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Discussions

# *Interactive comment on* "Sea-level rise along the Emilia-Romagna coast (Northern Italy) at 2100: scenarios and impacts" *by* Luisa Perini et al.

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The article by Perini et al. provides an estimation of sea-level rise impacts (in terms of erosion and flooding in the Emilia-Romagna region). Interestingly, the article considers regional subsidence patterns, which have a high spatial variability as shown by previous observations based on SAR interferometry. The article also illustrates how the application of European directives can stimulate studies and discussions regarding the future impacts of climate change. Overall, I think that the article provides an interesting perspective, and that it is relevant to NHESS.

What is missing, to my opinion in the article, is a real discussion of the significance of the results and their implications. I can suggest a few recommendations in this respect: - It could be interesting for the reader to know how such work (which is apparently



strongly connected to regulatory processes such as the European flooding directives, e.g. page 3 line 22) will (or is expected to be) integrated in regional to local adaptation. I have no specific suggestion here, but I just remind that the AR6 IPCC reports to come will require information on the implementation of adaptation (including its successes and limitations). I think that the authors can make a useful contribution here. - The authors clearly list their assumption all along their study (e.g. section 3.2), but the reader would like to see a discussion on the impacts of these assumptions in the final results. I suggest that uncertainties in the results could be given more attention in a discussion section (see below further suggestions). - Finally, if possible, it would be interesting to see to which extent this study agrees or disagrees with previous impacts assessments performed in the same region (e.g. Wolf et al., 2016) and why.

I provide below detailed comments, which are hopefully useful if the authors decide to discuss uncertainties: - Subsidence: I wonder to which extent it is realistic to assume that subsidence is linear in time. In practice, the authors show that it has not been the case in the past (with acceleration in subsidence rates with increased fluid extraction in section 2.1), and this seems to me relatively common in cases of subsidence caused by groundwater extractions (Le Mouelic et al., 2005; Wang et al., 2012; Raucoules et al., 2013). I wonder if the authors would agree that in their table 5, they provide the maximum benefits of an adaptation strategy consisting in mitigating subsidence through reduced fluid extractions. A small point Page 5 line 12: "compaction of sediments" is unclear to me. I assume the authors refer here to natural (and later, anthropogenic) variability of water content in various geological layers, resulting in a reduction of their volume. - Extreme water levels: The authors use value of water heights during storms (subsection 2.3). However, it is unclear which processes have been incorporated. Of course, the references to the project Micore and other studies suggest that tides, atmospheric surge, wave setup (Stockdon et al. 2006) have been taken into account, but I suggest naming these processes explicitly. Note that the wave setup can account for an additional contribution of several 10cm, which is not negligible considering the magnitude of sea-level changes to come. If no information is available on this process, this source of uncertainties can be assumed dominant for the decades to come. - Mean sea-level projections: Sea-level projections used in this article rely on global models, which have not the ability to represent processes taking place at the Gilbratar straight (subsection 2.4). This can result in deviation of some 10 cm from sea-level projections in the Atlantic, west of the Gibraltar straight. Also, is the area affected by 3D circulation modifying water levels by +/-10cm as it is the case in the gulf of Lion? I suggest to discuss these processes in a discussion on uncertainties. They are discussed for example in Adloff et al. (2015, 2016, both in Climate Dynamics) and also in our article Le Cozannet et al. (2015 in Environmental Modeling and Software). Furthermore, the wording "Worst" or "best" cases scenarios (page 9 line 22 and several times after) is not appropriate for ranges of uncertainties representing likely confidence intervals (see Church et al., 2013a, 2013b) and can be misleading for coastal managers in charge of adaptation (Hinkel et al 2015). This should be rephrased. - Impacts : The authors have presented their results in two ways : "land losses" due to sea-level rise and subsidence (e.g., conclusion) and "areas lying below mean sea-level" (e.g. in table 5). I am personally in favor of the second formulation, as it makes no assumption on the adaptive responses to come (e.g., beach and dunes nourishment...). In both cases, the results assume no morphological changes, which, again, would deserve a discussion. There is a huge bibliography in this area. Finally, can the authors explain why storm surge impacts have not been assessed in both CS1 and CS2 hazard assessments (page 14 line 5)? Finally, I wonder if figure 3 and 5 could be merged. I hope these comments are useful.

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