

Interactive comment on “Can current moisture responses predict soil CO₂ efflux under altered precipitation regimes? A synthesis of manipulation experiments” by S. Vicca et al.

Anonymous Referee #2

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The manuscript addresses a very interesting question, using a very robust and thoughtful methodological approach. Vicca et al. aim to evaluate to what extent the actual relationship between soil moisture and soil CO₂ efflux (SCE) can be used to predict the response of SCE to altered rainfall patterns. The paper is topically appropriate for this special issue, and it might make a suitable methodological contribution to be taken into account for future experimental and modelling works in relation to this topic.

However, I miss a more direct answer and discussion to the question presented in the title. From the initial 58 experiments considered initially in this work, for one season or another, only data (temperature, moisture and SCE) for 31 of them could be extrapolated to predict SCE in plots exposed to a different precipitation regime. According to

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these results the answer to the question could be “NO” (at least with data provided by the experiment included in this work). Thus, I would recommend clarify this fact in the abstract and in the discussion section.

Even though, fortunately this manuscript goes further and also explore the reasons why this extrapolation sometimes can not be possible or does not work (<10 data points, residuals not normally distributed, not robust results,. . .). It is interesting the description carried out in this work about how should be the experiments and results obtained with them in order to be able to use the approach presented here and to implement it for further model projections of future climate. Thus, it is very important to mention in this manuscript that not all data of soil CO₂ efflux from datasets could be suitable for doing future projections, which it is of critical importance for modelers and the interpretation of their results.

In addition, I have some additional concerns and I think that some clarifications are still needed:

- Fig 1 includes all the 58 experiment, however from all of them only 31 give robust result in the extrapolation to predict SCE under different precipitation regimes. It would be interesting to discuss a bit about the general response of SCE to precipitation manipulation for these 31 experiments, and the experiments where extrapolation was not successful. Does decreased precipitation typically reduced SCE whereas enhanced precipitation increased SCE? Which climate regions do they represent? In addition, it would be desirable to describe in more detail the type of vegetation that is represented in these experiments. As the main experimental effort has been carried out in grasslands, further experiments need to be done in other natural ecosystems (forest, shrubland, agriculture. . .)

- According to the results, for experiments with median measurement intervals of SCE larger than 11 days, H1 was never rejected, whereas H1 was rejected for seven of the 14 experiments with intervals ≤ 11 days, which included all five experiments with daily

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measurements. In relation to this you also point out that “we have missed important SCE treatment responses in experiments with larger measurement intervals”. Thus, we could consider that although in some cases current moisture responses could be used to predict SCE under altered precipitation, this prediction could not be accurate because the infrequent measurement schemes have insufficient capacity to detect shifts in the climate-dependence of SCE. Taking into account all mention above, these results emphasize the unsuitability of predicting SCE under altered precipitation using current moisture responses; instead of “...the need for high frequency SCE measurements to fully capture the response of SCE to changes in precipitation and other climatic variables such as temperature” (that although it could be true, it is not emphasized for the results obtained in this work). In addition, it would be desirable to clarify in the discussion the possible reasons why H1 was rejected for 7 experiments (explaining it in general terms instead of going case by case)

- In the discussion section the results from particular cases are commented in a quite deep detail, although there are not representative of any clear general tendency. This fact makes the discussion section hard to follow, and the reader can get lost in the details, forgetting about the main points. Thus, I would suggest to the author to avoid the discussion of particular cases when it is not necessary. Maybe a shorted discussion section but more focus in the main points that need to be mentioned could be much more interesting for the readers.

- Some other important points that should be included in the discussion or mentioned in more detail could be: a) the effect of rainfall amount as well as timing, frequency, intensity. In relation to it, more experiments manipulating precipitation should be conducted to elucidate the impact of a wider range of possible scenarios; b) the fact that precipitation and temperature effects can have complex interactions in the ecosystem could come to unsuccessful predictions if only single factor experiments are considered. Under a climate change scenario, changes in precipitation regime may have associated changes in temperature. In relation to it, in which extent the experiments

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considered in this work are realistic? Did all the precipitation manipulation experiment present a range of temperatures close to the climate change projections for these rainfall scenarios? This is also an important fact that needs to be taken into account in order to discuss the results obtained in this manuscript.

-In the conclusion section I would except: a) a more complete list of reasons for which moisture responses from some current experiments can not be used to predict soil CO2 efflux under altered precipitation regimes and, b) a more complete description of how should be these manipulation experiments in a future if we want to be able to compare their results and use them for future predictions carrying out approaches as the one presented in this manuscript.

Technical comment

- Pag 872, line 7: The sentence “photosynthesis or ecosystem respiration are can be measured at high frequency” need to be rewritten.

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