Response to RC3: Anonymous referee #3

> *In this manuscript, the authors define the new concept of transient sea level sensitivity that is inspired by the transient climate sensitivity but that is adapted to the sea level problem. In particular it relates the sea level rise over a century with the average temperature anomaly compared to a steady state over the same period. I think this concept, even with all its drawbacks, has the potential to be useful but the arguments developed in this manuscript needs to be further developed to be convincing. Especially since the authors make important claims about the underestimation of future sea level rise by the IPCC AR5 and SROCC process-based method.*

We agree that there are limitations, and are convinced that the TSLS will be a useful tool.

Revision plan

- Elaborate substantially on the limitations of the metric.

> *General comments:*
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> *An important motivation to define the TSLS is the linear relationship between sea level change and GMST in both observations and models. However that relationship is not very convincing. I agree with the theoretical points mentioned by referee #2 so I will not come back on those but I will focus on the observations and model data used in Figure 1:*

The monotonous relationship with almost no scatter is an important justification. To be useful it does not have to be a linear relationship, but a linearization has to be a reasonable approximation that characterizes most of the response. It will be an approximation, and you should be careful extrapolating. Different time periods can have different sensitivity, and we expect the response to become non-linear for intense warming scenarios (as seen in SROCC). Admittedly, this we can be more explicit about in the text.

We hope that you will take a look at our responses to referee #2, and check if we have addressed the concerns you share.

> *1) The observational data used here to back up such a relationship is weak. There are only three points, moreover the pre-industrial and tide gauge periods are very close to each other. With therefore the main point driving the slope of the linear relation being the satellite period which is only around 25 years. I would suggest that if the author think 25 years is enough to estimate the TSLS then the tide gauge period could be split in a few 25 years periods.*

We agree that it is probably possible to make a better estimate of the historical TSLS, using a more sophisticated statistical analysis of the full historical data. However, this is not trivial. E.g. it is important to take uncertainty autocovariance of the tide gauge record properly into account. There are multiple reasons why we decided to restrict our analysis to published estimates rather than our own statistical analysis of the tide gauge record:

- We are writing a short letter that may be seen as controversial by some. It seems more appropriate and more convincing to use published estimates, rather than making a highly technical statistical analysis with lots of assumptions, which would seem to add to the controversy.

- We are convinced that this approach yields conservative uncertainty estimates.
- It is a better assumption that the three historical estimates are independent, than if you slice the tidegauge record into shorter sections, in which case they definitely will not be.
- Downsampling of the tide gauge record has been done before (e.g. Rahmstorf 2007). It sparked criticism of the statistical assumptions, which is key to us to avoid.

Finally, our TSLS estimate should not be the final word on the subject – we want to add a new element into the assessment of all available information about sea level rise information.

The historical TSLS is estimated using data from 1850-2017. It is correct that the shortest slice of data is the altimetry record which is only ~25yrs. It is, however, also the least noisy.

We disagree that pre-industrial and tide gauge rates are close. The pre-industrial rate is centered around 1875, and the tide gauge rate is centered at 1945.

Revision plan

Explain that we only use published estimates, and motivation.

> *2) For model data the uncertainty lines are obtained from the assumption of full covariance between GMST and sea level uncertainties in IPCC projections. But that is not the case at all, there are many sources of uncertainty in the sea level projection that are independent of temperature. For example Greenland and Antarctic ice dynamic contribution, glacier model uncertainty (four different models are used in AR5 and SROCC). The assumption is justified by the fact that when it is made it shows a linear relationship between GMST and sea level but this is what the authors try to demonstrate. Also for SROCC the linearity doesn't seem to hold at all.*

First, we want to emphasize that the near-linear relationship in the models is demonstrated by the central estimates alone. So, we do not see the point of claiming a circular argument here.

It is unfortunately so that the IPCC reports offers very little information that can be used to infer the uncertainty covariance. We know that the process based models for the ice contributions are directly driven by temperatures in AR5 (see sections 13.SM.1.3 – 13.SM.1.5). So, a priori we know that any uncertainty in temperature will be directly reflected in the modelled rate. I.e. we know there will be a high degree of uncertainty covariance. We chose to go with the simplest assumption: full covariance. We did, however, look into an alternative method of estimating covariance.

The IPCC reports provides us with central estimates and a likely range. We can frame that as $T \pm \sigma_T$ and $\dot{S} \pm \sigma_{\dot{S}}$. So, we know the uncertainty ellipse has to fit inside a rectangle with width=$2\sigma_T$ and height=$2\sigma_{\dot{S}}$. Knowing how the ellipse is oriented inside the box is equivalent to knowing the uncertainty covariance matrix. From the central estimates we have some idea of how sea level rate depends on temperature. In lack of better information, it seems reasonable to assume that one axis of the uncertainty ellipse should be aligned with the curve between central estimates. From figure R3.1 we see that when the line between central estimates approaches the corner of the rectangle (panelA) then we have a situation that approaches full covariance. This is almost exactly the situation we have in figure 1 in the manuscript. The high and low end estimates fall on the same curve as the central estimates. Notice: You can see that if you have a situation like figure R3.1B then the top right corner of the red box would fall above the line between central estimates. If we use this more complicated approach outlined here, then we estimate uncertainty correlation coefficients of more than 0.95 for both AR5 and SROCC

(derived from the uncertainty covariance matrix). We decided to not use this approach to derive the covariance matrix because:

- Need to assume symmetric gaussian errors.
- Need to assume a "local" linear relationship. (Not great for SROCC).
- Impossible to avoid assumption concerning how to orient the ellipse.
- It is rather complicated to explain.

In short, we prefer to keep the imperfect "full covariance" assumption. It is much simpler, and can better deal with non-linearity. These principles, we wish to make more explicit.

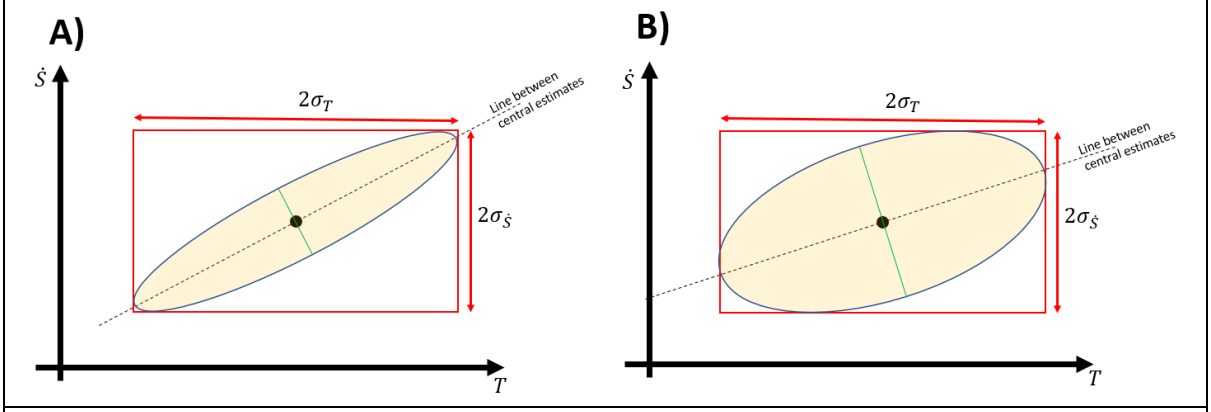

Figure R3.1: A central estimate (black dot) with associated uncertainties σ_T and $\sigma_{\dot{S}}$ (red). The uncertainty covariance matrix is represented by an ellipse. The ellipse must be inscribed in the rectangle. If we assume that the one axis of the ellipse is aligned with the curve between central estimates, then we can infer the ellipse parameters (=the covariance matrix). The panels compare two situations: One where the curve between central estimates curve nearly hit the corner of the uncertainty rectangle (panel A), and one where it does not (panel B). In panel A the ellipse approaches full covariance.

Revision plan

- Add a more complete description that explains that full covariance is unlikely, and how it impacts results.

> *l.47: "This does not automatically demonstrate a bias in model projections, but as a minimum call for a detailed explanation."*
> *Since this is the main claim of this short paper I think attempting to provide an explanation falls on the shoulders of the authors. There is already some literature on that subject see for example Slangen et al. 2017, in particular section 4:*
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> *"When all the contributions are combined, the models add up to a GMSL change of 92 6 47mm for the period from 1901–20 to 1996–2015 (Table 4, Fig. 9a). Compared to the average of the four reconstructed global mean time series for the overlapping period from 1901–20 to 1988–2007 (Table 5, Fig. 9a, the model simulations clearly underestimate the observed GMSL and explain only 50% 6 30% of the observed change (using 61.65s of the models to the mean of the observations)."*
>
> *And the following discussion on adding corrections to the sea level computed from the models to solve the issue.*

Thank you – this is useful context.

It is important to note the context that AR5 and SROCC does not provide their own hindcasts using the same process-based model used for projecting sea level rise. I.e. the aggregate projections are not adequately validated against the historical record. It is very disconcerting that there is a discrepancy between the historical response and models of the future. Slangen et al. (2017) is really useful here.

The main contribution is in our opinion the concept.

Revision plan:

- Discuss Slangen2017 as context.
- Stress even more limitations of a comparison between two different periods: historical and projections.

> *Small comments:*
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> *- Figure 1: I can't find an explanation for the numbers in PI11, TG7, Sat9 and others.*

This was a leftover from an early version of the manuscript. This will be removed, and the caption expanded.

> *Slangen, Aimée B. A., Benoit Meyssignac, Cecile Agosta, Nicolas Champollion, John A. Church, Xavier Fettweis, Stefan R. M. Ligtenberg, et al. "Evaluating Model Simulations of Twentieth-Century Sea Level Rise. Part I: Global Mean Sea Level Change." Journal of Climate 30, no. 21 (November 2017): 8539–63. https://doi.org/10.1175/JCLI-D-17-0110.1.*