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Ocean Science Discussions

OSD

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Interactive Comment

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

A. Bentamy et al.

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We would like to thank the two reviewers because their comments helped us improve the manuscript. We feel really sorry about several typo mistakes in the first version. We did our best to avoid any new problem in the revised manuscript. All reviewer comments were considered and responses are provided hereunder.

Note that all figures can be found here: `ftp://ftp.ifremer.fr/ifremer/cersat/tmp/OceanScience/Reply_Reviewers_osd-2006-0071.doc`.

A. Review 1

The reviewer is right. Previous methods were developed and used (Millif et al, 1999) to estimate high spatial and temporal surface winds from satellite and NWP data. This study does not aim to compare the existing methods in terms of mathematical procedure or in terms of resulting wind fields. It only aims to provide useful information to users and readers about surface wind estimates from blending near real time satellite observations and ECMWF operational analysis. This work is performed within MF-STEP and MERSEA projects. To our knowledge, the study aiming to merge such wind sources over the Mediterranean Sea is done for the first time. We agree with reviewer that comparisons with existing blended over the Mediterranean Sea will be very useful. It is expect to be performed with longer time series in future.

SPECIFIC COMMENTS:

1. Reviewer: Page 444. The second Xa in equation (1) should be Xb.

- Reply: Thanks

2. Reviewer: Page 444. Is the expected analysis value Xa corresponding to the satellite measure (as suggested by equ. 2)?

- Reply: Xa indicates the expected analysis value that should be estimated from satellite observation Xo and background data Xb (see page 444, lines 1- 2)

3. Reviewer: Page 444. Define ta, tb and N in equation (2).

- Reply: The following sentence is added: *ta, tb are the beginning and end time of the analysis. N is the number of available and validated observations.*

4. Reviewer: Page 444. 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].

- Reply: In our opinion, EQ. (2) is quite clear and widely used for interpolation issue. Indeed, the estimation $\hat{\varepsilon}_i$ of the expected difference, $\tilde{\varepsilon}$, between the observation (satellite) and the background (ECMWF) at the grid point M_i, is calculated as a linear combination of the N difference values located in neighbourhood of M_i and collected between t_a and t_b.

5. Reviewer: Page 445. 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.

- Reply: Thanks.

Equations (4) is derived from the development of $Var(R) = E((\tilde{\varepsilon} - \hat{\varepsilon})^2)$ based on the formula: $Var(X - Y) = Var(X) + Var(Y) - 2cov(X, Y)$. Where X and Y are random variables.

Equation is obtained from the formula: $Var(X) = E(X^2) - (E(X))^2$.

6. Reviewer: Page 446. the first equation 8 have X0 instead of M0 (subsript?). Is that correct?

- Reply: Thanks. The right subscript is M_0. Change is made.

7. Reviewer: Page 447. Bentamy et al 1996 or Bentamy et al. 1999 (see bibliography).

- Reply: The reference Bentamy et al (1996) was missed. It is added to the reference list.

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8. Reviewer: Page 447. Equation (12). The second term of the Cov operator is not defined (may be epsilon is one of the previously defined).

- Reply: The right equation is $C(\delta h, \delta t) = Cov(\tilde{\varepsilon}(M_i, t_i), \varepsilon(M_j, t_j))$

9. Reviewer: Page 447. Equations (13) and (14) need to be better explained.

- Reply: The following change is done:

  G is the spatial and temporal structure function of difference variable . It is only function of the spatial separation , δh between grid $M_i$and M_j, and of the temporal separation, δt between t_i and t_j.

Using the first intrinsic assumption (eq. 7), equations (12) and (13) :

$$G(\delta h, \delta t) = 2(C(0,0) - C(\delta h, \delta t))$$

10. Reviewer: Page 448.ok epsilon is small but the authors should write whether this parameter has been set to zero or not in the subsequent steps.

- Reply: The following text is added: ε_p is found small and remains nearly constant as a function of geographical area as well as a function of period. Therefore it is considered as negligible and set to zero.

11. Reviewer: Page 449. 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?

- Reply: Indeed, N is the number of ECMWF wind vectors located at a given spatial distance from satellite wind cell. Change is made.

12. Reviewer: Page 451.'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?

- Reply: Using much more moored buoys moored in North east and west Atlantic, and in North Parcific (NDBC), the comparisons performed between satellite and buoys located offshore (more than 50km far from coast) exhibit better results than those derived from collocated satellite and nearshore buoy data. For instance, when considering buoys moored off-shore, the wind speed and direction correlation values increase to 0.94 and 1. 90, respectively. The rms difference values are about 1.50m/s for wind speed, and 17° for wind direction. For buoys located near-shore (distance from land less than 30km), the wind speed and direction correlations are 0.86 and 1.64, respectively. Such accuracy behavior may has several sources such as scatterometer and radiometer spatial resolutions and the retrieved surface winds at locations near coasts (see for instance Picket et al, 2003).

13. Reviewer: Page 452. In table 4 buoys 2008010 and 3155039 seem to be over land (please control coordinates, I have just checked qualitatively).

- Reply: We have checked the coordinates. They seem right. The following figure indicates buoy locations (indicated by * symbol).

14. Reviewer: Page 452. 'Eˇ 10 m buoy winds are calculated from raw data and 6-hourly averaged..'. At which eight 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 ata 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).

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- Reply: The following sentence is added: Atmospheric measurements are made at a height of about 4 m. For comparison issues, the Liu-Katsaros-Businger (LKB) model (Liu et al. 1979) is used to calculate 10-m wind speeds at neutral conditions.

15. Reviewer: Page 452. 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?

- Reply: As indicated in the document the buoy data are 6-hourly averaged. The latter are available at the four time epochs 00h:00, 06h:00, 12h:00, and 18h:00. No temporal collocations are requested for comparisons to ECMWF and blended 6-hourly wind estimates. Several publications (see for instance references provided in this paper) shown one-hour window is valuable for buoy (hourly averaged) and instantaneous satellite (spatial averaged) wind comparisons.

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

- Reply: Indeed, since 2003 QuikSCAT scatterometer wind data are assimilated in ECMWF. However, we are still wondering if the assimilations of in-situ and satellite data are performed in same way. For instance which kinds of weights are applied to both data? Which kind of covariance and error are considered for each source?.

B. Review 2

NRT Issues

Reviewer: 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.

Reply: As clearly indicated in the paper, the development of the method merging satellite and ECMWF wind data aims to meet MFSTEP and MERSEA requirements. One the main issue related to the forcing data is: "*How to improve NWP outputs with additional observation sources?": routinely and globally available in near real-time, satellite data can be used to improve the forcing fields derived from NWP outputs. This is in particular the case for surface wind, wind stress and turbulent heat fluxes, which can be improved directly by the use of scatterometer high resolution wind data. Although scatterometer wind data are already assimilated in some NWP systems (including ECMWF), the meteorological assimilation truncates generally most of the small spatial and temporal scales, which are critical for ocean dynamics. This priority on scatterometer data is also justified by the availability of global high resolution (25/12.5 km) wind data from SeaWinds (on board QuikScat and ADEOS-2) and ASCAT (on board the METOP satellites) in the 2004-2008 time frame.* Therefore, the paper (first version) aims to provide concise information for MFSTEP and MERSEA groups and users as well as for Ocean Science readers. According to reviewer comments, some technical details are added to the paper:

Reviewer: When, and from where, are the ECMWF background fields made available?

Reply: ECMWF data analysis are extracted from GODIVA data base (`http://www.nerc-essc.ac.uk/godiva`) using operational SOAP (Simple Object Access Protocol) procedure. In general speaking, the data availability delay does not exceed 24 hours.

Reviewer: 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)?

Reply: As indicated in the abstract, NRT satellite data are available at Météo-France. They are pushed to IFREMER using operational ftp procedure in very near real time (time delay from satellite data acquisition is about 4 hours). Most of remotely sensed data, needed for blended wind estimation at 00:00h; 06:00h; 12:00h, and 18:00h, are available within 24 hours.

Reviewer: 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?

Reply: The procedure takes less than 30 minutes including the estimation of gridded wind vector and wind stress.

Reviewer: How big are the files?

Reply: quite small: about 31Ko

Spectral Analyses

Reviewer: 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 mea-

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sured 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.

Reply: We know all the quoted references. Again, the paper aims to provide the first useful information about data, method, and the validation results related to the estimation of blended wind fields from NRT remotely sensed wind observations and operational ECMWF analysis. We agree with the reviewer that spectral analysis is a valuable tool for characterizing the wind spatial patterns. However, such analysis is out of scope of this paper. It only aims to indicate how the resulting blended winds compare to buoy averaged data, and how they are close to the used satellite observations during one month January 2004.

The method and the related algorithms have been used to estimate longer time series of blended wind fields during the period January – December 2005. We have started performing some spectral analysis to investigate blended wind spatial patterns and how they compare to ECMWF. Hereunder, the reviewer will find examples of such analysis. The analysis is performed along the section located at 18°E from 6-hourly ECMWF and Blended wind speeds, zonal components, and meridional components. Through these examples, the blended winds exhibit higher PSD values, especially during winter season and for distance great than 100km. However, and as the reviewer should know there is no straightforward evidences from these analyses. They are related to several factors such used observations, covariance matrix, objective/subjective method, smoothing procedure. Therefore spectral analysis results request further investigations. They are expected to be published in future.

Other Scientific, Technical and Operational Concerns

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

Reply: The main aims of the accuracy study (section 4) are to investigate how the blended winds are close to the remotely sensed wind observations, and to assess the quality of the blended wind estimates through comprehensive comparisons to buoy 6-hourly averaged wind speeds and directions. The quality of NRT satellite data is investigated trough comparisons to off-line data (section 2). The accuracy methods cited by the reviewer are efficient to characterize the error of SVW on satellite swath (Level 2 product). Such methods are based on statistical assumptions of error variables that is hard to verify for this study. It is obvious that the 6-hourly averaged ECMWF, blended, and buoy winds are not independent.

Reviewer: 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.

Reply: In section 3, it is indicated that background winds (from ECMWF analysis) are interpolated in space and time over satellite swath (Page 444; Line 14). The remotely sensed wind observations are used without any averaging in space or time. Buoy data are used for covariance matrix determination.

Reviewer: 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

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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.

Reply: The following sentence is added in sub-section 2.2 : "One can notice that wind directions are not retrieved from SSM/I measurements".

We agree with the reviewer about the importance of wind direction (or wind components) and the related parameters such as wind curl and divergence. However, the wind speed is still an important factor for users whatever involved in ocean modelling (global or regional) and/or in ocean-atmosphere interaction studies (Flux determination, process investigations, . . .) . See for instance (Blanke B. ; S. Speich, A. Bentamy, C. Roy; B. Sow, 2005 : Modeling the structure and variability of the southern Benguela upwelling using QuikSCAT wind forcing J. Geophys. Res., *Vol. 110, No. C7, C07018*). Moreover, the paper provides statistical parameters characterizing the quality of blended zonal and meridional components (see Tables 3 and 5 as well as figures 5, 6, and 7). It is expected to perform more analysis of resulting blended winds including wind vector, wind stress, and the related parameters using a long time series (January 2004 – April 2006).

Reviewer: 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 _o, _b, for the Xo and Xb terms)?

Reply: Thank you for this information. No there are no particular issues for the objective method.

Reviewer: 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.

Reply: This is completely right. The paper aims to show the feasibility of the method and the expected quality of the resulting blended wind fields. Even if the study deals with blended wind field estimated from data collected during January 2004 in, the spatial and seasonal behaviour of the variograms were investigated (see page 448, line 4 and line 25). Using longer time series, we started performing some tests to investigate the impact of the regional and seasonal variogram variability on the resulting fields.

Reviewer: 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 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).

Reply: The reviewer is right. Due to the instrument physics and specification, we cannot expect strong influence of QuikSCAT on nearshore wind accuracy and resolution. However, some influences, related to the spatial and temporal wind variability, are there and induce differences between blended and ECMWF analyses. In collaboration with colleagues involved in oceanic and/or wave modelling (MERCATOR, ROMS, WAVEWATCH III) we expect to point out the impact of the nearshore blended winds and to investigate their improvement over such regions (see sampling scheme impact: page 450, line 3 through 13).

Reviewer: Equation 1; change the subscript on second X.

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Reply: Thanks. It is done

Reviewer: Equations 2,5,6,8,9,11; what is N?

Reply: The following sentence is added : *ta, tb are the beginning and end time of the analysis. N is the number of available and validated observations.*

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

Reply: It is done.

Reviewer: 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".

Reply: Equations (5) comes from equation (2). Equations (6) is deduced from (5). Please see the corrected version. Thanks. $Var(\hat{\varepsilon}(M_0))$ is the second term of the right hand of equation (4).

Reviewer: Equations 8: check subscript on left hand sides in both cases

Reply: Thanks. It is done.

Reviewer: Equation 9; check subscript on λ.

Reply: Thanks. It is done.

Reviewer: Equation 12; should there be a subscript or superscript on the second _?

Reply: It is corrected; Thanks

Reviewer: Equation 13; should the square be outside the parentheses?

Reply: OK.

Reviewer: Equation 18; subscripts on d need repair.

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Reply: Thanks.

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