Review of manuscript tc-2022-260 “On the importance of the humidity flux for the surface mass balance in the accumulation zone of the Greenland Ice Sheet” by Dietrich et al

General comments

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.*

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.”

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

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

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.

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

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

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.

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

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)

214 – how is 0.1 a 'zero-bias'? please revise.

Figure 8 – please provide further detail on how the seasonal curves for the factors *m* and *b* are derived?

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.

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.

237 – "bias of -1.3Wm−2" it is awkward to introduce an energy term here without context. A mass unit would be better suited.

242-246 – the distribution of biases in surface and air temperature, and incoming LW needs to be shown here.

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.

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.

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.

Editorial comments

249 – "RMSE = 0.73Wm−2" – correct units