

Interactive comment on “How do more extreme rainfall regimes affect ecosystem fluxes in seasonally water-limited Northern Hemisphere temperate shrublands and forests?” by I. Ross et al.

Anonymous Referee #2

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This ms tackles the complex issue of climate-change induced altered rainfall patterns on CO₂ fluxes in woody ecosystems using Fluxnet data. The study included an analysis of CO₂ fluxes vs. rainfall patterns after correcting for differences in total rainfall amount. Main findings are reductions in GPP and RE with increasing precipitation intensity and extreme rain events, particularly in drier site-years. Since GPP was more affected than RE, NEP too decreased with more extreme rainfall patterns. Findings also included some correlations between an increase in extreme rain events and microclimatic variables, such as vpd and global radiation. While this is an interesting and

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important study, a few issues need to be addressed.

1) Major point: For relationships between CO₂ fluxes and annual rates of precipitation, both site-years and site means were used, and results were similar. However, for relationships between CO₂ fluxes and more extreme rainfall patterns, the main topic of the study, only site-years were used. This poses a key question: To which degree are findings on more extreme rainfall patterns driven by differences between sites rather than differences between years within sites? As a consequence of a strong inverse relationship between extreme rainfall patterns and total rain amounts, this study might just show, hypothetically, that CO₂ fluxes are lower at sites with lower annual precipitation amounts. This topic deserves proper attention.

2) All chapters of the discussion section are in need of some additions and clarifications: Chapter 3.1: Results of the current study were compared with results of Knapp et al. (2008), particularly concerning dry ecosystems dominated by herbaceous or woody vegetation. Authors described the consequences of rainfall patterns for soil moisture in shallow layers and plant productivity in herbaceous systems according to Knapp et al.'s model. However, they did not present an alternative model for the potential impact of changing rainfall patterns on soil moisture at depth and subsequently on the observed biological responses in dry woody systems. This might provide some mechanistic understanding of the observed responses.

Chapter 3.2: It was correctly put forward that factors additional to soil moisture might be involved in the observed biological consequences of changed rainfall patterns. The short discussion of these factors could profit from some extensions, particularly concerning studies on the consequences of high vpd.

Chapter 3.3: Effects of precipitation pulses on CO₂ fluxes were discussed, but it remains unclear, whether changes in the frequencies and intensity of such pulses were to be blamed for the biological responses observed in this study.

3) Structure of the ms: This ms abandoned the usual arrangement of chapters by

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combining methods and results into one section. While this is a possible way to go, it comes sometimes at the expense of clarity in the methodology (see below), since methods were held short to avoid confusion of the results. It is suggested to add the necessary text for clarification of the methods, and then to have a hard look at the legibility of the results. If necessary, methods and results should be separated.

4) Contrast of dry and wet sites: The classification of sites into dry and wet ones was arbitrary (p. 9820, l. 12). While this can work, more meaningful classifications should also be attempted, e.g. by climatic zones or by maximizing the variation that can be explained by the models for the dry sites.

5) Specific comments:

Title: The term “temperate” seems here to indicate non-tropical ecosystems, and includes a very broad spectrum of sites located between arid and subtropical climates. This is misleading, since most readers would expect ecosystems from the temperate climate zone when reading “temperate shrublands and forests”.

Introduction: The Introduction is extensive, and even too long at times, concerning changed rainfall patterns and their impact on water availability and ecosystem responses, particularly concerning grasslands. It is suggested to shorten this part into a more concise discussion of those topics. On the other hand, it would be helpful to include the two following topics that are of importance for the rest of the text: a short introduction on seasonally dry “temperate” forests and shrublands, and some details on responses to changes in rainfall intensity (event size) and changes in intervals between rainfall events.

Methods: R95%tot: This measure for extreme rainfall events was based on the distribution of rainfall within each year at each site. Would such a measure be calculated from a long-term climatic time series, there would be years without extreme rainfall events. How might this difference in the calculation of R95%tot affect the obtained results?

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P. 9822, l. 8-12: Both the concept and the methods used were presented here in one sentence stretching over 5 lines. This is impossible to understand. Methods need to be more carefully explained, with a formula, if necessary. What is the meaning of a predictor minus the mean (of what? all data, wetness group?) and divided by the standard deviation (of what?)?

P. 9822, last paragraph: The meaning of magnitudes of slopes needs to be clarified. The predictor is a complex metric, and it is not clear how the positive slopes of rainfall amounts can be compared with the negative slopes of rainfall intensity and extreme events.

P. 9824, l. 23 - p. 9825, l. 1: Information should be referenced.

P. 9826, first paragraph: a) How would those factors affect the result of this study? b) The extent of geochemical sources of CO₂ in drylands should not be overrated. Thus, “in many of the drier sites” would better be replaced by “in several ...” or “in some ...”.

P. 9829, l. 19-20: Move to methods.

Table 3: It needs to become clear from the table caption what standardized variables are. Units of slopes should be indicated. Metrics for goodness of model fits would be helpful.

Fig. 1: Metrics for goodness of model fits are lacking. Units of F_{max} and P_s should be indicated, at least in the caption to the figure.

Fig. 2: Units of slopes should be indicated, at least in the caption to the figure.

Fig. 3: Do results indicate means and variation among sites?

Fig. 4: Units of slopes should be indicated, at least in the caption to the figure.

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