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

Anonymous Referee #1

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General Comments:

Havel et al. present an assessment of hydrologic response to wildfire using the SWAT model, applied at multiple spatial scales. The authors provide a good overview of the problem and clearly state the goal and objectives. 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. The study analyzed runoff (using SWAT) for the study domain over a period of 2000-2014, with the wildfires occurring in 2012. Therefore, the burned condition is only represented in the short-term. The title and all such references/inferences in the paper to long-term effects of fire should be revised to more clearly depict unburned and burned periods evaluated. The methodology is explained reasonably well, except where indicated below, and substantial material is

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Discussion paper

provided through appendices. The findings are supported reasonably well throughout except where noted below. The study is relevant within the scope of HESS, although the novelty needs better depiction. Conclusions could be more substantial. The length of the paper and associated elements is appropriate.

Specific comments: 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.

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.

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.

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.

Pages 5-6, Lines 24-30 and Lines 1-4: More specifics needed here and explanation of Range-Grasses approach is merited.

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

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.

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