

Reviewer 1

This manuscript constitutes a multi-faceted case study for cyclone/medicane “Daniel” which, at different stages in its lifecycle, delivered extreme weather and devastating impacts in parts of both Greece and Libya, in early September 2023.

My reactions on reading the response to reviewers and the revised article were diametrically opposite, and very hard to reconcile.

Firstly, the response letters that directly referenced reviewers’ comments were excellent, and covered almost every point that was made in an entirely satisfactory manner. Likewise, improvements made to the figures and captions, which was mainly what I commented on, were almost all comprehensive. The figure and caption quality, integrity and fitness for purpose have markedly improved as a result.

Secondly, I can note that, disappointingly, there was no direct response to my initial overview comments, which are quite fundamental, and then, in part because of this, the manuscript text remains in an unacceptable state. There are far too many grammatical and English errors, and there are also numerous contradictions, factual inaccuracies and unjustified sweeping statements. For some sentences I could not disentangle what the authors were trying to say. In the parts that I have gone through in detail – the Abstract, the Introduction and the Summary and Conclusions sections – I was encountering almost one error per sentence. I am sorry but it really is not the job of a reviewer to propose corrections at this level, so I confine myself to commenting on those 3 sections alone. This is time consuming enough! In the end it is the authors that should ultimately make the paper fit for purpose, not the reviewers. I can imagine that this may be a case of having “too many” authors without one electing to take full responsibility for making the final content coherent and consistent, but clearly I cannot know that for sure.

From an English standpoint I know that creating good sentence structures may be challenging for non-native speakers but I would advise that there are many tools out there that can help – e.g. Grammarly – and maybe the authors could benefit a lot from using such a tool to put together any re-submission?

Line numbers in the points below refer to the manuscript version which shows the corrections, not the cleaned version.

We thank the Reviewer for their comments and for the appreciation that the responses to the previous comments were excellent. All new queries have been addressed, and the text underwent substantial editing.

Detailed Points

1. L35. Daniel did not form as an intense cyclone. It formed as a weak cyclone that intensified markedly several days later as you highlight in the text.

Corrected.

2. L40. *“conducted” is better than “aim to conduct” as otherwise it sounds like you haven’t done the work.*

Corrected.

3. L41. *“Hazardous weather relevant to extreme precipitation”. To my mind extreme precipitation is hazardous weather. Reorganise the sentence.*

Cyclone systems can produce hazardous weather due to windstorms, storm surges, sea waves, hail, etc. This was relevant to precipitation, floods, and significant sea wave activity. We changed to “hazardous weather conditions related to extreme precipitation”

4. L44-46. *Not sure what you are trying to say in this jumbled sentence. For example what do you really think are the implications of your findings for NWP? If that’s a big part then surely the abstract should give the reader some clues as to what they are. Please re-write.*

Changed to: *“Given the climatologically extreme precipitation produced by Daniel, we examine the capacity of numerical weather prediction models to capture such extremes, and we finally investigate potential links to climate change.”*

5. L47. *Gives the impression that this was rapid cyclogenesis from the outset, whereas it was anything but. Please rewrite.*

This comment is not entirely clear to us. We reexamined the phrase in the abstract and believe our statement is correct. If any additional corrections may be necessary, we shall implement them as required.

6. L49. *“Even in” is a bit odd. Implies that near to the centre there were big impacts, which I have not seen. “notably in the region of Thessaly which was actually quite far from the centre” would be much better wording to my mind.*

Corrected to *“At this stage, it produced significant socioeconomic impacts in Greece, i.e., in areas far from its center.”*

7. L49-50. *“As it intensified...it peaked after landfall...” is bad English. It developed markedly just prior to landfall, reaching peak intensity over land.*

Corrected as suggested.

8. L51-53. *Sentence reorganisation needed – e.g. Considering short lead times (around four days), cyclone formation exhibited low predictability, whilst landfall in Libya was more predictable.*

Corrected as suggested.

9. L53-55. *This statement is not correct. You highlight a shortfall in precipitation totals for the Greece case; in fact with a lead time of 2 days – useful for forewarning - the observed gauge peak was 2.5 times the ECMWF HRES forecast total (see news item by Hewson in*

<https://www.ecmwf.int/sites/default/files/elibrary/12024/81535-newsletter-no-178-winter-2023-24.pdf>). Your reference to a somewhat smaller but still notable shortfall corresponds to analyses which cannot generally be used to forewarn, and moreover I do not know if the values quoted were in the same location. So this doesn't really provide "crucial information on the expected severity", or at least not without much more work on cross-referencing model climates etc, which you don't discuss. Likewise, regarding the devastating floods in Derna it is very clear that for these we did not have "crucial information on the expected severity" of the riverflow (Figs 4b and 11) (never mind the dam collapse) as you acknowledge in the text.

Extremes of precipitation and river discharge are statistically treated from model outputs despite their bias from observations. We show that the extreme character of the event was fairly well captured in ECMWF NWP and GLOFAS. Consequently, we support the idea that information on the severity of the weather event could be deduced from the forecasts. We decided to leave the phrase as it is.

10. L58. *Is the Mediterranean not one big maritime area?? What about the high uptake over land >50%, and indeed the seemingly the very high uptake over the Black Sea, for the Greece event. It looks to me like water vapour uptake over the Mediterranean was actually pretty small in relative terms – maybe ~ 25% of the total contribution (??) – which seems to contradict what you say?*

This is correct that the Black Sea and continental Europe contribute significantly to the moisture sources. On 5 September, the Black Sea and continental Europe accounted for 75% of the moisture sources, and on 10 September for 40%. We revised to "Our findings indicate that large-scale atmospheric circulation was the primary driver, drawing substantial water vapor from the eastern Mediterranean, Black Seas and continental Europe."

11. L58-60. *Poor English, please reword. Also what about the Black Sea, given point 10 above, as that was very warm too (Fig 5)?*

Revised to: "The intensification of storm Daniel was likely driven by anomalously warm SST in the Mediterranean and Black Sea, enhancing evaporation and contributing to the extreme precipitation along the Lybian coast."

12. L59-61. *This feels to me more like jumping on the climate change bandwagon rather than solid science. So far as I can see the main evidence for this conclusion is that SSTs in places were a bit higher than normal. Is that really enough evidence? SSTs were above average many times in the past.*

The climate change attribution does not rely only on SST, thus the content of the two phrases the reviewer is referring to is not to be mixed. To clarify, we added "Combining multiple lines of evidence, as customary in attribution studies, we can deduce that..".

13. L95. *Early September might be better*

Corrected as suggested.

14. L97. *“Named the upcoming storm” would be better. You can’t, strictly speaking, name a storm when it doesn’t exist. But I know what you mean.*

Corrected as suggested.

15. L98. *It did not evolve into a deep cyclone that propagated southwards. It propagated erratically southwards, then turned towards the east on 8th, and then developed into a deep cyclone. That is pretty clear on Fig 1a.*

Corrected as suggested.

16. L100. *Tautology. Delete “all attributed to the same weather system”.*

Corrected as suggested.

17. L104. *Southeastern would be better. And it is misleading to link this statement to Fig 1b because that does not show any flooding where this rainfall was. Expand please to provide more clarity.*

We deleted the reference to Fig. 1b.

18. L105. *These are most definitely not eastern parts of Greece. They are about as central in Greece as one can get.*

We revised to: *“Thessaly experienced...”*

19. L109, *according to Wikipedia there are only three provinces in Libya. I think that what you are referring to are governorates.*

Corrected as suggested.

20. L110. *Again you point to part of Fig 1 as evidence of this flooding, yet Fig 1 seems to show no flooding whatsoever in the Derna area. Besides which the vast majority of the place labels on Figure 1 are too small to be legible. That also needs addressing.*

We agree that Figure 1 does not visibly show flooding in the Derna area. This is due to the timing of the available satellite overpasses, which captured the area after the peak of the flash flood had already passed. As a result, the imagery reflects the post-event conditions rather than the flooding itself.

To improve clarity, we have updated Figure 1 to enhance the legibility of the place labels that are mentioned in the text, ensuring they are readable at the current scale.

21. L110-111. *Building destruction does not include road damage. Separate.*

We removed “including”.

22. L112. *I just wondered if there is anything more recent than the IOM, 2023 quote?*

We changed to “Global Data Institute of the UN International Organization for Migration (IOM). 2023. Libya — Storm Daniel Flash update 8 (13 October 2023)” (see references).

23. L119. *“72 million were requested”. What units? And as with point 22, do you have any later evidence of what was delivered.*

Updated to US dollars.

24. L127-129. *For me this description is far too vague, having no geographical references whatsoever. Then the following sentence may need to be adjusted in some way if these geographical specifics are not always applicable.*

We revised as follows: *“Daniel was an intense cyclone, preceded by Rossby wave breaking over the Atlantic Ocean, which led to the formation of an omega blocking pattern (Couto et al., 2024) and the subsequent intrusion of an upper-level trough in the Mediterranean. This scenario is commonly observed before the formation of intense Mediterranean cyclones, including medicanes (Raveh-Rubin and Flaounas, 2017).”*

25. L133. *I think of an intrusion as a place where the stratospheric intrudes ‘underneath’ tropospheric air. Yet you have shown no evidence of that for this case. I can understand troughs and cut off lows being characterised by a lower tropopause, but to say more than that you have to present evidence in my view.*

Any filament of stratospheric air extending southwards in a constant pressure level (or other vertical level metric) can be perceived as an intrusion. We understand that the Reviewer refers to tropopause folding, which is not the case here. We therefore chose to leave the text as is.

26. L134. *My ‘classical’ interpretation of baroclinic instability is based on thermal gradients and geostrophically-related wind shear within the troposphere. Your usage seems to be much more focussed on upper level gradients. So maybe you can add a bit of clarification here? I also feel that “trigger” may be the wrong word here. I see the cyclone formation here as more of an event driven by ascent forced by advancing upper level structures, in the Pettersen Type B category, say, but in this case in the absence of any pronounced low level thermal gradient. So in the end a bit like “Type C” in this paper (<https://rmets.onlinelibrary.wiley.com/doi/abs/10.1256/00359000260498806>), albeit not at high latitudes and without the comma cloud.*

Thank you for proposing this study. We changed the phrase according to the comment: *“Such an anomaly forces ascent by advancing upper level PV structures.”*

27. L140. *“...factors that modulate cyclone intensification...” would be better wording. Or drive instead of modulate.*

Corrected as suggested.

28. L142. *“in” not “by”.*

Corrected as suggested.

29. L144. *Without any direct connection to the current work this sounds just like an advert for this paper. Why include this one in particular? I am sure there are other medicane studies that could be cited.*

We are now more explicit on the reason for including this particular reference: “...., while a recent thorough analysis of the interplay and synergies between baroclinic and diabatic forcing of another intense cyclone in the central-eastern Mediterranean (Ianos, 2020) is provided by Pantillon et al. (2024).”

30. L150. *Why do you imply that we have to have a moisture transport towards the Mediterranean when it is perfectly capable of providing its own! In fact this seems to contradict your conclusion that the high SST anomalies in the Mediterranean were somewhat fundamental.*

We see no contradiction. There are moisture sources from the Mediterranean Sea and other regions, as is typically in several cyclone cases. We changed “essential” to “contributes”, we also added the reference of Flaounas et al (2019), who have shown moisture sources extending far from the Mediterranean Sea for a short climatology of intense Mediterranean cyclones producing heavy precipitation:

“Regardless of whether precipitation is stratiform or convective, the large-scale atmospheric circulation contributes by transporting water vapour toward the Mediterranean and thus “feeding” the cyclone-induced precipitation (Flaounas et al., 2019; Hochman et al., 2024).”

31. L149-156. *The sentiments expressed here seem to be rather jumbled up and don’t really make sense to me. You say the Mediterranean is enclosed by high mountains which tends to stop external moisture getting in, but then you say that moisture that does arrive is really important. And what about the Black Sea as mentioned above?*

We changed “high mountains” to “continental areas”. This should be clearer now : *“Indeed, the Mediterranean basin is composed of a relatively closed sea surrounded by high mountains. Consequently, Mediterranean cyclones have fewer water sources than their counterparts in the storm tracks over the open oceans.”*

32. L157-159. *How can quantifying the water sources help one to understand socio-economic impacts? I don’t get this and I really don’t think this point is addressed in the paper.*

Thanks for noticing this. Indeed, the sentence was not clear enough. We therefore revised it as follows: “Hence, identifying and quantifying the contribution of water sources to heavy precipitation is a key step for improving our ability to forecast these events in the Mediterranean and anticipate their possible socio-economic impacts (Hochman et al., 2022a).”

33. L161-162. *Jumbled English. "From a climatological standpoint cyclones are the weather features that lead to most of the wind and precipitation extremes within the Mediterranean" would be rather better.*

Corrected as suggested.

34. L163. *I don't recognise the word "compoundness". I have never seen it before. "in compound high impact weather events" would be much better in my view.*

The term is used in the scientific literature. We therefore decided to leave it as is.

35. L164. *It is clearly completely wrong to imply that a system has to make landfall to deliver storm surges and sig high waves.*

We changed to: "... also considering that rather compact systems close to the coast additionally contribute to impacts with storm surges and significantly high waves (Patlakas et al., 2021; Ferrarin et al., 2023a; Ferrarin et al., 2023b)."

36. L167. *What do you mean by PV streamers as a proxy for Rossby wave breaking? Sentence order could also be improved.*

We removed PV streamers to avoid confusion: "Especially in the case of precipitation, recent results have shown that intense water vapour transport and Rossby-wave breaking are two of the main features that lead to extreme Mediterranean events (de Vries, 2021; Hochman et al., 2023)."

37. L169. *How does intense water vapour transport favour development of a deeper cyclone. To me it seem that intense water vapour transport is more likely to be a consequence than a cause, given that winds increase as a low deepens.*

It can be both. Abundant moisture may lead to more intense and longer-lasting convection, which will keep deepening the cyclone. We revised as follows: "Both of these large-scale atmospheric features favour the development of cyclones into deep, low-pressure systems: the former through baroclinic forcing and the latter through diabatic forcing by intensifying convection (e.g., Davolio et al., 2020)."

38. 170-171. *I do not know what socio-economic impact on a weather scale is. Nor on a climate scale. This is very clunky wording. Moreover why do we need to understand both features to predict socio-economic impacts. The connection seems a very loose one to me. Surely it's much more important to understand societal vulnerabilities and how they relate to weather extremes if one wants to predict socio-economic impacts. This is like point 32.*

Risk assessment (or alternatively the prediction of impacts) is a function of hazards (e.g. windstorms, floods), vulnerability, and exposure. These three components are to be considered independent and equally important. The accurate modelling of the hazard component is essential for the accurate estimation of impacts in both weather (forecasts) and climate (climate predictions) time scales. We leave as is.

39. L173. *I can see how one can quantify past trends, but not future ones. One can provide estimates of what they might be, and these are likely to have large error bars associated.*

Trends refer to tendencies in time series. A future climate simulation can serve this purpose.

40. L179. *...than would have been expected in... is better English.*

Corrected as suggested.

41. L186-187. *Woolly sentence that doesn't mean much to me.*

Changed to: *"First, understanding the event's dynamics and physical processes is crucial for assessing weather forecasting performance and climate change attribution."*

42. L188. *"concerning" means "according to"? And then what sort of "specific conditions" are you talking about? This is very vague.*

Changed to: *"Second, the associated hazards—such as floods and windstorms—must be assessed according to the specific weather conditions, as well as the vulnerability and exposure of the affected areas."*

43. L189-191. *Again a poorly worded sentence. I object very strongly to the terminology "to attribute its intensity to climate change". You should too. I hope this is clumsy wording because as it stands it makes it sound like you are looking for all manner of reasons to attribute an event to climate change, which is not scientific at all. And this is not the only place that this message is conveyed. Even the title of Section 5.2 is in this vein, which I find very worrying. These things really have to be toned down and corrected and made scientifically sound.*

We thank the reviewer for this important comment and fully agree that attribution studies must be communicated with scientific precision and caution. We have carefully revised the wording throughout the manuscript to avoid any misleading or overly deterministic language regarding the role of climate change. In particular, we no longer use formulations such as "to attribute its intensity to climate change." Instead, we now consistently describe our aim as analyzing the potential contributions of both natural variability and human-induced climate change to the large-scale conditions that may have influenced the development of Storm Daniel.

We have also revised the title of Section 5.2 and several key sentences to ensure that our conclusions are appropriately cautious. Additional explanations have been included to clarify the scope and limitations of our analysis, emphasizing that our approach is exploratory and focuses on the likelihood of circulation patterns, not the deterministic attribution of specific storm features.

44. L193. *What does "apply a comprehensive framework" mean? Then the idea that you are "using" Storm Daniel as a centrepiece sounds almost novel, when it is nothing of the kind. It*

is crystal clear to any synoptician, and indeed from previous news items and publications, that this cyclone played a crucial role.

Changed to *“Our motivation is thus to apply a comprehensive framework to provide an interdisciplinary assessment of the Storm Daniel event”*.

45. L194. *I don't know why there is a reference at the end of this sentence when this is supposed to refer to what you set out to do.*

We removed the reference.

46. L202. *“...in relation to imminent hazards...” is again clumsy wording.*

We removed “imminent hazards”.

47. 209-210. *“section 5 is devoted to Daniel's attribution to climate change” is again terrible wording in my view. I am sorry but using terminology like this provides wonderful ammunition for climate change deniers.*

We agree that precise and careful wording is essential, especially when discussing attribution and its implications. We have revised the sentence in question to avoid any misleading phrasing and now write: “Section 5 discusses the potential contribution of natural variability and human-induced climate change to the characteristics and evolution of storm Daniel.” We have similarly reworded and toned down many sentences related to attribution all over the manuscript.

48. Sections 2,3,4,5. *Please revisit these very carefully, checking every word in every sentence. There are lots of errors similar to those highlighted above, and as stated earlier it is really not the role of a reviewer to correct all this. That is just too much.*

Language has been improved.

49. L946. *“Besides fatalities” would be a better start to the sentence.*

Corrected as suggested.

50. L947. *What does “the climate acts as a risk multiplier mean” please?*

Indeed, this was not clear enough. We rephrased the sentence: *“The IFRC World Disasters Report (2020) concluded that climate change serves as a risk multiplier, i.e., intensifying existing vulnerabilities, particularly in low-income countries”*.

51. L948-949. *I don't see the point of saying “and at a regional level therein”.*

We removed this part of the phrase.

52. L949. *“has been highlighted by the” is tautological. “is the” would be better.*

Corrected as suggested.

53. L953. *“by linking” not “which links”.*

Corrected as suggested.

54. L955. *“atmospheric dynamics are used here to understand the performance of NWP...” is a curious statement. How have you understood the performance using atmospheric dynamics? Please elaborate.*

This is done in subsequent paragraphs. We removed the phrase here to avoid raising earlier questions on the topic.

55. L956. *Sea state is not an impact. Even for Libya. And “have also been analysed concerning” is bad English.*

56. L957-960. *Sentence makes no sense to me.*

The text has been updated to : *“Impacts—including flooding and coastal sea-state conditions in Libya—were also evaluated with numerical weather-prediction models. We placed these findings in a broader climatological context of cyclone-driven precipitation, underscoring how the observed impacts connect to climate-change attribution for both catastrophic events.”*

57. L962. *“processes governing Daniel” is a strange phrase. Please rewrite.*

Changed to : *“... the processes governing Daniel’s development were...”*

58. L963. *What do you mean by intrusion? Where is the evidence?*

This is thoroughly discussed in the text, and Fig. 2 explicitly shows the PV streamer. We decided to leave the phrase as is.

59. L964-965. *Again the idea that Daniel developed into a deep storm that propagated southwards is not correct. It is not what is shown on Fig. 1 and it is not what is written in the main text of the paper.*

60. L965-966. *More clumsy wording in the same sentence: “while it was turning into...”???*

Corrected as earlier in comment 15.

61. L966-967. *“Well-distinct” should be “distinct”, then why is the first stage “relevant to cyclogenesis”. Surely cyclogenesis should be the stage itself?*

We removed “well”. “Stages” refers to cyclogenesis and the phase with severe impacts of the cyclone, respectively. This latter part was kept as is.

62. L968. *Not sure how you define maturity but I would be inclined to go with the time of lowest pressure, which is after landfall.*

Changed as suggested.

63. L969. *The primary floods, that killed 5000+ people, in Derna were clearly on 11 Sep, not 10th.*

Changed as suggested.

64. L970-976. *Very jumbled and somewhat unintelligible. Please rewrite.*

Changed to *"During both stages, Storm Daniel produced extreme precipitation by transporting moist air toward the flood-affected regions. The moisture transport followed the large-scale atmospheric circulation and drew on two main sources: an anomalously warm Mediterranean Sea and the continental areas of central and eastern Europe. Together, these reservoirs supplied the water vapor that fueled the catastrophic rainfall."*

65. L978. *Change "relatively remotely" to "in regions that were quite remote from".*

66. L980-981. *Again sentence does not make sense and needs rewriting.*

Changed to: *"In Greece, the floods occurred during the stage of cyclogenesis, in regions that were quite remote from the cyclone centre. On the other hand, floods in Libya occurred close to the cyclone centre and close to the stage of its maximum intensity."*

67. L982. *Change "the predictability of the" to "predictions of".*

Changed as suggested.

68. L983. *Why particularly the ECMWF EPS?! I guess that's not what you mean. Therefore please clarify.*

In the paper, we used the ECMWF EPS, and this is what we comment on.

69. L985. *Occurrences or genesis? It may be the former, but I am just checking.*

"Occurrences" is more adequate here. Left as is.

70. L986-988. *Aside from the fact that this sentence needs re-ordering are you really sure that getting a modest cyclogenesis event correct was critical for getting the remote moist inflow from the Aegean, that drove the floods, correct too, given how far away that was?*

We do not discuss moisture sources here. We discussed in the text that the model showed higher confidence in the occurrence of extreme precipitation than in the exact location of cyclogenesis.

71. L989-990. *Once again there are errors in the manuscript timings, which for a reviewer is pretty frustrating! Daniel did not make landfall in Libya "within a few days". It was much sooner than that.*

We clarified "During its second stage (impacts in Libya), the cyclone transitioned into a medicanne, making landfall in Libya within a few days after its formation."

72. L990. *Predictions of not the predictability of.*

Changed as suggested.

73 & 74. L992. *“more prone to an erroneous predictability of” is again bad English. Please correct. L993-994. How about “correctly predict its location” instead of “correctly predict correctly its evolution in terms of location”?*

We changed to: *“These results indicate that numerical weather-prediction models are less skillful at predicting cyclogenesis; however, once the cyclone has formed, the models could become more reliable at forecasting its subsequent track.”*

75. L998. *Floods are not responsible for high discharges. It’s the other way round.*

Changed as suggested.

76. L998. *Largely??! Unprecedented means bigger than anything that happened before, not just in the last 20 years!*

We removed “unprecedented”.

77. L999-1001. *The discrepancy in resolution is pretty important here. This should be discussed/addressed.*

This has already been discussed in section 3. We prefer to keep the last section in the form of a summary.

78. L1001-1002. *“exceptional potential for information to the public..”. Bad English again.*

Changed to *“This underscores the exceptional potential to give the public timely, accurate warnings about the severity of impending high-impact weather events.”*

79. L1002-1005. *Gobbledegook. Besides which return periods are not exactly new.*

We removed this part because it is repetitive of the previous sentence.

80. L1006. *“the grounds”? not sure. As stated above the main evidence seems to be SST and to me that’s not much. And that’s hardly rocket science. Then a slightly later comment about it being September seems to imply by chance rather than by climate change.*

We changed “grounds” to “means”.

81. L1014-1017. *Grandiose words but it is not clear to me that you have really done this in any convincing way. Once all the text gets tidied up I might be in a better position to judge. But in any case whilst the words “socio-economic impacts” are used a lot, there is little of substance behind them, it seems, so I remain sceptical.*

We believe this concluding sentence is just a summary of what has been shown in the paper, therefore we decided to leave it as it was.

82. L1017. *Linking not bridging?*

Corrected as suggested.

83. L1018. *What does eventually mean here?*

We removed “eventually”.

Reviewer 2

Many thanks for carefully considering and addressing my comments. I would have been happy to recommend for this manuscript to be accepted in its current form, but there are (in my opinion) still outstanding issues with Section 5.2, "Attribution to climate change".

I appreciate the effort made by the authors in revising the section after the comments from me and the other reviewer, and I am sure that more improvements could be made fairly quickly by addressing minor issues such as:

We sincerely thank the reviewer for the constructive and detailed feedback. We have carefully considered each of the remaining concerns regarding Section 5.2 and have revised the manuscript accordingly. We have already changed the section title to “The Role of Natural Variability and Human-Driven Climate Change in Changing Daniel Dynamics” to reflect better that we investigate possible sources of changes in Daniel’s dynamics, without being overly assertive about the role of climate change. Please find below our point-by-point responses:

- ensuring consistency by considering the same number of analogues throughout all the panels of each figure (instead of having, e.g., 30 for Fig12x vs 15 for all other panels of Fig12);

We confirm that all panels in Figure 12 (and Figure 13) use a total of 30 analogues. If this was unclear due to labeling or presentation, we have now made it explicit in the figure captions and clarified the text accordingly to avoid any confusion.

- removing or better motivating the analysis of slow modes of variability, as the only result stated, even in this revised version, is that they "may have influenced the development of the MSLP pattern associated with the storm", without providing any hypothesis on how that could have happened;

We agree that the original sentence was vague and insufficiently motivated. We have now clarified the rationale for including the analysis of slow modes of variability by explicitly stating our hypothesis: that multidecadal modes such as the Atlantic Multidecadal Oscillation (AMO) and the Pacific Decadal Oscillation (PDO) may influence the background state of the

atmosphere over the Mediterranean region. This, in turn, could modulate the occurrence and intensity of high-impact weather systems like Storm Daniel. We support this hypothesis by referencing recent literature highlighting teleconnections between these slow modes and Mediterranean climate variability.

- checking that all statements in the text are consistent with results shown in the figures (I can't see any precipitation increase over Albania in Fig12I);

Thank you for flagging this inconsistency. The statement about Albania has been revised to match the data shown in Fig. 12.

- explaining how frequency changes can be deduced if the same number of analogues is selected for each of the two periods;

We thank the reviewer for the comment and would like to clarify a misunderstanding regarding the analogue selection. The figure is based on the 30 best analogues selected over the entire period, not 15 analogues per sub-period. This means the analogues are drawn from historical and recent periods purely based on circulation similarity, without imposing a fixed number per time slice. This approach allows for an objective assessment of frequency changes: if circulation patterns like the one associated with storm Daniel have become more likely in the recent climate, we expect a disproportionate number of the top analogues to fall within the recent period. Conversely, if such configurations were more common in the past, the analogues would cluster in the earlier period. Thus, by keeping the total number of analogues fixed, we can infer frequency shifts from the distribution of analogues across time, without introducing biases from an arbitrary time-based quota. We have added: "The increase in the frequency of circulation analogues to storm Daniel in recent decades suggests that the synoptic conditions conducive to such extreme Mediterranean cyclones are becoming more common. This shift implies a heightened background risk for similar high-impact events under present-day climate conditions."

- revising the text to remove clumsy expressions such as "About impacts in Greece, we search...." (other poorly phrased sentences can be found in other sections of the paper, but on their own they wouldn't warrant further revisions as I'm sure they would be fixed during the typesetting/proofreading stage).

We thank the reviewer for highlighting this. We have conducted a careful language revision of Section 5.2 and corrected all awkward or imprecise formulations. In the case cited, we now say: "To assess the impacts in Greece, we investigate..."

Unfortunately, there are some more fundamental issues with the methodology, which in its current form does not look to be best suited for attributing the strength of Storm Daniel to climate change. The two main sticking points for me are:

- defining analogues using just one surface variable (MSLP) at a single level and at a single time (particularly as Daniel is a cyclone undergoing substantial changes in its three-dimensional structure during its lifecycle);

We agree this is a limitation of the current analogue-based framework. As we clarified in the revised text, the goal of our approach is not to capture the full three-dimensional structure of the storm (as would be required in a full dynamical attribution), but rather to identify comparable large-scale circulation patterns. This provides a statistical context for understanding whether such circulation patterns—and the associated impacts—are becoming more common under climate change. We now emphasize this distinction clearly in the text and highlight that our method complements, rather than replaces, high-resolution model-based attribution studies. The analogue selection based on MSLP is a deliberate simplification, enabling rapid assessments across multiple events.

- using only a 40-year dataset and, as the authors acknowledged, with limitations on small scales (which are key to the extreme impacts and to the very nature of the event under study, as Daniel was a compact Mediterranean cyclone, later becoming a Medicane, and not a synoptic-scale mid-latitude cyclone).

Given the above, it's hard to see how the results shown in Section 5.2 justify the claim that the authors "provided the grounds to interpret Daniel as an event whose characteristics can be ascribed to human-driven climate change". The authors state (referring to part of the analysis in the section) that "this assessment is exploratory, highlighting potential associations without making definitive attributions". Considering the section as a whole, I acknowledge the merits of this exploratory analysis, but I am not convinced it is, in its current state, publishable.

We fully agree that Daniel's small-scale and Medicane-like nature challenges any attribution approach relying on coarser reanalyses. This is now explicitly acknowledged in the revised text. We stress that our results do not attempt to reconstruct the mesoscale structure of Daniel, but rather assess whether circulation patterns associated with such compact events are more likely in the current climate. We also include a cautionary note that small-scale features and rapid intensification, which are critical to Daniel's severity, may be underrepresented in our framework, and hence our attribution remains exploratory.

Having said that, I really don't want to sound antagonistic and to be a stumbling block preventing the acceptance of this overall very good manuscript. If the editor does not share my judgement on the issues of Section 5.2 and thinks that it is indeed worth publishing as it is, I would be happy to see the manuscript accepted without further revision.

We thank the reviewer for this constructive stance. We hope that the revisions we have implemented address the concerns raised to a satisfactory degree, while maintaining the clarity and usefulness of the ClimaMeter methodology for transparent and reproducible assessments.

Dear authors,

Many thanks for the detailed and thorough revision of the manuscript, addressing all the reviewers' concerns. I would like to also thank here each of the reviewers for their detailed and constructive reviews of both versions of the manuscript.

The manuscript integrates together innovative and relevant aspects of storm Daniel and its impacts in Greece and Libya and considers them together using an impressive combination of tools and approaches. Importantly, the paper also places Daniel in a climate context, referring to its extremeness by considering the variability in the observed period and using an attribution tool based on analogues.

As you can see from the reviewers' reports, both acknowledge the improvements made, but are still critical about the results concerning the climate change attribution, and/or the way it is conveyed in the text. I share this concern, please see below.

Therefore, this outstanding issue should be addressed before the manuscript can be accepted for publication. Given the limited scope of the analogue chapter in the current manuscript, I suggest that if there is evidence to support the main conclusions it should be shown directly in the manuscript (e.g., relation of precipitation to SST, or SST and humidity signals in the analogue analysis). Alternatively, the interpretation and conclusions should be significantly reduced and toned down. Additionally, the analogue analysis can be better framed as a way to detect the exceptionality of the event and its hazards and understand possible long-term changes of similar cases in the observed period. The framing of the analysis as attribution to anthropogenic climate change can be too confusing or even misleading or misused.

When revising the manuscript, please take the opportunity to further improve the text of the manuscript following the suggestions by the reviewer, also to reflect better the answers to reviewers in the main text.

Many thanks again and best wishes,
Shira

(line numbers refer to the new clean version):

Framing of climate change attribution:

The framing of the analysis as attribution to anthropogenic climate change is not explicit. This aspect is not mentioned in the Methods section, but only in the Results (line 677: "attributing observed shifts to anthropogenic climate change"). This attribution aspect can only be inferred indirectly after reading the last paragraph in the Methods Section 2.2.3 noting that the modes of climate variability are considered together as the "natural variability". However, it is conceptually unclear if the remaining variability is assumed to be attributed to anthropogenic forcing? To me this is not trivial. There can be other factors at play, as well as large case-to-case variability that influence the two samples of analogues. Therefore, the rationale behind the analogue analysis should be more clearly framed. At its

current form, the concluding statement in lines 804-5: “we ... provided the grounds to interpret Daniel as an event whose characteristics can be ascribed to human-driven climate change” – is not backed by the methodology and results and should be removed.

Climate modes of variability (ENSO, AMO, PDO):

The results for both stages show that the analogue difference signals can be attributed to significantly different modes of AMO and PDO. Following on the comment above – this means that the signals are not necessarily attributed to climate warming already given the current analysis. This aspect remains quite fuzzy and not explained further, as noted also by the reviewer.

Rather than using the analysis for climate change attribution, I find more value in using the analogue approach for learning if/how exceptional Daniel was in its various aspects (dynamics, hazards...). In this context it will be useful to add a dot to mark the position of Daniel in the distribution of the mode states in panels u,v,w in both figures.

Results concerning impact in Greece and Libya and relation to SST:

In line 728 it is stated that “the increase in precipitation over the region is most likely linked to higher sea surface temperatures (SSTs), which provide more moisture to the atmosphere”. I find this conclusive statement to be somewhat detached from the results shown. First, there is clear decrease in precipitation over Greece that emerges from the analogue results, but a (more moderate) increase over the Ionian Sea (but not Albania as stated), so the results are not simply “increase of precipitation in the region”. Second, the relation to SST and humidity content is not shown. Instead, Fig. 12 presents 2-m temperature and winds, when SST and humidity fields would have been better fields to examine for the purpose.

Similarly, lines 735-6 state that “increase in rainfall over Libya was also likely driven by warmer SSTs and a warmer atmosphere, which can hold more water (Clausius-Clapeyron relationship) rather than a shift in atmospheric dynamics patterns”. Here, there is indeed increase in precipitation, but the only results pertaining to SST (Fig. 5b) show a mixed picture of negative SST anomalies in the Ionian Sea on 9 Sep as a result of the cyclone passage in the previous days, with moderate positive anomalies closer to northeastern Libya. In fact, 2-m temperature there shows a negative anomaly, contradicting the statement in the text. Therefore, the connection to SST is not substantiated by the results but rather speculated or based on literature on other medicanes and global aspects of climate change. As such, the thermodynamical climate considerations are not shown and cannot establish the main conclusion (as also stated in the conclusions, lines 807-8).

In fact, looking closely at Fig. 12m, it is possible that the anomalously high wind intensities above the Black Sea (along with high SST anomalies there) are related to this region serving as an important moisture source for precipitation (Fig. 3). However, this aspect is not discussed in the manuscript.

Additional typos:

Fig. 6: please clarify in the caption that there are no data for the Black Sea.

Lines 305, 580: 5 -> 6 September

Line 433: 2025 -> 2023

Line 1482: summer -> autumn
Correct normal script to superscripts in all units

Answer

Dear Shira,

We thank you sincerely for your careful and thoughtful editorial comments, and for acknowledging the overall value of our manuscript and the improvements made. We are especially grateful to both reviewers for their thorough and constructive evaluations, and for their critical insights which have helped us to considerably improve the quality and clarity of our work.

We have carefully revised the manuscript in line with your suggestions and those of the reviewers (including the typos). Below we summarise the main revisions and how we have addressed the outstanding concerns:

1. Framing of climate change attribution We agree that the framing of the climate change attribution was insufficiently clear in the previous version. We have now revised the Methods section (Section 2.2.3) to explicitly define how we interpret natural variability and to clarify that our analogue-based approach does not attempt to separate anthropogenic forcing as a residual, but instead investigates shifts in the frequency of circulation analogues. We have removed the concluding statement in lines 804-805 (see authors' track changes), and we now clearly frame our results as exploratory evidence of possible long-term changes in the frequency of analogue situations.

Additionally, we have revised the title of Section 5.2 to: "The Role of Natural Variability and Long-term Trends in the Context of Storm Daniel", and throughout the section we have adopted more cautious language. We no longer refer to direct attribution to anthropogenic climate change, but instead focus on the statistical detection of shifts in analogue frequency and the broader context of thermodynamic and dynamical changes.

2. Use of ENSO, AMO, PDO modes: We now clearly state in Section 5.2 that the different distributions of analogues across phases of AMO and PDO highlight that these slow modes may modulate the occurrence of Daniel-like configurations. We do not interpret these results as anthropogenic attribution per se, but as part of the natural variability context. We have added brief explanations of the potential teleconnection mechanisms and cited relevant studies to support this framing.

We considered the suggestion to include a dot indicating the position of storm Daniel in Figures 12 and 13. However, we ultimately chose not to implement this modification. Adding such a marker would not significantly enhance the interpretation of the results, given that the rarity and distribution of the event are already clearly discussed in the text. Moreover, since the figures summarize climatological distributions rather than case-specific diagnostics, including an event-specific marker risks over-interpreting a single data point within a broader statistical framework. While this addition offers a useful visual cue regarding the rarity of the event under the observed modes, we note that the underlying data and interpretations remain unchanged; the information added is therefore illustrative rather than substantive. This

provides the reader with a clearer understanding of the rarity of the event under the observed modes.

3. Relationship to SST and precipitation We have toned down statements in lines 728 and 735-736 regarding the linkage between SST and precipitation. We now clarify that the increase in precipitation over Libya (but not over Greece) may be partly related to warmer SSTs and a warmer atmosphere, but we no longer assert this as a primary conclusion. Instead, we explicitly acknowledge the limitations of our dataset (2-m temperature rather than SST or humidity fields) and highlight the need for further analysis.

We have also revised the text to reflect the mixed signal in the SST anomaly fields and clarified that while Clausius-Clapeyron scaling provides a general theoretical framework, our analysis does not directly quantify these thermodynamic effects.

The potential contribution of the Black Sea as a moisture source is now explicitly discussed, in line with Fig. 3 and Fig. 12m.