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Ocean Science
Discussions

Interactive comment on "Evaluation of real time and future global monitoring and forecasting systems at Mercator Océan" *by* J.-M. Lellouche et al.

Anonymous Referee #1

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The authors present a description of two analysis/forecasts systems developed at Mercator Ocean. They then present a series of comparisons between analyses and observations. I find the description of the model and data assimilation systems too brief to benefit the reader. Many important details are left out. I identify specific details that I would like to know below. The evaluation of the model is generally appropriate. However, the authors mostly present comparisons between analyses and observations, not forecasts and observations. The comparisons that do include forecasts (eg Figure 15) are generally not favorable. I would like to see more comparisons with the forecasts. The English is poor throughout the paper, often very loose, and occasionally misleading. I include some examples below, but there are many more. I recommend that a

native English speaker review this document. This paper is worthy of publication, but several aspects of the paper and analysis, detailed below, should be considered first.

The most concerning aspect of this paper is the data assimilation. The authors say "the analysis is performed on a reduced horizontal grid (1 point every 4 in both directions) . . ." (page 1129, line 24). They also state that they "use a weighting function which sets the covariances to zero beyond a distance defined as twice the local spatial correlation scale" (page 1129, line 16). The correlation scales are shown in Figure 2. Both the zonal and meridional length-scales are less than 100 km for most of the globe. I presume a smooth localizing function is used (although this detail is not included in the paper), so even data that is 100 km from a grid point is down-weighted. So, for the $\frac{1}{4}$ degree model, analyses are computed on a 1 degree grid; and data influence is set to zero beyond about 200 km, and down-weighted 100 km away (i.e., the next grid point in latitude). It sounds like this configuration is close to one-dimenstional assimilation for each grid point. The horizontal spatial structures of the ensemble appear to be almost completely discarded by the localization. The authors might consider commenting on this in a revised version of the paper.

Specific comments

Data assimilation: Page 1129, line 6: Can the authors be more precise about how they construct the anomailes that underpin their data assimilation? They describe it as "anomalies . . . with respect to a running mean so that they can give and estimate of the 7-day scale error . . .". To be of value to the ocean forecasting community, a precise formulation/definition would be beneficial. Page 1129, line 22: How are the correlation length- and time-scales computed? What are they based on? Page 1129, line 24: What is the impact of producing analyses on a 1 degree grid instead of the native $\frac{1}{4}$ degree grid of the model? Page 1130, line 4: What are the details of the "balance operator". I presume a geostrophic balance is involved – what happens at the equator? Page 1130, line 7: I understood by oral presentations from researchers from Mercator Ocean, that the initialization approach was a "double backwards analysis

increment". Has that been abandoned? Page 1130, line 10: Is there a reference for the bias correction? The details in this paper are insufficient to stand alone. Page 1130, line 23: The concept of "pseudo-observations" is clear. Where and when are these applied? Based on what criteria?

Loose and sometimes misleading language: Throughout the paper, the English is often poor. Page 1123, Line 1: At first glance, the "evaluation of a future monitoring and forecast system", as the title suggests, would seem an impossibility. Page 1124, Line 15: The abstract says that the paper shows how the "validation impacts on the quality of the systems". The authors clearly mean that the paper shows how refinements, or adjustments to the system based on validation impacts on the quality of the system". Validation itself is passive – with no direct impact on quality. Page 1125, Line 26: ". . . Mercator Ocean, which is in charge of the global ocean . . ." is a poor choice of words. The mean the ". . . Mercator Ocean, which is responsible for the global ocean forecasting efforts under MyOcean . . .". I note that Mercator Ocean is neither "in charge" of the global ocean, nor are they the only forecast center responsible for global ocean forecasting. They merely take responsibility for this under a European Project. Other forecast centers within Europe, and outside of Europe also produce global forecasts. Page 1126, line 2: "What is meant by "It is declined in different configurations."?

Page 1126, line 11: Add reference for the GODAE OceanView "regions".

Page 1126, line 6: The "global intermediate resolution" system is defined as the IRG – GIR would be logically more correct, and would be consistent with the definition of the "high resolution zoom" as HRZ.

Page 1134, Section 3.2: It is not clear shy this section is included in this paper. It adds very little.

Page 1136: The email quails@mercator-ocean.fr does not work. Does this email exist? I emailed it, but my email could not be delivered.

Page 1138, line 19,20: "What does the "best analyses" refer to? Do the authors simply mean "analyses".

Page 1140, line line 5: "all the systems were closer to observations than climatology". An alternative formulation to the Skill Score in equation (2), has climatology as the denominator. The authors might consider this metric to confirm the above statement.

Page 1140, line 18: Regarding comparisons with ". . . OSTIA observations (not assimilated) . . .". Although OSTIA SST observations are not assimilated, the data used to construct OSTIA (noting that OSTIA is an analysis, not observations), including AVHRR and AMSRE data, are assimilated by Mercator. So these comparisons are not independent as implied by "(not assimilated)".

Page 1143, equation (1): could be simplified by simply using speed, instead of the absolute value of velocity. It is really the mean relative speed bias, not velocity bias.

Page 1143, line 20 and Figure 13: Why do the authors not show maps of the RMS residuals. The mean of the residuals is interesting, but it is only part of the story here.

Page 1146, line 17: Where is "the OVIDE repetitive section"?

Interactive comment on Ocean Sci. Discuss., 9, 1123, 2012.