Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-278

Anonymous Referee #1

Referee comment on "EuLerian Identification of ascending AirStreams (ELIAS 2.0) in numerical weather prediction and climate models – Part 2: Model application to different datasets" by Julian F. Quinting et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-278-RC1, 2021

General Comments

In this study, the authors demonstrated the application of a new CNN-based WCB identification method in different datasets and compared it with the trajectory-based method. The new CNN-based method introduced in this paper can obtain comparable results with the trajectory-based method, while the trajectory-based method requires higher spatial and temporal resolution, as well as more expensive computer power. It could be a powerful high-efficiency process-oriented tool for multiple purposes. The authors did a great job in the analysis and comparisons. Overall, I think this manuscript is publishable in GMD.

Specific Comments

(1) Line 90 "For WCB ascent, a fifth predictor is the 30-day running mean trajectory-based climatological WCB occurrence frequency." Does it mean that for a specific dataset or model if you want to use the CNN-based approach, you need to run the trajectory-based approach first to obtain the 30-day running mean climatological WCB occurrence frequency?

(2) Line 103-104 "Here, we test for this relationship by matching the trajectory-based and CNN-based masks of WCB ascent with the extratropical cyclone masks …". In my understanding, for the relationship between cyclones and WCBs, "matching" in this study is defined using only the WCB ascent, while the "matching" in the trajectory-based method (line 76-78) is defined using all WCB objects, including inflow, ascent, and outflow. That is confusing. Why did not the authors use a consistent definition of "matching" between WCBs and cyclones?

(3) At lines 294-301, the authors discussed the potential reasons for the differences between the two approaches nicely. But do the authors have any ideas why the differences for WCB inflow and outflow are larger than the difference for WCB ascend in Figure 4?

(4) At lines 310-311 "For all three WCB stages the objects are generally larger with the CNN-based approach (not shown)." This is interesting. What is the difference (%) of the WCB objects between the CNN-based approach and the trajectory-based approach on average?

(5) The difference of BSS in Figure 5 is relatively large. In addition to the difference of WCB sizes as described at lines 310-312, are there any other reasons?

(6) Line 401-404, these datasets have quite different horizontal resolution. Is the CNN-based approach sensitive to the horizontal resolution? In this study, all datasets were remapped to 1x1 degree grid. Is that a necessary step for the CNN-based approach?

Technical Corrections

(1) Line 28-29 "on the order of 20K", is that 20K through the WCB life cycle or per day?

(2) Line 77 "an extratropical cyclone mask", please define it when it appears for the first time? Does it have a consistent definition through this manuscript?

(3) Line 115 "masks of blocks", similar with the last comment, please define "masks of blocks".

(4) Line 129-130, please clarify what are "CY45r1, CY46r1, and CY47r1".

(5) Line 197 “Due to the overall highest WCB activity during Northern Hemisphere winter (DJF)”, please add a reference here.

(6) Line 285 “For WCB ascent, the biases are generally smaller than for WCB ascent.” Do the authors mean “… smaller than for WCB inflow”?