

## ***Interactive comment on “Impact of climate evolution and land use changes on water yield in the Ebro basin” by J. I. López-Moreno et al.***

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On behalf of all authors of this work, we want to sincerely thank the effort done by the reviewers as they provided helpful and constructive comments. Their suggestions have helped us to improve our revised manuscript. Following we respond to the different concerns and suggestions of the reviewers, explaining all changes introduced to the revised version of the manuscript. We want to sincerely thank the effort done by the reviewers as they provided helpful and constructive comments. Their suggestions have helped us to improve our revised manuscript.

Referee 1.

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- P2655 L13 "large scale atmospheric patterns": maybe add which ones: We have added the North Atlantic Oscillation, Mediterranean Oscillation and Western Mediterranean Oscillation. We have also added the reference to the paper Vicente-Serrano et al., 2009, where the influence of such patterns is highlighted.

-P2655 L24 ff: A recent paper by López and Justribó (2010) highlighted the upstream downstream relations in the Ebro basin with detailed figures: At the time of the preparation of the manuscript we did not know about this interesting publication. It has been cited in the study area and the introduction.

- P2657 L2 ff: How are the temperature and precipitation stations distributed over altitude? With view to the pervasive altitude bias in gauging networks, this would be an interesting point: We have added two phrases indicating that distribution of meteorological observations does not cover adequately high elevation sites. It hinders the possibility to conclude whether it exists dependence between climate evolution and elevation

- P2657 L5: "In terms of climate data there is a reasonable spatial coverage with respect to hydrological observations across the entire Ebro basin": Sentence not quite clear – climate or hydrological?: He have deleted “with respect to hydrological observations” as it resulted confusing

- P2657 L27: One of the methods for making trend analyses was interpolating the data to a 1 km<sup>2</sup> grid. Given that Daly’s PRISM interpolation scheme was used, this is defensible. The authors should, however, make a more explicit note that this procedure enables them to make analyses for individual catchments, as opposed to a more conventional analysis of station values.: This comment has been added in the suggested paragraph

- P2659 L16: "A1B" (like it says on P2663 L18) instead of "A1B1"?: It is A1B1, the error has been corrected.

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- P2660 L4 ff: This paragraph could be a little better organized: The paragraph has been changed as follows: Most of sub-basins exhibit negative MK tau coefficients, and any place has shown an upward trend. However, only a few small areas exhibited a statistically significant decrease ( $\alpha < 0.05$ ).

- P2660 L21: Is there a significant altitude trend in warming? (If the data and methods are sufficient for making a statement, that is.). As we mentioned above, the lack of stations at high elevation sites does not allow concluding about a dependency between warming and elevation

- P2661 L13-15: How does the observed behavior in rainfall-snowfall ratio compare to the observed behavior of temperature? Such comparison is a very difficult issue at the basin (or sub-basins) scale. In that phrase we argue that the generalized absence of trend in runoff during winter, whereas precipitation shows negative coefficients, could be related with the positive evolution of temperature which logically will cause a minor retention of water in solid phase. However, the establishment of direct relationships between runoff, precipitation, temperature and snow/rain ratio needs of specific information and analyses which we think exceed the scope of the article.

- P2662 L2 "exhibited the greatest explained variance, being this punctually reduced in gauging": What does "this" refer to? The sentence is hard to grasp: "this" refers to coefficient of determination (explained variance). We have replaced "this" by "coefficient of determination".

- P2662 L11 / L12: Give r-squared always either as fraction or as percentage: Now it is only as a percentage.

- P2668 L6 "land cover monitoring networks": Can the authors be more specific in terms of what is required? For example, is remote sensing sufficient?: In the revised manuscripts we indicate the usefulness of temporal series of remote sensing images and the periodic edition of vegetation and land cover maps.

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- Figures 4, 5 and 6: Is it possible to include a measure of significance? (There only seems to be the strength and direction of the trend indicated.). The symbols inform about the statistical significance of the trends, as the classification between strong, moderate and no trend is based on the alfa parameter (see last paragraph of data and methods).

Referee 2. - The runoff calculations based on ratio of precipitation and discharge are fine under the assumption of precipitation - runoff response occurring at the same period (month here). In large watersheds like this, lag effects (time of concentration) can be substantial. Sometimes the lags can be several days, weeks, and months. While this paper studies interaction within a month it will be useful to specify this assumption.: We completely agree with the necessity to take into account the lag in the hydrological response to climatic conditions. This question is indeed one of the main research interest of our hydrological investigation in Spain (considered in other publications, e.g. Vicente-Serrano and López-Moreno, 2005, Hydrology and Earth System Sciences 9: 523-533). However, we think that this lag has a minimal influence on the results presented in this manuscript. We use monthly data but they are then aggregated into seasonal and annual series. Correlation between climate and runoff series is conducted at annual basis and using hydrological instead of natural years. Thus the effect of time of concentration should not affect to the presented results.

- The land use changes are assessed indirectly through a regression with climatic factors. This assumes that land use and climate are major factors. While such elimination method can provide general trends, it will be much useful to quantify land cover in the regression. Other factors like groundwater discharges, soils, landforms, and diversions are not explicitly modeled. This is a weakness given that land use change is highlighted in the title. We agree that it would be much better to have quantified the magnitude of land use change to be introduced as predictor variable in the regressions. However, there is not information on land use change for the whole of the basin (as occurs for the majority of the Mediterranean area). It is necessary to take into account that it

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would be needed annual maps of vegetation to be added as predictor. The use of remote sensing series is promising for this purpose, although it will be also difficult to implement this information in studies of this nature because of: i) satellite images are only available after the late 70's, which precludes the possibility of analyzing long-term hydrological series; ii) the classification criteria will noticeably affect the results, and by hence derived conclusions will be subjected to uncertainty. For these reasons, we preferred to infer the signal of land use change from the unexplained variance of runoff from climatic data. We are aware of the associated uncertainties (and we try to highlight them in the revised manuscript), but the existence of trends in the residual in sub-basins that practically are unaffected by human activities are a clear signal of the influence of land use changes. We also provide a large number of references (many of them from experimental catchments in the Pyrenees and other Mediterranean areas) to support the conclusions.

- The analysis is done with climatic and discharge variables at different spatial locations. There exist cumulative and interactive effects that are difficult to assess using the approach of studying them individually. For example, upstream runoff accumulated over geographic space within a sub-basin and having effect on downstream flows. Land use and climate change variables also interact to create combined effects on flow regimes. I recommend discussing these issues in the introduction. We have mentioned this fact in the discussion section. Thus, the main obtained results are preceded by this added text “Presented results for individual sub-basins must be interpreted with caution since cumulative and interactive effects may difficult the understanding of the hydrological changes in a particular area. For example, upstream runoff accumulated over geographic space within a sub-basin will affect on downstream flows. Land use and climate change variables also interact to create combined effects on flow regimes. However the high number of analyzed sub-basins, many of them located in headwater sectors permits to obtain conclusions that can be generalized for the whole basin. Thus, the occurrence of. . .”

-Page 2657, line 18 – How are the distances calculated?: Shortest linear distance, this is now stated in the text. - Page 2657, Line 21 – Why are the residuals interpolated?: This is a common and widely accepted procedure when mapping punctual climatic data over space (see the cited references). The procedure enables to use the observations for removing (partially) local climatic effects that have not been considered by the predictor variables. -Page 2658, Line 3 – Is the ratio assuming no carry-over to next month?: The ratio does not assume carry over to the next season (we are always working at seasonal and annual basis). This is the reason by which we argue the low significance of winter trends. - Page 2658, Line 20 – Need to add  $\tau = 0$ ? No, it is not necessary because it is non significant independently if  $\tau$  is positive or negative. -Page 2658, Line 25 – The elimination method used will work if major factors contribute to high variation in flows. There are other factors that can be influential. It will be useful to add the assumption behind this method. It is true that air humidity, wind speed or solar radiation affects (together temperature) the evaporation rates. We have considered to include this observation, but we have finally decided that it would introduce more complexity to the text only to highlight a well-known fact. - Page 2659, Line 17 – Please add some discussion on why this GCM is chosen compared to others: The selection of the HIRHAM model is discussed with the following text:” Selection of this model was based on its ability to reproduce temperature and precipitation over the Pyrenees and the center of the Ebro valley. Moreover, it was compared with a set of regional climate model projections from the PRUDENCE dataset, as this model is representative of the mean sign and magnitude of the multimodel ensemble average (López-Moreno et al., 2008a)”. - Page 2659, Line 21 – The time span should be written 2015-2080 (typo). Changed - Page 2660, Line 12 – Here are elsewhere “stationary situation” is discussed. Please add some discussion on this based on reasons for such stationarity. We prefer avoid such discussion in benefit to keep within the main scope of the article instead of developing a discussion of the drivers of climatic variability in the basin. - Page 2662, Line 12 – Temperature only affecting on average 4% of variation (based on  $r^2$ ), and in majority it is not significant. Please relate this to climate change

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impacts with a focus on uncertainty in impacts: In the discussion we have added this paragraph: “According to the results, temperature has not played a major role in the hydrological evolution of the basin. However, the strong warming conditions projected for the region can cause an increasing influence of temperature in a next future, as it has been already detected in other Mediterranean basins (Lespinas et al., 2010)”. - Page 2662, Line 17 – The decline in runoff coefficient can be from reduction in precipitation and increase in discharge (as per the ratio defined). Given precipitation is stationary, please add some more discussion here. In page 14, lines 15-21, this question is considered with the following discussion: “A generalized decrease in river flows during a period when precipitation levels have remained largely stationary can only be explained by a decrease in the water yield in the basin. Thus, the runoff coefficient reflects a spatial pattern that is even more marked than that observed for discharge (Fig. 6), involving a generalized decrease in water yield across the Ebro River basin (55 strong trends and 7 moderate trends). The seasonal patterns were practically identical to those found for trends in river discharges”.

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