

## ***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.**

**A. Jordi et al.**

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We thank very much the referee for his/her constructive comments. We have followed them in detail and modified parts of the manuscript accordingly. We believe that this new version of the manuscript is now much better focused and better justified. We answer point by point questions or comments expressed by the referee.

### **General comments**

We agree with the referee that the manuscript does not describe any operational system, although the model is designed for operational runs in the near-future. Therefore, as suggested, we have removed any reference to 'operational' in the manuscript.

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In section 2.2, we have included a more detailed explanation of the model with reference to the spin-up phase and justifying assay duration.

### Specific comments

1. In the new version, we explain the advantages of a non-hydrostatic model. Running a hydrostatic version is beyond the scope of the study, however Tseng et al. (2005) compare hydrostatic and non-hydrostatic versions of DieCAST in Monterey Canyon showing that the non-hydrostatic effects near steep slopes cause vertical acceleration and small-scale features.
2. We now specify that heat and water fluxes are taken directly from theHIRLAM model in section 2. The model does not really use air-sea coupling, hence, we agree that the use of the term 'air-sea coupling algorithms' can be misleading. The last sentence of section 3.3 and the conclusion have been modified accordingly.
3. We have added a new paragraph at the end of section 3.1 explaining this important point. The Northern Current and the anticyclonic eddy were already present in the initial fields. However they propagate during the model run and small-scale structures are generated. Conversely, the vertical motions related to the submarine canyons fully develop during the model run.
4. We have included the reference of Estournel et al. (2003).
5. In section 2.2, we have explained how the interpolation between the global Mediterranean model grid and our grid is performed. As stated, the interpolation is linear in the vertical. We believe that 30 vertical levels should adequately represent the complex topography in the area, as shown by the results.
6. Unfortunately, observations to compare the model vertical velocities were not available during the model run. However, Pascual et al. (2004) estimated similar vertical velocities from hydrographic data in the Palamós Canyon. We have included a reference to this work in the manuscript.

7. We agree with the referee and we have indicated this point in the comparison between model surface temperature and SST satellite. Nevertheless, we would agree this to be a major problem on summer but not during winter when strong mixing occurs.

8. A zoom in the old fig. 8 (now fig. 10) has been done to enhance the misfit in surface layers. We also agree with the referee that the agreement found in the deeper levels is normal because ARGO profiles had been already assimilated into the large scale ocean model. We have included this statement as suggested.

### **Technical corrections**

All technical corrections suggested by the referee have been done.

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

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