**Biogeosciences
Discussions**

Interactive comment on "The role of tectonic uplift, climate and vegetation in the long-term terrestrial phosphorous cycle" *by* C. Buendía et al.

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Received and published: 14 April 2010

We really appreciate the feedback provided by Reviewer 2, and the time spent to carefully look at the manuscript making useful comments about the assumptions we used to develop this simple model. We understand that soil processes and ecosystem are rather complex and that no model makes an exact representation of it. We were particularly interested in being able to solve analytically the steady-state solutions of our system since that allowed us to make interesting sensitivity studies that increased our understanding of the interactions between the main processes. Therefore, we choose to develop a rather simple model. We believe that is a strength of our model, and therefore we prefer not to significantly change the model but rather justify and discuss our assumption better taking into account the important points that were brought up.

Thank you for the useful comments and suggested references.

Abstract - It is unclear from the abstract why this is a novel result. The cycling of all elements, and certainly P, is required to maintain availability of nutrients. I think this paper does more than the abstract tells us.

1) In the abstract we include a final statement explaining the advantage of our model and its possible applications. (L22-25)

P303 L8 - The repetitive citations of the Introductory Brady and Weil textbook does not serve the case well. There is lots of literature about P cycling, why cite an introductory soils textbook?

2) We wanted a general behavior of the P global cycle, and that is why Brady and Weil gave us a good starting point. Nevertheless, we agree with the reviewer that it is important to mention what the community has done recently, although it is hard to go from a local study to the global level. We added some other references, changing with this some parts of the introduction.

P303 - L28 - The idea that Amazon productivity is in some places supported by dust inputs is not in contrast to Mahowald et al. The current the losses of P from the Amazon are partly a result of biomass burning and land use change, which represent new phenomena. The long term P cycle was dependent on inputs, it is now being radically altered by land transformation in some places.

3) We did not mean that. We reformulated the whole paragraph; we hope that after this change the message is now more understandable. (L 81-93)

P305 L 20 – Crews did not show that the tropics are often P limited. In fact, Crews did not do an experiment that demonstrated limitation at all. Later fertilization experiments (Vitousek and Farrington, 1997, Vitousek, 2004) showed that young wet premontane Hawaiian Forests were N limited, while old forests were P limited.

4) We agree with the reviewer. The word limitation is hard to use. We reformulated that

paragraph changing the message and including some relevant references. (L 113-117)

p306 L 2 - But see Richter et al, 2006 for a discussion of relatively rapid turnover of "insoluble P".

5) Thanks for the reference. We are aware that at the scale of ecosystem development, long-term studies, such as the one by Richter et al, 2006 are a feasible way to address decadal changes in soil feature. However, since our study focuses on longer time scales we did not include this reference in the introduction, but only in the description of the model (L 354-355)

P306 L 17 - How do you parameterize porosity here?

6) Porosity is defined as a dimensionless parameter that can vary from 0 to 1. For the simulations presented in the paper we assume a mean porosity of 0.4. This parameter can be change to account for the different soil types. (L 191-192)

P307 L 3 – Why would dust inputs go directly into the dissolved pool? They are in mineral form (at least some of them, and likely most of the exogenous dust to a particular system. Thus it seems they should be subject to the same weathering constraint that mineral apatite is.

7) the point is well taken. Dust inputs do not necessarily go into the dissolved pool, that is a particular problem if we want to apply the model to dry regions. However, since we just have one input (exogenous or dissolvable) of mixed nature, soil dust, biogenic particles, ashes and animal excrement, there was no way of including how they will become available at different rates. The different contributions are also hard to estimate therefore we assume it directly reaches the dissolved pool. (L 211-214)

P307 L4 - Animal "inputs" are not exogenous if the animals are eating plants in the same place they are excreting. Only in the case of sea bird colonies and a few other special cases do animals actually bring more P into the system over long timescales.

8) We only account for the animal exogenous P inputs, from the ocean or fresh water

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ecosystem to terrestrial ecosystem. We rephrase that paragraph to stress that point (L 214-218).

P307 L12 - It is really the erosion rate that matters, since erosion exposes new rock to weathering, and thus represents both a P loss (from topsoil) and a P input (as rock is converted to soil). Whether or not this increases or decreases P availability depends on how weathered the soil is.

9) We assume that in steady state uplift equals erosion. (L 224-225)

P307 L21 - The role of climate on weathering is clearly key here. I applaud the attempt to quantify it, but relying on Brady and Weill here is a real problem. There is some relevant literature on the coupling or decoupling of chemical and physical weathering (see Dixon et al., 2009, Von Blankenburg 2005 and many others) as well as on the effects of climate alone on weathering (Chadwick et al, 2003, Porder and Chadwick, 2009). In addition, physical weathering removes P from the system, and chemical weathering may transform it into a less available form. Assuming these "cancel" each other out without a bit more justification (and sensitivity analyses) seems problematic. Certainly areas that are frozen for a large portion of the year weather differently than those that are not. Bob Berner's Geocarb model, which is widely used to assess changes in atmospheric CO2 over geologic time, assumes a very strong feedback between weathering and temperature. Several other models do as well.

10) We agree with the reviewer; therefore now we have estimated the weathering parameter not only for NZ but also for Hawaii. This parameter, in the revised version of the MS represents the annual temperature variation and the state of the parent material (L 244-246)

P308 L 10 – I agree, the lack of secondary minerals is a major limitation in the Porder 2007 analysis. However, assuming that secondary mineral formation is simply proportional to P weathering is not supported by the admittedly scant literature. For example, apatite P is weathered out of the Hawaii chronosequence sometime between 2 and

20ky, whereas strong P binding mineral forms don't really pop up until at least 150ky (under humid conditions).

11) Secondary minerals appear after the weathering of primary minerals. The concentration of Al, Fe, Mn and Ca in primary minerals to account for differences in soil types, kc is there for that. What is good about the secondary mineral pool is that it is changing in time, and thus it represents the fact that older weathered soils have a higher occluding capacity. Olander and Vitousek 2005 also show experimentally that sorption capacity of soils increases with soil age. This fact is also used in the Wang et al., 2007 model. (L 264-268)

P308 L20- Again, there is a huge literature on soil P binding, why cite Brady and Weil? I don't think the assumption that P sorption is simply proportional to the amount of P weathered is valid. It depends heavily on the redox state and amount of iron, and the pH (for binding with Al). Building a linear model and the "adjusting the parameter" to get "reasonable" occlusion rates seems to open the door to making this parameter whatever one chooses.

12) It makes sense that as soils start to form also secondary minerals will. We believe that including this pool is much better than omitting occlusion or having a fixed ratio.

P308 L22 - Is there any data to support this assumption. I can see why C and N could be closely tied to transpiration rate, but P? Does the Porporato paper actually discuss P? If not, is there justification for this assumption?

13) We believe that at those time scales and including both active (of course also mediated by mycorrhizal associations is a safe assumption. Adding together active and passive uptake along with the dilution effects, as discussed in some detail in Porporato et al. (AWR 2003), tends to make uptake a more linear function of soil moisture. Thus we assume that transpiration will be proportional to water uptake, which will act by driving the P dissolved in soil solution near the root system, and use the parameter ku to correct for the active uptake of P. We modified our description of P vegetation uptake

to make it more clear (L 277-290).

P309 L12 – I'm confused here. Plants are not fixed in their C:P, I agree, but neither do they keep the same C:P in foliage and litter. At least in theory, the more P deficient a system the more P is resorbed before plants lose their leaves (higher C:P in litter than leaves). That well documented process seems unaccounted for if you just say P in litter is a fixed fraction of plant biomass.

14) Looking at the steady state solution (Fig. 2-4) the reviewer will see how actually in regions that are very wet, $s < 0.5$ the P in vegetation does not increase, although productivity does so. Actually the total loss is just described as a fraction of total P and not C.

P309 L 15 – 20 P mineralization is also affected by phosphatase activity, which is, in turn, affected by temperature, the amount of N available to plants (to make the phosphatase; e.g. Houlton), species composition, soil mineralogy, etc. I'm not sure just modeling just temperature is useful.

15) In our model mineralization depends on soil moisture, temperature and P. The possible importance of nitrogen is now mentioned. (L 309-311)

P310 L8 - But where soils have high P binding capacity virtually no P makes it to streams in dissolved from, though DOP losses can be substantial. However, there is a literature on how P losses vary with climate (Porder and Chadwick, 2009) that might be worth looking into in this context.

16) The model does well in that respect, as it accounts for the binding capacity of the soils, and Od are very low. Losses of organic P (Oo) (described in Eq. 9) also include losses in the form of DOP

P311 L12 - Richter et al describe what I think is "deocclusion" on much shorter timescales but in oldish soils. P12 L12

17) Since P de-occlusion only seems to play a role in shorter time scales, we account

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only for losses related to erosion processes.

P314 L1 - There are data to suggest otherwise, P losses depend heavily on climate (Porder and Chadwick, 2009).

18) This line does not say anything about the losses (only about the steady state of weathering.). However, the model takes into account the fact that P losses depend on climate.

P314 L7 – The reference to the Okin paper for soil concentrations is actually to Cross and Schlesinger (1995). They have only 1 value for Oxisols, and 7 for Ultisols, it is worth mentioning that P content of these soil orders varies substantially.

19) Thanks for the correction. We implemented it in the text: L 419-422

P315 - It is unclear to me how leaching of dissolved organic phosphorus is accounted for in this model. More P in organic matter may mean less phosphate leached, but may mean more DOP losses. Is this in there somewhere that I missed?

20) Losses of organic P (Oo) (described in Eq. 9) also include losses in the form of DOP.

P316 L2- I'm not sure what it means that "when ecosystems without tectonic uplift reach their steady state, soil processes no longer play an important role." Just because inputs = outputs doesn't mean there aren't important internal transformations of P that affect P status substantially.

21) In our model soils with no uplift reach the steady- state only when the secondary mineral are no longer accounting for P losses. This does not coincide with Walker and Syers "terminal steady state". We clarified that a bit better (L 542-546)

P316 L19 - Both the Hawaii and NZ chronosequences were selected because erosion is minimal. Leaching occurs, but particulate removal via water flow is assumed to be near zero.

22) Erosion may be minimal in the observed time, but if we want to run the model for 10 Myr it should not be omitted.

P316 L 23 - How does reduction of organic biomass diminish losses? How do animal inputs factor in? In the case of sea bird colonies, I get it, but in the Amazon (for example) that's a internal transfer of P, not a net input.

23) Right, we did not explain well this mechanism; we changed the text to improve the explanation (L 504-510). Amazon animal inputs come from the extensive river system, which carries nutrients mainly coming from the Andes; some of the input however also comes from low-land basins.

P317 - The Franz Joseph sites are on glacial till, so parent material that was deposited. Not formed from bare rock after the soil was removed.

24) We included this part to explain why the P concentration in parent material is so low, and why weathering in this sequence occurs so fast P (L 518-522)

P317 L 11 - gP/m2 to what depth?

25) We assume 0.7 m of starting parent material (which seems a reasonable assumption).

P326 - The atmospheric input of to Hawaii has been calculated by Kurtz et al, no need to use Okin's paper, which is based on Mahowald's model. The P input from dust in the Hawaii sites is roughly 9 times what you use here.

26) Thanks for the correction; we changed the reference and re-run the simulation with the new number. Fig. 6 and Table 3.

P331 - How do the various pools of P-organic (Po,Pv,Pd) change over time? Note that neither Walker and Syers or Crews do any modeling of vegetation P. I think that's a strength here, but it would be nice to see how those separate pools change in model space.

27) Thanks for the suggestion. We changed all the transient simulation plots, now they show the evolution of Po and Pv.

P332 - Again here this does not really match Crews et al at all, except for the fact that rock P disappears over time.

28) Reconsidering the previous comment about the weathering temperature dependence, it should be noted that FJ is probably a very special place, since it has strong physical weathering (due to the temperature seasonal variation), and as mentioned the parent material is depositional, which in comparison to Hawaii basaltic soils is much more easily weathered. We estimated kw with respect to FJ, this is probably very high, while Hawaii, does not have a strong seasonal cycle, (not so much physical weathering breaking the rocks). Therefore in this new version of the manuscript we reduce the value of kw to account for the lower temperature gradient in Hawaii and also for the parent material type. The comparison of our model run with Crews was not possible since Crews only considered the top 50 cm while we start this simulation with a pool of 5 meters of parent material. Therefore we decide to compare our results to the actual weathering rates by Chadwick et al., 1999. Our model makes with this change a better job representing this chronosequence. For the steady state solution as well as for the Amazon we choose a weathering parameter in between the value for FJ and the value for Hawaii. That resulted in some changes on the values, but not on the general behavior of the model.

Interactive comment on Biogeosciences Discuss., 7, 301, 2010.