

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

Anonymous Referee #2

Received and published: 12 November 2008

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 main channel is suddenly flowing in the middle of the 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.

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.

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?

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.

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.

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?

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.

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?

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

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.

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

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?

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

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?

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

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?

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

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper