Review of ‘The sea state CCI dataset v1: towards a sea state climate data record based on satellite observations’ by Guillaume Dodet et al.

The authors describe a new sea-state dataset, for which they cross-calibrated ten satellite radar altimeters. They do a rigorous validation of the resulting dataset against another altimetry-derived dataset, in-situ data and wave models/reanalysis. Then the applications of the dataset are described and finally a list of shortcomings is provided.

Major comments

I think the structure is clear and the manuscript is easy to follow. The results are quite convincing, because of the extensive validation. However, I have the feeling that sections 4 and 5 lack depth. In most cases an observation is made, but the details or a possible explanation are never provided. It therefore gives the manuscript the character of a technical report. Therefore I have to recommend a major revision.

Section 5 holds a discussion of applications, but there is only a comparison with other sea-state datasets and a hind that the geostrophic vorticity is connected to wave spectra. This should be expanded and rewritten.

Section 6.2 discusses the implementations of new retracking algorithms, but does not really provide the potential for near-future improvement. I relies largely on the delay/Doppler, but this only helps CryoSat-2, Sentinel-3 and Sentinel-6, which is only from 2010 onwards. Whatever retracking algorithm you implement, you will only find limited improvement in sea state near the coast.

General comments

Make sure that in the captions clear information is provided. For example: in figure 10 a ‘wave model hindcast’ is used, but it is not directly clear which model is used here.

Minor comments
Page 2.
Line 1: As this is not a oceanography journal, remind the reader what is meant with sea state.
Line 14: I do not see how an acceleration in sea level would translate into changes in sea state, except maybe for coastal waters. A line of explanation or a reference needed.
Line 30: Remove the dot between cm and year.

Page 5.
Line 4-19: The time series are filtered with a 1-hour filter, but most time series have a sampling rate of an hour of more. Should it be interpolated? Be also aware that in coastal zones an hour is quite long, so the interpolation/filtering method is quite important.

Line 18: I think the validation should be shown for both, because you mention in the introduction that sea state is important for coastal processes.

Page 8.
Line 25: The figures seem quite convincing, but I wonder if there is latitudinal dependence remaining in the SWH differences, or even a water-depth dependence. The CCI sea-surface height products are corrected for a latitudinal dependence, which does not necessarily have to be orbit as we also observe it between Jason-1&2.

Line 30: Are you considering only LRM data from CryoSat-2? If not, how is the SAR mode data processed: as Pseudo LRM or delay/Doppler?

Page 11.
Table 3: I would split TOPEX-A&B, because the instruments show clearly different properties. For TOPEX-A: is the cal1 applied or not and is it consistent with TOPEX-B?

Page 12.
Line 23: "The IMFs … algorithm." I think there is something missing in this sentence, maybe: "If the IMFs .. algorithm."

Page 14.
Line 13: For white noise.

Page 16.
Line 2: Why is there a negative bias for ERS, I would expect that the biases are removed by the cross-calibration? There is also a positive bias for the other missions, is this a problem in the data or the model?

Page 17.
Merge section 4.1 and 4.2, it is very short.

Page 18.
Figure 12: A bias between TOPEX and Jason-1 appears to be introduced in the lower figure.

Figure 12: Maybe I missed it, but I am not sure it is clear how the ERS series is de-biased with respect to the Jason series. Maybe this can be added to table 3.

Line 13-18: A bit more in-depth discussion is required why the differences are there. Is it related to the filtering or the de-biasing for example? The reader should know which dataset they should use when they have a certain application in mind. Also the number of 2.5% looks quite nice, but as both datasets are coming from the same dataset I am not sure this is small.

Page 19.
Line 5: I have the feeling that an issue in ENSO, PDO and IOD modeling are affecting the climatology. The authors have a strong oceanography background in the team. It should be possible to speculate at least what is causing this.

Section 5.1: What I am missing here is a comparison of geographical trends and a comparison of the variability. As mentioned in the introduction, changes in sea state are important for a variety of reasons, but the change is completely left out of the discussion. It will probably also give you more insight into the previous comment.

Line 10: For illustration purposes.

Section 5.2: This section suggests that the wave energy is higher in high-geostropic-vorticity regions. Instead of showing a couple of examples, I would compute an average spectrum over low-, medium- and high-vorticity regions. The authors show a correlation between the two quantities, but do not discuss how both are affecting each other. I am not sure what I should get from this section, it should be further elaborated. The topic also seems a bit selective.

Page 21.

Figure 14: The figure for vorticity is too small.

Line 13: Explain fading noise.

Line 15: Quantity and quality.

Line 13-15: One of the major issues is the change of wave shape in shallow areas, which is not discussed here. This is not modeled in standard retrackers.

Line 22: Multilooking is already applied to reduce speckle in LRM waveforms. Typically 90 waveforms are averaged. The great benefit of delay/Doppler is the enhanced along-track resolution, which allows you to get closer to the coast under certain angle-of-attacks. On top of that it increases the number of independent looks per second by a factor of +/-2, which can be used to improve precision (reduce speckle).