Hydrology and Earth System Sciences Discussions

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Comment

Interactive comment on "Analysis of soil and vegetation patterns in semi-arid Mediterranean landscapes by way of a conceptual water balance model" *by* I. Portoghese et al.

I. Portoghese et al.

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Response to REFEREE #1

We are grateful to the reviewer for his comments to our manuscript. His comments have been useful in order to improve the quality of the revised paper. Nevertheless we believe it is helpful to better explain, in this reply, some of our reasoning and results that could have been misleading with respect to the main objectives of the work. In the following the Referee's comments are discussed one by one.

General comments

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RC: The authors use a variation of a classical single bucket monthly water balance model (probably first used by Thorntwaite and Mather, 1955) to investigate the water balance of four types of crops frequent in a subhumid-semiarid Mediterranean climate area. In a second part of the paper, the authors claim that the results of the model are in agreement with the distribution of the water holding capacity for three of the crops investigated in the area, providing evidence for the validation of both the model results and the hypothesis that the present vegetation patterns, in such a deeply manmanaged landscape, are the result of the adaptation of crops to the natural soil-climate conditions.

AC: It is worth stating what are the research questions that inspired our work and the investigation approach, as reported in the following points:

1) A primary motivation for this study is the development and implementation of a simple water balance model for regional applications in semi-arid Mediterranean landscapes, suitable to investigate the impact of climate change on regional water budget, and identify critical climatic and landscape controls over large spatial domains.

2) The testing hypothesis is that the analysis and detection of CSV interactions may provide a priori information which can be easily exploited, in large-scale water balance studies helping to reduce parameter uncertainties that arise in absence of accurate soil databases.

3) As a validation of such kind of reasoning, we provide evidence that the landscape feature (vegetation, soils, topography etc.) prevalent in the study region being the outcome of an evolutionary adaptation to the multi-scale climate variability, could be considered as keys to understanding the underlying water balance regimes at regional scale.

RC: In general the paper is verbose, providing more results and discussions than the reasonable for the simple model and data.

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AC: We recognize that the paper could be shortened. Nevertheless, the referee's comment is a little un-specific. Therefore, parts of the paper were re-written and some model output representations (figures 3 and 5) have been removed in the revised manuscript.

Specific comments

RC: None of the claimed results is supported by any statistical test.

AC: We believe this point is not much pertinent to the paper results. In fact we mostly adopted basic statistics to describe the average hydrological behaviour and its dispersion at the annual and monthly timescale. No inference on the stochastic behaviour of the involved quantities is actually invoked except for the pdf of the soil water holding capacity that is assumed to be Gamma-distributed according the referenced literature.

RC: There are many simplifications in the course of the paper: the root depths of crops (e.g. grape plants are known to be able to extend their roots as deep as 30 m), and citrus trees are commonly irrigated in semiarid areas. Common well known agricultural concepts like the crop yield factor (quotient between the relative crop yield and the relative evapotranspiration), which varies between 1.15 for wheat and 0.2 for olives are not taken into account.

AC: To estimate the broad-scale elements of the water cycle (e.g. Milly, 1994), simplifications (e.g. grape plants are known to be able to extend their roots as deep as 30 m and citrus trees are commonly irrigated in semiarid areas) are necessary in model applications (e.g. Yates, 1997; Arnell, 1999: Oki et al., 2001; Doll et al., 2003) and particularly they are here introduced because we want to focus on (quite commonly experienced) conditions of absence of accurate soil databases (see point 2 here-above). In fact, conceptual and physically based models almost always face with data scarcity. Our assumptions on root depth ranges are taken from the reference literature and refer to the equivalent soil depth containing most part of below ground biomass. Deepgrowing roots are considered as a response to anomalous environmental conditions

(shallow soils over fractured rocks). In this sense, the lower AWC values in the experimental soil samples in Fig. 10 can be justified. Referring to the citrus trees, we confirm that this should not be considered among the typical naturally adapted vegetation as it is not suitable for rain-fed farming. As a proof of that, in the considered soil samples, less than 0.5% corresponds to citrus plantations in the study region.

RC: Finally, the conclusions are not clearly supported by the results and reasoning, whereas the more relevant are really obvious or unoriginal: on the one hand, it may be assumed that, beyond the subsidies, farmers need to adapt their crops to the capability of their soils and climate (or they irrigate them), and, on the other, there are other published papers that really demonstrate the adequacy of the monthly bucket water balance model.

AC: It obviously appears that we didn't convey properly the motivation of the paper. Our aim was not to provide instructions for farmer's practice neither we aim to assess the hydrologic performance of such a simple model that has been widely performed in the hydrologic literature. We firstly observe that, despite human efforts and technologies, the water balance of the observed ecosystems, also for cultivated species, results to be mainly controlled by natural factors that are not merely climatic but are strongly affected by climate-soil-vegetation interactions. This is not obvious and it is not un-useful. It is particularly useful since our goal is the reduction of uncertainty of water balance model at regional scale in conditions of absence or scarcity of soil-related information. In such a case the respect of eco-hydrological principles may provide useful and non-conventional information which is today mostly not exploited in hydrology.

Response to REFEREE #2

We would like to acknowledge the great work made by the referee. His comments and suggestions have been useful in order to improve the quality of the revised paper. In the following, the comments by Referee #2 are discussed one by one.

General comments

RC: At the first stage of the study a simple water balance model was showed, which one estimated the soil water storage response S(t) using parameters from literature. At the second stage of the study, a statistical analysis of relative frequencies was done between soil water storage obtained from the model and field surveys to each crop, with the aim to find vegetation and soil distribution patterns. In general, the paper is very difficult to read and understand what do you pretend with it, because are very disperse. I suggest reviewing the redaction and structure of the paper and the relevance of the each of figures presented. If you have an extensive geo-database, you should take advantage of it.

AC: The study focus on the development and implementation of a simple water balance model for regional applications in semi-arid Mediterranean landscapes, suitable to investigate the impact of climate change on regional water budget, and identify critical climatic and landscape controls over large spatial domains. In this direction, we accept the suggestion from the referee to condense the redaction around the relevance of the each of the presented figures. In the revision of the paper we tried to shorten it by cutting figure 3 and figure 5, and restructure it by focusing on the results that are consistent with respect to the objectives of the papers. Such objectives are now more clearly stated in section 2. Moreover, we have introduced a few details concerning the geo-database so as to provide a broader picture of the available soil data (see below).

Specific comments

RC: i) The Model is a simple water balance model, when aspects like different soil layers, rooting depth and steady biomass are simplified. The model was not calibrated. And in spite of in page 3917 row 9-12 you report that monthly crop coefficients adopted in the model has incorporated local observations, it will be interesting to corroborate that the parameters of water use efficiency in Table 2 (Allen, et al. 1998) are in agreement with the "natural" behavior of the crops in the study zone.

AC: i) Our motivation for adopting a simple model derives from the necessity to avoid

calibration practices trying to do the best from the available knowledge. In fact, as reported in section 2, the literature crop coefficients by Allen et al. (1998) are modified on the basis of lisymeter studies developed in the study region to account for the peculiarity of climate that reflects on plant development and therefore soil water exploitation (phenology). The most evident adjustment in the crop coefficients is that of the winter wheat with its anticipated development and maturation in the early spring.

RC: ii) In Eq. (1), P represents the rate of precipitation, but is not specified if it is the effective precipitation, since in arid and semiarid zones it is very important the interception process like losses of water and seems that data needed for its estimation are available.

AC: ii) P is total precipitation. Interception loss is not modelled in our model since we assume that monthly evaluations are dominated by soil water storage rather than leaf storage, particularly when dealing with crops that are typically characterized by low leaf area index compared to forested covers.

RC: iii) It is not mentioned the aquifer influence in the model, moreover, point out if the crops in the study zone either taken water either from it or not. The aquifer level in the study region lays far below the root zone almost everywhere as typically occurs in carstic limestone environment of the Mediterranean. This non-negligible element has been clarified in section 3.

RC: iv) If the vegetation is in steady conditions (page 3918 row 5-9), (i.e. permanent tree crops are referred to mature plants with no biomass growth through the years), what is the sense to do many intra-annual simulations? Why do you evaluate the plant productivity? It will be better to do annual simulations to observe the inter-annual behavior of S(t) and ET to several initial conditions of soil moisture (may be, the initial value of soil water storage has high influence on model results).

AC: iv) Soil water balance is dominated by the seasonality of climate and vegetation development, therefore monthly evaluations have been reported as long as they provide

insights into the variability of water fluxes and the way they influence annual behaviour by way of different initial conditions of soil moisture. In this sense, inter-annual variability of annual ET is considered as a proxy of plant productivity performance from year to year and is investigated with respect to different soil water holding capacity of the root zone. This idea has been better clarified in section 3.2.

RC: v) Knowing that the vegetation crops could harvest at least once time per year, why did is not mentioned the harvest practice in seasonal crops or prune in permanent crops? Or if the vegetation are considered in natural regime, what really happen with the evapotranspiration rates with seasonal crops? In this case, you should estimate the factor Kc for natural vegetation under non-standard conditions (Allen et al., 1998).

AC: v) Concerning the definition of the harvest and pruning practices, we have mentioned the respective times in Table 2 by adopting bold characters. For seasonal crops in the Table 2 caption, it is now specified that the months following harvest time are considered as affected only by bare soil evaporation in the water balance (value set to 0.3). Non-standard evapotranspiration is accounted by the water stress coefficient Ks as a function of soil water content (see Appendix A, Equation A5 and A6).

RC: vi) In the section 2 (Methodology) -page 3915 row 16- is not specified the nature of "direct measurements", I suppose that these are the 4000 soil samples available in a geo-database. In any case, there is not specified neither they were done (date and spatial distribution) methodology used nor results obtained. A figure could be needed to see the soil samples coverage.

AC: vi) During the period 1997-2000 a soil survey was undertaken to develop a new soil map for agricultural land planning and management. Consequently, in the soil geo-database available for the study region (Caliandro et al., 2005), soil moisture storage capacity being the landscape feature that enable vegetation survival in water-limited environment (Rodriguez-Iturbe et al., 1999) is expected to reveal typical patterns related to the vegetation distribution. The survey uniformly covered the agricultural por-

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tion of the Apulian territory in southern Italy (covering about 14.700 km2). Most of these observations (about 4000) were bases on soil cores dug to a maximum depth of 1.50m and analyzed on site by trained experts adopting simplified qualitative methodologies. Total soil depth and available water capacity (AWC) were recorded among other features such as soil texture and structure, terrain elevation, slope and aspect, landuse, vegetation, litology, groundwater level, drainage condition, and permeability.

RC: vii) In section 3 (Results and discussion) many results (sensitivity analysis) and figures are showed. Since the model's simplicity, a great extension of results it does not permit; the statistical analysis is too basic, it will be better to use statistical analysis test more robust. It will be better to find a concise way to show the results, and address to the conclusions, since the present conclusions are not clear moreover obvious, they must be based on paper objectives.

AC: vii) A simple model has been adopted on purpose because our investigation is focused on control factors that regulate climate-soil-vegetation interactions at a scale that is consistent with climate and vegetation seasonality. This allowed to consider the soil water storage capacity as the sole landscape parameter to control the water balance partitioning for a given climate and vegetation type as in many large scale SVAT schemes. Moreover, a daily resolution of the water balance model would involve additional soil hydraulic properties even for a simplified representation of the of soil permeability as a time-dependent function of soil water storage. Such an increase in model complexity was exemplified in the Appendix B not to create sensible differences in the water balance if compared to the predictions obtained with the monthly model. The statistical analysis of the soil database has been extended in section 2 with a multivariate approach to analyze the impact of soil environmental factors on the species composition. This analysis is used to statistically relate abundance of species with respect to environmental gradients described by explanatory variables (with an acknowledged or hypothesized influence on the response variables). In our case it was used to summarize community patterns and compare the suggested gradients

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with the independent knowledge of environmental controls on vegetation abundance that results from model experiments. In conclusion, we agree with the reviewer on the need to present the results more concisely, and above all to focus on those findings that are conductive to the paper objectives i.e. to explore water balance response due to different vegetation types, and in this way trying to explain the spatial patterns of soil-vegetation occurrences analysed in the available geo-database. Therefore, in the revised paper we have eliminated figure 3 and figure 5 in section 3, presenting the only simulation results that clearly refer to the most interesting conclusions of the paper. Nevertheless we recognize that the main lacking element of the paper is a traditional validation scheme based on real observation of water balance fluxes. This limitation is not unusual when dealing with large scale impact studies (e.g. climate change) in which a simplified approach to the water balance modelling is often needed to overcome problems of data availability. Nevertheless, we believe that in the developed case study the geo-database of fundamental soil feature has provided the opportunity for a non-conventional validation of the presented water balance response connected to climate-soil-vegetation interactions.

Other comments

RC: i) To specify what kind of statistic is the "central values".

AC: i) We refer to the central values meaning the mid values in the typical range of variability for each of the investigated model parameter. The central value is one kind of descriptive statistic, which indicates a 'typical' or 'average' figure for a range of possible values.

RC: ii) To specify the bc parameter use.

AC: ii) Actually bc stays for Bucket Capacity and indicates the soil water storage capacity in the Sbc parameter that is considered in the sensitivity analysis. It is true anyway that the parameter does not appear explicitly in any of the model equation therefore we have specified in section 2.1 that Sbc is the main model parameter controlling

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evapotranspiration under non standard conditions (i.e. indicating the relative soil water content) and we have mentioned Sbc in equation A6 of appendix A.

RC: iii) Page 3910 row 20: When you mention "regional applications" is referred either due to particularity of the study zone and therefore the model is only applicable on it, or due the spatial scale of the model (mesoscale).

AC: iii) Although some of the hypothesis behind the simplifications in the model formulation result from peculiar features of the study region, our interest is in the development of mesoscale to regional hydrological model with effective and parsimonious landscape parameterization.

RC: iv) Page 3912 row 24: When you say "In particular, the habitats selected for plant domestication are chosen so as to provide reduced competition, improved fertility, and reduced disease incidence to the introduced vegetation, thus allowing increased productivity" Are you pointed out that these reasons are decisive to choose a crop or another one? What happen with others criterion like terrain slope, facility and feasibility extensive terrain to crop or socioeconomic factors?

AC: iv) To this point we should have better explained the idea that most improvements in plant adaptation to edaphic characteristics have occurred by chance through the selection for yield operated along the years by farmers (Zobel, 1992). This search for increased yield of course encompasses other criteria like terrain slope, facility and feasibility. Other factors explaining the dominant crop species are related to socioeconomic aspects in general and to the maximization of profit, in particular. This aspects are exemplified in the analysis of the soil database where more profitable crops (i.e. wheat) become more abundant with respect to the less productive ones (i.e. olive groves) as soon as the available water capacity of the soil is favorable (see section 2).

RC: v) Page 3928 rows 15-19: "Model simulations are utilized to explain and explore differences in the soil moisture response due to different vegetation types and their impact on the temporal variability of water balance, and in this way explaining the spatial

patterns of soil-vegetation occurrences extracted from the statistical analysis of available data over the study region". This text is a good summary of the aim of the paper; it should be put in section 2.

AC: v) As we have anticipated in the above the aim of the paper is now more clearly stated in section 2. Around this statement we have re-organised the presentation of the results in the revised paper.

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