Ocean Sci. Discuss., 3, S131–S137, 2006
www.ocean-sci-discuss.net/3/S131/2006/


Ocean Science Discussions



# *Interactive comment on* "Central Mediterranean Sea forecast: effects of high-resolution atmospheric forcings" *by* S. Natale et al.

**Anonymous Referee #1**

Received and published: 7 July 2006

The manuscript describes the results of three different numerical experiments, carried out in forecast mode, based on the ocean model POM (the local operational implementation being named SCRM). In such experiments, different atmospheric forcing have been used to provide surface boundary conditions. The outputs from the three experiments have been compared to QuikSCAT and AVHRR measurements, in order to assess the impact of each atmospheric model on the ocean forecasts.

General comments:

The topic is potentially interesting, but the overall approach is weak and need substantial improvements. As a consequence of such weakness, conclusions are not robust.

Three different atmospheric data sets are used. One is from the ECMWF deterministic forecast system, the other two from the regional model SKIRON, run at two different horizontal resolutions. The authors didn't analyse the different forcing impact of a given atmospheric model, simply changing its spatial resolutions. They only tested the impact of different atmospheric models. It is not only a matter of different resolution. The physics involved is different, the configuration of the systems is different (i.e. SKIRON is nested in ARPEGE, so even the large scale signal may be different) and so on. Therefore, either the title or the analysis is wrong.

I'm sceptical also about the comparison whit available observations (see main remarks #1 and #2). While RMSE is a standard score in forecast verification, and it is widely used, it can be misleading if not carefully interpreted. In particular, the basin-averaged RMSE, if not decomposed, do not permit to assess if the spatial structures of the three experiments differ or not.

Validation of wind-driven currents, in addition to the analyses performed on wind stress and SST, would have been a key point to be addressed within the aims of this work. Even if I understand that current data are not easily available, this lack basically makes the authors unable to detect and assess the likely major impact of the different forcing. As a consequence, the authors should substantially rethink the aims which this manuscript is supposed to address.

Main remarks:

1) QuickSCAT-retrieved momentum stress has been used to assess the performance of these three experiments. The resolution of this dataset however, is not really useful for the assessment of small scale structures potentially resolved by LAMs. The authors claim that momentum stress assessment is a probe of the quality of the driven current field, which they cannot validate. If so, I believe the wind stress curl is a key quantity to be assessed, together with magnitude and direction. In addition, I'm not convinced by the approach used to carry out RMSEs. The authors interpolated QuikSCAT data



OSD

3, S131–S137, 2006

(0.5° horz. res.) onto the model grid (1/16°) to carry out the comparison. I suppose (unfortunately information on the data sets are scarce, see general remark #8) that QuickSCAT data are average over the 0.5x0.5 cell size. It makes more sense to me to average model outputs onto the corresponding QuikSCAT grid. Even if this procedure downgrades the resolution of the ocean model outputs, this is the actual resolution of the measurements the authors have at hand.

2) Regarding the SST comparison: using LAMs forcings, one would expect to find some mesoscale processes on the SST field that the coarser resolution atmospheric model may not be able to drive. This does not necessarily provide better RMSE (think for example to the double penalty error). On the other hand, the authors didn't calculate any score which could show the spatial variability of the errors; The analysis was only for some basin-averaged RMSEs. Should any difference exist on the spatial structures, it will not be clearly assessed until one does not decompose the RMSE. The authors just showed a single snapshot field comparison (useful, by the way), but an overall quantification of some scores, describing the internal variability of the fields, would be desirable (for example, anomaly correlation coefficients).

3) In the conclusion (and in the abstract) the authors claim that results from the used slave models do not differ from the large scale model. However, throughout the manuscript, the authors spent very little effort to justify this. If the authors believe their assertion is interesting, they should at least include the large scale model outputs in some selected RMSEs analyses, so the reader can figure out the similar (or dissimilar) quality of the results.

4) The paper is focussed on 5 forecasts only. If the forecasts are routinely provided in NRT, you may think to enlarge your analysis at least to the whole winter season.

5) I wonder if some surface drifters data of MFSTEP are available in that period. If so, the authors should think to include them.

6) Throughout the whole manuscript, the authors use the acronyms ECMWF (and



OSD

3, S131–S137, 2006

Interactive Comment

LAM2-NH1) even when they are talking about POM experiments (forced with ECMWF etc.). This is unfair. Please use ECMWF when you are strictly talking about ECMWF products. If you are talking about POM+ECMWF (or POM+LAM2/NH1) use for example, EXP 1/2/3, or EXP a/b/c or whatever. Modify table 1 accordingly.

7) It is recommended to show (or included in the discussion) also "prediction intervals" on RMSE estimates. Bootstrap can be used for this purpose (see Forecast Verification: a practitioner's guide in atmospheric science by Joliffe and Stephenson [2003] for a general overview). This helps the interpretation of the differences amongst RMSEs.

8) The section about "methods" is unbalanced. The model description can be definitely shortened, some equations dropped out ( eq.7 and 8 are really not necessary) eventually pointing to the proper literature. On the other hand, the "data analysis" section is poor. Please add some more info on the datasets, algorithms used to retrieve the quantities of interest, add some references. Which parameterisation has been used to convert Seawinds wind field to momentum stress field? Which are the errors associated to the measurements? Also what do you exactly mean with "daily means" of AVHRR data is unclear. Then, cloudy region are flagged out or filled via OI?. SSTs are skin? bulk? In my opinion, the authors should also (briefly) explain the impact on the analysis of remotely sensed SST versus model SST which, I suppose, in the deepest part of domain can be even representative of, say, 50m thick model layer.

Specific remarks:

The abstract should be rewritten, since it is not consistent with author's findings. Line 1: unclear statement. line 8: "due to their ability to detect transient disturbances": the authors didn't asses such skill. "the ocean model in the slave model was not able to detect dynamics different from the driving model": see general remark #3.

Introduction: state clearly which are the aims of this work.

P 641, line 7: please don't use the acronyms LAM2 here. The meaning is explained 5



OSD

3, S131–S137, 2006

Interactive Comment

pages later.

P 642, line 1-2. It has been already stated on page 641 line 2-3 that the model resolution is 1/32 and 24 sigma level.

P 642, line 6: The cited Mellor's and Blumberg's paper doesn't say anything about pressure gradient error (and BTW, it's 1985). I suppose the correct reference should be: Mellor, Ezer, Oey, 1994. The Pressure Gradient Conundrum of Sigma Coordinate Ocean Models. J Atmospheric Ocean Tech, 11: 1126-1134.

P 646, line 25-28: why calling one model LAM2 (limited area model 2) and the other NH1 (non-hydrostatic 1) if both models are LAM and NH? This may be confusing.

P 646, line 11: is there a non-grey literature about SKIRON?

P 646, line 12: please state what ARPEGE is (add reference).

P 646, line 18: ECMWF does provide precipitation forecasts (if you ask for them). Please explain or reshape the statement. It's probably a matter of agreement between MFSTEP project and ECMWF.

P647, line 14- l 17. please rewrite. State only what you are going to show in the results section.

P 647, line 34-35: strictly speaking, in your configuration the wind stress is a SCRM output. The wind field is an input.

P 648, line 21-23: Unclear statement. If the "agreement" is between observations and model outputs it is a questionable statement (you should do a mean daily data comparison, filtering out intra-day variations). If the "agreement" is between model outputs, I would say they are simply different.

P 648, line 25: you cannot assess the current field just estimating some domain-averaged RMSE of the momentum stress. For example, is the wind stress curl irrelevant?



OSD

3, S131–S137, 2006

Interactive Comment

P 649, line 3-6: obvious sentence.

P 649, line 16: please remove “for the period 1-3”. You are simply showing the mean daily wind stress of 5 Jan 2005.

P 649, line 21. Add references in the para from line 21 to 28 to support the dynamical interpretation you are providing.

P 651, line 16-end. I don’t see the point to speculate why ECMWF did not reproduced the vortex south of Sardinia when you don’t even know if it occurred (if you know it did occur, please show some evidences). Since Quikscat data are not useful to this purpose, you should collect available coastal wind observations from standard meteorological networks to see if any signal (even if weak) supports the existence of such vortex.

P 652, line 6. useless sentence.

Page &52, line 24-27. If this is true this analysis is not useful to assess the impact of the resolution itself. In addition, why you didn’t use directly the shortwave radiation from the ECMWF GCM?

Page 653, line 19. Looking at figure 8 mid panel: I don’t see the claimed discrepancy.

P 655, line 1-2: You should state this also in the discussion adding some references about the “present knowledge”.

P 655, line 3, again about the atmospheric vortex in the Sardinia channel: I suggest to remove the full sentence, unless you show measurements that support such structures.

P 655, line 25-28 (sentence regarding current forecasts for oil spill, pollutants). Better to state this in the introduction.

Table 1:

-) It is fine if you compress the three lines [drag coefficient/zonal wind stress/merid.

wind stress] in just one: momentum stress via Hellerman and Rosenstein.

-) downward and upward shortwave flux entries are confusing. Reed formula provides both sw flux, not only the upward. Stick with "following Castellari et al" only or add carefully the proper literature.

-) what do you mean with water flux following Castellari et al.?

Figure 2-3-8: which model outputs are you talking about? Are they 1-hourly? 3-hourly? averages? instantaneous values? Please specify.

Figure 2-3-8 (the upper panel): authors are comparing mean daily measurements with unspecified time series of model outputs. Please compare daily mean values with daily mean values. If the intra-day behaviour is important, show it in an additional panel.

---

Interactive comment on Ocean Sci. Discuss., 3, 637, 2006.



OSD

3, S131–S137, 2006

Interactive Comment