Hydrol. Earth Syst. Sci. Discuss., 8, C2102–C2104, 2011
www.hydrol-earth-syst-sci-discuss.net/8/C2102/2011/

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 #1

Received and published: 10 June 2011

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.

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. 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.

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

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. 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.

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.

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.

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.

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.

Oudin, L., V. Andréassian, C. Perrin, C. Michel, and N. Le Moine (2008), Spatial proximity, physical similarity, regression and ungaged catchments: A comparison of regionalization approaches based on 913 French catchments, Water Resour. Res., 44, W03413, doi:10.1029/2007WR006240

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