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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrological response of a small catchment burned by experimental fire" *by* C. R. Stoof et al.

Anonymous Referee #3

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Overview The MS describes the effects of an experimental prescribed fire on the short-term (1-2 year) hydrologic characteristics of a catchment in Portugal. The topic area is suitable for this journal and would be of interest to its readership. The research questions posed are relevant and justified based on the literature review. The paper is very well written, the methods are robust and the conclusions are generally reasonably supported by the data.

Specific comments The authors state in the abstract and the introduction that "catchment scale studies on the hydrologic impact of fire are scarce". I don't agree with this- there are many catchment scale fire effect studies in the literature, especially in the US and recently in Australia. Nested studies are less common, as are European studies,

and the authors may be better placed to argue the scarcity of these types of study.

Abstract line 5; the authors claim this is a "unique" approach. I think this is a bit strong. The approach taken is very sound, but I don't think it is unique.

I like to see more quantitative information in the abstract. If the runoff coefficient changed, by how much? The same is true for statements about water repellence, soil water etc. The abstract is often the first (and sometimes the only!) exposure people have to your paper and it should be as informative as possible.

Last paragraph of introduction; The authors hypothesis is that hydrology is changed due to "canopy interception and water repellence", however the methods then cited in the following sentence only include "streamflow, canopy interception, and soil moisture" Shouldn't the methods include water repellence testing if this is $\frac{1}{2}$ of your hypothesis?

The section on data storage and analysis does not need to describe the database, and could include more detail on the ANCOVA.

Looking at the streamflow data ie a control catchment and a treatment catchment, with data before and after the treatment was applied, did make me wonder why you did not analyse the data using a conventional paired catchment approach ie determine the behavior of the treated catchment as a function of the control, then compare the predicted behavior of the treated catchment with the observed behavior of the treated catchment. I imagine you have considered this, and if so it would be good to articulate why you did not use this approach.

The canopy interception results were very interesting – I didn't expect such high values. With such high values, and such a large change with fire I feel like you could potentially make more out of this data by maybe considering how much of your observed change in hydrology could be explained by this aspect of the system (eg. via modeling maybe?).

Equation 2 should quantify a lower bound for P so that TF does not become negative

In the Streamflow section I had trouble understanding exactly the meaning of the QQ

plots (Fig 3); some more detail here would be helpful.

Fig 4 c ; I would like to see the data overlaid on this function as it is difficult to evaluate how good it is.

Discussion: I really liked the discussion; I think the authors have done a great job to bring together the various pieces of the story into a coherent and plausible explanation of the processes dominating the post fire hydrologic change in the catchment. My only reservation, again, would be that perhaps the authors could make more out of some of the results by modeling the system so as to show in a more quantitative way how some processes dominate the post fire hydrological change.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 4053, 2011.