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Natural Hazards and Earth System Sciences
Discussions

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

Anonymous Referee #2

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GENERAL COMMENTS

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.

SPECIFIC COMMENTS

ABSTRACT

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

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%.

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?

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.

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. 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.

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.

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).

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

A very interesting idea and impressive analysis and results

Section 6.3

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.

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 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 inflenced by local winds.

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.

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).

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?

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 * Uˆ2 /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 $\sim$40km of the northern half of the model domain is strongly connected (not divided up into separate bays).

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.

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.

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.

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