

Interactive comment on “2-D reconstruction of past sea level (1950–2003) using tide gauge records and spatial patterns from a general ocean circulation model” by W. Llovel et al.

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SUMMARY

This paper intends to improve upon previous studies of the so-called sea level reconstruction, primarily using EOFs, (e.g., Chambers et al., 2002, Church et al., 2004, Berge-Nguyen et al. 2008) to use a 44-year (1060-2003) OPA/NEMO ocean model run and tide gauge records (1950-2003) to reconstruct or model the sea level signal from 1950-2003. I agree with Reviewer 1 that the methodology for the analysis has been clearly described, and the results mostly robust. Although I agree with Reviewer 2 who suggests that the authors more clearly or strongly articulate the primary objective of the

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study, which is determination of the regional patterns of the sea-level trend, I found that the conclusion appropriate in just stating the results obtained, which is an excellent reconstruction on the interannual signal, but with decadal or longer signal reconstruction in doubt. Therefore, it is only proper as the authors have concluded - there perhaps is a (large) uncertainty in the determination of the sea level trend variability over the last five decades. I recommend that the paper be accepted for publication provided that the authors address the following comments.

COMMENTS (INCLUDING MINOR COMMENTS)

- The use of the phrases "dynamic heights" and "sea level" are used as the same quantity. Is that true? If not, please provide a clarification. I see that sea level is being reconstructed. An equation may help definitively define the quantities in question.

- ABSTRACT. "...compute spatial covariance signal", what is a "covariance signal"? Please explain, writing down the equation may help.

- INTRODUCTION. "...the main cause of regional variability in the rates of sea level change is non-uniform thermal expansion of the ocean (Cabanès et al., 2001, Cazenave and Nerem, 2001)". Is the statement correct? I thought that looking into more tide gauges (covering more of the ocean surface) that the above conclusion is not necessarily true. The statement may also depend on the analysis data span. Are there other (plausible) causes?

- Analysis method. A mean trend has been removed in all the cases. I understand the logic - I think - however, it is suggested that authors provide more clear explanation why not removing the mean bias in the trend map is a bad idea. Is this technique also should apply the case when the time series in long (40 years)?

- I agree with Reviewer 1 that Figure 6a/6b, Section 5.1, that the similarity between the reconstructed model trend map and the truth (i.e., TOPEX trend map) for 1993-2003, may be over-stated. There are amplitude differences as well as regional variability

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differences. Please perhaps rework the Section describing results of the comparison.

- P. 3, line 3, "Large uncertainties" for "Important uncertainties"
- P. 4, line 10, "or when there are not used" for "or not".
- P. 5, line 12, ICE-5G, which viscosity model, VM2, or VM4?
- P. 5, Section 4., " reanalysis" for "reanalyse"

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