# **Response to RC1 comments on the submitted paper** ***Reconciling the surface temperature–surface mass balance relationship in models and ice cores in Antarctica over the last two centuries***

We thank the reviewer for their constructive comments. We have responded to all of them and will modify the paper accordingly. Our point-by-point answers follow.

Please note that review comments are in grey italics while our answers are not. Changes/additions to the original manuscript are indicated in blue.

# Answers to RC1

*In this article, Cavitte and co-authors analyse the link between surface mass balance (SMB) and surface air temperature (SAT) over the Antarctic ice sheet, at annual resolution. They focus on the last 200 years (1871–2000).*
*They use a series of climate model simulations: four global climate models including water stable isotopes (iGCMs) and a regional climate model (RCM) without isotopes. They also use observation-based results : the temperature reconstruction of Nicolas and Bromwich, 2014 (NB14) and ice-core annual to 5-year-mean $\delta^{18}O$ (Stenni et al., 2017) and SMB (Thomas et al., 2017).*
*SAT is supposed to be recorded by the $\delta^{18}O$ signal of ice cores. But $\delta^{18}O$ and SAT are generally correlated with SMB, as they both result of large scale advection of warm and moist air from lower latitudes. The aim of the authors is to understand how much SMB and SAT are correlated based on climate simulations and ice core records, and what can explain the strength of correlations at the regional and local scale. They also want to understand what is lacking in our current understanding to explain the observed lower correlation of $\delta^{18}O$-SMB in ice cores than SAT-SMB in models.*
*It is a very interesting study that I recommend for publication in The Cryosphere.*
*However, I pointed out major issues that need to be answered before publication.*

***Major (method)***
*This is a minor remark, but important for improving the readability of the article. In the article, the main assumption is that $\delta^{18}O$ is a proxy of SAT. However, you show 'SMB-SAT' and '$\delta^{18}O$-SMB' correlations. I suggest to write it the same order for both, e.g. 'SMB-SAT' and 'SMB-$\delta^{18}O$', even if it has no effect on correlations.*
**Answer:** This has been changed everywhere in the manuscript.

***iGCM members averaging***
***P5L121*** *'we average over their ensemble of simulations to obtain a mean representation of SMB, SAT and 18O for each iGCM.' I think this might be a major issue, as when averaging each variable across different simulation, the interannual variability is dampened. The correlation of average is not the average of correlations. It might not change dramatically your results but it should be corrected.*
**Answer:** We are sorry that the wording of this sentence was confusing. What we did was: (1) we calculated the correlation of the two variables (SMB-SAT or SMB-$\delta^{18}$O) for every grid point for each model member (3 members for iCESM1 and 7 members for iHadCM3). (2) We calculated the mean of the correlation values obtained per grid point over all the model members that belong to iCESM1 or iHadCM3 to get the mean SMB-SAT and SMB-$\delta^{18}$O correlation for each model. (3) We interpolated then all four iGCM correlation results (ensemble means for iCESM1 and iHadCM3, ECHAM5-MPI/OM and ECHAM5-wiso correlation results) onto the RACMO27 grid, for both the SMB-SAT annual correlations and the SMB-$\delta^{18}$O 5-yearly correlations. (4) We then calculated the mean over all four iGCMs to get the resulting iGCM plots shown in Fig.1. The sentence has now been changed to: "As the iCESM1 and iHadCM3 ensembles include three and seven simulations, respectively (each has slightly different initial conditions), we first calculate the correlation of the two variables (SMB-SAT or SMB-$\delta^{18}$O) for every grid point for each ensemble member (3 for iCESM1 and 7 for iHadCM3). Then we obtain the mean of the correlation values per grid point over each ensemble of simulations for both iCESM1 or iHadCM3. These ensemble means can then be compared to the correlations calculated for ECHAM5-MPI/OM and ECHAM5-wiso."

***iGCMs evaluation***

*P5L123 'Dalaiden et al. (2019) provide an evaluation of the iGCMs used here'. Model evaluation is too weak. You should show in supplementary key evaluations for the 4 models.*
*You should add all Dalaiden et al. 2019's evaluations related to the 3 iGCMs you use in your article, and add evaluation of iCESM1, that is not provided in Dalaiden et al. 2019.*
*As SMB-SAT relationship is driven by atmospheric circulation, it would be good to see how large-scale circulation of these models compare with reanalyses for the satellite era. Showing at least an evaluation of sea level pressure patterns on average for the common period is needed to understand how large the models' biases can be.*
**Answer:** As suggested, we will provide an evaluation of the SMB, SAT and SLP (Sea-Level Pressure) for the 4 iGCMs in the supplement. SMB and SAT will be evaluated against RACMO27, while SLP will be evaluated against ERA-Interim as the RACMO27 grid does not extend far enough north. The evaluation will be done over the 1979–2000 AD time period, the longest period of overlap for all the models examined. We will further discuss the relevance of this evaluation in the manuscript in this section (P5). Note that the Dalaiden et al. (2019) The Cryosphere Discussions paper has now been published in The Cryosphere and all references have been changed to Dalaiden et al. (2020), here and in the manuscript.

***Model selection and averaging***
*P5L122 'all four iGCMs show similar spatial variations of the correlation between SMB-SAT and $\delta^{18}$O-SMB on the continent scale [...] To compare the iGCM continent-wide correlations to the RCM-derived correlations, we interpolate the iGCM results onto the RCM grid and average over all four iGCMs.' From Fig. S1 we can see that ECHAM5-MIP-OM gives significantly different results than the other 3 iGCMS at the ice sheet margins for $\delta^{18}$O-SMB. From Dalaiden et al. 2019 Fig. S7 and S8, we can see that ECHAM5/MPI-OM have a major issue for modelling SMB. It is of major importance for this article, because as you average the 4 isotopic global climate models (iGCMs) correlations into one single map of correlation, it suppose that you consider equal skills of the 4 models. Consequently, you should consider disregarding ECHAM5-MIP-OM simulations, or at least giving it less weight in the average.*
**Answer:** We agree that ECHAM5-MPI/OM differs from the other 3 iGCMs in representing SMB, based on Dalaiden et al. (2020) Fig. S7 and S8. Nevertheless, selecting a criteria to keep or remove a model from our analyses may be difficult to justify objectively as all models have strengths and weaknesses. We propose to keep all 4 iGCMs in this study, but move figures S1 and S3 (that display the SMB-SAT and SMB-$\delta^{18}$O correlations over the 1871–2000 AD time interval) to the main manuscript. The goal is to discuss the similarities and differences between the models more explicitly and in particular discuss if the conclusions that could be derived from ECHAM5-MPI/OM differ from the ones obtained in the other models. We will also use the iGCM evaluations from the point above to support the discussion in this paragraph of the manuscript.

***Major (results and interpretation)***
***Consistencies between models and time scales***
*P6L183 'Moreover, the maximum and minimum correlations obtained are consistent between iGCMs, in magnitude and spatial distribution (see supplementary Fig.S1-S4).'*
*Can you develop the area you think are consistent? Because I see more differences that analogy between the 4 models. ECHAM5-MPI-OM is the only iGCM with a clear loss of correlation at the ice sheet margins in East Antarctica (for both SAT and $\delta^{18}$O). If you exclude ECHAM5-MPI-OM, I see some consistencies between the 3 models left, but still, it's not very clear given the patchy patterns.*
**Answer:** We will expand the description of the similarities in this paragraph, and also include a description of the areas that are dissimilar. We will define several areas (specific boxes) over the continent, some where we observe a consistently positive correlation (a specific area situated over the Antarctic Plateau),

some where we observe a consistently weak correlation (an Amery Embayment, Peninsula, and a WAIS area), and we will calculate the average/max correlation, and also the % of non-significant values for each of these boxes for each iGCM to compare the iGCM correlation results quantitatively in our description.

***P7L190*** *'This implies that the correlation of SMB and SAT is similar over the 1871–2000 AD and the 1979–2016 AD time intervals, and from a spatial resolution of >1 down to 5.5 km.' Please clarify what you think is similar between the resolutions and the time periods. I see many differences, so if you want to highlight the consistencies, you should detail them (e.g. high correlations for West AIS?).* ***Generally, I am concerned about the too optimistic way of presenting consistencies between simulations.***

**Answer:** We agree that we were too vague in our description and will describe the similarities and dissimilarities in more details in that paragraph, the same way that we propose to do it for the iGCMs (see comment above). We will use the same areas ("boxes") to quantify the average/max correlations and the % of non-significant values for each box.

***Wind effect***

***P7L196*** *'There are a few areas, spatially consistent between the RACMO27, RACMO5 simulations and the iGCMs, where the SMB-SAT correlation is not as strong.' Again, I am not sure to agree with the authors. If you look at SMB-SAT correlation on Fig.S4 compared to Fig.2, it is not clear that there is a loss of correlation in the iGCMs at the "same areas" than in the RCM. It seems that combining the 4 iGCMs gives by chance the same pattern as in RACMO2?*

**Answer:** We expect that the SMB-SAT correlation won't be identical. We agree with the reviewer that we were too general and optimistic in our description. In light of having changed Fig.1 and Fig.2 to show the individual iGCM correlation results (see a few comments above), we will discuss more specifically the areas that we see as showing a similar loss of SMB-SAT correlation between the iGCMs and RACMO2.3.

In particular, we will stress that the Amery Embayment shows as non-significant in RACMO2.3 as well as ECHAM5-wiso and iCESM1. The loss of correlation along the AP seen in RACMO2.3 is visible also on iCESM1 and ECHAM5-wiso. iCESM1 shows a bit of the coastal variability seen in RACMO2.3, in part due to its finer grid. ECHAM5-wiso also has a fine grid but because we only keep model grid points that are entirely continental using the ECHAM5-wiso mask, we have less constraints along the coast.

***P7L204*** *(1) 'Large-scale air masses, originating over the Southern Ocean and further north, bring warm moist air towards the interior as they flow up-slope, thus inducing a strong and negative correlation': You suggest that at the interannual time scale, when you have upslope winds (so more negative) you have higher temperature and SMB, and the more positive is mean slope in mean wind direction (MSWD) the colder and the dryer. It seems reasonable, but it would be really good to explicit this, because I had trouble trying understanding the positive/negative correlations with wind.*

*(2) Can you show time series of MSWD together with time series of SAT and SMB at some specific locations, so that we can understand what the correlation means? E.g. at locations where it's significantly positively/negatively correlated to SAT/SMB, and on the coast/on the plateau, or at least examples for your cases (1) and (2), and for Adelie Land vs. Amery Embayment.*

*(3) In addition, I am wondering how much annual MSWD is a good indicator of the mechanism you want to highlight. Advection of warm and moist air by cyclones are punctual whereas surface winds generally flow downslope all year long. Consequently I am not sure how much you capture the cyclone activity with MSWD.*

*(4) You discuss this with cases (1) and (2), but it seems very speculative. Basing your statement on time series will help developing more robust analyses. I was lost reading the wind considerations, but after*

*re-reading it a couple of times, I think I agree with most of your conclusions. It would be better to re-write this part before I can give a better feedback.* ***I do think it is very interesting to analyse the strong correlations between MSWD, SMB and SAT at the interannual time scale.*** *You should use the different components of SMB available in RACMO to analyse the effect of wind on this components (precipitation, drifting snow fluxes, sublimation, etc.), instead of trying to guess why it works this way.*

**Answer:** Point #1 - We agree that the sign of the positive/negative correlations is perhaps unclear, we now describe the signs explicitly in the text which we have moved below the description of Eq (1) for clarity: "Since we define a positive MSWD as a wind pointing down-slope, we expect winds coming from the coast up into the interior to have a negative MSWD, and winds flowing from the interior to the coast to have a positive MSWD. Large-scale air masses, originating over the Southern Ocean and further north, bring warm moist air towards the interior as they flow up-slope. At the annual time scale, stronger up-slope winds (negative MSWD anomaly) will therefore bring a higher temperature and SMB (positive anomaly), and thus induce a strong and negative annual MSWD-SAT and MSWD-SMB correlation. Similarly, winds flowing down-slope (positive MSWD) will bring drier and colder air (negative anomaly), and will also induce a strong and negative annual correlation of MSWD with SAT and SMB. Any area of the AIS that does not show this negative annual correlation between wind and SAT or SMB implies that the changes cannot be interpreted simply in terms of large-scale circulation variability at the annual scale, and additional processes are affecting the descending cold air from the interior." Furthermore, we will add a small sketch for clarity.
Point #2 - We will show time series of MSWD with time series of SAT and SMB. We will use the AP as a location where MSWD and SAT/SMB are weakly/positively correlated to exemplify the Foehn effect's influence. We will use the Amery Embayment where MSWD and SAT/SMB are weakly/positively correlated and exemplify the influence of the katabatic winds. We will use the plateau region near Dome A (80.3667°S, 77.3005°E) and Adélie land where the MSWD-SMB/SAT correlation is strong and negative. We apologize but we could not figure out what the cases 1 and 2 the reviewer was referring to were.
Point #3 - The hypothesis in this paper is that, even if cyclone activity is punctual, by correlating MSWD to SMB/SAT over 1979–2016 AD, we will be capturing the variations of cyclonic activity at interannual timescales, i.e. the variability in the path of cyclones, and therefore variability in the sources of heat and moisture, whether they are dominant or not over katabatic winds. It would be interesting to compare to the SMB and MSWD at the daily-level, to look at individual cyclonic events more reliably. However, at present, daily RACMO2.3p2 simulations are not freely available. We have added a short sentence to this effect in the manuscript in this paragraph: "We are aware that such cyclonic activity is punctual. However, by correlating MSWD to SMB and SAT over 1979–2016 AD, we make the hypothesis that we capture at the first order the variations of cyclonic activity at interannual timescales, and therefore the variability in the sources of heat and moisture."
Point #4 - Although not shown here, we had looked at the influence of the various SMB components on the total SMB and observed that SMB is strongly dominated by precipitation at the annual timescale. Correlating SMB with its precipitation component over 1979–2016 AD gives an average correlation over the whole continent of 0.96. We will calculate the correlation of MSWD with the various components of SMB (sublimation, blowing snow, precipitation) which we will add to the supplement and describe the results in this paragraph of the manuscript.

***Related minor comments:***
***Eq. (1)*** *You must remove the arrow on MSWD as it is a scalar.*
**Answer:** It has been removed.

***Fig.S9*** *Can you show MSWD too? To see where it's negative and positive?*
**Answer:** Yes, it has been added as a Figure S10

***P7L215*** *'We then remove areas of the AIS with a negligible slope (0.001) as in these areas, MSWD will be close to null and will introduce a lot of noise when correlating it with SMB or SAT.': Shouldn't you remove areas where SMB interannual variability and MSWD interannual variability are small too? I would suspect that all the high (negative) correlation for MSWD-SAT and MSWD-SMB found in the EAIS plateau are because of very small interannual variability in wind and SMB(?).*
**Answer:** We argue that areas with a small SMB and MSWD interannual variability are also interesting to look at. We only remove areas that have a negligible slope because correlating such a small number with SMB or SAT could introduce a lot of noise. However, the results are not radically different with and without the threshold applied on the slope (see Fig. 1 and 2). It reduces the average continent-wide correlation strength and increases the number of grid cells with $p > 0.1$ but not significantly.

***P8L223*** *'Agosta et al. (2019) also show a strong link between modelled surface topography (surface curvature in their case) with SMB over the continent when wind speeds exceed 5 m s-1.': but in Agosta et al. 2019, it is a spatial link of time-averaged values, not a temporal link.*
**Answer:** We agree that Agosta et al (2019) clearly look at the spatial link of time-averaged values while we look at the temporal link, but we wanted to highlight the parallel between their study and our regarding the impact of the interactions between surface curvature and wind speed. We will change the wording in the text as follows: "Agosta et al. (2019) also show the strong impact of surface topography (surface curvature in their case) - wind interactions on modelled SMB by examining the spatial link of time-averaged values. They observe that above a certain threshold, winds will affect SMB locally in pattern that matches that of drifting snow fluxes as modeled by RACMO2.3.". We plan to also cite the work of Dattler et al. (2019) who show that, in their study area, the variability in accumulation is correlated with MSWD variability also.

***P8L264*** *'We therefore expect that the areas regularly under the influence of strong katabatic winds will show a weaker MSWD-SMB correlation due to the episodic but persistent reduction in their SMB through wind scouring'. Here is one out of many examples where you can use RACMO outputs to verify your hypothesis, since you have access to all surface mass balance fluxes, including drifting snow fluxes, in this model.*
**Answer:** We agree, and related to a comment we responded to further up, we will show the correlation of MSWD with the various components of SMB in the supplement and provide discussion of these correlations in this section of the manuscript.

***P10L288*** *'Perhaps here snowfall input from further north is so high that it dominates the SMB and SAT records.' The same: you can use RACMO outputs to clarify what's happening.*
**Answer:** We agree, same comment as above.

***Suspicion of wrong SMB interannual variability in ice cores***
*Sections 3.2 and 3.3 study the SMB-SAT link in ice core data, and aims at understanding why it is much weaker in ice cores than in models.*
***10L307*** *'We observe a weak-to-null annual correlation between SAT and SMB in the ice cores (Fig.7) with an average value of 0.09±0.18 over all the ice cores, versus a continent-wide average value of 0.57±0.10 for the iGCMs and 0.54±0.22 for RACMO27.' I am wondering whether the low correlation between ice cores SMB and NB14 SAT is because of a too low or incorrect interannual variability of*

*SMB in ice cores. I think this analysis is very interesting, but I cannot evaluate it further if I am not sure that the difference between ice cores and models is not because of wrong SMB interannual variability in ice cores. Can you show annual time series of ice core SMB vs. RACMO SMB? The results will have a major impact for reviewing the rest of the study.*

**Answer:** We agree with the reviewer that it is difficult to quantify whether the low correlation between ice cores SMB-SAT is because of a too low or incorrect SMB interannual variability of SMB in the ice cores. Ice core SMB errors are difficult to constrain (measurement errors, representativity errors due to processes such as a wind ablation that removes a part of the deposited annual snowfall). In addition, missing one year in the dating of the ice cores, for example, can shift the time series, and then the correlation between ice core-derived SMB and the independent SAT time series would be lost. However, we show (Figs. 7 and 8 of the manuscript) that the correlation between SMB and $\delta^{18}$O, both ice core-derived records, is also low. It suggest that the low correlation is perhaps unrelated to a bad synchronization of the SMB and SAT variables. RCMs also include errors that are difficult to trace (difficulty of representing blowing snow or diamond dust). RACMO2.3 for example includes blowing snow processes but Agosta et al. (2019) have shown that its spatial variability is under-represented and that it is underestimated by a factor or 3. When comparing RCM and ice core results, it is thus not simple to assess the origin of the differences in interannual variations. The advantage of RCMs is that they are self-consistent: the SMB and SAT values simulated will be linked by the physics of the model. Ice cores on the other hand can have different biases dependent on different variables (ablation, diffusion, measurement errors, location, etc). However, RCMs can only go back as far as we have direct measurements (i.e. 1979 as previous to that, measurement biases increase, e.g. Bromwich et al., 2007). We need the ice core observations to go back further in time. We will add a sentence to that effect in the introduction and in these sections to provide more context to the readers as to why we compare model correlations to the ice core correlations, which we had perhaps not stated clearly enough.

***Minor comments***

***P2L35*** *'temperature' : heat*

**Answer:** Replaced.

***P2L35*** *remove 'usually'*

**Answer:** Removed.

***P2L35-36*** *'that collect heat and moisture from further north, including the Southern Ocean, which they can release onto the AIS'. It's the same idea and mechanism than described in the previous sentence: 'Large-scale atmospheric circulation strongly controls SMB in Antarctica, bringing air masses with a high moisture and temperature content'. Merge the sentences.*

**Answer:** The two sentences have been merged as: "Large-scale atmospheric circulation (100s of km) strongly controls SMB in Antarctica. This large-scale atmospheric circulation embeds synoptic-scale cyclones that collect heat and moisture from further north, including the Southern Ocean, which they can release onto the AIS (Gorodetskaya et al., 2014; Sodemann and Stohl, 2009; Wang et al., 2019; Lenaerts et al., 2019)."

***P2L43-44*** *'In addition, based on the Clausius-Clapeyron relationship, the increasing surface air temperature (SAT) due to climate change should induce a greater moisture holding capacity of the air': It's the increasing of air temperature (in the mid and upper troposphere) that induces an increase in moisture holding capacity and more precipitation, not the increase in *surface* air temperature. The confusion here comes from the fact that increase in SAT and in tropospheric air temperature are strongly correlated.*

**Answer:** We agree and the sentence has been appropriately reworded: "In addition, based on the Clausius-Clapeyron relationship, the increasing mid and upper troposphere temperature due to climate change implies that the air can include more moisture before saturation, and therefore snowfall is increased (Frieler et al., 2015; Fudge et al., 2016). If this predicted increase in SMB is linked to increasing temperatures in the 21st century, it will be interesting to see if SMB and SAT are linked in the past too since an increase in SAT and tropospheric air temperature are strongly correlated. In which case, SMB records over time will be a helpful tool to constrain past climates (Dalaiden et al., 2020)."

***P2L49*** *'is often used as a proxy for SAT': Is it used as a proxy of SAT or a proxy of snowfall-weighted SAT?*

**Answer:** We have changed this to "is often used as a proxy of snowfall-weighted SAT (and by extension of SAT, Stenni et al., 2000[...]".

***P3L84*** *AP is not introduced before*

**Answer:** We noticed that the first instance that "Antarctic Peninsular appears is P2L42, and have therefore defined the acryonym there: "Antarctic Peninsula (AP,...)"

***P3L86*** *'over historical timescales': give the time period and the resolution.*

**Answer:** We have modified the sentence as follows: "Our work follows from that of (Dalaiden et al., 2020), in which they show that SMB and SAT are strongly correlated interannually at the continental scale over millennial (1000–2005 AD) and historical (1850–2005 AD) timescales"

***P4L91*** *'1. Which processes link SAT and SMB at regional scales and how do they scale down from conclusions at the continental scale': You don't answer to this question.*

**Answer:** We have reformulated the question as "Do the processes that link SAT and SMB at the continental scale also play a dominant role at the regional scale?". The title of section 3.1 has also been changed accordingly.

***P4L98-99*** *'We hypothesize physical mechanisms that could explain the discrete areas of the AIS where the SMB-SAT relationship is weak.' Here you suppose this relationship is always strong. But in this article, you don't analyse why the SMB-SAT relationship is generally strong.*

**Answer:** This was indeed omitted because P3L86, we briefly summarized Dalaiden et al. (2020)'s conclusions on the SMB-SAT relationship at the continental scale, but this was likely too succinct. We propose to not change this paragraph much (except adding that it is a "positive SMB-SAT relationship" that we compare at the regional and continental scale. But provide more discussion in section 3.1.1 where the strength of the SMB-SAT relationship is described and discussed.

P4L98-99 is now: We start with a brief description of the data sets and models used in this study. Then we compare the positive SMB-SAT relationship, as well the SMB-$\delta^{18}$O relationship, obtained in the models at regional scale to the continental-scale results of Dalaiden et al. (2020).

And we have added the following at the end of Section 3.1.1: "[...] Whatever the temporal and spatial scale, the correlation between SMB and SAT is positive for a large majority of the model grid points. The simple concept that Antarctic precipitation originates mainly from lower latitudes, in the form of warm and wet air masses, that explained the covariance of SMB and SAT at the continental scale for Dalaiden et al. (2020), therefore also applies at the regional scale here. Indeed, for the temporal and spatial scales investigated, the annual correlation between SMB and SAT is positive for a large majority of the model grid points. The correlation between SMB and SAT remains true for summer- or winter-only months [...]"

***P5L122*** *'all four iGCMs show similar spatial variations of the correlation between SMB-SAT and $\delta^{18}$O-SMB on the continent scale' : You didn't give on which time scale you correlate the time series. You should emphasis the time step (annual, from the text after) in the method, by always associating "correlation" with "annual".*

**Answer:** We agree and have added "annual" before correlation in this paragraph, and all the relevant places in the manuscript where we are talking specifically about the annual correlation of two variables, and not correlation values in general.

***P6L172*** *'Furthermore, the spatial distribution of the ice cores over the AIS is not homogeneous, with the majority of the ice cores located in the coastal areas, and very few in the interior (see supplementary Fig.S5). This certainly introduces a spatial bias in our ice core-based correlation towards coastal signals and processes.'*

*The lower the accumulation, the lower the ice core resolution. So it is expected that annually resolved ice cores will be at the ice sheet margins. In addition, you should not write "coastal", as most of the ice cores are not at the coast but inland. You can maybe divide Antarctica in "low elevation" (<2200 m) and "high elevation" (>2200 m).*

**Answer:** We agree with both comments, and have reformulated the paragraph as: "Furthermore, the spatial distribution of the ice cores over the AIS is not homogeneous. If we divide Antarctica into low elevation areas (i.e.<2200 m) and high elevation areas (i.e. >2200 m), the majority of the ice cores are located in the low elevation areas, and very few in high elevation areas (see supplementary Fig.S5). This certainly introduces a spatial bias in our ice core-based annual correlation towards coastal signals and processes."

We will also add a line to mark altitude 2200 m on Fig. S5.

***P7L185*** *'We repeat the annual correlation of SAT and SMB using the RACMO27 simulations over 1979–2016 AD (see Fig.2).': Merge Fig. 2 to Fig. 1 so that we can compare patterns between iGCMs and RACMO2.*

**Answer:** These will be merged. Following earlier comments, we propose that Fig. 1 will now show the SMB-SAT correlation for each of the 4 iGCMs (previously supplementary Fig. S3), the iGCM average (previously Fig. 1, panel c) and the RACMO27 SMB-SAT correlation (previously Fig. 2). And Fig. 2 will now include the SMB-$\delta^{18}$O correlation for each of the 4 iGCMs (previously Fig. S1) and the iGCM average (previously Fig. 1, panel a).

***P7L200*** *'Winds are known to affect SMB and SAT locally, through wind-based redistribution of SMB, turbulent warming from katabatics and Foehn warming effects on leeward slopes.': you should specify that the loss of correlation because of wind-based redistribution of SMB can be modelled by RACMO only, because it is the only model in this study that includes drifting snow modelling.*

**Answer:** We agree and have modified the text adapting your input as follows: "[...]turbulent warming from katabatics and Foehn warming effects on leeward slopes. Additionally, the impact of wind on SMB and SAT due to wind-based redistribution can modify the link between the two variables but this can only be resolved by the RACMO2.3 simulations, because it is the only model in this study that includes drifting snow (although according to Agosta et al. (2019), drifting snow is strongly underestimated). To evaluate whether the lack of correlation between SAT and SMB is due to any of these wind effects, we correlate[...]"

***P7L200*** *'turbulent warming from katabatics and Foehn warming effects on leeward slopes': katabatics* *and surface winds in general (e.g. pressure gradient winds superimposed with katabatics) are also directly concerned by adiabatic warming, the same process involved in the foehn warming.*

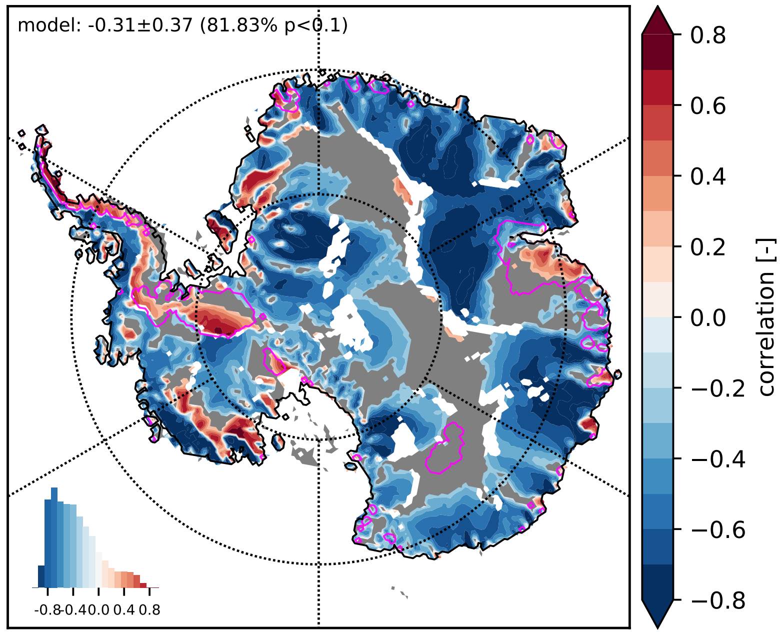
**Answer:** We agree that in both cases, there is a warming effect, so we have modified this line as follows: "turbulent and adiabatic warming from katabatic winds and Foehn effects on leeward slopes"

***P11L347*** *' Turner et al. (2019) show that more than 70% of the annual accumulation consists of extreme events that have a very short duration (one or more consecutive days).': warning. Turner et al. (2019) show that more than 70%* ***of the variance*** *of the annual precipitation is explained by extreme precipitation events, meaning the interannual variability, not the mean value.*

**Answer:** We have modified the text accordingly: "Turner et al. (2019) show that more than 70% of the variance of the annual precipitation is explained by extreme events that have a very short duration (one or more consecutive days)."

***P14L427*** *'to improve our confidence in using SMB as a direct proxy for SAT over the entire AIS.' You never mentioned this objective before. Can you develop it in the introduction?*

**Answer:** This was mentioned briefly in the introduction already P2L46 "If this predicted increase in SMB is linked to increasing SAT in the 21st century, it will be interesting to see if SMB and SAT are linked in the past too, in which case SMB records over time will be a helpful tool to constrain past climates." but perhaps the goal "to improve our confidence in using SMB as a direct proxy for SAT over the entire AIS." should be stated more explicitly. We have added this more explicitly at the end of our Introduction section, P4L97: "Answering these questions will help constrain our confidence in using SMB as a direct proxy for SAT over the entire AIS. With relatively few in-situ observations, additional SAT proxies would be extremely beneficial for Antarctic climate reconstructions."



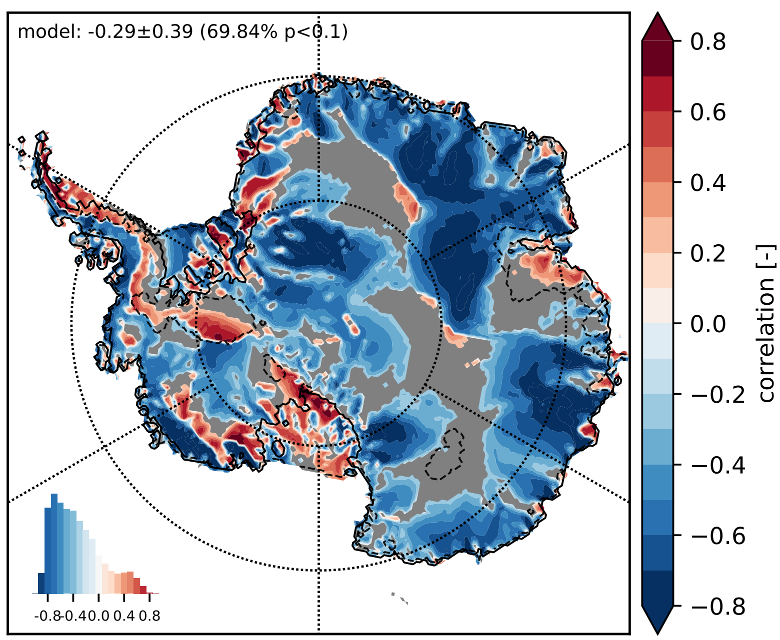




Figure 1: Annual correlation between MSWD and SAT (top) with and (bottom) without the slope >0.1% threshold, using RACMO27 simulations over 1979–2016 AD.

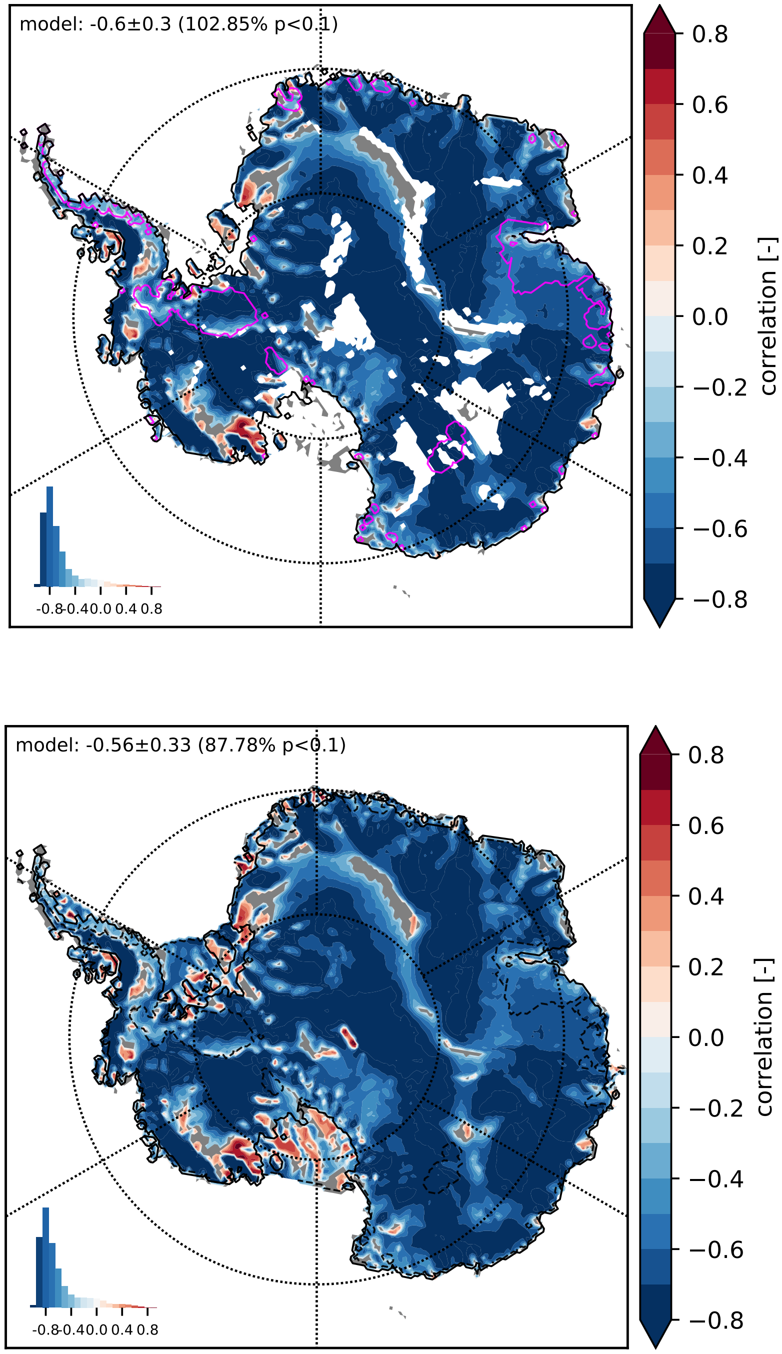



Figure 2: Annual correlation between MSWD and SMB (top) with and (bottom) without the slope >0.1% threshold, using RACMO27 simulations over 1979–2016 AD.