Reviewer Comments

The manuscript presents some interesting results and the results are now quite clearly presented. While some aspects have been improved in the revision, other aspects remain problematic, including some issues already pointed out by earlier reviewers. The four main issues I see are:

- Inadequate understanding / discussion of the limitations of the NMR method
- Precision and clarity of terminology
- A discussion section that does not go beyond what is already known
- English language

Limitations of the NMR method

Liquid ^{31}P NMR is a powerful method to study the presence of certain P forms in soils, and it is of interest to combine results produced by different studies in a meta-analysis. However, it is absolutely necessary that the authors are aware and clearly and transparently communicate the limitations of liquid ^{31}P NMR on NaOH-EDTA extracts. There are many other methods out there to study P cycling (Kruse et al. 2015), and only if results are correctly interpreted in light of the limitations of the methods can readers integrate knowledge gained from one method with knowledge gained from another method. Firstly, as already pointed out by reviewer 1 (first round), NaOH-EDTA extraction never extracts all soil P and often only a small portion of total soil P. Please state the fraction of total P extracted by NaOH-EDTA for all soils used in your analysis. Secondly, ^{31}P NMR is a tool for P speciation, providing important information on P stocks, it does not, however, provide information on dynamics. Turnover, exchange kinetics, mineralisation rates, etc, which is what I understand under "P dynamics", can only be assessed using isotopic techniques (Frossard et al. 2011). Thirdly, liquid ^{31}P NMR is not the preferred method for studying inorganic P species, since it cannot give information on predominant inorganic P species such as Fe-, Al- and Ca-phosphate. XANES is a more preferred method for looking at inorganic P species. This does not mean that the results on pyrophosphate, orthophosphate and polyphosphates concentrations are not useful, however, the paper reads as if these are the only inorganic P species of importance in soils. Please be more honest on the potentials and limitations of this approach.

These limitations need to be stated clearly and transparently in the introduction, since they provide the scope for the meta-analysis.

Precision and clarity of terminology

As mentioned above, the word "dynamics" is misleading for a study looking at P forms. I suggest changing the title to something more accurate, e.g. "The impact of soil, climatic, and temporal drivers on inorganic and organic P compounds".

An often-recurring term is "complex P compounds". From my understanding, "complex" is used to refer to high-molecular weight organic compounds of variable composition (McLaren et al. 2015). Please use a more precise term than "complex P compounds".

Discussion section

The discussion section remains the main weak spot of the article. As it stands, the discussion could be summarized by Fig 1 from the introduction. This is a problem because Fig 1 is a very generic and commonly used figure in the field. I suggest the authors follow Mensh and Kording (2017), who provide useful tips for structuring a discussion (Mensh and Kording 2017).

There follows a few ideas for rewriting the discussion that authors may use if they find them helpful. The first paragraph can more or less stay as is, it summarizes the results. The next two paragraphs could be dedicated to outlining the limitations of the analysis. Here it would

be worth pointing out that climate and weathering drive soil properties, so that it is not entirely appropriate to compare their influence on P forms, because much of the variation in soil properties can be explained by climate and weathering. Also, it might be worth discussing why variation in polyphosphates could not be explained by the models. The final two paragraphs could point out how this study adds to the literature. The focus of these paragraphs should not be comparisons to individual NMR studies, since they are anyways incorporated into the data, rather it would be interesting to weave the findings together with insights from studies using XANES, enzyme activities, or isotopes, to start painting a full picture.

English language
Authors stated that a native English speaker revised the manuscript. However, I don't think that the quality of the writing has improved from the previous version. There are still many grammatical issues and awkward writing, which make the manuscript difficult to read at times.

References

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