual temperature? This could also be a separate figure of course.
Considering the suggestion of the reviewer, we will add a separate figure showing the time series and spectrum for the mean insolation over the local astronomical half-year summer insolation and the modeled summer temperature (see Figure R1 below). We are not in favor to add a wavelet/transform figure for the simulated annual mean temperature from the 10x accelerated simulation because the annual mean temperature is more strongly affected by ice volume (which is not the case for the mean summer temperature as explained in Section 5), but ice sheet changes were not considered in our 10x accelerated simulation, so a wavelet/spectrum figure of this simulated annual temperature would be misleading. However, the time series of the simulated annual

mean temperature is shown in Figure R3 and this figure will also be included in the revised version of the manuscript.

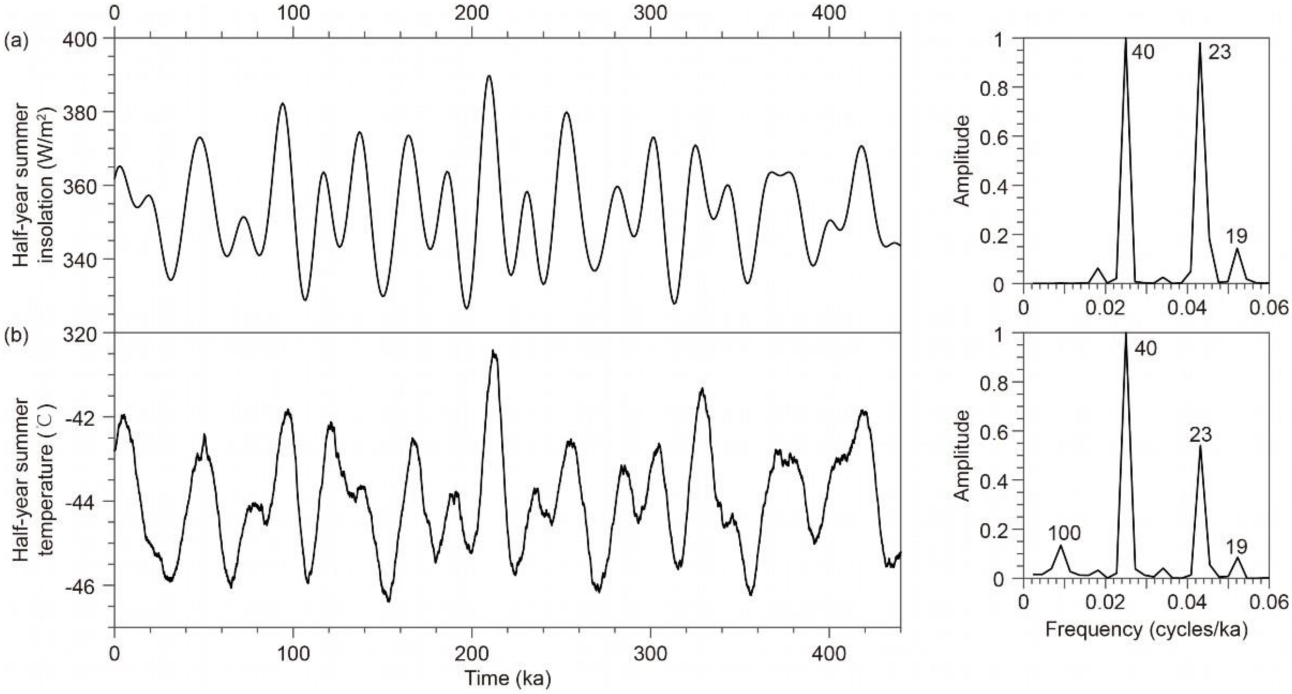

Figure R1. Time series and spectrum for (a) the mean insolation over the local astronomical half-year summer and (b) the modeled summer temperature.

In Figure 2 could you please add a panel with the correlation between Half-year summer insolation and the modeled summer T

Yes, we will add in Figure 2 a panel (see Figure R2 below) with the timeseries and correlation of mean summer insolation and simulated summer temperature.

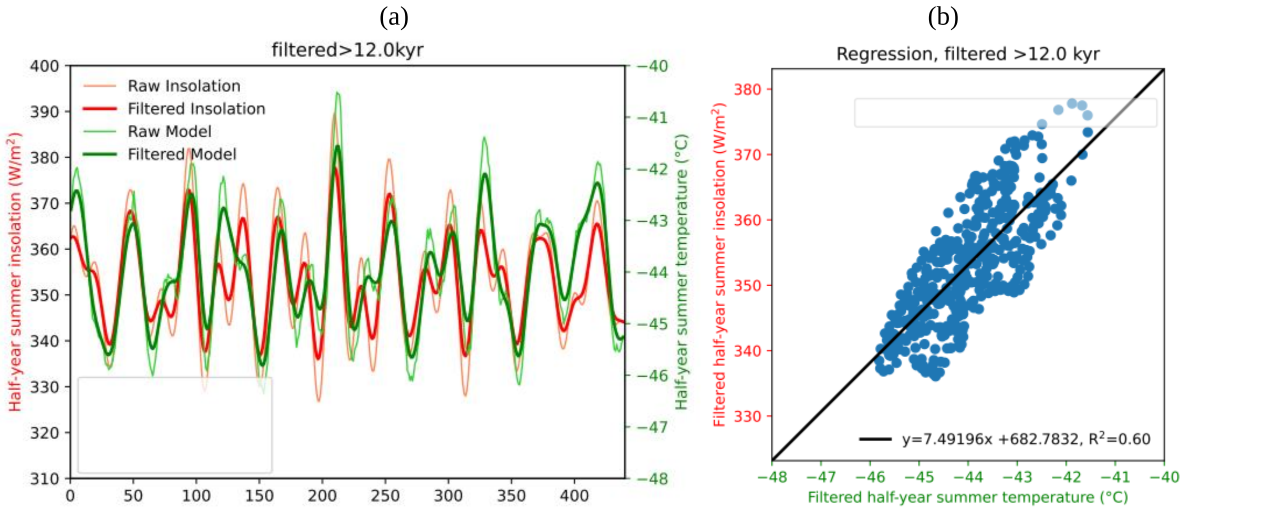

Figure R2. (a) Raw and filtered timeseries of the half-year summer insolation and the half-year summer temperature, (b) correlation between the two filtered timeseries.

We will also add a panel in Fig. 4 to show the wavelet analysis of the mean summer insolation (see Figure R3 below). The reviewer is right in the fact that there is no 100-ka cycle in the mean summer insolation at 75S (Figure R3c), and the simulated summer temperature is driven by both insolation and GHG although insolation plays a dominant role. This is why the 100-ka cycle is still apparent in the simulated summer temperature due to GHG forcing.

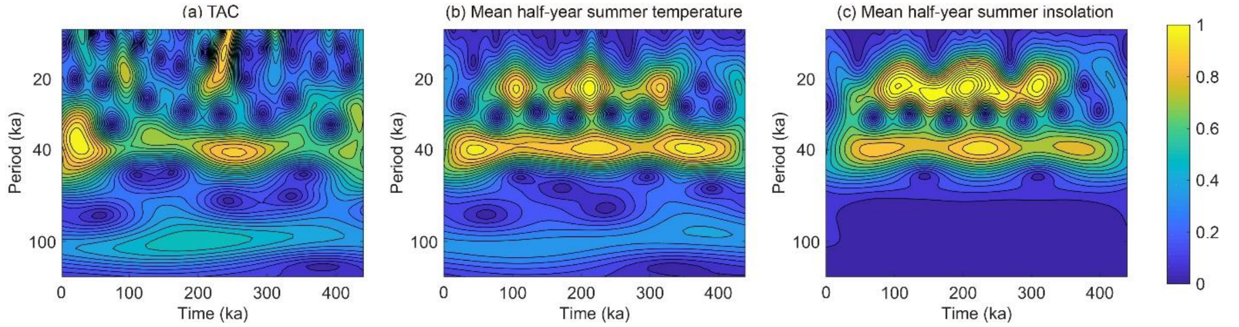

Figure R3 (updated Figure 4). Continuous wavelet transforms of (a) the low-pass (> 12 ka) filtered TAC record, (b) simulated mean half-year summer temperature at the EDC site and (c) mean insolation during astronomical half-year summer at 75°S.

(3) Could you provide a figure that compares the accelerated simulations with the non-accelerated simulations for both summer and annual-mean temperatures? It seems important to assess how well these agree with one another.

We will add Figure R4 below in the revised manuscript to show the good agreement between the accelerated and non-accelerated simulations for both summer and annual-mean temperatures.

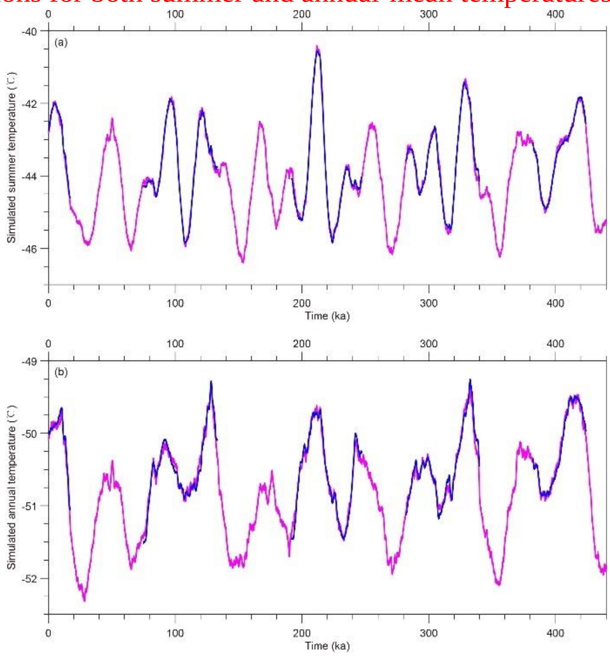

Figure R4. Comparison of 10x accelerated (purple) and non-accelerated (blue) simulations for the simulated mean half-year summer (October to March) temperature (upper panel) and mean annual temperature (lower panel) at EDC. Both are OrbGHG simulations without considering ice sheet changes. 1000-year running mean is plotted.

(4) How well does the model succeed in simulation the modern-day EDC annual temperature cycle? That may be important in assessing how well it can simulate the seasonal contrast back in time. Does the model simulate the Antarctic inversion, or is this not resolved in an EMIC? My sense is that the success of the model in simulating the seasonal temperatures will be closely linked to how well it can simulate the strong Antarctic

inversion. The inversion is likely to respond in different ways to ORB, GHG and ICE forcings, which may bias the model output. Please add some discussion and/or caveats to the paper on this aspect of the modeling.
Figure R5 below shows the comparison of the modern climate in Antarctica simulated by LOVECLIM1.3 with observation. LOVECLIM1.3 reproduces quite well the spatial pattern and the magnitude of surface temperature over Antarctica in winter, summer and annual mean (Figure R5a). It is slightly cooler in western Antarctica in the model probably related to its low resolution. The seasonal temperature cycle at the EDC site is also well reproduced by the model (Figure R5b). The model also simulates the Antarctic inversion (Figure R6). To address the comment of the reviewer on the potential impact of Orb, GHG and ICE forcings on the inversion, the mode results at a few selected dates are plotted in Figure R6. It shows that these forcings can slightly affect the vertical temperature distribution, but they do not alter the inversion. Figures R5-6 and related discussions will be added in the revised manuscript.

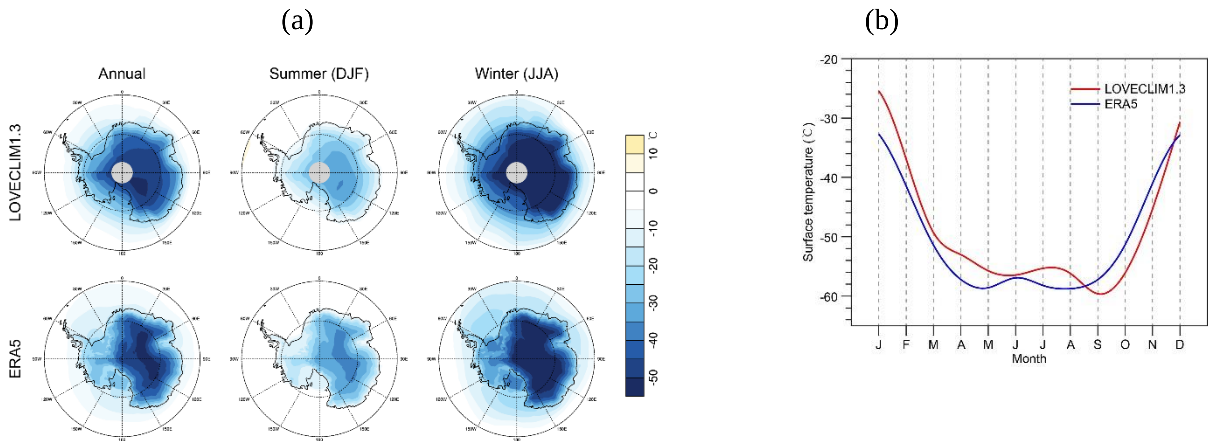

Figure R5. Comparison of the 1971-2000 mean climate in Antarctica simulated by LOVECLIM1.3 and the ERA5 reanalysis (https://cds.climate.copernicus.eu/cdsapp#!/home). (a) Surface temperature for the mean annual, summer (DJF) and winter (JJA); (b) The seasonal cycle of the surface temperature at the EDC site.

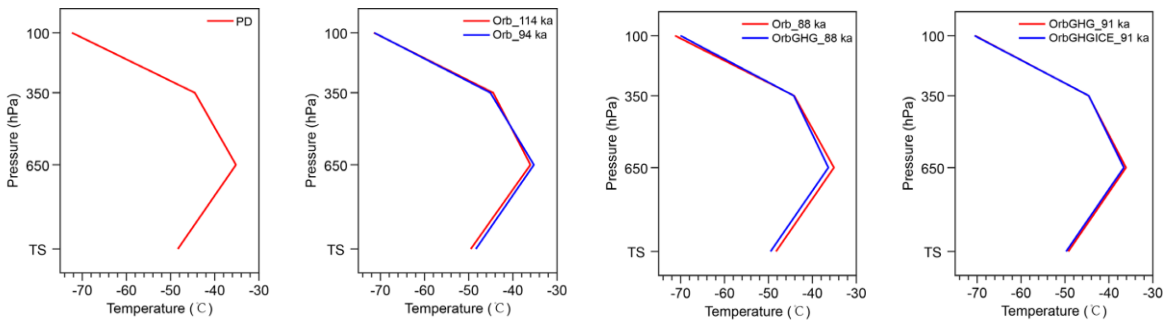

Figure R6: Temperature profile simulated by LOVECLIM 1.3 for annual mean. From left to right: the 1971-2000 mean; effect of obliquity, with small obliquity at 114ka (22.38°) and large obliquity at 94 ka (24.256°), both having similar precession; effect of GHG, with CO_2 being ~70 ppmv less in OrbGHG_88ka than in Orb_88ka; and effect of ice sheets at 91 ka, with the NH ice volume being ~20 million km^3 larger in OrbGHGIce_91ka than in OrbGHG_91ka. TS means surface.

Minor comments:

Line 20: "summer insolation" is vague in this context. Do you mean "mean insolation over the local astronomical half-year summer" again, or something else?

Yes, it means “mean insolation over the local astronomical half-year summer”. To avoid confusion, at line 20, we will change “local summer insolation” to “mean insolation over the local astronomical half-year summer”. In the main text, to avoid frequent repetition of such a long sentence, we use “mean summer insolation” to represent “mean insolation over the local astronomical half-year summer”, as stated in line 137 in our original manuscript.

Line 25-26: “Mean annual temperature”: technically it would be the “precipitation-weighted condensation temperature”
We would like to keep the beginning of the introduction more generic and less technical for a better readability for most readers, so we suggest to use “Mean annual temperature” in Line 25-26, but in line …. at the beginning of section 5, we will mention that it is “precipitation-weighted condensation temperature”.

Line 35: I think Southern Ocean sea ice extent is another key parameter here
Thanks for the suggestion. Southern Ocean sea ice extent will be included in this sentence.

Line 83: and “a” detailed description….
Yes, corrected.

Line 91-98: why not change SH ice sheets? Those would be the most important for EDC, no?
It would be ideal to change also SH ice sheets, but there is a lack of well reconstructed/simulated geometry of the SH ice sheets for our study period that could be easily prescribed in our model. We agree that it would be interesting to test the impact of the Antarctica ice sheet change on the EDC TAC record in future studies. We will add that idea in the perspectives of the revised manuscript.

Figure 1B: the magnitude of EDC glacial-interglacial temperature change is uncertain, as per the discussion in the paper later on. It may be better to just plot d2H or d18O instead.
We will change it to the EDC δD record in the revised version. It does not change the spectral characteristic of this proxy.

Line 114 -115: I understand what is meant here, but the language is imprecise as there are not different “types” on insolation. Maybe rephrase to: “Comparing a proxy record with insolation is not necessarily straightforward because different metrics of insolation exist and their relationship with climate is not always clear”
Thanks for the proposition. It will be changed to metrics.

Line 126: perhaps remind the reader that 75 degrees S is the latitude of EDC.
Yes, the sentence will be changed to “the mean insolation over the astronomical half-year summer at 75°S (the latitude of EDC).

Figure 2: Please add a third row of panels in which you compare the half-year summer insolation to the modeled half-year summer temperature. That way the reader can evaluate the relationship between these two key parameters better
Such a third row will be added.

Line 172-175: is it possible to evaluate how well LOVECLIM simulates the cited reconstructed seasonal temperatures from West Antarctica to validate the model simulations?
In Figure R7 below we plotted our simulated summer and winter temperatures at the WDC site against Fig.2 of Jones et al 2022. Our simulated summer temperature compares well with the reconstructed summer temperature in both the trend and the magnitude of temperature change. Both show that summer temperature in west Antarctica had an increasing trend from early to mid-Holocene and reached a maximum at ~4 ka BP followed by a decreasing trend. Our simulated winter temperature is quite different from the reconstructed one especially in the early Holocene during which our model simulates a cooling trend like in the ORBIT simulation of HadCM3, but the reconstruction shows a weak warming trend. According to the GLAC1D and ICE-6G simulations of HadCM3, using different ice sheet configurations could significantly influence the

simulated winter temperature at WDC. Another possible reason for the mismatch between our simulated winter temperature and the reconstruction is that our model has a relatively low sensitivity in response to CO_2 change so the simulated warming due to a small CO_2 increase in the early Holocene is too weak in our model, letting the orbital forcing dominate.
A brief introduction of this comparison will be added in our revised manuscript.

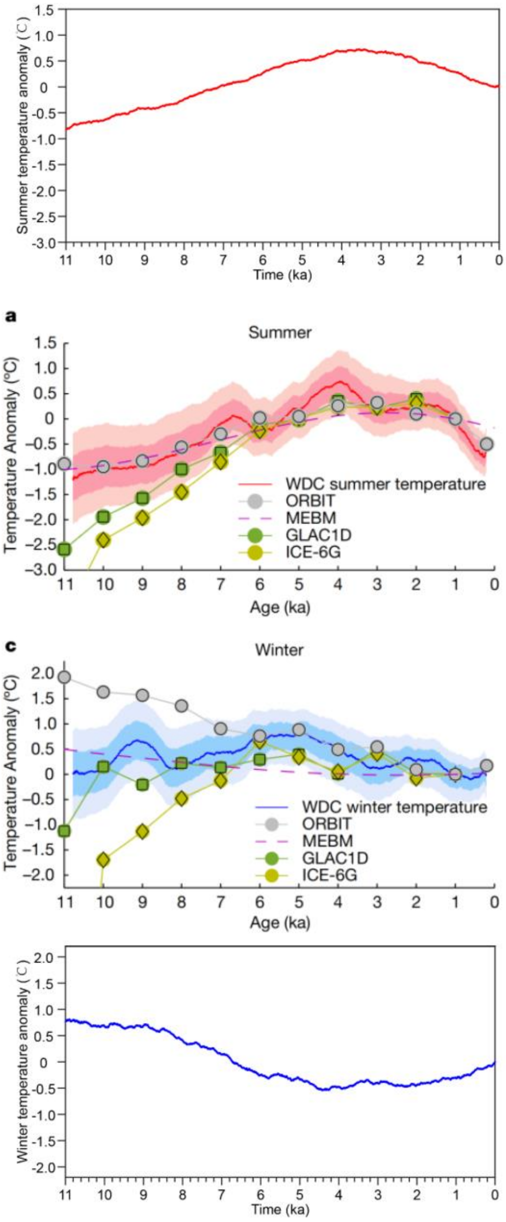

Figure R7. Top and bottom panels: Simulated summer and winter temperature anomalies at WDC by LOVECLIM 1.3 OrbGHGIce transient simulation; Two middle panels: Fig. 2 of Jones et al. 2022.

Line 181-182: Please provide more justification for this statement. Is this based solely on the increased correlation coefficient?
The linear coefficient between TAC (filtered) and simulated summer temperature indicates that about 58% of the TAC variability observed at EDC over the last 440 ka is explained by the half-year summer temperature. Nevertheless, this statement is based solely on this correlation coefficient, but there are more factors affecting TAC, as mentioned in the paper. Please see also more explanation in our reply to main comment 1.

Line 186: “This is information….” (information is singular)
Done.

Line 185-191: Can you clarify this discussion? Is TAC just a proxy for summer temperature, or is summer temperature actually driving TAC variations? The discussion here still assumes that TAC is driven by insolation directly.
We do not mean that TAC is driven by insolation directly. As explained in our reply to main comment 1, we mean that local summer insolation controls the temperature and the vertical temperature gradient in near-surface snow. Then, the surface snow structure is physically affected by changes in summer temperature. This surface structure change driven by summer temperature controls TAC. So, TAC can be used as a proxy for summer temperature. Furthermore, more detail on the physical mechanisms is explained in section 6. Our view is: insolation controls summer temperature, and summer temperature controls TAC, so TAC can be used as a proxy for summer temperature.
This discussion will be clarified and elements will be added in the revised manuscript.

Figure 5: For comparison, can you please also plot the mean annual temperatures from the accelerated simulations that span the full 440ka period?
As mentioned in our reply to main comment 3, a new figure will be added on the comparison of the annual mean temperature between the accelerated and non-accelerated simulations.

Line 217: “Response of Antarctic climate” (remove the ‘a’ at the end of Antarctica)
Yes, corrected.

Line 221-222: In light of the discussion that follows, perhaps rephrase in more neutral language? For example: “One may also note that the magnitude of the temperature change between glacial and interglacial is significantly smaller in the model as compared to the reconstruction”
OK, we propose to rephrase in the revised manuscript the sentence such as “One may also note that the magnitude of the temperature change between glacial and interglacial is significantly underestimated in the model as compared to the reconstruction”.

Line 223: It is my understanding that Antarctic elevation changes would exacerbate the problem, as EDC likely had lower elevation during the LGM.
It is an interesting point that deserves to be further studied. In principle, during glacial periods, the reduced surface accumulation rate leads to lower surface elevation (see Figure R8 below taken from the appendix of Raynaud et al., 2007). But there are also dynamical effects which make these reconstructions of surface elevation uncertain. Surface elevation changes affect our study in two different ways. First, TAC should be corrected for atmospheric pressure changes to get a record of porosity at close-off. Some of these atmospheric pressure changes are due to variations in surface elevation, another part might be due to change of atmospheric conditions (like the temperature of the air column). Because the reconstruction of surface elevation changes are uncertain, we have chosen to not correct for this effect. Second, surface elevation changes should also be taken into account in our simulation of summer or annual temperature at EDC by LOVECLIM. Unfortunately, in our climate model, the Antarctic temperature is fixed and we did not account for surface elevation changes. We could apply an a posteriori correction for surface elevation changes, but because the temperature variations (either annual of in summer) are probably under-estimated, applying this a posteriori correction would have a too strong influence. Finally, we should note that these two elevation corrections (for TAC and for the climate model) go in the same direction: during glacials, a corrected summer temperature would be warmer, and the corrected TAC of the ice would be smaller, so there is a chance that these two corrections would cancel each other and that the overall correlation between TAC and modeled summer temperature would not be much affected.
We will include this discussion in the corrected manuscript.

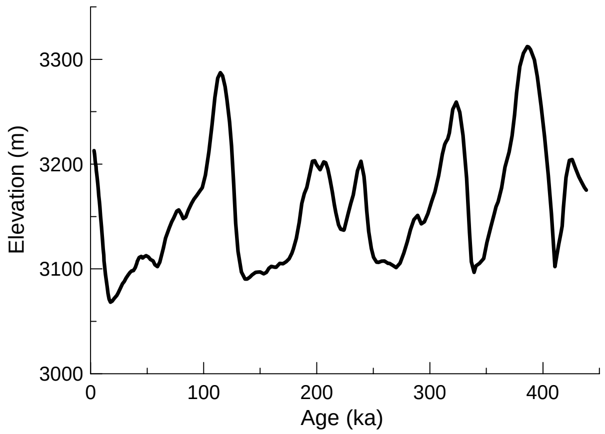

Figure R8. Simulated elevation changes at EDC from Raynaud et al. 2007.

Line 239-251: This is really excellent, and very instructive. What is the role of winter temperature? Is it fully controlled by GHG, such that in the annual average both GHG (winter) and ORB (summer) show up?

Thanks for this inspiring question. The wavelet analysis of the simulated mean half-year winter temperature at EDC (Figure R9 below) show very strong ~20-kyr cycle and an obvious ~100 kyr cycle, but the ~40-kyr cycle is very weak. The 100-kyr cycle results most probably from the effect of GHG. The very strong 20-kyr cycle but very weak 40-kyr cycle is quite intriguing. As far as insolation is concerned, the low-latitude insolation is dominated by the ~20-kyr precession cycle. As the solar energy received in Antarctica is very weak during local winter, the strong 20-kyr cycle in the simulated winter temperature could reflect a strong effect of the low latitude climate on the Antarctica temperature during austral winter, via for example meridional ocean and atmosphere heat transport. Figure R9b shows a high, negative correlation between the simulated EDC winter temperature and precession. This indicates that the EDC winter temperature is strongly affected by boreal summer insolation in low latitudes (small precession parameter leads to high boreal summer insolation and vice versa). It is also shown in Yin and Berger (2012) that during some interglacials such as MIS-5e, -15 and -17 which are characterized by very strong boreal summer insolation, a strong warming could be induced over Antarctica during austral winter, a warming which is even stronger than in many other regions due to polar amplification.

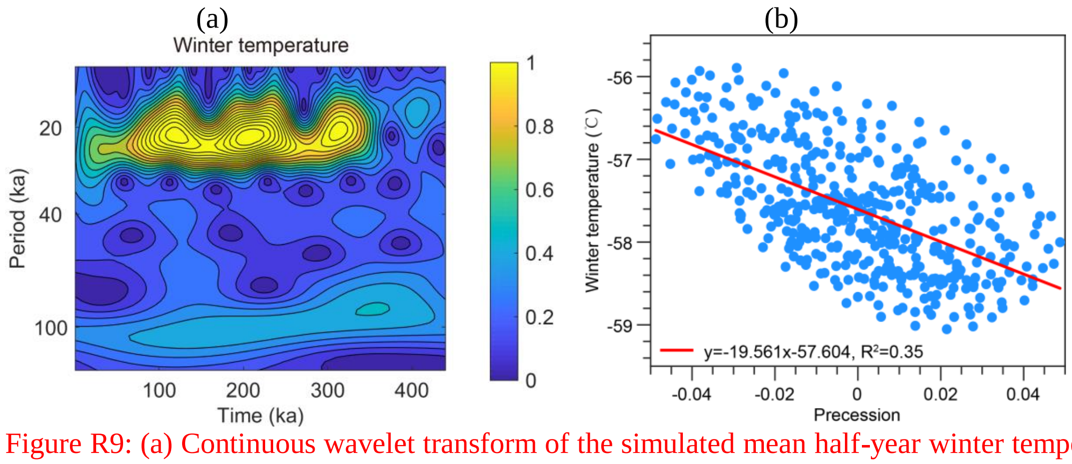

Figure R9: (a) Continuous wavelet transform of the simulated mean half-year winter temperature at the EDC site from the 10x accelerated OrbGHG simulation, and (b) Correlation between this winter temperature and precession.

In our manuscript, the relative effects of insolation, GHG and NH ice sheets on the summer and annual temperature at EDC have been studied using the Orb, OrbGHG and OrbGHGIce experiments for the period 133-75 ka. The same experiments are used here to analyze their effects on the winter temperature. Figure R10 below shows that similar to what happens to the summer temperature, orbital forcing plays a dominant role also on the winter temperature at EDC. As explained above, the winter temperature at EDC is actually strongly driven by precession and boreal summer insolation, so on precession timescale, the orbitally-induced temperature variation in winter is in anti-phase with the summer temperature which is strongly driven by austral summer insolation. This anti-phase relationship leads to a strong weakening of the orbital signal especially the precession signal in the mean annual temperature (see Figure R10 below), making the effect of GHG and ice sheets more pronounced and leading to strong glacial cycles in the mean annual temperature.

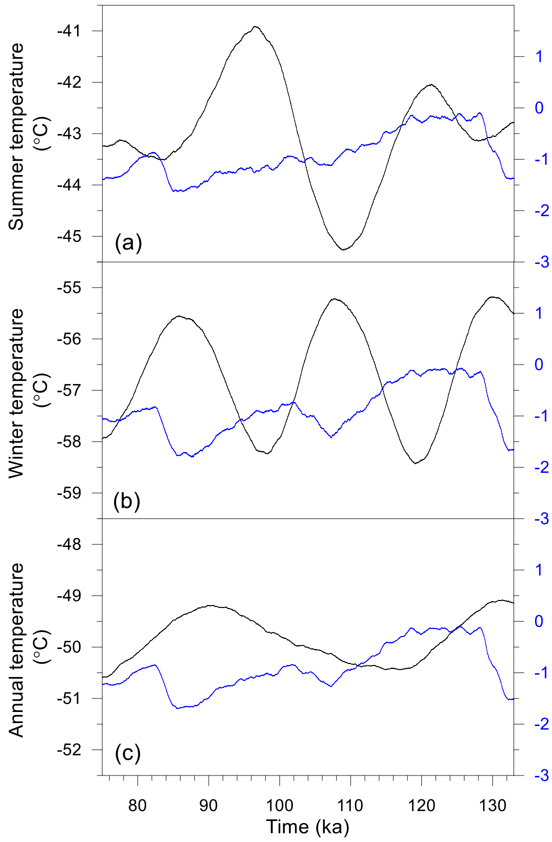

Figure R10: (a) Mean half-year summer (October to March) temperature, (b) mean half-year winter (April-September) temperature and (c) mean annual temperature at the EDC site from the LOVECLIM1.3 transient simulations without acceleration for the period 133-75 ka. Black curves are the results from the Orb simulation under only orbital forcing; blue curves are the difference between OrbGHGIce and Orb simulations, showing the joint effect of GHG and ice sheets.

These discussions and figure will be added in the revised manuscript.

Line 249: The GHG effect is 1 degree on both mean-annual and summer temperatures, so it is actually the same. Perhaps better to phrase it as the *relative* importance of GHG is bigger on annual-mean.
Thanks for the suggestion. It is indeed better to use "relative". The sentence will be changed to "These clearly show that as compared to insolation, GHG and NH ice sheets have relatively weaker effect on the summer temperature but they have relatively stronger effect on annual mean temperature".

Fig, 6: The model appears to simulate a seasonal temperature cycle of around 16 degrees (twice the offset between summer and annual-mean). The observed seasonal cycle is closer to 40 degrees (from -25 in summer to -65 in winter). Please comment. How well does the model capture the seasonal variations in the modern day?

Please see our reply to main comment 4. The model captures quite well the seasonal temperature variations at EDC. New text and figures will be added in section 2.2 to comment on the performance of the model for the modern seasonal temperature cycle at EDC.

Section 6: can you me more clear in your usage of the words snow and firn? How do you define these? Except for the very fresh surface snow, anything older than 1 year I would call firn. But it appears you use the terms differently.

Indeed, there are no generally accepted definitions of snow and firn in the literature, and often these two terms are used interchangeably. Some papers on snow/firn metamorphism use only the term "firn" (e.g. Alley, 1987) while others, conversely, use only "snow" (e.g. Maeno & Ebinuma, 1983). The definition adhered to by the reviewer is more commonly found in the American literature (see, for example, Cuffey & Paterson, 2010, The Physics of Glaciers). However, in our work we follow the definition introduced by Anderson and Benson (1963), who associate snow and firn (névé in the original) with two different stages in the transformation of dry snow into bubbly ice. These two stages differ in the mechanisms that dominate the densification process: the particle rearrangement controlled by linear-viscous grain-boundary sliding (GBS) dominates in snow, whereas the power-law creep (PLS) dominates in further densification of firn. The snow-firn transition, viewed as a transition between the two densification regimes, is identified by the bending of the density-depth profile, which indicates a decrease in compaction rate and is usually observed when the critical snow density, equal to about 550 kg/m3 (which corresponds to a relative density of D_0 = 0.6 and is not a constant!), is reached. At this density, the number of contacts per grain approaches 6–7, thus making the sliding impossible. "Recognition of the critical density introduces the possibility of making a physical distinction between snow and firn, or névé" (Anderson & Benson, 1963). This conceptual framework has been adopted in a number of papers dealing with the physical modeling of snow-to-ice transformation (e.g. Arnaud et al, 1998, 2000) and the results of these works have been used in (Raynaud et al, 2007; Lipenkov et al, 2011) as a basis for discussing a possible mechanism of the effect of summer temperatures on the pore volume of firn at the close-off depth. We will clarify the usage of these two words in the revised manuscript.

Line 273: "ice grains" instead of "snow grains"?

Yes, ice grains would be the correct wording.

Line 274: "upper few meters of THE snow column"

Done.

Line 278: the critical density you refer to here is around 550 kg/m3, correct? Or do you mean the critical density for pore closure? Please specify for clarity. What depth is this at EDC?

Yes, the first is correct. We also felt that the whole sentence was a bit misleading. To make the text clearer, we will change it as follows in the revised manuscript:

"According to the model proposed by Arnaud (1997) for the Antarctic ice sheet, the pore volume at close-off, Vc, should increase with the mean annual surface temperature through the competing densification mechanisms: higher temperature leads to an increase in the relative critical density at the transition between snow and firn (D_0~0.6, reached at a depth of about 25m at EDC), which in turn implies a greater proportion of the ice-grain edges occupied by pores at close-off, and hence a larger Vc."

Line 279-280: is it no correlation or a poor correlation? Has to be either, can't be both.

It is poor correlation. It will be changed in the revised manuscript.

Line 281: there is no surface temperature record. Do you mean a correlation between TAC and the modeled summer temperature

Yes, we mean a correlation between TAC and the simulated mean summer surface temperature. It will be modified.

Line 283: what is the number of pores per grain? Simply the ratio of the nr. of pores to the number of ice crystals?
Yes. We will define this term in the revised version.

Line 284: do you mean the critical density of firn?
The critical density at the transition between snow and firn. Please see our replies above.

Line 284: This transition between GBS and PLC occurs at around 550 kg/m3, correct? Please specify for clarity. What depth is this at EDC?
Yes, this is correct. Please see our explanations and replies above.

Line 304-307: See my main comment, I would appreciate more clarity on what the authors think the causal relationships between insolation, summer temperature, and TAC are.
The causal relationships between insolation, summer temperature and TAC could be explained by that (1) local summer insolation is controlling the near surface summer temperature conditions, (2) this summer temperature conditions affect the near surface snow structure and then (3) the continuous nature of snow transformation until the trapping of the air in ice, i.e. TAC. So, the mechanism for the EDC TAC-summer temperature relationship is that insolation controls summer temperature and summer temperature controls TAC.

Line 308: Ultimately this insight does not come from the TAC record, but from the climate model. Do you agree? Without the climate model, one would not have interpreted TAC as a summer temperature proxy.
We don't totally agree. In Raynaud et al. 2007, TAC was already proposed to reflect summer temperature, and a reasonable physical mechanism could be proposed (also in the present manuscript) to explain how summer temperature could affect TAC. In this manuscript, the good comparison between TAC and the simulated summer temperature confirms the TAC could be a proxy for summer temperature. TAC itself has very different spectral characteristics from the δD which is used as an annual temperature proxy. So the statement of line 308 could be made based on both the TAC record and the model results.

References:

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