

Interactive comment on “Stride Search: a general algorithm for storm detection in high resolution climate data” by P. A. Bosler et al.

Anonymous Referee #1

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The paper presents a new approach to detect features in highly resolved climate data and is certainly a valuable contribution to Geoscience Model Development (GMD). The paper is overall well written and structured. Yet some problems need to be solved prior to a possible for publication. So I recommend minor to major revision as outlined below.

Major comments

One of the major caveats is that the authors argue that cyclone tracking algorithms use the formula (3) on page 7735. I think this is only true for the TSTORM method particularly developed to identify tropical cyclones where this simple distance measure is similar to the geodesic distance. In cyclone detection and tracking methods for extra-tropical cyclones (Neu et al 2013) such an approach is normally not followed. Rather all the methods search for a local minimum in sea level pressure or maximum in the

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Laplacian of SLP / vorticity at 850 hPa, i.e., the eight neighboring grid points must have a higher SLP than the central point. Having identified such points other criteria are used to filter out artefacts. The often mentioned method of Blender et al. (1997) uses therefore the mean gradient of SLP around a center in an area $1000 \times 1000 \text{ km}^2$. This is given in kilometer to illustrate the fact that these authors use the geodesic distance function with a corresponding radius. Also the tracking uses the geodesic distance to identify in the next time step the nearest center in a radius (defined by an assumed maximum velocity of the cyclone). Other authors (e.g., Simmonds and Murry 1993) use central pressure thresholds to filter out artefacts, which is independent from the distance function used (but may have problems as mean SLP depends on the latitude). To connect the tracks these authors use also the speed and direction of the cyclone in ‘predict’ a potential area of occurrence of the cyclone in the next time step. Vorticity-based methods use thresholds for central vorticity and again make a prediction ansatz for the tracking (Hodges, 1994). So the use of the geodesic distance is not new in cyclone tracking and thus not a new advance. This needs to be clarified throughout the manuscript. The new element in the proposed method is formula (7) and this could be highlighted instead.

Another major comment is that by including formula (7), which is responsible for the speed up and certainly valuable for highly resolved data, is that secondary cyclogenesis will be ignored (e.g., Hanley and Caballero 2012) as only one minimum is identified in the rather large area encircled. This needs to be discussed in the conclusions.

Minor comments

7728, l8: The authors need to say why grid point search may fail?

7728, l8-10: The sentence reads not well and needs clarification.

7728, l10-12: Rather than saying that you compare something say what the result is.

7728, l14: I would not say that data analysis is a problem – this seems a bit awkward.

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7728-7729: The first two paragraphs contain statements, which need to be underpinned by references.

7738, l15-22: How computationally costly is the removal of duplicates?

7739, l23, 27: Certainly not a big issue but I am curious why the authors use different resolutions?

7743, l9: 'one a category 1 storm' is wired, I suggest to remove 'a '.

7743, l25-27: This is expected, as the authors did not adjust the TSTORMS method to the new area. Normally one would adjust a cyclone detection and tracking method to the area of interest.

7745, l4: Please change to 'climates become increasingly'

7745, l5-11: Certainly wrong – I asked my colleague R. Blender about this issue. See also major comments.

7746, 7747, 7748: It remains unclear how TSTORMS performs for polar lows.

7748l14-21: I suggest removing this paragraph and Fig. 8 as it is not relevant for the paper.

7748/7749: The authors need to say something how their procedure could be generalized to other features – this remains unclear although the title suggest this 'a general algorithm'.

Table 2: It is a bit weird that the numbers change – I agree that this might be due to splitting the tracks into two. I suggest using a different metric which is less sensitive to this, i.e., just count the time steps when the hurricane has the category – I guess that then the number are almost identical.

Fig. 3: In the manuscript you never mention that the new approach finds more artefacts, e.g. over the continents. Please mention this to be balanced. Maybe you can also give

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a reason for this.

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