The responses to the reviewer are below in blue.

The authors have addressed most of the reviewers' comments, and the manuscript has improved accordingly. I do think the paper adds useful information to the extreme event attribution community, particularly given the need for multiple methodological approaches to improve comparability across studies. However, I consider that the concerns raised by reviewer 1 have only been partially addressed, particularly regarding the framing of the study.

In its current form, I think the manuscript does not yet clearly position its added value relative to the existing literature. Given the growing number of studies analysing this event using different methodologies, a key contribution of this paper should be a more explicit comparison with previous work, as well as a clearer articulation of the research question and the specific advantages and limitations of the approach used here. Rather than following a structure similar to earlier studies, the manuscript (in particular, the introduction and discussions sections) would benefit from being framed more explicitly as complementary to the existing literature.

I hope these comments are taken as constructive suggestions aimed at improving the clarity, visibility, and overall contribution of the manuscript.

We sincerely thank the reviewer for the constructive assessment of our manuscript. We appreciate the recognition that the study provides a useful contribution to the extreme event attribution community, particularly in the context of the growing need for complementary methodological approaches.
Following the reviewer's suggestions, we substantially revised both the Introduction and Discussion sections to more clearly position our study within the existing literature, and to better articulate the specific research questions, methodological scope, and limitations of our approach.
We also revised the Abstract to clarify the main scientific focus of the study from the beginning, now explicitly stating that the work aims "to assess the influence of anthropogenic climate change on the synoptic-scale thermodynamic evolution of the event" using "high-resolution (~9 km) storyline simulations from the European Union's Destination Earth initiative".

In particular, the revised manuscript now:
(i) includes a more explicit review and comparison with previous attribution studies of the event;
(ii) clarifies the complementary nature of different attribution methodologies used for this particular event;
(iii) better motivates the focus on the thermodynamic and longer-term synoptic evolution of the event within a storyline framework; and
(iv) expands the discussion of methodological opportunities and limitations associated with spectrally-nudged coupled global simulations.

We are grateful for these suggestions, which we believe have significantly improved the clarity, framing, and overall contribution of our manuscript to the existing literature.

Introduction:

As several studies have already addressed this event, the introduction should more clearly distinguish between what is already known about the influence of climate change and what remains uncertain. I suggest restructuring the introduction to summarise existing findings (including a brief explanation of the methods used in previous studies and the research questions addressed, which has not been done yet), and then clearly state the remaining gaps and how this study addresses them. Also, because a detailed description of the event is already provided later in the manuscript (results section), the introductory description of the event should be shortened; I would merge and shorten paragraphs 1 and 2 and remove paragraph 3. The emphasis should be on the novelty of the approach and the specific gap addressed, rather than on the event itself.

In addition, paragraphs 4 and 5 would require further adjustment rather than simply being moved earlier in the text. In particular, the discussion of the storyline approach in paragraph 5 appears redundant, as it is already introduced in paragraph 4 together with related methodologies.

So, to summarise, I suggest structuring the introduction as follows: very brief description of the event (one short paragraph). What do we already know about the influence of climate change on this event? (including a brief overview of previous studies and the methods used). What do we still not know, and how does this study address these gaps?

We thank the reviewer for this constructive suggestion. We agree that, given the growing number of attribution studies addressing the October 2024 Valencia event, the Introduction needed to more clearly distinguish between (i) what is already known regarding the influence of anthropogenic climate change on the event, (ii) the remaining scientific uncertainties, and (iii) the specific contribution of our study within this broader attribution context. Following the reviewer's recommendation, we substantially restructured the Introduction to better frame the manuscript as complementary to the existing literature rather than as an isolated attribution analysis.

More specifically, the original Introduction has been reorganised into six substantially revised paragraphs:

Paragraph 1 now provides a concise description of the Western Mediterranean context and the event, merging and shortening material previously distributed across former paragraphs 1 to 3. As suggested by the reviewer, the detailed meteorological description of the event was removed from the Introduction and reserved for the Results section.

The new paragraphs 2 and 3 were completely rewritten to provide a structured overview of previous attribution studies on the event. These paragraphs summarise the methodological approaches used in previous studies, such as probabilistic, circulation-conditioned, storyline, and pseudo-global-warming approaches, the research questions they address, and their partly differing but complementary conclusions regarding the influence of climate change on the event.

Paragraph 4 focuses on the remaining scientific gaps and the specific motivation for our study. In particular, we clarify that previous studies have mainly focused on precipitation outcomes, event probability, or storm-scale thermodynamic amplification, whereas less attention has been paid to the spatiotemporal evolution of the synoptic-scale thermodynamic environment before and during the event.

The description of the storyline framework was also substantially revised to avoid redundancy with the previous attribution overview. Rather than introducing storyline approaches in general terms, the revised text now specifically motivates the use of spectrally nudged kilometre-scale coupled global simulations within the DestinE ClimateDT framework to investigate these unresolved aspects. These methodological aspects are described in paragraph 5.

Finally, paragraph 6 more clearly defines the specific research question addressed in this study and explicitly positions our work within the growing body of complementary attribution analyses of the Valencia EPE.

Overall, we believe these revisions substantially improved the framing and structure of the Introduction in line with the reviewer's recommendations, with a stronger emphasis on the complementarity of attribution approaches, the remaining scientific uncertainties, and the specific added value of our process-based storyline framework.

Discussion and conclusions:

This section would also benefit from clearer positioning relative to previous studies. I recommend starting with a more direct comparison with earlier work, highlighting what different approaches have shown and how the present study contributes to or differs from them. This would help clarify the added value of the study. The physical interpretation of the results can then follow this comparison. Hence, keeping a similar structure to the introduction, but with more direct comparison and discussion.
Furthermore, the manuscript would benefit from a more detailed discussion of the limitations of the approach used, as well as a clearer identification of remaining gaps that should be addressed in future attribution studies.

We thank the reviewer for this valuable suggestion. We agree that the previous version of the Discussion section might not sufficiently position our results relative to the growing body of attribution studies on the event, particularly given that several relevant studies became available during the reviewing process.
Following the reviewer's recommendation, we substantially reframed the Discussion section to better situate our study within the emerging multi-method attribution literature.
More specifically, the original Discussion section consisted of seven paragraphs primarily organised around the presentation of our own results (synoptic description, methodological aspects, thermodynamic amplification, and precipitation response). In the revised manuscript, the Discussion has been restructured into nine conceptually reorganised paragraphs with a clearer progression from comparison with previous studies, to physical interpretation of our results, to methodological opportunities, limitations, and future research needs.

In the revised Discussion, paragraph 1 now provides a concise contextualisation of previous attribution studies on the Valencia EPE, summarising what is currently known from different approaches. Paragraph 2 then presents the objective of our analysis and the simulations used, highlighting the use of kilometre-scale, spectrally nudged global DestinE ClimateDT simulations to examine the spatiotemporal evolution of the synoptic-scale thermodynamic drivers of the event.

Paragraphs 3 and 4 discuss our estimates of thermodynamic amplification of the synoptic-scale drivers and compare them directly with previous studies. Paragraph 5 discusses the nonlinear precipitation increase over Valencia and its consistency with previous storyline-based estimates, while paragraph 6 provides a physical interpretation of the super-Clausius-Clapeyron response.

Finally, paragraphs 7-9 discuss the broader opportunities offered by the DestinE ClimateDT simulations, expand the limitations of our approach, and identify remaining gaps for future attribution studies.

We think that these revisions substantially improved the structure and framing of the Discussion section, placing stronger emphasis on methodological complementarity, process-based interpretation, limitations of the current framework, and future research directions within the broader extreme event attribution community.

Other minor comments:

- Please justify the use of a 1950 baseline climate. This choice should be better explained, and its limitations discussed (e.g. why not use a pre-industrial baseline such as in DAMIP, which may provide a more appropriate reference for assessing climate change?). I guess this is because of the design of DestinE, but this should be acknowledged. In addition, line 166: 1950 does not represent a pre-industrial climate. Line 167: please clarify which forcing is being referred to (radiative forcing?).

We thank the reviewer for this comment. We agree that the choice of a 1950 baseline climate requires further clarification and discussion. This setup follows the experimental design of the DestinE ClimateDT storyline simulations (Doblas-Reyes et al., 2026; John et al., 2026). The use of a 1950-like baseline reflects a compromise between physical consistency, computational feasibility, and the availability of long-coupled storyline simulations. We additionally clarify that our results should be interpreted as quantifying the sensitivity of the event to the transition from a cooler mid-20th century climate to present-day conditions, rather than as a strict pre-industrial attribution statement.

We have revised the manuscript to clarify that the Counterfactual experiment does not represent a fully pre-industrial climate, but rather a cooler mid-20th century climate state with radiative forcing from ~1950 using CMIP6 historical simulations. In John et al. (2026), who provides the technical description of the simulations, this scenario is referred to as an early-industrial climate, so we would like to use the same terminology in our manuscript for consistency.

We have clarified in the revised manuscript that “forcing” refers to the radiative forcing conditions following the CMIP6 historical and SSP3-7.0 scenarios used in the IFS-FESOM storyline simulations (John et al., 2026).

Accordingly, new L144-148 now read:
- Counterfactual (cooler ~1950s) – representing an early-industrial-like state based on 1950 boundary conditions with CMIP6-historical radiative forcing. In this experiment, the forcing is held fixed at its 1950 values for the entire 2017-2024 simulation period.
- Factual (present-day) – representing current conditions (2017–2024) forced with the SSP3-7.0 scenario. In this case, the external forcing is transient and evolves year by year.

- Lines 161–172: please clarify what is meant by “boundary conditions.”

We thank the reviewer for pointing this out. We agree that the term “boundary conditions” could be ambiguous in the context of a global coupled model configuration.
The term “boundary conditions” follows the terminology adopted in the original description of the DestinE ClimateDT storyline simulations by John et al. (2026), who designed and performed the experiments used in this study, to refer to the background climate state and the radiative forcing (i.e., greenhouse gases, anthropogenic aerosols, etc.) used in each scenario.
To maintain consistency with the original experimental framework while avoiding ambiguity, we have clarified the text to explicitly specify what is meant by “boundary conditions” in the context of these coupled global simulations.

In new L141-143: The scenarios differ in their ocean initial states and boundary conditions (i.e., external climate forcing and background climate state), allowing the evaluation of how identical meteorological events would evolve under different climate states

- Lines 177–181: the description of the nudging procedure for the ensemble members needs clarification. In particular, please specify more clearly when nudging is switched off and reactivated, and over which exact period the model evolves freely.

We thank the reviewer for this question. We have revised the description of the ensemble generation procedure to clarify more explicitly when spectral nudging is disabled and reactivated, as well as the exact duration over which the model evolves freely in each ensemble member. We now also reference the lag-based ensemble methodology following Brenowitz et al. (2025). The reason for choosing this procedure over the more traditional approach of adding a random perturbation to a model variable is technical: due to computational constraints, the original simulation was initialized with only a single ensemble member in 2017, and the lag-based method provided the simplest way to generate additional ensemble members.

The new L154-162 now read:

To ensure robustness in the differences between the Factual and Counterfactual experiments, we use four additional ensemble members for each variable, branched from the main simulation. Following the lag-based ensemble approach of Brenowitz et al. (2025), spectral nudging is temporarily disabled at the beginning of the simulation period for different durations in each member before being reactivated using the July 2024 restart files. Specifically, nudging is switched off from 1 July 2024 for 1 day in member #1, 2 days in member #2, 3 days in member #3, and 4 days in member #4, after which nudging is re-enabled for the remainder of the simulation period. This procedure allows the model to evolve freely for a short period, creating slightly different atmospheric states that stabilise during the subsequent nudged period and provide a representation of internal variability. The limited ensemble size is primarily due to the high computational cost of conducting global km-scale simulations.

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