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Interactive comment on "Interannual Variability of Summer Surface Mass Balance and Surface Melting in the Amundsen Sector, West Antarctica" *by* Marion Donat-Magnin et al.

Anonymous Referee #1

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Summary – The authors present a significant amount of work on MAR model validation and ultimate evaluation of drives of summer surface mass balance (SMB) and melt over the Amundsen Sea sector of Antarctica. They provide ample background of previous studies of the climate in West Antarctica, MAR model description as well as numerous observations used in the evaluation, full descriptions of the climate indices investigated, and, of course, their findings. The model reproduced temperature, wind speed, SMB, and melt intensity and days, and thus, the authors indicate that MAR is sufficient to evaluate drivers of SMB and melt in this sector. Specifically, they find that the longitudinal position of the Amundsen Sea Low (ASL) is the primary driver (relative to ENSO and the Southern Annular Mode, SAM) of SMB variability, whereas the

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variability in the ASL central pressure and to a lesser extent ENSO drive melt with increasing control moving westward. The SAM was not strongly related to either SMB or melt. The authors finally surmise that there might be a 6-month lag between ENSO and West Antarctic climate via either sea ice anomalies and their transport or a lag in Circumpolar Deep Water intrusions.

The paper is generally well written and very thorough (much appreciated!) and is organized in a logical fashion. Many of the insights are not necessarily new; however, the paper presents new model work and output and attempts to present a cohesive picture of drivers of West Antarctic climate. All of the analyses presented appeared appropriate and well thought out, and the tools used were appropriate. The work presented is very thorough yet easy to understand, making it a pleasure to read.

Major Comments

One important consideration that is missing is a description of why summer SMB is critical and how it relates to the annual SMB. December-January-February (DJF) clearly are the relevant months for surface melting, but I think there should be more discussion as to why the paper specific isolated summer SMB. Specifically, melt is the highest in DJF, but snowfall is typically the lowest in DJF (Lenaerts et al., 2012). Please consider evaluation of winter (or other seasons of SMB relevance) or add language justifying the importance of summer SMB.

The relationship between melt and SMB is not investigated. The paper provides background on the importance of the role of melt on hydrofracture of ice shelves and potential rapid disintegration of an ice shelf, but it does not discuss the role of firn pore space. According to Table 2, nearly all of the surface melt refreezes within the firn column, so this mechanism should be introduced as well. The paper also notes that into the future there will be more snowfall and melt, but did not mention that the enhanced snowfall could potentially also provide more pore space for meltwater infiltration and refreezing. Please consider additional discussion of the role of SMB (or snowfall) on

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providing addition pore space for surface melt.

There is no discussion on the relatively small proportions of variance explained by the climate indices. For instance, over western WAIS 20-40% of the summer SMB variability can be explained by the ASL longitude; however, it explains <6% of the Abbot, Cosgrove, and Pine Island catchments. None of the indices is significantly correlated with SMB over those catchments. Thus, the impact of ASL longitude is only relevant from Thwaites moving westward. The paper should make this clear and also potentially investigate other drivers of change for the eastern catchments or at least add clarifying statements that the drivers in eastern WAIS are unknown and potentially postulate why. Along similar lines, while ASL central pressure is a clear control on all catchments, the explained variance range from 12-21%, suggesting that there are additional factors at play when it comes to surface melt. Would investigation of multiple regression with the different indices help clarify how they interplay (for example, perhaps the combination of some movement and strengthening or weakening of the ASL is more strongly related). Please consider adding more multivariate relationships and discuss other potential influences on meltwater production since only a small portion is explained.

The postulation of potential lags is not adequately investigated. The hypothesis regarding sea ice reduction and transport from the Ross Sea could be tested as MAR using the sea-ice concentration from ERA-Interim. Thus, please consider adding analysis of sea ice concentrations to support this postulation. Although not as clear cut, intrusion of marine CDW could be evaluated by looking at the effective wind stresses as done by Steig et al. just off the continental shelf and attempt to quantify a six-month lag between strong wind events and surface melt. Also, there is no mention of potential preconditioning of the snowpack/firn for melt. An additional important variable in control of surface melt in the summer is the amount of snow that fell the prior winter, and it should be added to the analysis presented and included in Table 4. This signal might not matter at all, but also could lead to misinterpretation of an ENSO lag. Please consider all potential snowpack preconditioning variables that might explain melt from

year-to-year.

Minor Comments

Line 16 – change to “Amundsen Sea glaciers”

Line 58 – change “underlaying” to “underlying”

Line 114 – remove the ‘;’ at the beginning of the line

Line 140 – change “estimates” to “estimate”

Line 185-186 – remove the sentence “These data were collected over the Thwaites and Pine Island basins.” as it is redundant with Lines 181-182.

Section 2.3: Are these indices derived from ERA-Interim for consistency with the MAR output? If not, please state that and justify their use.

Line 273 – Are “overestimate” and “underestimate” confused? Shouldn’t it be “The model tends to underestimate and overestimate highest and lowest wind speeds”?

Line 299 – Remove “(Medley et al. 2013, 2014)” as it is already mentioned in the sentence.

Line 382 - add “is” after “mechanism”

Figure 10/11 – Please add in the legend that blue represents moisture convergence for clarity.

Paragraph beginning with 530 – Perhaps it is important to mention here that DJF makes up the smallest percentage of annual accumulation, so it is not surprising that the findings do not match Medley and Thomas.

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