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Ocean Science Discussions

Interactive comment on "The global distribution of the M1 ocean tide" *by* Philip L. Woodworth

Anonymous Referee #1

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The small degree-3 M1 ocean tide has been previously studied by analyzing data from regional (mostly North Atlantic) tide gauge networks and by synthesis of normal modes in the world ocean. The present work extends the tidal analysis of M1 and the nearby degree-2 M1' tide to the global ocean based on over 800 sea level records and a special parameterization of tidal constants and nodal factors in the frame of a least-squares fit. The distribution of M1 (and M1') in the various ocean basins is characterized using scatter plots of tidal constants and a numerically derived M1 solution.

This is a neat paper and indeed the first time that the ocean response to the degree-3 component of the tidal potential has been examined globally. The study is also a timely scientific contribution in light of the future SWOT (Surface Water and Ocean Topography) wide-swath mission, which will map shelf and coastal tides at high spatial resolution and with a target error of 1 cm. Having exactly this magnitude in several places, M1 will no longer be considered "esoteric"; instead, many groups will consult

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Discussion paper

the present paper for further detail on degree-3 diurnal tides and the cluster of degree-2 terms in the same spectral band (M1'). Although the numerical tidal modeling in the last part of the paper could be more sophisticated, it suffices the needs here. I have only a few minor corrections that should be addressed during the revision process:

– I found the second part of the Introduction (after the author sets out the objective) a bit jumpy and incoherent. References to the Cartwright and Platzman papers alongside re-prints of their figures are used to elucidate the main characteristics of the M1 tide. This section could do with some streamlining, e.g., by gluing the individual paragraphs together. Also, lines 13-14 describe symmetry properties of the degree-2 and degree-3 diurnal tidal potential, which are nicely illustrated in Fig. 2 of Ray (2001). It would make sense to aid a reader's imagination and directly refer him/her to Ray's figure.

– The M1' tide is introduced without any additional explanation in the abstract.

– The section on the prior tuning of the numerical model for leading degree-2 constituents (M2, K1) lacks some quantitative measures. Which values of horizontal eddy viscosity and bottom drag coefficient were used? Does the model run with a quadratic term for bottom friction or a simple linear parameterization? Instead of stating that the obtained maps for M2 and K1 are acceptable, include a comparison to data-constrained solutions in terms of RMS values and explained variances.

– Something that can be added to the comparison of the model-based M1 chart to the tide gauge estimates: similar to the author's solution for K1 (SI Fig. 1c), the model appears to overestimate amplitudes, particularly in the Southern Ocean/Indian Ocean. I have the strong suspicion that this results from treating Antarctic ice shelves as fully grounded and excluded sub-shelf cavities from the model domain. Check out the paper by Wilmes and Green (2014):

Wilmes, S.-B., and Green, J.A.M. (2014), The evolution of tides and tidal dissipation over the last 21,000 years, JGR Oceans, doi:10.1002/2013JC009605.

– page 5, line 2: "the tidal software" – is this the NOC software introduced in Section 2? The first paragraphs of Section 3 were not specific in this regard.

– The tidal potential used for forcing the tidal model is specified on line 22, page 10. I wonder if a bit more information on the amplitude is required at this place. Is this the amplitude of the equilibrium tidal potential listed in Table 1 of Ray (2001)? If yes, there might a slight mismatch in numbers (1.27 mm in Ray's paper vs. 1.2 mm here). The text also mentions a factor of 0.80 to account for the effect of elastic body tides (see below), but it is not clear whether or not the quoted formula includes this correction.

– Getting picky, yes, but using "Doodson number" in the second column of Table 2 is not fully correct. What is shown are actually the integer multipliers for the 6 Doodson variables that define the argument of the tidal term.

– Wahr (1981) is cited as Wahr (1991) in the both the main text and the supplement. I also recommend a slight re-formulation of the last sentence at the bottom of page 3: "... a special value of 0.74 for K1 to allow for resonant perturbations of body tide Love numbers close to the diurnal eigenfrequency of the Earth's fluid core."

– Finally, it always brings a smile on my face when a single author of a paper uses "we" in the active voice (such as in the abstract here). There can be different takes on it, but I to wonder with whom did he/she write the paper ...

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