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The Cryosphere Discussions

Interactive comment on "Surface Mass Balance of the Antarctic Ice Sheet and its link with surface temperature change in model simulations and reconstructions" *by* Quentin Dalaiden et al.

Anonymous Referee #2

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This paper from Dalaiden and co-authors addresses the question of the relationship between surface air temperature (SAT) and surface mass balance (SMB) in Antarctica, from the past 1000 years to the last decades, in view of using the SMB information for reconstructing past SAT.

Given the short and sparse observational coverage in Antarctica, reconstruction of the Antarctic climate further than the last decades rely on the interpretation of proxies. The isotopic composition of the snow (in particular δ18O in ice cores) is the most widely used proxy of SAT in Antarctica

First the authors show that the strong link between SMB and SAT, already acknowl-

edged in the literature (e.g. Frieler et al 2015), remain valid in GCMs during the past 1000 years and the past 200 years. They also show that the relationship does not stand when considering the last two reconstructions of surface air temperature (based on ice cores δ18O, Stenni et al., 2017) and surface mass balance (based on ice cores accumulation, Thomas et al., 2017), but does exist when using an Antarctic SAT reconstruction based on weather stations (Nicolas and Bromwich 2014, NB14) instead of the SAT reconstruction based on ice cores δ18O.

Then the authors use isotope-enabled global climate models to perform an offline data assimilation of δ18O and SMB over the past 200 years. They obtain more consistent results with NB14 SAT over West and East Antarctic ice sheets when combining the assimilation of δ18O and SMB.

I think using both SMB and δ18O for reconstructing SAT with an assimilation method is novel and relevant for the cryosphere and climate community. The overall presentation is clear and figures are nicely shaped. Conclusions seem robust and interesting. However I have some concerns about some of the interpretations, and I also have comments on the methodology. Therefore I recommend this article to be published after addressing the following issues.

Major

1) I think the GCM evaluation is of interest, in particular the plots comparing SMB by elevation bins, but I disagree with the conclusion that GCM are doing a good job in Antarctica. I think this is not a critical point for this study, so the authors should minimize or remove the section about GCM evaluation (Section 4.1, one or two sentences and citing supplementary would be enough) and extend the analysis on the SMB/SAT relationship (Section 4.2). Fig. 2 is not necessary, Fig. 3 and Fig. 4 could be moved to the SAM/SAT section, Fig. 4 could be extended with a scatterplot comparing SMB/SAT sensitivity factors (% K-1) of West vs East. This way the result section would follow the plan detailed in the introduction: i) SMB/SAT in GCMS over the past millennia and

centuries ii) data assimilation for the past centuries.

In detail:

* Abstract "Here, we show that Global Climate Models (GCMs) can reproduce the present-day (1979–2005) AIS SMB and the temporal variations over the last two centuries."
P17 "The GCMs are able to simulate relativity well the current AIS SMB"
-> Should be rephrased or removed (see hereafter).

* P8 "Overall, the AIS SMB simulated by GCMs is in good agreement with the SMB simulated by the regional climate model RACMO2 over the last decades (1979–2005, R2 = 0.53; Fig. 2 and S1 for the SMB of each model)."
-> I see huge differences, spatially and integrated over the ice sheet (Fig. S1 and S2). How is computed this correlation coefficient? What is the bias?

* P8 "Both display high values of SMB along the coast (>300 mm w.e. year-1) – especially for West Antarctica and the Antarctic Peninsula – and lower values at high elevations (e.g. the Plateau: <100 mm w.e. year-1)."
-> This is really the minimum feature a model can do, because of the general circulation and the ice sheet topography.

2) I found interpretations in contradiction with the figures.

* P9 "Nevertheless, when analyzing the individual simulations of the ensemble performed with CESM1-CAM5, the contrast between East Antarctica and West Antarctica is as large as in recent observations (Fig. 4). This indicates that 1) the observed SMB trends between the two regions are within the range of the simulated values; 2) internal variability has an important role in the current Antarctic SMB changes."
-> Reconstruction is a clear outlier of the GCM's scatterplots, so reformulate the conclusion in agreement with your figure.

* P12 "For most regions, the link between surface temperature and SMB (r=0.70 on

average over the seven subregions for the 1850–2000 period) is higher than that between surface temperatures and δ18O (r=0.55 on average over the seven subregions for the 1850–2000 period)." (. . .) "The results with the outputs of ECHAM5-wiso and ECHAM5/MPI-OM are similar (Figs. S6 and S7)."
-> It does not appear to be true when looking at Fig. S6 and S7: blue dots (SAT/δ18O) are often higher than green dots (SMB/SAT). I regret this over-interpretation and the fact that the authors focused on the iHadCM3 in the main text without specifying it and explaining this choice.
P18 "On the one hand, models show a strong correlation between δ18O and SMB for all the Antarctic regions"
-> It's not true: red dots in Fig 7, S6 and S7. Is there a typo here? But even SAT-SMB relationship is not strong for all regions (Fig S5).
"we showed that the relationship between SMB and surface temperature is often higher than the one between surface temperature and δ18O. This is true both on the continental and regional scale."
-> That's not true when considering ECHAMwiso and ECHAM/MPI-OM

3) Methodology

Data assimilation (DA) must be evaluated with independent datasets. It is the case for SAT (NB14 is not assimilated) but not for SMB. The authors assimilate SMB from Thomas et al. (2017) and evaluate their results with Thomas et al. (2017). I suggest to use independent and annually resolved datasets, such as the radar transects resolved annually in West Antarctica (Medley et al. 2014 https://doi.org/10.5194/tc-8-1375-2014) and stake line transects (JARE, CHINARE).

* P19 "Considering our good results regarding surface temperatures and SMB reconstructions,"
-> This sentence is not fair if you evaluate your result with the data you assimilate.

* P19 "our data assimilation-based reconstructions suggest that the strong simulated

correlation between surface temperatures and SMB in GCMs is not a model artefact"
-> DA is a weighted average, so if the SMB-SAT relationship exists in the models, isn't it conserved in the reconstruction by construction?

4) A remark

Results of data assimilation seem less variable than the other reconstructions (Fig 8 and Fig 9). Is it due to the assimilation method? What is the confidence on the DA temporal variability?

Minor

Abstract

"with a linear correlation coefficient with the observed surface temperatures (1958–2010 CE) of 0.73"
I don't think this number is meaningful, I suggest to remove it.

P2

"(Rignot et al., 2011)"
Update with Rignot et al. (2019) https://www.pnas.org/content/116/4/1095

"(Wouters et al., 2013)."
Idem, update the reference.

"from stable isotope ratios of oxygen"
From water stable isotopes, and in particular δ18O

P3

"According to Monaghan et al. (2008), the observed sensitivity of Antarctic snowfall accumulation to surface temperature was about 5% K-1 during the 1960–1999 period."
Why Monaghan and not a most recent and complete reference? (e.g. Frieler 2015)

"These results suggest that in some regions, especially along the AIS coasts, the vari-

ability of thermodynamic processes (such as the Clausius-Clapeyron effect) on SMB is dominated by the large-scale atmospheric circulation, limiting the correlation with δ18O."
Do you mean: SMB variability is dominated by large-scale atmospheric circulation rather than by thermodynamic processes?

"While the statistical methods classically used to infer past surface temperature (see for instance Stenni et al., 2017) rely on the length of the calibration period, on the quality of the record during this period, and on the stationarity of the link between the proxy and the variable of interest, which can be strong assumptions in the case of the δ18O-temperature relationship (Klein et al., 2019), data assimilation does not."
Doesn't data assimilation rely on the quality of the assimilated record too? One step further, a short sentence about the limits of the assimilation method is missing, to be fair. E.g. changes in the number and quality of assimilated data?

P4

"The simulation of ECHAM5-wiso, which only includes an atmospheric component, was performed by Steiger et al. (2017) and covers the period 1871–2011 CE at âĹij 1° resolution. The model is driven by the sea surface temperature and sea ice from the Rayner et al. (2003) dataset."
You have to mention that the Rayner et al. (2003) dataset is not relevant before 1973: "2.1.3. Antarctic Atlas Climatologies Before the advent of satelliteâĂŘbased imagery in 1973, sea ice concentration data for the Antarctic are not available, and sea ice extent data are not readily available for individual months, seasons or years, although some visible and infrared data do exist for 1966–1972 [Zwally et al., 1983] and some undigitized charts reside in national archives (e.g., V. Smolyanitsky, personal communication, 2002). Readily available information was limited to two historical climatologies of sea ice extent. Therefore our sea ice concentration analysis before 1973 is derived indirectly, and does not include any interannual variability, though there are some trends resulting from the differences between climatologies for different periods."

"Comparisons of the results of these three isotope-enabled models with modern δ18O observations indicate that they all reproduce the main characteristics of the spatial distribution of the isotopic composition of precipitation over Antarctica (see reference for each model)."
Add a word about their known biases.

P5

"(4) the output of RACMO2 for the AIS SMB agrees very well with available measurements (correlation coefficient with observations of 0.9; van Wessem et al., 2018)."
A high correlation coefficient alone is not a proof of good performance. Correlation can be equal to one with a very large bias.

P6

"This temporal averaging reduces uncertainties in dating linked to the noise induced by non-climatic processes (e.g. Laepple et al., 2018; Fan et al., 2014)."
The temporal averaging is not described before, and I understood latter in the paragraph that you were talking about the 5-year and 10-year average. The whole paragraph is strangely shaped, please rephrase.

P7

"each ensemble member, called particle, is compared to the proxy-based reconstruction by computing its likelihood, taking into account data uncertainties."
Give a description of this likelihood function. How do you compute it?

P8

" The median of the SMB over the entire AIS simulated by CMIP5 models is 1.16A median computed from 12 values is not robust. This number is hiding large discrepancies between the models.

Figure 2: You show the average while above you give the number for the median.

"who have shown that due to the lower spatial resolution of GCMs in comparison to the regional model, SMB is underestimated at the coasts while an overestimation occurs in the interior of the ice sheet."
Resolution might play a role but model's physics also plays a major role. E.g. Fig S1 shows that MRI-CGCM3 and ECHAM-wiso have much large SMB at the margins than RACMO2, whereas they have a lower resolution.

Fig. S3: Add the isotope-enabled models

"confirming that the spatial resolution has a crucial impact on the simulated SMB."
This is not convincing and not the dominant factor in my point of view.

P11 "According to these reconstructions, this sensitivity has increased a lot for the recent period (1950–2005; 15.52 Do you think it is realistic? I don't find such an increase in sensitivity in Frieler et al. (2015)?

Figure 6: I don't understand why for WAIS and AP, 'reconstructions' (black line) is lower than model mean, while for the combination of both (West Antarctica), 'reconstructions' is larger than the model mean? + typos in the legend.

P12

"The analysis of isotope-enabled models results reinforces this hypothesis (Fig. 7): the iHadCM3 outputs show high correlations between these two variables."
In the sub-section 4.3, you only focus on the iHadCM3 outputs without explicitly announcing it and explaining why you did this choice.

P16

"(estimated by the weighted variance of the particles with non-zero weight)"
Define this weight/metric in the method section. What is the threshold?

"When assimilating both δ18O and SMB, the SMB reconstruction is in good agreement with the reconstruction of Thomas et al. (2017)."

As expected as Thomas is assimilated.

P18

"who suggest an increase of the SMB sensitivity to surface temperature for the future in Antarctica,"
Can you give a number?

"The GCMs may have biases in the simulated temperature changes or in their response to anthropogenic forcing."
This is very general, what are the known biases in GCMs?

"This may contribute to an overestimation of the contribution of the simple thermodynamic link between temperature and precipitation and thus snow accumulation while it underestimates the role of changes in atmospheric circulation variability."
Any reference on this point?

"According to Neukom et al. (2018), uncertainties in the reconstructions (the noise in proxy data and the deficiencies in the reconstruction methods) and the data sampling could be an explanation of the observed discrepancy between models and reconstructions."
Give some key details on how it is proven.

"surface temperature over the period 1958–2010"
Add the reference (Nicolas and Bromwich, 2014)

P19

"Regarding changes in SMB over the last two centuries, our reconstruction shows large regional differences in SMB trends, both in magnitude and in sign, in accordance with Medley and Thomas (2019; Fig. S12)."
A word on the fact that DA assimilate Thomas 2017, which use the same ice core dataset as in Medley and Thomas 2019? So it is not surprising that patterns are similar?

"This is supported by a strong link between these two variables in observations, in particular for East Antarctica (r=0.82, statistically significant)."
Specify that is is between Thomas et al 2017 and NB14, and does not work with Stenni 2017

"By using data assimilation, no assumption such as stationarity or long calibration periods is required to estimate th link between variables"
Please also include the limitations of the data assimilation method.
th> the

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