Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2018-130-RC2, 2018

Biogeosciences Discussions
Open Access

Interactive comment on "Contrasting biosphere responses to hydrometeorological extremes: revisiting the 2010 western Russian Heatwave" *by* Milan Flach et al.

Anonymous Referee #2

Received and published: 2 May 2018

Review for bg-2018-130, Flach et al., "Contrasting biosphere responses to hydrometeorological extremes: revisiting the 2010 western Russian Heatwave."

Flach and colleagues, using a multivariate spatiotemporal anomaly detection algorithm on both climate and ecosystem variables, assess the response of productivity to the Russian heat wave of 2010. Motivated by the potential for inconsistencies in the climate event and the biospheric impact (which they suggest is a function of disciplinary divides) they find that an anomalous spring warming event in both the biosphere and climate increased GPP prior to the actual heat wave itself, which occurred later in summer, thus offsetting the negative productivity effects. They note that the compensation

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occurs in different ecosystems–losses dominated in lower latitude managed ecosystems, such as crop land, while spring gains dominated in higher latitude forested regions. During the heat event itself, they attributed the differential response of forests and crops to different water management strategies of the vegetation classes. Overall the paper is a nice contribution and appears methodologically sound (if not a bit overcomplicated in places). I have a few comments and suggestions for the authors to consider that I hope will help improve the clarity and argument of the paper.

Main comments:

1. Stated motivation: While I am sympathetic to the larger issue that climate extremes and climate impacts are distinct domains and that extremes may not necessarily map to impacts, I find parts of the introduction to be somewhat of a 'straw man.' The hydro and bio perspectives generally do agree on the Russian heat wave–warm temperatures, along with dry soils leads to carbon loss. Consider the fact, for example, that the authors' very own agnostic algorithm finds the same two events in both the met and bio fields; it suggests that the RHW at least, this disconnect does not lead to inconsistent interpretations or conclusions among different disciplines. The notion that there isn't a one-to-one mapping between the geophysical event and the biophysical impact is certainly important for accurately representing the total effects as a function of the differential vulnerabilities of ecosystems. The authors rightfully emphasize this. However, the notion that this issue is emblematic of some kind of disciplinary divide is over-reach, or at the very least, is not supported by the literature the authors cite here. I heartedly agree that a call for an integrative perspective is a good one, as it can provide both a richer treatment of an extreme event and a basis for better impacts prediction, but the way the introduction is cast at present overstates the extent to which disciplinary perspectives are or were an issue in some kind of misdiagnosis of the RHW. This can be seen, for example, at 3.10, where the authors state that because the GPP declines were not as large as the temperature anomalies in Fig. 1, that this is somehow reflective of "different disciplinary perspectives" rather than of the complexity of the Earth

system itself. . .leading the authors to “suspect [. . .it] might become an issue in studies of this kind.” If the authors provided a stronger basis in the literature of inconsistent conclusions of the impacts of the RHW or similar events based on disciplinary divides, then sure, the way the intro is written can stand, but I think as is, it overstates it as a problem and diminishes the scientific conclusions of the paper, which are interesting in and of themselves. The point is, those interesting results and the science itself, gets a bit lost in the straw man discourse. Edits to the text can fix this.

2. Two events v. one event: My comment here is a corollary to the above about how the paper is cast relative to the literature. The authors are taking two separate events in 2010, an anomalous spring and an anomalous summer, and integrating the impacts across those two events and casting it as the net effects of the RHW, rather than simply examining the net consequence of the RHW itself. Certainly the spring event is crucial to providing a picture of GPP over the growing season and this approach makes sense for the effects of the full growing season on GPP: the extent to which the spring anomaly primed, compensated, or otherwise interacted with the RHW is important. But conceptually the authors need to make clear that simply combining them does not constitute the carbon response to the RHW, for as written, the RHW impacts are presented as the net effects of two separate events, rather than just the heat wave. Given the motivation the authors lead with (i.e., that there is an inherent potential for some kind of mismatch from the atmosphere down and the biosphere up), calling the impact of the RHW the integration of two distinct events seems like an issue. Perhaps the results should be recast around the compensation effects of spring growth on total growing season GPP in the year of the Russian heat wave. I think just making this distinction clearer is important. The net impact of the RHW is not growing season GPP, which includes the anomalous spring, it’s just the GPP loss during the RHW. These integrations can be seen in Tables 1 and 2, S4.1, etc. Further complicating this is the fact that the actual losses and gains of GPP are domain integrated, and the domain integration is a function of the detection algorithm. Certainly the authors discuss that the compensation occurs in a fundamentally different part of the domain and land cover

class than the heat wave impacts, so I find the combination a bit misleading–it occurs in a different location and time than the actual heat wave–1TgC in crops is fundamentally different than that for forests (though from a carbon accounting perspective perhaps not). This again, is just about how the results are presented, particularly the res+/res-, not the results themselves.

3. Merits of the detection approach: Part of the basis of this manuscript is that a much more sophisticated detection approach is needed to accurately represent the biophysical impacts of climate extremes. If one simply did the detection–as is typical–at the grid point scale on the hydrometeorological fields and then composited on the biophysical fields for the same dates as the meteorological anomaly, would the results and/or conclusions substantially differ? At 5.10 the authors claim that for a short time series a traditional threshold approach would be problematic. Is there evidence for this? The authors still have to perform a sensitivity analysis of their results to the chosen threshold (S4.1). At some places the paper feels needlessly complex–perhaps the authors could better justify their complicated analytical choices?

4. Model of factors explaining the GPP response. This section (S3), which is referred to in the main, but relegated to the Supplemental could be better emphasized and explained. For example, the factors in the hierarchical modeling approach are not independent. Are interaction variables used to address this issue? Given the confounding of latitude and temperature and land cover class, why not add latitude to the regression hierarchy to see its explanatory power, given the sentiment at 12.3?

5. Attribution to uWUE differences. The authors attribute the reduced GPP declines during the summer event of forests in part due to the uWUE. Certainly this has a role to play. One could also imagine uWUE being an explanatory variable in the model presented in section S3 as well–could the authors add that? It seems like the authors are positioned to better attribute whether it was the absolute magnitude of the temperature itself (which diminished as a function of latitude) or something innate to the land cover classes (and their underlying WUE), which just so happens to vary as a function of

latitude. The model seems like an ideal place to disassociate these factors. Regarding the spring event and soil moisture depletion carry-over effects under forcing discussed at 12.22-13.6, Mankin et al., Journal of Climate 2017 and Mankin et al. GRL 2018 note that increased productivity is associated with such carry over effects in some of the models, regionally and globally under forcing.

Minor comments:

11.5: I don't understand the soil moisture in Fig. 7. Is it the normalized measure? Is it the m3/m3? Can the authors add contours if the forests separate by latitude in 7b?

Grammar/spelling throughout could be improved.

1.16: (e.g., a vegetation index) inconsistency in comma usage after e.g. and i.e.

2.29: not sure the name is "heat summer"

2.32: a, not an, hydrometeorological

5.8: grammar ("in high")

5.4: Why not leave them as missing data?

21.32 "spatiotemporal" not "...temporla"

Author contributions: "wrote" not "ote"

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