

Reviewer #2:

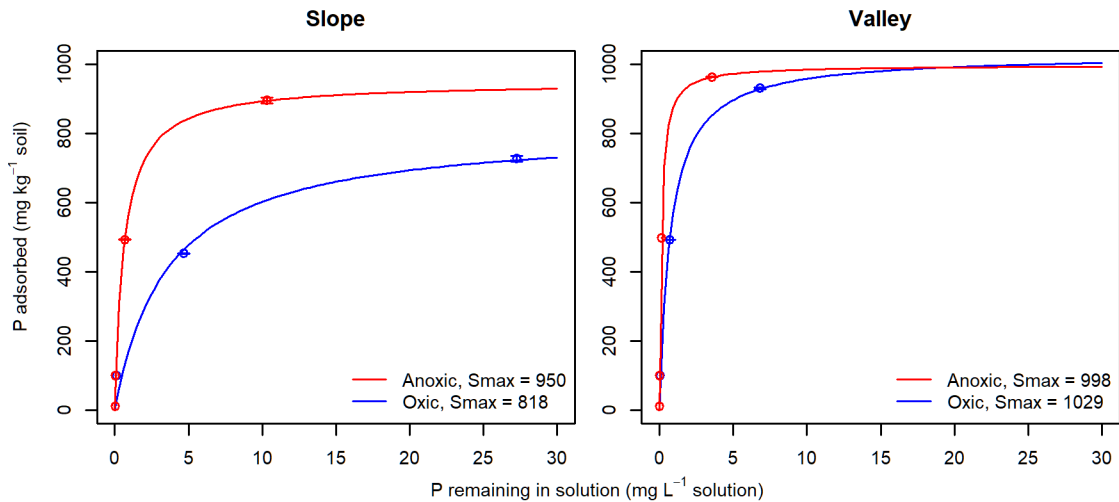
General Comments The manuscript "Anoxic conditions maintained high phosphorus sorption in humid tropical forest soils" describes an experiment using tropical soils from the Luquillo experimental station to assess the effect of anoxic conditions on the adsorption capacity, kinetics and P sorption strength in those soils. Two sets of soils, two topographical units (slope and valley) and two oxic conditions (anoxic and oxic) were used. The results are of great importance for the biogeochemistry community because: (1) few studies on the influence of anoxic conditions to the P sorption capacity/strength in tropical soils are published, (2) because it contradicts the established assumption that reducing conditions increase P availability. While I have no specific grammar corrections, the fluidity of the text should be revisited. Moreover, inconsistencies in the methods, results and discussion sections of the manuscript are visible. I would recommend the publication of the study, after the following major revisions.

We appreciate reviewer's comments. We improved the fluidity of the text during the revision. We also fixed the inconsistencies identified later by the reviewer, including the lack of background soil chemistry data (Table 1) and the inappropriate use of the term 'rate'. We made sure that the rate of P sorption was only used when discussing coefficient k of the P sorption curve (e.g., Page 9 Line 14).

Major comments Although the authors have executed a very interesting work, lack of information on the sample prior to analysis and subsequent analysis during the discussion are not well documented. The authors justify their chosen topographical units as inherent more oxic and less oxic sites yet, no information on the P content, FeOA, AIOA, FeHCl and AlHCl prior incubation is available in Table 1, while the reader is referred to several citations, the addition of this data to Table 1 would be very beneficial. Moreover, while the authors aim to compare the effect on anoxic conditions on the soil capacity to adsorb P no information on the soil specific surface area (SSA) before and after the experiment is available. Moreover, I would recommend the use of SSA for isotherm to analyze the P loading on the minerals (g P/m²) while adsorption occurred.

We added background soil information to Table 1, including concentrations of HCl-extractable Fe(II) and Fe(III) and AO-extractable Fe and Al. We agree with the reviewer that SSA or P loading data would be interesting, but were beyond the scope of this experiment. Given the high surface area of AO-extractable minerals, we consider their concentrations as a surrogate for SSA.

We also question the use of just 4 points in each isotherm, without lower values or zero, the lack of fit statistics (r², among others) and general lack of detail. Later the authors also rely in the lower values of PSI to analyze and compare their sorption curves, while they also refer to the PSI concentration as rate in page 6 line 19. The authors also mention several times the precipitation of viviatine, while no evidence more than the mention of a MINTEQ simulation to the reader. Although the information in in the supplementary section the reader is never referred to it. The details of this MINTEQ simulation are also omitted in the Methods section.



For the justification of no zero or low values, please see the response and additions mentioned above for Reviewer 1. We also included model efficiency as a measure of model fit (Table S1, Page 7 Lines 23-24). We only used four points in the isotherms because 1) our anoxic treatment was limited by the size of the glove box, and we already have a large number of samples ($n = 128$); 2) including low P concentrations did not significantly affect the shape of isotherm, as our preliminary trials showed that samples receiving 10 and 50 mg P kg⁻¹ had extremely low P concentration in solution (< 0.1 mg P L⁻¹). We also justified our focus on the PSI values calculated at 1000 mg P kg⁻¹, as this P concentration is comparable to past studies and facilitated comparison with P sorption time curves measured at the same P addition level (Page 6 Lines 29-31).

We avoided the inappropriate terms including ‘PSI rates’ and ‘P addition rates’. We justified the presence of vivianite using literature and also provided information on how MINTEQ simulation was conducted (Page 6 Line 16-22) and introduced the supplementary table in the main text.

At the results, the authors describe the P concentrations added as rates, this is extremely confusing as rates refer to a quantity over time. Which would be the kinetic data. The authors also continue to discuss data from figure 1 (adsorption isotherms) while comparing p-values for the different concentrations of the isotherm while no table or figure is mentioned. The data that this refers is supplementary table 1, where the author refers to the rates as levels, yet in the paragraph no mention to this supplementary table is made.

We no longer refer to P concentrations as rates and abandoned terms such as ‘PSI rates’ and ‘P addition rates’. Rate was only used when discussing coefficient k of the P sorption curve (Page 6 Line 35). We also added a supplementary table (Table S1) to show the P concentrations from the isotherms and indicate statistical significance.

In the discussion, the authors disregarded their kinetic and solubility data and support their discussion on the PSI and its correlation with AIOA and FeOA. The author does not seek to discuss the nuances a faster sorption rate at their simulated anoxic condition in the slope sites. On the other hand, the authors never discuss what minerals/solid phases could be the ones extracted by their HCl(FeII) and HCl(FeII) and how is this related to the higher P soption and rates in some soils. They base their conclusion on their solubility analysis yet this information is never related to the previous analysis. I would recommend the authors to discussing their results in comparison with the study “Sorption isotherms and kinetics of sediment phosphorus in a tropical reservoir” by Adhityan Appan, and Hong Wang; which is very similar to theirs.

We clarified our discussion of the results mentioned by the reviewer, including the kinetic and solubility data (Page 9 Line 13-30), faster sorption found under anoxic conditions (Page 9 Line 31 to Page 10 Line 12), and the phases of HCl-extractable Fe (Page 9 Line 33-35). We also discussed Adhityan and Appan 2000 in the text (Page 10 Line 24).

Minor revisions

Page 12 line 9: change The to their

The text was revised accordingly.

Page 12 line 10 change “due to the” to “do to the tropical soils”

We changed the text to ‘Due to the high P sorption capacity of tropical soils’.