

Interactive comment on “Ocean signature of intense wind events in the Western Mediterranean Sea” by Francesco Ragone et al.

Anonymous Referee #1

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The paper presents a statistical evaluation of the ocean response to strong winds in the Western Mediterranean region. In this study, the authors use composite products of SST, SSH and of wind fields to identify the ocean response(s) to strong wind and evaluate the duration of the anomalies formed. The methodology is correctly described and the results (only composite averages of SST and SSH) appear quite convincing, even if physical explanations are often mentioned but are not really documented.

My main concern is about the data used, in particular for the wind field with the CCMP product that has a space/time resolution of $0.25^\circ/6\text{hrs}$. It seems that it can miss some very fine/short events that are frequent in the Mediterranean basin (cyclones, medicanes, thunderstorms, or bora events for example). In the annexe, it appears that a downscaling of the ERA-interim reanalyses done with WRF at a 4km-resolution is

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available and shows larger values of wind speed compared to CCMP. The WRF run also has the advantage of being homogeneous in time. So, it would be very useful to add a discussion about the issues linked to the use of CCMP (not only trend but also underestimation!) and the potential impact on the results (notably for what concerns the validity of the thresholds chosen).

I also suggest the authors to revise the paper when speaking about deep water formation (notably the introduction) as, first, it is not enough mentioned that it is an intermittent process with a large interannual variability (Somot et al. 2018). In particular, the ocean vertical stratification is a key factor for this interannual variability. Moreover, very recent results moderate now the direct role of the wind on deep water formation (Giordani et al. 2017, Waldman et al. 2018).

Considering these two main points, I suggest the authors to revise the paper before publication, in particular the introduction and the conclusion where discussions could take place. In my opinion, the abstract should also be revised, because the results shown here can not clearly permit to relate the upper ocean response to any enthalpy flux (heat loss) or diapycnal mixing. This also goes back to my comment about, in general, a lack of documentation of the ocean processes involved.

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Other comments:

- p1, line 2 (abstract): 'The effects ... are...'
- p4, line 14: 'winds are intense enough'
- p5, line 8: delete 'exactly'
- p5, equation 1: please, explain Ω

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Giordani, H., C. Lebeaupin-Brossier, F. Léger, and G. Caniaux (2017): A PV-approach

for dense water formation along fronts: Application to the Northwestern Mediterranean, *J. Geophys. Res. Oceans*, 122, 995–1015, <https://doi.org/10.1002/2016JC012019>.

Somot, S., Houpert, L., Sevault, F. et al. (2018): Characterizing, modelling and understanding the climate variability of the deep water formation in the North-Western Mediterranean Sea. *Clim. Dyn.*, 51: 1179. <https://doi.org/10.1007/s00382-016-3295-0>

Waldman, R., Brüggemann, N., Bosse, A., Spall, M., Somot, S., and Sevault, F. (2018): Overturning the Mediterranean thermohaline circulation. *Geophysical Research Letters*, 45, 8407–8415. <https://doi.org/10.1029/2018GL078502>

Interactive comment on *Ocean Sci. Discuss.*, <https://doi.org/10.5194/os-2018-95>, 2018.

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