Replies to comments of Reviewer #2

This paper presents an interesting strategy to address surge and inundation in the bay of Bengal, based on publicly available model and forcings. The quality of the results is much larger than the previous efforts and a detailed analysis on process dominance is presented. The proposed strategy, although apparently customized to this site, is interesting and worth publishing. Below I have outline major and minor issues to be solved before approval, some of them requiring new runs. Furthermore, the paper is extremely long (sometimes it looks more a report than a scientific paper) and lacks now and then a clear presentation direction. The paper should thus be reviewer for conciseness and easy of reading.

I consider the paper to be accepted with major reviews.

Reply: We thank the reviewer for his/her insightful comments. Following the comments, we propose the removal and revision of several paragraphs as explained hereafter, in order to reduce the length and to increase the clarity as well as the readability of the manuscript.

We also propose to reorganize the current Appendix to Supplementary materials to keep the main manuscript concise.

Abstract

"Despite recent advancements, the complex morphology and hydrodynamics of this large delta and the associated modelling computational costs impede the storm surge forecasting in this highly vulnerable region." – Nowadays, super computers are available to perform forecasts of much larger and more complex domains, integrated with atmospheric models. The author should review this sentence maybe focusing on the quality of the forecasts and the necessary grid refinements and process-knowledge for high quality results.

Reply: We agree with the suggested changes. We propose the following revision at L1-3:

"Despite recent advancements, the complex morphology and hydrodynamics of this large delta and the associated modelling complexity impede accurate storm surge forecasting in this highly vulnerable region."

"This article shows the proof of the maturity of our framework for operational implementation, which can particularly improve the quality of localized forecast for effective decision-making" – Is the framework generic or only applicable to the bay of Bengal? The authors should clarify this issue at the abstract.

Reply: Our modelling system, as well as the forecasting procedure demonstrated in this study, are generic enough to be directly applicable to other regions. We thank the reviewer for suggesting this improvement. We propose to update the line L8-9 as follows:

“The article shows the proof of the maturity of our framework for operational implementation, which can particularly improve the quality of localized forecast for effective decision-making over the Bengal delta shorelines as well as over other similar cyclone-prone regions.”

Line 36 “global weather and forecasting system” – a word is missing of the “the” word needs to be removed.

Reply: We suspect a misunderstanding here. To us, our sentence is grammatically correct:

" Over the last decades, global weather and forecasting systems have advanced significantly."

Lines 44-45 “Nowadays, operational surge forecasting systems typically run on high-performance computing systems, either on a scheduled basis or triggered on-demand during an event (Khalid and Ferreira, 2020)” – The authors should include other references of such system, either applicable at a specific site or of generic application.

Reply: We agree. We added the following recent references: Loftis et al., 2019; Oliveira et al., 2020

Lines 50-52 “Storm surge forecasts have shown their potential to better target the evacuation decision, to optimize early-engineering preparations, and to improve the efficiency of the allocation of the resources (Glahn et al., 2009; Lazo and Waldman, 2011).” – again an updated and more comprehensive review is missing, along with the identification of what are the major challenges in developing and keeping in operational mode this type of systems.

Reply: We have updated the segment as following –

“Storm surge forecasts have shown their potential to better target the evacuation decision, to optimize early-engineering preparations, and to improve the efficiency of the allocation of the resources (Glahn et al. 2009; Lazo and Waldman, 2011, Munroe 2018). Availability of a spatially-distributed forecast of storm surge flooding can further increase the fluidity of communication toward the public (Lazo et al. 2015). Keeping in mind the cyclonic surge hazards over the densely populated Bengal delta, having a reliable real-time operational forecast system in the region would be extremely valuable and would address a societal demand (Ahsan et al. 2020). The major challenges in operating and maintaining such systems are manifold for the Bengal delta, including lack of expertise, limitations of funding resources to operate and maintain necessary infrastructure and dataset, as well as availability of reliable modelling systems in operational mode (Roy et al. 2015).

Lines 58-60 “In the past decade, unstructured-grid modelling systems are getting more and more popular due to their efficiency in resolving the topographic features and their reduced computational cost compared to structured-grid equivalents (Ji et al., 2009; Lane et al., 2009; Melton et al., 2009).” – all these references are not from the last decade. Part of them do not address operational forecast systems. There are several examples of unstructured grid forecast system in operation, some recent some in operation for over a decade. The authors should review carefully the state-of-the-art and improve the current text.

Reply: In this particular sentence, only the unstructured-grid modelling aspect is focused, irrespective of forecasting context. However, we agree with the reviewer regarding the state-of-the-art forecasting systems and propose to add the following references - Fortunato et al. 2017, Khalid and Ferreira 2020.

Both of these references are already cited elsewhere in the current manuscript.

Line 100 “Due to this interaction, the highest surge is obtained for a storm making landfall around 2 hours before the high tide.” – a detailed explanation (or references explaining it) is needed. Is it associated with the specifics of the geometric/bathymetric characteristics of the bay or generic? Is it tidal amplitude dependent?

Reply: To clarify, we propose to add references to Krien et al. 2017 and Antony et al. 2020 as references to numerical investigation, and Antony et al. 2013 for simplified mathematical insight. The tide-surge interaction is a generic phenomenon, which evolves dynamically, and which is prominent at locations with shallow submarine zones and/or strong tidal currents. These past studies suggest that the tide-surge interaction is generic and depends on the tidal amplitude and tidal phase, which are primarily controlled by the regional bathymetry, regional coastline geometry, and the ocean bottom roughness. The contributing processes include – shallow water effect (depth), advection term, bottom friction term. The “non-linear” interaction is thus dependent on these regional features and does not have a one-to-one relationship with tidal amplitude.

The statement is a rough estimate when the highest water level can be observed based on previous experiments (e.g. Krien et al. 2017). In the English channel, for instance, the maximum surge height is reached 4-6 hours before high tide (Idier et al. 2012). However, no estimate is yet available across the Bengal delta shorelines. Due to above-mentioned contributing processes, the response is expected to vary along the coastlines and inside the estuaries.

We propose to update the line as following –

"Due to this interaction, the highest surge (water level – tide) does not coincide with high tide."

Line 165-169 "Our bathymetric dataset is a blend of two digitized sounding datasets in the nearshore zone – one from navigational charts produced by Bangladesh Navy, and another being a bathymetry of the Hooghly estuary provided by IWAI (Inland Waterways Authority of India)"- are there any common areas between the two sources? if yes, what was the combination procedure? If not, substitute "blend" by "combination". How old is the data?

"The river bathymetry is composed of a set of cross-sections obtained from the Bangladesh Water Development Board (BWDB)." – what is the spacing between profiles? How old is the data?

Reply: As suggested, we adopt "combination" to better represent the characteristic of the merged dataset. Depending on the locations, the data are 7 to 20 years old.

The spacing of the sections of the river profiles is typically 10-30Km. However, these cross-sections points are interpolated using dedicated interpolation tool (HEC-RAS + GIS) to about 300 m spacing before combining with the rest of the bathymetric dataset. Depending on the regions, the cross-sections are 10 to 15 years old.

We propose the following modification to L165-169:

"Our bathymetric dataset is a combination of two digitized sounding datasets in the nearshore zone – one from navigational charts produced by Bangladesh Navy, and another being a bathymetry of the Hooghly estuary provided by IWAI (Inland Waterways Authority of India). Depending on the sounding points, these observations are 7 to 20 years old. The river bathymetry is composed of a set of cross-sections obtained from the Bangladesh Water Development Board (BWDB), which is further interpolated at about 300m resolution using dedicated 1D river modelling tool and GIS techniques."

Lines 194-195 "At each of the upstream river open boundaries of Ganges, Brahmaputra, Hooghly, and Karnaphuli, we implemented a discharge boundary condition" – what is the source of the discharge values?

Reply: For the benchmark tidal simulation, for Ganges and Brahmaputra, the discharge is estimated from observed water level provided by BWDB and appropriate rating curves. For Hooghly and Karnaphuli, climatological discharge is taken from Mukhopadhyay et al. 2006 and Chowdhury et al. 2012 respectively. Finally, for the storm surge simulations in this study, a climatologic discharge is estimated from the discharge timeseries at Ganges and Brahmaputra.

We propose the following revision for L194-195 –

"At upstream river open boundaries of Ganges and Brahmaputra, a discharge time series from BWDB is forced for the benchmark tidal simulation, and a climatologic discharge timeseries is applied for storm surge simulations during Amphan. A climatological discharge is applied at Hooghly (Mukhopadhyay et al. 2006) and Karnaphuli (Chowdhury et al. 2012)."

Fig 4 – the map is unreadable. Place it at a larger scale.

Reply: We propose to revise the layout of Figure 4, making it full-width, as follows (Figure C1):

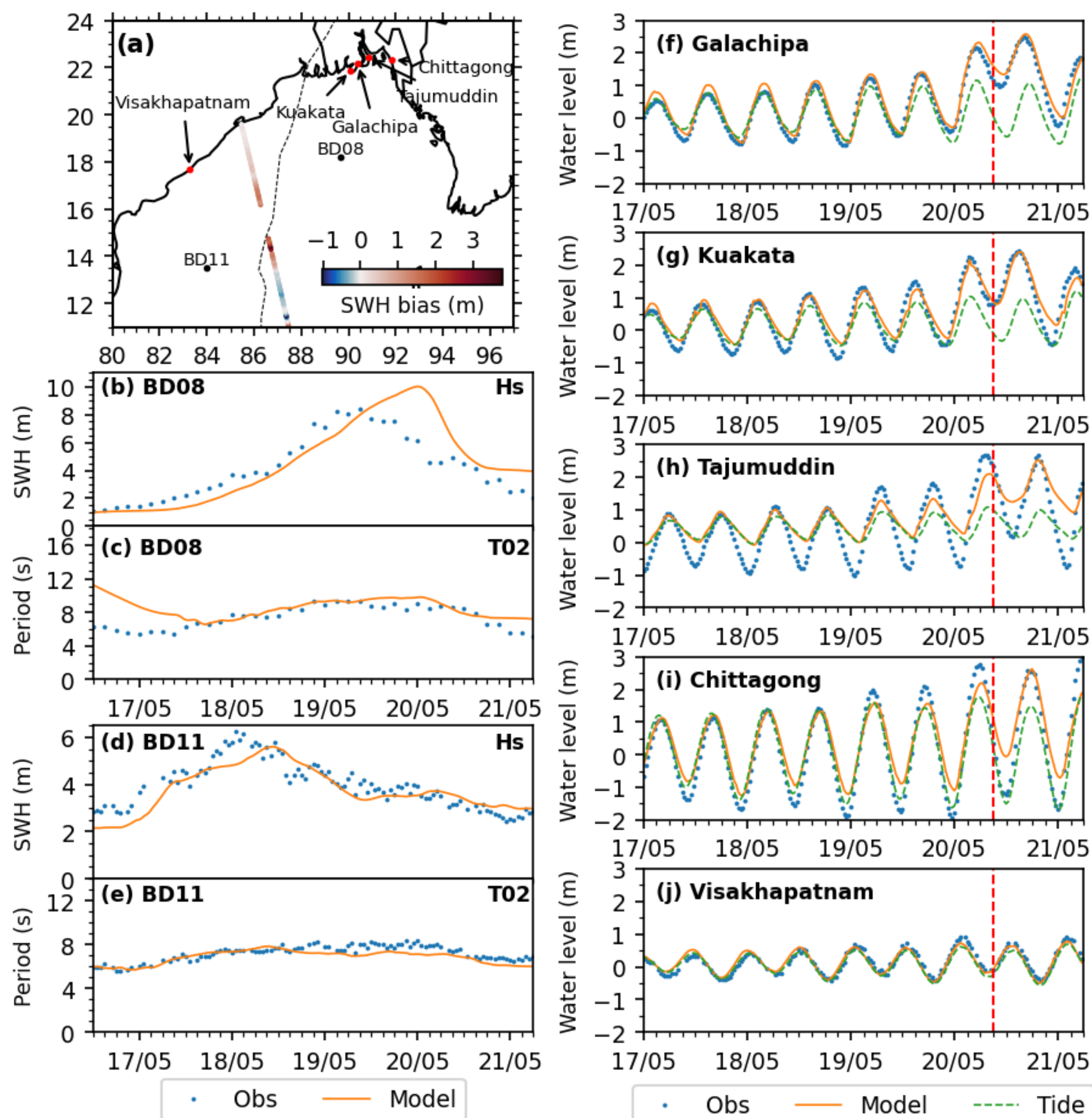

Figure C1. Comparison of simulated (in orange) and observed (in blue) water level, significant wave height (SWH) and mean wave period. (a) The map shows the along-track bias in SWH compared to the one calculated from Sentinel 3B altimeter overpass at 2020-05-18 1603Z. Bottom panel shows the modelled SWH and means wave period (orange line) compared to buoy observations (blue dots) at BD08 (b-c) and BD11 (d-e) provided by INCOIS. Comparison between observed (blue dots) and modelled (orange line) water level at the station locations – (f) Galachipa, (g) Kuakata, (h) Tajumuddin, (i) Chittagong, and (j) Visakhapatnam. Green dashed lines in (f)-(k) indicates the modelled tidal water level. Location of the buoys and the water level gauges are shown in (a). The vertical red lines in water level plots indicate the time of landfall.

Lines 200 to 209 this paper aims at evaluating a procedure (framework is not adequate in this context) to forecast storm surges and evaluates the procedure using a past event. However, when running an operational model, reanalysis are not available. Therefore the quality of the model should be evaluated with a past event (so data is available) but for a run under operational forecast conditions. The analysis of fig 4 should therefore by re-done under these conditions.

Reply: We do not believe it would be relevant to include the hindcast and model validation of another different event, for the sake of the conciseness of an already long paper. Indeed, we already tested a modeling framework in hindcast mode, for several past events with a configuration very similar to the one used in the present study (Krien et al. 2017). Essentially, our objective, as stated in the introduction (L. 80-81), is to assess the model in an operational mode. Still, we understand the need for validation of the present model. The objective of Section 4.3 and Figure 4 is precisely to evaluate the model. Thus the presented hindcast experiment is performed after-event. These results can be considered as the best we can achieve just after the cyclone has passed.

Lines 213 and following: why is the comparison limited to the storm path? The data is available in the whole domain.

Reply:

The significant wave height is estimated from available along-track altimetric measurements. Thus, the comparison is also along the track of the altimetric record over the whole domain. Given the repeat cycle of Sentinel 3 (27 days), we consider ourselves lucky in this regard to have Sentinel 3B flying nearby the cyclone track during a cyclone which lasted about 7 days. Additionally, outside of cyclone track region, the wave heights were small (Hs < 2m) (current Figure 4, beginning time of the BD08, and ending time of BD11), which is also well reproduced by the model.

Lines 274 and following: this text belongs in the introduction. Remove.

Reply: Agreeing to this point, we remove L274-L280 “A realistic… … …evacuation orders”. To maintain the continuity, we add the following sentence – “In this section, a near real-time storm surge forecasting scheme is presented using publicly available atmospheric forcing dataset.”

Lines 282-284: “We communicated the results to Bangladesh local government authority through personal communications, as well as to the scientific community through social media.” – this sentence is off context and has no scientific link with the remaining of the chapter. It should be moved to other parts of the paper (introduction?)

Reply: We agree and propose to shift these lines to the end of L425.

Fig 6 only deals with time. It should be improved with a plot on space definition of forcings. The use of the “blend” should be reviewed.

Reply: We agree. We propose to update Figure 6 (current manuscript) with the inclusion of the spatial definition of the forcing, as below (Figure C2). We confirm “blend” is appropriate in this case as overlapping regions are treated with various weighting factors:

1. From the storm center to 3xRm: only analytical field from JTWC/HWRF
2. From 3xRm to 10xRm: linear transition from analytical fields to GFS fields
3. Outside of 10xRm: only GFS field

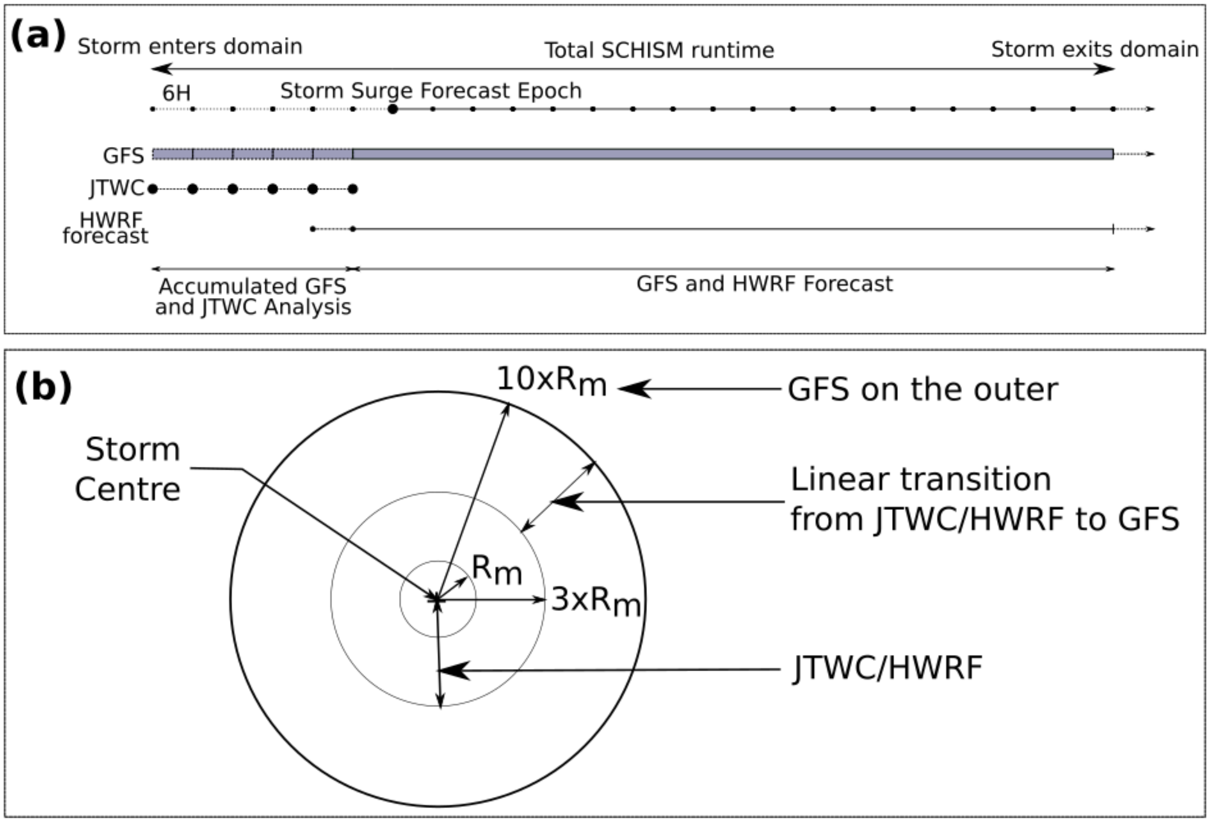

Figure C2. (a) Temporal combination scheme of the JTWC, GFS, and HWRF forecasts for each 6-hourly storm surge forecast epoch. (b) Spatial blending of the analytical and GFS wind and pressure field.

Page 13 – the proposed methodology seems too linked with the specific physics of the site and of this particular event. Small variations of the coupling should be tested and compared with data.

Reply: We disagree with the Reviewer on this point as all the datasets used in Section 5.1 (Page 13) are global and publicly accessible, thus not specific to the particular site or the event. Also, this modelling system is used to hindcast storm surges and flooding in many regions of the world (e.g. Gulf of Mexico, East US coast, Western Europe, etc.), with model configurations that differ little compared to the one used in this study.

Varying the forcing strategy (assuming this is what the Reviewer means by "coupling") is alas not possible, as HWRF forecast is the sole product operationally available over the domain of our study forecasting the essential wind and pressure fields information.

One other forcing option is relying only on publicly available forecast fields (e.g. GFS at 0.25° resolution). In such resolution, the cyclone core is typically not well defined and results in a weaker wind and pressure field compared to merging technique as discussed in a later comment reply.

Lines 315 and following: errors are necessary for the forecast runs. The discussion is very weakly supported without them.

Reply: We agree. We assessed the errors of the various forecast runs with respect to the hindcast run, in terms of maximum surge level simulated. We propose to include the results in a modified version of Figure 7 (current manuscript), as shown here (Fig. C3). We propose to include the following text in the revised version of the manuscript, at L317:

"To substantiate the gradual increase in the quality of the forecasts, we compared the maximum surge level predicted at the various forecast ranges (T-60hours, T-36hours, T-12hours) with the hindcast experiment, along a line encompassing the near-shore delta (segment displayed on Figure 6). The results show that, in the T-60h forecast, the location of maximum surge appears offset eastward, by as much as 150 km. The magnitude of the maximum surge is also poorly predicted, with an overestimation of about 3m. In the T-36h forecast, the location of the maximum surge appears largely corrected, but the magnitude of the peak remains overestimated by about 3m. In the T-12h forecast, both the location and magnitude of the peak surge are in relatively good agreement with the hindcast. Overall, along this coastal section, the standard error of the maximum surge level amounts to 2.06m / 1.73m / 0.66m, for the T-60h forecast, T-36h forecast and T-12h forecast, respectively."

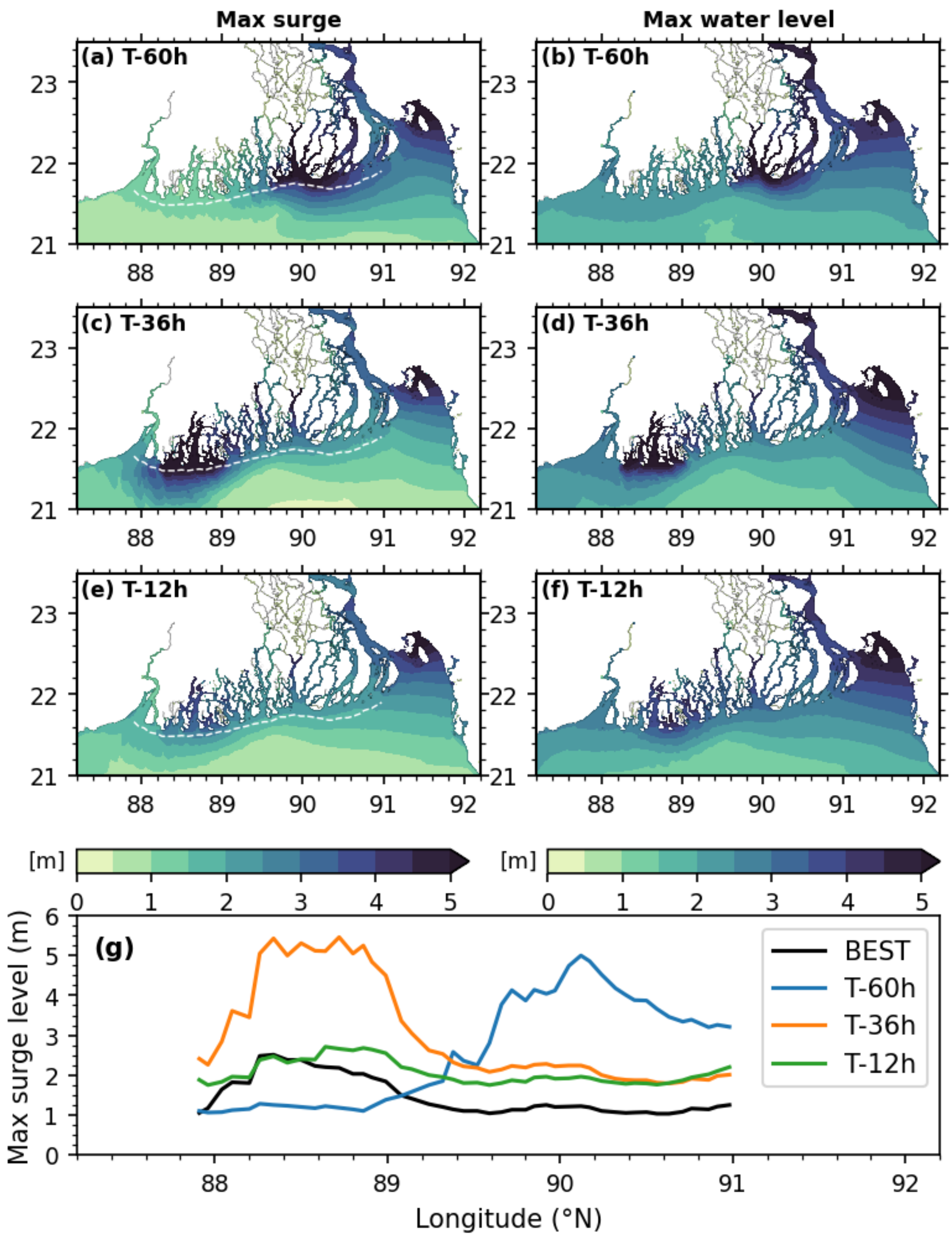

Figure C3. Maximum surge (a, c, e) and elevation (b, d, f) evolution for forecast initiated at (a-b) T-60 hours (2020-05-18 0000Z), (c-d) T-36 hours (2020-05-19 0000Z), (e-f) T-12 (2020-05-20 0000Z) hours before landfall. (g) Comparison of maximum surge level extracted along the section shown in white line in (a, c, e). Hindcast results (BEST) is extracted along the same line shown in Figure 6(b).

Current Figure 5 (will become Figure 6 in the revision) is shown in Figure C4.

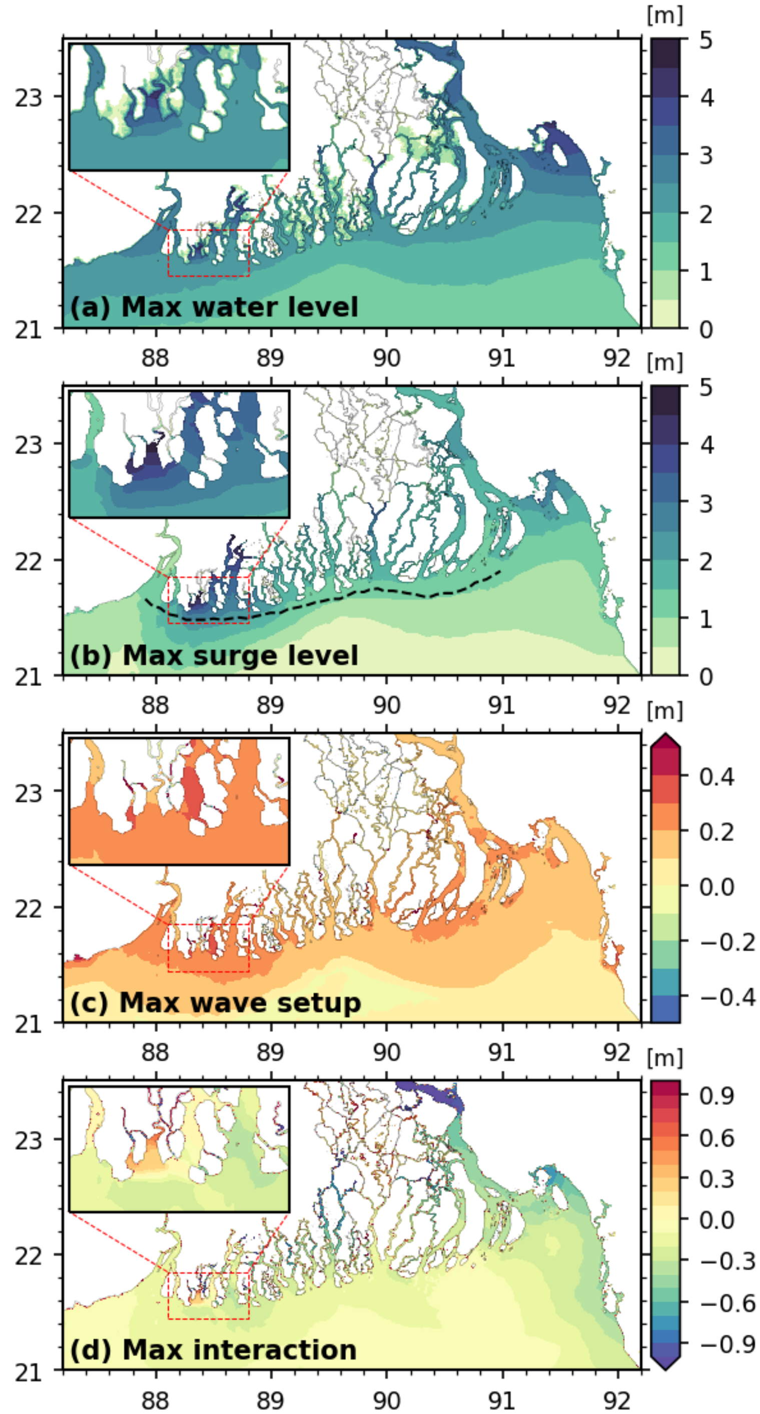

Figure C4. Hindcast of (a) maximum water level, (b) maximum surge (c) wave setup/setdown (d) maximum non-linear interaction between tide and surge. For (a), for the areas above mean sea level, the water level is converted to water level above the topography for consistency. The inset maps show a close-up (75 km × 45 km) of the landfall region. The black dashed line shown in (b) is the segment for error analysis of the forecast experiment in Section 5.2.

Lines 327 until the end of page 15 belong in the introduction as motivation for this study.

Reply: We agree to move the part from L327 to L332 to introduction, to replace the part from L71 to L74.

Lines 344: “The best way to avoid the error from the analytical wind field might be not using these formulations and rely on the full-fledged atmospheric forecasts”. In spite of the limitations of existing atmospheric forecasts, this sentence and the next ones would be better supported with a simulation just based on the available forecasts. I suggest the author to repeat the simulation without the analytical model and evaluate the differences.

Reply: We performed a dedicated sensitivity test, with a hindcast simulation forced only with the GFS data (accumulations of forecasts for each 6 hours cycles over the cyclone period). Compared to our nominal forcing strategy, the central pressure along the cyclone trajectory is much overestimated in the GFS data, by about 20hPa on the day of landfall. Similarly, the wind is slightly weaker in the GFS data (typically by about 5 to 10 m/s) (see Fig. C5). As a result, the surge height is largely underestimated in the region of the cyclone landfall typically by 50% (Fig C6). As this conclusion was largely expected based on the numerous studies published on this issue so far (e.g. Zhang et al. 2013), we believe it is not relevant to include this in the manuscript.

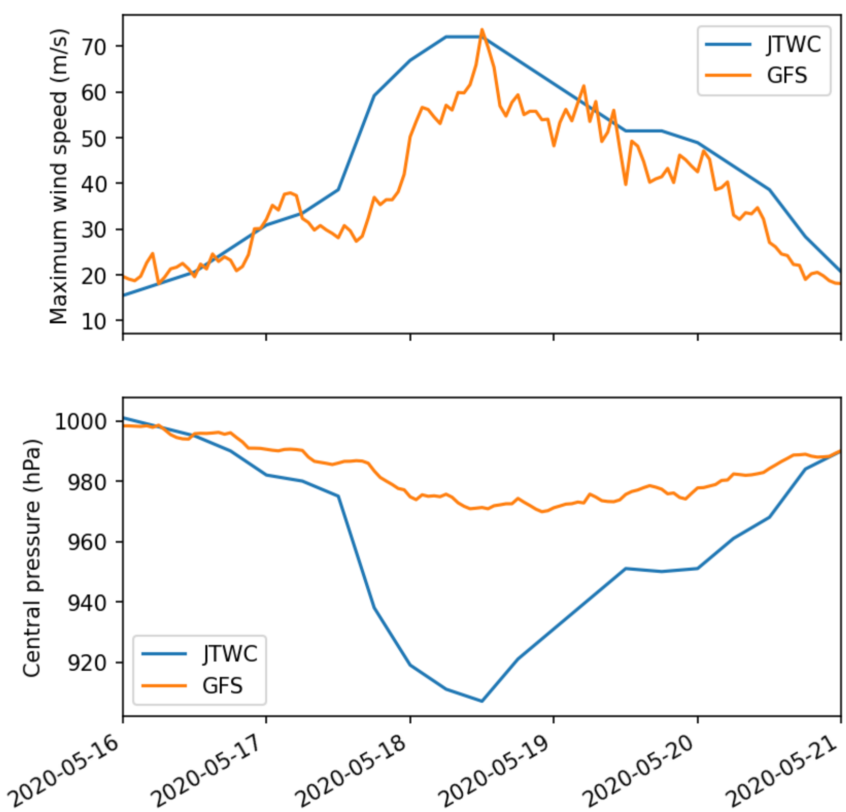

Fig C5. Comparison between JTWC and GFS maximum wind speed, and central pressure.

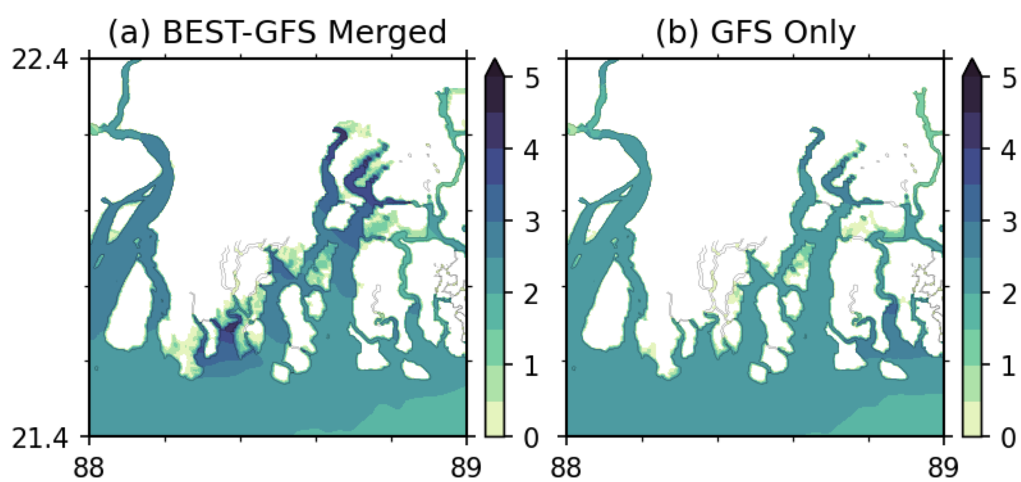

Fig C6. Maximum water level for (a) nominal forcing strategy used in this study combining analytical and GFS fields, (b) only GFS fields.

Fig 8 – why are the inundation patches for the hindcast experiment and not for the forecast runs? I suggest that those are included too, with another figure.

Reply: The objective of Figure 8 is not to assess the quality of the forecast, rather point towards the challenges in modelling the inundation. As suggested by the reviewer, we have added the inundation patches for the forecast experiments as a separate figure (Figure C7). They show that the flooding of the forecast runs is largely in line with the hindcast experiment, in particular over the various sub-regions discussed in the corresponding section (Section 6.3). Thus, to keep the main manuscript lean and focused, we propose to add it as a supplementary figure in the revised manuscript.

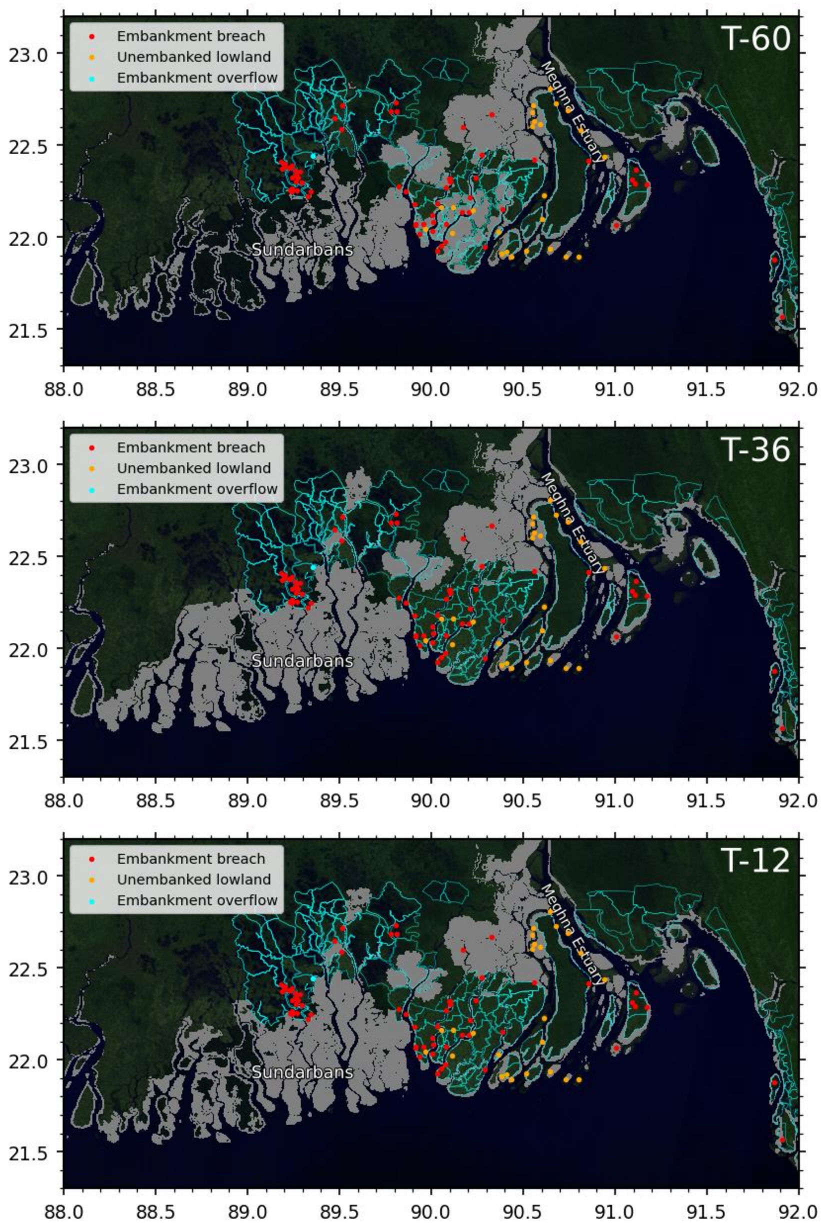

Figure C7. Same as Figure 8 for forecasts at T-60, T-36, and T-12 hours.

Lines 400-401 – refer to table A2. In table A2, correct the 2nd link as it is not a public link.

Reply: We are sorry for the misplacement of the link. The correct link is https://www.emc.ncep.noaa.gov/gc_wmb/vxt/HWRF/index.php

References:

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