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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 2 (*Reviewers comments in italics*).

With great pleasure I read this long-awaited paper on combining the two important techniques of generating gapless spatial information from sparse remote sensing data: Optimal Interpolation (OI) and Empirical Orthogonal Function (EOF) fitting. I am impressed with the complex theoretical framework presented to put DINEOF into an overarching OI framework, allowing taking overall error estimates into account which are theoretically not part of the DINEOF approach. The different approximation methods of handling the combination of OI and DINEOF are well treated, showing the merits

of the theoretical framework. This work definitely deserves publication OS. However, I have some recommendations on the way the results are communicated to ensure proper uptake in the relevant communities. My main suggestion is to simplify and/or shorten the paper, to allow readers without too much mathematical background or time or to grasp the essentials of the paper too, yet at the same time being complete for readers with solid mathematical background and sufficient time. I have a couple of suggestions for that.

We thank the reviewer for having taken the time to analyze the paper's content and we will try to take into account his suggestions for simplifications as follows:

First, I suggest simplifying naming conventions by renaming them, or actually simplifying them. The number of available methods treated is a bit too much to illustrate the essential message of the paper: to combine OI and DINEOF in a theoretically solid manner. There are too many permutations due to the number iterations (0,2,10 vs. 20) x process order (1 vs. 2) x theoretical approximations (K1, K01, DINEOFOI vs. DINEOF + K2). The result is dazzling to a reader new to this matter. For instance, the cases in Table 5 (0 and 10 it) do not match the ones in Table 4 (0, 2 and 20 it). Also the meaning of subscripts varies (in K1, the 1 refers to a process while in K01, the 1 refers to a gain matrix formulation) forcing the reader to go back to previous sections all the time. Perhaps it would be an idea to add one extra table with the relevant equations that match the labels in Table 5, and add textual labels in addition to only operators with indices.

We will try to find a better naming for the methods and add a table with the naming conventions. As far as iterations are concerned, the 0,2,20 iterations were used in the schematic case for finding guidelines and getting a feeling on convergence of iterations, whereas in the realistic case is was only used to check if iterations actually improve results. As improvements in the latter case were relatively small we did not investigate further the effect of changing the number of iterations (also in order not to overload the tables). If we move the results and tables of section 3 into the appendix (as suggested

OSD
11, C338–C342, 2014

later), there is less need for the tables to match.

Second, I tend to suggest removing treating the order of process 1 and 2 from the paper. First, mathematically these methods are identical anyway ("Similar relationships hold for K2 (as we can just interchange indices 1 and 2)." line 21 page 899). Secondly, in your conclusions you suggest to use 1 for the signal with the highest signal-to-noise ratio, which would be an obvious choice anyway. This complex paper might be greatly simplified by stating this as an assumption upfront, and moving all material to support this assumption to appendix or even supplementary material (keep it for reader with enough time).

As we tried to highlight, mathematical methods are only equivalent if the inversions are done exactly. With a limited number of iterations results are not identical anymore. We will indeed only retain the slightly expanded conclusions/guidelines of Section 3 and move the results and tables into the appendix.

Third, your conclusions, and uptake of your DINEOFOI, might benefit from a recap discussing the different approaches to combine EOF and OI in general terms, without using codified information such as P1a and P2b in the conclusion, thereby making reading the conclusions possible without reading the body of the paper first.

We will indeed revise the paper so that a recap discussion could be read without needing to remember the definitions of specific methods. This will certainly help the readability.

The conclusions might also benefit from focusing more specific on the novel idea of combining DINEOF and OI and its benefits, instead of an abstract description of combining two "combinations of analysis tools". The same applies to the abstract, where DINEOF and OI are currently mentioned as a mere examples, instead of as the main finding. Is the theoretical framework the main message, and DINEOF-OI a mere example (as it is currently written), or is DINEOF-OI the main message, and the theoretical framework a mere justification (what I suggest)? The latter would allow the last

OSD
11, C338–C342, 2014

paragraph of the discussion to suggest using the theoretical framework also for cases beyond DINEOF-OI.

In our opinion the generality of the theoretical framework is more important than the specific improvements for DINEOF, which is also somehow presented in the title but which we would adapt further to "Multi-scale optimal interpolation by combining single process analyses: example of DINEOF analysis spiced with a local optimal interpolation". Or in other words, while DINEOF users will certainly be happy to have access to this new possibilities, the general finding is hopefully relevant for a larger community which we target with the paper.

Fourth, the theoretical framework plus the examples represent an enormous amount of information which makes this paper very time consuming to ingest. A suggestion to show the merits of this work would be to re-use suitable test examples from previous OI and DINEOF publications from your group, and show how the new DINEOFOI variants improves these tests. This would reduce the amount of new material in this paper, allowing readers to focus on the DINEOF-OI combination as new material. Plus it would also provide good evidence that the new method is indeed better than simply DINEOF or OI alone as in previous papers, with the OI addition to DINEOF superior in representing fine scales than pure EOF modes.

This is a valid suggestion but would demand a full rewrite of the realistic test case description and would only help readers which are well aware of our previous works with DINEOF. Since we aim at a more general theoretical contribution we took a test case with sufficient large scale and small scale features to test the methodologies and would prefer to keep the approach. The fact that DINEOF+OI works better than previous approaches is in our opinion sufficiently shown in the metrics and tables we show.

*Technical corrections: * Shouldn't (DIN)EOf be (DIN)EOF? (small f, e.g. page 897) * Is subscript i is missing for second (Eq. 1)?*

OSD

11, C338–C342, 2014

DINEOf corrected but Eq.1 is correct in its original form.

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