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Continuous seiche in bays and harbors,

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Response to Reviewer #2.

We are grateful to the reviewer for their careful and critical review of the manuscript. Please note that the reviewers comments are listed below, with our responses indented under each comment.

Paper Structure I think the paper structure could be significantly improved. The paper lacks of a method section that help the reader to understand how the analysis will support the main conclusions of the manuscript. Section 3 is a very short section that could be included in Section 2. Also, the authors should consider to discuss/present the results in terms of the bay geometry similarity in order to extend the conclusions of this work to other locations and hence re-organized the material presented in Section 4. Also, the paper lacks of a discussion sections to put in perspective the present findings with respect to previous studies at the same locations. Finally, the conclusions presents a summary and discussion of the paper and hence should be re-written and significantly shortened.

Thank you for these suggestions to improve the presentation of the paper.

With respect to Section 3 Continuous modes, although it was quite short we feel that it is a primary and distinguishing topic of the paper, and would prefer for it to remain a distinct section. Further, based on the suggestions of Reviewer #1, this section has been clarified.

Section 4 is where we present the spectral analysis and subsequent identification of the shelf-modes, including section 4.2 which explicitly discusses the geometric/dynamic similarities between bays. Although it may be useful to discuss this at the end of section 4 after the individual bays have been analyzed, we feel that it is helpful to present this idea in the front matter, close to the introduction of figure 3 where the dynamic similarities are manifest in the spectra.

Regarding the addition of a methods section, if it were to appear at the front it would be a terse description of the methods used since it seems a bit unwieldy to present in detail mode identification based on the dispersion relation/spectral analysis, the introduction of metamodes via EMD of modal time series, and the kinematic analysis of modal energy. Since the EMD/metamode analysis might be considered an atypical analysis technique it would warrant significant description that may seem laborious without reference to application/results. Nonetheless, we agree that additional description of these overall techniques and their objectives in the front-matter will benefit the clarity of the presentation and have modified the introduction accordingly.

Finally, we have added a discussion section and rewritten the conclusion as suggested.

Resonance analysis
To my knowledge in order to identify resonance it is required the analysis of at least two signals obtained at different locations. These spatially separated signals should have: (i) a highly coherent variance in the water level, (ii) a phase relationship corresponding to a standing wave conditions (cross-spectra of the two signals) and (iii) an amplification of the water level at a given frequency. In order that resonance is demonstrated all of the above must be satisfied. Thus, I encourage the authors to present such analysis (i-iii) at those locations where data is available.

> Thank you for this interesting comment. We agree that a demonstrated amplification is the hallmark of resonance, and that appropriate phase and coherence will exist between the forcing and resonant waves, as well as between resonant waves themselves. We are not entirely clear if the reviewer is suggesting that the forcing (tidally-driven shelf-modes), or the bay/harbor modes themselves should be shown from the data to satisfy these conditions. We will try to address both.
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> We recall that since resonance is a property of the system, not the forcing, it is not required for the forcing to be at a resonant frequency. Initiation of resonance requires input energy displacing the system from equilibrium, i.e. a pendulum. This is a central argument of the paper: a tidally-driven shelf-resonance, which is not a bay/harbor resonance, provides energy (water level elevation) which drives and sustains the natural bay/harbor resonances.
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> Another hallmark of resonance is a highly tuned oscillation with a large Q-factor (signal-to-noise ratio) wherein the amplification is nearly singular in frequency. The shelf-resonances at Monterey and Hawke Bays are prime examples. In such cases it is difficult to conceive of geophysical processes that could produce such highly-tuned amplifications other than a structural resonance, and when one considers the close agreement between the physics and observations (now table 4), we suggest that a resonance is the most plausible explanation.
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> Regarding both the shelf and bay/harbor modes at Monterey, the resonance structure was known for some time based on observations inside the Bay alone without cross-spectral analysis (e.g. Wilson et al. 1965).
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> So another perspective would be that to 'identify resonance' requires observation of the resonant oscillation and appropriate physical system parameter matching, while attributing the forcing or internal wave structure of a resonance must satisfy the cross-spectral conditions noted by the reviewer.
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> As noted by Munk the 'harbor paradox' allows for long-waves to be effectively amplified at the tide gauge. Evidence of this is presented in Figure 3 spectra at Honolulu with comparison of the harbor (black) and reef (red) energy at the shelf-resonance period, exhibiting amplification inside the harbor. So there are at least two stages of amplification for this shelf-mode, the mode itself on the shelf, and its amplification inside the bay/harbor. In any case, this is distinct from the natural resonances of the bay/harbor that are excited by the amplified shelf-mode. We are therefore unsure of the utility of demonstrating a coherent phase relationship outside/inside the harbor for the shelf-modes.
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> The other possibility is demonstration of phase coherence of one of bay/harbor resonances itself across varying spatial locations in a bay/harbor. As noted by the reviewer this requires multiple

sensors within a bay/harbor, which unfortunately is not included in our data.

However, we can analyze the shelf-mode amplification and show that it is indeed a coherent wave between offshore and the tide gauge. A brief analysis as suggested by the reviewer follows below. As noted above, we are unsure if this adds significantly to the results other than as a verification of the harbor paradox since the shelf-mode is not a bay/harbor resonance. If it is felt that this is a constructive addition to the manuscript, we will be glad to add it.

At Honolulu the coherence and phase between the offshore (outside reef) and harbor gauges at the 27.1 minute shelf-mode are:
Phase : 0.1972 rad = 11.3 degrees
Coherence : 0.81, 0.84, 0.88 (lower 95%, Estimate, upper 95%)

Assuming a shallow water wave speed of sqrt(9.81*150) = 38.36 m/s, and noting that the cross-shore distance between the harbour and reef gauges is 1530 m, the propagation delay would be 1530 m / 38.36 m/s = 39.89 s. Temporally, the fraction of a resonance cycle of this propagation delay is 39.89 s / (27.1 * 60) s = 0.025. From the cross-spectral phase estimate the fraction of a cycle phase-delay is 11.3 deg / 360 deg = 0.031. Since there will be a reduction of the wave phase-speed as the wave shoals and encounters friction into shallower waters it is expected that the actual phase delay will be longer than that estimated from linear wave theory (38.89 s). This is consistent with the slight increase of phase-delay observed (cross-spectral data), and even without it, the first-order estimate (2.5%) is not widely different from the observed estimate (3.5%).

Reference

Wilson, B. W., Hendrickson, J. A., and Kilmer, R. E.: Feasibility study for a surge-action model of Monterey Harbor, California, Tech. Rep. 2–136, United States Army Corps of Engineers, Waterways Experiment Station, Vicksburg, MS, 1965.