

Interactive
Comment

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.

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The reviewers have submitted their reports and all three acknowledge the relevance of this paper and its suitability for the special number of BG. The three were positive about publishing the paper, after providing clarifications on a number of issues. Please, consider their reports and answer their comments, indicating clearly whether you accept them or not, and the reasons for doing so. Based on the reviewers reports and on my own reading of the paper, following are some of the main points that need to be addressed in the next round: 1. Strength of the approach

The reviewers found as an important asset of the paper the procedure used to as-

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sess whether past experiments modifying rainfall can help us understand future climate change impacts on soil CO₂ efflux (SCE). This is a strong point that merits being highlighted. Elaborating in the discussion on the pros and cons of this approach, and comparing with similar attempts on other processes of relevance aiming at extrapolating current experiments into a future climate is worth highlighting in the discussion.

2. Highlighting the limitations of our current experiments

An issue that needs to be given further consideration is the limitations that our current experiments have for realistically mimicking future conditions, and how much our experiments thus limit further extrapolations. Water manipulations are carried out in a context of current non-rainfall climate (i.e., drought treatments are implemented in temperature conditions that may not coincide if a true drought had occurred; probably, something similar could be said about irrigation experiments). This may or may not affect the results of the experiments used in this paper. Whether this is relevant or not for the process under consideration is something that authors should further mention and discuss.

3. On the representativeness of current experiments

The reality of extant experiments is probably far from being optimal for the question of concern. There are biases in the selection of biomes, of sites within biomes, and of vegetation-types within sites (e.g., grasslands are over represented with respect to the rest of vegetation types), among other. In section 4.1., when initially discussing the results of the paper, it is important to indicate how biased current experiments are in terms of biomes and vegetation types within biomes. Really and truly, the available dataset is not representative of extant vegetation in the world.

4. Treating experiments at the same site as independent data points

The approach followed uses all available experiments as if they were independent from one another, even if several sites had 2 or up to 19 experiments. The assumption of

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independence of these experiments, and its consequences for the approach used, needs to be considered. For instance, even if no statistical testing was done, in 31 experiments out of the 38 considered, H1 was not rejected. This is a strong result that is interpreted without needing any additional testing. How would these figures change if, instead of experiments, sites (with repeated measures within sites for experiments and vegetation types) would have been considered? Since this paper has a strong methodological component, this is something that merits attention.

5. On the limited discussion of your results

SCE is an important process, and answering the question you posed is utmost relevant. The reviewers felt, and I agree with them, that after doing your analysis you did not rightly discuss your results, as one would expect from this exercise. It is very important to further delve into this point in the discussion, and answer your question, and the limitations of your answer in a much clearer way. Additionally, much of the discussion went to the details of some experiments. The important point in those cases is finding the generalities or the particular processes that may contribute to support or not support your conclusion, or to help guiding future research. This is not fully captured in your discussion and it needs further focus.

6. Many experiments did not even pass an initial scrutiny

In the discussion, before going to the CART, something needs to be said about the fact that, before an analysis could be run, out of 58 cases only 38 could be used, that is only 65% of experiments were proper to further analysis. This is something that needs to be discussed as well. Discarding 35% of the studies after your selection of the ones you thought were appropriate is not a minor issue.

7. Defend your hypothesis

You need to believe your hypothesis and defend it, whether it is verified or not. If not, withdraw the hypothesis. You ignore the fact that, out of 38 experiments, the

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hypothesis could not be rejected in 31, and only in 7 was rejected. It is difficult to accept that you find that 37 out of 38 experiments go along with your reasoning, and later you basically not discuss this because you found that measurement frequency is the relevant variable. The apparent contradiction that the hypothesis is accepted when the frequency of available data is low and that it is rejected when data frequency is high needs further elaboration. If that is the case, you need to enter into the details of why this is possible. How is it possible that such a relevant, but that one could consider a priori trivial, finding went unattended? You did not discuss at all this point. This is important in a perspective of future experimentation in order to not continue doing the wrong things, should this be the case.

8. Modeling soil moisture content vs. measuring it

This was a well thought approach. However, you virtually ignored it in the results, discussions and conclusions. Actually, the presentation of the results is misleading. There is potentially a lot to learn from this exercise, positive or negative. Just going to the negative, if we cannot use this type of modelling, you need to put your findings in context, because this is a relevant result. This part deserves the consideration that you gave it when planning the experiment. So far, is missing, and it should not.

The reviewers indicated other minor points, such as better reconciliation of the abstract with the results and conclusions, clarification on the conclusions, and additional details on the sites used for each purpose. The data are there, but being a dense paper, any effort to improve the readability and help the reader understand what you did will be greatly appreciated. Other minor comments include:

1. Page 7, lines 11-14 (Extremes...2012): This is a rather limited presentation of changes rainfall-related extreme events in the world. Please, modify it to be more comprehensive, even if this not the focus of your paper.
2. Page 8, lines 13- 26. (We... factor): The figures and tables are presented in a rather haphazardly way. Please, organize your text so that the figures and tables in both

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the main text and appendices as well as in the supplementary material are presented correlatively and in order. Following is their listing as they were called in: Figures: Fig S2, Fig S3, Fig. 1; Fig. 2, Fig. S1; Fig. S2; Fig 1; Fig. 1; Fig. 3a; Fig. 3c; Fig. 3d; Fig. 4; Fig. S3; Fig. S3; Fig. 5; Fig. S3; Fig. 5; Fig. 5; Fig. S2; Fig S2. Note that Fig. 3b has not been called. Tables: Table C1; Table A1; Table S4; Table C1; Table C1; Table A1; Table B1; Table C1; Table B1; Table B1; Table C1; Table C1; Table C1, Table C1; Table B1; Table B1, Table C1; Table B1; Table C1; Table C1; Table C1; Table B1; Table C1. Note that Tables S1 to S3 have not been called in, except generally as SI.

3. Page 9, line 3-6: SR is not defined. Please, use consistently SCE, here and throughout the text and appendices and supplementary material.
4. Page 9, line 12: call out Table S2 and Table S3.
5. Page 9, line 15: on the use of model 4. While this is reasonable, it is unclear to what extent model selection (one for all vs. the best for each) affected your results. This is something that needs to be considered.
6. Page 10, line 4: Edit (experiments at Solling and. . .)
7. Page 11, line 8-9: SR is not defined. Use SCE consistently, here and elsewhere.
8. Page 12, last line: a minor comment declaring the bias in the type of vegetation within biome is also appropriate.
9. Page 13, line 10: Edit (Fig. 13b to 13d) to call all panels in Fig. 13.
10. Page 13, line 14: Edit (4 instead of four), for consistency.
11. Page 13, line 15: Edit (indicate the column name [Robust?]) to help the reader).
12. Page 13, line 16: you indicate “results for individual experiments were confirmed when SWC in model 4 was replaced by the bucket model results...”. However, the results [Robust?] were coincident in 7 out of 37! I am not sure that this sentence captures your results. This needs further clarification. Please, call out the specific

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table you are referring to in the SI (Table S5) and add a column in this table to signify whether the results from this exercise were coincident or not with the results based on the actual measurements in the field.

13. Page 13, line 19: Edit (7 instead of seven), for consistency.

14. Page 13, line 4 (starting from the bottom) to page 14, line 3: When Pi...in SCE-treatment.”: This is a clarification that belongs to the methods section.

15. Page 14, line 7: Experiments with daily measurements showed a significant trend. Please, bring to the discussion and, eventually, analyze whether the number of measurements could have influenced your results.

16. Page 14, 2nd paragraph, and until the end of the results section: This text is more relevant in the discussion (Section 4.3): Why you did or did not find a trend, and the generalities that emerged from this, etc.

17. In the supplementary material, you are calling for Table 2 on four occasions, but there is no longer a Table 2. Please, check the numbering of figures and tables in the main text and supplementary material for correctness.

18. Table S1. It is unclear what is meant by “(dominant)”. Please clarify the text to make unambiguous. Additionally, BG does not have instructions as to how list species and whether full listing, following the nomenclature code, should be followed. Personally, I would suggest that listings are complete or at least a reference to which floras or relevant documents are used. In some entrances (Tolfa sites), the second name of the binomen of the species is given in capital letters (Arborea), but should be lowercase (arborea). Names are recommended to be in italic. Additionally, in some sites (Walker-Branch), only the genus is given (Quercus spp., Acer spp.). In the original publication the dominant species are given. Please refer to it and provide at least the most relevant species.

19. Fig S1: Note that in both X and Y axis SR instead of SCE (units) is given. Please,

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modify both legends in all graphs.

20. Fig 5 and S2 and S3: The representation of time in the X axis makes very difficult to follow the course of events. A continuous measure (DOY) would have facilitated reading, plus seasons where these are relevant. Please, consider this in your final submission.

21. Fig. S2 and S3: small black circles are mentioned, but there are not such circles. Black diamonds are used instead. Please, correct the captions.

22. Caption Table. S2 and S3: “For each experiment (add: with more than 10 data points), the . . .”

23. Test for artifacts, line 8: “lilliefors” should be Lilliefors (capital L).

24. Supplementary information content: Please, number the pages.

I will be happy to consider the acceptance of your paper after these issues have been satisfactorily addressed, and I look forward to your response.

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Interactive comment on Biogeosciences Discuss., 11, 853, 2014.

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11, C268–C274, 2014

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