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Revised submission

Author's Reply to Anonymous Referee #2

The authors appreciate the second round of useful and constructive comments that helped to improve the submitted manuscript:
High current speed events in a harbor channel driven by resonant sub-hourly sea level dynamics: an example from Varna, Black Sea

In the reply, *revised text is given in italics*. The line numbers refer to the revised MS submission.

Comment 1
I still have one concern regarding the numerical modelling approach. The computational domain appears to simplify the two connecting channels into a single equivalent channel.
Reply 1
Initially, we made this simplification to set up the analytical study to find the seiche periods when connecting a larger lake to the channel(s). When attempting to introduce two different channels connected to a lake, we could not find explicit formulas for the standing modes, and the analytical treatment became unreasonably complicated. Already, treatment of the channel-lake system required solving the implicit equation Eq(A3). We note that the two parallel channels were of nearly equal length and depth (lines 377-380). Merging the parallel channels into a single channel allowed for analytical determination of resonant periods. Experiments with different configurations of the channel-lake system revealed that resonant periods of modes 1-5 were of similar magnitude (lines 398-419) and were not highly sensitive to the selection of geometrical parameters.

We also added a text in line 402:
Sub-experiments with a channel width of 80 m (not shown), corresponding to a single channel 3, revealed approximately the same resonant periods as with merged channels 1 and 3 of total width 300 m.

Comment 2
Since frictional effects play a key role in the propagation and attenuation of long barotropic waves, such a simplification may influence the simulated water-level gradients, flow velocities, and consequently the circulation patterns within both the channels and the lake.
Reply 2
We modified lines 384-386 to:
In this interpretation study, detailed consideration of topography and coastline, as well as of non-linear and frictional effects on the flow patterns, was deferred to future studies.

Comment 3
To strengthen the credibility of the numerical results, I encourage the authors to provide additional evidence demonstrating that this simplification does not significantly affect the conclusions. This could be achieved, for example, by:
(i) comparing the simplified configuration with a model that explicitly represents the two individual channels, showing that the differences are negligible;
Reply 3
When planning the study, we considered including 2D modeling on a high-resolution grid, but we left this out because the sub-hourly atmospheric forcing fields that could excite resonant current and sea-level oscillations in the channels and the lake were not available (lines 445-449). We tested an option to add noise to the available hourly-interval forcing to mimic the sub-hourly forcing, but this initial attempt was not successful in obtaining realism of simulated current and sea levels towards

the observed time series. Since the manuscript is already loaded with various study topics, we did not extend the present study to full numerical modeling, including both channels.

Comment 4
or (ii) presenting a comparison between simulated and measured water levels and/or current velocities at the observation stations to demonstrate that the simplified model reproduces the observed hydrodynamics with reasonable accuracy.
Reply 4
Simulation of observed time series was not possible due to the lack of sub-hourly atmospheric forcing data. Therefore, such comparisons were not possible in the present study. Regarding the two channels and the lake, observations were conducted only in one channel.

Comment 5
Although I do not regard this issue as a fundamental obstacle to publication, I believe that some additional justification of the modelling assumptions would increase confidence in the numerical results.
Reply 5
In the conclusions, we have written: The parametrization of the forcing factors for sub-hourly variability – including the effects of meteotsunami generated by moving weather patterns, wind gusts, sub-hourly intense air pressure, and wind perturbations – needs further study to establish their relations with background conditions for the forecast of high-amplitude event occurrence.

We modified the formulations for "numerical model" and/or "model" as listed in our additional author's comment. Moreover, we changed the text as follows.

In lines 365-368:
For the analytical and numerical solution of the waves, primarily for estimation of the resonant periods, the navigation channels 3 and 1 were approximated by rectangular basins with length L = 2,400 m, constant depth H = 7 m, and a total width B = 300 m (merging both of the channels); the lake was approximated with length L = 11,000 m, depth H = 10 m, and width B = 1,900 m. The widths are much smaller than the lengths; therefore, the conditions for 1D dynamics are satisfied in the first-order approach for both the channels and the lake.

In lines 378-380:
Note that for a 1D channel, the results are independent of the channel width; width comes into play for connected and/or branching channels, determining transmission and reflection of long waves in individual branches.

In lines 384-386:
In this interpretation study, detailed consideration of topography and coastline, as well as of non-linear and frictional effects on the flow patterns, was deferred to future studies.

In lines 402-404:
Sub-experiments with a channel width of 80 m (not shown), corresponding to a single channel 3, revealed approximately the same resonant periods as with merged channels 1 and 3 of total width 300 m. The largest period changes occurred for the longest mode 0 period, which increased with more complex basin configurations, from 98 min in Exp. 3 and 4 to 196 min in Exp. 5, corresponding to the lake-amplified observed peak period P4 of 206 min. These free, frictionless oscillation modes just yield the resonant periods.

Additional Author's comment

Upon checking our text again, we found an inconsistency in the use of the term "numerical model". We applied numerical (solving) methods to extend the analytical solutions of the linear shallow-water equations in one spatial dimension. Similar uses of the term "model", oriented mainly to process understanding rather than full prediction of complex ocean conditions, have been used in many papers and textbooks. For example, the textbook by Dijkstra, "Dynamical Oceanography" (2008), uses "model" to denote many simplified approximations that allow analytical solutions for quasi-geostrophic motions, baroclinic instability, and many other cases. However, this simplified approach contrasts with the contemporary oceanographic understanding that a model (especially a numerical model) is a set of algorithms that follow hydrodynamic partial differential equations and parametrizations to calculate and predict time-dependent ocean variables across two or three spatial dimensions with maximum realism.

To minimize ambiguity in terminology, we reduced the use of "numerical model" and "numerical modeling" by reformulating the terminology in lines 10, 65, 298, 367, 378, 382, 387, 408, 422, 435, 436, 575, and 576.