Anonymous Referee #1

SUMMARY ===

The manuscript by Donat-Magnin describes a model experiment in which the future surface mass balance (SMB) and surface melt in the Amundsen Sea Sector of the Antarctic Ice Sheet is investigated. This is done so by using anomalies of a CMIP5 multi-model mean anomaly added to a present-day forcing to the regional climate model MAR.

This paper is interesting both for its results and for its methodology. The Cryosphere would be a logical venue for publication, and the subject is relevant for the journal. I am enthusiastic about the methodology (including the discussion of its shortcomings) and by the results.

I would recommend publication of this paper after care has been taken to improve the manuscript on the following points.

→ We thank the reviewer for this positive review and careful reading. Our responses are shown in blue.

GENERAL ===

Throughout the manuscript, the terms melt and runoff should be used with more care. It starts at P2 L18: This entire section makes sense but the use of "melt" and "runoff" needs to be more careful here. Runoff is produced only when water runs off into the ocean, and is lost from the ice sheet. In situations with ponding or hydrofracturing, leading to ice-shelf collapse, there is no runoff according to the definition, but only surface melt. If there were no ice-shelf collapse, the surface meltwater would refreeze. While runoff is currently probably about 1000 x smaller than SMB, surface melt is about 5% of SMB. It is not runoff but surface melt that triggers hydrofracturing. All over the manuscript, meltwater ponding and hydrofracturing is described as runoff, but it should be considered surface melt and not runoff, unless the water is really lost from the ice- sheet system. There are many instances with this confusion, like P10 L16 or P21 L14 and further.

→ We apologize for the poor use of the term “runoff” in the submitted version of our manuscript and we understand why it may have sounded puzzling. We believe that this is a problem of terminology rather than a misunderstanding of the physical mechanisms. What we meant by “runoff” was actually the excess of meltwater and rainfall with respect to the saturation of the snow/firn column and refreezing, which could be referred to as “surface liquid water budget” or “net production of surface liquid water”. Our MAR configuration removes this excess from the system (which is why we abusively called it runoff) because there is no representation of ponds or horizontal routing of liquid water. In the real world, the liquid water in excess can either form ponds, or flow horizontally toward crevasses or the ocean, but our modelling framework is not able to address the fate of this water. In our study, we used our “runoff” model variable to estimate the liquid water production beyond saturation of the snow/firn column and refreezing, not to estimate the actual runoff.

Following fair recommendations from the 3 referees, we have reformulated all the paragraphs and figures mentioning runoff.

Regarding the use of melt rate instead of runoff, we make the point in this paper that surface melt rate cannot be the relevant variable for hydrofracturing, because most melt water is retained in the annual snow layer (without ever saturating it, or, in other words, without depleting a substantial fraction of its air). Hence, in most cases, meltwater is not able to accumulate in ponds or to flow into crevasses (potentially inducing hydrofracturing) or the ocean (potentially inducing ice-shelf bending). So what we suggest in this paper is that there is only a potential for hydrofracturing ("potential" because there are also ice mechanical criteria, see, e.g., Lai et al. 2020) when the melt to snowfall ratio is high enough. Following comments from the other referees, we have also included additional comments on the role of rainfall.

We have prepared a revised version of the manuscript in which (i) we refer to the liquid water in excess as "net production of surface liquid water" instead of "runoff", and (ii) we better explain the connection with potential hydrofracturing.

SPECIFIC ===

Title: I would suggest to replace "Amundsen region" by "Amundsen sector"; remove or move the words "ice-sheet", and replace "melting" by "surface melt". So: Future surface mass balance and surface melt in the Amundsen sector of the West Antarctic Ice Sheet.
→ We have replaced with the suggested title.

Page 1 line 10: along -> during (or: in)
P1 L11: melting -> melt
P2 L2: hypothetically stable climate -> hypothetical, stable climate
P2 L8: ice cores -> firn cores
P2 L15: a -> an
→ All of these have been corrected as suggested.

P2 L31: surface melting -> surface melt (everywhere in the manuscript)
→ This has been corrected everywhere.

P2 L31: the exponential relation between temperature and surface melt is valid.
→ Ok, we have replaced "is expected to increase" with "increases".

P3 L13: perhaps replace the reference to Lenaerts et al. (2018) to Van Wessem et al., Modelling the climate and surface mass balance of polar ice sheets using RACMO2, part 2: Antarctica (1979–2016). The Cryosphere, 1–35 (2018). The latter is more of a benchmark publication for RACMO2.
→ We have added this reference.

P3 L14: slightly expand the text here to discriminate between forcing with reanalyses (Donat-Magnin, 2020, Fettweis 2013, Datta 2019) and GCMs (Trusel 2015, this paper).

→ We have slightly reformulated and expanded the text to better distinguish reanalyses and GCM forcing.

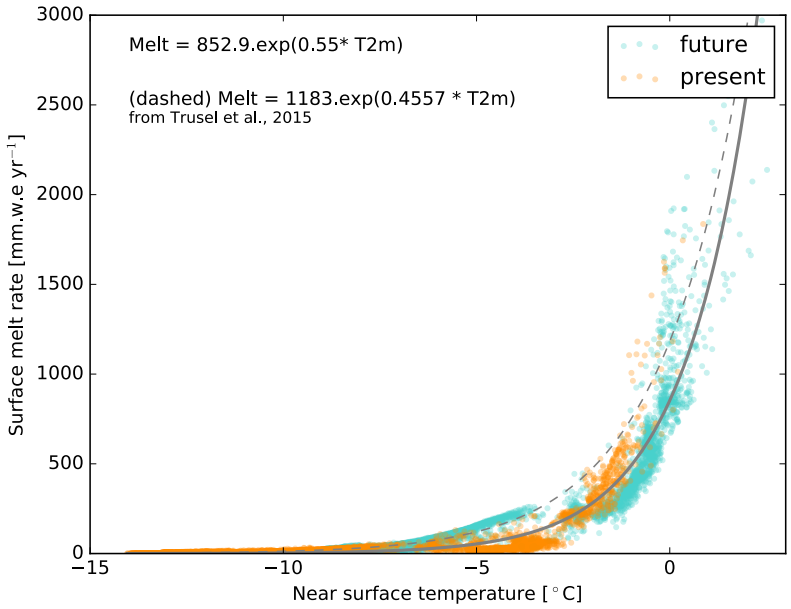
P4 L16: are these sensitivities based on climatological means or instantaneous values for temperature and wind speed?
→ They are based on instantaneous values at the time of snow deposit. This has been added.

P5 L8: move this listing of models to a table or appendix.
→ The list of models has been moved to Appendix A.

P7 L4: remove "potential" (doubles with possible)
→ We have removed “possible”.

P10 L16: referring to the above comment, lateral transport of meltwater (into ponds for example) is not runoff in an SMB definition. If it refreezes or remains in the ice sheet it is not runoff.
→ This has been rephrased (see our general response).

P10 L22: actually it would be very interesting to show the T-M relation for your model, along with the expression from Trusel et al. It gives insight to the sensitivity of your model melt to temperature compared to previous work. Please include a figure and brief discussion here.
→ Here is the temperature-melt relationship in our simulations compared to Trusel’s fit (dashed). In our case, each circle represents an ice-shelf grid point in the future or present-day simulation.



We obtain fit parameters that are slightly different from Trusel et al. (2015). However, we want equation (2) of the paper to be valid for various conditions as is the Clausius-Clapeyron formula. We have used Trusel’s fit because it was calculated over 48 ice shelves all around Antarctica and is therefore more likely to remain valid far beyond the present-day

Amundsen conditions. We then use equation (3) to calculate an alternative fit of the melt to snowfall ratio based on our own simulations, which is used to estimate the uncertainty on our climate extrapolations, but we directly fit the melt to snowfall ratio.

We have added the equation of our fit in this sentence: “Recalculating an exponential fit for melt rates in a similar way as Trusel et al. (2015) also gives a stronger sensitivity (MLT = 853 $exp(0.55\ T)$), which can be a specificity of either the Amundsen region or our model configuration.” But we have not included the figure as there are already similar figures in two papers (Trusel et al. 2015 and Kuipers Munneke et al. 2014) and a lot of figures in this paper.

P10 L29: remove "also"
→ It has been removed.