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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 #3

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An interesting and useful study, showing variability in the sea level associated with phase-locked and non-phase-locked internal tides in the Caribbean Sea. In addition to providing nice results on the energetics of these waves in the model and satellite altimetry data, the paper describes methodology that can used for other regions and models. Presentation is very clear.

Specific remarks:

p. 16, line 16: "The findings of this study indicate that a substantial fraction of non-phase-locked tidal sea level variability may be predictable with an ocean forecast system"... Using self-verifying analyses, this is an overstatement. I guess I am picking on the use of word "predictable" here. As the author point out themselves, the forecast is verified against the nowcast, not independent data. The data assimilated in the now-

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cast do not constrain the internal tide signal directly, and only potentially improve the background conditions for the internal tide propagation. We do not know if they do. All we can say from this analysis is that the internal wave propagation is sensitive to the changes in the background conditions implied by data assimilation. It is not shown (by comparison to the independent data) that nowcasts have better representation of the internal tide field.

The rationale for the use of the steric height is explained on p. 8, line 6. Can this note be moved to a place earlier in the text, before the first result using the steric height anomaly is discussed?

Can the author present details of how the steric height anomaly is computed? Steric height is discussed in textbooks as associated with thermal expansion of the water column, right? And NCOM is probably a Boussinesq model, conserving volume (the model water column does not expand due to the heating). Is there contradiction here? If the answer is common place, please ignore this remark. Otherwise, I would appreciate a short note in the text.

How is the phase-locked baroclinic tide separated from the barotropic tide along the track? The answer is probably in the earlier papers on the subject. Can the author provide a comment in this paper? Or point to a reference?

It is surprising to see the spatial structure of the non-phase-locked tide (Fig 6c) and its error (6d) look like radiating waves, similar to the phase-locked tide. If the eddies scatter the internal tide wave, resulting in the non-phase-locked tide, can one expect a more irregular pattern? In connection to this, what is the main difference in the subsurface background stratification of the T+0 and T+3.5 forecasts? Is it dominated by the eddy composition or more by the basin scale change in the vertical stratification (the depth and the strength of the thermocline)? As a suggestion, can the author show the detided, daily averaged zonal vertical sections of T and S (and/or density) in the top 500 m or so, at 14N say, T+3.5 and T+0 solutions, and their difference, for the same

date as Fig 6 and 7?

Fig 8: Can you discuss the possible reasons for the maximum rms amplitude of the non-phase-locked tide over Aves Escarpment? Can “InternalâĂŘtide generation and destruction by shoaling internal tides” (Kelly and Nash 2010) be a possible mechanism?

Figure 11: the horizontal scale of the waves in the packets seems to be finer than the AMSEAS resolution. Again, the statement that “waves are predictable by the AMSEAS system” (in conclusions) is not supported by the analysis (e.g., of wave speed, dispersion properties, vertical structure, term balances, etc.).

Minor remarks:

Figure 3. Include the color bar.

p. 5, line 1: typo... “snapshot”

p. 9, line 27: typo ... “magnitude”

Figure 7: add the date in the caption.

p. 13, line 7: check the use of word “both”... seems to be out of place

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