This study performs a process-understanding projection of Storm Boris, with a very interesting approach that combines climate and weather information. The paper also shows the potential of a methodology to disentangle the dynamic and thermodynamic signals of climate change on specific events and assess their contributions separately. This stands in contrast to other methodologies that either ignore the dynamical information of the event or constrain it so tightly that part of the signal is lost. I very much enjoyed reading the paper; the structure and language are very clear and easy to follow.

A concern relates to the model performance in simulating mesoscale extreme events within this type of cyclones. Other studies using the same model (Dolores-Tesillos et al. 2023; Karwat et al. 2024; and from some of the authors themselves Binder et al. 2024; Joos et al. 2024) have raised questions in this regard. I understand that, as the authors state, this work can be seen as a "meta-*attribution*"* study rather than a definitive *attribution* analysis. However, I think it is important to discuss whether CESM is suitable for simulating this type of storm. My understanding is that the model does not reproduce weak cyclones very well, especially in the Mediterranean. Could this limitation influence your results? Also, more generally, what are the limitations of using a single model, and how do the authors justify using a high-end, unrealistic, rcp scenario (Hausfather & Peters 2020)?

*I understand that the authors apply a methodology within the framework of extreme event attribution and therefore repeatedly frame the study in terms of "*attribution*." However, in my view this may be somewhat misleading for communication purposes, and in several places, I would suggest rephrasing in terms of "projection." In the attribution literature, "counterfactual" typically refers to a climate that could have occurred (without anthropogenic forcing) or that might occur under d2ifferent forcing conditions, while "factual" refers to the presetn climate. For projections, the concept of "future counterfactuals" is also used (e.g., Ermis et al. 2024). It is therefore confusing to use "counterfactual" for the present climate and "factual" for the future climate (e.g., lines 136–138). A clarification of terminology here would be very helpful.

Other points that would benefit from justification or clarification:

- Choice of the domain. It seems quite large; why do you need to include the main PV reservoir over Scandinavia? As you state in the discussion, the domain can strongly influence results. Have you performed sensitivity tests, e.g. focusing only on the cut-off low?
- I understand (and like) the use of PV as the first step to define the event. However, as the authors state, most analog studies use slp or geopotential height, and storm Boris had a peculiar track of the surface cyclone. Have you considered, for comparison, first identifying analogues of SLP (or Vb tracks, as in Ginesta et al. 2024, using the same dataset) and then constraining them with a cut-off low based on PV? Do you think this would yield more/better (or fewer/worse) analogues, or perhaps a stronger precipitation correlation?
- Am I correct in assuming that you compare with Z500 analogues here because the SLP signal of this cyclone is too weak for a meaningful comparison?
- Can you justify the choice of the domain of target/analogue slp identification (red box)?
- Line 328: should analogues also be defined at the time of maximum precipitation intensity, or is this criterion applied only for Boris itself?
- In the discussion/conclusion you argue that simulating Boris in a future climate while nudging the present-climate PV structure would yield an unrealistically strong precipitation event, so nudging frameworks should be applied with care. I agree. However, isn't that precisely the purpose of some storyline approaches—to strongly constrain dynamics in order to isolate the thermodynamic response? I see your point that a full picture of climate-change effects requires considering both dynamics and thermodynamics, but I think it would be worth clarifying how you distinguish your approach from these alternative frameworks.

Typos:

Line 315: Fig 2a. I think is Fig. 2b

Line 322: reduction → difference?

Line 328: is 'close' referring to the target region?

Line 333: Fig S3 I cannot identify where storms are born; is it possible to highlight cyclogenesis?

Line 580: 'that emerges yearly analogs' → emerges *when* yearly

Links to references

Hausfather & Glen P. Peters, 2020 https://www.nature.com/articles/d41586-020-00177-3

Dolores-Tesillos et al. 2022 https://wcd.copernicus.org/articles/3/429/2022/

Karwat et al. 2024 https://journals.ametsoc.org/view/journals/clim/37/4/JCLI-D-23-0160.1.xml

Ermis et al. 2024 https://iopscience.iop.org/article/10.1088/2752-5295/ad4200

Ginesta et al. 2024 https://journals.ametsoc.org/view/journals/clim/37/21/JCLI-D-23-0761.1.xml