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Interactive comment on "Accuracy of the mean sea level continuous record with future altimetric missions: Jason-3 vs. Sentinel-3a" *by* L. Zawadzki and M. Ablain

Anonymous Referee #2

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This manuscript concerns the uncertainty in bias of mean-sea-level estimates between successor altimetric satellites. It goes on to consider the impact on estimates of trends in mean sea level. This is a worthwhile – indeed necessary – topic for study and publication in due course.

The approach is to use a model ocean reanalysis as the sea-level signal, interpolated to the track(s) of the prior and successor satellite. Noise representing sea-surface-height "errors" is added to the signal. Then the altimetric series of the two satellites are correlated to give a bias estimate; this is repeated many times with changed noise to give a bias distribution.

I am not a specialist in the errors that may affect altimetry for sea surface elevation, but it seems clear that an appropriate specification of these is critical to the results. However, for readers like myself very little is said about the nature of the errors considered (atmospheric effects? Instrument noise and bias? Satellite orientation? . .) or the basis for characterising the errors.

It also seems to me that the trend uncertainty [as represented by equation (1)] is incomplete. It represents the effect of bias but not the effect of finite series length with errors, oceanic variability etc. even using the same satellite throughout. The conclusion might be rather more optimistic against that more uncertain background in that appropriately weighted new information should surely decrease the trend uncertainty.

Here follow some more specific comments, many substantiating the more general points above.

Page 1513 lines 13-21. This seems to prejudge the answer to the question posed on page 1514 lines 5-7.

Page 1515. Line 15. Please say what "errors" you are considering here (c.f. comment on page 1519 lines 16-17). Line 17. "only a few seconds" corresponds to tens of kilometres in which distance I believe atmospheric effects might change significantly. Line 21. "our analyses show the correlation . ." If this is previous analysis, please give a reference. If this is analysis in this manuscript, please don't anticipate. How is it known whether what is being correlated is error or true variability?

Page 1516. Line 2. "considered identical" – not quite, see comment on page 1515 line 17. Lines 6-8. It is important that the added "noise" represents all the sources of difference between the satellites; they are separated even if on the same track. Lines 10-11. It may be reasonable to aim for a similar correlation as between Jason-1 and Jason-2. However, the reader does not have the information about the character of error considered to be able to judge this. And the correlation might be affected by the "few seconds" separation if this is changed from Jason-1/Jason-2. Lines 12-13. Is Za-

wadzki and Ablain (2014, given via a Web address) refereed. If not, some detail should be given here to enable the reader to judge the noise specification. Line 14. Table 1 shows a correlation period 30 days which is very long for any atmospheric effect. Lines 28-29. “over a given location” Indeed, how close does Sentinel-3a necessarily go to any given Jason-2 location?

Page 1517. Line 14. “need to be identical”. This certainly isolates the impact of SSH error decorrelation. But is the impact the same as with different ground tracks (without ocean variability)? Line 17. “without paying attention to” – maybe “but removing”?

Page 1519 line 17. “uncertainty on the relative bias”. This suggests that aliasing error (sparse space-time coverage) is part of “SSH error” since Table 3 has “0” under “ocean variability sampling”.

Page 1521 line 6. I think equation (1) is OK for the trend uncertainty attributable to the bias. However, overall trend uncertainty is certainly not zero for t < tC owing to the finite series length and various measurement uncertainties. Would a more complete approach weight all the data (before and after tC) with an inverse error estimate so that weighting were reduced to represent increased uncertainty due to bias? Or, I believe there is a statistical approach to estimating steps in a time series. I guess (1) is a lower bound for the overall trend uncertainty. These comments are not from specific knowledge on my part but from a reluctance to accept that more information (from the successor satellite) should degrade the trend estimate if handled appropriately.

Page 1522 lines 14-16. Despite the previous comment I agree with this!

Interactive comment on Ocean Sci. Discuss., 12, 1511, 2015.