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Biogeosciences Discussions

Interactive comment on "A novel isotope pool dilution approach to quantify gross rates of key abiotic and biological processes in the soil phosphorus cycle" *by* Wolfgang Wanek et al.

Anonymous Referee #1

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The manuscript presents an improved version of the isotope dilution approach to quantify gross P immobilization-mineralization rates and their control by abiotic and biotic processes. The originality of their approach is to apply the isotope dilution approach to sterile and non-sterile soils in order to separate the contribution of biotic and abiotic processes to P release or uptake. This is particularly important in the case of P since this element rapidly circulates in mineral and organic pools in soil. My feeling is that the experimental approach is solid: the improved proposed approach is compared to the classical/older one, the microbial immobilization of P quantified by the isotope dilution is checked with another method (extraction with chloroform), two methods of P extraction are compared and several soils are tested. Given the importance of P

for biological organisms and its coupling to N and C cycles, studies on P are highly desirable. The proposed method will facilitate the study of ecosystem P dynamics. Therefore I strongly support this manuscript. Below some comments with the aim of improving the manucript 1) The study includes combines two methods of P flux measurements, different extraction procedure, kinetics....it's not easy to follow. It might be useful to build a figure with an overview of all experimental works 2) It might be useful to specify as soon as the introduction that the method of Bartholomew was initially designed for N. Then this method was adopted for P. 3) A big limit of your approach: when you estimate the contribution of abiotic and biotic by calculating the difference of P immobilization (or mineralization) between sterile and non-sterile soils, you assume that biotic and abiotic P fluxes are additive. In reality, the two types of process interact, e.g. competition for soluble P. This question is rapidly evacuated which is not a good idea. More generally, you are too subjective when you consider the different limits of your approach. You always came to the conclusion that your method is perfect. It would be more relevant for the community to see where the main limits and possibilities of improvements for the future are? 4) I was surprised by the statement that most of the organic P is present in the microbial biomass. Is it a general result or specific to some soils? 5) The way you calculate the P fluxes with the dilution isotopic approach is clearly explained, especially because you present the equations used. This is not the case for the IEX/ID method. Could you make an effort to clarify this? 6) There is an interesting pattern that you do not discuss: in tropical soils the Pi is rapidly/mainly immobilized by minerals whereas the Pi is released from microoganisms. On the long run this functioning cannot be maintained since the microbial pool P will exhaust. In nature, plants might desorbs this Pi to re-inject it in the living/organic cycle. I suggest you to read the studies on the "bank mechanism" that has been initially developed for N but could work for P (Fontaine et al 2011; Shahzad et al 2012; Perveen et al, 2014...). A little conclusion on the ecological relevance of your findings might be welcome. 7) On the writing: try to shorten your discussions, some sections are too long and dilute your main message. For example, section 4.5 is very technic and we do not see im-

mediately the relevance. Your sentences should not exceed three lines and certainly do not reach 5 lines.

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