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Hydrology and Earth System Sciences Discussions



# *Interactive comment on* "A conceptual dynamic vegetation-soil model for arid and semiarid zones" *by* D. I. Quevedo and F. Francés

**D. I. Quevedo and F. Francés**

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Anonymous Referee #1 Received and published: 27 November 2007

The paper describes a coupled land surface-vegetation model for semi-arid ecosystem. A sensitivity analysis of the model is performed. At the present the model cannot be considered for publication. Indeed, there are major concerns that are described below in details.

Main comments:

1) pag. 3473, row 3-4: the first main objective is "Developing a conceptual vegetation model for arid and semiarid ecosystems that represent the vegetation response to the soil moisture fluctuations"; this objective is not reached by the paper. The authors

developed a vegetation model for one type of woody vegetation (Quercus) only. This type of woody vegetation is easily simulated since it is mainly constant during the year. Hence, why should I simulate dynamically it in a hydrologic model? Why not put jus a constant value? Why should I increase the model parameterization?

We think the referee has not understood our work:

i) The model is focused to the modelization of plant functional types, as it is mentioned in page 3473 lines 14-16. We choose Quercus coccifera L. specie because it is one of the most important Mediterranean plant and because we had enough information to estimate the needed vegetation parameters (ze, Ψ*, Ψw, An,mx, Bpot, Tmx, c, k, q and Imx) showed in Table 1. Other plant functional types can be used, just changing properly the values of the vegetation model parameters.

ii) In spite of Quercus coccifera L. is evergreen specie, its biomass has seasonal variability (as it is shown in Figure 4). It is true that from the water cycle point of view and without changing the climate characteristics, there will be a constant value for the leaf biomass with low reduction of the model performance. Actually, this is the case for all the hydrological models. But if we agree this constant leaf biomass is a function of the climate, soil characteristics, slope orientation, ... the question will be for example: which is the new value if the climatic and/or edaphologic conditions are different? Or what happen the years after a fire? We are trying to develop a parsimonious model which will permit to answer these types of questions without the need of estimation or calibration for each different scenario of this constant leaf biomass: the only way is considering leaf biomass as a state variable instead of a parameter.

2) Pag. 3476, row 13, equation (8): it is a main concern, R in equation (4) is the fraction of vegetation, while in equation (8) becomes relative biomass production. The two things are not the same. Then, in pag. 3480, row 14 and in Figure 4 the authors talk of "daily biomass simulated with CDVSM". The model simulates the relative biomass production which is not the biomass. All these terms (fraction of vegetation, relative

biomass production and biomass production) are related but they are not the same. A lot of work was made by several authors to relate these terms. The authors need to be careful.

The state variable R in all equations is understood like relative leaf biomass respect to potential leaf biomass value. The derivative of R with respect to time defined by equation (8) must be understood as the relative leaf biomass production or temporal change of relative leaf biomass.

It is true in figure 4 we really show the daily leaf biomass estimated from daily relative leaf biomass (R). It must be added in the text and/or figure caption that leaf biomass is R times Bpot.

3) Pag. 3479, rows 11-15: soil parameters are estimated on the basis of literature data. No calibration is performed since soil moisture and hydrologic flux (evapotranspiration, drainage, etc.) observations are not available for this case study. But soil parameters are very important model parameters. If you are developing a new model, first of all you must demonstrate that this model works well. You must compare model predictions and observations. Hence, why are you using this very poor data set? You can go on fluxnet database available on the web site and test the soil model deeply. Otherwise the hydrologic model (without validation) cannot be considered acceptable and publishable at this time.

To calibrate and/or validate any mathematical model it is needed to have observations of all inputs and at least the observation of one of the model state variables. Which is desirable is that the observed state variable would be the most important one and/or the one which is related with the parameters which are less known. In our case it is obvious that the vegetation part of the model is the most important one.

More precisely, we are interested in:

i)The Quercus coccifera L. leaf biomass response and ii)in semiarid climate (PET much higher than P) in order to have only the water availability as the main vegetation control factor.

As far as we know, in the fluxnet database there are not measurements which accomplish these two conditions. In fact, we could not find any perfect set of data. By the way, we will appreciate if any reader could provide us with it and there is a high probability that this will happen.

In this “information scenario”, we preferred to use the vegetation biomass evolution after a fire from Cañellas and San Miguel (2000) without any other state variable measurement, in order to calibrate the most unknown parameter: the exponent c in eq. 8. With this objective, it was enough to use a representative Mediterranean soil and meteorological series. Our experience in hydrological modelling permits us to be confident in the estimation of the rest of parameters without any specific calibration.

We agree that model validation is an important modelization stage. See our recent paper in the calibration and validation of distributed hydrological models: F. Francés, I. Vélez and J. Vélez, “Split-parameter structure for the automatic calibration of distributed hydrological models” (in Journal of Hydrology, 332, 226-240, 2007). For the case study we are discussing now we did not have enough information to do a validation (temporal or spatial) because it was used for the calibration. But it must be taken into account we are calibrating only one single parameter in a multiparameter model, which permits us to say no validation doesn’t means directly our model is not acceptable for publishing just for this absence.

4) Pag. 3480, calibration of vegetation equation. This is a main concern. I don’t think that this case study is very attractive.

4.1) Observations of biomass are available sparsely (17 observations over 36 years).

The 18 Cañellas and San Miguel (2000) Quercus coccifera L. age and biomass mea-

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surements after a fire represented in Figure 4 and used for the calibration of the exponent c, are the average of around 20 measurements in different experimental plots (with different soil characteristics and meteorological past) but with a climate similar to our case study. Therefore, these data can not reproduce the particular daily hydrometeorology of our location, but can be used (and it was used) to calibrate the vegetation evolution starting from no vegetation at annual scale.

4.2) Seasonal observations during the same year are not available.

The referee is partially right. Seasonality observations of leaf biomass are not available, but the resulting vegetation dynamic at monthly scale is in agreement with our knowledge about phenology of the Quercus coccifera L. See also our reply to referee # 2 comment viii.

4.3) The vegetation type is the Quercus, which is evergreen and doesn’t show strong seasonality. The vegetation dynamic model can easily predict biomass dynamics since it is mainly constant.

See our reply to comment # 1 above.

Moreover, after the first 2-3 years of vegetation growth, biomass is mainly constant (between 4 and 5 t/ha). Only two peaks above 5 t/ha are observed during the year 1973 and 1975. And the model is not able to predict these two peaks. Hence, I have serious doubts on the ability of the model to predict biomass dynamics.

It is not true for Quercus coccifera L. after 2-3 years of vegetation growth the biomass is mainly constant. As it is shown in Figure 4 this happens after 8-10 years. May be the confusion is because the referee assumes each dot represent one year, which is not the case.

Concerning the two peaks in 1970 and 1975, they correspond to experimental plots with 5 and 9 years old after fire, but meteorological conditions do not correspond to 1970 and 1975 respectively. As mentioned above, these dots must be used to understand the annual evolution of Quercus coccifera L.

I would like to see the model predicts the biomass of more dynamic vegetation species, for instance some grass species that are more water-controlled. Indeed, one objective of the paper is to predict biomass "based on its water demand and water-soil availability". But this is not the right case study, because the Quercus dynamics does not seem water controlled. My guess is that this vegetation model is too simple for predicting biomass of vegetation types more dynamically controlled by soil water availability. Again (see comment 3), use a more robust dataset from fluxnet. Test the model accurately. Otherwise the model cannot be considered acceptable and publishable, since this is a new model and must be first of all widely tested.

Quercus coccifera L. is an important specie for us. Including its simplicity, if this is the case. Also, what is important in the Cañellas and San Miguel (2000) data set is that in the way we are using them is more than 10 years length and starting from no vegetation.

The vegetation in our model is clearly controlled by soil water availability, as it is explained in our reply to referee # 2 comment viii.

5) Before to make the sensitivity analysis of the model to two parameters only and two model inputs only, you must validated deeply the model. And a sensitivity analysis to all the model parameters can be performed using, very important, more robust techniques, such as global multivariate approaches, which allows for investigation of the sensitivity to each parameter, while varying all the other parameters (e.g. Franks et al., 1997).

The more complex sensitivity analysis done in Franks et al. (1997) is based in a Monte Carlo framework, but

i)Its main objective is to solve the equifinality problem in a physically based model with a high number of parameters. ii)The sensible variable is the model efficiency, instead



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of any state variable.

This is not the case of our model, whose intention is to be parameter parsimonious and our sensitivity analysis is focused to the important state variables. In any case, the results/conclusions reported in our section 4 will not change with a more sophisticated Monte Carlo analysis.

6) Pag. 3485, conclusion section: "It can be proved CDVSM reproduces well the biomass dynamic based on soil water balance, considering VWS as an index of its dynamic in semiarid and arid zones, due to a great extent; VWS determines the growing season, water-uptake dynamic and can help to understand the adaptations strategies of the vegetation to shortage of water". Again (see comment 4) I don’t think that the model is validated with this dataset. I have serious doubts on the model reliability. At this time the sensitivity analysis seems just a synthetic exercise (and not complete, see comment 5), because the model is not validated.

See our reply to comment # 4

7) A sensitivity analysis to soil root depth is performed but the model doesn’t consider vegetation root dynamics. This may be a serious lack of the model, which can alter this sensitivity analysis.

Keep in mind that our model tries to be as parsimonious as possible and our focus is not to model vegetation root dynamic. We are interested to model leaf biomass, since it is directly related with transpiration and soil moisture.

8) Since the biomass is mainly constant during the whole dataset, why not use just a constant value of biomass in the soil model? Why a hydrologic modeller should use this vegetation model and increase the model parameterization?

See our reply to comment # 1

Minor comments:



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9) pag. 3471, row 21-23: it is not true that all hydrologic model consider vegetation like a static parameter. Let’s say "most of them".

In page 3471 lines 21-23 says “Most of the hydrological models are able to represent hydrological processes, at watershed scale, but all of them consider the vegetation like a static parameter”. What must be said (and will be corrected in the final text) is: “Most of the hydrological models are able to represent hydrological processes properly at watershed scale, but all of them consider the vegetation like a static parameter”. What we want to say is that there are good hydrological models for the watershed scale, but we don’t know any vegetation-hydrological model for this particular objective scale. This will be one of our next steps in this research: to apply the presented plot model into a distributed hydrological model. If the referee gives us the proper reference, we will change immediately the sentence to his suggestion.

10) Pag. 3479: Equation (10), which describes the soil water retention curve model, should be in the "model description" section and not in the "parameter estimation" section.

The true model parameters are the maximum upper soil available water and the specific water contents related with the vegetation. The soil water retention curve is needed to the estimation of these parameters using the proper soil and plant characteristics.

11) Pag. 3479, row 14: a wilting point at 3 MPa for a Quercus may be underestimated. The wilting point for such species may arrive at 6 MPa. A sensitivity analysis?

As mentioned in the text, Laio et al. (2001) reports a representative value of 3MPa. Also, in Corcuera et al. (2002) is reported a value 3.2MPa like leaf water potential at turgor-loss point. It is possible a value of 5MPa (Laio et al., 2001) and, as suggested by the referee a value of 6MPa. We are just using the representative value and, in any case, the results are low sensitive to an increase of this value (for example, using 5MPa the January mean relative leaf biomass decrease from 0.76 to 0.75 (with 3 MPa).



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The exact reference for Corcuera et al. (it will be added in the final text) is: Corcuera et al., (2002), Functional groups in Quercus species derived from the analysis of pressure–volume curves, Trees 16, 465–472.

12) Pag. 3479: again (see comment 10), equation (11) is part of the model description. It must be included in the "model description" section.

It will not be corrected for the same reasons than in reply to comment # 10.

13) Figure 6: is it for the basic run? Please, clarify.

No exactly. As it is mentioned in the text (page 3484, first paragraph) the unique difference is the initial condition for R.

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