

**Review of "Greenlands Topography Triggers Cyclogenesis: Synergy between Lee  
Cyclogenesis and Jet Streak" by Cheng You submitted to ACP**

**General comments:**

In this paper a study is presented in which the respective roles of Greenland's topography and an upper-level jet streak in a case of lee cyclogenesis are determined through model experiments in which the topography is removed while parts of the atmosphere are nudged to maintain the original flow. The experiments themselves are interesting in terms of design and some potentially interesting results are presented. However, the novelty of the research is very overstated (and associated literature not cited), methodological details are insufficiently explained, and the analysis lacks depth. The presentation of the manuscript also needs improvement. Hence, I recommend that this manuscript be rejected.

**Major specific comments:**

**Abstract and L56** Here it states "Notably, lee cyclogenesis typically associated with large topographic barriers has not been observed on the lee side of Greenland". This statement is false. For example see Mc Innes et al. (2009, <https://doi.org/10.1002/qj.524>) which presents an analysis of the mesoscale structure of a mature lee cyclone southeast of Greenland that was observed during a flight with a research aircraft during the Greenland Flow Distortion experiment (GFDex). There are also several other studies that have documented lee cyclogenesis due to Greenland without considering additional local observations from field campaigns (see the introduction to the Mc Innes et al. paper). These studies include studies in which the impact of modifying the orography of Greenland on the cyclogenesis has been assessed (Petersen et al. 2003, [https://doi.org/10.1175/1520-0469\(2003\)060<2183:FITLOI>2.0.CO;2](https://doi.org/10.1175/1520-0469(2003)060<2183:FITLOI>2.0.CO;2), and Kristjansson and McInnes 1999, <https://doi.org/10.1002/qj.49712556003>), the same experiment as has been performed in this submitted paper although without the nudging to a global run. Also, many climatologies of extratropical cyclones have shown a pronounced genesis region in the lee of Greenland (e.g., Hodges et al. 2002, [10.1175/1520-0469\(2002\)059<1041:NPOTNH>2.0.CO;2](https://doi.org/10.1175/1520-0469(2002)059<1041:NPOTNH>2.0.CO;2)) whereas in L244 it is implied that such climatologies do not exist: "climatological analysis of lee cyclones near Greenland would offer valuable insights into future research". Hence this work is not as novel as claimed in the abstract and previous literature on lee cyclogenesis due to Greenland needs to be included.

**L40** The statement that "Basically, Arctic cyclones share common dynamical mechanisms with their extratropical counterparts." airbrushes over some important differences between Arctic cyclones and midlatitude cyclones. For example, see the composite analysis of Arctic cyclones by Vessey et al. (2033, <https://doi.org/10.5194/wcd-3-1097-2022>).

**Methods** When the topography of Greenland is removed how do the fields below the topography height get initialised? The information provided about the model simulations is far too limited. Line 83 refers to a table S1 in the "supporting information". I could not find the supporting information on the ACP webpage (apologies if I have missed this) but there is a Table 1 in the main paper. Is this the table you meant to refer to?

**L130** Please add some additional explanation of the negative pressure difference region that extends from the east coast near the south tip of Greenland towards Iceland. The text refers to downslope winds, which might well exist (though are not explicitly shown), but the relevance of these winds to the lower mean-sea-level pressure in the run with (compared to without) the topography is not clear.

**Definition of  $\text{GH}_{total}$  and  $\text{GH}_{low}$**  In the caption for Fig. 3 this it says "Greenlands upper-tropospheric orographic forcing  $\text{GH}_{total}$ " but in the caption for Fig. 4 it says "Greenlands upper-tropospheric

orographic forcing  $GH_{low}$ ". The terms are calculated from different simulations. However using the same description for the two terms is confusing and makes it difficult to interpret what they mean.

**Verification** At no point in this manuscript is the simulated cyclone (for the simulations without the removal of Greenland) compared with reality. The abstract (and later text) make the point that this cyclone "was observed" during the MOSAiC field campaign, so presumably there are local observations that could be used. At the very least though the simulated cyclone should be compared to an operational analysis or to a reanalysis.

**Approach** The approach used in the study, with fields being nudged to "reality" above different heights to infer the impact of Greenland's orographic forcing in both the lower and upper troposphere, is interesting. However, more analysis needs to be presented to demonstrate that the conclusions are valid. In particular, it is not obvious to me that the impact of orographic forcing in the upper troposphere can be inferred from the difference between experiments that purport to demonstrate the impact of orographic forcing on the lower troposphere and whole troposphere given the baroclinic feedbacks between upper and lower levels. An alternative approach could be to consider the quasi-geostrophic forcing from the different levels, for example as in Deveson et al. 2002, <https://doi.org/10.1256/00359000260498806>.

### Minor specific comments:

**L50** What does "primarily supportive" mean? As opposed to what?

**L72** Is the resolution nearly 10 km or the grid spacing (as these are different things)?

**Fig. 1** The color-filled contours are presumably mean-sea-level pressure rather than surface pressure (which would be  $\sim 700$  hPa over Greenland's plateau). Similarly for Fig. 6.

**L123**  $LC_{total}$  is defined here but then given in an equation on L161. It would be sensible to move the equations earlier to where the associated terminology begins to be defined.

**L125**  $SP_{8km_0}$  is defined twice on this line in addition to on line 116. Also eq. 1 is referred to here well ahead of where the equation is given in the text. Ideally the equation should be on the same page (or an earlier page) to where it is first referred to in the text.

**Fig. 3 and 4** What are the dashed grey contours?

**Fig. 4** The colourbar is labelled "Lee cyclogenesis at lower troposphere (hPa)". This should read "Lee cyclogenesis in the lower troposphere (hPa)" (note spelling error).

**Paragraph beginning L183** It would be helpful if the reader could be pointed to which figure(s) illustrate the points being made in this paragraph. I think we're being asked to compare Figs. 4, 5 and 6.

**L229** Here additional simulations with the ice edge removed are referred to. More details need to be given for the conclusions from these simulations to be included in the paper. Similarly a citation should be added to support that the small Rossby radius at high latitude indicates reduced energy dissipation, possibly Woollings et al. (2023, <https://doi.org/10.5194/wcd-4-61-2023>).

### Technical errors:

The English language level in the text has several glitches. I have included a few that I spotted here, though this is not a complete list.

**L43** Grammar. "Upper tropospheric jet streaks are...".

**L43** Why are the author names capitalised in this reference?

**L83** By "summed up" do you mean "summarised"?

**L188** "...forcing in the upper..." should be "...forcing is in the upper..."

**L210** "sustain" should be "sustains".