

## ***Interactive comment on “A coastally improved global dataset of wet tropospheric corrections for satellite altimetry” by Clara Lazaro et al.***

**Anonymous Referee #1**

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### MAJOR COMMENTS

In this paper, the authors present a novel dataset of Wet Troposphere Correction (WTC) to correct the sea level anomaly (SLA) derived from satellite radar altimetry. The dataset is particularly important for coastal altimetry being well known that this correction is the most critical in the coastal zone. The new correction (known as GPD+) computes the Water Path Delay (WPD) for all along-track altimeter points where the default correction (from onboard radiometer) is unusable. The method adopts an objective analysis approach to estimate WPD from a number of sources (coastal and island GNSS stations, satellites carrying microwave radiometers, valid on-board MWR measurements). The method is applied to all conventional missions and CryoSat-2. The validation of the dataset is made through statistical analysis of SLA and the metric used

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is the reduction of variance.

The author provide a clear description of the datasets apart details specified below in the minor comments. However, the validation of the dataset (that is the part of interest to users) is really poor in showing the improvements, in particular with reference to the coastal zone. The authors titled this paper “A coastally improved global dataset...”, unfortunately the reader does not see any zooming in the coastal zone. The metric used is certainly appropriate for open ocean but not for the coastal zone. The results do not provide a clear measure of confidence of the dataset in these challenges area. All plots are global and some plots globally averaged when quantities are showed as a function of distance. Instead, the reader expects to see a selection of relevant coastal regions in the world, based e.g. on bibliography (i.e. areas where users already applied coastal altimetry) or peculiar characteristics (e.g. authors mentioned Indonesia). Moreover, the testing in the coastal zone has to be at 20Hz being the available re-tracked products at this rate. The RADS product is fine for open ocean studies but not in the coastal zone. Also the metrics has to be different, as in the coastal zone we can use tide gauges as an independent measure of SLA. Therefore, by changing the wet troposphere (default vs GPD+) in the altimetry formula, absolute differences of SLA along the track would show the distance of the coast at which noise increases in the specific region. Comparisons with TGs would show the improvements in terms of statistical indicators ( correlation and rms error).

Having said that there are other important remarks that I would like to highlight.

First, the authors are discussing a product at 1 Hz, when users need a product at 20 Hz in the coastal zone. So this product after publishing would not be usable for the typical non expert coastal users.

Second, I also see insufficient the strategy of showing results related to only one missions. As multi-mission approach is essential in the coastal zone to have more coverage in space and time, the reader expects to see the validation extended to all missions.

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Third, one important input for the estimation of an improved WTC in the coastal zone is the presence of GNSS station. The authors provide poor information about distribution in space and time. There is just one figure related to Envisat showing the number of GNSS stations over mission time. The authors have to add same figures for the other missions. Moreover, a map has to add concerning the geographical distribution (areas well covered and areas where no GNSS stations are available. These figures are important for the users that after zooming in their coastal regions of interest can perceive the space and time coverage.

In summary, the paper in the actual version fails to convince the reader that in the coastal zone the new correction cannot be immediately exploited by users (because not at 20 Hz) and that misses a thorough validation in selected coastal areas of investigation (i.e. zooming locally where the user would use SLAs).

Therefore, the paper calls for significant revision in order to fill the gaps in term of exploitability of the product and validation of the correction in the coastal zone.

MINOR COMMENTS Pg 1, abstract: "The results are presented with vague sentences (e.g. GPD+ WTC is the most effective. ....). The reader expects here to see quantitative results that show the improvement with reference to the state-of-the-art and discussion of these results. In the present version, the abstract is substantially an introduction to the dataset that should be the core with more details, e.g., distance from the coast, etc..

Pg. 1, rows 13-14, "SLA dataset over open ocean accurate to the centimetre-level": The authors in the previous sentences refer to sea level rise (which means mm/yr error level). The reader might be confused with cm level accuracy that is generally a target for oceanography. Moreover, accuracy is not enough for trends, there is also a need of "stability", and here it is the case of wet tropo not drifting over time. Please rephrase properly

Pg. 2, row 44, "with a centimetre-level radial error": Please provide reference where it

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is demonstrated.

Pg. 2, row 44, "precise SSH": You used "accurate" before. It depends on what you refer, e.g., global mean sea level requires accuracy; fronts requires precision, etc.)

Pg. 2, row 55, "Chelton et al. (2001)": please refer to recent bibliography (Satellite altimetry over oceans and land surfaces (Detlef Stammer & Anny Cazenave Editors), Earth observation of global changes book series, CRC Press Taylor & Francis, London, UK, 670 pp, doi:10.1201/9781315151779, 2017).

Pg. 3, row 67, "as large as  $2.3 \pm 0.2$  m", is this cited in Fernandes et al. 2014? If not, please provide reference.

Pg. 3, row 67-68, "calculated with millimetre-accuracy, provided the surface atmospheric pressure is known at each location": as we are talking about coastal zone, the authors have here to specify that pressure has to be know at surface level. This pressure is generally retrieved from coarse models that can fail in steep coastal regions.

Pg. 3, row 69, "dry and wet tropospheric corrections (negative values)": why negative ? please explain.

Pg. 3, row 70, "DPD and WPD to the corresponding absolute values": What do you mean with "absolute"? what is the difference between DTC and DPD, WTC and WPD?

Pg. 3, row 73-74, "possessing an absolute value less than 0.50 m.": Please specify how 0,50 is estimated. Please also specify the meaningful of "absolute" vs "relative".

Pg. 3, row 73, "Contrasting": Maybe you mean "in opposite"

Pg. 3, row 79-86, "Radiometers .. 12 km": please explain the different impact of the three radiometers on the retrieved measurements, e.g. with reference with data quality. Are there differences in the coastal zone in retrieving data ?

Pg. 3, row 88, "precise modelling": I think the word "modelling" is confusing. WTC can be derived from models too. However, here we are talking about "observations".

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Pg. 3, row 91, “flagged as invalid, being therefore discarded, or non-existent due to several reasons.”: The sentence is vague. Why data are flagged invalid? What is the criteria used ? What re the reasons for missing data ? please explain

Pg.4, row 96, “surface emissivity”: Coastal zone has also non homogeneous scattering due to variable waves, winds, surfactant streaks, etc. Are they influencing the retrieval of a valid measurement?

Pg. 4, row 105, “is to describe and grant access”: The access to a dataset cannot be an aiming of a paper. I think the authors have to reformulate clearly the main goal of this paper that is presenting and validating a dataset and then elucidate specific single objectives

Pg. 4, rows 111-115, “The main objective”: Objectives have to be stated in the introduction. Also description of sections has to be moved in the introduction.

Pg. 4, row 118, “GNSS network of stations”, please provide a map of GNSS stations used so the reader can appreciate the global coverage

Pg. 4, row 123, “This way”, please add “In”

Pg. 5, row 118, “this way are given at station height”. The GPS stations are over land. So you measure the column at land point. It is not clear to me (and probably to most of not expert people) how this value is extrapolated to the ocean

Pg. 6, row 156, “In fact, GPD+ is an upgrade from the GPD methodology”: Please better clarify differences between GPD and GPD+. Apparently you say that GPD+ was for coastal zone but now global. Is the reason related to CryoSat-2 ? as it has no radiometer onboard.

Pg. 5, row 158-164: as you provide a table it is redundant here to report names of the mission. It is important to add space and time resolution of single MWR sensors in the table. A matrix has to be added showing the MWR sensors available for each altimetry mission. Again, this is an important figure for the reader. Some comments about

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substantial differences between sensors should be recalled here from cited references

Pg. 5, row 173-176, “It is known that, in addition to TCWV, WPD also depends on temperature. Expressions such as Eq. (3) account for an implicit modelling of this dependence. Fernandes et al. (2013b) have shown that this expression leads to similar results as those obtained by adopting formulae that make use of explicit values of atmospheric temperature given e.g. by an NWM.” The reader might not understand what you mean here with “Implicit” and “explicit” values. Please show examples of comparisons with WTC derived from NWP in open ocean and in coastal zone.

Pg. 6, row 179-180, “We recall that the WTC is the symmetric of the wet path delay and the quantity of interest in satellite altimetry” Please rephrase and specify what you mean with “symmetric”

Pg. 6, row 180, “RA data necessary to compute”, Please specify the sources you used for corrections, orbit, MSS, etc.

Pg. 7, row 191, “Threshold values used in this criterion depend on the RA mission”: Please specify thresholds

Pg. 7, row 194, “at distances from coast”: The authors use some editing criteria. I am curious to know what happens when tracks are parallel to the coast , but also some situations, e.g., Indonesia where the altimeter crosses successive land segments due to presence of closest islands.

Pg. 7, row 203, “number of 18 Hz measurements to compute the 1 Hz”: is the global product at 1 Hz (i.e. around 7 km spaced for all missions)? While in open ocean it makes sense, I am bit skeptical the coastal zone might benefit from this product if not provided at 18/20 hz. It has been demonstrated that we need high resolution data in the coastal zone (and in fact waveforms are retracked at that rate and SLAs computed at that rate). Otherwise, the user will not be able to exploit the product.

Pg. 7, row 203-204, “For approximately 10% of all oceanic points”: What do you mean

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with “oceanic domain ? Does it include coastal zone ? at which distance ? The value seems for Envisat only. What about the other missions?

Pg. 8, row 220, “parameters have been obtained for Envisat”: Please provide parameters for all missions

Pg. 8, row 240, “For all satellite missions but CryoSat-2 and for each along-track point deemed as invalid”: The sentence is unclear, please rephrase

Pg. 9, row 275, “50 km from the ocean”: The setting of this value has to be justified

Pg. 9, row 278, “Figure 4 gives an example of the GPD+ WTC for Envisat’s cycle 12”. I don’t understand the message of this figure. The upper map is substantially unreadable. The lower map is not providing information as the reader would like. Moreover, one cycle per one mission would be only for visual purposes. There is no comments in the paper. The reader expects quantitative results about the improvement. Pg. 10, row 289, “respectively, are provided at 1 Hz.”. Previously, the authors mentioned 20 Hz. People using the product in the coastal zone need 20 hz data. I don’t understand the utility of publishing a product that then in practice it is not usable from coastal zone users (who are not experts in altimetry). The authors refer to RADS that cannot be considered a “coastal altimetry product”. In my opinion, the authors have to satisfy the user requirements if they want to publish this dataset.

Pg. 11, row 315-318: “For results concerning algorithm.”: The reader is confused here and reminded to previous paper. Indeed, the reader wants to see statistics of all missions here with the application of the algorithm described here. The authors have to add relevant statistics of all missions.

Pg. 11, row 320, “The GPD+ WTC is here compared to the ECMWF Reanalysis WTC”: This kind of comparison make sense in open ocean but not in the coastal zone. The authors provide a title “A coastally improved global dataset. ....”. They clearly state previously that models fail in the coastal zone and now they use for validation.

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Pg. 11, row 335, “Figure 5 shows the GPD+ WTC for some Envisat tracks”: The reader expects to see the map showing where the passes are located and identification of the segments where the new corrections improves. The discussion of Figure 5 is not provided. The plots have to be commented in relation to the places touched over ground.

Pg. 11, row 340, “interesting results”: please remove being subjective

Pg. 11, row 346, “most of these points are located at high latitudes and in coastal regions”: This statement is not demonstrated in the figure. The authors expects to see zooming in coastal regions to see improvements.

Pg. 11, row 361, “for the whole Envisat mission”: the authors have to provide the same figure for the other missions too

Pg. 12, row 369, “The results are shown in Fig. 7”. The authors state the product is at 1 Hz (7 km) and in the plot show values at less than 5 km

Pg. 13, row 393-395, “Therefore, three SLA datasets of collocated along-track points were derived using the same standard corrections (Sect. 1) but the WTC, which can be the Composite correction present in AVISO CorSSH L2P products (Comp), the GPD+ or the ERA Interim WTCs.”. This comparison makes sense only in the open ocean and not in the coastal zone (0-50 km)

Pg. 13, row 406, “Fig.8a”: Fig. 8c is not commented in the text. Moreover, there is a strange behavior around cycle 95

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