Ocean Sci. Discuss., 3, S373–S375, 2006
www.ocean-sci-discuss.net/3/S373/2006/

Ocean Science Discussions

Interactive comment on "DINEOF reconstruction of clouded images including error maps. Application to the Sea-Surface Temperature around Corsican Island" *by* J.-M. Beckers et al.

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Received and published: 1 September 2006

I wish to provide a constructive contribution to the discussion concerning the paper submitted to OSD by Beckers et al. Although my experience mainly concerns active sensors, specifically satellite radar altimetry, the paper drew my attention with reference to the region of the Mediterranean Sea chosen by authors to test their methodology.

It is well known that this region is prone to cloud cover and substantial gaps are often observed in the use of passive forms of remote sensing. The authors address the problem of reconstructing gappy SST maps starting from the Pathfinder dataset. They propose an extension of the DINEOF method capable of providing an error estimate.

The quantification of the associated error helps users understand how much confidence they should have in the reconstructed maps and whether the data are appropriate for their applications. This is an important aspect for better decision making by data integrators. The focus on the error fields rather than validation should be better stressed in abstract and sections (considering that the authors state they deeply addressed validation in another paper). Nevertheless, in my opinion, as also suggested previously by Rixen, the readership would expect some quantitative results from the validation in the study area. The idea of using the pathfinder data set itself, e.g., simulating artificial pixel gaps, is certainly a potential way which would be less time consuming at this stage of using external data sets. The validation exercise might be synthesized extracting some statistics from the previous simulations and also providing a comparison in terms of color maps.

Concerning the chosen region, there is no mention why authors selected it (has the region peculiar aspects related to the methodology? Is the region of “opportunity” (e.g., ready availability of pathfinder data set)? Or is really a case-study for future applications?. In my opinion a short statement should be provided at the beginning of section #6.

I would like to add suggestions when authors present the region (section #6) from an oceanographic point of view. I would add the term “permanent” to the basin-wide cyclonic circulation involving the Liguria Sea. Moreover, while the fact that is more intense in winter is reasonable as observed by Larnicol et al. 2002 using satellite altimetry (and in turn induced looking at ECC and WCC water flow observations), the statement that this structure would be driven by “mainly” wind stress should be proved (and this does not happen in Larnicol et al. 2002 Ĕ. Please see for discussion on this aspect the following papers: Vignudelli et al. 1999, GRL, Vol. 26, NO. 5, 623-626, 1999 and Vignudelli et al. 2000, JGR, Vol. 105, NO. C8, 19,649-19,664).

I would suggest adding that the Northern Current flowing westward along the coast then completes the cyclonic loop.

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With reference to the large cyclonic circulation it is important to mention that three different regions can be distinguished (coastal periphery, frontal and central). This is generally well reflected in the SST maps.

You should also mention the dipole structure (anticyclonic/cyclonic) usually observed in the northern part of the Tyrrhenian Sea and excited by the wind coming year-round eastward from Strait of Bonifacio (e.g., for a review see Astraldi and Gasparini, The Seasonal Characteristics of the Circulation in the Tyrrhenian Sea, in “Seasonal and Interannual variability of the Western Mediterranean Sea”, Coastal and Estuarine Studies, AGU, vol. 46, 115-134, 1994).

Finally, the paper calls for some revision (which should also account for comments of the other people that I fully agree), however, I am optimistic that making appropriate changes and integrations it deserves to be published.

Interactive comment on Ocean Sci. Discuss., 3, 735, 2006.