Responses to the reviewer are given in **green***.*

General Comments Reviewer 2:

The paper presents an analysis of humidity flux for a site in the accumulation of the Greenland Ice Sheet. Data from summertime direct eddy-covariance (EC) observations, AWS-based bulk aerodynamic model estimates and output from a regional climate model (RCM) simulation are presented and discussed. The bulk estimates are found to give good agreement with summertime EC data. The RCM simulations are shown to have seasonally dependent biases. Correction functions are derived, and the corrected humidity fluxes used to comment on the importance of humidity fluxes for the surface mass balance. The topic should be of high interest to readers of TC and the paper presents a useful combination of new observations and modelling results relevant to the topic. A mix of established and new methods are used, most of which are well described and suitable. The figures are very well made, and the text is concise and mostly clear. Several useful results are presented and discussed. Relevant previous research is cited and discussed. However, further discussion and additional results are needed to support the stated conclusions: While the idea of a scale and offset correction appears to be a useful way to scale the RCM simulations, I would argue the results show that MAR does not capture the all the relevant processes well, particularly in the wintertime. This may limit the applicability of MAR results in other areas, and further discussion is needed. More analysis and discussion of the reasons for the biases in MAR humidity fluxes is needed. This should include the interaction of LW radiation, katabatic winds, blowing snow, and sub-surface temperatures on the profile of temperature and humidity in the lower atmosphere. I am concerned about the exclusion of blowing snow from the MAR simulations, particularly as it has been shown elsewhere to greatly affect the surface humidity flux. The choices made here need further justification, and at least some sensitivity analysis within MAR is needed to show the effect of including blowing snow or not. Further considerations of the limitations of the bulk method for estimating accurate wintertime humidity fluxes, especially in blowing snow conditions. With these additional results and discussion, the paper should make a useful contribution to The Cryopshere.

We thank the reviewer for the positive and constructive feedback. We agree that a deeper analysis if the systematic errors in the temperature and, thus, humidity in MAR is needed. However, this is not an easy task and not the focus of this study. In this study, we merely aim to estimate the potential impact of these systematic biases on the SMB and the consequent potential underestimation of the role of humidity fluxes in the SMB. It is now stated more clearly that the correction applied in this study has no impact on the simulation itself but is offline. Furthermore, we agree with the reviewer's concern about excluding blowing snow in the simulation and defend and discuss this decision more clearly (see detailed comments below).

Specific comments

Title – given the above uncertainties in both the bulk and RCM simulations, a more targeted title is warranted e.g. "Correcting regional climate model estimates of humidity flux in the accumulation zone of the Greenland Ice Sheet with in-situ observations."

As the aim of this study is not to provide a new, more accurate simulation of the LHF but to point out the importance of the LHF that seems to be underestimated in current MAR simulations, we would prefer to keep the current title.

7 – It would be more consistent to present the humidity flux here in mass units (i.e. mm w.eq.)

Both values are now given (-1.3 Wm-2, -1.65 mm w.eq.).

97 – the explanation given for why blowing snow in MAR was turned off is unclear. Please expand.

The blowing snow module was turned off in our analysis for two main reasons: 1.) To keep simplicity and to avoid the potential canceling of two error sources (i.e. temperature biases and systematic errors in the blowing snow simulation), and 2.) to exclude local spacial variations introduced by the blowing snow module from the analysis that can not be expanded to the surroundings of EGRIP due to missing observations. By this approach, the mismatch between the model and the observations can be directly accounted for all error sources, namely, the uncertainty of the observations, the known temperature biases in MAR as well as the neglect of blowing snow in the simulation. Since post-correcting the LHF solely based on the temperature(-gradient) biases in MAR led to a strong improvement of the match between observations and the simulation, this could indicate that blowing snow plays a smaller role at EGRIP than for

many Antarctic locations. However, this is speculation, and more observations are needed to benchmark the role of blowing snow in EGRIP and to evaluate MAR.

We acknowledge that the discussion of neglecting blowing snow in the simulation has not been comprehensive enough in the original version of this study and we now provide a clearer discussion of the potential impacts on our key results, the correction, and the limits of the applicability of the post-corrected data (ll.393-356).

127 – please provide further detail of how the setting used to process the EC data. In particular whether any data were excluded or gap filled from the summer records.

We updated the paragraph with relevant information (ll. XX): We now provide information on the integration time which indeed has been shown to guarantee optimal stationarity and averaged to hourly data to fit the model's frequency. The filter that was applied to the published dataset is a simple noise filter by applying a cut-off threshold of fluxes <-20 W/m2 and >40 W/m2 that is now given in the manuscript but no filtering based on the wind speed or temperatures is applied.

130 – please either provide additional details and statistics of the EC comparison, or exclude this comment

Additional information will be provided for the reviewer but since this work is part of a planned publication led by a Master student that is not published yet, we removed the lines in the manuscript.

162 – earlier you comment that 2019 was warmer and more humid than other years – how does this affect its representativeness.

Indeed, this is formulated too strongly. By "good representative" we were referring to the fact that the distribution of the LHF in the summer 2019 is very similar to the total distribution in all four summers. Using an example (in Fig. 3 & 6) makes it easier for the reader to understand the different types of systematic errors in winter and summer. We removed the valuation "good" and present the summer 2019 only as an example now (l.XX).

165 – please clarify the direction of a 'negative' humidity gradient in the text – the definition above only states it is the difference between 2m and surface.
We updated this (l. 184). A negative humidity gradient refers to a higher specific humidity at the surface compared to 2 m.

175 – please clarify it is the 'MAR simulated LHF' (as the bulk estimates are a simulation of sorts).
We updated the line (l. 189).

198 (also 212 and elsewhere) – it would be clearer to talk about 'offset' and 'scale' biases here – where the summer is primarily an offset bias, whereas in the winter and offset in the magnitude of the flux also produces a bias (difference between means of observations and simulations)

We like the idea of differentiating the two types of biases and added offset and scale, where applicable.

214 – how is 0.1 a 'zero-bias'? please revise.
The offset bias b is normalized in such way that it is exactly zero on January 1st. The average bias for the entire winter (December and January) is 0.1.

Figure 8 – please provide further detail on how the seasonal curves for the factors m and b are derived?
b is a function of the monthly averaged surface saturation specific humidity and m a function of its inverse. We post-correct the LHF by applying this simple function in order to estimate both the potential impact of the systematic errors in the MAR simulation and the role that humidity fluxes play in the SMB at EGRIP. We give the exact calculation now in the text (ll. XX) and attached the python code in the Appendix.

224 – there appears to be an inconsistency in how the annual and summer humidity flux contributions are reported as the text differs (i.e. "In the corrected simulation 5.1% [4% to 6 %] of the annual mass gain (snowfall + deposition) sublimates again," vs "During summer, the amount of sublimated mass corresponds to 31% [26% to 34 %] of the total mass gain". If these are calculated in the same way, consider using the same language for each to avoid confusion.

We apologize for the confusion here. In fact, both is calculated the same way and we updated the language to make it sound more similar (ll. 252-253).

230 – "Our evaluation shows that MAR captures all relevant processes driving the humidity flux and captures the distribution of the LHF remarkably well (Fig. A8)" I would disagree with this statement. The existence of the biases in the uncorrected simulations highlight that important processes are likely missing from the simulation. But that with correction can be corrected to represent the distribution of humidity flux. Please revise.

We agree with the reviewer's critique and removed the statement that all relevant processes are captured but replaced it with a more accurate assessment that both the distribution of the wind speed and the latent heat flux are well captured when an appropriate roughness length is chosen (ll. 262-263).

237 – "bias of -1.3Wm−2" it is awkward to introduce an energy term here without context. A mass unit would be better suited.
We now give both values (-1.3 Wm-2, -1.65 mm w.eq.), l. 276.

242-246 – the distribution of biases in surface and air temperature, and incoming LW needs to be shown here.
A figure providing the distribution for surface and 2 m temperature as well as the LW fluxes is added to the Appendix.

248 – "systematic error in the LHF is not a bias" as for line 198, the error is a bias, just a different sort of bias – please revise.
We completely agree and updated the line (l. 303)

255-262 – please show the distribution of biases of variables related to the humidity gradient. These are key elements of evaluation and deserve further description and discussion.
See response to the reviewers comment for ll 242-246.

291 – how do we know that blowing snow events are rare? The average wind speed is high, and with a dry snow surface, it is likely that blowing snow events are frequent.

In fact, information on the occurrence and extent of blowing snow at EGRIP (or comparable locations) is very limited. This statement was based on visual observations reported by co-authors that ran the eddy-covariance measurements in the summers 2016–2019. The statement is updated accordingly (ll. 353-354).

Editorial comments

249 – "RMSE = 0.73Wm−2" – correct units
We corrected the units to m s-1 (l. 304).