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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 #2

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The manuscript by Bentamy et al. reports on a method for producing 6-hourly, high-resolution ($0.25°$) surface wind fields from satellite observations and ECMWF analyses, presumably in near real time (NRT). The authors identify by name the MFSTEP project as the potential user of the NRT winds (speed and direction), presumably to force ocean model simulations and forecasts. The satellite datasets identified in the paper include surface vector wind (SVW) retrievals from QuikSCAT and surface wind speed retrievals from SSM/I platforms F13, F14, and F15. ECMWF SVW analyses serve as a background field.

Two major concerns with the paper in its present state include:
[1] a lack of discussion regarding NRT issues with respect to the proposed method; and

[2] a spectral analysis validation of the blended wind product that is missing but essential to make the case for the proposed method.

Beyond the major concerns, there are a number of scientific, technical and operational concerns as well. These are described in the second section of this review. The notation and typographical errors in the equations, and the grammatical and word-selection errors in the text, render this manuscript ambiguous in places, and difficult to read throughout. The manuscript is not ready for publication in its present form.

Major Concerns

NRT Issues

Given the title for the manuscript, I expected to read about data latency and processing turnaround times, delivery schedules for analyses, and the NRT blended winds to support a variety of operational applications. There was only the tangential mention of MFSTEP and the importance of high-resolution winds noted in a few reports. When, and from where, are the ECMWF background fields made available? How long does it take to assemble the satellite vector winds (NRT data from NOAA NESDIS) and wind speeds (SSM/I from RSS of NASA MSFC)? How long does it take to generate a blended wind estimate for the whole Mediterranean Sea? How will the blended winds be distributed to potential users? How big are the files? If the paper is really about NRT winds, these questions should be dealt with. If instead, the paper is about a blending method that might one day be adapted to NRT applications, then how different are this method and this paper from prior works by the first author?

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Spectral Analyses

Numerous authors have documented an approximate power-law behavior, down to Nyquist scales, in kinetic energy (velocity variance) wavenumber spectra from scatterometer winds (Freilich and Chelton, 1986; Wikle et al., 1999; Milliff et al., 1999; 2004; Patoux and Brown, 2001; Chelton et al., 2006). The power-law dependence (i.e. linear slope) in wavenumber spectra is a distinguishing property of the SVW retrievals from scatterometer systems *versus* spectra from numerical weather prediction (NWP) models. The NWP wavenumber spectra can be deficient in power by orders of magnitude at scales of $O(100\,km)$ (e.g. see Fig. 1, Chelton et al., 2006). The authors should consult an MFSTEP report at: *www.bo.ingv.it/mfstep/WP3/Docs/ingv_final_rpt.pdf* for surface wind kinetic energy spectral comparisons in the Mediterranean Sea.

The validity of the blended winds proposed here must be measured in this same context. Does the blended wind product exhibit power-law behavior in wavenumber spectra, down to Nyquist wavenumbers? If not, at what spatial scales do the spectra depart from approximate power-law behavior, and by how much? For the implied purposes of forcing high-resolution numerical ocean models (e.g. MFSTEP), the high-wavenumber properties of the blended winds are critical.

Other Scientific, Technical and Operational Concerns

[1] The error analysis in the present paper is not consistent with the state of the art for comparing SVW retrievals from satellite, SVW estimates from forecasts and analyses, and SVW observations from *in-situ* buoys. In addition to the older papers cited by the authors, there should be references to Freilich and Vanhoff (2006) and Chelton and Freilich (2005). See also the comprehensive report by Stoffelen et al. at: *www.knmi.nl/ stoffele/BCRS_QSCAT6a.pdf*. Scatterometer errors are most accurately described in terms of a random component error that is a function of wind direction

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(i.e. alongtrack vs. across-swath).

[2] The space-time support differences in the data types must be recognized (i.e. point measurements averaged in time from buoys, spatial averages of instantaneous obs from satellites, volume and time averaged values from numerical models). These considerations will affect error model development and covariance estimates.

[3] The distinction between wind speeds (scalars) from SSM/I, and SVW (vectors) from QuikSCAT, is well-known to the authors but might not be well-known to the *Ocean Science* readership. Calling SSM/I related data “winds" is confusing in this regard. A clear statement that SSM/I retrievals are wind speed only, comes late in the paper (page 450, after line 25). Moreover, comparing wind speeds to validate the blended product is not a very stringent test. The wind speed accuracies of all systems (QuikSCAT, analyses, SSM/I, buoys) are probably not the critical factor for users. Wind direction (and implied curl and divergence) are more problematic. In addition to spectra, these derivative fields are more valuable tests of the validity of the blended product.

[4] The NOAA NESDIS NRT QuikSCAT winds have been assimilated in the NCEP and ECMWF forecast models since January 2002. This means the NRT QuikSCAT data influence the forecast/analysis system. They are an ill-defined part of the ECMWF analyses in January 2004 used in this paper. Are there independence and identifiability issues for the Kriging method (i.e. in assigning error terms ϵ_o, ϵ_b, for the X_o and X_b terms)?

[5] A single generic covariance model for all seasons, and for the entire Mediterranean is a serious over-simplification. The seasonality and regionality of strong wind events is described in the introduction (i.e. Mistral, Etesian, Sirocco, Bora). Intuition suggests that the important high-wavenumber variability of the surface wind process is better supported with regional and seasonal (error) covariance models.

[6] The limitations of the QuikSCAT data within about $35\,km$ of shore are not clearly described. The blended winds cannot be strongly influenced by QuikSCAT very close

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to coastlines. These nearshore limitations seem to be confused in part with intermittency due to the polar-orbiting, swath-based sampling of QuikSCAT (e.g. see line 20 page 451 through line 2 page 452).

Editorial Comments

I cannot, in reasonable time, provide an exhaustive list of the typographical, grammatical and word-choice errors that plague the manuscript. I urge the authors to find a thorough, independent editor before submitting any revisions. Maybe the journal can provide one at this stage to save everyone a lot of tedious effort.

I will focus here on the mathematical notation errors that were most glaring.

a) Equation 1; change the subscript on second X.

b) Equations 2,5,6,8,9,11; what is N?

c) Page 444, line 18; the definition of the M notation need only be specified once. Change “state for longitude...".

d) Page 445, lines 1-5; the definition of the Expectation Operator E is awkward.

e) Page 445, lines 5-10; where do the equations (5) and (6) come from? How do they relate to (4)? How can the left hand sides be identical? The “l" superscript is easy to confuse with “1" or “i".

f) Equations 8: check subscript on left hand sides in both cases.

g) Equation 9; check subscript on λ.

h) Equation 12; should there be a subscript or superscript on the second ϵ?

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i) Equation 13; should the square be outside the parentheses?

j) Equation 18; subscripts on d need repair.

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