Review of "Impacts of Atmospheric Dynamics on Sea-Ice and Snow Thickness at a Coastal Site in East Antarctica"

The paper explores complex interactions between atmospheric dynamics and sea-ice/snow thickness variability at a coastal East Antarctic site near Mawson Station. Using in situ SIMBA buoy measurements from July to November 2022, combined with atmospheric reanalysis and PolarWRF modeling, the authors identify key factors driving changes in sea-ice and snow thickness. Main findings highlight the dominant influence of the seasonal solar cycle on sea-ice thickness, while atmospheric processes such as cyclonic forcing, katabatic winds, and atmospheric rivers (ARs) significantly impact snow thickness variability. Overall, the study provides valuable observational insights and model analyses, essential for refining Antarctic climate projections.

I appreciate the comprehensive sea ice observations and the detailed use of PolarWRF. However, several key issues related to model implementation, representation of the chosen site, and methods used for data analysis and interpretation need to be addressed. Consequently, I recommend major revisions before the manuscript can be considered for publication.

General Comments:

1. The introduction is more like thesis general introduction instead of the scientific paper including clear motivation and contectual logic. For example, authors selected Mawson Station, but the introduction lacks a robust justification explaining why this particular site is chosen. Is it representative? Is it an area significantly influenced by atmospheric rivers or other dynamic processes compared to other coastal sites? Secondly, it shifts somewhat abruptly between general background, specific processes (e.g., atmospheric rivers and katabatic winds), and observational/modeling studies without clear transitions. Thirdly, one of the major flaws is the absence of explicitly stated research questions or clear objectives in the introduction. The introduction does not adequately highlight the novelty or unique contribution of this particular research. How does this work build upon previous studies, and what new insights does it aim to provide?
2. Data section: Again, the narrative style, while detailed, is more typical of a report than a concise scientific paper, making it harder for me to quickly grasp which datasets were used and their specific purpose. Also, the description of datasets (e.g., SIMBA, ERA-5, AMSR, MODIS, AWS data, sounding profiles) is embedded in paragraphs without clear structuring or consistent formatting, please use table to make your scientific question more clearly.
3. Equations (1)-(5) detailing verification are presented in substantial detail, and they should be better placed in a supplementary materials section. Equestion 7-9, and I am not sure why authors want to list every details in the method part since all different method will compromise the focus of the paper and lose direction, in which some of them should also be put into supplementary. For example, the identification of TTT events can provide valuable context, in this study no significant TTT event was identified during the main AR episode (mid-November 2022). The detailed explanation and equations for the TTT index thus add complexity without substantially advancing core analysis. The same problem is also existing in TPV trackling, which is relatively peripheral to the primary observational focus.
4. PolarWRF description. This part needs significant improvement. Although ERA-5 reanalysis data are mentioned as boundary conditions, details about precisely which variables are prescribed or nudged are scattered and unclear. Additionally, the distinction between default and adjusted sea-ice concentration/thickness (“PWRF” vs. “PWRF_SIE_SIT”) and their specific forcing sources (ERA-5 versus satellite data) should be explicitly clarified. The overly detailed description of model physics parameterizations overshadow essential information and could be reduced or moved to supplementary material.
5. Line 490: It is very unclear. The authors state "parameterization schemes" are used to calculate surface sublimation and blowing snow sublimation/divergence, but these are not explicitly defined or cited. The authors should clearly state the specific parameterizations used, are these internal PolarWRF parameterizations or externally applied?

6. SIMBA sea ice thickness and snow depth deduction: While thresholds for distinguishing interfaces (air-snow, snow-ice, ice-water) are provided (lines 229-238), but uncertainties in thickness estimations arising from the threshold are not clearly explored. Although the authors mention initial manual measurements of snow thickness, sea ice thickness, and freeboard at deployment (lines 206-212), there is no clear mention or detailed presentation of subsequent manual validations or calibrations. For example, identifies air-snow and snow-ice interfaces based solely on thermistor temperature gradients after heating, then how is the potential error sources from flooding or snow-to-ice transformations in the event like AR-induced snowfall?
7. AR detection (line 389-392): the authors chose MERRA-2 instead of ERA-5 to identify ARs (lines 389-392), but this choice is not justified clearly.
8. Linking AR and sea ice response: While the authors suggest a clear association between AR occurrences and changes in snow depth, the analysis is primarily qualitative. For instance, they claim a response of about 0.06 m in ST to ARs, but I don't see how robust these associations are statistically. The discussion of AR effects on SIT is even more speculative, especially given the minimal observed changes (0.04 m), which may be within the noise or SIMBA measurement uncertainty. Also, current analysis relies heavily on visual inspection of figures (Fig. 2 and 3), I feel it is difficulty to definitively attribute observed snow and ice changes directly to AR events, given the influence of other processes such as oceanic forcing, seasonal variability, or katabatic winds. More importantly, although some hypotheses are stated regarding snow-to-ice metamorphism and potential snow-ice formation (lines 528-538), can you find the evidences for that?
9. The authors briefly discuss katabatic and foehn winds but don't fully disentangle these from AR-driven changes (lines 522-527). There is insufficient clarity on whether the snow depth changes are primarily due to AR-driven snowfall or secondary processes such as blowing snow removal and sublimation, making conclusions unclear.
10. The authors mention multiple atmospheric phenomena (ARs, katabatic winds, foehn winds), but it remains unclear how confidently observed thickness changes can be attributed exclusively to AR events versus other atmospheric or local processes (katabatic winds, sublimation, blowing snow, ice deformation).
11. In section 4, it is still not adequately demonstrated whether observed precipitation and snow depth changes are uniquely caused by AR-driven moisture transport or influenced by local katabatic processes or other dynamics. Since the authors are using WRF, please consider the appropriate sensitivity simulations using PolarWRF with and without AR-induced moisture to explicitly isolate the contribution of AR moisture to observed snowfall and resulting snow depth and SIT changes. The current case study overly attributes observed sea-ice and snow changes primarily to the AR, without sufficiently considering alternative explanations such as local ocean-ice processes, ice dynamics (e.g., deformation), or blowing snow processes that might have simultaneously influenced SIT and snow depth.
12. PolarWRF: (1) The authors recognize a persistent dry bias and overly strong boundary layer mixing (lines 739-748), the authors do not adequately discuss how such model limitations specifically impact their ability to quantify AR-driven effects on sea ice and snow. (2) Although model performance is evaluated against AWS data, there is no clear, quantitative comparison between PolarWRF-derived snowfall and the actual snow accumulation measured by SIMBA.
13. Discussion: (1) The authors again acknowledge the limited observational period (July–November 2022), noting its inadequacy for statistically robust conclusions (lines 860-862). While this limitation is mentioned, the authors should clearly explain here how future studies or additional observations could specifically address these gaps. (2) since the previous sections has already identified some weakenss, e.g., boundary-layer dynamics, excessive mixing, surface albedo issues, and sensitivity to sea ice representation, it is no clear recommendation or acknowledgment on precisely how to address these issues in future research. (3) The discussion does not clearly revisit the identified uncertainties or assumptions regarding SIMBA buoy-derived SIT and snow depth.

Specific Comments:

Check carefully for spacing and punctuation errors throughout the manuscript, especially in lines 522 and 834, where incomplete sentences or extra dots are present.