Reviewer 2

We thank Reviewer 2 for their appreciation of our manuscript and for the useful comments and suggestions. In the following, we respond to the general as well as specific comments by the Reviewer, which are reported in italics (our response in normal font). We also describe how we would implement the associated changes in the manuscript during the revision.

Review of "Sea-level rise in Venice: historic and future trends"

This paper is a review paper about sea-level changes around the city of Venice, Italy. It discusses the observed changes, its relation to land motion and atmospheric forcing, and it provides projections of future sea-level changes for various scenarios. I enjoyed reading the parts on sea-level observations and land movements. These sections give a clear overview on the subject and I think that these sections are an important contribution to the existing literature. Especially the derivation of long-term land motion, its connection to shorter records from GPS and inSAR, and its temporal variability are very insightful and can function as a blueprint for similar studies in other regions.

However, in my opinion, the sections on sea-level variations, atmospheric forcing and ocean dynamics are sub-par compared to the other sections and need a lot of work before they are in a publishable state. These sections introduce a lot of different processes, but it feels like the coherence between these processes is missing. Also, the mutual consistency between the studies is not discussed (notably missing around L589). At the end, I wonder what is the relative importance of each process. Also, a lot of processes are introduced, but the physics needed to understand how these processes has been left out, while insight in these physics is necessary to understand the role and spatial coherence of these processes. That leads to odd situations, such as with the NAO, which is listed as one of the main processes, but which is not connected to wind and pressure fluctuations, while in reality, these processes are intrinsically linked. Also, there's a disconnect between the processes discussed in Section 5 and the projections in Section 6.

In retrospective, we agree that sections 5 and 6 can be substantially improved. The change to be implemented to both sections during the revision would include their general restructuring. We would better emphasize consistencies and inconsistencies between published results, and better link statistical connections with associated physical mechanisms.

For the specific comment on the results illustrated around line 589 (Scarascia and Lionello, 2013) and their apparent inconsistency with those illustrated around line 500 (Marcos and Tsimplis, 2008), we plan to present both studies together within section 4.3 and clarify the reasons for the different conclusions they reach. Specifically, Marcos and Tsimplis (2008) computes the expansion of the water column missing the contribution of the redistribution of mass, while the approach of Scarascia and Lionello (2013) accounts for it. Further, Scarascia and Lionello (2013) consider explicitly the Adriatic Sea, which is not present in the maps of Marcos and Tsimplis (2008), whose approach produces negligible values of sea level rise in shallow water areas.

Regarding the connection between NAO and Venetian sea level, we would restructure Section 5 to have one specific subsection on the NAO, after the physical processes have been introduced. The new structure of Section 5 would be:

5. Climatic drivers of Venetian sea-level fluctuations

5.1 Lateral boundary forcing at the Strait of Gibraltar

5.2 Air-sea interaction within the Mediterranean basin

5.3 Linkage with the NAO and other teleconnection patterns

The role of IBE and wind setup for the NAO connection was already mentioned in the original manuscript (lines 531-537). To better support this connection, in addition to changes in the structure of the section we plan to add a figure showing surface wind and sea-level pressure anomalies over the Euro-Mediterranean region linked to Venetian sea level variations as well as with different phases of the NAO.

Regarding the connection between processes discussed in section 5 and their relevance for section 6, in the revised version we would include an introductory paragraph and enhance references to section 5 in section 6 as far as possible, making sure that comparable relevance is given to each process in section 5 and 6.

A possible starting point to re-structure and clarify these sections could be to first discuss basin-wide fluctuations in the Mediterranean Sea at various time scales and how they are linked to sea-level variations in the North Atlantic Ocean. These changes and their linkage to the Atlantic Ocean are for example discussed in Fukumori et al (2007), Calafat et al. (2012), Volkov et al. (2019), Landerer & Volkov (2013). As far as I'm aware, almost all decadal and multi-decadal dynamic sea-level variability, as well as longer-term trends in Venice can be explained by basin-wide fluctuations in the Mediterranean, which are in turn linked to alongshore wind forcing along the East coast of the North Atlantic. With such a link established, it will be much easier to couple this knowledge to projections of sterodynamic sea-level changes from CMIP-style climate models. The second step then would be to determine which processes cause significant sea level anomalies in Venice relative to Mediterranean-averaged changes, for example due to local atmospheric forcing or local sterodynamic effects.

Thanks for the suggestion and for pointing at these papers, which we would include in the revised manuscript. As mentioned above, we plan to substantially restructure section 5 following the Reviewer's recommendation.

Some papers that could be useful as examples for a more structured description of dynamic sea-level variations on various temporal and spatial scales are Calafat et al. 2012 who explicitly shows for each process the amount of explained variability, or Wahl et al. 2013, Dangendorf et al. 2013, 2014, Frederikse et al. 2016, 2019 for papers investigating the dynamics that affect sea-level variability in the North Sea. Another recent example is Piecuch et al. (2019). Therefore, I recommend to thoroughly revise and rewrite the sections on atmosphere and ocean dynamics before the paper can be published.

Thanks for pointing at these papers, which we looked at carefully and got inspiration for the revision of our manuscript. We are confident that a revised manuscript with a substantially restructured and rewritten section 5 as outlined in this response would be suitable for publication.

Another possible way forward could be to just limit this review to the sections on vertical land motion and subsidence, which on their own would already be a very useful contribution to the literature. I have brought up a lot of points for sections 4, 5, and 6, but nevertheless, I hope the authors find them useful, despite their sheer number.

We believe that sections 4, 5 and 6 are a necessary part of this literature review. We are confident that the changes we plan to implement in the revised manuscript and illustrated in this response will convince the Reviewer that the paper is worth being published with all its components.

Detailed and line-by-line comments:

At first, I'd recommend to double-check the definitions from Gregory et al. (2019) throughout the paper. For example, there's an awkward separation between MSL and RSL throughout the paper. I'd recommend getting rid of MSL altogether to avoid any ambiguity and use RSL for sea-level changes relative to land (tide gauge observations) and use GSL (geocentric sea level) for sea-level changes observed by altimetry and tide gauges corrected for vertical land motion. Also please check the definitions of the inverse barometer effect. That effect only includes the static response to atmospheric pressure variations (1 HPa of pressure drop causes a 10 mm sea-level rise), but does not fully represent the sea-level response to pressure changes (Wunsch & Stammer, 1997).

The subsidence correction of the RSL series in Zerbini et al. (2017) was mainly based on time series of benchmark heights, obtained during levelling surveys, and are referred to the Zero of the Italian altimetric network in Genoa. Therefore, despite the Altimetric Zero in Genoa being stable (hence at a rather constant geocentric height), the corrected sea level is not strictly speaking geocentric. We would therefore avoid using GSL to refer to VLM-corrected RSL. We propose to update the nomenclature regarding sea level and use the following acronyms:

- Relative Sea Level (RSL) change: change in local sea level relative to the local solid surface (Gregory et al., 2019);
- Geocentric Sea Level (GSL) change: change in local sea level with respect to a geocentric reference, namely a Terrestrial Reference Frame or, equivalently, a reference ellipsoid (Gregory et al., 2019). The acronym GSL is therefore used for satellite altimetry sea-level data;
- Subsidence: land surface sinking (UNESCO, 2020; see also: Gregory et al., 2019);
- VLM-corrected RSL: local sea level derived from tide-gauge RSL data corrected for vertical land movements;
- Global-mean sea level (GMSL): spatially averaged sea level over the World Ocean.

Also a point on the significance on numbers, but I admit this is a bit a matter of personal taste: in an expression like 1.230.13, the last numbers are not significant, so something like 1.20.1 avoids a false sense of accuracy.

We understand this comment to refer in particular to section 2, where indeed some reported values prospect a high accuracy (as also reported by Reviewer #1). We clarify that when reporting results from published papers, we kept the original number provided by the authors. In the revised manuscript we would amend the text so that it is clear that this is the case. Also, the numbers provided by our own calculations will be carefully checked in that they provide a true sense of accuracy.

Finally, I encourage the authors to deposit all scripts and data resulting from this paper in a public repository, such as Zenodo, Github, or Figshare.

We agree to publish relevant data and scripts in a public repository, or even as a supplement to the paper upon final publication.

Abstract

L39 "An unresolved issue": do you just want to say here that altimetry doesn't measure sea level in the Venice Lagoon because there's no track overlapping ground track?

The issue is the contrast between tide gauge and satellite altimetry data. As far as the Venice Lagoon is concerned, the issue is twofold. First, the altimeter may not cross the lagoon depending on the ground track configuration design. TOPEX/Poseidon and Jason series do not cross the lagoon. Sentinel-2b instead crosses the lagoon, so data could be recovered if we improve the processing. As detailed in the main text, the Venice Lagoon is a challenging target due to several factors, among which are presence of land and specular reflections, and efforts to retrieve data are ongoing. To avoid misinterpretation or confusion, in the revised manuscript we would change the sentence in the abstract as: "An unresolved issue is the contrast between the observational capacity of tide gauges and satellite altimetry, with the latter tool not providing reliable data within the Venice Lagoon yet. It is therefore currently not possible to take advantage of the full potential of along-track altimetric data in the inter-technique comparison."

L40 Water mass exchange: I think this gives the false impression that the issue lies within understanding what's going on at Gibraltar. The real issue here is what's happening to the Northeastern Atlantic Ocean, which directly drives this water mass exchange, see for example (Volkov et al, Calafat et al.).

In fact, to better estimate Mediterranean sea- level variations under future global warming scenarios it is important to better understand and simulate both, the water mass exchange within the Strait of Gibraltar and precursors of its variability. To better stress this in the abstract, in the revised manuscript we would change the sentence as follows: "Water mass exchange through the Strait of Gibraltar and its drivers currently constitute a source of substantial uncertainty for estimating future deviations of the Mediterranean mean sea-level trend from the global-mean value."

L42: Subsidence and regional: : : and beyond. These sentences are valid for each and every coastal location, so it's rather trivial. In fact, there's no single known process that causes a true spatially homogeneous sea-level rise. What would be useful here is to explain which processes will cause large deviations from GMSL on various time scales.

We agree this is a rather general sentence. We would drop this in the revised version.

L45: "non-negligible differential trends": this is strange wording.

We agree. We would skip the quoted sentence.

Section 1

L52: Remove 'critically'

Agreed

L65: As said above, please avoid the term 'MSL' here and throughout the manuscript, and just distinguish between GSL (not ASL, Gregory et al. 2019) and RSL.

Agreed

Section 2

L129: The altimetry era actually started one year earlier with ERS1 (and even before that with SEASAT).

We agree that the United States were the first to fly a satellite-borne altimeter, with the Skylab and Geos3 missions, Seasat in 1978 (the first satellite to provide data) and Geosat in 1985. Seasat had only 110-day lifetime (end of mission due to a malfunction). Topex/Poseidn was launched in August 1992. Ers-1 was launched in 1991 but the revisiting was initially 3 days and not exploitable for sea-level studies the ground track coverage is too coarse. The 35-day phase started in 1992. The continuous altimeter era is considered starting on1992. We would avoid reference to the beginning of the satellite era and modify the sentence as follows in the revised manuscript: "Since the first satellite altimetry missions in the mid-1970s, the accuracy of sea-surface height measurement has increased considerably until high-precision and routinely measured altimetric data were made available in the early 1990s with the launch of the TOPEX/Poseidon mission"

L140. One question that is hanging over this section is, to which extent will sea level variations in Venice deviate from sea level a few km off the coast? My guess would be that that effect is rather minimal, except for very short temporal scales (like tides, waves and surges). This is especially relevant, as a few lines further down the paper, altimetry is directly validated with tide-gauge data, under the assumption that both would measure the same sea-level variability. The answer to this question also circles back to the open challenge described in the abstract, about the challenge of getting altimetry observations as close as possible to the coastline. If these variations are about the same as variations a few km's offshore, why worry about getting closer to the coast?

We agree that sea level in the Northern Adriatic is rather homogeneous. The non-trivial point is to determine how sea level signals in the Northern Adriatic propagate into and then within the Venice Lagoon, for which satellite altimetry data are not available yet. As detailed later in section 4.2, the deviation between sea level in Venice measured by tide gauges and in the open sea in the vicinity to the lagoon measured by satellite altimetry provide estimates of interdecadal sea-level trends that do overlap indeed within uncertainties, but they do so only marginally after the tide-gauge data are corrected for vertical land movements, for which the estimate is about half that from satellite data. This motivates the open issue pointed out in the abstract. In the revised manuscript, we would take advantage of a newly published paper (De Biasio et al., 2020) to better assert that the best approach to obtain robust estimates is not to assess each site independently from the others, rather to use multiple sites and exploit synergies between available measuring systems (namely altimetry and tide gauges).

In the revised manuscript we plan to add a sentence like the following one at the end of the last paragraph of section 4.2: "So, despite satellite- and tide gauge-based trend estimates still overlap within their uncertainties after tide-gauge data have been corrected for subsidence, they do so only marginally. The underlying causes remain to be understood, which motivates the search for improved approaches to integrate both measuring systems (De Biasio et al., 2020)."

Section 3

L197: For non-paleo readers, please add an approximate date of MIS 5.5

Agreed, in the revised manuscript we would specify "subsidence using the Marine Isotope Stage (MIS) 5.5 event between 130 and 120 kyr ago as a reference to separate geologically older and newer RSL changes"

L269: Your current definition of GIA encompasses both the response to past and contemporary ice mass changes. Following Gregory et al. 2019, it might be a better idea to use GIA for the response to past ice changes, and use 'contemporary GRD effects' for the local sea-level response to contemporary ice-mass changes.

We agree that there could be some confusion. We propose to differentiate the terminology for past and contemporary ice-mass changes by using "GIA" and "GIA including contemporary ice mass changes", respectively, since the two phenomena are governed by the same physics.

L365: Please define 'MOSE'.

We would add the definition of MOSE in the revised manuscript: "(so-called "MOdulo Sperimentale Elettromeccanico" or MOSE, see Lionello et al., 2020a)"

Paragraph 4.1

General: in this section, the authors try to determine the secular trend in sea level in Venice and compare this trend to the global mean. There are a few fundamental problems with this section:

1. What do the authors mean by 'secular trend'? Is there some linear background trend? And if so, which processes are meant to be represented by this trend? To my knowledge, except GIA and maybe some other long-term geologic processes, no single process could cause such a trend. Processes like ice mass loss and thermal expansion are far from linear over 100-year time scales. The idea that this trend has to do with climate change is reinforced by the 'climate component' label in Table 6. I also wonder whether the different numbers found in Table 5 are just caused by computing linear trends over different time spans. Given that sea-level changes often show a lot of multi-decadal variability, even small changes of the period over which the trends are computed can lead to differences in the range of the differences shown in Table 5. In line 400, the authors make a statement about the non-linearity due to subsidence. This argument is valid for most other processes as well.

Following the aim of this literature review, this section summarizes published estimates of the observed rate of sea-level rise in Venice, and highlights and attempts to explain differences between them. We certainly agree that such estimates should not imply that the underlying process is necessarily linear, despite estimates being often obtained from the application of linear statistical techniques. In the revised version of the manuscript we would change "secular trend" with "Average rates of sea-level rise over centennial periods" and would add some clarification in this regard, by adding a text along the following lines in the introductory part of section 4: " Average rates of sea-level rise are often calculated using some linear fit to the available data. Given the variety and non-linearity of the processes known to contribute to sea-level trends on the considered centennial and multidecadal time scales – some explicitly accounted for in the trend calculation -, such estimates should not be intended as necessarily representing a linear process (see Section 6)."

We also certainly agree that there is substantial multidecadal variability in the sea-level records, which can explain differences between estimates of the secular rate of sea level rise provided by different authors (e.g., the apparent discrepancy between the conclusions of Marcos and Tsimplis, 2008, and Scarascia and Lionello, 2013, quoted above). We would better state this in the discussion by adding something along the following lines: "The presence of substantial variations in Venetian RSL and VLM-corrected RSL multidecadal trends contributes to explain, together with methodological aspects in the calculation, the different estimates of the average rate of sea-level rise obtained by different authors considering different periods."

2. The comparison to GMSL. The authors compare the trend in Venice to GMSL. This is generally not a good comparison for multiple reasons: due to its proximity to the Greenland Ice Sheet and many glaciated regions, their contributions (which together explain more than half of observed GMSL 1900-2018, Frederikse et al. (2020)) to Venice RSL will be much smaller than to GMSL. On the other hand, the Atlantic Ocean is accumulating heat at a higher rate than the Pacific and Indian Oceans (e.g. Zanna et al. 2019), so the steric component won't follow the global mean either. Therefore, the fact that Venetian and global sea level show similar trends is merely a coincidence, and you cannot expect these numbers to be comparable. Thus, expecting consistency with GMSL doesn't make sense. In fact, it may lead to the false assumption that future sea level rise in Venice will be comparable to GMSL rise. Since various contributors to GMSL rise vary over time as well, this coincidence may not hold. The consistency noted in line 436-437 reinforces this issue: the contributions from Greenland and glaciers in the second half of the 20th century are much smaller than for the first half of the 20th century. Therefore, they do not explain the deviation to be especially large in the second half of the 20th century. In contrary: if it were due to the aforementioned ice melt, one would expect a large difference around the 1930s and a smaller one over the second half of the 20th century.

We agree with this comment. In fact, we did not expect consistency to be necessary between GMSL and sea-level variations in Venice. Our 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 Carbogning et al., 2010). Possibly, this is better explained in the accompanying editorial to the special issue (Lionello et al., 2020, discussion paper available at: https://nhess.copernicus.org/preprints/nhess-2020-367/), where Venetian sea level is more appropriately compared to the midlatitude eastern North Atlantic sea level.

We would still keep the comparison between GMSL and Venetian sea level trends in the revised manuscript, but would add an introductory sentence to explain the rationale of the comparison along the following lines: "As illustrated in Sections 5 and 6, Mediterranean sea level, hence Venetian sea-level variations are tightly connected to sea-level variations in the midlatitude eastern North Atlantic, whose underlying processes differ from those in other oceanic basins. Therefore, any statistical consistency between historical Venetian RSL/VLM-corrected RSL and GMSL rise should not give the false impression that both variables are interchangeable and that any consistency in the historical period necessarily holds in the future. Still, it is instructive to compare estimates of Venetian RSL/VLM-corrected RSL and GMSL rise during the 20th Century."

We would then refer again to the comparison in the restructured Section 5 and provide an explanation along the one provided by the Reviewer in this comment.

Moreover, we would add a sentence clarifying the implications of associating trends in GMSL and Venetian sea level in the revised abstract, along the one that follows: "Even if consistent with each other, Venetian and global-mean sea-level trends are caused by a different combination of processes, whose individual contribution varies through time, hence future projections of Venetian sea-level rise should not build on global-mean estimates."

Line 405: What are 'secular tide-gauge records'? Also, the stations listed here might not be affected by large local subsidence the way Venice is, but I don't think they're unaffected by GIA-induced VLM. Most of these stations have some GNSS records available as well, which can be used to assess whether these stations don't show any VLM.

By "secular tide-gauge records" we meant records spanning over one century or more. The precise time spans covered by each time series are indicated in the text (at lines 400 for Venice and 404 for M. di Ravenna). The wording would be revised by replacing "secular" with formulations like "centennial" or "spanning over a century".

The stability of the tide gauges included in this discussion is provided in the references reported at line 408 of the original manuscript. These include (also) GPS-based results, see in particular Sanchez et al., 2018 (their Figures 8 and 14).

Line 429. The trends denoted in the IPCC report and Hay and al. refer to global-mean relative sea level, and not global-mean geocentric sea level (See Gregory et al. 2019), although the difference between both is probably not very large. However, on local scale, the difference can be substantial, even when ignoring local VLM effects. See for example Lickley et al. 2018. Therefore, comparing local geocentric sea level to global-mean relative sea level is not fair.

We definitely agree that there might be substantial difference between local and global-mean sea-level estimates. Obviously, we also agree that geocentric and relative estimates of sea-level variations can differ, this is actually the rationale for the whole discussion provided in Sections 2 and 3. However, the following points should be considered:

1) The subsidence correction of RSL series in Zerbini et al. (2017) was mainly based on time series of benchmark heights, obtained during levelling surveys, and are referred to the Zero of the Italian altimetric network in Genoa. Therefore, despite the Altimetric Zero in Genoa is stable (hence at a rather constant geocentric height), the corrected sea level is not strictly speaking geocentric;
2) In fact, the focus of Zerbini et al. (2017) is regional rather than local as six tide-gauges characterized by centennial time series along the coasts of the Mediterranean Sea were considered;
3) hopefully, the impact of VLM on GMSL from tide-gauge records is limited. Over the altimetric period, in fact, the GMSL rise derived from tide-gauge records is consistent with altimetric measurements (e.g. IPCC 5th report, Chapt. 13, Fig. 13.7).

In the revised manuscript, we plan to modify the text as follows: "[...] full-period trends in both the original RSL (2.53 +/- 0.14 mm/year) and the VLM-corrected RSL (1.23 +/- 0.13 mm/year)"

Moreover, for clarity, on line 389 we would replace 'relative MSL' with 'RSL'.

Overall, in the revised manuscript we would change the last paragraph of section 4.1 along the line of the following paragraph:

“As illustrated in Sections 5 and 6, Mediterranean sea level, hence Venetian sea-level variations are tightly connected to sea-level variations in the midlatitude eastern North Atlantic, whose underlying processes differ from those in other oceanic basins. Therefore, any statistical consistency between historical Venetian RSL/VLM-corrected RSL and GMSL rise should not give the false impression that both variables are interchangeable and that any consistency in the historical period necessarily holds in the future. Still, it is instructive to compare estimates of Venetian RSL/VLM-corrected RSL and GMSL rise during the 20th Century. Venetian sea-level trends are smaller than GMSL trends reported in the fifth assessment report of the Intergovernmental Panel on Climate Change (IPCC-AR5), quantified as 1.7 [1.5 to 1.9] mm/year (likelihood >90%, period from 1901 to 2010, see: Church et al., 2013). They are, however, consistent with revisited estimates of historical GMSL rise that include significantly slower rates than reported by the IPCC-AR5 for the pre-altimetry period, e.g., 1.2±0.2 mm/year (90% confidence interval, Hay et al., 2015), 1.1 ± 0.3 mm/year (99% confidence interval, Dangendorf et al., 2017) and 1.56 ± 0.33 mm/year (90% confidence interval, Frederikse et al., 2020). Figure 6 revisits the connection between Venetian RSL/VLM-corrected RSL and GMSL trends on time scales ranging from interannual to centennial. Clearly, the significant difference between centennial trends in Venetian RSL and GMSL is strongly damped when the contribution of subsidence is removed, confirming the critical role of vertical land motions in determining local RSL variations. Nonetheless, the Venetian VLM-corrected RSL appears to rise at a lower rate than the GMSL over the second half of the 20th century (Fig. 6a). Note that the GMSL-Venetian sea-level discrepancy observed in the first portion of the record is resolved when uncertainty in GMSL estimate is considered (not shown)”

Paragraph 4.2:

‘Multidecadal trends’: what do the authors refer to when talking about ‘multidecadal trends’? Just ‘linear trends over 1993-present’?

Similar to what we plan to do for paragraph 4.1 we would change the title to “Rates of sea-level rise during the satellite altimetry era”.

L445: Sorry, but I really disagree on this. One can fill whole bookshelves with papers looking into regional sea level from altimetry.

In the revised manuscript we would rephrase the sentence as: “An overall GMSL trend of about 3 mm/year during the satellite altimetry period is consistently reported by several studies (Hay et al., 2015; Chen et al., 2016; Dangendorf et al., 2017; Quartly et al., 2017). Regional trends can deviate considerably from the global mean (e.g., Scharroo et al., 445 2013; Legeais et al., 2018; Cazenave et al., 2019).”

L464: Altimetry also observed the seasonal cycle in sea level. So, when comparing both, why do you need to correct for the seasonal cycle in tide-gauge observations?

In fact the removal of the seasonal signal is just a common procedure. We would rephrase the statement to clarify this in the revised manuscript, also citing the reference paper by Carrère and Lyard (2003).

Ref: Carrère L., Lyard F (2003) : Modeling the barotropic response of the global ocean to atmospheric wind and pressure forcing – comparisons with observations. Geophys Res Lett 30(6):1275.doi:10.1029/2002GL016473

Paragraph 4.3 In general, this section seems to lack focus. It's a mixture between variabilities of seasonal sea level, some remarks about peaks in the sea-level spectrum, a wavelet analysis, and reference to some correlation with sunspot cycles. I'm guessing here, but it might be the case that the authors have mixed up 'periodicity', oscillations that occur with a fixed frequency (such as the seasonal cycle or the M2 tide) versus 'low-frequency variability', variability that occurs on some typical time scales, but cannot be described as (a sum of) periodic functions, such as ENSO or the NAO. The former will cause a clear peak in the spectrum, while the latter is more associated with the behavior discussed in this section, such as intermittent signals in wavelet analyses, and correlations that vary over time. This low-frequency variability is common in global and local sea-level observations. Peaks will show up when computing a spectrum from tide-gauge data, but I wonder about the significance of any of these peaks. Beyond the conclusion that sea level in Venice shows decadal and multi-decadal variability, what should we distill from these peaks? How did the authors determine the significance of the peaks relative to a signal with a red spectrum? An alternative approach here could be to determine which processes act on with time scales.

As correctly stated by the Reviewer, in this case the term "periodicity" was intended in the sense of a spectral peak at a certain frequency. To avoid confusion, in the revised version we would replace the term "periodicity" with "variability mode and significant spectral component". For instance, we would rephrase the sentence at lines 477-478 as follows: "Hereafter, we indicate detection of a statistically significant spectral component around a period of XX years with $O_{XXyears}$, where O means order of magnitude."

We remark that, in the spirit of a literature review, we report the presence of significant spectral components detected by previous studies in the tide-gauge record of Venice. We provided the analysis of the updated seasonal tidal records for autumn and winter in order to update published results and to confirm/revise their conclusions. Similarly, we illustrate possible linkages with large-scale atmospheric and oceanic phenomena as they are discussed in published papers. An unambiguous association of each variability mode with a well-defined process has not been done yet, and it is out of the scope of this review. In fact, we would better highlight in the revised manuscript that variations in the phase lag and intermittent significance in the wavelet coherence spectra indicate that caution should be taken when establishing connections between local sea-level variability and large-scale climatic modes, as they may not be robust over multidecadal or centennial time scales.

Concerning the determination of significance of the spectral peaks, the spectral peaks were tested against a red-noise (lag-1 autoregressive model) null-hypothesis. A lag-1 autoregressive model characterized by high power spectral density at lower frequencies. It is the most widely used model for geophysical purposes since a large class of geophysical processes produce output statistically compatible with red noise hypothesis (Allen and Smith 1996 and references therein). The theoretical power spectrum of a red-noise process is known and the parameters of the model are deduced from the analysed record. The power spectrum of the tide-gauge data was compared with that of the null-hypothesis, at a certain confidence level, and those peaks with power higher than that expected from the null-hypothesis have been defined as significant. Details are provided in chapter 10 of Alessio (2016).

Ref: Allen M, Smith L. 1996. Monte Carlo SSA: Detecting irregular oscillations in the presence of colored noise. Journal of Climate, 9(12): 3373–3404

Ref: Alessio SM. 2016. Digital signal processing and spectral analysis for scientists: concepts and applications. Springer.

L490: The sunspot cycle. This seems a far-fetched link to me. How does the 11- year sunspot cycle cause significant local sea-level variations? I guess that this link is merely coincidence.

In the spirit of a literature review, in the original manuscript we opted for including as much details as possible from the references quoted in the sentence. In the revised manuscript we would omit the link to 11-year solar variations (which is anyway dealt with in detail in the companion paper Lionello et al., 2020b) and only refer to the presence of significant decadal variability in the record of Venetian storm surges. The revised sentence would read as follows: "Lionello (2005), Barriopedro et al. (2010), Troccoli et al. (2012) and Martínez-Asensio et al. (2016) consistently identify significant decadal variability in the time series of autumn Venetian surge events for the period 1948-2008 (for an updated analysis see Lionello et al., 2020b, in this special issue)."

L494: What is a 'statistically significant fluctuation'? Significant with respect to what? White noise? A linear trend? Why is a wavelet analysis a good tool to study this?

Fluctuation refers to the amplitude of spectral components. Significance of observed spectral peaks was tested against a red-noise hypothesis, namely a lag-1 autoregressive model characterized by high power spectral density at lower frequencies. It is the most widely used model for geophysical purposes since a large class of geophysical processes produce output statistically compatible with the red noise hypothesis (Allen and Smith 1996 and references therein). According to Grinsted et al. (2004) the significance is computed through the distribution of an ensemble of surrogate series describing a red noise process with the lag-1 parameter and variance estimated from the analysed time series.

Wavelet transform is commonly applied since it is an evolutionary spectral method which allows to examine the spectral evolution of the analysed record in a time-frequency(period) domain and to find localized intermittent periodicities (Grinsted et al., 2004). Thanks to the multivariate version of this method, namely the Wavelet Coherence, also the coherence between two time series, at certain frequency bands, can be detected as done in this study.

For the sake of clarity and completeness, we would add the first paragraph of this response in the revised manuscript and modify the caption of figure 8 as follows:

"Shading (thick black contour) is the portion of the spectrum exceeding 90% (95%) confidence against red noise (lag-1 autoregressive model) hypothesis (see Grinsted et al., 2004, for details)"

Ref: Allen M, Smith L. 1996. Monte Carlo SSA: Detecting irregular oscillations in the presence of colored noise. Journal of Climate, 9(12): 3373–3404

L500: This stationarity here has been attributed to Greenland and glaciers above, and here it is due to atmospheric processes. What is it?

We explain the apparent discrepancy between the trend estimates and the conclusions obtained by Marcos and Tsimplis (2008) and Scarascia and Lionello (2013) partly as a result of the different focus region, partly to different period considered in the two studies and of the strong interdecadal variability observed in sea-level records in the Adriatic and Mediterranean seas, and partly due to the different consideration of the processes contributing to sea-level variability (see our response to the general comment above). It is among the conclusions of our review that there substantial differences in the rate of sea-level change in Venice can be obtained if the analysis is conducted on periods of a few (say up to 5) decades. It is true that different trends in different periods can be determined by different mechanisms being active or anyway predominant. We are convinced that this will be clear in the revised manuscript, not only in reference to different periods, but also as far as the relation between Venetian sea level and the GMSL is concerned, as outlined in our responses above to the specific comments by the Reviewer in this regard.

L505: What is "the integral of the absolute trend differences for bidecadal and shorter periods"?

Thanks for pointing at this convoluted sentence. We referred to the sea-level difference, in absolute values, obtained by integrating (or summing up) through time the linear trend estimates for the GMSL and Venetian sea level over the given period of time, subtracting them for each other, and keeping the absolute value of such difference. It is meant to quantify the Venetian sea-level anomaly associated with the local linear trend with respect to the GMSL trend. We would revise the sentence as follows: "Accordingly, the linear trend can yield a local sea-level anomaly in Venice from the GMSL of about 10 cm (but up to about 20 cm occasionally) over bidecadal and shorter periods, and a rather small anomaly (generally <5 cm) over interdecadal and longer periods (not shown)."

Sections 5.1 and 5.2

General: like section 4.3, these sections lack focus, tend to introduce a lot of different processes, but at the end, I still don't understand the relative importance of each process. Furthermore, a general introduction of the physics behind the processes is necessary here to understand what's going on. For example, something like: "The North Atlantic Oscillation causes large-scale atmospheric pressure variations on interannual scales. The geostrophic winds caused by these variations drive a barotropic sea-level response in Venice, especially in winter."

In the revised version we would improve the description of physical mechanisms involved in sea-level variability, in a widely restructured Section 5. For the NAO, we plan to have a dedicated subsection, which will benefit from some introductory statements along the one proposed by the Reviewer. Please see our response to the more general comments by the Reviewer above, our response to the specific comments on the NAO below as well as our responses to the comments by Reviewer #1.

L511: Steric effects. What about steric effects within the Mediterranean? Do they play an important role? They are discussed in the projections section, but what about observed changes? One could estimate the size of this effect over the last few decades from gridded hydrographic observations, such as EN4 (Good et al. 2013) or Ishii et al. (2017).

We plan to better illustrate and discuss steric effects on Mediterranean sea-level change based on available literature (for instance Jordà and Gomis, 2013; Carillo et al., 2012; Scarascia and Lionello, 2012), also taking advantage of the substantial restructuring of Sections 5 and 6 in the revised manuscript.

Concerning the additional analyses suggested by the Reviewer, whereas we provide updated estimates for some components to sea-level variations, it is beyond the scope of this literature review paper to estimate the size of all individual effects to Mediterranean sea-level variability. We acknowledge that the availability of updated oceanic datasets as those reported by the Reviewer can be valuable for better constraining the different contributions to sea-level variability within the Mediterranean Sea, and we plan to discuss this opportunity in Section 7.

L520: What do the authors want to say in this paragraph?

This part would be removed in the revised manuscript.

L531: Geostrophic wind is something different than large-scale wind (https://en.wikipedia.org/wiki/Geostrophic_wind)

We certainly agree. The sentence would be removed in the revised manuscript as it was confusing.

L534: "Mass exchange can dominate: : :": Isn't NAO forcing just a local barotropic response to wind forcing and as such, just added on top of basin-wide fluctuations?

In fact, the NAO has been associated with Mediterranean sea-level variability both as large-scale driver of winds over the Strait of Gibraltar, hence influencing water-mass exchange between Atlantic Ocean and Mediterranean Sea, and therefore basin-mean sea-level fluctuations in the Mediterranean, as well as driver of local wind anomalies within the Mediterranean, thereby contributing to spatial heterogeneity in sea-level variations within the basin. We plan to restructure the section and have a subsection devoted to the NAO-Venetian and Mediterranean sea-level connection, possibly with the addition of a figure illustrating changes in the pressure and wind fields over the Mediterranean region associated with different NAO phases to support the text.

L539: What is "explained linearly"? L540: They: : :EAWR. This is vague. What happens under the hood here?

The sentence would be removed from the revised manuscript

L547 This link is also discussed above, but here, the notion that is link is not very strong is omitted. I'd just leave the sunspot studies out.

Agreed.

L553. I don't see this strong correlation from the wavelet coherence plot. The correlation looks weak to me: it does not hold throughput time, goes in and out of phase. From this result, I'd make the opposite conclusions, namely that there's only a weak correlation between winter sea level and the NAO. In for example Piecuch et al. (2019), a wavelet coherence plot that looks similar to this one (their Figure 1) is used to argue for a weak correlation.

We overall agree that the NAO-Venetian sea level connection does not emerge as clearly for both seasons as just briefly discussed in the quoted lines of the original manuscript. In fact the lack of a robust connection with the NAO especially in autumn agrees with Zanchettin et al. (2009), where other climatic modes such as the SCA and the EAWR appeared to be predominant. We would cite

Piecuch et al. (2019) and Zanchettin et al. (2016) to put caveats on the interpretation of coherences with variable phase through time. We would also discuss the results in the light of a new figure showing atmospheric circulation anomalies over the Euro-Mediterranean region around different NAO phases.

Ref: Zanchettin, D., Bothe, O., Rubino, A., and Jungclaus,J. H.: Multi-model ensemble analysis of Pacific and Atlantic SST variability in unperturbed climate simulations. Clim. Dyn., 47(3),1073-1090, doi:10.1007/s00382-015-2889-2, 2016.

L555 In autumn: : : how significant is this statement? Given the difference between interannual NAO variability and multidecadal subsidence variability, I doubt whether you're just looking for an explanation of insignificant changes.

As explained in the response to the previous comment, we overall agree with the Reviewer. We would therefore restructure the section substantially in the revised manuscript, also including an additional figure to illustrate the connection between NAO and Venetian sea level in autumn and winter. Our revised conclusion is that the NAO has a significant impact in winter whereas it exerts a less clear influence in autumn. In both autumn and winter, the patterns of sea-level pressure and wind anomalies under different NAO states superpose well on those that correspond to variations in Venetian sea level, including large-scale low pressures enhanced over the northern Mediterranean Sea and Sirocco-like northeastward wind anomalies over the Ionian Sea. However, amplitude and spatial extent of the significant anomalies in the atmospheric forcing fields are larger in winter compared to autumn, confirming the weak imprint of the NAO on Venetian sea level in the latter season.

L558. The contribution of the IBE effect to sea level has been quantified way before 2009 as 1cm of sea-level rise to 1 mbar of pressure drop (see Wunsch & Stammer, 1997). Using regression, you might find other correlation coefficients between sea-level pressure and sea level, but that's because sea-level pressure and sea level interact in many more ways than just the inverse barometer effect. See for example Woodworth et al. 2010. It may also be a good idea to repeat the conclusions from Calafat et al. (2012) that the IB effect only explains a marginal fraction of variability (see their Figure 4).

In the substantially restructured Section 5, we would briefly illustrate IBE with a paragraph along the one that follows: "Local atmospheric mechanical forcing is primarily exerted through local pressure anomalies, associated with the so-called Inverse Barometer Effect (IBE), and wind anomalies. The IBE is quantified by the hydrostatic equation in about 1 mm of sea-level rise per 1 mbar of sea-level pressure drop. Calafat et al. (2012) quantify in 25% the IBE contribution to decadal winter sea-level variability in Trieste for the period 1950-2009. The highest IBE contributions to seasonal Venetian sea-level variability over the period 1872-2003 in autumn (about 32%) and winter (41.5%) estimated by Zanchettin et al. (2009) can be explained by the regression model between local sea-level and local sea-level pressure which could embed also other contributions than IBE alone (e.g., Woodworth et al., 2010)."

L563-574: What is the exact point of this paragraph? Hard to follow.

This paragraph would be removed from the revised manuscript, with relevant parts of it embedded in different other paragraphs where appropriate.

L574ff: Please carefully re-read Calafat et al. 2012: they discuss alongshore coastally-trapped wave propagation along the Atlantic coast, affecting Mediterranean Sea level as a whole. They do not look into the Adriatic Sea in particular.

Indeed, thanks for catching this. The sentence would be amended in the revised manuscript.

L584-585: Accordingly -variability. This is a vague sentence. What is an atmospheric bridge in this context, and what does "constitute a potential precursor to multidecadal variability" mean?

The sentence refers to the possibility that the remote connection between multidecadal variability in Venetian sea-levels and North Atlantic climate is determined by atmospheric teleconnections, following the analysis between Euro-Mediterranean sea level pressure and the Atlantic Multidecadal Oscillation by Mariotti and Dell'Aquila (2012). Accordingly, atmospheric circulation would act as a bridge.

We plan to rephrase this in the revised manuscript within a restructured section 5, as follows: "In addition to the NAO, statistical connections identified in the literature between Venetian RSL and climatic modes include the atmospheric patterns known as Scandinavian and East Atlantic Western Russia (Zanchettin et al., 2009), showing prominent variability at interannual to decadal time scales in the autumn and winter seasons, and the Atlantic Multidecadal Oscillation or AMO (Scafetta (2014), describing multidecadal fluctuations in North Atlantic sea-surface temperature and influencing atmospheric variability over the Euro-Mediterranean region (e.g., Mariotti and Dell'Aquila, 2012; Maslova et al., 2017)."

L586: "This could contribute to explaining the statistical connection between bidecadal variability of Venetian RSL". Or it could not. This statement needs some evidence, as it now looks like guesswork.

We agree and our sentence was not meant to provide any conclusive evidence, rather point at a possibility. We plan to rephrase this part as illustrated in our response to the preceding comment.

L588: What is "multi-scale acceleration analysis"?

MSAA is the technique used by Scafetta (2014). We would omit this detail in the revised manuscript as non relevant.

L589ff: This section contrasts with many of the cited papers above, or at least, it needs more explanation. It's namely very unlikely that ice melt can explain 1.3 mm/yr in the Mediterranean, due to the magnitude of past ice melt and GRD effects, causing the Mediterranean to be affected much less than the global mean. How about large-scale changes in the Atlantic Ocean that propagate into the Mediterranean?

Scarascia and Lionello (2013) explain interannual VLM-corrected RSL by the combination of steric effect and the mechanical action of the atmosphere. The authors observe that these factors have no net trend for the period 1940-2005 and therefore cannot be used for explaining the SL trends. In this respect, there is no contradiction with other studies that are cited in section 5, which mostly describe the same variability with different perspectives. The new structure of section 5 will better separate the different factors and clarify the lack of contradiction among studies. We agree that the word “remote” is confusing and it should be replaced with “external to the Mediterranan Sea”. We agree that the specific attribution to ice-cap melting is not supported by the cited study and it will be dropped in the revised version.

L603: Lots of terms that need explanation: “amplitude modulation of water transport”, “migration of the eastern hydraulic control”.

We would rephrase the first quoted text as “modulation of the water transport”.

Then, there are two hydraulic controls in Gibraltar. One is located over the Camarinal Sill, the other is a moving hydraulic control that basically is locked in-phase with the bore propagation within the Tarifa Narrow. These details are not necessary, therefore we would simplify the sentence as follows: “Local dynamics are strongly influenced by tides, which are responsible for the modulation of the water transport and the hydraulic control (Armi and Farmer, 1988), as well as for the substantial vertical mixing that has been observed (Garcìa-Lafuente et al., 2013).”

Section 5.2 In its current setup, this section reads like it has been written without a clear focus in mind. What message do the authors want to tell with this section? It now reads like an unconnected collection of papers that each describe an individual problem, but an overarching story is missing. What models are available, reanalyses, operational forecasts? A good starting point may be the model results from Fukumori et al. (2007).

We will substantially restructure section 5 during the revision, and merge the sections regarding the physical mechanisms underlying Venetian sea-level variability and numerical modelling. As detailed above, we plan to have an introductory part and then three separate subsections dealing with the most important aspects of climate forcing of Mediterranean and Venetian sea level, which will embed any relevant aspect regarding associated numerical modelling issues.

Section 6 Similar to the previous sections, this section is also somewhat unorganized and lacks focus. After reading it, I am unable to determine a conclusion from the section. Why don’t the authors just use the SROCC projections? They should contain all the processes discussed here, except for the vertical land motion part. There is also a serious lack of information on the methods, and where methods are named, it feels like a bit of a grab-bag of individual estimates. I see SRES scenarios, AR5, SROCC, local projections... Are they combined in a consistent way? Where does the atmospheric forcing come from? Therefore, please thoroughly revise this section with a consistent treatment of scenarios and processes, together with a clear methods section.

The feeling that this section - as others in this manuscript - is a collection of estimates from different authors, based on different data and methods, stems from the fact that this is meant to be a literature review. Our aim is to collect all relevant information and try to make sense of the differences in the conclusions reached in the various studies. We would carefully revise section 6 concerning content, structure and presentation, as outlined by our responses to the specific comments below. In particular, we would have a closer and clearer focus on projections for the local sea-level change in Venice, also taking advantage of the references suggested below by the Reviewer.

We will take extreme care in detailing the different contributions to Venetian relative sea-level rise and how they are combined in our estimate. For the mentioned atmospheric forcing, in particular, we rely on published estimates indicating that associated sea-level variations are generally less than but up to about 10 cm (as stated in line 677-679 of the original manuscript). As there is no quantitative precise estimate for this contribution for the case of Venice or the Northern Adriatic, we would amend the text highlighting that further research is required in this regard and avoid including this specific contribution to our total estimate.

L728: This statement is not true. There's no single process that causes a uniform sealevel rise. Therefore, each single process, from ice mass loss to GIA and sterodynamic effects causes a local deviation from GMSL. You might reach the conclusion that the resulting local changes are close to the global mean changes, but that's something different.

We agree that, in its present form, the second part of this paragraph is confusing. An important conclusion of this review is that the Mediterraean mean sea level follows that of the midlatitude eastern North Atlantic on multidecadal time scales. However, changes over interdecadal periods can distort the detection of forced trends over rather long periods of time (e.g., Jordà, 2014). Further, regional atmospheric patterns can determine differences up to 10 cm between the RSL in the Mediterranean and in the midlatitude eastern North Atlantic and within different parts of the Mediterranean basin itself. Finally, RSL in Venice can differ as land movements (subsidence) could provide additional and important contributions at local scale. The text will be revised accordingly.

Section 7 L736: Similar as in the previous sections: except for VLM, how different are sea-level variations at the coast and just off the coasts as measured by altimetry? Or in other words? Why would we need altimetry closer to the coast?

Essentially, availability of altimetry data very close to the coast would be important for a more direct comparison with tide-gauge data. This would allow us to better understand why both tools provide different - though not mutually inconsistent - statistics, such as trends (see Table 4). As a further scientific motivation, altimetry is the only tool currently capable of measuring sea level from the open ocean to the coasts, hence allowing to assess whether coastal sea level is rising at the same rate as open-ocean sea level. An example is sea level near the coast of western Africa, where observed trends are significantly different than offshore (Marti et al., 2019). We would better clarify this in the revised manuscript and also better report the issue in the abstract as follows: "An unresolved issue is the contrast between the observational capacity of tide gauges and satellite altimetry, with the latter tool not providing reliable data within the Venice Lagoon yet. It is therefore currently not possible to take advantage of the full potential of along-track altimetric data in the inter-technique comparison."

Ref: Marti, F., Cazenave, A., Birol, F., Passaro, M., Léger, F., Niño, F., ... & Legeais, J. F. (2019). Altimetry-based sea level trends along the coasts of western Africa. Advances in Space Research, doi: 10.1016/j.asr.2019.05.033.

L795: The method to determine the trend and uncertainties critically depends on the purpose: what should the trend encompass? That should set the method you want to use. For example, neither method gives you a number that should be extrapolated into the past or future.

Here, we refer to the trend within its statistical definition. Following the International Statistical Institute (ISI, 2003), this would be the long-term movement in a time series, which may be regarded, together with the oscillation and random component, as generating the observed values. The issue, as stated in the manuscript, is meant to be exquisitely practical, as often in time series analysis the long-term trend is removed. We are convinced that some general guidance is needed in this regard, hence this part of our manuscript.

In the spirit of a literature review, we highlight the determination of the statistical trend as an open issue, and only provide two exemplary statistical models that could be used to calculate the trend. We do this for two time series - the raw RSL series and the VLM-corrected RSL series - to highlight differences and possible caveats due to the different considered processes.

We agree that neither trend should be extrapolated, and we see no reference to such extrapolation in our manuscript.

In the revised manuscript we would better clarify the aim of this paragraph, first of all by determining what is meant here for "trend" as outlined above and, then by adding a sentence like the following: "The presence of substantial variations in Venetian RSL and VLM-corrected RSL multidecadal trends contributes to explain, together with methodological aspects in the calculation, the different estimates of secular average rate of sea-level rise obtained by different authors considering different periods."

Ref: ISI (The International Statistical Institute): The Oxford Dictionary of Statistical Terms. Yadolah Dodge (ed.), Oxford University Press, Oxford, 506 pp, ISBN 0-19-850994-4, 2003.

L800: Similar to above: what does "the shape of the local RSL rise" encompass?

We would rephrase this as "the shape of the trend in sea level", hoping that the meaning is sufficiently clear in the light of the changes to be implemented in the preceding sentences, as outlined in the response to the previous comment.

L803: What is "energetic variability"?

We would change this to "significant variability"

L809: This is not the first attempt to create regional sea-level projections for Venice. They are for example in the AR5 and SROCC report, Kopp et al. 2014 and Slangen et al. 2012. All provide local RSL projections with uncertainties. In this respect, Kopp et al. (2014) should be discussed here, since it uses a statistical model to estimate and project land motion not related to GIA.

We acknowledge the fundamental contribution of the Slangen et al. (2012), AR5 and SROCC to study sea-level changes and the implications of sea-level rise for low-lying islands, coasts and communities. Despite there is no explicit reference to sea-level projections for Venice in these references (while Venice is indeed mentioned in the SROCC in a few instances regarding analysis of the associated tide-gauge data), we agree that they are relevant for this study as they provide regional and local relative sea-level, from which, somehow, one could "pick-up" sea-level projection for Venice. In this sense, Slangen et al. (2012) is indeed a pioneering paper, and we would cite this in the revised manuscript.

Also, thanks for pointing at Kopp et al. (2014), which is indeed very relevant for our paper as it contains explicit projections for Venice. They use the global tide-gauge PSMSL dataset and assume that the recorded sea level is represented as the sum of three Gaussian processes, including (1) a globally uniform process, (2) a regionally varying, temporally linear process, and (3) a regionally varying, temporally autocorrelated non-linear process for each tide-gauge site. The process (2) is retained as the “background non-climatic local sea-level change” corresponding to GIA, tectonics, and other non-climatic local effects. They then use this background linear estimates and its uncertainty for projections. In Venice, they estimate a background subsidence of 0.72 +/- 0.33 mm/yr with their technique (see their supplementary material, Table 8). The subsidence rates provided in Table 4 of our manuscript, which stems from various measurement methods, are overall higher than that by Kopp et al., which stems from a statistical method applied to tide-gauges record. Furthermore, Kopp et al. (2014) quote as a note of caution : “Third, our background rate estimates are the result of an algorithm applied to a global database of tide-gauge data, with different sites having been subjected to different degrees of quality control. Some tide-gauge sites may have experienced datum shifts or other local sources of errors not identified by the analysis. We recommend that users of projections for practical applications in specific regions scrutinize local tide-gauge records for such effects”.

We would refer to Kopp et al. (2014) in the revised manuscript as follows: “In particular, Kopp et al. (2014) provide probabilistic sea-level projections for Venice as part of a global set of local sea-level projections for three different representative concentration pathways. Their projections build on the decomposition of the recorded historical sea level into a number of processes, among which is the “background non-climatic local sea-level change” corresponding to GIA, tectonics, and other non-climatic local effects. This background linear estimate and its uncertainty (0.72±0.33 mm/year for Venice, see supplementary material, Table 8) is then included in the projections, together with the other components. The 5th-95th percentile range of sea level change from their projections at year 2100 is 29-79 cm for RCP2.6 and 41-107 cm for RCP8.5.“

Our projections largely overlap with those by Kopp et al. (2014), suggesting that they are overall consistent despite some differences that reflect the fact that methods, models and assumptions differ between the two studies. We would mention the need to understand such differences as an opportunity for progress in the revised manuscript.

We would use the following arguments to better motivate our own projections.

- firstly, in published literature the sterodynamic component in the Mediteranean basin is derived from CMIP3 or CMIP5 multi-model ensembles while it has been shown that CMIP model results in the Mediterranean basin have strong biases (see for instance a discussion in Thieblemont et al., 2019, notably their Figure 2). Accordingly, we rely on the lateral boundary forcing at Gibraltar as explained at line 710 of the original manuscript. We would better clarify this in the revised manuscript.

- second, as far as we understand, the papers reported by the Reviewer include regional/local estimates of vertical land motions in the sense that they account for some GIA contribution, intended as present-day viscoelastic response of the Earth’s crust to changes in ice masses throughout the last glacial cycle, hence they do not provide a full characterization of vertical land motions in Venice, which is instead a salient point in the present study.

We would also revise the text by removing any reference to ours being the first attempt to develop local RSL change scenarios for Venice, and discuss differences between our projections and those obtained by Kopp et al. (2014) as outlined above.

Figures

There is no reference to Figure 3.

Thanks for picking this. Figure 3 is relevant for section 2.2, and we would refer to it as follows: “The results show that a reasonable increase in quantity and quality of data can be achieved compared to standard products up to a few kilometers from the coastline. Figure 3 illustrates the example of the Gulf of Trieste, where three missions cross the area and a data gap exists with standard products. In this case, the number of outliers along the Jason-1 and Jason-2 tracks is almost always less than the standard product and the improvement is clearly evident until 6 km from the coast.”

Figure 6: How have confidence intervals been determined? What noise model has been used?

The trend and associated confidence intervals are obtained by linear regression analysis. The matlab function “regress” is used for the calculation. We plan to publish the script in a repository so the procedure should be clear. We are open to include more details in the revised manuscript in case the following clarification in the caption of the figure is deemed insufficient: “black contours illustrate where the GMSL and Venetian sea-level trend estimates do not overlap within 95% confidence intervals obtained from the linear regression [...]“

Figure 7 as well: how have all the errors been computed? For panel B? Why use model data from CMIP3, while we have had CMIP5 and now CMIP6 has become available as well?

Concerning panel A, the errors are 90% confidence level obtained from linear least-squares regression analysis. We would clarify this in the revised manuscript.

Concerning panel B, the plot is a replica of the original figure in Slangen et al. (2016). The uncertainties are the spread among the simulations with only differing Atlantic boundary conditions (blue) and the spread among the simulations with differing socio-economic scenarios (red). We would clarify this in the revised manuscript.

Concerning panel C, the uncertainties correspond to the combined uncertainty of each sea-level component (i.e. glaciers, ice-sheets, sterodynamic, ...) calculated as the square root of the sum of the squares of each component uncertainty. Note however that contributions that correlate with global air temperature have correlated uncertainties and are therefore added linearly (this concerns sterodynamic and ice-sheet surface mass balance components). See Church et al. (2013) for more details. We would clarify this in the revised manuscript.

We agree that the figure needs to be carefully described as it encompasses different aspects of the issue and different panels refers to different data and/or publications. We plan to amend this with improved caption and improved description in the text. We are aware that CMIP6 data are being made available, but an updated analysis on such data deserves a dedicated study beyond the current literature review. We would highlight this in the revised manuscript with a sentence along the following lines at the end of section 7: “Updated scenarios of Venetian sea-level future change are expected as output from the 6th phase of the Coupled Model Intercomparison Project is made available.”

Ref: Church, J.A.; Clark, P.U.; Cazenave, A.; Gregory, J.M.; Jevrejeva, S.; Levermann, A.; Merrifield, M.A.; Milne, G.A.; Nerem, R.S.; Nunn, P.D.; et al. 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 ed.; Cambridge University Press: Cambridge, UK, 2013.

Figure 8. Middle row: how should I interpret this plot?

The panels represent the wavelet spectra for the autumn (left) and winter (right) Venetian sea-level time series, after correction for subsidence. Since the interest is on portions of the wavelet spectrum where wavelet amplitude exceeds statistical significance (against a lag-1 red noise), we only show this and omit the representation of wavelet amplitude. Specifically, “Shading (thick black contour) is the portion of the wavelet spectrum exceeding 90% (95%) confidence against red noise (lag-1 autoregressive model) hypothesis (see Grinsted et al., 2004, for details)”. We would report this in the caption.

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We would add all relevant references in the revised manuscript.