Review: "Robust and Flexible Tidal Reconstruction from Sparse High Water - Low Water Observations" Joris (J.G.W.) Beemster[1], Pascal Matte[2], Silvia Innocenti[2], Bas (D.S.) Van Maren[3,4], and Ton (A.J.F.) Hoitink[1]

Overall Assessment:

This is a strong and potentially impactful paper. The authors visit a long standing question of how to provide tidal predictions given just high and low tidal data, i.e. height and times of turning points, rather then regular time series of tidal heights. They develop and describe a modernized High–Low Tidal Analysis (HLTA) framework that operates directly on the tidal extrema (i.e. HW/LW measurements) rather than continuous water-level time series records. The paper goes on to provide a means of predicting the time series tidal values given the HW/LW predictions. I recommend acceptance with minor revisions.

Summary

The paper is well motivated by the need to exploit such historical HW/LW datasets. The paper develops develop two formulations of HLTA - the long-period harmonic (LPH) and empirical–astronomical (EA) formulations and compares the formulations with results from Harmonic Analysis methods.

General Comments

1. The manuscript repeatedly argues that HLTA can outperform harmonic analysis and is particularly advantageous in shallow, nonlinear systems. The claims of general applicability of the HLTA framework and its two formulations, LPH and EA, are perhaps somewhat overstated since the study was carried out at just two tidal stations (Bath and Melle) in the Scheldt Estuary. It is worth examining whether its predictor selection and lag structure generalise to regimes where diurnal inequality is large and the dominant modulation is not the spring-neap cycle. The paper mentions diurnal tidal regimes only in passing (section 4.1) but does not explicitly evaluate the framework for mixed or diurnal regimes. The HLTA framework as presented assumes four extrema per lunar day. An explicit statement about how the framework should be adapted, or fails, for diurnal systems would prevent misapplication. The authors should either test the framework on more contrasting tidal locations/sites or they should consider qualifying the scope in their conclusions.
2. The claim of 38% improved timings and equivalent comparable heights for LPH relative to HA of 10-minute records at Bath, is certainly noteworthy. However it too would benefit from further comparative analysis at more locations. Regarding (line 517) the comparisons with Harmonic Analysis as directly applied to HW/LW values, it is well known that HA works poorly on HW/LW records because, to put it crudely, the harmonic method does not "know" that the data points are turning points, losing a most valuable piece of information. The statement that "both HLTA approaches substantially outperform harmonic analysis applied directly to HL observations" (line 518) is therefore not surprising. The authors repeatedly labour this point in varying detail throughout the paper (see lines 7-9, 34-37, 84-88, 112-119, Section 3 128-184, 518 referring to the inaccuracy of HA to analyse HW/LW records. Perhaps this point is too frequently made. Fortunately, the study does indeed make the more reasonable comparison between HLTA and with HA using a ten minute time series in Figure 5 and Line 495.

Specific Comments

1. Perhaps the paper understates the extent to which the study relies on the work of Horn 1948 and Lubbock 1837. It states "This idea builds on" . A better statement would be that the method is directly derived or a direct descended from the work of those earlier authors. Line 39.
2. The choice of 5-year calibration (line 360, Sec 5.5) (2015–2019) and 2-year validation (2020–2021) is reasonable, but it would be worth noting whether the validation period is anomalous in any way — for instance, the COVID-era may affect shipping and fluvial flow and in principle influence surge residuals.
3. It would be useful to know what is the likelihood that the results of the comparisons on Figure 5 , are or are not statistically significant. What is the margin of error on RMSE and ME values. Would these benefit from wider testing.
3. Despite the length of the paper, I feel it lacks sufficient detail for a reader to readily duplicate the LPH and EA methods, and to evaluate its performance on their data. This is after all a basic requirement of the scientific method - the ability to duplicate results. It leaves the reader somewhat confused over the details of method used. For example in section 4.3, it is not clear whether these empirical factors are station-specific or whether they are stable across calibration sub periods. Therefore, Section 4 would benefit from more detailed description of the method and role of the algebraic terms.
4. The Github repository download furthermore does not contain adequate instructions on how to run the code and the various input data requirements. It would be helpful if the Github download readme file contained an overview and sufficient instructions to enable it to be readily run. See https://github.com/JorisBeemster/HighLow_TA.

Technical Corrections

1. Line 2 is incorrect in implication. Is it meant to mean "This method suffers from poor accuracy when applied to ...
2. Line 51 IRLS not yet defined
3. Line 145 Define what is meant by "minimum frequency distance"
4. The manuscript alternates between: "high-low observations", "HW/LW observations", "extrema observations". Consider standardizing terminology throughout.

Conclusion

This paper makes a valuable methodological contribution. The primary weaknesses of the study are not methodological flaws but rather limitations in the breadth of validation. Thus the authors should consider including more contrasting tidal locations/sites or they should consider qualifying the scope of the method in the conclusion.

Recommendation:

I would therefore suggest the paper is accepted subject to a number of minor revisions which address the above issues.