Dear Editor

The reviewers raised many good questions and concerns, but none raised any critical issues with our analysis. The most serious concerns were centered around that we were not sufficiently explicit with regards to the limitations of our new TSLS metric. Secondly, it has become clear that the very brief letter structure of the original manuscript was not ideal. The reviewers wanted a more expanded discussion of the manuscript. We have restructured the manuscript to follow a more traditional paper outline (intro-data-methods-results-discussion-conclusion).

In the replies to the reviewers we stated how we planned to address each of their concerns. The individual points of the revision plan is collated below, followed by a description of *the revisions in red italics*.

Best regards,

Aslak Grinsted and Jens Hesselbjerg Christensen

Description of revisions

Revision plan and revisions:

Limitations of the metric

- Elaborate substantially on the limitations of the metric.
- Emphasize more strongly the limitations of the comparison to the observational estimate. Especially in abstract.
- Expand the discussion and emphasize more strongly the limitations of the comparison to the observational estimate.
- Stress even more limitations of a comparison between two different periods: historical and projections.
- Talk more about the limitations of the TSLS.

We have expanded considerably on all these points in the introduction, discussion, and in the conclusion. The revised text is much more explicit on the limitations of the metric. Here are some examples of some revisions:

Introduction: "… However, there may be processes that can cause future sensitivity to be different from the past (Church et al., 2013). These changes can broadly be categorized as being due to a non-linear response to forcing, or due to a non-stationary response where the response depends on state of the system. E.g. the sensitivity of small glaciers to warming will depend on how much glacier mass there is left to be lost, and we therefore expect this to have a non-stationary response. Nature is complex and will be both non-linear and non-stationary, and this places limits on extrapolation."

Discussion: "Future TSLS may well be different from the past due to non-linearities or non-stationarities in the relationship (Church et al., 2013). …"

Conclusion: "Future sensitivity may be different from the past as the relationship between warming and sea level rate may be non-linear or non-stationary. We reason that a non-linearity cannot explain the mismatch as the required curvature would be inconsistent with process knowledge encoded by model projections assessed in SROCC and expert expectations (Oppenheimer et al. 2019; Bamber et al., 2019). Based on our analyses we cannot fully reject that the sensitivity has changed between the historical period (1850-2017) and the projection period (2000-2100)."

Future may be different from past

- Discuss the physical mechanisms behind the relationship and thereby stress that sensitivity may be different in future from past, and that this could potentially explain "the discrepancy".
- 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.
- 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.
- Stress that we do not expect TSLS to be constant over time.

We have expanded considerably on past vs future which is closely linked to the limitations of the metric. The revised text is much more explicit on the limitations of the metric. Here are some examples of some revisions:

Introduction: "… However, there may be processes that can cause future sensitivity to be different from the past (Church et al., 2013). These changes can broadly be categorized as being due to a non-linear response to forcing, or due to a non-stationary response where the response depends on state of the system. E.g. the sensitivity of small glaciers to warming will depend on how much glacier mass there is left to be lost, and we therefore expect this to have a non-stationary response. Nature is complex and will be both non-linear and non-stationary, and this places limits on extrapolation."

Discussion: "Future TSLS may well be different from the past due to non-linearities or non-stationarities in the relationship (Church et al., 2013). …"

Conclusion: "Future sensitivity may be different from the past as the relationship between warming and sea level rate may be non-linear or non-stationary. We reason that a non-linearity cannot explain the mismatch as the required curvature would be inconsistent with process knowledge encoded by model projections assessed in SROCC and expert expectations (Oppenheimer et al. 2019; Bamber et al., 2019). Based on our analyses we cannot fully reject that the sensitivity has changed between the historical period (1850-2017) and the projection period (2000-2100)."

Extrapolation is a comparison, not projection

- Expand the discussion to better clarify that extrapolation is only used for a comparison, and not a projection.
- Stress that extrapolation is not a projection but plotted for comparison.

We now explicitly and systematically refer to the extrapolation of the historical relationship as an extrapolation. The projection vs extrapolation is discussed in the introduction, and in general where we discuss the limitations of the TSLS metric.

Clarity on time periods

- Discuss time periods more clearly. Both in figure caption, and when introducing TSLS.
- State time-intervals in figure caption.
- Discuss "century time scale" choice more in main text.
- Explain more in main text time period.

Done! In the previous manuscript the periods used where mainly written in the data & methods section which was almost an appendix. We have restructured the manuscript so that the periods used are much more clear in data section, discussion, and conclusion and in the captions.

The "century time scale" is discussed in the context of non-stationarity, limitations of the metric, and past is not future.

Figure

- Remove superscripts from figure.
- Expand description of figure.
- Expand caption – Explain what each point is, especially their time span.

Done!

Statistics

- Explain statistical methods in detail.
- Add a more complete description that explains that full covariance is unlikely, and how it impacts results.

Done! We have restructured the manuscript so that it now has a separate methods section.

Miscellaneous

- Check if it makes sense to move GMST definition into the main body of text.

Moved.

- Point out that AR5 & SROCC have no hind casts in their presentation of the SLR discussions and it has therefore not been demonstrated that these models can reproduce past sea level rise.

Done!

- 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.

Done! (see also past is not future and limitations of the metric)

- Discuss non-linearity and non-stationary.

Done. New text in intro: "However, there may be processes that can cause future sensitivity to be different from the past (Church et al., 2013). These changes can broadly be categorized as being due to a non-linear response to forcing, or due to a non-stationary response where the response depends *on state of the system. E.g. the sensitivity of small glaciers to warming will depend on how much glacier mass there is left to be lost, and we therefore expect this to have a non-stationary response."*

- Consider discussing common misconception.

Distraction. Not included.

- Call for hindcast validations for future sea level projections.
- Call for historical validation of models used for sea level projections. Not just of the individual contributor models, but also of the aggregate model.

We now discuss hind-casts in both the discussion and conclusion. It should be clear from context that we consider hind casts are very desirable (even ideal), but we do not explicitly call for them. We write: "Ideally, we would test the models using hind casts to verify their ability to reproduce the past. Unfortunately, such hind-casts are unavailable for sea level projection models assessed in both AR5 and SROCC. This is critical as Slangen et al. (2017) identified substantial biases in hind-casts of Greenland surface mass balance, glacier mass loss, and deep ocean heating. These biases increase the modelled sea level rise over the 20th century by ~50%."

- Be explicit about baseline motivation.

Done!

- Explain that we only use published estimates, and motivation.

Done!

- Discuss Slangen2017 as context.

Done! (See discussion and conclusion and notes about hind-casts)

- Add more to the motivation part of the manuscript

The introduction has been expanded.

- Add an outlook for how TSLS discrepancies can be addressed.
    - <u>Brainstorm to consider when revising:</u>
    - Ensure that projection models also have hindcasts of the historical past.
    - Look into the transient sensitivity of individual contributors.
    - Understand how TSLS changes over time
    - Model studies to understand the limitations of TSLS.

We have not added an outlook as the most important point is highlighted in the manuscript elsewhere. "Ideally, we would test the models using hind casts to verify their ability to reproduce the past. Unfortunately, such hind-casts are unavailable for sea level projection models assessed in both AR5 and SROCC. This is critical as Slangen et al. (2017) identified substantial biases in hind-casts of Greenland surface mass balance, glacier mass loss, and deep ocean heating. These biases increase the modelled sea level rise over the 20th century by ~50%."

- Consider adding a short speculative paragraph on uncertainties in balance temperature.

We have not added this. A distraction and possibly too speculative.

Response to RC1: Tal Ezer

> *General Comments:*
>
> *The (very short) paper looks at linear relations between global sea level rise (SLR) rates and time-mean temperatures in both observations and climate model projections- the results suggest that models may underestimate future sea level rise, which is a very important finding. The study is clearly written, and the results are interesting, though since I am not a global climate modeler, I am not sure if this result about the SLR-SST relation in models is new or already known to climate modelers. There are several caveats in the study with its very condensed presentation (only one figure and 1 table), that are needed to be explained (with potentially expanded calculations).*

It was our intent to write a brief discussion letter where we introduce the Transient Sea Level Sensitivity metric. We hope this metric will be adopted by the community as a simple way to compare the first order transient response between different models. In the paper we plot the results of published work in a thought-provoking way. It is therefore our opinion that this is much better suited as a discussion letter, rather than a longer more traditional article.

We are not the first to note that there must be some relationship between sea level rate and temperature. Awareness of this is evident already in the first IPCC assessment report, and the idea was explicitly exploited by Rahmstorf (2007) to construct a semi-empirical model projection. So, we do not consider this to be the main contribution of the paper, although clearly many modelers are not aware of these developments. In our opinion the main contributions of our paper are more prominent and here emphasized explicitly in order of importance:

1) The introduction of the Transient Sea Level Sensitivity (TSLS) metric.
2) The finding that a straight line is a good approximation to the transient response in the models assessed in AR5 and SROCC. I.e. that TSLS is a useful metric that captures most of the transient response according to present physical understanding.
3) The highlighted apparent discrepancy between the TSLS of models used for sea level projections and historical data.

Note especially, that we do not consider the observational extrapolation to be a projection. We explicitly say: "*This does not automatically demonstrate a bias in model projections, but as a minimum call for a detailed explanation*". This is intended as a very clear and explicit caveat, and a call for further work. So, we agree with the referee that more analysis is needed to understand the discrepancy, but also that this is beyond the scope of this discussion letter. However, we gather from the full set of reviews that we need to be more explicit about the limitations of the TSLS metric and will make this point more prominent.

Revision plan:

- Elaborate substantially on the limitations of the metric.
- Discuss the physical mechanisms behind the relationship and thereby stress that sensitivity may be different in future from past, and that this could potentially explain "the discrepancy".
- Expand the discussion to better clarify that extrapolation is only used for a comparison, and not a projection.

> *Major Comments: There are several assumptions that are not completely correct, so their impact should be addressed more extensively.*
>
> *1. SLR rates are far from being linear, they are in general accelerating, but there are also significant multi-decadal variations in SLR rates (e.g., see Frederikse et al., Nature, 2020, doi:10.1038/s41586-020-2591-3). Therefore, the assumption that the SLR-SST linear relation in the past should be the same as in the future may not hold. Moreover, the period chosen for time-averaged SST and SLR may affect the results some experiments to see how sensitive the results are to different chosen periods may be useful.*

We do not assume "*that the SLR-SST linear relation in the past should be the same as in the future*". We simply compare past with future sensitivity and note that there is a discrepancy. But we also stress that "*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 do this for exactly that reason – we will emphasize this even further.

We do not assume a steady acceleration over time. There is multi decadal variability temperature, and that should be reflected in the sea level rate. We get the reviewers point though: that the simple straight line cannot capture all variability. We acknowledge that the TSLS metric is a simplification of a complex system. It can only characterize the first order response. But this is no different from established metrics such as the Transient Climate Response which have proven their usefulness. We will emphasize that this is exactly how we see the value of studying TSLS.

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..Discuss time periods more clearly. Both in figure caption, and when introducing TSLS.

> *2. The SLR-SST relation assumes that SLR is related to SST through thermal expansion, but what about the contribution from water masses? In recent years and in the future contribution to SLR from ice melt will increase relative to thermal expansion (Frederikse et al. 2020, and many others). This by itself may explain the main results here. To see if this is the case, you may add to the calculation results from the same models over the same period as the observations to see if the results are due to model biases or the neglection of water mass contribution.*

We agree that changes in the state of the climate system between the 20th and the 21st century, could potentially explain the discrepancy between the sensitivity in past and in the future. But without further analysis this is speculation. We write: "*This does not automatically demonstrate a bias in model projections, but as a minimum call for a detailed explanation*".

We also agree that it would be great if we could plot the results of the AR5&SROCC models for the historical period to compare to the historical data. Unfortunately, that is simply not possible because such historical runs were never made with the same aggregate model that was used for projections. This lack of a validation is precisely the reason why we feel that it is necessary to compare past and future sensitivity even if this may be an imperfect comparison.

The SLR-SST relation does not hinge on an assumption "*that SLR is related to SST through thermal expansion*". We do not assume this, and we do not neglect the ice mass contribution. Every point in figure 1 include both expansion and water mass contributions.

We do not assume that the relative proportions of the different sea level contributors remain the same. Further, changing proportions is insufficient explanation of the discrepancy. We illustrate this in Note R1 at the end of this document.

Revision plan

- Point out that AR5 & SROCC have no hind casts in their presentation of the SLR discussions and it has therefore not been demonstrated that these models can reproduce past sea level rise.

> *3. Linear regression in Fig. 1 is obtained from only ~5 points, can accuracy be improved by regression over several models, not just the mean of each scenario? Are there for example, models (recent high-resolution) that do follow the observed line? These suggestions may be outside the scope of the study but would greatly help to explain the results and its implications.*

The problem is that sea level rise is not an output from current generation Earth System Models ESM. E.g. The contribution from Greenland is calculated by driving an ice sheet model and a regional climate model with projected weather from an ESM. The total sea level rise is the sum of the contribution from many processes – each with their own model. It is therefore challenging to talk about a recent high-resolution model, as it is a combination of many different models. This is what the IPCC provides, and they also attempt to account for modelling uncertainties as well as possible. The likely range of the IPCC projections are presumably intended to be a fair representation of the modelling uncertainty, and should therefore span recent high resolution models.

A way to understand the discrepancy we observe would be to study how the IPCC models reproduce the historical rates of sea level rise. If hindcasts can reproduce the PI, TG, and SAT rates, then there is no issue. If not, then the historical sea level budget of the models can be dissected to understand if there are issues. Unfortunately, this is not done in the IPCC reports, as they only run the models used for projections for the 21st century and do not show hind casts.

Revision plan:

- Point out that AR5 & SROCC have no hind casts in their presentation of the SLR discussions and it has therefore not been demonstrated that these models can reproduce past sea level rise.
- Call for hindcast validations for future sea level projections.

> *Minor Comments:*
>
> *4. Lines 9-10: "To understand this discrepancy"- I am not sure this is a real discrepancy or just different estimations of future changes.*

This is a question of wording. The difference is a discrepancy, even if there is an as of yet unknown explanation for it.

Revision plan:

- Stress that sensitivity may be different in future from past, and that this could potentially explain "the discrepancy".

- Emphasize more strongly the limitations of the comparison to the observational estimate. Especially in abstract.

> *5. Line 38: ". . . century averaged temperature"- can you define exactly over what period the averaged was calculated (in Fig. 1 it says CO2 since 1850). As mentioned before, it will be useful to know how sensitive the results are to the chosen period, given the non-linear nature of SST and SLR.*

In the current version of the manuscript the time periods are mentioned in the methods section. In the plot the different points were calculated over different time intervals:

- SAT: 1993-2017
- TG: 1900-1990
- PI: 1850-1900
- AR5/SROCC/Experts: 2000-2100

At the moment this is very briefly mentioned in the methods section. Thus, the observational fit is based on data from 1850-2017, and projections are based on data from 2000-2100.

Revision plan:

- State time-intervals in figure caption.
- Discuss "century time scale" choice more in main text.

> *6. In Fig. 1, what are the superscript numbers above labels (numbered references left from a previous submission?)*

That is correct. This will be fixed.

Revision plan:

- Remove superscripts from figure.
- Expand caption – Explain what each point is, especially their time span.

> *7. In Table 1, only 1 out of 4 sensitivity numbers is statistically significant. . . can this be improved by larger set of data from different models, as suggested above?*

As mentioned above then there is no ensemble of models that we can draw from. Also, the aim of the IPCC assessments is to capture the full uncertainty. So presumably the AR5 and SROCC would span the distributions based on different models.

In our view the significance test is just a tool to help us avoid over-interpreting small differences between rows in the table. It is better that these tests are conservative, and we have therefore no goal of improving the significance. Indeed, it would be nice if the entire table was insignificant because that would mean that all the estimates were more consistent with the observational estimates.

There are four TSLS rows in table 1, and we test if they are significantly different from the first row (the observational estimate). So, there are only three tests for TSLS, not four. These shows:

- That expert estimates are not incompatible with historical data.

- That the AR5 TSLS is significantly smaller than the historical TSLS.
- That the SROCC TSLS (as estimated over the entire range) is in better agreement with historical data.

Revision plan:

- Explain statistical tests in methods section.

> *Is there physical meaning to the "balance temperature"?*

Yes. It can be framed as the amount of cooling needed to stop sea level rise (in the short term).

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.

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 20th to the 21st 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.

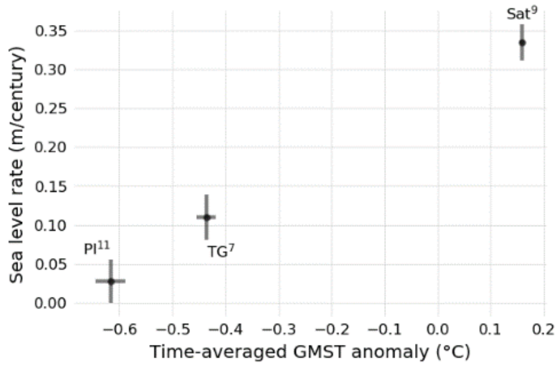
We regret overlooking the numbered references left in the figure from a prior version of the manuscript. This will be fixed in the revision.

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.

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:*
>
> *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 <u>published</u> 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.

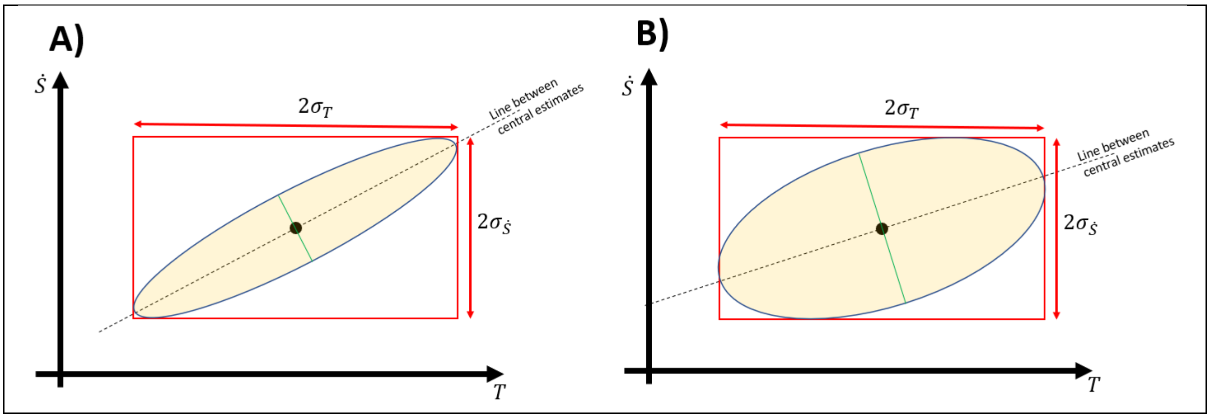


Figure R3.1: A central estimate (black dot) with associated uncertainties σ_T and $\sigma_{\dot{S}}$ (red). The uncertainty covariance matrix is represented by an ellipse. The ellipse must be inscribed in the rectangle. If we assume that the one axis of the ellipse is aligned with the curve between central 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:

"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:*
>
> *- 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.*

Response to RC4: Anonymous Referee #4

> *The paper The transient sensitivity of sea level rise by Grinsted and Christensen discusses the relationship between global mean surface temperature and global mean sea level rise on a time scale of the order of a century. The authors acknowledge earlier work on the topic and frame the relation between temperature and sea level rise as an independent proxy for the evaluation of recent assessments of sea level rise projections that are biased low compared to observations. The article claims a linear sea level sensitivity of 0.4 m/century/K based on observations and either lower sensitivity in AR5 or higher balance temperature in SROCC and Bamber et al., 2019., respectively.*
>
> *General comments*
>
> *The paper is very short and concentrates on the discussion of the discrepancy between the parameters of linear regressions between averaged global mean surface temperature and global mean sea level rise, based on observations (past) and climate projections (future). In the face of high and rising stakes on the response to sea level rise additional proxies for the evaluation of projections of sea level rise are needed. The paper contributes to this end in bringing back the sea level sensitivity into the discussion. I think it is worth to be published and discussed in the community. The paper misses the opportunity to go deeper into the matter and offer thoughts or strategies how to address the discrepancies in transient sea level sensitivity between observations and climate projections.*

Thank you. We agree there are limitations to the metric, but also that it is serves as a useful reality check on sea level models – the comments here indicate that we may expand on the underlying ideas. In particular, we gather from the full set of reviews that we need to discuss limitations more.

Revision plan:

- Add more to the motivation part of the manuscript
- Elaborate substantially on the limitations of the metric.
- Add an outlook for how TSLS discrepancies can be addressed.
    - <u>Brainstorm to consider when revising:</u>
    - Ensure that projection models also have hindcasts of the historical past.
    - Look into the transient sensitivity of individual contributors.
    - Understand how TSLS changes over time
    - Model studies to understand the limitations of TSLS.

> *Specific comments*
>
> *I wonder whether we could learn something more about the impact of model development if the current analysis would include older projections like AR3 and AR4. Those were already below GMSL rise according to Rahmstorf 2007, Horton et al. 2008.*

It is a great idea to look into the TSLS of sea level models used in past IPCC reports (including FAR and SAR to complete the picture). However, this is a distraction and beyond the scope in this manuscript.

> *One weak point of the analysis, as I see it, is the different ranges of GMST used for the regressions of the observations and model projections. Would it be possible and useful to include model estimates from paleo runs that had GMST anomalies in the same range as those projected for the 21st century?*

We agree that this is an important limitation, and based on the full set of reviews we also realize that we need to more explicitly discuss this limitation. Unfortunately, there is very little we can do about it as AR5 and SROCC has not published hindcasts with the same models used for hindcasts.

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.
- Expand the discussion and emphasize more strongly the limitations of the comparison to the observational estimate.

> *The regression lines in Fig. 1 should pass through the mean time-averaged GMST anomaly and the mean sea level rate. Is there any information contained in the scatter of the mean GMST and mean GMSL rate of the individual regressions?*

For the observational trend, and the AR5 trend then the scatter around the trend line is so small compared to the uncertainty of the individual points that I would be careful to read anything into this. However, SROCC responds more non-linearly and there is deviation a straight line fit. The sensitivity is clearly increasing with warming. The TSLS we report is an average over the range of scenarios plotted.

If you were asking for more details about the statistical procedures, then please take a look at our replies to reviewer 2.

> *It would be interesting to discuss some of the physical processes, thresholds, time scales and limitations, that would render the relationship between averaged GMST and GMSL rate non-linear. It would help to establish the transient sea level sensitivity as a metric next to equilibrium sea level rise on longer time scales.*

We agree.

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. Discuss time scales.

> *Are current climate models or model ensembles good enough so that their uncertainty in GMST was smaller than the uncertainty in balance temperature in Table 1? Is the spread in balance temperature inherent in climate models or does it come from the combination of climate models (GMST, steric) with process models (ice sheets dynamics)?*

The answer here will be a little speculative, and so we have not added it to the manuscript. I believe the uncertainty in balance temperature is a consequence of the long equilibration time scales for several of the contributors. It requires a long spin-up of both the ocean and the ice sheets to ensure that it has the full memory of the long term forcing. This will be reflected in the model balance temperature. It will also put strong demands on the long term forcing. We will consider to add a short paragraph on this in the discussion, if it helps to reassure other parts of our discussion.

Revision plan:

- Consider adding a short speculative paragraph on uncertainties in balance temperature.

> *From Table 1 one could deduce sea level rise of 0.28, 0.05, 0.17 and 0.17 m/century at balance temperature. The 0.28 m/century sea level rise in the observations at balance temperature is already above the 0.1-0.2 m/century sea level rise for the 20th century. Since sea level rise is accelerating we are probably above balance temperature since at least the satellite era. This seems to point to a contradiction in the data and the assumption of a linear process. How can the balance temperature be interpreted or how well can we know it?*

There appears to be some confusion with the meaning of the terms “balance temperature” and the “baseline temperature”. The quoted numbers (0.28 etc.) are the sea level rate at T=0, calculated as $\dot{S} = TSLS \cdot (0 - T_{balance})$. So, the 0.28m/century is the sea level rate when temperature is equal to the baseline temperature reference (rather than “at balance temperature”).

This may seem like a minor point but: we would disagree that we assume a linear process. Rather we argue that a linearization is a reasonable approximation to the response. There are limits to how far that linearization would work, but that does not mean that TSLS is not useful. It just means that the state of the system can change so much that the sensitivity to warming changes.

> *Technical corrections*
>
> *l6: assessments from the Intergovernmental Panel on Climate Change implies*
> *l20: and melts.. A perturbation*
> *l20: perturbation in greenhouse gas concentrations change*
> *l47: table 1 and figure 1*
> *l52: table 1*
> *l63: figure 1*
> *l69: figure 1*

We have checked the lines mentioned, but we cannot understand what technical corrections the referee has in mind. These specific lines look good to us.

The transient sensitivity of sea level rise

Aslak Grinsted[1] and Jens Hesselbjerg Christensen[1,2]

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Abstract. Recent assessments from the Intergovernmental Panel on Climate Change (IPCC) imply ies that global mean sea level is unlikely[1] to rise more than about 1.1m within this century, but will increase with further increase beyond 2100., Eeven within the most intensive future anthropogenic carbon dioxidegreenhouse gas emission scenarios are higher levels assessed to be unlikely. However, some studies conclude that considerably greater sea level rise could be realized, and a number of experts assign a substantially higher likelihood of such a future. To understand this discrepancy, it would be useful to have scenario independent metrics that can be compared between different approaches. The concept of a transient climate response sensitivity has proven to be useful to compare the response of climate models. Here, we introduce a similar metric for sea level science. By analyzing mean rate of change in sea level (not sea level itself), we identify a near linear relationship with global mean surface temperature (and therefore accumulated carbon dioxide emissions) in both model projections, and in observations on a century time scale. This motivates us to define the 'Transient Sea Level Sensitivity' as the increase in the sea level rate associated with a given warming in units of m/century/K. We find that model projections fall below extrapolation based on recent observational records. This comparison indicates suggests that the likely upper level of sea level projections in recent IPCC reports would be too low.

1 Introduction

Our planet is warming as anthropogenic emissions are increasing the atmospheric concentration of carbon dioxide. This warming causes sea levels to rise as oceans expand and ice on land melts. A perturbation in greenhouse gas concentrations changes the balance of energy fluxes between the atmosphere and the ocean surface, and in the balance of mass fluxes to and from glaciers and ice sheets. However, the oceans and ice sheets are vast and it takes centuries to heat the oceans, and millenia for ice sheets to respond and retreat to a new equilibrium (Clark et al. 2018; Li et al., 2013; De Conto and Pollard, 2016; Oppenheimer et al. 2019; Clark et al., 2018). In this sense the ice sheets and oceans have a large inertia: An increase in forcing result in a long-term commitment to sea level rise. Simulations by Clark et al. (2018) indicate an equilibrium sea level sensitivity of ~2m/100 GtC emitted CO_2. The equilibrium sensitivity can be compared to paleo-data (e.g. Foster and

[1] The following terms are adopted by the IPCC to indicate the assessed likelihood of an outcome or a result: Virtually certain 99–100% probability, Very likely 90–100%, Likely 66–100%, About as likely as not 33–66%, Unlikely 0–33%, Very unlikely 0–10%, Exceptionally unlikely 0–1%.

Rohling, 2013). Initially the response to a perturbation in forcing is a flux imbalance, i.e. a change in the rate of sea level rise. ~~The relationship between the temperature and the rate of sea level rise has previously been noted (e.g. Warrick and Oerlemans, 1990), and has been used to construct semi-empirical models of sea level rise (Rahmstorf, 2007; Grinsted et al. 2010; Church et al. 2013; Kopp et al., 2106; Mengel et al., 2016).~~ Hence, sea level rise by 2100 does not immediately reflect the temperature in 2100, instead the entire pathway since the forcing change was introduced is important. We therefore expect 21st century sea level rise to better correlate with the century averaged temperature than temperature itself by 2100. Following this, we here propose to ~~consider~~ linearize the relationship between average rate of sea level rise and temperature increase representing the entire preceding century ~~(Figure 1)~~. The slope of this relationship shows how sensitive sea level ~~deduced from observations or models~~ is to century time-scale warming, and is referred to as transient sea level sensitivity (TSLS). The intercept - where the sea level rate is zero - ~~is~~ we interpret~~ed~~ as a *balance temperature*. The relationship between the temperature and the rate of sea level rise has previously been noted (e.g. Warrick and Oerlemans, 1990), and has been used to motivate semi-empirical models of sea level rise (Rahmstorf, 2007; Grinsted et al. 2010; Church et al. 2013; Kopp et al., 2016; Mengel et al., 2016). A key assumption ~~in~~behind such semi-empirical model projections is that the sensitivity implied by historical records is stationary and hence can ~~can~~ be extrapolated into the future. However, there may be processes that can cause future sensitivity to be different from the past (Church et al., 2013). These changes can broadly be categorized as being due to a non-linear response to forcing, or due to a non-stationary response where the response depends on state of the system. E.g. the sensitivity of small glaciers to warming will depend on how much glacier mass there is left to be lost, and we therefore expect this to have a non-stationary response. ~~Reality~~Nature is complex and will be both non-linear and non-stationary, and this places limits on extrapolation. Regardless, the sea level response can always be characterized using the TSLS metric, and we can compare and contrast different estimates.

2 Data

Here we restrict our analysis to published estimates of the Global Mean Sea Level (GMSL) rate. We use three estimates of the historical rate: 1) the tide gauge record (TG) for the period 1900-1990 (Dangendorf et al., 2017); 2) the satellite-altimetry record (Sat; Ablain et al., 2019) from 1993-2017; 3) a reconstruction for the 1850-1900 pre-industrial period (PI; Kopp et al., 2016). The corresponding temporally averaged temperature anomalies and uncertainties are calculated from the HADCRUT4 observationally based ensemble of Global Mean Surface Temperature (GMST) reconstructions (Morice et al., 2012). We follow IPCC's fifth assessment report (AR5; Church et al., 2013) and use a 1986-2005 baseline for temperature anomalies to avoid introducing additional uncertainties from in re-baselining the IPCC assessed projections. The historical estimates are compared to the projected sea level rate and temperature from 2000-2100 from two recent IPCC reports for a range of scenarios: the AR5 (Church et al., 2013), and the Special Report on the Ocean and Cryosphere in a Changing Climate (SROCC; Oppenheimer et al., 2019). Finally, we show the results of an expert elicitation (Bamber et al., 2019) which pertain to scenarios with 2°C and a 5°C warming by 2100 relative to the pre-industrial. These ~~data is~~estimates are shown in Figure 1.

3 Methods

The relationship between temperature and GMSL rate is estimated for each group of points using linear regression. The observational estimates of both temperature and sea level rate (Figure 1, black) are uncertain. We use Monte Carlo sampling to propagate these uncertainties to our estimates of the line parameters listed in Table 1. Uncertainties in the ~~AR5 and SROCC~~ projections assessed in AR5 and SROCC are ~~given~~specified as a central estimate and a likely range for both temperature and sea level (Church et al., 2013; Oppenheimer et al. 2019; Mastrandea et al., 2010). The IPCC sources do~~es~~ not provide information on the uncertainty covariance between projections of temperature and sea level. However, we observe that the upper and lower likely limits of temperature paired with the corresponding limit of sea level falls very close to the curve between central estimates (see Figure 1). This indicates that there is a very high degree of covariance. For simplicity, we therefore assume full covariance between uncertainties in projected temperature and projected sea level, and depict this using the slanted error bars displayed in Figure 1.

Table 1 reports several estimates of TSLS, and we want to understand if ~~the~~ each is substantially different to the corresponding observational estimate considering the uncertainties. We therefore test if the absolute difference is larger than zero considering uncertainties in both estimates, using a standard two-tailed hypothesis test assuming normality.

We show the total cumulated anthropogenic CO_2 emissions associated with a given temperature as a secondary horizontal axis in Figure 1 (IPCC, 2013; Meinshausen et al., 2011). We established this relationship using both historical data, and the mid-range temperature projections for the RCP scenarios, and thus does not account for uncertainties in the e.g. climate sensitivity. The cumulated emission and temperatures were averaged over the same time intervals.

4 Results

The estimates of the temporal average rate of sea level rise against corresponding temporal average of GMST from a variety of sources are shown in Figure 1. The AR5 and SROCC projected rate of sea level rise over the 21st century from different scenarios show a close correspondence with projected temperatures (Figure 1, red and blue). We fit straight lines to these projections, and the slope gives a TSLS of $0.27^{+0.03}_{-0.01}$ m/century/K for AR5, and $0.39^{+0.04}_{-0.03}$ m/century/K for the ~~SROCC~~ models assessed in SROCC (Table 1). The historical rate of sea level rise in three different periods also show a close relationship to warming (Figure 1, black). From this we estimate a TSLS of 0.40±0.05 m/century/K. Finally, we ~~plot~~represent the results of expert elicitation of 21st century sea level rise under two different warming scenarios (Bamber et al. 2019), which yield a sensitivity of $0.42^{+0.31}_{-0.09}$ m/century/K. The balance temperatures corresponding to all TSLS estimates are listed in Table 1.

5~~2~~ Discussion

We find that both model projections and observations show a near linear relationship between century averaged temperature change and the average rate of sea level rise (Figure 1). A linearization captures the bulk of the sea level response on these time scales. This shows that the concept is sound and that TSLS is a suitable new metric for assessing the graveness of global mean sea level changes.

The relationship deduced from model projections ~~are~~differs systematically ~~below the~~from extrapolation of the observational relationship (Table 1 and Figure 1). Sea level projections assessed in AR5 have~~s~~ a substantially smaller TSLS than exhibited by historical observations, whereas SROCC is more comparable (Table 1). The greater SROCC sensitivity is driven by the warmest scenario and the higher TSLS is accompanied by a warmer balance temperature that is far from the observationally based estimate (Table 1). Future ~~Observation based data indicate a TSLS of 0.39±0.05 m/century/K with a balance temperature of -0.71±0.08 °C (see Table 1 and Data & Methods). Future~~ TSLS may well be different from the past due to non-linearities or non-stationarities in the relationship (Church et al., 2013). Thus, the discrepancy highlighted by Figure 1 does not necessarily demonstrate a bias in model projections, but as a minimum call for a yet to be prepared detailed explanation. Ideally, we would test the models using hind casts to verify their ability to reproduce the past. Unfortunately, such hind-casts are unavailable for sea level projection models assessed in both AR5 and SROCC. This is critical as Slangen et al. (2017) identified substantial biases in hind-casts of Greenland surface mass balance, glacier mass loss, and deep ocean heating. These biases increase the modelled sea level rise over the 20th century by ~50%. The discrepancy between historical and projected sensitivities is ~~disconcerting~~puzzling considering the lack of possibilities for a validation of the model projections.

~~It is clear from Figure 1 that I~~In ~~-~~order for non-linearities to explain the discrepancy between the past and future relationship between warming and sea level rate, it is evident from Figure 1 that ~~these~~ these would have to be sub-linear. This is incompatible with our current understanding. Major non-linearities are not expected this century according to the process knowledge encoded in the ~~AR5 and SROCC~~ model projections assessed in both AR5 and SROCC, with SROCC ~~showing~~presenting some signs of a super linear response (Figure 1). Antarctica, in particular, may have a super-linear response (Oppenheimer et al. 2019; DeConto and Pollard, 2016; Edwards et al. 2019; Bamber et al. 2019). Further, expert elicitation results overlap with the relationship found for the historical period but with a higher sensitivity (Table 1), which may be due to an anticipated super-linear response not captured by AR5 and SROCC assessment of model results. Antarctic rapid ice dynamics was considered as scenario independent in the IPCC fifth assessment report (AR5; Church et al., 2013), in stark contrast to later results (Oppenheimer et al. 2019; DeConto and Pollard, 2016; Edwards et al. 2019). We therefore propose AR5 to have a TSLS likely upper bound, which is biased low.

~~, as the sea level response is not necessarily completely linear in warming (Church et al. 2013). Antarctica in particular may have a super-linear response (Oppenheimer et al. 2019; DeConto and Pollard, 2016; Edwards et al. 2019; Bamber et al. 2019). Expert elicitation results overlap with the relationship found for the historical period but with a higher sensitivity ($0.42^{+0.31}_{-0.09}$ m/century/K), which may be due to an anticipated super-linear response. It is therefore disconcerting that the relationship deduced from model projections are systematically below observational constraints (table 1 and figure 1). This does not automatically demonstrate a bias in model projections, but as a minimum call for a detailed explanation. Antarctic rapid ice dynamics was considered as scenario independent in the IPCC fifth assessment report (AR5; Church et al., 2013), in stark contrast to later results (Oppenheimer et al. 2019; DeConto and Pollard, 2016; Edwards et al. 2019). We therefore propose AR5 to have a TSLS which is biased low. The IPCC Special Report on the Ocean and Cryosphere in a Changing Climate (SROCC; Oppenheimer et al., 2019) has a larger TSLS of 0.39±0.04 m/century/K in better agreement with the observations but has a balance temperature far from that of the observations (table 1). Our analysis therefore implies that the model states used for the assessment in SROCC are too close to balance for present-day conditions and at the same time underestimate TSLS. Taken together this suggests that the projected global sea level rise by the end of this century in various IPCC reports are at best conservative and consequently underestimate the likely sea level rise by the end of this century.~~

~~Data & Methods~~6 Conclusion

We define a new Transient Sea Level Sensitivity (TSLS) metric, which relates the rate of global mean sea level rise to global mean surface temperature. We find that this metric can account for most of ~~the~~sea level response to temperature increase on a ~~one century~~~~hundred year~~hundred-year time scale. The TSLS metric is useful as it allows for model sensitivity comparisons, even if the models have not been run for the same set of scenarios. By framing the transient sensitivity in terms of temperature we separate the sea level sensitivity from climate sensitivity to a large extent. This allows for easier comparison between sea level models that are forced by different E~~e~~arth system models.

We compare the ~~IPCC~~ model projections over the 21st century assessed by the IPCC with historical records from 1850-2017. We find that the ~~both AR5 and SROCC~~ model projections assessed in both AR5 and SROCC fall substantially below an extrapolation of historical records (Figure 1). This is reflected in the estimates of TSLS and balance temperature, which does not match the historical estimate (Table 1). Future sensitivity may be different from the past as the relationship between warming and sea level rate may be non-linear or non-stationary. We ~~argue~~reason that a non-linearity cannot explain the mismatch as the required curvature would be inconsistent with process knowledge encoded by ~~SROCC~~ model projections assessed in SROCC and expert expectations (Oppenheimer et al. 2019; Bamber et al., 2019). ~~In this study~~Based on our analyses we cannot fully ~~exclude~~reject that the sensitivity has changed between the historical period (1850-2017) and the projection period (2000-2100). The major sea level contributors have characteristic response times of several centuries (Clark et al. 2018; Li et al., 2013; DeConto and Pollard, 2016; Oppenheimer et al. 2019; Church et al. 2013), which suggests that the sensitivity is unlikely to change substantially between these periods. ~~However,~~The ~~results~~outcome of an expert elicitation is more consistent with ~~the~~an extrapolation of the historical relationship than AR5 and SROCC (Figure 1 and Table 1). Further, Slangen et al. (2017) identified substantial biases in process model hind-casts, which draws into question whether the AR5 and SROCC assessed models would be able to reproduce historical sea level rise. This is supported by our interpretation of the TSLS discrepancy between past and future. Our analysis ~~suggests~~implies that the model states used for the assessment in SROCC are too close to balance for present-day conditions and at the same time underestimate TSLS. ~~Our analysis therefore implies that the model states used for the assessment in SROCC are too close to balance for present-day conditions and at the same time underestimate TSLS.~~ Taken together this suggests that the projected global sea level rise by the end of this century in various IPCC reports are at best conservative and consequently underestimate the upper bound of likely sea level rise by the end of this century.

Data availability

All data has previously been published and are publicly available.

Author contributions

AG designed the research study and conducted the analysis. AG and JHC interpreted the results and wrote the manuscript.

Competing interest

The authors declare that they have no conflict of interest.

~~Temporally averaged temperature anomalies and uncertainties are calculated from the HADCRUT4 observationally based ensemble of Global Mean Surface Temperature (GMST) reconstructions (Morice et al., 2012). We follow AR5 (Church et al., 2013) and use a 1986-2005 baseline for temperature anomalies, and define 1850-1900 as the pre-industrial (PI). Uncertainties in AR5 and SROCC projections are given as a central estimate and a likely range for both temperature and sea level (Church et al., 2013; Oppenheimer et al. 2019; Mastrandea et al., 2010). We simply assume full covariance between uncertainties in projected temperature and projected sea level, and depict this using slanted error bars in figure 1. The uncertainties fall very close to the line connecting central estimates, supporting this assumption. We use three estimates of historical global mean sea level change rates based on: 1) the tide gauge record (TG) for the period 1900-1990 (Dangendorf~~

et al., 2017); 2) the satellite-altimetry record (Sat; Ablain et al., 2019) from 1993-2018; 3) a reconstruction for the pre-industrial period (PI; Kopp et al., 2016). Finally, we show the results of an expert elicitation (Bamber et al., 2019) which pertain to scenarios with 2°C and a 5°C warming by 2100 relative to the pre-industrial. We show the cumulated anthropogenic CO_2 emissions associated with a given temperature as a secondary horizontal axis in figure 1 (IPCC, 2013; Meinshausen et al., 2011). We established this relationship using both historical data, and the mid-range temperature projections for the RCP scenarios, and thus does not account for uncertainties in the e.g. climate sensitivity. The cumulated emission and temperatures were averaged over the same time intervals.

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References

Ablain, M., Meyssignac, B., Zawadzki, L., Jugier, R., Ribes, A., Spada, G., Benveniste, J., Cazenave, A., and Picot, N.: Uncertainty in satellite estimates of global mean sea-level changes, trend and acceleration, Earth Syst. Sci. Data, 11, 1189–1202, https://doi.org/10.5194/essd-11-1189-2019, 2019.

Bamber, J. L., Oppenheimer, M., Kopp, R. E., Aspinall, W. P., & Cooke, R. M.. Ice sheet contributions to future sea-level rise from structured expert judgment. Proceedings of the National Academy of Sciences, 116(23), 11195-11200, 2019.

Church, J.A., P.U. Clark, A. Cazenave, J.M. Gregory, S. Jevrejeva, A. Levermann, M.A. Merrifield, G.A. Milne, R.S. Nerem, P.D. Nunn, A.J. Payne, W.T. Pfeffer, D. Stammer and A.S. Unnikrishnan: Sea Level Change. In: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)]. Cambridge University Press, Cambridge, United Kingdom and New York, NY, USA, 2013.

Clark, P. U., Mix, A. C., Eby, M., Levermann, A., Rogelj, J., Nauels, A., & Wrathall, D. J., Sea-level commitment as a gauge for climate policy. Nature Climate Change, 8(8), 653-655, 2018.

Dangendorf, S., Marcos, M., Wöppelmann, G., Conrad, C. P., Frederikse, T., & Riva, R., Reassessment of 20th century global mean sea level rise. Proceedings of the National Academy of Sciences, 114(23), 5946-5951. doi:10.1073/pnas.1616007114, 2017.

DeConto, R. M., & Pollard, D., Contribution of Antarctica to past and future sea-level rise. Nature, 531(7596), 591, 2016.

Edwards, T. L., et al., Revisiting Antarctic ice loss due to marine ice-cliff instability. Nature, 2019, 566.7742: 58.

Foster, G. L., & Rohling, E. J. (2013). Relationship between sea level and climate forcing by CO2 on geological timescales. Proceedings of the National Academy of Sciences, 110(4), 1209-1214, 2019.

Grinsted, A., Moore, J. C., & Jevrejeva, S. (2010). Reconstructing sea level from paleo and projected temperatures 200 to 2100 AD. Climate dynamics, 34(4), 461-472. doi:10.1007/s00382-008-0507-2.

IPCC, Summary for Policymakers. In: Climate Change 2013: The Physical Science Basis. Contribution of Working Group I to the Fifth Assessment Report of the Intergovernmental Panel on Climate Change [Stocker, T.F., D. Qin, G.-K. Plattner, M. Tignor, S.K. Allen, J. Boschung, A. Nauels, Y. Xia, V. Bex and P.M. Midgley (eds.)], 2013.

Kopp, R. E., Kemp, A. C., Bittermann, K., Horton, B. P., Donnelly, J. P., Gehrels, W. R., Hay, C. C., Mitrovica, J. X., Morrow, E. D., Rahmstorf, S., Temperature-driven global sea-level variability in the Common Era. Proceedings of the National Academy of Sciences, 113(11), E1434-E1441, 2016.

Li, C., von Storch, J. S., & Marotzke, J. (2013). Deep-ocean heat uptake and equilibrium climate response. Climate Dynamics, 40(5-6), 1071-1086.

Mastrandrea et al., Guidance Note for Lead Authors of the IPCC Fifth Assessment Report on Consistent Treatment of Uncertainties (IPCC, New York, 2010), 2010.

Meinshausen, M., Smith, S.J., Calvin, K. et al., The RCP greenhouse gas concentrations and their extensions from 1765 to 2300. Climatic Change 109, 213 (2011). doi:10.1007/s10584-011-0156-z, 2011.

Mengel, M. et al. Future sea level rise constrained by observations and long-term commitment. Proc. Natl. Acad. Sci. USA 113, 2597–2602, 2016.

Morice, C.P., Kennedy, J.J., Rayner, N.A. and Jones, P.D., Quantifying uncertainties in global and regional temperature change using an ensemble of observational estimates: the HadCRUT4 dataset. Journal of Geophysical Research, 117, D08101, doi:10.1029/2011JD017187, 2012.

Oppenheimer et al., Sea Level Rise and Implications for Low Lying Islands, Coasts and Communities, Chapter 4 in IPCC Special Report on the Ocean and Cryosphere in a Changing Climate [H.-O. Pörtner, D.C. Roberts, V. Masson-Delmotte, P. Zhai, M. Tignor, E. Poloczanska, K. Mintenbeck, M. Nicolai, A. Okem, J. Petzold, B. Rama, N. Weyer (eds.)], 2019.

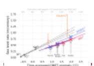
Rahmstorf, S., A semi-empirical approach to projecting future sea-level rise. Science, 315(5810), 368-370, 2007

Warrick, R. A. & Oerlemans, H. in Climate Change, The IPCC Scientific Assessment (eds Houghton, J. T., Jenkins, G. J. & Ephraums, J. J.) 257–281 (Cambridge Univ. Press). 1990.

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.

Table 1: Transient sea level sensitivity, and balance temperatures estimated from different sources. Intervals are likely ranges (17-83%). Symbols indicate that the difference from the observational estimate is significant at p<0.05 (*), and p<0.1 (†) using a two-tailed test assuming normality.

	Sea level sensitivity m/century/K	Balance Temperature °C
Observations	0.40 [0.35 – 0.44]	-0.70 [-0.77 – -0.64]
SROCC	0.39 [0.36 – 0.43]	-0.14† [-0.42 – 0.23]
AR5	0.27* [0.26 – 0.30]	-0.63 [-0.70 – -0.41]
Expert elicitation	0.47 [0.33 – 0.85]	-0.37* [-0.36 – -0.05]



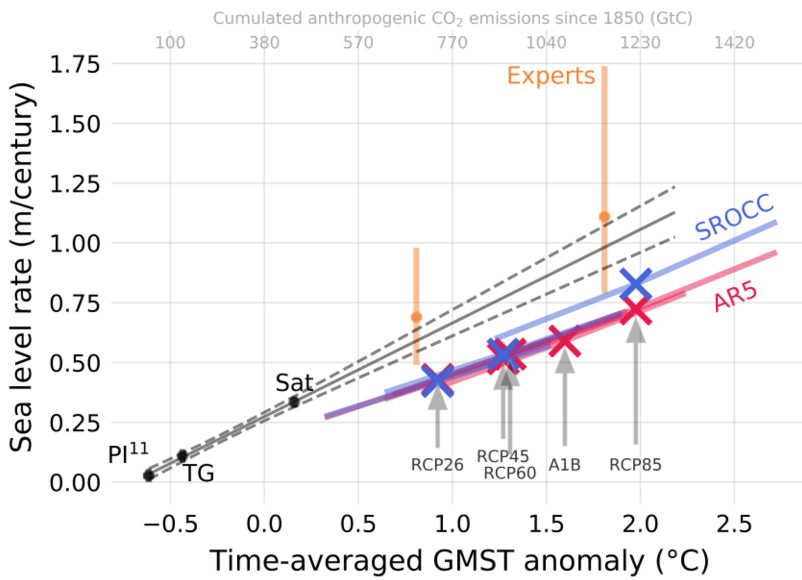


Figure 1: The rate of sea level rise versus long term average temperature as seen in observations (black), in model projections (red/blue), and expectations in an expert elicitation (orange). Each point represents an average over a time period (PI: 1850-1900; TG: 1900-1990; SAT: 1993-2017; AR5/SROCC/Experts: 2000-2100). ~~Likely ranges for SROCC and AR5 are shown as slanted error bars.~~ Sea level projections as assessed in AR5 and SROCC systematically fall below what would be expected from extrapolating ~~the~~ observations to warmer conditions~~,~~ as well as ~~to~~ below the expert elicitation~~s~~. Error bars show ~~the~~ estimated likely ranges (17-83%). Likely ranges for SROCC and AR5 are shown as slanted error bars.