Review of: "The accuracy of barotropic and baroclinic tides in global non-assimilative ocean general circulation models" by Ansong et al., *egusphere-2026-5064*

Review by: Brian Dushaw
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(1)
The paper's aims are to compare and test the barotropic- and baroclinic-tide variations computed from four high-resolution ocean general circulation models, unconstrained by data, to a "gold standard" barotropic tidal model, TPXO8, and globally-averaged Root-Mean-Square-Amplitude of baroclinic tide sea-surface heights derived from the 17-year record of satellite altimeter data. The discussion includes interesting and technical details of model corrections, adjustments, and parameter tuning to refine the models and obtain optimal model comparisons to data.

In the spirit of tidal model comparisons of Stammer et al. (2014) (comparisons of global barotropic models in elevation and currents) and Carrère et al. (2021) (comparisons of global mode-1 baroclinic tides derived from altimeter data), the comparisons are useful and informative, conveying the present state-of-the-art of the latest high-resolution global general circulation models in accurately representing tidal variability. While the tides are interesting in their own right, they are also a relentless driving mechanism for a multitude of other ocean processes, on the one hand, and nuisance noise for some data types, that perhaps can be reduced by such models, on the other.

The paper is therefore a useful, valuable contribution. That said, I have a number of criticisms and comments on the manuscript. I perceive these as motivating not exactly major revisions to the content, but adjustments to the framing of the discussions. I am not very "model literate", so most of my concerns are the discussions of internal tides.

It would be nice if the paper concluded with a paragraph that hints at plans or developments to come next. Perhaps some of my concerns can be raised in such a paragraph.

Scientific Significance: 1
Scientific Quality: 3
Presentation Quality: 2

(2)
An essential aspect of the paper is the division of the internal-tide variability (mode-1 mostly) into coherent and incoherent parts. This is an artificial and unhelpful construct - it suggests there are two separate processes involved, but that is not the case. The internal tide is a narrow-band process, and over vast regions of the oceans, the width of that band is narrow indeed. The paper gives an undue emphasis on the "incoherent part", while providing no quantification of it.

The paper assumes, ipse dixit, that there is a coherent part. The existence of coherence is hardly to be taken for granted. The paper Dushaw et al. (2011) was the first to demonstrate the remarkable coherence across ocean basins. Described by a reviewer as a landmark paper, the paper had a determined resistant reception. Sparing the details of events, I note that some of the authors of the Stammer et al. (2014) review paper still would not accept its conclusions. Acceptance and acknowledgment of the contributions of this paper are long overdue.

Over the Northeast Pacific Basin, the internal-tide coherence can be illustrated by the M2 internal tide mode-1 amplitude there (Dushaw 2015), shown in Figure 1 below. Mode-1 internal amplitude is the preferred variable, rather than SSH, since that amplitude is more directly indicative of the energy. (The SSH expression of these waves diminishes significantly toward Alaska in response to the changing stratification.) In these solutions, which closely fit the altimetry data, beautiful coherent wave trains traveling north from Hawaii and south from the Aleutians are evident. The figure comprises several lessons:

(1) The solution for the M2 internal tide field has been separated into northward and southward wavenumbers. The field across the North Pacific is obviously an interference pattern of these wave trains, extending across the ocean basin. This means the integrity of the phase *differences* between the wavetrains has been preserved over the data record length. The interference pattern is one of the most unstable phenomena in nature. Yet the phase patterns across this extensive interference pattern, derived from 7 years of altimeter data (2000-2007), have remained stable.

(2) Taking the northward wavetrain as an example, the wave pattern is undisturbed from Hawaii into the Gulf of Alaska, indicating no obvious spatial incoherence or deterioration with increasing range. With phase speed of 3.5 m/s, the M2 internal tide crosses the distance between Hawaii and the Aleutians in 10 days, or 20 tidal cycles.

(3) Taking into account reductions in amplitude from cylindrical spreading, here is little obvious evidence of any attenuation at all. Any incoherence would also cause a loss of signal and a reduction of amplitude in the solution; not evident.

(4) For those that still insist there is an unseen incoherent part, I note that the tidal amplitudes of these solutions match those of independent estimates along tracks of acoustic tomography data, data that are unfiltered (e.g., Figure 3).

A similar wavetrain is evident in the North Atlantic - a cylindrical wavetrain that propagates from the Azores to the coast of Brazil - extracted from a complicated soup of an interference pattern.

The evidence for the extraordinary coherence of these waves, over most of the ocean, is myriad. Figure 2 shows the energy density of the northward propagating Pacific wavefield. A complicated, stable interference pattern, but no obvious indication of wave attenuation.

In short, the paper would be better off discussing these waves as a narrow-band phenomenon, as interference patterns, and highlighting the opportunities offered by the extraordinary coherence of the waves over most of the ocean basins. Models should be aiming to capture these features in detail.

I call the authors' attention to the publications Dushaw and Worcester (1998) and Dushaw and Menemenlis (2023) concerning "Resonant Diurnal Internal Tides in the North Atlantic". In addition to being interesting, the papers highlight the comparison of models to tomography data for model testing. Below is a list of available data - unfiltered time series of mode-1 amplitude - derived from the tomography data of several experiments. Inasmuch as your analysis involves the extraction of model SSH along altimeter tracks, with a similar extraction of model SSH along tomography tracks, tests of models against the high-quality, in situ tomography timeseries will be straightforward.

The resonant nature of the diurnal tides is a direct window into mode-1 internal tide attenuation. The amplitude of the resonanting waves between Puerto Rico and Bermuda depends critically on attenuation. I noted your paper extensively discusses this question of internal tide attenuation as implemented in the models.

The IT harmonic constants from the altimetry data are derived from a 17-year record length. When computing estimates for Dushaw et al. (2011) or Dushaw (2015), I found that constants derived from a 7-year record were better than constants derived from a 10-year record, by comparison to the in situ tomography data (I chose the record length centered on the tandem-track data.). Using the full record

seems guaranteed to underestimate internal-tide amplitude, particularly when comparing to model record lengths of O(1-yr.). Unaddressed is the question of possible secular drifts to the internal-tide amplitudes or phases in the 17-year record as a consequence of climate change. I would suggest dividing the along track SSH record lengths into sections of 5-7 year duration, for a more accurate test. While also giving 3 independent realizations to compare to.

The model tests for of the internal tides were obtained by a general comparison of the globally-averaged Root-Mean-Square-Amplitude baroclinic sea-surface height, model values compared to 17-year record length values along the altimeter tracks, rather than a point-by-point comparison of amplitude and phase. This "bulk" test is not a refined one; see Dushaw et al. (2011; Figure 3 below), Dushaw (2015), Dushaw and Menemenlis (2023). The models should be aiming to reproduce the mode-1 internal-tide amplitudes and phases derived from available data, including the tomography data. Is that not the goal, with SWOT in mind? The high-quality, in situ, unfiltered tomography timeseries are a real asset in that regard.

(3)
In a number of places, the paper convolves discussions of internal waves in general, with mode-1 internal tides, which comprise the focus of the paper, in ways that I found confusing. Perhaps the text could be revised to better isolate the discussions of internal waves generally, from the mode-1 internal tides. For example, in the first paragraph: *Internal tides may break in the vicinity of topographic features if they have high vertical modes, which are more susceptible to breaking because of their smaller vertical scales. Low-mode internal tides tend to propagate for thousands of kilometers to locations where they dissipate their energy. As internal waves propagate through the ocean, they experience large spatial and temporal variabilities in their energy flux ...* It seems to me that mode-1 internal tides also break against topography (there is no evidence of reflections of waves back out of the Gulf of Alaska, for example). I see that the last part is about internal waves generally, except that the citations given pertain to internal tides, as I recall. Low-mode internal tides do not generally experience "large spatial and temporal variabilities". Better might be: *"Generally, as internal waves propagate through the ocean, they experience large spatial and temporal variabilities in their energy flux... On the other hand, low-mode internal tides tend to propagate for thousands of kilometers to locations where they dissipate their energy. Internal tides may break in the vicinity of topographic features, with higher-vertical modes more susceptible to breaking because of their smaller vertical scales."*
It is important to make clear the distinction between "internal waves" generally, "internal tides" generally, and mode-1 internal tides, specifically. The paper's primary content is the latter.

I doubt that mode-1 internal tides are affected by any of the smaller-scale internal-wave processes alluded to in the paper. There is not much evidence of such effects that I am aware of. Could the paper make clear what are speculations, assumptions, or ambiguities? Much is made of damping, drag, attenuation, etc. - my impression is most of this is unknown, perhaps patching up of models by attenuation to make up for deficiencies elsewhere in the model. There seem to be implications that when a model settles on a particular internal-tide drag parameterization to get it to agree with observations, that that is the actual physical drag experienced by ocean internal tides. It doesn't seem to me that the models are at that level of general accuracy yet. Much was once made of the phenomenon of "Parametric Subharmonic Instability (PSI)" and how it might cause a catastrophic damping of mode-1 internal tides at 28.9N. But there is no evidence from altimeter SSH of any dramatic change in mode-1 amplitude at that latitude (Dushaw et al. (2011)).

There is no mention of testing model barotropic tidal currents (see Dushaw and Menemenlis (2023)), which are the driving mechanism for generating ITs. Perhaps just a citation to the Stammer tidal paper on this point is sufficient. To get internal tides correct, any model must first have accurate barotropic tidal currents.

I am not sure that Figure 4 is all that useful. The panels are too small to see much. Could a smaller region be used - for example the North Atlantic?

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Dushaw, B. D., 2022. Surprises in physical oceanography: Contributions from ocean acoustic tomography, Tellus A: Dynamic Meteorology and Oceanography, 74, 33−67. https://doi.org/10.16993/tellusa.39

Dushaw, B. D., and D. Menemenlis, 2023. Resonant diurnal internal tides in the North Atlantic: 2. Modeling. Geophysical Research Letters, 50, e2022GL101193. https://doi.org/10.1029/2022GL101193

Dushaw, B. D., 2024. Ocean acoustic tomography, physical oceanographers, and scientific disagreement: An essay. Zenodo. https://doi.org/10.5281/zenodo.11544629

Available Data Sets

N.B. The internal-tide time series available here, and as published in the papers above, have not been temporally filtered, other than a basic high-pass filter to separate internal waves from mesoscale variability. The time series are as measured; the clear tidal signal just dominates.

Dushaw, B. D. (2015). An Empirical Global Model for Mode-1 Internal Tides (1.1). Zenodo. https://doi.org/10.5281/zenodo.18726454 (animated gifs, thermistor data, global solutions for internal-tide harmonic constants, global mode properties computed from the WOA, etc.)

Dushaw, B. D. (2022). Acoustic Mid-Ocean Dynamics Experiment (AMODE), 1991, North Atlantic: High-frequency Baroclinic Mode-1 Amplitudes (1.0) [Data set]. Zenodo. https://doi.org/10.5281/zenodo.6662719

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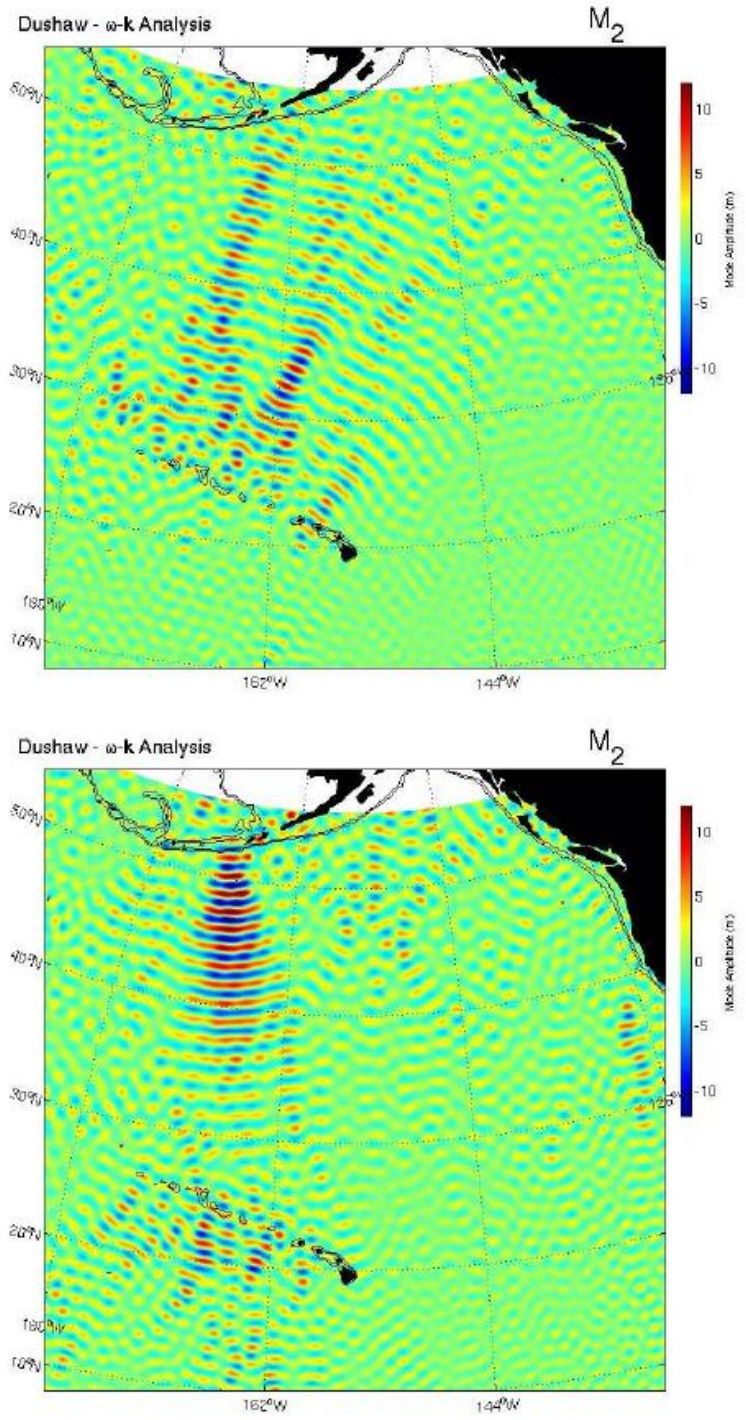


Figure 57. Amplitude of the estimated mode-1, M_2 internal tides of the North Pacific. Top panel: Northward propagating wavenumbers. Bottom panel: Southward propagating wavenumbers. The estimated interference patterns can be decomposed into their underlying wavenumbers, revealing regular wave trains that extend across the Pacific basin. "Beams" extending across the basin are evident, which are a product of extraordinary coherence.

Figure 1: From Dushaw (2015), Figure 57

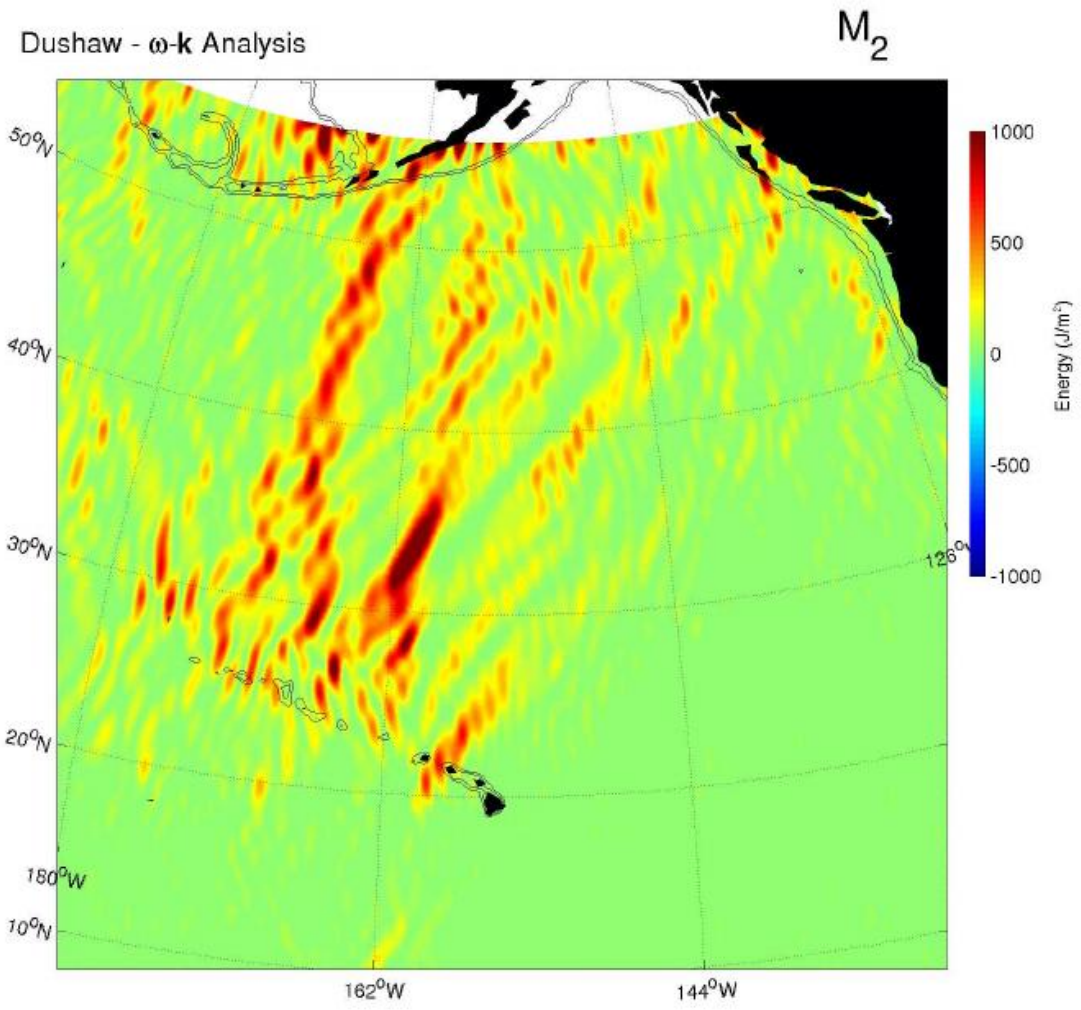


Figure 58. Energy density of the estimated mode-1, M_2 internal tides of the North Pacific. Only northward propagating wavenumbers were used for this computation, hence this energy originates primarily from the Hawaiian Ridge. Allowing for the effects of cylindrical spreading of the wavetrain, little decrease in energy density is evident, indicating little loss of coherence of the northward propagating wavetrain.

Figure 2: From Dushaw (2015), Figure 58

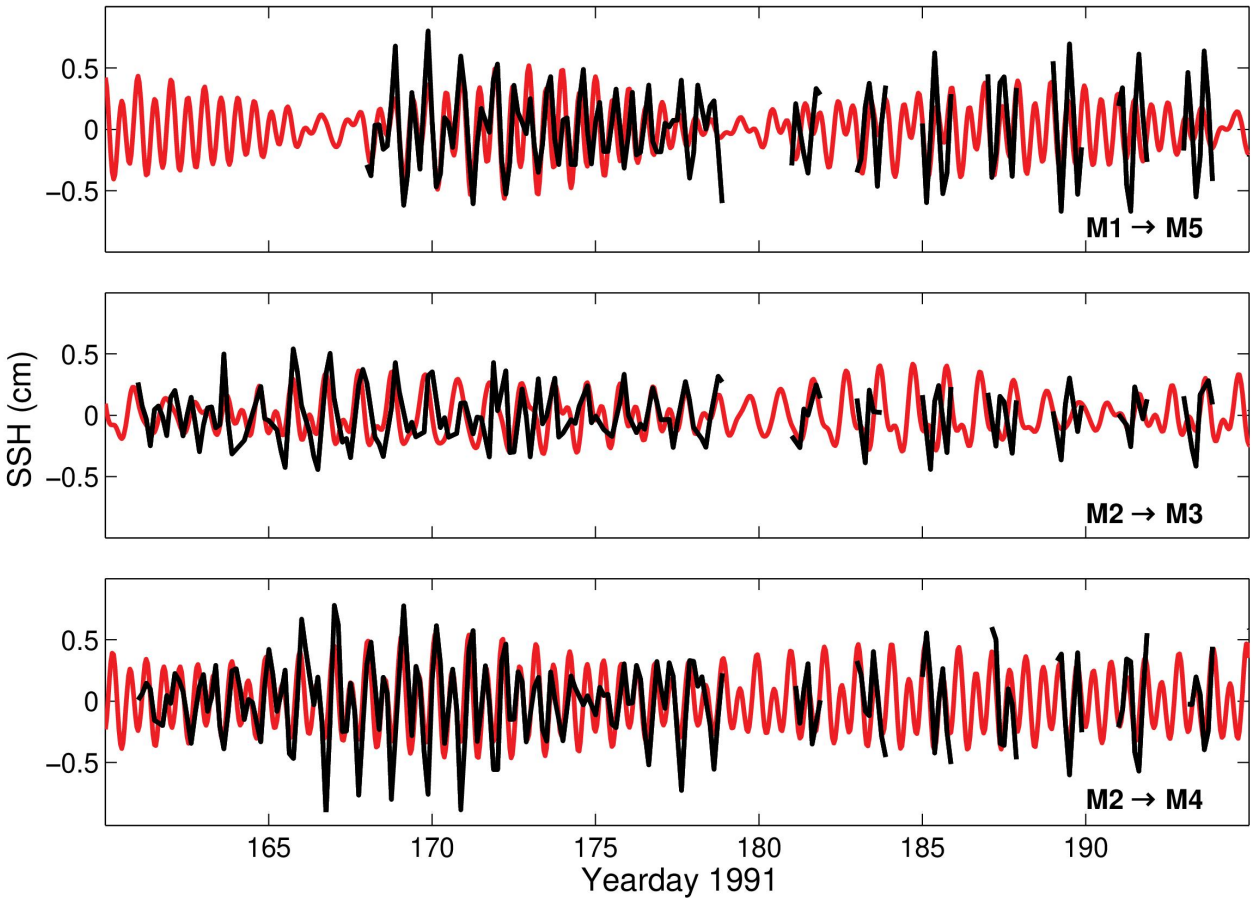


Figure 3: Comparison of time series of SSH inferred from travel time data from six of the 15 paths of the 1991 AMODE tomography pentagonal array (black lines) to the prediction from the mapping tidal analysis results (red lines). Only 35 days of the O(200 day) record lengths are shown. Altimeter data from 2000 to 2007 were used for the SSH predictions, 9-16 years after the experiment. (from Dushaw et al. (2011))