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Interactive comment on "Phosphorus and carbon in soil particle size fractions – A global synthesis" *by* Marie Spohn

Anonymous Referee #2

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General comments

The study encompasses a relevant and interesting topic and a review on this topic would be a good addition to the existing literature. However, the number of studies involved is relatively small to draw conclusions on a global scale, especially because some of the dominating drivers of the differences among the included studies are not (or cannot be) taken into account or addressed/discussed:

- Differences in horizon and sampling depth are not taken into account or addressed for the global meta-analysis. This can have large consequences for the total and relative amount adsorbed to different soil fractions, as both carbon and P tend to accumulate in the top few cm of the soil, as shown in figure 5a.

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Discussion paper

- Differences in land uses are only addressed in the section on land use change, but are very likely to be a main driver of the differences in the entire dataset. Phosphorus (and carbon) inputs widely differ among agricultural and natural systems, and this should be addressed (more prominently) in the current study.

- The effect of soil pH is not addressed. The author mentions adsorption of (organic) P to metal (hydr)oxides as one of the main drivers. The affinity of different P compounds to adsorb to reactive surfaces strongly depends on pH, as the author mentions in the introduction. This point does not receive any further attention in the rest of the paper.

Considering that these factors remain largely unaddressed and taking into account the limited size of the analysis (only 11 studies were incorporated), I think the manuscript is unfit for publication in its current form.

Additionally, the manuscript is not written very elegantly. There are many repetitions, errors, and grammatically incorrect sentences throughout the manuscript that give it an unfinished look.

Specific comments

Introduction:

- The introduction can be written shorter and more concisely towards the goal of the study. Some examples:

- The first two paragraphs (p. 1, l. 5) can be summarized in two or three short sentences that indicate P is important for organisms and distinguishing between IP and OP in soil.

- The information on different inositol phosphates (p. 1, l. 18) is not relevant here.

- The entire paragraph on sorption of P monoesters and diesters might be replaced with one or two sentences on the difference in sorption affinity between higher-order inositol phosphates, P diesters and inorganic P (orthophosphate). The last remark on competition between inorganic P and organic carbon comes out of the blue and does not serve any purpose here.

- The section on the C:P ratio of the plant input (p. 2, l. 11) can be shortened. There is no need to discuss the results of these meta-analyses in detail. Additionally, in l. 14 the author suggests a decreasing order, but the signs suggest an increasing order.

- On p. 2, l. 22 the author describes the objectives of the study, but the effect of soil depth and land use are not mentioned anywhere in the introduction.

- When the effect of P inputs on OC/OP ratio is discussed (p. 3, l. 11), the author mentions plant material, but not other inputs like (organic or mineral) P fertilizer or animal waste/manure. This would be acceptable if the meta-analysis was restricted to forest ecosystems, but that is not the case.

Materials and Methods:

- The search method is not reproducible. I would expect a search query.

- It is not clear to me why studies that report only two pools of organic P in the Hedley fractionation are included?

- Saunders and Williams describe several methods in their paper. I assume the author is referring to the ignition method.

- In the Saunders and Williams ignition method, it is organic P that is determined as the difference between total and inorganic P, not the other way around as the author states on page 4.

- Both methods to determine the soil P fractions have their pros and cons, yet the influence of the procedure or the possible implications of including different methods are not discussed. I would expect a section on this topic or an analysis to the effect of the method used in the studies, as it can significantly affect the results and interpretation.

- Not enough information is provided on the NMR analysis. NMR analysis results may depend on many variables, including the type of NMR (solid vs. liquid state), the extraction procedure used, delay and acquisition times, etc.

Results and Discussion:

- In general, there is not enough comparison to the existing literature on the implications of the findings.

- The soil clay fraction usually contains more Fe and Al oxides as mentioned in the discussion. It is a shame there is no information available on the Fe and Al content of the soils in the studies, as this could have helped strengthen this point. Adsorption to clay should not be overlooked here either, as clay minerals can substantially contribute to the reactive surface area of a soil (see Gerard et al. 2016 Geoderma 262:213-226)

- The conclusion about increased P losses from erosion (p. 8, l. 17) looks a little odd, without any context. The fact that the soil clay fraction is richer in organic material and nutrients is well known, so this conclusion is of limited novelty.

- I am not convinced that the lower OC:OP ratio in the clay size points towards a further degraded OM source (P. 8, l. 20). It might indicate that it contains OM of a different origin, or a preferential adsorption of organic compounds with a relatively high P content.

- The ratio between P monoesters and diesters seems very low (relatively high abundance of diesters) compared to other previous analyses (e.g. Menezes-Blackburn et al. 2018 Plant & Soil 427:5-16). I am wondering why this is the case.

- It seems to me that latitude and MAT should be strongly correlated. Yet their relation to the measured P pools is dissimilar. This topic should be disentangled and explained further.

- Land-use change had no effect on P stocks in this study. The data do thus not support the suggestion made in the last sentences on this section (p. 10, l. 21). As data on additional inputs (fertilizer, manure) and outputs (crop yield) of C and P are not provided, any conclusions on mineralization or leaching of organic P are purely speculative and should be avoided.

- In the conclusion, the author makes a remark about the potential of soil OP to replace P fertilizer inputs. This comes out of the blue and should be discussed in a discussion section first.

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