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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

Anonymous Referee #1

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The papers 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 an hydrologic model? Why not put jus a constant value? Why should I increase the model parameterization?

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.

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.

4) Pag. 3480, calibration of vegetation equation. This is a main concern. I don't think that this case study is very attractive. 1) Observations of biomass are available sparsely (17 observations over 36 years). 2) Seasonal observations during the same year are not available. 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. 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

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these two peaks. Hence, I have serious doubts on the ability of the model to predict biomass dynamics. 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.

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).

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.

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.

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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 an hydrologic modeler should use this vegetation model and increase the model parameterization?

Minor comments: 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".

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

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?

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

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

References: Franks, S. W., Beven, K. J., Quinn, P. F. and Wright, I. R., 1997. On the sensitivity of soil vegetation atmosphere transfer (SVAT) schemes: Equifinality and the problem of robust calibration, Agric. For. Meteorol., 86, 63 –75.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 4, 3469, 2007.