Response to Referee

Dear Referees,

We would like to reiterate our thanks for your time and helpful comments. We did our best to take them all into account in this new version.

In the present document we provide a detailed point-by-point response to all referee comments and specify all changes in the revised manuscript. Our response to the Referees is structured as follow: *(1) comments from Referees,* (2) response to interactive discussion, (3) our final changes in manuscript. When we refer to a line, we refer to the file with the track of the modifications.

REFEREE 1

RC1.1: *It is not clear that this tropical cyclone model leads to accurate forecasts of storms. Changes in wind patterns can have no effect in this model. Is the cyclone model in this paper as accurate as the models developed by Emanuel? Or is this model a step backwards?*

AC1.1: Interactive discussion. We reiterate that building precise cyclone forecasts is not the aim of CATHERINA. We propose an algorithm designed to assess the future cost distribution for country-level damage assessment. The cyclone intensification process used is inspired from STORM model (Bloemendaal et al., 2020), which includes a single climate variable, and extended following Holland (1997) and Emanuel (1988) to encompass 2 more variables. We found that this extension provides statistically significant instrumental variables in the description of tropical cyclone intensification, which is the aim of the algorithm. Another step forward in our methodology is the use of state-of-the-art bias correction module for integrating climate model projections.

Consequently, even if some thermodynamical processes have been simplified in this approach, our approach is still a step forward with respect to the existing state-of-the-art in the context integrated assessment model (IAM) for climate impact analysis. Indeed, our approach can integrate any CMIP simulation with limited set of available variables (only a few vertical levels, some only available at monthly time scale, with some variables not always available). The adaptability of our algorithm to any CMIP exercise and simulation comes with a constraint implying necessary simplifications. Our approach combined with a bias correction module makes our algorithm easy to implement, more sophisticated in terms of processes included with respect to existing IAM and bias-corrected. We will add this explanation in the section 3.2.3 of revised version of the manuscript.

AR1.1: Final response: As mentioned in the interactive discussion, we added disclaimers in the introduction (texdiff file line 79) and conclusion (line 668) to reiterate the main objective of the paper. We also reiterated the initial objective of creating signals material for financial practitioners in the abstract (in a bottom-up fashion) (texdiff file line 10).

<u>RC1.2:</u> *The estimates of the effect of each hurricane are crude. The model assumes that all damage is from wind whereas only 40% of cyclone damage is wind related. Another 40% of cyclone damage is from storm surge. But storm surge strikes largely just the coastline. The remaining 20% of damage is from excess precipitation which often falls far from where the cyclone strikes land.*

<u>AC1.2: Interactive discussion</u>. The model does not distinguish sub-perils, associated with key thermodynamical processes of cyclones (heavy precipitation, storm surge and associated flooding, strong winds) but instead uses a statistical relationship to estimate the global damage induced by a cyclone from a proxy variable given by the maximum wind speed (which is the proxy used in Saffir-Simpson Hurricane wind scale to define the intensity of a cyclone). We will include these elements in the third section (3.2.1) of the paper. The damage function is fitted on multiple events from the total damage reported in the global disaster database EM-DAT (Guha-Sapir et al., 2018). This database, used in most studies on the topic, accounts for the total reported damage (sum over all sub-perils) and does not distinguish damages from sub-perils. So, despite the relevance of the reviewer's comment, for our application (see our comment in the introduction of our reply, which are now included in the manuscript introduction to clarify the context of our study), distinguishing the sub-perils generating the impacts is not needed.

<u>AR1.2: Final response.</u> We describe explicitly the sources of damage / sub-perils in section 4.1 (line 451). However, as mentioned, we cannot integrate this information because EMDAT does not provide the split according to those sub-perils.

<u>RC1.3:</u> *The model depends a great deal on the damage function. But it is not clear how this damage function was estimated.*

<u>AC1.3: Interactive discussion.</u> We use region-specific damage functions from Eberenz, Lüthi, et al. (2020). This method uses a parametric function following Emanuel (2011). The parameters are estimated for each region with machine learning techniques from the reported damage estimates in the International Disaster Database (EM-DAT) Guha-Sapir et al. (2018) crossed with cyclone tracks (IBTrACS), and geographic and socio-economic information along these tracks.

We recall the main steps of the optimization performed by Eberenz, Lüthi, et al. (2020) and Lüthi (2019) to define the regional damage functions. The authors first defined the event damage ratio (EDR) as a fraction error between normalized reported (NRD) and simulations (SED) for each cyclone and the total damage ratio (TDR) is defined in each region summing over events. For each event, there is a value for vh allowing to optimally calibrate the explicit damage function described in Emanuel (2011). Then, the authors proposed two complementary optimization methodologies to find the value of vh maximizing the prediction of the regional damages Eberenz, Lüthi, et al. (2020):

(i) Root mean square fraction (RMSF), minimizing the spread of the event damage ratios (EDR) - defined as the ratio of simulated damage vs. reported damage.

(ii) Total damage ratio (TDR), finding the value of vh, such that the ratio of total simulated damage - obtained summing over event damage - and total reported damage is closest to 1.

We will clarify the calibration of these functions in section 4.3. In particular, we will review the approach of Eberenz, Lüthi, et al. (2020) to find the values of vh (c.f. technical supplement).

AR1.3: Final response. We included the equation used by Eberenz et al. (2021) to define the optimal vh coefficient per region in section 4.3. We also refined the estimation introducing a correction ratio by country.

RC1.4: *The estimates of how national assets are distributed across space are crude. Light times population is not going to allocate national assets carefully. I am specifically concerned about how well they model the assets near the coast.*

AC1.4: Interactive discussion. We chose to build our model based on state-of-the-art estimates, in such a way that the methodology is uniform country-wise. This dataset (Eberenz, Stocker, et al., 2020) is also used for the calibration of damage functions in Eberenz, Lüthi, et al. (2020) (discussed in Q3). Therefore, using this data allows to estimate the exposure in a consistent manner. To verify the accuracy of estimation, a back-test has been performed (Section 4.4). As we mention in the beginning, the only way to improve the estimates of asset value distribution would be to use the actual asset distribution from asset-level databases, but such databases are not yet available at the global scale. We will add this explanation when introducing the exposure dataset in section 2.4.

AR1.4: Final response. We added a remark in line with this concern in section 2.4 and 4.4 to stress that regional application would need to be refined by both finer asset allocation and specific damage calibration.

RC1.5. *The model appears to assume the spatial distribution of assets are fixed within a country.*

AC1.5: Interactive discussion. The model assumes that the spatial distribution varies with population changes proposed in the Socio-Economic Data Application Center dataset presented by Jones and O'Neill (2017, 2020). In particular, the spatial distribution of the population is different in varying shared socioeconomic pathways (SSPs). These projections are available with a one-eigth degree resolution.

AR1.5: Final response. The dynamic of population in SSPs is borrowed from the literature and was better exposed in the manuscript.

We added a map (Figure 20) showing that spatial population distribution clearly varies in SSPs (in particularly SSP3). The spatial distribution is therefore not fixed within countries.

RC1.6 *The paper does allow national assets to change over time, but they do not describe how this is done.*

AC1.6: Interactive discussion. To estimate future exposures along the cyclone track in each scenario, we use the downscaled estimation for the exposed wealth and the coefficients representing the change between the current state and the future scenario. We use the most granular projections of GDP per capita variation curves (Figure 2 - Data Source : https://tntcat.iiasa.ac.at/). Merging the two datasets (regional GDP per capita and local population) we build a dynamic projection of exposure factor.

AR1.6: Final response. We included a section to describe this issue in details (Section 2.6 and 4.5).

RC1.7. *There is no effort to measure adaptation by the country being hit or how that might change over time.*

AC1.7. Interactive discussion. Indeed, we left this question for further research. Supposing that adaptation increases with time alone would not be a relevant hypothesis. However, this question could be one of the direct applications of the model. For example, measuring the investment costs required to shift the value of vh (or vt) - and thus reduce the risk of future damage - can be a research question derived from this model simulations. In the revised paper, we will present more clearly the possible application of the model integrating the adaptation scenario, changing the values for the vulnerability parameter (vh and vt) in the section 4.2.

AR1.7: Final response. We added a discussion of this point in section 4.2.

RC1.8. *The initial forecasts of windspeed from the climate models are very inaccurate. The corrections appear to matter a great deal. However, these corrections have been made are on the historic data. So once they adjust historic data to actual historic outcomes, they do fine. But how well the model predicts future wind speeds is unclear.*

AC1.8: Interactive discussion Our bias correction approach is the standard in the climate community (see http://ccafs-climate.org/bias_correction/) . We do not have reanalysis data for the future. Therefore, there is no 'reference' value[1] to evaluate the prediction of the model. Therefore, we control the bias using the past distributions, where we can compare climate models and reanalysis and assume that errors between the two are similarly distributed in the future. We reiterate that this assumption is relatively classical in the climate community, and we will integrate these precisions in the paper in section 5.1.

AR1.8: Final response. We included the reference on bias correction (provided in the interactive discussion) in the manuscript.

RC1.9. *Figure 19 suggests the model predicts a small probability of very large damage but an expected value that is quite small. What explains this large tail to the distribution of damage? Is this simply the probability of a large storm striking a large coastal city? What is the expected value of damage?*

AC1.9: Interactive discussion. We ran the 7 models over 300 representative years to obtain these distributions. There is an effect due to certain large coastal cities exposure for the 'very unlikely' band (between 95 to 99 percentile) of annual damages. However, given the scale observed more than one city have been hit by storms. Because the aim of the model was also to stress test the resiliency of the financial and economic systems, looking at the expected value of damage was less interesting that studying the quantile value especially in the context of events with large tail risk. Coronese et al. (2019) investigating the increase of economic damage due to extreme natural disasters supports this thesis showing that the impact of climate change is particularly striking for

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[1] Navarro-Racines, C., Tarapues, J., Thornton, P., Jarvis, A., and Ramirez-Villegas, J. 2020. High-resolution and bias-corrected CMIP5 projections for climate change impact assessments. Sci Data 7, 7, doi:10.1038/s41597-019-0343-8

extreme events (See for example, Coronese et al. 2019, Figure 2A). The table below contains the expected value of damage after bias correction.

The revised version will integrate this summary table with the expected value of the damage in the section 5.2 as well as the precisions above to explain the focus on quantiles in the visualization.

AR1.9: Final response. We included the comment above, the reference and the table of expected damage in the manuscript.

RC1.10. *Why does going from historic (1980-2020) to RCP2.5 lead to more damage than going from RCP2.5 to RCP8.5? Going from historic temperature to RCP2.5 is a 1C increase whereas going from RCP2.5 to RCP8.5 is going from 2C to 5.4C? Given the assumption that wind speed increases more rapidly as sea surface temperature rises, this outcome is hard to understand.*

AC.1.10: Interactive discussion Socio-economic change leads to wider differences than climate change, and this was expected (cf. Mendelsohn et al. (2012), Figure 3 for example). The explanation for this is contained in the dynamics of (i) GDP and (ii) population in SSPs. In the revised version we add further explanation about this result including more references to discuss the results of our simulations.

AR1.10: Final response. We explained that socio-economic factors' contributions to shift in exposure is important (or even dominant) in the revised manuscript.

RC1.11. *How much confidence do the authors have that they understand the relative damage caused by tropical cyclones at the end of the century across countries? How much of this is simply assuming the same distribution as today?*

AC1.11. Interactive discussion. Thank you for this very interesting question. We can see in Figure 4 (20 in the paper) that the distribution across countries is different from one SSP to another. For example, we have sensibly the same distribution in SSP2 and SSP5 with a higher expected damage in SSP5 because of the growth hypothesis this scenario relies on. However, SSP3 (rocky road) or SSP4 (inequality) are distributed differently. The scenario emphasizing inequalities -and its interpretation by scientists in terms of (i) socioeconomic developments (Riahi et al., 2017) and (ii) population distribution (Jones & O'Neill, 2017) - increases damage concentration in the United-States. On the other hand, the rocky-road scenario, linked to higher and more rural population, lower GDP and national rivalry sees the damage more equally distributed on other nations. We integrate this precision in the final version.

AR1.11: Final response. The distribution of damage across countries varies in SSP, but this variation is somewhat reduced when introducing the correction factor in damage estimation.

RC1.12. *It is not likely that anyone could design adaptation measures from this study given the crudeness of both the tropical cyclone predictions as well as the damage predictions. Is there any reliable prediction of a change in tropical cyclone outcomes from current outcomes other than they will get uniformly more powerful?*

AC1.12. Interactive discussion. The current dataset - with low resolution data, and maybe not entirely sufficient number or realizations - might not be accurate enough to calibrate adaptation measures. However, we believe that the framework presented here is perfectly adapted to project a dense set of trajectories, compute expected and damage percentile over the next decades and therefore measure the investment required for adaptation and mitigation measures in the next fifty years. This work also reflects a practical exercise not carried out until now, of cross-referencing the latest data sets, putting into perspective both the socio-economic and climatic development hypotheses, and carrying out a bottom-up, rather than top-down, damage calculation. The conclusion of the revised manuscript will mention the limits of the current application and better explain the scope of applicability of the model.

AR1.12: Final response. We added multiple disclaimers in the paper for the exposed results. We also added a correction factor to reduce the errors in damage modeling as well as other improvements relative to other referee comments. Furthermore, we reiterate that this model description paper presents an integrated methodology, rather than a fully operational software.

REFEREE 2

RC2.1. *Table 1: the selection of GCMs used should be justified. This could be through reference to model performance literature for key parameters, a specific evaluation process or perhaps simply availability of required variables for the analysis (though I note the variables used are available for all CMIP5 models).*

AC1.1. Interactive discussion. The choice of the CGMs was driven by the availability of the variables of interest in the Copernicus Climate data store (CDS) in the representative concentration pathways used in the exercise (RCP 2.6, 4.5 and 8.5 W/m2) in both single pressure level and multiple pressure levels monthly data in the same ensemble (r1i1p1). We also aimed at having multiple regions represented.

AR1.1: Final response. We added this explanation in the manuscript (line 118).

RC2.2. *Section 2.2: the MSLP from ERA-5 is sampled 500 km from the centre of the cyclone. Is the same done for the other variables? Since the data are sampled from monthly means, it's possible the sampled values may not accurately represent the conditions at the time of TC passage (especially relevant for variables with sharp gradients such as SST).*

AC2.2. Interactive discussion. We retrieve both pressure (MSLP) and humidity (RH) away from the center because TC maximum potential intensity (MPI) - through thermodynamic efficiency and moist entropy - arises from the deviations from the normal conditions.

We acknowledge that monthly averaging may indeed “smooth” values so that the data may not represent the conditions at the time of cyclone passage. Therefore, using monthly means, this translation is mainly made for reasons of theoretical coherence. In future studies, this model will be applied with higher temporal resolution and performing this translation would be more important. In the present version of our paper, because the CMIP5 projections of the sea-level temperature were only available at monthly frequency in the CDS, we chose to perform the exercise using monthly data to illustrate our approach. In addition, the monthly sampled data allowed us to build a statistically significant description of the MPI in the historical period. The possibility of improving the model using high frequency data will be emphasized in the revised version of the manuscript.

AR2.2: Final response. We added this explanation in a footnote of the revised manuscript (line 139)

RC2.3. *Section 3.2: Given the literature of TC track generation methods, comparison with common metrics is encouraged. Specifically, as landfall is critical to reliable performance of a damage model, it would be helpful to present a comparison of the observed and simulated landfall rates (see for example Hall and Jewson, 2007; Lee et al., 2018; Arthur, 2021). This would strengthen the quality of the track generation results significantly.*

AC2.3. Interactive discussion. In the revised version we will compute landfall rates and compare them to relevant results from the literature.

AR2.3: Final response. We added a comparison with past landfall rates of “damaging” cyclones (Figure 7). We note however that the framework can be improved by modulating number of initialized cyclones to match the exact average number of cyclones making landfall. This would require an additional optimization.

RC2.4. *Eq 3 - note that most best track data used wind pressure relations (WPRs) to determine Pc. Typically the work flow involves determining the Dvorak T number, converting this to a sustained wind speed, followed by regionally-specific WPR to determine Pc. The conversion back to wind speed from reported Pc using a single WPR will introduce errors, as an array of WPRs are used to operationally estimate Pc, not only between basins but within basins as well (e.g. Harper, 2002; Courtney and Knaff, 2009; Courtney and Burton, 2018; Courtney et al. 2021).*

AC2.4. Interactive discussion. We acknowledge that the use of a single WPR introduces errors. [..]

AR2.4: Final response. We made the suggested correction and introduced the parameters at a basin level. We included the comment that it could still introduce an error alongside with the provided reference. We find that the basin level estimation is a sufficient proxy in the context of this illustration of the framework.

RC2.5. *Eq 10 describes the dominant control on the maximum intensity of TCs (maximum pressure drop - MDP). This is tied only to SSTs. The model uses maximum potential intensity (MPI) to control the depression dynamics (i.e. intensification rates). The formulation of MPI is directly applicable to the problem of estimating the maximum intensity, accounting for factors beyond SST alone that control maximum intensity. This suggests using SST as the only predictor of the MDP is deficient.*

AC2.5. Interactive discussion. Indeed, we already acknowledge that the SST alone in not a good predictor of whether individual TC will intensify. Therefore, we use the thermodynamic definition in the cyclone dynamics specification. On the other hand, we still define a “MPD” taking the maximum observed pressure drop for a given SST across all events in each basin. You are right to point out that this appears to be inconsistent. However, - we use this maximum depression (MPD) estimated over the historical period for a given sea surface temperature only to cap the depressions in the simulations, to avoid generating events intensifying beyond past observations and make the simulated tracks more realistic. This is a limitation of our approach; however, this is relatively common in "statistical" models. Alternatively, we could make the maximum depression depend on the four variables of interest, however, this would make estimation more difficult and reduce the significance of this statistic.

In the revised version we will compare our definition with an alternative definition of the MPD, using the thermodynamic definition used for the MPI, and substituting extreme values of temperature and humidity.

AR2.5: Final response. The MPD serves as a capping value only. Therefore, we kept this statistical formulation in this version. However, to account for wider variation due to other factors, we added a translation upward to relax the constraint, and potentially allow storms to intensify further, accounting for those factors in the projection.

RC2.6. *Further, Chen et al. (2021) suggest rapid intensification is dependent on dynamical (e.g. upper divergence and wind shear) as well as thermodynamical factors. While the difference between Pc and MPI is a factor in predicting rapid intensification, and the dynamical factors are probably accounted for by the random innovation (Eq. 12), these other dynamical factors should be acknowledged.*

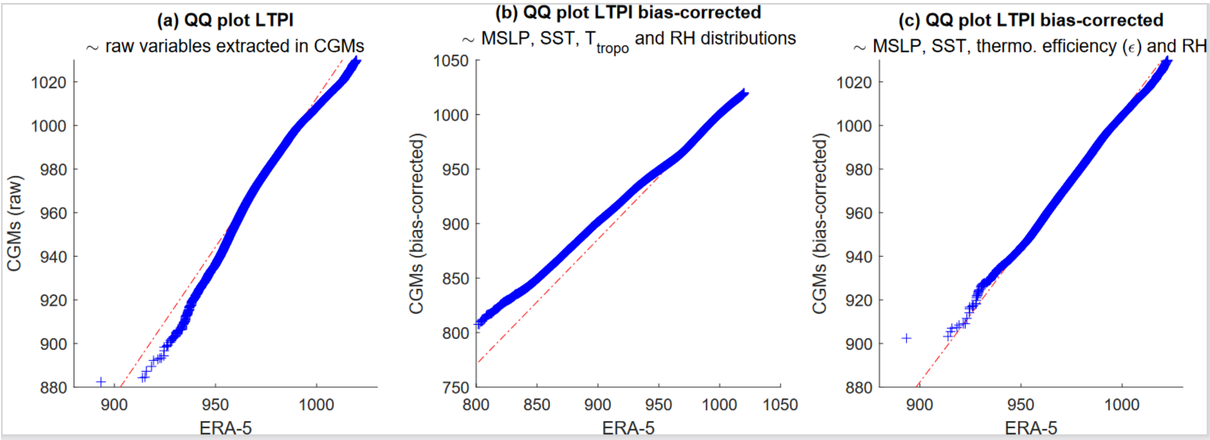
AC1.6. Interactive discussion. Indeed, the components explaining the noise term in the pressure dynamics should be better identified, and we will acknowledge them in the next version of the manuscript. However, in the context of our exercise we had to focus on explanatory factors that are available in the CMIP5 simulations which reduced our scope to thermodynamical factors.

AR2.6: Final response. We clarified the description of the depression dynamics module in the revised version. Thanks to your comment (and those of R3) we could correct the fitting of the parameters used in the cyclone dynamics depression equation in this new version.

RC2.7. *Apply CDF-t to model variables, then evaluate MPI - I suggest comparing quantiles of ERA5 MPI against the bias corrected CMIP MPI values to demonstrate the effect of bias correction. Q-Q plots would be an effective way to do this. One risk with this approach is that correcting individual variables may lead to unrealistic combinations when evaluating MPI - e.g. extremely low tropopause temperatures in combination with very high SSTs that lead to unrealistic lapse rates and therefore unrealistically large MPI. Two solutions present themselves: 1) apply the bias correction methods to calculated MPI or (2) consider the joint distributions of variables when evaluating the bias corrections.*

AC1.7. Interactive discussion. This is a very relevant point. Indeed, individual variables entering the MPI computation may be strongly correlated. In the revised version we will follow the reviewer's suggestion and apply bias correction directly to calculated MPI.

AR2.7: Final response. Indeed, the local thermodynamic potentials defined using variables corrected independently introduced a bias (b).



We made the correction on the thermodynamic efficiency which corrected the low bias of the MPIs generated by the algorithm. The final QQ-plot (over all model and basins) is represented in Figure (c).

RC2.8. *The distributions of SST presented in Figure 16 do not appear representative of SSTs sampled in the vicinity of TCs, and is inconsistent with the distribution shown in Figure 10. SSTs of 26C (299K) are typically considered a lower bound for TC formation (Gray, 1979), but median values from the ERA5 are well below that - for example based on Figure 16 the median SST for the South Pacific basin along synthetic tracks is 290-292K, for the Western Pacific 295K. Only the N Indian basin has a median SST near 300K. This suggests that the synthetic tracks are traversing areas not typically covered by TCs, or occurring at the wrong time of year for the respective basin leading to the unusual SST distribution.*

AC1.8. Interactive discussion. The bias-correction module is indeed fitted on a larger range of climate conditions. For the genesis of the cyclones, the time of year and location are in line with historical cyclone data. However, in the bias-correction module, the synthetic tracks are generated without climate constraints, i.e. cyclones are allowed to drift relatively far away from their genesis location (in the limits of their initial basin), and therefore can cover conditions which do not lead to the formation of tropical cyclones. At this stage, these tracks are not to be considered as `TCs tracks' but as 'candidate' tracks. In the following stage, TC tracks will be generated from candidate tracks by filtering those ones where meteorological conditions for cyclone formation are satisfied.

AR2.8: Final response. We explained that the bias correction was done on a sub-sample build from track candidates in the new version.

RC2.9. *Completely absent is any discussion on TC rates in the projections. Comprehensive literature reviews and expert elicitations indicate a global decline in TC frequency (albeit with generally low-medium confidence) (Knutson et al. 2020). Changes in TC rates will have a significant impact on the annualized losses. This is an important component that should be addressed.*

RC2.10 .*In parallel, there is no discussion on changes in track behaviour. Observed trends in TC translation speed (Kossin, 2018) and poleward migration of maximum intensity (Kossin et al., 2014) should be considered in projections of TC activity. This has profound implications for TC-related risk in key marginal areas (e.g. Bruyere et al., 2020) where vulnerabilities are high, but present-day frequency of TCs is low.*

AC1.9-10. Interactive discussion. These two comments will be included in the conclusion of the revised version of our paper as they reflect important limitations of our exercise. Indeed, we kept the genesis rates constant for each basin. The number of cyclones each year are drawn from Poisson distribution. It is possible to reduce the intensity parameter in the projections, and to introduce cyclones in regions where the present-day frequency is low, however, in this study, we focused on the changes in thermodynamic potentials. Moreover, as our approach is a statistical one, we had to focus on areas where relationships could be extrapolated from historical data. We will add a comment to account for this possible improvement.

AR2.9-10: Final response These comments were included in the new version.

RC2.11. *Section 5.2: Consideration of SSPs in determining the effects on damage is novel, but the explanation is very limited. Given growth of exposure is constrained in existing high exposure regions, regional growth may not be in areas exposed to TC impacts.*

AC2.11. Interactive discussion. In the revised version, we will provide more explanations about the shared-socioeconomic pathways used to project exposure. Indeed, we did not consider that areas subject to cyclones would face additional economic growth constraints in our projections. Historically, high exposure regions were not particularly constrained in terms of growth (e.g. the East Coast of the United States of America can be considered as a high exposure region as well as most regions in South Korea, Japan, Australia). In addition, climate change increases tropical cyclone intensity allowing them to reach regions where current TC impacts are low.

RC2.12. *The description of the implementation of projections of local physical asset value dynamics is very limited, but probably the most novel part of the connected modelling system. There should be a more substantial discussion on how the SSP definitions are used to modify asset values.*

AR2.12: Final response We detailed this in the new version (Figure 1, section 2.6, section 4.5)

REFEREE 3

RC3.1. *Line 17: I disagree that we are lacking tools to assess impacts of future TCs. See for example Geiger et al. (2021)*

RC3.2. *Line 7 and Line 390: I disagree with the claim that the framework is 'a simple solution'. The framework requires expertise across multiple disciplines.*

AC3.1-2. Interactive discussion. Thank you, in the revised version we will remove the mention of simple solution, include the reference provided and rephrase the text as follows: Tools to assess the impact of future cyclones in shared socioeconomic pathways are starting to appear in the literature, for example, Geiger et al. (2021) evaluate the population exposure. Our study instead focuses on tropical cyclones damage costs with the aim to include these advanced signals in integrated economic modeling.

AR3.1-2: Final response. Thank you, we made the corrections.

RC3.3. *Line 32-34: It seems odd to make this assertion in the introduction without any supporting evidence. I suggest reframing this statement as a hypothesis to be tested.*

AC3.3. Interactive discussion. You are correct to point that this sentence requires supporting evidence. […]

AR3.3: Final response. We rephrased this statement.

RC3.4. *This is perhaps my most important comment. I don't think the difference between your TC model and STORM is made clear enough. STORM appears to use the same SST-pressure drop relationship as you do, and STORM also uses MPI (calculated using the Bister and Emanuel formulation) to limit TC intensification. I don't understand what is new in your TC intensity formulation. Please clarify exactly what is new in the text. Is it the use of local MPI and SST along the synthetic tracks?*

AC3.4. Interactive discussion […]

AR3.4: Final response. The main differences between our approach and STORM are (i) the possibility to use the model with CGM projections to compute realistic trajectories of future cyclones in the context of climate change, whereas STORM focuses on generating cyclones with identical characteristics to those in IBTrACS database. This objective in particular required to introduce a state-of-the art bias correction module, adapted to the cyclone modeling exercise; (ii) the use of three climate variables (SST, tropopause temperature and relative humidity) in the formula describing cyclone intensification. It was necessary to include these additional variables because it has been demonstrated in the literature that they have a strong impact on tropical cyclones and will change with the advent of climate change.

RC3.5. *On a related note, the paper highlights the importance of this new representation of the thermodynamic influence, and makes claims on lines 43-45 that is it better, but this has not been demonstrated. Is it possible (if not too onerous) to run projections with and without this new representation of thermodynamic influence to demonstrate its importance.*

AR3.5: Final response. We acknowledge that the suggested exercise is of great interest and that measuring the sensitivity of the damages to the inclusion of each climate variable is a relevant research question. However, the main advantage of including additional variables is probably related to better representation of the future cyclone intensification processes in the context of climate change. Since for the future period we do not have a reference dataset of cyclone tracks to which alternative simulations could be compared, in our view, the choice of variables should rather be guided by theoretical considerations, and the literature suggests that TC intensification in not driven by SST only.

RC3.6. *It's not clear to me how you calculate local SST and MPI along the synthetic tracks. If I am correct, the synthetic track generation samples from the IBTrACS record. If so, how do you assign a calendar year to each synthetic track to extract SST and MPI (from either ERA5 or CMIP)? If it's a random year then the environment might not necessarily be favorable for the synthetic TC (i.e., too cool SST or low MPI).*

AC3.6. Interactive discussion. The process of generation of tropical cyclones is the following. For each year between two dates (2075 and 2100 for example), we generate several cyclones per basin following the Poisson law with parameters provided in STORM: 14.5 for the East Pacific (EP), 10.8 for the North Atlantic (NA), 2.0 for the North Indian (NI), 12.3 for the South Indian (SI), 9.3 for the South Pacific (SP), and 22.5 for the West Pacific (WP)). These parameters would have been smaller if estimated using our filtered database. However, we maintain these parameters to take into consideration the fact that some events will be generated in conditions not favorable for the development of cyclones and be cleared out of the database. More precisely, for each event, we retrieve a latitude, longitude, and month re-sampling the IBTrACS past distributions. Therefore, cyclones are generated in similar months as historically observed cyclones (cf. Fig 7 of the manuscript). Then, the starting day and hour of the day are randomly attributed so the tracks can be defined with a three-hour step. With this procedure some 'candidate tracks' can be initiated in a location, or in a year which is less favorable for intensification. This would have the effect of underestimating the number of cyclones in the simulations therefore we kept higher intensity values to compensate this effect. Overall, we obtain relatively similar landfall counts per basin in the simulations and in the historical dataset. In the revised version, we will consider adjusting the cyclone frequency to match the landfall counts from the historical dataset precisely.

AR3.6: Final response. We improved the description of the cyclone genesis module. A calendar year is indeed assigned to each synthetic cyclone. If the conditions are not satisfied (for e.g. too cool), the local MPIs would be closer to normal conditions (i.e. potential pressure drops smaller), and resulting cyclones will be less intense. We present the landfall counts for storms with wind speeds higher than 35m/s and show that they are similar to those observed in historical data.

RC3.7. *ERA5 is still too coarse resolution to capture the most intense TCs. I suggest on Line 110 to change to 'better resolves than climate models'.*

AC3.7. Interactive discussion We made the change

RC3.8. *Line 110-113: Your method to use data away from the storm center is fine but I don't think it's necessary. You are using monthly data that should smooth out the influence of TCs. This is just a comment – I'm not suggesting to make a change.*

AC2.8. Interactive discussion Indeed, with the current spatial and temporal resolution, this translation is mainly made for reasons of theoretical coherence. In future studies, this model will be applied with higher temporal resolution and performing this translation would be more important.

RC3.9. *Line 117: I note that ERA5 is now available back to 1950, but is considered preliminary.*

AR3.9: We made the precision line 623.

AC3.9. Interactive discussion We will include this remark that could allow us to increase the fitting period. However, as climate change affects the values of the parameter we might prefer focusing on recent historical period.

RC3.10. *Line 122: Please be more descriptive of what you mean rather than the ambiguous term 'erratic'.*

AC3.10. Interactive discussion The trajectories in North Indian basins are not well captured by our statistical framework. For displacement, the latitude and longitude description are less statistically significant (Tables A3 and A4). For the maximum pressure drop the relationship is not statistically significant (Table A6). We will include this description in the revised manuscript.

AR3.10: Final response. We rephrased the explanation. The lower significance of Indian basin trajectories is mainly due to lower number of observations, in particular when focusing on cyclones above 35m/s.

RC3.11. *I'm not sure what I learned from Fig. 3. I think this can be removed.*

AC3.11. Interactive discussion: We will place this figure in the appendix. The aim of this figure is mainly to compare the depression dynamic produced (Figure 12) to existing (and most famous) ones.

AR3.11: We removed this Figure.

RC3.12. *Section 5: I think it would be useful to remind readers that you are keeping TC frequency and genesis distribution constant.*

AC2.12. Interactive discussion. In the revised version, we reiterate that genesis frequencies are kept constant in section 5.

AR3.12: We made the precision line 623.

RC3.13. *Line 278-279: Please further explain why you wait 3 steps before applying the decay.*

AC2.13. Interactive discussion.This step is inspired from STORM : " *When the TC eye is over land for at least three time steps (totaling 9 hours), the decay in TC wind speed in the STORM is modelled following Kaplan & De Maria (1995)*". The decay function we use was introduced in Kaplan & De Maria (1995) who showed that it provides an acceptable approximation for t_L>12h. As each time step is 3 hours, we let the TC intensity be driven by the pressure dynamic module the 9 first hours and apply the decay function for t_L>12h, e.g. after 3 steps.

AR3.13: We made the precision line 411.