Nat. Hazards Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/nhess-2020-75-RC2, 2020

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Coastal Impacts of Storm Gloria (January 2020) over the Northwestern Mediterranean" *by* Angel Amores et al.

Anonymous Referee #2

Received and published: 5 May 2020

Suitability. The subject of the paper, i.e. the study of the Coastal Impacts of Storm Gloria (January 2020) over the Northwestern Mediterranean falls within the fields covered by NHESS.

Summary. The paper objective it twofold, concentrating on the shorelines of the eastern Spanish coasts and the Balearic Islands: (1) quantify at a regional scale the physical mechanisms at play along the different coastal areas in the basin, including the storm surges and the effect of waves, and discuss their differences, (2) at a more local scale (Ebro Delta and cliff of the eastern Mallorca Island), simulate the impacts of the storm, accounting for the storm surge and wave setup. The paper provides key results on the significant contribution of wind-induced storm surge at the regional scale along the mailand, and wave overtopping at Mallorca cliff site. It also provides flood modeling

results on the Ebro delta.

General comment.While the manuscript is very clear, well written and provides interesting insights in the knowledge of the Gloria storm marine forcings, the manuscript has some weaknesses which deserve to be tackled before publication: discussion (or integration?) on neglected marine forcing especially for the local flood investigation and for the regional model validation (tide, water level fluctuations induced by 3D circulations), the method used to validate the storm surge model, the validity of regional model to properly estimate wave setup with a grid resolution of 1-2km. In addition, the manuscript would benefit from a bit more physical interpretation of the results.

Major remarks

1.Role of neglected processes?

3D Mediterranean circulation induce seasonal water level fluctuations of several centimeters to tens centimeters (Arnicol e al., 1995, Bouffard and Pascuale, 2008). For example, Arnicol et al (1995) indicate variations of +/- 10 cm at the scale of the whole Mediterranean Sea and of each of the two basins. Such fluctuation is far to be negligible for flood issues in micro-tidal areas as the study sites. A bit of discussion on this water level contribution during the Gloria storm would be useful and could reinforce the confidence in the results, if, for instance, this contribution contributed for almost zero during Gloria storm. In addition, all the modeling experiment seem to neglect the tide. The authors should make more explicit that they neglect the tide and discuss the implications on the results for the flood investigations (Ebro Delta and Balearic rocky cliff). Indeed, for instance, the maximum tidal range seems far to be negligible (0.85 m in Barcelona after http://www.portdebarcelona.cat/en/web/el-port/101#2) in front of the Gloria storm surge. But what was the tide during Gloria storm?

2.Model resolution for the wave setup quantification

Without more justification, a coastal resolution of 1-2km is probably too coarse to cap-

ture the local wave set-up contribution. Either I am wrong, then the authors should prove that this resolution is enough for their study site. Or I am true, and then, I am afraid that the authors should remove the analysis of the wave setup contribution (at the regional scale). But they could probably discuss it for the Ebro Delta, where the grid resolution falls to 30 meters (and thus is probably fine enough to capture the wave setup).

3.Model validation

First, regarding the wave model results, the manuscript would benefit from explanations for the Hs underestimation. Second, and more importantly, I have some doubts with the method which consists in comparing the water level model outputs in the 5km radius to the local tide gauge measurements. Indeed, depending on the grid points, some points may include a part of the wave setup (probably less than the reality due to the too coarse resolution of the model, except close to the Ebro delta area), others not. As illustrated in Figure 4, there is a strong variability in the model outputs in the 5km radius, which makes not fully convincing the conclusion of a model providing satisfactory prediction compared to the tide gauge measurements. I would suggest at least to add the model outputs of the nearest point to the tide gauge (simulation #1 and #2). In theory (if the grid resolution is high enough to capture the wave setup), the tide gauge measurements should be comprised more or less between the results of simulation #1 and #2, for the nearest point. If there are discrepancies, the authors could discuss the location and resolution of the model close to the tide-gauge (with maps) and also discuss the local knowledge of wave setup contribution to the tide gauge measurements. To contribute to provide a clearer validation and keep the 5km radius, another idea could be to plot all the model outputs for simulations #1 (first) but with a colorscale (on the time series of model outputs) indicating the distance of the model outputs to the tide gauge, and put in thick line the closest point (together with the tide gauge observation of course). The same figure could be done with the simulations #2 (together with the tide gauge observation of course). Of course, the authors are

free to follow other ideas, as long as it makes the validation clearer by at least showing results on the closest point. I think this an important issue. Refining the validation could also help identifying to which extend the seasonal water level fluctuations induced by 3D circulations are negligible or not during the Gloria storm.

“On-line” Remarks

- Title: for me, the main focus of the paper is not on providing information on coastal impacts, but more on investigating the relative forcing contributions. I would suggest to modify the title to better illustrate the paper content.

- Abstract: The abstract could be a bit more informative regarding the key results.

- Line 38: please provide the geographical coordinates of the Mahon buoy.

- Line 72: Figure 4 is called before Figure 3 -> reorder the figures?

- Line 85: “contains” -> “contain”

- Line 104-107: Test 3 & 4 are done with the 2DH hydrodynamic model or with the coupled model? If the first case, the authors should make it more explicit, and then in Lines 107-108 stress that these tests 3 and 4 are used to estimate the contribution of Patm and wind on the atmospheric storm surge.

- Line 110-112: not clear if the 0% and 3% come from theoretical analysis or from the modeling results. Please clarify.

- Line 121-132: it seems that steady forcing conditions (for SWASH) are used in terms of wave spectrum and still water level. More justification/explanation of this choice and its implication wuld be wellcome.

- Line 129: “an initial integration time of 0.05 s” -> “an initial computation time step of 0.05 s”.

- Line 131: “0,5 m” -> “0.5 m”

- Line 149: explain/justify why the tide gauge data have been low-pass filtered using a Butterworth filter with a cutoff period of 30 minutes. I guess this is due to some local physical reasons, but some justification would be welcome.

- Line 161: add a subsection title?

- Line 164: not sure the authors can use “ocean” for the Mediterranean Sea -> reformulate?

- Line 174-175 / “This pattern is caused by the winds blowing towards the mainland”: I do not fully agree. Indeed, for me, the results are also strongly influenced by the bathymetry. I remind that the analyss of the 2DH shallow water equations show that wind-induced storm surges increase with decreasing water depth (see e.g. Flather (2001)). I think the authors could easily check it using their simulation results (by having a look on 2D spatial maps of simulation #4). This remark leads also to the suggestion to add a bathymetric map in the paper. This will support the analysis of the forcing contributions.

- Line 207-210: these sentences are not clear to me. Please clarify.

- Line 204-214: the comments on the validation/comparison of the model results in terms of flood are not really clear to me. Indeed, when I compare the Copernicus map and the model results, the model seems to provide a larger flooded surface, but predict no flood in one of the N-E area, while there was flood. This this is not clear to me why the authors seem to think that the model underestimates the flood. The manuscript would probably benefit from quantitatively comparing the Copernicus map and the model results, for instance with a map showing the following classes: Copernicus and model predict no flood; Copernicus and model predict flood; Copernicus indicates flood, but the model predicts no flood; Copernicus indicates no flood, but the model predict flood ; Copernicus and the model predict no flood. If not accessible, the Copernicus map could be digitized. In addition, at the Ebro scale, this could be interesting and relevant to investigate the spatial variations relative contribution of wave

set-up, pressure induced and wind induced storm surges (more in details that in figure 6).

- Legend of Figure 2: “c and c” should be “a and c”?

References

- Arnicol G., Le Traon P.Y., Ayoub N., De Mey P. (1995) Mean sea level and surface circulation variability of the Mediterranean Sea from 2 years of TOPEX/POSEIDON altimetry. J. of Geophys. Res., 100(C12), 25, p. 163-25, 177.

- Bouffard J and Pascual A. (2008) A review of altimetry Applications over European Coasts (invited talk). Second Coastal Altimetry Workshop, Pisa, Italy, 2008.

- Flather, R. A.: Storm surges, in: Encyclopedia of Ocean Sciences, edited by: Steele, J. H., Thorpe, S. A., and Turekian, K. K., Academic, San Diego, Calif, 2882–2892, 2001.

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