

Interactive comment on “An Accurate and Homogeneous Altimeter Sea Level Record from the ESA Climate Change Initiative” by Jean-François Legeais et al.

Anonymous Referee #3

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Title: An Accurate and Homogeneous Altimeter Sea Level Record from the ESA Climate Change Initiative

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General Comments:

This paper presents the ESA CCI v2.0 sea level dataset, as introduced in detail by Quartly et al. (2017). The dataset described is an advance from the initial v1.1 release,

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and represents a significant contribution to the user community. The data has unique components, is largely complete and is considered to be useful for a wide user group. The paper is generally well structured and well presented. I have identified a number of issues that require clarification – these largely concern how some data quality issues are articulated and presented within the manuscript. I have also identified a number of minor corrections that should be considered. With this in mind, I consider the work to be within scope of ESSD and worthy of publication pending consideration of the issues articulated below.

Major Issues:

1) There is somewhat of a contradiction that flows through this manuscript, and remains present into the conclusions. The work claims to present an accurate GMSL record, yet awkwardly introduces the systematic bias associated with TOPEX-A, as brought to attention by Watson et al. *Nat. Clim. Chg.* (2015). The work also cites Valladeau et al (2012), Dieng et al (2017) and Beckley et al (2017) around this issue, noting the omission of a citation of the relevant Chen et al., *Nat. Clim. Chg.* (2017) work.

As the authors correctly highlight, these recent advances have led to a downward systematic revision of the rate of change in GMSL (that isn't accounted for in the CCI v2.0 dataset). Given the news piece by Tollefson (*Nature News*, 2017) and subsequent letters to the editor by Nerem, Cazenave and Church (*Nature*, 2017), it is important to avoid confusion on this issue.

This manuscript describes a dataset that *does not* correct for the TOPEX related issues, yet parts of its accuracy assessment (e.g. assessment of the sea level budget, Figure 9) *does* correct for these issues (Pg8, Line 25), thus presenting a somewhat misleading perspective. The authors also present trend differences (record minus ensemble mean) as a measure of how robust the CCI record is (Table 1) – this is somewhat misleading as all of these records contain the error associated with the TOPEX record. I'm not convinced the related uncertainty section is robust without at least some

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discussion of issues associated with TOPEX (in particular, the A/B bias, retracking etc) I contend that these issues need to be made entirely clear in a revised manuscript. The conclusions need to include some remarks on this important issue, pointing towards the future release of a reprocessed TOPEX dataset. I would also recommend revising the title to “An improved and homogeneous...”.

2) The section on regional rate uncertainties is presently poorly evidenced. The manuscript cites work that is “in prep”, and no real mention of meaningful uncertainties around regional trends are provided. This should be addressed in a revised version of the manuscript.

Mostly Minor Issues (P=page#, L=line#, suggested rewording is given between dots “...”):

P2 L4: Reference required after GCOS requirements

P2 L7: Global Mean Sea Level is abbreviated here as MSL but later as GMSL

...heat added to the ocean...

P2 L8: ...monitoring of sea level...

P2 L10: validation of climate models

P2 L11: “supposed to correctly” isn’t this implicit? I recommend removing.

...altimetry mission have delivered...

P2 L13: Suggesting adding e.g. before these references as this is not exhaustive

P2 L17: ...This program aims to realise the full...

P2 L20: ...distributed to the user community.

P3 L5: I don’t think these brackets are required.

P3 L11: The sentence beginning Thus a single... doesn’t make sense and needs to

be reworded.

P3 L16: The early section defines v2.0 incorporating 2014-2017 data. This needs to be clarified – it is really 2014-2016 inclusive? If so, does it include Jason-3. This needs clarification.

P3 L19: ..since sea level estimation from altimetry requires. . .

P3 L21: Again, insert e.g. before references.

P3 L23: . . .most appropriate solution/algorithm to ensure. . .

P3 L26: remove the word analysis

P3 L31: remove quotes from around pole tide

P4 L11: . . .SL_cci products (including the SLA) (reference). . .

P4 L12: . . .both datasets should be. . .

P4 L31: . . .for users. The . . .

P5 L8: . . .has been carried out over different spatial and temporal scales.

P5 L12: Here the CI is given as 95% but on page 14 it is 90%. Clarify.

P5 L12-25: See my significant remarks around how this section is articulated (and should be improved) in the top section of my review.

P5 L16: . . .However, over decadal time scales, the v2.0 GMSL trends are significantly different to those from v1.1. . .

P5 L25: I notice here that Chen et al. Nature Climate Change, 2017 is not referenced, and should be.

P5 L25: After mentioning the 1.5 mm/yr in terms of the GMSL trend, the date period for this metric should be provided.

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P5 L28: GMSL rise is not accelerating, GMSL is accelerating. Rise is typically thought of as a rate of change. That rate is increasing, i.e. the level is accelerating. You have one too many derivatives here.

P6 L6: ...ocean models. Users interested...

P6 L11: ...correction, and present in the Radar... (This sentence is unclear, why is RADS introduced here? Clarify).

P6 L20: ...smoother periodic signal... (it is unlikely to be a pure sinusoid)

P6 L21: Figure 5 – is this correct? I'm surprised by the lack of agreement in the annual terms from other labs. Please confirm this has been computed correctly using the same smoothing across all data centres.

P6 L26: ...sea level difference computed against in-site data...

P6 L32: What independent data? Clarify.

P7 L4: ...both datasets...

P7 L9: ...Southern Ocean, the...

P7 L16: Are these differences significant given the difference in data span? See my remarks around how regional uncertainties are addressed in the top section of my review.

P7 L32: ...variance estimated...

P8 L14: The glacier mass term in the equation has an additional (t) in it that needs to be removed.

P8 L17: ...refer to changes in mean glacier, Greenland and Antarctic mass balances, land water storage (LWS) and atmospheric water vapor (AtmWV).

P8 L25: See my significant remarks around how this section is articulated (and should be improved) in the top section of my review.

P8 L30: The use of “ensemble mean” is slightly unclear. . . in this section, is the ensemble mean referring to the sum of the terms in the budget? This needs to be clarified in captions and text.

P8 L31: . . .that looking at solely the trend does not allow. . .

P9 L1: This statement is somewhat disingenuous – it is in fact equal closest as the CSIRO dataset has the same difference.

P9 L3: Regarding Fig 10, around 1996-1999 it appears CU and CSIRO are quite different to the ensemble mean, but CCI is not. Interestingly, CU and CSIRO seem anti-correlated. This doesn't appear to be mentioned in the manuscript but is likely an important point worthy of a remark.

P9 L4: This sentence could be improved – “no anomaly” is not exactly the case (refer to the TOPEX issue, see comments at the top of my review) and “better” is a poor choice of words, consider revising.

P9 L8: . . .using the NEMO. . .

P9 L14: “freshwater constraining” is constraining the correct word here?

P9 L18: . . .which makes the estimation of GMSL (and its seasonal cycles) non trivial.

P9 L26: . . .(middle panels, Fig. 11). . .

P10 L25-30: This discussion does not mention regional uncertainty at all.

P11 L10: . . . gyre. Consistent with the findings presented here, it is possible to. . .

P11: Sometimes SPG is abbreviated, sometimes it isn't, be consistent.

P11 L14: Is advanced the best word here? Perhaps new?

P11 L26: This repeats line 14. Revise.

P12 L34: datasets (no space)

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P13 L1: ...improve much compared. . .

P13 L2: one dec place to be consistent

P13 L27-30: Font issue here

P14 L6: ...In Figure 18, annual. . .

P14 L8: ...represented (for model datasets, the thermos-steric component is used).

P14 L10: Put see reference in brackets.

P14 L13: ...which have high negative trend values, especially... The especially part here is ambiguous as the sentence refers to differences between models, and then between modes and obs. Suggest reword this sentence.

P14 L15: Error not Errors

P14 L17: estimation not estimations

P14 L19: 90% or 95% as earlier indicated?

P14 L25: This section is weak – there is no robust discussion around regional uncertainty. The reference is “in prep”. If this is published, update, else remove and reword this section. There should be discussion around the noise model used to estimate “order of 2-3 mm/yr”, as I’m sure that isn’t a 90% CI value when an appropriate noise model is used. I also note that this section doesn’t mention the reference frame or issues associated with inter/intra mission biases (in particular TOPEX A/B). For the later, I refer back to my overarching remarks in the top section of my review. For the former, it may be worth adding a sentence and referring to Ablain et al (2015) if appropriate.

P15 L5: The conclusion fails to mention a major issue present in the record. See overarching comments in the top section of my review.

P15 L7: Improved rather than accurate?

P15 L9: spatial scales

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P15 L25: Defining GPD+ this way masks the fact it includes numerous other radiometers – perhaps worth a mention?

P15 L27: ...At the regional scale...

P15 L29: These differences are also a factor of the difference in time period, correct?

P16 L16: ...are still not reached over some specific spatial and temporal scales (e.g. ...

P17 L18: Spelling/font issue at end of line

P17 L21: Ref needs updating. Incorrect abbreviation as well.

P19 L33: Is this accepted yet?

P20 L4: Authors shouldn't cite in prep work. Update or remove.

P20 L33: This ref is missing some additional bibliographic info.

P22 Table 1: Make it clear that “Ensemble mean” is mean of observed GMSL. This table is also partly disingenuous as it presents the agreement in trend as evidence the product is robust. In fact, as shown by Watson et al (2015), later confirmed by Chen et al (2017), Dieng et al (2017) and Beckley et al (2017) these records are all in error. To avoid confusion, this needs to be clearly stated.

P22 Table 2: The time frame of CCI v2.0 was mentioned as 2014-2017 – the difference in date ranges here needs clarification.

P22 Table 2: What is the source of the user requirements?

P23 Figure 1 caption: ...ECV). Annual and semi-annual signals have been removed.

P23 Figure 2 Units on the colour bar. Font size here is pretty small!

P24 Figure 3 Why not combine this figure with Figure 1? (and separate that into 1 and 2?). Yellow is a poor colour choice here as it is hard to see.

P24 Figure 4 I recommend removing the lines from the legend, or at least from the

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1993-2014 label as this isn't actually shown. Why does this figure only go up to 2015.0 whereas Figure 3 goes up to 2016.0?

P25 Figure 5 As per comment, is this really correct?

P27 Figure 9 This figure has a title which is inconsistent with other figures. This figure is also inconsistent with the dataset provided – it has corrections for the TOPEX issue – see comments in the top section of this review.

P29-31 Figures 11/12/13: Poor resolution in the PDF I have.

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2017-116>, 2017.

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