Hydrol. Earth Syst. Sci. Discuss.,
doi:10.5194/hess-2015-538-RC1, 2016


Hydrology and Earth System Sciences
Discussions
Open Access
EGU



# *Interactive comment on* "Flood risk reduction and flow buffering as ecosystem services: a flow persistence indicator for watershed health" *by* M. van Noordwijk et al.

**Anonymous Referee #1**

Received and published: 29 February 2016

The paper presents an interesting attempt to develop a simple measure of flow persistence and address the issue of the ability to detect and attribute variations in daily river flows to the effects of land-cover change in large catchments (river basins), a well-known issue in hydrology. The authors then use a model for estimating flow persistence to try to demonstrate how difficult it can be to identify the effects of land-use change in four tropical catchments. Their illustration of the sample sizes needed to identify effects is also interesting but difficult to interpret as too little information is given on the catchments used as examples. I am in full agreement that the issues of detectability and attribution are important for hydrologists to investigate because decision makers need evidence that catchment restoration can reduce flood risks and increase flow per-



Printer-friendly version

Discussion paper

sistence by redirecting water from flow paths with rapid responses to rainfall to those with slow paths. This evidence can then be used to demonstrate that those benefits are being realised. The authors also note that these effects are well documented in hillslope and small catchment studies but there is little evidence of such effects at in large catchments (river basins). I have no problem with their argument that some form of measure of hydrological change is important, but (a) their method of deriving the persistence indicator and (b) of applying their model to the four catchments is not adequately explained. Overall the paper needs substantial work and possibly reworking in to two papers.

I have divided my review into two sections based on the two main components of the paper: (a) flow persistence and (b) flow change detection and attribution. There are some typographic errors but I have not gone into these as the paper needs rework.

A) The one part of the paper addresses both flood risk and flow buffering by measuring aspects of the flow responsiveness of a catchment using a simple index (Fp) of the flow persistence. Flow persistence is defined as identical to the 'recession constant' (pg 7 line 7). However, I would argue that flow persistence is only half the picture, what is needed is actually a measure of flow responsiveness to rainfall because flooding can depend on how rapidly the flow increases versus factors that constrain that flow and cause water levels to rise. Flow responsiveness is also directly influenced by antecedent wetness, rainfall event depth and duration, and other factors which they do discuss but do not seem to incorporate in their approach. Maybe I missed it, but I did not find a clear statement that Fp is only being calculated for the descending limb of a hydrograph. Yet this must be so because the range for Fp is constrained to the range 0-1. I would argue that to understand flood risk you also need to measure the ascending limb of the flows (the rapidity of the rise in response to rainfall patterns). I think would be probably be necessary to have indices for both the ascending and the descending limb as they are rarely symmetrical. Attempting to parameterise Fp for the various flow pathways seems to me to add complexity but not much insight given

their focus on large catchments where the relative importance of different flow paths can vary considerably across space and temporally. I did not find Figure 1 helpful as an illustration of the causal pathway or for placing the wide range of factors into their context. It has too much detail presented at the same level, is not adequately explained, and has too many terms that are not explained in the caption. A figure should be able to stand on its own with its caption, but in this case it does not, even with an extensive caption. A part of the problem is the use of two sets of arrows with different meanings in ways that are confusing. A more common way of representing this kind of diagram is a flow chart where the factors that influence (solid arrows) the (relative) magnitude of the water flows (hollow blue arrows) are represented in ways which make their role much clearer. I do not understand why rainfall is not made the start of the diagram and why the caption ends with number 0 rather than beginning there as one would intuitively expect. It is also not clear that land cover has various influences at both the "plot" level (whatever that may be) AND at the hillslope/landscape level. It is also not clear to me why there is a blue water flow arrow directly from rainfall to #2 and to #3 without passing through the landscape (and why #2 is in brackets). Why are some of the arrows broad and others not? Is the "triangle" to the left of human population density an arrow? Why are human population density and topography (subsidence) linked? What is the relevance of subsidence? Why is topography placed here and not within the sets #0 and #1 given its importance as a factor in the generation and flow of surface and subsurface runoff at both plot and hillslope levels? For a paper that attempts to explain how land cover changes affects catchment flow responses I find it inexplicable that there is almost no reference to: (a) the very extensive body of hillslope hydrology research into flow pathways and the temporal effects of different water partitioning and surface/subsurface on flow response to rainfall inputs; and (b) how hillslope responses might scale up to larger parts of catchments and large catchments. Even the brief mention of the different ways in which overland flow can be generated (e.g. Hortonian versus variable (saturated) source areas) fails to cite the original research papers and the insights they provide in the catchment responsiveness. Despite reading section 2

a few times I am still not entirely clear on the logic of the various deductions that are made about low flow, seasonality and the influence of varying Fp on the form of the hydrograph. Perhaps this is because the text is not always very clear. For example, the authors present the following (page 7 line 19 onwards):

"If we consider the sum of river flow over a sufficiently long period, we can expect $\Sigma$Qt to closely approximate $\Sigma$Qt-1, and thus $\Sigma$Qt =Fp $\Sigma$Qt-1 + $\Sigma\varepsilon$ (equation 2) From this relationship we obtain a first way of estimating the Fp value if a complete hydrograph is available: Fp = 1 – $\Sigma\varepsilon$/ $\Sigma$Qt (equation 3)"

The only way I can derive equation 3 from 2 is to assume that $\Sigma$Qt-1 = $\Sigma$Qt and so $\Sigma$Qt can be substituted for $\Sigma$Qt-1. However, if this is so, then the only way equation 2 can hold is if Fp = 1 and $\Sigma\varepsilon$ = 0. If this is so, how can this relationship then be used to estimate Fp? Or am I missing something here? In section 2.4 I assume that a model with a set input of daily rainfall and flow responses to that rainfall was used to create the ascending flow limbs so that Fp values could be used to generate the descending flow limb? And so that Fp could be varied? I also had similar difficulty in following parts of the methods section. For example, on pg 12 line 9 the term Qadd is abruptly introduced without an adequate explanation of its meaning. This is followed by the 'apparent Qadd' and Fp,try, again with no proper explanation. I should not have to go and find the paper cited (in fact a user manual) or to download the spreadsheet for an adequate explanation of the terms or to find a proper explanation of the FlowPer algorithm.

B) The second part of the paper deals with the application of the GenRiver model for assessing the impacts of land cover on river flow and its attribution and detectability. The entire model and its application is only introduced in the methods but its structure and use should really be described already in the introduction. The model is said to be spatially explicit but it is not clear how that is realised in practice (i.e. it a distributed model?). Tables 1-4 do not provide an adequate description of the study catchments – what does dominant land cover mean? Although the authors note the importance





Printer-friendly version

Discussion paper

of knowing what changes where in a catchment in relation to flow paths and times and attributing responses to changes and other factors, Table 1 does not give any indication even of how much of each land cover there is the baseline situation. Why not provide summaries or maps? Nor are we given information on where, in relation to this baseline state, the changes in land cover are made for the different scenarios. Why not provide a summary or a map? In Table 4 there is a repeated use of "some" in describing the changes made. This to me is not acceptable. We are not given an adequate explanation of how the single values of each of the 13 parameters of the GenRiver (Table 2) were obtained. Those parameter values are all ones that would vary a great deal spatially and with different land cover types (e.g. interception), but only a single value is given with no indication of their variability in the study catchments or how representative each value is. Providing definitions of the terms in a user manual the reader would have to look up is simply not acceptable. Table 3 also gives values for three important parameters for each of the land cover types with no explanation of what their sources and ranges are (BTW surely interception [Table 2] differs between forest and annual crops and so is land cover specific?). Table 3 also introduces the term relative drought threshold with no explanation of what it means and how the model uses it. The legitimacy, accuracy and representativeness of these values, together with the land cover changes, are critical to our confidence in the model outputs and thus in the analysis of the detectability and attribution of the changes in flows to changes in land cover. A study should be repeatable and this hypothetical modelling exercise certainly is not given the information included in the paper.

In summary I am not entirely sure what to recommend overall. The idea of deriving a simple but robust measure of flow change (i.e. flow responsiveness) which can be causally related to land cover changes is sound, and necessary. Flow persistence (Fp, recession) is an interesting measure and can be related to changes in the relative importance of different water flow paths, but it is also evident that it is not straightforward to derive and could be masked by the effects of location and catchment heterogeneity. I do think that a measure of the flow recession is not sufficient, the nature of the





Printer-friendly version

Discussion paper

whole response to rainfall needs to be assessed for flood risk. The flow persistence component of the paper needs careful thought to make sure that the measure(s) are clearly and thoroughly explained. Even so, I still am left with the question of whether a simpler approach would not be to examine the slopes of the flow recession curves (in relation to rainfall event sizes and sequences [antecedent conditions]) for possible shifts due to land cover changes. Alternatively, using shifts in the flow duration curves as measures of changes in the relative importance of flow pathways, as has been done elsewhere, would be more effective and understandable. Another alternative would be to use the relationship between rainfall event sizes and sequences (e.g. antecedent wetness) and flow response to those events and sequences to infer changes.

The modelling component needs a lot more information to back up the chosen parameter values for both the hydrological (Table 2) and land cover-specific values, as well as specific information on the extent of the land cover changes and their spatial configurations. It also needs to provide information on how well the outputs it generates for the different land cover types compare with the findings of other studies (i.e. how well does the model perform). Overall I need more information on the model structure and setup to interpret how well it performs in this application. This would require expanding the paper substantially.

Overall, my conclusion is that perhaps this paper attempts to cover too much ground and should be two papers: - One on the issue of catchment responsiveness to rainfall as a measure of land cover change, including flow persistence - One on modelling of the effects of land cover change on river flow responses and the difficulties of detecting and attributing changes in flow responsiveness to changes in land cover (and relating this back to the changes in the relative importance of flow paths linked to the changes in land cover).

---

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., doi:10.5194/hess-2015-538, 2016.