This manuscript is an intensive study of soil stocks of carbon, nitrogen, and phosphorus, as well as their stoichiometric ratios, along gradients of soil depth and forest succession. The scale of the dataset is impressive, with these nutrient stocks measured in 100 subplots in each of three forest types at three separate depths. However, the paper is weakened by three factors: an absence of rigorous hypotheses/predictions, poorly described methods, and statistical analyses that come across as 'fishing expeditions' rather than hypothesis tests. Below I provide suggestions for improving each of these aspects.

1. Framing of the manuscript – the authors justify their study by discussing potential changes in nutrient stocks and C:N:P ratios along gradients of succession. However, they fail to provide any justification for *why* these parameters should change with forest succession, and they do not appear to have any directional hypotheses. I understand why this may be so: throughout the process of succession, an ecosystem will experience changes in both abiotic and biotic conditions, which could be expected to have interacting influences on belowground processes. For example, as the canopy closes, soils may experience less insolation and more buffering from temperature extremes; meanwhile, the plant community may shift in such a way that the average chemical quality of litter inputs changes. Given the complexity of these processes, is there any reason to expect that soil stocks of C, N, and P (or their ratios) should exhibit generalizable, directional shifts along successional gradients? If so, what should we expect these patterns to be? If not, then how do the results of this particular study shape our understanding of feedbacks between plant communities and soil properties during succession?
2. Experimental design: Much of the statistical analyses focus on relating C, N, and P stocks to stand-level attributes (e.g. tree community diversity) as well as topography and soil texture. Nowhere in the manuscript are these measurements described. Were they taken from another study? Were these measurements taken at the level of the 10x10 m subplot, or at the level of the three 1 ha forest plots? If the latter, the multivariate models are severely overfitted. This brings me to my third point:
3. Statistical issues: Several aspects of the statistics appear to be poorly thought out. For example, in Table 3, correlation coefficients are reported between stocks of a single nutrient (e.g. SOC) and then the ratio of SOC and TN (C:N). By definition, these variables will be highly correlated – one is derived from the other. In Tables 4 and 5, the authors report a multiple regression with no fewer than fourteen explanatory variables, several of which MUST be highly collinear (e.g. the Shannon index and species richness). This comes across as a fishing expedition, not a rigorous hypothesis test, and it is nearly impossible to interpret the results of such an analysis. Similarly, why analyze both C,N, and P concentrations AND stocks? Does the concentration data provide any insight that the stock does not?

The discussion is extensive, and makes a great deal of generalizations that are probably unwarranted (e.g. 'a low C:N ratio implies that soil organic matter is accumulating slower than it is decomposing;' 'a C:N ratio lower than 10 indicates that less organic matter is being merged into the soil.' These simplistic statements belie an understanding of how plant litter C is incorporated into SOM). Individual significant correlations are discussed, but there is no synthesis that relates these patterns back to the specific successional trajectory of this forest ecosystem.

There is a large amount of data here, and there is absolutely the potential to say something valuable about soil nutrient cycling in relation to succession. However, the manuscript must be thoroughly revised in order to do the dataset justice.