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Interactive comment on "Importance of considering riparian vegetation requirements for the long-term efficiency of environmental flows" *by* Rui Rivaes et al.

Anonymous Referee #1

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General comment: This manuscript presents a research modelling the effects of environmental flow regimes implementation, with the novelty of considering ecological requirements of riparian vegetation as an alternative to traditional environmental flows generally based on the requirements of a single biological group, mostly fish. The analysis is applied to two reaches (<500 m) located in the Ocreza River that, although very close between them, their catchment areas are very different as well as its general valley typology. Authors employ vegetation and hydrodynamic modelling techniques together with valuable field data of riparian vegetation and fish communities, under three different flow regimes: natural, environmental flows only considering fish requirements and a third environmental flow regime incorporating flushing flows very important for

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Discussion paper

riparian vegetation persistence. Results show that environmental flows disregarding riparian vegetation requirements promoted vegetation encroachment, while the consideration of flushing flows were able to maintain riparian vegetation near natural standards in addition to maintain fish habitat availability similar to natural habitat (change less than 16.17 %).

This work is very stimulating and potentially of great interest as it encourages the consideration of other biological groups in environmental flows design. However, I think it should be improved in some aspects:

My main concerns are the following: 1. The methodological section should include some clarifications about the structure of modelling applications. It is not clear what authors obtained from one model and use as input for the other model. I think that a methodological scheme specifying steps in boxes would greatly improve the understanding which is crucial for research reproducibility. If it is no possible because of pages limit, some indications should be included in the text. My main question is: after obtain three different habitats configuration from the three flow regime modellings, how did authors applied the hydrodynamic modelling to calculate WUAs? If CASiMiR-vegetation model only reproduces the riparian area, not the aquatic zone (as authors said in page 6 lines 37-38), and also uses a fixed topography, why authors expect that conditions for fish change in the aquatic area? Explanations about how the models work, how are they connected and limitations of both are needed. For example in page 9 Lines 25-28, authors said “Consequently, of the latter, the microhabitat analysis demonstrated that changes in the riparian habitat induce modifications in the hydraulic characteristics of the river stretches”. If the aquatic zone is not modify by CASiMiR and fish live in aquatic zone (punctually in other zones when zones are flooded), does it matter if the hydraulic characteristics of the riparian corridor covered by woody vegetation are modified? 2. About flow regime definition (section 2.3) authors mentioned that “the considered environmental flow regimes were adapted from the environmental flow regime proposal for the future Alvito dam (Ferreira et al, 2014)”, but in the paragraph

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after authors mentioned that “Eflow was determined according to the Instream Flow Incremental Methodology (Bovee, 1982)”. Then, how was really? They are adapted or they were created for this paper? Or they were created in Ferreira et al., 2014 according to Bovee (1982) methodology and used here? It is no clear. 3. Regarding environmental flows considering riparian vegetation, I know the paper from Rivaes et al. (2015) that propose flushing flows to maintain the ecological succession equilibrium of riparian vegetation. Have authors think about the consequences for bed channel that would exist if a very small discharge is maintained along the time (in your case 0.99 m3s-1), and after two years dam release a high discharge of “clean water” without any sediment? Those floods are probably going to produce incision in the main channel. Then vegetation encroachment may be avoid, but with catastrophic consequences for the main channel in my opinion. Discussion about this type of limitations will enrich the paper and contextualize the results. 4. Regarding vegetation modelling, CASiMiR model lacks of a crucial process such as the morphological evolution of the river as it uses a fixed topography. The interaction between river morphodynamics and riparian vegetation has been widely studied, with bi-directional influences. Riparian vegetation affects channel morphology and flow dynamics affect riparian vegetation. Then, given that in the ten years of modelling some floods occur, the sentence (page 6, lines 9-11) “Such modeling period was considered to be long enough to avoid the influence of the initial vegetation conditions, while river morphological changes still do not assume importance in vegetation development (Politti et al., 2014)” is not truly appropriate, nor the sentence neither the reference. The reference is not appropriate because Politti et al., (2014) applied also CASiMiR model although in a climate change context. Then, in the case that they conclude this (I think that they don′t conclude this), I disagree because using a fixed topography for a vegetation-modelling is incompatible with that conclusion unless they compare with a variable topography, which is not possible in CASiMiR-vegetation model. The sentence is not appropriate because there is a wide list of literature that that say the contrary. For example: Corenblit, D., Baas, A.C., Bornette, G., Darrozes, J., Delmotte, S., Francis, R.A., Gurnell, A., Julien, F., Naiman,

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R.J., Steiger, J., 2011. Feedbacks between geomorphology and biota controlling Earth surface processes and landforms: A review of foundation concepts and current understandings. Earth-Science Reviews 106 (3-4), 307–331. Corenblit, D., Steiger, J., Gurnell, A., Naiman, R.J., 2009. Plants intertwine fluvial landform dynamics with ecological succession and natural selection: a niche construction perspective for riparian systems. Global Ecology and Biogeography 18 (4), 507–520. Gurnell, A., 2014. Plants as river system engineers. Earth Surface Processes and Landforms 39(1), 4-25 Gurnell, A., Bertoldi, W., Corenblit, D., 2012. Changing river channels: The roles of hydrological processes, plants and pioneer fluvial landforms in humid temperate, mixed load, gravel bed rivers. Earth-Science Reviews 111(1), 129-141.

5. About results presentation, now this section is a bit confuse and I think it will benefit from the emphasis of main results, for example, answering explicitly to the questions that authors propose at the end of introduction section.

Specific comments:

Title: As your study encompass a decade, talk about “the long-term” is not very appropriate. Paper from Frissel et al., (1986) relate the “reach system” (that could be equivalent to reaches in the paper) to a time scale of 10 to 100 years. Then, authors choose the minimum threshold, not really the long-term. Authors should avoid using that expression for a period of 10 years with riparian vegetation context.

Introduction: Introduction section provides an appropriate “stat-of-the-art” about the main topic. As authors have been able to formulate the objectives as questions, they should take the advantage and give results to clearly answer those questions.

Methods: Study site: Page 3 line 9: Authors use a very general reference to talk about the flow regime of a typical Mediterranean river. Are there discharge data of the river? Because the sentence should describe the real flow regime instead of use a general reference. As in “Flow regime definition” authors use a “natural regime” and also flushing floods, information about return periods also should appear in the

description of the study area. Figure 1. Figure 1 is not very informative. The zoom of drainage network could include catchment delimitation of each reach. Caption figure should include photos authorship.

Data collection Please, give a brief description about field procedures like for example if electrofishing was used, although all details can be seen in Boavida et al., (2011).

Riparian vegetation modelling Page 5 lines 36-39: "the hydrological regime is inputted into the model in terms of maximum annual discharges as these discharges are considered as the annual threshold for riparian morphodynamic disturbance that determine the succession or retrogression of vegetation." In this case with extreme low flows proposed in Eflows, dessication could be also dcrucial for vegetation retrogression. Not only minimum discharges (quantity) but also duration. Have the model consider that? If not, please explain explicitly. Page 6 Line 8: Authors have included many supplementary material which is very appreciate. But, please specify the supplementary material in each case along the entire manuscript, i.e. the number of table or figure because otherwise is confuse. Page 6 Lines 12-13: "The resulting riparian. . .hereafter named natural, Eflow and Eflow&Flush habitats". The word "habitats" is ambiguous because in each of this flow regime there are "habitats". . .I suggest using "scenarios" or something similar instead of "habitats". ". . .hereafter named natural, Eflow and Eflow&Flush scenarios habitats". Please, check and be congruent along the manuscript.

Table S5 in supplementary material contains some of the vegetation model parameters. The Resistance to shear stress (N m-2) that authors used differs greatly from parameters used by Politti et al., (2014). While the current paper use 30, 30, 50, 300 and 300 N m-2 for IP, PP, ES, EF and MF respectively, Politti et al., (2014) used 1, 3, 40, 25, 60 and 400 as critical values of shear stress for "Initial phase", "Pioneer phase", "Herb phase", "Pioneer shrub", "Shrub phase" and "Early successional woodland" . The classes and not totally equivalent, but why are they so much different?

Hydrodynamic modeling Page 6 Lines 29-31. "The hydraulic characteristics of each

habitat (roughness, flow depth and velocity) were compared using a t-test (confidence level of 99%) in R environment (R Development Core Team, 2011) in order to determine the existence of mean significant differences between habitats." I understand that authors are looking for significant differences between scenarios (natural, Eflow and Eflow&Flush). Then, did authors mix hydraulic values (water depth, water velocity and roughness) from different zones (IP, PP...etc.) for all riparian area in each scenario? For what purpose? Are fish going to use EF or MP zone? Why not looking for differences between scenarios considering the different zones (IP, PP, etc.)? Knowing those differences would be more interesting that the main value for all riparian area.

Results: In general, this section could be better structured with some sub-sections. Also, main results should be explicitly written, for example as brief conclusions for each part. I mean, along the text there are many comparisons in %, but a sentence summarizing what they mean in general terms when it is possible (the consequences of applying one flow regime or other) would greatly improve the understanding for readers, it is a suggestion. Page 7 Line 19: Here authors use "habitat" in a different context. That is why I recommend using "scenario". Page 7 Line 36: "The changes undertaken by the riparian vegetation facing different flow regimes are able to modify the hydraulic characteristics of the river stretches (Figure 4)". Do these values (Figure 4) refer to the entire riparian area in each scenario? As in my previous comment about "hydrodynamic modelling", I suggest that results be presente for each zone because not all zones affect fish habitat. Page 8 Lines 3-6: Comments about ks, it is not clear which comment refers to figure 4 and which comment refers to supplementary material. Figure 4 and Table S8, S9, S10 in supplementary material contain the same information? I mean, figure 4 graphically and tables with tests? It is not clear what authors try to differentiate with tests. Please, include some clarifications in the text. Figure 5: It is not indicated to which reach refer each set of graphics. I suppose that upper graphics are from OCBA and the lowers from OCPR. Please, indicate it. Also regard to this figures, the colors and line types used are not truly appropriate. Authors should use line types that are distinguishable when they overlap. And the thickest line should be finer. Page

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8 Line 34: “The Eflow habitat consistently provides less habitat suitability during autumn and winter months for the barbel and nase, c. 50% and 38%, respectively, while the habitat suitability increases in approximately 46% of calandino.” May be is because graphics are not adequately labelled, but the “less suitability for the barbell during autumn and winter” is not really lower. In the graphic it seems the same. Please, check that text describes correctly the graphics.

Discussion: As I mentioned before, the modelling techniques that authors used have some limitations that should be comment in the discussion section. Page 9 Lines 27: Authors use “habitats” and in line 28 use “scenarios”. Please, authors should homogenize the terms used. Page 10 line 19: “Accordingly, the implementation of such measure can provide significant positive ecological effects in downstream reaches (Lorenz et al., 2013; Pusey and Arthington, 2003) and results in additional ecosystem services (Berges, 2009; Blackwell and Maltby, 2006) while imposing minor revenue losses to dam managers (Rivaes et al., 2015).” Significant positive ecological effect. . . compared to what? To the natural regime??? To the Eflow regime? What type of ecosystem services? Could you specify? Please, rewrite this paragraph because it is not clear.

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