EGUsphere, referee comment RC1
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Comment on egusphere-2022-136

Anonymous Referee #1

Referee comment on "Effects of a warmer climate and forest composition on soil carbon cycling, soil organic matter stability and stocks in a humid boreal region" by David Paré et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-136-RC1, 2022

The objective of the present study was to assess changes in SOC stocks, quality and C fluxes to and from the soil along a climatic gradient occupied by two dominant and important stand types: balsam fir and black spruce. The more intensive aspects of the study leverage four black spruce and three balsam fir sites along the climate gradient, and flux measurements occurred between 2-4 years in duration, with some variation in frequency across the sites. While the climate for the study region is deemed "humid" throughout, there appears to also be a gradient in precipitation (Table 1). The data presentation and objectives are fairly straightforward and should be of broad interest to boreal ecologists. There are a few areas that should be clarified in minor revision prior to publication, however. (I) There are some methodological aspects that were not clear to evaluate, at least to me. It could be these are better explained in prior works by this team (e.g., is soil n only n=5, and is this enough power to assert changes?; why was a fixed value of 2 used for Q10, and yet Q10 was also determined directly?), but it would be good to clarify in this text. (II) There are now quite a few gradient studies examining C fluxes and soil C stocks in boreal conifer forests, including other work briefly mentioned in the text for Canada (for example, Boreal Forest Transect Case Study- Price and Apps), but also for Alaska and Fennoscandia, which would be great to discuss for context. This context would help in explaining co-variates with temperature along the gradient. As such (III), it would be really nice if the authors could somehow evaluate the covariance of changes in precipitation and temperature along the climate gradient. I believe these issues should be addressable in revision, and otherwise offer comments by line number, below, and hope they are helpful.

Line65: Appalachian Mountains?

Table 1: The mean “annual precipitation” appears to differ by “site” along the gradients. Some statistical exploration of this would be good.

Line149: The “L” layer was not sampled? I think this needs to be justified. There could be big differences in the L layer (Oi soil horizon) in spruce vs. fir forests.

Line151: I don't understand: 5F+5H+5mineral is 15, and site n is still equal to 5. Is n=5 sufficient to capture site level variation for these systems, without bulking or taking composite samples? For example, n=5 in Pare et al., 1993, but each "n" was the bulk product of 3 replicates (as such, 15 cores per site were taken). Ziegler et al. (2017) bulked 9 cores per site in their gradient study.

Line225: Excuse my ignorance, but I don't see why an assumed Q10 value would need to be used (contradictory to your equation 2, above)?

As far as I can tell, it was Rayment and Jarvis (2000) who nominated the relatively consistent Q=2 for black spruce. Since you are comparing across gradients and two dominant species cover types, I would recommend using your measured Q10, as in equation 2.

Line285: Regarding the assertion that there were no effects of “climate”, does this include precipitation? Can you be more specific?

Table 3: It would be so much better if precipitation was included in this analysis.

Figure 3: See for context, Kane ES, Valentine DW, Michaelson GJ, Fox JD, Ping C-L. 2006. Controls over pathways of carbon efflux from soils along climate and stand productivity gradients in interior Alaska. Soil Biology and Biochemistry. 38: 1438-1450.

Vogel et al. 2008. Carbon allocation in boreal black spruce forests across regions varying in soil temperature and precipitation. Global Change Biology.

Line371: Regarding the “uncertainty”, I think it would be appropriate to have a nod to the low apparent power (soil n=5) for soil sampling in this study.

Line375: This may be true, but as stated this is a bit of an over-simplification. As you state below, there are other factors varying here besides just "climate" in these studies. In Fennoscandia, the latitude gradient is confounded with N deposition. Moreover and as discussed in the Ziegler paper, precipitation co-varied with the climate gradient in Norris et al. 2011. Across climate gradients in AK which controlled for precipitation, texture, and had similar N deposition, soil C declined with increasing soil growing degree days (Kane et al. 2005; Kane and Vogel, 2009). See also, earlier Vogel et al., 2008 GCB reference.

Line395: “We were not able to explain the large variability in soil C stocks across sites. This property is highly variable at small scale and has notoriously been difficult to map (Paré et al. 2021). A much larger dataset would be required”

This is a very important point. If you are not capturing the variance at each site, can you assert that soil C stocks are truly not changing across the gradient? A quick power analysis could answer this question.

Line405: “Both species also showed a stable litter C:N ratio along the climate gradient, suggesting

that the stoichiometry of C to N is not affected by climate.”:

This is really interesting!

Line445: See earlier comment about Q10 being fixed at 2, and kindly disregard if I was off base there.

Line491: “Our results show no evidence of net SOM losses or a reduction of the most active SOM

fraction with a warmer climate”

Can this be said, if the site level variance in SOM stocks is not being captured (vis a vis, line 395)?