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Natural Hazards and Earth System Sciences
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Discussions

Interactive comment on "Model sensitivity for the prediction of extreme sea level events at a wide and fast-flowing estuary: the case of the Río de la Plata" *by* Matías G. Dinapoli et al.

Anonymous Referee #1

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This paper by Dinapoli et al. presents a sensitivity analysis of extreme water level predictions at the Rio de la Plata Estuary. This topic fits well with NHESS, but, in the present form, I cannot recommend its publication, for the main following reasons:

-Once optimized, the model predictive skills are still not convincing, at least based on figure 4. Indeed, there might be an inconsistency between this figure, where the model departs from the observations by 0.5-1m a large part of the time, and figure 3 and 5, which suggests errors < 0.5 m. Considering that the correct figure is figure 4, I would rather expect ERMS of the order of $\sim$0.3-0.4 m, which is not convincing compared to recent studies (e.g. Bunya et al., 2010 ; Bertin et al., 2015). A possible reason for these relatively weak predictive skills is the wind that was selected (NCEP), which is

way too coarse to compute storm surges accurately. From my own experience, a wind forcing of at least 0.2° resolution should be employed. Why not considering the CFSR reanalysis (Saha et al., 2010), which is fully available and offers a spatial resolution of 0.2° and a time resolution of 1 h.

-The authors evaluated directly total SSH, which does not allow to understand if their errors come from tides, storm surges or a bit of both. What is the impact of baroclinic circulation in such a large estuary, baroclinic effects are even not mentioned in the paper? I would suggest separating model results for tide only, surge only and model + surge. This would namely improve understanding, tide-surge interactions (e.g. Idier et al., 2012), which might be quite important in such a large estuary. I understand that it would lengthen the paper substantially but the present paper is quite short compared to the average of NHESS papers.

-There is a confusion between wind speed and surface stress. Why evaluating surface stress and not wind speed directly? Indeed, using the surface stress implies defining a drag coefficient, which is a very large source of uncertainty, namely because it depends on wind speed but also, for a given wind speed, on the sea-state. In the end of section 2.4, the author state that "the wind drag coefficient has a relative small effect on the parameterization…": what does it means given that Cd increases linearly with the wind speed? What is the idea behind keeping Cd constant while it is known that it increases with wind speed? Considering the sensitivity analysis on the wind speed where I ranges from 0.5 to 0.5, eq. (3) implies that Cd should vary by a factor of 3.

If the authors are willing to revise their manuscript or to submit it to another journal, I have also identified the following along-the-text less important problems:

-P2, L12: "surface stress" is more common.

-P2, L18-23: strange to start the introduction by presenting the study area.

-P2, L25: what do you mean by "system", has ENSO broader impacts than river runoff?

-P2, L35: “along its margins”

-P3, L5: I strongly doubt that the RdP is located in one of the most cyclogenetic region of the world. Regarding tropical storms, the Southern Atlantic is on the contrary the only Ocean where the cyclonic activity is almost nil (https://earthobservatory.nasa.gov/IOTD/view.php?id=7079). Regarding extratropical storms, please consider that, in winter, storms develop continuously at high latitudes.

-P3, L8: “yields” rather than “originate”.

-P3, L16-20: please explicit the corresponding surges.

-P3, L30: a forecast is never “exact”, rather use “quality”.

-P4, L7: explain why a wide estuary turns more challenging the estimation of the wind speed and direction.

-P4, section 2.1: rather explain what are the key differences of ROMS_AGRIF compared to the original version. As said above, please better justify why baroclinic circulation can be neglected.

-P5, L9: “aims to establish”.

-P5, L30: this is not true that RMSE is “very much related” to the correlation. For instance, let’s consider a perfect model (RMSE = 0 m) that you divide by a factor of 2: the correlation coefficient will be stick to 1 while the RMSE will get large.

-P6, L6-7: what is the specificity of the RdP compared to any other coastal zone bordered with a large shelf/shallow waters?

-P6, L17: what is the reference height for this open water wind measurements? Usually, this is hardly 10 m and therefore wind speed cannot be compared directly with 10 m wind speed from atmospheric models. If a correction is made for the comparison for instance applying a logarithmic model, which Z0 is used?

-P7, L9: rather “momentum balance”.

-P7, L12-18: why combining a linear with a quadratic bottom stress? This is not a common procedure. Please better justify through adequate references.

-P7, section “c”: if runoff is that important, why baroclinic circulation is not? Also, better justify through references why tide-surge interactions are huge?

-P8, eq. (3): where this parameterization comes from? Smith and Banke? Large and Pond?

-P10, L5: what does “it” stands for?

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