

Interactive comment on “Drainage reorganization and divide migration induced by the excavation of the Ebro basin (NE Spain)” by Arnaud Vacherat et al.

Anonymous Referee #1

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The authors make use of a standard topographic DEM to find knickpoints and identify potential drainage divide migrations in the Ebro-Duero catchment boundary. The subject is of interest for the understanding of divide migration and the ms. would spread a geomorphological region that is not so well documented.

The title is appropriate and the abstract responds to the contents, though the abstract could be more direct and comprehensive.

The authors explore a number of captures, most prominently the Homino River one. This one is well known to geomorphologists, and documented not only by Mikes 2010, but also for example in the report accompanying the corresponding chart of the Ge-

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ological Map of Spain, published by the IGME. I also recall public panels on display for the random visitor in the Hontomin village, explaining the fluvial captures. I'm sure further bibliographical research will bring even more appropriate references.

I counted 5 mentions of the Duero basins as being "almost still endorheic". The expressions used are inappropriate and misleading. The entire Duero Basin is exorheic today. Perhaps the authors mean that the top of the sedimentary infill is relatively well preserved, and incision is small/recent relative to the Ebro. This needs clarification because it seems to be central point of the article.

647-650: The logics behind this reasoning are obscure. Again, what is a "quasi-endorheic configuration"? And how does the Ebro piracy effect on it? In terms of drainage area, the Duero is still larger than the Ebro basin, so area is not the problem.

Some formal aspects need attention: two entire paragraphs of the abstract are copied as such in the main text, f.e., the last par. in the Introduction. Instead of the latter, I would expect an explanation of what is to come later in the paper, what question is addressed and what strategy they follow. Also the first half of the conclusions are not conclusions. Lines 415-454 are really redundant because that derivation is shown in many earlier papers, and is not relevant to the paper. I would highlight Perron & Royden, ESPL, 2013, as the original authors. Point "3.2.2" should in my view be the "Results", which seem poor relative to the Discussion. Overall, the ms. at its present stage focuses more on interpretation/speculation than on its new objective results. I don't report here any further on formal issues.

Line 46: A sentence as strong as that needs mentioning Gilbert, 1887; Brocklehurst & Whilpple, 2002; Perron & Royden, 2013; for example. Line 120: Specify the ages proposed for the drainage change by the various authors, otherwise the ongoing subject for discussion is not clear. Line 123: River capture AND sediment colmatation of the basin are the two competing processes proposed in the cited references. 132-135: the use of the word 'opposed' here, and the whole sentence, are unclear. Line 222:

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the “dynamically stable” sentence is not understandable to me.

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