Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2017-304-RC1, 2017

Interactive comment on "Soil solution phosphorus turnover: derivation, interpretation, and insights from a global compilation of isotope exchange kinetic studies" *by* Julian Helfenstein et al.

Anonymous Referee #1

Received and published: 8 September 2017

In general, this paper presents some very interesting results on soil solution phosphorus (P) turnover, which, as the authors pointed out, is a very important concept in describing the kinetics of bioavailable P.

However, I do have several concerns about the methodology and interpretations of results.

The major concern I have is the possible impacts of microbial processes on the results. The authors did not clarify the possible impacts of microbial uptake and turnover in the paper, but emphasizing the new insight is about the diffusion-based mechanism. One guess I have is that the authors accept the assumption from isotopic exchange kinet-

Printer-friendly version

Discussion paper

ics studies that during the short-term batch experiment (100 minutes), there is only physiochemical exchange but no biological exchange. It will be better if this argument is clearly stated in the beginning of the method section. Moreover, assuming this assumption is taken for granted, there is still recent evidence showing the strong active role of microbes during the short-term batch experiment (Bunemann et al. 2012). It also seems that the microbial inhibitors don't always work as a perfect solution due to various reasons (Bunemann et al. 2015). It would be not only interesting but also necessary to see if any results of microbial impacts could be drawn from the current dataset.

The second concern I have is about the evolution of the equation 2 and also the determination of parameter m in the dataset. As far as I know, there is a simple version, a version without the $r(\infty)/R$ term, and a full version of the equation from papers in the dataset; and for the parameter m, it is sometimes directly using the value $r(1)/R$ and sometimes a fitted value. How reliable are the results given the huge inconsistency of the dataset, particularly because Km derivates from the full version of the equation and is calculated using m and n values?

The third concern is that some of the hypothesis and discussion section are seemingly self-verifying. For example, in the third hypothesis, E(t) is mathematically already defined as a function of Pw (Eqn. 4), meaning the authors are only looking at E(t) and Km; in section 3.3, the authors concluded that Km is 'an important predictor of isotopically exchangeable P at exchange times of less than 1 minute', but in fact it is because it is defined/derived in this way mathematically (as shown in SI). I would suggest reconsidering some of the sayings used in the paper, as the authors have already mentioned that many of the terms discussed are calculated by the same parameters.

Some technical/specific corrections:

P1, L25-30: the sequence of the three points is a bit difficult to follow

P2, L11: PBC should be abbreviated here rather than at L15

P2, L15: any reference for it?

P2, from L23: from the content of the paper, Km is the main topic, but this is not mentioned in L10 ('In this study, we investigate. . .'). And it came too late in this paragraph, would be better if it comes earlier and uses an equation, in parallel to PBC.

P3 L5: as far as I know, Frossard et al. 2011 is a book chapter which doesn't publish any new data, maybe cite this in another way?

P5 L29: no need for the abbreviation of conc.

P7 L23: misuse of hyphen

P7 L29: loose (typo)

SI: the numbering and alignment of equations

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2017-304, 2017.