

## ***Interactive comment on “DUACS DT2014 : the new multi-mission altimeter dataset reprocessed over 20 years” by M.-I. Pujol et al.***

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Received and published: 18 April 2016

Referee : This paper is a very valuable contribution to the altimeter data user community and is likely to be heavily cited because of its comprehensive summary of the improvements made to the latest version of the DUACS altimeter datasets (more commonly referred to colloquially as the “AVISO dataset”). In addition to a general overview of all of the changes that were made between the old altimeter dataset (named DT2010 in this paper) and the new DT2014 dataset, the two most important contributions of this paper are the documentation of the estimated errors of the gridded products in Sec. 3.2.3, and the extensive validation effort by comparisons with in situ data in Secs. 3.2 and 3.4 (monthly mean tide gauge sea level; dynamic height derived from temperature-salinity profiles; and surface drifter data). While the value of this paper is indisputable, the present version needs a lot of work. I list below two major issues, followed by

C1

numerous specific comments, questions and suggestions and, lastly, a list of minor comments, mostly editorial in nature. My comments are mostly focused on the gridded products, which are what I have used almost exclusively for my own research. My comments that follow are embarrassingly extensive, especially for a journal that posts reviews in their entirety in a Discussion section. The level of detail of my comments reflects the amount of time I've devoted in the past to thinking about these issues with the old DT2010 dataset. This paper is an opportunity to put all of those issues to rest once and for all for any future researchers using the DT2014 altimeter dataset.

Authors: We warmly acknowledge Rev. #2 for his detailed review. All the comments and remarks have been considered and we think that they have contributed to the improvement of the manuscript. In the next paragraphs we present the reviewer's comments followed by our point-by-point reply.

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### Major Comments

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1. Referee : Users have long been frustrated that it is so difficult to track down the details of the OI procedure used by DUACS to generate the gridded SSH fields. The complicated paper trail that users must claw their way through to understand all of these details is evident from lines 10-12 on page 2 of this manuscript in which the authors list a sequence of 7 publications (to which I would add Le Traon and Ogor, 1998) that describe the evolution of the DUACS system and associated products. My own frustration with this is summarized near the beginning of Appendix A2 of Chelton et al (2011, Progress in Oceanography) where it is stated that [...] The details of this procedure have evolved somewhat over the years and an upto-date and comprehensive documentation of the procedure is not available. Many of the details are described in a sequence of published papers: Le Traon et al. (1995, 1998, 2003), Le Traon and Ogor (1998), and Ducet et al. (2000). [...] we summarize here our understanding of the

C2

procedure as it has most recently been implemented. In addition to the above publications, this summary is based on personal communication in December 2010 with G. Dibarboure at CLS in Toulouse, France who presently oversees the AVISO processing. Rather than include the subsequent description of all of the details, I would have much preferred to point to a single publication in which users would find all of the details in a single place. Not including the details in this paper would perpetuate the difficulty for users to understand how the parameters of the OI procedure impact the interpretation of the data. One consequence of this is that many (probably even most) users believe that the new DT2014 dataset resolves features with spatial scales of  $\frac{1}{4} \circ \frac{1}{4} \circ$  and temporal scales of 1 day. This is, of course, not even vaguely close to the true spatial and temporal scales of the features that can be resolved. But without easy access to the specifications of the spatial and temporal autocorrelation functions used in the OI analysis, it is understandable that users will make such gross misinterpretation of the resolution capabilities of the DUACS/AVISO dataset. This paper is an opportunity to provide users with “one-stop shopping” for everything they need to know about the dataset.

Authors: The details of the DUACS DT2014 processing are given in Sect. 2, from Level2 input data acquisition, to Level4 SLA products and derivate delivery. The section 2.2.1 was completed with a description of the correlation scales and observation errors used in the OI processing (see “Multi-mission mapping” part), the clarification of the different along-track filtering applied (see specific comment # 3) and the methodology used for SLA derived products generation (see comment specific # 10). Additionally, an appendix B was added in order to summarize the OI mapping methodology.

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2. Referee : The English grammar and spelling are in need of extensive work by a copy editor. There are also a lot of instances where the authors used incorrect words. These English problems are understandable since English is not the native language of any of the authors. And their English is infinitely better than my French. But the extent of

C3

the problem is quite extreme. I did not carry out a careful statistical analysis, but I have a sense that more than half of the sentences have an English problem of one sort or another, albeit many times only a minor problem. Much of the time, the reader can guess what the authors are trying to say. But there are quite a few cases where I just couldn't figure it out. I draw attention to some of those in the Minor Comments below.

Authors: The authors will ask for an English grammar and spelling correction service.

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#### Specific Comments

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1. Referee : Page 1, lines 14-15: It is stated here and in several other places in the paper (e.g., page 3, lines 12-13, and somewhat less forcefully on Page 21, lines 23-25) that the main improvement in the DT2014 dataset compared with DT2010 is in the use of a 20-year reference period rather than the 7-year reference period used previously. I think this is actually a relatively minor point. Indeed, the authors explain how to change the reference period in a very short appendix at the end of the paper. The main improvements in the DT2014 dataset are the reduced smoothing of the along-track data, improved orbit accuracy, improved ionospheric and atmospheric corrections and the refined scales of the correlation functions used in the OI procedure (the details of which the authors relegate to a reference to Dibarboure et al., 2011, instead of including them in this paper).

Authors: The authors do not fully agree with the reviewer remark that considers the change of reference period as minor point.

Indeed, the change of the reference period does not directly contribute to make the SLA more accurate, but at the opposite of all the other changes, it can be strongly impacting for some users/applications. The use of a new reference period means for some users that they need to change at least the way they interpret the altimeter SLA

C4

signal, and in some case, the field they are susceptible to combine with SLA for their applications. An example is the SLA assimilation into numerical models. Many users combine SLA with their own MDT field. They need to change their MDT (or the SLA) in order to be consistent in terms of reference period.

AVISO users feedback clearly revealed that a large number of users were not aware of the need to have a consistency between the SLA and the mean sea surface (MDT and MSS) in terms of reference period. As said by the reviewer, this paper is the opportunity to educate users and we think that it is important to highlight this reference period change, not only with a specific appendix.

Nevertheless, the authors agree with the fact that all the other changes are not highlighted as they should be since they directly contribute to improve the DT2014 products quality. The list on these changes was added in the abstract as “improving quality change”. The change of the reference period is rather identified as “impacting change for users”. Sect. 2 was also changed in consistency with the major comment 1).

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2. Referee : Page 6, lines 13-14: “Reference mission” is defined on lines 2-4 on this page, but “secondary mission” is not clearly defined anywhere. The informed reader will know that the sequence of secondary missions is ERS-1, ERS-2, ENVISAT, Cryosat, AltiKA and HY-2A, but this should be spelled out here to avoid ambiguity.

Authors: The sentence was changed in order to specify the “secondary missions” term and clearly list the corresponding missions. The full spelling and corresponding acronyms of the altimeter missions is given in the second paragraph of the introduction.

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3. Referee : Page 8, lines 2-3: I’m not certain I understand what is meant by “The wavelengths ranging nearly 200 to 65 km are filtered”. I think that the short end of this may be imposed by the along-track smoothing applied to the track data. But from

C5

page 11, lines 11-12, I think that 65 km is a global average for that, rather than a fixed number everywhere. And furthermore, the wavelengths shorter than 65 km are also attenuated, so there should not be a lower range of 65 km for attenuation by filtering. Is the 200 km upper end of the range something that is imposed explicitly in the OI procedure? In any case, the wording is awkward. Perhaps say something like “SSH variability with wavelengths shorter than about 200 km is attenuated.”

Authors: Two different along-track filtering are applied:

1) One for along-track SLA, delivered as along-track products. In that case, the aims of the filtering is to reduced the measurement noise error signature, keeping as safe as possible the dynamical signal

2) The other filtering is applied on along-track SLA that are then used in the mapping (note that these filtered along-track products are not delivered to the users). In that case, the objective is also to reduce the signature of the small scale signals that cannot be correctly retrieved on map product, mainly due to the limits of the altimeter constellation sampling capabilities.

This point was not clearly explained in the manuscript. The Sect. “2.2.3 L3 Along-track noise filtering” was included in the section “2.2.1 Overview of the DUACS DT processing; Along track noise filtering”. Additionally, the description of the filtering applied in view of the mapping processed was moved from section “2.2.1 Overview of the DUACS DT processing; Along track noise filtering” to “2.2.1 Overview of the DUACS DT processing; Multi-mission mapping”.

The remark of the reviewer refers to the filtering described in 2). The 200 km upper end is a trade-off between the physical scales of interest and the multi-mission sampling capability (along- and across-track). The sentence describing this filtering was changed by : “in view of the mapping process, the along-track SLA are low-pass filtered applying a cut-off wavelength that varies with latitude in order to attenuate SLA variability with wavelengths shorter than nearly 200km near the equator, to nearly 65km for latitudes

C6

higher than 40°”

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4. Referee : Page 8, lines 8-15: This is the place to include the specifics of the parameters for the OI mapping procedure. In my opinion, it is not enough to simply say that the parameters used previously for DT2010 were refined for DT2014 and then send the readers to Dibarboure et al. (2011) to find those details.

Authors: Done. (See major comment #1)

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5. Referee : Page 8, lines 16-18: I would follow this first sentence of this paragraph with a qualifying sentence like, “But note that the time scales of the variability that are resolved in the DT2014 dataset are not substantially different from DT2010; these time scales are imposed by the temporal correlation function used in the OI mapping procedure.”

Authors: We thank the reviewer for this suggestion that will avoid misunderstanding for many users. The sentence was introduced in the paper as well as some information about temporal correlation scales (see major comment #1).

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6. Referee : Page 8, lines 21-22: Change “an improved resolution” to “a higher grid resolution” so that readers don’t draw the incorrect conclusion that the smaller grid spacing using essentially the same OI correlation parameters somehow magically resolves smallscale variability.

Authors: done. (Note that this part of the manuscript was moved on an Appendix C)

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7. Referee : Page 8, line 22: Prior to the sentence that begins “These latitudes in-

C7

clude the main part. . .”, insert a qualifying sentence like “Note, however, that the spatial scales of the features that are resolved in the DT2014 fields are about the same (perhaps slightly smaller) than in the DT2010 fields; these spatial scales are imposed by the spatial correlation function used in the OI mapping procedure.”

Authors: These message was introduced in the text, after the sentence that begins “These latitudes include the main part. . .”.

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8. Referee : Page 8, lines 25-26: I’m not sure I agree with the statement that the new gridding “reduces the capability of the gridded products to accurately represent the mesoscale signal in high latitude areas.” The  $\frac{1}{4} \circ \frac{1}{4} \circ$  is much finer than the spatial correlation scales used in the OI mapping procedure. I therefore do not expect there to be much difference in the feature resolution capability of the new  $\frac{1}{4} \circ \frac{1}{4} \circ$  gridded product compared with the old  $1/3 \circ 1/3 \circ$  Mercator gridded product.

Authors: We do agree with the reviewer. This part of the sentence was removed.

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9. Referee : Page 8, line 29: I am wondering what happens at the highest latitudes in the DT2014 dataset at times when data are not available “from the recent altimeters like C2”. Are the gridded values flagged as missing, or are they filled with some other value such as the mean?

Authors: The grid is defined up to the pole (i.e. highest latitude value = 89.875°N) all along the DT2014 time serie. When no valid measurement are available in the spatial&temporal selection bubble, the grid point is filled with a clearly identifiable default value as for continent (defined in the netcdf \_fill\_value attribute of the variable considered). Before C2, only ERS-1/2 and EN allowed a sea surface sampling in high latitudes. They are able the sample the surface up to 82°N. This means that during the important sea ice melt events, they eventually allowed an SLA interpolation up to

C8

10. Referee : Page 9, line 8: After this first paragraph summarizing the processing for SSH, it would be helpful to include a summary of the details of the calculation of geostrophic velocity. For example, it is my understanding that the DT2014 processing used a wider stencil as suggested by Arbic et al. (2012) to compute the SSH derivatives. Arbic et al. (2012) is included in the reference list, but I could not find it cited anywhere in the paper. The authors apparently intended to discuss the changes in the derivative calculations but either forgot to include that text or inadvertently deleted it from an earlier draft of the paper.

Authors: A section “Derived products generation” summarizing the details of the calculation of geostrophic velocities was indeed present in a first draft of the paper. It was reintroduced in the reviewed version.

However, the authors finally decided to focus the paper on SLA field validation. This point was not clearly specified in the first version of the paper delivered. This was corrected (Abstract, introduction and summary). As a consequences, the geostrophic current considered in the comparison between DT2014 and DT2014 were calculated using the same methodology (here centered differences), as specified in section 2.3.1 “Altimeter gridded products intercomparison” and reminded in the new “Derived products generation” section.

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11. Referee : Page 9, line 12: The phrase “The mesoscale signal is indeed better reconstructed...” in the discussion of the all-sat-merged product compared with the two-sat-merged product is ambiguous. The meaning of “better” should be defined. In lines 20-21 on this same page, it is stated that the OI parameters are the same for both products. The all-sat product therefore doesn’t have any better space-time resolution

C9

than the twosat product. So “better” evidently means “more accurate”. This is an important point because virtually all users believe that the all-sat product has higher spatial resolution, which is not the case since it uses the same correlation parameters in the OI mapping procedure. But the all-sat product is presumably more accurate. Authors: The term “more accurately” was used as recommended by the reviewer

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12. Referee : Page 11, lines 5-7: Defining the resolution of the along-track data as the point where the spectra of signal and noise intersect is too liberal. With a signal-to-noise ratio of 1, it is impossible to distinguish between signal and noise. In my experience, based on visual assessment of various fields with simulated noise added, a more reasonable choice is a S/N variance ratio of 10, which corresponds to a signal standard deviation that is about 3 times larger than the noise standard deviation. At a minimum, a S/N standard deviation ratio of 2 is needed to distinguish signal from noise (i.e., a S/N variance ratio of 4).

Authors: We agree with this comment and the revised manuscript tries to use a wording that is slightly more accurate.

The paragraph was reworded for clarity.

1)The description of the noise reduction by filtering was moved in Section 2.2.1 (see major comment #3)

2)The objectives of this filtering were better explained: removing noise preserving as much as possible the physical signal

3)The synthesis of the results fully described in the Dufau et al (2016) manuscript were simplified (see comment #13)

4)The wavelength obtained by the signal-to-noise ratio of 1 was defined as “the minimum wavelength associated to the dynamical structures that altimetry would statistically be able to observe with a signal-to noise ratio greater than 1”. A sentence was

C10

added in order to aware the reader that this is not a perfect noise removal since in practice a signal-to-noise ratio of 2 to 10 (cut-off with a wavelength of 100-150 km or more) would be required to get a noise-free topography.

We illustrate the rationale for choosing this cut-off value in Figure A. We simulated proxies of SSH following a simple K-2 law (0.99 cm RMS) and added white noise (1.22 cm RMS) to get noisy SSH samples (1.58 cm RMS). The noise floor was set so that both PSD intersect at 10 km exactly.

Following our SLA filtering method, we used as 10 km cut-off lanczos filter (green curve). As a result the smoothed topography has RMS of 0.99 cm and we removed 1.22 cm RMS of high-wavenumber signals. As expected, the filter is not perfect: we destroyed a small fraction of signal (yellow zone, 0.16 cm RMS) and we left a small fraction of the noise (red zone, 0.12 cm RMS). Figure B shows on an arbitrary sample that the filter is not perfect but generally consistent with the truth. This is why we consider this point as the smallest cut-off frequency if one wants to preserve as much signal as possible.

In contrast, we also used a 30 km cut-off filter (SNR = 10) to illustrate the difference. Figure A shows that we better smoothed out noise (red region = 0.12 cm RMS). Unfortunately we also smoothed the true signal up to 50 km (pink region, 0.3 cm RMS). The net result is therefore negative (hence 0.93 cm RMS after smoothing). Figure B shows that we did lose many small scale features that were partially observed and that could have been used.

The point where both effects (oversmoothing of true signal in pink zone VS. lack of smoothing of noise in red zone) balance out is between SNR=1.2 and SNR=1.3 (here 11-12 km). The 10 km filter (SNR=1) is slightly better if one wants to preserve the signal, and the 11-14 km filter is better if removing noise is more important. In practice, in a sample like Figure B, the difference is very difficult to see. Using a larger cut-off wavelength should be used only if perfect noise removal is the highest priority (read: to

C11

get a very pure signal estimate by sacrificing the higher wavenumbers).

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13. Referee : Page 11, line 7: It should be clarified that the 55 km mesoscale resolution capability corresponds to wavelength resolution. And the apparent contradiction between 55 km on line 7 and 65 km on line 12 should be clarified.

Authors: Additionally to the points listed in response to comment #12, the paragraph was simplified. The 55km value does not appear anymore. The 65km value is defined as the spatial mean wavelength value for which the signal-to-noise ratio of 1 is observed. As said in the manuscript, "It was defined with 1 year of Jason-2 measurement, over the global ocean excluding latitudes between -20°S and 20°N as in Dufau et al (2016).

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14. Referee : Page 14, lines 18 and 25: I am very surprised, and indeed skeptical, that changing the gridding from the original  $1/3^\circ 1/3^\circ$  to  $1/4^\circ 1/4^\circ$  is a bigger factor than the improved processing in determining the increased SSH variance of DT2014 compared with DT2010. I would have thought that the primary explanation for the increased variance in DT2014 is the reduced smoothing applied to the along-track data that are used in the OI procedure. I wonder if the authors can somehow assess the impact of the different along-track smoothing independently of other changes to the gridded products.

Authors: The smoothing applied on the along-track data that are used in the mapping process was indeed slightly reduced between DT2010 and DT2014 version, as explained in the first paragraph of "Multi-mission mapping" chapter of the revised manuscript. The correlation scales were also modified. The cumulative impact of these two changes is a +1.5% increase of the variance. The effect of the along-track filtering change was not assessed separately from the correlation scales change, but the

C12

expected impact would however be limited and mainly visible in the near equatorial latitude band, where the filtering cut-off wavelength change is maximum:  $\sim 250\text{km}$  in DT2010 vs  $\sim 200\text{km}$  in DT2014.

The authors agree that the grid resolution change from  $1/3^\circ \times 1/3^\circ$  to  $1/4^\circ \times 1/4^\circ$  does not impact the variance of the products. However as explained the authors considered the differences between DT2014 product, directly computed on the  $1/4^\circ \times 1/4^\circ$  grid, and DT2010 “QD” product that the users could download at that time. This “QD” was interpolated from native Mercator  $1/3^\circ \times 1/3^\circ$  grid as mentioned in sections 2.3.1 and 3.2.1 and this interpolation step explains 3.6% SLA variance increase in DT2014, as explained in the section 3.2.1.

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15. Referee : Page 15, lines 11-21: I find the discussion in this paragraph to be rather confusing. But in any case, I disagree with the statement on line 14 that wavelength scales of 80- 100 km are “fully observable”. With DT2010, SSH is fully resolved only for scales longer than 200 km (Chelton et al., 2011, Progress in Oceanography). It is asserted later (Page 24, line 3) that DT2014 has a wavelength resolution of 150 km (but see comment #21 below). The statement on lines 11-21 that scales of 80-100 km are fully resolved is therefore incorrect.

Authors: The discussion is indeed not clear and confusing mixing along-track and gridded products resolution. It was removed from this section and reported in the discussion section where the limitations of the DT2014 in terms of spatial resolution are discussed. The paragraph was also rephrased in order to clearly distinguish along-track and grid products. The 80-100km refers to along-track products (as discussed in Dufau et al, 2016). The authors refers to Chelton et al (2011; 2014) for the resolution of the gridded products which is estimated to be “nearly  $1.7^\circ$  i.e. slightly less than 200 km at mid latitudes”

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C13

16. Referee : Page 15, lines 29-30: It is not intuitive that the variance in DT2014 can be 20% smaller than the variance in DT2010 in low variability areas and near the eastern boundaries. It would be good to include some discussion of why this happens.

Authors: The paragraph, which indeed was confusing, has been rephrased clearly pointing at the EKE instead of SLA variance: The EKE deduced from DT2014 is globally higher than the EKE deduced from DT2010 (see Fig 9 and 10 of the reviewed manuscript). This increase in EKE observed with DT2014 reaches 20% (up to 80%) of the DT2010 EKE signal in high variability areas (Eastern boundary currents).

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17. Referee : Page 16, lines 2-5: Same comment as #14 above for SSH variance: I am very surprised that the finer gridding has a bigger impact on geostrophic current variance than do all of the other changes in the processing.

Authors: As explained in page 14 (see comment #14), the authors chose to intercompare the products delivered with the cartesian  $\frac{1}{4}^\circ \times 1/4^\circ$  resolution, i.e. DT2014 vs “QD” DT2010 dataset the users could access. The interpolation process to obtain QD from the Mercator  $1/3^\circ \times 1/3^\circ$  induces a smoothing leading to an EKE reduction.

To clarify, the sentence “Nearly 10% additional energy is the signature of the direct computation of the SLA on the  $1/4^\circ \times 1/4^\circ$  Cartesian grid for DT2014 (see Sect. 2.3.1)” was replaced by “Nearly 10% additional energy is the signature of the interpolation of the native DT2010 SLA  $1/3^\circ \times 1/3^\circ$  Mercator grid on the  $1/4^\circ \times 1/4^\circ$  Cartesian grid (see Sect. 2.3.1)”.

To quantify the effect of the DT2014 filtering, subsampling and correlation parameters, two gridded SLA experimental datasets were specifically constructed over 1 year period:

1) The first one, DT2010 like dataset, was constructed using SSH, filtering, subsampling and correlation parameters corresponding to the DT2010 processing but directly

C14

at the DT2014 grid resolution. Centered difference methodology was used for EKE computation.

2) The DT2014 like dataset was constructed as for 1), but using filtering, subsampling and correlation parameters corresponding to the DT2014 processing

→ The comparison between the two dataset shows that the temporal mean EKE from 2) is slightly less than 6% higher than EKE from 1) over the global ocean, excluding equatorial region ( $\pm 5^\circ\text{N}$ ) and high latitudes ( $> 60^\circ$ ).

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18. Referee : Page 16, lines 27-31: The error estimates are stated as 4.9 cm<sup>2</sup> in low variability areas, 32.5 cm<sup>2</sup> in high variability areas and 1.4 cm<sup>2</sup> in very low-variability areas. These numerical values seem to differ from the numbers given elsewhere in the manuscript. For example, the three numbers that are stated on lines 21-23 of the abstract on page 1. There are perhaps similar inconsistencies elsewhere in the paper.

Authors: The numbers were corrected and now should be consistent in the whole manuscript.

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19. Referee : Page 18, lines 1-15: It would probably be easier to interpret comparisons of speeds rather than velocity components since the velocity fields are very anisotropic so that analysis of each velocity component separately is difficult to interpret.

Authors: We agree that it is also interesting to analyze the current in terms of speeds. We however prefer to show differences with drifters separating the two components as the. It's of interest notably to assess the maps in each direction.

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20. Referee : Page 22, lines 5-7: Same comment as #14 and #17 above.

C15

Authors: The sentence was changed in order to point out the DT2010 interpolation processing rather than the DT2014 finer gridding. (see response comment #17)

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21. Referee : Page 24, lines 3-8: This discussion of the resolution capability of DT2014 versus DT2010 is important but confusing. On line 3, the authors assert that the resolution of DT2014 is 150 km, which can be compared with the 200 km resolution estimated for DT2010 by Chelton et al. (2011, Progress in Oceanography). The authors cite the OSTST presentation by Chelton et al (2014) for evidence of the 150 km resolution for DT2014. If they want to include this level of detail in this paper, then I think they should include a figure showing this result, rather than pointing the readers to an abstract for the OSTST meeting. On line 6, the authors again state the resolution of the along-track data to be 65 km, but as I have noted above, I believe this is a global average number. So they should refer to the cutoff as less than about 65 km, rather than a rigid  $< 65$  km. In line 8, I can't figure out where the 300 km end of the range 300-65 km comes from.

Authors: The reference to the Chelton et al. 2011 was added. The authors chose to keep the Chelton et al OSTST presentation (2014) reference in complement. The link to the slides showing the figure is given in the reference. The discussion chapter was also modified (see also comment #15).

The discussion of the comparison between SLA PDS deduced from along-track and gridded products was simplified. The range 300-65km range initially considered was replaced by the 200-65km range, and the estimation of the energy loss with gridded products was consequently adjusted. The 200km end of the range corresponds to the wavelength for which energy observed from gridded SLA is twice less important as the energy deduced from along-track measurements. This part of the manuscript was changed by: "The comparison with the spectral content deduced from full resolution AL 1Hz along-track measurements (not shown) shows that nearly 60% of the energy observed from along-track measurements on wavelength ranging 200-65km is missing

C16



in the SLA gridded products. In other words, nearly 3/5 of the small-mesoscale variability is missed with DT2014 gridded products. This is clearly linked to the mapping methodology, combined with the altimeter constellation sampling capabilities”

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Minor Comments (mostly editorial in nature)

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1. Referee : The word “restitution” on page 1, line 26 and on page 4, line 10 does not seem to be the correct word. My dictionary defines “restitution” as “the restoration of something to its original state.” I think “representation” is a better choice.

Authors: corrected

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2. Referee : The word “homogeneous” is used frequently throughout the paper. While I believe I know what the authors have in mind by this, it should be defined unambiguously the first time this description is used. In most instances, I think the word “consistency” is better than “homogeneous”.

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3. Referee : Page 2, line 26: The phrase “large panel of ocean signals” is unclear.

Authors: The sentence was rephrased as follow : “It upgrades the previous version (called DT2010; Dibarboure et al., 2011) and still pursues the same objectives that first consist in generating time series as homogeneous in terms of altimeter standards and processing with an optimal content at both mesoscales and large scales”

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4. Referee : Page 3, line 2: Chelton et al. (2011) is missing from the list of references.

Authors: The reference was added

C17

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5. Referee : Page 3, line 7: The phrase “large panel the AVISO’s users” is unclear.

Authors: “large panel of” was replaced by “different”

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6. Referee : Page 3, line 16: I think the authors mean “derived products” rather than “derivated products”. My dictionary doesn’t include the word “derivated”. This shows up also on page 4, line 26, and on page 24, line 19.

Authors: corrected

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7. Referee : Page 4, line 12: Here and at various places later in the manuscript, “ERA Interim” should be replaced with “ERA Interim Reanalysis”.

Authors: corrected

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8. Referee : Page 5, line 24: The word “homogene” should be “homogeneous”. But see minor comment #2 above.

Authors: corrected

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9. Referee : Page 5, lines 15-16: I am confused about how close to land the various altimeter measurements are retained. Here it is stated that “detection of erroneous measurements was strongly restricted in coastal areas”, but I can’t find a clear description of what these strong restrictions are. On line 17, it is stated that non-repeat data are omitted within 20 km of land. But then on page 7, line 14, it is stated that the proximity to can range anywhere from 0 to 15 km. I guess that this is for exact repeat data. What are the details on how it is decided whether an along-track measurement near

C18

land is erroneous?

Authors: The restriction of the data selection described on this section (page 5) only concerns non-repetitive missions. The section was modified in order to:

- 1) Give a reference giving all the details of the data selection processing
- 2) Clearly list the missions for which the selection processing was changed in DT2014

As deduced by the reviewer, the description given on page 7 is for repetitive tracks. This part of the manuscript indeed focuses on MPs description. As explained at the beginning of this section ("Along-track SLA generation") section, use of MPs is possible only with repetitive tracks.

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10. Referee : Page 8, line 4: "Keep one point over two" is awkward wording. I think the authors mean "keep every second point along track".

Authors: Yes. This was corrected

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11. Referee : Page 12, line 21: The use of the word "traduce" here and in other places in the manuscript is not correct. I had never seen this word before. My dictionary defines it as "to speak badly of or tell lies about". I don't think this is what the authors have in mind, but I'm not quite sure what word to substitute. Perhaps "introduces" works, at least in some cases.

Authors: the word "traduce" was replaced by "express", "show", "is the signature of" according to the situation.

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12. Referee : Page 13, line 10: What reference level was used for the dynamic height calculations?

C19

Authors: the DHA was referenced to the 900dbar level. This information was added in the text.

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13. Referee : Page 13, line 24: As noted above from Page 11, lines 11-12, I believe the 65 km is a global average, rather than a fixed value everywhere.

Authors: as explained in Major comment #13, the 65km value is a mean average of the wavelength characterized by a signal-to-noise ratio of 1. This value was retained as the low-pass filter cut-off wavelength applied on along-track data in order to reduce the signature of the 1Hz measurement noise. At this time, the filtering applied on along-track measurements is uniform over the global ocean. This 65km cut-off wavelength is thus constant and unique over the global ocean. (note that we describe in this section the impact of the filtering applied on L3 along-track product disseminated then to the users; as explained in sect 2.2.1 "Along-track SLA generation", it is different from the filtering applied on along-track SLA that are then used in the mapping process (and that are not disseminated to the users)).

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14. Referee : Page 14, lines 1-2: Define "short wavelength signal".

Authors: done. It corresponds to the wavelength < 65km

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15. Referee : Page 15, line 11: I'm not sure what "twice more important" means here. I think the authors mean to say "twice as energetic". Same comment on page 15, line 26 where it says "EKE is more important".

Authors: "twice more important" was replaced by "twice as high as"

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C20

16. Referee : Page 15, line 30: The phrase “can represent below 80%” is awkward. I think this should be “can represent less than 80%”.

Authors: the correct formulation is “it can represent up to 80%”. It was corrected in the text.

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17. Referee : Page 16, line 23: The along-track data are not entirely independent of the gridded SSH fields since the mapping procedure is based on spatial and temporal smoothing of the along-track data from the various altimeters.

Authors: by “independent” the authors means “that were not used in the mapping process”. This definition is given in section 2.3.2 “Comparison between gridded products and independent along-track measurements”

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18. Referee : Page 18, line 5: Change “meridian” to “meridional”.

Authors: done

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19. Referee : Page 18, lines 25-26: I can’t figure out what is intended by the phrase “a thinner data selection”.

Authors: the word “thinner” was replaced by “tuned more finely”

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20. Referee : Page 19, line 31: Define GIA.

Authors: The acronym was replaced by its definition: glacial isostatic adjustment

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21. Referee : Page 20, lines 15 and 16: The use of the word “regional MSL trend” and C21

“at such hemispheric scales” seems contradictory. Regional usually applies to much smaller than hemispheric.

Authors: the sentence was changed by “In order to highlight the improved MSL trend estimation between Eastern and Western hemisphere with DT2014 product, the trend deduced from altimeter products were compared to the trend deduced from in-situ T/S profiles”

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22. Referee : Page 20, lines 23-29: The word “underline” appears three times here and in many other places in the manuscript. This is not the correct word, but I don’t think a single word can capture every instance of “underline”. In some places, “emphasize” can work. In other places, “underscore” can work.

Authors: the term “underline” was replaced by the appropriate word (ie. underscore, emphasize, show, present, . . . according to the situation)

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23. Referee : Page 21, line 4: The acronym LWE is defined on page 6, line 16, but I had long forgotten its definition by the time I got to page 21. Since it only appears three times in the whole manuscript, two of them on page 21, maybe it doesn’t need an acronym that will require readers to go back 15 pages to find the definition.

Authors: The LWE acronym was replaced by its definition

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24. Referee : Page 21, line 13: The phrase “was quite two times more stronger” is unclear. I think the authors mean “was not quite two times stronger”.

Authors: the sentence was replaced by “The same comparison done with DT2010 products shows that this residual error was almost twice as high as in the DT2014 version [. . .]”

25. Referee : Page 21, line 18: Change “the last altimeter standards” to “the most up-to-date altimeter standards”. The standards used in this paper are most assuredly not the last standards that will ever be established.

Authors: corrected

26. Referee : Page 22, line 1: Describing the dataset as more energetic is not correct. It is the SSH variability in the DT2014 dataset that is more energetic than in the DT2010 dataset.

Authors: corrected

27. Referee : Page 22, line 16: The phrase “is quite 10 times less important than” is unclear. I’m not sure what the authors mean. Perhaps “is not quite 10 times as important as”?

Authors: the phrase was replaced by “is almost 10 times less important than”

28. Referee : Page 23, line 26: I believe that “between eastern and western basin” should be changed to “between eastern and western hemispheres.”

Authors: Done

29. Referee : Page 24, line 9: I think that “liked” should be “linked”.

Authors: corrected

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2015-110, 2016.

C23

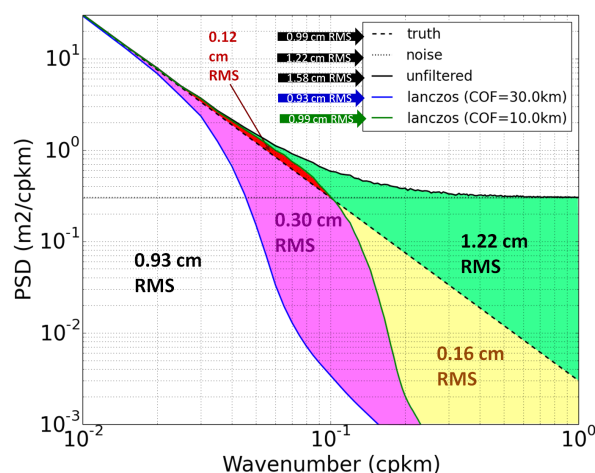


Figure A :  $K^{-2}$  spectrum (dashed) is added to white noise (dotted) to simulate noisy data (black, plain). The green curve shows the PSD after a 10 km cut-off lanczos filter is applied. The blue curve shows the PSD after a 30 km cut-off lanczos filter is applied.

Fig. 1.

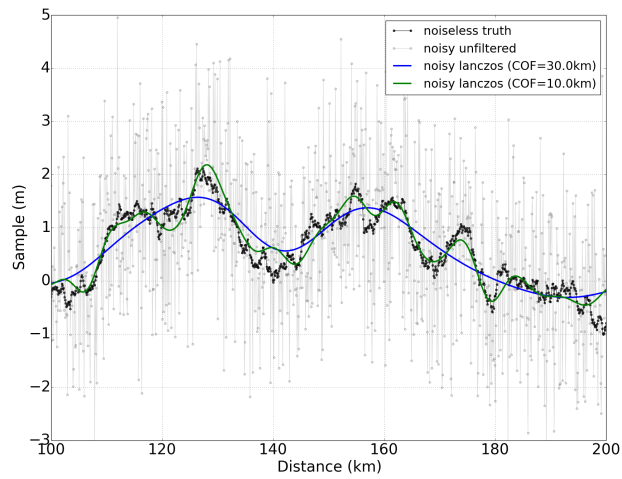


Figure B :  $K^{-2}$  spectrum (black dots) is added to white noise to simulate noisy data (grey). The green curve shows the smoothed result after a 10 km cut-off lanczos filter is applied. The blue curve shows the smoothed result after a 30 km cut-off lanczos filter is applied.

**Fig. 2.**