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Comment on bg-2022-86

Anonymous Referee #1

Referee comment on "Interannual variability of the initiation of the phytoplankton growing period in two French coastal ecosystems" by Coline Poppeschi et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-86-RC1>, 2022

In this manuscript the decadal years variability of the IPGP (Initiation of the Phytoplankton Growing Period) is analysed at two coastal stations located in the northern (Iroise Sea) and eastern Bay of Biscay (Bay of Vilaine). The phytoplankton biomass is related to fluorescence measured by instrumented buoys. The sensitivity analysis for identifying the major causes of the variability is made through a 1-D biogeochemical model applied to the year 2015. The major result of this work is that, despite different environments, the IBGP days are very similar at the two stations. No significative trend in the IPGP is observed in the time series. However the variability is high and seems to show an earlier IBGP in the middle of the period -around year 2010- (at about day 60 against day 90 for the beginning and the end of the period). The results are interesting but this manuscript presents some flaws. The data used are fluorescence-derived Chlorophyll-a, the identification of the factors influencing the IPGP is made through a model and the conclusions are evasive. On the first point, auxiliary analysed Chl-a concentrations collected bimonthly (which is a large interval for this purpose) corroborate nevertheless the IBGP derived from fluorescence data. The main issue comes therefore from the discussion based on model results. The model is considered as perfect and the causes of the variability are discussed from its outputs. From the introduction to the conclusion and throughout the discussion the real issues of the IBGP have not been considered with sufficient care. Blooms at local stations in river plumes may or may not occur, the true question is what happens at large scale? What is the connection with the dynamics of phytoplankton in the whole area? We know since the end of the 90's that strong early blooms within the Gironde plume may consume a large part of the winter Phosphorus stock at the beginning of March or even earlier. This has been attested also by satellite observations at broader scale. There is in the understanding of these late winter blooms (between day 50 and 90) a critical issue for identifying the "major cause" of the "major disturbance" of the biological environment over the continental shelf of the Bay of Biscay. For the initiation of early offshore blooms, the light is the prevailing factor, not the SST; hence a verification of the critical depth hypothesis as formalized by Sverdrup. Although criticized with real arguments, this simple theory may be locally verified in the bay of Biscay. The blooms occur in the clear, relatively cold, and stratified waters of the outer

river plumes (Loire, Vilaine, Gironde). These blooms, sometimes very strong, have a high impact in biology as they provide foods for benthos at the end of winter and they consume a large part of the phosphorus stock in the surface layer with consequences in the phytoplankton size. As the concentration of Phosphorus in the rivers has been declining at high pace for these last twenty years, these blooms could have a stronger impact in the future. My feeling is therefore that this study presents some interesting results but they have to be considered as a very local representation of much larger dynamics. Considering these time series at the stations together with satellite data of chlorophyll-a and a 2 or 3-D model appear to be the next steps to propose for future investigations. A better consideration of the atmospheric environment would also benefit the understanding of these late winter blooms as anticyclonic conditions associated to high solar irradiance and low wind (hence lower turbidity) generally prevail at the onset of the late winter blooms in the Bay of Biscay.

Specific comments: My general comments have implications in the abstract, the introduction, the discussion and the conclusion.

Abstract: "The use of a one-dimensional vertical model coupling hydrodynamics, biogeochemistry and sediment dynamics shows that the IPGP is generally dependent on the interaction between several drivers. Interannual changes are therefore not associated with a unique driver (such as increasing sea surface temperature)." Nobody would dare say that temperature is the unique driver of the IPGP. "Extreme event also impact the IPGP". Obvious but how is it quantified in the text? Not useful mentioning it.

Introduction: "Moreover, theories proposed for the open oceans are not relevant in coastal zones." Really? Discussion Extreme events: "In coastal stratified regions (e.g. under the influence of river plumes), strong wind and tidal mixing can enhance the mixing and break down stratification. Such conditions can also enhance phytoplankton production (Joordens et al., 2001). During the IPGP, except during floods, both regions are weakly stratified and are then less sensitive to combined wind/tidal short events." Not useful. In fact the stratification acts positively for initiating blooms in coastal and open waters at the end of winter.

Conclusion: You could imagine something of higher ambition than adding horizontal advection in the model ...!

Figure 7: From the legend we are looking for the Chl curve. It would be better to change the legend for something similar to "Day at the IPGP and environmental drivers: . Illustrations in 2011, 2013 and 2014."