**Ocean Science
Discussions**

Interactive
Comment

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.

J.-M. Beckers et al.

Dear Damià,
Thank you for your thorough and very constructive review. In preparing a revised version we will take into account your comments as follows

Pag. 741, expression (13): as it is written it seems that by truncating the EOF expansion at a number N one exactly eliminates the actual noise (the variance of which is defined in (7)) and keeps the true signal from the original data set. Obviously this is not ensured by the method, otherwise you would have a perfect method !. I would suggest to state here (instead of after (17) and prior to (18)) that you assume that the first N EOFs contain signal and the remaining EOFs some noise, but stating also that this

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'noise' does not necessarily match the actual noise in the sense of the OI definition (7).

Response You are right, some of the rejected EOFs could contain signals which the method is unable to reconstruct when adding artificial clouds during the cross valida-tion. On the other hand, when we look at the singular values of the rejected EOFs, their spectrum is flat and no other useful signal seems to emerge. We will adapt the text according to your suggestion

Pag. 743, l. 15: when introducing (18) it could be useful to remark that the assumption of uncorrelated errors (i.e., that $R = \mu I$) is a strong one in the case of satellite data, and that this point will be addressed in section 6.3.- Section 6.3 (pag. 754): the way of circumventing the problem of considering a nondiagonal error matrix R leading to (51) is not clear to me (but perhaps it is clearly explained in Barth et al., 2006).

Response Right again. We will announce the problem earlier and explain the approach to deal with a nondiagonal matrix R as in Barth et al 2006; An analysis experiment is performed with non-diagonal matrix R (and hence correlated errors at a prescribed scale L). Then the analyis is repeated with a diagonal matrix where the diagonal is, compared to the non-diagonal version, inflated by a factor r . It turns out numerically that when the inflation factor (52) is used, both approaches are closest.

Still in section 6.3 (pag. 755, l. 26-27): it is stated that the correlation length of the SST anomalies should be larger than the correlation length of the observational error. This is usually the case for in situ measurements, but what prevents satellite data to be contaminated by large scale noise, e.g. derived from atmospheric corrections applied to raw data ?.

Response: You are still right. The problem becomes more and more difficult when the errors have similar structures or scales than the data. To separate the two is then almost a mission impossible. We will add a comment in the text

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Related to the previous point: the value of the correlation length of the SST anomalies is computed by fitting an exponential function exp-d/L to the correlation of SST data. Thus, this value would be different if the fitted function was different (e.g., a gaussian exp-d2/2L2). Is there any way of ensuring that these definitions of L are consistent with the L appearing in (52) and (53) ?

Response In reality we estimate L simply by the distance where the correlation is reduced by a factor e. It happens that the correlation function resembles an exponential and the estimate is coherent. We used this simple approach since the correlation curve appears not to be affected by much noise. You are right, the exact value of L depends on the different correlation functions but since equation (52) and (53) are approximations anyway we contented ourselves with an approximate value of L

Section 6.4 (pag. 759): why not using an artificially clouded image, so that the results of DINEOF, OI and the error maps can be checked against actual values ?. Moreover, the chosen case does not seem the most adequate, in the sense that the cloud coverage is somehow related (in shape) with the spatial structure of the actual field (or at least with the spatial structure of the recovered field).

Response: You are right, the case is one in which clouds have a shape similar to the recovered field. We will take an additional image to show that this is not an artefact. Concerning the artificial clouding, this is an approach we used in Alvera et al 2005 to validate the analysis itself. For error fields, it is more difficult to visually compare an actual error field to the error bars and we will probably have to resort to hypothesis testings.

Pag. 759, l. 18-20: it is stated that "It is unlikely that an OI method using an isotropic and homogeneous error covariance would be capable of reconstructing the Northern Current in a situation where very few data are available". This is true, but it is also true that when OI is used to fill data gaps in satellite images, the correlation function is usually not taken as homogeneous and isotropic. Satellite data are precisely one of

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the few data sets that allow to use point-to-point (i.e., non-homogeneous, non-isotropic) correlation functions.

Response We will add a comment in this sense, highlighting the fact that our approach uses similar point-to-point correlations with the strong advantage of being able to factorize the resulting covariance matrix so that the costs are reduced drastically

Pag. 760, l. 5: it is stated that "the SST on 30 December is notably colder than on 26 December". How do you know it if the image of December 30 is completely clouded to the west of Corsica ? (are they real clouds or artificial clouds so that you know the actual values ?).

Response: Clouds are all real and at least for the unclouded pixels, temperature clearly has dropped east of the Strait of Bonifacio. We will adapt the text to refer to this temperature drop.

Other minor points:

Response Typos will be corrected.

Pag. 741, just before (13): should the upper index be m instead of n ? :

Response: it does not matter because an SVD decomposition has at maximum min(m,n) non-zero singular values

Pag. 747, l. 1: when stating the equivalence between (29) and (30) it would be helpful to send the reader to the Appendix (the reduction of the dimension of the problem stated in (29)-(30) is a crucial point) **OK**.

Pag. 756, l. 12-15:: from the given values, the factor between the internal radius of deformation and its associated wavelength is about 6. I have seen other works setting this factor to 4, since the wavelength is considered equal to 2 times the diameter of the structures. Any comment on this ?

Response: For a baroclinic instability and the standard definition of the deformation

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radius, the most unstable wavelength is found around 6 times the deformation radius. Different factors arise for different base currents and/or different definitions of the deformation radius, but the order of magnitude "several times" the deformation radius remains. I think we will just add a reference to Cushman-Roisins GFD book.

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

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