

***Interactive comment on “Effects of precipitation on soil acid phosphatase activity in three successional forests in Southern China” by W. Huang et al.***

**Anonymous Referee #1**

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**General comments** This manuscript presents data from a precipitation experiment in three different forest types in Southern China. In the context of global climate change such experiments are certainly valuable. The present manuscript shows the effects of at least 2 years of different precipitation regimes on phosphatase activity, available P and other soil properties. My main criticisms are: 1) the data base is rather limited with only two sampling points in time. And 2) more importantly: what does phosphatase activity really tell us? In this study on acid soils (about pH 4) it was measured at pH 6.5. . . Is this really acid phosphatase activity? Does it come from microbes or from plants or from both? This should be kept in mind also during the discussion (in particular p. 168 l. 20-25). In fact, phosphatase activity was negatively correlated with available P. This

C82

could mean a repression of phosphatase synthesis, or it could simply be related to microbial biomass. . . (see next comment). The precipitation regimes have a substantial effect on microbial carbon. Thus one should also look at specific phosphatase activity (per unit microbial C). Some results are a bit awkward. For example, total P changes between wet and dry season -how is this possible? As far as the statistical analysis is concerned, I don't understand why the correlations are only done per season and not over the whole data set. The discussion is not entirely convincing and in some parts not very clear (see specific comments below). Importantly, the N input with the rainfall or its effects on available N in the ecosystems should have been measured. In conclusion, I expect major revisions of the manuscript according to these general comments and the more specific ones below before it can be published.

**Specific comments** Abstract p. 158 l. 5: do we really have good proof that phosphatase activity reflects the capacity of P supply to ecosystems??? This neglects the entire inorganic side of P supply, and even for organic P I am not convinced. . . p. 158 l. 7: experiment with precipitation treatments p. 158 l. 17: these results indicate that. . . p. 158 l. 20: the conclusion about reduced P supply because of lower phosphatase activity is not well justified - in fact, available P is lower in moist than in dry soils, isn't it? And other factors such as limited diffusion would probably be more important to determine P supply. . . p. 159 l. 13: phosphatase activity cannot be applied - enzymes can be applied (but this is not what you mean), or the method can be applied p. 159 l. 21: have any investigations been done? Then you should name them. p. 160 l. 1: such as the Mediterranean, dry conditions lead. . . p. 160 l. 13: in response to p. 160 17-20: difficult to read. Substitute the hyphens with brackets or commas. p. 160 l. 26. What is meant by different P requirements? p. 161 l. 1: than in the dry season p. 161 l. 4: what is meant by "patterns of effects" - this is very imprecise. In fact, the entire hypothesis 3 is not clear to me. p. 162 l. 14. Slope aspect p. 162 l. 25: what is a PMKit? You should give the measurement principle (e.g. TDR) p. 163 l. 26: and swirled the flask slightly p. 164 l. 16: did you analyse separately (not separated) for each season AND forest, or only per season? p. 164 l. 16: correlation coefficients cannot be performed,

C83

only calculated. p. 164 l. 22: what do you mean by "dry" - 10% in some cases is not really dry. . . p. 165 l. 4: please give also the water holding capacity of the soils in the three forest types. By the way, information about the texture of the soils is missing. p. 165 l. 11. Insert reference to Table 1 at the end of this sentence. p. 166 l. 17: what do you mean by "not remarkable": significant or not? p. 167 l. 12: does not dose p. 169 l. 17: but this is not true - available P was negatively correlated with phosphatase activity during the wet season! p. 169 l. 20: what is end production of phosphatase? p. 169 l. 25: key limiting factor p. 169 l. 26: I don't understand this sentence. p. 170 l. 1: what do you mean by wholesome self-regulating mechanisms? In my opinion, for the discussion of MEBF it is important that the soil in the NP treatment was still relatively moist in this forest type. p. 170 l. 12: but the difference in pH was significant only for MEBF in the wet season. p. 171 l. 17-19: you are saying the same thing twice (phosphatase activities and forest succession are positively related) p. 171 l. 27: the conclusion about P limitation is not valid (in relation to phosphatase activities) Table 1: please show also the statistics between the seasons.

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