

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

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REVIEWER COMMENT The authors describe changes of catchment response after a fire experiment, and infer variations in internal catchment processes. The ‘plus’ of the paper are that the case study is interesting, the paper is generally well written, and things are well explained. The limitations of the paper are (i) the time series is too short to derive meaningful conclusions, (ii) the authors tend to extrapolate their conclusions beyond what can be reasonably inferred from their analysis of the data, (iii) the analyses are very basic, and (iv) the authors appear to rely too much on software names, (v) the quality of the figures is generally poor. I will elaborate these points below.

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REVIEWER COMMENT Unfortunately the time period of the case study is very short. Data length is less than 1 year for pre and post fire monitoring, which constrains a meaningful interpretation of catchment behaviour. Catchment response dynamics are critically affected by changes in forcing, which, especially in this region, have a strong seasonal variation. Differences in summaries of catchment response, such as runoff coefficients, can be simply due to natural year to year or season to season variability, rather than fire. The length of the time period does not allow verifying this.

AUTHOR RESPONSE Fire effects on hydrologic and geomorphic processes are highly transient. Because of often rapid vegetation recovery, the greatest effects on hydrology and erosion generally occur shortly after the fire. As the catchment recovers from fire we would see a change in response and the impact of the fire would be less obvious. Focus on the short-term response is therefore most interesting for assessing the effects of fire on the risk of flooding and land degradation, and is therefore the strength of this study. To study the direct effect of fire we can either compare short time series across neighboring catchments (as in this study), or long time series in a single burnt catchment (with pre and post fire and recovery). We agree that natural variability is always present and cannot be avoided. This is why we used a control catchment and we demonstrate that this also changes. The change in the burnt catchment is analyzed, taking into account the change in response in the control catchment, i.e. taking into account the year to year variability. We believe that analysis of the relatively short time series is more than justified with the variety of comparisons between the treated and control catchments, including ANCOVA analysis and in the revised manuscript also the traditional paired catchment analysis. We agree that given sufficient time and sufficient budget it would have been great to study the short term and long term responses, preferably across different years and different catchments. However, given the current research climate, this is as distant as travel to the next galaxy. This is why we suggest that a meta-analysis of older research could deliver some new insights, and this could specifically focus on differences between short-term and long-term responses.

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REVIEWER COMMENT The authors for example mention that the occurrence of large rain events was higher after the fire than before (paragraph 3.1). Clearly this cannot be attributed to fire, and may well be the cause of the increase of runoff coefficient that the authors have observed.

AUTHOR RESPONSE Of course the higher rainfall cannot be attributed to the fire, what we discuss is that this natural variability influences our results and explains the limited fire effects on the hydrology. This is discussed in the Results section (page 4065, line 12-18 and further) as well as in the Discussion (page 4068 line 24 to page 4069 line 7).

REVIEWER COMMENT My suggestion to partially overcome this problem is to look at other neighbouring catchments where longer time series are available, and evaluate the year to year variation of runoff coefficients.

AUTHOR RESPONSE It would be great if there were any neighboring catchments with sufficient data in addition to our control catchment Espinho. Regrettably this is not the case, at least not in the immediate neighborhood nor of similar size. Any further comparison with such catchments would be rather pointless due to the additional complexities (elevations, climate, aspect, etc.) as there is no single first order control on runoff. Monitoring in these two catchments was specifically set up for this research. Again, this is why we suggest the meta-analysis of existing research as a next step in this area.

REVIEWER COMMENT In addition, the authors should use also the most recent data after the fire experiment (until 2011).

AUTHOR RESPONSE Our aim was to study the direct short-term hydrological impact of fire, because the risk of flooding decreases with vegetation recovery and therefore time since fire. The data collected in the second post-fire year will therefore be part of a future paper dealing with the recovery of the area after the fire. There is limited research on this direct impact of fire in the European Mediterranean (see Introduction,

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second last paragraph).

REVIEWER COMMENT The authors use a neighbouring catchment, but the hydrological similarity of the two catchments is not verified. The fact that two catchments are close does not imply that they are similar (Oudin et al, 2010).

AUTHOR RESPONSE This comment is a variation on the earlier concerns raised by this reviewer in relation to the comparison of the two catchments, but it is somewhat contradictory. His earlier suggestion is to compare to more catchments, which are even more likely to be dissimilar. Aside from that, we do not only compare the burned and unburned catchments, but are also comparing pre and post fire, using the pre fire as a control on the post-fire comparison. In other words, we agree that the catchments are different but attempt to take this variation into account during our analysis. This is common hydrological practice. While recent research suggests that even this is impossible as the rainfall runoff response might not be time-invariant (Milly et al. 2008; Wilby, 2005), it is currently the best analysis available. Until we can unravel all the intricacies surrounding the differences between the catchments, the comparison is reasonable given the data available.

REVIEWER COMMENT Figure 6 is taken as the evidence that (i) rainfall is a good predictor for discharge, and (ii) there is a significant change in the relationship pre and post fire. The relation between rainfall and discharge is not surprisingly poor (which is the main reason for developing more complex hydrological models), and the authors do not prove that the change in the relationship is significant. The large scatter makes me think that if the authors plot the linear relationship with the associated uncertainties (parametric+ residual), they may find that the change in slope may not be that significant.

AUTHOR RESPONSE Figure 6 is a scatter plot that is given to illustrate the effects of the ANalysis of COVariance (ANCOVA). With the ANCOVA, we tested the effect of fire on flow while adjusting for rainfall, i.e. while statistically controlling for variation in flow

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caused by variation in rainfall. To do this, we plotted rainfall against flow before and after the fire, and compared the linear regression lines that we fitted through the data points. Results of the ANCOVA analysis then indicated whether the before-and-after linear regression lines were significantly different. In all cases, the interaction between fire and rainfall was not significant, which means that the change in slope was not significant. The p-values displayed in the lower right corner of each frame in Figure 6 refer to the significance of the effect of fire, or the intercept of the regression line. We will update the text here so that this is clear. Although Figure 6 does show that rainfall affects discharge, we do not state that this figure is the evidence that rainfall is a “good predictor” for discharge, as the reviewer suggests. The fact that rainfall is a good predictor for discharge follows from the results of the ANCOVA analysis, which is specified on page 4066, line 8-10.

REVIEWER COMMENT Apart from this, instead of trying to linearly relate rainfall to discharge, they should at least include soil moisture in their relation (since they have the data). Hence, instead of $Q=a+bR$, they should try $Q=a+bR+cS$, where a , b and c are parameters, and Q , R and S are discharge, rain rate and soil moisture. This will probably give them better predictions and help to draw more meaningful conclusions.

AUTHOR RESPONSE It would have indeed been very interesting to add soil moisture to the relationships. However, since we only have soil moisture data for the Valtorto catchment and not for the control catchment Espinho, we can unfortunately not perform this analysis.

REVIEWER COMMENT The last part of the discussion is largely speculative and can be removed, including what refers to figure 10. The authors should stick to their results, and better address the limitations of the study, rather than commenting on aspects that are not covered by the paper. 5 paragraphs of discussion are really unsupported by the results of this study. Please shrink to 1 paragraph of ‘relevant’ discussion and 1 paragraph of limitations.

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AUTHOR RESPONSE Most of the reviewers’ comments about the limitations of our manuscript are discussed elsewhere in this document (short time series, software names, poor figure quality). The reviewer’s other two limitations were 1) basic analysis, and 2) extrapolation of results. Regarding the reviewer’s comment about the basic analyses, we don’t understand the point the reviewer wants to make, as s/he doesn’t indicate what the limitations of our analyses are. We could easily argue that simple is often better, however the variety of analyses we used are adequate for the questions we wanted to answer and the conclusions we wanted to draw from our data. Regarding the extrapolation of our data, reviewer #1’s opinion completely opposes reviewer #3’s, who stated “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.” We think it is important to not only present our results, but also synthesize our and other’s results in order to improve our understanding of the interplay of the different processes after fire. This comprehensive synthesis of the various hydrological changes after fire is currently lacking in the scientific literature. Given reviewer #3’s enthusiastic remarks, we feel justified to leave this synthesis including Figure 10 in the discussion. However, we will look into ways to rephrase things in order to shorten the discussion.

REVIEWER COMMENT The figure quality is very poor and makes them difficult to read. Labels are very small, captions are also unclear. For example, when the author refer to the subplots, the description of the figure sometimes follow the letter of the subplot, like in figure 2, sometimes it precedes it, like in figure 3. In figure 2 the lines are too thin, it is not clear which lines are associated to the primary and secondary y axis. Figure 7 should be done also for the control catchment.

AUTHOR RESPONSE The figures are indeed quite small, much smaller than in the document we submitted. We will contact the editorial office to see how we can improve this in the revised version, and we will correct for the inconsistencies in the figure captions. We can unfortunately not repeat Figure 7 for the control catchment, since

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soil moisture data was collected in the (to be) burned catchment only (see Table 2)

REVIEWER COMMENT Whether the time series were stored with mysql, excel, R or whatever other software is not relevant here. Also, the type of statistical analysis that is made is more important than the software that is used to make it. So please be more specific on the names of the statistical analysis (e.g. Pearson's correlation, cross correlations, etc.), and not the software, otherwise one may get the impression that the authors are getting some numbers without knowing how they are generated.

AUTHOR RESPONSE This is an important point that was also raised by the other two reviewers. In the revised version, we will include more detail about the statistical methods that were used, and also discuss why we have chosen these methods. The reason why we mention that analyses were done in R, is that most of the methods used are functions in R. However, the manuscript should clearly also be understandable for non-R users, and this is apparently not the case now.

REVIEWER COMMENT The authors cite a lot of unpublished work. The referenced papers are strongly related to this work, and they should be made available to the reviewers.

AUTHOR RESPONSE Please refer to the table (pdf enclosed) for links to the external conference papers, which are all available online. Also included in this table is an overview of the status of the citations of our own work. Pdf files of these papers will be sent to the editor of HESSD.

Milly, P.C.D., Betancourt, J., Falkenmark, M., Hirsch, R.M., Kundzewicz, Z.W., Lettenmaier, D.P., Stouffer, R.J., 2008. Stationarity is dead: whither water management? *Science* 319, 573 - 574.

Oudin, L., V. Andréassian, C. Perrin, C. Michel, and N. Le Moine (2008), Spatial proximity, physical similarity, regression and ungauged catchments: A comparison of regionalization approaches based on 913 French catchments, *Water Resour. Res.*, 44,

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W03413, doi:10.1029/2007WR006240

Wilby, R.L., 2005. Uncertainty in water resource model parameters used for climate change impact assessment. *Hydrol Proces* 19, 3201-3219.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/8/C4746/2011/hessd-8-C4746-2011-supplement.pdf>

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

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