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Ocean Science Discussions

Interactive comment on "Ocean state indicators from MyOcean altimeter products" *by* L. Bessières et al.

Anonymous Referee #1

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General comment: The paper is well organized and clearly written. The subject is interesting and I think the paper should be published provided some modifications and a few clarifications are introduced. As a preliminary concern, I would like to understand why the authors found it necessary to build some of the new indexes based on box averages that mainly seem to reproduce EOF amplitudes, instead of directly projecting observations (even observations that were not used to build the EOF base) on the relevant modes. Then, I would like to stress that 'product quality assessment' and 'assesment of the impact of using products with different accuracy levels for the ocean state monitoring' are two very distinct concepts that should not be confused. More detailed comments are thus included in the following:

Section 1 I don't think it is a good idea to mix monitoring issues with validation issues, unless a more comprehensive description of how this can be done is presented. In this context, I feel the word "intricately" (line 24, pag. 2083) is not particularly suited to describe the altimeter data processing, giving an idea of un-necessary complication and unclear distinction between the various processes involved. I guess the authors here really mean 'intimately' (or something similar). Then, I would suggest modifying the paragraph restricting the main aim to the development of indicators, or adding more details on how the quality of altimeter data could be assessed through the indicators (which could probably be done only by comparing them to additional/complementary information from other sensors/platforms or models, though weakening the significance of the new indicators by themselves).

Section 2 Though equation 3 can be formally correct (in the end it is just a matter of definitions), the way the EOF decomposition is written (namely including the eigenvalues in the reconstruction of the observation vector), together with some unclear/incorrect wording in the text, may be misleading. More specifically, the paragraph saying that "For each mode, the eigenvalue gives the relative contribution of the mode with respect to the full signal whereas the eigenvector shows the spatial distribution of the variable associated to a time series, called Principal Component (PC) Time Series" is not clear and should be modified. A more clear definition would be: "The eigenvectors are chosen to be orthogonal and built to account for as much variance as possible. For each mode, the eigenvalue gives the relative contribution of the mode to the total variance, whereas the eigenvector shows the associated spatial pattern. The projection of the observations onto the eigenvectors gives a time series for each mode, called Principal Component (PC), which provides the contribution of that mode to the observed field at each instant. In this way, long time series can generally be analysed by looking at a few independent modes explaining a high percentage of the variance.

I would then remove lamda(i) from eq. (3).

Section 3.1 I suggest adding some references.

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Section 3.3 The motivation for using DT vs NRT should be better discussed, possibly avoiding qualitative adjectives as 'best' or 'slightly lower'. Given the authors introduce this issue, it might be interesting to know if and how the indicators would be affected by using one product instead of the other. Moreover, it is not clear if the authors used the 'reference' or 'updated' products (i.e. if they are using a consistent dataset in terms of data coverage/sampling). More details on how the preliminary temporal filtering of the data has been performed should be added, even considering that another temporal filtering is applied at a later stage (3-month smoothing). It would be nice to see a discussion (at least some comments) on the physical reasons driving the observed differences between the SLA based and SST based indexes and also on eventual advantages in considering these differences in order to better describe the ocean state. Actually, SLA data reveal changes in the steric and eustatic components of the sea level and it might be interesting to know (for example) if the authors think that the new index may identify changes to the stratification better than the SST index alone (if the steric component is assumed to dominate the variability at these scales).

Section 5. I would suggest reducing section 5.1 to approx. the same size of corresponding sections for the previous indexes. This could be achieved by significantly shortening the review of the proposed mechanisms of origin of the decadal oscillation, which distracts the reader from the main objectives/topics of the paper. As the first mode obtained from the 13-month filtered SLA is actually described and discussed in the text, I suggest including the corresponding figure. In the corresponding text, it is not clear what trend is being analysed.

More in general, I would suggest to reduce section 5.2 trying to avoid comments on results that are not shown/not particularly relevant to the main results.

Conclusions Please see the general comment and comments to section 1.

Interactive comment on Ocean Sci. Discuss., 9, 2081, 2012.