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Ocean Science
Discussions

Interactive comment on "A new assimilation tidal model for the Mediterranean Sea" *by* D. N. Arabelos et al.

Anonymous Referee #1

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The paper describes the ocean tides in the Med sea from a hydrodynamic modelling assimilating sateliite altimetry. The authors then carry out validation with independent satellite data.

The paper is generally scientifically sound but it lacks substance. There is also several points that needs clearification and inclusion.

The first point is the Marmerais Sea and the Black Sea. Why are these ignored. In my opinion they are a part of the Med Sea similar to the Adriatic Sea and even though the tides are small in these seas they should be included.

Secondly the authors use representer point for the model outside the Med Sea in the Atlantic Ocean. This means that their modes goes outside the Med Sea ???. This is

not clearly defined in the manuscript.

The Second part deals with the method and it seems to be very identical to the word by G. Egbert from his 1994 manuscript. I urge the authors to reference this work instead of (useless) repetition. The listing of parameters in section 5 is much more interesting with respect to setting up the model. Why are these parameters given without any information and evaluation and explanation? This concerns the fine-tuning of the model and these should be considered with respect to the Med Sea. Similarly how are bottom friction modeled and what does linear versus quadratic friction coefficients do to the model. The section needs much more substance.

The omission and removal of gauges close to the Strait of Gibraltar is very problematic. On which ground are good versus bad gauges selected. If the phase changes randomly between these there is a good change that they are in error. Otherwise the model is most likely in error. Have this been investigated prior to rejection with i.e. comparison to a global model. To me this more seems like an error of the hydrodynamic model and the parametrisation of the friction in the region combined with a moderately bad bathymetry model like ETOPO2. Not really state of the art bathymetry.

All in all I am not convinced that the model really gives us more information about the tides in the Med (excluding the Black Sea) than we knew before. Especially as the subsequent comparison with tide gauges is very loose. I would like to se vector differences for the model (both constituent-vise but also combined) and these numbers compared with other global model to show that the MED10 model is actually an improvement. Similar to the table of 102 gauges that are always used to evaluate tide models (now this must be done for a limited number of gauges in the MED.

The question might be with which gauges the comparison should be made. I would suggest that the authors select maybe 10 gauges that are NOT used for the assimilation as there are more than enough tide gauges for running the model. Furthermore, the evaluation with JASON is not really an independent validation as the representers

are placed in the crossovers used to compare with and that the comparison has to be done with the exact same number of constituent in order to be valid.

Interactive comment on Ocean Sci. Discuss., 7, 1703, 2010.