

Interactive comment on “Multivariate analysis of extreme storm surges in a semi-enclosed bay” by Yao Luo et al.

Anonymous Referee #1

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Review of "Multivariate analysis of extreme storm surges in a semi-enclosed bay".

This paper refers to Beibu Bay, also known as the Gulf of Tonkin, in the South China Sea, where there is data from 2 tide gauge stations around 200 km apart: Dongfang on the island of Hainan and the city of Beihai on the mainland to the north. The premise of the paper is that extreme storm surges at Beihai can be predicted a few hours in advance by surges at Dongfang. The method presented fits a statistical model (a bivariate form of MGPD) to the data at the two sites. A large set of random data is generated according to that model, from which likelihoods are derived, concluding that after removing tidal effects a 50-yr-return surge at Dongfang gives a 95% chance of a 50yr surge at Beihai.

This is a worthwhile goal, and there is a good set of tide data to work on, but I have

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major concerns about the methodology used, outlined below. Correcting these would require such substantial reworking that I recommend that the paper is rejected at this time.

There is a fundamental flaw to the paper which must be addressed before any further consideration, which is that the tides are not correctly handled. The surge data used is all non-tidal residuals, and the authors assume that the tides are therefore completely removed. But it is very obvious in Fig 2 that there remains a periodic component. It is possible that this is due to incorrect calculation of tidal harmonic constituents. However it is also possible that there is a nonlinear interaction of surge and tide. For example one effect of the storm surge is to sometimes to alter the timing of the tide, as discussed in eg Pugh Woodworth's 2014 book, chapter 7. This has the effect of creating an apparently very large surge during the rising tide, but a smaller residual at the high tide. A quick check is to calculate the standard deviation of residuals for different tidal levels. To estimate the magnitude of this effect the authors need to consider the depth of the Bay, described as "shallow", and the tidal phase difference of the sites, but it would be worth checking for existing work in this area, as there may be modelling studies to draw upon.

Either way, the distributions of non-tidal residual surge at Dongfang and Beihai cannot be considered independent, invalidating the statistics used and the conclusion, since the correlation may be due to the tides and not the extreme events - and the extreme events may be exaggerated. A different approach may be required, and at the very least the tides cannot simply be dismissed from the analysis.

Some other concerns - I have not attempted to be comprehensive.

Line 196 is over-optimistic about the fit of the distribution. Fig 3 and 5 show an increasingly poor fit for the most extreme values. From figure 3 in particular I see no justification for extrapolation to 50-yr returns. And the model is only tested on the same data used to generate it.

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Line 119 "only" 0.023% missing data... that is pretty low, but do those 2 days correspond to 2 really big storms? If so, how much difference could it make?

The paper could be better structured to aid the reader. It is not obvious early on that the multivariate analysis is used to compare two sites, rather than for example surge and wave variables.

Section 2 is unnecessarily hard to follow, with little explanation of where the sequence of definitions is leading, and a few careless errors. Why does the reader need to know these details? Why do we change from x_d to x_n ? Does $W_r(x)$ refer to a vector x - if so it would be usual to use bold or underscore. Why the change from X to x ? Eq. 4 should be $\frac{\partial^2 W}{\partial x \partial y}$. I assume the transform to "polar" coordinates is what is meant by "converted" in Fig 4... and so on.

It might be better to integrate section 2 to section 4. It may then become apparent which definitions are important and whether key information is missing.

Line 225 reveals that there has been no consideration of the precision of the method or error analysis of the data - I find it extremely unlikely that the probability of a 50-yr surge at the Beihai, given one at Dongfang, can be predicted to 4 significant figures.

A useful addition to 4.3 would be the risk of false-negative - if a warning system were based on extreme surges at Dongfang, what is the likelihood of missing an extreme surge at Beihai?

Fig 1 - a better map would be useful to the reader, indicating at least the bathymetry, but also eg the major tidal constituents, the dominant typhoon tracks, locations of coastal cities.

All figures could have better captions.

The language is generally good.

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