

Response to authors comments :

We thank the reviewer for their insightful and constructive comments! Comments and replies are copied here and also in the attached file which may be easier to read (color-coded).

Comments: 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.

We thank the reviewer for their insightful and constructive comments!

1-Role of precipitation/aridity:

The design involved the selection of sites along a mean annual temperature gradient. We did not pay much attention to the role of precipitation because the study area is within a wet climatic region with few limitations of ecosystem processes due to water availability.

However, we concur with both reviewers that the role of precipitation should be considered more carefully because it is a global concern and we do have some potentially useful data to discuss this issue. This is what we propose: We will add an aridity index to the description of sites (Table 1). We used the Penman-Molteith equation as recommended by FAO (<https://www.fao.org/3/x0490e/x0490e00.htm#Contents>) the ASCE standardized reference evapotranspiration (https://www.mesonet.org/images/site/ASCE_Evapotranspiration_Formula.pdf) calculated daily from May to October over a 30 year period. We found out that for balsam fir sites only, there is a strong positive correlation between temperature (DD) and aridity: warmer sites are dryer ($R^2: 0.86; p < 0.0001$). This relationship is not significant for black spruce sites ($R^2=0.11; P=0.3537$) as our cold black spruce sites included both wet (high elevation) and drier sites (high latitude). We explored the relationships between aridity and soil C stocks, litterfall, and soil respiration. Significant relationships

were found only for fir sites between aridity and litterfall (a positive relationship: dryer = more productive). In addition, we also calculated RS10 (estimated soil respiration at 10°C i.e. respiration adjusted for temperature) for each plot measurement event, generating about 4050 point measures. We compared RS10 with soil water content of the soil top 20cm assessed with a TDR probe. No relationship was found between soil moisture and respiration between sites or within the season. We also explored these relationships at the site level and found the same outcome. We will discuss this aspect in the paper and we will add the relationship in the form of graphs with statistical descriptions in the supplementary material.

In summary, for balsam fir sites, we cannot distinguish the impact of aridity from that of temperature, because both are strongly correlated. However, because we did not find any significant relationship between soil respiration and soil humidity measured in the field and because we do not find significant relationships between aridity and soil C stocks or soil C cycling for black spruce sites and for all sites together, we may conclude that aridity does not play a major role in controlling C stocks and C cycling under the wet climatic conditions of this study. We thank the author for this comment and we think that this addition will strengthen the paper. We will refer to Kane and Vogel (2009) and to Vogel et al. (2008) that are useful to better frame the context of our study. They found a reduction in soil C storage with warming past a certain threshold. However, these studies were conducted in a much drier climate. Our study region has an aridity index comparable to Amazonia (Trabucco, A., and Zomer, R.J. 2018. Global Aridity Index and Potential evapotranspiration (ET₀) Climate Database v2). In addition, in Vogel et al. (2008) precipitation and temperature were positively correlated, while we observe the opposite. This gives support that the accelerated C fluxes and the absence of change in the soil C content that we observed with a warmer climate are likely the results of having no or little limitations of ecosystem processes by water. We will make this point clearer.

Line65: Appalachian Mountains?

2-the original text refers to the Southern Appalachians region

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

3-See comment # 1.

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

4-We only discarded the loose portion of the top litter because it is short-lasting and varies during the season. It represents a very small portion of the humus layer. We will add some precision to the text.

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.

5-Recognizing that soil carbon stocks are highly variable at the plot level, we would like to stress that our study sites only covered an area of 400m². Our sampling intensity is greater than what is used for national carbon inventories (NFI) and compared favorably well with scientific studies. We will make the sampling description clearer. (l. 150). We sampled 3 cores (not five as indicated) around each sample plot (5 per site). This generated 15 samples per soil layer (organic; 0-20cm and 20-40cm). Each sample was analyzed individually.

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)?

6-We estimated a Q10 value to compare sites along the climate gradient and between species. A Q10 of 2 was only used to interpolate the value of RS10 between measurement periods in the estimate of cumulative seasonal soil respiration (May to November). We will make this clearer in a new version. This methodology is derived from Lavigne et al. (2003). In fact, Lavigne et al. (2003) are citing four studies indicating that using a unique value of Q10 for the whole season can lead to overestimations. The rationale is that Q10 may change during the season. For example during periods of important root growth, it could be influenced by greater availability of root C to soil microbes. Nevertheless, the estimated respiration rate is the same on measurement day, regardless of the method used. It is only for the interpolation between measurement dates that they may slightly differ. In short, an RS10 is estimated for each site and measurement day with a Q10 of 2. This RS10 value (not Rs) is interpolated linearly between measurement days. To convert daily estimated RS10 to daily Rs values for no-data days, recorded soil temperature and a Q10 of 2 are used to back transfer Rs10 to Rs values. Finally, and recognizing that there is no standardized way of calculating these fluxes, we compared the two approaches, the one we used and the one using a Q10 that varies with site but that is the same for the whole season. The overall difference was 2% (the ratio of this second approach to the one we used ranged from 0.8 to 1.32, also suggesting a comparable but slightly skewed to higher values). We will refer to these results in the text and we would be happy to show the comparison in a table in the supplementary material.

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.

7-See above comment (6)

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

8-See above, we addressed this in point 1.

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

9-See above (1)

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*.

10-thanks for the suggestions, both similarities (increase NPP with temperature) and divergences (declining soil stocks with temperature vs no change in our study) are found; it is interesting to note that the climate of these studies is dryer and this may explain the divergence. We will introduce them in the discussion.

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.

11-See above comment (5)

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, and 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.

12-Interesting point! Two aspects, our climate is much wetter than those of that cited and in our study, at least for balsam fir, colder is wetter while in those studies colder was drier. (see point 10)

Line395: “We were not able to explain the large variability in soil C stocks across sites. This property is highly variable at a 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.

13-see (5) we think that our sampling intensity is adequate to capture within-site variability. However, we may add that what matters here is the large variability between sites.

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!

14-Thanks! However, we are not sure if we can make more of this observation; homeostasis in plant nutrition is common.

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

15-See comment above (6)

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)?

16-see (5) and (13)

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