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Interactive comment on "Distribution of phosphorus fractions of different plant availability in German forest soils and their relationship to common soil properties and foliar P concentrations" *by* Jörg Niederberger et al.

Anonymous Referee #1

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General comments:

The paper of Niederberger et al. al addresses a very important issue for both forest ecologists as well as forest managers, namely the identification of a suitable soil P (fraction) assessment which permits a direct precise judgment of the nutritional status of forest trees. This issue is currently getting ever more important, because many forest stands in Europe apparently are drifting from former N limitation into P limitation.

In the paper of Niederberger et al., the well-known Hedley P fractionation method is

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applied for a large number of forest soil samples, which constitute a large portion of a nation-wide soil and forest nutrition inventory in Germany. The ample work allowed a test whether and with which precision the P nutritional status of important forest tree species types in Germany can be predicted by different Hedley fractions performed on mineral topsoil samples. To my knowledge such a large-scale assessment has been performed for the first time.

The main result of the paper is that – unfortunately – none of the different Hedley fractions was strongly correlated with the P nutritional status of forest trees as assessed by foliar analysis. To me this is not too surprising because both the forest floor as well as the subsoil which both are of critical importance for the P supply of forest trees particularly under conditions of poor P supply have not been included in the study. Nevertheless I feel that the study is an important step forward, in increasing our knowledge on interrelations between soil P fractions and tree nutrition – at least it convincingly shows that Hedley fractions obtained on mineral topsoil horizons enable a rough estimation of the P nutrition status of Pinus sylvestris, but are inappropriate to characterize the P nutrition of Picea abies and Fagus sylvatica. I therefore recommend publication of the paper after the concerns and issues raised below are addressed and the manuscript has been improved accordingly. Perhaps some of my recommendations might even result in an increased predictive power of the Hedley fractionation results.

1. The paper is rather long and sometimes cumbersome to read. It should be shortened by 20-30% to attract more readers and focused on the most relevant issues.

2. Soil texture and SOC at least in a given depth increment most often are strongly correlated with each other, because low sand and high (silt)/clay contents favor SOC accumulation by formation of organo-mineral associations and aggregates which impede SOC mineralization. This multicollinearity effect could be used either form an amalgamated predictor or to remove one of the two variables in order to shorten the paper.

3. The authors should test whether splitting the sample collective into 2 sub-collectives (non-calcareous soils [pH < 6.5] vs. calcareous soils [pH > 6.5]) may improve the predictive power of the Hedley fractions for characterizing the P nutritional status of the trees.

4. Moreover, I suggest to test whether the calculated total topsoil stocks of the different Hedley fractions (the latter should be available according to the statement made in page 12, line 19/20 of the manuscript) may improve the predictive power of the Hedley fractions for characterizing the P nutritional status of the trees.

5. There are several papers dealing with the relation between (operationally-defined) soil P fractions and the P nutritional status of German forest ecosystems, whose results could be compared with the results of the Hedley procedure. For example the papers of Prietzel and Stetter (2011), Prietzel et al. (2013; DOI 10.1007/s11104-014-2248-9); and the recent paper of Manghabati et al 2018 (JPNSS; DOI: 10.1002/jpln.201700536) all found a good predictive power of citric-acid extractable soil P on tree P nutrition, whereas HCO3 was suitable only under particular conditions (Manghabati et al.).

6. At least in some pages, the paper contains a lot of typos/spelling mistakes and sloppy grammar. For example, page 10 reads

L2 “showed significant higher amount of organic P forms” L5 “influence of microorganism” L13 “These effect could also be observed” L13/14 “this increase (. . .) were (. . .) not significant”

The English should be brushed up before resubmission.

Specific Comments

P2 L34: Reference De Schrijver et al 2012 is missing in the Reference Section

P3 L10: C content. Is this total C or organic C? Should be clarified. The text on P4 L $\frac{3}{4}$ suggests that “C content” means SOC, whereas the large Max C/N values (437; 61) presented in Table 1 indicate that at in the calcareous soils C content included

inorganic C in addition to SOC.

P4 L9: In deciduous tree stands, leaves are always from the current year

P4 L22: Nitric acid digestion does not completely retrieve total P, because Si-bound P is only partially mobilized. Underestimation is between 15 and 37% (Schwartz & Kölbel, 1992; Z Pflanzenernähr Bodenkd 155: 281–284; Hornburg & Lüer 1999; J Plant Nutr Soil Sci 162:131–137.

P5 L3: Has the MWU-Test been corrected for multiple comparisons? Please indicate!

P13 L3: forest floor mass and Corg/Porg ratio or P , Porg content should be used

Figure 2: see comment to P4 L22

Figure 3: Nice figure.

Table 2: Pi residual: Is this really Pi or may it also include Porg which is liberated and converted into Pinorg by nitric acid/H2O2 digestion?

Table 2; Table 5: As the data are non-normally distributed would it make sense to describe the variation in box plots rather than by standard deviation, which requires normal distribution?

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