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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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Response to reviewer 1 (*reviewers comments in italic*)

We thank the reviewer for the constructive comments and questions which we hope will help us improving the paper. Before responding point by point we can note that several comments are related to covariance modeling questions. There is a large literature on how to model and calibrate covariances (parametric functions, reduced rank representation, ensemble approaches, smoothness or diffusion operators, see for example Gaspari and Cohn 1999, Weaver and Mirouze 2013) and the purpose of the present paper is not to defend or propose one particular version. So the specification of the covariance model is secondary which is also coherent with the reviewers comment to move the calibration of covariances to the Annex.

So we start with the premise that the covariances have been chosen and focus on the case when it is a combination of two representations which do not lead to a direct solution method for the analysis step.

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:

We tried not to multiply the number of papers by cutting it into pieces but wanted to include sufficient experiments to validate the approach, which might have lead to the impression of too many information. We will try to streamline according to the responses to the detailed comments (see below).

(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.

We respectfully disagree, scale separation is of interest for a large number of people and Fourier methods or wavelets are certainly used to separate scales. Here we can provide analysis of the different processes if the covariance model is correctly chosen.

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It is then worth investigating which iterative strategy not only provides the best overall analysis but also the best individual fields.

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.

This is a problem of covariance specification which is not the focus of the paper as it is strongly parameter, region, and scale dependent. Adding the proposed test or discussion will even add more discussions as we will have to decide on what is a typical error on the covariances and see its effects. It is also a general problem of optimal interpolation, not specific to our approach. Our calibration approach is also not more complicated than in single scale approaches as it is done on each scale with a single EOF based or parametric covariance function. Because we calibrate one after the other, the method provided quite robust results even when providing incorrect calibrations in the first step; indeed the second step (calibration of local OI covariances on residuals) to some extent corrected for the incorrect calibrations of the first step (see also original manuscript p 916 l 25ff)

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).

Indeed the differences are sometimes small, but the ranking were always extremely robust so that conclusions are in our opinion very strong (the probability that you get the same ranking for different test cases with non significant differences is quite low). Furthermore, with 100000 independent realizations we should roughly get 5 significant

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digits on the relative error variance we show. (To clarify we will add into the table everywhere the information that we look at the average relative error variance based on 100000 independent realizations)

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.

We agree that adding time is just adding another dimension with some specific scale and conclusions should hold. As the conclusions drawn from the spatial interpolation problem also lead to an efficient and convergent algorithm in the realistic time-space case of Section 4, we do not think we need an additional experiment (further increasing the paper scope) but we will add a small discussion and references.

(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.

There are several reasons why we did not use only a standard OI as first step a) if we can solve the problem with standard OI for large scales, then adding a small scale covariance does not change the computational burden and the problem is solved. This situation (see l 23 p 838 of the discussion paper) does not need to go through a new algorithm. b) using EOF from DINEOF to construct the covariance is still an optimal interpolation with just another choice of the covariance model, which by the way require less parameterizations (and discussions) than the standard parametric function approach.

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Interactive Comment

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 is not entirely exact: the original DINEOF reconstruction uses simultaneously time and space dimensions (exchanging the time and space dimension would lead to exactly the same results). Only the error field deduced relies on the interpretation of the reconstruction as if done by spatial covariances only (as DINEOF itself has no direct error estimate, see discussion in Ocean Science paper doi:10.5194/os-2-183-2006). But indeed we needed to reformulate the analysis step for residuals as applying DINEOF to residuals would generate new covariances from the residuals instead of using the covariances from the large scale processes. In order to make this clear for the readers we will more clearly explain the revised DINEOF version used here for the residuals.

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).

If we understand correctly we need to better justify the covariance model we have chosen for K1. It is just a simple combination of the idea: a point is correlated in space to all other pixels at the same moment with the temporal averaged data correlation. Similarly the correlation in the time direction can be estimated by data correlations from statistical averages in space. So the combination of the two will exploit time and space correlations.

For the second part of the comment, it is true that EOF might contain small scale features, but the parametric OI is calibrated on the residual (observation - OI analysis with EOFs). If the recurrent small scale structure are already captured by the OI analysis with EOFs, then the residual will be small and not deteriorate the work already done by

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OI analysis using EOFs. But again this amounts to a covariance model choice

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.

See response above

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.

See response above

(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 suboptimal, 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

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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.

We are not certain which discussion we should add compared to the previous ones already mentioned above concerning generalisations from space to time-space. There are two different problems: a) the theoretical specifications of time-space covariances and b) the practical and sometimes suboptimal numerical resolution. Here we showed how we can exploit large scale covariances together with small scale covariances in a feasible way without the need to explicitly invert huge matrices. This was possible by a particular choice of the covariance model and the new iterative method we proposed.

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)."*

We will use the suggested rephrasing but finish the phrase with "under clouds", because when no clouds are present adding enough EOFs will allow to capture the movements.

Section 5. I suggest to move the details on how the K2 decorrelation scales have been computed in an appendix.

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Ok

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.

We tried to squeeze the information into a table rather adding long phrasings but we will add some more description

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.

Ok we can drop this and just leave a general comment on the sometimes beneficial filtering effect of DINEOF.

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