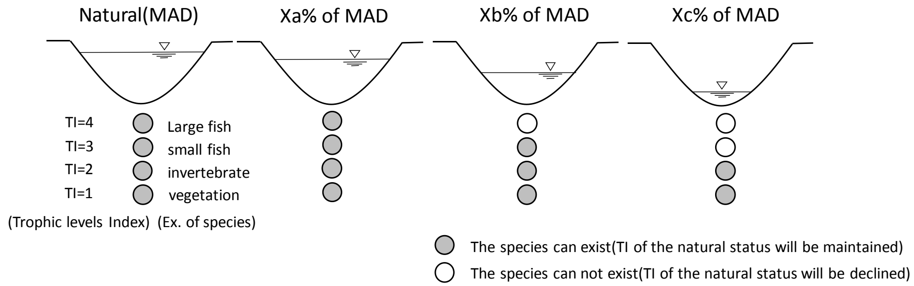
We greatly appreciate the reviewer's insightful comments on our paper. These comments have significantly helped us improve the manuscript.

We would like to answer for each comment and to express the direction for revising the paper. Please check the attached file.

	Referee's Comments	**Authors' Answers**
1	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.	As you indicated, the threshold of 10%, 30% and 60% of MAD in the Tennant method is a management level which focus on more or less artificially modified rivers and these levels are not correspond to ecological structures. The incremental levels in our model are independent from the Tennant's threshold. The authors assume that flow relates to ecological structure. That is, simpler ecological structure is expected to tolerate more flow reduction These assumptions will be supported by several classical theories and previous studies. The authors are well aware of this point, however, our explanation in the paper may be misleading and should correctly be described with corresponding references; Our model which assess the structure (TI and vulnerability) from primary productivity will be supported by two classical theories in ecological richness: Species-energy theory (Wright 1983), and species-area theory (Macarthur and Wilson 1967). Species-energy theory advocates positive correlation between species richness and energy available, in turn, primary productivity in the area. This theory has also been supported by studies for riverine ecosystems. Oberdorff et al. (1995), and Guegan et al. (1998) investigated that the NPP is a surrogate for fish diversity in rivers. Species-area theory implies that species richness increases as surface area. The explanations for that are: larger area has lower extinction rate, higher speciation rate and higher habitat diversity (Hugueny et al. 2010). In regard to this theory, Guegan et al. (1998) showed that the total surface area of the river and the mean flow are the dominant factors for fish richness. Based on these ideas, in our study, we relate the amount of flow and habitat size, and suppose the rate of flow reduction that the target ecosystems can tolerate is different according to the TI. In addition to the species-area theory, the following facts may reinforce the correlation between flow and TI. For instance, large fish (which corresponds to the species of TI=4) most directly affected by flow reduction in the consequence of habitat reduction or disappearance, because large predators need larger territory for their life history and daily predation (Bunn 2002). Also large predators avoid shallow areas in order to hide themselves from birds and terrestrial predators (Creed 1990, Power 1995). The minimum threshold level in our model is set according to ecological structures expressed by TI.



The following figure shows the conceptual relations with tolerance of each TI and flow reduction.

For the region of TI=4, if flow is reduced to Xb% of MAD, large fish at the top of the trophic level may difficult to survive. Therefore, Xa % of MAD is the minimum threshold level. Similarly, for the region of TI=3, in which small fish is the top of the trophic level, have relatively more tolerance against flow reduction, and minimum threshold level will be Xb % of MAD and so forth. On this minimum threshold level, additional rate will be added according to vulnerability (CDE and ERT).
There are no obvious threshold rates in ecological richness and flow, however, we set threshold rates as 60%, 30% and 10% of MAD in reference to the existing studies suggest environmental flow objectives.

Reference:

1. Wright, D. H.: Species–energy theory: an extension of species–area theory. Oikos,41, 496–506. 1983.
2. MacArthur, R. H., and E. O. Wilson.: The theory of island biogeography. Princeton University Press, Princeton, 1967.
3. Oberdorff, T., Guégan, J. F. and Hugueny, B.: Global scale patterns in freshwater fish species diversity. Ecography 18, 345–352, 1995.
4. Guégan, J.F., Lek, S. and Oberdorff, T.: Energy availability and habitat heterogeneity predict global riverine fish diversity. Nature (London) 391:382–384, 1998.
5. Hugueny, B., Oberdorff, T. and Tedescco, P.: Community Ecology of River Fishes: A Large-Scale, American Fisheries Society Symposium, 73, 2010.
6. Bunn, S.E. and Arthington, A.H.: Basic principles and ecological consequences of altered flow regimes for aquatic biodiversity, Environmental Management, 30, 492–507, 2002.
7. Creed, R.P.: Direct and indirect effects of crayfish grazing in a stream community, Ecology, 75, 2091-2103, 1994.

		8. Power, M.E., Sun, A., Parker, G., Dietrich, W.E. and Wootton, J.T.: Hydraulic food-chain models, Bioscience, 45, 159-167, 1995.
2	Their assumption also erases from the calculation of EFR 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 levelsset 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.	As you indicated, the practical EFR should be incorporated to the management objectives with social aspects. Tennant's threshold is combined with the management level (societal based). Our model, in contrast, set the EFR set by ecological tolerance focused on the potential productivity of fluvial ecosystems without any human impact. We have a perspective to combine this threshold with social management objective in our following research, for example: $EFR = Q \times A_1 \times A_2$ $= Q \times$ (societal based objective) $\times$ (ecological based objective) where A_1 is the Tennant's management level and A_2 is the ecological tolerance in our model. In this paper, A_1 is considered as 1.0 (natural), therefore management level is the highest. For instance, when Q=mean annual discharge (MAD), $A_1 = 0.3$ (management level =fair) and $A_2 = 0.6$ (TI=4), EFR will be 18% of MAD. The proposed EFR in the existing model, Sone River (18.9% of MAD for moderately modified status, Joshi et al, 2014) and downstream of Zab river (18% of MAD using hydrologic methods, Abdi et al, 2015) are correspond to this level. We will add the explanation in our paper. *Reference:* 1. Joshi, K.D., Jha, D.N., Alam, A., Srivastava, S.K., Kumar, V. and Sharma, A.P.: Environmental flow requirements of river Sone: impacts of low discharge on fisheries, Current Science, 107, 478-488, 2014. 2. Abdi, R. and Yasi, M.: Evaluation of environmental flow requirements using eco-hydrologic-hydraulic methods in perennial rivers, Water Science and Technology, 72, 354-363, 2015.
3	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.	The sentence was rephrased as follows; Originally, the environmental flow objectives have been mainly focused on habitat suitability for representative fish species, however, many researchers are now regarded that it is not sufficient to evaluate complex fluvial ecosystems as a whole (Acreman and Ferguson, 2010; Shafroth et al.; 2010, Pahl-Wostl et al., 2013).
4	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.,	We appreciate for the information about the important research. According to the paper by Power et al. (1995) and other researchers, we revised the sentence as follows; Stream flow is the major determinant of physical habitat and thus, a major determinant of

	et al. "Hydraulic food-chain models." BioScience 45.3 (1995): 159-167) to clarify.	biotic components (Bunn and Arthington, 2002). Power et al (1995) developed a hydraulic-food chain model using causal linkages between hydraulic parameters (depth, velocity and width) and trophic dynamics. The flow rate, determines other hydraulic parameters, considered as the master variable for evaluating ecological features of a stream. *Reference:* 1. Bunn, S.E. and Arthington, A.H.: Basic principles and ecological consequences of altered flow regimes for aquatic biodiversity, Environ. Manag., 30, 492-507, 2002. 2. Power, M.E., Sun, A., Parker, G., Dietrich, W.E. and Wootton, J.T.: Hydraulic food-chain models, Bioscience, 45, 159-167, 1995.
5	Pg. 4, line 7: Tharme 2003 is not correct reference for IFIM. Check and correct alignment of methods and original sources throughout paper.	We referred original sources for IFIM and PHABSIM: 1. Bovee K.D.: A Guide to Stream Habitat Analysis using the Instream Flow Incremental Methodology. US Fish and Wildlife Service Biological Services Programme, Co-operative Instream Flow Service Group, Instream Flow Information Paper No. 12. FWS/OBS-82–26. 1982.
6	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.	Short summery at each paragraph will be removed and redundancies will be eliminated throughout the paper.
7	Pg. 5, lines 3 and 4: change PRC to RPC.	PRC was corrected to RPM (riverine productivity model)
8	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.	As you pointed out, to apply such criteria at a global scale, it is necessary to simplify the model without omitting a fundamental mechanism of the system. To this end, the authors tried to establish the TI based on the classical but authorized basic concept: species-energy theory. The theory implies that the greater primary productivity may lead to higher trophic diversification. The purpose of the TI is to offer simple boundaries of flow- related ecological structures (as is shown in the figure of answer1) for setting environmental flow criteria. If we try to express real trophic levels of a complex fluvial ecosystem, we have to consider metabolic process at each trophic levels, species interactions, as well as regional differences in metabolic rates. However, in order to apply the model globally, we simplify the mechanism as possible and used single set of target species. The other reviewer also commented to the structure of TI. Please refer to the additional explanation we will post to the other reviewer.

<table>
<tr>
<td>9</td>
<td>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.</td>
<td>The thresholds proposed in this study were set independently from Tennant's incremental levels. To make this clear, we rewrite the paragraph. (Please refer to the answer No.1)</td>
</tr>
<tr>
<td>10</td>
<td>Pg. 10, line 20: the switch from ratios (FV 80-8000) to flow magnitudes (>8m3/s) is unexpected and unexplained here. Is it correct?</td>
<td>The threshold values in the paper are correct. We used the four factors to classify the hydro-climatic regions through conditional branch (The Table below). The sentence was unclear, so we explain all of these thresholds according to table.

<table>
<tr><th>type</th><th>MMD</th><th>FV</th><th>MaxMD</th><th>AMT</th></tr>
<tr><td>Extremely Arid</td><td>< 0.03</td><td>-</td><td>-</td><td>-</td></tr>
<tr><td>Savanna</td><td>≧0.03</td><td>> 1,000</td><td>-</td><td>-</td></tr>
<tr><td>Monsoonal</td><td>≧0.03</td><td>≦1,000, 80<</td><td>-</td><td>-</td></tr>
<tr><td>Wet-moderate</td><td>≧0.03</td><td>≦80</td><td>> 8</td><td>-</td></tr>
<tr><td>Moderate</td><td>≧0.03</td><td>≦80</td><td>≦8</td><td>> 0</td></tr>
<tr><td>Spring spate</td><td>≧0.03</td><td>≦80</td><td>≦8</td><td>≦0</td></tr>
</table>
MMD (Mean Monthly Discharge):m^3/s/100km^2
FV (Flow Variability): maximum monthly discharge/ minimum monthly discharge
MaxMD (Maximum Monthly Discharge): m^3/s/100km^2
AMT(Average Monthly Temperature at coldest month): ℃</td>
</tr>
<tr>
<td>11</td>
<td>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"</td>
<td>We will add the following explanation in our paper;
There are two ways to get NPP: to calculate by a model, or to get measured values. Measured values are available from such as NASA, however, the advantage of using a model is that is able to calculate NPP under a variety of climatic conditions, for example using the data of future climate.
We have a perspective to simulate environmental flow under several climatic patterns, thus, Chikugo model is useful because it can calculate global NPP from basic climatic information. That is the reason why we used Chikugo model.

As you pointed out, the Chikugo model calculates terrestrial NPP. Besides solar radiation, NPP in a river is affected by other physical and chemical factors such as water temperature, nutrient concentration and turbidities (Woodward 2009). As far as authors know, none of the model to calculate fluvial NPP available and thus we applied terrestrial NPP in this model. Of course the terrestrial NPP does not completely correspond with the fluvial NPP, however, previous studies have been indicated that terrestrial and aquatic NPP co-vary closely (Livingstone et al., 1982, Oberdorff et al., 1995) mainly because aquatic plant production depends on the same latitudinal factors</td>
</tr>
</table>

		as terrestrial primary productivity. Using estimates of terrestrial NPP probably does not underestimate the energy available for riverine ecosystems (Hugueny et al., 2010) Some previous researches have been applied terrestrial NPP to assess the aquatic fish richness, since freshwater NPP was not available at a global scale (Oberdorff et al., 1995, Guegan et al. 1998). The purpose of our study is not to reproduce the complex fluvial ecosystems, but to highlight regional characteristics under the same evaluation process. To this end, we regard Chikugo model as the most appropriate model available so far to estimate primary productivity. (We will add the reason for choosing Chikugo Model instead of using "well established"). *Reference:* 1. Woodward, G.: Biodiversity, ecosystem functioning and food webs in fresh waters: assembling the jigsaw puzzle, Freshwater Biology, 54, 2171-2187, 2009. 2. Livingstone, D. A., M. Rowland, and P. E. Bailey.: On the size of African riverine fish faunas, American Zoologist 22, 361–369, 1982. 3. Oberdorff, T., Guégan, J. F. and Hugueny, B.: Global scale patterns in freshwater fish species diversity. Ecography 18, 345–352, 1995. 4. Guégan, J.F., Lek, S. and Oberdorff, T.: Energy availability and habitat heterogeneity predict global riverine fish diversity. Nature (London) 391:382–384, 1998. 5. Hugueny, B., Oberdorff, T. and Tedescco, P.: Community Ecology of River Fishes: A Large-Scale, American Fisheries Society Symposium, 73, 2010.
12	Pg. 11, line 30: _ 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?	The length of the grid cell is different in latitudinal direction. It results in the difference in channel length. When the parameter α changes from 2 to 4 (see pg.11 line 30), it does not show a linear increase since the flow velocity of each cell is different. Thus, when applying the single parameter (α=3 as an average) , the calculated biomass will be slightly overestimated.
13	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.	Confluences of tributaries which are identified on the 0.5x0.5 gridded cells are considered here. In the river channel network model applied in this study does not actually distinguish confluences and lakes. Both of them are expressed as a grid to which two or more upstream cells are connected. However, we used "lakes" where we obviously identify the large lake on the 0.5x0.5 gridded model. Confluences play as a biomass pool for downstream cells because of the following reason; If the catchment area is the same, biomass accumulation rate at the confluence cell is faster than that of without confluence, into which upstream biomass comes down step by

		step and certain amount of biomass dissipated at each cell.
14	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.	We carefully examine the calculation process and figured out thet the length of the upstream reaches is the dominant factor for longer ERT. Therefore, we rephrased the sentence "This is because…" and added the new explanation. If the CDE is the same rate, ERT is longer where the length of upper reaches is longer more dependent on biomass transported from upstream (B_u). For instance, at the middle of Ebro River, ERT is 56 and B_u is 13% of total biomass, while at downstream of Parana River, where length of upper reaches is about 6-fold longer than Ebro, ERT is 180 and B_u accounts for 60%. The latter case, more than half of the biomass originate from allochothonous, however, as only a small proportion of B_u is transported downstream at each time steps across long distances, ERT becomes longer.
15	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.	To state the casual relationship correctly, we rephrased the sentence; These regions are characterized as longer ERT, resulting in lower resilience.
16	Pg. 15, line 9: provide citations supporting that Smakhtin et al, results have been "widely applied in water resource assessments".	We rephrased and added the supporting references. The model of Smakhtin et al (2004) offers a first estimation the water required for the maintenance of freshwater ecosystems at the global scale. Their estimation have been referred by several global water recourse assessments (for example, Hanasaki et al., 2008, Rockstrom etal. 2009, Gleeson T. et al, Bonsch et al., 2015). *Reference:* 6. Hanasaki, N., Kanae, S., Oki, T., Masuda, K., Motoya, K., Shirakawa, N., Shen, Y. and Tanaka, K.: An integrated model for the assessment of global water resources –Part 2: Applications and assessments, Hydrol. Earth. Syst. Sci., 12, 1027-1037, 2008. 7. Rockstrom, J., Falkenmark, M., Karlberg, L., Hoff, H., Rost, S. and Gerten, D.: Future water availability for global food production: The potential of green water for increasing resilience to global change, Water. Resour. Res., 45, 2009. 8. Gleeson, T., Wada, Y., Bierkens, M.F.P., van Beek, L.P.H.: Water balance of global aquifers revealed by groundwater footprint, Nature, 488, 197-200, 2012. 9. Bonsch, M., Popp, A., Biewald, A., Rolinski, S., Schmitz, C., Weindl, I., Stevanovic, M., Hogner, K., Heinke, J. and Ostberg, S.: Environmental flow provision: Implications for agricultural water and land-use at the global scale, Global Environ. Chang., 30, 113-132 ,2015.
17	Pg. 15, line 23: Reference is made here to "feasible goals", which refer back to	As is explained at the answer No.2, we suppose the management objective is the highest

	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.	status (or natural), in order to highlight difference of ecological structures without any human impact. On the other hands, in the global assessment (Smakhtin et al 2004), EFR is assumed as a "fair" condition, in order to demonstrate a feasible management goal. Considering the management aspect, the equation (5) of Pg.9 should be expressed as follows. $EFR=A_1\times(x_1+x_2)\times MAD$ Where A_1 is the management level. The A_1 will decide if the EFR is feasible in a management perspective.
18	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.	As you indicated, the expression of "improvement of Tennant method" is improper. It should be rephrased that we proposed a new principle of the thresholds focused on the ecological structure estimated by primary productivity which cannot be evaluated by flow regime only. In the future version of the model, the EFR should be combined with the threshold which has a management perspective, such as the method of Tennant.