Response to referee comments on: *On the potential vegetation feedbacks that enhance phosphorus availability - insights from a process-based model linking geological and ecological time scales*
By C. Buendía et al.

We thank Dr. Runyan for her technical corrections and for her deep reflections about the assumptions and implications of our model. She raised important points that needed to be clarified. Although it is beyond the scope of our manuscript to provide complete answers for all her questions we discuss her concerns here and in the revised manuscript.
In italics is what is now included in the revised manuscript.

1) RC: The model is initialized with the concentration of P in different lithological formations. Within a given lithological formation, is there any information pertaining to how much variation in P concentration there is? Does the concentration of P in a given lithological formation remain relatively constant over geologic time scales? Are there no processes that would have altered these concentrations substantially over geologic time scales? If it does not remain relatively constant, by initializing the model with current P concentrations, does this not reflect historical processes that have led to current day conditions?

AC: For this proof-of-concept paper we kept the analysis as simple as possible, therefore the model utilizes a coarse map for the lithological classes and just one value for the P concentration for each class. Newman (1995) provides a range for the concentration of P in parent material for two of these lithological classes. We are aware that more detailed lithological information and P content exist for some regions of the world and that we could have included those in our analysis as has been done by others (e.g., Yang et al. 2013). However, we were concerned that by including higher level of details in some regions (those that are well known and explored) while ignoring them in other regions could result in artificial patterns of P variability that are difficult to interpret. This would make it even more difficult to focus on the major aim of this (already very long) manuscript, which was to understand how uplift, lithology, climate and vegetation most probably interact, in a rather general sense. This also applies for potential historical changes in lithologies over geologic time scales. We are not aware of any approach that could be used to quantify these changes in a model. Because our study shows how lithology determines the transient and steady state results of our simulations, we agree with the reviewer that in future studies it would be meaningful and important to include sensitivities regarding lithological mineral composition and spatial patterns. To highlight the importance of lithology for our results, we introduced the following paragraph in the Summary and Conclusion section:

*Lithology determines the concentration of P in the primary minerals and also how fast material weathers, which is a result of the calcium reactions considered in the regolith model of Arens (2013). Therefore, including lithology in the model was necessary to simulate the observed differences in weathering rates and steady-state development in (for example) the Western and Eastern Amazon. However, for some lithological classes, variations in the P concentration have been reported (Newman 1995), and P concentrations also change through time. We have ignored these variations because such information is not available for all lithological classes used in our global analysis.* [Section 4.1]

> 2) RC: Given that the model is run using daily data for the time period between 1960-1989 and that it is run (in some simulations) for 150,000 years, could the authors comment on how sensitive the model (and the simulated feedbacks) might be to the variation in climate that has occurred over geologic time scales. P availability has been found to be sensitive to variations in soil moisture (e.g., Buendía et al., 2010; Resende et al., 2010) and inter-annual precipitation variability (e.g., Runyan and D'Odorico, 2013). What mechanisms would be affected by climate variability? For instance, maintenance respiration is dependent on temperature via the Q10 relationship and organic matter decomposition is a process mediated by soil microbes whose rate of activity depends on

(among other things) soil moisture and temperature. How might the considerable variability in temperature and precipitation over the time scales examined in this modeling framework differently affect the three feedbacks considered and in turn the results that were obtained?

AC: This is an interesting question that although we cannot fully answer, it gives us the opportunity to clarify some of the modeling assumptions we used.
In the discussion now comment on that:

*The main effect of glacial interglacial cycles is that soils in temperate regions were removed during glacial times, after glacial retreat the soil formation processes restarts. We run the model for 150,000 years to let the soils reach their steady-state as we mainly wanted to understand how ecosystems remain productive over time, particularly on old weathered soils like those of the Amazon. Results for 10,000 years and 50,000 years are presented to illustrate transient dynamics and to represent soils that experienced glaciations. Temperature and humidity drive terrestrial primary productivity and soil organic matter decomposition. In contrast to tropical regions where primary productivity, litter production, and mineralization is high, productivity and litter production rates in arid regions are lower, which may lead soil organic matter (or at least P) to accumulate. Therefore, if climate shifts from relatively dry to wet as it happened in the Amazon during the last 21,000 years, the accumulated soil organic matter is de- composed and used for primary production leading to a new steady state. In contrast in regions where climate shifted from wetter to dryer conditions, like in the Sahel region, some of the organic and occluded P that was produced during wet periods is preserved, for example as fossilized animal bones or in paleo-lakes (e.g. the Bodélé Depression in Chad; Bristow et al. 2010). Remains of P from paleo-environments are visible in contemporary soil data and they may be regarded as very local phenomena (Yang et al. 2013) and cannot be captured by our model (see Appendix B Figure B1).*
[Section 4.1]

Regarding the P feedback mechanism we think that because they depend on ecosystems NPP, when climate changes the feedbacks will quickly adjust to the new climatic condition.

Because we know hardly anything how much 'ancient P' might have contributed to observed present-day stocks of P in arid soils, nor how it may have contributed to observed present-day primary production, we feel the exploration of these dynamics could deserve a future study. In fact, with the availability of continuous climate data for longer time-periods (spanning several thousands of years) and better understanding of how P is preserved in soils under changing climates, this might be an interesting topic to explore. However, running the model with climate driver data covering climate dynamics over glacial-interglacial cycles is not trivial because climate models struggle with reproducing glacial climate conditions (Singarayer JS, Valdes PJ, 2010). An additional challenge is to appropriately account for the effect that sea level, precipitation, and atmospheric carbon level changes have on weathering rates.

> 3) RC: In this study, the authors bring up the point that soils in the tropics and specifically, the Amazon are quite old (on the order of millions of years). Results from this study where the model was run for 150,000 years show a reasonable agreement in the tropics. Thus, I wonder if the results from this study pertaining to P-limitation are overestimated (given the short time of running the model relative to the age of some soils that have remained relatively undisturbed by volcanism etc.)? What might happen if the model was run for a longer time period (i.e., a million years) in such areas?

AC: As explained above, the regolith model reaches relatively fast steady state and therefore simulations of 150,000 years are close to their steady state with respect to P, particularly in areas that are humid, like the central Amazon. Therefore, we do not expect an increase of P limitation with simulation time.

> 4) RC: This study found that biotic de-occlusion was inefficient due to high carbon costs. How sensitive was the model to parameters accounting

for carbon costs associated with biotic de-occlusion? What is the variability of parameters associated with this term and how reliable are the estimated values of these parameters? Because the microbial pool is not modeled and microbes also mediate the release of P occluded in secondary minerals, is it possible that the carbon costs associated with this feedback could be overestimated?

AC: Actually biotic de-occlusion was inefficient due to the lack for replenishment of the P that was occluded. Simulations including this feedback lead to the unrealistic scenarios where, particularly in the tropics, unrealistically low amounts of occluded P were found at end of the simulation. That is why we choose to run the longer simulations excluding the de-occlusion feedback. This is explained in the discussion section. To clarify this we introduced the following sentence:

*The inclusion of P de-occlusion mediated by vegetation resulted in unrealistically low amounts of occluded P in, especially, tropical soils, which could mean that this process is not correctly specified in the model or that this process does not alleviate P limitation in these old ecosystems.* [Section 5]

Regarding the carbon cost of P de-occlusion, 80 % of root exudates were respired to account for soil respiration (which includes microorganism) and then it was further degraded in the soil following the carbon decomposition equation. The C that remained could be exchanged with the occluded P in a 6 to 1 C: P ratio. (see Eq. 14). The real question that motivated testing this feedback was: If plants can use P occluded in the clays why do old and humid soils, like those of the Amazon, contain so much occluded P? The answer from our model simulations is: only to a very low extent do plants take up occluded P. We think that at least in the Amazon basin other processes, like nutrient redistribution by animals, are responsible for keeping the system productive.

5) RC: In this modeling framework, the C contained in the microbial biomass is not accounted for, correct? The availability of P in the soil can

also be enhanced by microbes that exude phosphatases (as the authors mention in the discussion section; e.g., Kroehler and Linkins, 1988). In Runyan and D'Odorico (2013) we built upon the framework in Runyan and D'Odorico (2012) to also include a dynamic vegetation component and to investigate the role of microbes in enhancing P availability and reducing P losses as well as the ability of vegetation to recover following deforestation for systems affected by this feedback. We found that once P contained in the more recalcitrant organic fraction was depleted due for example to repeated deforestation in a P-limited area that the system exhibited bistable dynamics and remained within a state of low vegetation and low microbial biomass. This occurred because the microbial biomass was dependent on vegetation for a source of carbon, while vegetation was dependent on the microbial biomass for enhancing P availability and reducing P losses from the system. I don't believe there is data pertaining to the relative proportion contributed by roots and mychorrizae (i.e., biotic active uptake as considered in the model) versus microbes (e.g., Reed et al., 2011), but by not considering the microbial pool (and the role they play in enhancing P availability), I wonder if the carbon costs of this feedback (i.e., BAU) could be overestimated?

AC: Our model has a pool that represents all soil organic carbon (C_o), this pool includes microorganisms and their respiration (see description of decomposition section 2.4.1). However, this does not distinguish the effects of microbes vs. micorrhiza and may thus lead to an overestimation of the carbon used by the feedback mechanism proposed in the model.

*The model assumes that root exudates are readily respired as they reached the soil when BAU is activated as well as when it is not. In the scenarios when BAU and BD are not activated root exudation only increases soil respiration, in those scenarios the cost might be very high because the only feedback that remains is enhancement of weathering. As they are proposed in this model those feed- backs are not* *exclusive. In that sense including more feed- back processes could reduce P limitation further.* [Section 4.2]

AC: Thanks for sharing your results. It would be very insightful to include more of these feedback processes combining what you proposed in your paper and what we propose here, which however goes beyond the scope of this manuscript. Thanks for providing these references, which we will include in the revised manuscript.

> 6) RC: This modeling framework is useful to understand the role that vegetation-P feedbacks play in providing conditions more favorable to the growth of vegetation. While I realize that the goal of this model is not to obtain exact predictions of soil P and vegetation, what role might nitrogen limitation (especially given that a system could go from being N to P limited on the time scales considered in this model) play in these feed- backs and the results that the authors obtained? Could any of the feedback processes have high N costs which in turn could lead to one feedback being more efficient than another (despite high carbon costs)?

AC: We have not found references stating that mycorrhizal-mediated uptake of P imply a nitrogen cost. Regarding the process of biotic de-occlusion, I would say that there is no direct nitrogen cost associated to it, whereas microbial mineralization of P (which is so far not included) requires nitrogen. Generally I would say that there is no fixed carbon cost for nitrogen uptake as it depends on the conditions. If the system is fertilized with N then this process is "cheap" but if plants have to invest in nitrogen fixation it would be rather unlikely that after investing in N they will use the N to get P.

I would like to emphasize that none of the P feedback process included in the model was limited by the availability of carbon, but by the availability of P. That is a consequence of how the model was designed. Related to your question but a bit far is the fact that since our model does not include N constrain in biomass production and photosynthesis we expect biomass production and photosynthesis in early stages of soil formation is over estimated, since normally on early soil stages nitrogen acquisition is costly. Now included in the text:

*The challenge is to include these processes at the global scale without site-specific parameterization while accounting for the nitrogen cost related to it. Nitrogen fertilization studies have shown to increase the phosphatase activity and with that the P available for vegetation (Reed et al., 2011; Kroehler and Linkins, 1988). That means that this mechanism has an associated N cost, which might increase the carbon cost of P uptake.*

7) RC: Technical corrections

AC: Thank you very much for taking the time to carefully read the paper and correct the text. They are now included in the revised version of the MS.