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# *Interactive comment on* "Field-warmed soil carbon changes imply high 21st century modeled uncertainty" *by* Katherine Todd-Brown et al.

**Anonymous Referee #2**

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This is a great study, in which Todd-Brown et al. creatively combined experimental soil warming data with Earth system model outputs to illustrate the observation informed uncertainty in future responses of soil organic carbon to warming. I'd recommend publication after addressing a few issues outlined below.

1. While it's hard to move away from steady state assumption (it leads to very convenient mathematical forms), I think it is important to at least illustrate the bias that steady state assumption can have on estimated Q10. It would be hard to do for observed SOC responses to warming without very crude assumptions about base turnover rate, but seems realistic for the ESM output. The term on the left side of equation 1 can be extracted from the ESM output and used instead of 0, and baseline k as well as Q10's can be reverse-engineered. I think the steady state bias could be quite substantial,



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because the 10% (or even 1%) difference between C input and output can have a cumulative effect over time. With observation-derived Q10 estimates, I suggest excluding sites that are with high degree of certainty are not in steady state. An example is Delta Junction site in Alaska, which experienced stand replacing fire in 1999. Even though warming experiment lasted for 10 years it takes many decades for the SOC to recover after a stand replacing fire (Fu et al., 2017), so equation 4 would not be appropriate for this site.

2. This leads me to equation 4. I would strongly suggest not omitting the ratio of control inputs to warmed inputs. As authors rightly pointed out, if ratio is less than 1, Q10's are going to be underestimated, and if warming leads to decrease in NPP, the Q10's would be overestimated. A meta-analysis study by Wu et al. (2011) illustrated that warming increases total NPP on average by 15%, and belowground NPP by 52%, which has very substantial implications for Q10 (please see the attached image illustrating the effect of change in productivity on Q10's for Delta Junction, AK). I think looking up changes in NPP estimates for every site would make the observation-based Q10 estimates more defendable.

3. Lastly it wasn't completely clear to me why authors chose to estimate observation-based Q10's with the method described in section 2.3 instead of directly solving for it using equation 3 or 4. Could you please touch on that in the updated version of the manuscript?

Below, please see a list of minor issues:

P1L25: "stimulate"?

P1L27: "(the opposing carbon flux)" can be omitted in my view

P3L20: It seems that the model is not accurately specified: vector C(t) of size n by 1 cannot be multiplied by n by n matrix K. The accurate specification of the negative term would be "A*(Q10*K)*C(t)"





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P3L27: “scalar”?

P4L2: “vice-versa”?

P6L16: “data-driven”?

P6L18: “grid cell”

P8L10-11: Please revise the topic sentence, it’s not very clear.

P8L16: did you mean Figure S3?

P8L25: typo: “ESM-Q10”

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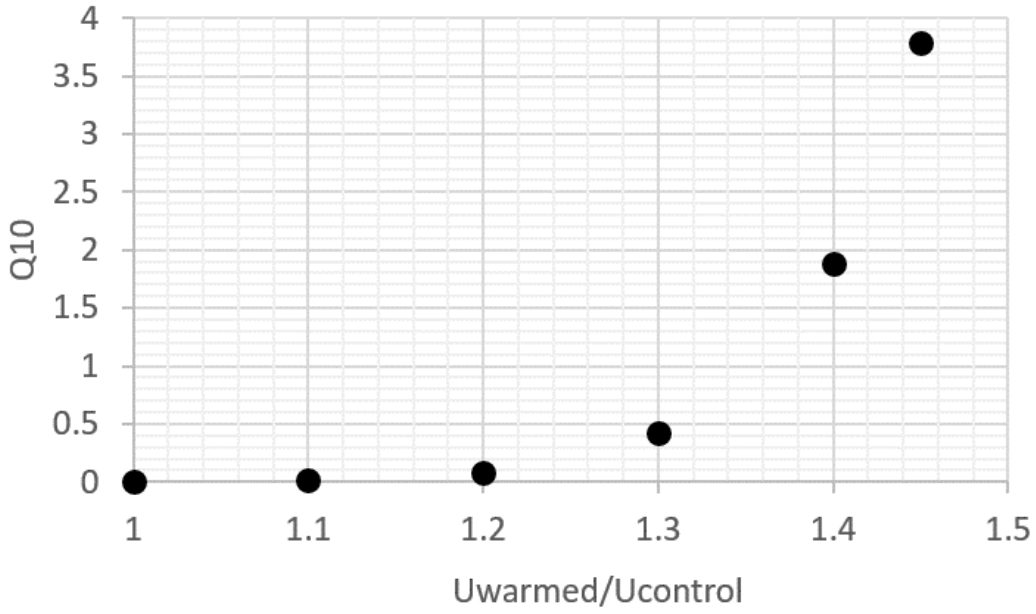


**Fig. 1.**



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