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Interactive comment on "Reviews and Syntheses: Ironing Out Wrinkles in the Soil Phosphorus Cycling Paradigm" *by* Curt A. McConnell et al.

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Dear reviewer, Thank you for your incisive review of the MS. We hope to address all your comments here. The references provided are also very pertinent and informative and will improve the manuscript.

R1: "...it lacks in-depth insights of current "P models" to justify its statement that these processes should be improved in models. It is partly because some of the processes or features are simply ignored in current models, such as phytate, and partly because the proposed processes by authors are not particularly discussed by modeling community yet."

Authors: The original intent of the review was to compare individual models by re-

constructing their respective P cycling components. That intent veered to the current manuscript with emphasis on missing components. As Referee 1 (R1) noted, there are many terrestrial biosphere models (TBMs) and landscape/watershed models. It was tedious and not necessarily informative to reconstruct them all due to the many overlapping frameworks and processes.

With knowledge of individual models from this earlier research, we redirected our efforts to address concepts not generally (or never) included (for example, phytate dynamics) or those that may not be representing a specific process completely (P-driven "biochemical" mineralization). We believe it is important for the development of biogeochemical models and experimentation to draw attention to those concepts ignored in models or not yet discussed by the modeling community.

In response to the comment regarding "improving P models", we think that the matter is more semantic than conceptual. We may also say "include processes not addressed in current models", which of course would be done to improve models. Our aims are to highlight areas of knowledge that need to be included in models in relation to P cycling, to acknowledge the need to converge on common or complementary frameworks well supported by theory, and to acknowledge that uncertainty still exists due to conceptual ambiguity or the ability to measure P pools and fluxes.

R1: "...'P cycling models', which in practice does not really count as a sub-category of models"

Authors: The semantics here can be subjective. Any model refers to a system with arbitrary boundaries. Whatever comes from outside the boundaries are "forcings" or inputs. Any model "component", let's call it a submodel if inside a well bounded model, is in itself a model with its own forcings. Maybe we can make a stop here and highlight that is difficult to be a purist: setting boundaries does not make the boundaries correct, as there are feedbacks that could affect the forcing variables themselves. For example, a change in land use will affect the air temperature and air moisture through

changes in the surface energy balance. For models that couple the atmosphere with the earth surface, climate variables are mostly forced in, regardless of the land use, and the feedback is ignored. The errors introduced by ignoring the feedback are in most cases pretty minor (and of course there is no assurance that including a feedback will make the outcome better). In any case, the model and sub-model language is context dependent. We will try to avoid any confusion.

R1: "...perhaps the authors could try to narrow down the scope of models, for example, only to land surface models and TBMs"

Authors: R1 provided many models which we could include as examples of certain P processes. In terms of catchment/watershed-scale models, R1 suggested including EPIC (Jones et al., 1984), GLEAMS (Leonard et al., 1987), and ANIMO (Schoumans and Groenendijk, 2000) in our review. GLEAMS was a standalone model. During the early stages of the development of EPIC, it absorbed GLEAMS to represent P processes. SWAT (as well as APEX) uses EPIC as the base crop and soil model for each simulation unit (SWAT is a semi-distributed watershed model and APEX is a pseudo-distributed model mostly applicable to smaller watersheds) (Gassman et al., 2007; Wang et al., 2012). In using SWAT, we were encompassing this conglomerate and avoiding cumbersome explanations and redundancy. We will henceforth refer to SWAT when dealing specifically with SWAT and to GLEAMS-X when referring to models that used GLEAMS components. We will also include a more in-depth analysis of the models recommended by the referee (JSM, QUINCY, COMISSION, SEAM, MEND, MIMIC, ECA, etc.).

Section 1: R1 in reference to Line 118: "the modeling work has also supported that CP cycling is largely decoupled from CN cycling (Yu et al. 2020, GMD)"

Authors: We had missed the work of Yu et al. (2020) while finishing up the review. Indeed, Yu et al. 2020 is a good example of a model representing CP-CN decoupling. This would be an appropriate source for our review. We agree with other comments in

the review. The uncritical inclusion of a Century-like approach and the need to represent soil properties variation with depth as an emergent, and not an imposed property, has also been pointed out and partially addressed by using the carbon saturation concept (e.g. Kemanian and Stöckle, 2010) and so did Yu et al. (2020). We think that conceptually, Yu et al. (2020) and our review are convergent.

R1 in reference to Line 187: "Schimel and Weintraub did not simulate phosphatase"

Authors: The intention was not to convey that Schimel and Weintraub (2003) used phosphatase activity in their theoretical model, rather to highlight that improving phosphatase activity assays would benefit model development employing approaches like that of Schimel and Weintraub, who account for enzymes in SOM decomposition.

R1 in reference to Line 194: "please check Yu et al. 2020, in which a dynamic enzyme allocation approach is used to mimic such a relationship"

Authors: The point that we were making referred to measuring or explicitly modeling the relative release of phosphatases by both plants and microbes. The Yu et al. (2020) use of SEAM (Wutzler et al., 2017) and the ECA approach (Tang and Riley, 2013) addresses a different if related area, pertaining to the dynamic allocation of microbial phosphatases to litter or microbial residue and nutrient acquisition by plants and microbes.

R1 in reference to Line 209: "I would argue many terrestrial ecosystem models (ESMs) did include the P-driven mineralization. From as early as the CENTURY model (Parton et al. 1988) to the more recent ESMs such as JSBACH (Goll et al., 2012), ORCHIDEE (Goll et al., 2017), For SAFE (Yu et al., 2018), E3SM (Zhu et al., 2019), QUINCY (Thum et al., 2019). And the overall role of P-driven mineralization (biochemical mineralization) is crucial for plant growth in some of these models."

Authors: We agree that many models include P-driven mineralization implicitly (CENTURY) (Achat et al., 2016) but those that do so explicitly (Goll et al., 2012, 2017; Yang

et al., 2014) are parameterized on limited data (Reed et al., 2015). Goll et al. 2012 stated that biochemical mineralization "parameter values for the adjustment intensity is set arbitrarily" and that in-field P-driven (biochemical) mineralization assays are not currently possible, thus limiting data. This is similarly stated by Thum et al. 2019 in reference to P-driven mineralization: "Further observations are required to constrain this flux better (Reed et al., 2011)." In other words, one can easily come up with a P flux from P-driven mineralization given P demand through a sort of inverse modeling. Our argument was that P-driven mineralization is not always explicitly included, and it is difficult to obtain P-driven mineralization as an emergent property. When P-driven mineralization is explicitly modeled, it is not well constrained. We could include more models into that categorization to make things clear and the review more thorough.

Section 2: R1 in reference to Line 325: "please check Lang et al. 2017, SBB."

Authors: The work by Lang et al. 2017 was extensive and informative and we will expand on changing P nutrition with depleting P stocks. Lang et al. 2017 is a very fitting reference for the "potential shift to P-driven Po mineralization processes" statement (Line 324-25). The work by Lang et al. 2017 reflects the Walker Syers model (1976), showing that as soils weather and overall P stocks degrade, Po becomes the more available P source as Pi is either lost from the system or occluded. Although not explicitly stated by Walker and Syers, McGill and Cole (1981) discussed the change in nutrient strategies with the depletion of P stocks: "Although Po has not started to decline, it has leveled off and the decline in total P is substantial. Under such conditions demand for internal cycling of Po to meet the needs for P increases."

The objective of the paragraph in question referenced by R1 was to describe how C:P ratios were variable and that they become more decoupled (wider range of possible C:P ratios) as soils weather. This may be due to the depletion in P stocks and changes in P nutrition strategies. It could also be due to variable C demand and the assumed prevalence of extracellular "P-driven 'biochemical' mineralization" to scavenge P. In a highly weathered soil where P availability is limiting, P could be scavenged without

C mineralization, thus further decoupling C and P. Therefore, this decoupling would be dependent to some degree on the relative P to C demand and the extracellular mineralization of Po.

R1 in reference to Line 366: "The real challenge (for modeling) is to account for the huge stoichiometry differences between plant litter, microbe, and SOM, especially C:P ratio (Xu et al., 2013; Mooshammer et al., 2014). This requires an explicit microbial pool and a number of microbial adaptation processes to be included in models. Additionally, how to extrapolate these mechanisms from site-level to regional and global level is another complicated problem"

Authors: We agree. Our intention was to convey this in the review while acknowledging that phytate dynamics are possibly related to the wide ratios in C and Po stoichiometry. The issues identified by the reviewer are on point but were also well understood. The dynamics of phytate is barely in the modeler's radar. But phytate is clearly just a component of the larger picture. We will rebalance the narrative.

Section 3: R1 in reference to Line 398: "there are some models implementing the OC saturation dynamics (or similarly clay-related C sorption capacity), such as MIMICS (Wieder et al. 2014), MEND (Wang et al. 2015), RESOM (Tang and Riley 2015), and COMISSION (Arhens et al. 2015). The recent JSM (Yu et al. 2020) has also include N and P in the OC sorption, which also complies with the saturation principle, although Po is not specifically separated as a competing sorbate of OC"

Authors: Wielder et al 2014 (MIMICS) include the effect of clay on SOM protection, not the concept of carbon saturation which is different (see Table 1 in that paper, row for Cscalar).

Wang et al 2013 and 2015 (MEND) does not address carbon saturation explicitly. There are parameters to represent C affinity to the mineral matrix, but no explicit mention of soil C saturation. Perhaps it is an emergent property of the model. However, the steady state mineral associated organic matter in equation 25 in Wang et al 2013 (variable M)

does not seem to be saturating (i.e. when inputs go to infinity SOC approaches an asymptote). That paper presents steady state equations, but not the limit when inputs go to infinity.

Tang and Riley (2015), figure 12b in the supplemental shows a linear increase in SOM with increasing mineral surface area (many models surrogate MSA with clay content despite the limitation of such simplification). There is however no indication of saturating behavior, although it might be an emergent property if the model is tested with increasing C inputs. It should also consider aggregation (see work by Six et al team, Kong et al., 2005) Tang and Riley model does not address aggregation.

Ahrens et al. (2015) is explicitly saturating (Table 1, Eq 6 in their paper) and as acknowledged in that paper, it follows work by Hassink and Whitmore (1997).

Ignoring fluxes between layers, the soil carbon steady state in Arhens et al is: $Cq = Kads * qmax /(Kads + kdes/Cdoc)$

Cq is soil organic carbon, qmax is the maximum allowed soc, Cdoc is carbon in DOC, and Kads and kdes are DOC sorption and desorption rates. As Cdoc goes to infinity $Cq = qx$.

This is conceptually and mathematically similar to Hassink and Whitmore (1997) model, Kemanian et al (2006), Kemanian and Stockle (2010), and in a more formal formulation in Kemanian et al. (2011). A paper by White et al. (2014) in Biogeosciences followed up on the impacts on N dynamics. Arhens et al. (2015) is a good reference to include in this review, acknowledging that there are earlier precedents. And there is always a possibility that we are missing other relevant work.

We think that the general point that organic C saturation (not the response to clay) is not explicitly included in many models stands, as well as the need to consider the implications for P cycling, much as White et al. (2014) addressed the implications for N.

R1 in reference to Line 445: "in principle, what is discussed in this paragraph makes sense, however, given the existing uncertainties in Pi sorption (as discussed in section 4), it is really a challenge ahead of our current focus to consider the role of Po and interactions with OC saturation. The role of OC in Pi sorption has been partially considered in QUINCY (Thum et al. 2019), which proves to have an important role in regulating the P availability and thus affect plant growth, but this is not specifically discussed in the model description paper."

Authors: Thank for the comment. It seems that R1 agrees with the point. Stating the knowledge gap does neither ignore ongoing efforts to address it (even if they have not been published yet) nor the difficulties inherent to the problem.

Section 4: R1 in reference to Line 501, 506: "DI and STP not defined"

Authors: DI = dionized water, STP = Soil test phosphorus

R1 in reference to Line 517: "The work by Helfenstein et al. 2020, BG, has shown that there is a certain level of correlation between the Hedley Pi pool and Isotopic Exchange Kinetics Pi pool. And our recent work actually shows, with the implementation of double-surface Langmuir on current Pi pool structure, there is a possibility of utilizing the Hedley data for model validation"

Authors: An excellent point, a noteworthy inclusion in our review. However, the point we were making was that the numerous modifications of the Hedley method complicate the analysis of Hedley data. We did not intend to say that correlations can't be drawn, just that the lack of methodological consistency makes this difficult or contributes to a lingering uncertainty about the compatibility of chemically extracted and modeled P pools.

R1 in reference to Line 525: "Dari et al. 2015 not found in reference. And the effect of OC content on Pi sorption is already partially implemented in QUINCY (Thum et al. 2019, GMD)"

Authors: Dari et al. 2015 reference is listed below. Its exclusion was unintentional.

Dari, B., Nair, V. D., Colee, J., Harris, W. G. and Mylavarapu, R.: Estimation of phosphorus isotherm parameters: a simple and cost-effective procedure, Front. Environ. Sci., 3(October), 1–9, doi:10.3389/fenvs.2015.00070, 2015.

R1 in reference to Figure 1: "Isn't the weathering of P coming from apatite P? are the three Pi and Po pools forming a continuum of stability? Do they transfer with each other? If they do, please add internal fluxes between pools. If they don't, do they all directly transfer phosphate to solution Pi?"

Authors: Yes, apatite P is the most prevalent source, the use of "P minerals" was purposefully generic, and referring to it as apatite seems to be most common in the literature. There is likely exchange between the various pools through stabilization and destabilization as mineral-PO_4 bonds are formed and broken. However, there should be direct exchange between all soil pools and the soil solution. This is particularly pertinent to organic phosphates. There may be more "stable" Po forms, such as phytate, but P can be hydrolyzed using specific phosphatases such as phytase. P from any Po pool can be directly hydrolyzed into the soil solution, albeit at different rates depending on the stability of the Po form under consideration.

Actual "pools" are less well-defined than those operationally defined by chemical extractions. But for clarity, we could add arrows indicating internal fluxes between Pi and Po pools. All pools can directly transfer to the solution pool; P does not have to cascade down the gradient of pool stability until it reaches the "solution" pool. It is likely that all pools exchange P with the solution and with one another. Representing this will increase the number of arrows and complexity of diagram, but will make it more representative.

R1 in reference to Figure 3: "Are there any references for the C:P ratios values in the figure?"

Authors: Value (description) Reference. Microbial biomass (MB). Threshold Elemental Ratio (TER) 25 (MB C:P ratio, low of range) Capek et al. 2016; 186 (Soil C:P ratio, global average) Cleveland et al. 2007; 350 (MB C:P ratio, high of range) Capek et al. 2016; 1400 (TER, Heuck et al. 2016; Zechmeister-Boltenstern et al. 2015); 4000 (Decomposition stop point, Zechmeister-Boltenstern et al. 2015)

The references will be added to Figure 3.

R1 in reference to Figure 6: "the resolution of the figure is too low to read. What are the differences between the two dash lines in each sub-panel?"

Authors: We will improve the resolution. The dashed lines are regressions for all the Land Use/Vegetation categories in the study. The dotted lines are regressions for Land Use/Vegetation whose slope was significantly different than other systems (e.g. the "Wetland Organic" slope is negative in the top-left regression). We could describe which system regressions differ from the rest in the figure legend if that improves clarity. The important point is that the r2 values are lower for C:P in soils and microbes than the C:N.

R1 in reference to Table 2: "it needs an appropriate caption. There are quite some different equations for C sorption capacity and PSP, why do you choose those specific ones. I am not particularly sure about the purpose of these equations here, particularly the ones calculating PSP."

Authors: PSP equations are listed because they were mentioned in Line 498. The work can be solely cited and equations excluded if that would suffice. But showing the equations makes the model structure unequivocal.

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