

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

Anonymous Referee #1

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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.

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'.

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

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

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.

4) "standards" 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.

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

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.

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

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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.

4) Table 1 columns are variable-span. In 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.

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

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

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.

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 a lot (if this document is only about changes, as above).

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).

2)"For non-repetitive missions (ERS-1 during its geodetic phase, Cryosat-2, Hayaing-

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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.

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

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 starting to lose my patience with this paper now.

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.

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 absence of information. Is it secret?

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.

4) p8 line 11-12: "However, in the equatorial band ($\pm 20^\circ\text{N}$), 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'.

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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 $|\text{track}|^2 = 1.4\text{cm}^2$. This is for a 'low variability' region. But how low? Easy to answer: list the $|\text{track}|^2$ and $|\text{map}|^2$ values as well.

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.

7) p9 line 1-2 "Positions and velocities of drifters are interpolated using a 3-day low-pass 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.

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

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.

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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.

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

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.

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.

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