

The authors have addressed my primary concern by including a paragraph discussing the differences with the study that introduced the original methodology, as well as by making minor revisions throughout the text that clarify the objectives. I think the aims of the study are now clearer, and these changes have strengthened the manuscript. I have a few remaining comments:

- (Regarding the robustness of the results to internal variability): Some analogue-based studies assess modes of internal climate variability to quantify the extent to which the findings can be attributed to anthropogenic climate change versus internal variability. For example, if most analogues occur during El Niño conditions in the historical climate but during La Niña conditions in the future climate, ENSO could be explaining an important part of the observed changes. Finding a similar distribution of analogues across different phases of key internal variability modes would increase confidence that the results are primarily driven by anthropogenic climate change (if this is indeed the aspect being tested). I do not think such an analysis is required for the present study, although it would further strengthen the manuscript and represent an improvement over the original methodology proposed by Ginesta et al. At a minimum, I would recommend mentioning in the discussion that internal variability has not been assessed in depth and that future studies could explore this aspect.
- (Regarding the use of an absolute intensity threshold to identify extreme analogues based on ERA5): If the percentile used to define extreme analogues is adjusted separately for the historical and future climates, then I agree that the analysis essentially compares the total number of storms in each period. However, if the historical threshold is retained and future extreme analogues are identified relative to that fixed threshold, then the comparison is no longer equivalent. Again, I am not suggesting that the authors should adopt this alternative approach; rather, I encourage them to justify their methodological choices more thoroughly and to discuss the associated limitations clearly in the main text.
- Lines 475–477: Please clarify why a direct comparison with Ginesta et al. cannot be performed. The methodologies remain very similar, with the main difference being the way the number of analogues is identified. Although the studies examine different storms, I think the comparison is still informative and relatively straightforward.
- Line 214: I do not see why this approach necessarily provides a “cleaner” insight into future frequency changes. Rather, it relies on a different set of methodological choices, each with its own assumptions and limitations, and therefore offers complementary insights rather than a cleaner perspective.