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Interactive comment on "Warming increases soil respiration in a carbon-rich soil without changing microbial respiratory potential" *by* Marion Nyberg and Mark J. Hovenden

Anonymous Referee #2

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Please see PDF for comments with correct formatting:

Review of "Warming increases soil respiration in a carbon-rich soil without changing microbial respiratory potential" by Nyberg and Hovenden.

The study presents results from an established warming experiment in carbon-rich soils located in Tasmania. It investigates the mechanisms that drive warming-induced responses in soil respiration, and whether changes in the plant community or changes in microbial soil respiration potential are important drivers. The researchers used both field manipulation and laboratory incubation experiments. There was a consistent effect of warming over time and across all plant community treatments in the field experi-

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Discussion paper

ment, suggesting that plant community composition manipulations do not influence soil respiration responses to warming. Warming also did not affect microbial respiration in incubation experiments. They conclude that the warming response is most likely due to increased autotrophic respiration and more labile substrate availability to the rhizosphere. Overall the study presents novel results, is well-written and represents obvious effort and contribution to the field. There are few studies that investigate the mechanisms by which soil respiration will increase as a result of warming in carbon-rich soils. Yet, these soils are most likely to contribute to CO2 efflux when warmed. I have a few suggestions for the authors to consider that may enhance their message: âĂć I found it a bit strange that plant community composition data was not presented. In the Discussion (line 475) they state that removing the dominant species did not appear to cause any functional shifts in the community, and that the dominant species may have been replaced by a functionally-similar species. If the authors are able to present community composition data after 1 year, that would help clarify whether the overall plant community changed in some way after removal of the dominant species vs. random removal vs. the control. It would answer the question of whether the plant community was really altered enough to expect possible changes, or whether the plant community treatment just wasn't strong enough to elicit changes. âĂć It's unclear in the Methods whether the removed plant biomass is replaced on the plots as litter or just completely removed. Please clarify this point. âĂć The models presented in Fig 3 and Fig 4 came across as an off-shoot from the main story. If developed further, perhaps including data from similar studies conducted in other soil types, I thought that those two figures could be expanded into a different manuscript that is more broad-reaching. I suggest you remove that information from this paper and just focus in on the experimental results. âĂć I was surprised that there wasn't more of a mention of the effect of warming on soil microbial community composition in the discussion. There is a broad base of literature on this topic, and it is likely that warming/drying not only alters microbial physiology but also community structure. I suggest you expand the background literature and Discussion to address this point. For example: https://doi.org/10.1098/rstb.2019.0112

A few minor edits: Line 11: capitalize Earth Line 17: due to plant community Line 34: it has been suggested Line 53: This would be more effective if you specifically mention Century model examples Line 120, this sentence is unclear: The experiment consists of forty 2 x 2 metre plots, with 3 metres between each plot, of which 20 were warmed using hexagonal polycarbonate 120 open-top chambers (OTC) with an internal diameter of 1.5 m, with the remainder of being unwarmed, ambient plots. Line 123, sentence starting with: "To control for possible effects of removing. . ." is run-on and difficult to understand. Revise. Table 1: indicate significant differences Line 268-270: "Neither removal, i.e. neither dominant nor random biomass removal (F2,33=0.89, P=0.42), nor a warming x removal interaction (F2,33=0.57, 270 P=0.57) affected CO2 efflux, as indicated by ANCOVA.", is awkwardly worded. Please revise. Line 401: Change to: There are 4 possible mechanisms whereby Rs could have increased (or similar)

Please also note the supplement to this comment:
https://www.biogeosciences-discuss.net/bg-2020-144/bg-2020-144-RC2-supplement.pdf

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