

## ***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.***

**I. Ross et al.**

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Many thanks to the reviewer for useful comments on our manuscript. Here, we address the major issues in the review – minor comments will be incorporated into any revised manuscript we submit. Below, reviewer’s comments are in *italics*, our responses in normal text.

*1) Major point: For relationships between CO<sub>2</sub> fluxes and annual rates of precipitation,*

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*both site-years and site means were used, and results were similar. However, for relationships between CO<sub>2</sub> 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 CO<sub>2</sub> fluxes are lower at sites with lower annual precipitation amounts. This topic deserves proper attention.*

Both reviewers commented on this section of the manuscript, and it is clear that the treatment we offered was not sufficient, and that the effect sizes that we presented that combine inter-site spatial and intra-site temporal variability could potentially lead to exactly the conclusions that the reviewer suggests.

Although we are somewhat limited by the amount of data available (in particular, out of the 28 sites we are able to use, 3 have only one year of data and 10 have only two years), we propose that any resubmitted manuscript should present decomposed effect size results showing both the inter-site spatial variability, the overall combined inter- and intra-site variability and (where sample size constraints permit) inter-site temporal variability.

*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*

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*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.*

We are honestly unenthusiastic over the prospects of offering an alternative to the Knapp et al. model in our paper. The Knapp et al. model is a reasonable way to look at things for some arid and semi-arid grassland ecosystems where soil-water-plant interactions are relatively simple because of the restriction of the vegetation to shallow-rooted species of low stature. In woody ecosystems, soil-water-plant interactions are potentially much more complex, with multiple vegetation layers with different rooting depths and consequently different responses to changes in precipitation regime.

A lot of work has been done in physiological and gap modelling of seasonally dry forests in the Mediterranean – in particular, Zavala & de la Parra (2004) coupled a physiological model of a single Mediterranean tree species (*Quercus ilex*) with a stochastic model of rainfall variability and soil moisture (based on the ideas of Rodríguez-Iturbe & Porporato (2004)). They considered some variations in rainfall regime of the kind that are relevant to the considerations of our paper, but the results of these modelling efforts are difficult to interpret in a fashion that would allow us to offer a “schematic” model to compare with Knapp et al.’s grassland model. This, despite the fact that Zavala & de la Parra’s model is an effort to build a more analytically tractable ecosystem model to help answer just the type of question we are addressing. Further, the applicability of the extensive work done on Mediterranean forests and savanna ecosystems to other regions of the world is uncertain.

We can certainly describe some of this modelling work, but we are reluctant to attempt to present a “headline” model to explain the results we find across a range of different ecosystems in a simplified manner.

*Chapter 3.2: It was correctly put forward that factors additional to soil moisture might*

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*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.*

We will add some appropriate material here.

*Chapter 3.3: Effects of precipitation pulses on CO<sub>2</sub> 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.*

This is something that we could address directly by considering precipitation and ecosystem flux data at daily resolution, rather than in the aggregated fashion we are using. We could then look to see if pulses of GPP and RE are correlated with pulses of precipitation (considering potential time lags). This would definitely be interesting.

However, there is a paper in preparation by another group using the FLUXNET data that addresses this point, coordinated by Christopher Williams at Clark University (entitled *Carbon dioxide and water flux responses to extreme weather and climate anomalies*). The FLUXNET Publication Policy is set up to prevent overlap between publications and, while it would be convenient and interesting to present an event-based view of the response of ecosystem fluxes to precipitation pulses, we do not feel that we can submit such a proposal until we have seen what will be presented in this other paper. If the Williams et al. paper does not address these issues, we will certainly propose a follow-up to our existing paper in this vein.

For the moment, perhaps a clearer treatment of the effect sizes, more clearly separating out the effect of differences in overall precipitation and the effects of differences in precipitation intensity, would address this question to some extent?

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

We will take this comment on board. The structure of the manuscript was chosen to try to present more of a “narrative” to the analysis, but we were insufficiently attentive to maintaining a reasonable level of detail in the description of methods – there is a balance to be struck between narrative and detail, and we clearly got it wrong. We will add the necessary explanatory material to any new manuscript and review the structure before resubmission.

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.*

Following a suggestion from reviewer #1, we have now replaced our exponential model for the overall precipitation amount model with a piecewise-constant model. We use this model to determine the division into “dry” and “wet” sites by selecting the dry/wet boundary in total precipitation so as to maximise the variance in GPP and RE explained by the model. Although this approach is necessarily tied closely to the data set that we have available, it seems a more justifiable path to follow than our original (admittedly arbitrary) division into “dry” and “wet” sites. The division based on maximising the variance explained by the total precipitation amount model also eliminates a potential source of spurious within-group variability, in that the overall variance in fluxes between wetter sites is smaller than that between drier sites because of the saturation of flux

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values at higher precipitation amounts.

*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”.*

Agreed. We have removed the word “temperate” from the title and have adjusted the manuscript text to remove any possible confusion here.

*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.*

Agreed again: it seems strange that the introduction talks so much about grasslands when the paper is about woodlands and shrublands. There doesn't appear to be very much (or any) literature in the overlap between the two categories mentioned, but we will replace the extensive discussions of grasslands with a description of seasonally dry woody ecosystems and describe both modelling results and what experimental and remote sensing results exist concerning responses to changes in rainfall distribution (unfortunately, though unavoidably, these will almost certainly mostly be about grasslands).

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*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?*

The presence of years without extreme rainfall events in the long-term climate time series would mean that the baseline precipitation distribution used for the calculation of the R95%tot statistic would have a smaller 95th percentile (compared to the distribution in a single year that does have extreme precipitation events). We would thus find larger R95%tot values for the years considered in this case than we do using the “pseudo-R95%tot” statistic presented in the manuscript, since more of the precipitation in a year with extreme events would occur beyond the 95th percentile of the reference distribution.

In general, it is impossible to say what impact this would have on the results of our study, since statistics for a long climatic reference period are not available to us for most of the sites. For some sites, it is possible that the years that we examine are anomalously extreme compared to the long-term climate, while for other sites the years we examine may be less extreme than the long-term distribution.

What we can do, and will do, is to consider the impact of our definition of R95%tot based on data from single years in the context of sites where we have multiple years of data – eight sites have four or more years of data available, and we can examine the impact on different definitions of R95%tot of subsampling these years of data to construct the reference precipitation distribution. This should help to give some idea of whether the definition of R95%tot that we use is reasonable.

This type of problem is one that appears often when performing synthetic analyses using published database of observations and is, in general, very difficult to resolve. The FLUXNET La Thuile dataset has daily meteorological variables measured *at the sites* for the years when flux data was collected, with data treated in a reasonably coherent

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manner that allows for inter-site comparison. The long-term daily precipitation data needed to calculate R95%tot in the normal way is simply not available in this database. In order to calculate long-term precipitation distributions for the sites, we would have to identify nearby meteorological stations with long-term daily precipitation measurements and somehow control for the difference in topographic and microclimatic factors between the FLUXNET sites and the meteorological stations. While this might be possible, we would then be open to the criticism that the details of the upper tails of the precipitation distributions that we are interested in are subject to strong spatial variability and that we have no justification for using data from meteorological stations away from the FLUXNET sites to construct our reference precipitation distributions.

In a situation such as this, we feel that it is better to use our “pseudo-R95%tot” statistic based on single site-years’ precipitation distributions, using results presented in terms of precipitation intensity for comparison, to ensure that the R95%tot results are reasonable. That said, we agree completely that it is worth spelling out in detail exactly how our “pseudo-R95%tot” differs from the more normal R95%tot statistic, and we will add some material to any new manuscript to address this issue.

*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?)?*

We agree that this section needs to be reworked – the whole of the description of the effect sizes comparison is confusing. Some of this confusion should be alleviated by the use of a simpler model for the overall precipitation amount effect, but we will extend the method description here to make it quite clear just what we are doing. (Both reviewers commented on the inadequacy of this section.)

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*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.*

Agreed: this is all part of showing and explaining the effect size differences more clearly.

#### References

I. Rodríguez-Iturbe & A. Porporato (2004). *Ecohydrology of Water-Controlled Ecosystems*. Cambridge University Press.

M. A. Zavala & R. B. de la Parra (2005). A mechanistic model of tree competition and facilitation for Mediterranean forests: Scaling from leaf physiology to stand dynamics. *Ecol. Model.* **188**(1), 76-92.

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