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Hydrology and Earth System Sciences Discussions

Interactive comment on "Hydrologic and land-use change influence landscape diversity in the Ebro River (NE Spain)" *by* A. Cabezas et al.

A. Cabezas et al.

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AUTHOR COMMENT TO ANONYMOUS REFEREE 1

The paper report about the morphological and ecological changes of a reach of the Ebro River over the last eighty years. The paper is well organized and reasonably well written. The methodology is somehow not entirely supported by enough explanations and references, but the results are certainly very relevant. For its argument and contents, I think that the paper is worth to be published by HESSD.

In general, I’ve got some doubts about the use of the Shannon index and the ecotope succession scheme to estimate the ecological value of a channel reach. Consider the case of avulsion. If the mail channel is suddenly flowing in the middle of the

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Interactive Discussion

Discussion Paper

forested floodplain due to an avulsion, the ecotope diversity indexes would be very low for hundred meters around the main channel (i.e., on figure 4). However, this would be a very natural phenomena. Another extreme example could be the case of a very high magnitude flood. After the flood there would be a sudden disappearing of the islands, mature forested floodplains and pioneering-intermediate stage ecotones around the thalweg for hundred of meters. This would reduce the ecotype diversity for a wide distance around the thalweg. It would be advisable to have a comment of the authors on the discussion chapter about the possible shortcomings of the applied methodology.

Author Response (AR).1. As you have indicated, we will include those commentaries in the revised manuscript. What you say it is completely true, but this is exactly what we wanted to reflect through the diversity spectra. The channel avulsion between 1927 and 1946 caused exactly the effect you describe, and this can be observed through the spectra, since this is the cumulative spectra going from the main channel to the outer floodplain (diversity increased in 1927 and 1946 peaking at 300 m from the main channel). When we talked about diversity per year we compare values considering the whole area. For that calculation, the effect of a meander avulsion increased the total diversity, since it created areas of young ecotypes at certain floodplain areas.

Another critical point could be raised about the chapter 4.3. In fact, the whole chapter is rather general and quite disconnected from the rest of the paper, and don’t really take advantage of the results. It is not clear if the speculation is about the whole middle Ebro floodplains or just the study reach. In the first case, the Authors should clearly point out that the speculation is rather conceptual, and in the second case, there should be a more locally-based approach on the proposed river restoration strategies. Given the consistent knowledge about the evolution of the study reach over the years, the Authors could reason around some particular dyke or levee removal. Also, there should be at least a mention about the issues related to the sediments availability and management.

AR.2. You are right, we will clarify this part of the manuscript in the revised manuscript.

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The conceptual guidelines are suggested for the study reach. However, to suggest more specific restoration activities we strongly believe that some complementary data and experiments should be performed prior to develop such important actions. We will also reflect that in the revised manuscript. Finally, issues related with sediment availability and management will be included, as we consider them extremely important, although we could include it in our analyses. Minor comments

Pg 2762, row 15: Is the slope the mean channel slope? If not, please add this value here. Pg 2762, row 17: Could you also report a mean channel width and a general description of the channel morphology here? Has the reach a island braided or a wandering morphology? How long is the study reach?

AR.3. These data will be included in the study area section at the revised manuscript.

Pg 2762, row 17: Has the mean discharge been calculated from the flow measurements in the period 1927-2003? In this case there would be a difference between the mean flow discharge calculated for the periods before and after the construction of the dams. Is it the case? If not (as I suspect, since the major influence of dams in on low to moderate flows and not on the maximum annual peaks), it would be interesting to report it.

AR.4 These data will be included in the revised manuscript.

Pg 2762, row 18: How did you estimate the area that would be inundated by a 10 years recurrence interval flood? It would be desirable to report at least a reference for the readers.

AR.5. This is presented in the manuscript, 2764, 25, and one reference is supplied (Losada et al, 2004).

Pg 2762, row 20. what is 0.37? Did you mean 10.37? Pg 2763, row 6. What do you exactly mean with rate of change? Do you mean shape of the floods?

AR.5. Yes, it will be changed in the revised manuscript.

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Pg 2763, row 8. Is the gauging station measuring just the flow level? If it is the case, you should briefly state how was derived the stage-discharge curve, the maximum discharge measured in the field and a range of confidence for the relationship.

AR.6. The gauging station is measuring level , I will try to get this data from the CHE and will be presented it in the revised manuscript.

Pg 2763, row 13. Did you start from a daily series of flow stages measured at a certain hour every day or from an average daily value? In this last case it would means that you have a hourly data series. Is it the case?

AR.7. Yes

Pg 2764, row 15. It would be advisable to add some details about the field surveys. Could you briefly provide some details on the factors (i.e. season, water level) that can potentially limit the applicability of your methodology, and a range of confidence in the identification of the ecotopes from the aerial photos

AR.8. It will achieved in the revised manuscript. This has been indicated by another referee, I let you know my answer:

In recent aerial pictures (2003 and 1998), each digitized polygon was delimitated at the field over printed aerial pictures (prior and after the 2003 flooding, the reason why we considered the 2003 aerial picture interesting). Since we have done it “a priori” and not “a posteriori” we consider that any statistical validation as the Kappa index has no sense, the study area is not that big. We have to explain this more clearly in the revised paper.

For the historical pictures, this kind of validation is not possible, we have not vegetation maps. An interpretation key was used (2763, 15) as it has been done in another cases for the interpretation of historical pictures. Those references will be included in the revised version, although

Geerling, G. W., A. M. J. Ragas, R. Leuven, J. H. van den Berg, M. Breedveld, D.

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Liefhebber, and A. J. M. Smits (2006), Succession and rejuvenation in floodplains along the river Allier (France), Hydrobiologia. 565, 71-86.

Pg 2764, row 22. At this point, it seems that the whole analysis is based on the assumption that the thalweg of the main channel does not change over time.

AR.9. The calculation was made for the thalweg at each year, so the main channel migration was considered.

Pg 2765, row 5. Could you briefly describe the Shannon index and how you calculated it?

AR10. This will be explained in the revised version in order to clarify how we calculated the index.

Pg 2765, row 2. The magnitude of flood itself is not changed in time. What have changed is the frequency of floods of certain magnitude. Pg 2765, row 12. It is not entirely clear how the natural transition differs from the anthropic transition. Can you provide an example of human-affected transition? How did you distinguish from the natural ones?.

AR11. As explained in the text, human affected transitions are those ones towards “anthropic” ecotope, so it has to be excluded for interpretations of the disruption of river-floodplain interactions and ecotope succession.

Pg 2766, row 26. The distance of the ecotope diversity peak from the thalweg might be strongly influenced by the magnitude of the last flood before the aerial photo were taken. Would you comment about this possibility?

AR12. This will be commented in the revised manuscript.

Table 1. Is the young island in fact a mid-channel bar?

AR13. Yes it is.

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Figure 2. I think that all the rejuvenation-succession arrows should least have two heads. Even in this case, some rejuvenation passage can be very quick and not requiring intermediate step. For example, a mature island being totally eroded by a high-magnitude flood would suddenly become main channel, open water or gravel. Is it contemplated in you analysis?

AR13. Yes, this is right and this has been contemplated in our analyses. The succession scheme is made with the assumption of evolution without erosive floods, when ecotopes at any stage of succession can be rejuvenated at ant initial stage.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 2759, 2008.

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