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Interactive comment on "Comparing soil biogeochemical processes in novel and natural boreal forest ecosystems" *by* S. A. Quideau et al.

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This paper covers an important under examined subject: viz. which differences exist between soils under native vegetation on the one hand and in reclaimed and reforested post- oil/tar mining lands on the other in Canada. This topic fits perfectly in the scope of biogeosciences. The methodological approach is holistic and well adapted to study: 1° nutrient availability, SOM composition and microbial community structure were all assessed for 42 sites; 2°A very complete specifically-tailored statistical approach was used to maximally utilize this diverse multivariate dataset to answer the study's primary question. The statistical approach explicitly moves beyond the classical principal component analysis, but unfortunately has become difficult to follow for the majority of readers. The abstract, introduction and results sections are well written and very

clear to me. The discussion reads nicely and I like the structure in which a subdivision was made between environmental and human-induced controls on variability in SOM composition, the microbial community composition and nutrient availability. The one downside of the holistic approach is that in fact perhaps too little details are given on the specific differences in SOM composition and microbial community structure between the studied sites. I recommend publication of this paper after minor revision.

Remarks: -4.2 Since novel and natural ecosystems differed most strongly in their SOM content, I would like to see some more details about these differences. Provide details on the NMR bands which differed most strongly and try to explain these differences as well.

-I cannot really interpret Figures 1 and 2 since my knowledge of the applied statistical techniques is too limited. It would help if the authors could provide a more elaborate explanation of the rationale for using NMS instead of 'common' (PCA, DA) multivariate statistical procedures. A sentence like l172 'All ordinations attained a very low stress (< 10 in all cases) after 40-70 iterations.' will be meaningless to most readers and I hope that also in the results section the authors would be prepared to add some more explanation. Please help the readers with interpreting table 2 as a stand-alone source of information. This could be achieved by providing some additional guidelines on how to interpret the data in the caption or in the table's footnotes.

-4.1 It was not clear to me why the authors specifically wanted to analyze a >53 μm low density SOM fraction by NMR. This needs further motivation. I would like to see an expanded discussion of the NMR data as well: In the interpretation in lines 281-286, I would mention that we in fact would have expected most differences in SOM composition in this 'young' SOM fraction amongst the different vegetation types. For example, an obvious hypothesis could have been to expect more alkyl C under the spruce and pine vegetation when compared to the aspen vegetation.

Specific comments: Line 136 μm instead of mm. Also specify the density cutoff used.

Line 284 why suddenly make a link to carbon availability. This was not assessed in any way here Line 290-293. I do not see the link between the start and the end of this sentence. Which three PLFAs are you mentioning? What is meant by this?

Interactive comment on Biogeosciences Discuss., 10, 7521, 2013.