<u>Response to Reviewer 1's comments</u>

We thank Reviewer 1 for the time to go through our manuscript in details. This manuscript describes a new and efficient method to produce a physical TC event set in the western North Pacific basin. In general, reviewers think after careful revision, the results of this study is of great interest and relevance, and it will be a useful contribution to the field of TC occurrence risk assessment. Here is our point-to-point response to Reviewer 1's comments.

<u>Response to Major Issues</u>

You have explained that you only used 6-hourly surface wind speed (i.e., lines 67-69 and discussed in Section 4.2 as well), is your risk assessment model able to comprehend a full TC risk? What about TC-rainfall (you only mentioned about it in the last 2-3 sentences in Conclusions) and TC-induced storm surge, which are more significant than winds in many regions of the world. For example, the recent Hurricane Florence in USA and Mangkut in South China caused significant damaged due to surge and rainfall, respectively. Therefore, under such circumstances, what is the applicability of your proposed approach? You need to think about the generalization, replicability, and adoptability of your approach from a wider perspective and not only from the study area.

We are thankful for the reviewer's comment to the overall impact of Typhoons and their differentiated reasoning in form of the different meteorological variables concerned. In this study, we present a method, which addresses the basic, critical issue in typhoon risk assessments – a robust methodology to determine the real frequency of a tropical cyclone (TC) occurrence with high socioeconomic impact *potential*. We are doing so by automatically identify and tracking severe, damage relevant tropical cyclones in a data set being representative of several thousand years of "observations", the TIGGE archive. Based on the large amount of data from the different multi-model multi-member ensembles to be analysed, in this study, we use thus a computationally inexpensive approach: by applying an impact-oriented tracking algorithm WiTRACK (Leckebusch et al., 2008; Kruschke, 2015; Befort et al., 2020) on multi-model ensemble forecasts to generate a large, physical consistent TC event set. This *identification* of major events is thus based on one meteorological variable only (wind speed), but is capable of identifying events of *general* loss relevance. We recently demonstrated in Befort et al. (2020) that the tracking algorithm used in this study (WiTRACK), can automatically identify the relevant (over 90%) of TCs with high overall socioeconomic impact (e.g., above 3,000 million RMB or 440 million US$ to mainland China). This implies the event set generated by our approach is in principle suitable for general TC risk assessments, as well as for an assessment of the hazards frequency-intensity distribution specifically. As we fully agree with Reviewer 1 that TC-rainfall and TC-induced storm surge are also important factors in assessing the full impact (risk) of TC to society, we would like to point out that the events identified and used in this study are including those where potential loss is caused not only by the direct wind force but also by secondary natural hazards, e.g. flooding by precipitation, or potential storm surge by the combined wind and pressure impact.

Therefore, the applicability of our proposed approach is directly visible and fundamental. While studies like e.g. Sajjad and Chan (2019) are focussing on an overall assessment of integrated risks (hazard x vulnerability/resilience; further comments to this topic, please find below) based on observed tracks from only some 60-70 years (of inhomogeneous data of different quality), our approach utilizes data (in this case multi-model synoptic forecasts) representative of ~10k years. To quantify robustly the probability of occurrence of e.g., a 100-year event from 60-70 years of observations is widely impossible (because it may potentially not have realised during this short observational period). Consequently, such an approach is not used e.g., in industry applications and is only of limited use for risk assessments as the **underlying** hazard component probability distribution is not sufficiently known.

The reviewer mentioned the topic of "*generalization, replicability, and adoptability*". We fully agree and this is exactly the reason why we developed this approach.

- By using an impact-based identification of the natural hazard relative to the datasets typical characteristics (i.e., the 98th percentile of surface-near wind speeds), the assessment of the hazard is accounting of potential biases in the data set under investigation (e.g., AOGCM, NWP, or RCM simulations) and is linked to overall losses out of those events relative to the climatological distribution of these data sets.
- By utilizing ensemble forecasts (i.e., TIGGE's unrealised TC events), we secure the statistical robustness of our intensity-frequency distribution assessment to create an event-set being representative of the risk of occurrence of events of potentially damaging characteristic (so called tail events).

We have demonstrated the applicability and transferability of this approach in several studies for different hazards and different regions (e.g. Leckebusch et al., 2008; Nissen et al., 2014; Osinski et al., 2016; Befort et al., 2015; Befort et al., 2016; Befort et al., 2019; Walz and Leckebusch, 2019)

A more in-depth investigation of the contribution of different drivers (e.g., extreme precipitation and storm surges) of loss and damage during a (non-realised) severe TC event would be beyond the scope of this study. Nevertheless, once a representative TC event set is derived, which provides robust information of the frequency of high impact TCs, the impact of extreme precipitation and storm surges could be integrated e.g., following the approach developed and published by us in Befort et al. (2015). **We have included the above information in Sect. 5 summary for clarity.**

Recently, Sajjad and Chan 2019 and Sajjad et al. 2020 proposed typhoon risk frameworks based on TC hazard (wind-based similar to yours), vulnerability, and disaster resilience, which provides a comprehensive information on TC risk. They found that the Pearl River delta region in Guangdong (area primarily mentioned in your case, Line) is a statistically significant hotspot of TC risk. How do you see the usefulness of your method for such frameworks? A thorough discussion regarding this is necessary

Similarly, most of the discussion in the manuscript revolves around TC-hazard and neglects the vulnerability and resilience within the regions where TCs are making landfalls. For instance, you say on Lines 236-238 that overall impacts of a storm is related to many factors such as size, duration, and intensity. However, the impacts are not only related to TC-

associated factors but vulnerability and resilience are also integral parts of overall impacts and risks associated with TCs, as discussed in Sajjad and Chan 2019 and Sajjad et al. 2020. How do you incorporate these characteristics within the TC risk discussion of yours?

We think Reviewer 1 may have misread the purpose of our study because we do not share the same definition of the term "risk". The term "risk" in this study refers to the possibility of occurrence of an event (e.g. Vickery et al., 2000; Emanuel et al., 2006), as outlined in the title. In the context of the papers cited by Reviewer 1, this manuscript focuses on the hazard component in the framework of a classical impact modelling and assessment approach. Please note that in terms of an impact modelling approach, the components would be the hazard (and its risk of occurrence), the vulnerability (which would include measures of resilience), and the exposure of values at risk. **We have replaced the term "risk" by "hazard" in relevant places in the manuscript to make this point clearer**. To avoid confusion, hereinafter, we use the term "hazard" to represent the possibility of occurrence of a natural event; and the term "risk" to reflect a systemic integrated perspective of resultant impacts by the combination of information about the hazard, the vulnerability, and the exposure. This terminology would be more similar to the approach used in Sajjad and Chan (2019) and Sajjad et al. (2020), and will also account for the common practice in industry applications (e.g., CAT models).

Additionally, you need to detail the current limitations of your method. For example, how well this method could perform at higher resolution assessments, which are more important for policy and decision-making in the context of DRR efforts? What are the future prospects of your study?

Many thanks for this comment. In comparison to other methods to generate large TC event sets, our specific approach is **limited** mainly by the source of data used. The current TC event set constructed on synoptic scale forecasts archived in TIGGE, is strictly spoken representative only for the current climate state. Any longer-term climate variability (e.g. multi-decadal fluctuations like the PDO) and their impacts on any TC frequency-intensity distribution are not accounted for in this setting. Nevertheless, the presented approach would be equally applicable to data sets representing that kind of variability on longer time scales (e.g. decadal predictions or transient climate model simulations). Another limitation is obviously that we do not account for a direct assessment of the damage (loss) contribution of individual meteorological variables (e.g., precipitation leading to flooding, as mentioned above). **We added a specific section on limitations in Sect. 5 [Lines 440-446].**

It is not fully clear what Reviewer 1 refers to as "**higher resolution assessments**". The TIGGE archive provides forecast data on a spatial scale (~0.56° - 1.25°), which is not matched by any other data source of comparable length (equivalent to 10k years of observations of TCs). Further, we intentionally linked our (forecast) model based assessment to in-situ point observations from stations: the ultimate downscaling test. As we have demonstrated in section 4.3, one of the potential applications of our event set is to improve the return-period/return level calculation of the wind hazard at the local scale. Wind speed values are used in practice to decide on payments out of e.g. parametric insurance products (Swiss Re, 2016). Consequently, reliable wind-based trigger points of typhoon parametric insurance can be determined. This will further improve the suitability and flexibility of parametric insurance for DRR applications. Ultimately, this will improve the speed of post-disaster recovery. **We added a respective paragraph to the discussion [Lines 359-363, Lines 468-476]**.

With regard to **future prospects** of this study, we discuss this in Section 5. In particular, unlike event sets generated from a stochastic approach, the TC-associated precipitation field is simulated directly by the model. This means a more complex compound TC hazard assessment, as mentioned above (limitations), can be done as well in principle. The event set that we have constructed contains all necessary information for applications in the DRR context. Once robust trigger points for the local hazard are available (including their uncertainty), the targeted application of parametric products in disaster relief application is possible. Especially, when it comes to the evaluation of the basis risk (the risk not covered by payments out of a parametric product). This study is merely the first step toward a statistically robust, full physical model based TC hazard assessment. **We added a respective paragraph to Sect. 5 [Lines 468-476]**.

Specific Comments
Lines 10-13: *The sentence is long and it is difficult to follow. Would be better to break it into two sentences, if possible.*

We thank Reviewer 1 for pointing this out, **we have modified this sentence accordingly**.

Line 22: *There is no such thing as "natural disaster" but only natural hazards. Disasters always involve human agency. Therefore, please avoid using this term and check the manuscript thoroughly for this issue. For further details, you are encouraged to see https://www.undp.org/content/undp/en/home/blog/2017/5/18/Natural-disasters-don-t-exist-but-natural-hazards-do.html*

Many thanks for pointing this out. Although we agree in principle that the disaster aspect includes a men-made perspective in its impact on human influenced structures the personal position expressed in this blog is document of a narrow understanding and perspective of nature. A natural hazard can be a disaster for the environment, even without human influences. Following good scientific practice, we prefer to use peer-reviewed literature for scientific studies and not personal comments. We used the phrase "natural disaster" as a generic term to indicate a natural event with sudden, large negative economic or environmental losses. The definition of "disaster" that we used here is similar to the definition stated in the IFRC webpage (see https://www.ifrc.org/en/what-we-do/disaster-management/about-disasters/what-is-a-disaster/). We are not trying to argue whether "natural disaster" exists or not as this is beyond the scope of this study. However, the use of this phrase is in line with literature, e.g. Cavallo and Noy (2011), Smith and Matthews (2015), Ye et al. (2016), Bakkensen et al. (2018). Nevertheless, to avoid any confusion and as we are in general agreement with the reviewer **we rephrased line 22 to: " … lead to an increase of risk to humans and for economic loss potentials from natural hazards e.g., tropical cyclones, with potentially disastrous**

consequences." We also checked the whole manuscript and corrected for a more precise use of the terminology in the revised manuscript.

Line 34: *you mean "livestock"?*

We thank Reviewer 1 for pointing this out and the reviewer is correct. **We have corrected this**.

Lines 145-149: *It is mentioned that VIF is used to resolve the issue of collinearity and 17 variables are selected to construct the final LRC model. How many total variables were included initially? Are the VIF values for all of these 17 variables less than the normal threshold (i.e., VIF ≤ 7.5)? It would be useful to add the VIF values of the final variables in Table 3.*

The list of variable initially used is presented in Table 2. We have modified the caption of Table 2 for clarification.
Yes, those 17 variables stated in Table 3 have VIF value of less than 5. We are not sure whether including the VIF values of the final variables would be useful for this manuscript. However, Reviewer 1 pointed out that we did not state the criteria which we used for the variable selection.
We have included the criteria (i.e. VIF < 5) for the variable selection in the manuscript. Line 176-177: "*Variables with VIF value larger than 5 are excluded*."

Lastly*, a thorough intermediate level editing is recommended to remove "several" grammatical and language errors throughout the manuscript.*

We thank Reviewer 1's advice, and **we have edited the manuscript accordingly**.

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