"Greenland's Topography Triggers Cyclogenesis: Synergy between Lee Cyclogenesis and Jet Streak"

Author: You

Recommendation: Reject

Overview:

This study utilizes a series of nudging experiments to examine the role of topography for inducing the development of a lee cyclone during the MOSAiC field campaign. The authors find that the removal of topography strongly limits the development of the surface cyclone, and that the upper-level effects of lee cyclogenesis help to encourage the persistence of the cyclone for several days after the cyclone developed. While there are several interesting aspects to the author's analysis, unfortunately, I find the novelty of the work to be strongly overstated, the methodology is lacking critical details to ensure the reproducibility of the work, the methodology does not consider the sensitivity of results to the adopted modeling approach, and more detailed, quantitative analyses can be performed to better carve out the uniqueness of lee cyclogenesis dynamics near Greenland compared to other regions and to further quantify the influence of orographic processes on the cyclone's development and persistence. Given that these recommendations will likely require substantial changes to the manuscript, I must unfortunately recommend rejection of the manuscript at this time.

General Comments:

1. Key details are missing from the methodology section, which unfortunately complicates my ability to evaluate the veracity of subsequent analyses and inhibit the reproducibility of results. These methodological complications are discussed below:

    a. Details regarding the model set-up are rather sparse. For example, the author only provides information regarding the model's initial conditions and the horizontal grid spacing, but no information is provided regarding the number of vertical levels and their spacing, which are presumably very important to a study on orographic effects. Furthermore, there is no discussion of the parameterizations used within the model. In order to make this study fully contained, and given the study relies extensively on model output, more detail about the model set-up should be provided either in the main manuscript or a supplementary section. Additionally, the author is encouraged to provide information about a run script to reproduce the model results, or take efforts to make parts of the model dataset more widely available instead of through a request to the author.

    b. A key aspect of the methodology is the removal of topography, but few details are provided as to how the topography is removed. For instance, removal of the topography requires modification and assignment of atmospheric variables to grid points that lie below ground in the topographic simulations. There are many ways to do this interpolation, and each approach likely will have a substantial influence on the subsequent results. At the very least, the authors should consider providing information on how the topography was removed, how atmospheric data was interpolated to points that were below ground, and perform sensitivity tests to ensure that their results are robust against whatever interpolation approach is adopted.

c. A key component to a modeling study is to ensure that the resultant circulations bare similarity to a verification dataset. However, only model results from the four experiments are shown. It would be beneficial to include a figure that shows the evolution of the case either within ICON analyses, a separate reanalysis dataset, or within a model simulation nudged everywhere to ICON analyses in order to provide context for how the model simulations deviate from what was actually observed.

d. The authors choose 4 km and 8 km as their cut-off levels for their nudging experiments. 4 km has more justification given that the height of the Greenland Ice Sheet extends to 3.7 km. However, the altitudes of wind speeds associated with a jet streak can extend throughout a substantial vertical depth of the troposphere and well below 8 km. I am curious whether the results are sensitive to this choice of 8 km. Additionally, no details are provided regarding how the nudging exercises were performed other than a few qualitative statements that make it challenging to reproduce results. Last, the results could be very sensitive to the time of initialization, especially since a cyclone is already evident less than 24 h after initialization in the **nudge_8km_1 run**.

2. In its current form, the paper's primary contribution, from my perspective, is to highlight the role of lee cyclogenesis downstream of Greenland. This results, on its own, does not rise to the level of publication for a high-impact journal such as *ACP*. Namely, existing climatologies already highlight areas southeast of Greenland and immediately adjacent to its topography as a local maximum in cyclone frequency (i.e., Fig. 1d from Sprenger et al, 2017; https://doi.org/10.1175/BAMS-D-15-00299.1) and Egger (1974) considered the processes conducive to lee cyclogenesis in Greenland (https://doi.org/10.1175/1520-0493(1974)102<0847:NEOLC>2.0.CO;2). A quick search of AMS journal articles also highlights a long list of articles that discuss and examine lee cyclogenesis in the vicinity of Greenland over the past 40 years, but these studies are not considered extensively in the manuscript. Therefore, I strongly disagree with the assertion that lee cyclogenesis has not been studied before.

3. Following from the previous comment, there are a number of ways the author can leverage their existing analyses and expand them to construct an interesting study that would be suitable for publication. Such an approach might consider calculating a vorticity budget in the vicinity of the cyclone center to identify the role of subsidence and upward vertical motion induced by upper-level divergence associated with the jet streak (i.e., vertical stretching) to more closely isolate the effects of such processes on surface cyclone intensification. They might also consider expanding their analyses to more cases to build out a more robust climatology, construct a larger ensemble of simulations of varying topographic heights, or perform a series of modeling studies that compare differences in observed cyclone intensification between Greenland, the Rocky Mountains, and the Alps, perhaps. Indeed, one of the unique aspects that sets Greenland apart is the adjacent ocean surface. Investigations could focus on the added role that latent and sensible heat fluxes from different low-level leeside boundaries may play on a lee cyclone's evolution. However, I disagree with the author's comment that a jet streak does not play a role in contributing to lee cyclogenesis in regions outside of Greenland, as prior climatological studies in the Rocky Mountains do show evidence of an attendant jet streak during some of the more impactful events in that region (see comment on L216–222 below). These additional diagnostic approaches and analyses described above are options the author may consider to further bolster their analyses as part of a revision.

Specific Comments:

Abstract
L16 (and L57): I might recommend softening the language here and elsewhere in the manuscript, as the text suggests that lee cyclogenesis has never occurred here before – when I'm fairly positive a thorough examination of reanalysis data would suggest it is rather ubiquitous under a variety of westerly upper-level flow regimes. Indeed, a quick search of AMS journals highlights numerous articles that consider lee cyclogenesis near Greenland. Instead, I think it might be more accurate to state that the role of lee cyclogenesis has received less consideration in this region compared to other more well studied locations.

1. Introduction
L39–41: Consider expanding these statements with a bit more detail, if possible, to further motivate the importance of this work. Namely, what are the effects of Arctic cyclones on sea-ice concentrations? What are their common characteristics relative to midlatitude cyclones, etc.

L49–51: This statement is a bit confusing to me. Namely, what do you mean that the role of the jet streak is largely supportive and how is this different compared to midlatitude cyclones?

L53: More detail could be provided in this paragraph to offer some conceptual foundation as to how lee cyclones develop in a manner that is different from other classes of cyclones. This type of discussion can foreshadow the subsequent diagnostics applied later in the manuscript and those described as part of my third general comment above.

2. Methods
L73–75: Given that the model set-up is a crucial component of the forthcoming analyses, I recommend including more detail here instead of referencing prior studies. For example, did these prior studies simulate the same case under consideration here, or did they just adopt a similar model set-up? What were the details of the initial conditions (i.e., resolution)? How many vertical levels were utilized? What are the details concerning model parameterizations? How were the data nudged quantitatively? The answers to these questions can be very relevant for interpreting the forthcoming results and for ensuring the study's reproducibility.

L79: The choice to remove Greenland requires substantial changes to the atmospheric profile at grid points that were below ground level in the topographic simulations. What assumptions were made regarding atmospheric parameters at levels that were initially below ground but no longer are below ground in the no-topography runs?

L81: The naming conventions do not seem rather intuitive at this point in the manuscript and might be best moved until after the next paragraph, which introduces more details concerning the model runs.

L88: When during this time period did the lee cyclone develop? Was there any sensitivity of results to the choice of initialization time? Was there any sensitivity of results to your choice of the number of vertical levels for the nudging experiments? How did the nudging parameter specifically change with altitude?

3. Results

L100: Before diving into the simulations, it might be useful to show the actual evolution of the case from a reanalysis product or ICON analyses to demonstrate that the representation of the cyclone within this experimental environment is consistent with what was observed.

L103–104: It is unclear how this flow pattern is indicative of lee cyclogenesis, as the cyclone is upstream of the trough axis and beneath the left jet-exit region, which are favorable synoptic-scale conditions for lee cyclogenesis.

L111–113: It still appears at the start time of the evolution for **nudge_8km_0** that a ridge is located just east of Greenland, just like it is in **nudge_8km_1**. Could you clarify more what is meant by the disappearance of the ridge and how this relates to lee cyclogenesis?

L125: This equation might be more effective if it were introduced coincident with the text described in this paragraph rather than later into section 3.3.

L134: Consider adding a figure panel reference to this statement to help direct the reader's attention accordingly.

L141–142: Consider expanding more on this assumption, in terms of the physical processes that allow you to make this assumption.

4. Summary and Discussion

L216–222: I strongly disagree with this statement, as prior studies suggest that strong lee cyclogenesis events downstream of the Rocky Mountains are also accompanied by a jet streak that compliments the cyclone's development (e.g., Winters and Walker 2022; their Fig. 10).

Winters, A. C. and C. L. Walker, 2022: A jet-centered framework for investigating High Plains winter storm severity. *J. Appl. Meteor. Climatology*, **61**, 709–728, doi: 10.1175/JAMC-D-21-0211.1

Figures and Tables:

Table 1: Consider adding more information about the details of each model run as part of the table (i.e, parameterizations, resolution, nudging, vertical levels, etc.)

Fig. 3 and subsequent figures: Consider indicating in the caption what the gray dashed contours correspond to.