Review of NHESS-2021-329

Title: Storm surge hazard over Bengal delta: A probabilistic-deterministic modelling approach

The paper mainly has shown an application of the hydrodynamic model SCHISM combined with a large ensemble of synthetic storm events generated for the Bengal delta region based on the climatic conditions of the 1981-2015 period for flood risk analysis. I want to applaud the authors for their sheer volume of work. This study will undoubtedly contribute to the storm surge risk awareness of the coastal community and the region's local government; however, I have a few questions about some of the methods used in the analysis. Also, some sections throughout the paper need more details and better clarity to improve the model results' reliability before considering it for publication.

Questions are written below using page numbers and text quoted from the main body.

Specific comments:

1. The author has mentioned about a custom high-accuracy regional bathymetry data a couple of times. Is there any study done to check the quality of the data itself (i.e., accuracy)? Is it publicly available?

Reply:

We would like to thank the reviewer for posing this very important question, as it is one of the main strengths of our modelling work.

Dataset

The initial version of the bathymetry was first developed by Krien et al. (2016) combining the following datasets -

1. 4 (four) navigational charts from National Hydrographic Office (NHO), and 60 (sixty) charts from Inland Waterways Authority of India (IWAI) for Hooghly river. They amount to 16,500 and 123,000 digitized sounding points respectively.
2. 1 (one) chart from the Bangladesh Navy, and 3 (three) charts from the Mongla Port Authority.
3. River cross-sections from Bangladesh water development board.
4. A 50-m resolution digital topography model developed by the Center for Environmental Geographic Information Services (CEGIS) through dedicated surveying.

This dataset is supplemented by GEBCO (2009) and ETOPO2 (from 89.3E to 92.3E and from 19.90N to 21N approximately).

When compared with the global GEBCO dataset, GEBCO is found to be much shallower compared to our dataset, on average by about 3m. The comparison is discussed in detail in Krien et al. (2016).

The dataset developed by Krien et al. (2016) was further updated by Khan et al. (2019) by adding 34 new Navigational charts collected from the Bangladesh Navy. This amounts to 77,000 new digitized points shown in Figure C1.

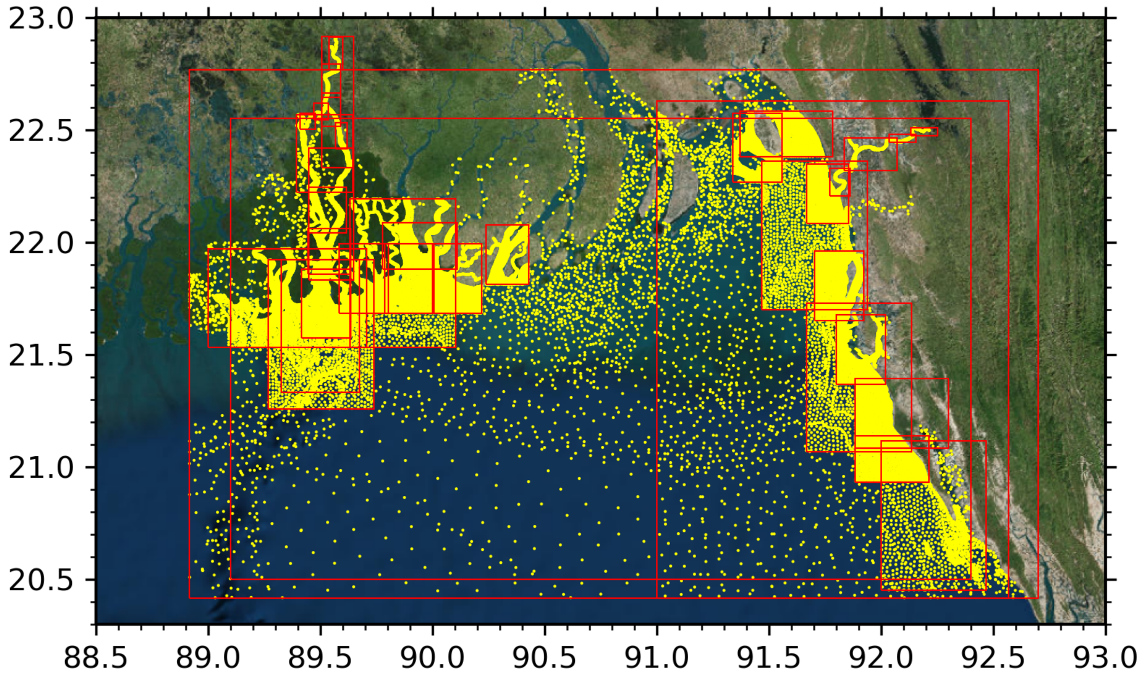

Figure C1. 77,000 digitized sounding points (in yellow). The coverage of the corresponding individual charts is shown in red outlines. The background image is taken from ESRI World Imagery services.

It is noteworthy here that, in Krien et al. (2016), embankments were also included as uniform height along the trace of the crests. For all embankments the same height was assumed (4.5m MSL). The embankment outlines were provided by the Bangladesh Water Development Board.

In the dataset assembled by Khan et al. (2019), which forms the basis of this paper, the crests heights still remain uniform over a single embankment but are replaced with respective measured (when available), or designed crest height for each embankment.

Quality check

As published in Krien et al. (2016), GEBCO global dataset is too shallow and has interpolation artifacts in the nearshore zones. GEBCO/STRM also shows very high topographic height over the mangroves (about 5m) due to the bias induced by the canopy (Shortridge and Messina 2011). As no other dataset is available from cross-validation, Krien et al. (2016) demonstrated the quality of the dataset by modelling the tide in the region. As tidal propagation in the shallow water area is strongly controlled by the depth, a good regional reproduction of tide indirectly indicates the quality of the bathymetry.

The tidal model developed by Krien et al. (2016) shows 2-4 times improvement of the complex error compared to the state-of-the-art global tidal solutions, as well as consistent reproduction of tidal constituents (both amplitude and phase) all along the shoreline. The model once updated by Khan et al. (2019) shows error further reduced by 10-30% at most stations . These improvements are in large part due to the proper representation of the bathymetry, indirectly indicating its quality as mentioned above.

Data availability

The individual datasets used to derive this custom bathymetry can be queried from the original sources. We can not assume the right to redistribute the dataset freely, as we do not own the copyrights of the various sources of our composite product. Please note, however, that the oceanic part of the bathymetric data used in Krien et al 2016 is freely available.

2. In page 12, lines 284-285: 'With an average annual frequency of 0.70314 cyclone, the ensemble of 3600 cyclones considered here ...'

What is the main reason behind selecting 3600 cyclones? Why this number?

Reply:

The choice of the cyclone number is subjective, guided by the scale of the problem as well as the previous experiences of the authors. Indeed with the method of Emanuel et al. (2006), more (or less) number of cyclones could have been generated. That would have meant more computing requirements for both cyclone generation and storm surge computation.

As we have shown in Figure 4, this large number of cyclones allows a dense spatial sampling of cyclones, which in turn allows us to compute a large-range of the return period of storm surges with a high level of confidence.

3. These synthetic tracks are considered equivalent to 5000 years of storm activity to estimate the storm surge-induced water level at various return periods. Is it possible to identify any relationship between the return period of the maximum storm intensity and the surge water levels from the synthetic track properties? It could help recognize the role of different storm properties such as the maximum storm intensity and track translation speed in generating the surge level.

Reply: Given the large size of the sampling, our expectation is that it should shed some light on the role of various properties of the cyclone on the surge. For a given point of interest (POI), we have listed the following storm properties -

1. Storm intensity (wind and pressure)
2. Landfall location (left or right of the POI)

3. Approach angle with the coast
4. Translation speed of the storm

As discussed in the paper, as well as Krien et al. (2017), Khan et al. (2020) - the storm surge generated from a given storm characteristic will further be modulated by the following physical and hydrodynamical property -

1. Location of the POI (e.g., depth)
2. Phasing with tide, and tide-surge non-linear interaction

Because of this interaction and interdependence between tide, surge, total water level, and local bathymetry setting, understanding the role of various parameters is not straightforward to analyze or compare. Each analysis needs to be segmented into multiple bins of storm properties and multiple bins of water level for distangingling the tide. The analysis also would need to be performed segment-by-segment over the Bengal coastline.

Hence, the analysis regarding the role of the various storm parameters on the surge level would deserve a separate study by itself, which is beyond the scope of the present paper. We have plans to continue working on this large ensemble dataset to further disentangle the role of various factors and quantify their relative contribution and communicate our results in the future as a full-fledged article.

4. In page 9, lines 222-223: 'Following their findings, we have used a combination of Emanuel and Rotunno (2011) and Holland (1980) formulations to derive the parametric wind profile.'

The Holland model's parametric wind and pressure field representation could generate unrealistic storm surge, especially when the track is slow-moving before the landfall and has higher intensity. As the study depends heavily on this atmospheric representation, it is crucial to show additional verification here. The author could add a comparison of the surge level generated during a historic event (e.g., cyclone Sidr), using a global reanalysis wind dataset (e.g., ERA5) and the parametric representation used in this study. This analysis can show the maximum surge bias using the parametric wind model and add more credibility to the later results.

Reply:

We understand the concern of the reviewer, and upon reviewing the section in question, it is clear that all the details of our analysis were not presented. There are several folds of the answer to this query, and we will try to cover them thoroughly one by one.

About the ability of reanalysis wind fields to capture the storms

Our findings show that reanalysis fields over the Bay of Bengal are not well-resolved to get the extreme water level. To demonstrate that using reanalysis fields solely is not enough for hindcasts, we present a comparison of maximum wind speed and central pressure during cyclone SIDR between JTWC Best Track (based on satellite observation), and two state-of-the-art reanalyses (ERA5 and CFSR v2) in Figure C2. This comparison clearly illustrates that both intensity parameters of the storms - wind and pressure - are reproduced poorly by the reanalyses, with a large underestimation of their intensity.

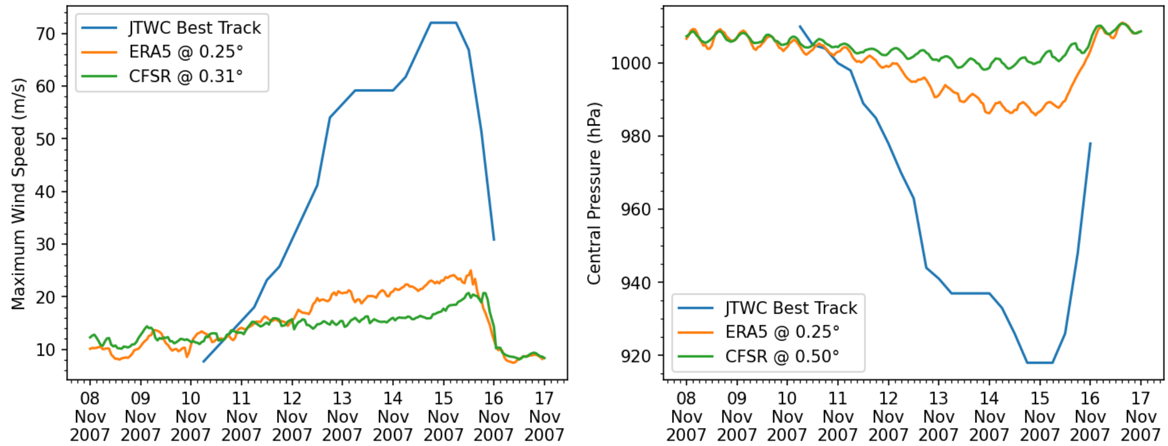

Figure C2. Comparison of Maximum wind speed (left) and Central pressure (right) during cyclone SIDR among JTWC Best Track (blue), ERA5 (orange), and CFSR (green).

This question was discussed in our previous paper (Khan et al. 2021) aimed at simulating the cyclone Amphan. We have tested storm surge modelling particularly for GFS, and we found that the GFS vastly underestimates the drop in the central pressure. The storm surge is not well reproduced for a GFS-only simulation. This is illustrated in Figure C5 and Figure C6 of the Author reply document of our article (https://doi.org/10.5194/nhess-2020-340-AC1). The pdf file can be directly accessed here - https://nhess.copernicus.org/preprints/nhess-2020-340/nhess-2020-340-AR1.pdf

In other words, global reanalysis fields are too strongly biased to bear relevance for a meaningful assessment of an analytical wind and pressure field in a cyclone, hence no further revision is done in the manuscript in this regard.

Bias in analytic wind field

We agree with the Reviewer that there is some bias in individual analytical wind field formulas (Krien et al. 2018). The keyword here is 'individual'. We would like to clarify that we did not use solely the Holland model for wind, but an ad-hoc combination of Holland (1980) and Emanuel and Rotanno (2011) models based on radial distance, so as to minimize this bias. In the preprint, L302-304 reads as follows:

“Here, for the inner core of the cyclones (r < r50), the wind field is estimated using the model of Emanuel and Rotunno (2011). On the outer core (r >= r50), we used the Holland (1980) model.”

This combination technique used here is based on the findings by Krien et al. (2018). By comparing the analytic fields with satellite scatterometer data, they showed that Emanuel and Rotanno (2011) model fits well to the inner core of the cyclone, and Holland (1980) fits well to the outer core. Hence, by combining these two analytical models the bias from a single analytical model (which is the concern raised in the review question) is reduced.

Although we discussed this combination technique in Section 2.3 and referred to Krien et al. (2018) there, the reference was left-out in the description of Section 3.2. We revised our manuscript to explicitly mention it again in Section 3.2 for clarity.

L302: “...as explained in Section 2.3. For the wind fields, we again employed a combination of parametric models based on the findings of Krien et al. (2018) to minimize the bias in individual parametric models. Here, for the inner core...”

Simulation detail

We have done two simulations for Sidr -

1. Same as Krien et al. (2017) with an analytic wind and pressure field in the inner core of the cyclone, and CFSR V2 dataset in the outer field of the cyclone. This combination of an analytic wind field and a background field from an atmospheric model gave a good reproduction of the wave fields at two buoys in the Bay of Bengal (see Krien et al. 2017). Similar results were found by Khan et al. (2021) for cyclone Amphan - where the analytical field was combined with GFS fields.
2. A sensitivity simulation of Sidr, only with an analytical field, to illustrate that the peak water level is captured.

Indeed, we can follow the strategy of combining an analytical field with a modelled atmospheric field in case of an observed/ongoing cyclone (Krien et al. 2017, Khan et al. 2021). But for the ensemble, no such atmospheric model field can be accessed - hence can not be merged. The objective of the Sidr simulation with only the analytical wind field is to illustrate that the analytical wind field captures the water level peak correctly around the landfall location. Hence, we purposefully used a simulation of Sidr with only an analytical wind field (Simulation 2 above) for consistency with the ensemble simulations.

We revised the corresponding text (L243-251) to clarify the simulation details of cyclone Sidr, as follows -

“To supplement the results shown in Khan et al. (2021a), we also performed a hindcast of the water level generated by cyclone Sidr – another comparatively well-documented historic cyclone (Krien et al., 2017b). Figure 3 shows the water level evolution at four tide gauge stations around the landfall location. The modelled water level is shown in black lines, and the observed water level records are shown in dots. As noted by Krien et al. (2017b), the tide gauge stopped working at Khepupara at the time of very high water. The error of the peak modelled water level typically amounts to 10-20 cm. It is to be noted that, in their hindcast, Krien et al. (2017b) shifted the time of landfall by half-an-hour to get a better match of the tidal propagation. Here we retained the original JTWC track hence the slight shift in phase of the water level. Moreover, both Krien et al. (2017b) and Khan et al. (2021a) combined their analytical wind and pressure field (around the storm center) with a background field (far from the storm center) taken from global atmospheric models. Here we used only the analytical wind fields to be consistent with the forcing strategy used for the storm surge computation from the cyclone ensemble (Section 3). The results from this experiment illustrated in Figure 3 show that the peak water levels around the landfall are well captured. This peak water level is the main variable we will deal with in our storm surge hazard assessment."

5. In page 16, lines 367-369: 'The inundations in the mangrove region around 89.5°E seem to have a large saturation effect ... slightly rising from 2.5m at 50-year return period to 3m at 500-year return period'

If we compare the water level during Sidr in Figure 3 with Figure 6, we can see that close to 89.9 and 22 the floodwater during Sidr represents a 250-500 yr event. Is it realistic? How does it compare with the historical information?

Reply:

There is unfortunately no observational historical record to check whether our estimate is correct. However, the consistent behaviour of our model and data throughout the Bengal coastline indicates the estimates are reasonable. Our reasoning is as follows -

First, Sidr is one of the strongest events in recent decades, with an estimated death toll of 15000 people. Sidr was an Extremely Severe Cyclonic Storm (Indian Meteorological Department classification). With peak 1-minute sustained winds of 260 km/h (about 140 knots), it was a Category-5 equivalent tropical cyclone on the Saffir–Simpson scale. If we consider the distribution of Figure 4(b) in the manuscript, Sidr was a very rare event in the record.

Second, the Bengal delta coastline is long, and the flooding pattern is primarily restricted to the surroundings of the landfall location (Khan et al. 2021). There are other historical events - such as the 1970 Bhola cyclone which took 500000 lives, or the 1991 Bangladesh cyclone (also known as Gorky) which took 138000 lives. These cyclones made landfall over the Chittagong coast (over 200km to the east of the Sundarbans mangroves) - where the tidal range is high and our results also show significantly higher total water levels at all return periods. In other words, our method is predicting high water levels, where historically multiple very large storm surges were recorded.

Third, our modelling platform has been tested previously in Krien et al. (2017) and Khan et al. (2021) and performed well in this region. We also show in the manuscript that the storm climatology in our ensemble is consistent with the available observed climatology (Figure 4). Hence, we tend to trust the estimate of return level we get from these models and data. Additionally, mangroves are known to damp the tidal-surge waves by acting as an energy damping zone (implemented as manning friction in our model). Hence, the saturation we have seen is expected and our modelling result is showing as such.

In a brief, using a realistic storm surge model forced with the realistic distribution of storms, we can expect the output to be realistic too. The objective of this paper is to fill in the knowledge gap of the storm surge hazard in the study region that arises from the absence of historical data, as stated in the Introduction section (see L59-65).

6. In page 16, lines 382-383: 'Along the Hooghly estuary, the sensitivity of the water level to the return period is moderate for the first 100km but amplifies considerably further upstream.'

Why do we see almost no change for 50 and 500 yr events at the downstream side, but then it shows a significant increase for the upstream part? How is this surge generating there?

Reply:

First of all we would like to point out that the keyword here is water level, not surge. As we have explained in our manuscript (L35-40, L121-127), due to non-linear tide-surge interaction, the water level is not a linear combination of tide + surge + wave setup but rather a non-linear, dynamic one. We have thus consistently discussed the water level, instead of surge (surge being defined as total water level minus tidal prediction L20). In Figure 7, we have shown the amplification of the water level estimate with respect to the water level at the 50-year return period.

The reason behind this is not entirely clear to us, but one possible reason is the amplification of higher levels of surges. In our 2020 paper (Khan et al. 2020), we show that tidal properties change substantially along the Hooghly estuary. To add further justification to our reasoning, we analyse two points from Figure 7 (original manuscript) - Sagar Roads (located at 25km) and Diamond Harbour (located at about 100km).

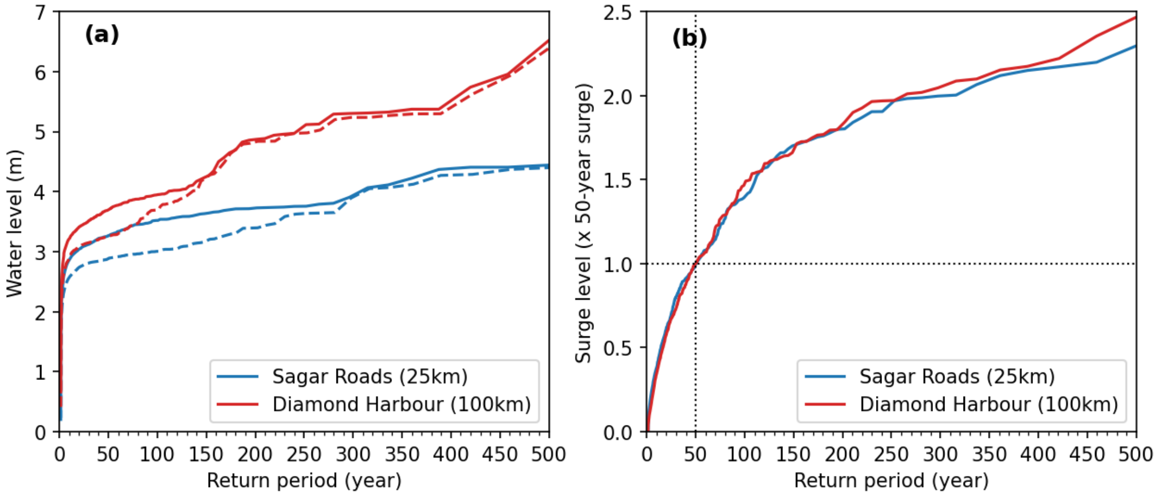

Figure C3. (a) Total water level at the given return periods for Sagar Roads (blue) and Diamond Harbour (red). The solid lines are from the ensemble simulation with coupled tide, surge and wave and the dashed lines are only coupled tide and surge but without waves. (b) Only surge level as multiple of 50-year surge level for Sagar Roads (blue) and Diamond Harbour (red). The surge level is simulated through the coupled wave model but without forcing any tide, i.e., always at mean sea level (MSL).

First, we analyse the total water level at various return periods for these two locations (Figure C3a). Two modelling configurations are used - one with full coupling of tide-surge-waves (solid line) and another is only tide-surge but without waves (dashed line). From the result, the contribution of waves in total water level is clear. The contribution varies among the two stations, but in general the amplification along the estuary remains the same as what we have reported in our manuscript - e.g., total water level amplifies after 100km compared to 50-year return period water level. This in turn indicates that the wave setup is not the component causing this amplification. Then the component that remains is the tide.

Second, to see if the tide is creating the upstream amplification, we extracted the surge estimate from the tide-free version of the storm simulations ensemble. The mean water level is fixed at 0m MSL for these simulations. The surge at various return periods is shown in Figure C3b, as a multiplication of the 50-year surge level. We see that between upstream and downstream the evolution of surge with a return period remains practically the same. This indicates the evolution of the total water level shown in Figure 7 is caused by the non-linear combination of tide, surge and wave-setup.

We have also revised our manuscript:

“Along the Hooghly estuary, the sensitivity of the water level to the return period is moderate for the first 100km but amplifies considerably further upstream. This amplification appears to be linked to the changes in the tide along the estuary (See Supplementary materials)."

7. In page 20, lines 429-431: 'We have first extracted the tidal water level from the 3600 cyclones that we simulated ... our estimation of surge amounts to 1.8m at Hiron point.'

This extraction needs to be explained better before going into the comparisons. How did the author separate the surge during high and low tides? Sometimes, the residual (total water level - tides) during a low tide could be higher than the residual during a high tide, and it doesn't necessarily represent flooding.

Reply:

We believe that by the separation of tide and surge the reviewer is referring to the issue of tide-surge interaction and their dependence. In practice, the surge is computed classically as the difference between the total water level and pure tidal water level. This mathematical definition means that, often a high surge occurs during a low-tide, but does not actually cause flooding. To alleviate such issues, new metrics such as the skew-surge have been proposed (e.g. de Vries et al., 1995), which computes surge as a difference between maximum water level and nearest maximum tidal water level.

However, the section indicated in the comment focuses on comparing with a published result by Lee (2013), as such it is necessary to analyse similar variables. As we have explained in the manuscript - L422-433, Lee (2013) used Ensemble Empirical Mode Decomposition to remove the tidal signal and for detrending the water level. Once the tidal signal is removed, they use the yearly block maxima to perform an Extreme Value Analysis by fitting into the GEV distribution. To have the maximum compatibility with the random variable used by Lee (2013) (e.g. maximum non-tidal residual), we have done the following as explained in the manuscript (L428) -

1. Computation of tidal water level for the full ensemble of cyclones.
2. Computation of the temporal maximum of total water level minus tidal water level to get a set of maximum surge values for the storm ensemble. This is the closest to what Lee (2013) analysed, in terms of the random variables.
3. Computation of the statistics as described in Section 3.3

In light of the additional confidence interval estimate, we have revised the corresponding sections in the manuscript. The sections now read as follows -

"Using a yearly-maximum method, in his extreme value analysis, he obtained a 1.66m [1.50-1.95] surge level at 50-year return period, and 1.75m [1.57-2.14] at 100-year return period. The range in the parenthesis is the 90% confidence interval.

In the previous section, our analysis was focused on the water level rather than surge level. To compare with the estimate of Lee (2013), we reprocessed the whole ensemble of storm event simulation results. We have first extracted the tidal water level from the 3600 cyclones that we simulated. Then, for each cyclone, we extracted the maximum surge level. Finally, on this maximum surge estimate, we applied the same ranking-based return period estimate. At Hiron point, our estimation of surge amounts to 1.77m [1.68-1.85] at 50-year, and 2.31m [2.12 - 2.47] at 100-year return period. The range in the parenthesis is the 90% confidence interval. At 50-year return period, with a difference of only 14cm (inferior in Lee (2013)), our estimated value is comparable to the estimated value by Lee (2013) from the observation time series. The confidence interval was about twice narrower compared to Lee (2013). To be noted that, the estimated 50-year return period water level from our ensemble is about 3m at Hiron point. At 100-year return period the estimate of Lee (2013) was underestimated compared to ours by 56cm. See supplementary for further details."

8. In page 20, lines 441-442: 'However, the limited and potentially biased sampling of the "strongest" cyclones (17 in total, over 40 years) leads to an overestimation of the storm surge level.'

Jakobson et al. (2006) estimated the return period water levels using historic storms; here, we are looking at synthetic cases in this study. I don't think it is a fair comparison here.

Reply: We agree this statement lacked clarity. The objective of our discussion was to compare with all the available previous results. Indeed, Jakobsen et al. (2006) tried to establish a return period of storm surge based on modelling along the Bengal coastline, and our objective was in no way to criticise this valuable work.

Saying that, Jakobsen et al. (2006) themselves mentioned that they analysed "17 severe cyclones", and identified it as a limitation of their study. We think that our comparison is fair as we have voiced the same limitation as the authors themselves in their original paper. Additionally, it is needed in the manuscript for comprehensiveness.

We propose to update the word "strongest" with "severe" taking verbatim the wording from Jakobsen et al. 2006 (L441).

9. In page 21, section 5.4: 'The maximum modelled water level reached about 5m around (88.4°E), which corresponds to a 250-year return period.'

Again, the statement here is from the results of this study. Can we verify it?

Reply:

We confirm the result is from our study. The results are shown in Figure 3 for Sidr, and Figure 6(g) for the 250-year return period water level. We humbly point out that no such validation dataset exists for return period estimates of water level, and we hope to fill in this knowledge gap through this paper itself (Please see Introduction section, L59-65).

We revise our manuscript as following -

“The maximum modelled water level reached about 5m around (88.4°E) (Figure 3a), which corresponds to a 250-year return period (Figure 7g).”

Please note that Figure 7g refers to Figure 6g in the initial manuscript.

10. In page 22, section 5.6

Before delving into this analysis, the author needed to show some model overland inundation comparisons with the high-water mark data sets for a storm event. Otherwise, there is no way to verify this crucial information and could be misleading because of the potential inaccuracies in the topographic data.

Reply:

We agree the potential inaccuracies of the topography is a very relevant issue. Unfortunately again, no such consolidated dataset of high water mark exists for comparison, to the best of our knowledge. Whenever we could find some information (e.g. Islam et al. 2011), it was highly unreliable due to missing datum references.

We have discussed this just before we delve into the inundation exposure (Section 5.5). We also indicate the limitation of our estimate regarding the possible existence of city protection embankments (beside the coastal protection embankments included in our model), for which data is not publicly available and not included in our model (L526-528). We also give a reminder about this limitation in the conclusion (L555-559).

Other comments

1. In page 4, line 95: ‘Additionally, the storm parameters ...’

What are these parameters?

Reply: We propose to update the line as following -

Additionally, the storm parameters - notably maximum wind speed, radius of maximum wind, and central pressure - are collected over a long period, and the homogeneity of the storm records is not well defined (Singh et al. 2020).

2. In page 6, section 2.1

The model runs are in 3D? If so, how many sigma layers are used? Also, how the Coriolis force is defined in the domain?

Reply:

We are sorry for not having mentioned this information explicitly in our manuscript.

The model configuration used in this study is a 2D barotropic one. Past studies demonstrated the model's capability to predict storm surge quite accurately in 2D barotropic mode (Krien et al. 2017, Khan et al. 2021).

As the model is discretized in spherical coordinates, the coriolis parameter is variable over the domain, computed locally at nodes, based on latitude using the exact expression $f = 2\omega \sin(\varphi)$.

We propose the following revision for the model description.

L161: "...built from the original SELFE (Zhang and Baptista, 2008). SCHISM solves the standard Navier-Stokes equations with hydrostatic and Boussinesq approximations in an unstructured grid, which can be discretized using a triangular or hybrid triangular-quad element. SCHISM also includes..."

L174: "... its surroundings (FIgure 2a). The model mesh is defined in latitude-longitude (spherical grid). We have used variables..." "... 1.1 million triangular elements." "..." "...The model transforms the coordinates and most of the calculation is done on a local frame. For all simulations described in this article, we have used a 2D barotropic configuration, which is shown to well-reproduce the tide and storm surges (Bertin et al. 2014, Krien et al. 2017, Khan et al. 2021). A time step of 300 seconds..."

3. In page 10, line 248: '... the time of landfall by half-an-hour to get a better match of the tidal propagation.'

Surge propagation?

Reply:

We agree our statement was misleading. We meant tide-surge evolution - e.g., total water level. Krien et al. (2017) postulates that the landfall timing reported by JTWC was off by half an hour, which in turn stimulated their model as a shifted peak water level.

We propose to replace "better match of the tidal propagation" with "a better match of the timing of the peak water level."

4. In page 11, Figure 3

Please show the cyclone Sidr track on top of Figure 3a.

Reply:

The track is now shown. The figure is updated as shown in Figure C4.

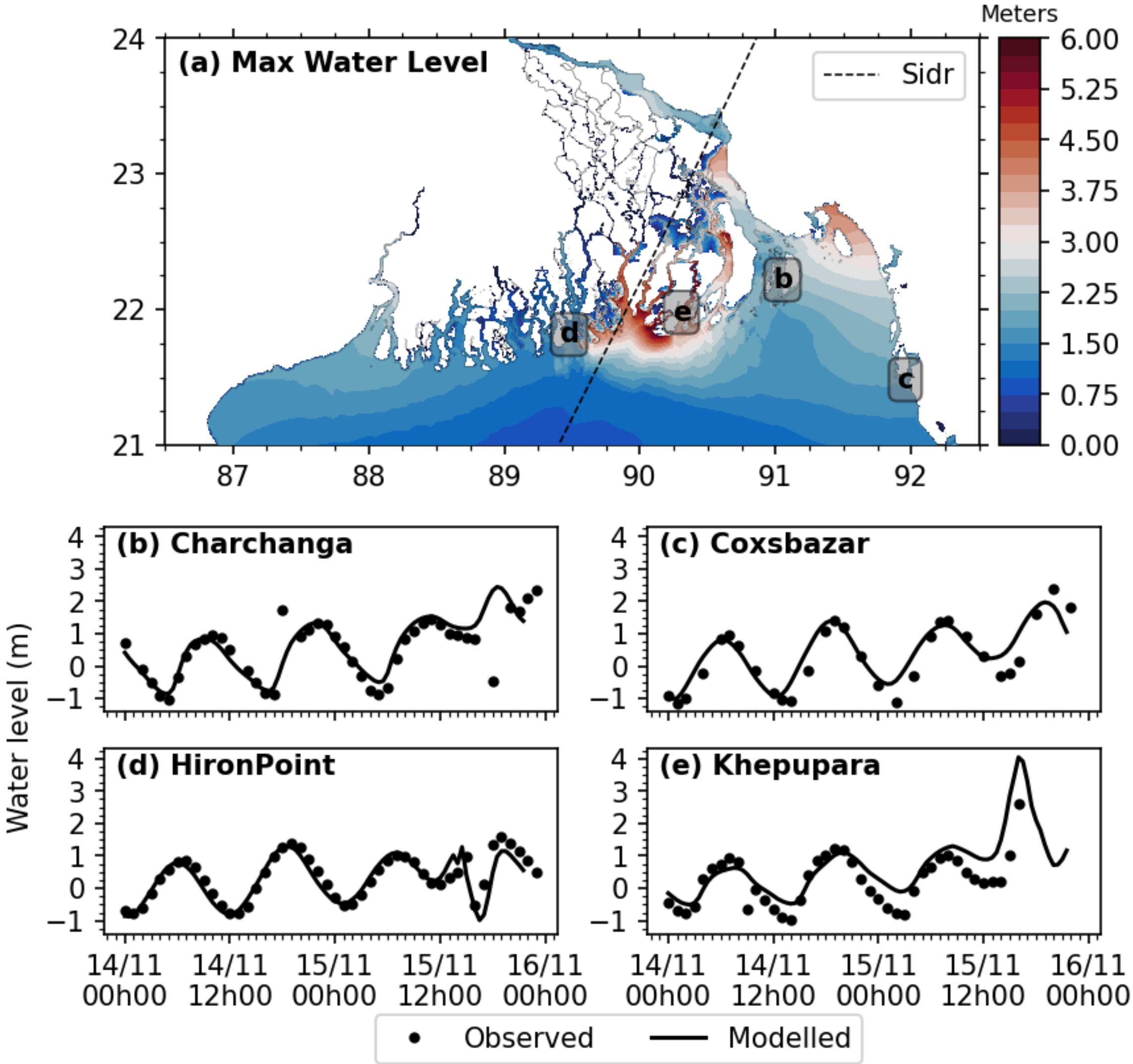

Figure C4. Updated Figure 3.

5. In page 13, lines 306-307: 'Second, with wind and pressure fields ... hindcasts described in the previous section.'

Do the tidal forces also match the timeline of the synthetic storms?

Reply:

Yes, the tidal forcing matches the timeline of the synthetic storms. However, the synthetic storms only have day, month, and hour (being a climatology of storms by design), but do not have any year attached to it. We have randomly chosen the year for each cyclone, with equal probability for each year in the climatology period 1980-2015.

We have revised our manuscript as follows -

“... and Kharnaphuli (Chowdhury and Al Rahim, 2012). Applied tidal forcing and the tidal water level boundary are also consistent with the timestamps of the synthetic cyclones. Similar to the model setup ...”

6. In page 14, line 347: ‘According to the polder embankments dataset used ...’

How are they incorporated into the current model setup?

Reply:

Reviewer 2 also posed a similar question. Our regular grid bathy-topo dataset (with highest resolution of 50m) does not capture these embankments. The width of the embankment in reality is in the order of 10-20m. The original embankment datasets are provided by the Bangladesh Water Development Board (BWDB) as line shapefiles. The correct characterization of the embanked areas would be - polder - e.g, a region surrounded by embankments. One such region is shown in Figure C5. In local use, polder became synonymous with the embanked region as well as the embankments themselves.

To incorporate the shapefile dataset into the grid, from the original line shapefiles, a buffered polygon is first created. The lines from the shapefile are taken as the outer edge of the polder during this process. The buffer size is controlled by the target mesh resolution. In our case, we took a buffered area of 300m - and assigned all the mesh node points inside this buffered area to have the height corresponding to the respective embankment height. Additionally, during mesh generation, we have forced the mesh generator to follow the embankment shapefile while assigning position of the nodes. This process assigns the embankment heights along the edge of an element. This process sets the embankment height to one or two nodes of a triangular element, sometimes all three (for example, when the embankment has a strong curve, or two side-by-side embankments are separated by sub-resolution distance.).

In this study we used 10cm as the threshold for the wetting-drying algorithm. That means, if all nodes of a given mesh element have a water level above 10cm, the element is considered wet, otherwise dry.

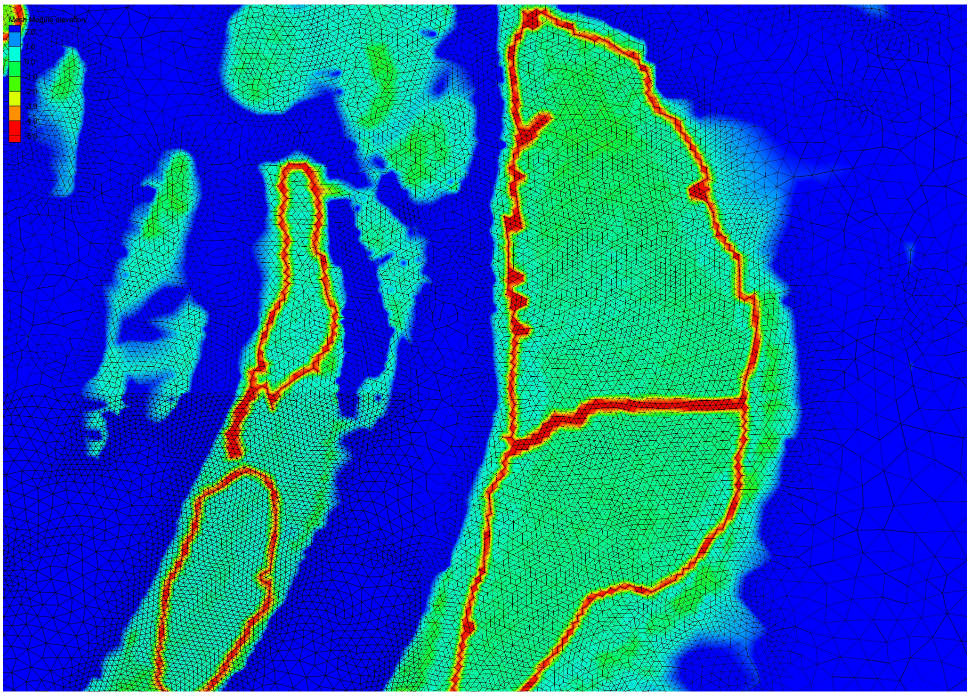

Figure C5. Embankments as incorporated into the model. Snapshot for Haitya Island (91.11°E, 22.24°N).

We have also revised the manuscript with the following lines -

“...1.1 million triangular elements. In order to take into account the embankments information, we have aligned the mesh nodes along the contour of the embankments and set the height values of these nodes to the dikes levels provided by BWDB. The flow above the top of the embankments is controlled by the wetting-drying algorithm of SCHISM just like everywhere else over the modelling domain. The model transforms...”

7. In page 15, lines 356-357: ‘... contrasting range of MHW along the shoreline – with two macrotidal poles ...’

A tidal MHW map needs to be added to Figure 5 as a subplot to illustrate this better. It will also help the description written in section 5.2.

Reply:

We thank the reviewer for this suggestion. We have updated Figure 5 with a tidal Mean High Water map.

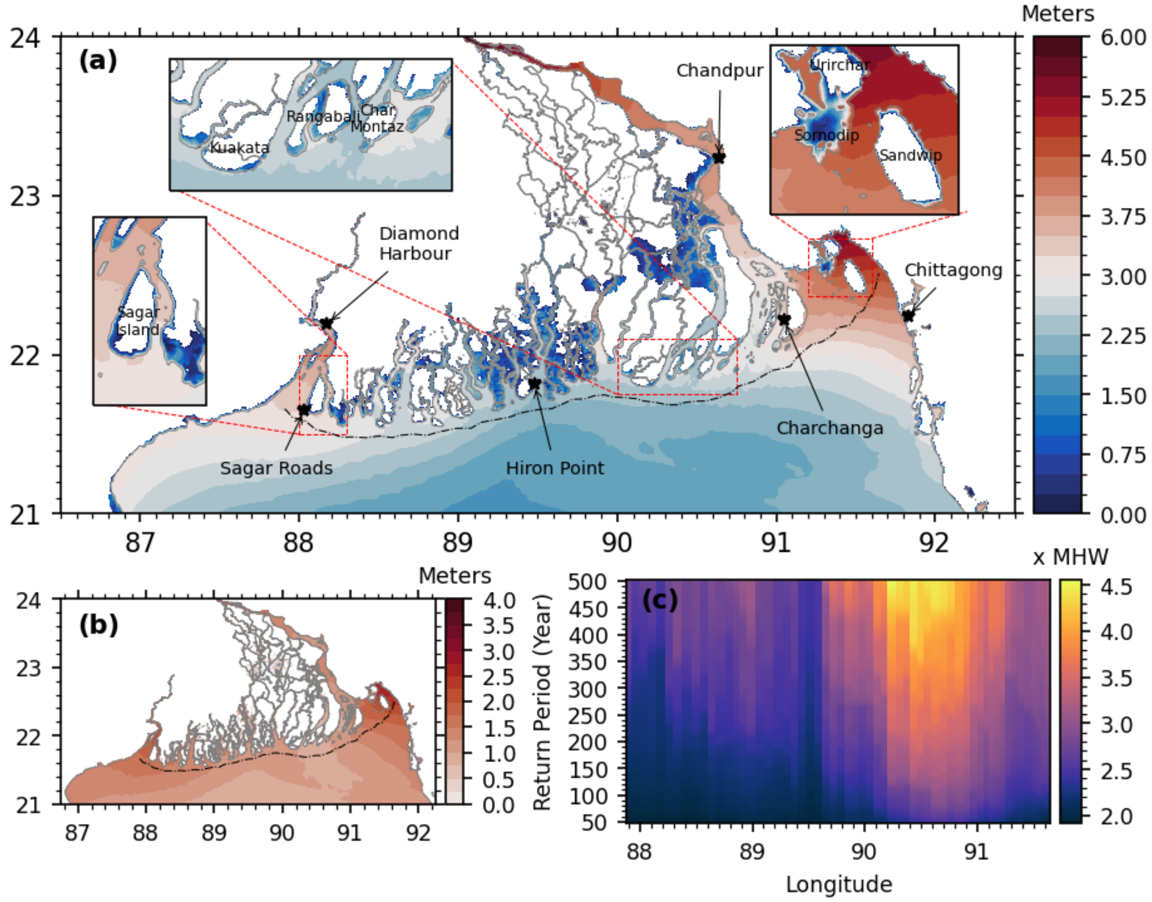

Figure C6. (a) Inundation extent and corresponding water level at 50-year return period. Black star shows the tide gauge location where the confidence interval is reported in Figure 6. (b) Mean High Water (MHW) derived from a year-long tidal simulation. (c) Water level for the 50-500 years return period expressed as a multiple of the MHW level along the nearshore dash-dotted line shown in (a) and (b).

We have also updated corresponding lines in the manuscript. In particular, now L337 reads as “The 50-year return period water level shows a similar spatial pattern as the mean high water, as shown in Figure 5b, as well as the tidal range (Khan et al. 2021)”

8. In page 19, line 400: ‘This landfall pattern corresponds to previous observations that the landfalling cyclones in the Bangladesh coastline tend to move north-eastward (Ali, 1996).’

Does the JTWC observed data also support this statement?

Reply:

This statement is supported by the JTWC dataset and discussed in further detail supplemented with numerical modelling in a recent article by Akter and Tsuboki (2021), for the storms recorded during the 1990-2019 period. The same conclusion was also drawn by Mondal et al. (2021). We updated our manuscript to add these references as following -

“This landfall pattern corresponds to previous observations that the landfalling cyclones in the Bangladesh coastline tend to move north-eastward (Ali 1996, Akter and Tsuboki 2021, Mondal et al. 2021).”

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