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Interactive comment on "Phosphorus fertilisation under nitrogen limitation can deplete soil carbon stocks – evidence from Swedish meta-replicated long-term field experiments" *by* C. Poeplau et al.

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This paper uses long-term fertilisation experiments (replicated across 10 sites in central and southern Sweden) to examine the SOC balance in arable systems under different fertilisation regimes with a focus on potentially interactive effects of N and P. The key finding is that higher P application may reduce SOC in the absence of N addition and consequently this is very topical given huge interest in stoichiometric relationships in soils. There is much speculation as to the mechanisms at play (though these are well covered in the discussion) and I think the data present more questions than answers but overall it is a very interesting study and it would certainly stimulate further debate

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in the literature.

The following are issues which I think once addressed would improve the paper:

1) The end of the introduction reads awkwardly. The final sentence could be expanded so that the study's questions are clearer;

2) Further detail is needed in the methods: a) How were soil samples treated following sampling? b) Were there any differences in protocol of dry combustion between initial and final samples? c) How was total C converted to SOC?;

3) "We did not find any negative effect of PK fertilisation on SOC stocks in the presence of nitrogen fertilisation (data not shown)." – I think it's important to show this data as it would allow the reader to assess relative context of the other treatments;

4) While the multiple regression is a good approach to uncover potentially important factors, the significant factors are not consistent at different levels of PK. However, in the discussion on the relative importance of different potential mechanisms this is suggested to support several of the potential mechanisms. This line of argument seems quite weak and I feel this part of the discussion needs to be amended or expanded;

5) Have the regressions in Figure 2 been done using the 3 datapoints presented or using more individual datapoints? If they use only 3 datapoints then I think they are rather suspect and if they use more datapoints this needs to be explained in the legend. To allow a reader to gain a better bivariate perspective I would suggest presenting error bars along each axis using data at greatest resolution;

6) I'm not sure in the value of presenting the data in Figure 3 as it is. What are the measured v modelled graphs intending to show that is not given with R-squared/P-values? One suggestion is to amend the datapoints to provide some other information e.g. datapoint size could be dependent upon the most important explanatory variable.

Specific points: P16529, L4: Some other references, in addition to Lugato et al (2006), may be useful to support this broad statement. L11: Kätterer et al 2012 is not in the

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reference list. Is this the correct citation or is reference missing? L17: Kätterer et al 2013 is not in the reference list. Is this the correct citation or is reference missing? L27: Change to ‘constraints’. P16530, L17: Change to ‘counterbalance’. P16533, L7: on SOC stocks or stock change? P16534, L1-2: This sentence is more appropriate for the methods/statistics section. L23: Change to ‘. . .after long-term fertilisation and even 1N0PK lead to slight losses. . .’. L24: ‘traced’ to ‘tracked’.

Interactive comment on Biogeosciences Discuss., 12, 16527, 2015.