

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

Anonymous Referee #1

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With an increasing number of studies on the fate and origin of soil P, a meta-analysis of published data is principally welcome. Yet, this paper hardly adds novel findings to what has been published, it ignores methodological differences among studies, and does not really go into depth with statistical analyses, which in this form are even wrong. I therefore have majors concerns regarding the publication of this paper in its present form.

The main criticism refers to:

Lacking novelty: There are numerous studies on the effect of land use on OP and IP in soil, and the conclusions drawn here do not add much to current process understanding. Also, correlations to climatic elements have been reported earlier and are no achievement of the current analyses. Generally, the discussion sections fails to make

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clear, which findings have been reported and discussed earlier in individual papers, and what is the additional merit when summarizing them.

Inadequate literature research and number of studies considered: This paper has 49 references, lacking quite a few publications on the related topic. The “meta-analysis” itself relies on 11 studies only (Table 1); even as I can understand that the authors wished to have all analyses within the same study, this is by far too small to discuss global variations in OP and IP distribution in relation to OM and other site indices. Searching for the combination of soil organic matter AND particle size fraction in Chemical Abstracts, for instance, already provides 842 hits. Further combining with phosphorus provides 84 hits, far more than considered here in this paper. One of the best reviews on organic matter in size separates was published by Christensen (1992; Adv. Soil Sci) –this review 26 years ago already had more references than this meta-analysis – which is not even cited in this study!

Statistical analyses inadequate: The author uses ANOVA to compare different sampling sets and transforms data for normality. Yet, i) ANOVA relies on independent data while the size fractions considered here are dependent data. If independent data sets would remain, ii) the main problem in ANOVA is inhomogeneity of variances, which has to be tested, but no info on this are included in the manuscript. Third, all conclusions can be made for transformed data only but not for original ones. Fourth, ratios are per-se not normally distributed, i.e., comparisons have to consider geometric means rather than arithmetic ones etc. Fifth, despite the author stating that some data had to be transformed to fit to normality, they perform linear regression analyses only. Sixth, in none of the statistical treatments they account for covariate interactions. And seventh, no tests were performed on the stability of statistical analyses. Overall, this is not sufficient for a high-level journal such as Biogeosciences.

Statistical parameter selection questionable: the author states to find novel insights by correlating P data to latitude. There are several issues here. Correlations and regressions that improve by additionally including latitude might be spurious, as latitude

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should be highly correlated with MAT at sea level. On the other hand, looking at the study by Makarov et al. 2004, all sites have the same latitude, but across altitudes there is considerable variation in MAT and vegetation. In the discussion, latitude is at times used interchangeably with climate, which at least for the study by Makarov is not true. For the same reason, it is not clear which sites you included in which climate zone (e.g., p.6 l.13/14). Further, many other site properties change with latitude, such as geology, interactions with vegetation type, land-use system etc., all of these have not been considered in statistical analyses.

Ignorance of classification systems: The author just compares the concentrations of OP and IP in clay, silt and sand, apparently ignoring that different countries use different size thresholds for silt and sand, e.g., $20\mu\text{m}$ according to ISSS and Australia, $50\mu\text{m}$ in France or US, $63\mu\text{m}$ in Germany.

Differences in methodology not considered: The NMR extraction methods changed in the last 20 years. How was this considered here? Also, extractability of P possibly affects NMR results, which was fully ignored in data evaluation here. Similar problems hold true for OP data. These were also obtained from pooling results of Hedley fractions. Which fractions were pooled and were they the same for all studies? Which were the criteria for OC data to be included in the study (methods etc.)? Besides, also the methodologies used for aggregate dispersion prior to particle-size fractionation likely varied between studies, which was also not discussed by the author.

P stocks not analyzed: Land-use effects on soil P should not be considered on a soil concentration but on a total P stock basis, i.e., by multiplying OP and IP concentrations with bulk density and mass of the fractions. This, however, has not been done. Yet, it might be important, as already Christensen et al. (1992; Adv. Soil Sci) stated that element enrichment within a size fraction is non-linearly, inversely proportional to clay content.

Simplified discussion: Several statements in the introduction and discussion section

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are very simplified or mere speculation, not justified by research. For instance:

- I doubt that information on OC in soils is limited (p.2, l. 21),
- the author calculates molar ratios (p. 4, l. 31) without knowing molecular weights,
- IP losses during size fractionation or incomplete extractability for NMR was not considered in the results section;
- both introduction and discussion are too strongly focused on the sorptive strength of the phosphate group as the only parameter explaining changes in OC:OP ratios and OP distribution into size fractions;
- lower concentration changes in soil P than of soil C may have nothing to do with persistence, but merely indicate that there is no significant gaseous P loss pathway as opposed to soil C, and that farmers fertilize P to compensate for these losses;
- the observed shift in IP:OP ratio with depth is a result of decreasing OM contents with depth and simultaneously increasing proportions of P containing minerals which are only partially weathered or not at all. This is, however, completely unrelated to the higher sorption strength of OP compared to IP or the fact that fine particles are eroded more easily as it is currently framed in the discussion (p.8, l. 15-20);
- If there was such a large range in particle-size distributions and other parameters for 11 studies only (p. 9, l. 21 and 33-34), how does this affect the results? Merely stating that this likely blurred the findings (p. 9, l. 21) is unsatisfying for a meta-analysis;
- I doubt that there was significant OP leaching – the authors should estimate cumulative leaching rates (not done) and related them to P stock changes (not calculated) before speculating that this process was relevant (without even citing related studies);
- there are sections in the introduction and discussion linking P in soils to P in vegetation and plant litter, but this is barely considered in the analysis.

Redundancy of several parts: e.g. p. 1, l. 1-27: hardly introduce into the objectives;

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p.2, l. 12-22: no connection to former paragraph; p. 6, l. 22-32: what is novel here compared to correlations shown before; p. 7, l. 18-26: well known, p. 8, l. 1-18: well known; p. 9: this is not the first study relating P to climate; p. 10, l. 26-37: Conclusions: too simplified, partly well known; p. 11, l. 1-2: mere speculation, no data have been provided on sorptive stabilization of SOP against degradation.

Poor technical presentation of the manuscript: This manuscript should have been checked by an independent corrector for punctuation, spelling and style as there are a number of mistakes, ranging from mere “typos” to completely wrong (e.g. abbreviations in table headings).

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