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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.

Anonymous Referee #1

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I find the paper by McConnell a very interesting piece of work, which coincidently covers lots of our accomplished and undergoing research regarding soil phosphorus cycling. Therefore I would like to discuss with the authors about some interesting points in the paper.

As a modeler working with P processes for years, I totally agree with the four points the authors brought up, and I am impressed by the range of literature they reviewed. However, I am not certain if anyone who does not have enough knowledge about models will be easily convinced by the authors. My overall perception of the paper is that it has a very nice flow of information regarding phosphorus process understandings and techniques, but 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. Nevertheless, I think the strength of the paper will be much enhanced if the discussion on the modeling side is improved.

First of all, the basis for discussion used by the authors is a very unspecific term, "P cycling models", which in practice does not really count as a sub-category of models. As I see from the few models that the authors cited, SWAT, CENTURY, and ECOSYS are terrestrial ecosystem models focusing on catchment-scale agriculture system, grass/forest/crop, and soil chemistry processes, respectively; CLM-CNP and CASA-CNP are global land surface models, which are developed from site-level terrestrial biosphere models (TBMs). There are a lot of other models fit within these two categories, such as EPIC (Jones et al. 1984), GLEAMS (Leonard et al. 1987), AN-IMO (Schoumans & Groenendijk 2000), and a number of other mathematic models (e.g. Buendía et al. 2010, Runyan &D'Odorico 2012) for the former one; as for the latter one, TBMs that already implement P cycling processes include JSBACH (Goll et al. 2012), ELM (Zhu et al. 2016), ORCHIDEE (Goll et al. 2017), ForSAFE (Yu et al. 2018), QUINCY (Thum et al. 2019). I noticed that the authors also try to cover OC processes in the paper, and there is also many soil organic carbon models, such as MIMICS (Wieder et al. 2014), MEND (Wang et al. 2015), RESOM(Tang and Riley 2015), COMISSION (Arhens et al. 2015), and JSM (Yu et al. 2020). I understand it might be too ambitious to cover all details in process understandings, techniques, and model descriptions/limitations, but perhaps the authors could try to narrow down the scope of models, for example, only to land surface models and TBMs.

Secondly, from a modeler point of view, the four points could be organized in a way that is easier to follow. The most intriguing points for me (modelers) are those processes/features that are already described in models and need better understandings to improve their model descriptions. These processes/features are the biochemical Po mineralization, the stoichiometry problem, and the methodological discrepancies

between P analyses and implied P dynamics. The other processes/features are also interesting but much less noticed by the modeling community. For example, I think the connection between stoichiometry and C- and P-driven mineralization is much stronger than phytate.

Below are some detailed comments following the order of sections.

Section 1:

Line 118: the modeling work has also supported that CP cycling is largely decoupled from CN cycling (Yu et al. 2020, GMD)

Line 134: Is the sentence complete?

Line 164: logically, I think it is better to place the last sentence at the beginning of this paragraph.

Line 187: Schimel and Weintraub did not simulate phosphatase

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

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), ForSAFE (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.

Section 2

Line 325: please check Lang et al. 2017, SBB.

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

Section 3

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

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.

Section 4

Line 501, 506: DI and STP not defined 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

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)

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?

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

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?

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.

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