REVIEWER #1

We would like to thank the Reviewer for his comments and corrections. Here we answer point by point to his/her queries.

General comment: The paper documents a new algorithm for extratropical cyclone tracking and the effect of the initial filtering for dataset. In my opinion, to scientific journals such as Journal of Climate or Climate Dynamics, this paper has an insufficient material for new findings in meteorology and does not clarify the point. However, I guess that this journal is just for a technical report, and then, in this meaning, I really evaluate this paper as publication with a single major comment and a couple of minor comments.

Major comments: The authors only mentioned the sensitivity of initial filtering, but there are many tuning parameters in the tracking algorithm. Though the authors claimed as few parameters implemented in the algorithm as possible, I found many subjective settings there. The sensitivity for other parameters should be additionally commented in the discussion. Satake et al. (2013) might be a good reference.

Tracking cyclones is certainly a delicate issue where several subjective criteria need to be set. Indeed, in all tracking methods there are different approaches that are based on the fact that there is no strict mathematical or physical definition of a cyclone. In our approach there are six main constraints/criteria used to track cyclones:

1) a spatial filtering is performed
2) tracking is based on a cost function
3) a relative vorticity threshold of $3x10^{-5}$ s^{-1} is set for defining cyclones
4) tracking is done within a 10° spatial window
5) relative vorticity of consecutive track points must differ of no more than 50%
6) consecutive displacements have to present an angle greater than 90°, if displacements are longer than 3°.

In the original submission of the article we mainly focus on the data filtering operation (first point), since we consider this to be the most determining element of our method for the tracks number and form.

To further explore the sensitivity of our method, in this revised version of the article we included two new sets of sensitivity tests on the 2^{nd} and 5^{th} point. The sensitivity tests are now presented and discussed in section 3. The other points correspond to constants for all different applications of the code.

Satake et al, (2013) is indeed a good reference for our work and is now cited.

Minor comments:

1) Equation (1) is a mathematically incorrect expression. Is 1/X to be 1/(2X+1)? Moreover it should be remarked that a spatial filter of 1-1-1 by 1-1-1 used here is not always a good spectrum property.

The equation is now corrected. There are many options for filtering relative vorticity, such as b-spline techniques (Hodges, 1995), time band-pass filtering (Hoskins and Hodges, 2002; Inatsu, 2009), 1-2-1 filters as the one proposed in Satake et al, (2013) and others. Here we use an alternative approach, been aware that all filters present advantages and disadvantages. It is for this reason that we performed sensitivity tests (filter3, filter5 and filter7) in order to explore the limits and "technical behavior" of our method. The smoothing operation we use here has been tested for several case studies providing realistic results.

Hoskins, Brian J., Kevin I. Hodges, 2002: New Perspectives on the Northern Hemisphere Winter Storm Tracks. *J. Atmos. Sci.*, 59, 1041–1061.

Inatsu, M. (2009), The neighbor enclosed area tracking algorithm for extratropical wintertime cyclones. Atmosph. Sci. Lett., 10: 267–272. doi: 10.1002/asl.238

Hodges, K.I.: Feature tracking on the unit sphere, Mon Wea Rev, 123, 3458–3465, 1995.

2) Figure 7 (including legend) and related sentences in the body do not use PDF but frequency distribution because these are not density.

Legend and text is now corrected.

Satake, Y., M. Inatsu, M. Mori, and A. Hasegawa, 2013: Tropical cyclone tracking using a neighbor enclosed area tracking algorithm. Mon. Wea. Rev., 141, 3539–3555.

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REVIEWER #2

We would like to thank the Reviewer for his comments and corrections. Here we answer point by point to his/her queries.

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.

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

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".

We changed the title as suggested, however we retained the code acronym (as demanded by the journal rules).

P1246:
L4-9: It is not clear how the tracking is done, please clarify.

The abstract is reformulated and clearer on the tracking step.

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

Done.

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."

Indeed "integration" is not adequate to describe the algorithm runs. The text is reformulated as proposed and the word "integration" has been replaced throughout the text.

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

We added several comments on tracking frontal vorticity maxima (please refer below to your first comment on section 2) and the reference of Hewson and Titley (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…"

"Phases" have been changed to "steps".

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

The phrase was more confusing than helpful and we feel that further explanations would not make a strong point. We removed the phrase.

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

Done.

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

Done.

L17: That -> This

Done.

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

Done.

L 24: "nearest neighbourhood approach" is better.

Done.

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.

The phrase was strongly critical and unsupported, we removed it.

P1248:
L7-8: "…also proposed by Hodges (1999). This tracking…"

Done.

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.

The article by Raible et al, 2008 is now commented.

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).

References on Sinclair, 1994, 1997 have been added.

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.

Done.

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.”

Done. Methodology has been changed to method throughout the text.

P1250:
L2: “The sentence “To better understand...” is awkward and needs clarification

Done.

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

It is indeed 6hourly data that we use. It is now mentioned.

L10 and L11: Phase -> Step

Done.

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

Done.

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…”

Done.

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.

This is an insightful comment and the example of the Fig. 1C illustrates exactly the Reviewer's point. Indeed, in high resolution datasets (e.g. regional climatic simulations) it is likely that frontal areas might be detected as cyclone centers due to local vorticity maxima. However, this

might also be true in pressure-based algorithms as frontal edges may also present a low pressure center. Consequently this is a universal problem for all tracking methods. We added the following paragraphs at the end of Section 2.2.

"It is likely that our method detects fronts associated with vorticity maxima as cyclone centers, especially when applied to high resolution datasets (e.g. regional climatic simulations). In order to avoid the detection of a frontal zone, additional criteria of high or low complexity should be considered (e.g. Hewson and Titley, 2010). However, such criteria could be dependent on several factors -as for instance the spatial resolution of the dataset- and would result to a "stricter" cyclone definition. The more precise the mathematical criteria, the more constrained are the tracking results to systems of specific characteristics. In the case of fronts, the latter could for instance exclude the early stages of certain tracked cyclones that emerge from high vorticity frontal areas of a "parent" cyclone.
An example of a front detection is illustrated in the two cyclones cases, presented in Fig. 4. Inspection of surface pressure charts (not shown) showed that the first track point of the second cyclone (red dot in Fig. 4b) corresponds to the front of an extra-tropical cyclone (the one depicted by the black track). In the following time steps (Fig 4c to 4f), this secondary vorticity maximum evolves to a strong cyclone (red track) which presents its own low pressure minimum. Here we capture the initial stage of the vorticity maximum, before the occurrence of a pressure minimum. Nevertheless, not applying additional criteria might demand post-treatment of the track results in order to exclude "wrong" tracks or tracks that do not match the research needs."

P1251:
L4: Formula (1) is wrong and could also include the threshold.

Corrected.

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.

There are many options for filtering relative vorticity, such as b-spline techniques (Hodges, 1995), time band-pass filtering (Hoskins and Hodges, 2002; Inatsu, 2009), 1-2-1 filters as the one proposed in Satake et al, (2013) and others. Here we use an alternative approach, been aware that all filters present advantages and disadvantages. It is for this reason that we performed sensitivity tests (filter3, filter5 and filter7) in order to explore the limits and "technical behavior" of our method. The smoothing operation we use here has been tested and stretched for several case studies providing realistic results.

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.

This small paragraph has been also added to the manuscript, also answers the query:

"....values exceeding the $3x10^{-5}$ s^{-1} threshold. The selected threshold value is a good trade off for detecting cyclones in coarse resolution datasets (e.g. 1.5°x1.5°, as in ERA-I used here) and in high resolution datasets (e.g. 20km regional climate runs). A threshold may function conveniently as a constant for better adjusting the filtering strength. Alternatively, one could keep the filtering strength constant and make the threshold value vary. However, it is only by varying the filtering strength that the vorticity field may be smoothed within the characteristic length scale of cyclones."

P1253
L13: methodology -> method

Done.

P1254
L1-2: Maybe it would nice to show also a similar Figure as Fig 2 for the Strom nearby Newfoundland.

L3: Phase -> Step

Done.

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

Done.

L7: "undergoing" could be removed.

Done.

L24-25: The sentence needs clarification.

Phrase is now reformulated.

P1255:
L1-4: I think also the Hewson and Titley (METEOROLOGICAL APPLICATIONS, 2010) method uses such an approach.

Indeed, Hewson and Titley use "Likelihood" score that is dependent to the "feature type transition" and the thickness change between 1000 and 500 hPa. Reference is now added.

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

These sensitivity tests are now presented.

P1256

L20: I think “calibration” is the wrong wording. You just try to find ‘optimal’ parameters for you method.

Title has been changed to “Application the tracking algorithm in a climatological context and sensitivity in different parameters”

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.

Done.

P1257
L5: ‘integration problem’

Done.

L9: forms -> shape

Done.

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

Done.

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

Done. PDFs have been changed to relative frequency distributions.

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?

We performed a simple analysis and we added the following into the text:

“A question that may arise is whether weak cyclones in the strongly filtered sensitivity tests correspond to strong cyclones in the weakly filtering tests. To address this question we took into account all points of the distributions in Fig. 6 and we associated the common points between *filter3* and *filter7* (points sharing the same timing and having a distance inferior of 5°). Results showed that *filter7* shared 52% of its points (2331 points) with *filter3*. The median of the intensity of the common points of *filter3* corresponded to the 78th percentile of all *filter3* points’ intensity. Consequently cyclones in *filter7* correspond to the strongest cyclones of the weakly filtered datas.“

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.

This phrase has been removed.

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.

Here we refer to the strong filtering run, not to a stronger threshold. This was a a typo mistake and the phrase is now corrected.

L23: 'Calibration problem' see above.

This paragraph has been removed

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

Done.

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.

The following has been added to the text:
"*The time series phasings are in good agreement between filter3 and filter5, presenting a correlation score of 0.91. On the other hand, the correlation score between filter5 and filter7 is 0.43, suggesting that the time series phasing between the two sensitivity tests is dependent to the weaker cyclones that are suppressed in filter7. This should not raise a question on the "correctness" of the different test results, but rather on the results independence to the different filtering strengths.*"

P1259
L4: Please remove 'algorithm'

Done.

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

This part has been removed since it does not add much on the sensitivity of our method but rather to a post-treatment of the results.

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

Panel d has been removed (corresponds to a removed part, see previous comment).

L29: Please remove ‘Indeed’.

Done.

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…

Also in the removed part.

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

Also in the removed part.

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

Also in the removed part.

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

Also in the removed part.

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

Done and done!

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.

Title has been changed.

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.

We changed the phrase to : As also in previous methods, our identification and tracking algorithm for cyclones uses the fewer constraints possible...

P1263:
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?

This part has been removed.

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.

The perspectives part is now reduced to a simpler phrase.

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

It is typo instead of: "....shown in Fig. 4b as in **(A)** but dashed line..." it should be "....shown in Fig. 4 **(B)** as in **(A)** but dashed line...". It is now corrected.

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

Done.

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

Titles are corrected.

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

The colorscale is now changed to include more and discrete colors

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

Done.

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

English has been reviewed.