

**Review of « Marine heatwave amplifies extreme multi-hazards of extratropical cyclone Babet ». Goswami et al. 2025.**

**General remarks**

This is a well-structured, clearly written, and overall good manuscript. The methodology is coherent, and the results are interesting, particularly given the growing importance of understanding the role of MHWs in modulating atmospheric extremes. The study addresses a timely question.

While the paper is strong overall, I provide below several suggestions that could improve clarity, precision, and interpretation. The explanation of boundary-layer processes in Section 4.1 is particularly strong and one of the highlights of the manuscript.

**Structure and Methods**

The manuscript is generally well organized. However, the Methods section could likely be improved, particularly in the detailed description of model components and coupling strategy. While transparency is appreciated, some of this information may be better summarized or moved to supplementary material to improve readability.

In addition, in the methodology, I wonder why a daily climatological SST (e.g., 1982–2011) was not prescribed in the forecast model as a reference baseline (with a call back at the daily step). Including such a reference could help better isolate the role of the MHW relative to climatological conditions and clarify the interpretation of anomalies.

Although 2020 exhibits values close to climatology in some respects, it lies within the 2003–2022 period, which already includes a substantial contribution from human-induced warming, aerosol forcing, and multidecadal internal variability.

For this reason, I am not fully convinced that the term “counterfactual” is appropriate. The experimental setup does not represent a pre-industrial or no-anthropogenic-forcing world, but rather a world without the specific MHW anomaly superimposed on an already warmed background state.

It may therefore be more accurate to refer to this as:

- a “no-MHW experiment”,
- a “climatological SST experiment”,

Finally, the manuscript (L426) refers to “attributing” the event. This is not strictly an attribution study in the formal D&A framework. This word could be changed to avoid confusion.

**Interpretation of the MHW Signal**

The manuscript sometimes attributes increased hazards directly to the MHW itself, whereas in practice the impacts likely arise from:

- the thermodynamic background SST anomaly,
- modification of air–sea fluxes,
- boundary layer preconditioning,

- or the interaction between internal variability and anthropogenic warming.
- can we disentangle the role of pre-moistened air parcels (L309 mentions already humid air masses)?
- L346–347: Could the MSLP gradient play a role? Is it possible that the MHW modifies the pressure gradient and thus influences wind intensification dynamically, beyond purely thermodynamic effects?

This raises a key conceptual question: Are the hazards driven by the MHW as a discrete event, or by the elevated background SSTs and associated thermodynamic state?

Moreover, it seems that the Clausius-Clapeyron is not valid over UK (at least for the summer months) :

[De Leeuw, J., Methven, J. and Blackburn, M. \(2017\) Physical factors influencing regional precipitation variability attributed using an air mass trajectory method. Journal of Climate, 30 \(18\). pp. 7359-7378. ISSN 1520-0442 doi: <https://doi.org/10.1175/JCLI-D-16-0547.1>](#)

The manuscript would benefit from explicitly discussing whether it is possible to disentangle the MHW contribution from seasonal and thermodynamic effects on the atmosphere.

Furthermore, a previous MHW occurred in June. Could accumulated heat or persistence effects have contributed to the atmospheric response later in the season? The cumulative aspect is not discussed and could be relevant.

### **River Discharge**

The relationship between rainfall and river discharge may not be linear, especially under saturated soil conditions. Rapid saturation due to both intensity and high cumulative rainfall can lead to enhanced runoff. Therefore, the link between MHW influence and river discharge is not direct over the full length of the event.

### **Discussion about large-scale dynamics and climate change**

- The discussion could be strengthened by addressing the influence of climate change on baroclinity, jet stream variability, and North Atlantic internal variability.
- The concept of preconditioning could be emphasized more clearly.
- In Section 5.3, it should also be discussed that in future experiments, internal variability patterns may remain structurally similar even if thermodynamic conditions shift. This assumption deserves explicit acknowledgment.

### **Minor comments**

- Marine heatwaves are sometimes abbreviated (MHWs) and sometimes written in full.
- MLD is inconsistently formatted.
- can you specify if possible when you mention surface air temperature when considering surface temperature related to the atmosphere?
- L416: “the role of MHWs” → perhaps “the effects of MHWs” would be more precise.
- L340: “stronger winds” → clarify whether this refers to mean wind speed, gusts, or both.

- L338–339: It would be helpful to provide quantitative values (e.g., correlation coefficients, regression slopes) to support the stated relationships.
- Figures 5 and 6: The detailed description of air-mass trajectories is interesting, but these figures might potentially be moved to the Supplementary Information if space is needed.
- L366: “atmospheric intensification” → specify which variables are concerned (rainfall rate, wind speed, pressure gradient, etc.).
- OHC appears rather abruptly in L431–435 and L466. The link between OHC and the rest of the argument is not entirely clear. If OHC plays a mechanistic role (e.g., through enhanced flux persistence or deeper mixed-layer anomalies), this should be better integrated earlier in the discussion.
- L438 It is not entirely clear that the experiment constitutes a true analogue in the dynamical sense. The term might overstate the methodological setup and could be clarified.