

The authors of the #nhess-2019-6 submitted paper have made a great effort to address my main concerns (which basically coincided with the comments of the rest two Reviewers). Authors have now clarified any misrepresentation of their notion of the negative SLAs drivers (i.e. tele-connected barometric lows or the immediate inverse barometer effect over the area of focus) leaving little space for any misconception in the revised text. The impact of the cross-basin SLP gradients due to the presence of cyclones is now better explained and the newly added wind patterns strongly back up the previously uncorroborated authors' claims. The paper is now better structured and clearly re-written and should be **accepted for publication**. Only minor issues are left to deal with, subjected to a last **minor revision**, mostly related to improvement of the background literature review and a few easy-to-do clarifications. In the following, I present a few last comments together with some typos corrections.

General Comments:

1) Introduction; Page 2:

The authors insist not to refer to proposed literature, probably to save space and unnecessary info in their paper. It is reasonable that out of the indicative bunch of proposed papers, Bengtsson et al. (2006) concerns only storm tracks under climate change, Calafat et al. (2012) deals with hindcast modelling of only Mean Sea Level variations, Campins et al. (2006) only deal with storm tracks in climate change, Vousdoukas et al. (2017) and Fernández-Montblanc et al. (2019) deal mainly with other divergent issues, and thus all could be indeed left out from the authors' literature review.

However that is not the case for the following: a) Marcos et al. (2011) refer to storm surge modelling under both a control run for 1950–2000 and hindcasts from 1958 to 2001 forced by dynamically downscaled ERA40 reanalysis data linking between atmospheric features and surges; b) Makris et al. (2016) present a lot of information about the prevailing synoptic systems over eastern Mediterranean, including hindcast analysis of a 50-yr reference period up until 2000, together with several comments on the relation of extreme storm surges to atmospheric conditions (wind patterns and synoptic conditions) in the east-central Mediterranean; c) Vousdoukas et al. (2016) also present results of modelled surges on the Mediterranean coastline for a baseline period from 1970 to 2000 validated against simulations driven by ERA-Interim atmospheric forcing, indicating good skills.

At least these three articles should be mentioned in the state-of-the-art of the Introduction as they are important cases dealing with similar issues presented in the submitted paper.

Furthermore, two more crucial papers of Androulidakis et al. (2015) and Ullmann et al. (2007) are only listed in the References but not cited/discussed in the main text of the paper. They should be mentioned in the text with a concise comment of their findings, as they are important background literature on the issues covered by the authors.

Marcos, M., Jordà, G., Gomis, D., Pérez, B. (2011). Changes in storm surges in southern Europe from a regional model under climate change scenarios. *Glob. Planet. Change*, 77(3): 116–128.

Makris, C., Galiatsatou, P., Tolika, K., Anagnostopoulou, C., Kombiadou, K., Prinós, P., Velikou, K., Kapelonis, Z., Tragou, E., Androulidakis, Y., Athanassoulis, G., Vagenas, C., Tegoulis, I., Baltikas, V., Krestenitis, Y., Gerostathis, T., Belibassakis, K. and Rusu, E. (2016). Climate Change Effects on the Marine Characteristics of the Aegean and the Ionian Seas. *Ocean Dynamics*, 66(12): 1603–1635.

Vousdoukas, M.I., Voukouvalas, E., Annunziato, A., Giardino, A., Feyen, L. (2016). Projections of extreme storm surge levels along Europe. *Clim. Dyn.*, 47: 3171–3190.

2) old Comment 3; Pages 7-8:

In the revised paper, the authors have made a decent effort to transform their previously rather untenable claims about negative SLAs' association with cyclonic motions in the atmosphere on sites practically far away at the opposite side of the basin. This clearly presented in the new "Discussion and Conclusions" section, thus any misinterpretation is avoided in the new text version. Yet, a minor further clarifying comment referring also to findings of Figs. 3-5 should also be added i.m.o., similar to the following: *"In general, the presence of a cyclone or a barometric low system is not considered as the immediate cause of negative SLA events, but a probable secondary driver of them. Negative SLAs can be only collaterally associated with cross-basin SLP gradients, with more certainty especially in cases where winds are driven by near cyclone centers, since correlation of parameters does not necessarily prove causation between the examined features."*

3) old Comment 5

My point was that there exist cyclogenetic centers (or areas with barometric lows in general) outside of the Mediterranean window shown in the paper that may influence the study area in terms of SLA response to them. Therefore it should be at least mentioned, and further clarified that *"teleconnection" is a notion referring to global scales (or at least usually larger regions than the one examined in the paper) that needs bigger windows of application, in order to pertain crucial dynamic cyclogenesis centers surrounding the Mediterranean or in the vicinity of it.* A good example of a typical teleconnection index is the NAO, whose effect on weather patterns and oceanographic features of e.g. the Mediterranean needs analysis covering large parts of the Atlantic Ocean and even North Sea regions.

Specific Comments:

Figure 11 and Page 13:

The authors refer to linear regression coefficient as a statistical measure to relate teleconnection phenomena to negative SLAs. Is it the classic simple linear regression r coefficient? If so, shouldn't it better be a statistical coefficient of determination, e.g. r^2 = the Pearson's product-moment coefficient,

in order to show a more robust approach on the dependence or association in the statistical relationship of moving cyclone centers to far-away negative SLAs?

Page 3, Lines 18-25:

The authors refer to a hindcast validation of HYPSE model in their 2013 paper, but if the paper presents new runs with the newest parallel HYPSE version does that mean that new simulations results are not validated? That is because the parallelization of codes is known to induce some numerical instability in results, besides their beneficial reduction of computational times.

Page 4, Lines 6-7:

In the last sentence, please correct the expression. The authors do not have in their possession long-term tide-gauge data for sea level elevation. They are available by National Hydrographic Services.

Page 13, Lines 1-3:

Referring to the contrast of Fig. 7's dense cyclone centers in Fig. 10 e.g. *a* or *b* graphs is the respective MSLP difference of marginally 5-10hPa large enough to support the authors' claims?

Page 14, Lines 8-22:

Please configure text about central/right column are referred, according to the changed Figure 12-13 new four columns.

Technical/Editorial Comments:

Page4, Line30: correct to "short lived features"

Page4, Line32: Put full-stop mark after "Europe"

Page13, Line32: correct to "the station show"

Page15, Line12: delete "anomaly" after "largest"

Page16, Line17: delete one "area"

Page16, Line20: correct to "Wm is the main overall..."

Page16, Line29: correct "causing"