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Dear Prof. Ursino,

We are pleased to submit a revised version of manuscript hess-213-544 by Lozano-Parra et al. The reviewers were very positive about the work. Most of their suggestions are of an editorial nature, and the major points required some clarifications that have been incorporated in the revised text.

We addressed all the reviewers comments in our reply to each individual reviewer and in that reply we also indicate the actions we have taken to correct the manuscript. We found the comments useful and we have incorporated most of the suggested changes. The only suggestion we have not incorporated is the one asking us to include the length of the simulation in the title of the paper, which is already long. The length of the simulation is basically akin the number of Monte Carlo runs we conducted to obtain the statistics described in the results. We believe that this is a methodological detail that does not belong in the title. We hope that the editor will support this decision

To facilitate the review we have tracked the changes made to the original text. We also summarize in the table below the major comments from the reviewers and the lines of the manuscript where we have made changes to address them.

We hope you find satisfactory the revisions and are looking forward to hearing from you.

Regards,

Javier

Summary of changes to non-editorial comments suggested by the reviewers

Referee	Comment	Changes	Lines
Referee #1 *Referee #2*	I believe that the title is misleading as it does not indicate that the study is a modelling exercise. This point needs to be made clear in the title so readers know that the results are the product of simulation. I'd suggest that the title include the word 'modelling' and perhaps the '300 year series. 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 been seen in the title.	The title has been changed to indicate that this is a simulation study. We do not think that including a mention to the length of the simulation is relevant as explained in the reply and in the letter to the editor	1
Referee #2	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?	We clarify that we do not average temperature over the soil profile.	182-183
Referee #1	The modelling results depend in part on the re-distribution of water both overland (steeper slopes) and subsurface, allowing higher some units to accumulate more water, and thus have greater productivity. However, it was not clear to me how such re-distribution occurred in the modelling. Therefore, it was not clear whether such modelling results should be given much credence. In general, my previous point relates to the need in a modelling exercise to be clear about which results are considered realistic (likely) as opposed to those that might be an artefact of the model design and structure. Running the model over a longer sequence of years will not remove defects or artefacts of the model, but it is the modellers who are most likely to be aware of the limitations of the model. An objective assessment of the model would be a good and useful supplement to the paper.	We have added a few lines describing the surface and subsurface processes included in the model that are relevant for the interpretation of the results. Readers interested in the performance of the hydrologic engine that supports the grass growth component are referred to Maneta and Silverman (2013).	209-218

Referee #2	Where did the tree density come from?	An explanation of the methods used to obtain tree density is incorporated in the revision.	268-274
Referee #1	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 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?	We indicate that we assume the longest record available (13 years) is representative of the current climatic conditions. We also extend the explanation of how the stochastic weather generator works.	283 287-291
Referee #2	Lines 14-15 page 15172, the author said: Annual potential evapotranspiration is 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?	We clarify that the second number is annual mean actual evapotranspiration. The first number refers to potential evapotranspiration, as already indicated.	444

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.**

We want to thank Dr Scott for his comments on the manuscript and for his editorial suggestions. In this reply we hope to clarify the reviewer's questions. We also indicate where we will make changes in the revised manuscript based on Dr Scott's suggestions.

D. Scott (Referee#1)

david.scott@ubc.ca
Received and published: 2 January 2014
Hess-2013-544 - Referee Comment

I found this to be a thorough and complete study that is well-written & carefully referenced. The paper is easy to read and follow. The modelling exercise appears to be described in sufficient detail to give readers a clear idea of the model structure and function, while it strikes a good compromise between adequate and excessive information.

There are some small errors relating to correct use of English, but these can be readily corrected and do not involve major work. I'm attaching a scanned copy of the manuscript that I annotated to point out these errors and suggested corrections.

I believe that the title is misleading as it does not indicate that the study is a modelling exercise. This point needs to be made clear in the title so readers know that the results are the product of simulation. I'd suggest that the title include the word 'modelling' and perhaps the '300 year series'.

Reply:
We will made explicit in the title that this is a modelling study. The title will be revised to be along the lines of: 'Climate and topographic controls on *simulated* pasture production in a semiarid Mediterranean watershed with scattered tree cover'. The title may be further edited before the submission of the final paper. We think that there is no need to specify in the title the length of the simulation since it is just methodological details.

I have a number of queries regarding the study:

1. A) In the simulation of the weather data, was the necessary co-variance between variables, that were being simulated separately, considered? (p. 15178, ln 26 & onwards). For example, one may expect that dry conditions would also coincide with larger hours of radiation and higher temperatures.
Reply:
Yes, the stochastic weather generator that we have used in our study conditions the generation of minimum and maximum daily temperature and of radiation to the precipitation time series. The documentation of LARS-WG describes that the daily

precipitation time series is generated from a semi-empirical distribution adjusted to observed precipitation data for each month. The precipitation status of each day is used to condition the temperature and radiation time series. Daily minimum and maximum temperatures are simulated from normal distributions calculated for each day with means and standard deviations conditioned to the dry or wet status of the day. The seasonality of the means and the standard deviations of the temperature record are approximated using different truncated Fourier series for wet and dry days. Cross-correlation between minimum and maximum temperature is preset at 0.6. Similarly, daily solar radiation is simulated from separate semi-empirical distributions adjusted from the available data for dry and wet days. We will clarify this in a section 3.4.

B) Were the simulations of weather such that these weather variables varied in association with each other?

The weather variables not simulated directly by LARS-WG are also conditioned to the variability of precipitation and temperature as explained in section 3.4. Daily longwave radiation was tied to the daily average temperature time series and precipitation using a deterministic relationship (Swinbank, 1964) and therefore dependent on temperature and precipitation. Similarly, relative humidity was calculated from a multiple linear regression using air temperature and precipitation as predictors. Daily wind was found to be uncorrelated to any of the other weather variables and therefore was simulated independently by cycling a measured series of 51 years.

2. A) Where did the tree density come from?

Reply:
Tree density was obtained by manually digitizing each individual tree with a point in a high-resolution aerial photography, then calculating the density of points using a 3x3 moving average kernel. The fraction of the area covered by canopy was calculated using a maximum likelihood supervised classification technique from the red, green and blue components of a 24-bit color submetric resolution aerial photography. The classification success was very high because green canopies where highly contrasting with the dry grass, yellow background. Once a canopy mask was produced, the canopy coverage was obtained by calculating the fraction of pixel classified in each of the larger pixels used in the simulation. A note clarifying this will be included in the methods section of the revised manuscript.

B) Did the tree density change at all over time (through the 300-year period of simulation)? If tree cover was static, and unable to respond to variations in climate, then I think this should be made explicit, as it seems rather unnatural (although not unacceptable in a modelling exercise).

Reply:
We assume that the number of trees is invariant but it is important to emphasize that the 300 year simulation is not meant to be a simulation of 300 years of climate. Rather, the study should be interpreted as a Monte Carlo simulation of the possible range of weather conditions that the site (in its current conditions) is likely to experience in a 300 year return period. Furthermore, in this type of land use (dehesa) the tree cover is

influenced by man, i.e. reducing the number of trees transforming the original oak forest to grasslands with a disperse tree cover.

3. The modelling results depend in part on the re-distribution of water both overland (steeper slopes) and subsurface, allowing higher some units to accumulate more water, and thus have greater productivity. However, it was not clear to me how such re-distribution occurred in the modelling. Therefore, it was not clear whether such modelling results should be given much credence. In general, my previous point relates to the need in a modelling exercise to be clear about which results are considered realistic (likely) as opposed to those that might be an artefact of the model design and structure. Running the model over a longer sequence of years will not remove defects or artefacts of the model, but it is the modellers who are most likely to be aware of the limitations of the model. An objective assessment of the model would be a good and useful supplement to the paper.

Reply:
The model takes into account the vertical and lateral redistribution of water and takes into account the effect of topography. Water in the subsurface moves downslope driven by gravity but disregarding pressure effects (kinematic approximation). Water can infiltrate into the soil or become runoff. Runoff can reach the channel or re-infiltrate downslope. Infiltration and lateral subsurface flows are controlled by soil hydraulic properties (hydraulic conductivity, absorptivity, porosity) and by the topographic gradient. When the soil is full, return flow happens. All these processes are explicitly described in the model and lend realism to the results. Still, as in any modelling exercise, some ottoms are made. For instance, the overland flow component assumes that depression storage is negligible and that overland flow is routed through the entire watershed within one day. Other important assumptions are that the bottom boundary of the soil (bedrock) is impervious. These assumptions are appropriate for the study site but will be clearly made explicit in the revised manuscript.

Specific minor points.

- Abstract, ln 9: physical not “physic-based”
- p. 15169, ln 5: derives (or some synonym) and not “incents”
- p. 15170, ln 14: I believe it is wrong to call a modelling exercise an experiment. Shorten the sentence to read “few studies of simulations over the entire range . . .”
- p. 15172. There is awkward language in several places in the descriptions (see attached annotated manuscript).
- p. 15173, ln 10: “crops out” is not correct English
- p. 15174, ln 25: weighed not “weighted” (weighting is to assign a weight or importance to a factor) I did not check the detail of the model description on pages 15175 – 15177.
- p. 15180, ln 1: mean annual precipitation, not “annual mean ppt”. ln 15: represents (?) rather than “present”
- p. 15182, ln 10: change “along the whole year” to throughout the year
- p.15187, ln 19: change “competence” to competition
- In many places the word “production” is used where productivity might be more correct.
- However, on p. 15189, ln 7, the correct word can only be productivity or the sentence is incorrect.

- p. 15190, ln 24: I suggest substituting topographic controls for the longer and awkward, “topographic structure of the landscape”
- p. 15191, ln 8,9: I suggest you end the sentence with the word “ . . nutrients.” The remainder of your sentence introduces speculation that is not a valid conclusion from your paper.
- p. 15192, ln 18: insert the Chow reference (from next page where it is out of sequence)
- Consider omitting Figures 10 c & d as I don’t think they add any value.

Reply:

These figures provide information on the range of productivity in different regions of the basin and this is discussed in the text. Although we would prefer to keep these figures it would be possible to omit them and shorten the text if the editor recommends to reduce the length of the next revision of the manuscript.

- The figure captions, generally, could use some work to clarify what exactly is being illustrated.

We thank again the reviewer for the editorial corrections on the text. These will be incorporated in the text in the revised version of the manuscript.

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.

We want to thank the anonymous referee for his comments that undoubtedly will help to improve the manuscript. In this reply we hope to clarify the reviewer's questions. We also indicate where we will make changes in the revised manuscript based on the anonymous referee's suggestions.

Anonymous (referee #2)

Received and published: 4 February 2014
HESSD, 10, C7692–C7694, 2014

In general, the manuscript is well-written, and addresses relevant scientific questions with the scoped 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 been seen in the title.

   Reply:
   As was discussed in the previous reviews, we will make explicit in the title that this is a modelling study, and it will be revised to be along the lines of: 'Climate and topographic controls on *simulated* pasture production in a semiarid Mediterranean watershed with scattered tree cover'. The title may be further edited before the submission of the final paper. We think that there is no need to specify in the title the length of the simulation since it is just a methodological detail.

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

   Reply:
   In lines 14-15 page 15172, we refer to annual potential evapotranspiration, while in lines 15-18 page 15184, we talk about annual actual evapotranspiration. The annual water balance is correct and typical of the region. Mean annual evapotranspiration representing more than 75% of water outputs of the catchment. The remaining amount between of 120 mm becomes runoff. The range of measured runoff values in the catchment oscillate between 10 and 190 mm depending on annual precipitation.

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.

   Reply:
   It is appropriate to average soil moisture measurements from sensors located at different depths. If the sensors are evenly spaced the arithmetic mean approximates the average water content of the profile; when the sensors are not evenly spaced a weighted average is used. An aggregate estimate of the soil moisture in the soil profile was necessary for comparison purposes since the model simulates the depth-averaged soil moisture content of the soil profile, not the soil moisture at specific depths.

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?

   Reply:
   Unlike soil moisture, the ecohydrologic model is designed to simulate two soil thermal layers and we use the measurement of the topmost soil sensor as the reference for calibration. We do not average temperature over the soil profile. This is corrected in the revised version of the manuscript.

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.

   Reply:
   The initial measurements were primarily aimed at determining pasture yield in order to estimate the aerial biomass production in every site by cutting twice a year. However, we observed that by using only this method we would not be able to simulate the phenological cycle, therefore we carried out complementary measurements. For this reason 16 measurement of plant height were taken biweekly during two hydrological years and added to the database, which reported a continuous record of the herbaceous biomass variation and supplemented the database.
   Also, quality indicators of pasture production indicated that the yield was properly simulated. We think that capturing the phenological dynamic of natural grasses was also very important. In this regard, we consider that the database of pasture production allows to simulate both pasture yield and phenological cycle, such as represented in Fig. 5 and Fig. 6-B, where we can see that the phenological cycle of the herbaceous plants in the study site was captured by the simulated data, as well as transpiration associated to the seasonal phenology.

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.

   Reply:

The entire modeling domain was discretized with a 30m x 30m grid based on the extensive experience of the research group simulating this catchment. The main objective of the paper was to determine the climate and topographic factors that control pasture production at the catchment scale. A lower grid size would start to introduce small and micro topographic effects into the ecohydrological processes, and therefore the focus of the paper would change toward smaller scale. A smaller grid size would also introduce a level of precision in the topographic description that is not congruent with the information available for other catchment properties. On the other hand, a coarser grid would introduce too much topographic smoothing that may not properly capture the spatial ecohydrological processes dependent on the physiographic characteristics of the landscape.

7. Lines 4-6 page 15178, the author said "Maps of soil properties such as soil depth, porosity, and other hydrologic properties (Fig. 2) where 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.

   Reply:
   We incorporate the suggested change in the text in the revised version of the manuscript.

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.

   Reply:
   The methods and data are from the reference. The experimental catchment is a savannah-like environment dominated by *Quercus ilex*. Unless there is logging, fires, or any other major impact, the density and structure of the tree layer in this type of land use does not change significantly in the decadal time frame.

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 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?

   Reply:
   The stochastic weather generator uses the statistical properties of a dataset of meteorological observations to generate a synthetic and typically longer dataset that maintains the statistics of the observations. The 13 yr of observations from our meteorological station include a wide range of variations, from very dry to very wet years, together with "normal" climate situations. Therefore, we consider that it is a good opportunity to create a synthetic weather series from observations of the original site. Similarly, the pasture growth model described in the manuscript and embedded in the

referenced ecohydrologic model is calibrated under the observed meteorological conditions so reproduce the pasture growth observations. Once calibrated the models was used to simulate the dynamics of pasture during the 300 years of synthetic weather data.

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?

    Reply:
    The 51 years were used only for daily wind, which was found to be uncorrelated to any of the other weather variables and therefore was simulated independently by cycling a measured series of 51 years.
    We used the official meteorological station located at 24 km to fill some small gaps in our database, and we found that this station has a strong correlation with weather variables of the experimental catchment due to proximity and a fairly similar and gentle topography, so that strong spatial variations of climate data were not observed. The small existing variations between our meteorological station and the station 24 km out were statistically corrected using linear regression. This last point, not explicitly indicated in the original manuscript, is included in revised version

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.

    Reply:
    The 4 years of observed data were utilized to calibrated the model, then we used the 300 yr-long to simulate how pasture responds to climate variations according to landscape variables, such as tree density or topography. The calibration stage was done with our best available information and the calibration was robust given that it was done with at a high temporal resolution (calibration period totaling 1460 days). Since the weather in the subsequent 300 yr-long simulation has the same statistical character than the weather during the calibration period (no extrapolations far outside the conditions of the calibration period), we are confident that the results represent the feasible range of pasture production within the basin.

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.

    Reply:
    We discussed this topic in section 4.2.3 by saying that tree canopy cover was found to be negatively related with pasture production, reflecting the importance of variables as rainfall and light interception, and water consumption by trees. However we argued that this interpretation may not be exhaustive since the production of pasture under tree canopies is a complex issue. For instance, trees may promote pasture production by enhancing soil fertility and structure. We acknowledge that these factors were not explicitly simulated in this study.

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

    Reply:
    This was one of the reasons why measurements of plant height were taken biweekly during two hydrologic years. The additional data densified the database, increasing the temporal resolution of observations, and captured the phenological cycle of pasture. Quality indicators of the model performance indicated that the measured values of pasture production were consistent with simulated values, and that the phenological cycle was correctly captured, such as shown in Fig. 5 and Fig. 6-B. Further increases in the temporal resolution of the pasture growth dataset would have decreasing returns in terms of improving model performance.