

Manuscript: “Seismo-acoustic and GNSS monitoring of a record-breaking storm in the Black Sea: Evidence of climate change and intensifying natural hazards” (egusphere-2025-1842)

This manuscript presents a multi-sensor case study of the August 2024 extreme storm that impacted Romania’s Black Sea coast. By comparing observations with ERA5 reanalysis, the authors analyze signals from broadband seismic records, infrasound, GNSS, and satellite lightning. While the multi-sensor approach is potentially valuable, in its current form the manuscript requires major revision. In addition, the title “evidence of climate change” is not supported by analyses conducted in this manuscript.

Major comments

1) Title and scope

The title “Evidence of climate change and intensifying natural hazards” is not demonstrated by the current work. This study is essentially a single-event case study, and robust statements about climate change require, for example, long-term trend analysis, formal attribution methods, or counterfactual assessment. If the authors do not include such analyses, they should reframe the title and key concluding statements to match what the paper actually provides.

While multi-sensor observations can be valuable, the manuscript should clarify what benefit is gained. From a single case (or short time window), it is not possible to claim improved accuracy, robustness, or “early warning” capability without validation against longer historical observations and quantitative evaluation across multiple storms. Overall, the tone of the manuscript is frequently too strong for the presented evidence and should be rewritten with more careful and appropriate claims.

2) PWV results

(a) Units for PWV

PWV is a column-integrated quantity, typically reported in mm, not “mm daily” or “mm hourly” unless the authors are explicitly presenting a rate of change (I think mm daily/hourly does not make sense). The manuscript reports PWV values such as >900–950 “mm per day” and “44 mm/hr”, which are physically implausible for PWV as normally defined. In Figure 12, these values are compared with ERA5 humidity, but with the current units/magnitudes this comparison is not meaningful. The authors must verify the PWV derivation, units, and labeling.

(b) Figure 12 inconsistency

Figure 12b is described as an hourly comparison for MNGA vs hourly ERA5 at a specific grid point, while Figure 12a shows daily PWV for multiple stations with daily averaged ERA5. However, the ERA5 values shown in (b) do not appear compatible with those shown in (a), even allowing for hourly vs daily averaging. Please check and verify the ERA5 extraction method, spatial sampling (grid point vs area averaging), and aggregation procedure, and ensure consistency across panels.

3) Results are over-generalized from a short window

Across Sections 4.1–4.3, the analysis is essentially a single-event, short-window demonstration (focused largely on 29–31 Aug, with selected “quiet days” for contrast). Nevertheless, the manuscript repeatedly uses language implying general reliability (e.g., “reliable tool,” “early warning,” “precursor,” “can serve

as...”). The authors should avoid general claims and rephrase the Results/Discussion so that conclusions are appropriately limited to what is supported by this case study, unless additional multi-event validation is provided.

4) Infrasound clustering

The manuscript states that K-means was performed with seven clusters in the Methods, but the Results describe the data being separated into six groups. Moreover, unsupervised clustering can be sensitive to feature scaling, feature choice, k-selection, and initialization (depending on random seeds), yet the Results mainly interpret clusters qualitatively. The authors should provide additional methodological details (e.g., normalization/scaling, cluster selection criterion, stability across seeds) and/or add quantitative support for the physical interpretation of clusters.

5) Microseism interpretation

The text attributes storm-time changes in the secondary microseism band to “wind-induced pressure fluctuations in the shoaling seafloor.” While such an interpretation may be plausible, the manuscript does not provide enough evidence to support it. To justify this claim, the authors should either (i) provide stronger supporting analysis (ideally using longer periods and/or independent wave parameters), or (ii) rewrite the interpretation more cautiously and clearly, with appropriate references and uncertainty acknowledged.

6) ERA5 “high-resolution”

The manuscript refers to ERA5 as “high-resolution” and states it has 1-hour temporal and $0.25^\circ \times 0.25^\circ$ spatial resolution. In a relative sense this is higher resolution than older global reanalyses, but the authors should avoid implying that ERA5 resolves convective-scale storm structure or peak precipitation intensities. A 0.25° grid corresponds to $\sim 25\text{--}30$ km and remains too coarse to represent many coastal events. Please clarify what “high-resolution” means in the manuscript’s context and discuss how ERA5 averaging may affect comparisons to point measurements.

Minor comments

Figure 11

There appears to be a mismatch between the plot and caption regarding the distance metric (“distance from coast” vs “distance from Constanța”). Please clarify and make the labeling consistent.

Figure 12

- No legend/colorbar is provided to explain the colored circles.
- PWV units/magnitudes appear implausible and require verification.
- The ERA5 series appears inconsistent between panels (a) and (b); this is not explained.
- Clarify whether panel (a) uses the same ERA5 variable and spatial sampling approach as panel (b) (e.g., same grid point vs area average).