Interactive comment on “Spatial distribution of water level impact to back-barrier bays” by Alfredo L. Aretxabaleta et al.

Anonymous Referee #2

Response to Reviewers, comments in plain text, response in bold

<u>*GENERAL COMMENTS*</u>

The paper is impressive and can be influential, with some excellent ideas and its broad perspective based on observations, detailed numerical modeling, analytical modeling, and also possible extension nationwide using an ADCIRC tide constituent database. However, the analytical developments are dense and could be explained better for a less technical reader. Also, and most importantly, the discussion of potential use with ADCIRC tide modeling results datasets in storm hazard assessment needs work. I believe a major shortcoming there is the neglect of local wind setup in storms. Back-bays can have a wide range of inlet sizes, bay area, and often have shallow water depths, and as a result, can have an important role for local wind setup in storms. The paper can acknowledge this, if the authors agree, and it will be a stronger paper. As a result, I recommend major revision.

We included the wind effects on the bay in two sections of the paper. Section 4.2 and 4.3 include the methodology, while Section 5.2 includes the results of the wind effect on the bay. We will be adding the wind effects in the discussion of the ADCIRC results as the Reviewer suggests.

<u>*SPECIFIC COMMENTS*</u>

ABSTRACT

A minor comment – text refers to “Inlet geometry and bathymetry” as being important in semi-enclosed bays - isn’t bay area also important?

Bay geometry was included two sentences before. We will add bay area to that sentence so that it reads: “Bay area and inlet geometry and bathymetry primarily regulate the magnitude of the transfer between open ocean and bay.”

The abstract says storm transfers were from 70-100% but I see ~50% in some cases- eg MAN at 5-day period, WAR at 2-days. So this should be revised to 50-100%.

Agreed. It will be revised according to the Reviewer’s suggestion.

The last several sentences of the abstract don’t seem very consistent with the paper’s discussion- differing topics are discussed. Also where is mention of the ADCIRC based transfer estimates?

The second to last sentence will be removed and an alternative will be added that explains the ADCIRC-based approach potential for expansion to other areas. It will read: “An extension of the methodology that takes advantage of the ADCIRC tidal database for the east coast of the United States allows for the expansion of the approach to other bay systems.”

INTRO vs METHODS

A part of the intro’s literature review says that wind controls backbay currents (Garvine 1985). But in the methods, the approach uses tidal current M2 are a proxy for bed friction.

While wind has a large influence in back-bay current variability, the character of the wind response is less predictable. The total current is mainly a result of the combined effect of wind and tides. In the absence of wind, the tides will always be there. Estimating bottom friction based only on the M2 tidal current might underestimate friction in cases of large wind currents. In reality, the linear friction estimate is based on the match of the M2 tidal amplitude in the numerical model and the observations and as such it includes any components of the velocity that has affected the observations and resulted in the measured water level signals. This point will be clarified in the text by adding:

“As most of the water level variability in the bay is associated with the M2 semidiurnal tidal constituent (Figure 3) and the distribution of the tide has been properly validated in the numerical simulations (Defne and Ganju, 2015), we can take the spatial distribution of the M2 tidal amplitude as a proxy for the internal frictional effects in the bay. Bottom friction caused by both wind driven and tidal effects is considered in the numerical simulations. By adjusting the water level based on the numerical M2 spatial distribution, we are approximating the complete frictional characteristics of the bay.”

Section 4.2, p. 6 – on wind's influence – analytical model – instead of saying "angular frequency" would it be more clear to say "cyclic frequency"? The figures show "cycles/day" and "angular frequency" just is a little confusing to me. It isn't measured as an angle (degrees), it's measured by cycles.
We will include the suggestion to avoid confusion. It will read, "… ω is the cyclic (angular) frequency".

Here, you also might refer to the two different stresses as "dynamic stress" (tau_s I believe) and "kinematic stress" (tau_w), as well as in the figure caption. It would have helped me a little in understanding the figure's values (values of order 1000) as I typically think in terms of dynamic stress in Pascals.
We will change the text accordingly.

I am a bit confused about why tau is written as a function of omega (parenthetically) here so it is probably a good thing to explain things more. I see wind stress and frequency as being independent variables.
The wind stress represented here is the magnitude of the wind oscillations that result in water level fluctuations at a specific frequency.

Also, does the denominator really include cos(Lx) here? I see L as being on the order of 10000m and x being from 0 to 10000 (meters).
That is a typo. The equation should read cos(kL) instead. It will be corrected in the text.

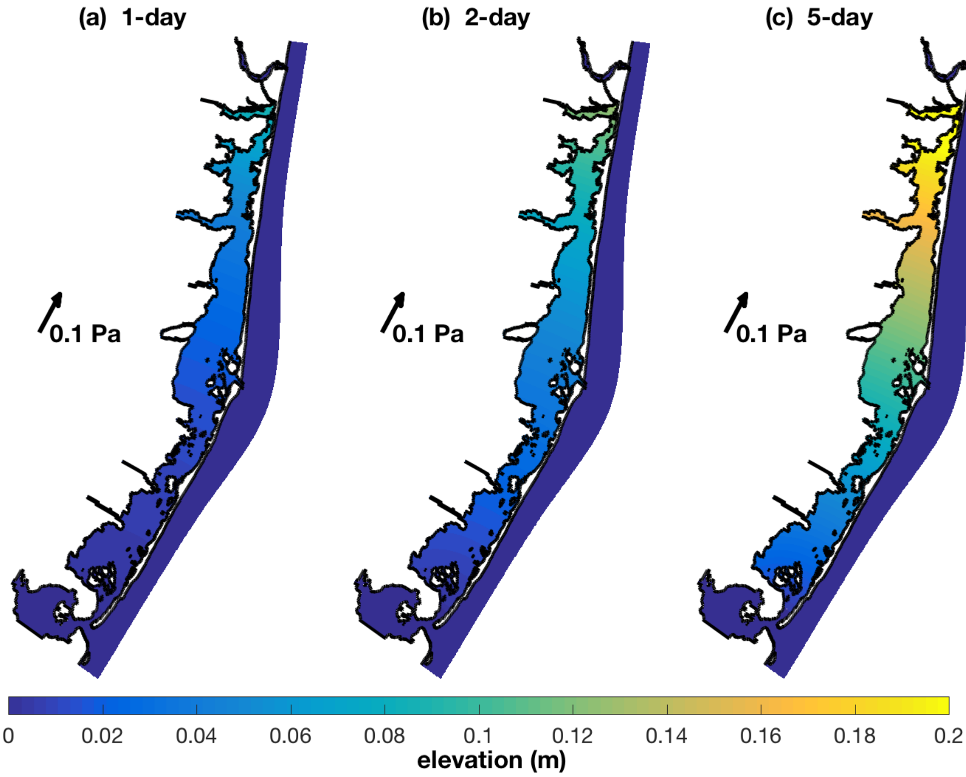
Section 6.1- aspects regarding sandy don't seem very useful here – see points below on local wind setup
"this far" – requires a minor revision to "thus far" I believe Section 6.2 **It will be corrected**

A very interesting idea and impressive analysis and results Section 6.3 **Thanks. We believe it has the potential to help in the prediction of bay water level hazards.**

Seems to be a fairly ingenious approach! are all US backbays really available and well-resolved in the ADCIRC tide data? In this case there is a large inlet that controls results, but I expect there are tougher cases.
Clearly not all back-barrier bays are well resolved in the tidal database, as some are relatively small. The ADCIRC model run provides resolutions down to around 50m in the latest database. The ADCIRC group keeps improving the resolution of their products. It is likely that their solutions will soon improve their quality and resolution, thus having the potential for providing even better back-bay response. The process of evaluating the quality of the current approach is underway, but so far it exhibits great potential.

A little more elaboration or demonstration here might be useful – it's the final landing point of the paper and seems like it could be helpful to illustrate this potential application with more detail.
The paper introduces the methodology based on the ADCIRC tides for the creation of offshore transfer estimates. The potential use for water level prediction in bays requires very careful calibration and skill assessment. The transfer maps represent an approximation to the average bay response to offshore forcing and specific events can depart significantly from the average response. Effects like overtopping of the barrier island, wave setup and runup, intense local wind setup, changes in frictional characteristics during a storm cannot be adequately predicted with the approach and need to be carefully included in the application to water level forecast.

The claim in the paper seems to be that local wind setup is small and negligible for storms, relative to transfer of offshore surge. The maximum wind setup mentioned is only 20cm (p10, line16). This all seems surprising to me, as I have learned that (large, shallow) backbay surge is often strongly influenced by local winds.
The reviewer is completely correct. There was an error in the calculations of the local wind setup. The formulas were fine (except from the typo mentioned above). The error was on the implementation of the formulas. After the error was corrected, the effect of the wind was much bigger (5-10 times). The maximum wind setup is now between 1 and 2 m. The text is being changed accordingly. The new Figure 8 shows the same pattern, but the magnitude is much bigger:

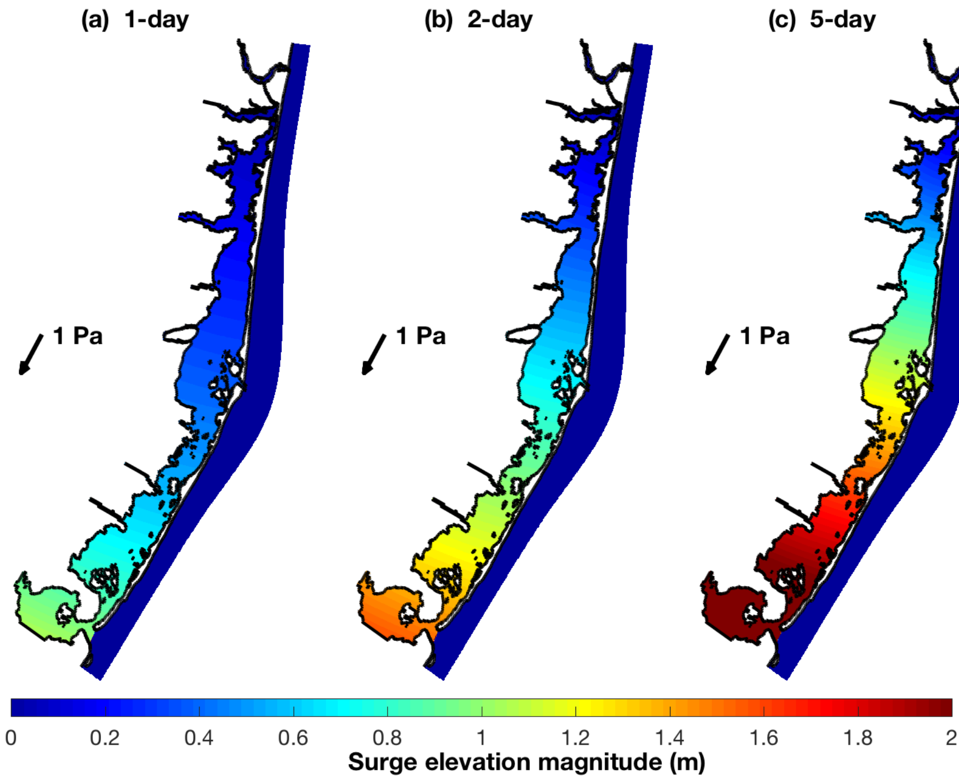


New Figure 8: Local wind setup inside the bay based on the Wong and Moses-Hall (1998) formulation for a wind stress of 0.1 Pa at specific periods: (a) wind with a 1-day period (e.g., sea breeze); (b) 2-day wind; and (c) 5-day wind.

The text will be modified to read: "Under persistent wind stress of 0.1 Pa (about 8 m/s wind speed) in the along-bay direction, the resulting setups varied depending on the frequency considered. Setup magnitudes over 0.2 m were estimated for the 5-day period wind (Figure 8c), while under half of that magnitude was achieved for the 2-day persistent wind (Figure 8b), and much smaller water level setup (peak smaller than 0.1 m) was estimated for the sea breeze (Figure 8a)."

A challenge: the wind setup for the north winds prior to Sandy's landfall was studied, when winds blew water toward (fortunately!) the main inlet. How about computing and comparing setup for when the wind turned around and blew from the south after landfall, toward the nearly dead-end northern end of the bay? If we are interested in hazards, then this was the primary backbay damage-causing period of the event. I believe the local wind setup became quite large and abrupt at that time.
The estimation of the wind setup during Sandy goes beyond the scope of the paper. The method can provide an estimate, but it is only a relative approximation. The available numerical model simulations of the area during Hurricane Sandy (Defne, Z., and Ganju, N.K., 2019, Collection of USGS Barnegat Bay hydrodynamic model simulations for Hurricane Sandy: U.S. Geological Survey data release, https://doi.org/10.5066/P99K85SW, available from the USGS Hurricane Sandy model portal: https://cmgdata.usgsportals.net/) represents a better estimate of the water level dynamics during the storm.
Nevertheless, we performed the estimation using the corrected implementation of the Wong & Moses-Hall formulation for both winds from the south (as requested) and also from the north. As the setup in the formulation is directly proportional to the wind stress, a wind stress of 1 Pa from the south would have resulted in elevations of around 2 m with the same pattern as in the new Figure 8 above. The formulation estimate for a wind stress of the same intensity (1 Pa) from the north is shown in the figure below. The figure shows the wind surge magnitude and provides an approximation for the gradient in water level caused by the wind stress. The total elevation would be the result of the addition of local wind setup plus offshore influence. So the elevation

would match the offshore signal in the south near Little Egg Inlet and it would be a set-down (negative elevation with respect to MSL if no offshore elevation considered) in the north of the bay. Numerical model simulations (Defne and Ganju, 2019) show a similar pattern prior to Sandy's landfall. The magnitude of the wind effect (set-down with wind from the north, setup with winds from the south) is of the order of 1-2 m.



The focus of the present study is not the study of the water level dynamics during Hurricane Sandy, but rather the characterization of the average response of the bay to hazards. The Sandy example in the paper is mostly used as a discussion point.

During Sandy, water levels at Mantoloking rose about 2.5m in 12 hours when the wind rotated to come from the east and then south, reaching a maximum that was very close to the open ocean or Little Egg values (perhaps 30cm lower) (USGS station 01408168).
The analysis of observations and model results during Hurricane Sandy goes beyond the scope of the current study. The analysis of numerical model results for Barnegat Bay during Hurricane Sandy is underway and the basic characteristics are available at the USGS model portal (https://cmgdata.usgsportals.net/).

Does the analytical approach capture the effect of this large wind setup? If not, does this issue show that local wind setup can be a challenge for using the ADCIRC tide data to estimate storm hazards? Or, is Sandy too unusual of a case, in which case a nor'easter might be a better discussion point for the paper?
The new corrected results of the wind setup effects show that the approach described in the paper represents a match with the expected behavior. The addition of the ADCIRC-derived offshore transfer and the analytical wind setup has the potential for adequately estimating water level in back-barrier bays.

Using a back-of-the-envelope computation with an admittedly simpler, but well- established method to compute the wind setup for a 20m/s wind, 50km fetch, 2m deep backbay, if fully developed, is 2m (the Zuider-Zee equation, or similarly, making the computation using a steady state vertically averaged momentum budget - Pugh and Woodworth, 2014, p. 156 in Section 7.3 on Storm Surges). It takes only a matter of hours to fully develop. I agree on the 1Pa wind stress for Sandy- this is reasonable.

U=20m/s eg post-landfall Sandy
depth d=2m
fetch F=50000m
setup S= 0.000002 * F * U^2 /g /d = 0.000002 * 50000* 400/9.8/2 (Zuider-Zee equation) S = 2m setup
I believe "L" in the analytical wind formulation is the basin length (for each of several small basins). I am computing the wind setup for a 50km long backbay, so perhaps using a longer fetch. But I believe this is appropriate as they are really not disconnected and the ~40km of the northern half of the model domain is strongly connected (not divided up into separate bays).

The new corrected estimates are consistent with the values described by the reviewer. The modified text in Section 6.1 of the paper will now read:
"The wind setup effect inside the bay due to local wind can also be estimated for Hurricane Sandy using the approach in Section 4.2. Maximum wind stress during the storm was about 1 Pa. To obtain a maximum effect (worst-case scenario) the wind was assumed to be persistently in the along-bay direction and that maximum stress was maintained for the duration of the storm. The maximum resulting water level considering the Wong and Moses-Hall method is linear with regard to wind stress magnitude (Figure 7b) and would have been 10 times larger than the setup in Figure 8b. The maximum wind setup would have been between 1 and 2 m, which was of the same order of magnitude as the surge produced from offshore sources. The cross-bay contribution to the wind setup during Sandy was comparatively small as wind direction was predominantly along-bay. Surge estimates from simple analytical formulations (State Committee for the Zuiderzee, 1926; Pugh, 1987) that do not consider storm duration produce similar magnitude results and are also dependent on the frictional response of the bay."

Sandy's surge might be viewed as having a "slow" 1-m surge with timescale of 3 days, plus a fast 1-m surge with a timescale of 1 day. I think for either case the ADCIRC- based transfer results in this paper suggest reduced transfer (maybe 60%; figure 10) for Mantoloking. In contrast, Sandy, the worst extreme event, shows that local wind effects lead to a similar surge there as seen offshore (perhaps 90% transfer). The transfer uncertainty estimates in Figure 11 (eg 4% in the 2-day storm band?) aren't evaluating this wind setup contribution, so aren't worth much.

The reviewer is correct about the relative magnitude of the two effects. The text of the paper will now include a section describing the need to add the wind effect to the ADCIRC derived offshore transfer: "The effect of local wind setup will also need to be added to the ADCIRC-based estimate, especially during severe storms. The approach discussed in Section 5.2 or even a simpler surge calculation (e.g., from the steady state vertically averaged momentum equations, as in Pugh (1987), from the traditional report of the State Committee for the Zuiderzee (1926), or the updated frequency domain equivalent from Reef et al., 2018) could be used and the resulting elevation could be added to the offshore transfer estimate obtain based on the ADCIRC tides. Thus, the production of bay water level predictions will require accurate wind forecast products."

I think this local wind effect pushing water into the northern end of the bay is what causes the unexplained high transfer at Mantoloking (Figures 4-5) in the 2-day storm band. The model captures it because it includes wind forcing (and Sandy), but a tide- only model will not.

Local wind setup is likely the reason for the enhanced transfer response during 2012. The magnitude of the storm is not as important as the fact that during 2012 the general pattern of the pressure systems over the Atlantic was conducive to wind in the along-bay direction. The effect was a potential enhancement of the local wind setup that showed up in Mantoloking and not in other stations (e.g., Waretown) because the water was able to setup in the northern part of the bay. The text in Section 5.1 will include a sentence describing this:
"The model reproduced the enhanced transfer in the storm band at Mantoloking during 2012, suggesting a physical mechanism for the change that the model was able to capture but remains unexplained. The likely explanation is that the location of the Azores-Bermuda high-pressure system over the Atlantic in 2012 (Mattingly et al., 2012), associated with the negative phase of the North Atlantic Oscillation, resulted in average winds that lined up with the axis of the Bay and caused enhanced wind setup in the northern part of the bay."

To conclude, I think the method presented would, for storms, often be low-biased for peak water level risk estimates, due to local wind setup. I suggest proceeding very carefully and validating storm hazard estimates, using observational data.... or evaluating case studies carefully to determine whether local wind setup corrections or larger uncertainties can be added for the storm driven flood hazards. Underestimating storm-driven flood risk is worse than not estimating it at all.

REFERENCE Pugh and Woodworth, 2014, Sea Level Science, Second Edition, Cambridge University Press, Cambridge, UK.

We greatly appreciate the reviewer's comments that forced us to review the implementation of the wind setup approach discovering an error. When the error was corrected, the method of combining the transfer of offshore fluctuations, via the analytical or ADCIRC-based approaches, and the local wind setup effect showed the potential for estimating bay water levels in response to tidal and storm forcing. We agree that under or over-estimation of storm driven flood risk has severe consequences for coastal hazard mitigation. Proper skill assessment of the methodology will be needed before the implementation of this kind of approach for storm impact. The text will include a discussion of this point:
"Careful consideration needs to be given to the estimation of coastal hazards especially for the forecast of intense storm effects. The inclusion of meticulously validated methodologies that consider both offshore influences (e.g., using the transfer estimated from ADCIRC tides) and local wind setup (e.g., Wong and Moses-Hall, 1998; Reef et al., 2018) is necessary. Skill assessment of storm hazard estimates using adequate observations is critical to avoid producing under- or over-predictions of flooding and inundation."