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Hydrology and Earth System Sciences Discussions

Interactive comment on "Vegetation dynamics and soil water balance in a water-limited Mediterraneanecosystem on Sardinia, Italy" *by* N. Montaldo et al.

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General comments

The manuscript entitled "Vegetation dynamics and soil water balance in a water-limited Mediterranean ecosystem on Sardinia, Italy" by Montaldo, Albertson and Mancini presents an extension of a coupled vegetation dynamics (VDM) and land surface model (LSM) that was originally developed to model grasslands only. The extension allows the user to distinguish between areas covered by grasses, woody vegetation and bare soil. The new model is calibrated to reproduce two years of data from a mixed mediterranean ecosystem, including components of the energy balance, soil moisture dynamics, leaf area index (LAI) of grass and woody vegetation and evapotranspiration (ET). The model was then run for an additional year, for which observations were available.

The authors use the model runs to assess the influence of the environmental conditions on the dynamics of LAI in each growing season and conclude that grass LAI is very sensitive to rainfall in the early growing season, while woody vegetation LAI remains at a constantly high value throughout the investigated period. They show a relationship between the 15 days average grass LAI and the preceding 15 days cumulative rainfall during the growing season.

The model reproduced the observed fluxes and state variables remarkably well, given that only 10 parameters were tuned. This is indeed encouraging and suggests that the model can be used for interpolating gaps in observations or for data assimilation schemes as suggested by G. d'Urso (HESSD 5, S748211;S74, 2008). The analysis of the response of grass LAI to the climate forcing is very instructive, as it highlights the flexibility of grasses in their adaptation to the environment. The model represents a parsimonious way of modelling the complex vegetation dynamics in semiarid ecosystems and the data can help increasing our understanding of semiarid ecosystems.

The description of the methods is clear and concise and the approach used does not contain any fundamental flaws. One issue deserving more attention is the photosynthesis model (P_g and ϵ_P in Table 2). The values for the parameters a_0, a_1 and a_2 are not given and the shape of the curve does not appear to be consistent with established photosynthesis models, which generally saturate with increasing PAR. The authors should explain this equation in more detail, as it is the driving component of the model. For this reason, it would also be important to provide a comparison of the simulated and observed photosynthesis rates. The net CO_2 exchange has been measured simultaneously with ET on the site, so it should be possible to present this very important data in the paper.

The presentation of the results and conclusions is partly misleading and needs improvements. See the specific comments below for details. I would also suggest addition of a discussion of the advantages and limits of the model. One of the advantages I see is the fact that the model simulates the dynamic responses of vegetation to environmental forcing without prescribing phenology a priori. The main disadvantage in its current state is the need for calibration. Some of the values of the calibrated parameters may not be realistic (see below) and it is unlikely that the same calibrated parameters would be valid at a different site. This means the model is only useful where sufficient data is available for calibration. As far as I can see, many of the calibrated variables could be replaced by literature values or observations on the site (e.g. specific leaf area of dead biomass, respiration coefficients, death rates, saturated soil moisture) and the allocation parameters could be investigated from the perspective of optimal adaptation. The potential of the model to be used without calibration in the future makes it very attractive.

Specific comments

The authors state in the abstract that the paper demonstrates that the use of the VDM in the LSM is essential when studying water-limited ecosystems, but the paper does not discuss any alternatives to the use of the VDM. The abstract would probably be more accurate if this sentence was left out.

On pages 222 and 228, the authors explain which VDM outputs are used in the LSM, but not how the LSM outputs affect the VDM. This should be added for clarity.

On page 226, the authors should describe how the “averaged θ time series” were calculated, e.g. if they used any weighting between the different probes. The definition of specific leaf area in lines 21-23 is not consistent with the common definition. Presumably, the authors meant “leaf area divided by leaf dry mass”, but this should be clarified in the text. It would also be helpful to explain whether the numbers represent the quotients of the mean leaf areas and dry masses of all leaves or the means of the quotients taken for each individual leaf. The number of leaves measured and the

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means and variances of the measurements would also be useful for further reference.

On page 229, thin soil and semiarid conditions are said to be responsible for the dominance of infiltration excess over saturation excess for overland flow generation. This is counter-intuitive, as thin soil should lead to more frequent saturation and hence more saturation excess runoff compared with deep soil. The authors' line of reasoning should be made clearer here.

On page 232, the authors describe how f_{vt} is calculated from LAI and f_v, but they do not state how f_v was parameterised for each plant functional type (PFT). In fact, the values given for f_{vt} for woody and grass vegetation on page 233 suggest that f_{vt} was prescribed as a constant and not a function of LAI as implied on page 232. This should be clarified.

On page 233, the authors state that the model was calibrated for the first two hydrological years and then validated using the last hydrologic year in the data set. However, none of the plots shows the validation data set separately from the calibration data set, which makes it very hard to assess the model's "predictive" capabilities. To satisfy the claim that the model has been validated, goodness of fit statistics should be shown for the validation period separately from the calibration period. Alternatively, the authors could use just some of the time series for the calibration (e.g. energy balance compoenents) and validate the model using other observations (e.g. LAI and photosynthesis). The available data is extensive enough to allow such an approach. The presentation of simulated and "observed" cumulative ET in Figure 9a is misleading, as 30% of total "observed" ET is made up by simulated ET that was used to fill gaps in the observations (page 234, line 2). I would suggest to remove the cumulative ET plot from the manuscript and replace it with a scatter plot of the observed and simulated ET for the validation period only. If there was no bias in the scatter plot, the correspondence of the observed and simulated cumulative values would follow and the claim that the model "predicts" total ET well (page 236, lines 2-4) would be substantiated.

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On page 234, lines 4-6, the authors suggest that the main contribution to errors in the simulated soil moisture is due to a discrepancy between the observed soil moisture peaks and the rain gauge imput. The data presented in Figure 2, however, suggest that the variability in soil moisture is systematically under-estimated by the model throughout the observation period, as both peaks and troughs are under-estimated in the simulation results. This should be reflected in the text describing the results. On line 10, it should also be mentioned that the prediction of woody vegetation LAI is not as good as the prediction of grass LAI in 2006. This mismatch does not constitute a major concern but should be pointed out to the reader.

The authors speak about favourable conditions for grass growth, starting at the bottom of page 34, without defining what they mean by "favourable". They mention high soil moisture and high potential evaporation (E_p) in connection with high LAI, but they do not explain why high E_p favours grass growth. E_p is a function of both net irradiance and atmospheric vapour pressure deficit (VPD). Increased irradiance is favourable for photosynthesis, but increased VPD can be detrimental for photosynthesis if stomatal conductance has to be reduced in order to conserve water. Since E_p increases with both net irradiance and VPD, it is not obvious why high E_p should consitute a favourable condition for growth. Instead of using E_p, I would recommend correlationg LAI with irradiance and VPD in Fig. 10. The plot of E_p in Fig. 10 is not very helpful, anyway, as the different years are very hard to distinguish. The authors stated that grass LAI did not increase after day 100 in 2005 because of low E_p compared to 2004, while soil moisture was similar in both years. This explanation only makes sense if the increase in E_p was due to increased irradiance, not due to increased VPD in 2004.

On page 235, the authors describe a correlation between moving windows of LAI and rainfall and refer to Fig. 11 as evidence for saturation of LAI at a value of 2 with increasing rainfall. This statement is repeated on page 236, lines 16-18. Given the low number of data points and the large scatter, it appears that the authors may be over-interpreting the plot. Both statements, that there is a positive correlation between rainfall and LAI and that this correlation should saturate at high rainfall values are intuitive, but the data presented do not necessarily support the claim that this saturation is reached on the site, as leaving out one single data point (e.g. the top right or bottom right point) could lead to very different conclusions. The scatter in the data suggests that factors other than rainfall play a large role, too. Perhaps, the analysis would explain more of the variance if it included irradiance and VPD.

The statement that predicted ET is accurate to within 99% of the total observed ET on page 236 is misleading, as the authors compared the cumulative ET for the whole data set, where 2 out of the 3 years were used for calibration of the model and hence should not be added to "predicted" ET. In addition, 30% of the total "observed" ET is actually simulated ET, as the gaps were filled using simulated data. For a valid assessment of the accuracy of prediction, the authors should only add observed data points that were not used for calibration of the model.

Lines 4-9 on page 236 belong in the Results section. A discussion of why the woody vegetation LAI is so much less sensitive to the environmental forcing than grass LAI should also be provided. The authors stated that the responsiveness of grass LAI to the environmental forcing is high because of the limited soil depth and the absence of available groundwater, but these conditions should equally apply to the woody vegetation. The difference in the response of woody vegetation and grass LAI to the environmental forcing suggests that the explanation given may be incomplete.

In the last paragraph on page 236, the authors conclude that climate change poses a danger for mediterranean ecosystems. This conclusion, though intuitively correct, is not supported by the analysis presented, as the investigation focussed on seasonal vegetation dynamics and not on long-term trends. I would suggest to remove this paragraph from the conclusions.

In Table 1, the calibrated values for specific leaf areas of the dead biomass are by a factor of 5 to 14 higher than the specific leaf areas of the green biomass. This is not realistic, as it implies that leaves would either expand their surface area at constant biomass or reduce their biomass at constant leaf area by a factor of 5 to 14 once they die. The authors should explain in the text either how this can be interpreted or what realistic values might be and how they would influence the model results. The saturated soil moisture of 0.65 also appears unrealistically high as typical values for silt loam are around 0.45. A brief reference to a source where such high values have been observed in nature would be helpful.

Technical corrections

The below list is not complete. Please check the document again for spelling and grammar errors.

220, 4: Should be “competing for water”

220, 6-8: I would rephrase the sentence to: “An extensive field campaign (...) was performed with the objective ..."

221, 2: Should be “climate variation, fires, etc."

221, 21-24: This sentence needs rephrasing.

222, 1-3: This sentence should be split in two.

223, 17: I believe “Dancus cerota" would be “Daucus carota".

226, 2: First use of the greek letter θ. Please give a definition.

226,14-20: This part belongs in the Results section.

227, 26-: This sentence needs rephrasing and preferably splitting.

230, 25: Should be “adapted from Montaldo et al. (2005), who derived"

232, 14-15: c_g and c_d should also be explained here.

234, 3: Please explain what “rmse” means (root mean square error?).

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234, 20-25: Since the plots all show the day of the year on the horizontal axes, the text should refer to the day of the year rather than to the month of the year. A simple DOY range (in brackets) for each period would suffice to help the reader find the respective information in the plots.

235, 6+13: Should be “could not" and “does not” instead of “couldn’t” and “doesn’t”. Please check for similar occurrences.

236, 2-3: Should either be “in these ecosystems” or “in this ecosystem”.

236, 18-20: This sentence needs rephrasing.

238, 1-2: Please give the page numbers wherever the reference is a book, otherwise it becomes very difficult to find the relevant section.

238, 2: Should be “Finally, the effect of VPD on stomata opening was modelled following Jarvis (1976):”

246: Please mention the averaging depth for the given soil moisture in the caption.

252: The axes of panels (c) and (d) should be adapted to the plotted data range, and some goodness of fit statistics would be helpful for all panels.

254: The y-axis in panel (a) should be "Grass LAI". Panel (c) is not very informative, as the different lines cannot be distinguished.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 219, 2008.

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