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Interactive comment on "Multi-scale optimal interpolation: application to DINEOF analysis spiced with a local optimal interpolation" *by* J.-M. Beckers et al.

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I read this paper with great interest, and I think it contains novel and relevant results, surely deserving publication in OS. In particular, I found all section 2 quite original and instructive, viz. the theoretical aspects related to the optimal interpolation of multi-scale processes and the new related methodologies proposed. On the other hand, I must also say that the way the paper is organized does not help the reader to get a conclusive message and clear vision on the methodologies presented. Basically, I feel the authors describe a huge amount of work that would require more than one paper to be fully acknowledged, if all considered crucial. On the other hand, I think that part of

what presented could be completely removed (and part of it eventually moved in appendix), focusing only on the main results. A few important aspects would also require clarification, especially concerning the differences between purely spatial interpolation and space-time interpolation approaches, which presently look a bit confusing. More detailed comments follow:

MAJOR COMMENTS:

(1) Section 3. I do not see a particular interest in discussing the ability of the methodologies to retrieve a single process and suggest concentrating on the overall signal. Moreover, as far as I understand, the Kalman gain matrixes for each of the two processes are assumed known (i.e. could be directly estimated for each process separately), and it is not clear/discussed how the different length scales/Kalman gain matrixes can be extracted in realistic cases (true fields potentially include more than two scales, and are not known a priori). It would also be interesting to see a discussion on how eventual errors in the definition/extraction of the dominant scales from true observations could affect methods' performances. In fact, many of the methodologies seem to lead to extremely small differences and, despite following a Monte-Carlo approach, significance of error differences is not explicitly reported in the text (i.e. as confidence intervals). Finally, as presented right now, the whole section only considers processes characterized by different spatial scales and all interpolation algorithms are only spatial (see also comment (3) below). I suspect that the conclusions could probably be extended to higher-dimensional cases, namely thinking of grid spacing as generalized distances, and suggest the authors consider these aspects more carefully in their discussion.

(2) Section 4. The theoretical derivation in section 3 is based on multi-scale optimal interpolation, namely searching the combination (iterative or not) of single process Kalman gain matrixes that better approximates the optimal field. In this section, the large scale OI is substituted by a new DINEOF-based algorithm. Though I have no major concerns on this, in terms of practical applicability, I wonder why the authors did not start by considering only a standard OI as a first step. In fact, the authors introduce

a modification of their standard algorithm, originally based on purely spatial reconstruction, by iteratively combining spatial and temporal EOFs. This modified 'inner' technique by itself would require a more detailed validation and/or a more detailed discussion, even considering that DINEOF, in my view, does not necessarily reconstruct only large scale features (recurrent small scale features, e.g. related to coastal/bottom topographic features might explain a large fraction of the covariance in specific areas). Moreover, given the authors themselves adopt a space-time OI approach for K2, it is not straightforward to understand why they did not explore first a similar approach also for K1, and go to the DINEOF-based one as a second step, if they still see it as an advantage, as it might eventually be, either in terms of accuracy or in terms of computational efficiency. As said, it is also not straightforward (and should thus be commented) that conclusions from a purely spatial analysis (as in section 3) can be directly extended to space-time interpolation. Then again, see comment (3) below.

(3) As a more general comment, I would really like to see a discussion on the impact of concentrating on spatial interpolation alone with respect to considering space-time covariance models. In fact, approximated OI approaches using analytical (parametric) covariance functions are easily extended to higher-dimensional state vectors, e.g. including also temporal decorrelations (or even higher dimensional spaces as, for example, in Buongiorno Nardelli, JTECH 2012). These functions clearly decay at increasing generalized distances, and allow excluding from the analysis the observations that are found far from the interpolation point, which makes the algorithms theoretically sub-optimal, but computationally efficient. On the other hand, these more complex models, even if solved in a sub-optimal way, might better describe the system evolution than models based on simple spatial covariance. Actually, in 'truly optimal' space-time interpolation, aiming to get interpolated fields from satellite images, one should effectively consider a temporal sequence of the images as the observation vector, and several realizations of these space-time data should be used to estimate the covariance. This would clearly lead to a huge (computationally unfeasible) matrix inversion in the OI. By the way, I think that, if the authors prefer to avoid sub-optimal approaches, this can be

done more easily with DINEOF, e.g. building the observation vector as a sequence of daily/hourly images and applying the SVD on the most convenient observation matrix dimension.

MINOR COMMENTS:

Introduction. "In some situations it appears however that the truncation of the EOfs series rejects some interesting small-scale features by interpreting them as noise (Sirjacobs et al., 2008). This is due to the fact that under clouds the method is not able to recreate those small-scale features using EOfs only and therefore globally rejects small scales by the EOf truncation." I suggest rephrasing as: "In some situations, however, the truncation of the EOFs series can reject some interesting small-scale features that only give a small contribution to the total variance, and that can often be split in several modes (Sirjacobs et al., 2008). This is due, on one hand, to the fact EOF truncation is related to the percentage of variance that would be associated with noise and, on the other hand, to the limits of EOF decomposition itself in identifying evolving mesoscale features in a single mode (actually, any feature propagating across the spatial domain is split in several modes)."

Section 5. I suggest to move the details on how the K2 decorrelation scales have been computed in an appendix. The differences between the application of the various methodologies to the whole MED or to the Western sub-basin could be discussed more clearly in the text. Finally, I found the front detection analysis very distracting and not really focused on the main subject of the paper. In fact, I think it does not provide robust indications on the methods performances, as it remains very qualitative and it would require a better definition of what is meant by 'front', as well as a discussion of the limitations of the front detection algorithm itself.

Interactive comment on Ocean Sci. Discuss., 11, 895, 2014.