Reviewer 1

Major comments

1. Please structure the manuscript in a more appropriate format for a scientific paper. It is not usual to have different methodologies as sections with their own Data and methods, Results and Discussions subsections. I understand that the authors might find it as a way to separate them and be clearer about each of them, but as a reader, it is not ideal. I could advise following Barriopedro et al. (2025), to re-structure this manuscript into a more suitable article format considering multiple attribution approaches.

We will restructure the article in line with this suggestion, with separate Methods and Results sections containing subsections for each approach.

2. The reader shouldn't feel lost when trying to identify the key results. In Section 5, what the authors describe as Conclusion and Discussion is not clear, and separated in different subsections that are not targeted to the main results, but to the use of FAR in the probabilistic attribution and the indirect attribution of impacts.

We will also restructure this section into separate 'Discussion' and 'Conclusions' sections. The former will provide an overview of the main results from the different hazard sections and the considerations for attributing impacts (see also the response to point 4). The final conclusions will then present a summary of the study as a whole, including the multi-method approach and the overarching findings and implications.

3. In the conclusions, the authors argue that "The circulation analogues analysis in section 2 does not provide a probabilistic assessment of hazards and is therefore not combined in the quantitative conclusions of this study." and very briefly mention its contribution in the conclusion with a sentence. If the results of this part of the study are not so relevant to be included in the manuscript's conclusions, why does it have a full section? Even though the analogues do not provide a probabilistic attribution, they present a quantitative attribution of the change in magnitude of events associated with a similar atmospheric circulation and show what can be expected in future climates. This should be discussed.

The reviewer is correct to point out that we could add more discussion around the assessed change in magnitude and its implications now and in the future. In line with this and other comments, we are going to add further analysis into this section. In particular,

we will conduct a similar analysis using ERA5 data, using the same approach but also probing the sensitivity of our findings to the choice of analogue selection criteria (for instance, using spatial correlation vs. Euclidean distance, the domain for finding analogues and accumulating the precipitation, the number of analogues used etc.). We will then use the findings of this sensitivity test to inform the analysis of HighResMIP climate models, combine both the ERA5 and HighResMIP results, and discuss the findings in more depth. This should both provide a novel addition to the wider field of flow analogues, lend robustness to the findings, and provide a more comparable result to the probabilistic approach by combining both observation-based and climate model results. We will then integrate this more clearly into the discussion.

4. It is also puzzling, the emphasis given to the attribution of impacts in the conclusions, when only an indirect connection can be assumed through the change in likelihood of the associated extreme weather conditions studied in this research. The conclusion should be targeted at the main outcomes and results obtained. It is a nice add-on but deserving just 1 or 2 paragraphs (not a subsection) of what can be expected in terms of impacts in the case that there is an intensification and higher recurrence of events like the Super Typhon Odette.

As part of the restructuring for point 2 above, we will shorten this piece and remove the separate subsection. However, we will also add more context for the discussion of impacts with a line or two in the introduction, explaining that we do so because of the value added by this multi-method approach considering the multiple compounding hazards of storms like Odette.

5. It is good practice to be transparent about uncertainty. However, some of the results presented raise questions regarding their interpretability and usefulness. Several estimates exhibit extremely large uncertainty ranges, which substantially limit their relevance. For example, the abstract states that the likelihood of Typhoon Odette has increased by “about 2 (0.1 to 290)”. If the event was historically a 1-in-100-year occurrence, the median estimate implies an increase to a 1-in-50-year event. But the uncertainty range also includes outcomes ranging from a 1-in-1000-year event (meaning that due to global warming it could also be significantly much rarer) to approximately a 1-in-4-month event (much more frequent), which makes the result difficult to interpret. The authors should therefore better justify the presentation of such results or provide additional discussion to clarify their practical meaning.

We agree that such wide uncertainty ranges are difficult to interpret. We will provide a larger focus on the more confident qualitative results that are able to combine not just the analysis done here by physical theory and other literature, as well as a clearer comment on statistical significance.

General comments

We will address all general comments. Point 6 particularly will be addressed in the restructuring described above.