SECOND REVISION

RESPONSE to Comments by Anonymous Referee #2

Below, we make an attempt to respond to the comments and critiques of the referee point-by-point

1. Specifically this relates to Figures 5 and 6. According to this new manuscript https://doi.org/10.1007/s11707-022-0971-8, WRF-NoahMP has a large warm bias over Antarctica due to its deficient treatment of surface albedo, being too low and therefore absorbing too much shortwave radiation in summer and making the near-surface temperatures too warm. This is almost certainly the cause of its deficient temperature simulation, especially for South Pole. The authors don't state why they use potential temperature instead of physical temperature, but the latter is to be preferred. If physical temperatures were plotted then one could see whether the relative humidity values produced by WRF-NoahMP happened at low air temperatures. I have seen humidity sensors become insensitive at temperatures approaching -35C, and basically reflect the air temperature, i.e., it is conceivable that WRF-NoahMP is correct in its RH output, rather than the observations.

   The referee seems to be making two points here: (a) Noah MP in WRF has been shown to have a warm bias over Antarctica based on a recent paper and (b), NoahMP being correct in its RH output rather than the observations at low temperatures.

   The goal of this paper is to present a new modelling framework in WRF with an advanced snow model and blowing snow. We successfully did the technical work and presented a snapshot of results supporting this. We use NoahMP as a baseline setup, the observations as reference and data from our new model and compare the results 'out-of-the-box'. We show not just for South Pole but also 14 other stations (spread over the continent) in Figure 4 that the new modelling framework produces results as good as NoahMP if not better 'out-of-the-box'. It is not the goal of the paper, or indeed our research to try and understand why NoahMP or any other competing LSM model produces the results that they do. Neither is the goal of the article to understand and solve the long-standing problem of humidity measurements at very low temperatures.

   We created a new coupling framework, publicly released it and wrote an article showcasing its capabilities in three very different scenarios for processes occurring at very different length and timescales. The reviewer would agree that if we enter into discussions on the details, this article could conceivably be split into three separate manuscripts! And we indeed will follow up with many

further manuscripts to address the finer details. We don't feel appropriate to enter into these discussions in this article.

However, to satisfy the reviewer, we have added a reference to the article they link to and mention clearly the problem with measurements at low temperatures.

2. I continue to be concerned about the surface mass balance results shown in Figure 7 and whether the big discrepancy with long-term averages can be ascribed to interannual variability. At the very least discussion of this issue should be included in the manuscript rather than just ignoring it.

We have never ignored it! We stated clearly in the submitted manuscript:
"CRYOWRF outputs these quantities as two-dimensional fields directly as a part of the standard output via its specially implemented diagnostics package. An example of the surface mass balance output during this case-study is shown in Fig.\ref{fig:fig_7}. Since the simulation period is of only one year, it is not possible to quantitatively compare the results presented in this figure with those published in literature, which are typically calculated over climate time scales of at least 30 years."

We have added a line stating clearly how the long-term averages can be different from the those from this one year.

"… which are typically calculated over climate time scales of at least 30 years. In fact, given the interannual variability of SMB patterns, particularly at the coast (due to phenomena such as atmospheric rivers), it is likely that the numbers from one year as shown in Fig.\ref{fig:fig_7} may be different from long-term averages."

To assuage the concerns that the reviewer has with regards to the accuracy of CRYOWRF, we add below a figure from a recently submitted article (the pre-print should be available soon publicly) where the results of CRYOWRF are compared to MAR for a simulation period of a decade between 2010-2020. The reviewer can see that CRYOWRF's numbers are not only comparable to MAR but in many ways improve upon MAR in comparison to stake measurement-based SMB (which in our opinion is the most valid source of SMB data as opposed to other models including RACMO / MAR etc).

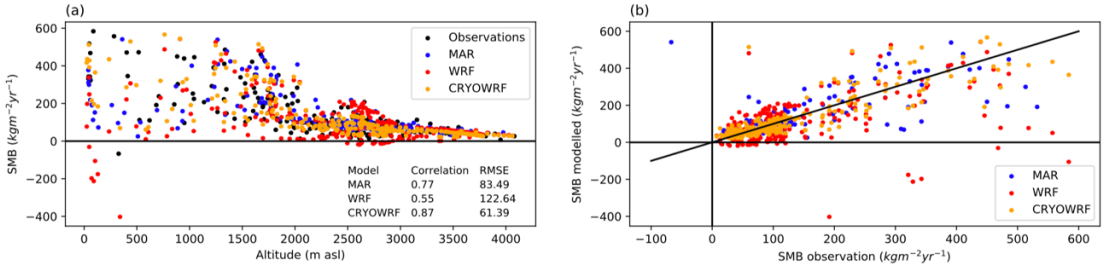

Figure 1: All rights belong to Gerber, Sharma and Lehning: CRYOWRF - a validation and the effect of blowing snow on the Antarctic SMB (2022), (submitted to JGR Atmospheres / ESSOAR)

3. Finally, I didn't find any manuscript response to my point number 6 about model vertical levels.

   Yes, this is indeed a mistake from our side. We have now added this information to each of the three case studies. We apologize for overlooking this question in the previous round of reviews.