

Interactive comment on “Effects of the 2003 European heatwave on the Central Mediterranean Sea surface layer: a numerical simulation” by A. Olita et al.

Anonymous Referee #1

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This paper attempts to assess the influence of the anomalous atmospheric heat wave in the summer of 2003 on the near surface circulation of the central Mediterranean Sea by running a numerical ocean model for the region of interest for a period of 5 years (2000–2004) with surface forcing based on the operational meteorological analyses taken from the European Center for Medium Range Weather Forecasts (ECMWF) and lateral open boundary conditions from the MFSTEP Ocean General Circulation Model (OGCM), and by applying Fourier and wavelet analyses to the atmospheric forcing data and the model’s sea surface temperature (SST) response. While the question raised by the title of the paper is interesting, the authors do not adequately address the topic from several points of view. There are also several major uncertainties and questions

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that need to be addressed and resolved as I will explain below. In its present form, I cannot recommend the paper for publication until major revisions are made.

The first problem with the manuscript is that it focuses heavily on analyzing the atmospheric forcing, while the main topic (response of the upper layer of the sea) receives very limited attention. Eleven pages (86-96) of the manuscript are devoted to describing the model and the analysis methodology followed by a “results” section in which four pages present the atmospheric data, but the SST analysis only warrants two pages, and the subsurface (AIS) layer gets barely one page. At a bare minimum the analysis of the ocean response needs to be significantly expanded. For example, they apply the Fourier and wavelet analyses to the simulated SST only. The comparison with observed SST data is limited to the spatial maps of the simulated August SST anomalies for the years 2001, 2002, 2003, and 2004 (Figure 12) with the observed monthly mean satellite SSTs (full fields rather than anomalies) covering the entire Mediterranean. It is nearly impossible to draw any conclusions from such a visual comparison. Figure 13 supposedly shows the SST time series and the model - observed differences for the entire period, but it is not at all clear if these curves relate to a specific point or if they are some type of model domain average. It is also not clear why they chose to use SST data from NASA-JPL (as noted in the acknowledgements) rather than the SST analyses that are available through MFSTEP. Furthermore, to make their results more convincing, I would expect them to apply the Fourier and wavelet analyses to the observed SST fields in addition to the simulated SSTs. The purpose of these analyses is to reveal the characteristics and behavior of the variability in the time series and it is important to know if the simulated and observed SSTs are similar in this respect.

The other major difficulty with the experimental setup is to blindly use the ECMWF operational atmospheric analyses for a five-year period without checking into any changes that might have been made in the analysis system, which could render the statistics of the time series as nonstationary. Were there any major mathematical/numerical changes made in the system (e.g. type of spatial interpolation, details of the atmo-

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spheric model, resolution) that could introduce non-physical jumps in the properties of the time series. In other words, are they sure that the signal of the 2003 heat wave is properly represented in the forcing data. This is exactly the issue that the reanalysis project was meant to address. Therefore they must carefully check the source and validity of the atmospheric forcing fields for their intended purpose. They should ideally have used the fields from the reanalysis (e.g., ERA-40), or if not, they must convince the reader that the fields that they used do not suffer from the above-mentioned problems.

An additional crucial point that they must clarify regarding their choice of surface flux boundary conditions is the addition of the “correction terms” in the heat flux (Eq. 3) and the freshwater (or salinity) flux (Eq. 5). These flux correction terms were added in MF-SPP during multi-year, climatological, perpetual year simulations in order to guarantee the correct long-term heat and salt budgets of the simulated Mediterranean Sea. In this paper, however, the authors are interested in a particular case of interannual variability. Therefore they must provide some evaluation of the magnitude of these correction terms as compared to the anomalous 2003 forcing that is the focus of this paper.

Finally it is not clear which OGCM data they used for the lateral boundary conditions. On page 91 lines 4-6 they mention the older version of the OGCM based on MOM with a 1/8-degree grid. However on page 94 line 4 they mention a nesting ratio of 2, which based on their grid of 1/32 implies that they used the newer 1/16-degree OGCM which is based on OPA. Did they use the daily hindcasts or some longer term mean or climatological value? Note that the newer 1/16-degree OGCM hindcasts did not become available until mid-2005, which does not cover the period of their experiment. So which is it?

Specific comments:

In section 2.1.2 (specification of lateral boundary conditions) do they include an integral constraint that is designed to conserve the coarse model normal component of the

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mass flux on the higher resolution regional model grid?

Page 97, line 17: “wavelet and FFT plots” should be “wavelet and FT plots”. FFT (fast Fourier transform) is a particular algorithm used to compute the FT.

Page 99, line 16: “adimensional” should be “dimensionless”.

Page 100, line 2 and ff: The use of the terminology “long wave signal” is confusing. They should find an alternative term to distinguish this from the long wave radiative heat flux.

Page 102, line 3: At the end of the line “in comparison” should be “relative to”.

Page 102, line 15 ff: They should switch the order of Figures 13 and 14 to make the text more readable. They mention Figure 14 before they mention Figure 13.

Page 102, line 19: As noted above, what exactly are the curves in Figure 13? Some point value or a domain average?

Page 104, lines 14-16. I am confused here. If the anomaly forcing is due mainly to changes in the absolute value of the latent heat flux, then why is it important to know that the sensible heat flux has the largest percentage anomaly? Especially if the magnitude of the fluctuations of the latent heat flux is more than twice the respective value of the sensible heat flux fluctuations?

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