Reviewer 2

Major comments

- Analogue-based results are still not accounted for in the discussion. This approach follows a report style. My old comment: “You bring together three different approaches, which is a strength of the paper; however, the analogue-based approach is not thoroughly considered in the final discussion, where instead a reader is expected to see a comprehensive discussion that integrates the different results.”

In line with this and other comments from both reviewers, we are going to add further analysis into the flow analogues 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 size of 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 in line with this comment.

- There is a very large uncertainty in the results from the probabilistic approach and inconsistency across the results based on the two different periods (December and JuneDecember), as also noted in my previous comments and in the text. I see well that the uncertainty is noted in the text, but this should be accounted for more prominently in the conclusions/abstract as it is difficult to draw robust conclusions on cimate change effects.

While the uncertainties are large, we believe that by combining methods as well as physical theory and other literature, the statistical significance itself does not prevent conclusions from being drawn about the (qualitative) role of climate change for precipitation, and the quantitative role when combining the multiple arguments including the wind speeds analysis. The addition of a more robust analysis of analogues in particular will help to underpin this argument further. We will draw this conclusion out more clearly in the text while also making it clearer that estimating the quantitative influence (at least for precipitation) is highly uncertain.

- Accordingly, several of my main concerns at the first round of revision remain not well addressed. (These are indeed related to major comments 2-5 of the new first referee.)
- Given the above, please ensure that all my previous comments have been considered (I understand we can disagree on some points).

We will also return to the previous set of comments and address these.

Other comments

- L60, so so there is no debate about changes in translation speeds *at a fixed location*, which is what matter for accumulated rainfall?

We understand the broader point that translation speed affects accumulated rainfall at a given location. However we unfortunately do not fully understand the reviewer's meaning here or the implication for the text in its current form, as we believe this point is captured.

- L127, I generally agree, but it is also argued that analogues allow for inspecting changes in dynamics for a given circulation condition, e.g. intensity of a storm.

We will mention this similar application for flow analogues.

- L274, "statistically", any statistical analysis is statistics, you mean you look at the probability, probably.

We will make this change.

- L277, make it clear you are using the same box as in the previous analysis, otherwise one wonders how results are comparable. Older comment: Succinctly clarify to the reader why models are needed. This is crucial for the reader to accurately interpret the net results. In principle, if one can derive observation-based estimates, one does not need models. Here, I think there is a matter of internal variability and sample size that somehow makes the observation-based estimates partially unreliable for the purpose of disentangling human-induced climate change, but it is not explained and remains unclear to people who are not familiar with WWA practice. Clarify this scientific aspect. -> I see some hints on this in the results

We will make it clear that this box is the same, and further clarify that models are needed to provide multiple realisations of the climate – in doing so making it possible to attribute any

detected trend (from observations) with a specific cause (human-induced warming). We had previously added the following text (Lines 397-401) to address this comment:

"Furthermore, such analysis of observations alone constitutes a trend detection rather than an attribution, as it represents only a single realisation of the changing climate. Therefore, to elucidate the attributable link between anthropogenic climate change and events like Typhoon Odette, the results from observations for each event definition are synthesised with the respective results from climate models. This gives an overall estimate of the influence of anthropogenic climate change that is not as heavily dependent on individual data sources."

- L437 onward, until the end of the section. There is a very large uncertainty in the results and inconsistency across the results based on the two different periods (December and JuneDecember), as also noted in my previous comments and shown in Figures 7 and 8, yet the author concludes that climate change increased the frequency of the events. I see well that the uncertainty is noted in the text, but this should be accounted for more prominently in the final message of the paper, See, for example, my previous comment: "It seems hard to make a simple likelihood statement—as you note around L502 and given the systematic model-observation discrepancy for the December case. Yet at L500 you report a clear figure (a doubling of likelihood, stated also in the abstract) based on June-December statistics. You then invoke physical understanding, which is, however, directly relevant to event magnitude under specific circulation patterns (cyclone), but not directly to likelihood (i.e., how often such cyclones occur), which remains more uncertain."

We stand behind our conclusion that climate change increased the frequency of events like Odette. In line with our response to Major comment 2, we will shift the focus towards qualitative result that climate change amplified the rainfall but will also underline the uncertainties around the quantification of this, particularly for precipitation. However, within the probabilistic framework that assess classes of events like Odette the magnitude and frequency framings are related on – if the magnitude of comparable events is increased, then the likelihood of exceeding such a magnitude as observed during Odette is also increased. We also have several complementary lines of evidence to reinforce this conclusion, from the probabilistic and flow analogue analyses to the evidence from physical theory and related literature.

- L 460 onward until the end of the section, it seems largely discussion material.

We agree and will integrate this into the new ‘Discussion’ section as part of addressing Reviewer 1’s comments.

- L497 Older comment: “Observed historical TC tracks between 1980 and 2021 from the IBTrACS database are stochastically perturbed by the model.” Does it mean that the approach is still conditional, that is, build an ensemble by assessing how the given observed cyclone tracks could have tracked differently (in space and intensity), but not whether different cyclones than those in IBTrACS could have formed due to large-scale internal varibaility? Please clarify, including how this affects the interpretation of the attribution statements (probability ratio).

We will clarify that this is indeed the case, but that over the ~42 years of IBTrACS data used, most modes of internal variability are covered, and that a large ensemble from ~10000 years of data with stochastic perturbation from the point of lifetime maximum intensity should well represent this.