

## ***Interactive comment on “Operational coastal ocean forecasting in the Eastern Mediterranean: implementation and evaluation” by G. Zodiatis et al.***

### **Anonymous Referee #2**

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\_ The CYCOM forecast system has been presented with details on the forecast methodology and the nesting strategy in a series of models (large scale-intermediate scale-coastal scale). A good attempt has been made to validate the model results by employing satellite and in-situ data. The CYCOM forecast results are quite satisfactory, especially in the absence of data assimilation. This is work that is worthy of publication. The improvements that have been planned for future simulations will probably take care of several of the current weaknesses.

\_ In view of the above, there is no need to exaggerate certain aspects of the agreement between model forecasts and observations. The satellite derived SST consistently

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show very different temperature in the west of the CYCOM domain. Although the authors admit that the assimilation in the basin-wide model did not manage to take care of this, they repeatedly claim a very good overall agreement in the SST (p. 408); this should be toned down. The effort to use the XBT section (Fig. 8) to possibly find cooler surface waters (p. 408) as in the model, is irrelevant, since the XBT section is not west enough to capture the area where the largest discrepancies between model and AVHRR derived SST's are seen.

\_ Although the paper focus is on the numerics, it is important to extend the discussion on the dynamics, by explaining why the differences between the models and between data and models occur. For instance, why are the coastal areas more prone to differences? The minimum depth is perhaps the same (the ALERMO min. depth is mentioned = 25m, but the CYCOM is not mentioned), does the higher CYCOM resolution affect coastal topography? Do the atmospheric fluxes have a different effect in the two models? Are some coastal dynamics present in CYCOM but not in ALERMO?

\_ The paper puts a rather too strong focus on checking CYCOM fields against the ALERMO fields, which are taken as the “truth”. Although this is OK up to a certain extent as a modeling exercise, it could be toned down a bit and shift the emphasis on the comparison to data. Also, explain what do we gain by having the CYCOM forecasts rather than relying on ALERMO. Perhaps elaborate on the “small scale features” observed in CYCOM.

\_ The section on the ALERMO description is not needed. Some parts are the same in CYCOM and can be merged, for more information on ALERMO, References should be given. One Reference on ALERMO is mentioned (Sarantis, 2006) but not given. Not clear what is the “upgraded” ALERMO or what is “a real freshwater flux boundary condition” (p. 402-403).

\_ In the Conclusions (p. 413), it is not justified to say that “neither active nor slave model temperatures drift significantly from the remote sensing observations”. They

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do drift and this is probably the weakest part of the paper. It is honest to use this comparison to discuss the need for forecast improvement, most likely starting from the basin scale forecast. Also, this is another area where involving some dynamics would strengthen the paper: why is this a “difficult” area? The Rhodes gyre is mentioned; if that is relevant, elaborate.

\_ Fig. 1 is a bit confusing. Perhaps the ALERMO full topography with a separate window with the CYCOM topography would be better.

\_ Fig. 9: all plots should be done on the same scale.

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Interactive comment on Ocean Sci. Discuss., 3, 397, 2006.

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