Comments on sp-2023-36 by Sergiu Dov Rosen*

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Estimeed authors,

First I wish to congratulate the authors for the excellent paper produced, covering the full range of processes leading to SLR and its future assessment for European seas.

We thank you for providing feedback on our paper.

My comments are mainly related to the processes contributing to SLR and ESL in the Levantine basin of the Mediterranean Sea, particularly in the Nile littoral cell, covering the South Eastern Mediterranean coasts from Egypt Nile delta to Israel. They are listed below and then a short list of references.

1. The paper mentioned that one of the factors affecting the SL is the ocean circulation and that regional models of higher resolution are needed to improve the assessments (page 2, lines 46-47). However, in the South Eastern Mediterranean, an important contribution seems that it has been overlooked as it was not mentioned in the paper. I refer to the discharge of Red Sea water via the Suez Canal in the Mediterranean Sea, which, since the beginning of the 1990's increased significantly, due to the Canal deepening and widening a few times, as already indicated by Rosen (2014). The Red Sea waters flowing to the Mediterranean, pass on their way through the Bitter Lakes region, where they collect salt and heat, entering the Mediterranean much saltier and warm than both the Red Sea and the Mediterranean Sea, sinking toward the bottom of the Mediterranean. My rough estimate, based on published cross sections and flow speeds (Eid F.M., et al., 1997; Alam El-Din K.A., et al., 1999; Elzeir M.A., and T. Hibino, 1999; Suez Canal Authority) is of a discharge of about 100 to 120 Km^3/year. Thus, a significant amount of salt has been added to the Mediterranean water volume. Published studies indeed confirm the presence of increased salinity waters off the SE Levantine basin. Furthermore, the warmer waters add to the heating of the lower waters of the region thus contributing to the thermosteric contribution of the Mediterranean. Unfortunately, so far model studies of the SLR in the Mediterranean did not include this contribution which lacks their assessments. This however has been observed from the analyses at the GLOSS station 80 Hadera which indicated a faster SLR of 5.9 mm/year for the period 04/1992-003/2014 (and about 5 mm/year for the period 04/1992-03/2023) relative the global average SLR rate for the same period of 3.5 mm/year. Furthermore, another flow change apparently has not been incorporated in the models. I refer to the Nile river discharge to the Mediterranean, which until 1965, when the High Aswan dam was finished, was of millions of m^3/year, reduced to about 5,000 m^3/year ever since. However, some part of the Nile water of unknown volume, used for agricultural use in the Nile delta coast, has been seeping to the Mediterranean, as indicated by satellite monitoring.

We thank the reviewer for the detailed explanation of the Suez Canal. We have added a sentence in section 6.4.1 about the impact of the warm and salty waters on the steric signal of the Southeastern Mediterranean basin. Regarding the impact on sea level changes of these water inputs, it is not clear from figures 3 (tide gauges) and 6 (altimetry). We prefer not to quote the relative sea level trend from a tide gauge, as it does not differ from many other tide gauges around the basin. Altimetric sea level trends do not support either a clear signal on geocentric sea level from the water flowing from the Red Sea or the Nile.

2. In the introduction of the paper (page 3, lines 61-62) flood coastal plains are mentioned. In my opinion a clarification would be appropriate, indicating that in the paper the flood plain is defined as that due only to the penetration of sea level. However, the true flood plain is of higher elevation, induced by reduced river flow discharge into the sea, caused by the reduced flow gradient due to elevated sea water levels at the river mouths during river floods.

We modified the sentence to:

"Sea level rise (SLR) is a major concern for Europe, where more than 50 million people live in low-elevation (≤ 10m) coastal zones and 30 million in the 100-year event marine coastal flood plains (Neumann et al., 2015)."

Coastal flooding due to sea level rise as well as pluvial/fluvial and marine compound flooding are addressed in a companion paper (van de Wal et al., 2023).

van De Wal, R., Melet, A., Bellafiore, D., Vousdoukas, M. I., Camus, P., Ferrarin, C., Essink, G. H. P. O., Haigh, I. D., Lionello, P., Luijendijk, A. P., Toimil, A., and Staneva, J.: Sea Level Rise in Europe: impacts and consequences, State of the Planet, submitted, 2023.

3. In section 2.1 (page 6, lines 124-125) the article refers as stated, mainly to the two IPCC latest reports, which were published a few years ago. However, it seems to have disregarded important new information published in November 2023, indicating potential much higher and faster sea level rise from melting ice caps (ICCI, 2023).

Indeed, section 2.1 focuses on IPCC SROCC and AR6.

The ICCI report of 2023 is not a peer reviewed report and as such not as rigorous as IPCC reports so it does not seem to be warranted to use it in the introduction here. At the same time, it is true that the latest IPCC report of Working Group 1 is already a few years old by now and science has made progress since then. The higher values mentioned in the ICCI report for Greenland based on the paper by Beckmann and Winkelmann (2023) are therefore discussed in section 4:

"Along a similar line of reasoning, Beckmann and Winkelmann et al. 2023 argued a substantial increase of mass loss in Greenland if extreme warm summers are added to the projections."

Thanks for pointing this out.

Beckmann, J. and Winkelmann, R.: Effects of extreme melt events on ice flow and sea level rise of the Greenland Ice Sheet, The Cryosphere, 17, 3083–3099, https://doi.org/10.5194/tc-17-3083-2023, 2023.

4. a. Both in the IPCC AR6 report and in your article (page 6, line 150), future SLR are referred relative to the average SL during the decade 1995-2014. The reasoning to refer sea level rise to a decade (1995-2014) has not been clearly motivated. Indeed one possible explanation I found is due to similar global average temperatures as during the Last Interglacial stage, about 125,000 years ago (global average sea surface temperature was about 0.5° ± 0.3°C (0.9° ± 0.5°F) above

the preindustrial level [that is, comparable to the average over 1995–2014, when global mean temperature was about 0.8°C (1.4°F) above the preindustrial levels] (USGCRP, 2017). Personally I am not convinced that such reasoning is adequate for selecting the average of that period for reference since the accuracy of the temperatures during the historic times is somewhat questionable. I would rather prefer reference to a certain year or series of calm years without strong NO and El Ninio/La Ninia activities.

On page 6, line 150 of the submitted version, we report IPCC AR6 projections of global mean sea level rise.

In IPCC AR6, the choice of the 1995-2014 baseline to inform on future changes is motivated by the fact that:

- Data are averaged over a 20-yr period to define a climate state. Indeed, 20 year-mean enables to remove most of the large internal variability which operates on interannual time scales and to focus on the climate state. This way we can compare a current climate state with projected climate states in the future without a large uncertainty induced by the internal variability
- The last two decades of the CMIP6 historical simulations are therefore selected. At the time of production of CMIP6 simulations, historical forcings were available until 2014. Projections therefore started in 2015. The last two decades of historical runs, corresponding to the 'recent past baseline period' used as a reference to evaluate future projected changes. Therefore, the 1995-2014 period was used for IPCC AR6.

This is standard in IPCC reports and climate change community.

4b. Furthermore, the use of Gregorian calendary years is wrong in reviewer's opinnion, as division by calendary years (January to December) from climatic cycle point of view, has the potential to place time neighboring dependent ESL events occurring in the December-January period in different years in the northern hemisphere, potentially affecting statistics of ESL. Hence a different yearly division would be much more correct, such as from April to following March end of next year or from October to following next year September end. Personally I use the April to March yearly meteo-marine division.

We follow standard community reporting methodology with calendar years.

5. On page 12, in Figure 2, on the Israel Mediterranean coast are shown 3 stations two with blue dots representing recording periods of up to 25 years and in the middle, one green dot, hardly visible as covered by the blue dots, of record length over 25 years and less than 50 years. In fact, the green dot should be for the Hadera GLOSS station 80, with a high frequency record of over 30 years. Another green station should have been for Tel Aviv Yaffo, also with high frequency SL over 25 years.

Figure 2 is based on PSMSL tide gauge records. In this database, Tel Aviv Yaffo data record covers 2011-2022 (hence less than 25 years of data) and the Hadera data record covers 1992-2022 (hence more than 25 years of data).

https://psmsl.org/data/obtaining/map.html#metadataTab

Regarding the high-frequency records, unfortunately they are not included in GESLA dataset, as they are not publicly available in any of the repositories that are part of GESLA.

6. On page 29, Figure 10 shows Median relative sea level regional projections (medium confidence) from the IPCC AR6 report around Europe. I would like to suggest that the abscissa title be more explicit, stating the following text: Mean sea level by 2100 w.r.t 1995-2014 (m), based on medium confidence projections, i.e. excluding ice sheet processes associated with deep uncertainty.

We further specified the legend.

7. On page 57, section 6.4.6 on wave climate, the statement on line 1222 in my opinion may be misleading, claiming "In winter, mean and extreme waves are highest in the western Mediterranean,". The statement is true if one refers to European coasts only, but since in the paper it is stated that the report covers European seas, and since in many places the whole Mediterranean is shown, it would be appropriate to include the Levantine basin too, and then the correct statement would be that "in winter, mean and extreme waves are highest in the eastern Mediterranean followed by those in the western Mediterranean." Similarly, the statements regarding fetches and correspondingly the return periods for the 100-year return level would increase to over 8 m deep water significant wave height offshore the Israeli coast.

Figures 4 and 5 of the recent wave reanalysis of Barbariol et al. (2021, cited in our manuscript) show that mean and extreme waves in winter are higher in the western than in the eastern Mediterranean, though a local maximum of the mean wave height is present also in the Ionian Sea. Values along the coast of Israel are remarkably high, but figure 10 of Toomey et al. (2022, cited in our manuscript) shows that the highest 100-y return values are located along the North African coast of the western Mediterranean basin.

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