Answers to Referee 3

April 15, 2022

1 Specific Comments

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

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

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.*

Assertion l32-34 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.

Your are correct to point that this sentence requires supporting evidence. We propose to rephrase it as follows: *Recently, Bloemendaal et al. (2020) developed a modeling framework to simulate realistic synthetic tropical cyclone tracks: the Synthetic Tropical cyclOnes geneRation Model (STORM). This model computes the maximum pressure intensity (MPI) associated to the sea-surface temperature (SST), and uses this potential as a predictor in the central pressure dynamics (James & Mason, 2005). In line with Merrill (1987), we find that although the sea-surface temperature plays a major role, this variable alone is not a reliable predictor of whether a given storm will intensify. Thus, we prefer to rely on Holland (1997) formulation and model the effect of climate change on the maximum potentials in the different scenarios through a better description of the underlying thermodynamic phenomenon, well described by Emanuel (1988) and Holland (1997) or Emanuel (1999).*[1]

Indeed, the maximum pressure drop and MPI in STORM are defined using the sea-surface temperature only (cf. Eq (4) in the next question). On the other hand, Merrill (1987) suggests, on the contrary, that this predictor is material

[1] See section 3.3.3. for further details.

as a capping function but does not provide (alone) a good indication whether a given storm will intensify:

> "*Empirical studies by Miller (1958) and Merrill (1988) and theoretical results of Emanuel (1986) imply that SST specifies an upper bound on tropical cyclone intensity. The SST capping function is developped for the period 1974-1985 (Fig. 3) by tabulation maximum winds by 0.5 degree climatological (Reynolds, 1982) SST classes and computing the 99th percentile (Fig. 4)[...]*
>
> *The reason for treating SST as a capping function rather than as a direct predictor is evident from Fig. 4. Compare the top three curves (90th percentile and greater intensity) which increase sharply above 27°C with the median (50th percentile) which is nearly above 26°C. Knowing the climatological SST reveals little about the intensity of the average storm but much about the extreme intensity likely to occur*" (Merrill, 1987, pp. 11–12).

Figure 1 provides the Figures 3 and 4 of the empirical analysis evoked in the quote above (Merrill, 1987, pp. 11–12) and the Figures computed in the context of our study that led us to the same conclusions. In our work, we plotted the maximum pressure drop only (and not all percentiles) to lighten the graph which already accounts for multiple basins. We will add some elements from this quote in a note in section 3.3.3.

Figure 1: SST-MPD relationship

Merrill (1987, pp. 11–12)

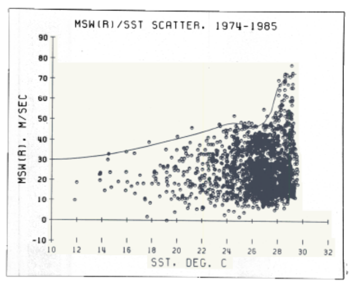


Fig. 3. Scatter plot of relative intensity versus climatological sea surface temperature. The SST capping function based on the 99th percentile (see Fig. 4) is superimposed.

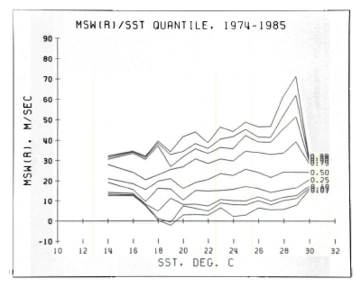
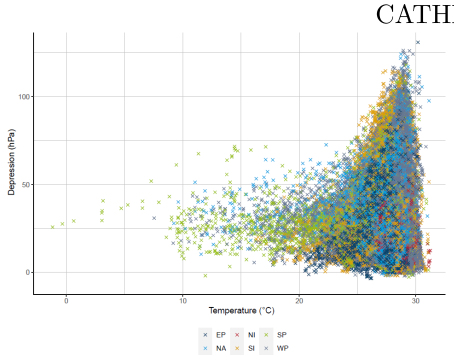
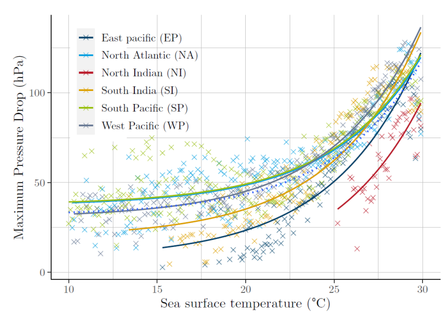


Fig. 4. Percentiles (values as in Fig. 1) fitted to the intensity-SST data shown in Fig. 3. The 30°C class (29.5°-30.5°) contains very few observations and is probably not representative of the extreme intensities possible.

CATHERINA





Unclear comparison with STORM 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?

Thank you very much for this comment, the distinction is indeed quite unclear in the current version. In Bloemendaal et al. (2020), the MPI is defined subtracting the maximum pressure drop (MPD) from the normal environmental pressure (MSLP). Prior to that, the MPD is defined as a function of the SST (rounded and grouped per 0.1 bins) grouped spatially. The definition of the MPD is very similar in STORM and CATHERINA, but the role of this quantity differs in the two models. In CATHERINA (in line with Merrill (1987) observation), the MPD is not used in James and Mason (2005) dynamic equation but only as an upper bound for the central pressure. Let us clarify the main differences.

Bloemendaal et al. (2020) describe the construction of the MPI instrumental variable as follows:

> "*[...], we group these monthly mean SSTs in 0.1 °C bins together with their corresponding pressure drop values. We then fit the mean SST and maximum pressure drop per bin to Eq. (4):*
>
> $$P^{env} - P = A + Be^{C(SST - T_0)}, \quad T_0 = 30C \quad (4)$$
>
> *The coefficients A, B and C are estimated using non-linear least-squares. Using this formula, we can calculate the maximum pressure drop at every 0.25° × 0.25° SST grid point. In the final step,* ***we subtract this maximum pressure drop from the*** P^{env} ***fields to derive the MPI.*** (Bloemendaal et al., 2020, p. 5).

Therefore, the MPIs are defined statistically subtracting MPD (function of SST), grouped on a 0.25x0.25 grid, and Bister and Emanuel (2002) values are used to bound their values only:

> "*To inhibit unrealistically low MPI values, the MPI values are bounded by the lowest MPI value per basin and per month derived by Bister and Emanuel (2002)*" (Bloemendaal et al., 2020, p. 5).

Table 1 reiterates the main differences of the two approaches. There is another minor change in the definition of the MPD that results from the use that will be made of this variable. The reason for relaxing the spatial constraint (fitted on 0.25 grid) is that we wanted to enlarge the groups on which we maximized the pressure drops (which have the effect of increasing the maximums). This choice follows from 'the role' of MPD in our process, not as the intensification factor but as a maximum obtainable pressure drop (Merrill, 1987) for a given temperature in each basin. These explanations will be inclulded in the revised version (section 3.3.3).

Table 1: Comparison STORM-CATHERINA cyclone intensification module

	STORM	CATHERINA
Nb of variables	2	4
MDP definition	SST-MPD relationship - Eq. (4) 0.25x0.25 0.1°C bins	SST-MPD relationship - Eq. (4) basin 0.1°C bins
MDP use	Infer MPI	Cap basin pres. drop
MPI definition	MPI = P^{env} – MPD	Holland (1997)
Unrealistic values	Bister and Emanuel (2002)	MPD

Evidence of 'better description' 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.

The sentence line 43-45 is indeed not clear enough. The better performance of the inclusion of thermodynamic variables concerns the intensification process rather than the long term risk assessment. We propose to rephrase this sentence as follow: *Coupling STORM methodology with an extended the thermodynamic module fitted on four climate variables and CLIMADA, our approach provides a novel long-term tail risk assessment at a national level, providing an adaptive framework to estimate investments required to mitigate this risk.*

Concerning the better statistical significance (of the four variables MPI from Holland (1997)), we compared statistics of the non-linear fits using the two instrumental variables, i.e. conducted the same experiment with two candidates:

- Holland (1997) MPI derived using 4 climate variables (reiterated section 3.3.2 of the manuscript) and,
- the MPD from SST/MPD relationship used in Bloemendaal et al. (2020)

We used our definitions given in Table 1 and the results are provided in Table 2.We posed the following autoregressive stochastic depression dynamics (James & Mason, 2005):

$$\Delta P_t^c = c_0 + c_1 \Delta P_{t-1}^c + c_2 e^{-c_3 [P_t^c - X_t]} + \varepsilon_t^P, \tag{1}$$

$$\varepsilon_t^P \sim \mathcal{N}(0, \sigma_{P^c}), \tag{2}$$

where the distance to maximum potential, $P_t^c - X_t$, can be computed either with our four dimensional MPI, or with the MPD depending on the SST only. Table 2 presents the estimates, confidence intervals and significance level of the relationship. The relationship fitted with the MPI that includes tropopause temperature, relative humidity has narrow confidence intervals, and all parameters are statistically significant. On the other hand, when we use the maximum pressure drop (MPD), we obtain much less significant results fitting equation (1). The confidence interval of the parameters are wider, and c_2 (multiplicative factor associated with the exponential term) is not statistically significant.

Table 2: James and Mason (2005) depression dynamics (Equation (1)) parameters , standard errors and confidence intervals estimated using nonlinear least squares on the full sample.

	Estimate	Std. Error	2.5%	97.5%	Signif.
	Using MPI in the exponential				
c_0	17.966	3.1618	11.770	24.162	***
c_1	-0.516	0.00274	-0.522	-0.510	***
c_2	-19.528	3.180	-25.760	-13.295	***
c_3	0.00776	0.00134	0.005	0.010	***
	Using MPD in the exponential				
c_0	-25.961	11.809	-27.982	-8.073	*
c_1	-0.415	0.004	-0.423	-0.407	***
c_2	0.753	1.585	-0.187	0.323	
c_3	-0.003	0.002	-0.008	-0.002	*

Signif. codes: 0 '***' 0.001 '**' 0.01 '*' 0.05 '.' 0.1 ' ' 1
The MPI and MPD are computed based on the climate data extracted along tracks from ERA-5, and the depression dynamics are derived from IBTrACS

Table 3: Coefficient correlation

	c_0	c_1	c_2	c_3
c_0	1,0000	0,0071	-0,9998	-0,9937
c_1	0,0071	1,0000	-0,0066	-0,0077
c_2	-0,9998	-0,0066	1,0000	0,9929
c_3	-0,9937	-0,0077	0,9929	1,0000

We propose to include this comparison with STORM in a technical appendix.

Calendar year / favorable conditions 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).

The process of generation of tropical cyclones is the following. For each year between two dates (2075 and 2100 for example), we sample a number of cyclones per basin[2] following the Poisson law with parameters provided in Bloemendaal et al. (2020). For each event, we retrieve a latitude, longitude and month resampling 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 hours step. It is true that 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. On the other hand, the parameters λ_B would have been smaller if estimated using our filtered database on TCs exceeding 35 m/s. However, we maintain Bloemendaal et al. (2020) parameters to compensate the fact that some events will be generated in conditions not favorable for the development of cyclones, and be cleared out of the database. Overall, we obtain relatively similar landfall counts per basin in the simulations as 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.

Resolution 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'.

Thank you, we made the suggested change.

Shifted extraction 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.

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.

[2] 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). In a footnote we also precise that: The parameters λ_B 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.

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

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.

North Indian basin Line 122: Please be more descriptive of what you mean rather than the ambiguous term 'erratic'.

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.

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

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.

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

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

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

This step is inspired from Bloemendaal et al. (2020): *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 and DeMaria (1995).* The decay function we use was introduced in Kaplan and DeMaria (1995) who showed that it provides an acceptable approximation for $t_L \geqslant 12h$. As each time step is 3 hours, we let the TC intensity be driven by Eq. (1) the 9 first hours and apply the decay function for $t_L \geqslant 12h$, e.g. after 3 steps.

2 Technical Corrections

- Fig 1: Correct 'Tranform' to 'Transform'

\- corrected

- I don't see a reference in the text to Figure 5.

\- added

- Figure 8: Please explain the distinction between the red shading vs. the red tracks.

\- red sheding build with heatmap from IBTRACs red tracks are generated with CATHERINA

- Line 81: Please correct 'AOCGM' to 'AOGCM' and expand the acronym.

\- done

- Line 273: Correct 'Algorithm 1' to 'Figure 1'.

- There should be an Algorithm close to this section (which was sent at the end of the paper is this format)

- The reference to Figure 18 should be to Figure 17. If I am correct, then I'm also not seeing a reference in the text to Figure 18.

\- corrected

References

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