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Discussions

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Interactive comment on "Tracking winter extra-tropical cyclones based on their relative vorticity evolution and sensitivity to prior data filtering (cycloTRACK v1.0)" *by* E. Flaounas et al.

Anonymous Referee #2

Summary

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The authors present a new cyclone detection and tracking method and show its ability in the context of ERAinterim data. They investigate the sensitivity of input data filtering on the resulting cyclones.

General comments

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Overall it is fine to present a new cyclone detection and tracking method, although

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there are several existing approaches. This is due to the fact that there is no clear definition of what a cyclone is and thus new approaches might deliver new insights by focusing on specific characteristics of cyclones. Thus I think that the paper is of value for GMD. However at the current state, there are a number of problems which needs to be solved first to recommend publication. The wording needs to be improved and the sensitivity of the method to other parameters needs to be presented. Therefore I recommend major revisions.

Specific comments

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Title: The title is a bit awkward, also the acronym of the method should be avoided (it is anyway never introduced in the manuscript). My suggestion is "Tracking winter extra-tropical cyclones based on relative vorticity: Sensitivity to prior data filtering and other relevant parameter".

P1246:

L4-9: It is not clear how the tracking is done, please clarify.

L10: I think you do not realy show a validation because therefore you need an absolute definition of what a cuyclone is, which does not exist. So just write "We apply the algorithm to ERAinterm data of the Northern . . ."

L11: You do not make any "integration" but sensitivity tests, so please avoid integration throughout the manuscript. My suggestion for L11-15: "winters of 1989–2009. For this data we investigate the sensitivity of the cyclone detection and tracking method on the prior filtering the relative vorticity field. Therefore three different filtering strengths are used. The filtering of the relative vorticity fields only yields an impact on weak cyclones, while in their majority the strong cyclones are independently detected and tracked."

L 19: Please remove 'the' before tropical.

L 25: I think fronts are also detected and tracked and could be mentioned here, the reference is Hewson and Titley (METEOROLOGICAL APPLICATIONS, 2010).

P1247:

L1: I think the wording is wrong. You use a two-step algorithm, the expression "phases" is misleading and should be avoided throughout the paper. Suggestion: "In general cyclone detection and tracking methods are two-step approaches: First the method identifies the location . . . given time and then in a second step, all features. . . "

L4-5: Why should the decisions have a causal relation to the flexibility?

L12: Please change the reference Blender and Schubert (200) to Blender et al. 1997, Monthly Weather Review).

L15: "For tracking cyclones, an algorithm needs . . ."reads better.

L17: That -> This

L21: Please remove "well designed" as it is judgement. It also implies that there are not well designed methods.

L 24: "nearest neighbourhood approach" is better.

L24-27: All algorithm suffer from the fact that identified centres could be wrongly connected, so it is not just a feature of the next neighbourhood method.

P1248:

L7-8: ". . . also proposed by Hodges (1999). This tracking . . ."

L15 and following lines: Raible et al (2008, Monthly Weather Review) were the first to compare different cyclone detection and tracking methods so they should receive some credit in this paragraph.

P1249

L10: Also the algorithm of Sinclair uses vorticity. I think there is also an assessment of filtering the data prior to the application of the method in these publications: Sinclair (1994,Mon. Wea. Rev) and Wea. Forecasting 1997).

L23-24: This sentence is awkward and should be revised. Please omit using the phrase "code" as this is a technical expression, just write method.

L26-29: "Methodology" is wrongly used here, please change to method throughout the manuscript. Suggestion: "In Section 2 the cyclone detection and tracking method is decribed in detail. In Section 3 the method is applied to the ERAinterim data set for the period 1989-2009 in winter (December-January-February) and results are presented for extra-tropical cyclones over the Northern Hemisphere. Finally, Section 4 hosts the conclusion and our prospects."

P1250:

L2: "The sentence "To better understand ..." is awkward and needs clarification

L5: Please mention that you use 6-hourly data (my guess).

L10 and L11: Phase -> Step

L12: Please remove the sentence "This is performed in three steps." as it is not needed.

L19-21: Please change to "to identify cyclonic circulations, the vorticity field is smoothed by applying a spatial correlation filter. The filtering is essential to suppress orographic or coastal vorticity maxima and to provide smoother gradients. . ."

Section 2: In high resolution data vorticity maxima are also located along fronts, so how do you avoid to select 'wrong' maxima or track along fronts and not track cyclone centres. A nice example is given in your Fig 1. There is a cyclone nearby Newfoundland with nice fronts visible in Fig1A. If we compare this with pressure maps (I did this) you will see that the centre of this cyclone is located just close to Newfoundland. IN Fig1c you would not be able to detect this location but rather the location of the warm front.

P1251:

L4: Formula (1) is wrong and could also include the threshold.

L17-21: The Hodges approach uses a smoothing to T42, which corresponds to 2.8x2.8 degrees So how does this influence your results. You could also think of using spectral smoothing of the vorticity field as an additional sensitivity test.

L24: The Threshold has an absolute value, but I think it should vary with the resolution as smoothing strongly decreases the absolute value of the input vorticity field.

P1253

L13: methodology -> method

P1254

L1-2: Maybe it would nice to show also a similar Figure as Fig 2 for the Strom nearby Newfoundland.

L3: Phase -> Step

L4: "Before combining the cyclone centres to a track, the algorithm sorts . . ."read better.

L7: "undergoing" could be removed.

L24-25: The sentence needs clarification.

P1255:

L1-4: I think also the Hewson and Titley (METEOROLOGICAL APPLICATIONS, 2010) method uses such an approach.

L10: It is not clear how you did the sensitivity tests? I also think that the authors need to present results of these tests.

P1256

L20: I think "calibration" is the wrong wording. You just try to find 'optimal' parameters for you method.

L21: You do no 'integration', just call it 'sensitivity of the method to three different filters applied to the input field'. So please avoid integration throughout the manuscript.

P1257

L5: 'integration problem'

L9: forms -> shape

L9-12: I think the Authors should also show the relative frequency distribution in Fig. 6 not only the absolute histograms.

L20: You do not show a PDF but just a distribution of relative frequency. So please avoid this expression.

L26-29: Here the question is whether weak cyclones in the strongly filtered data correspond to strong cyclones in the unfiltered data? So how does this affect your interpretation?

P1258

L7-9: For this statement there is no proof in the figure as Fig 8 only shows the cyclone centre density for all cyclones with no separation into weak and strong cyclones.

L13: Which threshold have you used, please quantify. Some test on the effect of this threshold on your results is missing and need to be added.

L23: 'Calibration problem' see above.

L24: Please change 'interannual distribution' to 'time series' and remove 'which resulted . . . integrations'.

Section 3.2: I suggest to also write in the text the temporal correlations between the time series shown in Fig. 9 in order to quantify whether the phasing remains the same

for different filters applied.

P1259

L4: Please remove 'algorithm'

L5-14: I do not understand what the authors do here, please clarify this part.

L27-29: I do not see this similar structure of the distributions, I would say they are quiet different in Fig. 7d.

L29: Please remove 'Indeed'.

P1260

L2-4: This conclusion remains unclear. All methods try to find their 'optimal' parameter set including sometimes a priori filtering, so it is certainly not an advantage of this method but just a necessity to find the ''optimal'' filtering. More important is how the authors define "optimal" and for which purpose. . .

L5-6: This could be removed as it is just explaining how filtering works in general.

L8-12: This sentence is awkward and needs clarification.

L14-16: This sentence is awkward and needs clarification.

L21: I would write composite life cycle and not evolution. (also elsewhere)

P1261

L14: You do not discuss the results, so I suggest using "Conclusions". Section 4: There are a lot of problem with the wording, so please involve a native speaker.

L27: The algorithm presented as only a few parameters – this is certainly true. However it sound like this is the only method with few parameters. As shown in Neu et al. (2013,BAMS) there is a number of methods which use only a few parameters.

1263:

L1-6: Please do not start with new results in the conclusion part. Additionally I would like know what a good skill is? In which sense?

L7-15: This is a rather long outlook and one could ask why the authors do not wait with their publication until they included some of the extensions mentioned.

Figures

Fig. 5: What is shown in Fig 5B? This is not explained in the caption.

Fig. 6: label of x-axis: Relative vorticity (x10ˆ-5 sˆ-1)

Fig.7: No PDF is shown, just the distribution of the relative frequency

Fig 8: The colorsclae could be improved. Please use discrete colours, a good example is given in Neu et al. 2013, BAMS)

Fig 10: Please label the panels with a,b,c and explain this in the caption (just to be consistent with the other figures).

All: I suggest asking a native speaker to 'polish' the English.

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