

Interactive comment on “Regionalization of patterns of flow intermittence from gauging station records” by T. H. Snelder et al.

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Re: Hydrol. Earth Syst. Sci. Discuss., 10, 1511-1551, 2013, www.hydrol-earth-syst-sci-discuss.net/10/1511/2013/. doi:10.5194/hessd-10-1511-2013 Title: Regionalization of patterns of flow intermittence from gauging station records Authors: T. H. Snelder, T. Datry, N. Lamouroux, S. T. Larned, E. Sauquet, H. Pella, and C. Catalogne We sincerely thank the reviewer and your constructive comments on our paper. These have helped us to improve the scientific content of our paper. We have replied to the anonymous reviewer in the order of their the comments. We have attached as supplementary material a marked up copy of the revised manuscript to show how the changes have been made where not explicit here. Specific comments Anonymous Referee #2

1. P1517, L1-5. My query with removing gaps in the hydrological record is whether

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these gaps are associated with zero flow periods. In my experience, sometimes gaps (i.e. missing data) clearly occur during periods of zero flow, which can be identified by zero flows in the days preceding and/ or following the missing data, in conjunction with the absence of runoff during the same period. I wonder if removing these years from the analysis may be therefore underestimating the frequency and duration of zero flows, which has some implications for the key findings of this paper.

We have looked at the overall level of gaps in the data used for analysis. On average gaps (of any duration) accounted for only 0.14% of the days of flow record. The intermittent gauges had a similar proportion of gaps. We have included these figures in the first paragraph of the results section. We consider it unlikely that the results are strongly influenced by gaps.

2. P1529, L3-13. How do these relationships between environmental variables and each flow regime class compare with other flow regime classifications in other parts of the world (e.g. Kennard et al. 2010)? Are there any comparable relationships across different areas? Based on my experience, I suspect that these relationships between environmental variables and flow regime variation are quite consistent with similar studies. These similarities (and unique aspects) could be used to strengthen the paper by making general statements about the climates and catchment characteristics where intermittent flow regimes occur.

We think direct comparison of the present study with that of Kennard et al. (2010) is not appropriate. The classification of Kennard et al. (2010) was of flow-regime types and included indices describing hydrological characteristics such as seasonal pattern of discharge, variations in flood magnitude and frequency and other aspects of flow predictability and variability as well as intermittence (i.e. perennial versus intermittent). Therefore, the environmental variables that discriminate any particular class in the classification of Kennard et al. (2010) may represent relationships with characteristics other than degree of flow permanence. Relationships between intermittency and environment in the classification of Kennard et al. (2010) are effectively confounded by

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the presence of these other hydrological characteristics.

3. This study identified that there are three hydrologically distinct intermittent flow regime classes (e.g. Figure 2) in France. Coming from a more ecological versus hydrological background, this is an important point, because different forms of intermittent flow regimes contribute to different ecosystem (in terms of structure and function). Although it is not the main point of this paper, I wonder if including some plots summarizing the key hydrological characteristics of each of the four flow classes presented in Figure 2 would be useful to emphasise that there is substantial variation in intermittent flow regimes. Kennard et al. 2010 present a useful example.

We agree that there is substantial variation in intermittent flow regimes. We demonstrated this by analyzing the frequency and duration of zero flows. Figure 4 (Figure 5 in the updated manuscript) shows how variable these quantitative aspects of flow intermittence are across sites. The study is not concerned with other aspects of the flow regime and therefore we do not think it is appropriate to consider other hydrological characteristics.

There could also be potential to incorporate this variation into the Discussion section to broaden the scope of this paper. For example, if we can get a grasp of how different the intermittent flow regimes are, we can then compare this to ecological studies testing for differences in ecological characteristics across different flow regimes. By verifying that the different intermittent flow regimes have some ecological validity (could be based on published information; I am not suggesting that new data be collected to verify this), however this would give some support to the current hydrological classification if there is concordance with ecological patterns

We consider this is already a long and quite complicated paper. Consideration of whether the different types of intermittence are associated with distinct ecological characteristics is well outside the scope of the present study, but would certainly make a great study in the future. Moreover, with the help of > 10 references to recent ecological

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studies, the first section of the introduction indicates how much frequency and duration of dry events have influences on ecosystem processes and biodiversity.

Specific comments: P1512, L3. Could an extra sentence be added here to describe why/ how the previous sentence is important? As an ecologist, variation in flow regime intermittency is becoming consistently associated with patterns and variation in biodiversity and ecosystem functioning in rivers. Emphasising why understanding spatial variation in flow intermittency in the Abstract may engage a broader readership.

Agree. We have amended the first two sentences to now read: “Understanding large-scale patterns in flow intermittence is important for effective river management. The duration and frequency of zero-flow periods are associated with the ecological characteristics of rivers and have important implications for water resources management.”

P1513, L5. Suggest changing “estimates” to “predictions” to strengthen this key point.

Done.

P1513, L9. Larned et al. 2010b is cited before Larned et al. 2010a (first cited P1514, L4).

Corrected.

P1515, L15. Check citation for Olden et al. 2011. The date should be 2012.

Corrected to: Olden, J.D., Kennard, M.J. & Pusey, B.J. (2012) A framework for hydrologic classification with a review of methodologies and applications in ecohydrology. *Ecohydrology*, 5, 503-518.

P1529, L25. Could the low frequencies of zero-flow periods also be influenced by surface-groundwater connectivity, regardless of catchment size? This connectivity is to some extent determined by catchment geology.

We agree that differences in our intermittence subclasses are likely to be due to differences in geological conditions, and SW-GW interaction patterns. However, our study

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was equivocal in this regard as no geological variable was retained by the RF model of the intermittence classification. The geology information was rather coarse and we have acknowledged this in the discussion.

P1530, L20-23. This is a style issue which may be acceptable for this journal, but it is rare for direct reference to be made to material presented in the Results section, in the Discussion section

We consider that our discussion must be supported by our results and that it is necessary to refer to the results in the discussion.

P1533, L23. Check the citation to Acuña et al. 2005. The journal name is incorrect and should be Journal of the North American Benthological Society

Corrected.

Specific comments Anonymous Referee #3

2. Materials and methods

Page 1516 Line 7: The use of latitude only is hardly adequate to locate the study area
Yes, we have added longitudes.

Page 1517 Line 2: the choice of 20 days needs to be qualified. Why choose 20 days as the threshold?

This was a subjective decision that was based on a pragmatic compromise: a lower value would have led to fewer qualified years. The choice of 20 days resulted in gaps (of any duration) accounting for only 0.14% of the days in the entire flow record (i.e., all 628 sites). This was similar also for intermittent gauges (0.2% of the day in the entire flow record for these 123 sites). We have included these figures in the first paragraph of the results section. We consider it unlikely that the results are strongly influenced by gaps.

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Page 1517 Line 8: “.. the frequency of zero-flow periods”. Does this mean that a period of 1 day would be treated in the same manner as a period of 19 days?

Yes.

Page 1517 Line 22: use words rather than this symbol . I suppose that means ‘about’ or ‘roughly’. There are a few more places in the text where this symbol is used.

Changed to “approximately “throughout.

Page 1518 Lines 24-25: Why use a “drainage density that was independent of our DEM-based network”? I would have expected that the same network used in the study would be used for estimation of the drainage density. There is need for consistency here, especially given that the two are likely to give different densities. The authors need to justify why they used a different network for the drainage density.

The DEM-based network is defined automatically and has a network density that is determined by computational criteria (primarily catchment area). The 1:250,000 scale BD Carthage[®] river-channel map is based on the observed network using a different mapping procedure. We considered that the observed (rather than automatically produced) drainage network density may reflect relevant soil and geological characteristics such as perviousness of the surficial material and that this may provide a useful predictor variable. We have clarified this in the manuscript.

Page 1519 Line19: delete ‘retained for analysis’. There is no need to have that phrase and it is a repetition as it has already been mentioned in the sections before this one.

Changed.

Page 1519 Lines 23-26: the line beginning ‘In the intermittence...’ is difficult to follow. Rephrase

Changed.

Page 1519 Line 26: ‘We grouped the 123 gauging...’, The figure 123 just pops out of

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nowhere. There is no mention of 123 gauging stations being on intermittent segments in the text before this. Some statement relating to this should be inserted somewhere in the text.

We have removed the number completely – this is a result and is not needed to understand the methods.

Page 1520 Line11: Delete the first ‘dissimilarity’ so that the statement reads ‘Our first matrix described the dissimilarity. . .’

Changed.

Page 1520 Lines 14-15: A brief explanation of how the permutation procedure would establish the significance of the statistic would be desirable here.

A brief description of the method has been added.

Page 1520 Lines 19-21: Replace ‘due to’ with ‘as a result of’. Also here I do not understand how “... station record had not commenced or had ended” is a problem. Some justification is required here.

To be more clear we have reorganized this as: “The calculation of dissimilarities was complicated by missing data for some years as a result of gaps or because the station records had differing durations within the analysis period”.

Section 2.7: this section is rather too long and over-detailed with a lot of descriptions of the methods but very little reference to the subject of the paper. Such detail is not necessary and should be reduced. Relate the descriptions to the intermittence subject at hand.

This is really an editorial decision. The description of Random Forests provides only the essential elements of the method. These are need for the reader to understand the modeling process, the importance measures, partial plots and the variable reduction procedure.

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Page 1525 Lines 9-10: Difficult to follow. Rephrase.

“At least one station had its highest zero-flow frequency or longest zero-flow duration in 10 almost every year (i.e. at least one value of 100% occurs in most years in Fig. 3).”

We deleted this sentence and edited the preceding sentence to read: “However, the year in which the highest zero-flow frequencies or longest zero-flow durations occurred at each station was variable and there was no common year in which the highest frequencies or longest durations occurred (Fig. 4).”

Page 1525 Lines 18-20: What is the justification for assigning the three outliers to the closest subclasses? How important are these outliers? Is there any common thread running through the outliers? These outliers should be discussed a bit more as they could be significant in assessing the methods used.

These are not outliers. They are sites that fell outside the nominated boundaries. Given these boundaries are nominated for the purpose of defining groups of broadly similar sites, our assignment to the closest group is pragmatic and sound. Statistical grouping procedures such as clustering are based on similar processes (assigning objects to their closest group).

Page 1526 Line 1: See my comment above with regards permutations. The method is mentioned in passing but it seems to have a bearing on the results. A statement describing the method would suffice.

Done in the methods section.

Page 1526 Lines 25-27: Delete the second ‘river’ to read ‘: : : 39% of river segments represented by our network

Done.

Page 1527 Line 1: What does ‘higher level of accuracy’ really mean? The statement

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on its own is meaningless and should be qualified by an additional statement.

We have edited this to say that they were well predicted. The following sentence quantifies this with a test of the observations versus the predictions.

Page 1527 Lines 6-20: Very good results. This is a significant part of the study. My concern is that the discussion of these results is missing even in the discussion section.

We have dealt with these results in the discussion. We have slightly altered the discussion to acknowledge that were expected relationships (i.e., they are not surprising).

Page 1528 lines 1-6: it seems to me that this paragraph is a repetition. Check that the same thing has not been said in the preceding sections.

We have reduced this and removed results.

Page 1529 Lines 7-9. The statement beginning ‘The probability of: : ::’ does not read well. Rephrase.

We edited this to read: “The probability of intermittence had significant but more complex relationships with the environmental variables SumWinRain and Perm.”

Page 1533 Line 4: The use of the word ‘accurately’ must be qualified. What does ‘accurately’ mean?

Edited this to read: “Although predictions of intermittence were not accurate at the segment scale, when aggregated by HER they produced good estimates of the proportion of intermittent segments at regional scales (Fig. 10).”

Page 1533 lines 10-12: Rewrite the line to read “preliminary estimates of how climate change could impact the frequency of”

Corrected.

Table 1: variables Allu, Chalk, Lime all have the same description.

Corrected.

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Figures 1 & 6: the captions are not informative enough. One would have to go back to the text to understand the figures.

We have replaced the caption for Fig. 1 with: France showing the Hydro-EcoregionR (HER) boundaries (Wasson et al., 2002). The number of gauging stations included in this study in each region is shown in parentheses in the legend.

We have edited the caption for Fig. 6: (Now Fig. 7) to read as follows: Receiver operating curves (ROC) plot (left) and threshold plot (right) for the flow-regime classification. The black circles on the threshold plot indicate the probabilities thresholds that maximize the classification performance as measured by Cohen's kappa and the percent correctly classified (PCC).

Figure 3: is squashed and difficult to read. An increase in size may help, or a re-labelling of the x-axis.

We have improved the readability of Fig. 3 (Now Fig. 4) by rotating the x-axis labels.

Please also note the supplement to this comment:

<http://www.hydrol-earth-syst-sci-discuss.net/10/C979/2013/hessd-10-C979-2013-supplement.pdf>

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