

Interactive comment on “Climate and topographic controls on pasture production in a semiarid Mediterranean watershed with scattered tree cover” by J. Lozano-Parra et al.

Anonymous Referee #2

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In general, the manuscript is well-written, and addresses relevant scientific questions with the scope of HESS. It presents a physics-based, spatially-distributed ecohydrologic model, and gives some interesting results or conclusion. While, there are some parts need to be made clear or rewritten. I suggest that the manuscript can be accepted after major revision.

(1) The title is not suitable for the manuscript. The ecohydrologic model is the core of the manuscript, and the 300 yr long climate dataset is one of the highlights for the paper. But, model and 300 yr climate dataset can not be seen in the title.

(2) Lines 14-15 page 15172, the author said: Annual potential evapotranspiration is

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twice the annual rainfall amount. But, Lines 15-18 page 15184, the author said: Annual mean value of evapotranspiration for the whole catchment was 390mm while annual mean precipitation was 508 mm. It is incongruous obviously. Are the model results wrong?

(3) Lines 15-18 page 15174, the overall soil moisture of each site was considered to be the depth-averaged soil moisture of the sensors. However, the soil water content (SWC) at 5 cm depth can change very fast, and SWC at 30 cm depth may not. I am not sure it is suitable to average the soil moistures at different depth.

(4) Lines 15-18 page 15174, the overall soil temperature of each site was considered to be the depth-averaged soil temperature of the sensors either. But, in lines 5-6 page 15174, the soil temperature was measured at 5 cm depth only. How to get the depth-averaged soil temperature?

(5) Lines 20-21 page 15174, the natural pasture production were measured from Sept 2008 to August 2011. While, in lines 1-3 page 15175, the plant height were measured from 1 March 201 to 31 August 2012. It is confused that why the measurements have not been taken during the same periods.

(6) Line 21 page 15177, what's the resolution of the digital elevation model (DEM)? It is suitable for the model? It should be made clear.

(7) Lines 4-6 page 15178, the author said “Maps of soil properties such as soil depth, porosity, and other hydrologic properties (Fig. 2) were derived from the geomorphologic characteristics of the basin as described in (Maneta et al., 2008)”. Since the data of Fig.2 from the reference of Maneta et al(2008), it should be clarified in Fig.2.

(8) Lines 7-8 page 15178, the author said, “Tree density and tree canopy cover maps were obtained from aerial photograph interpretation and through image classification methods (Fig. 2) (Maneta, 2006)”. Were the maps from the reference of Maneta(2006), or the methods from the reference? If the maps were from the reference (published in 2006), the maps data may be out of date for the case study.

(9) In the section of “3.4 Generation of atmospheric forcing”, the generation of a 300 yr-long climate dataset was chosen and used. But the reviewer is confused that the

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13 yr of data from the meteorological station (2000–2012) are enough to generate the 300 yr-long data. Furthermore, during the 300 yr-long periods, what will happen to pasture growth, and how to consider about the dynamic change of pasture growth?

(10)Line 2 pages 15179, 51 yr data from a station located at 24 km from the study site were used. Do we need to consider about the spatial variation of climate data? When using climate data from other place, are there some model uncertainty because of this?

(11)In the section of “3.5 Model calibration”, 4 years data were used to calibrate the model, and then predict 300 yr-long change. The reviewer is not sure about this.

(12)Line 15 page 15190, the water consumption by trees was referred to. While, nutrient consumption of trees is important also. It should be discussed in the section of 4.2.4.

(13)Fig.5 page 15210, it can be seen from the Fig.5 that the observed data were limited or not enough maybe.

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