General comments

This study takes an in-depth look at how kilometer scale regional climate models (RCMs) resolve the interactions between atmospheric rivers (ARs) and complex topography in a couple case studies centered around the Amundsen Sea Embayment. Their most consequential results indicate that the boost in resolution in these three RCMS (MetUM, Polar-WRF, and HCLIM) leads to the resolution of non-trivial rainfall quantities over the Thwaites and Pine Island ice shelves, especially during the summer AR event. The ERA5, normally considered the gold standard for Antarctic precipitation studies, is unable to depict the rainfall quantities from these AR events. The final experiment of using the MetUM shows that even model resolutions considering high (12 km) are too coarse to simulate topographically influenced rainfall on the Antarctic coastline.

The authors make a great effort using SNOWPACK to calibrate the ground observations for a better comparison with the RCMs and ERA5 instead of just comparing the models directly with the station observations. Despite the consideration, there remains a great deal of uncertainty in validating the performance of the RCMs and ERA5 at this high resolution where the authors meticulously highlight the potential shortcomings of studying this remote region. Other efforts like incorporating IMERG rainfall data and the discussing its shortcomings emphasize the level of detail that went into this study. From my perspective, all the most relevant research was cited and is usefully employed to place the results in context with the existing literature. I believe this study conveys the potential unrealized threat of heavy rainfall on Antarctic coastlines and the need for advanced modeling and observations to further realize this problem. I've made specific and technical comments below thar hopefully help the authors clarify a few details, but I believe this manuscript will be an important contribution to the field after some minor revisions.

-Jonathan Wille

Specific comments

Line 23-25: Can you specify that you used the MetUM for the resolution experiment here?

Revised "RCM-simulated" to "MetUM-simulated"

Line 33-34: Can you elaborate on this mass loss from ice shelves? Like are the ice shelves themselves shrinking in mass or is something else happening? Also please include a reference for Smith et al. (2020)

We have added additional details to this sentence, as follows:

"The ice shelves restraining TG and PIG have been shown to be vulnerable to damage and weakening, and are changing very rapidly**, <u>thinning and retreating in response to oceanic and atmospheric warming</u>** (Lhermitte et al., 2020; Alley et al., 2021)."

And have included the reference for Smith et al – thanks for spotting that it was missing!

Line 39: The wording is a bit confusing here. If accumulation is not compensating for SMB losses on the ice shelves, then perhaps instead of saying "constrain", try "… but snowfall still modulates (or controls) the timing and characteristics of ice shelf collapse, or recession and thinning"

We have revised "constrain" to "quantify"

Line 70-80: This rainfall paragraph is well-written but it could be beneficial to cite (Vignon et al., 2021) which characterized rainfall patterns around Antarctica (i.e. it happens more often than you'd expect) and uses GCMs to show increased rainfall in the future.

Thanks, we had incorporated this study in an older version of the document, but agree that we should include it here. Added the following:

"**Vignon et al. (2021) show that rain falls up to 100 days per year at low elevation regions around the Antarctic coast, including over ice shelves**."

Line 158: Can you state how deep convection is treated in the HCLIM? This is described for the other models so it would be good to know here as well.

Deep convection is resolved and explicitly represented by the HCLIM's nonhydrostatic dynamics. However, it uses a shallow convection parameterization based on the eddy diffusivity mass-flux framework (de Rooy and Siebesman, 2008; Bengtsson et al. 2017). We have added this information to this paragraph.

Section 2.3: Since this becomes relevant later in the results, can you describe the topography surrounding the stations so that the reader can get a feeling for how orographic lifting might behave here? Also it might be important to state any potential snow measurement issues at these stations like blowing snow?

Thanks for this suggestion. We include mention of the stations' location relative to the slope of the ice stream as follows:

"The Cavity and Channel stations are located approximately 20 km from the grounding line region of the TG ice stream where the terrain begins to slope steeply upwards."

Because we discuss the issues that potentially arise with blowing snow later in the discussion section, we chose not to include it here. We keep this section as a simple description to be consistent with the other descriptive sections in S2.

Section 2.4: Is this method of diagnosing snowfall using SNOWPACK sensitive to blowing snow related errors or would it actually reduce the impacts of blowing snow?

We would argue here that SNOWPACK diagnoses local accumulation, and we then attribute the local accumulation to snowfall. However, if the snow depth measurements are from an area with net convergence of drifting snow mass, the local accumulation would exceed local snowfall, and vice versa. This means that we would not call it a blowing snow related error, but rather that SNOWPACK diagnoses accumulation using the snow depth measurements, rather than snowfall.

We include a brief discussion of the uncertainties and assumptions at the end of section 2.4 as follows:

"In this study, we compare the local accumulation at the Cavity and Channel stations to modelled snowfall rates. However, it is important to note that the local accumulation can be substantially impacted by net snow erosion if the station is located in a region of drifting snow divergence, or by deposition when the station is in a zone of drifting snow convergence. Other sources of uncertainty concern the density of new snow, which can be in the order of 20-30% (Keenan et al., 2021), and which directly impacts the estimated accumulated mass from snow height measurements. Our approach also assumes that decreases in snow height are correctly captured by the model. Sublimation generally leads to very small changes in snow depth over the period of the described events andmelt during the events was also estimated to be too small to lead to any substantial surface height decrease. The snow depth measurements show a strong increase in snow depth, suggesting that any possible wind erosion of the snow surface was overshadowed by the net increase in mass in the area."

Section 3.1: It would be helpful if you gave more details on the temporal evolution of both case studies. Like providing dates for the AR landfalls. Also at the risk of self-advertising, it could also be helpful to state that these events were detected AR events from the Wille et al. (2021) detection algorithm (I already verified this).

Thanks for this suggestion – we think this information strengthens the statement too. We have included more details in this section. We have not included a great deal more information about the temporal evolution of the cases because we feel this has been done at length in Maclennan et al.

Section S2.1: It was instructive to see IMERG data being applied for this study even if it just highlights the unreliability of IMERG data over Antarctica. Does the IMERG data used in this study have a quality control variable that can be used to mask out unreliable data? From personal experience, I know the IMERG Final Run has this variable. If this is something you didn't consider, I wouldn't recommend rerunning your analysis of the IMERG data, but perhaps just mentioning that the quality control wasn't implemented since this isn't an important aspect of the study.

Thanks for this suggestion. We didn't use the QC flag for the IMERG work as it did not prove to be important to the conclusions, but we have added mention that it was not used in the discussion of the data in the supplementary material.

Figure 2: Can you add date labels to the two panels in this figure?

Good idea, we have included date labels.

Line 285: From the sentences that follow, this seems dependent on which station you are describing. Like you say the RCM estimates at Channel are quite accurate in the next sentence.

This assessment is correct. We have revised this sentence to make this clearer, as follows: "In **the** summer **case**, in comparison to SNOWPACK simulated accumulation, all RCMs under-estimate snowfall and do not capture the exact timing of the snowfall related to the ARs (Figure 3a), **although this depends somewhat on the station considered."**

Line 299-300: Careful with the terminology here. The way this sentence is structured, it sounds like you are making a general statement about RCM performance during the winter and summer. When addressing your case studies, perhaps it is more intuitive to say, "winter event" and "summer event".

Thank you, we have revised this paragraph and the previous to make it clearer that we are describing the behaviour only in these cases.

Lines 364-372: I suggest reorganizing this paragraph so that you discuss all the summer event results first and then finish with the couple results on the winter event rainfall or the other way around.

Thank you, we have reorganised to discuss the winter rainfall results first, and then the summer results.

Line 432-433: Perhaps rephrase to "which may indicate that in the most extreme AR conditions during winter, liquid precipitation can still occur". It's not very surprising it rains during summer AR events, but the winter rain is more impressive.

Thank you. Revised to "However, two out of three simulate modest quantities of rain during the winter case, which may indicate that even in winter liquid precipitation can still occur during the most extreme AR conditions."

Figure 5: Can you add date ranges to the figure caption?

Added.

Figure 6: Similar comment as before, please mention which date the transects are from.

Added.

Figure 7: Same as previous comment.

Added.

Figure 8: Same, a date range would be helpful in the caption

Added.

Section 3.4: This section does an excellent job at covering many different facets of rainfall behavior during the two AR events. There are some details about the influence of the foehn wind on increasing temperatures and altering the precipitation patterns, but it would be helpful to see an organized paragraph discussing the timing of the foehn relative to the rainfall and whether the foehn leads to enhanced sublimation of precipitation. In its current form, it appears the foehn helps warm the surface allowing more rainfall to happen even though one would expect greater sublimation. Are there differences between the RCMs in how they resolve the timing of the foehn and magnitude of sublimation?

Thank you, we are glad you found this section useful. We plan to do some further analysis to investigate the timing of the rainfall relative to the foehn event and expand the description of the influence of the foehn on temperature and precipitation patterns during the case.

Line 473-475: These sentences could be shortened and combined for simplicity. Like "… indicate the maximum height of the 0 C isotherm which can be considered the melt layer". It's obvious that below the melt layer, precipitation would fall as rain or sublimate.

We've chosen to keep this in, for clarity of understanding.

Line 484: Do you mean sublimation instead of evaporation?

Evaporation is accurate here, because we are talking about the loss of rain specifically (i.e. the transformation of liquid to the gas phase).

Lines 544-547: I feel like this message about "the difficulty of verifying rainfall but that the RCMs are simulating it" is repeated a few times already like in lines 401-404 and 431-433.

Especially here this message doesn't need to be repeated but do keep the interesting discussion on liquid precipitation at low temperatures.

Thanks, we have removed some repetitive language in this paragraph.

Lines 558-559: It might be worth mentioning that ARs are shifting poleward along with the general storm track (Chemke, 2022).

Thank you, we have included reference to Chemke (2022) here.

Technical corrections

Line 68: "the precipitation phase" or "precipitation phasing". Also the Gehring et al. (2022) study is perhaps one of the more closely related studies so it could be worthwhile spending another sentence describing how they found that local foehn winds and orography can completely sublimate the precipitation in some AR cases while AR orientation is crucial for controlling snowfall amounts

Thanks, we have included more discussion of Gehring et al:

"That study also showed how the specifics of how the airflow interacts with steep topography can strongly influence precipitation phase, for example via the impact of foehn on the sublimation of precipitation particles."

Line 114: Don't forget about the oxford comma.

Thanks

Line 115: Do you mean the models have 16, 20, and 14 levels in the 1km nest?

We mean that there are this number of levels below 1000 m altitude. We have revised for clarity as follows:

"**including** 16, 20 and 14 levels below 1 km **altitude.**"

Line 187-188: This sentence repeats itself, "Then, the observed snow height is compared to the observed snow height to determine the accumulation terms."

Woops. One of those should be "calculated". Changed.

Figure 3: It could be helpful if you labelled "summer" and "winter" on the figure so that the reader quickly identifies which event is which.

Thanks for this suggestion, implemented.

Line 423: "where the terrain beings to slope upwards more strongly" doesn't sound accurate. How about "where the terrain begins increasingly steepening"?

Revised to "where the terrain begins to slope upwards more steeply"

Line 550: "ARs"

Revised, thanks.

Line 568: "showed"

We use the present text to refer to study findings elsewhere, so we will keep this as "show" here for consistency.

References:

Chemke, R. (2022). The future poleward shift of Southern Hemisphere summer mid-latitude storm tracks stems from ocean coupling. Nature Communications, 13(1), 1730. https://doi.org/10.1038/s41467-022-29392-4

Vignon, É., Roussel, M.-L., Gorodetskaya, I. V., Genthon, C., & Berne, A. (2021). Present and Future of Rainfall in Antarctica. Geophysical Research Letters, 48(8), e2020GL092281. https://doi.org/10.1029/2020GL092281