

Authorship: A new author has been introduced in the revised version of the manuscript. This is irregular and should be justified. This new author is not mentioned in the author contribution section.

The description of field work is incomplete. At the beginning of section 2, when describing the sampling area, the authors must also indicate the sampling dates for all data used in the article. If the sampling took place as part of wider programme, this must be indicated, together with references to published studies that report on other properties of the system during the same study. It would be helpful to have a table with sampling location, station names, and sampling date, instead of including part of this information in the figure legends. This would be particularly helpful to understand the sampling schedule during the time series experiments. The bottle experiments on-deck must also be better explained.

Title: The title states that sequential nutrient uptake maintains high productivity and a balanced nutrient content of phytoplankton, but the validity of these statements is not actually proven by the data. Strictly speaking, productivity (e.g. net phytoplankton biomass accumulation) has not been measured here, nor has the phytoplankton elemental composition. The title should be re-written to make it clear that this is a mechanism that is being proposed (and which certainly is consistent with some of the observations), but not a mechanism that has been observed. I suggest including in the title a phrase along the lines: 'Sequential nutrient uptake as a potential mechanism for phytoplankton to maintain...'

A similar comment can be made in relation to the sentence on lines 271-272.

Similarly, the linkage between sequential nutrient uptake and the maintenance of phytoplankton stoichiometry near Redfield values (C:N around 7), which the authors make in the last section of the Discussion (lines 366-370), is tenuous at best, given that C:N ratios in particulate matter do not reflect phytoplankton elemental composition alone. These limitations should be explicitly acknowledged.

Specific comments

L 29 Insert: 'According to this hypothesis...' (to clarify this is not yet a result)

L31 Re-write: 'These processes would result in...'

L38-39 Sentence is awkward, as it seems to refer to vertical profiles at the nutricline. Please re-write.

Line 44 Remove 'and'. The phrase 'subject to the homeostatic stoichiometry' is vague and may be confusing. In fact the work highlights the stoichiometric plasticity of phytoplankton, rather than its fine regulation.

Line 47. This second part of the sentence is incorrect: there are many studies showing the results of phytoplankton to natural nutrient pulses supplied by mixing. See for instance Glover et al J Plankton Res (2007) 29 (3): 263-274 for an open-ocean example and also the works of Jonathan Sharples and colleagues for shelf-sea examples.

L57 Recently

L58 Remove 'in these waters'

L69 re-write '...with low C:P and N:P ratios'

L81-82 Re-write to make it clear that this assessment, albeit difficult, is not impossible. There are a few examples of direct measurements of elemental ratios in situ both for bacterio- and phytoplankton. See Segura et al Plos One 2016 for a recent example and relevant references: <http://dx.doi.org/10.1371/journal.pone.0154050>

L105-105 Related to comment above regarding line 47, here the authors need to be careful when referring to 'nutritional status', which in this study is inferred but never measured, since there are no measurements of phytoplankton elemental ratios. Data of elemental ratios of suspended organic matter (which in any event are difficult to interpret in this context, because of the unknown influence of non-phytoplankton material) are reported in Fig. 10, but they were not obtained during the time-course experiments. The authors need to acknowledge the limitations in their approach, as they are inferring phytoplankton nutrient content (and hence nutritional status) from observations of nutrient concentration in seawater, but the latter can be affected by many other processes in addition to phytoplankton uptake alone.

L115-116 re-write: reaching a daily production of up to xxx and an annual production of up to xxx

Line 188 Indicate bottles were maintained on-deck.

L 186-187 Actual light attenuation percentages should be indicated.

L 245 Should be 0.0, not 0:0 (which suggests that both nutrients were exhausted)

Legend to Fig. 9: The labels +N/+P and +N/+Si are confusing. If, as the legend indicates, they represent the ratio of added N over added P or added N over added Si, why should they change over time? Those ratios refer to the initial nutrient amendment but once the experiment is proceeding, the only ratios one can measure are the actual nutrient ratios in the bottles (indicated by the other 2 lines). So what do the data labeled +N/+P represent?

Section 2.4 explains the experiment described by figure 7 (in this experiment, all nutrients were added together), but not the experiment described by figure 9, in which multiple treatments were used (including additions of single nutrients). This experiment should be described in the Methods section, and in particular the concentrations of each added nutrient should be indicated.

Lines 260, 264. To avoid confusion, clarify whether these ratios refer to ambient or uptake ratios. In fact, this applies to the manuscript in general, it should always be specified whether nutrients or nutrient ratios refer to ambient concentrations, uptake, or inferred phytoplankton composition.

Line 260-262. This is difficult to follow. It is stated that 'The N:P ratio decreased faster after a single addition of N or P alone than with additions of N and P together (Fig. 9-3)', but in Fig. 9-3

the treatments which had only added N or added P are not shown. This again goes back to the problem that it is uncertain what the treatment labeled +N/+P refers to.

lines 276-277. Nutrient recycling should also be mentioned here. In particular, P is recycled much faster than N, which in turn is recycled faster than Si.

lines 366-370. As the authors know, elemental ratios of suspended organic matter are affected by the presence of non-phytoplankton material, such as detritus and heterotrophic bacteria. The contribution of these non-phytoplankton components to total POC and PON stocks can change rapidly and is quite difficult to ascertain. In this section, the authors should acknowledge this fact. The observation that C:N of suspended matter is close to Redfield would also be consistent with a high (non-Redfield) C:N in phytoplankton in combination with a substantial contribution of bacterial biomass with a low C:N ratio. The statement 'This demonstrates the lack of ambient nitrogen limitation on the cellular nutrient stoichiometry' is not warranted, as it is based on the assumption that the POC:PON ratios reflect solely the contribution of phytoplankton.