Antarctic atmospheric Richardson number from radiosoundings measurements and AMPS

acp-2022-352, Qike Yang et al.

Recommendation: Reject

General Comments

This paper aims to evaluate the performance of the Antarctic Mesoscale Prediction System (AMPS) model against routine radiosonde observations from three sites across Antarctica. More specifically, the authors evaluate the differences between simulated and observed values of the reciprocal of the Richardson number, *Ri*, between March 2021 to February 2022. While this is an interesting topic and the methods appear sound, it is the opinion of this reviewer that there are issues with the scope, motivation, and interpretation of the results that would require substantial revisions to make appropriate for publication in *Atmospheric Chemistry and Physics*.

Fatal Flaw

The primary issue with this study is its lack of analysis or contextualization of results that warrant dissemination in a journal such as *Atmospheric Chemistry and Physics*. This issue stems from its introduction, which does not adequately frame the motivation for this study apart from generally calculating differences in Richardson number between model output and radiosonde observations. For example, there is little physical insight into the mechanisms behind these differences, how the choice of model physics affect these differences, or statistical significance for any of the computed statistics. The primary conclusions of this study seem only to comprise a report of profiles of 1/*Ri* differences between an experimental model and radiosondes, a collection of seasonal cross-sections from the model, and a brief comparison of boundary-layer height estimates. This study reads more like a technical report than a journal article with well-defined research questions based on physical mechanisms and addressing a pronounced gap in the literature. Without such analyses, it is difficult to recommend publishing in *Atmospheric Chemistry and Physics*, whose scope is "…focused on studies with important implications for our understanding of the state and behaviour of the atmosphere". Therefore, it is this reviewer's opinion that this manuscript requires too significant of changes throughout to 1) align with the scope of *ACP* and 2) provide substantial contributions to the existing literature such that the paper should be rejected with re-submission to another journal encouraged.

Major Comments

1. This manuscript would greatly benefit from a dedicated English language review to ensure the authors' points are properly worded and explained. There are numerous instances throughout the paper where rewording/restructuring sentences and paragraphs would highly improve the readability. Some (but not all) of these instances include lines 46—57, 195—210, 281—284, 302—304, and 383—384.
2. The motivation behind the use of 1/*Ri* instead of just *Ri* is not entirely clear beyond citing that another reference did so. One of the citations mentions that 1/*Ri* provides an easier dynamic range to plot, but in my experience this can be overcome by plotting *Ri* on a logarithmic axis to maintain the physical interpretation. Please consider changing to use *Ri* throughout or at least substantiate the use of 1/*Ri* more thoroughly.

3. In general, there is a lack of substantial analysis or interpretation of the results presented that contributes to addressing gaps in the literature. I would like to see the following addressed to improve upon this aspect of the manuscript.
    a. What are some potential underlying reasons for the discrepancies between the models and obseravtions? For example, how does the choice of model physics impact the resulting dynamic stability in simulated profiles? How does the physics in the AMPS model differ from the ECMWF analyses used by Hagelin et al. (2008)? How well does the model perform against other observations in general? How do errors depend on model forecast lead time?
    b. If the model biases can be related to parameterization schemes, what are the implications for global numerical weather prediction models? Are there underlying stable atmospheric boundary layer or marine boundary layer processes that need be more accurately accounted for in global/regional models?
    c. The monthly bulk statistics provided are informative, but could be expanded upon for emphasis. Please provide case studies from, e.g., when or where the model performed particularly well or poorly to determine if there are other large-scale phenomena biasing the model or if local-scale dynamics are not being represented properly.
    d. The determined value of critical $1/Ri$ determining the boundary layer depth should be expanded upon by a discussion of other metrics typically used to estimate the boundary layer depth. I recommend citing and discussing the work of, e.g., Pietroni et al. (2012) and Petenko et al. (2019) to better contextualize the results from section 4.2.4.
4. The linear regression analysis performed to correct model-estimated profiles of $1/Ri$ in section 4.2.2 seems interesting, albeit reads more like a calibration lab report as opposed to a journal article. Why was the form of equation (2) chosen? How does the linear regression model compare with other results in the literature? How does this regression depend on seasonality or ambient flow speed and direction? Please tie these results back to your motivation, because right now they are just presented without much practical application.
5. The vertical cross-sections in section 4.2.3 are interesting, but I am struggling to understand the value they add to the discussion on model performance versus observations. Observations do not seem to be discussed much at all in this section. Was this supposed to demonstrate the capability of the model to estimate the Richardson number close to the surface? Please tie these results back to the paper objectives of evaluating model performance.
6. Figures 2—5 are interesting, but perhaps present an information overload. I suggest condensing the figures into seasonal median plots instead of monthly medians. Please also consider utilizing a color palette that is more colorblind friendly for all figures in this manuscript (e.g., https://doi.org/10.1175/BAMS-D-13-00155.1).

Minor and Technical Comments

1. Line 36: please remove the break to a new paragraph here, as the content is a continuation of the sentence ending at line 35.
2. Line 40: Omit the sentence beginning “The Polar WRF model was developed…” or provide a citation instead.

3. Line 41: The sentence beginning “The simulated *Ri* by Polar WRF…” is vague and does not tell the reader much about the performance of Polar WRF in simulating turbulent exchanges of heat, momentum, etc.
4. Line 42: What specifically have the previous studies verifying the performance of AMPS find with regards to turbulence production? Please elaborate on this literature review.
5. Line 45: The sentence ending with “needs to be extended” should be elaborated upon. This may be accomplished by considering point 4 above.
6. Line 46: Please omit the phrases “And so far,” and “still tremendous”.
7. Line 49: The use of the word “sketchy” is too colloquial for discussion in a journal article. Please rephrase to discuss the relative lack of performance evaluations of models in Antarctica.
8. Line 54: Please elaborate on the “practical applications” mentioned at the end of point 2 to improve the motivation of this study.
9. Line 66: Please remove “or instability” to be more concise.
10. Line 91: In the sentence “The balloon scans the atmosphere,” please consider replacing “scans” with “observes” or “measures” for clarity.
11. Lines 98—109: Please make it more apparent which model forecast times are selected for analysis. Please also add discussion on the choice to use forecasted fields instead of model analysis instances.
12. Line 119: Please avoid beginning a sentence with a lower case variable.
13. Lines 141—143: This information better belongs in the caption for Figures 2 and/or 3. Please consider moving.
14. Line 154: What months are being referred to when discussing the model and observation differences here?
15. Line 175: Specifically, what heights are these parameters being interpolated to? Maybe this could be depicted in a figure.
16. Lines 179—182: This paragraph discussing the use of NCL is not necessary for the presentation of results. Please instead discuss how derivatives are calculated (i.e., centered finite differencing, fitting an analytical function, etc.) and how this choice may impact the resulting profiles of $1/Ri$.
17. Line 184—185: The sentence beginning “This is because the value of $1/Ri$ can oscillate…” is vague and potentially misleading. Please clarify what is meant here.
18. Line 194: Please add a reference to a figure or citation to contextualize the discussion at the end of this paragraph.
19. Line 195: The phrase “…are afraid to conduct quantitative analysis…” is too colloquial and does not accurately portray the gaps in the literature. Please rephrase.
20. Line 210: Please provide additional discussion for why correlations are lower in the winter other than it is harder to collect observations.
21. Figure 6: This is an interesting presentation of your results, but the third dimension seems redundant to plot when the points are also colored by height. Please consider plotting instead log(1/Ri_meas) versus log(1/Ri_AMPS) in two dimensions while retaining the color shading to denote altitude. Otherwise, please discuss the results from this figure in more detail.
22. Lines 240—244: This paragraph is largely just reporting the values from table 2. Please add more substantial analysis on the resulting linear regression (see major comment 4).

23. Figure 7: From where are these terrain heights obtained? Please add information in the figure caption.
24. Figure 8: Please denote the markings of each plotted field in the figure caption.
25. Line 281—283: Please reword this sentence to better portray the importance of forecasting 1/*Ri* accurately.
26. Line 297: Please elaborate on the comparisons with sodar observations, this seems to be included without proper contextualization.

References

Petenko, I., S. Argentini, G. Casasanta, C. Genthon, and M. Kallistratova, 2019: Stable Surface-Based Turbulent Layer During the Polar Winter at Dome C, Antarctica: Sodar and In Situ Observations. *Boundary-Layer Meteorol*, **171**, 101–128, https://doi.org/10.1007/s10546-018-0419-6.

Pietroni, I., S. Argentini, I. Petenko, and R. Sozzi, 2012: Measurements and Parametrizations of the Atmospheric Boundary-Layer Height at Dome C, Antarctica. *Boundary-Layer Meteorol*, **143**, 189–206, https://doi.org/10.1007/s10546-011-9675-4.

Stauffer, R., G. J. Mayr, M. Dabernig, and A. Zeileis, 2015: Somewhere Over the Rainbow: How to Make Effective Use of Colors in Meteorological Visualizations. *Bulletin of the American Meteorological Society*, **96**, 203–216, https://doi.org/10.1175/BAMS-D-13-00155.1.