Improving Model Calibrations in a Changing World: Controlling for Nonstationarity After Mega Disturbance Reduces Hydrological Uncertainty

Elijah N. Boardman, Gabrielle F. S. Boisramé, Mark S. Wigmosta, Robert K. Shriver, Adrian A. Harpold

Response to Reviewer Comment RC1 – Revision 2

We appreciate the reviewers' continued engagement with our manuscript, and we have addressed the remaining minor comments in this revision. Our comments are interspersed in blue.

1. Timing of model updates on page 7

Thanks for the clarifications on this, but it seems like we phrased our initial comment in a way that was not clear. The main question was why this should be updated annually at Oct 1st, instead of immediately after the fire. If real conditions change, so should the model right (in other words, isn't getting changes to the simulated fluxes the reason for updating the model)? Is it some model constraint that vegetation can only be changed once a year? It probably doesn't matter a ton given that the fire happened in September, but it's still worth clarifying.

There is no fundamental model limitation requiring the October 1st updates, but we chose this date for parsimony in the model implementation across years, in addition to the reasons specified below. We further explained and caveated this approach in the Methods section:

The DHSVM vegetation maps are updated on October 1st in the year of a fire, i.e., about one month after the September 2020 Creek Fire ignition. The October 1st date is used for annual model updates because this date represents the start of a new water year, and Sierra Nevada watersheds are typically near their driest condition around this time of year, which limits the impact of model changes on simulated hydrological fluxes. Additionally, fires typically burn in the late summer in the Sierra Nevada, so the October 1st date is close to the typical end of the fire season. However, studies focused on short-term post-fire effects may need to update vegetation maps on the dates immediately after each fire, or even multiple times as a fire progresses (e.g., the Creek Fire burned for several months).

2. Calibration of Correction Factors

That said, there is clearly a difference in point of view between the authors (forcing + model is the system of interest) and the reviewer (model is the system of interest). To avoid this turning into a yes/no back and forth, can the authors add some information that shows to what values these forcing correction parameters were calibrated across the 30 final parameter sets, and discuss accordingly? If the values are approximately the same, then the reviewers' point is moot. If there's wide dispersion in calibrated correction factors, there's some reason to believe that what these parameters are doing is not unambiguously pushing incorrect forcing data closer to their true values. The third discussion paragraph could be a good spot to add a few words about this.

We agree that the model experiment could be approached from two different perspectives: (model + calibrated forcing) to explore the full range of real-world uncertainty, or (model + static forcing) to more tightly constrain within-model dynamics. We have added two sentences to this effect in the Discussion:

In our current study, we choose to treat the combination of model and forcing data as a single inferential system, so uncertainty in hydrological parameters is not independent from meteorological biases. This unified approach is useful for our goal of simultaneously constraining multiple sources of uncertainty in the post-fire streamflow change, but other studies may choose to use the same meteorological assumptions across all models to more tightly constrain and explore within-model dynamics.

We agree that the actual calibrated biases should be stated somewhere, so we have added this section to the Results:

The 30-member behavioral ensemble has residual meteorological uncertainty ranging from -11% to +4% precipitation biases and -3.4 °C to +1.0 °C temperature biases. Considering only the 8-member stationary sub-ensemble, the precipitation bias uncertainty remains the same (-11% to +4%) and the temperature bias uncertainty decreases slightly to a range of -3.1 °C to +0.3 °C. Although these calibrated meteorological biases are substantially smaller than our ±25% calibration range (Table 1), the precipitation uncertainty remains roughly the same magnitude as the post-fire streamflow change, highlighting the potential confounding impact of forcing data on post-disturbance change-detection studies.

3. Leaf Area Index (LAI) Clarification

Clarifications helped to better understand Figure 4, but it would be good to add some extra words that explain why spatially averaging LAI covers such a large range. If LAI is derived empirically from fractional tree cover and tree cover is derived from vegetation maps (p7), wouldn't LAI be a static value that doesn't change between the different parameter sets?

We have clarified in the caption that the Figure 4 values are calculated from the maps of tree cover and LAI, which are both calibrated maps (Table 1). The maps are just used to derive the baseline pattern, and the absolute scaling of the maps is calibrated (see caption of Table 1).

4. Citation Suggestions for Equifinal Parameter Sets Producing Divergent Predictions

A cursory literature search suggest various papers that may support this statement in a general sense (e.g., Kelleher et al., 2017: demonstrating that parameter sets equifinal with respect to streamflow and SWE produced markedly different annual and seasonal simulations of water table depth), in the context of changing conditions (e.g., Melsen et al., 2018: showing that ostensibly equally plausible models can have markedly divergent predictions on future conditions), and specifically in the context of disturbances (e.g., Seibert & McDonnell, 2010: showing the impact on streamflow of forest clearing practices in a paired watershed experiment).

We thank the reviewer for suggesting these citations. We have added the two new ones (Seibert and McDonnell 2010, Melsen et al. 2018) to the statement in question (the Kelleher et al. 2017 citation is already present elsewhere and less relevant to this particular point).