Response to Reviewer #1

We thank you for your careful read of our manuscript and probing comments; they have improved the manuscript. We appreciate the time this has taken and have addressed all comments. Our responses are below in blue. The line numbers refer to those in the revised manuscript.
Best regards, The authors

The article by Familkhalili et al. examines the interactions among river discharge, storm surge, and tide in an idealized converging channel. Overall, in my opinion, the study provides insights and good explanations to results that are long found in numerical studies, e.g. the increased storm surge with a deepened river channel. Overall, I don't find any major issues. Here are some of my suggestions.

Thanks for the close read of the text.

A discussion about the impact of wave reflection will be very helpful and broaden the readership of the paper, since in many estuaries and coastal bays, reflection of tidal wave (even the storm surge, which is also a wave) is very normal.

This is a good point, and it would surely be interesting to extend our model to look more directly at reflection and partial reflection effects. We have added some additional text and clarification to the model development section. We note that a convergent geometry already includes an incident and reflected wave, but that we do not consider partial or total reflections off of abrupt geometric shifts. We reference some studies that have shown this to be important for tides. We write: "Though a reflected wave is produced by convergent geometry in analytical models (Jay, 1991), we neglect the partial reflections caused by depth and width changes, and do not consider the case of a reflective upstream boundary. Such factors are important for tidal changes in many estuaries, particular locations that are near resonance such as the Ems (see Ensing et al., 2015) or near where total reflections occur (see Ralston et al., 2019)."

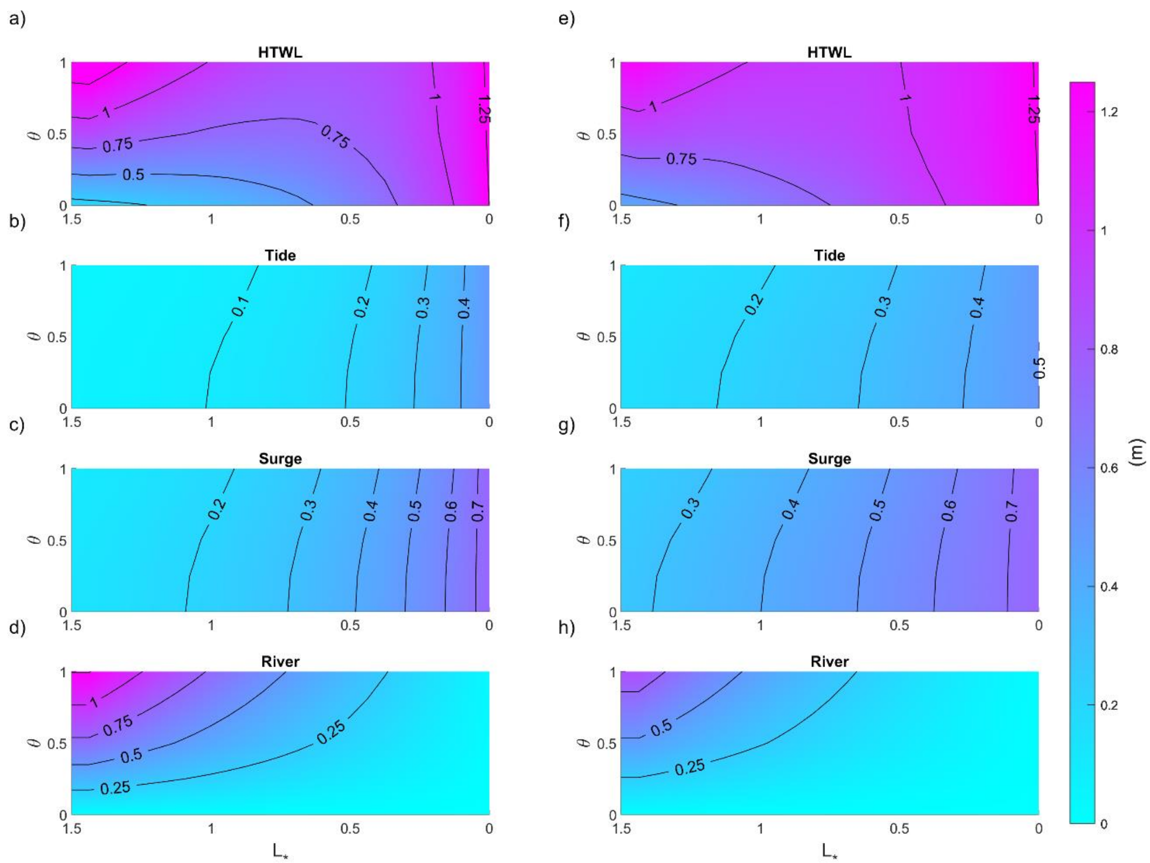
In the idealized model, there no flooding over the shallow area (i.e., a hard shoreline), which make the sea-level rise have the same impact as with channel deepening. But in reality, the inundation and related wave dissipation over the shallow banks could lead to a very different story. Please clarify such limitations of the current analytical model.

The reviewer is certainly correct that the hard shoreline assumption is a limitation, and that future flooding of low-lying areas will modify the results discussed here. We have chosen to analyze the simplest, hard shoreline, case for several reasons. Perhaps the most important are: a) confining the flow to a channel is in some sense "worst case"; i.e., showing the largest impacts; b) any attempt to include overland flooding greatly expands the relevant parameter space, depending on the configuration of the inundated area; and c) engineered responses to higher sea levels would have to be considered, but this would require treating specific cases, not a general parameter space. Also, our 1D modeling framework does not allow consideration of wind-waves; that would require a 2D numerical model. We highlight the assumptions involved as: "Our approach idealizes storm surge as the sum of two sinusoids, and neglects factors, such as the potential role of wetlands and the floodplain, in order to gain insight into some of the important, along-channel factors that govern the system response to a compound event."

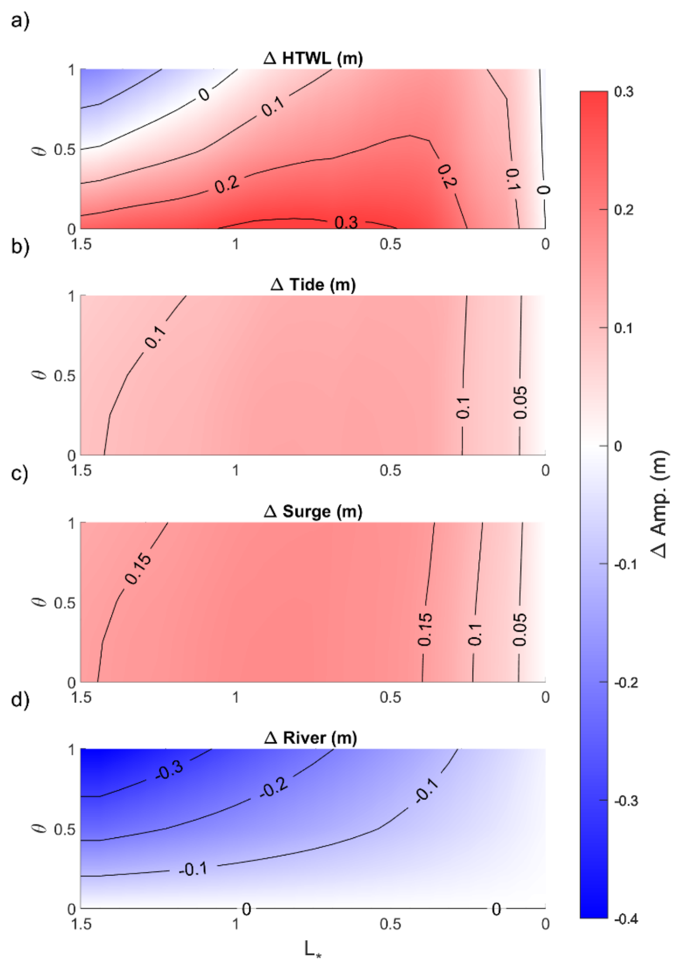
We also mention the modelling approach assumptions as: "Similarly, we neglect processes such as Coriolis acceleration, wind waves, and gravity waves, and focus on the specific case of an incident long-wave that propagates from the coast in the landward direction and is eventually completely damped out."

I suggest to use same color scale for subplots in Fig. 9 and 10. It is a bit of misleading when using different color scale within one plots. Using just contours instead of colored contours is also an option.

We now use one colorbar-scale for all subplots within a figure to avoid any confusion (see below). Also, we used a lighter colormap that allows the contour text to be read better.



We used blue-white-red colorbar for Figure 11 (see below); zero always corresponds to white color where the crossover point is located. Therefore, location of crossover point is highlighted in Figure 11a.



Response to Reviewer #2

The authors would like to thank you for your thoughtful review; it has led to improvements in the revised manuscript. In the following section, responses to comments are given in blue. Changes can also be seen in the “track changes” version of the revised manuscript.
Best regards, The authors

General comments
The manuscript discusses compound flooding using an analytical model after validating the model with an idealized hydrodynamic model. The authors conducted sensitivity tests and investigated what the important factors for compound flooding are. The methods are appropriate and the results are interesting, but my concern with the manuscript is seemingly lack of novelty. The analytical model has already been developed and the results are mostly in line with the past literature. Some results are new, but they don’t look like scientifically novel.

The novelty of our approach is that we consider storm surge within a semi-analytical framework and evaluate its interaction with river flow and the M2 tide. Our earlier effort (Familkhalili et al., 2020) neglected the influence of river flow. To our knowledge, these processes (river flow+ surge +tides) have not been explored analytically, nor over a large parameter space (most compound flood papers use either statistical analysis or a case study approach of an individual estuary or region). Moreover, we further develop the insights from Jay et al., (2011) and Talke et al., (2021) concerning the competing effects of tide/surge change and river flow. Deepening an estuary, in many cases, causes increased tidal and surge amplitudes but decreases the subtidal water levels. So, some regions experience larger total water levels, and others smaller, depending on how close to the coast one is. Our investigation of this phenomenon covers a much larger parameter space than was possible in previous case-studies and is therefore scientifically novel. There are other aspects of the results, as also discussed in the text, which are novel.

We note that though our model setup may be superficially similar to multi-constituent tide models (such as Giese and Jay, 1989 or Buschman et al., 2009), there are important differences in the boundary conditions. For example, storm surge amplitudes are comparable to or even exceed the M2 tide, unlike most tidal models in which M2 dominates. Moreover, the period of the storm surge wave can be much larger than typical tidal bands (4-24 hrs). These facts lead to important differences in non-linear frictional interaction, which are elucidated in our results. Our model also provides insights that a numerical model cannot easily achieve, both because of run-time considerations but also because a numerical model cannot unambiguously isolate tide-surge interaction. Typically, a numerical surge model is run with and without tides, and the difference between the two model runs is held to be the nonlinear interaction. However, this residual includes the frictional effect of tides on surge, the frictional effect of surge on tides, and the difference in phase propagation speed for surge and tides caused by differences in water depth during the two runs. Many statistical studies in fact consider the phase propagation artifact to be the most important source of tide-surge interaction (e.g., Williams et al., 2016). It is therefore important to have a study that focuses on isolating the frictional interaction in an estuary, albeit under idealized conditions.

Another often-neglected issue is the effect of channel deepening on the subtidal slope. Some studies point this out (e.g., Buschman et al., 2009; Jay et al., 2011; Talke et al., 2009). However, to our knowledge, this parameter space as it interacts with tides plus surge waves has not been explored in an analytical framework. We mention about reproducing subtidal effects and constituent interaction found in a 2D numerical model. This discussion is added to Conclusion section of the manuscript in lines 594-599.

Also, an equally concern is lack of “quantitative” analysis. The authors presented the results, but did not seem to quantitatively discuss the results in depth. e.g. comparison between analytical and numerical models in terms of their capabilities and caveats and the relative importance of depth, friction, river discharge, convergence length etc. on the resulting compound surge levels. It should be possible to rank them to discuss further quantitatively what’s the most/least important and why. Use of analytical model enables you to do such analysis. Given that, although this manuscript is supposed to discuss the insights of compound flooding, I am not convinced that this manuscript provides the insights. I suggest the authors to include more quantitative analysis of the model results and discussion of the findings.

The main questions that we investigated in this study are stated in the introduction section as:
"a) What factors determine the region in which river flow effects or tide/surge effects dominate the total water level?
b) How does the transition from coastal to fluvial dominance shift as geometry changes or as properties of storm surge (e.g., time scale and magnitude) and river flow (magnitude) change?"
So, ranking the parameters based on their influence on surge amplitudes is beyond the scope of this study, but we propose that the non-dimensional friction number presented in this manuscript can characterize the importance of each factor. For example, we note that increasing the surge time scale has a similar effect as increasing the depth; however, we note that our model is slightly more sensitive to depth, due to the cubic relationship in the friction term, rather than the squared effect of time scale. Therefore, it is important to mention that the order depends on the scaling factors. We added more discussion to the conclusion section (see lines 608-614).

Overall, we do not think that a more specific ranking of effects is appropriate, in that this depends on the specific values adopted for the various non-dimensional numbers and their ranges across the spectrum of estuaries. That would entail a broad survey of estuarine systems (beyond our scope) and would still be subject to the objection that we neglected an outlier with a yet more extreme value of a specific parameter. We also note that there is already quite a bit of evaluation, analysis, and discussion of results, e.g., through the analysis of non-dimensional numbers. The paper is already rather long and doing a deeper dive into the wave mechanics and ranking individual contributions is something we may explore in future contributions.

We do agree that a discussion of the relative merits of the analytical and numerical modeling approaches is a good idea and can help identify the novel aspects of our approach. We have now added the following paragraph to section 3.2:
"The results of the model comparison (Fig. 3, 4 and 5) show that both the analytical and idealized numerical models produce broadly consistent results. Therefore, our neglect of acceleration in the subtidal model (Fig. 4) and the use of linearized friction is justified. Both numerical and analytical models are complementary tools. A 3D model with resolved bathymetry is clearly best used to evaluate the specific effect of bathymetric alterations in a particular estuary (e.g., Pareja-Roman et al., 2020; Helaire et al., 2020), or to run simulations using complex, real valued boundary forcing (river and coastal). But our analytical model runs substantially more quickly than even the idealized numerical models, facilitating investigation of a larger parameter space. Moreover, numerical models cannot unambiguously separate tide, fluvial, and surge effects. Currently, the best-practice approach is to run the numerical model with and without relevant forcing; for example, by running a surge model with and without tides, one can approximate the effect that tides have on total water level (Shen et al. 2006). When combined, tide and surge wave travel faster (due to deeper water depth; see Horsburgh and Wilson, 2007), and frictional energy loss in each wave component is also larger (Familkhalili et al., 2020). Due to the multiple feedbacks and nonlinear interactions, decomposing numerical results into individual surge and tide wave transformations, is inherently ambiguous. The analytical approach, while not including all interactions (such as the phase modulation caused by depth variability), is able to individually estimate transformations in the primary surge and tide constituent amplitudes, also under conditions of different river discharge. This approach, to our knowledge, has not previously been approached to understanding the fundamental bathymetric and boundary condition factors that influence compound events."

Also, I strongly recommend the authors to add a table on all model parameters with number as well as a table or glossary for all variables with unit you used in the manuscript and be consistent throughout the texts. I was struggling to read this manuscript due to inconsistency of the variables and confusion of their use. In addition, it's better to be more specific about the variables you use in the text such as "waves" as you include multiple waves (primary/secondary surge and tides) or assign unique variables (I recommend the latter). I was having difficulty reading the manuscript because it's not clear to me for example what waves the authors are referring to.

We revised Table 2 and added more parameters used in the models. Also, we added a glossary to the text (see Appendix), as suggested, to explain parameters and units used in this study. Moreover, we added a new table (Table 1) that represents analytical and numerical model configurations used for validation. We went over the manuscript and made sure that all the parameters are in the body of the text.

Specific comments for each section

Thank you for the detailed review and the constructive comments. Please see our detailed response to the comments below:

1. Introduction: It is not clear to me as to what's the missing pieces of compound flooding studies, which motivate your study and what's the advantage and limitation of your analytical model over numerical model. This should be addressed in introduction or in method section.

We have now added the following paragraph to section 2:

"Both, analytical solutions and numerical models are regularly used to explore the mechanism of surge and tidal waves propagation along an estuary (see Talke and Jay 2020 review). While numerical models can simulate tidal wave propagation more accurately than analytical models considering the measurements in a real system, numerical models are typically calibrated for an existing bathymetric, meteorological, and boundary forcing configurations (e.g., Brandon et al., 2014; Bertin et al., 2012; Orton et al., 2012). On the other hand, idealized numerical models with simplified configurations can be used to develop sensitivity studies to investigate the effects of changing hydrodynamic variables on surge and tidal wave interactions in a system (e.g., Shen and Gong, 2009; Familkhalili and Talke, 2016), but a downside of these numerical approach is that studying an entire parameter space is computationally expensive. In contrast, analytical models rely on fundamental underlying physics and are transparent. Thus, they are good tools to explain some of the factors (e.g., channel depth, convergence length, river discharge, and surge amplitude and time scale changes) that alter flood levels in an estuary."

2. Methodology: The method section includes the analysis of some model results (section 2.2) as well as the description of the analytical model and validation. Those should be separated so I suggest to move a portion of the section 2.2 to section 3; then, maybe section 3-5 can be combined with the portion.

We have rearranged and renamed sections. Section 2 is "Methods" that includes analytical model descriptions and section 3 is "Model validation" in which we compare our analytical model results against two numerical models, one with similar sinusoidal waves as boundary condition and another one with parametric hurricane model which is simplified in our analytical approach to sinusoidal waves at ocean boundary.

3. I suggest adding a table including name, unit, and variable of all model parameters (e.g. Bg, B0, dx, domain size, run time, frequency, amplitude, and phase of surges and M2 tide, Cd, depth, etc.). Table 1 includes some, but it is incomplete and should be put near Figure 1.

We added Table 1 in the new version of the text; it describes the validation model configurations. In addition, we added more parameters and explanation to Table 2. Combined with the glossary, it helps the readers to follow the modelling approach. Each table is put in its section and referred to figures if needed.

4. Results and discussion: As for section 3-5, I don't understand why you need another model comparison as you already have one. Also, this is a new configuration compared to the one used in texts, which should be explained prior to the results/discussions (e.g. methodology). The section provides the results which were not presented in section 2-2; however, I am not convinced that the authors need this section as a separate section. Maybe the author can think about combining section 2-3 and 3-5. Just merging 3-5 into 2-3 does not seem to work. In addition, I suggest that the authors should include in depth analysis of the model results and their comparison with the numerical model and other similar studies to discuss the analytical model.

We combined section 3.5 with model validation in the revised text. The validation in section 3.1 compares the analytical model results against numerical models developed with similar domain and boundary condition, especially at the ocean boundary as we model surge wave as two sinusoidal waves. In other words, these are mainly tidal models that have three sinusoidal waves at the ocean boundary.

In section 3.2 we compare the results of our newly developed analytical model with a numerical model that doesn't model surge as a boundary condition and has modeled hurricanes with parametric wind and pressure field. Therefore, the comparison of our developed analytical model with the numerical model results (Familkhalili and Talke 2016) is essentially different from previous numerical models as this new one is a case study of Wilmington (NC) and not a general conceptual model. We added more information to this section to help the readers to better understands the reason behind this comparison.

Line-by-line comments
Line # Sentence Comments
94, 98 Non-stationarity
I am a bit confused about the word, non-stationary (or non-stationarity) as this manuscript does not discuss nonstationarity (changing conditions over time) much. Please clarify.
Replaced with 'time varying'

149 A is channel cross-section
A is channel cross-sectional area?
Yes, edited accordingly.

145,146,161, 165,173,187, 239,252 Equations 1-8
Each variable should have unit in texts.
Units of each variable are added to the text.

153 Le

Have you defined Le prior to the equation? If not, please define.

Change made.

157 Tidal amplitude to depth ratio

ξ was defined as tidal water level elevation on line 149 so the ratio is not amplitude/depth, but tidal elevation or tidal level/depth.

Thanks for pointing it out. We edited Figure 1 and the text to read as ξ is tidal amplitude.

161, 166 Equation 3 and UR+UT

Define UR and UT. Are they different from uR and uT? If not, the authors should use the variables consistently. I suggest the authors to check all variables in the manuscript to make sure that they are consistent.

We added a glossary for all variables with units used in this manuscript (Appendix). We added "where UR and UT are maximum river and tidal velocity, respectively"

166 U(x)

Is upper case U, the maximum value of current and I wonder the same applies to UR and UT? Nothing was mentioned in text.

Yes, we made it clear by adding "$U_{(x)}$ is a function of x and is the maximum value of the total current $(U_R + U_T)$, where U_R and U_T are maximum river and tidal velocity, respectively"

169 Figure 1b

h is mean depth and is supposed to be constant. But in Figure 2b, two arrow lengths of h: one at ocean boundary and the other at upstream are not same in length. It's just a schematic illustration, but it's nice to be consistent. Also, is Z at ocean boundary always zero as illustrated in Figure 1b? I guess it's very small, but I don't think it's zero. And where is surge level?

Made changes in the Figure. Z is the perturbation in the water surface elevation due to river discharge and is assumed to be much smaller than the mean water depth and zero at the ocean boundary. We keep the schematic illustration simple and disregard showing surge amplitude as we modify tidal equations to be used for calculating surge amplitudes along an estuary.

170 Figure 1 caption

Figure caption should include all parameters in the figure and/or put them in a new table with the note in the caption.

We added a glossary to the text, as suggested, to explain parameters and units used in this study and noted in the caption to see the glossary for more information.

173 Equation 5

Isn't ur supposed to be always negative for the coordinate on Figure 1?

We added a negative sign to the equation to account for the direction of coordinate system.

183 Landward boundary

The authors mentioned that the landward boundary is extended 100 km to avoid tidal reflection. If so, the landward boundary is where the river discharge was prescribed? What's the size of the domain? It's hard to see that on Figure 1 as there is no scale and no list of parameters and their values.
Figure caption is edited to include that "The convergent section of the model domain is 1.5 times the convergence length and the river channel at the left-hand side extends an additional 100 km to enable tidal and surge constituents to damp out". Also, Table 1 and 2 and glossary list together represent model configurations and parameters used in this study.

184 Seaward boundary
What's boundary condition at the seaward boundary? Radiation?
The seaward boundary (see Fig. 1) is forced by 3 sinusoidal water level signals, two of which combined represent surge wave and the third one is major tidal component.

209 The presence of river discharge uR
Q is regarded as discharge (u=Q/A on line 151), but uR is also defined as discharge here. uR is velocity in m/s and the discharge is in m3/s. So I am confused. Please clarify.
To avoid any confusion, we rewrite the sentence as "The presence of river discharge Q_R and tidal transport Q_T causes stronger ebb currents ($|Q_T| + |Q_R|$) and weaker flood currents ($|Q_T| - |Q_R|$)."

209-210 Stronger ebb currents (ur+uT) and weaker flood currents(ur-uT)
ur and uT are positive landward according to Figure 1, aren't they? If so, flood currents are -ur+uT and ebb currents are -ur-uT. If that's true, the authors should modify the text. Still stronger ebb currents and weaker flood currents, though. Correct me if I am wrong.
See response to the comment above.

215 Tidal discharge amplitudes
What are tidal discharge amplitudes?
We changed it to tidal transport (Q_T).

226 Tidal amplitude
ξ was defined as tidal water level elevation (Line 149) and here the authors re-defined ξ as tidal amplitude. Tidal amplitude and tidal elevation are different (only equal when the elevation is the maximum). Better to define it with a different character, e.g. Atide as A is used as amplitude in the text.
See response to the comment above (ξ is tidal amplitude).

228 River flow velocity
This should need a clarification. Velocity has a unit in m/s and river flow velocity (θ) has no unit as it is normalized. Maybe you can add 'normalized'?
Change made.

228-229 River flow velocity applied at the upstream boundary
How can you apply normalized flow velocity at the upstream boundary? It is supposed to be a unit of velocity (and height). It seems that the authors use river flow velocity (line 228) and river flow ratio (line 231) interchangeably, which also confused me.

We apply river discharge as an upstream boundary condition. It has been normalized to the tidal transport at ocean boundary, and we call the ratio (θ) for further discussion in the text; see Glossary.

253 H & is elevation and h(is the mean water level

The choice of variables is very confusing. In Figure 1, H is the total depth and h is mean water depth. Here H & is defined as elevation (of what?) and h(is mean water level. We often assume h (is mean of h and same for H & . Suggest to use the variables consistently throughout the texts.

We modified the text to read as "$\overline{H}$ is total water elevation and $\overline{h}$ is the mean water level (the overbar denotes the tidally averaged value)."

255 Considering the first and third terms in Eq (4), ….

There is no third term on the right hand and the left hand sides in Eq (4).

Typo, edited.

257 Section 2-3

This section partially include results and discussion that is less related to the validation, but more to the results. The authors may want to consider moving some paragraphs to the result section.

We rewrote model validation section (now section 3) and moved discussion to the results and discussion section (section 5).

258 Tidal amplitude variation

Delete amplitude?

Done.

261 Wave amplitudes

I am a bit confused. First, wave amplitude of what? Second, amplitude is supposed to be constant (e.g. Apri and Asec in Eq 6). It seems that the wave amplitudes mentioned here and after this are spatially varying amplitude, not a constant value of amplitude. The authors should clarify this as I am not sure what amplitude the authors are referring to. Maybe define a new variable, e.g. A(x) or A(x,t) so that readers are clear about what amplitude the authors are referring to?

Tidal amplitude is defined as the elevation of tidal high water above mean sea level (i.e., ξ in Fig1) and is spatially variable as a tide wave propagates upstream along an estuary. Wave amplitude is set to a known value at model boundary and the spatial variation of wave amplitude is analyzed.

271 Figure 3 shows the spatial pattern of the dominant tidal constituent amplitude

Again, M2 amplitude is spatially varying M2 amplitude, not D2 in Table 1? If so, all amplitudes at each grid occurred at the same time stamp? I doubt that all maximum water levels come at the same time at all grid points due to phase lag associated with tidal distortion due to topography and/or friction etc.

The numerical model is forced at the ocean boundary with three tidal components (K1, M2, and M3) and we use similar period and amplitude in our analytical model. Figure 3 shows the spatial variation of maximum amplitude of M2 tide wave as it propagates along the estuary.

276 Figure 3

I can see that the difference between numerical model and analytical model is larger between L*=0.3-1.0 for q=1, but not for q=0. Likewise, I can also see that the difference is larger for L*=1.0-1.5 for q=0, but not for q=1. Can you please explain why? Also, the authors should flip x-axis to be consistent with other figures. It confuses readers. Another suggestion: It may be nice to add the extent of Le and where b(x) is equal to Bc in Figures 3,4,5.

Figure 3 x-axis is flipped and now L* = 0 is at the right of the plot (ocean boundary) and increasing landward to the left of the figure. It is stated in the text that Bc=1100m which is corresponding to the estuary length of 1.5xLe with 5km width at the ocean boundary (B0=5km). In another word, the estuary width at ocean is 5km and is exponentially decreasing for 120km (equal to 1.5xLe) and reaches a constant width of 1100m at the end of estuary (i.e., start of river section).

272, 287, 364 Figure 3, Figure 4, and Figure 6

Are these figures the model results at what time? Or just maximum value at each point?

The plots show the maximum values along the estuary.

284 The RMSE between …. Are 0.03, 0.08, 0.09, 0.10.

Why does RMSE become larger as the river flow (θ) increases? Please explain.

Analytical models are used as a tool in this study, and we don't state that they are the most accurate tools compared to numerical models. Generally, the analytical models are more explanatory than fully realistic. It is also worth mentioning that θ>0.5 represent cases in which river and tidal flows are comparable and very large river discharge might violate the assumptions that reduces the equation of motion to zero-order balance between the pressure gradient and the friction term. It can increase the RMSE as river discharge increases. Also, the analytical model does not change the mean depth upriver, when flow increases.

288 Figure 4

Is the ocean boundary at L*=0? X-axis is different from the one in Figure 3. Should use the consistent axis range (i.e. from 0 to 1.5. Positive values).

We revised Figure 3 so that both plots have ocean boundary at =0 (L*=0), with x increasing landward toward the left of the plots.

Another questions: 1. On (a) with θ=1, water level from analytical model is larger than the numerical model at L*=-1.5. but smaller near the ocean boundary with the intersection in the middle (L*~=-0.8), but it looks like the intersection is shifted landward with decrease in θ.

2. The same trend applies to (b) with shifting the intersection as h increases. Please explain why. The answer to this question along with others I asked will clarify why analytical model and numerical model behaves/responds differently.

Please see comments above regarding the reason why RMSE increases for larger river discharge.

293 Higher mean water levels (Z)

Could this be Higher mean water levels (h()? I am not sure if I understand the difference between h(and Z and also ∂H & ,∂x and ∂Z ,∂x(or ∂Z ,∂L $*$). Both are defined as mean water level and surface slope, respectively.

$\overline{H}$ is total water elevation and $\overline{h}$ is the mean water level. Z is the perturbation in the water surface elevation due to river discharge and is imbedded in H. We edited the typo.

330 Supri time scale

Supri period as you use periodic function in eq 6.

This is a little complicated, because a surge has a broad power spectrum with many frequencies, so "time scale" is a reasonable term. On the other hand, we are representing this phenomenon with two specific frequencies, so "period" is also appropriate. We have changed time scale to period throughout.

339 A*

Boundary is ocean boundary or land boundary?

"Ocean" added to the text.

346 Figure 5

Y-axis is not amplitude, but surge level or elevation. Amplitude is supposed to be constant.

Revised the plot.

361 L*=1.5

There is no L*=1.5. L* ranges from -1.5 to 0. Figure 3, 4, 6 and 8 has different x-axis range. Suggest to use consistent x-axis range for the figures.

Done

364 Figure 6

Where exactly is L*=-1.5 in Figure 1? Is it where b(x)=Bc or the location of the land boundary where river flow is prescribed? If the latter, it's strange because the authors mentioned that the channel was extended 100 km (line 156) to allow tides to dissipate due to friction and that's where river flow was prescribed. Am I misunderstanding? Also, ζ = 1.0 m in the legend. Even if amplitude of primary and secondary surges are combined, the amplitude is not 1.0. Where did ζ = 1.0 m come from?

This plot shows the effects of wave period (i.e., D2=12 h and D1 = 24 h) and amplitude (0.5 m and 1m at the ocean boundary on tidally averaged water level (Table 2). We added the characteristics of D1 to Table 2. In this case, we study the effects of wave period and amplitude.

L*=1.5 is where the constant width river starts. We define the estuary to extent from ocean to 1.5 times the convergence length scale and therefore plot the model results along the estuary (L*=0-1.5). We state this in several places, for example see lines 184-186: "The constant depth channel is routed upstream for 100 km, to enable the tide wave to dissipate and prevent reflection off an upstream boundary" and also Figure 1 caption.

Although we extend the model domain 100km further upstream of end of estuary, but the purpose of that extension is to make sure that waves completely damp and there is no reflection in the model from landward boundary. Thus, extending the plots doesn't seem necessary and would not provide useful information for our discussion.

358 Wave time scale (T=1/w)

T is also used for bed stress divided by water density on eq 1. Do not use the same name for different variables. Is this the same as wave period? Also, increase in wave period of what? primary or secondary surges? Maybe a subscript (throughout the texts) could be helpful.

We edited Eq 1 and used K for bed stress divided by water density term ($K=\frac{\tau}{\rho}$). T is surge time scale that is equivalent to wave period.

Here we have only two different waves with period of 12 and 24hr (i.e., D1 and D2) modelled and explaining the effects of wave period and amplitude on how they decay along the estuary.

358 and 364 Wave amplitude (ζ)…

What wave amplitude is ζ? I guess ζ is constant as in the legend of Figure 6. If yes, the amplitude should be noted as A as in Equation 6.

Wave amplitude is a function of *x* and time and changes along the channel as wave propagates.

358 Wave time scale

Wave period

Changed "time scale" to "period".

372 A*

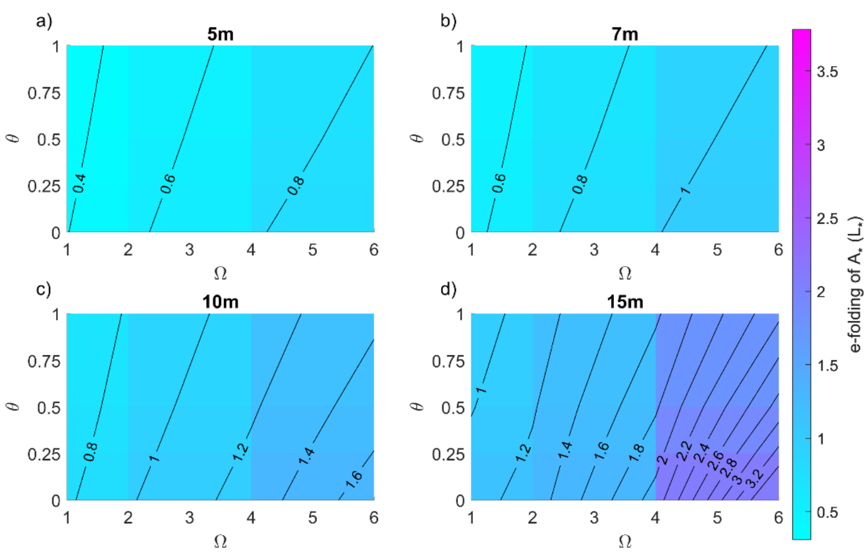
Use same notation as the one in line 339.

Fixed.

394 Figure 7

It is hard to see the contour lines and labels. Use a different color or use a different colormap. A monotonic colormap may help. Also, I am a bit confused with the figures as θ and Ω only have 4 numbers in table 1, but the figure looks like there are more data. Did you use shading color (e.g shading interp on MATLAB)? If so, it is better to use flat (no-shading). Please clarify.

We attempted different colormaps and chose a light color that would help to see the contour lines better. There are four values for θ and Ω. We also included more time scales (Ω = 1, 2, 3, 4, 5, and 6), corresponding to the surge primary period of 12hr, 24, 36, 48, 60, and 72hr, and the results are similar as plotted. Thus, and to have less number of combinations, we chose 4 values for Ω to plot. Shading interp is also necessary as you can see the following plot doesn't have shading (flat shad) and the colors don't follow the contour lines. We also used the same colormap in Figures 9 and 10 for consistency.



426 TWL=T+SS+R

Have you defined all T, SS, and R in the text? The authors used T=1/ω (line 358) and it confuses readers with the same variable name, but defined more than once as a different variable. I am confused with the various definitions from the beginning of the manuscript. Please make them clearly defined.

We removed the equation to avoid confusion.

435 Figure 9

Why does tidal and surge amplitude decrease as river flow (θ) increases? Is that a distortion of tide and surge due to non-linear processes as river flow increases? Please explain the details. Also on (a) and (e), the contour of 0.75 looks different at 0.3<L*<1.0. Can you add an additional contour e.g. 0.8 or 0.9 to see if the contour on (e) is similar to that on (a)? I think they should be similar as tide, surge, and river look similar between the two depth cases. But if not, what process could cause the difference?

We discussed the effects of river discharge on tide and surge waves in section 5.2, explaining how higher river discharge increases the damping of waves amplitudes.

Regarding the contour of 0.75 on subplot (a) and (e), they do not look similar as explained in the text, subplots (a and e) show the total highest possible water levels (HTWL) for depths of 5 m and 10 m, respectively. While the trends of in tide, surge, and river flow are similar, the values are not. For example, at L*=0.5, tide amplitude is 0.2m in a 5m deep channel while it is 0.34m in a 10 m channel. For surge and river discharge: at 5m channel surge and river discharge at L*=0.5 are 0.4m and ~0, respectively, and 10 m channel surge at L*=0.5 is 0.55m). Therefore, adding these values would result in different values for HTWL.

455 Estuary boundary

Please define estuary boundary in Figure 1.

The text has been edited; it now reads: "long-wave magnitudes decrease more quickly, the larger they are at the ocean boundary". The model domain is explained in the text and the upper and lower boundaries are oceanward and landward. We explain internal and external boundaries in the text. For example, see lines 214-217: "… This approach produces a system of 2N linear equations with 2(N-1) internal, one seaward, and one landward boundary conditions. The landward of our analytical model is forced by a no-reflection condition with constant discharge and the seaward boundary (see Fig. 1) is forced by 3 sinusoidal water level signals."

477 River effects are larger than marine effects

"effect" is a vague word. It is unclear to me as to what you mean by effect. Please define it more precisely.

We edited the text; it now reads: "The differences in the response of river flow and storm surge to a depth increase lead to a crossover point, which we define as the location in which river flow effects on HTWL are larger than marine effects, for a given set of forcing conditions"

481 A decrease in mean river discharge may also cause a landward migration in the crossover point.

I don't get the point. Can you explain why? If this is indeed true, there should be a way to quantify.

We define a crossover point as a location in which river flow effects are larger than marine effects on the highest total water level. The zero-contour line in Fig. 11a represents the location of crossover point along the estuary and under different river flow conditions. In a convergent system, decreasing river flow will logically move this point landward. Also, we explain in the text that the model results show that this location would change when the channel depth increases (see Figure 12).

484 Long wave amplitudes
Are they total amplitudes (primary/secondary surge and tide)?
It could be amplitude of tide, surge (both primary and secondary waves), or combination of tide and surge waves.

489 Increases in channel depth, wave time scale, and decreased length scale.
Could this be "increases in channel depths and wave time scale, and decreases in length scale"? I don't understand what "increases in decreased length scale" mean. Also, wave time scale should be wave period What's the relative importance of each parameter on the total water level?
We edited the text as "This number suggests that increases in channel depth (h) and wave period ($T = \frac{1}{\omega}$) and decreases in length scale (L_e) have similar effects on wave amplitudes." The importance of each parameter is studied throughout the text. For example, section 5.3 investigates the effects of length scale Le). As suggested by the non-dimensional friction number itself the importance of these parameters on wave amplitude is not linear. Also, we note that the model is more sensitive to depth changes, due to the cubic relationship in the friction term, rather than the squared effect of wave period. Moreover, the non-dimensional friction number suggest that the effects of surge amplitude at boundary (ξ) and drag coefficient (C_d) have a lesser, but still important, influence on the spatial damping of surge as the depth.

497-498 The transition zone may be sensitive to changes in estuary geometry, such as depth…….
This should be discussed further quantitatively and your analytical model allows you to do that.
We have already showed that the cross-over point location changes as channel depth increases or river flow changes. See Figures 11 and 12. We believe that the parameter space chosen in this study (Table 2) provide a wide range of cases to show the effects of wave characteristics, river discharge, channel depth, and width convergence length on wave amplitude along the estuary and the comparison between the analytical and numerical models increase our confidence in the results.

508 Section 3.5
I feel strange to see another model comparison here at the end of the manuscript. The authors compared the analytical model with numerical model on section 2-3 and compared the model again for another idealized model with a different configuration. Though the section provides some new information, I am not convinced that it is necessary to be added as a section. Suggest to remove it or combine it with the validation on section 2-3.
As stated earlier we combined section 3.5 with model validation in the revised text. See response to the comment 4 above.

512 Idealized numerical modeling of Familkhalili and Talke (2016)
Is this idealized model different from Delft3D the authors used in this study? If so, please explain what the difference between the two models is and why the authors used a different model. That statement and a brief introduction of the model should be included in the text.
Yes, this model is an idealized numerical model developed based on a case study of the Cape Fear River estuary, but since the numerical model configurations are within the parameter space studied by our

analytical model, we briefly compare our analytical model results against the idealized numerical modeling of Familkhalili and Talke (2016). We added more information to the text as explained above (see section 3.2).

512 River kilometer (Rkm) 12.
What is Rkm 12?
Rkm 12 is where the exponentially decreasing part of the Cape Fear River Estuary (CFRE) begins. The CFRE has a garlic shape that the estuary width increasing from the ocean up to Rkm 12 and then the estuary has a funnel-shaped between Rkm12 and 50. To avoid repetition, we refer to the original paper (Familkhalili and Talke, 2016) for more details.

514 Shipping channel was increased
Shipping channel was deepened.
Done.

532 Figure 12
Why does the analytical model consistently overestimate A* over the numerical model across all depth settings?
We added more text to the section on validation of our analytical model with the CFRE idealized numerical model (see section 3.2). The numerical model includes a parametric model of hurricane wind and pressure forcing to the continental shelf which allows the externally induced surge waves to propagate into the estuary. It is forced at the shelf boundary with six semidiurnal and diurnal tidal constituents. In our analytical approach we simplified the model boundary conditions and reduced the numbers of tidal constituents to one (M2) which is the main tidal constituent. We further assumed that the two surge waves are symmetric with no phase lag while in the numerical model each hurricane configuration is run 13 times, with the timing of the storm spaced in 1 h increments over the tidal cycle and thus create a fill area around the mean values. As mentioned earlier, the surge waves are symmetric and therefore we expect that analytical results would be closer to the maximum numerical values (higher end of fill area). Therefore, analytical model somewhat underpredicts normalized amplitude that increases for deeper channels but still within the range. Nonetheless, considering all simplifications, the analytical results are in good agreement with numerical model.