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Interactive comment on "Evaluation of predictive models for post-fire debris flows occurrence in the western United States" *by* Efthymios I. Nikolopoulos et al.

Anonymous Referee #2

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This paper is intended to present an improved model for forecasting debris-flow occurrence in recently burned areas. The presented material relies exclusively upon previously published data from other sources, and existing methodology. Given the relatively narrow target audience and lack of uniqueness of this topic, my criteria for recommending the publication of this type of study are as follows: 1) New insight into the mechanisms responsible for postwildfire debris-flow occurrence must be gained. 2) Unique methodology for improved predictions must be presented. 3) The methods used to compare model predictions must be defensible, and account for uncertainty in model predictions.

While this paper is well-written, significant revision is required in order to meet the specified criteria for the following reasons:

1) No new insight into the prediction of postwildfire debris-flow occurrence or the processes responsible for debris-flow generation have been presented within. The authors used an existing database of debris-flow occurrence, storm characteristics, and a select number of variables to test an existing modeling approach. The authors did not attempt to incorporate different metrics of topography, fire severity, or other physiographic properties of watersheds into this analysis. As such, the author's solely relied upon someone else's calculation of metrics and interpretations of the variables that are important for inclusion in this analysis. Hence, we did not learn anything about new factors or combinations of variables that may be relevant for the prediction of postwildfire debris-flow occurrence.

2) The authors present a method that utilizes an existing methodology (random forest analysis). It's unclear to me how or why this is significantly different, or a significant advancement, over the machine learning method presented by Kent et al., 2017. It seems that the comparison of model predictions focuses almost exclusively on those between the models presented by the author and the USGS logistic regression model.

3) The manuscript provides methodology for a binary classifier model used to predict the occurrence of a postwildfire debris flow. The authors compare their predictions to the USGS logistic regression model, but significantly simplify the USGS model predictions. Logistic regression analysis produces a continuous estimate of the likelihood of debris flow occurrence with values that range from 0 - 1, with 1 representing the highest likelihood. In this study, the authors consider the LR model as a binary classifier model, failing to consider that the model is not inherently binary. The analysis then proceeds to use ROC metrics for binary classifier models to compare the results. The Staley et al 2017 manuscript that the authors draw their comparative statistics from does use ROC metrics to compare findings for a portion of the results and discussion. In my opinion, this is a significant flaw in the Staley et al. 2017 manuscript.

By only viewing the USGS model as a binary classifier, the authors oversimplify the predictions made by the USGS logistic regression model by not accounting for the implicit uncertainty in the logistic regression model predictions. For example, the USGS model might estimate a 0.51 likelihood for a debris flow (i.e. just slightly better than a coin toss), but no debris flow might have occurred. This manuscript considers this to be a failed prediction, but is it really? I would argue that a model that estimated a 0.49 likelihood of not having a debris flow is not incorrect. Hence, the conclusion that the presented model significantly outperforms the USGS model predictions based upon the analysis of the model as a binary classifier is misleading.

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