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Ocean Science Discussions

Interactive comment on "Improved near real time surface wind resolution over the Mediterranean Sea" *by* A. Bentamy et al.

Anonymous Referee #1

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GENERAL COMMENTS:

The paper presents a method to produce in NRT a blended wind field over the Mediterranean Sea using remotely sensed wind observations (from several satellites) and ECMWF wind analysis and evaluates the accuracy of the obtained blended fields. The analysis is limited to the January 2004. As a general comment this paper is interesting because it deal with an important issue for Mediterranean Sea dynamic studies and ocean modelling efforts. In fact the need of an accurate wind fields over the complex Mediterranean basis is one of the main request of the modelling community. In my opinion the paper merits to be published but need some detailed revision based on the following general and specific comments. My major concern with this paper is about the blending method that need to be more clearly described. The authors also need to

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demonstrate while this method should be preferred to other methods already proposed.

The authors should briefly discuss the difference between the interpolation method used in this paper and a standard Objective Analysis using ECMWF winds as first guess and justify the choice of the interpolation scheme used in this paper. Moreover the authors cannot ignore the work of Chin et al. (1998) that proposed a blending method based on the use of some statistical and spectral properties as inferred from scatterometer data and some other more recent publications (e.g. Millif et al. 1999 and other more recent papers)

Chin, T.M., R.F. Milliff, and W.G. Large, 1998. Basin-scale, high-wavenumber sea surface wind fields from a multiresolution analysis of scatterometer data. Journal of Atmospheric and Oceanic Technology , 15, 741-763.

Milliff, R.F., W.G. Large, J. Morzel, G. Danabasoglu, and T.M. Chin, 1999. Ocean general circulation model sensitivity to forcing from scatterometer winds. Journal of Geophysical Research, Oceans, Vol. 104, No. C5, 11337-11358.

The mathematical description of the used blending method is often hard to follow due to several typographical errors (see specific comments below) in the equations and for lacks of some essential mathematical step.

A lot of space in the paper is dedicated to the description of the satellite data in section 2, giving a lot of details about the functioning of scatterometer and radiometers but no space is left for the description of the buoy data and ECMWF winds.

SPECIFIC COMMENTS:

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(1) The second Xa in equation (1) should be Xb.

(2) Is the expected analysis value Xa corresponding to the satellite measure (as suggested by equ. 2)?

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(3) Define ta, tb and N in equation (2).

(4) Eq. (2): there is a sum over the index j in the right side of the equation that seems to move over all the grid points of the satellite swath while the left side index is i (hard to read) and refers to a particular grid points of the swath. I think that this equation needs more clarifications. In this form the equation tell me that the difference between the measured (Satellite) and the background (ECMWF) wind is a linear combination of the differences in all the other points of the swath averaged over the time window [ta,tb].

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(1) in the second term of the right side of equation (5) j near lamda should be subscribed. In general it is hard to understand how equations (4) and (5) are obtained.

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(1) the first equation 8 have X0 instead of M0 (subsript?). Is that correct?

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(1) Bentamy et al 1996 or Bentamy et al. 1999 (see bibliography).

(2) Equation (12). The second term of the Cov operator is not defined (may be epsilon is one of the previously defined).

(3) Equations (13) and (14) need to be better explained.

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(1) ok epsilon is small but the authors should write whether this parameter has been set to zero or not in the subsequent steps.

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(1) in equation (18) N seems to be the number of ECMWF winds used to interpolate over each satellite wind cells. Is N the number of ECMWF wind vectors falling within a given satellite wind cells or something else?

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(1) 'The main discrepancies are found in near coasts areas'. How far from the coast the agreement becomes particularly good? Is the width of this coastal area related to the QuikSCAT and SSMI spatial resolution?

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(1) In table 4 buoys 2008010 and 3155039 seem to be over land (please control coordinates, I have just checked qualitatively).

(2) 'Ě10 m buoy winds are calculated from raw data and 6-hourly averaged..'. At which height are the buoy winds measured? Have the buoy winds been adjusted to the 10 m neutral stability? More in general must clearly be established for each source of wind data at which eight are referred. If 10 m are chosen, it must be indicated if they are actual 10 m winds (as measured by an anemometer sited at 10 m) or the 10 m neutral stability (as calculated by QuikSCAT).

(3) '..The buoy data are collocated in space and time with ECMWF and blended winds as well as with remotely sensed wind observations...'. In the next page (pag 453) the authors indicate half hour from satellite observations to construct the “simulated buoy data”. Is this criterion applied also here? If yes, could the authors justify this choice explaining the physical meaning of this threshold?

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(1) '...As buoy data are assimilated in ECMWF analysis, they cannot be considered independent...', this is correct but the same apply also for QuikSCAT winds.

Interactive comment on Ocean Sci. Discuss., 3, 435, 2006.