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Anomalous 2022 deep water formation and intense phytoplankton bloom in the Cretan area by Anna Teruzzi, Ali Aydogdu, Carolina Amadio, Emanuela Clementi, Simone Colella, Valeria Di Biagio, Massimiliano Drudi, Claudia Fanelli, Laura Feudale, Alessandro Grandi, Pietro Miraglio, Andrea Pisano, Jenny Pistoia, Marco Reale, Stefano Salon, Gianluca Volpe, Gianpiero Cossarini

The paper provides evidence of a phytoplankton bloom South-East of Crete island, Eastern Mediterranean Sea, whose location is displaced from the Rhodes gyre, where phytoplankton blooms have been frequently observed in the past. The authors propose that the bloom was triggered by strong vertical mixing events due a cold spell, which brought nutrient in the photic zone, followed by water column stratification. In this respect the succession of events would perfectly match the Sverdrup conceptual model. The authors limit their analysis to the description of the satellite observations and the results of model simulations. This is likely linked to the scope of the issue to which the paper has been submitted.

The pro of the contribution is that it is a good example of how the Copernicus products may be integrated to detect and describe ocean dynamics. Because of this it may contribute to the SP issue.

My perplexities about publishing the paper in its present format are the following.

1. The satellite coverage is quite coarse in time due to cloud coverage (see fig. 4 in the text) and the bloom area reported in fig. 1 which, if I understood well, is produced by the numerical model, does not seem to be supported by the observations, both in space and in time. This may question the estimates of the bloom relevance which, I assume, is based on the model.
2. The authors highlight that the location of the bloom is not the Rhodes gyre, where the cyclonic circulation and the convection often trigger phytoplankton accumulation. Indeed in their map on fig. 1 the South-West border of Rhodes gyre displays low biomass. One then wonders which 3-D dynamics was active so to produce a localized bloom. If the forcing was the strong negative heat flux, this should have acted over the whole area. Why the bloom occurred only in that limited area and there was no bloom in the Rhodes area. Having the model simulations for the whole basin the authors should discuss this aspect.
3. In Fig. 23 the authors show the time course phosphate concentration above the nutricline. Why phosphate? Because is considered the limiting nutrient? One wonders if the nutricline was relative to a specific nutrient or all the nutrient profiles overlapped.
4. There is a time mismatch between satellite and model. The authors acknowledge this, if it is not a mistype, on lines 175-176. However I do not understand why this " *...provides an assessment of the capability of the prediction chain to simulate specific events*". Do the author mean that the assessment suggests that the model did not simulate the event correctly? If so why they are mostly relying the simulations in discussing the event? A clarification would help.
5. Assuming that the model simulation captured to a reasonable extent the dynamics of the mixed layer, Fig. 2 shows that during the bloom time there were three, if not four, events of deep remixing of the water column. This questions the simple reconstruction of the bloom as convection-nutrient upward transport-surface stabilization. Could the authors analyze the dynamics in more detail?
6. The authors mention that "*..the local fishery community reported increased catches..*" but they do not say where and when. The bloom is quite far from the coast. Did the catches increase in the high sea?

The reference Josey, S. and Schroeder, K.: https://doi.org/10.5194/egusphere-egu23-5884, 2023 is never cited in the text.