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## ***Interactive comment on “Forecasting circulation in the Cilician Basin of the Levantine Sea” by E. Özsoy and A. Sözer***

**Anonymous Referee #2**

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This paper describes the development of a regional model of the Cilician Basin within the framework of the ALERMO regional model of the Levantine Sea. There is no reason given explicitly for this work and there are no significant scientific results. The technical implementation is straightforward and the paper seems more suited as a technical report to the MFSTEP project. In summary, in its present form I would not recommend publication in Ocean Sciences.

The abstract of a paper should summarise the main scientific results and not contain new information. Here we are given information about the model nesting information and boundary conditions, which do not appear elsewhere in the paper. No results/conclusions of the work are given at all.

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The layout of the paper could be improved. At present Section 1, called Introduction, gives a useful description of the region. However the paper would benefit from a paragraph explaining the purpose of the modelling effort and placing it in context. Environmental pressures are mentioned in passing but it is not stated if this is the justification for the work. This would be a genuine introduction and could be followed by the description as a separate section.

Section 2 is called Cilician Basin/Shelf Circulation modelling and includes details of the model, the experiments and the results. For clarity, the model results would be better in a separate section.

As mentioned above, the ALERMO model, which provides the boundary conditions and topography on which this model is based, is only mentioned in the abstract. A description here, with references, and a reference for the SKIRON atmospheric model would be given here.

Section 3 gives some examples of operational forecasts made using assimilation during a 6-month period of data collection starting in September 2004. It is stated that 5-day forecasts are performed weekly for the entire series of MFSTEP model domains. No further information is given as to what these are, nor any relationship between this and the previous results. Which choice of surface boundary conditions was made?

Section 4 is more clearly described and the results well presented. Since the text in the figures is rather small, the caption should state that the fields are salinity with velocity vectors. Some expansion on the difference in flux formulation between the coarse and fine model would perhaps help to explain the differences in temperature shown in Figure 10. Are the temperature differences uniform over the domain or would it be worth including figures comparable to 8 and 9 for temperature?

Technical corrections:

The reference for Wust, 1961 is missing.

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Figure 2a shows a satellite image of the whole Levantine Basin and Figure 2c a schematic of the circulation suggested from observations. Figure 2b is not really appropriate at this stage of the paper and as the temperature scale is very different from that of the satellite image, does not give any extra information.

There are no references for the UNESCO or DBDB1 bathymetric datasets

In Figure 3 the text on the scale is too small to read.

The relationship between the captions for Figures 3, 4 and 5 and the runs is unclear. The caption refers to run B-E, the text to runs A-D.

The caption for Figure 7 states that (h) is for 13 September 2005. This is outside the forecast period. Is the caption incorrect?

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Interactive comment on Ocean Sci. Discuss., 3, 1481, 2006.

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