Dear Editor and Reviewers,
We sincerely appreciate the time and effort you have dedicated to reviewing our manuscript. Your thoughtful and constructive comments have been instrumental in improving both the quality and clarity of our work.

Please find below a key revision we have made according to Reviewer 1's Comment 3 and Reviewer 2's Comment 5:

We identified an inconsistency in the treatment of atmospheric pressure data during the oceanic tidal extraction process. Upon thorough re-examination of our computational procedures, we found that in the calculation of Eq. (8) described in *Section 3.2.2 Oceanic Tidal Constituents*, the sea level pressure data (P_a) had been linearly interpolated from 6-hourly to 3-hourly intervals. However, in the LICOM OBP simulation as defined in Eq. (7), the 3-hourly P_a values were derived by holding the 6-hourly values constant — i.e., the values at 03:00, 09:00, 15:00, and 21:00 UTC were set equal to those at 00:00, 06:00, 12:00, and 18:00 UTC, respectively.

This 3-hour time mismatch introduced residual atmospheric pressure signals into the oceanic contribution to OBP, resulting in spuriously correlated oceanic tidal constituents with atmospheric tidal components, as evident in the original Figure 4. After correcting this mistake, we recalculated the oceanic tidal constituents. The revised results are more consistent with the observations in the AOD1B RL06 documentation, in terms of the spatial pattern. Nevertheless, due to the absence of explicit atmospheric pressure forcing in LICOM and the limited temporal resolution of its forcing fields, the amplitude of simulated oceanic tides remains underestimated, particularly the OCN-S2 constituent.

In light of these corrections, the CRA-LICOM non-tidal product has been updated and republished at: https://data.tpdc.ac.cn/en/data/4c18c7a0-da0a-4eac-95f8-349a2986dc44. Additionally, all relevant figures in the manuscript (Figures 2–4, 6–10, and 12) have been updated. Figure 4, depicting the tidal constituents, shows substantial changes, while the other figures reflect only minor differences. The revised figures are incorporated into the marked-up version of the manuscript, submitted as an attachment to the Author Comment.

Finally, we have carefully addressed each of your comments in a point-by-point manner. Detailed responses, along with corresponding manuscript revisions, are provided below.

Thank you again for your valuable feedback.

Sincerely,
Jiahui BAI (On behalf of all authors)

Response to the Editor

Topic editor initial decision: Start review and discussion (by Benjamin Männel):
Dear authors,
Thank you for the submission to ESSD. Based on the submitted manuscript, I started the review process. In the upcoming revision, I suggest clearly indicating the difference to the previous study by Zhang et al. 2025. Furthermore, I kindly ask you to update the GFZ name, which is now "GFZ Helmholtz Centre for Geosciences".

Response:

We sincerely appreciate the opportunity to revise and improve our manuscript. Below, we address each comment in detail and have incorporated the suggested improvements in the revised manuscript. Specifically, the comments are highlighted in blue, and our responses and actions taken to address those comments are in black. The key information in the responses is underlined.

According to your suggestion, the full name of GFZ has been updated in the revised manuscript.

Regarding the difference from our previous work by Zhang et al. (2025), we would like to clarify from the following two aspects:

1. Zhang et al. (2025) only address the atmospheric de-aliasing component; however, this study focuses on a complete atmospheric and oceanic de-aliasing. In addition, due to the absence of an oceanic compartment in Zhang et al. (2025), the consistency and coupling between the atmosphere and ocean were overlooked by Zhang et al. (2025); for example, the ocean mask used by Zhang et al. (2025) differs entirely from ours.
2. The emphasis of Zhang et al. (2025) is on the development of methodology for utilizing pressure-level CRA-40 reanalysis for atmospheric de-aliasing. However, this study directly adopts the method proposed by Zhang et al. (2025) and focuses on the product evaluation and validation. In addition, as an attempt to utilize CRA-40, the data record of Zhang et al. (2025) is limited; however, the product generated from this study significantly extends the record, covering the period from 2002 to 2024.

The differences mentioned above are also included in the revised manuscript to avoid confusion; see lines 204-205 in the revised manuscript.

Response to the RC1

I just read the manuscript "CRA-LICOM: A global high-frequency atmospheric and oceanic temporal gravity field product" submitted by Fan Yang et al. for possible publication in Earth System Science Data. The text describes a non-tidal atmosphere and ocean dealiasing product that is intended to minimize temporal aliasing artifacts in data products obtained from the sensor data of the GRACE/-FO satellite gravimetry missions. The paper is generally well written and fits nicely into the scope of the journal. However, a number of issues need to be resolved before publication can be recommended.

We are grateful for your positive assessment and the insightful comments, which have significantly improved our work. In particular, both you and the second reviewer raised a critical concern regarding potential errors in our oceanic tide estimation. After extensive re-examination of the data and code, we have confirmed and resolved this issue. A detailed note addressing this correction has been placed on the first page of this letter for transparency to editors, reviewers, and readers.

Fixing this error has led to substantial improvements in both tidal and non-tidal components of our analysis. Consequently, all relevant figures and results have been updated throughout the manuscript. The revised CRA-LICOM product, now with enhanced quality, has also been updated and publicly released. We deeply value the contributions of both reviewers in strengthening this manuscript and the associated dataset. We hope the revised version meets ESSD's high standards and welcome any further feedback you may have.

Comment 1. *(1) The paper discusses in great detail the tides simulated in CRA-LICOM and compares those tidal signals in both atmosphere and oceans to the signals found in ERA5 and MPIOM. On the other hand, authors are also stating that tides should be treated separately in the gravity field processing (line 52). I agree to that notion and suggest that authors just make sure that no distinct periodic variations associated with tides remain in their CRA-LICOM AOD data. Please note that those tidal lines can differ from the frequencies considered in AOD1B RL07 for reasons associated with, e.g., the differing temporal sampling. Figure 4 should be revised to include figures of all partial tides that were considered as relevant. A 1:1 comparison to AOD1B RL07 can be, however, omitted.*

Response:

Thank you for the constructive advice. As you suggested, due to the different temporal sampling, the tide lines estimated must also differ from AOD1B-RL07. In our study, since the sampling rate is 6 hours, we have removed all feasible tides (slower than 12 hours) to make sure that no evident periodic variation remains. The details of estimated tide lines have been addressed in *Section 3.1.1* and *Section 3.2.2* in our revised manuscript. Please be aware that our product still contains a major atmospheric tide called S_2 (with a frequency of 12 hours), which cannot be well fitted from the 6-hourly time series due to the limitation of the Nyquist sampling rule. In this sense, our product, compared to GFZ-RL07, is not a perfect non-tidal product. <u>However, this does not affect the use of our product for satellite gravimetry, if one can avoid the double bookkeeping of S_2</u>.

Additionally, we retained the original Figure 4 (to illustrate the major tides) in *Section 4.2* for clarity and readability of the main text. But as suggested, a more comprehensive figure that includes all partial tidal constituents considered in the analysis has been added as Figure R1 (Figure B3 in the revised manuscript) in *Appendix B*.

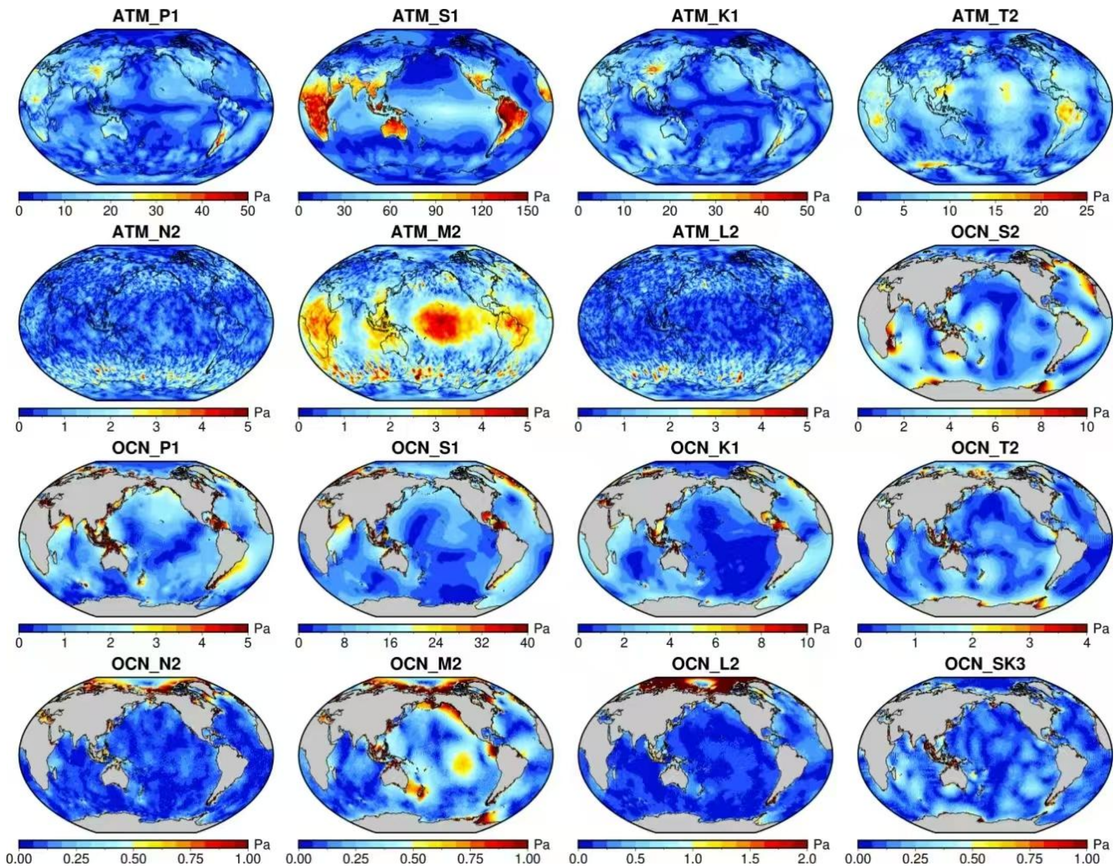

Figure R1 (Figure B3 in the revised manuscript). Amplitudes (Pa) of selected tidal constituents estimated by CRA-LICOM over 2007–2014. The first seven subfigures show atmospheric (ATM) tidal constituents, while the following nine subfigures present oceanic (OCN) tidal constituents.

Comment 2. *(2) LICOM misses atmospheric surface pressure forcing (line 230) and consequently misses a part of the high-frequency excitation of ocean bottom pressure. This limitation of LICOM should be emphasized more prominently in the article.*

Response:

Thanks for your helpful suggestion. We agree with the importance of atmospheric surface pressure in OBP simulations and have added the discussion in *Section 6*. Please review details on lines 520–524 in the revised manuscript.

Comment 3. *(3) The oceanic component of S2 appears to be a consequence of the applied IB correction. It would be adviseable to perform the de-tiding first and apply the IB correction afterwards.*

Response:

Thank you for the enlightening comments! The oceanic components of all tidal constituents, including S_2, were previously contaminated by residual atmospheric surface pressure signals in the calculation of the oceanic contribution to OBP. Specifically, the OBP simulated by LICOM includes 3-hourly sea level pressure (P_a) values that were populated by repeating the 6-hourly data — i.e., values at 03:00, 09:00, 15:00, and 21:00 were identical to those at 00:00, 06:00, 12:00, and 18:00, respectively. However, in the process of calculating the oceanic contribution to OBP (as described in Eq. (8), *Section 3.2.2*), P_a was linearly interpolated from 6-hourly data to 3-hourly resolution. This inconsistency led to artificial residuals in the value of $P_b - P_a$ (P_b is OBP), resulting in oceanic tidal components that closely mirrored atmospheric tidal patterns, as shown in the original Figure 4. Additionally, we would like to clarify that CRA-LICOM and AOD1B RL06 use different approaches to isolate the non-tidal dynamic OBP. Unlike ocean models with forcing fields including atmospheric pressure, the vertical integration of seawater density simulated by LICOM does not incorporate the influence of atmospheric pressure. Therefore, no IB correction is needed or applied after the de-tiding process in CRA-LICOM.

After correcting the P_a used in the oceanic calculation, we re-extracted the oceanic tidal constituents. All selected tidal constituents are presented in Figure B3 (see Comment 1). As shown, the spatial patterns of the oceanic tidal constituents no longer resemble their atmospheric counterparts, but instead agree better with those from the AOD1B RL06 product (Dobslaw et al., 2016). Accordingly, the major tides illustrated in the original Figure 4, as well as the accompanying description, have been updated. Please see lines 329–338 in the revised manuscript.

Comment 4. *(4) The spin-up period of the model experiment appears to be rather short: please provide a plot comparing the drift in your model experiment with the drift in the various spin-up cycles to demonstrate that there is no artificial drift present in the product.*

Response:

Thank you for your helpful suggestion. Figure R2 below shows the annual mean time series of global mean temperature and salinity from CRA-LICOM during six spin-up cycles under atmospheric forcing from 2002 to 2023. The SST and SSS reach equilibrium within six spin-up cycles. The relatively small trends in VOT and VOS result from the small imbalance in surface heat and freshwater fluxes, and no artificial drift remains in the system. The figure below has been added to *Appendix A* as Figure A1 in the revised manuscript. Please see lines 607–610 in the revised manuscript.

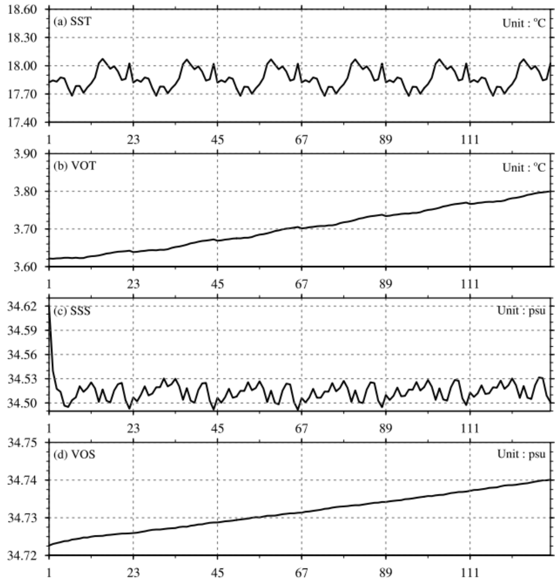

Figure R2 (Figure A1 in the revised manuscript). Annual global mean (a) sea surface temperature (SST; units: °C), (b) volume ocean temperature (VOT; units: °C), (c) sea surface salinity (SSS; units: psu), and (d) volume ocean salinity (VOS; units: psu) for CRA-LICOM during all the six cycles. The x-axis represents model time (units: Year).

Comment 5. *(5) It is surprising to read that the adopted value for g differs from the WMO constant (line 225). Please explain your choice in more detail.*

Response:

Thank you for your careful review. Actually, we adopted the WMO value as you suggested. However, in the main text, we made an error in rounding the number. We have corrected the *g* value from "9.806" to "9.80665" (line 242) in the revised manuscript.

Comment 6. *(6) Line 285: The "Earth's gravity system" reads odd. Please revise.*

Response:

Thank you for your careful review. This has been revised to 'Earth's gravity field' (line 335).

Comment 7. *(7) Line 307: The potential double bookkeeping of S2 has been an issue with AOD1B RL04 and earlier versions. since that time, AOD1B is defined as purely "non-tidal" and all atmospheric tides (including S2) need to be corrected with separate models. Please revise the statement.*

Response:

Your statement is accurate and correct. We apologize for the unclear description in the original manuscript. In the revision, we follow your statement and clarify that this issue only occurs before the RL04 version, as seen on lines 325-328.

Comment 8. *(8) Figure 6 is not really insightful, since the signal characteristics are so different between oceans and land. I suggest to explore alternative ways for the comparison with the official AOD1B RL07 product by GFZ.*

Response:

Thank you for your helpful suggestion. Considering the distinct behaviors of oceans and land, we decided to illustrate the comparison of ATM (mainly over land) and OCN (over oceans) separately in Figure 6. The corresponding analysis is updated on lines 365–376 of the revised manuscript as well. In this manner, we feel the comparison with AOD1B RL07 is more specific and more informative. The updated figure is shown in Figure R3 as follows.

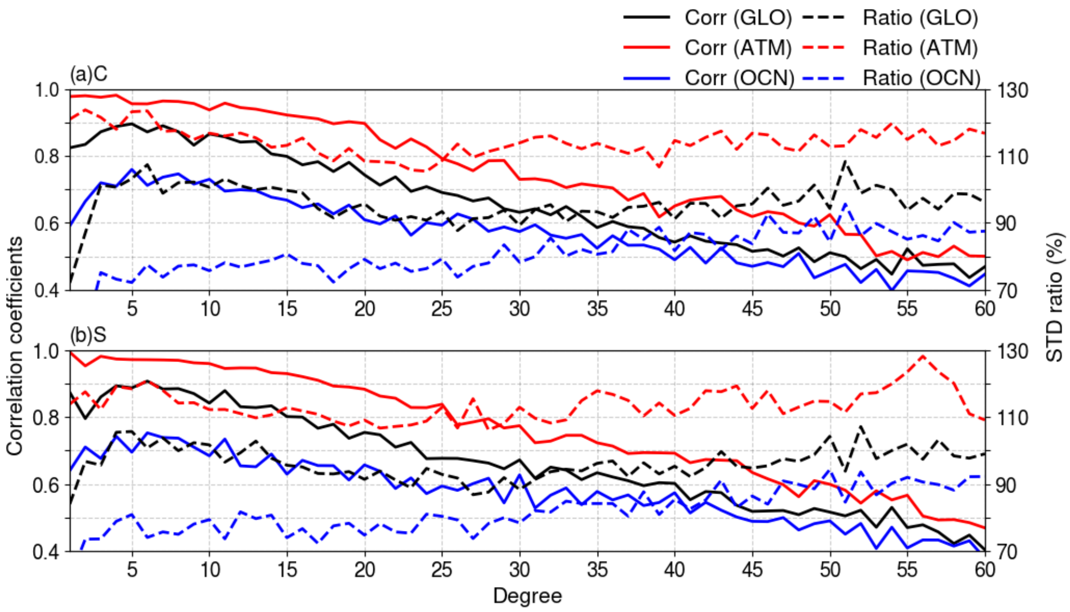

Figure R3 (Figure 6 in the revised manuscript). Mean temporal correlation coefficients (solid) and variation bias (dashed) for (a) C and (b) S of GLO (black), ATM (red), and OCN (blue) at each spherical harmonic degree between CRA-LICOM and GFZ-RL07 during 2002-2024.

Comment 9. *(9) It would be nice to plot postfit residuals in Figure 8 instead of the prefits. Many of the most prominent features in your plots will disappear so that smaller details become visible (and can be discussed).*

Response:

Thanks very much for sharing your insight. In the revised manuscript, we have illustrated the postfit rather than the prefit residuals, as suggested. And thanks to this change, the postfit-residual differences become much more prominent and thus more informative to readers as well. In addition, the spatial pattern of the postfit-residual differences is more consistent with the product differences, which is more aligned with our expectations. However, according to the statistical analysis, the main conclusion remains unchanged: GFZ-AOD1B slightly outperforms CRA-LICOM, but their differences are rather small, i.e., the global RMS is 39.85 nm/s, which is likely insensible for GRACE. Please refer to the detailed description on lines 415-428 of the revised manuscript, and the updated figure is attached as Figure R4 for your review.

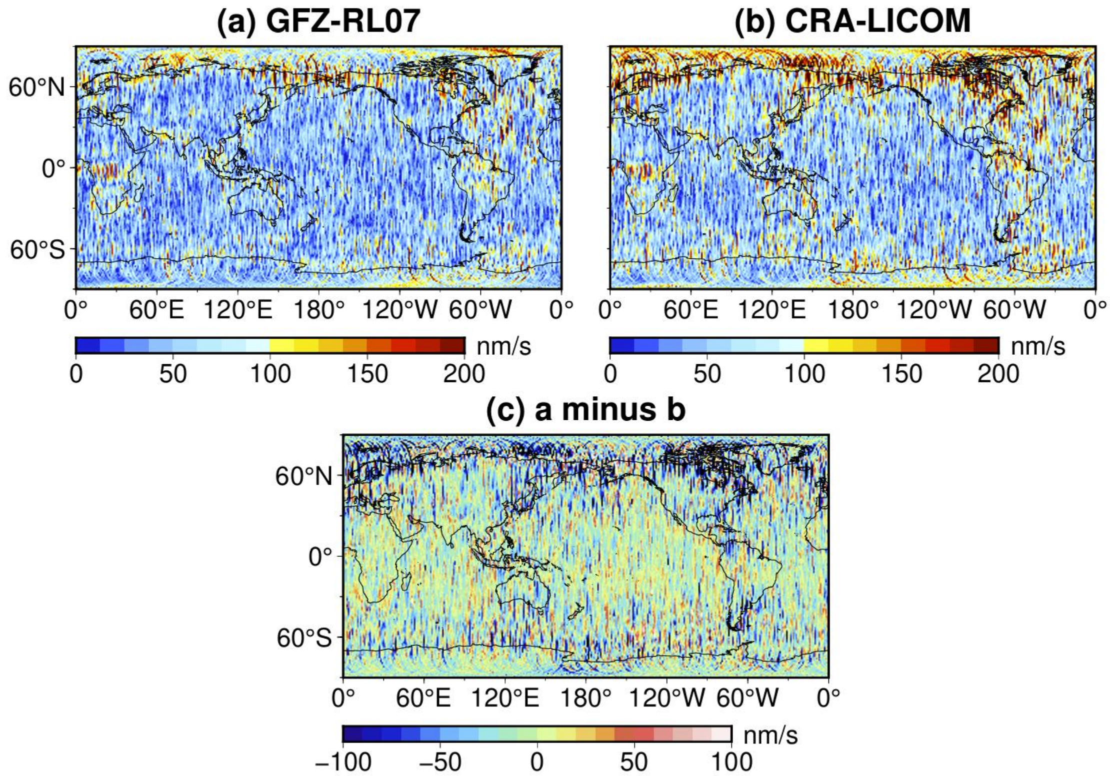

Figure R4 (Figure 8 in the revised manuscript). Postfit KBRR-residuals for GRACE using AO product, i.e., (a) GFZ-RL07 and (b) CRA-LICOM, respectively. One-month KBRR residuals in December 2010 were first assembled as gridded RMS (root mean square) by GRACE's ground track (mid of twin satellites) and projected into a map of $1^\circ \times 1^\circ$. The grid with a negative value at the map (c, GFZ-RL07 minus CRA-LICOM) may indicate where GFZ-RL07 outperforms CRA-LICOM and vice versa.

Response to the RC2

This paper presents the development and characteristics of CRA-LICOM, a new global atmosphere-ocean (AO) de-aliasing product for use in satellite-based gravity field computation. Comparison against the widely adopted de-aliasing model by GFZ (AOD1B RL07) and several types of observations show that CRA-LICOM is a reasonably accurate representation of atmospheric and oceanic mass changes on submonthly to interannual time scales. I commend the authors for their comprehensive effort (both in analysis and presentation) and think the manuscript should be considered for publication in ESSD after appropriate revisions. Many of my comments are minor, mostly addressing issues with the writing – see the attached PDF. There are nevertheless a few points that warrant being highlighted in my written assessment:

We appreciate your positive response to our manuscript. In addition to your comments below, we have carefully addressed all your annotations in the PDF. Please see the revised manuscript. Thank you very much for your efforts and time on those specific annotations, which we believe have greatly improved the quality of our AO product as well as this manuscript.

Comment 1. *#1: The description of several key datasets, particularly CRA-40 (Section 2.1) and the GFZ-RL07 AO model (Section 2.2), is too succinct. This makes it hard for a reader to appreciate differences between GFZ RL07 and CRA-LICOM and the underlying atmospheric data assimilation products, i.e., CRA-40 and ECMWF (re)analyses. I suggest that the authors include a brief synopsis of the GFZ modeling strategy and add more details on CRA-40 as a data-constrained reconstruction of the atmospheric state.*

Response:

Thanks for this constructive suggestion. We agree that a clear description of both CRA-40 and the GFZ-RL07 AO model is important. Accordingly, in the revision, we have clarified the independent sources of atmospheric and oceanic data used in the two products — CRA-40 and LICOM for CRA-LICOM, and ECMWF reanalysis and MPIOM for GFZ-RL07. In addition, more details on CRA-40 have been added to lines 90–96 in *Section 2.1*, and a brief summary of the GFZ modeling strategy has also been added to lines 107–116 in *Section 2.2.1*. We believe that these changes can make it easier for readers to follow the difference between the two AO products.

Comment 2. *#2: Similar to #1, too little detail is given on how the DART data were processed (Sections 2.2.4 and 5.2). The series at individual locations are made up from different recordings, have jumps, drifts, outliers and discontinuities, and primarily measure the tidal signal. Without proper consideration, these effects will corrupt any comparison to model-based or satellite-based OBP fields. Imperfect handling of the in situ data could be another source for the systematic biases noted near line 419.*

Response:

Thank you for your helpful suggestion. We apologize for the lack of clarity regarding the description of the DART data. The in-situ observations used in this study are already quality-controlled and de-tided OBP data from DART, whose processing procedures have been systematically addressed by Mungov et al. (2013), and that's why we did not specify the details in the original manuscript. Additionally, we have performed necessary post-processing of the DART data. Specifically, we converted the 15-second non-tidal OBP into hourly mean data and selected timestamps every six hours to ensure consistency with our GAD (from CRA-LICOM) product. As you suggested, these details and the reference have been added to lines 137–140 in *Section 2.2.4*. Nevertheless, we agree with your opinion and acknowledge the potential error coming from DART, which has been added to the revised manuscript, see lines 463–464.

Reference:

Mungov, G., Eblé, M., and Bouchard, R.: DART® Tsunameter Retrospective and Real-Time Data: A Reflection on 10 Years of Processing in Support of Tsunami Research and Operations, Pure and Applied Geophysics, 170, 1369–1384, https://doi.org/10.1007/s00024-012-0477-5, 2013.

Comment 3. *#3: Section 5.2, also on the comparison of CRA-LICOM against bottom pressure recorders: It is unclear whether this analysis was conducted based on monthly averages or high-frequency (e.g., daily) fields and what the contribution of the dominant seasonal/annual cycle is to the statistics shown in Figure 10. Regarding the authors' speculation about the vicinity to land (lines 420 to 423), this should not be an issue at d/o 180. I simply recommend removing the passage.*

Response:

Thank you for your insightful suggestion. The comparison between CRA-LICOM and in-situ observations was originally based on six-hourly non-tidal OBP datasets spanning 2002–2023, which contain signals across a wide range of timescales, from periods longer than 12 hours to those shorter than half the length of the time series at each station. In these records, the seasonal and annual cycles dominate variability at periods longer than approximately six months. However, for stations with relatively short observation periods, these low-frequency components are either incomplete or poorly resolved. As our analysis focuses on the performance of six-hourly OBP simulations, we did not conduct a detailed investigation of seasonal or annual signals in this study. We acknowledge that this is an important aspect and will consider it in future work.

Upon further examination, we identified an error in the previous version of Figure 10, the CRA-LICOM dataset used was the non-tidal dynamic OBP, which excludes the contribution of atmospheric pressure and therefore does not align with the in-situ observations. We have corrected this by using the appropriate non-tidal OBP from CRA-LICOM, which includes both the vertically integrated seawater density and the global mean sea level pressure. A revised comparison and analysis between the CRA-LICOM simulations and the in-situ data have been updated, and the revised figure is shown below as Figure R5. Please see the details on lines 457–464 in *Section 5.2*. In addition, we appreciate your comment regarding the discussion on lines 420–423 in the original manuscript, and we have followed your suggestion and removed the relevant text.

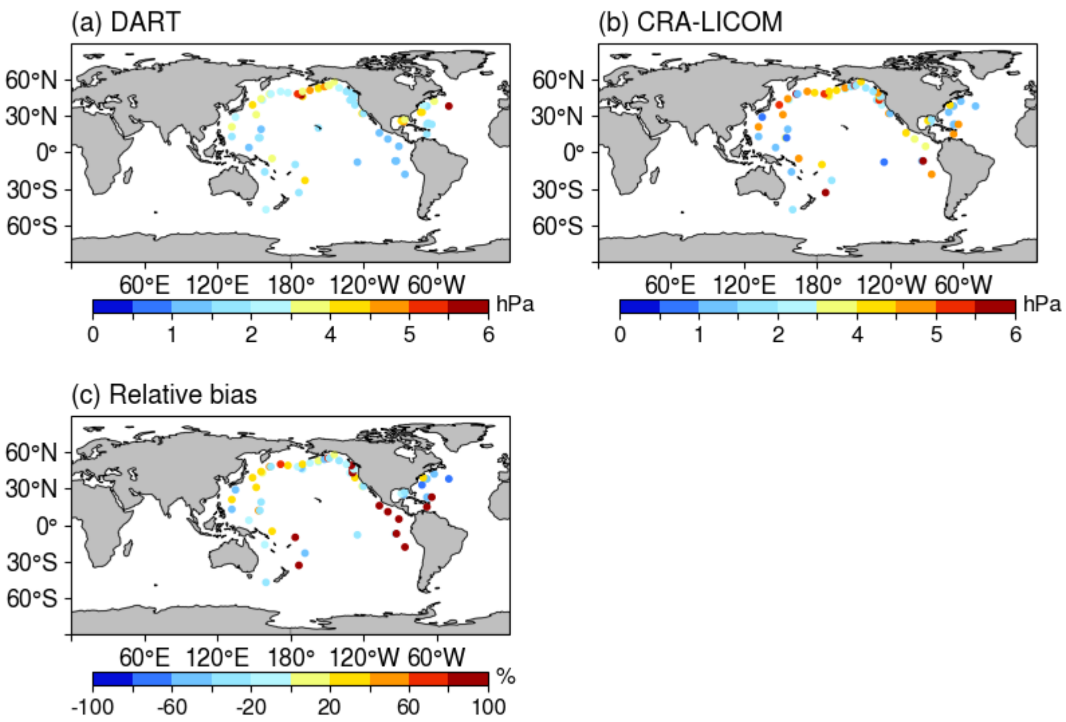

Figure R5 (Figure 10 in the revised manuscript) Standard deviations (hPa) of 6-hourly non-tidal OBP from (a) DART and (b) CRA-LICOM during 2002-2023. Panel (c) shows the relative bias between CRA-LICOM and DART.

Comment 4. *#4: I am surprised by the absence of S2 from the list of eliminated atmospheric tidal constituents (Section 3.1.1). Yes, S2 is not represented properly with 6-hourly sampled model output, but even as a standing wave, it can contribute substantially to the daily surface pressure variability at low latitudes. One way to pull the S2 signal from 6-hourly fields is to form, at each grid point, a daily mean composite (i.e., average over all 00UT data points, all 06UT points, etc.) and subtract that composite, day after day, from the full pressure time series. This will also absorb some of the S1-related variability, but the residual S1 signal is estimated as part of the harmonic analysis in the next step anyways.*

Response:

Thanks very much for sharing the insight. Initially, S2 was not removed from our product due to the limitation of a 6-hour sampling rate, and on the other hand, <u>we think it should be safe to retain S2 in the AOD product</u>, because

1. For satellite gravimetry, S2 should be accounted for as a background force model. This can be achieved either by a separate atmospheric tide background model (Dobslaw et al., 2016), or by AOD that contains S2 already. While the former approach is the recommended choice, the latter can be a compromised option when S2 cannot be removed from AOD, as is the case in our scenario. In fact, this deficiency also existed in the official AOD product before release 04. But in theory, for satellite gravimetry, AOD that contains S2 is not a big issue if one can avoid the double use of S2. This has also been addressed in <u>Comment 7 of Reviewer #1.</u>
2. In general, while S2 is known as a major atmospheric tide, all atmospheric tides collectively have a relatively small contribution compared to other background force models. In Lasser et al. (2020), all forces that a satellite gravity mission experiences are sorted by their contribution

or magnitude, where one can see that the atmospheric tide is the smallest. Therefore, containing S2 in AOD should be safe, although this is not a perfect strategy.

However, your approach makes it possible to remove S2 from our de-aliasing product for now. Therefore, we have followed your approach and obtained an alternative atmosphere de-aliasing product that does not include S2 (Figure R6). Here, for the distinction, the old product is named after AD1, and the new product is named after AD2. Therefore, although S2 cannot be modelled, one can retrieve its influence at any given time epoch by calculating the difference between the two products, i.e., AD1 minus AD2. The S2, as indicated by this approach, is named after 'Method-I'.

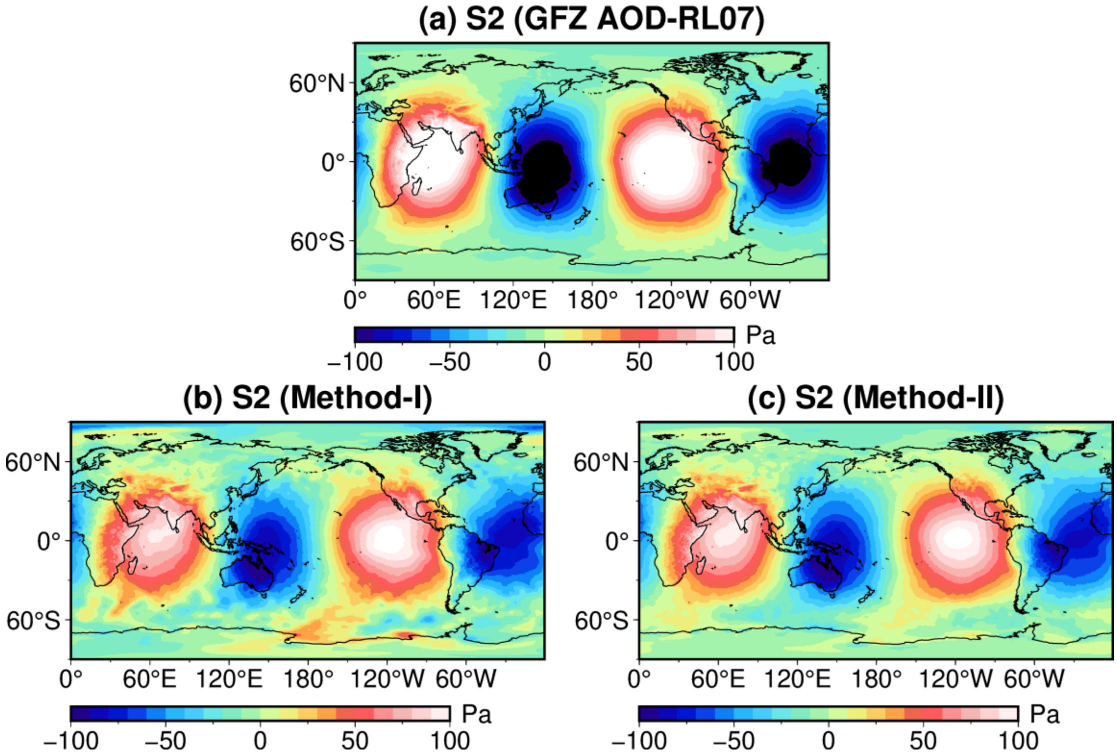

Figure R6. A comparison of S2 tide at the same time epoch (2007-01-01 12:00:00). In (a) the S_2 of GFZ AOD-RL07 is selected as the reference. In (b), the S2 is obtained with the reviewer's approach. In (c) the S_2 is obtained with least-square fitting from the 6-hour time series.

Apart from this approach, we can actually force to fit (with least square solution) the cosine term of S2 from the 6-hour time series, however, the sine term vanishes because of $\omega t = 0$ or n*2π so that $\sin(\omega t)$ always equals zero. This is reasonable due to the Nyquist sampling rule, and that's also why we claimed the infeasibility of fitting a complete S2 (only the cosine term is available) from 6-hour time series. But anyhow, using this approach, we can still obtain an incomplete S2, which is named after 'Method-II'. Then, in Figure R6, a comparison of 'Method-I' and 'Method-II' is illustrated. We are surprised to see that the S2 tides estimated from two methods are very close to each other, in terms of whether the spatial pattern or magnitude. <u>This experiment may suggest that Method-I actually removes only the cosine term of S2, instead of the complete S2</u>. One can see that the complete S2 (from GFZ AOD-1B), as indicated in Figure R6a, has considerable differences from those in Figure R6b-c.

Therefore, to avoid potential confusion (removing partial S2 but adding back a complete S2), we still prefer to retain S2 in the final CRA-LICOM product. However, if you believe it is necessary to remove the cosine component of S2, we can also update our CRA-LICOM to accommodate this request.

References:

Dobslaw, H., Bergmann-Wolf, I. (2017). Product Description Document for AOD1B Release 06. GFZ Scientific Technical Report

Lasser, M., Meyer, U., Jggi, A., Mayer-G¨urr, T., & Flury, J. (2020). Benchmark data for verifying background model implementations in orbit and gravity field determination software. Advances in Geosciences,55, 1-11

Comment 5. *#5: The authors' description of the IB correction (lines 200-201) is imprecise and incomplete. IB refers to a static isostatic response of the ocean to atmospheric pressure loading; that is, air pressure anomalies are compensated instantaneously by vertical displacement of local sea level. The IB effect has no impact on the pressure at the seafloor except for a time-varying, spatially uniform OBP change reflecting the net atmospheric mass change over the ocean. From Figure 4, it appears that the S1, S2, and M2 tides in LICOM, as interpreted on lines 309-317, are largely introduced by the IB correction. This mix-up should be corrected and the corresponding paragraph in the text needs to be reworked.*

Response:

Thank you for your helpful suggestion. We appreciate your clear explanation regarding the IB correction, and in the revised manuscript, the concept of IB is updated as you suggested (see lines 216–217 in *Section 3.1.3*).

Additionally, we appreciate your professional review of our ocean tide constituents. In fact, the reviewer #1 has the same observations as you. For this reason, we have double-checked our code and data, and we have eventually identified an issue with our strategy of retrieving oceanic tides. This issue has been explicitly reported to the Editor and both reviewers on the first page of this letter. Here, we would like to explain the issue once again for your review:

We find that the oceanic tidal constituents, as shown in our original manuscript, were indeed contaminated by the unintentional residual atmospheric surface pressure signals. Specifically, the OBP simulated by LICOM includes 3-hourly sea level pressure (P_a) values generated by repeating 6-hourly data—i.e., the values at 03:00, 09:00, 15:00, and 21:00 were identical to those at 00:00, 06:00, 12:00, and 18:00, respectively. However, in the process of calculating the oceanic contribution to OBP (as described in Eq. (8), *Section 3.2.2*), Pa was linearly interpolated from the 6-hourly data to 3-hourly resolution. This inconsistency introduced artificial residuals in $P_b - P_a$, where P_b stands for OBP, resulting in oceanic tidal components that closely resembled atmospheric tidal patterns, as shown in the original Figure 4. After correcting the Pa data used in the oceanic calculation, the selected oceanic tidal constituents were re-extracted and are now presented in the revised Figure 4 (Figure R7 shown below). We have updated Figure 4 and revised the corresponding description paragraph. Please see details on lines 329–338 in *Section 4.2*.

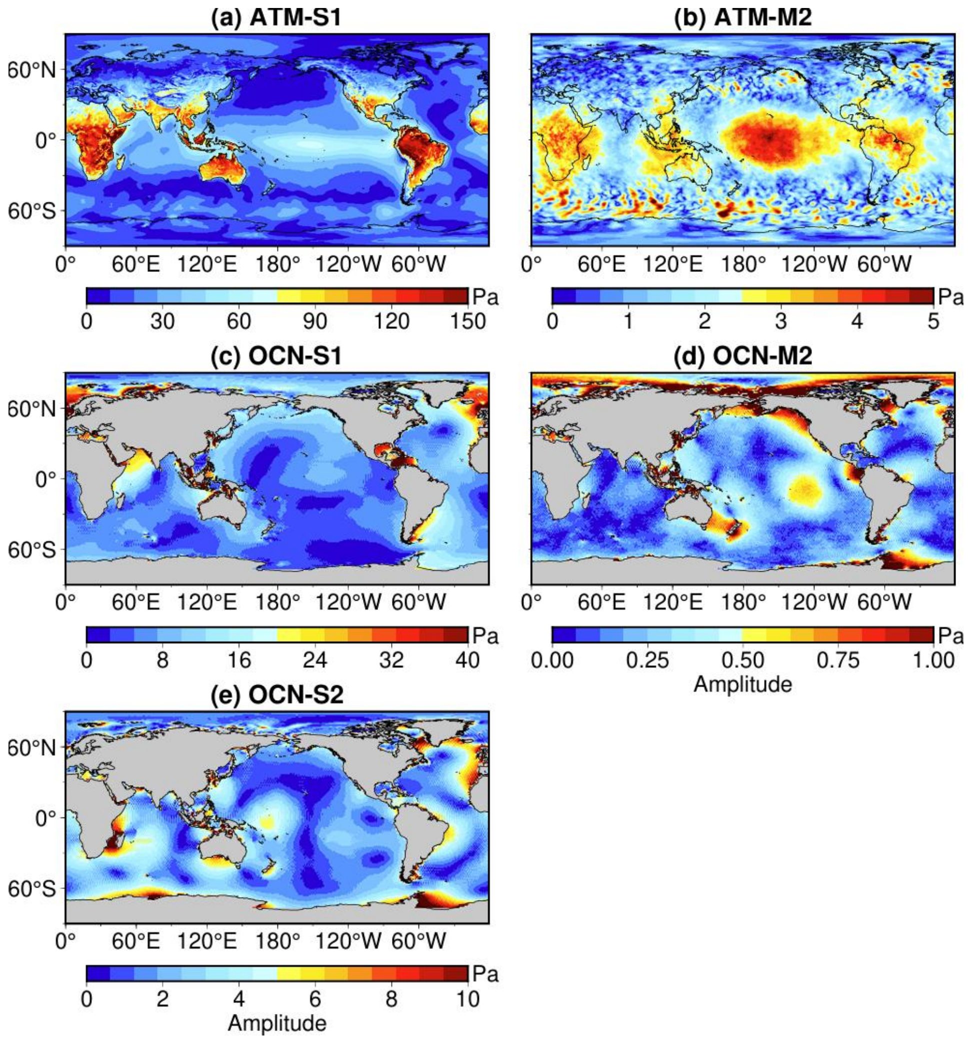

Figure R7 (Figure 4 in the revised manuscript). Amplitudes (Pa) of main tidal constituents estimated by CRA-LICOM over 2007–2014. The top panels show atmospheric tides based on 6-hourly data: (a) S_1 and (b) M_2. The middle and bottom panels demonstrate oceanic tides derived from 3-hourly data: (c) S_1, (d) M_2, and (e) S_2.

Comment 6. *#6: At the start of Section 5.3, the authors imply that GRACE measures the global ocean mass change (static effects; no currents), while GAD represents dynamic OBP variability related to ocean circulation changes. However, as GAD is imperfect, any de-aliasing product will leave a residual dynamic OBP signal to be picked up by GRACE. The separation made by the authors is therefore not fully correct, at least not the way it is currently formulated.*

Response:

Thanks very much for pointing out our inaccurate statement. We fully agree with your opinion and have followed your suggestion, updating the description to avoid misleading readers (see lines 485-491 in the revised manuscript).

In addition, for clarity, GAD contains the contribution of atmosphere pressure, which should be removed for obtaining the ocean mass change. To this end, we followed the approach of Uebbing et al (2019) to remove atmosphere pressure with IB correction applied to GAD. In this manner, we have updated Figure 12, where the ocean mass change indicated by GRACE+GAD is found to be more consistent with Altimetry-Argo than before. Here we attach the updated figure for your review (the analysis is also revised as well, see lines 495-508 in the revised manuscript).

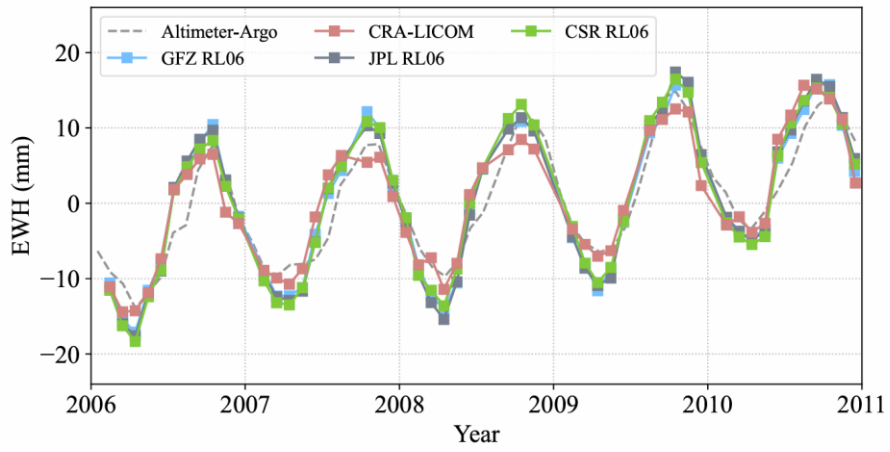

Figure R8 (Figure 12 in the revised manuscript). Global mean ocean mass change inferred from Altimeter-Argo and various GRACE solutions. The AO product GFZ-RL07 is reintroduced into GRACE's official gravity solutions, including CSR, GFZ, and JPL release 06, to derive the manometric sea level. Instead, our AO product, i.e., CRA-LICOM, is added back to our GRACE gravity solution (see *Section 5.1.3*) for consistency.

Reference:

Uebbing, B., Kusche, J., Rietbroek, R., and Landerer, F. W.: Processing Choices Affect Ocean Mass Estimates From GRACE, Journal of Geophysical Research: Oceans, 124, 1029–1044, https://doi.org/10.1029/2018jc014341, 2019.

Comment 7. *#7: In several places (e.g., line 364), arguments are made about errors in LICOM in western boundary (current) regions. As far as I can see from Figure 7, there is no systematic degradation of western boundary regions, but instead large RMSE values can be found on continental shelves and in many marginal seas. This in turn points to errors in the model bathymetry, wind stress parameterization or the bottom friction law. I suggest that the discussion in the respective places is modified to consider these points.*

Response:

Thank you for your insightful comment regarding the distribution of large RMSE values. Your discussion of the potential contributing factors is much appreciated. We have removed the text about western boundary regions in *Section 5.1.1*. In addition, we have incorporated a discussion of the potential causes of the discrepancies, including the points you mentioned. Please see details on lines 391–393 and lines 535–537 in the revised manuscript.

Comment 8. *#8: Section 6 'Limitations': I suggest to differentiate between structural model uncertainty, parametric uncertainty, and input data uncertainty, at least in the first paragraph of this discussion section.*

Response:

Thank you for this constructive advice. As suggested, we have added a classification of uncertainty sources in the first paragraph of *Section 6* and have reorganized the subsequent paragraphs accordingly. We have summarized them into three kinds of uncertainties. (a) The structural uncertainties include the coarse spatial resolution, the omission of atmospheric pressure forcing, the absence of tidal mixing, and the lack of SAL (Self-Attraction and Loading) feedback. (b) Parametric uncertainties involve the representation of bottom topography, ocean mask distribution, empirical parameters, the bottom friction laws, and the choice of parameterization schemes. (c) Input data uncertainty arises from the prescribed atmospheric forcing fields. Further details can be found on lines 513–548 in the revised manuscript.