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Ocean Science Discussions

Interactive comment on "Predictability of Non-Phase-Locked Baroclinic Tides in the Caribbean Sea" *by* Edward D. Zaron et al.

Anonymous Referee #2

Received and published: 3 July 2019

This study examined the predictability of non-phase-locked internal tides in Caribbean sea using AMSES's model forecast. It is identified that the forecast error of the non-phase-locked tides is correlated with the forecast error of the sub-tidal (mesoscale) SLA. The forecast errors are spatially and temporally varying related to dynamical origin of the non-phase-locked tides.

The study is timely for the upcoming SWOT mission, to which the non-phase-locked tides and internal gravity waves can be a challenge in further scientific application. Understanding and predicting non-phase-locked tides can be valuable in utilizing the small-scale (15-150km) SSH signals.

The paper is well written and the analyses are well presented. I recommend publishing after a minor revision.

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Main comments:

1. The non-phased-locked tides are defined as (page 8 line 5) the residual of the detided steric height minus its daily average. It contains both non-phased-locked tides and the supertidal internal gravity continuum. It may well be true that the non-phase-locked tides dominate the signal, but a frequency spectrum and variance quantification will be very useful for a proper justification, especially for the title, which can otherwise be misleading.

2. The model implementation of the tidal simulation needs elaboration.

Page 3 Line 10 reads: "Tides are not provided by the global model presently used for boundary conditions; instead, tides predicted with the OTIS barotropic tide model (Egbert and Erofeeva, 2002) are added to the barotropic currents and sea surface height data at open boundaries. In addition, the tide-generating force is applied within the model domain, and incorporates the effects of ocean loading and self-attraction consistent with the OTIS model.". But Page 6 line 8 reads "...have been subjected to least-squares harmonic analysis to identify the phase-locked tides at the M2, S2, K2, N2, 2N2, K1, O1, P1, and Q1 frequencies (provided through tidal open boundary conditions)". I may have missed something, but it could be helpful if more details were provided.

3. The phase-locked tides in the forecast need more validation.

3.1 Section 3 is dedicated to this evaluation, but needs more quantitative analyses and illustration. For example, the quantitative analyses in Appendix A are important and can be visualized in a map as a panel in figure 3.

3.2 As stated in the paper, the tide gauge cannot distinguish barotropic and baroclinic components, the mismatch between model forecast and tide gauge then cannot confirm the performance of the model forecast on phase-locked baroclinic tides.

3.3 The comparison in Figure 2 can be improved. It is said (Page 4 line 12) that

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"The small offset between the observed and modeled spectra may be explained by the different time periods used for computing the spectra, which is the 2010–2012 period for AMSEAS and the 2002–2009 period for Jason-1." Why not compare the simultaneous forecast and the altimetry observations?

3.4 Another related question is "If large discrepancies exist in stationary tides between forecast and real ocean, will these discrepancies be contribute to errors in non-phase-locked tides?"

Minor comments:

Gaultier et al., 2016 may not be an ideal reference for the topic of "distinguishing balanced motion from inertia-gravity waves in sea surface topography data". Their regional ROMS model does not have tides.

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