Response to RC2: Anonymous referee #2

> *Summary: The manuscripts defined a transient sea-level rise sensitivity as the linear dependency of the rate of sea-level with centennially averaged global mean temperature (surface?) temperature. The authors estimate this sensitivity from observations and from future climate simulations from the CMIP5 model ensemble. They conclude that the model-derived values are smaller than those derived from 'observations' and thus the future sea-level rise may become larger than those projected by climate models.*

Yes – we mostly agree with this summary. Importantly, we stress that we are aware that the discrepancy between the historical and projected sensitivities cannot be fully conclusive as it is comparing the response in two different periods. Hence the phrasing "*may become*" in the comment above.

It is correct that GMST refers to the global mean surface temperature. We define this in the data and methods section.

Minor disagreement: We would not call the AR5 and SROCC sea level projections, "*climate simulations from the CMIP5 model ensemble*". We note that the SLR projections in AR5 and SROCC is not projected directly by models, but rather using an afterburner to the models providing climate change projections.

Revision plan

- Check if it makes sense to move GMST definition into the main body of text.
- Ensure careful phrasing of the conclusions. We do not want to overstate the significance of "the discrepancy" between past and future. But we will emphasize the caveats related to the use of GCM climate projections further processed to get SLR information.

> *Recommendation:*
> *This is a surprisingly short manuscript, which in my view leaves many technical detailed unclear. It does not have a result section, and so it was for me difficult to interpret what the sole figure 1 and the sole table 1 is actually representing. The very concept of transient sea-level sensitivity requires a much deeper physical discussion. My impression is, therefore not positive. The manuscript seems in many respects to be incomplete.*

We plot published data in a deliberately provoking way, with minimal analysis. We strongly feel that the content is best suited for a short discussion letter rather than a long research article. Naturally, we are not satisfied that our condensed presentation apparently was unclear, and we will strive to improve that in an expanded revised version.

Figure 1 demonstrates that the transient sea-level sensitivity metric does captures most of the future model response. The IPCC assessments summarize our process knowledge. This is in our opinion a much stronger argument than physical discussions of how we might expect the system to respond to warming.

The primary objection seems to be that there may be physical mechanisms that could explain why the sensitivity of the 21st century would be different from during the historical period. I.e. there could potentially be an explanation for the discrepancy highlighted by figure 1. We want to stress that we absolutely do <u>not</u> assume that TSLS is constant through time. This is why we originally said: "*This does not automatically demonstrate a bias in model projections, but as a minimum call for a detailed explanation*", and "*Future TSLS may well be different from the past, …*". We will stress this even further in the revised manuscript.

Revision plan:

- Expand description of figure.
- Explain statistical methods in detail.
- Stress that sensitivity may be different in future from past, and that this can possibly explain "the discrepancy" and assess the involved physical mechanisms more clearly.
- Elaborate substantially on the limitations of the metric.

> *1) The definition of sea-level climate sensitivity, although used in some previous studies, is at least rather questionable, and it was clearly questioned also in the AR5 report itself. This manuscript should at the very least justify in the first place why this concept is meaningful.*

The AR5 questioned a universal linear relationship between sea level rise rate and temperature, and therefore questioned projections based on extrapolations of the historical relationship. We are fully aware of this and agree to this premise.

The main argument was that there may be physical reasons that cause future response to be different from the past. Some mechanisms could cause the response to be non-linear, and other mechanisms invoke non-stationarity where the sensitivity depend on the state of the system. Figure 1 shows that the process-based models actually do show a near-linear response. A linearization clearly captures most of the future response. This demonstrates that the TSLS concept is meaningful as far as our process knowledge is concerned. Figure 1 therefore directly eliminate a whole class potential problem raised in AR5.

Non-stationarity is another reason that future sensitivity may be different from the past. This could cause the TSLS of the 20th century to be different from that of the 21st century. We fully acknowledge this, and this is the main limitation of the comparison to historical data. On the other hand, experts align much better with the extrapolations than the AR5/SROCC projections. Nevertheless, we acknowledge that this is an issue and this is why we are generally careful to not overstate the implications of the discrepancy. E.g. we write: "*This does not automatically demonstrate a bias in model projections, but as a minimum call for a detailed explanation*".

Revision plan:

- Address explicitly the premises adopted in AR5 (and implicitly in SROCC) that a universal linear relationship between sea level rise rate and temperature is questionable.
- Stress that sensitivity may be different in future from past, and that this can possibly explain "the discrepancy" and assess the involved physical mechanisms more clearly.
- Discuss non-linearity and non-stationary.
- Emphasize more strongly the limitations of the comparison to the observational estimate. Especially in abstract.

> *For instance global mean sea-level rise is brought about by two very different mechanisms: expansion of the water column and melting of land ice. A backof-the-envelope calculation yields that the global sea-level rise caused by the capture of an energy flux of 1 w/m2 by the liquid ocean, and its subsequent expansion, is about 1.9 mm. This is very different from the sea-level rise caused by the capture of of 1 w/m2 by land-ice and subsequent melting, assuming the ice is already at 0C, (94mm). Of course, this also depends on where the heat flux is captured and many regional details, but the difference between 1.9mm and 94mm is in principle enormous. Therefore, the very concept of a linear relationship between energy flux imbalance and the rate of global sea-level rise is physically questionable, at least it requires a plausible justification, as the 'sensitivity' depends on the relative contribution of thermal expansion and melting.*

First, we agree that how the energy is spent will have a huge impact on the TSLS. But this just illustrates that the TSLS metric quantifies an important aspect of the sea level response.

The idea of a linear response may be surprising or '*physically questionable*', but it is simply a fact that the IPCC process-based projections have a 21st century response that is almost perfectly linear in warming. Figure 1 demonstrates that.

The main objection hinges on a common misconception. The reasoning seems to go as follows: Since we know that the relative proportions of ice melt and expansion are changing, and that melt and expansion may have very different sensitivities, then the combined sensitivity (TSLS) <u>must</u> be changing over time. However, this simply does not follow. Even in a model where every contributor responds linearly to warming the relative proportions can change. This is demonstrated in Note R1 in the end of this document.

Finally, the TSLS concept is just a metric that characterizes the first order response at a given point in time. You can always linearize the response and talk about the slope. This is essentially all we are doing. The concept does not require that the response is perfectly linear, nor does it hinge on the relationship being stationary in time. Non-linearity and non-stationarity would of course place limitations on how the metric can be used.

Minor note: There is a mismatch of units in your back-of-the-envelope calculation. The energy capture is given as a rate, but time is missing from the corresponding sea level rise. Should it be per mm/year?

Revision plan:

- Stress that we do not expect TSLS to be constant over time.
- Elaborate substantially on the limitations of the metric.
- Consider discussing common misconception.

> *This contribution is rather uncertain for the future, but it seems to me clear that in the near future melting will play a much bigger role through glacier melting, then perhaps a smaller role as glaciers are completely melted and then again a bigger role when melting in Greenland and Antarctica sets in. So it is really difficult for me to envisage a simple linear relationship to describe this dependency. It may be that in practice it works, but this needs to be justified. Unfortunately, I do not see which data could be used to justify this assumption. The centennial smoothing assumed in this study would require several millennia of data for a robust justification.*

The TSLS concept does not rely on a perfectly linear relationship. It is useful if a linearization is a reasonable approximation of the relationship. We show that it in practice works for AR5 and

SROCC models. But it is still a simplification, -just as transient climate response is only an approximation to how surface temperature respond to radiative forcing.

The objection here seems to be that there could be processes that change the sensitivity over time. We do not assume that the sensitivity is constant, and especially not over several millennia. We do, however, compare the historical sensitivity to the projection sensitivity. But that is just a comparison. We note that there is a disconcerting discrepancy, and that this needs to be explained. We may speculate that perhaps this is because the sensitivity has changed from the 20^{th} to the 21^{st} century. But that would only be speculation without further study. One way to address this would be to verify that the models used for projections can reproduce the sea level rates of the historical past. Unfortunately, the aggregate sea level models used in SROCC and AR5 have never been validated in this manner. We argue that the type of comparison we are doing in this paper is the next best thing. The discrepancy to observations is disconcerting.

Revision plan:

- Stress that sensitivity may be different in future from past, and that this can possibly explain "the discrepancy" and assess the involved physical mechanisms more clearly.
- Emphasize more strongly the limitations of the comparison to the observational estimate. Especially in abstract.
- Call for historical validation of models used for sea level projections. Not just of the individual contributor models, but also of the aggregate model.

Finally, it is a common misunderstanding that the total sensitivity must be changing because the relative contributions of contributors are changing. However, it is perfectly mathematically possible that the relative contributions change even if every contributor responds with a constant linear sensitivity. This just requires that each component is not equally close to being in balance. [See note R1 in the end of this response].

> *2) Related to point 1, the CMIP5 global climate models do not include land ice melting. This is the reason why the IPCC AR5 included a contribution to estimated sea-level rise by expert knowledge. But I wonder how the comparison between AR5 models and observations can be meaningful, when one of the key components is missing in the models. Therefore, it is not really surprising that the sensitivity estimated from models is smaller than that estimated from observations. This is again the reason why the IPCC augmented the estimated sea-level rise by 2100 with an approximate contribution from land-ice melting.*

We agree that the way ice contributions was treated in AR5 explains why AR5 has a too low slope. We also agree that it is not surprising, and we already explain this in the manuscript, so it is unclear what else we should do here.

Minor disagreement: AR5 did in fact include land-ice melting. It was only the dynamic contribution where they used an approximate contribution based on expert knowledge.

> *3) The approach in this manuscript seems rather similar to the approach by Rahmstorf (2007). The reader would like to know in what aspects both approaches differ, and how this difference my affect the results.*

The most important difference is that we are not making a projection, and we are not assuming that that the future sensitivity will be like the past. This is an important distinction.

Rather,

- We define the TSLS metric.
- We demonstrate that the TSLS captures most of the 21st century response in AR5 and SROCC. I.e. we address some of the concerns raised in the AR5 in response to semi-empirical models such as Rahmstorf (2007).
- We compare the observational sensitivity to the projection sensitivity, and highlight a disconcerting disagreement. We then "*call for a detailed explanation*".

Statistically there are also differences. Rahmstorf (2007) was criticized for assumptions concerning statistical independence, and degrees of freedom. We avoid these issues by relying on a single average sea level rate for each observational record. It is simply a better assumption that the TG and SAT rates are independent. However, a drawback is that we are left with only a few points to base our observational estimate of TSLS. Less data usually results in larger uncertainties. A more detailed time series analysis of the tide gauge record could potentially provide a TSLS estimate with tightened uncertainties (which would only make the numbers in table 1 even more significant). However, the statistical assumptions of such an analysis would be much more critical. Given the robust push-back here, then we are happy with our choice to use a simple but rock-solid approach for our uncertainties.

Another motivation to using long-term values for the TG or SAT rate is that these are published by the authors of the records. This means that figure 1 just is what it is. The location of the points does not rely on any analysis we make.

Revision plan:

- Stress that sensitivity may be different in future from past, and that this can possibly explain "the discrepancy" and assess the involved physical mechanisms more clearly.
- Emphasize more strongly the limitations of the comparison to the observational estimate.
  Explain that we only use published estimates, and motivation.

> *4) I struggle to understand what Figure 1 and Table 1 are exactly showing ? Certainly the caption or the main text should include a much lengthier description. Points that remain unclear to me are: what is the averaging window (100 years as suggested in the main text?) If yes, the global mean temperature observations would be just 1 point ?), What does the point labeled as Sat9 represents ? Probably it represents the data in the satellite era, but there is no mention of this in the main text, only one paper listed in the reference list. The same can be said about TG7. To be honest, at this point I wonder whether the authors have carefully checked the manuscript before submitting.*

We are of course not satisfied that our captions are not sufficiently clear, and we will work to clarifying this in our revisions.

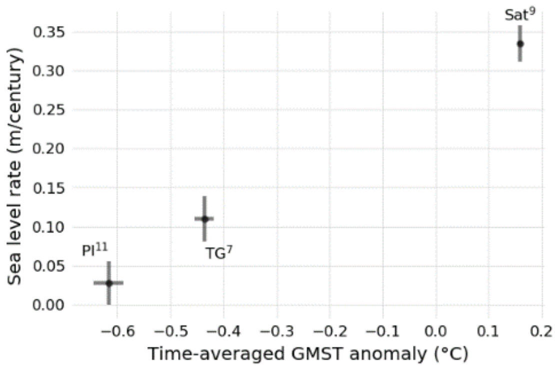
We regret overlooking the numbered references left in the figure from a prior version of the manuscript. This will be fixed in the revision.

Revision plan:

- Expand caption – Explain what each point is, including their time span.
- Remove superscripts from figure.
- Explain more in main text time period.

> *In the case of observations, if my interpretation is correct, the linear fit is constructed using two points, both with different characteristics (one represents centennial means, the other satellite-era means). Is linear fit with just two points enough to be extrapolated ? The extrapolation would be even more questionable when considering that the physical processes would change over time, as explained in my point 1. How were the uncertainties calculated considering that the errors in each of these data points are different ?*

We acknowledge that the statistical details were not described in detail in the manuscript. We will revise the manuscript with a more thorough description of the statistics. The extrapolation is just a visual comparison, and should not be taken as a projection.



The observational fit is calculated using three points:

- SAT: 1993-2017
- TG: 1900-1990
- PI: 1850-1900

The time intervals were chosen because this is what was provided by the cited studies. All points have their own uncertainties in both the x and y directions (where x:T; y=SLRate). The y-uncertainty is given in the cited studies, and the x uncertainty was extracted from the HADCRUT4 ensemble for the same period.

In this paper we take uncertainties in both of the displayed variables into account. We do that using Monte Carlo sampling. We make 10000 linear regressions, where each displayed variable are perturbed according to their uncertainties. This gives an ensemble of slopes and intercepts that we can extract statistics from. We report a TSLS based on these data and of 0.40 m/century/K [0.35-0.44]. For comparison standard weighted least squares regression (which only takes errors on the dependent variable - typically chosen to be on the vertical axes - into account) yields a substantially narrower uncertainties for the TSLS of 0.39 m/century/K [0.37-0.41].

Revision plan:

- Explain statistical methods in detail.
- Stress that extrapolation is <u>not</u> a projection but plotted for comparison.
- Stress that sensitivity may be different in future from past, and that this can possibly explain "the discrepancy" and assess the involved physical mechanisms more clearly.

> *Further points*

> *5) The main text mentions reconstructions of sea-level in the preindustrial period, but were have they been used? There is no mention of temperature reconstructions that could be used for the estimation of sea-level sensitivity.*

In the methods section we define the pre-industrial (PI) as 1850-1900 (following AR5). For that period we have an average temperature from HADCRUT4, and a sea level rate from Kopp et al. (2016). This is plotted as PI in figure 1. This point is used together with TG and SAT in the observational estimate of TSLS.

Revision plan:

State time-intervals in figure caption.

> *6) The caption of the table mentions a level of significance in the difference of the sea-level sensitivity. How has it been calculated ?*

We realize that we did not detail that it we used a two-tailed test and the assumption of normality. We will add this in the revision.

To be 100% clear we also have an expanded explanation here:

We want to look at the difference between $TSLS_{AR5}$ and $TSLS_{obs}$. But these numbers are uncertain, and we want to know if that difference is large considering the uncertainties in both estimates. E.g. We want to look at the difference between $TSLS_{AR5}$ and $TSLS_{obs}$. But these numbers are uncertain, and we want to know if that difference is large considering the uncertainties in both estimates. For gaussian errors standard uncertainty of the difference will be the $\sigma^2_{\text{difference}} = \sigma^2_{\text{obs}} + \sigma^2_{\text{AR5}}$. Then the p-value can be looked up in the CDF of the normal distribution. This is basically a particularly simple t-test. In order to make the test we need the standard errors. There is a one to one relationship between standard error and likely range as we have assumed normality (the conversion factor is 1.048).

Example calculation (comparison between $TSLS_{obs}$ and $TSLS_{AR5}$).
From table 1 we have:
$TSLS_{obs} = 0.391$ and $TSLS_{AR5} = 0.274$
$\sigma_{obs} = (0.391 - 0.349) \cdot 1.048 = 0.044$
$\sigma_{AR5} = (0.303 - 0.274) \cdot 1.048 = 0.030$

This yields:
$\Delta TSLS = 0.391 - 0.274 = 0.117$
$\sigma_{difference} = \sqrt{\sigma^2_{\text{obs}} + \sigma^2_{\text{AR5}}} = 0.053$

The probability of values greater than 0.117 in a normal distribution with zero mean and that $\sigma_{difference}$ is p=0.013. That is the p-value of a one-tailed test. The two-tailed probability will be twice as high. This is the p-value we report to be below 0.05 in table 1.

Revision plan:

- Write that it is a two-tailed test assuming normality.

7) The temperature anomaly are referred to the base line 1986-2005. What is the reason for this short base line, when the link between T and sea-level rate is assumed to be at centennial scales ? It does not seem consistent. I guess there is an explanation for it, but the manuscript is so short and concise that the reader is left wondering.

Here, we simply adopt the baseline from the IPCC reports. This choice of base line is just a translation of the plot and has no impact on the slope (TSLS) or the 'discrepancy'. By adopting the same baseline as IPCC, we avoid introducing additional uncertainty by redefining the baseline. This means we can plot the AR5 and SROCC values exactly as reported. We actually write: "*We follow AR5 (Church et al., 2013) and use a 1986-2005 baseline for temperature anomalies …*".

Revision plan:

- Be explicit about baseline motivation.

The latter are just examples of open technical questions that should be clear in a properly formatted manuscript, with proper length

We hope to address all the technical questions following the plan outlined in the answers above. The revised manuscript will also be more explicit about the limitations of the TSLS metric and the comparison between past and future. This will result in a longer text, but we still aim for a letter format.

Note R1: Changing proportions, yet constant sensitivity

The sea level budget is changing, and we expect ice sheet melt to increasingly dominate the budget. This might lead one to argue that the sensitivity must be changing as we don't expect the individual contributors to be equally sensitive to warming. In this section we present a case for why that is a flawed argument. We show that even in a completely linear model the relative proportions of the individual sea level contributors can change.

Let's assume for the moment, that the rate of sea level rise is just the sum of the contribution from ice melt ($\dot{M}$) and the contribution from thermal expansion ($\dot{E}$). We write:

$$\dot{S} = \dot{M} + \dot{E}$$

Let's also assume that these two contributions respond linearly to warming.

$$\dot{M} = a_M T + b_M$$

$$\dot{E} = a_E T + b_E$$

We insert and get a linear model for the sea level rate:

$$\dot{S} = (a_M + a_E)T + b_M + b_E$$

The proportion of sea level rise due to ice melt becomes

$$\frac{\dot{M}}{\dot{S}} = \frac{a_M T + b_M}{(a_M + a_E)T + b_M + b_E}.$$

This is not generally constant in T. This demonstrates that a changing proportion of ice melt does not necessarily imply a changing sensitivity to warming.