

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

Anonymous Referee #1

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Comments on the paper Operational coastal ocean forecasting in the Eastern Mediterranean: implementation and evaluation

This paper concentrates on the CYCOM model system, which is nested into the ALERMO forecast model.

General comments: The first section of the paper provides a generally good overview of the operational system. However I find the second section of the paper confusing, as it does not clearly relate to the present operational system. Given the title of this paper I would have expected to see in the model-observation comparison section a discussion only of the present operational model, and with some discussion perhaps of the quality

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of forecast from different lead times (one day, two day, five day) through comparison with the observations. It might be useful to describe the reasons why a five-day forecast is produced. Is it mainly for the surface meteorologically forced conditions ?

The second section of the paper as it stands at present is more like a report on investigations into which model configuration (slave or active) should be chosen for the operational system, rather than an evaluation of the present operational system. The authors should either change this section, or change the title of the paper. At any rate, be clear to not overlap with the companion paper of Sarantis.

The paper raises (though does not answer) the question of how to validate a high resolution nested model that is initialised from a coarser scale model. The method used here is to compare the nested model with the donor model, and to say that mostly they agree, except where extra detail develops. Without dedicated observations it may be impossible to decide which model is best.

Some more detailed comments follow below.

Section 2: Implementation

This section provides a clear exposition of the configuration and model setup. Perhaps a little more detail is called for in some places, eg the description of vertical mixing using MY needs additional information as to specification of mixing length, and is a Craig and Banner type surface wave source parameterisation scheme included ??

In the ALERMO description, the text doesnt make it clear where the freshwater flux is applied, is this the river inputs or to do with the specification of tracer mixing ? Also in this section the acronyms MFSP and TOP should be spelled out in full. And I presume that routine daily running of the CYCOM model continues (as is stated earlier) and not just During TOP. I think that the MFSTEP acronym has been spelled out earlier in the paper, so does not need re-spelling out on P403. [This suggests that sections of text may have been brought together from other reports]. One for the editors to note is the

spelling of weekly (not weakly) on P403.

The discussion on p404 assumes prior knowledge of VIFOP .The information about the extrapolation modulus is not enough on its own.

In the section 2.3 perhaps it could be made clearer that CYCOM is initialised once a week with DAY ONE of the ALERMO 5-day forecast, and in turn that ALERMO is initialised once a week with DAY ONE of the 10-day MFS basin model forecast.

Figure 2 does provide a clear illustration.

Section 3 Evaluation There is much discussion of the slave active modes of running the nested model, in the absence of data assimilation. There is suggestion that some of this is reported in a separate paper, and I have not yet read that other paper, so have not been able to determine if there is any overlap in material.

Section 3.1.1 I find on reading this that I do not know whether the system was running with daily update for the output described. If so, why pick on one date with only one four day forecast ? Perhaps a little more needs saying in the implementation section about the dates on which upgrades to the system were introduced.

I also think that this section is redundant, since the operational system already described states that VIFOP is used. The work to decide whether to use VIFOP or to use bi-linear interpolation should be reported separately, and then referenced in the present paper.

Similarly in 3.1.2 Is the discussion of slave active modes of running fully described in the paper by Sarantis ? if so, it need not be repeated here.

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