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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Assessing the long-term hydrologic response to wildfires in mountainous regions" *by* Aaron Havel et al.

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Response to anonymous reviewer #1: The authors of the paper would like to thank the anonymous reviewer for their valuable and insightful comments. We tried to carefully address all of the comments from the reviewer. The details of the revision are brought below:

The paper title and primary goal, aimed at characterizing and quantifying long-term hydrologic responses to wildfires in mountainous regions are perhaps a little misleading.

Abstract, Page 1, Line 8: The verbiage "long-term hydrologic responses to wildfires" seems misleading given only the first few years post-fire (2012- 2014) are part of a longer-term analysis (2000-2014). This should be addressed here and throughout to

more clearly state what was evaluated.

Regarding the title of the paper which contains long-term responses, the reason for using this term was that many studies are conducted on investigating the immediate response of the watersheds to wildfires (next few precipitation events after the fire). We used the term "long-term" to indicate that our study has been done to explore the hydrologic effects of the fire on continuous-time basis for a few years after the fire in contrast to a few precipitation events. Anyway, we removed the term long-term from the title to avoid any misinterpretation. We explained what we meant by long-term in the abstract and throughout the manuscript and explicitly mentioned the two year period for analyzing the effects of the fire in the abstract and other locations in the manuscript.

Abstract, Page 1: The abstract doesn't really present substantially novel findings. The results are somewhat typical for burned watersheds. Consider clearly presenting novel components of the work along with the primary findings.

Thanks. The abstract was revised to emphasis more on the novelty of the approach and important findings and implications.

Pages 2-3, Lines 30-32 and Lines 1-8: I'm not sure this content achieves the intended (assume to provide justification for the current study). The text here suggests the requisite approaches use static variables to represent dynamic properties. While this approach does have limitations, utilizing a dynamic approach that may not fully represent the dynamics at hand also has limitations. Anyway, the text doesn't necessary make a compelling case for one approach over another. Also, the word "components" need some reference/definition within this text.

That is a valid point that lacking the ability to capture the full dynamic of the problem has limitations. However, incorporating a dynamic approach even with its limitations can enhance the physical base of the simulation and help with more realistic representation of the processes compared to a static approach. We added more references and some more description of the method to better show the merits for its application. By

component we mean the modules in the model that provide the capability to implement a specific change in model. Here, LULC component is the land use change module. The explanation is added to the manuscript.

Page 5, Lines 7-9: This is a very broad statement and assumes the continuous model is accurately depicting the dynamic events. Perhaps additional citations would better support this statement as the norm. I'm not entirely convinced the approach used in this study demonstrates a better representation or just a different one. Both approaches can be effective, useful, and yield good results. Any general commentary on one versus the other should be clearly justified and include substantial citations in support.

Both event-based and continuous-time models are useful and one can better represent processes compared to the other depending on the study problem. We did not intend to imply that continuous-time models are better than event-based models or they represent the processes accurately. For this study we had to use a continuous time model and in this paragraph we explain why a continuous time model better represents the processes involved in this specific problem. Some more references were added to the text for this argument.

Pages 5-6, Lines 24-30 and Lines 1-4: More specifics needed here and explanation of Range-Grasses approach is merited. Thanks. The reason for updating the LULC to Range-Grasses was explained in more detail in the text.

Page 9, Line 2: I'm not sure I agree that the error statistics tell how accurately SWAT is representing processes exactly.

We used the error statistics to assess how good the model simulations conform to real observations. While the good error statistics does not guaranty that the hydrologic processes are represented completely accurate, it gives a good insight into the performance of the model. The text was reworded to avoid confusion.

Page 12, Line 13: The inference that it may be reasonable to use total burn area

percentage as a predictor requires some qualification here given the single study.

The idea that increase in percent burn area results in increase in runoff has been reported in several studies (Benavides-Solorio and MacDonald, 2001; Inbar et al., 1998; Lavabre et al., 1993; Robichaud et al., 2000; Scott, 1993). However, we were not able to identify studies where the relationship between runoff increase and percent total burned area was quantified similar to our study. Given the regression analysis done in this study and checking the metrics indicating the strength and significance of the regression model, we expressed that percentage of total burned area may be useful as an indicator of increase in runoff. Anyway to be more accurate we added "in the Cache la Poudre watershed" since as you have mentioned this is a single study on a single watershed. We also mentioned that more studies are needed to generalize the findings of the study.

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