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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.

Anonymous Referee #1

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Review of the ms by Minaudo et al : Eutrophication mitigation in rivers . . . (Loire)

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 dissolved 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. However, the study also present a few significant drawbacks that should be addressed before publication. 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. Regarding the discussion and data interpretation my main comments are : - 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 - 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 Âń has always been P-limited Âż ; 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 μg/L ; the authors should consider that light limitation of phytoplankton was 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). - Nutrient uptake calculations : inferring uptake from chla 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 Âń nitrate loss Âż 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).

Other comments - Using the term Âń pigment Âż instead of chlorophyll a can be misleading ; Âń pigment Âż can refer to any phytoplankton pigment ; Âń chlorophyll a Âż should be used throughout the text - A synthesis on the Loire basin by Oudin et al. (2009), which already contains long-term data, should have been referred to - The acronym Âń AELB Âż that appears in 2.2, 3rd page, is not standard ; the same is true for INSEE, same page - The terms Âń algae Âż and Âń algal Âż should be replaced by Âń phytoplankton Âż and Âń phytoplanktonic Âż, as cyanobacteria are not algae, but prokaryotes - The division between seasons, altough explained, 3.1, remains somewhat misleading - §4.2 the term Âń production Âż 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 & Descy 1996 ...) ; better to use Âń development Âż - P17313, line 15 : delete Âń bacterial Âż : respiration of all organisms including phytoplankton was involved in pH decrease - P. 17316, line 20 : what is Âń primary activity Âż ? probably Âń biological activity Âż, as all aquatic organisms including bacteria are involved - 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.

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