Reply document
This document includes our reply to Ambrogio Volonte. In the following, the Reviewer's original comments are shown in Italics and our reply in blue-colored text.

This manuscript contains a comprehensive analysis of Storm Daniel, choosing not to focus on one single aspect of its lifecycle, dynamics, impacts, drivers or climate change contextualisation but rather to include all those angles. I think this is what the authors mean with the term "holistic". While it is certainly needed a certain degree of effort to go through and understand the results from all the different analyses, in my opinion the authors do a very good job in presenting them in a single article that is successful in highlighting the importance of events like Daniel and describing "how they work".

I think that this manuscript could become a very worthwhile contribution to WCD and I don't have any major issues with it being accepted following revision. As I don't think the overall structure needs fundamental changes, I went into some level of detail with the line-by-line comments. There are quite a few of them (sorry!) but most should be quick and easy to address. I have also a few general comments, that in my opinion are more important. You can find them below. The last of those comments is on the section discussing the attribution to climate change. I understand that my tone there could possibly sound critical and dismissive and I would like to stress that that is not the case. I would be very happy to see the section retained in the manuscript but I think that there are some issues that need addressing. Should you find anything in my comments that is not clear, please feel free to contact me.

We would like to thank Ambrogio Volonte for his positive review and the many fruitful comments. Indeed, we wanted to provide a "holistic" view of the event, aiming to link weather and climate perspectives of the same case study from the point of view of different disciplines. In this revised version, we have made substantial changes and made corrections, including the section on climate change attribution.

To make it clear in the introduction, we included the following text to highlight our motivation better:
"When a high-impact weather event occurs, it encompasses multiple interconnected aspects often studied separately. First, understanding the event's dynamics and physical processes is crucial for assessing short-term forecasting and climate change implications. Second, the associated hazards—such as floods and windstorms—must be assessed concerning the specific conditions of the affected areas. This also raises questions about hazard predictability. Lastly, the event's severity must be placed within a climatological context to determine whether it produced extreme weather conditions and to attribute its intensity to climate change. Despite their interdependence, all these aspects of a specific weather event are rarely examined through an integrated approach. Our motivation is thus to apply a comprehensive framework, using Storm Daniel as the centerpiece of the September impacts in the eastern Mediterranean, and to provide an interdisciplinary assessment of the event (Shirzaei et al., 2025)..."

General Comments:
It is confirmed in the author contributions section that the manuscript was written by several of the authors and it does indeed give an impression of being a little "fragmented". My suggestion to improve readability would be that one of the authors could go through it and try to harmonise the writing style, choice of words and sentence structure, to improve its readability (see the line-by-line comments).

Thank you for the suggestion. Indeed, this was a collective effort, aiming to combine the expertise of several colleagues. The new version underwent substantial editing to better harmonize the writing style.

Abstract: I hope this comment doesn't sound too subjective, but I think that this abstract is quite complex and long-winded. There are many points in which the novelty and importance of this study are highlighted, but their effectiveness is hindered by some sentences being too long, convoluted and not direct enough. Could you make it neater and sharper? See also some specific comments in the "Line-by-line comments" section.

The abstract has been substantially revised.

Conclusions: I did not spend the same amount of time on them as I did on the abstract, so I would just recommend that as you improve the abstract you also amend the conclusions accordingly.

The section on conclusions has also been revised accordingly to match changes in the manuscript.

Attribution to climate change:
- I have some questions on the methodology used and on describing Daniel as hitting / making landfall over Greece, see in particular comments on Line 580 and 625.

We acknowledge the reviewer's concerns regarding the description of Daniel as "hitting" or "making landfall" over Greece. In our revised text, we will refine our wording to better align with the meteorological characteristics of the event, distinguishing between landfall (typically associated with tropical systems) and the impact of a Mediterranean depression over Greece.

- Coming to the conclusions that can be drawn from the section, it is stated that "Mediterranean depressions like Daniel hitting Greece and Libya show lower MSLP and higher precipitation in the present climate than in the past" and from this it is concluded that "[w]e thus interpret Daniel as an event whose characteristics can be ascribed to human-driven climate change". I understand the argument on heavier precipitation in a warmer climate (although it will have to be expanded without assuming that all readers are familiar with Clausius-Clapeyron and related implications) but no explanation is provided here on the changes in MSLP. As it stands, those conclusions look fairly weakly justified and not adding much to this particular study. For example, the "heavier precipitation in a warmer climate" link could have just been made by looking at the anomalously high SSTs during the event. Could you please make the case as to what the added value of this section is and why is it worth including it?

The reviewer notes that while the link between a warmer climate and heavier precipitation is understood, the change in MSLP is not well explained. We clarify that our analysis does not claim a significant decrease in MSLP over Greece but rather identifies trends in analogues of Mediterranean depressions in general. Specifically:

1. The analogue approach allows us to assess whether Mediterranean depressions similar to Daniel have changed over time, including pressure, precipitation, and temperature fields.
2. Our methodology explicitly isolates trends in precipitation, demonstrating increased rainfall associated with similar systems over the Ionian Sea and Albania in the present climate.
3. The significant warming over the Mediterranean Sea (Figure 12e-h) is a key driver of this increase in precipitation, consistent with Clausius-Clapeyron scaling.

To clarify this further, we will expand our discussion of precipitation trends and explicitly state that the observed MSLP changes in Greece are less pronounced than in Libya. Instead, we emphasize that the increase in precipitation and frequency of similar systems are robust signals of climate change.

The reviewer also questions whether our conclusions could have been drawn solely from the observed high SSTs during the event. While high SSTs are important, our methodology provides added value by contextualizing Daniel within historical atmospheric patterns. The key contributions of this section include:

1. Demonstrating that similar Mediterranean depressions have become more frequent in the present climate, particularly in December (Figure 12t).
2. Identifying statistically significant changes in precipitation and temperature patterns associated with these systems, independent of a single SST anomaly.
3. Showing that while large-scale climate variability modes such as ENSO, AMO, and PDO can influence atmospheric conditions, our analysis does not establish a direct causal link between these modes and the development of Daniel; rather, this assessment is exploratory, highlighting potential associations without making definitive attributions given the limitations of a 40-year dataset.
4. By comparing the event against a database of past analogues, we provide a broader climatological perspective rather than relying on one event's SST anomaly alone. We will clarify this in the revised text to highlight why this approach strengthens the attribution analysis.

In response to the reviewer's concerns, we have:

- Improved the phrasing of Daniel's impact on Greece to avoid misleading terminology.
- Explicitly stated that significant changes in MSLP are not evident in Greece, but changes in precipitation are.
- Expanded the discussion on precipitation changes, connecting them more clearly to Clausius-Clapeyron without assuming reader familiarity.
- Highlighted the added value of the analogue approach compared to SST-based reasoning alone.

We hope these clarifications address the reviewer's concerns and strengthen the clarity of our attribution findings.

Line-by-line comments:

Line 38: It would be nice if you could remove this "probably" as you will agree that it doesn't sound great in an article abstract. I appreciate that you won't have access to every weather report that has ever been produced in the region but, assuming some prior research (that you have likely already done), you could replace "probably" with "to our knowledge" or similar, or remove it altogether and add "in recent times" or similar (while also removing "ever").

Indeed, we did not find a more catastrophic flood event in the Mediterranean and therefore, we rephrased "*to our knowledge".*

Line 39-45: I like the creativity in the choice of words, but using simpler and shorter sentences could make it easier for the reader to understand what the aims, perspectives and strategies of this study are.

The abstract has been revised.

Line 46: "Our results … cyclone". This is a bit vague. Are you referring to its structure, its intensity or other properties? Are Medicanes included in this "any other intense Mediterranean cyclone" terminology?

Thank you for this comment. Indeed, the formation mechanism of medicanes is not expected to differ from the one involved in any other intense Mediterranean cyclone. We clarified this in the abstract:

"Daniel initially developed like any other intense Mediterranean cyclone, including medicanes, due to upper tropospheric forcing followed by Rossby wave breaking."

Line 50: "The predictability of the cyclone formation was rather low even in at relatively short lead times…".
Lines 52-53: "Our analysis of impacts shows that numerical weather prediction models are capable to capture the extreme character of …".
Line 58: "while in at its maturity".
Line 61: Another petty comment (sorry…), but the "event" is Daniel, not its impacts.

The abstract has been revised.

Lines 83-84: "Daniel was an intense cyclone, preceded by Rossby wave breaking over the Atlantic and the consequent intrusion of an upper-level trough": do you know if any weather report by national agencies was published showing this evolution? It would be nice to see this wave-breaking somewhere.

We added the reference of Couto et al. (2024), who examined the large-scale dynamics prior to the formation of Daniel.

Line 146: "Can Are numerical weather prediction models adequately for the prediction simulate climate extremes?"

Done.

Lines 192-195: Could you explain why "two-dimensional objects of extreme precipitation" are defined using 99th percentile values of precipitation and wind?

Thank you for pointing out a typo. The two-dimensional objects of extreme precipitation are, of course, defined by using the 99th percentile values of precipitation only. This has been corrected in the revised version of the manuscript.

Lines 206: "Greece and Libya boxes"

Done.

Lines 216-217: I would say "we identified moisture sources in Daniel and in the 100 most extreme daily precipitation events"

Done.

Line 219: "Precipitate" or just condensate, given that the criterion only considers specific humidity?

If specific humidity decreases during the last time step and relative humidity is higher than 90%, we define this as a "precipitating air parcel". This follows the definition by Sodemann et al. (2008) and assumes that the missing humidity during this time step will precipitate. Thanks to this question, we noticed a bug in our script during review, which led, however, to irrelevant changes in the moisture source distributions.

We adjusted the text to include this information:

"This method involves the tracking of changes in specific humidity along all trajectories that precipitate upon arrival, which are defined as air parcels showing a decrease in specific humidity during the last time step before arrival and a relative humidity larger than 90% upon arrival (following Sodemann et al., 2008)."

Line 220: "All subsequent moisture uptakes or losses weight a moisture uptake". Could you

rewrite this? I don't think I'm understanding what it means. Also, could you clarify what you mean by "weighted" in the following sentence?

Thank you for pointing out that more explanation is needed for the moisture source diagnostic. We reformulated this part and added a reference to the method's paper:

"Along each trajectory, an increase in specific humidity is interpreted as a moisture uptake, and a decrease in specific humidity is interpreted as a moisture loss. Each moisture loss reduces all previous moisture uptakes, weighted by their uptake amount. For a detailed description of the moisture source diagnostic, see Sodemann et al. (2008)."

Lines 240-242: "To account for the seasonal cycle in surface pressure and temperature data, we remove the average pressure and temperature values for the corresponding calendar days at each grid point and each day". I assume to do this you take advantage of the MSWX ensemble forecasts to increase sample size, given that there are only 22 years in each dataset. Could you give more information on this? For example, how many realisations do you use? At what lead time? ….

Thanks for this remark, MSWX does not provide ensemble forecasts. It only uses a single realization for producing its real time product.

Lines 272-274: Could you motivate the choice of considering the role of ENSO, PDO and AMO (all having different frequency) among the many modes of natural variability? Also, as these indices are just diagnostic, what value do they add to the analogue analysis? In other words, does it matter if two cyclones with the same MSLP pattern occur during different phases of ENSO?

The choice of these modes of variability comes from the fact that they are the main oceanic modes that influence the weather at scales longer than one year. The use of these indices allows building other counterfactual worlds, namely those with positive or negative phases of the indices, and therefore allows to determine whether the changes observed could be linked to natural variability instead of greenhouse gas emissions

Figure 1: There seems to be something wrong with the track. If red dots are shown every 6 hours, then a few are missing between 6 Sep 18Z and 8 Sep 18Z and too many are shown between 11 Sep 00Z and 11 Sep 12Z. Also, the location 11 Sep 00Z red dot is not consistent with that of the black dot in Fig 2b.

Thanks for noting this. Figure 1 has been modified, correcting the mistakes in dates and time indicated in the original figure. To accommodate the Reviewer's 1 comments, we have added more information on the plot.

Figure 1,caption: It should be "Storm Daniel" and not "Daniel storm" (please correct it here and elsewhere in the manuscript).

Corrected.

Lines 287-288: Are there any articles or reports showing the omega-blocking pattern and anticyclonic wave breaking that you could cite here?

We added the reference of Couto et al. (2024).

Lines 303-304: Any reference for the NOAAN rainfall observations?

This reference has already been included:

"Lagouvardos, K., Kotroni, V., Bezes, A., Koletsis, I., Kopania, T., Lykoudis, S., Mazarakis, N., Papagiannaki, K., and Vougioukas, S.: The automatic weather stations NOANN network of the National Observatory of Athens: operation and database, Geoscience Data Journal, 4, 4–16, https://doi.org/10.1002/gdj3.44, 2017."

Fig 2: It is difficult to see what the max rainfall values are in both panels and to agree that they are "underestimated by about 50% in the ECMWF analysis" (lines 305-306) as all values above 200mm are purple. I'm sure you'll have already tried many different colour scales and intervals, so one possible alternative solution could be adding a small marker at the location of peak rainfall and annotating the value next to it.

We have added some additional information about the differences between the IFS and observation maximum 24-hr accumulated precipitation:

"The NOAAN surface stations recorded more than 750 mm of daily rainfall and up to 1,235 mm within four days in eastern parts of the Thessaly region (flooded areas are shown in cyan colours in Fig. 1b). Notably these peak values are underestimated by about 40% in the ECMWF analysis (max IFS 24-h accumulated rainfall equal to 434 mm on 6 September 2023 00 UTC)."

Fig 3: In the caption, should "total moisture uptake" be replaced by "maximum moisture uptake", or am I getting this wrong? This comment is related to those regarding lines 219 and 220 (see above) as I have to admit that the methodology here is not totally clear to me. More generally, whilst I understand the need to show a large domain and thus for the Mediterranean region to be rather small, could you make the panels wider (and try playing with projections and domain edges, coastline and state boundary colours or adding zoomed-in boxes) to facilitate identifying locations in them? At the moment is not very easy to see where "the Aegean and Black Seas" (line 313) are, for example.

Thank you for pointing this out. The caption of Figure 3 should be "relative moisture uptakes" as they give the relative contribution per km^2 of each grid cell to the total moisture that contributes to the precipitation event for the respective day. The contour lines show another unit, which represents the percentage of the total moisture uptake, thereby summing up from largest to smallest relative moisture contribution per grid point. We've adjusted the caption to make this more straightforward. Concerning questions on the methods, please see the answer to the question regarding lines 219-220. Finally, we've adjusted Figure 3 so that it is easier to identify moisture uptake hot spots. This also includes adding the land/ocean fraction of the moisture uptakes and a comparison with results from a recent study (Argüeso et al, 2024). The text has been adjusted in the following way:

"Further moisture mainly originated from central to eastern Europe and the North Atlantic Ocean. These source regions are in general agreement with a recent study (Argüeso et al., 2024), which investigated moisture sources of rainfall over Greece from 3 to 9 Sep 2023 using a Eulerian moisture source diagnostic. Our moisture source analysis shows larger contributions from land (54.7%) than in Argüeso et al. (2024) (27%). The Lagrangian method used in our study shows relatively large moisture contributions from north of the Black Sea because most of the air parcels arriving on 5 Sep 2023 descended and took up moisture in this region before moving southwestward along the western flank of the PV streamer. The differences in the land fraction between the two methods might originate from different periods used for the moisture source calculations, different handling of moisture uptakes above the boundary layer, a lower explained fraction of the total moisture sources (84%) with the Eulerian compared to the Lagrangian diagnostic (explained fraction of 90%), and general differences in Eulerian versus Lagrangian approaches. An ongoing comparison study of moisture source diagnostics is investigating differences in these methods in detail and will

shed more light on disagreements between various moisture source diagnostics."

Line 315: "concomitant to the upper-level PV streamer". The wind barbs are at 850hPa while the PV streamer is at 300hPa. Why is it relevant that they share location and orientation?

Concomitant might be a strong word for the context. We revised it to "*having a similar orientation with the upper-level PV streamer."*

Lines 320-324: Moisture sources for Storm Daniel are "partly in contrast" with the climatology, but also "somewhat overlap" with it. Could you please rewrite these sentences, as the above claims seem to be in conflict with each other?

We revised as follows:
"The moisture sources shown in Fig. 3a largely overlap with the climatological moisture sources of extreme precipitation in the same area. However, for Daniel, they are mostly concentrated over the Aegean Sea and areas to the northeast. In contrast, the typical moisture sources for extreme precipitation in Thessaly extend further over the central Mediterranean (Fig. 3b)."

Fig 4: Please replace "1993-2023" with "Jan 1993 – Aug 2023" in the two panels and in the first line of the caption.

We replaced it with "Jan 1993 – Aug 2023" in the figure and caption.

Line 333: Replace "signify" with "indicate"?

We replaced it with "indicate."

Line 344: I don't think "events" is correct here. Please rephrase.

We removed "events."

Line 348: "in the central Mediterranean has been was anomalously high, by roughly 2 K respect to above the average"

This part has been revised.

Lines 362-364: "While weaker than earlier, the wrap-up of the upper-level PV streamer around the cyclone centre was proposed to be responsible for its intensification just before the cyclone made landfall". Do you see any resemblance with the "low-PV bubble" dynamics highlighted in WCD - The impact of preceding convection on the development of Medicane Ianos and the sensitivity to sea surface temperature ? I am not suggesting you should cite this work (of which as you know I am coauthor), it's just curiosity.

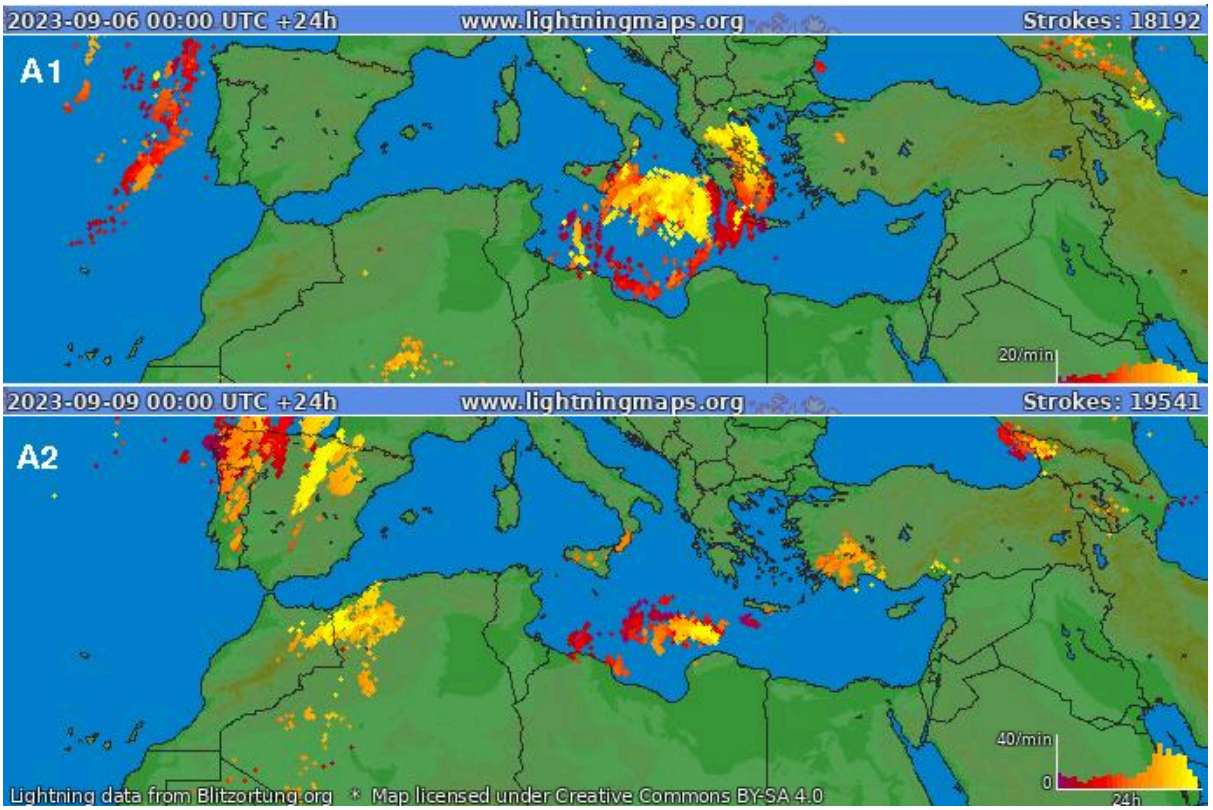
It may be that the outflow of deep convection hinders the eastward progression of the upper-level PV streamer and the subsequent interaction with the cyclone similarly to the case of Medicane Ianos. However, without dedicated sensitivity tests to factors controlling convective intensity as in Pantillon et al. (2024) and Sanchez et al. (2024), assessing such a complex feedback is challenging. Here we prefer not to speculate on the detailed dynamical mechanisms and rather emphasize the anomalous intensification during landfall.

Lines 354-356: Is it possible to see these features anywhere (papers/reports/publications)?

Unfortunately, we could find publications supporting our analysis, but our statements are

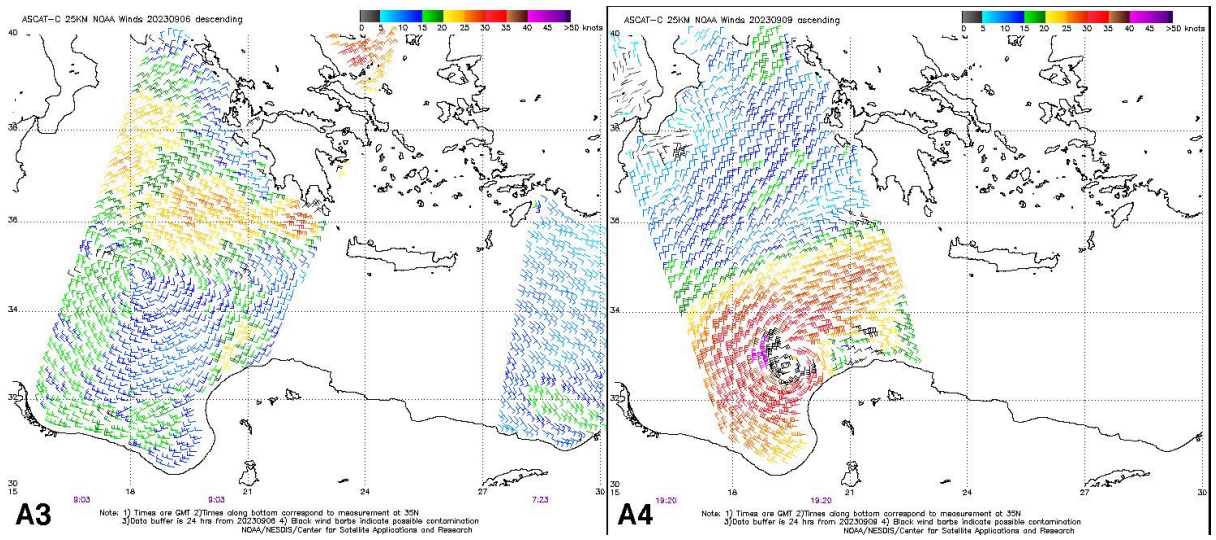
based on various open-source data that we decided not to include in the main manuscript.

Regarding deep moist convection and lighting activity, we have attached Figures A1 and A2, showing the total lightning activity that was recorded by the Blitzortung network (https://www.lightningmaps.org/blitzortung/europe/index.php). On 6 Sep 2023, the largest part of the Ionian Sea and Greece were affected by lightning activity (Fig. A1), whereas on 9 Sep 2023, lighting was limited close to the cyclone centre (Fig. A2).



Figures A1-A2 24-h accumulated total lightning detections (strokes) on 6 and 9 September 2023 by the Blitzortung network.

Figures A3 and A4 support our statement about the morphological changes in the wind field close to the sea surface. On 6 Sep 2023, the highest wind speeds that were estimated by the ASCAT instrument onboard MetOp-C satellite (https://manati.star.nesdis.noaa.gov/datasets/ASCATCData.php) were found far away from the cyclone centre (Fig. A3), and on 9 Sep 2023 the winds close to the cyclone centre were much stronger, with the highest wind speeds a few kilometers northwest from the Storm Daniel's centre.



Figures. A3-A4 Advanced Scatterometer (ASCAT METOP-C) near surface wind speed on 6 and 9 September 2023.

Line 388: "(Weather World Meteorological Organisation, 2023)"

Done

Lines 400-401: "Eventually, after landfall, Daniel dissipated fast over the Sahara Desert when it reached Egypt on 11 September 2023." Is this sentence relevant here? Also, without adding any more context, could it not be at least partially in contrast with the inland intensification described in Hewson et al. (2024) (included in your references) and your earlier discussion on the importance of the upper-level setting for the intensification near landfall? Please consider removing or clarifying it.

Thank you for spotting this. Indeed, the phrase was wrong, and thus it was removed.

Lines 425-426: "at the initial stage of Daniel, it is the timely prediction of cyclogenesis that would primarily provide useful information to civil protection". If, as you say, "most precipitation was produced in areas remote to the cyclone centre" and caused by moist flow impacting from NE, wouldn't it be that moist flow the key ingredient to be predicted rather than the actual presence of a fairly weak developing cyclone downstream? Can you elaborate on this? (here or in the section that is most suited to this discussion)

Thank you for this insightful comment. We revised these lines as follows:
"Therefore, accurately forecasting the time and location of cyclone formation (as shown in Fig. 8) may play a secondary role in predicting its impacts in Greece. In this context, the reliable simulation of moisture inflow—which appears to be more closely linked to large-scale circulation, as previously discussed—by the EPS members could be more crucial for impact prediction."

Lines 433-434: "numerical weather prediction models" (here and elsewhere).

Done

Lines 452-454: "the direct relationship between the Rossby wave breaking over the Atlantic Ocean and the accurate prediction of Mediterranean cyclogenesis." From what you show, I would say that the direct relationship is between the upper-level PV streamer and the surface cyclogenesis (although the link between the streamer and wave breaking, not shown

in this work, can certainly be mentioned and placed in the context of recent literature as currently done at the end of this paragraph).

Thank you for this comment. This part has been revised:

"The similar behaviour in the cyclone and PV streamer predictability relies on the direct relationship between the Rossby wave breaking over the Atlantic Ocean and the accurate prediction of Mediterranean cyclogenesis. This has been highlighted by Chaboureau et al. (2012) and Pantillon et al. (2013) for the case of the extratropical transition…."

Figure 7: In my view panels b,d,f,h should refer to 11 Sep 00z, to be consistent with Figs 2,8,9.

The Figure has been revised as suggested.

Lines 461-462; "while two members of the EPS do not even predict cyclogenesis (not shown)." Or is it shown anywhere?

This part of the text has been revised following the changes in the lead times shown in Figs. 7-9.

Line 474: Is it 10 Sep or 11 Sep here? See comment on the date and time issues in Fig 1.

Thank you for spotting this. "Mature stage" has been deleted. The 10th of September refers to the day of landfall.

Line 482: "MSLP spread does not have a clear pattern"

Done.

Lines 484-486: It would be nice if you could add a discussion here on the agreement between EPS members on the PV streamer for 11 Sep, as it does not seem to be higher than that for 5 Sep. This would be particularly interesting as you previously highlighted its relevance for Daniel's intensification at this stage.

We added the following at the end of section 4.2:
"Upper tropospheric forcing is crucial in accurately predicting cyclone intensity in this context. While Fig. 7b —unlike Fig. 7a— shows that some EPS members align with the location of this upper tropospheric feature (blue crosses), an average of 2 PVU and an agreement above 50% among the EPS members near the cyclone center is only evident at a lead time of approximately three days (Fig. 7f, depicted by green crosses)."

Figure 9: I suggest reconsidering the order of the figures, as (unless I'm missing something). Fig. 10 is mentioned before Fig.9 in the text. Figure 9, caption: "Percentage of overlapping precipitation objects"

The order has been changed.

Figure 10: "(b) As in (a) but as a time series for tracks of cyclone Daniel."

Done.

Line 522: Shouldn't it be three days, given what you've just said on the limited agreement in Fig 9a and considerable increase in Fig9b? (by the way, I think you mean Fig 9c there)

Thank you for the corrections. This part has been revised accordingly.

Lines 528-529: "This suggests that the EPS members have been more consistent in the production of extreme precipitation even if cyclone centres presented a comparably greater spread." This is consistent with my comment on lines 425-426, on the moist flow towards Thessaly being the key ingredient for the prediction of the floods rather than the actual cyclogenesis further downstream.

Thank you for this insightful comment. We revised these lines as follows:
"Therefore, accurately forecasting the time and location of cyclone formation (as shown in Fig. 8) may play a secondary role in predicting its impacts in Greece. In this context, the reliable simulation of moisture inflow—which appears to be more closely linked to large-scale circulation, as previously discussed—by the EPS members could be more crucial for impact prediction."

Lines 539-540: What does "pretty corrected" mean?

The phrase has been corrected to
"The probability strongly increases at shorter lead times (Figs 10f and 10h) mostly and all EPS members tend to converge to similar cyclone locations when reaching a lead time of one day (Fig. 8h)."

Figure 11: Is it time that is indicated on the x-axis? Please specify it (including the interval between ticks). Also, could you add (a),(b),(c) … next to each panel?

The x-axis indicates the lead time for each panel (initialization) in 6h time intervals. This was added in the revised figure, as well as letters [(a),(b),(c) etc.] in each panel. Figure caption has been extended for clarity.

Figure 11, caption: "Wandi Derna River" (here and in the text, line 551).
We used "Wadi Derna" throughout the manuscript, ensuring alignment with the accepted terminology in the literature.
Lines 551-555: The discharge predictability for Wadi Derna River is generally lower than for Pinios (particularly on the 1st, 2nd and 4th rows from the top). Can you elaborate on this?

This section has been thoroughly revised to explain the differences in predictability, addressing the challenges specific to Wadi Derna. Below is the revised text:

"The forecasts for the Wadi Derna River outlet (Fig. 11, right panels) exhibit significant variability and fail to converge during the earlier forecast initialization dates as well as at shorter lead times. This persistent lack of convergence can be attributed to distinct challenges of both temporal scales. For earlier forecast initialization dates, the primary source of variability lies in the westward displacement of extreme precipitation predicted by the EPS (Figs. 10b and 10d). For example, forecasts initialized on 9 September, during a critical period for implementing preventative measures, display a wide spread and a shortfall in the median forecast compared to the benchmark (red line). This variability persists even for forecasts initialized on 10 September. The failure to converge at shorter lead times is compounded by challenges inherent to the Wadi Derna catchment. The resolution of the precipitation forcings used in the forecasts combined with the relatively small size (575 km2) and flash-flood-prone nature of this basin amplify the uncertainties in predicting discharge, particularly in response to localized extreme rainfall."

Line 580: I wouldn't use the expression "landfall over Greece" (here and later in this paragraph) given that the cyclogenesis is SW of Greece and then Daniel moves further away

from it. Starting from this trivial comment, there is a more fundamental question that I would like to see discussed. If I understand well the methodology (apologies if this is not the case), the ClimaMeter framework uses single-time surface pressure patterns. This means that a cyclone going in the opposite direction to Daniel (e.g., eventually making landfall over Greece rather than moving away from it) would be considered a suitable analogue provided it has, for at least one time, a pressure pattern similar to Daniel's. This example cyclone could be associated with impacts throughout its evolution that are completely different from those associated with Daniel. I know that ClimaMeter has already been peer-reviewed and I'm not questioning its merits, but I would like to at least see a brief discussion of how the issue presented above can be considered acceptable, in particular in this study. Also, could this issue be avoided, if only partially, by selecting a substantially larger domain (and thus forcing a much larger region to have similar circulation?)

We appreciate the reviewer's careful consideration of the methodology and their suggestion to refine our description of Daniel's evolution. We agree that "landfall over Greece" is not the most accurate phrasing, as the cyclogenesis occurred southwest of Greece before Daniel moved further away. We will revise this wording to reflect the storm's trajectory better.

Regarding the broader methodological question, the reviewer correctly identifies that ClimaMeter uses single-time surface pressure patterns to identify analogues. This means that a cyclone moving in the opposite direction to Daniel could, in principle, be considered an analogue if it exhibited a similar pressure pattern at a given moment. However, this limitation is mitigated in our study in several ways. First, while our analogue search is based on surface pressure alone, the subsequent analysis examines associated temperature, precipitation, and wind speed patterns to ensure that the analogues share broader dynamical similarities with Daniel. This helps to filter out cases where the identified analogue might have evolved in a vastly different manner.

Second, the issue of analogues with different tracks is partly addressed by the regional domain selection. While a significantly larger domain could, as the reviewer suggests, constrain the analogues further by ensuring that a broader area exhibits similar circulation patterns, it would also risk including patterns that match at a large scale but deviate in local storm dynamics. The current domain size represents a balance between capturing the key features of the Mediterranean depressions and avoiding overly restrictive constraints that could reduce the analogue sample size.

To acknowledge this point, we added a brief discussion in the manuscript outlining this trade-off and explaining that while our approach prioritizes pressure pattern similarity, the additional analysis of precipitation and wind fields ensures that the identified analogues remain meteorologically relevant. We will also clarify that while increasing the domain size could help filter out analogues with very different storm trajectories, it would not necessarily resolve all limitations. This transparency will ensure that readers understand both the strengths and potential constraints of the analogue-based approach used in ClimaMeter for this study.

Line 583: I would remind the reader here that 15 analogues for each period are considered in the analysis.
Line 590: "persistence of all the cyclones".

Thank you. We have rephrased this part.

Figure 12, caption: Is "concerning" the correct word in "color-filled areas indicate significant anomalies concerning the bootstrap procedure"?

Thank you. We have changed the text for clarity.

Lines 592-593: "Figs 12q-s show no significant changes between the two periods (present and past climate)." If significance is evaluated using the test presented at line 595, then I would move its description before this sentence.

Thank you. We have rephrased this part.

Lines 593-594: "We can… present periods". There must be a word missing here. Possibly "that" or "which" after "Q"?

Thank you. We have rephrased this part.

Line 611: Is the period under analysis 10 Sep as written here or 10/11 Sep as in the caption of Figure 13?

Thank you. We have rephrased this part.

Line 614: No description of T2m changes?

Thank you, we have added the temperature changes.

Lines 604-606 and 619-621: Sources of variability "may" have influenced the event. Written in this way it sounds like we don't know anything more about it that we didn't before the analysis. Could you rewrite it less vaguely and highlight what the result is?

We have rephrased this part, also acknowledging the exploratory nature of this analysis.

Line 625: Daniel does not "hit" Greece (although some of the analogues may, see above). I think this choice of words is misleading.

We have used "impacted" now.

References:

Argüeso, D., Marcos, M. and Amores, A.: Storm Daniel fueled by anomalously high sea surface temperatures in the Mediterranean. npj Clim Atmos Sci 7, 307 (2024). https://doi.org/10.1038/s41612-024-00872-2

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https://doi.org/10.1029/2007JD008503