Review of DUACS DT2014 : the new multi-mission altimeter dataset reprocessed over 20 years by Pujol et al.

Reviewer: Graham Quartly

General

As the other reviewers have said before me, the DUACS datasets are very widely used, and a paper giving a complete and clear description of the methodology (data selection, editing, filtering and interpolation) would be very useful. Unfortunately this is by no means as clear as it should be.

Structure of Paper

There were a number of references to sections by their title e.g. 'the "Along-track SLA generation" paragraph' (bottom of p.6). If Ocean Sciences permits a 4th level of sub-heading i.e. sect 2.2.1.5 then this should be used; otherwise some re-organization to permit enumeration of these sub-sections using three levels. The situation was made much worse by the present section numbering being awry, which felt like the sections had had a last minute re-ordering without anyone re-reading the manuscript to check for continuity.

There seems to be some repetition of information, which might originate from a late decision to re-order the structure of the paper. The authors should read the paper in its entirety removing duplication, unless it is felt necessary.

There is also a confusing mix of different structures within the paper. Roughly up to p.15 there is description of method intermingled with some results (illustrative figures, percentages of data affected), which works well; subsequently the subsections are all methodology (p. 16-18) followed by separate results (p.19 onwards).

Writing style

In general I found the English in this version very understandable, although at times the sentences were too long e.g. "In recent papers .... (L33 and L4)." (p.3, l.13-17) and "Although small wavelengths ... Dussurget et al., 2011)." (p.30, l.2-8).

Summarising Processing (and changes)

The article aims to describe fully the DT2014 processing, and to compare its results to an earlier version (DT2010). The changes include new satellite data, new editing, improved corrections and orbits and changes to the interpolation scheme. Although I accept that these are all aspects of the methodology and analysis that should be covered, I did feel that the text felt very long. Some parts felt obvious e.g. a smaller correction scale for the interpolation leads to more short-scale variability being passed and currents determined via centre difference method being larger. Similarly better recovery of data near to the coast will result from a direct OI to chosen grid ('qd') rather than OI to Mercator 1/3° and then simple interpolation to qd. These conclusions seem obvious, so the main merit is in quantifying the difference between DT2010 and DT2014 in these regards. This paper has the potential to become a standard reference for all papers using DT2014, which will no doubt be many. However it needs to

clearly detail what has been done, rather than simply refer to improved editing. The details on the filtering seem quite clear; other aspects contain very little specific information.

i) There is a lot of useful information mentioned in the paragraphs on corrections. Could these be pulled out into a simple text table listing the corrections (wet trop, dry trop, iono, SSB, tides etc.) with specification of which corrections are applied in DT2014 and in DT2010. (Obviously some of these corrections are different for different satellite missions or vary with time; however I feel a tabular form would make the changes between DUACS versions more instantly understandable.

ii) Please provide more details of the editing criteria, rather than simply referring to AVISO/SALP (2015). I believe this is the sort of information users expect from this publication, rather than having all the details available somewhere in an unrefereed publication. Again, if this can be efficiently put in a table, it would be much clearer.

iii) I would like information on how the latitude-dependent biases and long-wavelength biases are removed, and also specifically on what their causes are believed to be e.g. time-tag bias, sea state bias?

iv) How are propagation speeds taken into account (p.12 l.9-12)? Eastward signal are only accepted if a few cm/s: what about Kelvin waves, which are much faster? Is it the case that long wavelength signals do not need such propagation effects to be explicitly included in the interpolation? Does the inclusion of expected values of propagation in the interpolation make it more likely that derived estimates for movement of eddies and Rossby waves will match the pre-conceived expectations, and that data matching anomalous propagation events are suppressed?

v) What is the "tuning of the grid definition near the coast" (p.23 l.27)?

Summary of Missions

Fig. 1 is a very familiar image from countless OSTST presentations, but still useful to keep. However, it could be improved with a slight use of colour, with one hue for the "reference missions" and another for those used in the "two-sat" solution, and another for the "tandem" phase. (Personally I always regard the 6-month intercalibration phase as "tandem' and the subsequent one as "interleaved phase", but accept that usage in the altimetry community is confused on this point.) Thus please i) define "tandem" at first use, so your usage is clear, and ii) explain TPN.

References

The bibliography list does contain a lot of CLS reports and OSTST presentations. Given that some of these are several years ago, is there not a refereed publication that covers these points? I believe your Fernandes citation should now be as

below: is this correct? If so, I would have thought that one of the authors of the current paper would have known!!

Fernandes, MJ , C Lazaro, M Ablain, and N Pires, Improved wet path delays for all ESA and reference altimetric missions, Remote Sensing of Environment 169, 50-74, doi 10.1016/j.rse.2015.07.023, 2015.

There is also a little inconsistency in the styling of references e.g. whether 2nd-nth authors should have initial before or after surname, a few have journal name in full whereas it is shortened for most, and some have capitalization for every word in title of paper, whereas most do not; two have the year in the wrong position. Reference list should have "Marcos", not "Marco". Finally Ablaiin (2009) and Aviso/DUACS (2014a) do not seem to be cited, and should thus be removed.

Percentages

I appreciated the authors' decision to express many things as percentages, since for many it is not clear whether a change of 1.4 cm^2 for example is large or small. However sometimes it is unclear what is a result or what is an artefact e.g. p.20-21 there is mention of 10% additional energy due to interpolation and +6% due to less filtering. Do these together constitute the 15% additional EKE? Is the change in EKE from using 20-year reference instead of 7-year negligible or is this a roughly 0.5-1% reduction (Fig. 10)?

Specific Questions

1) The interpolation to a regular grid (qd) takes notice of all observations within a certain distance. Is there a special adjustment for the isthmus of Panama, or can observations just to the north of Panama affect the gridded SLA just to the south (and vice versa)?
2) Why are ERA-interim data only used up till 2001 (p.5), with ERA Operational thereafter? There should be some explanation in the text.

Minor Points

1) Reviewer 2 pointed out that 'meridian' should be replaced by 'meridional'. This still needs to be done, including in figure labels.
2) Offset between J1 and J2 in "tandem" intercalibration mission is a few minutes not a few hours (p.7 l. 14).
3) Should be "DT2010" on p.7 l.19.
4) Is ERS-1 not used in MP generation (p.8 l.19)? If not, please add a sentence of explanation.
5) Not "-20˚S"! (p.10 l.3)
6) Is ERS-1 geodetic not included (p.13 l.4)?
7) What is the Lagerloef methodology (p.14 l.5)? A sentence or two of explanation plus a reference would be useful.
8) Consistency in the expression of timespans would make this easier to read. On p.14 l.23-25, one period is given as "[1993, 1999]", which reads like a pair of references and the other as "1993-2012]". The latter is much more clearly a timespan than simply 2 dates. I suggest the latter format is used throughout this paper not just in this section.

9) It is not clear how energy "falls drastically" (p.20 l.13) -- is this with product change, or with wavelength i.e. spectral slope. Fig. 8 would be clearer if x-axis only spanned 30-3000 km.
10) Does 'degradation' (p.23 l.14-15) simply mean an increase in EKE or something else? Possibly change the term 'degradation'.
11) 'which is more prominent' (p.23 l.20)
12) 'They reached' (p.24 l.17).
13) What is meant by the "polar equatorial band" (p.26 l.27)? Is it just an "and" missing?
14) Drop "globally" from "globally ... within the tropics" (p.29 l.1-2).
15) Should 'AL' be 'AltiKa' (p.30 l.15)?
16) 4th letter of CMEMS stands for "Monitoring" (p.31 l.10).
17) Change "approximate range" for "band" (p.34 l.3) -- 41.5 does not seem to be an approximate value!
18) The authors often use "important" when they mean large, and "less important" when "smaller" is intended.
19) Different definitions of low latitude band are used -- ±15° (p.12 l.2) and ±10° (p.23 l.10) -- could these be harmonised?