Ocean Science Discussions

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Reply on RC1

Douglas Keller Jr. et al.

Author 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-AC1, 2021

We would like to thank the reviewers for their challenging and insightful comments. They were extremely helpful in discerning where the article needed more clarity and rigor. Here we attempt to respond to their comments satisfactorily.

1. 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.

We are currently producing a second sister paper attempting to tackle the investigation of multiple years as well. We have added a few sentences to the discussion to clarify that 2012 to 2013 is an above average year in terms of destratification and deep convection and have discussed some inter-annual variability of deep convection.

2. 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 Kaisser Bessel, Butterworth or other.

While it is true that the transfer function for the moving average filter is poor and leaks some energy into the higher frequencies, the way we implemented the moving average intentionally kept the diurnal cycle by averaging along the same point each day. However, it is also true that the moving average isn't the best filter to preserve the low frequency energy either; see the frequency representation of the wind signal in Figure 1 of the supplemental file.

The red signal is overall at lower power in the frequency representation, even though it maintains a peak at the diurnal cycle. The blue signal is a Butterworth bandstop filter. At first glance, this filter appears to be a better pick as it more cleanly removes the frequencies between the monthly and daily frequencies (as would be a Kaiser-Bessel or another frequency focused filter). However, our goal is to remove the Mistral signal from the wind (and temperature and humidity, which show similar frequency domain representations) which has some presence in the lower frequencies, which we can see qualitatively with Figure 2 of the supplemental, which covers roughly 3 decades.

And more quantitatively with Figure 3 of the supplemental, which shows the time series of the wind speed with the bandstop, low pass portion of the bandstop, and the moving average filtered signals. The low frequency energy of the mistral is seen in the low pass signal, particularly around clusters of Mistral events (all Mistral events are shown with the green box overlay). The low frequency muddies the pulse like behavior of the Mistral at times, which is what we saw when we reran the Seasonal simulation with the Butterworth. Both this new Seasonal simulation and the one forced by the moving average filtered forcing had similar results: the pulse like behavior (and the ocean response to said behavior) was still present but less apparent in the Butterworth version (not shown). Because of the low frequency portion of the Mistral's signal found in the cleaner Butterworth filter, we chose the moving average filter because it appeared to remove the Mistral's signal more completely. We admit that this means using the phrase "anomaly timescale" is somewhat of a misnomer, and to account for this we've clarified its use and the presence of the lower frequency energy in the Mistral's signal in the article.

3. Table 1. I don't get why the authors are presenting the length 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.

We just added it for clarity and transparency when looking at the dominating feature of the Mistral, but you are correct it's not necessary. In terms of the similarity of the two standard deviations, we recalculated them and found the same results. Perhaps the larger mean value of the periods obscures this result?

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

We added the map, along with the locations of the Argo floats used as well (see below).

5. 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?

We added the analysis of Argo floats into the model validation subsection to improve the assessement.

Please also note the supplement to this comment:
https://os.copernicus.org/preprints/os-2021-72/os-2021-72-AC1-supplement.pdf