Biogeosciences Discussions

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Comment on bg-2022-41

Richard marinos (Referee)

Referee comment on "Climate and geology overwrite land use effects on soil organic nitrogen cycling on a continental scale" by Lisa Noll et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-41-RC2, 2022

In this work, Noll et al. examine controls on protein depolymerization rates, a known key step in the production of LMWON compounds that can be used by microbes and (sometimes) plants. They find that substrate availability is a key control on depolymerization, and in turn they identify soil pH, MAP, and Al/Fe oxyhydroxides as key controls on substrate availability. The study is a wide-ranging longitudinal soil survey across Europe, from the Mediterranean to the Barents Sea. They find that land use has a negligable effect on substrate availability and depolymerization rates, somewhat suprisingly. They reached these conclusions through a combination of anova/linear regression approaches and structural equation models.

The observational breadth and depth of this study is quite impressive. Fourty three sites across Europe were sampled, and exhaustive chemical and biological analyses were performed on the soils. The key measurements are well-supported from a theoretical standpoint. This seems like a monumental effort that was well-planned and carefully executed. The manuscrupt definitely needs some honing to make the central story stand out more, though. I also encourage the authors to rethink their statistical approaches; I'm not asking them to redo all of their analyses, but I think a shift in emphasis toward highlighting the analyses that deal with the highly correlated nature of the predictors, is warranted. The paper also needs to be brought into compliance with EGU's data policy.

I honestly really struggled through reading this paper. There are SO many measurements taken, and the results are presented in such exhaustive detail, that I found myself losing the thread often and wondering why data was being presented / what the main thrust of the argument was. I very strongly encourage the authors to revisit all of the topic sentences for each paragraph and make sure that the conclusions that should be drawn from a paragraph are clearly stated up-front. I also encourage the authors to think very carefully about what data are actually central to the story, and to shunt a lot of their other results to the supplement.

Regarding the analyses, the authors are dealing with a ton of very highly correlated predictor variables, an issue which they recognize. They lead their results and discussion, though, with exhaustive treatment of single-variable ANOVAs (over sixty ANOVAS) which do not do justice to this rich but highly correlated predictor dataset. The authors have a very nice conceptual model (Fig 1) that is very nicely examined through an SEM. I think that this should be the centerpiece of the story! Some ordination approaches also would make more sense to me in terms of understanding the highly correlated nature of the data, rather than picking apart tables of bivariate correlation coefficients.

This is a nice body of work, and some careful editing will go a long way to making the story in this paper shine. (I'm also sorry the authors have waited 5 months and had many declined review requests. Frustrating!)

Best wishes,

Richard Marinos, U @ Buffalo

Line items:

180 - I don't understand what the other factor, besides land use type, is in these models. More broadly, this analysis scheme doesn't make too much sense to me... your conclusions are that bedrock type is a key driver of depoly rates, and land use type is not. But bedrock type was only subject to a 1-way anova, while land use is subject to a 2-way anova which controls for bedrock type/climater. Why, for example, was the effect of bedrock type not analyzed with a 2-way ANOVA that controlled for the effects of land use? Given relatively low sample #s, it is unsurprising that there is not enough statistical power to detect an effect of land use type in a 2-way ANOVA when controlling for bedrock/climate/soil type, but the bedrock type analysis was not subject to the same dilution of statistical power, so it seems to me that the conclusions are drawn from incommensurable statistical approaches.

205- Are the +/- numbers one standard error of the mean? Confidence interval? Please state at the first instance.

Figure 4 - Is there really a clear enough justification to use polynomial regression?

360 - I have a hard time wrapping my head around how Fe and Al oxyhydroxides can simultaneously increase SOM stabilization AND increase SON availability.

405 - "As demonstrated by partial correlations..." This statement makes an assumption that Al/Fe oxyhydroxides and pH are the TRUE controls, and MAT is just a latent predictor, which I don't think has been fully justified.

468 - Biogeosciences requires data to be published in a FAIR repository, or else have the reasons for data remaining unpublished be clearly explained. This is statement is not in compliance with those requirements. I also encourage the authors to archive their code.