

Report #1

accepted but with better editing work

We thank the reviewer and we agreed that this article can be improved. All authors read it again with great attention to the English, and the writing and modifications have been taken into account.

Main manuscript modifications are highlighted in red.

Report #2

I have not seen the first version of this manuscript. The rebuttal letter and changes to the text show – in my opinion – that the remarks of the reviewers have been sufficiently addressed. Nevertheless, some responses can still be improved. I also have some additional remarks (see below), but I consider these to be minor remarks.

We thank the reviewer for his/her constructive review. We considered each point below.

Main manuscript modifications are highlighted in red.

Referee 1 asked how these local changes are connected to the dynamics in the whole area (Bay of Biscay). While it is now clearly stated in the objectives that the study concerns local changes in IPGP, it is still not clear how your findings relate to variability in the offshore (open ocean/shelf) environment. I believe that the dynamics in the Bay of Biscay are well-described, so maybe a short description of this can be given in the introduction, and it can be even more clearly stated in the discussion how the local dynamics relate (rather absence of relation) to the offshore dynamics.

To consider this comment, we better described the dynamics in the Bay of Biscay and how local processes are related to the offshore dynamics in the “Study area” section:

Our study focuses on two northwestern French coastal temperate ecosystems located in the Bay of Biscay, the Bay of Brest and the Bay of Vilaine, two ecosystems impacted by excessive nutrient inputs from watersheds, but exposed to different hydrodynamic conditions.

The Bay of Biscay is a region with a complex system of coastal currents influenced by the combined effects of seasonal wind regimes and important river discharges modulated by large-scale gyre circulation patterns (Ferrer *et al.*, 2009; Lazure and Jégou, 1998; Lazure *et al.*, 2006; Isemer and Hasse, 1985; Pingree and Le Cann, 1989, 1990; Le Boyer *et al.*, 2013; Lazure *et al.*, 2006; Charria *et al.*, 2013). In the Iroise Sea, at spring tide close to the islands and capes, tidal currents can reach 4 m s^{-1} (Muller *et al.*, 2010). This tidal circulation combined with meteorological forcings and sharp thermal gradients generate a strongly variable local circulation. In the vicinity of the Loire estuary, the freshwater discharges in the surface layers induce important density gradients driving a poleward circulation (about 10 cm s^{-1}) modulated by wind forcings (Lazure and Jégou, 1998; Lazure *et al.*, 2006). The river plumes can propagate under specific conditions towards the South-West.

In the discussion, we introduce those sentences to more clearly state how the local dynamics relates to the offshore dynamics:

The IPGP appears to be more controlled by local environmental drivers than by regional environmental drivers, the IPGP being earlier in one site than in the other during half of the studied years: for example, the 2012 IPGP is early in the Bay of Vilaine (day 53), but late in the Bay of Brest (day 80), related to strong wind activity and low PAR on the last bay. The offshore regional dynamics will induce limited impacts on local hydrodynamical features that will change IPGP.

In the rebuttal to Referee 2, it is argued that satellite images are not available (with reference to a figure below, on p. 12 of rebuttal). But on the basis of this figure, I would conclude that suitable images are indeed available, as the images show clear weather conditions and the two coastal bays seem to harbour enough high-resolution signal.

While we agree that satellite data are available for this region, we would also like to point out that the spatio-temporal resolution of the currently available products (even if we can catch interesting patterns, they remain truncated due to cloud coverage. Furthermore, the temporal resolution remains low) is not suitable for our study, and more specifically for the study of *in situ* data at high acquisition frequency. Therefore, we do not prefer to add satellite data that we do not find relevant to the issue addressed in our paper, which would make our analyses more complex or even biased.

On p 13 of the rebuttal: is it not strange, given the low Si:N ratios in both systems, that diatoms then dominate the phytoplankton? (cf the argument of the reviewer)

The Si:N ratios in both systems are indeed quite low before the IPGP. But, this ratio, combined with a high N:P ratio, is characteristic of ecosystems subject to high winter nitrogen fluxes. As explained p. 9 of the rebuttal, the median silicate concentration (Figure 1 of the rebuttal, respectively 38.1 and 8.1 $\mu\text{mol/L}$ in the Bay of Vilaine and the Bay of Brest) is highly above the half-saturation constant required for their assimilation by diatoms ($K_s = 2 \mu\text{mol/L}$, Del Amo and Brzezinski 1999). The phytoplankton growth is neither limited by the orthophosphate concentration (median concentration respectively equal to 0,8 and 0,4 $\mu\text{mol/L}$ in the Bay of Vilaine and the Bay of Brest). It is therefore not surprising to have a phytoplankton population dominated by diatoms before the IPGP. The nutrient concentrations have been added in Table 4.

In addition, I have also read through the revised manuscript and have some additional comments. I think that overall the language could be improved. In some instances, it is not clear to me what is meant, and these parts should be rephrased:

To consider this comment, the revised version of the paper has been carefully revised to improve the grammar and readability. As suggested by Reviewer 3, we rewrote the following sentences:

- Line 15 – ‘... the effect of climate-induced changes on...’

We modified the sentence, as follows: “... **climate-induced impacts on** ...”

- Lines 19-21 – this part of the sentence is confusing: ‘...available light depending from solar radiation, chlorophyll concentration and turbidity - sea temperature - turbulence driven by currents, wind direction and intensity and tidal mixing - nutrients from river flow.’ Why use the hyphens? It is also not clear to me why chlorophyll a is listed as an environmental factor potentially influencing variation in IPGP, as chl a is part of the IPGP.

The referee is right. In the revised version of the manuscript, we now say: “**In both coastal ecosystems, we observed a large interannual variation in IPGP influenced by sea temperature,**”

river inputs, light availability (modulated by solar radiation and water turbidity), and turbulent mixing generated by tidal currents, wind stress and river runoff.”

• Line 37 – what do you mean with ‘determined with specific scales’?

To clarify this sentence, we modified: “No consensus emerges among these hypotheses - especially because most of these concepts have been defined at specific temporal and spatial scales (Caracciolo *et al.*, 2021; Chiswell *et al.*, 2015) - and the debate is still open, in particular due to the use of more efficient models, the availability of new observations, and the ensuing collection of large *in situ* datasets (Boss and Behrenfeld, 2010; Rumyantseva *et al.*, 2019).”

• Line 50: ‘The variability of IPGP’ ◇ ‘Variation in IPGP ...’

We agree with the reviewer. We directly replaced: “Temporal variation in IPGP [...]”.

• Line 59 – what do you mean with ‘land-based transfers’? Transfers of what?,

We deleted this sentence because it was repetitive with the sentence just before "continental erosion".

• Line 94-95 – how does the macrotidal regime prevent the formation of green tides or the shift from diatoms to non-siliceous plankton?

We thank the reviewer for this comment. We now better explain this part in the revised version of the manuscript. We now say: “Due to the macrotidal regime, associated with a strong vertical mixing, the high nitrate concentrations do not generate important green tides (Le Pape *et al.*, 1997). Strong decreases in the Si:N and Si:P ratios did not exhibit dramatic phytoplankton community shifts from diatoms to non-siliceous species in spring (Del Amo *et al.*, 1997) because of the high Si recycling (Ragueneau *et al.*, 2002; Beucher *et al.*, 2004).”

• Line 96 – ‘according to’ = because of ?

We changed, as follows: “[...] because of [...]”.

• Line 120 – ‘every 20 and 60 minutes’ – do you mean that one buoy measures every 20 mins and the other one every hour

This is correct. We clarified the sentence in the revision: “Environmental parameters (SST, salinity, turbidity, dissolved oxygen and Chl-*a* fluorescence) are measured at 1 to 2 m below the surface every 20 minutes (COAST-HF-Iroise) or every hour (COAST-HF-Molit).”

• Lines 216-217 – I don’t understand what you mean with ‘too late IPGP’, ‘too early IPGP’, ...? Please rephrase.

We modify this sentence by: “[...] too late (method 1) or a too early (method 2) detection [...]”.

• Lines 359-360 – ‘In the Bay of Brest...’ I don’t understand this sentence nor how it is related to the previous sentence.

The sentence is now: “Due to the most unfavorable conditions, the IPGP occurs 9 days and 64 days later in the Bay of Brest and the Bay of Vilaine, respectively.”

• Line 549 – what do you mean with ‘distributing phytoplankton’? Vertical mixing and disruption of chlorophyll maxima?

To be clearer, we modified: “ [...] does not favor phytoplankton growth”.

In the introduction you mention three main theories to explain how blooms initiate. Would it be possible to very briefly explain these three hypotheses and how they differ? Also, you don’t come back to these in the discussion. Is this because these are not relevant in the context of shallow, well-mixed coastal systems under the influence of river plumes? It may be good to briefly address this issue.

We agree and we take into account the comment from the reviewer by explaining the three hypothesis in the introduction part and also by considering back those theories in the discussion part:

In the ‘Introduction’ section, we explained: “For Sverdrup (1953), phytoplankton blooms occur when surface mixed layer shoals to a depth shallower than the critical depth, according to light conditions. While Huisman *et al.* (1999) agreed with Sverdrup (1953), he proposed that relaxation of turbulent mixing allows bloom to develop if it occurs below a critical turbulence rate. Behrenfel (2010) observed blooms occurring in the absence of spring mixed layer shoaling, and declared that the initiation of bloom is controlled by a balance between phytoplankton growth and grazing rate, and suggested a seasonal control of this balance by physical processes.”

In the ‘Discussion’ section, we addressed the issue as follows: “The main theories to explain the initiation of phytoplankton blooms (Sverdrup, 1953; Huisman *et al.*, 1999; Banse, 1994) are not relevant in the context of shallow, well-mixed coastal waters under the influence of river plumes. In our studied region, the ecosystem does not evolve with mixed layer dynamics, as observed in deeper environments. Both bays are permanently vertically mixed mainly by tides, and vertical stratification only occurs on a thin surface layer due to river runoffs at short time scales.”