**Ocean Science
Discussions**

Interactive
Comment

Interactive comment on "Assessment of one year of high-resolution operational forecasts for the southeastern Mediterranean shelf region in the MFSTEP project" *by* S. Brenner et al.

Anonymous Referee #2

Received and published: 16 January 2007

Review of the ms. "Assessment of one year of high-resolution operational forecasts for the south-eastern Mediterranean shelf region in the MFSTEP project", by S. Brenner et al.

The paper describes the operational implementation of the MFSTEP south-eastern Levantine shelf model, a forecasting system based on the ocean model POM, nested in ALERMO, in its turn a regional application of POM, and forced with SKIRON surface meteo fields. The authors also performed a validation of the forecast products for the period Dec 2004-Dec 2005, comparing them to SST from satellite and in situ data (CTDs from a cruise in 12 Sep 2005 and 1 current meter during Dec04-Dec05). For

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

completeness, also forecasts/analyses from ALERMO and from the MFS model are included in the analysis. The skill assessment, by means of RMSE and ACC, shows that the shelf model performs better compared to ALERMO and persistence.

The topic of the paper certainly fits a MFSTEP dedicated issue, on the other hand the originality of this paper is low. Large part of the paper is similar, if not even an exact copy, to Brenner et al. 2006 in press in Journal of Marine Systems (available online on sciencedirect). In my opinion, since the value added compared to that paper (longer time series used for validation) is really limited, I cannot recommend the manuscript be accepted for publication in Ocean Science in the present status.

I strongly encourage the authors to:
1) better fit the Introduction to this manuscript (see below)
2) remove any unnecessary sentence already stated in Brenner et al JMS 2006. A comparison line by line of the two papers is somewhat embarrassing given their similarity. It is fine if you make this paper shorter instead of just publishing the very same stuff in two different journals.
3) expand the potentially original part, i.e., the assessment of the shelf model, adding some more in depth analyses.
4) write a paper providing traceability of results. Please provide more details on your data processing (see in particular remark 3 and 6)

Main remarks:

1) The introduction doesn't sound well fit. This is not a general review paper about worldwide nowadays ocean forecasting systems. I encourage the authors to stick spending words justifying the reasons why they run a regional operational ocean forecast. The general discussion about worldwide centres providing ocean/meteo forecast, differences between atmospheric forecast and ocean forecast so far, even wave forecast, data assimilation issues and so on are only loosely connected with this paper,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU

and, in my opinion, most of it can be skipped. Moreover, as stated above, the Introduction is a copy/paste from Brenner et al. JMS 2006. I strongly encourage the authors to completely reshape this section.

2) The section on the nesting procedure is a copy/paste from Brenner et al JSM 2006. Please dump it out -equations included- and point to your JMS paper as reference. Also, in the conclusion, the statement about the "nesting strategy that is stable and robust" is unjustified given the analysis done in this paper.

3) Section 3.1: It is unclear how the authors carried out the RMSE statistics. For example, the SST is provided on a grid of 6 km, while model data are provided with 1.5/3/6 km. Did the authors averaged results from the high resolution models in a box of 6 km (suggested)? Or took the nearest grid point value? Or what else? How did the authors aggregate the values over time? Results from fig2-3 are estimated averaging RMSEs or MSEs and then taking the root? Please spend some words on it.

4) Section 3.1:The two regional models are POM-based. This means that the authors are comparing SST from satellite with model temperatures that are representative of the upper sigma level, which can be very thick in the deepest region. What the authors believe is the drawback of such a comparison?

5) Section 3.1:The explanation for the "odd" behaviour in fig.3 is broken. Persistence is behaving exactly the same way as the shelf model and, of course, persistence cannot evolve and develop small scales. Please expand.

6) Section 3.2: which current meter did you use exactly? Can you explain a little bit more in detail the dataset? Did you de-tide the data? Or filter? How? If not why? And so on. In addition, you can perform some quantitative analysis, not only qualitative. For example, complex correlation coefficient (Kundu, Ekman veering observed near the ocean bottom, JPO 1976) or ellipses of variance (PCA) as in Emery Thompson, 2001, Data Analysis Methods in Physical Oceanography. Even some skill scores for currents can be drawn from the literature (Holloway Sou, 1996, Measuring skill of a topographic

stress parameterization in a large scale ocean model, JPO, vol 26; Penduff et al on the use of current meter data to assess the realism of ocean model simulations, Ocean Modelling 2006, vol. 11).

Fig.5: why not showing also ALERMO performance, since your system is nested in (and initialised with) ALERMO and not directly MFS?

Technical remarks:

Please add reference for SKIRON and the SST dataset used.

p 2068, line 1: I suppose it is DBDB1.

Interactive comment on Ocean Sci. Discuss., 3, 2059, 2006.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

EGU