

Interactive comment on “On the impact of the Bimodal Oscillating System (BIOS) on the biogeochemistry and biology of the Adriatic and Ionian Seas (Eastern Mediterranean)” by G. Civitarese et al.

Anonymous Referee #1

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General Comments

The paper “On the impact of the Bimodal Oscillating System (BIOS) on the biogeochemistry and biology of the Adriatic and Ionian Seas (Eastern Mediterranean)” presents an excellent analysis of the impact of the physical mechanism named “BIOS” on the biogeochemical and biological dynamic of the Adriatic and Ionian Seas (two Mediterranean sub-basins, connected by a narrow strait).

Based on an impressive, 20 years long, database of physical and biogeochemical

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observations, the authors revisited the relationship between salinity and nutrients in the Adriatic (the “Adriatic ingression”, which is considered a paradigm). They demonstrated that the BIOS mechanism (then the internal dynamic of the two sub-basins) drives the observed coupled variability of the sub-surface salinity and nutrients data in the Adriatic. Moreover, a review of biological observations is presented to confirm and reinforce the proposed hypothesis, as the presence or not of eastern Mediterranean species in the Adriatic Sea could be likely related to the different phases of the BIOS mechanism.

I really appreciate the paper, as it shows an excellent “mix” of different observations, in support to a complex but realistic hypothesis. I support the publication of this paper (see later for “minor corrections”). I have just two comments that I would like to discuss with the authors, to verify if any additional material could be integrated the paper. I will leave it to the authors to decide whether or not to integrate my questions (and their answers) into the manuscript. However, if they choose not to, I would really appreciate a public discussion on these topics on the “Biogeosciences Discussion”.

Discussion

I agree with the explanation proposed by the authors on the BIOS impact on primary production in the Ionian Sea. Less clear for me is the picture proposed by the authors for the Adriatic Sea. This area, and specifically its southern part, is the site of the most important bloom of the eastern basin. I’m convinced that authors have a well defined idea of the interplay between BIOS, convection, nutrients distribution and primary production in the area, and I would like to have more information about their “vision” of the mechanisms in play.

As the authors stated (figure 4), in the anti-cyclonic (cyclonic) phase of the BIOS more (less) nutrients are injected in the South Adriatic. On the other hand, in this phase, surface and sub-surface waters with lower (higher) salinity are also injected, and convection in the area should be reduced (increased). The authors state, “new produc-

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tion of the South Adriatic is mainly determined by the open-sea convection". In other words, if I understand this correctly, even if the nutrient concentration in the surface and sub-surface layers could be high (or more elevated than average), it is the strength of convection, as well as the number of mixing/restratification events, that determines the intensity of the bloom. Thus, as convection is enhanced in the cyclonic phase, primary production in the SA should be more phased with salinity than on nutrients. Consequently, nutrients and primary production should be in opposite phase.

So, my concerns are:

1. Why this conceptual picture is not explicitly indicated in the paper??? The authors have limited the discussion on the topic, only stating that "new production of the SA is mainly determined by the open-sea convection" and that "the upper layer buoyancy content, as determined by the BIOS mechanism, plays a crucial role in the winter convection and in the biological production". Data on primary production are probably not available, or they are available at very low spatial resolution (but see point 3). However, the authors could present some "speculative" hypotheses.

2. No indications about primary production are given in figure 4. In my opinion, the opposite behaviors of the nutrients and of primary production, driven by BIOS, which are totally unexpected, should be at least discussed. Or not??

3. In fact, why didn't the authors use ocean color satellite images to verify the point 1?? I agree that the production of a time series from a satellite archive is not trivial. The most widely used method (i.e. extract a box for each time step, average data, plot time series) is strongly dependent of the position and size of the box and of the number of good data accepted to compute average in the box. Additionally, when you search data on a time series evolution of a dynamic feature (i.e. SA bloom) the exercise is still more complicated (you can observe a thick and its opposite just moving box of 2-3 pixels). Nevertheless, satellite ocean color data are an invaluable tool and, in your specific case, they could really help in dissecting the mechanisms relating BIOS -> nuts

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Minor comments:

Pag. 6973 Line 20: A more accurate plan of the paper should improve the readability of the manuscript.

Pag. 6974, Line 8: Define here nitracline, as in figure 3.

Pag. 6975: Line 9 A good paper to cite on this subject is Stratford K, Haines K (2002) "Modelling nutrient cycling during the eastern Mediterranean transient event 1987-1995 and beyond". Geophysical Research Letters, 29, 10102-10106

Pag. 6977, Line 24: This sentence is not too clear. Maybe rephrase.

Pag. 6993, Figure 2: Could you indicate on the figure the years of observed deep convection in the South Adriatic area?

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