

## Comment on os-2021-72

Anonymous Referee #1

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Referee comment on "Untangling the mistral and seasonal atmospheric forcing driving deep convection in the Gulf of Lion: 2012–2013" by Douglas Keller Jr. et al., Ocean Sci. Discuss., <https://doi.org/10.5194/os-2021-72-RC1>, 2021

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Based on a single deep convection event in the Gulf of Lion, the authors are trying to quantify the Mistral effects to the convection. In particular, they quantified the background contribution, that comes from seasonal changes in the forcing, and the Mistral contribution, that comes as a transient and strong forcing. In my opinion the value of the paper is the use of two approaches for quantification of the above, in particular of development of the "simple" analytical model. On the other side, I feel that the authors invested a lot of energy to basically say that Mistral is generating deep convection events, which is not a new finding and is old as a half of century. Further, saying that some effects comes from seasonality is something to be expected and known, not just in the Gulf of Lion. Summarily, the manuscript can be published, yet after resolving some of potential problems:

- All the analyses are focused on just one year, which might be characterized with higher or lower deep convection - we do not know how to frame these results into the climatology of the deep convection in the Gulf of Lion. And, there are a great number of studies that are trying to quantify wintertime conditions in the Gulf of Lion at the decadal and climate scales. I don't expect that this might be answered or analysed in this manuscript, however some discussion on that should be placed in discussion and conclusion sections.

- Section 2.2. Why the moving average filter is used, as its transfer function is poor and is allowing some energy on higher frequencies to pass through (up to 20%)? Read e.g. <https://ptolemy.berkeley.edu/eecs20/week12/freqResponseRA.html>. The leakage of the energy can be even seen in Fig. 2. It would be better to use other filters, like Kaiser Bessel, Butterworth or other.

- Table 1. I don't get why the authors are presenting the lenght of the period between two Mistral events ( $\tau$ )? It can be easily estimated from the start date and duration of the

event. Also, I am not sure that standard deviation of duration is similar to the standard deviation of in between periods - it does not look from numbers - as presented in the footer of the table.

- Lines 164-168. There should be somewhere the map with locations of CTD measurements.

- Section 3.1. There are no comparisons with ARGO data or ocean reanalyses, like MEDSEA which assimilate all the data (including ARGO, which is not used here) through 4D-Var - why?