This paper presents sensitivity studies with the atmosphere-only, stretched-grid GCM ARPEGE, forced by two present-day and strongly diverging end of 21st century sea ice and SST conditions from (bias-corrected) CMIP5 models. The results show that the Antarctic SMB is sensitive to Southern Ocean conditions, resulting from temperature and general-circulation changes. Although the paper contains some interesting results, it is very poorly written, contains factual errors, is and does not seem to come up with any clear answer to the problem posed in the title. I think it would require a very considerable effort from the authors to rewrite and strengthen the paper. I have decided not to focus on the language, but that doesn't mean that the paper needs a thorough check – it contains a lot of textual and grammatical errors! Instead, I will focus on (what I think are) the major issues with this paper, and hope the authors are able to improve the paper considerably. The only reason I decided not to reject is that I think the paper contains some interesting (but preliminary) results, but it will need to be thoroughly revised.

Authors' reply: We thank the reviewer for accepting to review the manuscript and for doing so rapidly. For the language, we will thoroughly check the whole paper again and have it read by (at least) one native speaker.

Major issues (in order of appearance)

Title: I think the title is a bit too general, and the paper does not really address it (see below for details). Something like: "Impact of two diverging scenarios of 21st century Southern Ocean surface changes on Antarctic surface climate and precipitation".

Authors' reply: The reviewer is right. We will change the title in « Impact of two diverging scenarios of 21st century Southern Ocean surface changes on Antarctic surface climate » or something close to this formulation. This corresponds to the actual content of the paper which was not the case for the submitted version.

Abstract: the abstract needs a few introductory and concluding sentences, introducing the problem and motivation, and giving some concluding remarks ('what did this study find, in relation to the title?')

Authors' reply: We will modify the abstract, add some introductory and concluding sentences and adapt it to the main findings associated with this study and its title.

Surface mass balance: is only one term of the mass balance; importantly, not the SMB causes a decrease in sea level, but the change (increase) in SMB, assuming solid ice discharge doesn't change. Since SMB and discharge are intimately linked, it is incorrect to describe SMB as a negative term contributor to sea level rise.

Authors' reply: Ok, in order to re-conciliate these considerations, we propose to rewrite this sentence in the following way "Assuming no associated response of the glaciers dynamics, the increase of the ice-sheet surface mass balance is the only significant projected negative contribution to SLR... »

P2, L28: ...allowing the use of cloud-resolving atmospheric model configuration. I think you mean 'preventing' instead of 'allowing'?

Authors' reply: No, we in-deed meant 'preventing', but we propose to rephrase this sentence in the following way in order to hopefully make it less confusing : "*The marginal importance of atmospheric deep convection for Antarctic precipitation does not require to perform dynamical downscaling at very high resolutions and the use of a cloud resolving atmospheric model configuration is therefore not particularly relevant for Antarctic climate projection. However, the added value of higher horizontal resolutions, such as for instance CORDEX-like simulations (Giorgi et al., 2016) at 0.44°, with respect to driving climate projection at coarser resolution (1 to 2°) from the CMIP5 ensemble is significant in coastal regions*".

P2, L33: higher horizontal resolution leads to higher estimates of snow accumulation. This is factually incorrect – actually, Genthon et al. (2009) suggest the opposite (see their Fig. 1). In addition, Lenaerts et al., 2017 do not find any significant impact of resolution on (integrated) SMB in the Amundsen region.

Authors' reply: " The reviewer is right, this is indeed factually incorrect for present-day snow accumulation estimation. In Genthon et al. (2009), it is also found that resolution has no significant impact for model run at sufficiently high resolution (< 3°). Using 27kms and 5.5 kms set up of RACMO, Lenearts et al., (2018) for the Amundsen region and Lenearts et al., (2012) for Adélie Land indeed found that the area integrated surface mass balance and the coastal-inland precipitation gradient were not significantly changed. One of their conclusion is that 27 kms seems to be a sufficiently high horizontal resolution to represent the coastal-inland SMB gradient in West and East Antarctica. These conclusions are possibly no longer valid when we jump from 200 kms resolution used in CMIP experiments to 30-40 kms horizontal resolution used for instance in Cordex-like experiment, our study or the work from Lenaerts and others. However, the part of this sentence about climate projection is not incorrect as Genthon et al., 2009 found a strong sensitivity of projected Antarctic precipitations increase to resolution (higher increase for higher horizontal resolution) especially for resolutions below 2° (see their figure 2). Result from Agosta et al., (2013) who used LMDZ4 model at a horizontal resolution of 60 kms and downscaled these climate projections with SMiHil model at 15 kms agree with these findings. To our knowledge, there is no publication suggesting no or opposite effect of higher horizontal resolution on Antarctic precipitation increase in a warmer climate. To be factually correct about the effect of horizontal resolution on present-day and future changes in snowfall, and re-conciliate the findings of each study cited here above, we propose to rewrite this part of the article in the following way :

"For present-day climate, Lenaerts et al., (2016,2018) found no significant differences in area-integrated SMB and coastal-inland snowfall gradient using 5.5 and 27 kms set up of RACMO model. Genthon et al., (2009) similarly found reduced impact of horizontal resolution when excluding very coarse (> 4°) model of the CMIP3 ensemble. For future climate projections however, much larger precipitation increases were reported when using climate model at higher horizontal resolutions (Genthon et al., (2009), Agosta et al., (2013)."

P3, L7: RCM. These random acronyms lead me to believe that the authors have been sloppy and have not sufficiently rechecked their manuscript prior to submission. Make sure these are defined when used for the first time.

Authors' reply: Ok, we have defined the RCM acronym at this place in the manuscript and checked carefully the introduction of new acronyms elsewhere.

P3, L18-29: This type of information does not fit in the introduction, it is far too detailed and should be moved to the methods.

Authors' reply: Ok, we will move the content of L18-29 and integrate it to the content of the "Data and Methods" section

Table 2: What are the units? What is the significance of these results, based on how much it varies in ERA-Interim over 1981-2010?

Authors' reply: Units are hectoPascals. We will perform some proper significance tests using the variability of sea-level pressure in Era-Interim, but these errors are likely to be significant as we plotted the significance (not shown) of ARPEGE sea-level pressure bias with respect to ERA-Interim and it is significant almost everywhere (at $p=0.05$) South of 20°S.

P7, L20: 9.5 Kelvin/km. Where does this lapse rate come from? It would require a reference to back up this number.

Authors' reply: A dry adiabatic lapse rate of 9.8 $K.km^{-1}$ (there was indeed a small typo here) is used for instance in Bracegirdle and Marshall (2012) to correct surface temperature from meteorological reanalysis in order to compare them with in-situ observations in Antarctica. We will refer to this publication to justify to use of this lapse rate.

P9, L8 and around: This temperature bias is highly concerning, and instead of simply removing these areas, I would advise the authors to try to explain (and remedy) this bias. My intuition is that ARPEGE is not well able to represent strong surface-based temperature inversions (which not be surprising as many climate models struggle with this). Also, these simulations will likely need to be redone with ice shelves (mind the spelling) considered in the land model – that will allow the authors to analyze the effect of changing ocean conditions on ice shelves (which are a super-important component of the Antarctic glacial system – and located closest to the ocean, so should be most sensitive!). In any case, the authors will need to come up with an explanation why the ice shelves are so warm in the model, will need to remedy that bias, and apply that to new simulations. The current bias is alarming, because there is no reason why this bias wouldn't apply to other regions on Antarctica – where this bias is potentially compensated for by other model biases (radiation, clouds, albedo,...)?

Authors' reply: The reviewer is right in stating the fact that this temperature bias is concerning. We found it concerning as well and we tried unambiguously to identify its origin. First, we verified if ice shelves are indeed treated as land surface in the model, we plotted surface sensible and latent heat fluxes (see figures below) as well as surface albedo (SWU/SWD) and from this point of view nothing is abnormal and it compares reasonably with the same fluxes in MAR. The reviewer is also right in it is intuition that the warm bias over ice shelves (in winter) comes from ARPEGE lack of skills to represent very stable boundary layer and associated strong near-surface temperature inversions (as many climate models do). To investigate this, we plotted the difference between air temperature at 20 meters and surface temperature. We can see that the magnitude of the near-surface inversion compares reasonably well over most of Antarctica except over the ice shelves where the pattern (too weak inversion in winter and too large in summer) seems to be seasonally and spatially consistent with the biases (or difference) in near-surface temperature with MAR. The seasonality of the biases is the same over the high Antarctic Plateau. The reviewer is also right in saying that in other parts of Antarctica, ARPEGE lower skills for boundary layer are slightly compensated for by other biases (or difference with respect to MAR). In another ARPEGE experiment, in which we corrected atmospheric general circulation using nudging towards reanalyses (other paper in prep.), the warm bias over the High Antarctic Plateau increased slightly (1-2 K) as result of a decrease of a negative downward LW bias in the ARPEGE-AMIP experiment, but this warm bias in winter with respect to MAR and observations (3 - 5K) is in any case not much higher than what many other GCMs or even meteorological reanalyses are showing for the near-surface temperature of the Antarctic Plateau. It seems that the exceptional characteristics of the large ice shelves (extremely large and flat surfaces with few roughness) highlight more than anywhere ARPEGE lack of skills for extremely stable boundary layers.

Moreover, we draw the attention on the fact that a part of the large difference (10-12K) between ARPEGE and MAR over ice shelves in winter also comes from a cold bias of the MAR model as in her evaluation against 12 stations over the Ross Ice Shelf (https://zenodo.org/record/1256079#.XIuPd5zjIUF), C. Agosta found a -2.8K cold bias. This can also be seen in our comparison over the smaller ice shelves of the Dronning Maud Land region, as the comparison with MAR-ERA-I (Figure 4, left) suggest a large (5 to 9 K) warm "bias" in ARPEGE, while the warm bias of ARPEGE with respect to Halley and Neumayer weather stations are respectively only +1.2K and +0.9K. We also draw the attention on the fact that warm "biases" in winter over ice shelves is slightly reduced over Ross Ice Shelf and almost completely disappears over Ronne-Filchner Ice Shelf when RACMO2 is taken as reference (see Fig. below). This highlight how much large discrepancies can still exist over ice shelves, even between polar-oriented climate models regularly used as reference for Antarctic surface climate and mass balance such as MAR and RACMO2.

We precise that it is virtually impossible for us to redo all our ARPEGE experiment as our collaborator and co-author of the paper Michel Déqué is now retired and we therefore currently have no available computer time on the Météo-France supercomputer. Besides, fixing ARPEGE issues for stable boundary layer is a work beyond the scope of this paper and is actually the subject of a PhD thesis currently undertaken at Météo-France. Even if we agree that this bias is concerning, we think that many climate models or even meteorological reanalyses (see Freville et al., 2014, Bracegirdle and Marshall, 2012) show biases of Antarctic surface temperatures (High Plateau or ice shelves) that are the same order of magnitude as the warm bias over large ice shelves in our ARPEGE simulations. This, however did not prevent these data to be published and unfortunately sometimes widely used. So, we agree on the following for the future versions of the manuscript: avoiding to hide these biases, trying to be more explicit about their origin and warn potential users over ARPEGE reduced skills over ice shelves, being more critical about the skills of models (MAR and RACMO2) used to evaluate ARPEGE. Unfortunately, restarting the simulation while remedying the bias over the ice shelves will not be possible. To evaluate with reduced uncertainties the impact of climate change over Antarctic ice shelves, we propose to use our ARPEGE future projections to drive regional climate models (e.g. MAR or RACMO2) that are more skilled for ice shelves surface climate, which is also currently a work in progress.

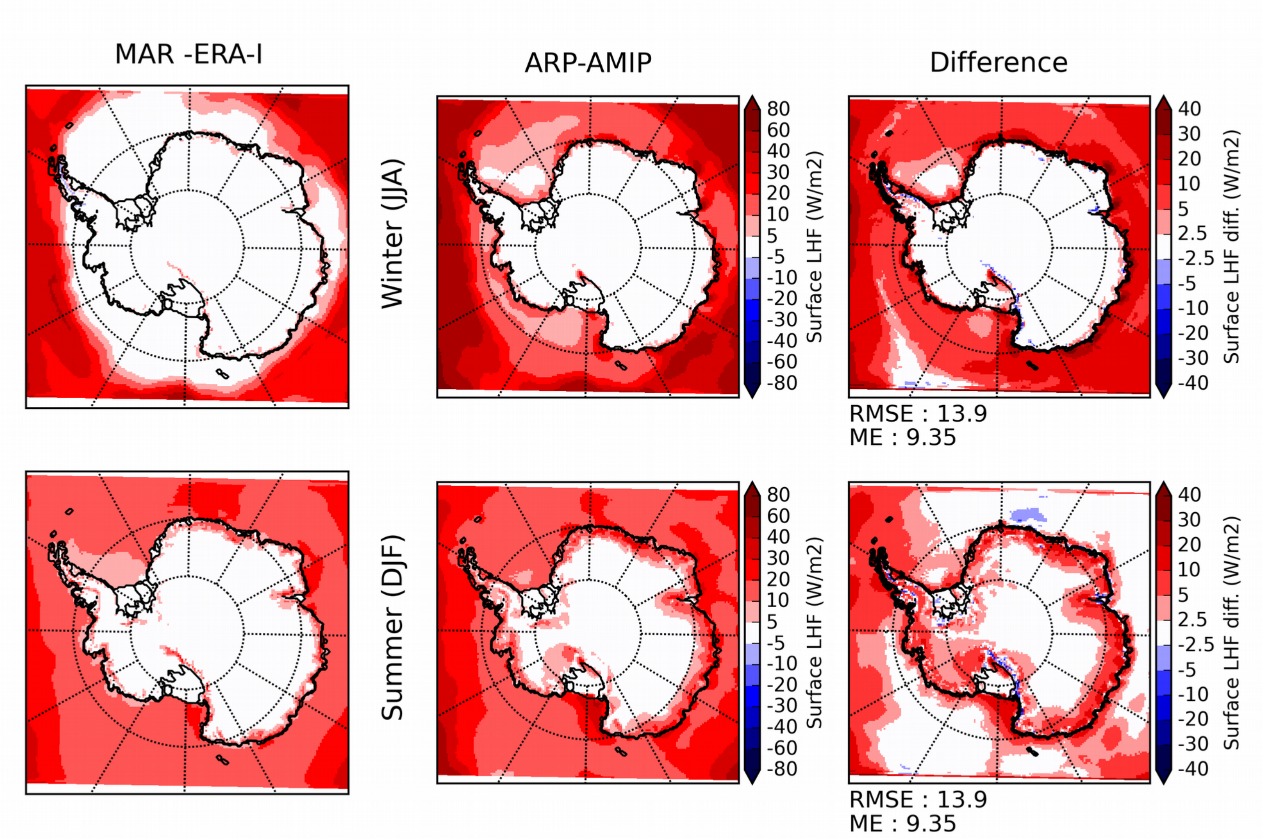

Fig 1: Surface latent heat flux (W.m^{-2}) for MAR forced by ERA-Interim (left), ARPEGE-AMIP simulation (centre) and ARPEGE minus MAR difference (right). Mean values for winter (JJA, bottom), mean values for summer (DJF, summer) computed for the 1981-2010 period.

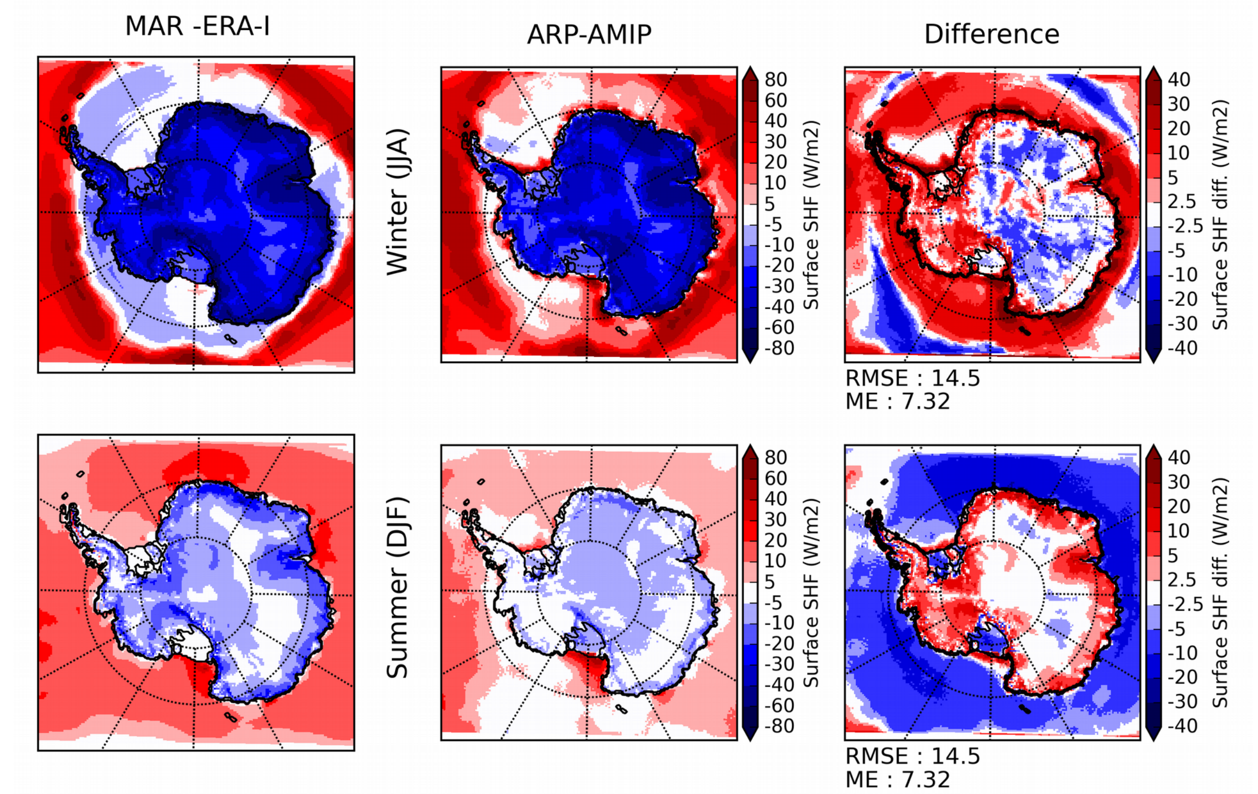

Fig 2: Surface sensible heat flux (W.m^{-2}) for MAR forced by ERA-Interim (left), ARPEGE-AMIP simulation (centre) and ARPEGE minus MAR difference (right). Mean values for winter (JJA, bottom), mean values for summer (DJF, summer) computed for the 1981-2010 period.

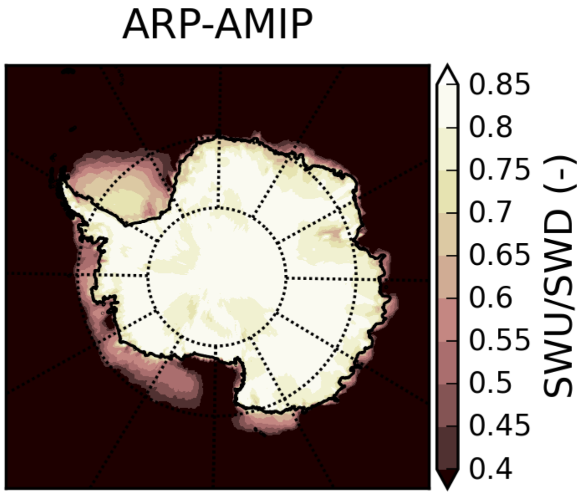

Fig 3: Mean summer (DJF) surface albedo (SWU/SWD) in the ARPEGE-AMIP simulation (1981-2010).

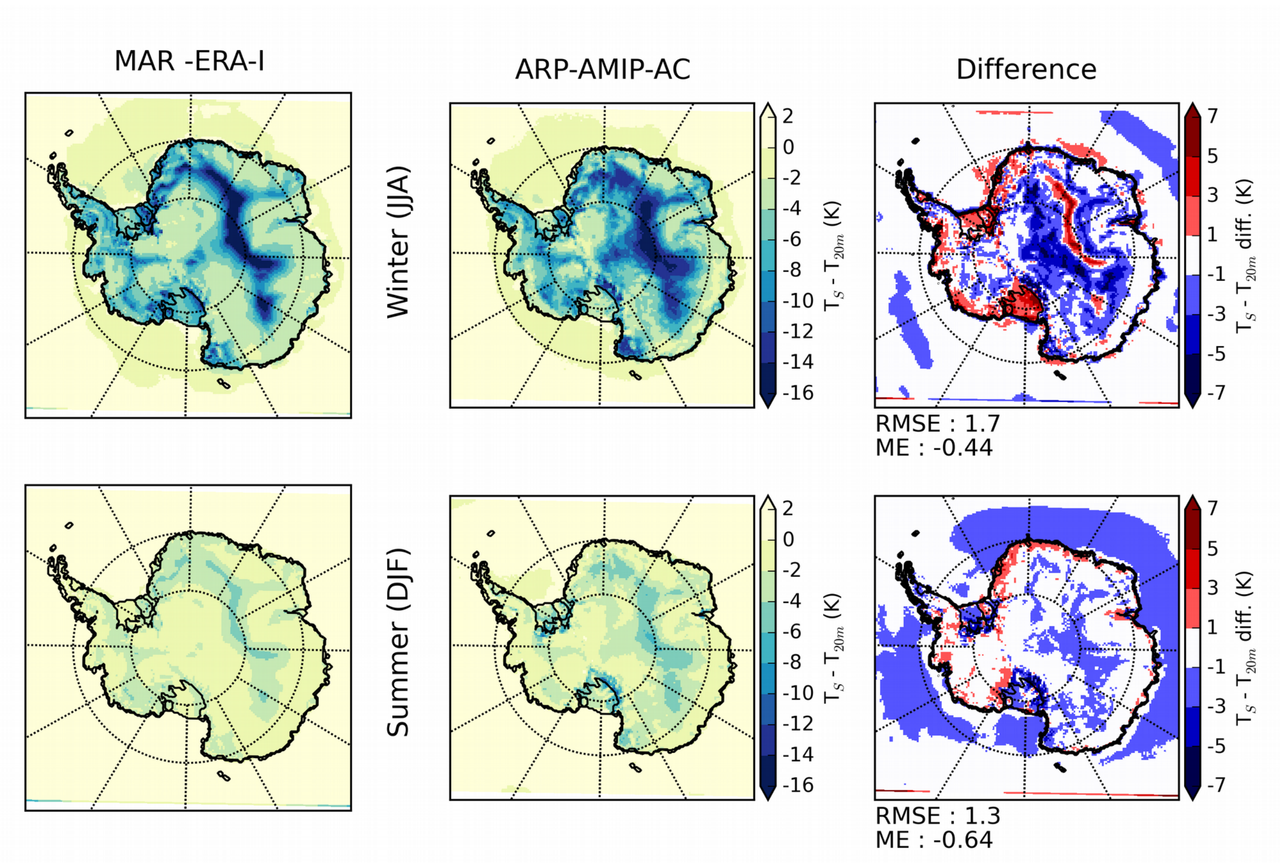

Fig 4 : Near-surface temperature inversion (T20m – Tsurf in K) for MAR forced by ERA-Interim (left), ARPEGE-AMIP simulation (centre) and ARPEGE minus MAR difference (right). Mean values for winter (JJA, bottom), mean values for summer (DJF, summer) computed for the 1981-2010 period.

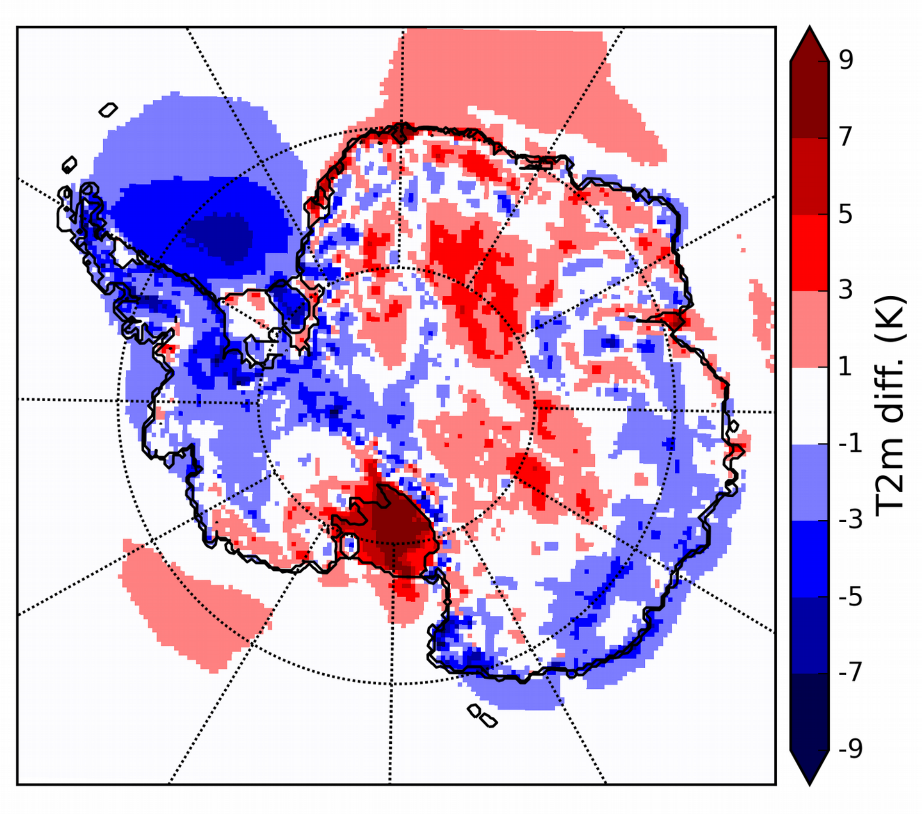

Fig 5 : ARPEGE-AMIP – RACMO2-ANT (Van Wessem et al., 2018) mean winter (JJA) 2 meters air temperatures difference over 1981-2010.

Table 3 is very poorly readable, enlarge and perhaps move to supplementary material. Again, don’t forget to mention units. Same for Table 4.

Authors’ reply: Ok, we will enlarge, reformat and put the units for Table 3 and 4. Table 4 will most likely be moved to the supplementary material.

P12, L12: this contradicts what was (falsely) mentioned in the introduction, as ARPEGE (the lower-resolution model) gives higher precipitation than MAR (the higher-resolution model)

Authors’ reply: It is incorrect here to consider that ARPEGE is the low resolution model compared to MAR. In the set up we used, (see figure below, that will be added to the supplementary material), the horizontal resolution varies from 30 kms near the stretching pole to about 45 kms over the northern end of the Antarctic Peninsula. We can thus consider the horizontal resolution to be fairly similar to the 35 kms horizontal resolution of the MAR simulation used for comparisons.

Differences for the AIS integrated precipitation between ARPEGE and MAR are here explained by differences in precipitation physics (in parts) and mostly (as we demonstrate in another publication in prep.) by errors in atmospheric general circulation in the AMIP-style ARPEGE simulation.

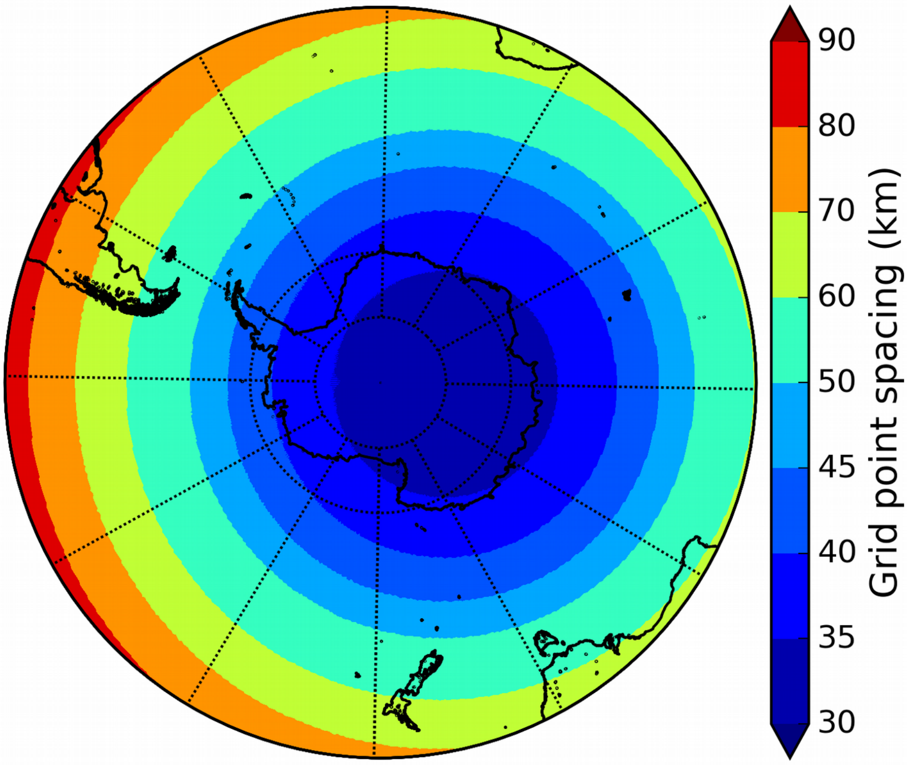

Fig 6 : Horizontal resolution (kms) of ARPEGE-T255 configuration with stretched grid over Antarctica.

P12, L18 and around: Runoff is the result of a complex interaction between atmosphere and snow conditions, and requires a sophisticated albedo and snow model, the latter which allows for percolation and refreezing of surface meltwater. The authors do not present any compelling evidence why the surface melt and runoff rates in ARPEGE are any realistic, which casts doubt on the reliability of simulated future melt and runoff rates. For example, Table 5 suggests that, on the grounded AIS, about one-third of the liquid water production (rain + melt) runs off in ARPEGE, which suggests that its snow model is not capable to retain and refreeze sufficient meltwater (for comparison: both MAR and RACMO2 produce almost no runoff with comparable liquid water production). I would therefore advise the authors to focus solely on precipitation and temperature, possibly surface melting (provided that the authors can show evidence of realistic surface melt patterns in the present-day simulation, compared to MAR for example), but refrain from analyzing future runoff changes.

Authors' reply: The reviewer is right in stating that ARPEGE is not able to represent the liquid water retention capacity of the Antarctic snow-pack and therefore the importance of refreezing which most likely yields an overestimation of run-off rates. This is possibly due to the fact that the first and second snow layers have an upper bound of 0.05 and 0.5m respectively as well as some possible density issues as ISBA-ES has been mostly calibrated using observations from temperate climate snow. Because of this, we agree on avoiding to analyse future runoff changes in our

ARPEGE simulation. Before producing the revised version of the manuscript, we will evaluate ARPEGE ability to reproduce the spatial distribution and inter-annual variability of Antarctic surface melt as the integrated value for present-day climate (31±19 to 55±34 Gt yr^{-1}) seems to be at first order roughly consistent with values from MAR (34±11 Gt yr^{-1}) and RACMO2 (46±16 Gt yr^{-1}). If the results of this analysis are encouraging, we will briefly comment future melt rates in our climate projections.

Table 6: Are these changes significant at all? What is the present-day variability? What is the relative change instead of / next to the absolute changes?

Authors' reply: An increase of the strength of the westerly wind maximum of 1.5 to 2 $m.s^{-1}$ represents a three to four times larger increase than the standard deviation of this quantity in present day climate in both ARPEGE-AMIP and ERA-Interim (respectively 0.5 to 0.6 $m.s^{-1}$) and this is a +15% to +20% increase with respect to the absolute value in ARPEGE-AMIP historical simulation (10 $m.s^{-1}$). A 0.8° to 3.8° southward shift represents about one to four times the standard deviation of the position of the maximum in ERA-Interim (0.8°). We will perform adequate significance tests and present the results in the table.

Conclusions: a concluding paragraph/section is missing on the actual conclusion of this work. What is the uncertainty of Southern Ocean conditions on Antarctic SMB? What is driving it? What is the impact of changing SIC vs. SST? What are the driving forces of the change in Antarctic SMB – the thermodynamic (i.e. increase in surface temperatures) or the dynamic (large-scale atmospheric circulation)? What is the impact of the radiative and turbulent fluxes? There are many open questions that the authors do not discuss, but that can be answered if the model simulations are analyzed in more detail.

Authors' reply: We will largely rewrite the conclusion in order to refer more precisely to the main findings of this paper. Regarding the impact of changing SST vs SIC., this has been done in other publications (Van Lipzig et al., 2002, Krinner et al., 2014, Kittel et al., 2018). We will refer to these publications in our discussion and if relevant, reinterpret our results in light of these studies. For future projection, we propose to analyze the evolution of the different terms of the surface energy balance (radiative, turbulent, surface sensible and latent heat fluxes…) in order to discuss their relative contribution to surface warming (and possibly melt).

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