Review of Cavitte et al. 2020 'Reconciling the surface temperature–surface mass balance relationship in models and ice cores in Antarctica over the last two centuries'

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 δ18O (Stenni et al., 2017) and SMB (Thomas et al., 2017).
SAT is supposed to be recorded by the δ18O signal of ice cores. But δ18O 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 δ18O-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 δ18O is a proxy of SAT. However, you show 'SMB-SAT' and 'δ18O-SMB' correlations. I suggest to write it the same order for both, e.g. 'SMB-SAT' and 'SMB-δ18O', even if it has no effect on correlations.

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.

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.

Model selection and averaging.
P5L122 *'all four iGCMs show similar spatial variations of the correlation between SMB-SAT and δ18O-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 signifanctly different results than the other 3 iGCMS at the ice sheet margins for δ18O-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 ECAM5-MIP-OM simulations, or at least giving it less weight in the average.

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 δ18O). 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.
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.

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 at in RACMO2?

P7L204 *'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.

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.

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.

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.

Related minor comments:

Eq. (1) You must remove the arrow on MSWD as it is a scalar.

Fig.S9 Can you show MSWD too? To see where it's negative and positive?

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(?).

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 s1 .'*: but in Agosta et al. 2019, it is a spatial link of time-averaged values, not a temporal link.

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.
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.

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.
P10L307 *' 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.

Minor comments

P2L35 *'temperature'*: heat
P2L35 remove *'usually'*
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.
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.
P2L49 *'is often used as a proxy for SAT'*: Is it used as a proxy of SAT or a proxy of snowfall-weighted SAT?
P3L84 AP is not introduced before

P3L86 *'over historical timescales'*: give the time period and the resolution.
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.
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.
P5L122 *'all four iGCMs show similar spatial variations of the correlation between SMB-SAT and δ18O-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".
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).
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.
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.
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.
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.
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?