

## ***Interactive comment on* “Rainfall intensification increases the contribution of rewetting pulses to soil respiration” by Stefano Manzoni et al.**

### **Anonymous Referee #1**

Received and published: 14 April 2020

I reviewed the manuscript n. bg-2020-95 entitled “Rainfall intensification increases the contribution of rewetting pulses to soil respiration”. In this manuscript authors assess the validity of a stochastic model for soil heterotrophic respiration to explain the effects of precipitation variability on soil CO<sub>2</sub> fluxes. The manuscript is very well written and the rationale for the need of this mode is well formulated. The theoretical considerations are sound, and the equations look correct. Authors also address the limits of their model in the discussion section (which is important when the readers will try to link to the model to their own experiment or for general extrapolation) and tend to not emphasize too much the findings of their model given the discussed limitation. Nevertheless the study is important because highlight an important aspect, which is the growing importance of rewetting events depending on precipitation distribution. This is an aspect

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that is often overlooked, especially in manuscript dealing with drought, and highlight the importance of context dependent effects. I have no reserves in recommending this paper for publication, with only minor revisions and suggestions from my side. This has happened to me rarely and therefore I must compliment the authors for the excellent work.

GENERAL COMMENTS: My general comments are mainly small recommendation and suggestions:

1) I would recommend the authors to be more careful when they refer to soil respiration and to heterotrophic respiration. Indeed, in all their theoretical background, consideration is given only to microbial respired carbon and therefore neglecting the contribution of plant roots to drying and rewetting events. While this is fine for the paper, I would urge the authors to refer to heterotrophic soil respiration rather than to soil respiration mostly throughout the paper. 2) I understand that for simplicity authors calculate average parameters between soil with high OC and low OC (L. 233-235) for further theoretical considerations. However wouldn't be also very interesting to get parameter for both (high and low OC) and understand whether we can draw similar conclusions in both cases? Also this would make it more easy to then look at the larger picture, as you in part address (L. 356-358), that systems that have low precipitation often have low C content, and identify if these type of ecosystems would be really less vulnerable to increased precipitation frequency or not. 3) I would suggest the authors to expand their discussion on plant contribution. Indeed, as plant respiration (or better, rhizosphere respiration) covers, often, the greater proportion of C derived from soil, it would be interesting to discuss more in details how would plant respiration contribute to these effects. Also another thing that the authors could discuss in this context is that all the theoretical background and the parameters that they take into consideration emerge from either laboratory incubation (therefore no plant contribution at all), or in field experiment where they calculated the contribution of heterotrophic to total. However in both these cases it is completely neglected the heterotrophic contribution to respiration

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of freshly produce plant compounds. I know this parameter is theoretically impossible to measure, but I think it is good to point it out. 4) Point number 3 could be further linked with a small discussion on the effects of changes in precipitation depending on the season. Indeed, as the magnitude of respiration depend on temperature (and also plant input) and also the rate at which soil dries, I guess we would expect to have different results in different seasons. This could be briefly discussed.

SPECIFIC COMMENTS: L.230-231 Could you provide the graph were you compare the respiration normalized by the amount of SOC?

L.388 observed by who? The author in the cited paper? Please specify.

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Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-95>, 2020.

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