

Interactive comment on “Interactions of local climatic, biotic and hydrogeochemical processes facilitate phosphorus dynamics along an Everglades forest-marsh gradient” by T. G. Troxler et al.

Anonymous Referee #1

Received and published: 4 July 2013

General comments:

This study intends to determine phosphorus transport (direction, fluxes) along a forest-marsh gradient and to explain this transport by geochemical factors (e.g. saturation indices), hydraulic patterns and a temporal trigger (precipitation). The concept of a transport controlling trigger has been previously suggested for semiarid and arid systems. The study therefore also tests the applicability of this concept for the investigated tree island. In a broader sense, these findings may help to explain the "tear drop" shape of Everglades' tree islands by transport of the limiting nutrient (phosphorus; P) from to-

C3246

pographically higher, P-rich tree island areas to lower oligotrophic marsh landscapes.

Hence, study objectives are within the scope of BG and the study tests a novel concept using a well-chosen hydro-geomorphological setting (a tree island characterized by exceptionally high P levels within an otherwise P-poor marsh landscape).

Two issues may limit the validity of the study: First, the investigation did not assess redox potentials despite considerable influence of the redox potential on P compositions in wetlands. Second, results do not provide direct evidence that P is actually the limiting nutrient in the oligotrophic marshes of the study area. Overall, these limitations may not call into question the study's conclusions. However, I recommend major revision due to substantial shortcomings of methodological descriptions. To me, these flaws largely limit the traceability of results, and the manuscript should be revised to meet these criteria (see specific comments).

Specific comments:

p.10292/l.20-p.10293/l.4: No information of replicate measurements is given for the various parameters analyzed.

p.10292/l.20: The core locations are not described despite the opportunity to include these in Figure 1. Core distributions among communities are listed only in Table 3 of the supplementary material. Yet, the text does not refer to this table. Core numbers per community given in the supplement do not fit to the piezometer and well distributions (Figure 1). Thus, core locations obviously do not match given piezometer and well locations and are therefore apparently not described.

p.10292/l.28: For this study, the fractionation method by Hieltjes and Lijklema has been modified (KCl instead of NH₄Cl). However, the rationale for this modification is not explained. Further, a brief description of the extraction conditions would be advisable for each pool despite citation of the corresponding articles.

p.10293/l.1: The NaOH extract was analyzed for "moderately resistant organic P

C3247

forms". A reference is missing, which verified the quantification of moderately resistant organic P by NaOH-Po. Although total organic P was not determined in this study, an estimate for the approximate content of NaOH-Po relative to total organic P would be useful (i.e. Is NaOH-Po generally a main fraction of total organic P or just a marginal part?). However, the main issue in that context is the apparently missing description of the method used for conversion of Po into Pi in order to determine NaOH-TP. For the determination of the overall TP content of the soil (given in Fig. 2A), acid-digested samples were used. Whether this method was also used for NaOH-TP is not described.

p.10293/l.4: For low residual P contents, suggestions for potential P forms of the residual pool are less important. However, residual P was by far the predominant P fraction at all investigated communities (Fig. 2A), and the entire text does not respond to this issue. As extraction conditions are not sufficiently described within the Methods section (e.g. concentrations of extractants), it is not instantly clear whether HCl-Pi and NaOH-Pi comprise most of the inorganic P pool and, consequently, whether residual P contains primarily organic or inorganic P.

p.10294/l.21-23: Which equipment/methods were used to determine these values (e.g. which electrodes)?

p.10295/l.5-8: As two different computer programs were used to determine saturation indices the rationale for selecting these programs should be explained. Further, it is necessary to document, which compounds were used to chemically define the system (e.g. for ion-pairing; probably some of the parameters given in Table 2). Moreover, selected apparent solubility products (with corresponding references) for each mineral and information about the calculation of activity coefficients (ionic strength correction) maybe required for traceability of those computer-based calculations.

p.10299/l.2-5: As redox potential has generally considerable influence on P compositions (see e.g. Hutchison and Hesterberg, 2004; Mortimer, 1971), the large water level differences between High Head and Wet Head may have caused some of the observed

C3248

geochemical differences (e.g. the low TP level at the Wet Head as compared to the High Head). Although redox potentials were not measured, it may be reasonable to discuss whether the redox state may have contributed to the differences between High Head and Wet Head or not.

Figure 6: What is meant by "no evidence of mineral exchange" or "minerals are absent from the shallow soils" (P-minerals or "non-carbonate minerals")?

p.10303/l.8-10: This implies that the "non-carbonate component" of the soils is just composed of Ca-bound P. However, other P pools maybe also part of the non-carbonate component. Likewise, the non-carbonate mineral content may comprise various other minerals (see Ross and Sah, 2011). It is described that "Ca-bound phosphate comprised approximately 50% of the soil by mass". This percentage seems to be extremely high. It is necessary to document the corresponding calculation.

p.10303/l.12: Bioapatite is structurally similar and frequently termed as HAP but shows also considerable chemical and structural differences (see Wopenka and Pasteris, 2005). When referring to a specific formula, probable substitutions and their potential effects for saturation indices should be mentioned (e.g. indicating saturation indices for other apatites). However, it is still advisable to use hydroxylapatite as a reference compound.

p.10305/l.26-27: The statement "strong potential for hydroxyapatite dissolution but potential for mineral precipitation" should be rephrased. Hydroxylapatite is also a mineral.

Table 2: Two columns of Table 2a are unnecessarily repeated in the supplementary material (Table 2a; some values differ slightly for some reason). Instead of repeating measured values, it would be useful to present raw data for all four seasons at least for key parameters such as TDP, SRP, pH, Ca and Cl. One of the main goals of this study is to validate that a temporal trigger (precipitation) explains fluxes of a limiting nutrient. Thus, it would be worthwhile to present temporal phosphorus distribution changes (as raw data in the supplementary material) despite presented flux calculations (Table 3).

C3249

Figure 6: To me, the explanation of this figure in the text is not sufficient (p.10302/l.24-26).

Entire MS: The usage of the same abbreviation for two different parameters is disadvantageous (P: Precipitation, e.g. Fig. 5; P: Phosphorus), especially when these parameters are correlated.

Technical corrections: p.10297/l.5: "P" is missing after "labile". p.10300/l.14: I guess TDP is meant here instead of TP because TP is quite low at the Wet Head (See Table 3).

References:

Wopenka, B., Pasteris, J.D., 2005. A mineralogical perspective on the apatite in bone. *Materials Science and Engineering* 25, 131-143.

Hutchison, K.J., Hesterberg, D., 2004. Dissolution of phosphate in a phosphorus-enriched ultisol as affected by microbial reduction. *Journal of Environmental Quality* 33, 1793-1802.

Mortimer, C.H., 1971. Chemical exchanges between sediments and water in Great Lakes - Speculations on probable regulatory mechanisms. *Limnology and Oceanography* 16, 387-404.

Interactive comment on *Biogeosciences Discuss.*, 10, 10287, 2013.