

## ***Interactive comment on “Developing a novel approach to analyse the regimes of temporary streams and their controls on aquatic biota” by F. Gallart et al.***

### **Anonymous Referee #1**

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#### General comments

This paper presents a method that extends the existing framework for permanent streams to the characterization of temporary streams habitats for the study of macro-invertebrate communities. It can be relevant for the implementation of the WFD, research on temporary stream biota, and for management authorities; and fits well within the scope of HESS. The paper reaches an overall high scientific and formal standard quality. However, it suffers from a lack of data mainly concerning method validation (few streams were monitored + few ecological data presented). It nevertheless consti-

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tutes a call to the scientific community for testing and challenging the method, which is in line with the “Open Source” philosophy of HESS. Moreover, improving certain (for most minor) aspects of the text (see below) by additional details or explanations will increase the overall impact of the manuscript. The paper is well-structured and written in clear and fluent English. The introduction nicely sets the theoretical frame of the study and discussed its potential implications for practice. The methods are of high scientific quality although a few points need clarification. Results are clear, concise and nicely discussed. The conclusions, although substantial and addressing all main issues, are sometimes confusing and poorly supported by data. They therefore need clarification. The authors did an extensive literature review that covers appropriately the main papers of this area of research. Supplementary material may be completed.

#### Specific comments

The title highlights the control of temporary streams hydrology on the aquatic biota. However the authors are vague when describing the mechanisms by which flow regimes influence the biota. The authors should either adapt the title or improve this aspect throughout the entire manuscript.

The authors made it clear that temporary stream biota is different from that of permanent streams. However, it remains unclear what is the importance of temporary streams for European waters and more specifically for the achievement of the WFD. Adding a few words addressing this issue in the introduction would improve the overall impact of the paper. The method has a main weak point: the definition of the boundaries between aquatic states. It is, in my opinion, essential to determine whether the estimation of threshold flow values without field observation is sufficiently accurate and reproducible among scientists before the method can be used routinely in temporary stream studies. The authors identified these issues and are currently conducting research to address them. However, no data is presented to verify the many hypotheses launched. Simple manipulation such as progressively changing the threshold flow values and comparing the resulting ASFGs can allow a more precise definition of the

impact of errors on the method.

The method is poorly performing when dealing with the analyses of the drier aquatic states. This is problematic because the drier states are most likely to impact biota strongly. Moreover, promising avenue of the method consists in the framework it offers for the study of these drier stages. The authors should discuss in more details potential solutions to overcome this limitation.

The authors are, in my opinion, right when saying that biological sampling should be adapted to the hydrological regime. However they remain elusive when explaining how to determine the optimal sampling period. The authors did not discuss the possibility of pooling seasonal biotic samples for an entire year to account for temporal variability. Addressing shortly these issues would improve the repeatability of the method.

The authors spend considerable efforts describing the ecologically relevant aquatic states with the goal of evaluating the ecological quality of temporary streams using fauna characteristics. However there is few information on how these states influence the aquatic biota beside the fact that the hyporeic zone can serve as a refugee during dry periods. The reader has to dig into a large list of reference. Although well known, a few sentences summarizing how the functional and structural characteristics of the aquatic biota change according to these states will improve the message. The same is true for the influence of streams with high/low Mf and Sd values on the biota?

The authors highlighted the possibility to adapt the method using EC and temperature data. However the discussion about the method to, and the advantages/disadvantages of doing so did not go over the fact that few data are currently available. Several questions that, in my opinion, can be answered easily in a few sentences were left aside. What are the advantages/disadvantages of EC and temperature over flow data? How can we adapt/improve the method using the former two metrics? Which type of data is, according to the authors, the best for applying the method (beside considerations of the availability of the data)?

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In addition, providing more explicit or detailed explanations concerning certain aspects will improve our understanding of the method. Below is a list of bullet points that summarizes these aspects:

-The link between ASFGs and Mf and Sd should be stated more explicitly: it is not clear how complementary these two things are and how they can be complementarily used in practice.

-Can we compare the ASFGs in a more formal and/or quantitative framework than by simply looking at the patterns on the graphics?

-What are the patterns of the flow duration curve that suggest a minimum discharge threshold?

-Table 2. can potentially be moved to supplementary material + it is not clearly stated whether the authors used a correction algorithm (e.g. Bonferonni, Holmes) for multiple testing when assessing significance? If not, this should be done.

-Although the message behind Table 3. is clear, it would be difficult for me to repeat the same analysis. More detailed explanations on how the maximum likelihood factor analysis was done and how the factor loading were calculated would improve the repeatability of the method.

## Technical corrections

Methods, p. 9645, l. 10: Typing: “whenever available.”

Methods, p.9647, l. 26: “This that”

Methods, p.9644, l. 23-25: “The first and second steps follow a logical sequence, but the third and four steps are rather independent although they remain consistent with the first two.” -> Not useful, remove. . .

Methods, p.9652, l. 4-9: make two sentences

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Methods, p.9653, l. 8-9: rephrase “adapted to a quick development”

Discussion, p. 9658, l. 5: “The methods”

References, p. 9660, l. 28-29: “Functional Significance of the Hyporheic Zone in Streams and Rivers”. -> change case

References, p. 9661, l. 28-29: -> change case

References, p. 9664, l. 16-17: -> change case

Table 4. If data for the other sites are available, they should be displayed in that table.

Figure 4. Enlarge captions within each ASFG

The Mf, Sd6, Df, P, C, and M metrics for the 8 sites may be added as supplementary table.

Personal comments

To what extent do the authors think that their method can be extended to the study of the ecological linkages between aquatic and terrestrial biota in temporary streams and/or frequently flooded areas?

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 9637, 2011.

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