**Ocean Science
Discussions**

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

Anonymous Referee #2

This manuscript presents a new estimate of tidal elevations and currents in the Mediterranean Sea using the methodology of Egbert and Erofeeva. The authors have collected and harmonically analyzed a set of 59 tide gauge stations, most of which are assimilated into the tidal model. Additional Pathfinder data (T/P) are also assimilated. Tidal elevations are verified against Jason-1 crossover data, and a set of 20 coastal gauges from Tsimplis.

Over-all comments: The authors appear to have used correctly the OTIS software to assimilate the tide gauge and altimeter data and estimate the tidal fields in the Mediterranean. It is unclear what guided the choice of inversion parameters and bathymetry, and the comparison with independent data is not sufficiently justified to claim the that the present solutions are an improvement over the state of the art.

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Some examples:

The tidal model was linearized around a state of rest, and a 2m/s friction velocity was used. Why? Is the prior (non-assimilating) solution sensitive to this value? I would assume so, since the Mediterranean has extensive shallow areas.

The model forcing error correlation scale of 37km was used. Once again, why? What is believed to be the important source of model error which is correlated on this scale? Or was the value chosen to be comparable to the spacing of the data sites? How sensitive is the solution to this parameter?

What is the significance of deviations from the Tsimplis gauges? Many of these are located along the southern boundary, and I wonder if they might be more useful assimilated in the model. Other gauges could be withheld for validation.

How can we asses the goodness of fit to the assimilated data? Can you tell if they have been over-fit? Given the ad-hoc nature of the model forcing error parameterization and data error estimates in OTIS, it seems especially important to asses the goodness of fit. And the comparison with validation data needs improvement. Are the differences between the models in Table 7 significant? How can you tell?

Specific suggestions and comments: While this paper is a good start on a revision of Mediterranean tides, I think it could be revised into a more useful and important manuscript.

Figure 1: It would be helpful to put place names on this map of the bathymetry. I was not familiar with a couple of locations mentioned in the text (e.g., Sfax).

"reduced basis representer" (after eqn 3) – I believe the correct citation is the Egbert, Bennett, Foreman paper. I don't think Bennett 1992 mentions the reduced basis part. Also, equation (7) is incorrect for the reduced basis, but it is correct for the full basis.

I would urge you to take a look at revising the drag formulation in OTIS. See the mk-Speed routine, which I think is still distributed with OTIS. Other approaches to lineariza-

tion are also possible. Consider tuning these with a non-assimilative model.

Eqn (16) and Table 2. If correlations are shown in the table, why are not the diagonal elements equal to 1? Does the table present covariances, instead?

Also, I think the data set you prepared (59 gauges) probably ought to be described in its own technical report. Why not take a careful look at the stations near Gibralter and determine if they are spurious? If not, then at least one should be used in the assimilation. [Also, the description of inference for P1 and K2 was vague. Did you use the equilibrium amplitude ratios and phase differences?]

In table 3 could you please list the geographic coordinates of each station so that others can use your data set?

I would suggest that you use some of the Tsimplis data in the assimilation, to get more even data distribution around the coastline. I think your general approach to withholding data is wise.

At line 24 on page 13, I did not at all understand why this approach to comparing the models was taken. Given the spatial inhomogeneity of the signal, and likely errors, it would seem much better to focus on specific regions for intercomparison.

I did not find the plots of transport illuminating (Figures 8-11). Would you consider plotting these on a log-scale? Or maybe show current ellipses? You might consider plotting energy flux instead, and relating this to the dynamics (i.e., standing vs propagating vs strongly-damped waves).

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