Review of "The influence of anthropogenic climate change on Super Typhoon Odette (Typhoon Rai) and its impacts in the Philippines" by Ben Clarke et al.

I am the anonymous referee #2, who provided detailed suggestions to the previously rejected version of this manuscript. I have not received a point-by-point response to those comments, which is understandable and makes sense given that this is a new submission.

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."
- 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 June-December), 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.
- 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).

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?

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.

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

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

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 June-December), 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."

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

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).