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Discussions

Interactive comment on "Review article: Sea-level rise in Venice: historic and future trends" *by* Davide Zanchettin et al.

Anonymous Referee #1

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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.

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.

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.

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.

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.

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

- l. 171: determine->determines

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

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

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

- l. 357: wrong reference to fig. 3

- 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.

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

- 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.

- 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.

- 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 b y Simoncelli et al (2017) available through CMEMS (https://resources.marine.copernicus.eu/?option=com_csw&view=details&product_id=MEDS 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.

- 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.

- 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.

- 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.

- 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.

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