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Interactive comment on "Impacts of droughts and extreme temperature events on gross primary production and ecosystem respiration: a systematic assessment across ecosystems and climate zones" *by* Jannis von Buttlar et al.

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We thank the reviewer for the positive and helpful comments. Please find below our detailed responses to their remarks (in italics):

Section 1.2 Provides thorough and very specific background on the response of photosynthesis to temperature and water stress (line 25 p. 3 to line 6 p. 4), but less detailed background on microbial responses to the same stressors (lines 7-14 p. 4). It would be great to balance this by providing a bit more background on microbial ecology, and *perhaps streamline the background on photosynthetic responses.*

We agree. We supplemented the discussion of extreme event impacts on ecosystem respiration with the following:

"Ecosystem respiration (R_{eco}) is the sum of autotrophic respiration and the CO2 emissions arising from the heterotrophic decomposition of organic matter in soil (e.g. Law et al., 1999, 2001; Epron et al., 2004). Like GPP, it is affected by changing soil (and, hence, ambient air) temperatures (Lloyd and Taylor, 1994; Kirschbaum, 1995; Davidson et al., 1998; Kirschbaum, 2006). Rising temperatures directly increase the kinetics of microbial decomposition, root respiration and the diffusion of enzymes. Hence, soil respiration is commonly modeled as an exponential function of temperature using the van't Hoff type Q10 model (van't Hoff 1898, Jassal 2008, Mahecha et.al. 2010) or other functions of similar shape (e.g. Kätterer et al., 1998, Kirschbaum, 1995 and Reichstein et.al, 2008). Even though enzyme activity generally decreases above a certain temperature optimum (Kirschbaum, 1995), such high temperatures rarely occur in extra tropical soils (Reichstein et.al., 2008), so high temperatures alone are rarely an inhibiting stressor for soil respiration.

In addition, the activity of soil microorganisms depends on soil moisture (Orchard and Cook, 1983; Gaumont-Guay et al., 2006; Liu et al., 2009; Epron et al., 2004). Drought conditions strongly reduce soil respiration because the microbial activity causing soil respiration is dependent on the presence of water films for substrate diffusion and exoenzyme activity (Davidson & Janssens, 2006, Jassal et al., 2008, Frank et. al., 2015). In addition, low soil water status may even cause microbial dormancy and/or death (Orchard & Cook, 1983). Indirectly, drought reduces microbial activity through different processes like the alteration of soil nutrient retention and availability (Muhr et al., 2010; Bloor & Bardgett, 2012) or changes in microbial community structure (Sheik et al., 2011, Frank et.al. 2015). Finally, interactions between the response to temperature and water status, such as changing temperature dependency due to changing soil water status (e.g. Reichstein et al., 2002, 2007), further complicate the picture."

Section 2.2 (line 5) is there a citation that could be added to justify the use of midday NEE as approximation for GPP and nighttime NEE for R_{eco}?

Thanks for bringing this to our attention. We added a reference for this:

“To assess whether this could bias our analysis, we also performed all of our calculations using midday NEE as a rough estimate for GPP and averaged nighttime NEE as a proxy for R_{eco} (Reichstein et. al. 2005).”

Section 2.3 (line 4 p.8) What is the relevance of noting that most sites have R^2 <0.9? Is this a source of concern, or is the exclusion of sites with R2 < 0.6 sufficient? Clarification would be useful.

We are sorry, but the “<“ was a mere spelling mistake. We corrected it to “>“:

“The correlation between downscaled and site-level data for air temperature was R^2 > 0.9 for nearly 90% of the sites.”

Section 2.3 (line 10 p. 8) If i understand this description correctly, the authors are stating that their approach assumes that the water storage capacity is assumed the same everywhere. In absence of good site-specific information, this approach may be reasonable, but it would also be useful to provide some commentary on how the results might be affected by this assumption. For example, what would the sensitivity of the results be to assuming a slightly lower or higher water storage capacity?

The reviewer raises a very valid issue here. The water storage capacity is highly dependent on various soil specifics. However, to consistently calculate a water status estimator for the wide range of sites investigated here without the availability of consistent and comparable water storage capacity data for all sites, we had to use this rather crude simplification. We added the following paragraph to the methodological description of WAI (page 8, line 18):

“The WAI does not account for local soil or vegetation specific properties such as soil texture, rooting depth, etc. such that the WAI may be interpreted as a ’climatological

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water availability metric'. The results are sensitive to the fixed value of storage capacity, which influences the timing and magnitude of extreme drought events. For example, a larger (smaller) storage capacity value would tend to result in a later (earlier) extreme drought detection. We are confident, however, that these changes would not strongly bias the qualitative and global patterns of the flux impacts investigated in this analysis."

Section 2.4 (p. 9 lines 2-3) Why is seasonal variation in sensitivity to water availability not expected? My intuition is that there could be quite a bit of sensitivity in ecosystems where phenology is driven by precipitation rather than temperature. Can the authors provide more explanation?

We agree, for several ecosystems the response to WAI can certainly vary seasonally. Simply de-seasonalizing the WAI time series (like we did with temperature) would, however, not be a good solution to this. To fully address this issue, it would be necessary to systematically stratify the extreme events according to the phenological stages or timing during the growing season. This was, however, not the scope of this first assessment, but may be the focus of future studies using the presented framework (see page 22, line 27).

Section 3.3 p. 13 line 23 – This is a small thing, but having the longer term reversal in the trend for R_{eco} described as "somewhat surprising" makes me wonder what evidence there is that we might expect any other trend. A little context on why the authors find it surprising would be helpful, or alternatively, I'd suggest just deleting the phrase "and somewhat surprisingly,"

The referee is right, it is not that "surprising" that the negative impacts on R_{eco} increased with duration. With our formulation we wanted to stress the importance of the fact that short extremes generally showed increasing effects on R_{eco}, whereas longer extremes resulted in mostly decreases in R_{eco}. We changed the paragraph as follows to clarify this aspect:

"With increasing duration, the response in R_{eco} was reversed: for short heat events (i.e.

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a duration of less than 18 days), R_{eco} increased with respect to normal conditions by up to 2 σ, whereas for events that last longer than a month, the response of R_{eco} was predominantly negative."

References:

(in addition to the ones already cited in the discussion version of the manuscript):

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