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

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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.

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.

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.

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.

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).

Also a point on the significance on numbers, but I admit this is a bit a matter of personal taste: in an expression like 1.23 ± 0.13, the last numbers are not significant, so something like 1.2 ± 0.1 avoids a false 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.

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?

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.).

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.

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

Section 1

L52: Remove 'critically'

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

Section 2

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

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?

Section 3

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

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.

L365: Please define 'MOSE'.

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.

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.

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.

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.

Paragraph 4.2:

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

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

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?

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.

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.

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?

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

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

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 inter-annual scales. The geostrophic winds caused by these variations drive a barotropic sea-level response in Venice, especially in winter."

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).

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

L531: Geostrophic wind is something different than large-scale wind

(https://en.wikipedia.org/wiki/Geostrophic_wind)

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?

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

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.

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.

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.

L558. The contribution of the IBE effect to sea level has been quantified way before 2009 as $\sim$ 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).

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

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.

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?

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.

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

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?

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

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).

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.

L728: This statement is not true. There's no single process that causes a uniform sea-level 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.

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?

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.

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

L803: What is "energetic 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.

Figures

There is no reference to Figure 3.

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

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?

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

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