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Interactive comment on "Boreal forest soil chemistry drives soil organic carbon bioreactivity along a 314-year fire chronosequence" *by* Benjamin Andrieux et al.

Anonymous Referee #2

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In this manuscript, the authors use 1-year soil incubations to analyze how "bioreactive" and "recalcitrant" soil C pools vary over a 350 year fire chronosequence. They use a linear regressions and confirmatory path analysis to test hypothesized cause-and-effect relationships between the soil pools and other soil chemistry variables. In general, the manuscript is informative, well-supported, and easy to read.

General comments:

1. I would like to see the correlation between incubation-derived estimate of Cslow and the acid-insoluble residue as a figure, since the latter is often used as a proxy for the former without direct comparison. In this manuscript, it's not clear which estimate of

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Discussion paper

bioreactive/recalcitrant C is used in the models and in the figures.

2. Why is soil texture hypothesized to influence moss dominance in the causal models?

3. Moss community composition (Sphagnum vs feather moss) is included, but is there any difference in moss abundance that could be included in the model? Even non-sphagnum mosses have distinct biochemistry and low decomposition rates compared to vascular plants.

4. The conclusion contains lots of new analysis not included elsewhere in the manuscript and would be better presented as an additional discussion section

5. Discussion of temperature generates some confusion about what is actually being measured. Since the incubation temperature (26°C) is much warmer than the MAT of the study site, the bioavailability assays correspond to "potentially available" C more than a realistic estimate of in situ soil respiration. This is a reasonable choice but leads to some confusion in the introduction and discussion:

-The value of analyzing the recalcitrant SOC fraction is justified in regards to the C-quality temperature hypothesis, since "recalcitrant" C should respond more to warming. But, since the incubation temperature is 26C, the "recalcitrant" C that is actually measured is SOC that is preserved even when temperature is increased to unrealistically high levels.

-Lines 378-383 seems to suggest that climate does not drive SOM decay rates or transfer between SOC pools, which is not supported by the study.

-The connection between these results and the temperature sensitivity of soil respiration (lines 385-390) is also unclear. This section could be improved by discussing the relationship between hypothesis FH1 and the C-quality temperature hypothesis, and the implications of the results for the validity of the CQTH.

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