

Dear Editorial Board, dear Reviewer 2,

please find our point by point response to the reviewer's suggestions below. Our text is in blue, sentences added or changed in the manuscript in bold. We hope with these changes the manuscript now is acceptable for publication in Biogeosciences. We also provide a tracked manuscript version alongside the revised manuscript.

Kind regards,

Wolfgang Wanek

bg-2018-519r1

Anonymous Referee #2 Report #2 - Submitted on 21 Jun 2019

For final publication, the manuscript should be

accepted subject to minor revisions

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

Thanks for addressing all comments and improving the manuscript.

I went through the answers and would like to comment on a few of them (using the numbers of the questions by reviewer 1 and subsequently your #:

1) this new figure is certainly helpful. Only the reason for the parallel steps acidification vs. isobutanol fractionation does not become clear. Maybe you could indicate what is derived from the comparison of the two ways of analysis?

Direct acidification followed by malachite green measures soil extract P_i concentration but LSC measures $^{33}P_i$ and $^{33}P_o$. In contrast isobutanol fractionation isolates P_i and therefore allows the measurement of concentrations and ^{33}P activity in pure P_i (no $^{33}P_o$ interference). However, since there was no $^{33}P_o$ formation the results of both approaches were similar. We added the following to the legend of Figure 2: **“Isobutanol fractionation separates dissolved P_i from P_o and thereby allows highly specific measurements of concentrations and ^{33}P activities in P_i , without interference by $^{33}P_o$. Direct acidification of bicarbonate extracts measures dissolved P_i using malachite green but LSC quantifies the sum of $^{33}P_i$ and $^{33}P_o$, the formation of the latter ($^{33}P_o$) however turned out to be insignificant.”** see lines 1119-1123.

2) Fine, although here you could maybe refer to Di et al. 2000

We added the reference to the following sentence **“The isotope pool dilution approach (IPD) of Kirkham and Bartholomew (1954) was developed as a general tracer approach to measure gross rates of soil element cycle processes, but was most frequently applied to nitrogen cycling processes such as organic N mineralization and nitrification (Booth et al., 2005). The IPD approach can however also be transferred to measure gross rates of P cycle processes (Di et al., 2000).”**, lines 79-82.

3) Mostly fine. I would just change the sentence “effects of increased P_i mobilization due to microbial lysis on P_i sorption-desorption could be tested in sterile soils”. How about deleting “of increased P_i mobilization due to”? I think this leaves what this is about – effects of microbial lysis on P_i sorption-desorption....

Done, the sentence now reads: **“Nonetheless, the effects of increased P_i mobilization due to microbial lysis on P_i sorption-desorption could be tested in sterile soils by adding increasing concentrations of non-labelled P_i alongside the $^{33}P_i$ tracer and then could be corrected for in future ^{33}P -IPD experiments”** in lines 503-505.

4) Fine.

5) Somehow the text that you give here in the response letter does not completely match the text in the manuscript. And even though you give some references above, I would say that a reference to one of Fardeau’s papers would be justified where you speak about extrapolations of $r(t)/R$.

Done, the sentence now reads: **“Short-term exchange kinetics are then extrapolated over the full time period of the moist soil incubation ($E(t)$) (Fardeau et al., 1991)”** in line 76-77.

6) Fine

7) Fine

#16: I leave this to readers to think about – the added sentence, however, is certainly useful.

Thank you.

#22: this should be explained in the manuscript!

We added this information in line 232-233: **“Time point 0 was assessed by adding the tracer solution and immediately extracting the soils with 0.5 M $NaHCO_3$ ”**.

#24 and 36: not clear if you made any changes to the text?

We changed the following sentence, and in revision 2 further changed from “seems favorable” to “recommended”, see lines 573-575: **“Given the continued extraction of P_i from exchangeable P_i pools in serial extraction tests, parallel determination of microbial P and ^{33}P by CFE is recommended compared to sequential extractions by sECE”**.

#28: I don’t understand the sentence “In these previous studies abiotic processes were not corrected from the final data.” And you should add “In the present study,” before “This abiotic correction was performed by applying IPD calculations”.

We corrected the text to make the point more clear, see lines 261-270: **“Calculation of gross IPD rates followed the mass balance equations of Kirkham and Bartholomew (1954), as later applied by others for soil gross P fluxes (Kellogg et al., 2006; Mooshammer et al., 2012). In these gross P flux studies abiotic processes were not corrected for, P_i influx rates therefore representing the sum of biotic (organic P mineralization) and abiotic (desorption) processes, the latter of which do not play a significant role in decomposing litter being devoid of soil minerals**

(Mooshammer et al., 2012). However, to calculate gross P_o mineralization for soils, gross rates of P_i desorption have to be corrected for in live soils. In the present study, this abiotic correction was performed by applying IPD calculations for influx (GI, gross influx; equation 1) for sterile soils (abiotic influx by P_i desorption) and live soils (total P_i influx), and taking the difference as biotic influx (i.e. gross P_o mineralization)".

#48: well, you should facilitate reading for your audience...

We did not change the numbering of the soils.

#50: ok, please add these sentences to the manuscript.

We added the following information on differences in specific activities between live and sterile soils in lines 337-339: "Specific activities of P_i were initially higher in live than in sterile soils (Fig. 4C). This was caused by the addition of the same amount of radiotracer to both, sterile and live soils, but autoclaving caused a flush of P_i from lysed soil microbes, which effectively lowered the specific activities of P_i in sterile soils".

Once these points have been addressed, the manuscript is acceptable for publication.