Interactive comment on “DUACS DT-2018: 25 years of reprocessed sea level altimeter products” by Guillaume Taburet et al.

Anonymous Referee #1

Received and published: 18 February 2019

Authors: We acknowledge Rev. #1 for his/her review. All comments and remarks have been considered. In the next paragraphs we present the reviewer’s comments followed by our point-by-point reply (blue color).

General comments: Sorry, but I ran out of energy before finishing reading this paper. I found it to be extremely superficial, repetitive and unclear. I think it would be very difficult for altimetry beginners to understand, and too vague to be informative for experts, so I’m not sure what audience it would be useful for. So I suggest it goes back for a major rewrite, and well as addition of more information.

Authors: Considering your comments, we have tried to make the necessary modifications to improve the manuscript. However, this article is not intended to provide a course on altimetry processes for beginners but rather to present a new dataset. The structure and organization of this article was intended to be very similar to what had been done in the article dedicated to the DUACS DT2014 dataset (Pujol et al., 2016). We realized that some sections deserved important clarifications. We added them to the new version of the manuscript.

Specific comments (written as I read the paper) Abstract 1) "new altimeter standards ... has been used" 1) what are ’altimeter standards’? I think I know but many people won’t. Especially in an Abstract, please use language that people will understand. 2) change ’has’ to ’have’.

Authors: The term “altimetry standards” regroups algorithms and parameters used to estimate the different fields of the equation: SLA=orbit-range-∑correction-MSS. The notion of “altimeter standards” or “standards” is commonly used in the literature and particularly in the last two articles concerning DUACS reprocessing: Dibarboure et *al*. (2010) and Pujol et *al*. (2016). The current manuscript being closely linked to these two papers we have chosen to keep this notion. A dedicated chapter of the manuscript entitled “Altimetry standards” presents and explains in detail what these standards correspond to (section 2.2 Altimetry standards).

As recommended by the reviewer (see also comments 4) 5) and 2.2 3)) and for greater clarity, we have added details to the specific chapter « 2.2 Altimetry standards ».

Intro 1) Sentence 1: "so called" -> "called". "Exists" -> "has existed"

2) p2 line 1 focus->focusses. I think I’ll stop noting grammar edits. There are too many.

Authors: The authors have asked for an English grammar and spelling correction service in order to improve the quality of the manuscript.

3) p1 l5 "Sentinel-3 L3 products are processed on behalf of EUMETSAT". This is confusing for some readers on 2 counts: they might not know what Sentinel-3 means. You just have to say "the Sentinel-3 altimeter mission". Don’t use the passive verb "are processed", especially straight after saying that who does the work has just changed. Say who now does it.

Authors: Done.

4) "standards" see above

Authors: See above

5) line 20. "standards". That words again, but this time I start to think I don't know what is meant. "processing from the standards to L3 and L4 products". This is terminology that is common among remote sensing specialists but is unfamiliar to a large fraction of the target audience. The previous paragraph referred to two products in meaningful language. Connect back to those products now via simple names. I don't think those are L3 and L4 but I might be wrong.

Authors: The sentence has been rewritten for more clarity.

Data Processing 1) "cumulated" I don't think this is a real word. I think you mean "26 mission-years". Ie the sum of all the mission durations. This term was used before but I let it slip.

Authors: Done.

2.1 2) "complementary" this is very vague. If you are going to mention HY2A and its problems there is no point being cryptic and making people guess what you mean.

Authors: This section has been rewritten.

2.2 3) "geophysical standards". OK this is where we define what was referred to earlier as "altimeter standards and geophysical corrections". I see now why you have chosen a nice simple term like 'standard' but I'm sorry, I think it is too meaningless to be useful. I know this debate is old but I think this solution is a very bad one. New users will be confused by it. I think you need a quick little explanation explaining the equation SLA=Range-range_corrections-orbit-MSS-HF_alias_terms, noting that the terms in that equation are not really 'corrections'. The altimeter measures what it measures, which is not quite what everyone wants, for all purposes. De-tiding is not making the answer more correct. It is making it wrong if you want the tide still there. Similarly for DAC. It is only for the purpose of making gridded SLA products that all these terms are needed, so start by saying that.

Authors: See the discussion above concerning the term "altimetry standards". This section has been rewritten. In addition, a paragraph concerning specific along-track (L3) products has been added in section 2.4. It introduces the possibility to remove specific geophysical effects that are taken into account in the DUACS processing.

This article is not intended to provide a course on altimetric processes for beginners but rather to present a new dataset. The readers are advised to refer to the existing literature presenting the altimeter measurements. We have added a specific reference: Escudier, P., Couhert, A., Mercier, F., Mallet, A., Thibaut, P., Tran, N., Amarouche, L., Picard, B., Carrère, L., Dibarboure, G., Ablain, M., Richard, J., Steunou, N., Dubois, P., Rio, M. H., and Dorandeu, J.: Satellite radar altimetry: principle, geophysical correction and orbit, accuracy and precision, in: Satellite Altimetry Over Oceans and Land Surfaces, edited by: Stammer, D. and Cazenave, A., CRC Press, Taylor & Francis, Boca Raton, 2018, https://doi.org/10.1201/9781315151779 and in particular section 1.6.2 (tides, high frequency signals).

4) Table 1 columns are variable-span. Ie some entries span several columns but it is not clear which. I'm not sure all the entries are defined, either. E.g. I can imagine people wondering what a GDR-E orbit is.

Authors: The authors have used the Copernicus Publications Word template to create Table 1. However, we have made it evolve for a better readability. This new format remains to be discussed with the publisher. In the line corresponding to the orbit parameter, the GDR mention has been replaced by POE. Indeed, Geophysical Data Record (GDR) corresponds to the generic term for L2 altimeter product whereas POE (Precise Orbit Estimation) is the exact and appropriate term. The acronym DAC have also been clarified: Dynamic Atmospheric Correction.

5) "FES2014 is the last version" I think you mean 'latest' - except that's wrong I believe. FES2015?

Authors: At the time the DT2018 products were computed, FES2014 was the latest version available. This preliminary version, noted FES2014a, has been produced in 2015 based on GOT4v8ac loading tide. Then new tide loading effects have been computed using FES2014a oceanic tide. These FES2014a tide loading effects have been used to produce the final model version noted FES2014b.

2.3 1) "homogenise" this is cryptic for most readers. I think you mean that the nonJason missions are debiased, taking Jason-class missions as 'truth' (once debiased, which is another thing to explain).

Authors: «homogenise » is used page 3 line 25 as an introduction of two different processes that are described in the following sections: global and regional bias reduction to ensure mean sea level stability and cross-calibration process to minimize inter-missions' errors at crossover. For a complete description of the processes, the authors explicitly guide the reader to a much more detailed reference: Pujol et al., 2016.

2) "...expose major changes that occurred in this DT2018 version. For an advanced description of the DUACS processing, readers are advised to consult Pujol et al., 2016. Say this earlier. However, see the next comment.

Authors: This is a reminder of the approach (see. p2L1) explaining that this article focuses on improvements of the DT2018 dataset compared to the DT2014. Thus, we think that it is adapted to keep these sentences in the introduction of section 2.3 "Evolution of the DUACS processing".

2.3.1 1) lines 25-33 "the cross-calibration step...." I see no mention of a change, so maybe this text can be shortened at lot (if this document is only about changes, as above).

Authors: Done.

2.3.2 1) "The along-track generation for repetitive altimeter mission is based on the use of a mean profile (MP) (Dibarboure et al., 2011 and Pujol et al., 2016). These MPs are necessary to co-locate sea surface heights of the repetitive tracks and to retrieve a precise mean reference for the computation of sea level anomalies. The methodology used for the DT2018 MP computation is the same as in DT2014." This is a perfect example of a sentence that I see no audience for. 'Experts' know this already. Beginners won't understand it: it is too unclear. Finally, it says there is no change since DT2014, contradicting 2.3 comment 2).

Authors: There is indeed no change in methodology (this is why the two references to Pujol et *al.*, 2016 & Dibarboure et *al.*, in review are mentioned) but the data selection has evolved (from line 5). Thus, the authors think that it is appropriate to briefly recall the interest of mean profiles without going into details. They mention references that are relevant for the uninitiated readers. The authors added a reference that precisely details the usefulness and processing of MP (Dibarboure et *al.*, in review). To facilitate the understanding, we considered appropriate to retain the short sentence "These MPs are necessary to co-locate sea surface heights of the repetitive tracks and to retrieve a precise mean reference for the computation of sea level anomalies".

2)"For non-repetitive missions (ERS-1 during its geodetic phase, Cryosat-2, Hayaing 2A, Jason-1 geodetic phase, Jason-2 geodetic phase, Saral-AltiKa geodetic phase), no MP can be estimated. The SLA is then derived along the real altimeter tracks using the gridded MSS." same comment as above. You need to either clearly explain the difference between MP and MSS, or assume it is understood.

Authors: The authors have chosen to keep the sentence to facilitate the understanding of the following paragraph about the MSS (2.3.3 L11). Nevertheless, and as suggested, we have added references (Pujol et al., 2016 and Dibarboure et al., in review) which can help the user to have access to more details.

2.3.3 lines 1-20. This is very uninformative. 'updated' and 'refined' are very uninteresting to read.

Authors: This section lacked details; we have enriched it. The words "updated" and "refined" have been deleted and replaced by more precise descriptions of the developments implemented.

2.4 lines 23-32: this is just repetition of what was said earlier in this paper. Nor is it anything new. It is well known. I'm startng to lose my patience with this paper now.

Authors: These lines are indeed redundant with the explanations given in the introduction. This has been simplified (p2 l11-19).

lines 11-13: "As a second difference, the reference used to compute the Sea Level Anomalies is a Mean Sea Surface (MSS) for all missions in the C3S products whereas a mean profile of sea surface heights is used...." Back to this issue again. Very confusing. See comment 2 on 2.3.2 above.

—to end of 2.4. As far as I can tell, this is all old information that experts don't need to be told, and beginners won't understand, the way it is described here.

Authors: According to the authors, this major difference between CMEMS and C3S products has never been addressed (and should thus not be considered as old information) and must be described to expose the specificities of the different Copernicus products.

The product dedicated to climate applications (C3S) is based on a stable number of missions (two) in the satellite constellation and has a specific processing (which is the interest of section 2.4 and particularly from line 11 to end), that follows the recommendation made within external R&D projects (such as the ESA Sea Level Climate Change Initiative project). Along-track data were not calculated with a MP but only with the MSS (and even for repetitive missions) which contributes to improve the mean sea level stability (especially for regional products). Thus, this should not be considered as "old information", since this has been implemented for the recent production of the C3S sea level products.

Section 3. 1) Results section. But I feel unready to read about results. All I have gleaned so far is that some updates have been made, with very few details given.

2) "Additional variance is observed for high variability regions in DT2018 products and is linked to the new OI parametrization." 'linked' is it? I'm getting more and more annoyed about this persistent abscence of information. Is it secret?

Authors: The wording of the sentence has been changed and details added.

3) p8 line 4-5: "At high latitude, the difference of variance is important (100cm2 to 200cm2) and is linked to the new MSS correction." It's not obvious to me how this could be true. It must be a fairly convoluted argument.

Authors: Pujol et *al.*,2018 shows the new MSS15 is more extended at high latitude than the old one. (see also figure 1 below). This allows us to compute the OI with much more precision and stability in this region. The figure 4 of Pujol et *al.*,2018 shows the difference of the variance of SLA along HY2A tracks. These differences are major at high latitude.

Figure 2 (below) shows the difference of SLA variance with DT2018 and DT2014 gridded products from the same point of view as figure 1. The difference in spatial coverage of the two MSS explains the difference in quality of the SLA grid products in this area.

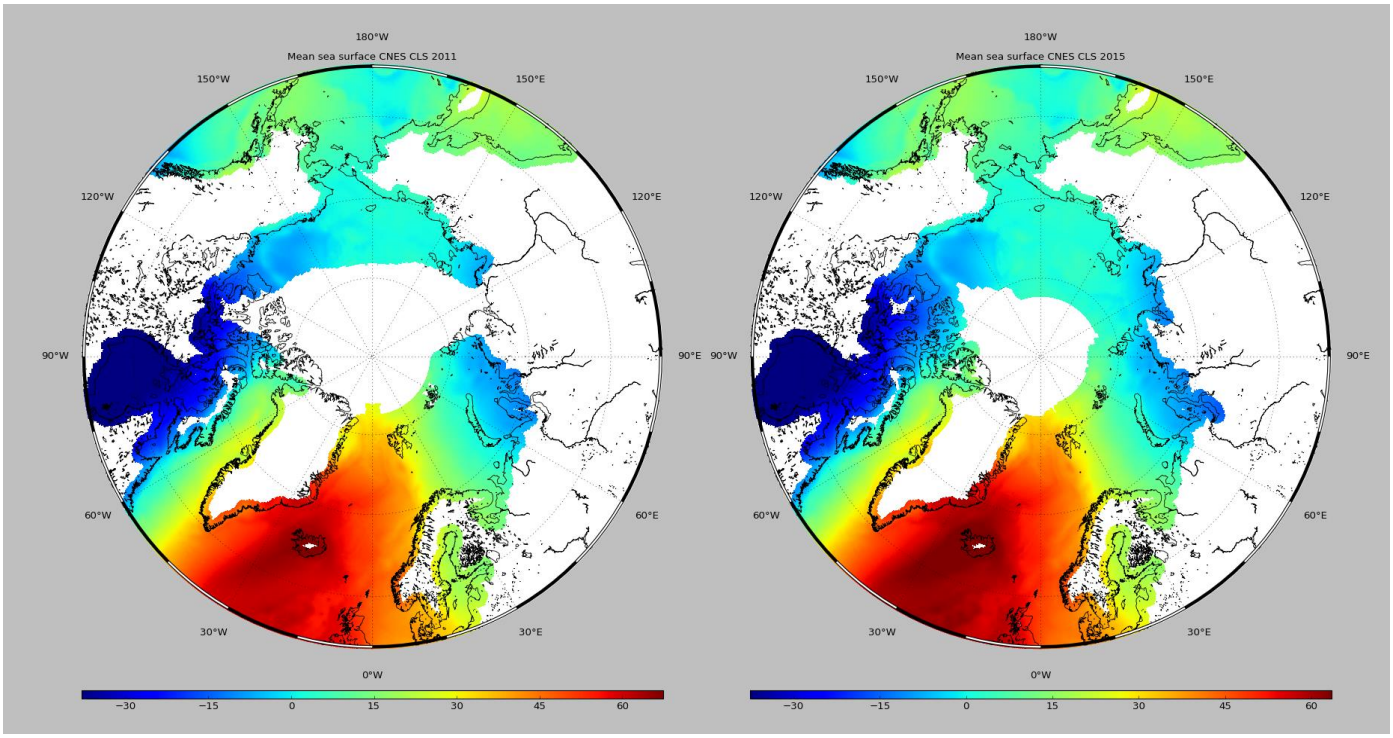

Figure 1: Mean Sea Surface CNES CLS 2011 version (left panel) and MSS CNES CLS 2015 version (right panel).

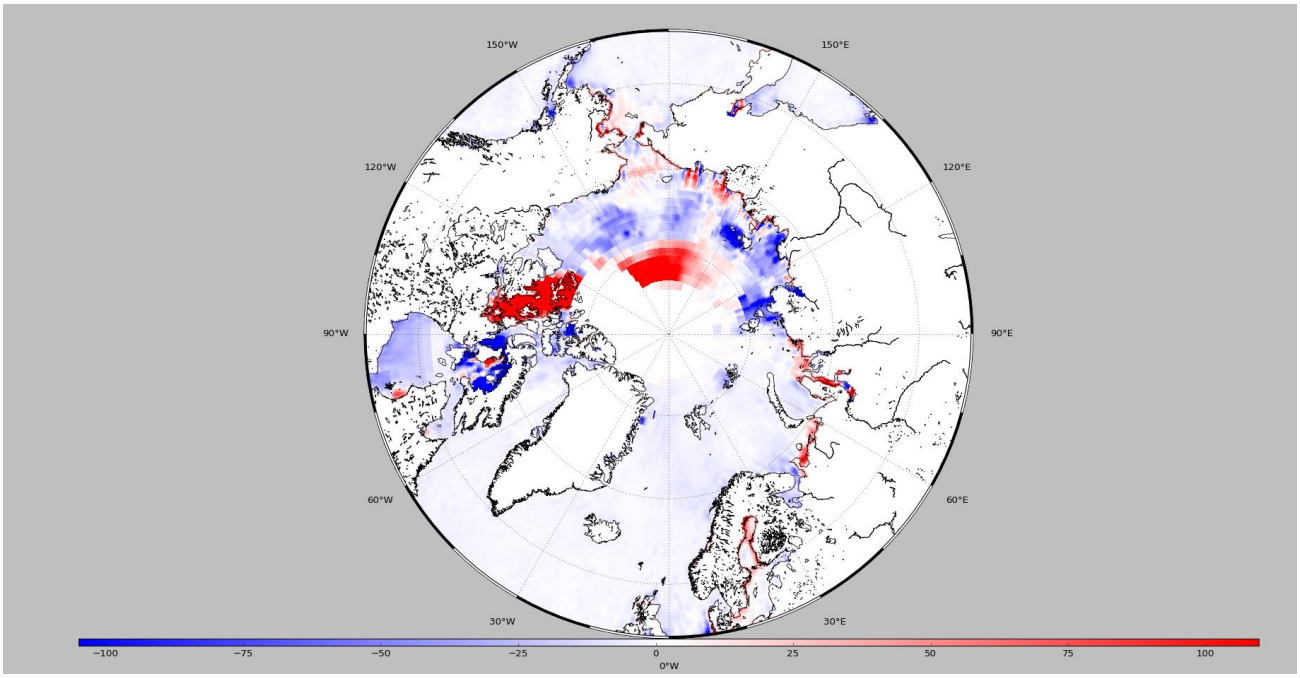

Figure 2: Difference between SLA variance observed with DT2018 and DT2014 gridded products. Same figure as figure 4 of the manuscript but centered on the North Pole. Units: cm².

4) p8 line 11-12: "However, in the equatorial band (±20◦N), the EKE in the DT2018 is less important (-17%). This is linked with the evolution of the noise measurement considered in the mapping process for all satellites." I'm getting really sick of this vague uninformative style: 'linked' and 'evolution'.

Authors: The sentence has been changed and details added.

5) p8 line 19-29: Discussion of table 3. This is an important part of this study, but lots of information is missing. Table 3 has just 2 values for each of 4 regions. Why trim it down to such a bare minimum of information? E.g. For the reference area |trackmap|ˆ2=1.4cmˆ2. This is for a 'low variability' region. But how low? Easy to answer: list the |track|ˆ2 and |map|ˆ2 values as well.

Authors: The low variability region has been introduced in Pujol et *al.,*2016. The authors found interesting to reuse it to have a reference area where observations errors are small. The SLA variability in this region is between 0 and 7 cm². This precision has been added to the Table 3. A figure (figure 5 in the new manuscript version) has been added in the manuscript to show the RMS difference (in % of RMS) between two-sat gridded products and along-track product for DT2018 and DT2014 versions.

We also added a discussion about improvements in the intertropical zone.

6) p8 line 19-29: Discussion of table 3. —-also: this is just for the 2-sat product. What about the multisat product? I hear the answer already: "Because none of the data are withheld". My response: this does not stop you listing the map minus track stats, which are then measures of the closeness of fit (as distinct from map error). To estimate map error, pick a time with many good satellites and rerun the OI, withholding one (e.g. C2) for use as the error measurer.

Authors: This issue has been discussed p8 between line 23 to 25. The error described here must be considered as the upper limit. We choose not to describe in the manuscript a configuration with more than two satellites. However, the authors also studied the period 2016-2017, and the

conclusions are similar with C2 as an independent along-track mission. (using Jason-2 and AltiKa for the mapping process).

The L4 all-sat validation is complemented by *in situ* drifter's comparison.

7) p9 line 1-2 "Positions and velocities of drifters are interpolated using a 3-day lowpass filter in order to remove high-frequency motions." I have 3 grumbles: i) don't use the passive voice ('are interpolated') - it leaves it to the reader to guess who did the interpolating - we assume it was you but we can't be sure. ii) this is a very brief 'Methods' section squeezed into the Results section iii) why remove 'high frequency motions?' A 3-day filter also removes a lot of low-frequency Eulerian velocity (a drifter can easily go 1/4 of the way around a well-resolved eddy in 3 days). So, instead of filtering then differencing, it is better to do differencing then filtering.

Authors: The authors added a relevant reference which explain the interest and the method used for 3-day lowpass filtering: Use of Altimeter and Wind Data to Detect the Anomalous Loss of SVP-Type Drifter's Drogue M.-H. Rio. 2012.
The main objective of the filtering process is to discard the tide and the inertia in drifters' data.
We know that: - we don't filter enough between 10S and 10N to get rid of all the inertia
- we filter a little too much at high latitudes, knowing that we don't want to go below 24 days for the tide.
The 3-day period is a compromise between these two. The methodology still needs to be improved.

8) Fig. 6: It seems to me that 2 panels are missing: the ones showing the DT2018- DT2014 difference.

Authors: The authors have added the missing plot and related comments.

9) p9 line 4-5 "the comparison reveals that DT2018 altimetry products underestimate absolute geostrophic current." This statement is not supported by Fig. 6, Table 4, or by the mention that someone (we don't know who, because passive verb was used) has done a Taylor diagram (but kept the results to themselves - all we know is that the results are 'strong'). As in comment 5 above, list the variance of the drifter and altimetric velocities in order to prove that the altimetry under-estimates the drifter velocities.

Authors: The authors modified the sentence. It is neither an improvement nor a degradation of the products' quality but it is rather described as it is. It was also noted by Pujol et al,2016 in the DT2014 version of the sea level products.

The authors also added the RMS difference between gridded and independent drifters' measurements for DT2018 and DT2014. Related comments have been added.

10) p9 line 10-17. This discussion only talks (vaguely, but I'm not going to mention this any more because it is everywhere) about DT2018 being better DT2018, which is good news, but what people really want to know is the error:signal ratio.

Authors: The error is estimated using independent data for the SLA and geostrophic current on high variability and low variability region, coastal areas… (Table 3 to 6). The authors do not see what additional information could be added.

3.3 1) p9 line 19-33. This is all repetition.

Authors: The authors have streamlined this section.

2) p10 line 1-10. This is an interesting result that is "not understood yet". I think you could try a little harder. I see red dots (DT2018 is worse) on W and E USA, Spain (as mentioned) but also Japan - all

30-45N. Let's see some example time-series of errors for each product individually, not to mention the two signals being differenced (altim and TG) individually as well.

Authors: We know from Saraceno et al, 2018 (Estimates of sea surface height and near-surface alongshore coastal currents from combinations of altimeters and tide gauges) that coastal processes are more difficult to resolve with altimeter data, because of two types of problems. First, and most importantly, intrinsic difficulties affect the corrections applied to the altimeter data near the coast (e.g., the wet tropospheric component, high-frequency oceanographic signals, tidal corrections, etc.). Thus, data are usually flagged as unreliable within some distance of the coast. Second, the interpolation of along-track data collected by just one or two satellites provides only marginal resolution of mesoscale and smaller-scale structure in ocean circulation [Le Traon and Dibarboure, 2002; Leeuwenburgh and Stammer, 2002; Chelton and Schlax, 2003], which is dominant in the coastal region.

We did compare some time series for tide gauges on the Portuguese coast. It is difficult to draw conclusions about a particular time period over which comparisons are degraded. We were unable to correlate these degradations with periods when there are fewer data (fewer satellites in the constellation, or anomaly on a satellite).

We know that the new tide correction is particularly important in coastal areas, but again we have not been able to explain these degradations with this correction.

We are not in a position to explain the degradation observed in these well-located areas of the globe (West Coast of the USA, Portuguese coast, etc.).

3.4-onwards

Sorry, but I am not prepared to read any further. I think this paper has too many faults to be published in close to its present form.