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

Anonymous Referee #2

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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.

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?

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.

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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.

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

P306 L 17 - How do you parameterize porosity here?

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.

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.

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.

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.

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).

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.

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?

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.

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

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just modeling just temperature is useful.

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.

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

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

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.

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?

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.

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.

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.

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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.

P317 L 11 - gP/m2 to what depth?

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.

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.

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

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