

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

**Anonymous Referee #2**

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### Main remarks

The manuscript deals with an interesting theme for HESS, both for a hydrological point of view and for a soil science point of view. Namely the authors have tackled a very complex problem that is rising the international audience interest, the catchment hydrologic response to wildfires. The authors have developed calibrated and validated a numerical model using a well-known code SWAT to answer this question. As far as I can judge, the English of the manuscript is good and the grammar correct. The title of the manuscript is self-explicative. The abstract provides the necessary information to be considered stand alone. The keywords are all necessary and pertinent. The introduction section is pertinently review the processes of wildfire induce on hydrologic behavior of catchments, but is not actually appropriate in describing the mathematical

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approaches. In fact, it lacks to review the state of the art of various modelling approaches that have been widely used in the last 20 years in hydrologic modelling, e.g. Clark et al. 2017 Hydrol. Earth Syst. Sci., 21, 3427–3440. And how this model can be considered among the variety of approaches so far used.

The materials and methods sections is very well constructed and I do appreciate the information available as appendix to fully explain the model construction and to allow repeatability of model set up. The model has been calibrated and validated versus flow data at the outlet of the catchment using 38 parameters, the number of parameters is quite elevated thus non-uniqueness of the solution is probable in such inverse problems. For this reason, the authors should at least provide more information about the sensitivity of model parameters. In fact, they claim that additional parameters respect to Foy et al. 2015 were introduced following the results of Ahmadi et al., 2014 (that conducted a study also on solute concentrations in another watershed), but no information is actually provided on which parameters were added and which global sensitivities they have found. In addition, since here only flow discharge is used as observation data, the authors should comment on the limitations of using a single source of data to calibrate a model. Finally, a key factor controlling the hydrological base flow is the parameterization of the saturated hydraulic conductivity field, that could be highly heterogeneous at the Hydrologic Soil Group scale, thus it would be useful to show sensitivities to this particular parameter.

Overall the results and discussion section should be improved with the above mentioned suggestions to better support the conclusions of this paper.

In my opinion the manuscript presents novel and robust data to evaluate the long-term hydrologic response to wildfires, so I feel that the paper could be accepted for publication in HESS after the minor corrections recommended.

### Specific remarks

Figure 2 the terrain slope range is scale is too high (0-9999) and the map display only

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one colour, please amend the figure.

Figure 2 the number “3” in the right upper corner is affected by distortion” please correct it.

Figure 2 The Watershed Outlet symbol is not visible in this figure and is only visible in fig.1, please correct it.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-604>, 2017.