

Reply to RC2 (Anonymous Referee)

Overall Assessment:

This paper develops new framework for sparse high-low tidal observations. The proposed HLTA framework is promising, but the current manuscript has significant gaps in verification (limited to one tidal regime), practical applicability (no tidal constant output, no minimum data length analysis), and structural clarity. If these issues are addressed, the paper would be a valuable contribution to the tidal analysis literature. My detailed comments are listed as following.

We thank the referee for the constructive assessment. We agree that the main limitations concern the breadth of validation and practical guidance, and have addressed these points through additional tests across contrasting tidal regimes, an analysis of record-length requirements, and an assessment of tidal-constant recovery from (interpolated) reconstructed signals.

Major Editorial Comments:

1. Section 2 is suggested to be merged into the introduction. And new section 2 aims to describe the methodology used in this paper, namely HA and HLTA. Section 2.1 should describe the equations of HA with derivation constraints. Section 2.2 can describe the limitations of HA in processing high-low data. Section 2.3 introduces the principle of HLTA, especially how HLTA solves the problems of special sampling structure and aliasing.

We agree that bringing the descriptions of HA and HLTA closer together could improve the clarity of the manuscript and reduce repetition between the current Sections 2 and 3. In the revised manuscript, we have therefore reorganized these sections, incorporating the relevant historical background more concisely into the Introduction and introducing the HA and HLTA methodologies after each other.

2. In the discussion, sections should be merged and new sections should be added: One section aims to describe the advantages and the limitations of HLTA, and some summary tables can be provided. For examples, a table compares HLTA and previous methods from Horn and Lubbock, emphasize the improvements and a table compares HA and HLTA, showing how HLTA overcomes the limitations of HA. The other section aims to provide an 'Applicability Guide'—under what conditions should EA be used and under what conditions should LPH be used? Providing a decision tree or comparison table would greatly enhance its practical value. Also, this paper use 5-year data to calibration, if the users only have one-year data to calibrate or even shorter, any precautions? For short-term data (such as half year or only one-month), OLS may overfit, and ridge regression may be applied which has been proven by Pan et al. (2024, <https://doi.org/10.1016/j.ocemod.2024.102372>).

We agree that the Discussion would benefit from a clearer synthesis of the applicability, strengths,

and limitations of HLTA. In the revised manuscript, we have therefore expanded the Discussion to address these aspects more explicitly, including the relative performance and applicability of LPH-HLTA and EA-HLTA, robustness to observational uncertainty, extension to non-stationary conditions, and the main limitations of the framework. We have also added practical guidance on the circumstances under which the two HLTA formulations may be more suitable. We did not merge the Discussion subsections or introduce a formal comparison table or decision tree, as we found that the revised subsection structure provided a clearer way to relate these points directly to the corresponding results.

In addition, we have extended the analysis to investigate the influence of calibration-record length, including records substantially shorter than the 5-year calibration period used in the main analysis. This allows us to provide more practical guidance on the amount of data required and on suitable model configurations for shorter records. We did not include ridge regression as an additional method, as we consider this beyond the main scope of the manuscript and prefer to keep the analysis focused on the proposed HLTA framework.

3. Some figures can be moved forward. For example, figure 3 can be moved into section 4.2 to illustrate the introduction of LPH. Figure 4 can be moved into section 4.3 to illustrate the introduction of EA-HLTA.

We appreciate this suggestion. The structure of the manuscript has been substantially revised, in part to accommodate the expanded 15-gauge analysis and the associated methodological and validation sections. As a result, the organization and role of the figures have changed, and we have not followed the proposed relocation of Figures 3 and 4 exactly as suggested. We have instead placed the methodological material and figures where they best support the revised flow of the manuscript.

4. Revise section 4 ‘Material and Methods’ as section 3 ‘Study area, data and model setup’. Section 5.4 can be moved into previous section and section 5.3, 5.5 and 5.6, 5.7 can be merged. Section 6.1 should be moved into previous sections.

We thank the referee for these detailed structural suggestions. As mentioned above, the manuscript has undergone a substantial reorganization, in part to accommodate the expanded 15-gauge analysis and the revised methodological and validation material. As a result, the final structure does not follow the proposed section moves and mergers exactly. Nevertheless, we have substantially streamlined and reorganized the Methods and Results sections and believe that the revised structure improves the overall clarity and flow of the manuscript.

Major Technical Comments:

1. The authors develop a high-low tidal analysis (HLTA) framework, but only test it in the Scheldt river estuary dominated by semi-diurnal tides. The performances of HLTA at two tide gauges in

the Scheldt estuary are very good, but this cannot guarantee its generality in other sea areas. There are a lot of scenarios needing verification. For examples, in the deep ocean with very weak tidal non-linearity and relatively small tidal ranges, can HLTA outperform HA derivations or they are comparable? How about the performances of HLTA in sea areas dominated by diurnal tides? (such as the Gulf of Mexico, the Gulf of Tonkin). In these special sea areas, only two extremes in one day, the number of tidal observations are much less. Furthermore, in some shallow bays, due to non-linear interactions and resonance effects, M4 or M6 tides may have very large amplitudes, thus, may be more than 4 extremes in one day, the process of HLTA needs to be adjusted? In general, tidal observations in different conditions (e.g. tidal types, tidal ranges, tidal non-linearity) should be examined or at least noted in the discussion section to clarify the applicable boundaries of the proposed HLTA.

Our initial motivation for focusing on the Scheldt was that its shallow and strongly distorted tidal conditions provide a particularly challenging setting for HW/LW analysis. We agree, however, that extending the evaluation to locations with different tidal characteristics provides a more transparent assessment of the general applicability of HLTA. In the revised manuscript, we included additional stations spanning different tidal form factors, including mixed and diurnal regimes, as well as different degrees of shallow-water distortion and tidal range.

We did not extend the framework to locations with double high or low waters in the present revision. Such cases may require adaptations to the method, depending in part on how these additional extrema are represented in the available HW/LW records. As these situations are relatively uncommon and fall outside the main scope of this study, we have instead noted them as a current limitation and possible direction for future extension in the Discussion.

2. Line 25, it seems that only HA-derivations can extract long-term changes in tidal regimes from past HW-LW data, although HLTA can better predict tidal extremes, it cannot output tidal constants of major constituents which impedes its application. If possible, can HLTA be merged into HA-derivations? As shown in Figure 5f, the main errors of HA deriv are from time (i.e. phase) because HA deriv lacks temporal constrains which can be provided by HLTA. Nevertheless, these should be noted in the paper.

HLTA does not directly provide conventional tidal constants. However, changes in tidal regime can also be assessed from the extrema themselves, for example through changes in mean tidal range, spring-neap variability, or high- and low-water levels and timing, all of which can be estimated directly within the HLTA framework.

In the revised manuscript, we additionally evaluate whether tidal constituents can be recovered indirectly by first reconstructing a continuous tidal signal from the HLTA-predicted extrema through interpolation and subsequently applying conventional harmonic analysis. The recovered constituent amplitudes and phases are compared with those obtained from harmonic analysis of the high-resolution observations and from harmonic analysis, with and without derivative constraints, applied directly to the HW/LW data.

We have also considered whether HLTA could be coupled more directly with harmonic analysis, for example by providing additional constraints on constituent estimation. We agree that this

is an interesting direction, but consider the development of such a hybrid approach beyond the scope of the present study. We have nevertheless mentioned this possibility in the revised Discussion.

3. Line 51-53, it is good to explain the modifications and improvements of HLTA approach compared to previous studies. However, these are not reflected in the experiments which let the readers be confused about these modifications. For example, you should design a simple experiment in which the performances of OLS-based HLTA and IRLS-based HLTA are compared to illustrate to meaning of using IRLS. Moreover, why the authors reformulate the extrema series? To answer this question, similar comparative experiments should be conducted (not just say to improve numerical conditioning, you need to provide values).

We agree that the methodological choices introduced in this section can be motivated more clearly. Regarding the use of iteratively reweighted least squares (IRLS), we consider this to be a well-established robust fitting approach, and an explicit comparison with ordinary least squares (OLS) would move the manuscript away from its main focus. We therefore do not include a dedicated OLS–IRLS sensitivity experiment, but we have clarified the role of IRLS in the methodological description.

We also reconsidered the proposed reformulation of the extrema series into mean and deviation components. The original motivation for this formulation was that it can concentrate part of the tidal variability into a smaller subset of the transformed series, potentially reducing the number of parameters required, limiting overfitting for shorter records, and providing a conceptually useful separation between mean-level variability and tidal amplitude or timing. Such a separation may be particularly relevant in non-stationary river-tide environments, where external forcing such as river discharge can affect these components differently.

However, we ultimately did not adopt this reformulation in the revised method. In mixed and diurnal tidal regimes, the transformation requires corresponding high- and low-water observations to form each mean–deviation pair. Such pairs are not always available in diurnal or mixed records, nor in incomplete HW/LW records. Applying the reformulation would therefore discard otherwise usable observations and unnecessarily reduce the amount of information available to the fit. For this reason, the revised implementation operates directly on the HW/LW observations. We nevertheless retain the motivation and conceptual interpretation of the mean–deviation formulation in the methodological framework, while making clear that it is not used in the experiments presented in the revised manuscript.

4. Line 304, the temporal resolution (10-minute) may be not enough to find accurate high-low tides since the timing errors of HLTA are close to 10-minute. May be 1-minute data is more suitable.

We agree that the temporal resolution of the underlying water-level records is important when determining HW/LW timing. In the revised analysis, we use observations from 15 stations provided by several agencies, with sampling intervals ranging from 1 to 15 minutes. The HW/LW observations were not obtained simply by selecting the maximum or minimum sampled water

level. Instead, event times and heights were refined using local cubic fits around each turning point, after which the estimated event times were rounded to the nearest minute.

To assess whether this procedure provides sufficient temporal accuracy at the coarser end of the sampling range, we performed a test using a harmonic reconstruction for Bath sampled at 15-minute intervals. The cubic-refinement procedure achieved a timing RMSE of approximately 6 s before rounding the estimated event times to the nearest minute. We therefore consider this approach sufficiently accurate for deriving HW/LW heights and timings from the available 1–15-minute continuous records, and do not expect the native sampling interval to impose a meaningful limitation on the HLTA validation.

Minor Comments:

1. Line 8 in Abstract, I think ‘poorly’ is not suitable, you can say HLTA outperforms HA constrained by derivations

We revised the abstract to state that HLTA outperforms harmonic analysis applied directly to sparse high–low data, which is constrained by aliasing and dependencies between constituents.

2. Line 30-31, do not know what this sentence wants to express.

Our intention was to distinguish irregular sampling at externally or randomly prescribed times from high- and low-water sampling, for which the observation times are determined by the tidal signal itself. We revised the text to make this distinction more explicit.

3. Line 33, not Ji and Guohong, but Ji and Fang. The author’s full name is Guohong Fang, Fang is the family name.

Thank you for the correction. We have revised the citation to “Ji and Fang” accordingly.

4. Line 34-35, overstate the drawbacks of HA constrained by derivations, at least in the tests provided in this paper, I think both cases (ill-conditioned and meaningless solutions) do not exist or are not severe.

We agree that the original wording may have overstated the limitations of derivative-constrained harmonic analysis. We therefore removed the reference to ill-conditioned and physically ambiguous solutions and revised the text to state more precisely that standard HA applied to HW/LW data can yield strongly correlated and poorly identifiable constituent estimates, even when derivative constraints are imposed.

5. Line 51., the full name and references of IRLS should be provided.

We provided the full name, iteratively reweighted least squares (IRLS), and added the relevant reference.

6. Line 71, not Zuosheng et al., 1989, but Yang et al., 1989. The author's full name is Zuosheng Yang, Yang is the family name.

Thank you for the correction. We have revised the citation to "Yang et al. (1989)" accordingly.

7. Line 124, I think may be two reasons for this phenomenon: One is nowadays high-low tidal observations are rare, the other is that HA_derivation is generally usable.

We agree that the limited availability of high-low water observations might be one of the reasons these approaches have received relatively little attention. However, we do not consider derivative-constrained harmonic analysis to be generally applicable, particularly in systems with strong tidal distortion and significant overtides. In such settings, the relationships imposed by the derivative constraints may not adequately represent the observed extrema. We therefore retain the original statement and demonstrate the limitations of derivative-constrained HA more clearly in the revised manuscript.

8. Line 125, I think 'poorly' is not suitable, you can say HLTA outperforms HA constrained by derivations

Thank you for the suggestion. We have removed the term "performs poorly" and revised the sentence to focus more neutrally on the limitations of harmonic analysis.

9. Line 190-192, may be some schematic diagrams can be provide

We have added a schematic diagram summarizing the overall HLTA framework, including the transformation of HW/LW observations into lunar-transit-referenced extrema series, the LPH-HLTA and EA-HLTA modeling approaches, the optional incorporation of external forcing, and the subsequent prediction and reconstruction steps.

10. Line 200, you do not explain why decompose them into a mean component and a deviation. In my opinion, this decomposition can to some extent solve the ill-conditioned problem since it redistributes information from two "similar and ambiguous" variables into two "orthogonal and clearly differentiated" variables.

We agree with the reviewer that decomposing the two extrema series into a mean component and a deviation component can improve the numerical conditioning of the regression problem. The two original extrema series can be strongly correlated and therefore contain partly redundant information, whereas the mean and deviation components separate the common water-

level variability from the difference between the extrema. This transformation may help reduce the number of tidal constituents that need to be fitted separately, thereby decreasing model complexity and potentially reducing the risk of overfitting.

In the original manuscript, we presented this decomposition as a possible approach for improving numerical conditioning. However, following revisions to the methodology, we decided not to apply the transformation in the final model formulation because it introduces complications when handling missing data and mixed or diurnal tidal records. We have therefore removed the decomposition from the implemented methodology, while retaining and clarifying its motivation as a potential alternative approach.

11. (9), do not know how $X(t)$ is generated. Also, provide some schematic diagrams to illustrate Eq.(8) since the readers may do not know related progress like us.

We thank the referee for pointing out this lack of clarity. In the revised manuscript, the notation has been harmonized by replacing $X(t)$ with $G(t)$, consistent with the notation introduced in the preceding stationary formulation. We have also substantially expanded the description of the EA-HLTA formulation, including the construction of the astronomical predictor set, the role of the individual predictor terms and interactions, and the treatment of station-specific parameters. In addition, the description of the non-stationary extension now explicitly clarifies that $H(t)$ represents the external forcing time series (river discharge in the present applications), while the linear and quadratic terms allow the astronomical coefficients to vary with the magnitude and nonlinearity of the forcing response. We therefore believe that the construction and role of $G(t)$, $H(t)$, and Eq. (8) are now considerably clearer. A schematic overview of the broader HLTA workflow has also been added to aid interpretation of the overall methodology.

12. Line 295-300, provide tidal constants of major tidal constituents at two stations, not only M4 and M6 amplitudes.

With the extension of the analysis from 2 to 15 stations, reporting the tidal constants of the major constituents for every station would result in a considerable amount of additional information. We have therefore chosen to characterize the tidal regime at each station using more compact and directly interpretable metrics. Specifically, the revised manuscript reports the greater diurnal tidal range and (derivative) skewness, which provide information on tidal range and tidal asymmetry, respectively. In addition, the revised station-location figure now includes the average tidal hydrograph for each station. Together, these additions provide a concise overview of the differences in tidal range, shape, and asymmetry across the 15 stations without requiring a full listing of constituent amplitudes and phases.

13. Line 364, nodal corrections should be included at Bath I think.

We agree that nodal variability should be accounted for in the harmonic-analysis-based approaches. In the revised manuscript, we therefore include conventional nodal corrections in

both the HA and derivative-HA formulations, including at Bath.

The treatment differs for the HLTA formulations. LPH-HLTA does not currently include explicit nodal corrections; instead, nodal modulation must be resolved from the data through long-period terms. Adopting the standard Rayleigh criterion, this requires a record length of approximately 18.6 years to resolve the nodal cycle directly. EA-HLTA, by contrast, represents nodal variability through its dependence on lunar declination. Because the response to declination can be learned from its much shorter dominant periodicity, the tropical month, EA-HLTA does not require the full 18.6-year nodal cycle to be explicitly included in the calibration record. We have clarified these distinctions in the revised manuscript and additionally discuss the explicit inclusion of nodal-cycle corrections in LPH-HLTA as a potential extension for future work.

14. Figure 2a, validation period is also 2015-2019?

Thank you for pointing this out. This issue is no longer relevant in the revised manuscript, as the original figure has been replaced following the extension of the analysis to the larger set of stations.

15. Figure 3, change 14.76 d to 14.77d to keep consistent with Line 420.

We changed “14.76 d” to “14.77 d” in Figure 3 (now Figure 6).

16. Line 421, Msf can also be generated by O1 and P1 tides, not only M2 and S2. Mm cycle can be generated by M2 and N2, not only N2 and L2. Mf can be generated by K1 and O1 tides, also K2 and M2 tides.

We agree that these long-period modulations can be generated by additional constituent pairs. However, the periods discussed here are the apparent periodicities arising from aliasing when individual tidal constituents are sampled once per lunar day, rather than the modulation periods of all possible constituent pairs. We added a sentence earlier in the paragraph to clarify this distinction and revised the description of MSf to state that it is “primarily associated with the spring–neap interaction of M_2 and S_2 ,” hereby acknowledging that other constituents may also contribute.

17. Line 423, K1-O1 is unrelated to 27.3 d, but Mf tide.

We agree that the beat period of the combined K1–O1 constituents corresponds to the Mf period. However, this is not the periodicity referred to here. The apparent 27.3-day period arises from aliasing caused by sampling K1/O1 once per lunar day, which is how the K1/O1 constituents appear in the spectrum of the height-deviation term (h'). We therefore retained the original text.

18. Line 508, not HW but LW.

Although we intended to refer to HW, we agree that the wording could be interpreted differently. As the sentence was not essential, we removed it from the revised manuscript.