**Response to both reviewers**

We thank both reviewers for their mostly positive and very constructive reviews. Many of their questions raised very interesting points which motivated new analyses and opened new perspectives. We however did not include any new figures in the revised paper as the paper is already quite long, but we are happy to include these in the supplement. We plan to revise our manuscript according to the reviewers' detailed comments (highlighted in blue) as outlined below. We also corrected all typos. Proposed modifications of the manuscript are given in italics.

## 1 Response to Reviewer 1

This manuscript presents a low-complexity energy and mass balance model for the surface of the Greenland ice sheet. Special attention is given to a parameterization of the diurnal cycle of melting and refreezing, based on a previous implementation of a very similar model, published by the same authors. Different climate boundary conditions are used to test the performance of the model. The text is complete and pleasant to read. The figures are adequate. I think the results are interesting and that this new model is a valuable addition to the existing literature. My comments are mostly minor but they include some questions on the underlying equations. In general, I would like to read more about the expected use cases for this new model. How fast does it run? Is there room for future improvements or would they slow it down too much? If dEBM is used in the framework of AWI-ESM or other AOGCMs, is the computational speed relevant compared to the climate model? What is the reason for the focus on monthly input data when GCMs can supply daily data? I believe that most of these questions will be very easy to answer but they are valuable as context.

We have added to the first paragraph of section 6: *"In its Fortran version the computational cost of the actual dEBM is similar to the cost of the necessary interpolations with existing interpolation weights. It takes about 5 seconds to compute the SMB of one year for a configuration with 360000 gridpoints on a Cray CS400. A matlab version of the model simulates the 1979-2016 SMB of the GrIS at 1km resolution in approximately 30 minutes on a Linux desktop PC. Requiring only monthly forcing also provides for an uncomplicated interface, as monthly forcing usually is more accessible in case of completed transient climate simulations such as simulations of the CMIP5 project (Taylor et al., 2012)."* We also list some possible use cases at the end of the same section.

### 1.1 Response to Detailed Comments

figure 2: The distribution appears to also be bimodal in air temperature.

In the PROMICE data, the temperature distribution does not appear to be particularly bimodal. We have analysed PROMICE weather station data and defined monthly mean air temperatures $T_{fair}$ and $T_{cloudy}$ analogously to $\tau_{fair}$ and $\tau_{cloudy}$, and we do not find a clear separation between the two modes (Fig. AC1-1).

fig. 4, 5: Are good matches in variability and mean mutually exclusive? Why (not)?

They are, and we propose that this is due to some systematic bias in the precipitation forcing. Due to the relative coarse resolution of the ERA-Interim reanalysis, it is plausible that the interpolated precipitation forcing systematically underestimates small-scaled maxima in accumulation over the margins of the ice sheet. In consequence, optimizing our scheme to the mean SMB would likely provide optimal parameters, that underestimate melting in compensation. On the other hand, the SMB variability and the multi-year trend will be primarily influenced by melt variability and will be less sensitive to such systematic bias related to the resolution of the forcing. Thus, we here decided to optimize the scheme only to spatial and temporal variations around the respective means of the local and integral observations. This rationale is supported by the finding that using MAR precipitation of higher resolution results in a substantially lower bias to the mean local and integral observations.

We have improved the respective paragraph at the end of section 3.1, which was intended to motivate this aspect of our calibration strategy and moved it to the beginning of section 3.2: *"For the evaluation of the dEBM scheme we take into account that the precipitation forcing is possibly biased: as precipitation is interpolated from the coarse resolution ERA-Interim data, the intensified snow accumulation at the slopes and margins of the ice sheet may be systematically underestimated. Also low accumulation rates in the interior may be relatively inaccurate in the ERA-Interim reanalysis."*

fig. 9: dEBM underestimates both melting and refreezing as compared to MAR. What consequences does that have for the
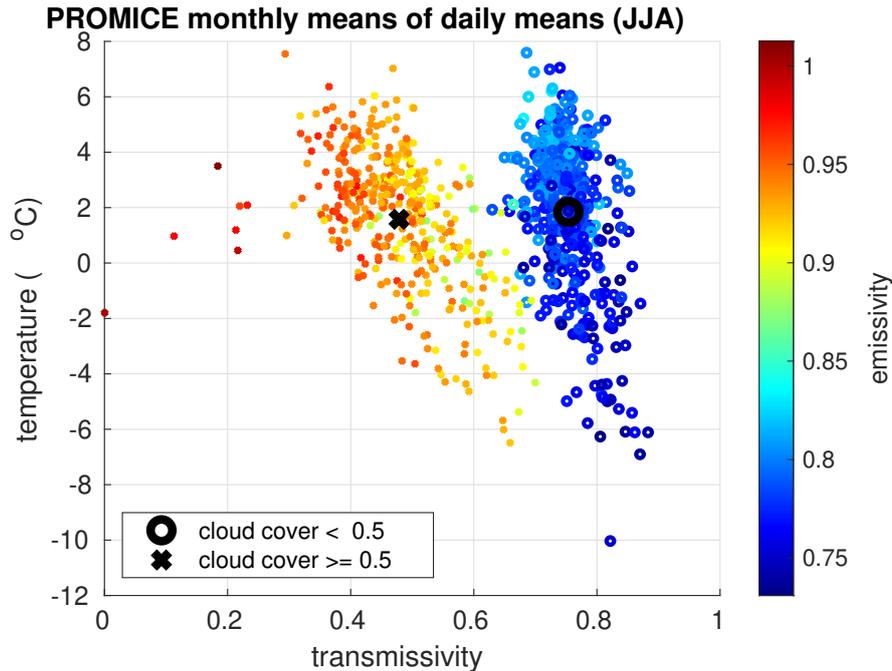
**Figure AC2-1.** Monthly mean air temperatures of cloudy and fair days, $T_{cloudy}, T_{fair}$ as a function of the respective transmissivity $\tau_{cloudy}, \tau_{fair}$ of all summer months as calculated from up to 11 years of daily observations from 17 PROMICE weather station. Every symbol represents the respective parameters as diagnosed for one individual month at one station. Colors reflect the respective monthly mean emissivities of cloudy and fair days respectively.

sensitivity under different climate boundary conditions?

Due to its monthly forcing and crude representation of the snow pack, dEBM has obvious limitations near the transition from ablation to accumulation zones. On continental-scale ice sheets we expect that these limitations are mostly restricted to the upper boundary of the ablation zone, being narrower where the slope is steep. As the upper ablation zone tends to become steeper
5   in global warming scenarios (Goelzer et al., 2020) we expect that uncertainties in the transition zone will not increase with warmer climates. To some extent these considerations are already part of the discussion, and given the length of the manuscript we would like to not extend it any further.

fig. 10: Albedo too high in dEBM, especially in low accumulation North East where snow aging is important.

This is true and we address this in the last paragraph of section 4.2. We have take this up and now state: *"...while albedo is*
10  *mostly overestimated in the North where accumulation rates are low and snow aging is most important."*

p2, l3ff: You could cite Aschwanden et al. (2019) in this paragraph for showing that calving becomes gradually less important in the future.

Will do.

p2, l15: The purpose of this sentence is not clear

15  We rephrased the preceding and following sentence: *"However, changes in insolation due to long-term changes in the Earth's orbit can influence the sensitivity of the SMB to temperature (van de Berg et al., 2011; Robinson and Goelzer, 2014). Also, field measurements from glaciers outside of Greenland reveal that optimal parameters for the PDD scheme strongly differ for different latitudes, altitudes, or climate zones (Hock, 2003). Therefore it is questionable if the empirical, Greenland based parameterization can be applied to ice sheets outside of Greenland (e.g. the ice sheets of the last ice age) or in different climates."*

20  p2, l20: Avoid brackets

OK.

p2, l26: Please consider referencing van de Berg et al. (2011) and Robinson and Goelzer (2014). van de Berg et al. could also be discussed in section 5.3.

We do so now.

p2, l32: "around zero", replace with "melting point"

OK

p3, l1: The optimal resolution is a matter of debate. I do not think that we can reliably say that 10 km is enough.

We change this to *"Therefore, melting occurs in a narrow strip along the ice sheets' margins which requires a resolution that is still beyond the scope of multidecadal global climate simulations or reanalysis products."*

p3, l4: Typo: Noël

OK.

p3, l 28: "the first section" This should probably read "second" or "following"

We have changed this to the "following".

p3, l33: remove "used in the following"

OK.

p4, l9ff: I found the three paragraphs at the center of the page hard to follow, possibly because they invert the logical progression of the following sections (2.4 -> 2.3 -> 2.2). Also, they contain several assumptions and it is not always clear if they have been tested, maybe in previous work, or if they are otherwise justified.

We tried to improve and shorten this paragraph, however, we kept the approach to first outline the general concept in a bottom-up fashion, to then develop the model top-down.

p4, l15: Within the two clusters there is a clear range of temperatures (Fig. 2), probably related to an implicit spatial signal. Please discuss how this impacts this important assumption. In addition, please discuss how the limited coverage of the PROMICE stations may skew the results.

The range of temperatures is predominantly related to the elevation range of the PROMICE stations (Fig. AC1-2). The cloud thickness may be reduced at high elevations and $\tau_{cloudy}$ is therefore elevation dependent. For $\tau_{fair}$ (the empirical parameter in our downscaling) the elevation effect is small by comparison. The temperature dependence in emissivities $\epsilon_{fair,cloudy}$ is in part related to the larger water vapor content of warmer air and is implicitly accounted for, as we do not constrain $\epsilon_{fair}$ but only prescribe $\Delta\epsilon$. Note, that the fractionated radiative fluxes of the cloudy and fair days are always consistent with the monthly mean forcing and the scheme is actually not very sensitive to the specific choice of its empirical parameter $\Delta\epsilon$ and $\tau_{fair}$ as long as these are modified consistently (e.g. if we assume that fair days are completely cloud free with $\tau_{fair} \approx 0.8$, then cloudy days must be also completely overcast and $\Delta\epsilon \approx 0.21$, Sedlar and Hock (2009)).

p4, l31: "implicates" -> "implies" (throughout the paper)

OK

p5, l8: typo: temperature; Does this statement concern monthly average values?

OK, and yes we now specify that this is monthly mean temperature, here.

p6, l1: This is a strong statement that requires a reference.

We changed this to: *as outgoing longwave radiation usually dominates the energy balance of a cold surface in clear nights (section 2.3).*

p6, l7: Maybe "upper limit" is a better phrase than "bounded above"

We changed this to "limited"

eq. 4: "mth" is not defined. I suspect is is the month, but that would be inconsistent with equation 14. I suggest to find a suitable symbol. Also, the square brackets are not necessary, I think.

We now use m for month throughout the text and also added m to the appendix.

p6, l15: "is" -> "was"

This sentence was replaced by *"Other contributions to the SMB such as sublimation, evaporation, and hoar are so far neglected by the dEBM as it is not expected that our downscaling approach can improve the respective mass fluxes from climate models, which simulate these processes on larger spatial scales but shorter time steps. With minor technical modifications, these fluxes can be individually added to snow fall $SF$ and rain fall $RF$ as an additional forcing (negative net snow fall will not pose a problem)."*

**PROMICE monthly means of daily means (JJA)**
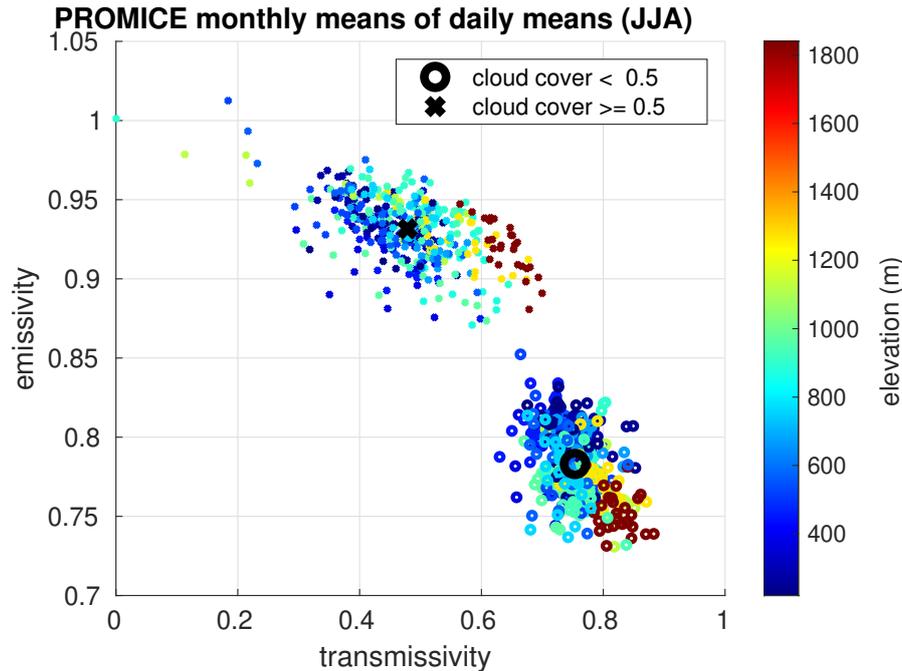


**Figure AC2-2.** Monthly mean emissivity of cloudy and fair days, $\epsilon_{cloudy}, \epsilon_{fair}$ as a function of the respective transmissivity $\tau_{cloudy}, \tau_{fair}$ of all summer months as calculated from up to 11 years of daily observations from 17 PROMICE weather station. Every symbol represents the respective parameters as diagnosed for one individual month at one station. Colors reflect the elevation of the respective weather station.

section 2.3: Almost every section starts with a reference to Krebs-Kanzow et al
We have removed two.
eq. 5: Why is the linearization around T0 needed? The presentation of these central equations would greatly benefit from a more detailed description of the individual terms. The term containing "a" contains the downward longwave flux, but I do not
5  understand why this term contains the emissivity of ice. This is also included in Krebs-Kanzow et al. (2018b).
The linearization is not needed indeed, but does not substantially affect the accuracy and simplifies the preprocessing and estimation of the elevation angle. We kept it because it allows to directly compare our scheme to empiric temperature index schemes. As the melt scheme is already published, we do not want to go into detail here. The emissivity of ice comes into play as we assume that the ice absorbs longwave radiation according to its emissivity (a good absorber is also a good emitter).
10  p6, l19: missing: "at THE melting point"
OK.
p6, l27: Why not use the LW down forcing directly?
The emissivity needs to be downscaled via emissivities to be consistent with downscaled temperature, also emissivities are needed to fractionate the radiative forcing.
15  eq. 7: sigma has not yet been defined. This is inconsistent with the Stefan-Boltzmann constant.
We have changed the nomenclature and now define $PDD_{3.5}$ in section 2.4.
eq. 7: I am confused over the units of SW, Q, LW, etc. The second line on equation 7 suggests that $SW_{MP}$ is smaller than $SW_{fair}$, because both the fraction of times and the q factor are smaller than one. This makes sense if SW is an energy. However, SW is also used in equation 5, where the second term uses the (linearized) Stefan Boltzmann law. It should thus have the
20  unit power, i.e., energy per unit time.
This was indeed not clear, we now state explicitly that *"$Q_{MP}$ represents a monthly mean energy flux"*
and rephrased the following paragraph to*"$T_{MP}$ is the near-surface temperature $T_{MP}$ during the melt period. $T_{MP}$ is param-*

**4**

*eterized by the positive-degree days per month as defined in section 2.4. The $\Delta t_\Phi$ is the length of the melt period when the sun exceeds the elevation angle $\Phi$. The ratio $\frac{\Delta t_\Phi}{\Delta t}$ converts the energy flux during the melt period to daily fluxes. The $q_\Phi$ is the ratio $\frac{SW_\Phi}{SW_0}$ (surface short-wave radiation averaged over the daily melt period relative to short-wave radiation averaged over the whole day). The parameters $q_\Phi$ and $\Delta t_\Phi$ are functions of the elevation angle $\Phi$, which is here calculated locally as we use spatially variable atmospheric emissivity."*

section 2.4: It might help to move the information about pre-processing further up.

We would like to keep the top-down structure here.

p7, l24: add: "...law DOWNWARD longwave radiation can be..."

OK

p8, l2: I think $H_int$ has not been introduced yet (see line 8).

We now introduce $H_{cr}$, $H_{int}$ and $H$ together with the forcing in section 2.1.

p9, l4: I think $\tau_{cloudy}$ is missing in the second equation.

No, we don't need $\tau_{cloudy}$ here, as we can diagnose $SW_{cloudy}$ from $SW_{fair}$, $SW$ and $CC$ according to the fractionation. We understand that this paragraph was hard to understand and we have reformulated the first part:

*"We fractionate downward longwave radiation and shortwave radiation for fair and cloudy conditions:*

$$
\begin{aligned}
SW\downarrow &= CC\,SW^{\downarrow}_{cloudy} + (1-CC)SW^{\downarrow}_{fair}\\
LW\downarrow &= CC\,LW^{\downarrow}_{cloudy} + (1-CC)LW^{\downarrow}_{fair}
\end{aligned}
\tag{1}
$$

*To avoid numeric problems, we only apply this separation, if monthly cloud cover is in the range of [0.1 0.9] and otherwise use unseparated $SW\downarrow$ and $LW\downarrow$ to calculate the energy balance $Q$, accounting (not accounting) for the diurnal melt period during the entire month, if $CC < 0.1$ ($CC > 0.9$), respectively.*

*Where we separate cloudy and fair conditions, we need to introduce two additional assumptions which here are based on an analysis of PROMICE automatic weather station data (Ahlstrom et al., 2008). Specifically we analyze daily radiation, cloud cover, and air temperature observations from 17 stations, which cover up to 11 years (Fig. 2). Applying equation 8 we diagnose distinct atmospheric emissivities $\epsilon_{fair}$ and $\epsilon_{cloudy}$ for fair or cloudy conditions and similarly we diagnose atmospheric transmissivities $\tau_{fair}$ and $\tau_{cloudy}$ according to*

$$
SW^{\downarrow}_{fair,cloudy} = \tau_{fair,cloudy}\widehat{SW}
\tag{2}
$$

*with atmospheric transmissivities $\tau_{cloudy}$ and $\tau_{fair}$ for fair and cloudy conditions.*

*...*

*To separate longwave radiation, we constrain atmospheric emissivities by defining $\Delta\epsilon$ to be the emissitvity increase due to cloud cover with*

$$
\epsilon_{cloudy} = \epsilon_{fair} + \Delta\epsilon.
\tag{3}
$$

*."* eq. 13: So $\epsilon_{fair}$ and $\epsilon_{cloudy}$ are both smaller than $\epsilon_a$?

Thank you for spotting this! Of cause there is a typo in the second equation and this should be:

$$
*\quad
\begin{aligned}
\epsilon_{fair} &= \epsilon_a - CC\,\Delta\epsilon\\
\epsilon_{cloudy} &= \epsilon_a + (1-CC)\Delta\epsilon.
\end{aligned}
\tag{4}
$$

p10, l9: "PDD" twice

We rephrased this.

p10, l10: Does that mean that $T_M P$ is not a temperature? The same equality between T and PDD is used in equation 7.

We now clearly define PDD to be the positive-degree days per month.

eq. 14: Inconsistent use of dT and "-1". See also "-12" on the second equation on the same page

We now use $\Delta t_m$ as the length of month m throughout of the paper.

p11, l11: remove "choice of the"

We replaced it with *"The albedo parameterisation...".*

p12, l5: This should reference equation 5, not 6.

corrected.

p12, l6: Does R not have units? Why were no positive values tested?

5    We added W/m2. We introduced R to primarily account for the heat flux to lower layers, that is why it was initially tested for negative values only. We have now extended the range of R to $[-2, -1, 0, 1, 2]\,\mathrm{W\,m^{-2}}$ and updated the figures accordingly.

p12, l8: Please include information about the grid resolution earlier in the ms., e.g., in section 2.4.

We now include the necessity to define a target grid in section 2.1: *"Furthermore a target grid of sufficient resolution needs to be defined and respective high-resolution surface elevation data H need to be available."*

10    The model does not require a specific resolution.

p13, l10f: Why are the biases negative. Is this shown in figure 5?

Unfortunately, Fig. 5 displays the data-model difference but the text refers to the model-data bias. We apologize for this inconsistency. In the revised version Fig. 5 will be corrected accordingly.

p14, l1: The best value of R is at the extreme of the tested range. See comment above about positive anomalies for R.

15    See reply above.

p15, l18: The biases cancel out, but their existence indicates that important physical processes are not captured. Please discuss.

We added *" and missing processes such as snow aging, which may particularly bias late summer melt"*

p16, l3: "multitude of reasons" I agree, but we need more detail here given the seasonal biases that may point to missing processes that are important under different climate boundary conditions.

20    We provide a list of processes which we expect to be most relevant *"the simplicity of the dEBM albedo scheme, unresolved sub-monthly variability or the (neglected) effect of humidity and high wind speed on turbulent heat fluxes which will be important at coastal locations."* and also discuss missing processes in section 6. Furthermore, it is somewhat promising that the extreme year of 2012 is well represented in our simulation (Fig. 6). Even though the model demonstrates a good skill, one should keep in mind that it was originally designed to replace the most simplistic PDD interface, which is still frequently used in ice sheet

25    modelling on multi-millennial timescales. We therefore now state at the beginning of section 2.1: *"The model is formulated with a focus on the ablation zone; if surface conditions do not favour surface melt, the surface mass balance is exclusively controlled by the accumulation of snow."*

p16, l5: I disagree with this assessment. Sub-monthly variations may play a role, but the lack of a snow aging algorithm is likely also important in the dry interior of Greenland. This is consistent with the anomalously high albedo (line 12).

30    True, snow aging will be important in the interior and we have extended this sentence to: *" This is likely related to unrepresented sub-monthly temperature variability, as temperatures exhibit stronger variability at high elevations (Fausto et al., 2011) and the constant albedo for dry snow which cannot account for snow aging in low-accumulation regions of the interior GrIS."*
It should be however noted that the albedo scheme includes some kind of memory effect. We therefore added to section 2.6: *"The scheme first tests whether the new snow of that month is likely to survive. If this is not the case the scheme includes some*

35    *element of persistence: if snow was wet (dry or new) in the previous month it is first tested whether conditions allow that the surface remains wet (dry).*

p21, l17: Is this grid different from the one above? Please make this explicit.

Yes, we changed this to: *"The AWI-ESM forcing is here downscaled to an equidistant 5km grid in contrast to the 1km grid used in the previous section."*

40

# References

Ahlstrom, A. P., Gravesen, P., Andersen, S. B., van As, D., Citterio, M., Fausto, R. S., Nielsen, S., Jepsen, H. F., Kristensen, S. S., Christensen, E. L., Stenseng, L., Forsberg, R., Hanson, S., Petersen, D., and Team, P. P.: A new programme for monitoring the mass loss of the Greenland ice sheet, Geological Survey of Denmark and Greenland Bulletin, pp. 61–64, 2008.

5  Fausto, R. S., Ahlstrom, A. P., Van As, D., and Steffen, K.: Present-day temperature standard deviation parameterization for Greenland, Journal of Glaciology, 57, 1181–1183, 2011.

Goelzer, H., Nowicki, S., Payne, A., Larour, E., Seroussi, H., Lipscomb, W. H., Gregory, J., Abe-Ouchi, A., Shepherd, A., Simon, E., Agosta, C., Alexander, P., Aschwanden, A., Barthel, A., Calov, R., Chambers, C., Choi, Y., Cuzzone, J., Dumas, C., Edwards, T., Felikson, D., Fettweis, X., Golledge, N. R., Greve, R., Humbert, A., Huybrechts, P., Le clec'h, S., Lee, V., Leguy, G., Little, C., Lowry, D. P., Morlighem,

10  M., Nias, I., Quiquet, A., Rückamp, M., Schlegel, N.-J., Slater, D., Smith, R., Straneo, F., Tarasov, L., van de Wal, R., and van den Broeke, M.: The future sea-level contribution of the Greenland ice sheet: a multi-model ensemble study of ISMIP6, The Cryosphere Discussions, 2020, 1–43, https://doi.org/10.5194/tc-2019-319, https://tc.copernicus.org/preprints/tc-2019-319/, 2020.

Hock, R.: Temperature index melt modelling in mountain areas, Journal of Hydrology, 282, 104–115, https://doi.org/10.1016/S0022-1694(03)00257-9, 2003.

15  Robinson, A. and Goelzer, H.: The importance of insolation changes for paleo ice sheet modeling, The Cryosphere, 8, 1419–1428, https://doi.org/10.5194/tc-8-1419-2014, https://tc.copernicus.org/articles/8/1419/2014/, 2014.

Sedlar, J. and Hock, R.: Testing longwave radiation parameterizations under clear and overcast skies at Storglaciären, Sweden, The Cryosphere, 3, 75–84, https://doi.org/10.5194/tc-3-75-2009, https://www.the-cryosphere.net/3/75/2009/, 2009.

Taylor, K. E., Stouffer, R. J., and Meehl, G. A.: An overview of CMIP5 and the experiment design, Bulletin of the American Meteorological

20  Society, 93, 485–498, 2012.

van de Berg, W. J., van den Broecke, M., Ettema, J., van Meijgaard, E., and Kaspar, F.: Significant contribution of insolation to Eemian melting of the Greenland ice sheet, Nature Geoscience, 4, 679–683, https://doi.org/10.1038/NGEO1245, 2011.