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Biogeosciences Discussions

Interactive comment on "Modelled interannual variability of vertical organic matter export related to phytoplankton bloom dynamics – a case-study for the NW Mediterranean Sea" *by* R. Bernardello et al.

Anonymous Referee #1

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GENERAL COMMENTS

In the manuscript, the authors validate a coupled physical-biological ocean model against observations and then use it to investigate the processes controlling interannual variability in the spring bloom and export of organic matter in the NW Mediterranean. The paper addresses a question of fundamental importance in biological oceanography and present very interesting results that apply not only for the Mediterranean, but for other regions of the world's oceans. I find it particular interesting how the authors explain the decoupling between the variability in the phytoplankton bloom

and export of organic of organic matter. The manuscript is well organized and the writing is clear. The authors clearly state their hypothesis, and their analysis support their conclusions. More detailed comments follow below.

SPECIFIC COMMENTS

This is just a suggestion. I think the authors should change the title of the manuscript to something more specific that highlights the paper's findings like "Factors controlling interannual variability of vertical organic matter export and phytoplankton bloom dynamics - a numerical study for the NW Mediterranean Sea". The current title does not reflect the interesting results shown in the manuscript.

The Introduction could be improved by shortening it and making it more focused on the core ideas of the paper, namely: (1) blooms play an important role in the vertical flux of organic matter, (2) blooms show considerable variability in time, space and intensity and (3) that variability is related to physical processes that affect vertical mixing. In its present form, the authors relate various results from studies in the North Atlantic and Mediterranean and explain basic ideas (Sverdrup hypothesis) in biological oceanography, which sometimes leaves the reader wondering where they are going with this.

In the last paragraph on page 9100 and the beginning of page 9101, the text refers to wind speed as WS but Table 1 uses WSP. The text and Table should use the same notation.

In the beginning of page 9105, I believe the units for the model estimate of export flux is "mg" and not "g" as stated ($\sim$40 gC/m^2/d). If not the model estimate is over 1000 times higher than observations.

RECOMMENDATION

In summary, I think this manuscript will be a valuable addition to the literature and I recommend it to be accepted for publication after minor revisions (see above).

Interactive comment on Biogeosciences Discuss., 9, 9091, 2012.