

Interactive comment on “Sea level variability in the Arctic Ocean observed by satellite altimetry” by P. Prandi et al.

Anonymous Referee #2

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The paper presents one of the first attempts to study the variability of sea level in the Arctic Ocean using satellite altimetry data. And this is the major merit of the paper. However, the way in which the authors presented the data and the methods they used for the analysis unfortunately does not convince me in the validity of the results. Therefore, I recommend a major revision. My major concerns are as follows:

1) The authors have produced their own satellite altimetry product. However, it remains unclear how good this product is. How does it compare with existing AVISO global products? The validation of data is not adequate. Correlation with tide gauges (fig. 7) is not a good measure for validation, because correlations are dominated by the seasonal cycle and they do not say how close the time series are. I would use RMS differences instead. Recently, there was a paper by Volkov and Pujol (JGR, 2012) that

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presents a validation of an AVISO product in the Nordic, Barents, and Kara seas. The paper also considers the seasonal cycle and linear trends in both altimetry and tide gauge data. Given that one of the co-authors is also from CLS, I am surprised that this study is not mentioned and not used for comparison. One of the results important for the manuscript by Prandi et al is that the observed rates of sea level change in the Greenland, Barents, and Kara seas are largely uncertain, because the signal-to-noise ratio is often less than 1.

2) I think that the way in which the authors averaged all available data records over the Arctic Ocean is not appropriate. This is because the sampling is very uneven in both space and time. How good is fig. 8 if there is probably less than one month of data in the Beaufort Sea and almost no gaps in the Nordic seas? The more appropriate way is to plot September trends and then (using a model) approximately estimate the representativeness as the difference between the September and total trends. The quality of some tide gauge records in the region is also suspect (as pointed out in Volkov and Pujol, 2012).

3) With regard to the hydrography data used in the study, the authors neither discussed the quality of this data nor they presented the mapping error. Considering that the spatial coverage is poor, especially in the Russian Arctic, can we actually compare these data to altimetry that is mostly available in the Nordic seas and along the Russian coast? I also have concerns about the use of GRACE data. The authors acknowledge that these data are contaminated by land signals that are not fully corrected. We do not know how good GRACE data are in the Russian Arctic seas containing a lot of islands. So, is it justified to include these data in the basin-averaged estimates (figs. 4 and 5)?

Remarks:

2376.4: the authors do not really study high-frequency signals;

2376.7: "tide gauges measurements/time series" -> time gauge measurements/time series (the same throughout the manuscript);

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2377.1: is it important only at a global scale? Regional changes in sea level can also be indicating of the climate change.

2379.19-21: Using NCEP/NCAR reanalysis to derive correction for tide gauge data is not consistent with what is applied to altimetry data. The MOG2D model the authors used to derive the dynamic atmospheric correction for altimetry data as far as I know is forced by ECMWF winds.

2380.18: GIS -> GIA?

2382.23-24: this is what should be shown in fig. 4 and 5.

2383.14-15: not necessarily. Coastal areas are known for stronger variability. And you show it later in fig. 10 (top right).

2384.28-29: what about the first mode?

2386.1-4: should be placed in data description.

2387.16-17: you did not demonstrate that tide gauge data are questionable.

Interactive comment on Ocean Sci. Discuss., 9, 2375, 2012.