Comments on the study "Quasi-tropical cyclone caused anomalous autumn coccolithophore bloom in the Black Sea" by Stanichny et al.

The study reports on anomalous coccolithophores bloom detected by satellites in the Black Sea in October 2005 after a quasi-tropical cyclone in September 2005. The authors analyse physical (mainly) and biological mechanisms explaining the observed phenomenon.
The study definitely deserves publishing. Additional revision, however, might still improve the presentation. I realize that some justifications are challenging given the information the authors have on hands (although sounds plausible). Below I list several suggestions the authors might want to consider to further improve the manuscript.

General comments

1. Indeed, the coccolithophores is the phytoplankton group that due to light scattering by its coccolith (a distinct from other phytoplankton groups feature) can be detected from satellite images. The authors referred to several reginal (developed for the Black Sea) empirical algorithms.

The paper would benefit from introducing a bit more insights on the satellite algorithms used, with an emphasis on how backscattering from algal and non-algal particles are treated. The algorithm exploited in this study for coccolithophores cell concentration (N_{coc}) is based on empirical relation to the light backscattering (b_{bp}) and via b_{bp} to remote sensing reflectance (Rrs) at 555 nm (Rrs555). Following Kubryakov et al. 2019b, the relationship was obtained in the assumption that all backscattering is attributed to scattering by coccoliths, when regionally calibrating the algorithm only in situ measurements from the areas not influenced by non-algal particles were considered. Are there any limitations because of this assumption when applying the algorithm for whole domain? (State it when introducing the method, whether/how or not/why it will impact the analysis).

As input information the authors use MODIS standard Level 2 data on chlorophyll-a concentration (Chl), Rrs and b_{bp}. The standard algorithm does not work for turbid conditions (case 2 waters) and provide (strongly) overestimated Chl (as the authors briefly mentioned). However, the conditions discussed in the study are quite turbid and one could expect backscattering by suspended matter (total suspended matter, TSM) along the cast especially in the river (not only Danube) inflow areas (Kopelevich et al. 2014). To support the discussion on the origin of the coccolithophores bloom (and the statement that Danube plume and Rrs on 5th October in Fig 5b are TSM free) estimates of N_{coc} based on the algorithms accounting and diagnosing also non-algal backscattering (Kopelevich et al. 2014) would be of a help. Alternatively, it would be possible to consider TSM retrievals that could be obtained with Polymer (https://www.hygeos.com/polymer) or SNAP (https://step.esa.int/main/toolboxes/snap/), working with MODIS Level 1 and Level 2 products. These packages would allow to use algorithms for case 2 water and obtain more accurate estimates of Chl if required (Currently as far as I understood the authors use this "artificially" high Chl to trace the Danube plum).

I have to state that this additional information will not, of course, cancel the fact of the observed coccolithophores bloom, but might support the discussion on mechanisms involved.

2. The main difficulty (but not a limitation for publishing) of the study is a lack off in situ measurements nutrients and phytoplankton composition, suspended matter during the investigated event. Are there any similar conditions/mechanisms explaining the discussed exceptional coccolithophores bloom event in October 2005 and more regular event like summer (June) or late-autumn bloom observed or monitored more frequently and complemented by in situ measurements of other quantities? Intercomparison of such events would also support the discussion with respect to phytoplankton composition and shifts in phytoplankton compositions. Generally, the authors provide consistent discussion on the coccolithophores trait allowing it to dominate among other phytoplankton groups (e.g. diatoms) under certain (discussed) environmental conditions. (I would just comment on general affinity of coccolithophores for nutrients, which according to Paasche 2001 high for phosphate to a larger extent than for nitrogen, that makes them strongly competitive even under condition with low phosphate concentration). The discussion could be further improved with explaining the October 2005 event given the phytoplankton composition information from well-understood events clearly presented as a background (What about the December events the authors present in Figures 6c and 6d?).

Unfortunately, it is most probably not straight forward to get now satellite information on diatom over the considered period of time. To my knowledge, there is no algorithm regionally developed for retrieving phytoplankton groups in the Black Sea. The global algorithm OC-PFT by Hirata et 2011 (further developed by Soppa et al. 2014) could provide information on diatoms but not necessarily suits the Black Sea. It could be one of the sources of the additional information on diatoms. However, it is worth remembering that the quality of the retrievals depends also on the quality of total Chl retrievals. There is a diatom (among other phytoplankton group) product based on the study by Xi et al. (2020) and available from
https://resources.marine.copernicus.eu/?option=com_csw&view=details&product_id=OCEANCOLOUR_GLO_CHL_L4_REP_OBSERVATIONS_009_082
Please check with respect to the Black Sea, original Xi et al. (2020) study did not present this basin.

Hirata, T., Hardman-Mountford, N. J., Brewin, R. J. W., Aiken, J., Barlow, R., Suzuki, K., Isada, T., Howell, E., Hashioka, T., Noguchi-Aita, M., and Yamanaka, Y.: Synoptic relationships between surface Chlorophyll-a and diagnostic pigments specific to phytoplankton functional types, Biogeosciences, 8, 311–327, https://doi.org/10.5194/bg-8-311-2011, 2011.

Soppa, M. A., Hirata, T., Silva, B., Dinter, T., Peeken, I., Wiegmann, S., and Bracher, A.: Global Retrieval of Diatom Abundance Based on Phytoplankton Pigments and Satellite Data, Remote Sensing, 6, 10 089–10 106, https://doi.org/10.3390/rs61010089, 2014.

Xi, H., Losa, S.N., Mangin, A., Soppa, M.A., Garnesson, P., Demaria, J., Liu, Y., d'Andon, O.H.F., Bracher, A., 2020.Global retrieval of phytoplankton functional types based on empirical orthogonal functions using CMEMS GlobColour merged products and further extension to OLCI data, *Remote Sensing of Environment*, 240, 111704. doi:10.1016/j.rse.2020.111704

3. In the conclusion, additionally, could be a highlight on why the identified physical processes induce the bloom of coccolithophores not other phytoplankton groups over the considered period of time.

Minor comments

line 58: introduce backscattering coefficient b_{bp} and and coccolithophores cell concentration N.

line 163 -167: very nice description of the Danube plume distribution (any relation to CDOM and TSM?)

line 230, 236: - nutrient rich conditions are not necessarily required for coccolithophores

Line 297: “low nitrogen and high phosphate conditions” also low phosphate conditions

Line 297, 305 and 306: “low nitrogen and high phosphate conditions”, “relatively high phosphate“ generally, coccolithophores has high affinity for phosphate, can grow in low phosphate conditions. The main precondition is that as the authors wrote there are still nutrients not consumed by others (diatoms) and grazing presser on other phytoplankton groups.

Line 307: “Second..” combine with previous paragraph, “Third, …” as well.

Line 552-554: belongs to introduction.

Typos

line 38: please mind space after “Kubrykov” -> “Kubrykov et al. 2019a”

line 59: delete comma after Kubryakov

Line 290: please replace “that” by “than”