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Ocean Science Discussions

Interactive comment on "A vertical-mode decomposition to investigate low-frequency internal motion across the Atlantic at 26° N" *by* Z. B. Szuts et al.

Anonymous Referee #1

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The vertical structure of ocean circulation is an important problem in oceanography, and, when combined properly with the available satellite altimetry data, can be used to infer depth-integrated oceanic properties, such as energy fluxes and transport variability. The underlying assumption, supported by past observations, is that the altimetry data reflect mostly the first baroclinic mode in the open ocean. In this paper, the authors revisited this problem by decomposing the RAPID/MOCHA hydrographic mooring data into vertical modes. They found that the vertical structure away from boundaries is almost entirely described by the first baroclinic mode, while the first baroclinic mode is

less dominant close to the boundaries, especially the eastern boundary. Even though these conclusions are not entirely new, I find this study is a timely reminder that the altimeter data does not reflect the first baroclinic mode everywhere. The authors did a good job in describing in details the mode fitting procedure adopted in this paper, which makes it easy for the readers to follow.

My main concerns are:

(1) The first baroclinic mode is indeed less dominant at WB2. However, at WB3, which is only 50 km east of Bahamas (Fig. 1a), the first baroclinic mode seems to, again, explain most of the variability (Fig. 7). Does this mean the assumption that the first baroclinic mode dominates the sea surface variability break down only when it is VERY close to the western boundary, at least, at this latitude? Instead of saying "near the boundaries" or "at WB2 and WB3", it will be more informative to estimate how far from the boundaries the assumption that the sea surface variability reflects the first baroclinic mode is not acceptable any more.

(2) The amplitude or energy of each mode varies with the number of modes and fitting methods chosen. Some of the choices seem a bit arbitrary. How robust are they? especially given WB2 is located on a very sharp slope. Fig. 6b seems to suggest that the contribution of the first baroclinic mode increases significantly at most moorings when the bottom slope is roughly taken into account.

(3) It is important to emphasize that only data at about 6 moorings at a single latitude were used for vertical mode decomposition. Therefore, the results of their vertical mode analysis are not readily to be generalized to other latitudes or other basins.

(4) The authors found the variability has long periods in the center of the basin, but short periods at the boundaries. No explanations were given. There are dynamical reasons to expect such difference between the side boundaries and ocean interior.

(5) No instruments at depth shallower than 140-200m were used for mode fitting. Imag-

ine there are good data to use at these shallower depths so that the fitting is much more constrained by data at these depths, will the agreement with the altimeter data be better?

Other comments:

1) The EBH synthetic profile is created by vertically concatenating the series of short moorings on the African continental slope. This sounds very crude. Have the authors done any test whether this works? even maybe in a high-resolution regional ocean model?

2) It is not clear to me how the depth-leveling approach works. On page 2054, it says "at the depths of sensors 1 and 2 the interpolated values are unchanged from those directly measured". Does this mean at the sensor depths the directly measured values are used with no corrections? A schematic will be useful here.

3) The zonal average of SSH across the Atlantic is subtracted to remove seasonal heating and cooling effects. Why is it important to remove the seasonal cycle of SSH? After all, no seasonal cycle is removed from the mooring data. If the seasonal cycle indeed needs to be removed, why not removing it by fitting a seasonal cycle at each SSH grid point?

4) On page 2068, the inversion to data is limited below 140 m. The signal in the top 140m must also project onto the first few baroclinic modes. Including good data in the top 200 m may change your relative mode energy.

5) Page 2068, the last paragraph. "The averaged error of the original signal". Do you mean "standard deviation"?

6) As the authors pointed out, it is problematic to directly compare data on a mooring with altimetry data, since the data processing procedures involved are so different. Satellite altimetry data near the coast have large errors due to the large errors in the standard tidal and atmospheric corrections there. This may contribute to the lack of

agreement at WB2 and EBH.

7) Page 2074, line 22. "The BC1 reconstructions accurately recover the....." It seems to me that the BC1 reconstructions underestimate the standard deviation of the original signal everywhere in Fig. 8b.

8) Page 2084, line 17. "Although these findings are a clear explanation for why mid-ocean waves or eddies do not strongly influence the methodology of using end-point density profiles to obtain basin-wide geostrophic transport...." I don' think a clear physical explanation is offered in this paper.

Interactive comment on Ocean Sci. Discuss., 8, 2047, 2011.