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Biogeosciences Discussions

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 #1

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The paper by Ross et al. (BGD 2011) examines how the intensity of daily rainfall influences annual, ecosystem-scale carbon fluxes in water-limited temperate shrublands and forests. The authors use flux tower data from 28 sites that have participated in the La Thuile FLUXNET collection. The main conclusion is that sites and years with more extreme daily rainfall intensity have reduced gross primary productivity, ecosystem respiration, and net ecosystem productivity. Their analysis has the potential to offer new insights from multi-site synthesis of direct gross and net C fluxes, and also by targeting woody ecosystems rather than grasslands, which have been the subject of relatively

more attention with respect to effects of changes in rainfall regime.

However, the analysis faces two challenges common in non-experimental, multi-site synthetic work. The first challenge is isolating effects of daily rainfall intensity from effects of total annual rainfall. This has not been addressed sufficiently to provide convincing and unambiguous support for the main conclusions. Some alternative suggestions for how the analysis might better overcome this challenge are proposed below. The second challenge is to appropriately pool multiple sites and multiple years of data to generate a sample population that clearly addresses the stated questions/hypotheses with minimal influence from confounding factors. Here, too, an alternative approach may be needed and some candidates are recommended below.

Overall, the problem statement is well framed and articulated in the introduction, the discussion is stimulating and fitting, and the skeleton of the analysis is broadly sound. There are some substantive issues that should be clarified and possibly resolved in the analysis / results for this work to be more convincing and scientifically rigorous and well-founded. Furthermore, there are a few fairly minor logical inconsistencies in the writing and interpretation in a few places as noted in the review that follows.

Main Concerns:

1) Weakness in approach to removing influence from annual rainfall with Equation 1: The structure (form) of equation 1 is loosely justified (though the statistics on fit are missing e.g. r2 and standard error). However there appears to be structure in the residuals and an alternative form may be more appropriate. Choosing an appropriate model for this is critical because it serves as a reference seeking to remove the effects of annual precipitation. Furthermore, and more importantly, the model in Eq1 does not provide a good fit to the relationships _within_ the dry and wet populations. For example, within the dry site population, the model seems to overestimate GPP and NEP at nearly all of the sites and years. Especially because these populations are analyzed separately in successive analyses, the structure to any residuals confounds

the analysis of control by daily rainfall intensity. One approach to address this would be to model fluxes versus annual precipitation for the dry and wet populations separately and then study their separate residuals versus the target of interest, annual mean daily rainfall intensity. Authors may also want to consider a simple bin-average as the reference for different levels of annual rainfall. While clearly a heavily empirical approach and specific to the dataset on hand, it has the major advantage of providing residuals with little to no structure. This general concern really should be resolved before the paper is awarded final publication. The good news is that it should be easy to make the adjustments.

2) Ambiguity in how/why data were pooled across sites and standardized in the way they were: It is not clear why and how the standardization of predictor variables was calculated. Additional information on this should be provided around page 9822, L8-21 to explain why and how. For example, does this part of the analysis rely on residuals from the C-flux relationships predicted by Eq 1 and based on annual precipitation? Probably not given that the slopes in Fig 2 and Table 3 are not alike, however this is unclear. Is precipitation intensity calculated for each year at each site, and then the site-level mean across years and standard deviation across years are used to normalize this? If yes, it seems that this would remove the between-site variability and isolate within-site anomalies, which could be desirable but should be explained. Alternatively, is each site's mean annual precipitation intensity normalized by the across-site mean and standard deviation of site-level mean annual precipitation intensity? This isolates between-site gradients and removes within site, year-to-year variation and would answer different questions. A third approach would be to retain each site-year of data as a sample in the multi-site, multi-year population, and normalize these by the mean and standard deviation of the mutli-site, multi-year pool. This offers still another possibility, and is probably what was performed. However, it leads to a confounded interpretation because one would expect the sensitivity of year-specific anomalies at a site to be different from site-to-site anomalies. The paper should be more clear about what, precisely, has been performed, presenting the equations that fully document the ap-

proach, as well as the justification for the approach adopted. Without this information it is not possible to judge the present interpretation. Authors might consider seeking to split the analysis into separate spatial and temporal elements, where standardization options one and two described above are adopted. Of course, sample sizes may become prohibitively small so it is not clear that this is feasible. It would also help if figures used different symbols for different sites, providing a better indication of the site-year distribution for each population (dry, wet, all), and if results are driven more by across site gradients, or within-site year-to-year variation. Fig 1 already provides some indication but this could be useful for follow-on figures (2 and 4).

3) Internal inconsistency in comparing effect sizes: Table 3 seeks to compare the sensitivity of ecosystem C fluxes to three different rainfall statistics, total annual rainfall, precipitation intensity, and the 95th percentile depth. Up to this point the overall analysis used the non-linear equation 1 to remove effects of mean annual rainfall, but this effect size analysis adopts instead a linear model. The change of framework adopted here leads to an abrupt and confusing change in interpretation, and presents a conceptual discontinuity in the approach and presentation. The weak justification is to allow for comparison of effects size between annual rainfall and intra-annual variability in rainfall (meaning the daily depth-intensity statistics). However, this raises a new problem, that the linear model is clearly flawed for the "All" sites population as shown in Fig 1.

Also, statistics for the linear regressions are not presented, namely r2, standard error of the regression and its parameters, and the intercept, so it is not possible to judge the veracity of these relationships. Slope alone is an insufficient representation of the relevance of this set of findings regarding effect size. The statistics reported in Figure 2 are for relationships with C-flux residuals after accounting for annual precipitation with Eq 1, and thus are not relevant in the current analysis that appears to rely on the raw C fluxes. The units on values presented in the table are missing (e.g. gC m-2 y-1 per standardized statistic). These omissions make it difficult to judge the relative importance of each precipitation variable, and the weight of evidence for corresponding conclusions.

4) Possible contamination from effects of precipitation in a frozen form: Frozen precipitation (snow, hail, graupel, etc) should be treated differently or possibly removed by focusing on warm season or growing season anomalies only. The intensity of daily precipitation during frozen events is not mechanistically expected to have direct influence on GPP, RE, and NEP in the same way that rainfall events would. At least some of the sites being examined receive winter precipitation (frozen form) so this needs to be addressed.

5) Improved precision of nomenclature: The terms “extreme” and “intra-annual” could be made more precise in their usage. An extreme rainfall regime could be one with large inter-annual variation, however the present analysis seeks to emphasize and isolate the daily intensity / frequency within a year. This should be clarified in the title and throughout. Similarly, intra-annual variability can refer to strong seasonality or alternatively, infrequent but large events evenly spread across a year. This too should be clarified and used in a more restricted and clear way throughout the manuscript, possibly “annual mean daily rainfall intensity”.

6) Consider use of defined abbreviations may help: As noted above, the approach to standardizing precipitation variables, as well as the carbon fluxes (residuals or raw values) might be better represented and clarified by the use of symbols or abbreviations that are clearly defined in the text and re-explained briefly in the figure or table captions. For example, the standardized precipitation statistics might be represented with a Z-score type symbol (e.g. Z_intensity, and Z_R95%).

Technical Comments and Recommendation of Specific Edits:

P 9815, L 20: “intensity” change to “intensify”

P9815, L21: clarify “little change in total rainfall”? this is not true regionally / locally, perhaps globally, but there are strong regional and local trends both observed and

predicted. Also, this is inconsistent with P 9828, L 24-26.

P9816, L3: but the paper here is not about interannual variability but rather event statistics. these should not be confused.

P9816, L5: “exert as” change to “exert a” or just remove “as”

P9820, L27: the term “extreme” does not seem to be used correctly here. extreme means statistically uncommon, whereas this seems to be discussing “high intensity – low frequency daily events”. the language should be adopted with more clarity, though it is admittedly cumbersome to write the long-form proposed here.

P9821, L13: same as above comment Fig 1 and Abstract: should report statistics of the model fit (r2, stnd error, etc.) to support the comment of “strong dependence of fluxes on mean annual precipitation”. Also, the analysis appears to use annual precipitation, not mean annual precipitation. If it does, in fact, use mean annual precipitation, this should be clarified with precision throughout, for example in the figure caption for Figure 2.

P9821,L6: “mean rainfall” to “annual mean rainfall intensity on days with rain, mm d-1”.

P9822, L18-21: this sentence is very confusing. same with table 3. clarify standardization, data populations, report units in the table, etc.

P9822,L25: “rainfall distribution” is ambiguous, please clarify. is this a seasonal distribution, or distribution of events within a season (wet season, growing season), etc.?

P9823: the water deficit metric (WD) depends strongly on the depth of the soil moisture measurements and the degree to which they represent the rooting volume, microbial community distribution, etc. and hence water status of the ecosystem’s biota. it is at least worth mentioning the crude and incomplete nature of this indicator, how it can vary between sites depending on what individual investigators decided to represent, and the implications of this for the analysis presented.

P9821, L27: “more widely-spaced” change to “less frequent”

P9825, L1-4: more extreme rainfall in a xeric system will _not_ necessarily increase water content if deep drainage or surface runoff is triggered to consume a larger fraction of annual rainfall such that higher intensity causes more rapid losses. i believe this is illustrated, for example, in Porporato et al. 2001 that you already cite.

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