Reply to Review 2

We thank Reviewer 2 for the constructive and insightful comments. Here is our point-by-point reply. The review comments are in black and our replies are in green.

General comments:
The manuscript explores the idea that there are no good reconstructions of summer temperature and suggests that TAC could be used as a proxy for summer temperature in Antarctica. The authors use a previously measured record of TAC from the EDC ice core, which spans the last 440 ka. They use the EDC TAC data to compare with a climate model simulation of summer temperature to make the case that TAC could be used as a summer temperature proxy.
Overall, the manuscript is good, and the idea for using TAC as a summer temperature proxy is important, especially if it could be shown to also be used on other ice cores (a subject for future research). I recommend its publications after the authors consider the below comments:

Specific comments:
Overall, it is difficult to follow if the authors main point is that the TAC is controlled by summer insolation or summer temperature, or both. The authors clearly describe that both summer insolation and summer temperature are well anti-correlated with TAC. To make this clearer, I would recommend reorganizing the conclusion, highlighting their main argument at the beginning of the first paragraph.
Reviewer 1 made the same remark. Here we use the same reply to Reviewer 1's main comment 1:
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 and reorganize the conclusion accordingly.

Line 190 the authors state that TAC can be used as a proxy for summer temperature, based on the strong anti-correlation with modeled summer temperature. Then in section 6 the authors highlight that summer temperature influencing firn metamorphism is only a low-accumulation site phenomenon. Does this mean that TAC could not be used as a temperature proxy in Greenland? If this is the case, maybe the language about using TAC as a proxy for summer temperature is too strong for their results. While their result is interesting, before saying that TAC is a proxy for summer temperature, the relationships should be verified at multiple ice core sites.
Thank you for this important question. We should make it clear that our conclusion applies to the EPICA Dome C but that it has to be demonstrated for other sites in Antarctica and in Greenland. Note that this is an on-going work led by a PhD student at IGE. Going in this direction and to be more precise we will change the title of our paper into:
"Past local summer temperature changes revealed by the Total Air Content record from the EPICA Dome C ice core".

Line 190, we will also modify the sentence into: "The good correlation between the two independent climate variables, TAC measured from the EDC ice core, and local summer temperature simulated by a model, indicates that the EDC TAC record can be used as a proxy for local summer temperature. Future studies will

investigate this relationship between TAC variations and local summer temperature changes in other ice core records drilled in Antarctica and Greenland".

We will also mention this point in the conclusion of the revised manuscript.

Line 49: What is the difference between V (air content) and TAC (total air content)? A differentiation of what the authors mean by V vs TAC is required.
We wrote (lines 49-50*:" During previous works V (for air content) and TAC (for Total Air Content) have been indifferently used for designating the same property. In this work we are using TAC, which is usually used in the recent works."*
So, there is no difference between the use of V and TAC, both designating the same property. As said, in this work we are using TAC, which is currently used in the recent works. We will make sure this is clear in the revised manuscript.

Line 84: What is a 10x acceleration, and why is it important in this context? I'm guessing the 10x model was used to save resources, but I recommend that more information is presented about why the accelerated models were used instead of the non-accelerated simulations.
Indeed the 10x acceleration was used to save computing resources and time. This technique can be explained by the text below cited from page 4 of Yin and Berger (2015) which is referred in our manuscript:
"Although being a model of intermediate complexity, LOVECLIM remains still costly for transient experiments, particularly when 5 interglacials and 10 transient simulations are considered. An acceleration technique similar to Lorenz and Lohmann (2004) was therefore used to speed up the simulations and reduce the computational costs. An acceleration factor of 10 is used, which means that at the end of each year of the simulation, the astronomical parameters and GHG concentration are advanced by 10 years. In such a case, the actual length of the simulation is reduced by 10 times. For example, a 20,000-year long simulation only needs 2000 model years. To test the impact of such an acceleration technique on our transient simulations, a non-accelerated experiment and a 10-time accelerated one have been done for two interglacials MIS-5 and MIS-13. Our results showed that the acceleration method has little impact on the surface air temperature and precipitation. However, the response of the deep-ocean temperature is delayed by 2-3 ka in the accelerated simulations as compared to the non-accelerated ones, similar results being observed also in other studies (Timm and Timmermann, 2007; Ganopolski and Calov, 2011). A detailed analysis made by Timm and Timmermann (2007) shows that a 10-time acceleration leads to a delayed response of the temperature only in the inner ocean. As here we are mainly interested in surface climates, the 10-time acceleration technique would not alter our conclusion about the phasing between the surface temperatures of different regions."

In our manuscript, the results of the 10x accelerated simulation are used for the entire 440 ka, but non-accelerated simulations for five glacial-interglacial episodes are also used to compare with the accelerated one. The comparison shows that the 10x acceleration technique does not affect the summer and annual temperature that were discussed in our manuscript. In our revised manuscript, we will give more information on these aspects.

Line 111: The sentence beginning "This spectral characteristic…" suggests that orbital astronomical forcing drives changes in TAC. The studies correlations do show a link between TAC and orbital patterns, but I think this sentence is misleading, and should be changed to reflect that TAC is only correlated with astronomical forcing, not actually caused by it. Later, I believe the authors make the argument that temperature gradients are what is causing TAC to vary, not insolation alone.
Thank you for this useful comment. We will rephrase the sentence in the revised manuscript. Also, as explained in our reply to referee 1, in the discussion on line 185-191 of the manuscript:
"…we assume that local summer insolation controls the temperature and the vertical temperature gradient in near-surface snow, but not TAC directly. Then, the surface snow structure is physically affected by changes in summer temperature. This surface structure change driven by summer temperature will control 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."

Line 127: I like the description of the astronomical half-year, and it is intuitive as to why it would be used as opposed to ISI.
Thank you.

Line 151: Here the authors say that TAC can be considered a proxy driven by mean local summer insolation based on the correlation. I'm a bit unclear here, because the correlation is higher (0.58) between TAC and summer temperature than between TAC and insolation (0.39). Do the authors mean that insolation drives summer temperature drives TAC? Or are they referring to previous works results using integrated summer insolation? If so, what was the correlation coefficient in that instance?
Yes, we propose that TAC is driven by summer temperature which is in turn mainly driven by local summer insolation. However, as explained in our reply to Reviewer 1's main comment 2, although local summer insolation is a main factor controlling summer temperature, other factors such as GHG could also contribute. This is why the correlation between TAC and summer temperature is higher than the correlation between summer temperature and insolation.

Line 149, 150, 170: The authors use the term 'correlated' when I think they mean anti-correlated, or negatively correlated. This needs to be clear, as it can cause confusion. I recommend all instances are reviewed.
Yes, sorry for the confusion, we meant anti-correlated.

Line 177: Figure 2 does not show any correlation between summer temp and summer insolation. Recommend adding this to figure 2 or deleting this line.
A figure will be added in the revised manuscript.

Line 280: What does "no (poor) correlation" mean? Please give a correlation coefficient. You reference section 5, but maybe figure 1 would be a better reference?
We mean poor correlation. You are right that Figure 1 is a better reference. We will modify this line accordingly.

Line 282: What does it mean to 'affect negatively" the critical density? Does this mean the other factor decreases the critical density?
Yes, this sentence needs rewording. The modified text reads: "This observation suggests that summer temperature has an inverse effect on Vc compared to the mean annual temperature". It will be included in the revised manuscript.

Line 308: I thought the comparison came from dD and modelled summer temperature, not TAC and dD?
We do mean the comparison between TAC and δD. A simple comparison of their spectral characteristics already indicates that their major drivers are different.

Technical Comments:
Line 3 – insert ",a" before "..tracer"
Done.

Multiple places EDC is referred to as Dome C. Recommend using EDC throughout the paper.
Done.

Line 155 – Recommend starting new paragraph here, where you start to discuss orbital tuning.
Done.

Figure 3 units – Are your units on the left hand side correct? I think they should be the same order of magnitude as the right side.

We are sorry, there is indeed a mistake in the units on this figure. It will be corrected in the revised manuscript.

Line 211 – Strange spacing issue here.
Indeed, there should be no space here. It will be changed.

Line 290: Recommend deleting the second ‘here’ in the sentence.
Done.

Line 312: Missing a word. Perhaps “Our transient simulation which allows us…”
Done.

Reference:

Yin, Q.Z. and Berger, A., 2015. Interglacial analogues of the Holocene and its natural near future. Quaternary Science Reviews 120, 28-46.