Response to referee #1

Dear Thomas,

Please find below our complete answer to your comments.

General

We have included in the revised version of the paper the comparison of our GMSL record to the GMSL curves you mentioned. See paragraph l.238 and associated figure 5.

Line-by-Line

L.3 / The syntax of Poseidon has been modified accordingly

L.22-23 / We modified the sentence to make the distinction between sampling and accuracy. We also add references to the accuracy numbers. See Lines 23-25.

L.72 and 79 / Modifications as follow has been added: “ … all grid cells within +/-66 degrees N/S (the Topex and Jasons coverage) are spatially averaged…" See L.82 now.

L.134 / We modified the sentence according to your comment, see l.145-148

L. 178 / We estimated the trend and acceleration over the 29 years period of the GMSL record, with and without filtering, and the results are identical. This was expected since the 2 months cut off period of the filter is low as compared to the total lenght of the record (i.e, 29 years). The border effects are thus not significant. This is also true for estimations over 5 years periods.

We apply such a fitlering on the AVISO GMSL record as we consider that we remove some high frequency noise and that we still do not degrade the trend and acceleration estimations. We nevertheless note that raw GMSL time series could be publicly provided. This will be done in a future release.

Figure 5 and 6 / Thank you for having noticed this point. We were using the wrong GMSL timeseries for Figure 5 and 6 (i.e., not corrected for the Topex-A drift). We now obtain, naturally, consistent values between Figure 3, 5 and 6.

L.294 / The ITRF uncertainty is certainly non-linear, this is a good point. We modified l.324 accordingly. We used the uncertainties published for the ITRF2014, indeed. The updated reference frame ITRF2020 should help reducing the associated uncertainties as: time series are longer, seasonal signals are now considered in the local movements of the ITRF2020, the models are enriched as well as more data is used to constraints the model (I.e., Galileo). Information has been added to the manuscript L.358.

L.409 / We are currently discussing publishing the scripts used to calculate the trend, acceleration and uncertainties. Unfortunately, it will take some time. In the meantime, Prandi et al. (2021) made public similar scripts to perform OLS estimation with uncertainties in the context of regional MSL. This code is based on the same theoretical approach as ours and can be used to reproduce our analysis. We added this information l.207.

Response to referee #2

There is here, from the start, a misunderstanding of the objective of the present study (and of Ablain et al., 2019). This study (and Ablain et al. 2019) does NOT intend to estimate the "systematic components" of the observed GMSL anomalies. We only intend to characterize the uncertainty in GMSL measurements due to the instrumental errors. We state it clearly in the manuscript on line 37 (and also in Ablain et al. 2019 page 1190, 2nd column, 3rd paragraph).

Our group produces sea level measurements from satellite altimetry level 1 data. We are involved in this activity with CNES managers and CNES engineers who developed the radar altimeters onboard the satellite altimeters because we are in charge of the delivery of the sea level scientific product for CNES. We deliver sea level products and associated uncertainties (which is a pure instrumental uncertainty) as a service to the scientific community who can then use these products and their associated uncertainty to evaluate further different elements such as the geostrophic circulation, the mean dynamic topography and its changes, the GMSL anomalies and, if they wish, the "systematic components of the observed GMSL anomalies". Here we certainly do not evaluate ourselves the "systematic components of the observed GMSL anomalies", we only provide the updated GMSL anomalies derived from satellite altimetry and its associated instrumental uncertainty. We also provide an estimate of the 1993-2022 trend and acceleration with associated uncertainties as a metric for the low frequency changes in GMSL. The uncertainty on this trend and this acceleration is an uncertainty ONLY due to instrumental errors. We never claim the trend or the acceleration represent any "systematic component" of the GMSL. We provide the trend and the acceleration with uncertainty just as a reference for the scientific community so they can realize the actual amplitude of the instrumental errors on such metrics that are largely used for many different purposes in the science community. Note that our objective is also to provide a reference calculation of the trend uncertainty and the acceleration uncertainty so people can check their own calculation of the instrumental errors on their trend and acceleration estimate when they use our error variance/covariance matrix.

The confusion here is very common in the community that analyses sea level rise. We believe this is because this community is very focused on the detection and attribution of the forced response of global mean sea level to anthropogenic forcing on the climate system. This forced response is expected to take the shape of a parabolic signal on global mean sea level at decadal time scales (according to climate model simulation). For this reason people in this community tend to interpret any parabolic signal in GMSL as a "systematic component" of the GMSL that has some predictive value.

Here we DO NOT do such things. We are addressing a community that is much larger than the single community that analyses sea level rise. We are providing an update of GMSL anomalies with instrumental uncertainties for all the science communities that use GMSL products. These communities range from the Earth water cycle community to the Energy cycle community and it includes many communities as different as the ocean circulation community (which intend to assimilate the GMSL anomalies in ocean models for example) and the geophysics communities (like GIA people or solid earth people who use GMSL anomalies as observational constraint). Many of these communities compute trends or accelerations in their application. For this reason, we compute here one trend and one acceleration (the one trend and the one acceleration over 1993-2022) with the associated instrumental uncertainty derived from the error variance covariance matrix. This is as a reference so they can get a rapid idea of the actual amplitude of the instrumental error on such metrics. This is also a reference against which they can check their own uncertainty calculation when they use our error variance covariance matrix.

From this review, we suppose that the parabolic signal of the observed GMSL anomalies has been interpreted as “systematic components “ of the GMSL. This is something that should NOT be done. A simple trend calculation or a simple acceleration calculation on the measured GMSL time series that we are providing here, does not give an estimate of any “systematic component”. There is a misunderstanding of the GMSL data in this interpretation. We are not totally clear on what is meant by the term “systematic component”. We suspect he means the forced response of GMSL to the anthropogenic forcing on the climate system. If so, there is actually a long way to isolate the “systematic component” out of the GMSL measurement we are providing here. To isolate this signal, one needs to estimate the internal variability of the climate system and also to estimate the response to other forcings on the climate system such as the sun variability and the long term tides from other planets of the solar system. One also needs to isolate the intrinsic variability generated by the ocean circulation.

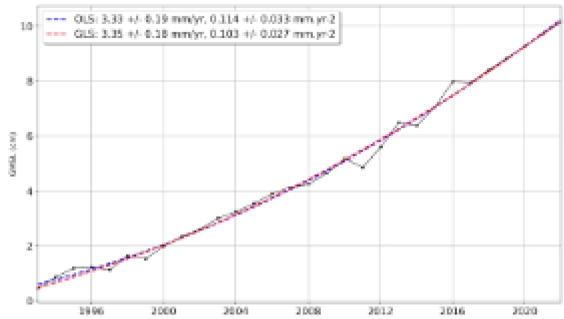
We clearly and explicitly explained this point in Ablain et al. 2019 (page 1190, 2nd column, 3rd paragraph). We repeat here in this paper that we are addressing only instrumental uncertainties and we are not trying to isolate the forced response to anthropogenic forcing. Reading your review, we understand that we have not been clear enough in the introduction of this manuscript. We thank you for pointing out this issue unintentionally. We have now added a supplementary paragraph in the introduction to clarify this positioning. See paragraph from line 40.

Because we were not clear enough in the introduction that we are addressing only instrumental uncertainties and do not estimate any “systematic components”, we believe that several comments of the review are actually not relevant. We explain this below.

Comment #1

We explained in detail in Ablain et al. (2019) why we use an OLS rather than a GLS. OLS estimate is known to be less accurate than GLS in terms of the mean square error, because its variance is larger. A generalized least square estimate would probably help in narrowing slightly the trend uncertainty, but the difference is expected to be small, in particular when the V/C matrix is not far from identity (Ribes et al. 2016). Important advantages of using OLS are that (i) OLS is consistent with previous estimators of GMSL trends as well as estimators of trends in other essential climate variables than GMSL (indeed OLS with V/C matrices is the approach used in the IPCC, see for example Hartmann, et al., 2014,) and that (ii) the OLS best estimate does not depend on the estimated variance–covariance matrix **Σ**. Reasons why the OLS estimate could be prefered, even if the V/C matrix is not the identity, were also discussed in the IPCC AR5 (e.g., Chapter 2, Box 2.2)

We tested a GLS estimate on a yearly average GMSL time series (for which the V/C matrix is now invertible) and checked that the result is very close to the OLS estimate. We do find very similar results with both estimates. See plot below. We added this information in the manuscript now on line 204.



Comment #2

There is a confusion here. We do not assume that a linear signal or a parabolic signal represent the GMSL time series and then test these simplistic models against the zero assumption. This approach is used by people trying to isolate the GMSL signal forced by anthropogenic emissions assuming it is a linear trend or a parabolic signal. Here we are not interested in this. Here we intend to provide the most accurate estimate of the observed GMSL. Then we consider the GMSL time series and try to derive the trend and the acceleration of the time series over the record length as metrics of the lowest frequencies included in the time series. In this sense we do not expect the disturbances to be zero and in this sense the OLS estimator itself is an unbiased estimator (see Ribes et al. 2016 and demonstration in the reference therein). Note that linear trend models are also used (and useful) in cases where the underlying shift is not linear in time (and so the “expected value” of the residual is non-zero; see again IPCC AR5, Box 2.2). In such cases, trends are used to measure the rate of increase in a time-series.

Comment #3

We never assume the quadratic model captures “the systematic variations in the observed GMSL”. This interpretation is biased towards interpreting the GMSL physical signals. Again, we estimate the trend and acceleration of the observed GMSL as metrics of the lowest frequencies included in the time series. See our answer to point #2.

Comment #4

We agree with this comment and we refer to our response of point #1 (where we checked that the GLS estimate and the OLS estimate lead to the same estimate) as well as point #2.

Comment #5

We would like to make two comments in response to this point. First, the estimated V/C matrix is taken into account in the uncertainty analysis, i.e., quantification of the standard error of the trend and acceleration coefficients. As a result, this uncertainty analysis is valid and reliable – and fully consistent with our V/C estimate. Second, our uncertainty analysis only accounts for the measurement uncertainty – this is now made clearer in the revised version of the manuscript (see our response to reviewer's introduction for more details). In particular, any uncertainty related to internal variability within the climate system (which can be large) is not taken into account here. So, our confidence range is not representative of uncertainty on human-induced SLR.

Comment #6

Indeed, if the objective of this study was to isolate the forced response of sea level to anthropogenic emissions we would need to account for the internal variability and the natural variability in GMSL in the least square approach. And the reviewer is right, we would need to model the serial correlation in the GMSL time series. But that is not our objective. Our objective here is only to deliver to the community the most accurate GMSL time series possible from satellite altimetry with associated instrumental uncertainty and to estimate the uncertainty due to instruments on the 1993-2020 trend and acceleration of the GMSL time series. For this reason we believe this comment is not relevant here.

Comment #7

See answer of the previous point #6.

Comment #8

Of course we expect the forced response of GMSL to anthropogenic forcing to be a response that is more complex than just a parabolic signal. For this reason, the acceleration and the trends are expected to change with time, we agree. But here we do not tackle this problem. We simply want to give metrics for the lowest frequency included in the 1993-2020 GMSL record derived from satellite altimetry. For this reason we focus on one unique trend and one unique acceleration : the 1993-2020 trend and acceleration corresponding to the full satellite altimetry era. Once again, because of the different perspective, we believe the comment here is not relevant.

Comment #9

See our answer to the previous comment #8.

Comment #10

That is the point here. We use the trend and acceleration as simple metrics for the low frequency and do not make any predictions with it. This would be indeed “an extremely costly blunder in climate mitigation decision making”. Any other metric could/should be used for sure to achieve this goal. We choose one metric and are clear about it so people in the community can test their own calculation against ours.

We DO NOT pretend those metrics represent any “systematic component” of the GMSL and we CERTAINLY NOT pretend those metrics have any predictive skills. For your recollection , we are only providing an observed estimate of the GMSL and low frequency metrics with the INSTRUMENTAL uncertainty. We clarified this point in the paper l. 197-198.

Comment #11

We do not make any predictive model in this study. See answer to comments #10.

Comment #12

Thank you for pointing out that “the efforts to build a full V/C matrix is a worthy endeavor for optimal analyses of GMSL anomalies”. We believe so as well. Indeed this is the goal of the paper and we insist on that point: for the altimetry measurements only.

But please note that we DO NOT intend to provide a full V/C matrix that represents the errors of the forced response of GMSL to anthropogenic emissions. Our work is much more modest. We ONLY provide a V/C matrix of INSTRUMENTAL errors in GMSL. Providing a description of the GMSL instrumental errors is a very important step for scientists of all communities that use the GMSL time series and not only for the sea level rise community. For the sea level rise community, our work provides the very first brick over which scientists can further build a complete V/C matrix of the forced signal. This is not our intention. We understand from this review that we were not clear enough. Now we clarify this in a whole new paragraph in the introduction (see lines 40-48). We hope this makes our objective clearer to the sea level rise community.