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Discussions

Interactive comment on "Evaluating primary productivity, ripple effect and resilience of fluvial ecosystems: a new approach to assessing environmental flow requirement" *by* Yui Shinozaki and Naoki Shirakawa

M. McClain (Referee)

m.mcclain@unesco-ihe.org

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This paper presents a new model for the estimation of environmental flow requirements (EFR) globally using a simulated raster river channel network with a spatial resolution of 0.5x0.5 degrees. The model differs from past models by incorporating biomass and biomass fluxes in setting the environmental flow requirement. Biomass increases in any cell via modelled inputs from a global NPP model, calculated inputs from adjacent terrestrial areas, and a calculated input from the upstream cell. Biomass decreases in a cell via decomposition and transport to the downstream cell. The influence of

biomass in EFR calculations is expressed by three indices, the Trophic Index (TI), contribution to downstream ecosystems (CDE), and ecological recovery time (ERT). The TI reflects the number of trophic levels present (1 to 5), which is expressed as ecological structure ranging from 'poor' (TI=2) to 'diverse' (TI=5). These levels are then interpreted to represent the levels of general condition of streams presented in the method of Tennant (1976) ranging from 'poor' to 'optimum' condition. TI is determined using a tropic model estimating the number of potential trophic levels based on the amount of biomass calculated in the fluvial portion of the model cell. This index is used to set an initial minimum and range of low flows that are subsequently modified based on the values of the remaining two indices. CDE is based on flow velocity and normalised such that CDE approaches zero for the slowest modelled velocities and 1 for the highest modelled velocities. ERT is calculated as the time (model annual time steps) required to reach stable biomass equilibrium if biomass is reset to zero. ERT is also expressed in the EFR calculation as a normalised value between 0 and 1, with 1 reflecting the fastest recovery time. The combined effect of CDE and ERT is that, as they increase in value, low flow requirements are set progressively closer to the maximum value of the low flow range, as originally set by Tennant (1976). A high flow component of the ERT is also calculated based on the annual variability in monthly mean flows, similar to the approach used previously. Higher intra-annual variability results in larger values for the high flow component.

The authors refer to their model as an improvement of the Tennant method. They also state that "Instead of offering specific values and criteria, in this paper, we provide a perspective on the conceptual method for setting EFRs." Specific values are, however, eventually offered and compared with previous results of Smakhtin et al. (2004), and the outcome of the comparison is presented in the conclusions and abstract.

The authors are to be commended for their efforts to add new, ecologically relevant variables (TI, CDE, and ERT) to the quantification of EFRs at a global scale. I do, however, believe there are significant flaws in the current formulation of the approach which limit the applicability of the model and its results.

My main concern is that the authors have misconstrued the meaning of the incremental levels of flow identified in the Tennant method. This is critical because the selection of the low flow minimum and range (x1 in their calculation) exerts a dominant control on the eventual EFR. CDE and ERT simply adjust low flow recommendations within the range established by Tennant. The authors have interpreted that the incremental levels reflect variable ecological structure (i.e. increasing number of trophic levels), when in fact the levels reflect the condition (from natural to increasingly degraded) of the river ecological structure and function, whatever the natural state might be. This follows from a simplified view of the natural flow paradigm that the risk of degraded ecological condition increases as anthropic flow alteration increases. Instead, the authors assume that the incremental levels relate to the natural levels of ecological structure. The implication is that, in the author's approach, systems which have increasingly simpler ecological structures are expected to maintain equivalent ecological condition (relative to natural) with increasing anthropic flow alteration. I know of no ecological theory or research to support this assumption. If the authors are to continue with this approach they should present a clear theoretical justification and supporting research results.

Their assumption also erases from the calculation of ERF the essential (societal based) process of setting objectives for ecosystem management, such as the requirement of achieving 'good' ecological status in all water bodies of the European Union. The levels set by Smakhtin et al. (2004) considered environmental management objectives (as required by best practice in setting ERTs), but the present paper does not. I recommend that the authors take note of this omission of management objectives in their approach and consider ways to rectify it.

In addition to these concerns, I offer the following comments for the authors to consider in the revision of their manuscript.

Introduction:

Pg. 2, line 13: parts of the text beginning "Early approaches aimed to define. . ." appear to be copied and pasted or slightly modified from Pahl-Wostl et al. 2013. Any copied and pasted or slightly modified text in the manuscript should be deleted. Citing the source of copied and pasted text is not sufficient. All text not contained in quotation marks must be original and attributable to the authors alone.

Pg. 3, line 9: "Stream flow has often been treated as the 'master variable' since it can be readily described by indices" This is not the reason stream flow is considered as the master variable. Revert to the original source (Power, Mary E., et al. "Hydraulic food-chain models." BioScience 45.3 (1995): 159-167) to clarify.

Pg. 4, line 7: Tharme 2003 is not correct reference for IFIM. Check and correct alignment of methods and original sources throughout paper.

Method:

Pg. 4, line 25: this section begins with the repetition of points made above. In fact there is quite a bit of redundancy throughout the manuscript that should be removed.

Pg. 5, lines 3 and 4: change PRC to RPC.

Pg. 9, TI section: the calculation of TI using this approach is overly detailed for the global scale and approach of the model. I recommend seeking a much simplified approach, taking into consideration my main concerns expressed at the beginning of this review.

Pg. 10, beginning line 9: as mentioned in the initial paragraphs of this review, the authors have misconstrued the purpose of Tennant's incremental levels of river ecological condition. Please review Tennant's paper carefully and represent accurately in this paper.

Pg. 10, line 20: the switch from ratios (FV 80-8000) to flow magnitudes (>8m3/s) is unexpected and unexplained here. Is it correct?

Pg. 10, line 32: first mention of the Chikugo model for quantification of NPP. This is at the root of all following calculations but is not well described. First, the indication throughout the paper is that fluvial NPP is being calculated, but from what I read in Seino and Uchijima 1993 (not 2010), the model calculates terrestrial (or generic) NPP. How is 'fluvial' NPP calculated? If the model is not 'fluvial' specific there should be an explanation of the rationale the authors use for the model. Also, the model is described as "well-established" (line 32) but according to Google Scholar Seino and Uchijima (1993) has been cited only 5 times in 26 years. What is the rationale for "well-established"?

Results:

Pg. 11, line 30: α is set as 3 globally, and length of grid cell is also the same, Therefore, it seems length is removed as a variable globally. Are there consequences to this simplification?

Pg. 13, line 12: tributaries and lakes are indicated as significant in influencing the results, but I do not understand how these are resolved (made significant) in the model. Are these resolved somehow independently in the 0.5x0.5 degree grid? If so explain.

Pg. 13, line 30: use of language like "this may be because. . ." suggests that the processes and relationships controlling ERT are not fully understood, but they are exactly known as represented in the mathematics of the model. Refer to the relationships in the model and explain more confidently.

Pg. 15, line 1: "These regions are characterised as having low resilience, resulting in longer ERT. . ." What is the research evidence (papers?) for the lower resilience of large rivers outside of monsoonal regions and savanna regions? The authors have defined resilience based on ERT. Do not turn this around and assume low resilience because of the ERT calculated.

Discussion:

Pg. 15, line 9: provide citations supporting that Smakhtin et al, results have been "widely applied in water resource assessments".

Pg. 15, line 23: Reference is made here to "feasible goals", which refer back to my concern about the lack of environmental management objectives in the approach of the authors. This needs more explicit attention in future versions of the model.

Conclusions:

Pg. 17, line 1: "We then improve the Tennant EFR. . ." as mentioned above I believe the authors have misconstrued the incremental levels of condition in Tennant, therefore I believe the Tennant method has been misused and not improved. Substantial attention is needed to address this in future versions of the model.

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