Answers to reviewers 2

The reviewer writes "I have to admit that I tried very hard to follow all the methodological steps and to understand some explanations. I have many queries concerning the relationship of negative SLA with cyclones"

We think that the meaning of some parts of the manuscript have been misunderstood by the reviewer. We add here below further explanation to better clarify the meaning of the results (possibly, the text was not sufficiently clear) and the teleconnection linking a negative sea level anomaly to the presence of a cyclone in the opposite part of the basin. Further, we point to some information that is already present in the manuscript, possibly not sufficiently emphasized.

We clarify that we are not denying that high pressure leads to a negative sea level. On this respect, our study confirms the importance of the inverse barometer effect and show that high pressure causes negative SLAs. We propose to add a new paragraph to the conclusions to clarify this:
"*We clarify that we are not denying that high pressure leads to a negative sea level. Our study clearly supports the importance of the local action of the inverse barometer effect for both positive and negative SLAs. The link between large negative SLAs and cyclones that is shown in this study does not describe a local effect, but a teleconnection, supported by a statistical analysis and explained by the large scale structure of the SLP fields. The connection between cyclone in the opposite part of the basin and negative SLAS at the station is mediated by the cross basin pressure gradient and the presence of a high that locally acts according to the inverse barometer effect"*.

Further, "*The link between large negative SLAs and cyclones is a statistical concept. It is not meant that cyclones are the cause of large negative anomalies, but that the synoptic condition leading to negative anomalies is frequently associated to the present of a cyclone in the opposite part of the basin.*" This clarification will be added in the conclusions, page 13, line 25.

Moreover, the role of anticyclones is confirmed adding a new text after lines 5-8 of the abstract:
"The inverse barometer effect produces a positive anomaly at the coast near the cyclone pressure minimum and a negative anomaly at the opposite side of the Mediterranean Sea, because a cross-basin mean sea level pressure gradient is associated to the presence of a cyclone. This often coincides with the presence of an anticyclone above the station, which causes local negative inverse barometer effect"

The reviewer support his negative evaluation with 10 short points. Here are our rebuttal/clarifications in replies the 10 items present in the review.

1. Abstract, page 1, line 2: the reviewer writes that the sentence ".. with dynamics involving different factors" is not valid since the authors discuss only the inverse barometer effect.
The reviewer thinks that the effect of the wind is also speculated…

The hindcasts are performed with a dynamical model (HYPSE, Lionello 2005) based on solving the shallow water equations for depth average currents. The model computes the evolution for sea level resulting from the action of surface pressure, wind stress and bottom friction. Therefore, all relevant factors are included in the dynamics leading to the computed SLAs. The inverse barometer effect has the advantage that it can be immediately diagnosed from the SLP field. The residual is due to the wind and eventually non-stationary, effects. The new versions of figures 11 and 12 contains the composite of the wind fields and show how the intensity of the wind blowing onshore and offshore is associated with the large residuals over shallow water areas. We hope that this clarifies the meaning of the sentence and the role of the wind.

2. The reviewer (referring to Section 1, page 2, lines 24-29) writes that "the objectives are not clear and robust. In the whole paragraph, the same objective is actually repeated with other words."

The text quoted by reviewer 2 is repeated here below. Bullets points are used to split the sentences and (in red) the subsections to which the text refers are added. We do not see the reason for the strong criticisms of the reviewer. The text in blue might be consider repeating the first sentence and it could eventually be deleted. However, this looks to us to be a question of taste and not a major criticism to the manuscript.

This study investigates the link of both positive and negative large SLAs along the Mediterranean coastline to the passage of cyclones over the region (figure 1) and describes how SLAs evolve and respond to the presence of cyclones. It includes an analysis:

- *of the dynamics of SLAs, (section 3.6)*
- *of the synoptic patterns associated with them (section 3.2) and*
- *of their variations with the position where the SLA occurs (section 3.3)*

It aims at contributing arguments for understanding the link between the variability and evolution of the MR storm track and of SLAs. It

- *describes position and track of cyclones that are associated with extreme SLA*

and (section 3.3)

- *shows the link between their intensity and the magnitude of the corresponding SLAs (section 3.5)*

3. Section 2: the reviewer thinks that, since the hindcast is based on a 2D barotropic model, this allows many simplifications in the results since the temperature variations are not considered. The reviewer thinks that this is an important limitation and could account for the big differences of SLAs in the observed and simulated time series.

At page 3, line 32-34, in the manuscript we write that
a) both observations and model results have been preprocessed using a HPF (High-Pass Filter) with a cutoff frequency of 1/30 days (Conte and Lionello (2013)) in order to cancel long term components (due to change of mass in the MR and steric effects) and to isolate the component that is caused by the short term meteorological forcing.
Consequently the comment of the reviewer is, in our opinion, already addressed in the text. The adopted procedure is meant to ensure that our results are not substantially influenced by steric effects.

Further, the discrepancies between observed and simulated SLAs have mainly strong implications for their ranking. At page 4, lines23-28 we write that
In general, the ranking of SLAs in the observed and simulated time series differs substantially. Consequently, the list of the 100 largest observed ("OBS") and of the 100 largest simulated ("MOD") events share only a fraction of events (table 1). The small number of common events is explained by the grouping of the largest SLAs in a relatively narrow range of values, so that small differences in their magnitude may correspond to large differences in their rank. Therefore, inaccuracies of the HYPSE model and of the driving meteorological fields imply substantial differences in ranking between observed and simulated SLAs.

4. Page 4, line 15: The reviewer asks what mean "depth of the cyclone" means
"The depth of a cyclone is an estimate of the differences between the pressure minimum and the surrounding background value (see Reale and Lionello, 2013 for details on its computation)." This clarification will be added to the text

5. Section 3.1, page 4, lines 24: The reviewer is surprised about these results and claims That the differences are enormous! He asks us to comment on that and wonders about the reasoning for the hindcast.

The explanation of the difference is the text above (see our answers to comment 3). The motivation of the hindcast is to provide long time series of sea level at locations where surges are relevant, but long observed time series are not available

6. Sections 3.3-3.4: The reviewer writes that "The association of the SLAs with the density of cyclones is rather arbitrary" and asks a series of questions.

why a radius of 20 degs from the coast station is selected for search of MSLP?
The search radius is a subjective choice, resulting from empirical tests.
Anyway, for positive SLAs, the outcomes of the search depends very weakly on this parameter. In fact, the resulting cyclone centers are closely grouped around the station at a distance much smaller than 20degrees (see figure 6). For negative SLAs a small search radius would miss to detect the presence of a cyclone in the basin and would not allow the analysis of the teleconnection between cyclones and negative SLAs.

Why the computation of the relative frequency is based on 10 deg radius?
The point for searching a cyclone center and computing the frequencies in table 2 and 3 is not the station, but the reference point around which the cyclone centers concentrate. Therefore, the former search radius and this parameter follow different logics. Ten degrees are about 850 km in the zonal direction in the central areas of the Mediterranean Sea. The average size of cyclones in the Mediterranean is about 500km, of course with non-negligible geographical differences (see table 7 of Lionello et al. 2006 after Trigo et al, 1999) and they move at the average speed of about 180km in six hours (time step of the ERA-Interim data). Consequently, the 10degrees radius is meant to detect cyclones passing close to the reference point at the time of the SLAs.

Trigo, I. F., Davies, T. D., & Bigg, G. R. (1999). Objective climatology of cyclones in the Mediterranean region. J. Climate, 12, 1685–1696.
Lionello P., Bhend J., Buzzi A., Della-Marta P.M., Krichak S., Jansà A., Maheras P., Sanna A., Trigo I.F., Trigo R. (2006). Cyclones in the Mediterranean region: climatology and effects on the environment. In P.Lionello, P.Malanotte-Rizzoli, R.Boscolo (eds) Mediterranean Climate Variability. Amsterdam: Elsevier (NETHERLANDS), 325-372

Why a time step of 10 days is selected?
This step is used for extracting independent SLP fields from the hindcast in order to estimate the probability that a cyclone is present. In general, the requirement that samples are independent is essential for a correct estimate to avoid double counting the same cyclone several times. Lionello et al 2016 show that in the Mediterranean cyclones lasting more than 5 days are extremely rare. Therefore, this step ensures that the climatological probability is estimated using independent samples and every cyclone is counted only once.

Why the reference point is located in the Ionian sea based on a subjective criterion?
First, it is important to note that the reference point has been located subjectively only in Iskenderun for negative SLAs. In all other cases "The reference position is the center of the 5deg wide lat-lon cell where the density of cyclone centers has a maximum." (lines 6-8 at page 8). For negative SLAs at Iskenderun (In this case cyclone centers are rather sparsely distributed) this criterion would locate the reference center at the eastern boundary of the map (lines 8-9), downstream of the Mediterranean region. To avoid this, the secondary maximum (largest value after the actual maximum) has been used leading to the point located in the Ionian basin.

7. The reviewers asks "why the negative SLAs are related with cyclones and not with anticyclones. This seems a more realistic thought and approach. "

A new figure and a table have been produced to describe the role of anticyclones
"*Figure new1 shows the centers of anticyclones at the time of negative SLAs. It is made following the same procedure that has been used for figure 6, which refers to cyclones. It reveals the location of centers of anticyclones in the areas where figure 5 shows high pressure systems. Anticyclones are actually concentrated around the stations, with the exception of Gabes and, to a lesser degree, Trieste, where the wind effect is much larger than the inverse barometer effect and anticyclones play a minor role. Therefore, negative SLAs are linked to the presence of a high pressure around the station. This is necessarily true for most stations, because of the inverse barometer effect. However (see table new1), the probability to find an anticyclone at a distance lower than 10 degrees from the reference position* at the time of negative SLAs is significantly larger than the climatological value only for three stations (Toulon, Thessaloniki, Iskenderun). On the contrary, in Gabes, the absence of an anticyclone is linked to negative SLAs (and this is justified by the dominant role of the wind at this station). The link with the presence of a cyclone in the part of the basin opposite to the station (table*

3) is stronger than what shown in table new1 for anticyclones." This explanation will be added to the manuscript at the end of section 3.4.

Further, an objective of this study is to show that a robust teleconnection, which is supported by a statistical analysis, links negative SLAs at some stations to the presence of a cyclone in the opposite part f the basin. This link does not describe a local effect. In fact, the connection between cyclone in the opposite part of the basin and negative SLAS at the station is mediated by the cross-basin pressure gradient and the presence of a high that locally acts according to the inverse barometer effect. We anticipated at the beginning of this rebuttal that new text to clarify this will be added to the manuscript

*: *The reference position is defined as the center of the 5deg wide lat-lon cell where the density of anticyclone centres (blue square in figure new1) has a maximum (same procedure that was adopted for table 2).*

8. The reviewer is "not convinced about the reliability of the results in sections 3.3 and 3.4". According to him "Many findings are speculated and not verified. The positive SLAs could be related with frontal systems that are not considered in this study.

The argument why the reviewer is not convinced are not given. It is therefore a bit difficult to argue against her/his reluctance to accept the content of these sections or propose changes/additions to convince him. He does not say which "findings are speculated and not verified". The results in table 2 and 3 are checked for statistical significance at the 95% confidence level. Mid latitude cyclones are characterized by the present of cold, warm, and occluded fronts, therefore, when considering a cyclone, the action of the fronts associated with it are included.

9. Section 3.5, page 11, line 4: The reviewer asks "why a linear regression is used? A lag correlation should be attempted since the effect of cyclones on the storm surges is not always instant."

A linear regression is the simplest tool to model the relationship between a dependent variable (in this case the SLAs) and an explanatory variable (in this case the SLP minimum). The two variables are sometime called predictand and predictor respectively. In this case a statistically significant linear relation is found and it shows hot the SLA levels are linked to the intensity (Minimum SLP) of the cyclones. We agree that this approach ignore the possibility of delayed effects, and therefore may conceptually underestimate the strength of the link (that could be stronger, not weaker, than what we have found). Therefore the approach is successful and we have no reason for rejecting it. Certainly, other approaches to reach the same goal are possible. The correlation intrinsically refers to two time series to be correlated. Here, there are no time series, but a set of SLP minima (predictors) and of SLAs (predictand).

10. Section 3.6: The term "dynamics" is not relevant since there is no discussion on the flow regime.
Dynamics is a branch of physical science and subdivision of mechanics that is concerned with the motion of material objects in relation to the physical factors that affect them (Encyclopedia Britannica). In section 3.6 we describe the resulting position of the sea surface (SLA) in relation to the sea level pressure and the wind fields that cause its motion. In our view the term dynamics is justified.