Reviewer 1

We thank Reviewer 1 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.

This is a comprehensive paper reviewing the present knowledge of sea level changes in Venice, including observations and mechanisms and also with some discussion about regional projections in the Northern Adriatic. The manuscript is complete and includes the state of the art in regional sea level in the Mediterranean basin, in addition to provide new computations that include the most recent data for Venice. One strength is the detailed description of vertical land movements, a major driver of relative sea level changes, at many different temporal scales and with emphasis of the distinct acting mechanisms. Another one is the summary of some of the main results in table format. In my opinion, this manuscript deserves publication and will likely become a main reference of sea level variations in Venice. Some parts of the paper are, however, confusing and I think should be reorganised. I am giving details on this latter comment below.

Thank you for the overall positive evaluation of our review. We will improve the manuscript based on your recommendations as described in our responses to the specific comments below.

General: The way section 5 is organised is confusing and, in my opinion, the separation into subsections is misleading. I am giving more details below but, essentially, I do not think that if the section is discussing climate forcing of MSL variability, it is convenient to separate the physical mechanisms from numerical modelling. I would suggest focussing on the mechanisms (namely the effect of atmospheric pressure and winds, the water mass exchanges through Gibraltar, surface fluxes) and discuss their spatial and temporal scales, together with the origin of the data, whether observations or models. Note that barotropic models are mentioned in 5.1.1 but separated from the section on numerical modelling. Overall, I think that this section would benefit from rewriting.

We would substantially restructure section 5 in the revised manuscript and merge the subsections regarding the physical mechanisms underlying Venetian sea-level variability and numerical modelling. We plan to have an introductory general part and then three separate subsections dealing with the three most important aspects of climate forcing of Mediterranean and Venetian sea level as they emerge from the screened literature, namely:

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

Each section/subsection will embed relevant aspects regarding the associated numerical modelling. The restructuring will benefit from an expanded list of references built on the Reviewers’ suggestion.

Section 7 discusses many of the gaps of knowledge in Mediterranean physical oceanography. In particular, around lines 766-794, the focus is on the limited knowledge on ocean circulation and the impacts that thermohaline changes in the lateral Atlantic boundary. However, the impact of these processes on basin-scale sea level and in Venice is small in comparison to other effects. This is especially true when climate projections are concerned. Indeed, in section 6.2, the authors include a nice summary of regional projections and uncertainty ranges which are by far much larger than the impacts of regional circulation changes.

In the revision of the manuscript we will homogenize sections 5 and 6 in terms of the relevance of the different processes responsible for Venetian sea-level variations and better highlight, where necessary, why different focus is given in different parts of the manuscript. In the restructured Section 5 we will clearly separate processes that lead to Mediterranean basin-scale sea level changes from those that lead to spatial heterogeneity in sea-level anomalies within the Mediterranean, with a focus on the Adriatic Sea and Venice. We would better confront thermosteric and mass contributions to sea-level, also in relation to the considered timescales. The restructuring will be facilitated by an expanded list of references, which builds on suggestions by both Reviewers.

Throughout the paper, MSL is used to refer to geocentric MSL in contrast to RSL. But MSL can be computed from RSL from tide gauges as well as from geocentric sea surface height, following the definition here and in Gregory et al (2019). I suggest to add geocentric when referring to RSL corrected for vertical land movements (e.g. section 4.1) to avoid confusion.

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 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 in the revised manuscript:

- Relative Sea Level (RSL) change: change in local sea level from 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.

Specific comments: - Line 90: “from the open ocean to the coastal zone”: strickly speaking this depends on the definition of coastal zone, as altimetry measurements are often only valid tenths of km offshore, where the land signal does not contaminate the observations.

We totally agree with the Reviewer that satellite altimetry becomes challenging when approaching the coasts, as this is also discussed extensively in our manuscript for the case of the Venice lagoon. To avoid confusion in this specific sentence, we would change the quoted part to “with an almost global coverage” in the revised manuscript.

- l. 118-119: how was the vertical datum determined to an accuracy of 0.1 mm?

The vertical datum is the one reported in Dorigo (1961). It was defined from the computation of a 25-yr mean tide level and we have no information about the reason for adopting the 0.1-mm precision; it was not an accuracy because it did not come from a measurement. Perhaps (but it is just a speculation) the choice was made for consistency with the 0.1-mm accuracy, typical of benchmark levelling.

In order to clarify this point in the revised manuscript we would modify the text as follows:

“According to Dorigo (1961), Mati established the tide gauge datum at Santo Stefano at 1.50 m below the CM of 1825. In 1910, a new datum was adopted, namely the mean tide level (MTL) of 1884-1909 (central year 1897), computed from the high and low waters measured at Santo Stefano. According to Dorigo (1961), it turned out to be 1.2754 m above the old tide gauge datum, corresponding to 0.2246 m below the CM of 1825. The new reference was named the “Zero Mareografico Punta Salute” (ZMPS).”

- l. 171: determine->determines

Thanks, this would be corrected in the revised manuscript.

- l. 225: b.s.l. -> below sea level (I guess)

Yes, this would be explicitly reported in the revised manuscript.

- table 2: shouldn’t GIA rates from section 3.1.3 be included in this table, for completion?

Table 2 reports the time evolution of natural land subsidence in the Venetian region. GIA is just one of the components that contributes to vertical land motions, therefore we believe that it would be misleading to report GIA rates in the table.

- fig. 5: please add a-b-c-d labels to subplots

Thanks, labels would be added in the revised manuscript.

- l. 357: wrong reference to fig. 3

Thanks, the correct reference is to Fig. 5, to be amended in the revised manuscript.

- table 3: the value of subsidence reported for the period 2008-2020 is notably larger than 2003-2010 and 2014-2020 (the latter also from a GPS station), but no comments are provided in the text. Given the differences and the likely overestimation of the GPS VEN1 I think it is worth to mention it in the paper.

This comment refers to the following estimates reported in Table 3:

Time span	Value (mm/yr)	Technique
2003-2010	1.0±0.7	SAR
2008-2020	1.7±0.5	GPS station VEN1(Riva dei Sette Mari)
2014-2020	0.9±0.6	GPS station PSAL (Punta della Salute)

These estimates result from different techniques and are not only referred to different periods of time, but also to different locations. As reported in the table's caption, the uncertainty associated with the SAR-derived rate represents the ground motion variability at the city scale. Therefore, the rates of the two GPS estimates are actually both consistent with the average value reported over the whole city. SAR has proven capable of detecting ground displacements at the single-building scale and the measured rates range between -10 and 2 mm/year (see lines 342-343 of the original manuscript). Besides that, one of the main purposes of Section 3 is to clarify that the subsidence behavior in Venice is definitely non linear. Therefore, temporal variations are to be expected even at short time scales.

We would better discuss differences between estimates reported in Table 3 in the revised manuscript, along the line of this response to the Reviewer.

- l. 402-404: please provide a reference for the value in Ravenna.

The reference is the same used for Venice, i.e., Zerbini et al. (2017). We would amend the sentence accordingly in the revised manuscript.

- Fig. 8 and l. 550-555: it is surprising the differences between fall and winter wavelets and coherence RSL-NAO. Generally, extended winter (Dec-March) NAO is used to correlated with RSL, since it is during these months when the signal is stronger. It is probably worth to comment on the differences found here.

We agree that the NAO is more stable in winter and therefore the connection between NAO and Venetian sea level is more significant during this period of the year, whereas other modes of large-scale atmospheric variability become more important than the NAO in other seasons. In fact, this was also shown in Zanchettin et al. (2009). In our manuscript, the inclusion of an analysis of autumn series was mainly motivated due to the relevance of this season for storm surges in the Venice lagoon. Also following a comment by Reviewer #2, we would better stress the seasonal difference in the NAO imprint on Venetian sea level. Our responses to Reviewer #2 provide more details about how we plan to revise the manuscript regarding the NAO.

As mentioned above, we would have a specific subsection dedicated to the NAO in the revised manuscript (section 5.3 "Linkage with the NAO"), which will also contain general introductory information about the NAO, and a more critical discussion about the results from the wavelet coherence analysis. We also plan to add a figure illustrating maps of sea-level pressure and near-surface wind anomalies linked to different phases of the NAO, to aid the discussion about the atmospheric mechanical forcing of Venetian sea level associated with the NAO. Overall, we expect the new section 5.3 in the revised manuscript to provide a much clearer presentation of what is known about the connection between Mediterranean and Venetian sea level and the NAO.

- section 5.1.1 discusses the effect of atmospheric pressure and winds. Yet, the title "atmospheric forcing" is misleading as it might also include heat/water flux exchanges. These processes are then discussed in section 5.1.2 instead. I suggest merging both sub-sections.

As outlined above, we would carefully revise the manuscript toward a better structure of section 5. The subsections mentioned in this comment will be removed, and their content merged. We will clearly present local atmospheric mechanical forcing (IBE and wind forcing) and ocean-atmosphere surface fluxes as different aspects in a dedicated revised subsection within section 5 (new section 5.2, see above). To better present local atmospheric forcing of Venetian sea level, we also plan to have an additional figure illustrating the sea-level pressure and near-surface wind anomalous patterns over the Euro-Mediterranean region associated to Venetian sea-level anomalies.

- section 5.2 on numerical modelling of the Mediterranean sea level is definitely too short. The authors should also describe other numerical simulations that are available and that provide sea surface height as an output, as well as the community effort developed by MedCORDEX in which the ocean component is very strong. For the former case, one prominent example is the recent 3D reanalysis by Simoncelli et al (2017) available through CMEMS (https://resources.marine.copernicus.eu/?option=com_csw&view=details&product_id=MEDSEA_REANALYSIS_PHYS_006_Also, section 5.2 refers only to 3D numerical modelling, while does not mention vertically integrated numerical models that have contributed in the past to unveil the role of atmospheric pressure and wind. Some of these works are discussed in section 5.1.1.

The term coastal sea level in the title is not particularly addressed either.

As replied above to the general comment by the Reviewer, in the restructuring of Section 5 we would get rid of the original subsection on numerical modelling. In the revised manuscript we would avoid having a complete list of model types and initiatives regarding the Mediterranean Sea and instead embed the relevant parts of section 5.2 of the original manuscript along the discussion of the physical mechanisms responsible for variations in Venetian sea level. Reference to models of different complexity should then become more straightforward.

We would still mention MedCORDEX and the availability of regional reanalysis in Section 6 (gaps of knowledge and opportunities for progress) as follows: "Accordingly, over recent years, considerable efforts have been invested into developing and applying regional climate and ocean circulation models approaching the issue of dynamical downscaling from different perspectives (e.g., Somot et al., 2008; Sannino et al., 2009; Artale et al., 2010; Naranjo et al., 2014; Sein et al., 2015; Turuncoglu and Sannino, 2017; Androsov et al., 2019; Palma et al., 2020), also in the context of coordinated international activities (e.g., MedCORDEX) including the use of data assimilation techniques to obtain regional reanalysis for the Mediterranean Sea (Simoncelli et al., 2014)."

- L. 632-634: GMSL and regional deviations from the global mean, instead of regional sea level changes. This distinction would come up naturally if the mechanisms are described in terms of their spatio-temporal variability in section 5.

Agreed.

- l. 641: where does the range 0.6-1 mm/yr comes from? The numbers discussed above are over 1 mm/yr for late Holocene natural rates of subsidence only.

The smaller rate was proposed in old studies (e.g., Carbognin et al., 1976), whereas more recent estimates are more consistently in the order of 1 mm/yr. We would amend this in the revised version and only refer to the current estimates (hence1 mm/yr).

- l. 800-802: worth mentioning other non-parametric methods such as Empirical Mode Decomposition or Singular Spectrum Analysis for computing time-varying rates of RSL change.

We would mention both techniques in the revised manuscript: "Further research (e.g., exploiting techniques such as empirical mode decomposition and singular spectrum analysis) is therefore needed …".

- l. 827: the range given for projected MSL here of 21-100 cm by 2100 should be framed into the corresponding RCPs. Otherwise can be wrongly interpreted. The climate scenario is one of the major sources of uncertainty in projections by 2100, so I think it would be better to state the numbers for the scenarios considered: 32-62 cm under RCP2.6 up to 58-110 cm under RCP8.5, including subsidence. This is important because it helps to interpret that the likelihood of the lower bound is different from than in the upper bound.

We would explicit the scenarios in the revised manuscript, as follows: "Projected climatically-induced Venetian sea-level rise from estimates for the GMSL corrected for further uncertainty associated with the regional redistribution of different mass contribution components such as glacier, ice-sheet and groundwater is in the range from 22 to 52 (from 48 to 100) centimeters by 2100 for the RCP2.6 (RCP5.8) concentration scenarios."