Comment on egusphere-2022-169
Anonymous Referee #1

Referee comment on "Reconstruction of Mediterranean coastal sea level at different timescales based on tide gauge records" by Jorge Ramos-Alcántara et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-169-RC1, 2022

Review of “Reconstruction of Mediterranean coastal sea level at different time scales based on tide gauge records” by Alcántara et al., under discussion in Ocean Science.

In this manuscript the authors combine sea level observations from tide gauges and satellite altimetry with output of an ocean model in order to produce a coastal sea level reconstruction with high spatial and temporal resolution along the western Mediterranean coast.

The paper reads very well, the methodology is clearly explained and the results quite relevant for a broad range of scientists and policymakers. In particular, the analysis of sea level variability over four different frequency bands is rather interesting and useful to understand the physical processes behind the observed changes.

I recommend the manuscript for publication in Ocean Science after a few minor issues have been addressed. My comments follow their chronological order and they are not sorted by relevance.

Line 44: “without some further data processing” is a quite vague statement that could be followed by a short explanation of what this data processing generally includes.

Line 75: please add a reference for the optimal interpolation method.
Table 1: it would be nice to show the location of all stations in a figure; alternatively, the authors could at least add a few labels to Figure 1, with the names of those stations that are explicitly discussed later in paper.

Line 98: please add a few more details about how datum shifts are correct for, the current sentence is quite concise.

Line 185: change “series” into time series or stations.

Line 189: how is it possible to use the frequency bands from the previous point when different stations are used? Are the frequency bands determined on the ensemble of stations, hence valid for the whole domain? This issue could be made more explicit.

Line 217: please add reference for equation 5.

Line 223: I understand the need to combine observations from different times, but it rests on the assumption that sea level variability is constant over time, which might not be the case. This issue warrants a more explicit discussion.

Line 272: since some stations are not reproduced very well by the reconstruction, it might be worth removing them from the final product. I wonder whether the authors have tried this. If not, they might want to discuss why they choose to keep all stations.

Line 305: please explain what do you mean by interpolation errors.

Line 332: This advantage of the reconstruction could be highlighted better in the abstract.

Line 345: most recent global reconstructions since Hay et al. (2015), especially those by Dangendorf and colleagues, actually estimate trends smaller than 1.5 mm/yr (more precisely, about 1.2 mm/yr until 1990 and about 1.6 mm/yr until the mid 2010s). The reference to Marcos and Tsimplis (2007a) is outdated.

Figure 12: I wonder whether the fact that the reconstruction is better correlated to tide gauges than satellite altimetry is not simply a direct consequence of the applied methodology. The author should be cautious in arguing that such a correlation is a proof that their construction is superior to satellite altimetry. I’m not saying that I disagree, but
such a claim requires a more detailed discussion.