

## **Response to Reviewer #1**

We thank the Reviewer for the appreciation of our work.

We have checked the typos evidenced by the Reviewer. Note that Otranto was correct as the statement indeed refers to the Adriatic basin, of which the Otranto Strait is the southern boundary separating it from the Ionian Sea.

## **Response to Reviewer #2**

We thank the Reviewer for the appreciation of our work and for the insightful comments she/he has provided about our revised manuscript. Below we provide our detailed response to the specific comments by the Reviewer (in bold and italic fonts).

### ***Review of “Sea-level rise in Venice: historic and future trends”***

***This is the second round of review of the paper, and the authors have substantially improved the manuscript, and I only have some minor comments left. The exception to this is the section on sea-level projections (section 6.2), which still contains a substantial number of confusing statements with regards to how sea-level projections are made.***

***When section 6.2 is properly revised and the other minor issues have been taken care of, I think the paper can be published.***

REPLY: We are confident that the newly revised version of the manuscript satisfies all the requests by the Reviewer.

### ***Remarks on section 6.2***

***L710: The SROCC report also provides regional sea-level projections based on CMIP5 model output. Now the text suggests that some sort of down-scaling is needed from these projections, while they are already available in gridded format. These projections form the basis of Kopp et al. (2014) and follow the methodology outlined in Slangen et al. (2012).***

***The big question, which is touched upon, but not answered in this section, is whether the CMIP5 models and other projections capture the relevant processes that cause sea-level changes in Venice. Can anything be said about this question?***

REPLY: We provide the following arguments to support the need for additional evaluation of sea-level rise scenarios for Venice.

First, an important caveat with the CMIP5 model results and the data provided in the SROCC and AR5 is their reliability for coastal assessments. For instance, the SROCC datasets contain jumps of up to 5 cm between estimates of sea-level rise at year 2100 under the RCP8.5 scenario between neighboring coastal pixels in the bay of Biscay. These jumps are not physical, rather reflect the fact that coastal pixels are not covered by all models. This issue applies also to many coastal pixels in the Mediterranean Sea.

Second, many studies revealed the possibility that General Circulation Models do not represent realistically the water exchange through the Strait of Gibraltar (see also our response to a comment below). A very recent dynamical downscaling work (EGU 2021 presentation from Chaigneau et al.) revealed very large sea-level discrepancies between a regional model and GCM simulations in the Mediterranean Sea.

On the premise that CMIP5 projections for the Mediterranean Sea may not be reliable, our method (following Meyssignac et al., 2017) allows recognizing the CMIP5 issue in the Mediterranean basin and inflates accordingly the uncertainty in projections of the stereodynamics component. In other words, this increase in uncertainty is consistent with the fact that there is a low confidence in the CMIP5 projections from the 12 GCMs that cover the Mediterranean Sea (see our response below regarding the Reviewer's comment about L779). Apart from that, improvements of sea-level projections for the Mediterranean requires regional high-resolution modeling to resolve the relevant dynamical processes.

This paragraph in the revised version of the manuscript reads as follows:

“Then, the stereodynamic component is derived from the outputs of the coupled climate-model simulations performed within the 5th phase of the Coupled Model Intercomparison Project (CMIP5). The rather coarse resolution of coupled climate models prevents an accurate representation of small-scale processes (e.g., water exchange at Gibraltar), which in turn affects regional sea-level estimates (Marcos and Tsimplis, 2008; Slangen et al., 2017). Another important caveat on multi-model assessments is their reliability on coastal regions where the contributing models may differently resolve the coastline and bathymetry peculiarities, thus yielding local anomalies in the gridded multi-model output that may reflect a bias originated by heterogeneous spatial resolutions across models rather than a physical process (e.g., Landerer et al., 2014).

On this premise, we propose probabilistic projections of Northern Adriatic RSL for two climate scenarios (RCP2.6 and RCP8.5) and one high-end scenario following Meyssignac et al. (2017) and Thiéblemont et al. (2019). The method allows to inflate the uncertainty in projections of the stereodynamics component by accounting for the low confidence in projections of coastal sea-level rise obtained from the limited number of global circulation models participating in CMIP5 and covering the Mediterranean Sea (see Figure 2 in Thiéblemont et al., 2019). Specifically, the Mediterranean stereodynamic sea-level projections are estimated by relying on those of the Atlantic area near Gibraltar. [...]"

## References

Meyssignac, B., Slangen, A. B. A., Melet, A., Church, J. A., Fettweis, X., Marzeion, B., Agosta, C., Ligtenberg, S. R. M., Spada, G., Richter, K., Palmer, M. D., Roberts, C. D. and Champollion, N.: Evaluating Model Simulations of Twentieth-Century Sea-Level Rise. Part II: Regional Sea-Level Changes. *J. Climate*, 30, 8565-8593, 2017

**L712: “Here “likely” corresponds to the IPCC uncertainty language, meaning that the probability of future sea-level change within this range is estimated from ≥66% to 100%, and therefore does not exclude values outside this range” What does this mean?**

REPLY: We have removed the last confusing part of the sentence

**L716: “Deep uncertainty” is a very specific term with a specific definition. Suggestion to remove it here.**

REPLY: The reference to Bakker et al. (2017) in the sentence with the quoted text specifies "deep uncertainty" in the context of West Antarctic Ice Sheet contribution. More generally, we quote a paragraph

from a recent paper of Haasnoot et al. (2020): "Despite the growth of scientific studies about Antarctica, its contribution to future rate of SLR is still highly uncertain and undergoing a strong scientific debate [Kopp et al., 2017]. In fact, the uncertainty in projected SLR increased recently [Garner et al., 2018; Bamber et al., 2019]. In decision making literature this is referred to as 'deep uncertainty' [Lempert, 2019], which occurs when experts do not have sufficient knowledge or when parties to a decision cannot agree upon the system processes and futures."

We have revised the text by adding the following clarifying sentence: "The high uncertainty and strong scientific debate on the contribution of Antarctic ice-sheet melting to the future rate of sea-level rise generates the so-called 'deep uncertainty' [Lempert, 2019], i.e., a condition where experts lack sufficient knowledge or parties to a decision cannot agree upon the system processes and futures (see also Haasnoot et al., 2020)."

#### References

Haasnoot, M., Kwadijk, J., van Alphen, J., Le Bars, D., van den Hurk, B., Diermanse, F., van der Spek, A., Oude Essink, G., Delsman, J. and Mens, M.: Adaptation to uncertain sea-level rise; how uncertainty in Antarctic mass-loss impacts the coastal adaptation strategy of the Netherlands. *Environ. Res. Lett.* 15, 034007, 2020

Lempert, R. J.: Robust Decision Making (RDM) BT—Decision Making under Deep Uncertainty: From Theory to Practice, Editors: Marchau, V. A. W. J., Walker, W.E., Bloemen, P.J.T.M., Popper, S.W. (Berlin: Springer), 23–51, 2019.

***L717: "Therefore": how does this sentence follow from the previous? And where does the "up to 2m" come from?***

REPLY: We agree, we have removed the "Therefore" and the parenthesis where the 2 m level was mentioned.

***L720: "Slangen et al. (2017) suggest": That paper doesn't "suggest" that number, it shows model results, and this number comes from model results and is not a 'suggestion'.***

REPLY: We agree, we have changed the sentence as "According to Slangen et al. (2017), the sea-level rise at the subtropical [...]"

***L722: The coupled climate models used for CMIP3/AR4, CMIP5/AR5 do simulate the Mediterranean Sea directly. See for example Landerer et al. (2007, doi: 10.1175/JPO3013.1). Sterodynamic effects are computed directly by these models (the 'zos' and 'zostoga' variables in CMIP5/6 models) and do not need to be computed offline. The same holds for the associated water mass redistribution: this effect is also included in these models and is stored as variable 'pbo'.***

REPLY: We have removed the sentences "The steric effects are computed from temperature and salinity changes using a diagnostic offline computation. This computation obviously depends on the water depth and tends to zero at the coastline. Therefore, [...]"

***L723: "Pioneering work in this regard is Slangen et al. (2012)" the pioneering work in this paper is not about the ocean models and statistical downscaling: it's about combining ocean sterodynamics from***

***CMIP models with GRD effects and GIA to make regional sea-level projections that include all relevant processes.***

REPLY: We agree, the position of the sentence is misleading. We have removed the sentence as Slangen et al. (2012) is correctly quoted in line 762.

***L742: As noted in the previous round: there's no reason to assume that GMSL is equal to sea level in Venice, so there's no 'consistency' when both numbers are close. Suggest to replace 'consistent with' by 'on the same order as' or 'similar to'.***

REPLY: We agree as we also explicitly state in the paragraph starting at line 440. We have changed "consistent with" with "similar to". We have also checked the usage of "consistency" throughout the text and changed where deemed necessary with "similarity" or analogs.

***L747: "which is ignored in the computation of the pure steric effect": This might be a bit of a strawman argument: to my knowledge, no projections just use the steric effect to approximate the total sterodynamic (steric + bottom pressure) effects. See also my comments for L722***

REPLY: We have removed the quoted text from the sentence.

***L764: "Their projections build on the decomposition of the recorded historical sea level into several processes, including the "background non-climatic local sea-level change" corresponding to GIA, tectonics, and other non-climatic local effects." This is not an accurate description of the Kopp et al. (2014) framework. They use the AR5 projections for most terms (or switch to an alternative projection for the ice sheets in the Kopp et al. (2017) update) and use a statistical framework to estimate the non-climatic component at each tide-gauge location.***

REPLY: We have updated the description as follows: "Their projections build on a combination of expert community assessment (the IPCC-AR5), expert elicitation (e.g., Bamber and Aspinall, 2013), and process modelling (e.g., the 5th phase of the Coupled Model Intercomparison Project or CMIP5) for most sea-level contributors. The "background non-climatic local sea-level change" corresponding to GIA, tectonics, and other non-climatic local effects was derived by applying a Gaussian process model to tide gauge records. This background linear estimate [...]"

Reference

Bamber, J. L., and Aspinall, W. P.: An expert judgement assessment of future sea level rise from the ice sheets. *Nature Climate Change* 3, 424–427, 2013.

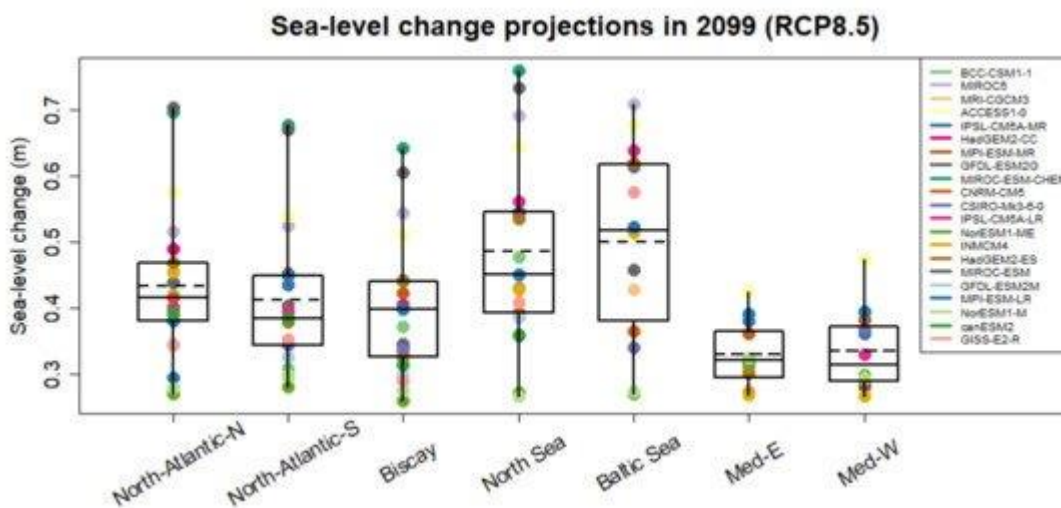
***L779: "The Mediterranean sterodynamic sea-level projections are estimated by relying on those of the Atlantic area near Gibraltar." This is an interesting remark, as it's a much 'coarser' approximation than the coarse CMIP models. Is it a better approach and do the CMIP models suggest something different? From Figure 7 in Slangen et al. (2017) that doesn't seem to be the case.***

We thank the Reviewer for this comment that indeed deserves slightly more explanations. Figure R1 below (from Thiéblemont et al., 2019) shows projections of the sterodynamic component from CMIP5 models from RCP8.5 by the end of the 21st Century for several basins in Europe. The figure shows that semi-enclosed seas are not fully covered by all models—among the 21 models, only 14 and 12 cover the Baltic and Mediterranean basins, respectively. Furthermore, the central estimates and the model spread in the Mediterranean sea are

found to be lowered compared to surrounding basins. These differences between model spatial coverage result in inconsistencies when computing multi-model ensemble statistics, which in turn could significantly affect the spatial homogeneity of regional sea-level rise projections.

Beside this multi-model sampling issue, Landerer et al. (2014) detected unrealistic SSH biases in marginal seas for some CMIP5 models (e.g. -15 m over the Mediterranean for MIROC-ESM historical simulations). They could not identify a reason for such biases but suspect that the model resolution could play a role. Parras-Berocal et al. (2020) found that MPI-ESM-LR is not able to represent the exchange through Gibraltar. Meyssignac et al. (2017), who analyzed CMIP5 historical simulations, argued that the coarse resolution of climate models does not enable the simulation of the mesoscale processes and the water exchanges at Gibraltar, which results in a poor representation of the Mediterranean sea level in GCMs. As a consequence, they excluded the Mediterranean basin from the sea level simulations and instead use the sea level in the Atlantic, off the Strait of Gibraltar, as an approximation for the Mediterranean sea level.

Based on these different elements and studies, we followed the procedure of Meyssignac et al. (2017) in the present work rather than relying on the 12 CMIP5 models that provide sterodynamic estimates in the Med. basin.



**Figure R1.** CMIP5 sterodynamic projections in 2099 (ref period 1986–2005) for the North-Atlantic-N, North-Atlantic-S, Bay of Biscay, North Sea, Baltic Sea, Mediterranean-E, and Mediterranean-W Sea under the RCP8.5 scenario. Whisker boxes display the multi-model 1<sup>st</sup> quartile, median, and 3<sup>rd</sup> quartile and the dashed line shows the multi-model mean. After Thiéblemont et al. (2019)

#### References

Landerer, F.W., Gleckler, P. J., Lee T.: Evaluation of CMIP5 dynamic sea surface height multi-model simulations against satellite observations. *Climate Dynamics* 43, 1271–1283, 2014.

Parras-Berocal, I.M., et al.: The climate change signal in the Mediterranean Sea in a regionally coupled atmosphere–ocean model. *Ocean Sci.*, 16, 743–765, 2020

**L787: I think I know what is meant by “added linearly” but it might be clearer to write out the equation for the combination of the uncertainties.**

REPLY: The adopted equation is as follows:

$$\sigma_{tot}^2 = (\sigma_{sterodynamic} + \sigma_{smb-a} + \sigma_{smb-g})^2 + \sigma_{Glac}^2 + \sigma_{LW}^2 + \sigma_{dyn-a}^2 + \sigma_{dyn-g}^2$$

where smb stands for Surface Mass Balance, Glac for Glaciers, LW for Landwater and dyn-a/dyn-g for Dynamic Antarctic and Greenland. It is basically the same equation as in the supplementary material of Chapter 13 of IPCC AR5, as mentioned in the manuscript. We have added this in the revised manuscript.

**L788: “The projections do not include expert elicitation and rely only on IPCC-like assessments, so the RCP2.6 is rather symmetric and RCP8.5 slightly asymmetric.” The IPCC process is pretty close to an expert elicitation, and the link to the symmetry/asymmetry of the uncertainties is not related to that per se.**

REPLY: We agree that the sentence may be confusing and we have removed it from the revised manuscript.

#### **Other remarks**

##### **The definitions on L69ff:**

**“The acronym RSL is therefore used for tide-gauge data”: I’d say: ‘Tide gauges typically measure RSL’ instead. Same for altimetry: they measure GSL.**

REPLY: We have changed “The acronym RSL is therefore used for tide-gauge data” with “Tide gauges typically measure RSL” and “The acronym GSL is therefore used for satellite altimetry sea-level data;” with “Satellite altimetry provides GSL”;

**“VLM-corrected RSL” That is the same thing as GSL:  $GSL = RSL + VLM$ . Like above: tide gauges corrected for VLM measure GSL.**

REPLY: We prefer to keep our notation to make it clear that those data are measured by a tide gauge and not by an altimeter.

##### **GMSL is spatially-averaged RSL.**

REPLY: Agreed, we have updated the statement and have checked the text for the correct use of GMSL in the revised sense.

**L443: ‘consistency’. I think the authors mean ‘similarity’ here. GMSL and sea level in Venice may have a similar trend, but that has nothing to do with ‘consistency’.**

REPLY: Agreed, we changed to “similarity”.

**L445: “to put local changes in the context of global mean changes”. Vague and a circular reasoning. It now reads like it instructive to compare both, in order to see if they’re comparable.**

REPLY: We change “instructive” with “relevant”. As we have reported in our response to the Reviewer’s public comment, this “analysis was indeed motivated by the fact that available sea-level rise projections for Venice are in some cases directly based upon estimates of the GMSL rise (see, for instance, Troccoli et al., 2012, and Carboing et al., 2010).” We have inserted this statement in the revised manuscript.

**L451: ‘connection’ as said above, there’ no connection. Only similarity. Check also the sentences after L450 for similar suggestions of connections/discrepancies etc.**

REPLY: Connection in this case was meant as a statistical relation between the data, not a physical connection between processes. We have rephrased by removing “the connection between”.

**L570-L575: “The two-way water exchange regime...” This and the following sentence are vague and I don’t understand what’s being said here. What message should I get from these sentences?**

REPLY: We have rephrased as “Watermass exchanges across the Strait...”

**L582: “strengthened by steric changes since the late 1950s”: what does that mean? From Frederikse et al. (2020): “Before the end of the 1950s, in situ observations are too sparse to derive unbiased steric changes”. How do we know they did not play a role before the 1950s?**

REPLY: The sentence was indeed shrunk too much. We have rephrased as follows: “There, GIA was predominant over the ocean-mass contribution to determine the upward sea-level trend over the 20th Century; since the late 1950s unbiased estimates of steric changes are also available, indicating a contribution to the sea-level rise comparable to GIA (Frederikse et al., 2020).”

**L885: “would be reliable only in the basin mean tendencies”: where does this conclusion come from? Same from the next sentences. A citation or an experiment to prove these statements is needed.**

REPLY: We have changed the sentence as follows: Overall, even under accurate representation of global steric and mass addition from the Atlantic, projections of Mediterranean sea-level change from current regional ocean models would hardly provide reliable local sea-level tendencies for Venice and the Northern Adriatic.”

**L888: “Improved assessment and progress is hoped in this direction as well”. Hope is the mother of disappointment. Suggest to remove this.**

REPLY: Agreed, sentence removed.

**L889: Good et al. (2013) and Ishii et al. 2017 are not about new observations, but they’re about optimal interpolations of in-situ temperature and salinity profiles.**

REPLY: We have changed “observations” with “datasets”.

**L896ff: Circling back to the first round of review. What would be the outcome of ‘further research’ on estimating a trend in a sea level record? I’d say that ‘The shape of the local sea level trend’ is just the first derivative of the time series of local sea level. You can decompose that time series into contributions from various processes (wind, subsidence, ice melt etc.), but I can’t see what approaches like SSH or EMD can**

***add. While these methods often create more confusion than that they solve, see for example <https://npg.copernicus.org/articles/22/157/2015/>.***

REPLY: We agree. Still, as we state at the beginning of the paragraph, this is aimed at solving a practical question related to data preprocessing performed in many studies where changes in the mean need to be removed before the main analysis. We have rephrased the sentence in lines 896-899 as follows: “As far as the higher rates of RSL rise observed in recent decades are concerned, the simple acceleration expressed statistically in terms of quadratic fitting seems therefore to be insufficient and further methods could be explored (but as a note of caution see, e.g., Chambers, 2015).”

#### References

Chambers, D.P. (2015). Evaluation of empirical mode decomposition for quantifying multi-decadal variations and acceleration in sea level records. *Nonlin. Processes Geophys.*, 22, 157–166, 2015, <https://doi.org/10.5194/npg-22-157-2015>