Paper wcd-2019-1

"Medicane Zorbas: Origin and impact of an uncertain potential vorticity streamer"

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Response to the Reviewers' comments:

We thank the reviewers for their many critical and constructive comments that helped improving the manuscript. All points have been carefully considered and will lead to the following important changes compared to the original revision:

- We better explain the objectives of the study, which focuses on the synoptic-scale processes involved in the formation of this intense cyclone.
- The introduction is restructured, following the suggestions by the reviewers.
- The paper contains a separate data and methods section.
- The cyclogenesis of 'Zorbas' is introduced in more detail.
- The analysis of the error amplification and propagation is improved, and a few aspects of our previous analysis are omitted.
- The final part of the study (cyclogenesis in the ensemble members) is completely rewritten, also based on additional analyses.

Below are the detailed replies to the individual comments (in blue).

Reviewer 1 (Ron McTaggart-Cowan)

Background

The authors investigate the predictions of a September 2018 medicane in the ECMWF ensemble system. They identify ensemble members that have differing day-3 representations of a PV streamer involved in the storm's development. They track these differences back to small differences in the initial conditions and show the progression of PV spread in association with an anticyclonic Rossby wave break.
The remainder of the analysis focuses on the interaction between the PV streamer and the developing cyclone: distinct precipitation and storm structures are identified in the different sub-ensembles.
Reductions in predictive skill associated with the development of sub-synoptic systems in the Mediterranean region are an important subject for investigation. Similarly, the limits of predictability imposed by PV streamer evolution and interactions between such features and lower-tropospheric circulations are not well understood. Despite these interesting fundamental underpinnings, the current submission suffers from a large number of flaws in organization, preparation and analysis. Although each of these may not be considered fatal in isolation, a significant amount of effort will be required to address all of them thoroughly. Any revised submission of this investigation will necessarily be heavily modified and constitute a new piece of work. I hope that the authors will find the comments below useful as they pursue this research.

General Comments

1. The manuscript lacks organization and logical flow. This extends from the highest level of structure (sections and subsections), down to the paragraph and even sentence level. It makes the manuscript difficult to read and follow because concepts and details are disjointed, scattered and frequently repeated throughout the text. Ordinarily, I would consider these kinds of organizational issues relatively minor and possibly within the domain of the authors' discretion; however, in this case they seriously detract from the work and make it very difficult for readers to follow the investigation. Addressing these problems will involve the rewriting of much of the text, but will yield a more focused manuscript that will likely be shortened by at least 1-2 pages.

a. High Level

i. The structure of the introduction is ineffective. It begins with a
very cursory review of tropical transition and medicanes (including PV streamers), then switches to a Rossby wave discussion that returns to error amplification twice, and then goes back to a very short summary of Zorbas. The latter also includes thesis questions and an outline of the remainder of the study that lacks section information or internal references.

We agree that the introduction could be better structured. We will streamline and rewrite parts of the introduction and provide section information when presenting the outline of the study.

ii. I do not think that the decision to replace the standard "Data and Methods" section with "Operational ECMWF Products" is effective. It means that additional data sources (satellite imagery, lightning detection, etc) have to be described in the text body, descriptions that seriously distract from the analysis when they occur. The same is true of methodological details (e.g. LAGRANTO, which is introduced twice) and the entire discussion of tracking and the CPS (L378-L392). Descriptions of all of these sources and techniques should be centralized in a "Data and Methods" section.

We agree and now include a proper "data and methods" section that introduces most methodological aspects of the study.

iii. There are numerous forward-references to section 6 throughout the earlier sections of the manuscript. While appropriate internal referencing is a useful tool, these repeated forward-references are likely an indication of poor manuscript organization, especially when they underpin important elements of the analysis (for example, the CPS referenced in sections 3 and 4 but never shown despite a reference to "Fig. 4a", which does not exist in the submission; L159). I think that the synoptic analysis (ideally shortened from its current length by enhanced focus) should include a discussion of the medicane itself, including the CPS. I understand that the medicane is not the intended focus of the work as is repeatedly stated in the text; however, the reader could be excused for thinking that it is because of (1) the title, (2) the multiple introduction subsections that deal with TC-like features, (3) the statement on L317 that the investigation of the "development of a medicane-like system" is an objective of section 6.2, and (4) the pervasive forward-referencing to the CPS analysis in the text.

We agree that the medicane itself should be more in the focus of the synoptic overview. We shorten some aspects of the synoptic overview, but add the CPS and the track of Medicane Zorbas.

b. Medium Level

i. Each section should begin with an introduction of the section contents so that the reader has an idea of how the section fits into the larger narrative of the work. A section should not begin with a description of data used in specific figures, as does section 3. Please revise each section introduction to ensure that the reader is logically guided through the work.

We agree that the text should be written in such a way that it guides the reader as elegantly as possible through the study. We consider this when rewriting the section introductions.

ii. Each paragraph should begin with an introduction to the paragraph contents, and should conclude with a statement that relates back to the material introduced. There are very few paragraphs in the manuscript that follow this basic structure. A particularly clear example of a paragraph that ranges far too broadly occurs on L50-64. The paragraph (and subsection) starts with a description of initial condition uncertainty, moves into ensembles more generally, then into PV error growth, and ends with a discussion of tropical cyclones. This lack of focus makes the study very difficult to follow despite the fact that the investigation itself is relatively straight-forward. In this case, the subject of error growth reappears in the paragraph starting on L73, which further adds to the confusion of the discussion. Please do not simply rewrite the paragraphs noted in this comment: the structure of virtually all paragraphs in the manuscript needs to be reconsidered and revised, an effort that will lead to rewriting of large portions of the work. The readability problems induced by the lack of logical internal paragraph are more than aesthetic in this case, and are serious enough that they significantly reduce the potential impact of this work.

We apologize for the obviously in parts confusing writing style in the original submission. However, we argue that the rule that *every* paragraph should begin with an introduction to the paragraph contents might be too restrictive. Again, we carefully reconsider the structure of the entire text in order to increase readability.

iii. I do not think that summary paragraphs at the end of a section are useful, particularly given the large number of relatively short sections in this submission. For example, the summary on L163- L167 is redundant with analysis undertaken in the previous page of the manuscript. Please remove summary paragraphs (they also appear at the ends of sections 5 and 6.1) in favour of making the text itself direct, clear and readable (see item 1.c.ii below).

We removed unnecessary summary paragraphs at the end of sections, which just repeated what has been discussed before.

c. Low Level

i. Reference to caption-level figure details within the text is highly distracting. For example, the fact that precipitation is shown in red solid contours in Fig. 1 is referenced three times in section 3 (once erroneously as "blue contours"; L130), while the fact that QG vertical motion is shown in red contours is referenced twice in the same section. These plotting details are described in the caption, and their appearance in the text detracts from the flow of the analysis. Figure and panel references should be enough to guide readers through the discussion. Please consider removing caption information from the text body throughout the submission.

We agree that the original version contained to many caption-level figure details. However, we are still convinced that, in some cases, they can help the reader to quickly grasp the relevant information from the figures. Therefore, we carefully re-consider the usefulness of caption information in the text and remove them where possible.

ii. The writing style is too informal and lacks the precision required for scientific text. For example, the outline of the manuscript is described as “a journey” on L101, the analyses in section 6.2 “hint” at airstreams (L321), and the first person plural (“we”) is used heavily throughout the submission. The introduction of section 6.2 (L320-324) can be summarized as, “we don’t do anything thoroughly here, but we show stuff that’s different and make some guesses about what that means; then in the next section we do it properly”. I don’t think that that kind of introduction (or approach to the analysis) will make readers want to continue to invest their time in the rest of the section. Throughout the submission, irrelevant details clutter the text (e.g. does it matter that 1800 UTC 26 September is “in the evening of the same day” on L126?), and ill-defined concepts reappear throughout the analysis (e.g. the “C-shaped” PV cutoff with a “dent” and “dent structure” on L138, 141 and 146, respectively). Every effort should be made to make the text succinct and readable, so that the analysis does not get lost in superfluous details and unnecessary bridging statements.

Here we ask the reviewer to please consider that we are not native speakers and, in some cases, we don’t realize that our language becomes too informal. We thought that the “C-shaped” cutoff would be a useful terminology, but obviously it isn’t. We improve the text and tried to avoid too informal terminology.

2. I think that cyclogenesis in cluster 3 is really interesting, but that the discussion in the current study misses the opportunity to capitalize on its uniqueness (I do not think that section 6.3 is sufficient in this respect). It looks to me like this is an excellent example of a nonlinear response / bifurcation leading to a real limit on predictability. Clusters 1 and 2 are simply phase shifts of the same cyclogenesis event. From a guidance perspective, both are reasonably useful at least in terms of situational awareness. Cluster 3, however, looks to me like the development of a different cyclone. There’s an 850 hPa circulation south of Turkey in all of the groups at 1200 UTC 26 September (Fig. 8, column 2). In fact, a cyclone has already formed in this region in many of the cluster 3 members and one of the cluster 1 members (also shown in Fig. 9a). In groups 1 and 2, the low between Crete and Cyprus disappears as the PV tail promotes development along the African coast. In group 3, the pre-existing cyclone intensifies and fractures the PV streamer as the low retrogresses towards Crete (Fig. 8, column 3). By 1200 UTC 28 September, the medicane lies in the central Mediterranean in groups 1 and 2, but it is a completely different storm that is centered on Crete in group 3 (this differs from the interpretation implied by discussions on L286-L290 and L303-304 of the submission). So the relatively small difference in the location of the PV streamer axis (a linear change from west to east of the observed location) leads to a highly nonlinear response in the form of development of a new cyclone (groups 1 and 2) or intensification of an existing circulation (group 3). (Note that a couple of centers form over northern Africa in group 3 at 1200 UTC 27 Sept – Fig. 8k; these are cases in which the response to the change in PV streamer position is linear.) The theory that

group 3 is fundamentally different from the others is supported by the precipitation patterns and tracks (Fig. 9; noting that the large track jumps between North Africa and Crete are unlikely to be accurate) parcel trajectories (Fig. 10) and parcel properties (Fig. 11). Because a nonlinear response / bifurcation is known to impose strong limits on predictability, identifying and describing such behaviour in this case would be an important outcome of this work. I hope that some of the length reductions achieved by improving the manuscript's focus and organization can be invested in a much more thorough analysis of this possibility.

Many thanks for emphasizing the strongly nonlinear effects seen in cluster 3. We agree that this is an interesting aspect and discuss it in more detail in the revised version of the paper.

3. The values of QG vertical motion seem too small to be very meaningful despite being described as "strong" in the text (L441). Vertical motions of ~0.5 mm/s and 1 mm/s (0.005 and 0.01 Pa/s) are plotted in Fig. 2, while values of up to ~5 mm/s (0.05 Pa/s) are plotted in Fig. 7. These values are all well within the typical rms of QG vertical motion at midlatitudes and mean vertical motions across the globe (Stepanyuk et al. 2017). If these calculations and plots are correct, then the vertical motion forcing from the upper levels is almost irrelevant to the real vertical circulations in most cases. Such weak vertical motions would need to be sustained in-place for many hours/days to have any appreciable impact on moistening or stability. For example, air in the peak ascending region in Fig. 2c ascends <10 hPa in a day in response to QG forcing, an ascent rate that is dwarfed by the 600 hPa ascent in the rising parcels near the centre. If the calculations are correct, then the relevance of the PV streamer to ascent and cyclogenesis needs to be seriously reconsidered in this case, an exercise that will likely lead to conclusions that are completely different from those arrived at by the current submission.

It is true that the values of QG omega shown here are very small compared to the full vertical motion. However, it was not intended and is also not "fair" to try to explain the full vertical motion with the QG omega shown here. Note that we show the QG omega, as forced by levels above 550 hPa (QG omega top) on 850 hPa. These values are expectedly much smaller than the full QG omega on 850 hPa or the QG omega top on higher levels (see also Fig. 1b in Davies (2015), which shows the effect of an isolated forcing region on the vertical velocity field in the surrounding atmosphere). We therefore do not intend to explain the full vertical motion with this variable, but the goal is to show the presence and location of the upper-level forcing.

However, we reconsider if it is important to show QG omega in both Figs. 2 and 7. If QG omega is still needed, we will make sure to put the values into context and make them better comparable to the values found by other studies.

4. The motivation for the case study approach adopted by the study is weakened by passages that highlight case-to-case variability, and is not supported by a clear statement of the useful aspect of the case study framework. The dominance of case-to-case variability is particularly emphasized on L38 and L84, with the latter appearing to be a direct criticism of the case study as a useful

analytic tool. It is good to identify the limitations of the adopted investigation technique, but this criticism should be balanced with a clear description of what the case study approach can provide that other types of analysis (e.g. climatology) cannot.

We agree and now better explain the motivation for the case study approach.

5. The analysis of vertical coupling in section 5 is not quantitative enough to be included in the study. Despite significant discussion of Fig. 7e-h (L241-253), the strongest conclusion that is draw is that it is “most likely” that baroclinic instability is active. Even this conclusion appears to over-reach the analysis given that no baroclinic growth rates were computed. Given that the Icelandic low is not the focus of this investigation and that the left-hand column of Fig. 7 shows a convincing evolution of short-wave anomaly growth, I think that the right-hand column of Fig. 7 and the associated discussions should be removed. If this analysis is to be retained, then there needs to be a real quantification of baroclinic coupling and associated growth rates [note that the 12-18h time scale is very rapid for pure baroclinic growth, which typically has a doubling time scale on the order of a day (Hakim, Encyclopedia of the Atmospheric Sciences) and suggests that moist processes are likely to be very important].

We agree that in the original submission it was not clearly shown that baroclinic instability is active.

We change Fig. 7(e-h) to show (based on the operational analysis) the synoptic setting in which the error growth is occurring, without claiming that baroclinic instability is active. Additionally, we add a Figure and extend the discussion about upper-level dynamics related to the jet streak, which more convincingly explains the error amplification.

6. The study of PV error growth by Baumgart et al. (2018) is referenced in the introduction, but not in section 5, where the left-hand column of Fig. 7 bears a striking resemblance to Fig. 3 of that work (albeit with a compressed time frame). The discussion of the importance of non-linear upper-level Rossby wave dynamics here follows closely that of Baumgart et al. (2018), so much of this description could be replaced by citations and comparisons. The Torn (2015) normalized difference is a useful measure, so compressing section 5 to focus on that metric in the context of the Baumgart et al. (2018) interpretation of this process would allow for a dramatic shortening of this section and serve to place this submission in the context of investigations by other groups.

We agree that Baumgart et al. (2018) could be referenced and some sentences of this section could be shortened. However, we still think that Fig. 7 needs to be discussed well, also because (a) we show a different measure than Baumgart et al. (2018), and (b) as you point out, the time frame is very different (we show lead times 6-42 h in 12-hourly time steps, whereas Baumgart et al. (2018) showed lead times 2-8 days every 2 days). We add a reference to Baumgart et al. (2018) in this section and shorten the text where possible.

7. Assessing the significance of the differences discussed in section 5 is important; however, the technique and in-text descriptions should be revised. Wilks (2016) provides a description of problems with the multiple-testing technique (as adopted in this study), which can lead to over-confident statements about significance. Please consider using the false discovery rate here. Additionally, the level at which the differences are considered significant is not identified in the text, and only appears in the Fig. 7 caption (is 0.05 used throughout?). Note that there is currently a reconsideration of the use of the term "significant", which appears to be leaning in favour of providing p-values rather than definitive statements about significance. I'm not very familiar with that discussion, but it might be of interest to consider during revision.

Thank you very much for pointing out this problem and mentioning the Wilks (2016) paper. We agree that, as we are using multiple-testing, a p-value correction is required to control the false discovery rate. We will correct the p-values using the Benjamini-Hochberg correction that is suggested by Wilks (2016) and make sure it is clear which p-values are used in the Figures.

8. I am surprised not to see any references to Wiegand and Knippertz (2014), who study the representation and predictive skill of anticyclonic RWB and PV streamer formation over the Mediterranean region in the ECMWF ensemble (i.e. an earlier version of the same system used here). That work seems so directly relevant to this study (including the conceptual diagram in Fig. 10 of that paper) that it should be leveraged heavily in this investigation, particularly in terms of putting the forecast uncertainty in this case in a broader context.

We apologize for not referencing this important study in the original version. This paper is now included and helps putting our case study in a broader context.

9. The numbering of clusters forces readers to remember the mapping: 1 is centered, 2 is west and 3 is east. Why not call the clusters C, W and E? Then the Fig. 8 rows could be reordered to W, C, E so that there's a progression in the columns rather than having the PV streamer location (and eventual cyclone location) jumping around.

Thanks, very good suggestion, which we adopted in the revised version.

10. Throughout the study, the "surface cyclone" is discussed by the 850 hPa heights are shown. Showing 850 hPa winds is useful, but I don't see anywhere in the manuscript that the 850 hPa heights are essential to the analysis. I think that all plots that currently show 850 hPa heights should be replaced with mean sea level pressure for consistency with the text.

We agree that 850 hPa geopotential heights are not ideal and SLP would be more appropriate. The plots will be changed accordingly.

11. Throughout the study, short-range ECMWF forecasts are used to estimate precipitation accumulations. To avoid model biases and potential "twinning", it would be preferable to use an

independent product. The GPM IMERG is readily available and would be a better choice for this study than stitched-together IFS forecasts.

No precipitation product is perfect for our analysis. The short-range IFS forecasts have the advantage that they are from the same model as the other data used. GPM data might also not be free of biases. Since the exact precipitation values are not essential for our study, we continue using the short-range IFS forecasts.

12. Advection of cold air over warm Mediterranean waters is identified as a factor that increases latent heat fluxes and promotes convection; however, this effect is not quantified in the current investigation. The OAFlux dataset covers the period of interest and is readily available for this kind of study. Please consider supporting the claims made in the manuscript with an analysis of OAFlux (or equivalent) surface flux estimates. An augmented surface flux analysis may particularly interesting if model-predicted fluxes are found to be very different between groups 1/2 and group 3 (see item 2 above). Such an analysis is essential if the categorical statements about surface fluxes currently found in the conclusions (L429) are to be retained.

We agree that the argument, that latent heat fluxes are active was not well supported in the original submission. Because of the general restructuring and in favour of a clearer focus of the paper, we remove the trajectory analysis in the ensemble members and instead provide a trajectory analysis including surface fluxes based on the operational analysis in the supplementary material, which is briefly discussed in the synoptic overview.

13. The manuscript really needs to be clear about whether the medicane itself is a focus of the study. In multiple passages, it is stated explicitly that the medicane is not going to be investigated as part of this work (e.g. L97, L161, L320). However, much of section 6 is dedicated to the evolution of the medicane, including trajectory and CPS analyses. The title of the manuscript also emphasizes the storm morphology and will attract readers interested in medicanes. It feels as though the work was initially focused entirely on the PV streamer, and that "mission creep" has led to the introduction of more storm-scale-relevant material. Please reconsider the statements that disavow the relevance of the medicane structure for this work in an effort to remove what seems like a fairly important internal inconsistency in the manuscript.

We apologize for the confusion about the focus of the study, which created the impression of internal inconsistency. The original idea was to submit a two-part paper, where the medicane would be the focus of the 2nd part. Obviously, when we decided to first focus on this paper, we didn't manage to clearly explain the role of the medicane aspect.

We will clarify this aspect better. In particular, we clearly define what we consider as a medicane in this study (and distinguish between medicanes that undergo tropical transition and the ones that don't). We make clear that in this study, we focus on the large-scale conditions that influence the predictability of a medicane. And we also make clear that we do not focus on the meso-scale dynamics that – once a medicane has formed – can lead to tropical transition.

14. Why are the ECMWF data coarsened to 1°, and how is it done? The result is very poor resolution in the graphics, and if it not done carefully, the operation could result in aliasing. Is a conservative remapping used? This is a particularly important question for the precipitation field, where the difference between sampling/interpolation and remapping/aggregation can be enormous when the degradation of resolution is so large.

We downloaded ECMWF analysis data on a 1° grid to be consistent with the resolution of the ensemble data. Such a coarsening of the ensemble data is required to cope with the huge amount of data. Note that, for all ensemble members, we download the 3D fields on all model levels, which is required to accurately calculate PV and trajectories. This data transfer needs to be done within a few hours after completion of the ensemble simulation, because eventually, fields are archived in MARS on a few pressure levels only. All grid interpolations were done with routines available in MARS.

15. Most published works do not consider "medicane" a proper noun (and it is therefore not capitalized). This is analogous to "hurricane", which is only capitalized when a specific storm is discussed (e.g. "Some think that Hurricane Katrina was a category 3 hurricane at landfall"). Consider using lower case "medicane" throughout except in named reference to Medicane Zorbas.

Thank you for the explanation! We changed the use of "medicane" accordingly.

16. The terms "air mass", "airstream" and "parcel" seem to be confused in relation to trajectory analyses (L139 and section 6.2). An "airstream" is a loosely defined concept, but I think that it would be represented by a high density of air parcel trajectories in a limited area. Then the phrase "trajectories of the airstreams" (Fig. 10 caption) doesn't make sense unless the airstream (a feature in storm-relative coordinates) is somehow tracked over time. Similarly, trajectories do not track "air masses" (L139), but parcels. The difference is important, because it is unlikely that all parcels in an "air mass" are ascending near the cyclone centre.

17. The trajectory analysis in section 6.2 is incomplete. The suggestion that moistening is occurring because of surface latent heat fluxes (L345-346) implies that the parcels are in contact with the surface; however, the vertical position of the parcels is never shown. It is also possible for parcels to be moistened by evaporation of falling precipitation or by turbulent mixing. It is therefore not demonstrated that enhanced surface fluxes are responsible for the moisture changes in groups 1 and 2. The same is true for the potential temperature analysis on L346-348: surface fluxes are only one possible reason for potential temperature increases, and only influence parcels if they are in contact with the surface (even at above-surface levels in the boundary layer, the moistening/heating mechanism would be turbulent flux convergence rather than surface

fluxes per se). The lack of information about the trajectories makes it impossible for reviewers or future readers to confirm the validity of the conclusions drawn at the end of this section (L356-370).

18. Section 6.2 ends with a set of suppositions and conjectures based on an incomplete trajectory analysis (see previous item) climatological behaviour. As a result, terms such as "could favour" and "might support" are used instead of definitive statements. If the analysis and descriptions in this section cannot be made robust enough to be able to conclude these statements definitively, then this section should be removed.

Reply to comments 16-18: Thanks for pointing out these weaknesses of the analysis. As mentioned above, we remove this trajectory analysis in favour of focusing on the most important aspects of our study (see reply to comment 12).

19. The description of the CPS (L386-392) is insufficiently detailed to allow independent confirmation of the results (a requirement for publication). Because of the small scales of medicane structures, the hurricane-based radii are usually reduced for studies of Mediterranean storms. Was the same done here, or were the original hurricane-based values used?

Consistently with previous studies (e.g. Gaertner et al., 2018), we have used a radius of 150 km. This is now mentioned in the revised version.

20. I don't understand the "deep warm core" (DWC) analysis in Fig. 12. Take groups 2 and 3, for example. They have 12 and 18 members, respectively. The average number of DWC in group 2 is 7.2, and 7.0 in group 3 according to Fig. 12. That number is "per ensemble member", so multiplying by the relevant ensemble size yields 7.2*12=86.4 for group 2 and 126 for group 3. However, the total number of DWC steps for group 2 is given as 43, and that for group 3 is given at just 14 at the bottom of the plot. In the text (L404) the reader is told to consider the group-3 DWC analysis "with caution, due to the small sample size". However, the average number of DWC steps per ensemble member is as large in group 3 as it is in group 2: why is the sample size so small? There seems to be something about the number of sequential DWC steps ("duration"), but that is never clearly stated in the text or caption. What is wrong with my interpretation of the DWC analysis?

Thank you for pointing out that this analysis has not been straightforward to follow. The missing piece is, that the analysis only includes ensemble members that actually have a deep warm core cyclone. So, the number of DWC steps has to be multiplied by the number of members in the considered cluster that have a DWC cyclone. This number can be read from Table 1. Hence, the sample size for group 2 is small because only 2 members form a deep warm core cyclone.

However, we most likely replace the Figure with another one that is clearer and additionally provides evidence for conclusions that are so far not very well supported (see your comment 23).

21. Throughout the text, equivalent potential temperature gradients are used to identify both baroclinic zones and moisture gradients [L125, L132, L137 (where the 850 hPa theta-e is inappropriately used to identify a "weak surface cold front") L153 and elsewhere]. Strictly, neither of these is guaranteed by a theta-e gradient, which may arise as a result of either in isolation. If baroclinicity is important, then potential temperature (or temperature on an isobaric surface) should be shown. If moisture is important, then it should be shown. Theta-e is a very useful quantity for assessing convective potential and is a useful way to identify the warm sector for the trajectory analysis, but it does not replace the more basic fields for questions of baroclinicity and moisture.

We agree that it is more appropriate to look at potential temperature and humidity when the focus is on baroclinicity or humidity. We reconsider the importance of discussing baroclinicity and moisture gradients for the storyline of the manuscript. Where needed, we show potential temperature or specific humidity, respectively.

22. There are a lot of very specific geographical references throughout the text, probably more than there need to be. I'm a geographer, but I still found myself having to look for specific place-names on maps. It would be very useful to have a new Fig. 1 that shows (at least) the storm track and labels for all place names referred to in the text.

We agree with this point and include a new Fig. 1 that shows the storm track and labels for relevant places.

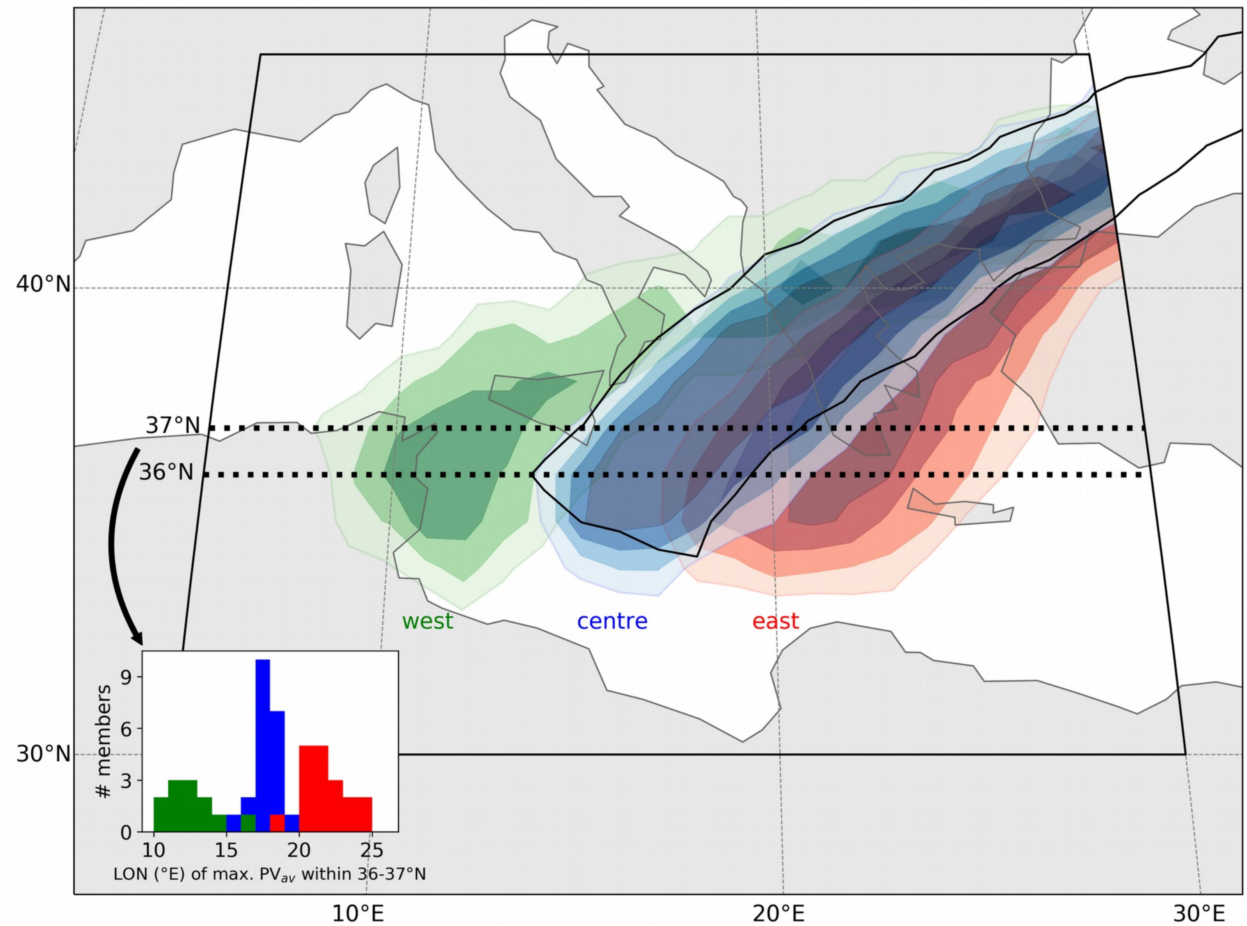
23. The conclusions of the study are not supported by the evidence provided in the text:

a. The "clustering" technique is not rooted in a mathematic definition and fails to guarantee the separation of the members into distinct "scenarios" as stated in the text (e.g. at L481). Is it true that there are three "distinct scenarios"? I agree that there are two (see General Comment 2), but I don't see why there are three. Groups 1 and 2 are distinguished only by the fractional overlap of the PV streamer, and there was no demonstration that there is any sort of heterogeneity in overlap space. This is a weakness in the analysis that results from the failure to use a true clustering analysis, and the decision to rely on a classification heuristic. There is no guarantee that group 1 and 2 events are separate from each other in any kind of meaningful way, and selection of a different overlap threshold (70%, for example) would result in the progressive reclassification of members from one group to the next. To demonstrate the presence of different scenarios, a true cluster analysis should be performed, and the optimal number of groups should be identified (e.g. using the "elbow method).

We agree that generally, and for climatological or operational purposes, an objective mathematical clustering is clearly preferable. However, the beauty of this specific case is that it is very clear to the reader that the uncertainty of the PV streamer prediction is large in terms of

the meridional position of its tip and its shape (both are somewhat connected) and that it is meaningful to separate the ensemble into a group where the meridional position of the PV streamer is more or less correct, another group that has the streamer too far to the east, and one that has it too far to the west. If you agree that groups 1 and 3 are different, then you could also agree that group 2 is different, because group 3 and group 2 are distinguished from group 1 in the exact same way (with group 3 having the PV centre of mass to the east and group 2 to the west of the analysis). We are very thankful that you pointed out the fact that cyclogenesis in group 3 shows a non-linear response to the PV streamer position (General comment 2), and that it shows also a clearly distinct surface cyclone scenario in response to the distinct PV streamer scenario. However, from the fact that the surface cyclone scenarios of group 1 and 2 are not as distinct as of 1 and 3 (albeit clearly shifted) we cannot conclude that there are no distinct PV streamer scenarios. We therefore argue that, in the present case, this kind of ad-hoc clustering is useful and suits the purpose of this case study. It is not at all guaranteed that an objective clustering provides more appropriate scenarios for this kind of investigation. For example, if we assumed that the overlap space is fully homogeneous and the PV streamers are shifted by constant distances from west to east, an objective clustering would not necessarily provide us with useful information how many clusters to choose. However, if we are interested in what the shift of the PV streamer does to the predictability of the surface cyclone, it would still be meaningful to split the ensemble into equal bins with one containing the westernmost PV streamers, one the more central PV streamers, and the third the easternmost PV streamer.
In the Zorbas case, we argue that the heterogeneity in the PV streamer distribution is captured very well by the ad-hoc clustering and an objective clustering technique is not required for this study.

We now add an inlet to Figure 4 (see below) of the initial submission which shows a histogram of longitude of the maximum PV value within the latitude band 36°-37°N. The three maxima in this histogram, representing each one of the clusters, hopefully convince you that – from the PV streamer perspective – there are three clearly distinct scenarios. Note that a clustering according to this measure would put a few boarderline members into another bin (see green and red "outliers"). However, we stick to the original clustering as this accounts for the uncertain positon/shape of the PV streamer on a larger domain.



b. There was no analysis of the near-surface flow induced by the PV streamer, so how is the conclusion about induced advection (L432-434) supported by the evidence provided in the submission? Particularly given the limited spatial extent of the streamer immediately prior to cyclogenesis, it is possible that the induced near-surface flow is very strong. For the arguments regarding air parcel modification by surface fluxes, the parcels approaching the centre in groups 1 and 2 must be in contact with the surface, putting them as far as possible from the upper-tropospheric streamer.

We agree that for this statement to be fully supported, a PV inversion would be necessary. From our general understanding however it is very sensible to assume that the PV streamer, as a strong upper-level PV anomaly, affects the lower-level circulation. Note that this conclusion is not about the air parcels modified by surface fluxes, but the ones that end up in the warm sector (which are never argued to experience surface fluxes, on the contrary they move almost adiabatically and without increase in specific humidity). However, as mentioned above, in response to above comments and for the sake of enhancing the focus of our study, we remove the trajectory analysis (see reply to your comment 12).

c. I cannot see what part of the analysis is used to conclude that the group-1 PV streamer was better able to "maintain the cyclonic circulation" (unclear whether this refers to the upper- or lower-level flow) than the group-2 or group-3 features (L434-438). There appears to have been a rigorous analysis behind this statement (something that determines the number of members that meet a "condition"), but I don't know what section this analysis was described in.

We agree that composites may not be fully ideal/fair to draw this conclusion because spatial shifts could also result in low PV values in the composite. We therefore provide an additional analysis that better supports this statement and make sure the formulation is clearer. We also rephrase the conclusion.

d. The increase in the amplitude of the cyclonic PV anomaly from about -0.5 PVU to beyond -2.5 PVU (combined with a rapid areal expansion) over the 24-h period ending at 1800 UTC on 25 September (Fig. 7b and d) is "rapid" as stated on L442. However, as noted in item 4 above, this growth rate appears to exceed that expected for typical midlatitude baroclinic growth. It is highly likely that moist processes are involved, but because no estimates of growth rates are made in this study, it is impossible to know. It is therefore also inappropriate to conclude that the observed growth is "as expected from baroclinic instability" (L442) because the expected value remains unknown in the context of this work.

We agree that this conclusion has been a bit shaky. Note that the argument about baroclinic growth was (at least this was our intention) mainly made for the 6/12-h period from 12-18 UTC 24. Sept. After this time, we argued that non-linear barotropic dynamics is mostly responsible for the growth and downstream development. Still we agree that this is very rapid for (dry) baroclinic growth.

The reviews sparked further analyses of this aspect. As stated in the reply to General Comment 5 we will revise the analysis in Figure 7 and make sure the conclusions drawn from it are well supported.

e. It is unclear to me what part of the analysis demonstrates that "the contributions of diabatic airstreams ... were negligible for the uncertainty amplification in this case" (L444-445). The non-conservative evolution of the PV streamer was remarkable in this case (Fig. 2), and the impact of diabatic PV reduction in WCB outflow on ridge amplification during the upstream RWB (Fig. 7a-d) was not analyzed in the study, as far as I can tell. This statement about the role of diabatic process on forecast uncertainty (L444-445) is very strong, inconsistent with previous work, and needs to be clearly supported by the presented analysis.

We meant that, for the uncertainty amplification shown in Figure 7 (which is before the PV streamer forms), WCB outflow was not present in the vicinity of the region with strong error growth. This was based on a careful analysis of WCB trajectories, but not shown, because of the few WCB air parcels identified in the domain. Of course, you are right that the PV streamer evolution was non-conservative, but with our statement about the contribution of diabatic effects we did not aim to characterize the later times but the timesteps shown in Figure 7. Also, we argue that it is not at all inconsistent with previous work, that direct modification by diabatic processes and diabatic airstreams are not very relevant for the amplification of forecast uncertainty in individual cases (see e.g. Baumgart et al. 2018). This of course, does not mean at all that we consider diabatic airstreams as generally not important for the amplification of forecast uncertainty. The attribution of the amplification of forecast uncertainties in Rossby waves to individual dynamical processes is a rather new research subject, but it seems that pure non-linear barotropic Rossby wave dynamics is very important (Davies and Didone 2013; Baumgart et al. 2018). Of course, diabatic airstreams can influence these dynamics, for example by placing a negative PV anomaly close to the wave guide. The absence of warm conveyor belts in the phase of the rapid error growth shown in Fig. 7 is a very strong indication that diabatic airstreams are not relevant for the uncertainty amplification in this case. However, this has not been discussed in the initial submission.

We now include the intersection points of WCB air parcels with 325 K in a revised version of Fig. 7 and/or in the synoptic overview. With this, we show that, even if the WCB was present at the later stage of the wave breaking, the few intersection points present in the early stage are far away from the region of the amplification and highlight this aspect when we discuss Figure 7.

Minor Comments

There are a relatively large number of grammatical errors in the submission, which I have not itemized here because of the major reworking of the text that will be required to address the issues identified above.

1. [L50] It is not clear why the introductory reference to parameterization uncertainty is useful here, where initial condition uncertainty is described in the subsequent passage. I would suggest starting this paragraph with “A major source ...”.

2. [L51] I don’t think that “slight uncertainties in initial conditions typically grow” (my emphasis), because the majority of uncertainties in any given analysis project onto decaying modes in the atmosphere (Privé and Errico 2013). I think that it would be more precise to say something like, “Slight uncertainties in the initial conditions that project onto the growing modes of the atmosphere can increase in amplitude during the forecast and potentially ...”. You could also just replace “typically” with “can” in the current phrase.

3. [L87-L94] Suggest dropping this subsection in favour of the analysis in section 3.

4. [L95-L97] Having a clear set of objectives is a good idea, but these questions are framed in a way that is too complex to make them useful for the reader (e.g. “what is a and what of b leads to c and d in e”). Suggest simplifying or removing these questions.

5. [L99-L105] Provide a standard outline with section references.

Reply to comments 1-5: We adopt the suggestions in 2 and 5, and consider 1, 3 and 4 when rewriting the introduction (see your General Comment 1.a.i)

6. [L108] How are the ensemble members "perturbed": initial conditions, stochastic physics, SPPT, etc?

It is the standard ECMWF operational ensemble forecast. Because the details of how the ensemble is created are not relevant for the conclusions in this analysis, we will not go into detail here, but add a short statement clarifying this point.

7. [L111 and elsewhere] The word "data" is plural, so "data are available", etc.

Thanks, we change it accordingly.

8. [L115] What climatology is used for the ACC calculation?

It is daily mean Z500 values from ERA-Interim from 1979-2014. We add this information in the corresponding section.

9. [L122] Reference to a URL is inappropriate. At the very least, an access date needs to be provided. Consider including lightning strike information on the plots, rather than making reference to external information that may not be permanently available.

We add a map of lightning strikes to the supplementary material.

10. [L152] How is conditional instability identified in this analysis?

It is not directly identified. We remove "conditional".

11. [L159] Figure 4a does not exist and Fig. 4 is not the CPS.

Apologies for this typo, it should have been Figure 5a. We corrected it.

12. [L207] Reference to Fig. 7 is out of order.

Thank you, we corrected it.

13. [L221] At what level are the differences significant?

Always 0.05 is used in the initial submission. We make sure that it is always clear which threshold for the p-value is used in each figure.

14. [L231-L232] This sentence doesn't make sense: does the amplitude "propagate" at a different speed from the difference? Are you differentiating between phase speeds and group velocities here? Please rephrase to make this clearer.

15. [L263] The section title should be much clearer, and not read like a news headline.

16. [L274] Three different time references begin this sentence. Please determine whether it is the time relative to streamer extension, Gregorian date/time, or forecast time that is most relevant here and stick to this description of the first column of Fig. 8.

Reply to comments 14-16: Thank you for pointing out these unclarities. We consider them in the revisions.

17. [L278] I don't see that cluster 3 trough is "clearly" shifted to the east of the analysis at 1200 UTC 25 September (Fig. 8i). Instead, I see a trough that is too narrow, notably on the upshear flank over Germany.

Thanks for pointing this out. We agree and change the text accordingly.

18. [L281] Why isn't significance plotted here as in the first column?

For visibility reasons. In the later plots, large areas would be covered by the significance shading. The significance for all timesteps is shown in the supplementary material. We make the reference to the supplementary material clearer in the figure caption.

19. [L304-L305] This looks like more than just smoothing of the ensemble mean. Because averaging is a linear operation, the area-averaged ensemble-mean precipitation should match the observed values if the ensemble does not under-predict rainfall.

We do not agree, that area-averaged ensemble-mean precipitation should match the observed values. As long as the "observation" lies within the range of the ensemble, we don't think that there is an under-prediction. The area-averaged precipitation can be highly different between the ensemble members and as long as there are (even only very few) ensemble members that have equal or higher amounts of area-averaged precipitation than the "observed", the ensemble is fine (unless this is the case for most ensemble forecasts over many cases). We computed area-averaged accumulated precipitation for each member and the analysis over the study region and it shows that the value for the analysis is around the 90th percentile of the ensemble.
We now add additional panels to Figure 9 that show the members with the highest and the lowest accumulated precipitation in each cluster in a box over the study region to illustrate the variability among members.

20. [L297] Are these SLP changes computed from the central pressures of the ensemble members, of from the ensemble mean? The search for a minimum central pressure is not a linear operation, so the results will likely be sensitive to the method. Particularly given the broad spatial distribution of group-2 centres, some/much/all of this apparent weakening may simply be the dilution of the ensemble mean if the ensemble averaging is done first.

Thanks for pointing this out. We computed it from the cluster mean. However, in the revised version we will compute the changes from the individual members and correct the numbers.

21. [L312-315] The four lines of hypothesis here would be much better invested in the actual analysis rather than this forward-referenced supposition (I recommend the removal of this whole paragraph as noted in item 1.b.iii above).

22. [L317 and L320-L321] There seems to be an internal inconsistency here. On L317 the objective of the section is stated to be "to investigates ... subsequent development of a medicane-like system". However, on L320-321 you state that you "do not identify low-level warm cores directly and do not investigate their formation in detail". Because the warm core is one of the primary structural ingredients that distinguishes medicanes from typical Mediterranean cyclones (considering the CPS), these two statements seem to be in direct conflict.

23. [L321] What do you mean that you don't identify warm cores directly? The CPS-based warm-core detection is the basis for a large part of section 6.3.

24. [L344] Do you mean a larger increase in specific humidity in groups 1 and 2, than in group 3? This sentence suggests the opposite, likely because the intended target of the pronoun "this" is unclear (although the construction suggests group 3).

25. [L353-L355] What is the physical relevance of this comparison?

26. [L393-L399] Why bother with a set of conjectures right before performing the actual analysis? A far more direct approach would be to explain why the fractions of medicanes in each group differ, based on the analysis presented in earlier sections. The conjectures do nothing to build suspense for the big "reveal" of Table 1, and just serve to consume five lines of text unnecessarily.

27. [L399-L401] This text contains every number shown in Table 1, without offering any physical insight. Choose to present these numbers either within the text, or in a table, but not both.

Reply to comments 21-27: Thanks for these helpful comments that highlight parts of the analysis and the text that require revisions. This part of the paper will be completely revised in the new version, considering these comments.

28. [L413-415] How is it concluded that “the detailed interaction between the surface cyclone and the upper levels become limiting factors” for predictability? Why can’t internal storm processes or air-sea exchanges be the limiting factors? Those processes have not been investigated or ruled out as limits on storm structure predictability in this analysis, as far as I can tell.

We conclude that “sub synoptic-scale processes including the detailed interaction between surface cyclone and upper-levels become limiting factors”. This does not exclude other sub-synoptic scale processes but maybe puts too much emphasis on the interaction of the surface cyclone and upper levels. We improve this sentence and also mention that internal storm dynamics/convection can be relevant.

29. [Fig. 6] Can the map resolution be increased a bit? (Similar in Fig. 7 zooms.)

Yes, we will do this.

30. [Fig. 6] At what level are the contours significant?

Again, 0.05. We make sure this information is clear.

31. [Fig. 6] The means are too similar to be usefully distinguished on the plot. Consider plotting the full ensemble mean only rather than solid and dashed contours.

We agree and change the Figure accordingly.

32. [Fig. 7] Is that a reference vector between (d) and the colour bar? If so, it should be highlighted and described in the caption. If it isn’t, then one should be added.
Thanks, we adopt these suggestions.
33. [Fig. 10] Use a fixed domain to ease comparison between panels.

34. [Fig. 10] What does the colour-coding of the trajectories represent (the last sentence of the caption is not clear about what is indicated “in colors”)? Are different members assigned random colours? Why are there fewer cyclone positions in the groups than members within the groups? Are there multiple trajectories ending at the same point because of the degradation of the grid resolution? If so, there should be some way to represent the number of overlapping triangles (potentially the size of the triangle).

Each ensemble member has a different color.

35. [Fig. 10] How does the maximum "percentage of ensemble members with an airstream occurring at the specific grid point" occur outside of the trajectory envelope? For example, the maximum departure frequency in Fig. 10b occurs poleward of any trajectory. Is it because these trajectories are actually averages of many trajectory calculations? If so, then there must be some unusual spatial distributions to obtain density maxima away from the means. How many trajectories are computed in each member?

The "average trajectories" represent means of several trajectories (~12, depending on the location) for each member. Therefore, it is possible that there is a density maximum (of the all actual trajectories) away from the starting position of the "average trajectory". We realize that this analysis is a bit confusing.

36. [Fig. 11] Why are radii the best way to identify the blue and green lines? It would be clearer to label the blue line "center" and the green line "warm sector" because the radii are technical details rather than relevant features.

Reply to Comments 33-36: As mentioned above, we remove the trajectory analysis to achieve a better focus of the paper and instead provide a trajectory analysis based on the operational analysis in the supplementary material (which we discuss in the synoptic overview).

References

Privé, N. C. and Ronald M. Errico (2013) The role of model and initial condition error in numerical weather forecasting investigated with an observing system simulation experiment, Tellus A: Dynamic Meteorology and Oceanography, 65:1, DOI: 10.3402/tellusa.v65i0.21740

Stepanyuk, O., Jouni Räisänen, Victoria A. Sinclair & Heikki Järvinen (2017) Factors affecting atmospheric vertical motions as analyzed with a generalized omega equation and the OpenIFS model, Tellus A: Dynamic Meteorology and Oceanography, 69:1, 1271563, DOI: 10.1080/16000870.2016.1271563.

Wilks, D.S., 2016: "The Stippling Shows Statistically Significant Grid Points": How Research Results are Routinely Overstated and Overinterpreted, and What to Do about It. Bull. Amer. Meteor. Soc., 97, 2263–2273, https://doi.org/10.1175/BAMS-D-15-00267.1

Wiegand, L. and P. Knippertz, 2014: Equatorward breaking Rossby waves over the North Atlantic and Mediterranean region in the ECMWF operational Ensemble Prediction System. Quart. J. Roy. Meteor. Soc., 140, 58-71.

Reviewer 2 (Florian Pantillon)

The paper investigates the large-scale dynamics that led to the formation of a tropical- like cyclone over the eastern Mediterranean in late September 2018, which was characterized by high forecast uncertainty in the operational ECMWF ensemble prediction system. A potential vorticity streamer issued from an anticyclonic Rossby wave breaking over eastern Europe was key in the Medicane dynamics. Two clusters of ensemble members with zonal shift of the streamer can be tracked along the Rossby wave guide back to initial conditions over North America. The evolution of the streamer into an upper-level cut-off low then controls the surface cyclogenesis, the stability and the advection of warm moist air that all support the Medicane formation.

Hybrid cyclones in general and Medicanes in particular are current sources of vivid discussions in atmospheric dynamics and objects of broad interest in the Mediterranean community. Contributions to better understand their dynamics and predictability are thus welcome and the paper presents interesting new material based on sound methods and high-quality figures. However, it suffers two major shortcomings: possible contributions from small-scale dynamics are largely ignored, although they at least partly explain Medicane formation, and the manuscript needs reorganization, as already pointed out by Referee 1. These shortcomings are linked somehow, as the tropical transition of the cyclone is actually assessed at the very end of the paper only. They are described below, as well as (many) specific comments.

The paper thus requires substantial revision before it can be considered for publication in Weather and Climate Dynamics.

General comments

Scales: as stated in the introduction, “the relative role of positive upper-level PV anomalies and air-sea interaction for the intensification of Medicanes is currently debated, as well as the question to which degree they are dynamically similar to tropical cyclones”. The paper focuses on the synoptic scale and is based on model forecasts that do not explicitly resolve convection. This is fine but (1) the focus should be explicitly stated, (2) the limitation should be kept in mind throughout the paper and (3) the results should contribute to the current debate.

Organisation: as already pointed by Referee 1, the structure of the paper is unsatisfactory. Please better organize the Sections, make sure important concepts are introduced early in the paper (then stick to the terminology) and methods are described in the appropriate section, and avoid referring to later sections. In particular, show the warm core structure early in the paper, and comprehensively, based on the analysis for instance; in the present form the reader must wait until the last subsection of the last results section to learn the cyclone actually developed a warm-core structure.

Thanks for these comments. We obviously failed to clearly state the focus and intention of the study and the organisation of the Paper was not as appealing as we thought.

When revising the manuscript, we will make sure that the focus is clearly stated and kept in mind throughout the study. In particular we state the definition of a medicane as suggested by the current literature, even if it is debated. In particular, we mention that there are cyclones classified as medicanes that do not seem to have any tropical dynamics while others undergo proper tropical transition. We then define how we identify medicanes in this study (which is by the presence of a deep warm core). This allows for a clearer terminology, which we make sure is consistently used throughout the text. We will state clearly that the focus of the study is to investigate the predictability of the medicane in the early stage (the formation of the deep warm core) and not the later phase, when Zorbas acquires more tropical-like appearance. Specifically, we emphasize that we link the differing position of the PV streamer in the clusters with differing probability for the formation of a deep warm core

We now show the CPS of Zorbas based on the operational analysis early in the paper and make clear which part of the life cycle we are looking at and why.

We also provide a standard data and methods section, and extend the synoptic overview section.

Specific comments

Title: the position and depth of the PV streamer exhibit some uncertainty in the ensemble forecast but the streamer itself is not uncertain; the link between PV streamer and Medicane could be more explicit.

Thanks for pointing this out. We change the title of the paper, most likely as follows:

Medicane Zorbas: Origin of an uncertain potential vorticity streamer position and impact on cyclone formation.

Abstract
l. 3-4 This statement is not clearly supported. l. 5 “uncertain” is not properly used here (see comment on title).

We change the wording and make sure it is clear that mainly the PV streamer position/shape was uncertain.

l. 7-8 “demonstrated”, “the dominant source”: not necessarily. See comments below.

Thanks for this comment. See replies to your comment below.

l. 9 Twice “strong(ly)”.

l. 12 More details about the two air streams and their key role?

Reply to above two comments: Thanks, we rephrase and reconsider the content abstract after the revisions.

1 Introduction

l. 19-25 All references relate to the North Atlantic, which should either be explicitly stated or extended to other oceanic basins.

We add two more references of studies in other ocean basins.

l. 19-20 ET could also be mentioned here.

We agree that ET is an important process, but not really essential for this study. Therefore, we decided to not mention ET.

l. 26-33 It is unclear what is the difference between subtropical, tropical-like and hybrid cyclones, if there is one at all. And do not they by definition undergo tropical transition?

We agree that the wording is unclear here. We now try to make a better distinction between the different terms in the introduction.

l. 29 “air-sea feedback” is not precise enough.

We make this more precise by mentioning the WISHE mechanism that becomes active, when tropical transition occurs.

l. 32 This “may” result in high damage, as Medicanes often remain over sea.

l.35-39 Confusing what “they” and “their” refer to.

l. 41-42 Forecast uncertainty and the link with process understanding and ensemble forecasting needs better introduction.

l. 42-49 This appears too early and several keywords are not introduced yet (Zorbas, warm core, practical predictability, . . .).

l. 50 The transition should be smoother between 1.1 and 1.2.

l. 63 Lamberson et al. studied “extratropical” cyclone Joachim.

l. 64-65 The link between the predictability of breaking Rossby waves and Medicanes is far from obvious and need more details; it was extensively explored for a case study in September 2006: Chaboureau, J. , Pantillon, F. , Lambert, D. , Richard, E. and Claud, C. (2012), Tropical transition of a Mediterranean storm by jet crossing. Q.J.R. Meteorol. Soc., 138: 596-611. doi:10.1002/qj.960
Pantillon, F., Chaboureau, J., Lac, C. and Mascart, P. (2013), On the role of a Rossby wave train during the extratropical transition of hurricane Helene (2006). Q.J.R. Meteorol. Soc., 139: 370-386. doi:10.1002/qj.1974
Pantillon, F. P., J. Chaboureau, P. J. Mascart, and C. Lac, 2013: Predictability of a Mediterranean Tropical-Like Storm Downstream of the Extratropical Transition of Hurricane Helene (2006). Mon. Wea. Rev., 141, 1943–1962, https://doi.org/10.1175/MWR-D-12-00164.1

l. 67 Who are "they"?

l. 67-69 The mentioned studies do not clearly attribute forecast busts to initial uncertainties rather than to the representation of diabatic processes. Both error sources would thus better be described together here.

l. 79 Which process?

l. 86 Even if only few case studies exist, there are certainly more than the two cited here.

l. 89 Some basic information about the case study are needed here (what, where, when). And where does the name "Zorbas" come from?

l. 91 Please explicit "short lead times".

l. 94 Which PV streamer? Either detail or remain general; referring to Section 3 does not help. l. 96 "leads" or "led" l. 100-106 Detailing Sections 2, 3, 4, . . . might be required.

Thanks for all the above comments that point out unclarities, inconsistency, and missing depth in the introduction. We completely revise the introduction and consider these valuable points.

2 Operational ECMWF products
l. 110 Why 46 members only? What is the operational short-term forecast?

For technical reasons, four ensemble members were missing in the data we downloaded operationally. And as this forecast is not in the core of the study, we thought that 46 members were sufficient. The operational short-term forecast are based on the first 12 hours of forecasts started at 00 UTC and 12 UTC each day and are used to get an estimate of actual precipitation. We now include all ensemble members of this forecast and explain better what we mean with operational short-term forecast.

l. 112 Forecast data is actually available at higher frequency.
This is true, but not with the full vertical resolution which is needed to compute potential vorticity appropriately. Note that we download ensemble data on full model levels right after forecast completion because much fewer (pressure) levels are in MARS.

l. 115 What is the reference for computing ACC?

It is daily mean Z500 values from 1979-2014. We add this information in the corresponding section.

3 Synoptic overview
Figures 1-3 Zooming in on the region of interest would be very helpful to follow the discussion. Large-scale dynamics play an important role but, e.g., the Irish and Red Seas are not relevant. Consider then merging the three figures to avoid jumping from one to the other.

l. 135-136 The spiral-like structure is hardly seen. Or do you mean the frontal band? Spiral often refers to a tropical structure. Again, zooming in would help.

l. 139 Fig. 2C

l. 140-141 Move to the methods section.

Thanks, we put methodological aspects into the methods section.

l. 144, 146 What are "they"?

l. 139-149 Is it convection and/or large-scale ascent? The 600 hPa in 24 h criteria suggests the latter, while lightning suggests the former. The ECMWF model cannot actually resolve convection but you could check whether the precipitation is issued from the convection scheme or not.
We agree that the 600 hPa in 24h ascending air parcels are not ideal to show here.

We quantify the contribution of convective precipitation and mention it in the text. Additionally, we show lightnings in the supplementary material

l. 135-149 There is a general confusion in the paragraph between what was stated by previous studies and what happens here.

l. 152 Fig. 3d

l. 152 Clarify the precipitation is from model data; using different colors would help distinguishing the −11, 8, 15 and 21 mm (6h) contours on Fig. 1.

l. 153 How can you know it is due to conditional instability?
We do not check directly what type of instability occurs.

We remove the "conditional".

l. 156-157 How do you discern a warm seclusion from a warm core?

l. 157-160 and 166-167 There is not enough evidence at that point to claim a tropical transition. Relying here on Sec. 6.3 is not a good idea and there is no Fig. 4a. Either show more details or keep for later.

Thanks, we agree.

As commented above, we now show the CPS early in the paper and make sure we have a consistent wording.

l. 162 This would be worth showing!

This paper doesn't aim to deal with the tropical transition of Zorbas and discussing the eyewall formation would be clearly beyond the scope of this study. For the sake of focus, we don't show this aspect of Zorbas and leave it for further studies.

Reply to all above comment for section 3 with no direct replies: Thanks, these comments are all valuable to improve the text and/or figures. We consider them when revising the paper.

4 Ensemble clustering according to position of PV streamer
l. 170-172 The sentence presents essential information but needs more support: why 00 UTC 27 Sep? Why 00 UTC 24 Sep? Is it perhaps the combination of valid time and initialization time resulting in largest spread? Can we see this somewhere? "Shown in Fig. 2 six hours earlier" is not too convincing.

These times were chosen because the PV steamer position showed this "nice" tri-modal behaviour. We add a sentence that states the motivation for these initial and valid times.

l. 172-174 Referring to a later Section to motivate the present one is surprising.

l. 174-176 Please move technical details to the Methods section.

l. 178 Average of 320 K and 330 K levels or are there additional levels in between?

It’s every 5 K, so we average 320,325,330 K.

We make sure this is clear in the revised text.

l. 178 Why set PV < 2 pvu to zero?

This was done to “mask” out the troposphere and just use stratospheric PV air when averaging PV vertically. This gives a field that is sensitive to the depth and PV values within the PV streamer. For example, we get the same values if the PV streamer is present just at one level with a PV value of 6 PVU or at three levels with each PV values of 2 PVU. Another way to look at it is that we want to cluster the ensemble members according to the PV streamer, and therefore reduce the contribution of tropospheric PV values to the averaged field.

l. 180 Remind there are 50 members?

l. 190-191 Again, referring to a later Section is surprising.

l. 192-193 “Decrease” rather than “drop”? Are these values of ACC particularly low? And why not color clusters 2 and 3 in green and red on Fig. 5 as on Fig. 4?

Thanks for this suggestion. ACC values are not particularly low. We did not color clusters 2 and 3 because the focus here is to show the better performance of cluster 1. If clusters 2 and 3 are added the plot is less easy to read. We adopt the suggestion to use “decrease” but decided to not colour clusters 2 and 3.

l. 198 Errors in the shape of the PV streamer have not been discussed yet, only the zonal shift.

Thanks for this comment. The shape is somewhat included in the sense that if the shape of the PV streamer is completely wrong, the overlap would be too low to satisfy the criterion for cluster 1. The overlap can be low because of a shift or because of a different shape, or of course, a combination. The member that has been excluded from the analysis because it did not fit into a cluster actually shows no zonal shift at the tip of the streamer but a very special shape, such that the overlap is not large enough.

l. 199 Which characteristics are relevant?

l. 200-204 ACC may not account for the cyclone at all, at least the link is not showed yet. The link with the PV streamer is not obvious either. Consider adding Z500 on one of Figs 1-3.

Thanks for this comment. We agree that Z500 may not account for the cyclone very much. But it is reasonable to assume that Z500 is linked to upper-level PV, which is what is needed for the argumentation in this section. We now add Z500 in the synoptic overview.

Reply to all above comment for section 4 with no direct replies: Thanks, these comments all contain valuable suggestions to improve the text and/or figures. We consider them when revising the paper.

5 PV streamer scenarios emerge from initial condition uncertainties and baroclinic amplification
l. 205-206 The jet streak has not been mentioned before. Consider either adding the large-scale dynamics leading to the PV streamer in Section 3 or, at least, shortly describing these dynamics here and motivating why they are the focus of the following analysis.

This is a good idea. We now include a Figure that provides an overview of the large-scale dynamics leading to the PV streamer.

l. 209-220 Please move to the Methods. Can you say some words about delta PV values, e.g., is there a threshold that indicates bi-modal distribution?

Delta PV values are just normalized differences and say something about how different the clusters are relative to the ensemble spread. We move this part to the methods and make sure the reader understands the meaning of delta PV values.

l. 221- 223 While the normalized PV difference is clearly highlighted, PVU and wind contours barely differ between clusters and are not discussed at all.

We agree and use ensemble mean or analysis wind contours now.

l. 230 The separation between clusters is hardly seen at that point.

We agree and only mention the PV difference in the text at this point.

l. 238-239 Remind Fig. 7d; better “stronger anticyclonic wave breaking” than “westward phase shift and larger amplitude”?

.

l. 242- 254 The description of Fig. 7e-h is difficult to follow and Fig. S1 suggests that differences in omega and Z850 are hardly significant in the region of interest. Consider removing altogether.

Thanks for these comments. We also received comments regarding this Figure from Reviewer 1. We completely revise Figure 7, especially the right hand panels, where we will present analysis fields with the goal to show the synoptic situation in which the error amplification takes place.

l. 244, 250 Either show or omit potential temperature.

l. 255- 263 This interpretation is meaningful and consistent with displayed material overall but (1) the formulation partly sounds speculative (“strong QG forcing”, “uncertain low-level wave”, “exponential growth”, “strong vertical coupling”) and (2) ensemble members differ not only in their initial conditions but also in their physical parametrisations or any other perturbations implemented by ECMWF to increase ensemble spread.

Thanks for this comment. Regarding point (2) we agree that the ensembles also differ in their physical parametrisations and errors might come from there but we can still see an initial condition uncertainty (at lead time 0, Figure 6) that seems to propagate into the North Atlantic and amplify there. We therefore argue that some of the uncertainty most likely comes from initial condition uncertainty. We will thoroughly revise this section and the analysis in Figure 7 and make sure our conclusions are less speculative and we acknowledge more clearly that we do not exclude contributions from model uncertainty.

Reply to all above comments for section 5 with no direct replies: Thanks, these comments point out unclarities or suggest an alternative wording. We consider them when revising the paper.

6 Diverging synoptic development impacts Medicane predictability
l. 268 What is a “Medicane-like” cyclone?

We will make sure our terminology is clearly defined in the introduction and consistently used throughout the text.

l. 270 The subsection provides a synthetic summary of the dynamics of all clusters, but what is the variability between members within each cluster?

We agree that the variability between members is not shown (except for the cyclone locations). We understand the goal of making clusters to reduce the information from all members into scenarios. The statistical significance test allows to draw the conclusion that the member within a cluster are really different from the members in another cluster. We now add information regarding the variability in cyclone intensities, their formation and upper level PV, most likely in the section where the results from the CPS analysis are presented.

l. 275-276 Mention the analysis PV is depicted by the black contour.

l. 277 What is meant by “exactly the ones”?

l. 278-279 There is no visible difference in PV between clusters in Fig. 8 a, e, i.

On the western side of the trough (marked by the p-values) slight differences are discernible. The point here is that the differences are maybe still small when comparing the full PV fields, but (as shown with the PV difference plots) they are significant and they propagated into the trough from upstream.

l. 288 Fig. 2c; slightly different time.

l. 288-291 Mention the cyclone in individual members is depicted by dots.

l. 295 Differences are substantial but not necessarily due to latent heat release (only).

We agree that the differences in the PV cutoff evolution are not necessarily all due to latent heat release. However, if PV is eroded in cluster 1 and not in cluster 3, and as erosion of PV cutoffs is known to be related to substantial latent heat release, this is a clear indication that cluster 1 experiences more latent heating or at least, a different one (i.e. one in the vicinity of the PV cutoff).

l. 297-298 Clarify these are mean values.

l. 305 Fig. 9d

l. 306-306 The smoothing effect due to averaging makes the comparison difficult for precipitation; how do individual members look like? You could e.g. compute PDFs of accumulated or instantaneous precipitation for each member and the analysis.

Thanks for this comment. We will include a Figure and add a sentence that provides information about the internal variability within the clusters to make them better comparable to the analysis.

l. 314-316 These arguments are too speculative and are better left to Sec. 6.3.

l. 318 Again, what is a Medicane-like system?

l. 318-325 This paragraph is confusing and must be rewritten/streamlined. How do you define a low-level warm core, a warm seclusion and a Medicane-like system, and why do you focus on two air streams?

l. 326-327 The analysis tool Lagranto belongs to the methods and is already mentioned above.

l. 330 Is the warm core formation shown somewhere?

l. 345 “weaker” not “stronger” increase.

l. 346 In clusters 1 and 2.

l. 347 The Mediterranean Sea is not an ocean.

l. 353-356 This would likely better fit at the beginning of the paragraph.

l. 363 Closer to the coast but the region remains the eastern Mediterranean.

l. 363-371 The discussion is speculative so far, as the cyclone thermodynamics have (still!) not been documented yet. There is also a general confusion between warm core, warm seclusion, warm sector and tropical structure.

l. 377-394 This all belongs to the Methods. What radius is used to compute CPS metrics?

l. 394-399 I expect clusters 2/3 to show more favourable low-level/high-level forcing but not necessarily to produce a stronger/weaker Medicane.

l. 400 Avoid introducing an additional name ("DWC"), which adds confusion, better stick to the terminology used up to that point.

l. 400-402 What about the two other CPS metrics, symmetry and low-level warm core? The upper-level warm core metric might be contaminated by the presence of the PV streamer/cutoff. And what about the cyclone intensity?

Thanks for this comment. Although it could be worth looking at these additional aspects, the focus of this study is to investigate the factors affecting the formation of the deep warm core in Zorbas, which is a major characteristic of medicanes. The low-level warm core is indirectly included in the sense that it is a necessary requirement for a deep warm core. However, as the upper-level warm core is a distinguishing factor that separates so-called "medicanes" from subtropical cyclones, it is in the main focus. Regarding parameter B, this is a measure of the frontal nature of the cyclone, but analysing the frontal structures of Zorbas is beyond the scope of this paper. In order not to extend the analysis, we have decided not to analyse those parameters. However, we now additionally analyze the maximum cyclone intensity in each cluster.

l. 403-404 But cluster 3 produces stronger upper-level warm cores than cluster 2, which contradicts the other results and interpretation.

This is true, but cluster 3 produces only 2 cyclones with an deep warm core, so this average has to be taken with caution. We revise the deep warm core analysis and now show individual members rather than box plots to make the clusters better comparable.

l. 407-409 The three-day long sustained deep warm core (Fig. 5a) appears unprecedented. Can you provide CPS diagrams for the analysis?

Yes, we will provide the CPS of the analysis in the introduction.

l. 412-413 Why?

l. 414-416 What about convection?
We agree that convection can also be a relevant sub-synoptic scale process. This sentence was not meant to exclude other factors. We make this sentence clearer and mention that also internal storm dynamics/convection can be relevant.

Reply to all above comment for section 6 with no direct replies: Thanks, these comments point out unclarities or suggest small structural or content changes of the text. We consider them when revising the paper. Especially we will make sure that the terminology is clearer.

7 Conclusions
l. 427-418 Again, what is a subtropical cyclone, a tropical-like system or a Medicane?

l. 431 More details about this “first case”?

l. 432 Which process?

l. 435-438 This is not shown here.

l. 445-446 How do you know this?

It has also been pointed out by reviewer 1 that it is not clear how we arrive at the conclusion that diabatic air streams were not relevant for the uncertainty amplification. We will support this point better with the analysis (Figure 7) by providing more information about warm conveyor belts and precipitation.

l. 446-447 Which process, baroclinic instability?

l. 455-457 Ensemble forecasts are computed with different perturbation methods thus the error growth cannot be attributed to initial conditions only here.

Thank you for this comment. As already commented above, we agree that model errors can contribute to errors in this case. However, we show that a patch of uncertainty is present at initial time of the forecast that then propagates over the North Atlantic where the amplification takes place in its vicinity. Even if we cannot exclude model error here, we argue that the analysis shows strong indication, that initial condition uncertainty was very relevant in this case.

l. 458-460 . . .and convection and its organisation.

l. 462 As the used data is from ECMWF essentially, it could be stated how to access it.

The data used is not available long term from ECMWF with this high vertical resolution (which was required to compute PV and trajectories). We downloaded this data immediately after the event occured

Reply to all above comment for section 7 with no direct replies: Thanks, these comments point out unclarities or inconsistencies in the conclusions. We carefully consider them when revising the paper.

References
Providing DOIs or URLs for all papers would be helpful
Thank you for this comment. We will provide DOIs.

Figures
Moving all figures to the end of the paper would ease the review.

In the submitted document for discussion they are all at the end of the paper.

Fig. 5: it is unclear which date relates to which tick mark.

Fig. 7 appears before Fig. 6.

Fig. 6: consider changing the color scale to [-1,5; 1,5] and plotting coast lines at higher definition.

Fig. S1: the title should refer to Fig. 7 not 6.

Reply to all above comment regarding the Figures: Thanks, for these comments. We consider them when revising the paper.

Reviewer 3

This interesting paper deals with the predictability of the Medicane Zorba, which affected the central Mediterranean in September 2018. ECMWF ensemble forecasts are used in this effort. The limit in the predictability of the cyclone is analyzed and discussed in connection with the upper-level PV, which also affected the low-level evolution.

This is one of the first paper that clearly identifies the relevance of PV features in the predictability of Medicanes. The results are a relevant contribution in the field; however, the analysis should be substantially improved. Some points are indicated hereafter.

Major points:
Line 27, Line 154-157: I would like to see some clarifications about the definition of Medicanes. Although there is no general consensus, in most of the literature (e.g., Miglietta et al., 2011; Picornell et al., 2014; Cavicchia et al., 2014), a Medicane is considered as an extra-tropical cyclone that acquires a symmetric, deep warm core in the Mediterranean region. At the same time, the presence of a deep, warm core is not always an indication of tropical-like processes going on: as discussed in Fita and Flounas (2018) and Mazza et al. (2017), a deep warm core is not necessarily associated with a WISHE mechanism, but it can also be induced by a warm seclusion. However, Miglietta and Rotunno (2019) have shown that the intensification of the same cyclones discussed in the latter two papers cannot be explained without considering the sea surface fluxes and the latent heat release, in analogy with the WISHE mechanism typical of tropical cyclones. For these reasons, I suggest to remove "occasionally" (Line 27) and the sentence "Warm seclusion have been previously linked to Medicane formation" (Lines 155-156). Also, at Line 317 and 321 you "investigate potential pre- cursors of a low-level warm core": however, the low-level warm core is not relevant for the following development of the cyclone in itself (see also Line 365, 396), but because of the high values of equivalent potential temperature that are responsible for potential instability and favor the development of convection at later times.

Thank you for these explanations. We will make sure that in the revised paper we have a clearer definition of medicanes and that the subsequent argumentations are consistent with this definition.

Figure 10: I found the understanding and interpretation of Figure 10 quite difficult; in particular, I did not understand if the back trajectories you show are averages over all the ensemble members, since this is not mentioned in the Figure caption and not clearly reported in the text (Line 334); also, the presence of high percentages far from the plotted trajectories (purple shading) is counterintuitive.

Line 350-355: for a more comprehensive analysis of the trajectories in Fig. 10, some information should be included about the change of height along them.

Thanks for pointing out these unclarities in Figure 10 and the related analysis. As the other reviewers were also critical regarding this analysis and because it is not the centrepiece of the paper, we consider removing this analysis in favour of enhancing the focus of the paper. We provide a trajectory analysis based on the operational analysis in the supplement.

Minor points:
Line 115: please provide the definition of ACC

Yes we can do this.

Line 130: red instead of blue

Lines 143-145: the role of upper level PV anomaly in the generation of Medicanes is also discussed in Miglietta et al. (2017)

Thanks, we now include this reference.

Line 201: were instead of is

Line 202: severely instead of severly

Line 207: please can you provide an approximate indication of the height the isentropic = 325 K corresponds to?

Isentropic surfaces are generally not horizontal, they can intersect the surface towards the equator and be in the stratosphere at the pole. We will provide information about the approximate pressure level of the 325 K in the specific location over the North Atlantic that is discussed in this part of the text.

Line 214: why do you give less weight to the regions of strong gradients?

The idea of standardized anomalies is to quantify how different two clusters are, relative to the ensemble spread at a specific location. Just looking at absolute PV differences can result in high values just because the spread is high. But in this case, we are interested in regions where the clusters start to separate within the ensemble. As a result, regions of strong gradients are usually given less weight, because that’s where the ensemble spread is usually large.

Line 221: explain why “stratospheric” side

This is because we use the 2 pvu contour to separate stratospheric and tropospheric air masses. The PV difference is located poleward of the 2 pvu contour and of the jet streak which is a region of stratospheric air.

Line 311: favorably instead of favourable

Line 317 and elsewhere: Medicane or tropical-like, not Medicane-like!

Figure 5 caption: why do you use only 46 members for the second ensemble?

Because this is what we downloaded operationally and for technical reasons, 4 members were missing in this forecast.
We now include the missing 4 members.

Figure 6: change contour line colors to facilitate interpretation

Figure 7 caption: please indicate that the black contour refers also to captions (a-d)

Figure 8 caption: (e,i) instead of (e,f); the black contours around the teal patches create confusion

Reply to all above comments with no direct reply: Thanks, for these comments that show wrong spelling or suggest Figure improvements. We consider them when revising the paper.

Supplement material, Line 9: "The results show that significant differences of QG are located in the region of strong QG on 1800 UTC 24 Sep 2018": it does not seem to be the case, at least at that time.

There are some patches of significant differences of QG omega in the region where QG omega is high. But we agree that this argumentation and analysis are a bit shaky. Also as response to comments from the two other reviewers, we revise the analysis shown in Figure 10. Most likely, in this new analysis we will not show QG omega anymore.

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Cavicchia, L., von Storch, H., Gualdi, S. (2014). Mediterranean tropical-like cyclones in present and future climate. J. Clim. 27, 7493–7501.

Miglietta, M.M., Moscatello, A., Conte, D., Mannarini, G., Lacorata, G. and Rotunno, R. (2011). Numerical analysis of a Mediterranean hurricane over south-eastern Italy: sensitivity experiments to sea surface temperature. Atmospheric Research, 101, 412–426.

Miglietta, M.M., Cerrai, D., Laviola, S., Cattani, E. and Levizzani, V. (2017). Potential vorticity patterns in Mediterranean “hurricanes”. Geophysical Research Letters, 44, 2537–2545.

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