Ocean Sci. Discuss., 3, S171–S174, 2006
www.ocean-sci-discuss.net/3/S171/2006/

Ocean Science Discussions

Interactive comment on "Operational analysis of the circulation and shelf-slope exchanges in the continental margin of the northwestern Mediterranean" *by* A. Jordi et al.

Anonymous Referee #2

Received and published: 13 July 2006

General comments

The paper presents a modelling study of the Gulf of Lions for a one month period (December 2005) using a non-hydrostatic model forced with 3 hourly atmospheric forcing and with lateral boundary conditions from a large scale model. The model seems to represent well the circulation and the dynamics induced by the topographic irregularities. An important result of the paper is the estimation of the shelf-slope water transport, which is computed using a model with real topography and real atmospheric forcing. I recommend it for publication, however, there are some questions or comments about the main focus of the paper that require further clarifications.

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Interactive Discussion

Discussion Paper

As I understand from the text, and contrary to what is stated in the title with the world “operational”, the authors are not describing (neither validating) any operational system producing ocean forecasts routinely in near real time. The model configuration is just a simulation experiment using a nested model forced with high frequency atmospheric analysis. Therefore, I don’t understand the reason why it is said page 588, line 15 "...ocean model was operationally run..." or, in line 9 " ... to validate the ocean forecast model... ".

The simulation starts from an interpolated field from a coarse ocean model and run the model for 30 days. But the authors don’t discuss if the interpolated fields are already in balance and there is not in the paper any comment or discussion about the spin-up duration. In this sense, the authors could enlarge the model simulation, at least to a complete winter season, in order to provide better estimations of the shelf-slope transports and to describe better the model performance.

Specific comments

1. The authors are using a non-hydrostatic version of the model with a horizontal resolution of approx. 1Km, which seems to be an important achievement. However, there is not in the text any justification or explanation about the relevance for this study of the use of a non-hydrostatic model, instead of a hydrostatic model. For instance, are the model vertical velocities in figure 5 very much dependant on this fact? Are the model shelf-slope transport very sensitive to hydrostatic vs. non-hydrostatic modelling?

2. One remark in the conclusions of the paper is the problem in the model air-sea forcing. The authors also states in page 593, line 15 that "...discrepancies could be attributed to errors on the atmospheric forcing or to inaccuracies on the model’s heat transfer...". However, there is not any description of the heat fluxes formulation: are the heat flux components taken directly from the HIRLAM model, or they are computed used bulk formulae? If it is the first case, what are the "air-sea coupling algorithms" cited in page 595, line 18. What things are being doing to improve the atmosphere-

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ocean couplings?

3. It is not clear from the text if the structures reported in 3.1 were already present in the initial conditions fields or if they are formed in the high resolution run.

4. Some references about recent 3D modeling studies on the gulf of Lions are missing (e.g. Estournel et al., JGR (109), 8059, 2003). Korres et al. 2000, is not a precise citation to describe the intense local episodic atmospheric events in the Gulf of Lions.

5. The model vertical resolution is rather coarse: only 30 levels and the first one of 10 m. Is the model topography well represented with this resolution? At the same time, the general circulation Mediterranean model has much higher vertical resolution (70 levels), how is done the interpolation between these two vertical grids?

6. Can the authors provide any observations to compare the model vertical velocities in Figure 4?

7. The direct comparison between model surface temperature and SST satellite is not direct because the 1st model layer is 10 m depth, while the satellite gives a skin temperature.

8. The figure 8 showing the ARGO-model comparison is not clear: a zoom to the surface layers is required to see better the misfit on days 14 and 29 December. In the other hand, if this ARGO profiles had been already assimilated into the large scale ocean model (from which the initial condition has been taken), it is normal the agreement found in the deeper levels.

Technical corrections

Page 591, line 26: last paragraph, "...was previously simulated by..."

Page 592, line 22: "Comparison with observations" instead of "Comparison with observation data"

Page 593, Equation 1, is it correct? The numerator and denominator are the same.

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Page 594, line 25 : last paragraph: "(Jordi et al., 2006). Since to a larger extent"

Page 596, line 16: "mixed layer" instead of "mixing layer"

Page 605, Figure caption 7: "...2005. The anticyclonic..."

Interactive comment on Ocean Sci. Discuss., 3, 585, 2006.