Review – Ambrogio Volonté
Objective identification of high-wind features within extratropical cyclones using a probabilistic random forest (RAMEFI). Part I: Method and illustrative case studies
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This manuscript contains a very interesting study, illustrating an objective approach at the identification of strong wind features in extratropical cyclones. The manuscript is well-written and insightful and the results shown are indeed promising. Novel methods are used, sometimes beyond the typical expertise of WCD readers (and reviewers!). On this, I would recommend addressing the main comments made by the first reviewer. My concerns echo those three key points and therefore I'm not repeating them here. I add below some other comments, all generally minor. I would be happy to see this manuscript accepted for publication once all these points are addressed.

Dear Ambrogio,

We thank you for your valuable comments that helped to improve this manuscript. For the three key points made by reviewer 1 please see the corresponding reply. We further clarified the remaining open issues you made us aware of.

Below are the responses to your individual comments in blue. Text changes are included in italics when suitable. Line numbers correspond to the revised manuscript.

Comments:
Abstract:
- Line 9, "of spatial dependencies and gradients": this is quite vague and not really clear.

This was changed to "*[…] independent of local characteristics and horizontal gradients, […].*" (line 9)

Introduction:
- Line 25, "belong to": I would write "can produce some of" or something similar.

This has been changed accordingly (line 25).

- Line 34: I would write "or, in short, the warm jet (WJ). Same point applies to the CJ.

This has been changed accordingly (line 34).

Characteristics of high-wind features:

- Line 92, "little or no precipitation": can you provide a reference for this statement? Warm conveyor belts are, as you write, airstreams ascending in the warm sector of the cyclone, and are associated with vigorous moist processes (condensation, ...). Therefore, the reader would be surprised to hear that little or no precipitation is associated with them.

We clarified the distinction between the WCB and our definition of WJ as also asked by Reviewer 1:

"*During the ascent, the WCB splits into a cyclonic and anticyclonic branch as seen by the red tubes in Fig. 1. While the cyclonic part forms the cloud head and usually causes heavy precipitation along a narrow region, the anticyclonic part rises above the warm front and brings more moderate precipitation over a wider area. Overall, the WCB is the main cause for long-lasting precipitation (Catto, 2016). Furthermore, the WCB can be the cause of strong convection along the cold front (Hewson and Neu, 2015).*

Contrary to Hewson and Neu (2015), we define the WJ as the region ahead of the cold front and its convection, hence ahead of the CFC feature (cf. Sect. 2.4), as displayed by the red shaded ellipse in Fig.1" (lines 95-101)

- Line 109, "ground 850 hPa": missing word here, perhaps "below 850 hPa"?

This has been changed to "*[…] stays close to the ground, i.e., below 850hPa […].*" (line 120)

- Section 2.5: I find feature naming not totally consistent here. If dry intrusion and post-cold-frontal convection are both subsets of CS, why does Figure 1 display CS + pCFC in the cold sector? Shouldn't it be DI + pCFC (= CS) ?

In an earlier version we included pCFC as a separate feature to see if its characteristics are closer to CS or CFC but it did not occur enough in observation data to derive meaningful statistics. We understand that it might be confusing in the current version and excluded it from Figure 1.

<u>Data</u>:

- Line 153: "wind speed at 10m" (same applies to wind direction)

This has been changed accordingly (line 168).

- Line 153: I'm being pedantic here, but could you replace "RR" with "R", given that it's precipitation amount and not rain rate? (or if it's actually rain rate, please state it)

We state that we have hourly data including precipitation amount, hence a rate.

<u>Method</u>:

- Line 218, "Section 4": this line is already in Section 4, so I guess this should be referring to a different section.

Thank you for making us aware of this. We removed the reference as we meant to refer to a former paragraph in the same subsection.

- Line 219: I would argue that wind speeds are not "enhanced" by a strong pressure gradient, as this is what causes winds to be strong (at meso-synoptic scale) in the first place! In my view, factors enhancing wind speeds are those not accounted for by the (gradient-wind-) balanced flow, such as convective downdraughts for CFC and symmetric instability for SJ.

> We agree and apologise for the misleading phrasing. We changed the sentence to "*[…], with an exceptional large pressure gradient leading to a stronger background wind field, such that […].*" (line 207)

- Table 2: could you include the height of max wind location? (and if it's > 800 m, as I think Zugspitze is, add the max winds below 800m?)

> We included the heights and have now two columns with maximum gusts – one above and one below 800m.

- Figure 2: could you make the colour progression more intuitive? (e.g., using green and dark green in consecutive years, instead of having red in between)

> Thank you for this advice. The order now matches a rainbow pattern.

- Lines 225-226: could you mention the features (after NF) according to the number of points, in decreasing order? I think it would make for an easier reading of the sentence.

> This has been changed accordingly (line 269).

- Line 228: I'm not sure I understand the rationale of merging CJ and SJ points. I would understand doing this if you weren't able to separate them, but you have just listed them separately, so I don't get why you then decide to put them back together. Is this because you think RAMEFI wouldn't be able to separate them? If so, state it explicitly.

> We labelled both SJ and CJ individually and trained an RF with these features separately to see if a distinction is possible with the used surface parameters alone. Unfortunately, but as expected, this was not successful, such that we included the SJ in the CJ category.
>
> We added the following for clarification: "*A first training with SJ and CJ as separate features showed that a clear distinction is not possible with the information at hand and that the SJ is mostly detected as CJ. Therefore, we decided to include it in the more frequent CJ feature […].*" (lines 271-273)

- Line 249-250: Yes, I think this is really important

- Line 267: "Appendix A1".

> This has been changed accordingly (line 317). Note that the order of the appendices changed.

- Line 286: "are provided in Appendix A2 (practical implementation) and B1 (mathematical formulation)". (Do you reference to A3 anywhere? I couldn't find where)

> This has been changed accordingly (lines 233-234). Thank you for pointing out that we did not refer to Appendix A3. We included a reference in Section 3.4 (former Section 6.1; line 337).

<u>Illustrative case study</u>:

- Figure 3: I'm not sure I'm reading it correctly, as it seems to me that also locations where v < 0.8 (i.e., outside that dotted contours) are included in panel d. Also, I can't see any solid contours. Could you clarify?

> We apologize for the confusion. We decided to include contours to indicate the regions of high winds. However, we used a simple interpolation approach to get the observations on a grid. Indeed, this did not work well enough. As we did for the feature probabilities, we now use Kriging to interpolate v and added a smoothing step to remove some noise. However, this leads to some stations still being outside the contours, although v>0.8 was measured. We hope that the changes are still satisfactory.

- Line 349: Where would a "hook-shaped structure of the winds" be considered in your algorithm?

> The hook-shaped structure is not considered in the algorithm. While we used this characteristic for the detection of a CJ, the RF seems to be able to detect it without. However, this might be one of the reasons, why the distinction between CJ and CS can appear difficult at times.
>
> We added a short note on this in Section 5.3: "H*owever, the occurrence of a hook-shaped structure cannot be accounted for in the spatially independent approach of RAMEFI making it difficult to distinguish these otherwise similar features.*" (lines 402-203)
>
> And further in Section 6.1: "*The main meteorological reason for this problem is the general similarity of the two features and that the hook-shaped structure, which is used for the subjective identification of a CJ, cannot be considered in the RF, such that […].*" (lines 432-434)

<u>Statistical evaluation</u>:

- Line 374: "in Appendix B2".

> This has been changed accordingly (line 337).

- Lines 366-367, "Further, [...] or 1". Could you rephrase this sentence? It is not very clear to me.

> Thank you for pointing this out. We rephrased the sentence by using the term "*confidence*" and not "certainty" (line 221). A prediction is said to be sharper, the more confident it is. In case of probabilities, a prediction is more confident if the predicted probability is closer to 0% or 100%, which are the most confident, i.e., the sharpest, predictions one can make.

<u>Discussion</u>:

- Figure 7 and all-pairs approach: Does the order of features matter? In other words, is (0 = CJ, 1 = CS) equal to (0 = CS, 1 = CJ). If that's not the case, the missing panels should be included and discussed.

> No, the order of the features does not matter, it only affects the orientation. If we choose (0 = CJ, 1 = CS), then a predicted probability of 70% corresponds to the occurrence of a

CS, and if we choose (0 = CS, 1 = CJ), it corresponds to the occurrence of a CJ. Hence, the two reliability diagrams of the two orderings are not identical but (some kind of) symmetric to each other and contain no additional information with respect to each other. Therefore, it is sufficient to consider only one of the two cases.

- Lines 476-478: Wouldn't high winds related to a strong synoptic-scale pressure gradient (which to me just means a deeper, more intense cyclone) still fall in the same categories (features) but with higher wind speed values than a shallower cyclone? Or are you implying that deeper cyclones have on average a different structure? Please clarify this.

Indeed, the same categories should apply for deeper cyclones, however, the distinction between the features is made more difficult and one cannot be certain if a feature really occurred or if the high winds are only caused by a rather unstructured background pressure gradient. We changed this to "*[…] sometimes high winds are mainly related to an exceptionally strong synoptic-scale pressure gradient […]*" (lines 512-513) and included more clarification in Section 7.2 (see next comment).

- Section 7.2: Still on the same point, but I think this is crucial for the understanding. I don't think the expression "the highest wind speeds are enhanced by a strong background pressure gradient" is physically correct (see my "Line 219" comment). I agree that with a deeper cyclone, more locations can record strong winds, but I would still assume that even if they're not directly related to CJ or WJ, they would still be located either in the warm or cold sector (and thus fall in the CS category in the latter case). Could it be that you need a WS category?

Again, we apologise for the misleading phrasing and confusion. We clarified the issue as also discussed in comments above in the first paragraph of Section 7.2.: "[…] unconnected to one of the four mesoscale wind features under study but can be enhanced by them. With an underlying strong wind field, the detection and distinction of the features might be more complicated." (lines 547-548)

- Lines 498-499: please explain the difference between front and convergence line here, to improve clarity.

We added the following sentence to the first paragraph of the subsection: "*While a cold front is associated with a second low pressure trough, a convergence line develops where two airflows collide and can occur independently of a cyclone.*" (lines 525-526)

- Lines 539-540: One could argue that this shows that the set of surface parameters used to train the RF is broad enough to allow it to include both SJ and CJ in the "CJ" category. Are you ruling out that separating SJ from CJ in the observation would lead to RF correctly identifying them individually? If so, on what basis?

As mentioned prior (see comment on line 228) we trained a RF with both features separately and added a comment on this in Section 4.1. Here, we want to show that the RF detects the SJ within the CJ feature as argued.

- Sections 7.2 and 7.4: My understanding is that using normalised values (of pressure, theta, etc...) you can identify features even in cases departing from "your climatology" (i.e., the mean values in the 12 cases selected). However, in these sections, you show that "anomalous" cases can be a challenge for RAMEFI. Do you expect this issue to be partially or totally solved when RAMEFI is trained on more data? Is this explored in the Part II paper?

> We normalise theta and wind speed to remove location-specific effects, such as exposure near a coast or hillside, i.e., consider local anomalies. However, we do not normalise pressure or theta with the synoptic background state of a given storm, i.e., we do not consider anomalous cyclone developments, as, using irregular distributed station observations, we would not be able to find a mean temperature or core pressure for normalising these parameters anyway as discussed in Section 7.4. For the sake of simplicity and since it seems to be working well for most cyclones, but also ok for anomalous cases, we argue that the spatial independence has more advantages than disadvantages.
>
> Part II will not focus on a newly trained RF but will use RAMEFI trained on the introduced 12 cases on two data sets of almost 20 years. Including more storms will probably not have a significant effect on the training.

Conclusions:

- Lines 574-575: What surface values do you think could best distinguish CJ from CS (apart from p)? Maybe spatial temperature/theta gradients could be useful? Otherwise, the most obvious to me would be cloud cover, but I'm not sure how many stations would have radiation/sunshine measurements.

> As mentioned in the manuscript and the comment before, normalising the pressure by the core pressure might help distinguish the two features. However, the problems with this outweigh the advantages as discussed. Same for theta. Gradients are not available for station data. Cloud cover might be an interesting parameter indeed but is not available in our data set and is probably difficult to obtain for a data set of 20 years over most of Europe.

- Lines 588-589: This is indeed an encouraging result, but this sentence is probably too bold, given you've just looked at 12 cases in observations vs 1 reanalysis dataset. Maybe you could replace "should" with "could"?

> This has been changed accordingly (line 624).