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Interactive comment on “Eutrophication mitigation in rivers: 30 years of trends and seasonality changes in biogeochemistry of the Loire River (1980–2012)” by C. Minaudo et al.

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We are grateful for comments and suggestions provided by anonymous referee # 1. This helps to improve significantly the manuscript. The following responds to all raised issues. You will also find as a Supplement file a marked-up manuscript version highlighting all the changes we made.

Main Comments Referee Comment (RC): The MS deals with long-term changes that have occurred in the River Loire (France), with emphasis on macronutrient concentrations and phytoplankton development. As a result of substantial decrease in P loading, chlorophyll a has declined over recent years, and that has affected variations of dis-

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solved oxygen, pH and nitrate, at different time scales. The paper has some merit, as such long data sets spanning several decades allow to explore trends independent on natural variability of hydrological processes among years.

RC: The intro is marginally OK, but fails to properly address factors that control phytoplankton development in rivers, which are key to understand the effect of eutrophication and other anthropogenic changes. Several syntheses have highlighted the control by hydrology and other physical factors that are major constraints on potamoplankton dynamics, and which tend to lessen the role of control of phytoplankton growth by nutrients in rivers. It also fails to capture the characteristics of a relatively unregulated river like the R. Loire.

Authors Comment (AC): We understand the referee's point of view and decided to add some elements in the Introduction part, based on several papers such as Reynolds et al. 1994 ("Are phytoplankton dynamics in river so different from those in shallow lakes?"), Krogstad & Lovstad 1989 ("Erosion, phosphorus and phytoplankton response un rivers of South-Eastern Norway"), Istvanoviks & Honti 2012 ("Efficiency of nutrient management in controlling eutrophication of running waters in the Middle Danube Basin") and Reynolds and Descy 1996 ("The production, biomass and structure of phytoplankton in large rivers"). Section 2.1 already describes the Middle Loire geomorphology ("the Middle Loire favors phytoplankton development, its multiple channels with numerous vegetated islands slowing down flow velocity and the valleys becoming wider"), nevertheless we decided to add some of these elements in the introduction to point out the characteristics of the Loire River as a relatively unregulated river.

RC: It is likely that N uptake by phytoplankton had a minor influence in nitrate seasonal variation, which depended more on seasonal variation of inputs from soils, depending on leaching of bare soils by rainfall in winter, and retention by land vegetation in the growing season; for assessing the processes a complete N budget in the watershed and in the river would be needed; observations on concentrations in surface waters can only lead to hypotheses which need to be tested.

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AC: We totally agree. We made a few changes in section 5.1 to clarify our opinion on this point. The fact that phytoplankton had a minor influence on nitrate seasonal variations is now included in the Abstract and the Conclusions.

RC: The N:P molar ratio was calculated on N and P concentration in the water: it never can be used in infer N or P limitation : that leads to the wrong conclusion that the R. Loire "has always been P-limited" ; P-limitation can be assessed from measurement of sestonic (i.e. particulate) or total, not dissolved, nutrient concentrations that in most systems helps assessing phytoplankton nutrient status, on which the reasoning based on the Redfield ratio applies ; the conclusion is contradicted by the data on SRP concentration : phytoplankton limitation can't occur when at SRP concentrations at $\sim 200 \mu\text{g/L}$; the authors should consider that light limitation of phytoplankton as more likely when SRP levels were high and that, given the high dissolved N concentration, N could never be limiting or even co-limiting ; P limitation of phytoplankton growth has indeed appeared as a result of P reduction measures in the Loire basin (see for instance Oudin et al, 2009 and Descy et al 2011).

AC: Although we found several references using DIN:DIP ratios for determining which among N and P is the limiting nutrient in different large rivers (Elbo, Rhone, Danube, Mississippi), we understand that it is not appropriate. As highlighted in the literature on the subject (Reynolds 1992, "What Vollenweider couldn't tell us"), the usefulness of citing nutrient ratios probably lies in determining which, if at all, is likely to become limiting during the phytoplankton increase phase.

We unfortunately don't have any data from measurements of sestonic N and P during the period 1980-2012, but to make sure we present in our paper the right evolution of the N:P ratio, we re-calculated this ratio with $N_{\text{tot}} = \text{NO}_3 + \text{NO}_2 + \text{NKj}$ and P_{tot} . Thus, we add a couple lines in the Methods section to explain how the N:P ratio was calculated. The results constitute the new version of Figure 5, which can be seen at the bottom of this document.

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These results indicate that P-limitation of phytoplankton growth has become a very significant factor. When the river hydrology remains stable in the summer, phytoplankton is potentially under P-limitation, suggesting an explanation for the apparent shift in seasonal phases of Chl. a concentrations (late summer blooms no longer occur, described in section 3.2).

We also corrected our interpretation in the Discussion and changed “P is the limiting factor” to “P is a potential limiting factor” as we totally agree other factors are controlling phytoplankton development, such as light penetration in the water column, water temperature, flow velocity, clam filtration or zooplankton grazing. These changes are also included in the Abstract and the Conclusions.

RC: Nutrient uptake calculations : inferring uptake from chl_a variations between sites is quite rough if not incorrect ; what that shows is a difference of biomass, not of gross production or growth rate, on which depends nutrient demand ; moreover, using simple calculation based on the Redfield stoichiometry is rough, as nutrient uptake depends on the cells nutrient status, regulated by utilisation of the nutrient cell content (i.e. the Droop model) ; again, more sophisticated calculation of phytoplankton growth – not increase – and cell quota would be necessary to estimate nutrient uptake ; hence the hypotheses proposed to explain the low “ nitrate loss“ aren’t necessary ; again a complete nutrient budget would be needed here to understand the variations of concentrations in the river (not mentioning other sources of DIN).

AC: We understand that this calculation appears too rough and do realize it wasn’t taking into account other significant processes (sedimentation, grazing by zooplankton and also the time required for cell division). Thus, we decided to delete from our paper these results and the associated discussion and hypotheses on the nitrate loss as we don’t have any data or biogeochemical model that would allow us to conduct more sophisticated calculations. As a consequence, Table 6 no longer exists.

Other Comments

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RC: Using the term “pigment” instead of chlorophyll a can be misleading; “pigment” can refer to any phytoplankton pigment; “chlorophyll a” should be used throughout the text.

AC: To avoid confusion, we decided to point out in the part 3.1 that the variable “Chl. a” also takes into account pheopigments. Thus, we changed “algal pigments” by “Chl. a” throughout the text, tables and figures.

RC: A synthesis on the Loire basin by Oudin et al. (2009), which already contains long-term data, should have been referred to

AC: We now refer to this synthesis.

RC: The acronym “AELB” that appears in 2.2, 3rd page, is not standard; the same is true for INSEE, same page

AC: AELB is now defined as the Loire Brittany river basin agency. INSEE is now defined as the French National Institute of Statistics and Economic Studies.

RC: The terms “algae” and “algal” should be replaced by “phytoplankton” and “phytoplanktonic”, as cyanobacteria are not algae, but prokaryotes

AC: OK, this is fixed.

RC: The division between seasons, although explained, 3.1, remains somewhat misleading

AC: We just decided to change “summer” and “winter” into italic fonts, to make sure the reader will consider the periods previously defined in the Methods part.

RC: §4.2 the term “production” may be inadequate; it is indeed likely that phytoplankton production began earlier but that photosynthetic rate was too low to compensate for respiration losses and that growth rate was too low to overcome dilution that occurred at high discharge (see e.g. Descy et al. 1987, Reynolds & Descy1996...) ; better to use “development”

AC: OK, we replaced “production” by “development”.

RC: p. 17313, line 15: delete “bacteria”: respiration of all organisms including phytoplankton was involved in pH decrease

AC: OK.

RC: p. 17316, line 20: what is “primary activity” ? probably “biological activity”, as all aquatic organisms including bacteria are involved

AC: OK.

RC: p. 17320, lines 15-17: the quotation is not fair. The publication by Descy et al (2011) aimed at simulating phytoplankton dynamics in a single year, and was based on an integrated model that included land use and point C and nutrient sources at the scale of the whole watershed, and it definitely included land use and non-point sources.

AC: Our sentence was apparently misleading; we changed it to “A potential numerical model of the Loire basin eutrophication should not only take into account climate and land-use changes, but also recent ecological changes (Descy et al., 2011; Pigneur et al., 2014)”.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/11/C8489/2015/bgd-11-C8489-2015-supplement.pdf>

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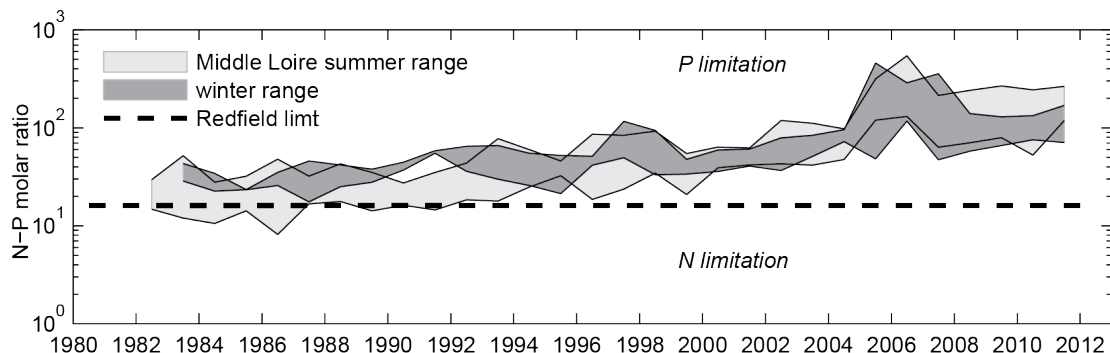


Figure 5. Variations of total nitrogen over total phosphorus molar ratios ranges during summer and winter in the Middle Loire (station 18) since 1980 and compared to the Redfield limit (dotted line). Each patch is composed at the bottom by the percentile 10% of the recorded data and percentile 90% at the top, and y-axis is logarithmic.

Fig. 1.

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