

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

N. Montaldo et al.

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The comments were helpful and have been addressed in the revised manuscript. We are pleased with the result and feel the manuscript is stronger. A point-by-point response to the comments, including a summary of the corresponding modifications to the paper, is included below.

General comments

1) Comment: "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

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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 (Pg and eP in Table 2). The values for the parameters a0, a1 and a2 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 CO2 exchange has been measured simultaneously with ET on the site, so it should be possible to present this very important data in the paper.

Response: Thanks to the reviewer for the useful suggestions. We regret the oversight regarding the values of the parameters a0, a1 and a2 of the photosynthesis model, since they are the same of Montaldo et al. [2005] model. However, according to the reviewer's suggestion the following text has been added "Coefficients of the leaf photochemical efficiency, eP, of the photosynthesis model are the same of Montaldo et al. [2005] (a0= 0.007, a1= - 0.0205, a2= 0.019 with PAR expressed in kW/m2)" These coefficients were estimated from Eagleson [2002, pag. 236], Dewar et al. [1998] and Rosati and Dejogn [2003] literature data, and tested by Montaldo et al. [2005]. The photosynthesis model used by Montaldo et al. [2005] does correctly saturate with increasing PAR. Unfortunately, we didn't measure photosynthesis directly (e.g., with Licor 6400). Our eddy-correlation system can only observe net ecosystem exchange, which is the difference between ecosystem carbon assimilation and ecosystem respiration. Hence, it measures ecosystem fluxes, and not the carbon fluxes of each land cover. However, since our objective is to provide answers to hydrologists, and we are publishing in an hydrology journal, it was more important to measure LAI, which is more commonly used than photosynthesis in hydrologic models.

2) Comment: The presentation of the results and conclusions is partly misleading and needs improvements. See the specific comments below for details. I would also sug-

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gest 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.

Response: we thank the reviewer for the suggestions. We deeply revised the manuscript following the specific comments. We also replaced some calibrated variables (e.g., specific leaf area of dead biomass, saturated soil moisture) with more physically based values such as suggested by the reviewer. We also added the suggested discussion of the advantages and limits of the model. For instance, the following text has been added: "From an operational point of view, the proposed model is attractive since it allows to predict the response of vegetation dynamics to atmospheric forcing without prescribing phenology a priori. Although the model does need some calibration, many of the model parameters, in fact, are readily taken from literature; the parameters estimated from observations on the site are close to typical literature values, so that the model can be considered also for applications on non-instrumented fields"

Specific comments

3) Comment: 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.

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Response: sentence canceled.

4) Comment: 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.

Response: Addition made. The following text has been added: "The LSM provides soil moisture and aerodynamic resistances to the VDM"

5) Comment: On page 226, the authors should describe how the "averaged q time series" were calculated, e.g. if they used any weighting between the different probes.

Response: Addition made. The following text has been added: "The averaged (through an arithmetic mean across all the probes) q time series are shown in Figure 2"

6) Comment: 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 means and variances of the measurements would also be useful for further reference.

Response: Thanks for the suggestions. The following text has been revised: "Finally, specific leaf areas (leaf area divided by leaf dry biomass) of predominant grass (= 0.01 m² gDM⁻¹, mean of 3 samples covering an area of 1 m² each and with a standard deviation of 0.0007 m² gDM⁻¹) and woody vegetation (= 0.005 m² gDM⁻¹, mean of 3 samples covering an area of 1 m² each and with a standard deviation of 0.001 m² gDM⁻¹) species were measured directly (weighting the dry biomass)"

7) Comment: 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

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and hence more saturation excess runoff compared with deep soil. The authors' line of reasoning should be made clearer here.

Response: Thin soils in semiarid conditions are typically characterized by hortonian overland flow, which is characterized by an infiltration excess mechanism. Since this behavior is well known to hydrologists, for brevity we just cited a common book (for undergraduated hydrology courses) of hydrology (Chow et a., 1988), in which at pag. 129 there is written:"... Hortonian overland flow is applicable ... for natural surfaces with thin soil layers and low infiltration capacity as in semiarid and arid lands". For clarity we have included the page number of the book in the reference.

8) Comment: 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 parameterized 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.

Response: sorry, there was a typing mistake. The constant values given on pag. 233 were for f_v and not for f_{vt} . The following text has been revised: "for the soil moisture budget we used the fraction of land covers of the field monitored by the soil moisture probes ($f_{v,WV} = 0.25$ and $f_{v,gr} = 0.6$)"

9) Comment: 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 components) and validate the model using other observations (e.g. LAI and photosynthesis). The available data is extensive enough to allow such an approach.

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Response: Per the request, the new Table 4 reports statistical results for model performance evaluation, separately distinguishing validation and calibration periods.

10) Comment: The presentation of simulated and "observed" cumulative ET in Figure 9 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.

Response: we removed the fill gaps in the observations from the Figure 9. In the figure we prefer to leave cumulative values of ET because they provide "an idea" of the total loss for evapotranspiration, which is important for soil water budget.

11) Comment: 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 input. 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.

Response: Following reviewer suggestions, the text has been revised. For instance, the following text has been added: "The model does not capture well the daily soil moisture variability of individual probes during several rain events, but well predicts soil moisture average dynamics" and "The dynamics of the WV are predicted less well than grass..."

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12) Comment: The authors speak about favorable conditions for grass growth, starting at the bottom of page 34, without defining what they mean by "favorable". They mention high soil moisture and high potential evaporation (Ep) in connection with high LAI, but they do not explain why high Ep favors grass growth. Ep is a function of both net irradiance and atmospheric vapor pressure deficit (VPD). Increased irradiance is favorable for photosynthesis, but increased VPD can be detrimental for photosynthesis if stomatal conductance has to be reduced in order to conserve water. Since Ep increases with both net irradiance and VPD, it is not obvious why high Ep should constitute a favorable condition for growth. Instead of using Ep, I would recommend correlating LAI with irradiance and VPD in Fig. 10. The plot of Ep 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 Ep compared to 2004, while soil moisture was similar in both years. This explanation only makes sense if the increase in Ep was due to increased irradiance, not due to increased VPD in 2004.

Response: As recommended by the reviewer we modify the Figure 10, which includes now plots of VPD and irradiance (in particular the PAR). We used color lines for distinguishing the different years in the Figure, so that it is much more readable. The text has been revised in several parts. For instance: "Soil moisture dynamics, PAR and VPD time series are also compared in Figure 10" and "In the year 2003 after a typical January precipitation (Figure 1.a) and a wet February (DOY 32-59) the precipitation strongly decreased during the key months for the LAI growth in Sardinia (March, April and May, DOY 60-151) so that low LAI values were observed during the high irradiance period, and finally a very dry Summer occurred. During the Spring 2004 the best hydrologic conditions occurred for this site – high soil moisture until the end of the Spring season, when high values of PAR are observed – so that extremely high LAI values were predicted (solid line in Figure 10a). In 2005 the environmental conditions were not so favorable and less grass growth was predicted. Indeed, grass growth was limited before (DOY 85-120) due to atmospheric conditions (low PAR) and after due to the soil moisture"

13) Comment: 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.

Response: As suggested by the reviewer we include other factors on this analysis. The new Figure 11 includes a panel 'b', where the LAI is related to PAR for the highest precipitation values (> 30 mm). This new plot highlights the impact of the irradiance on LAI when vegetation growth is not limited by soil moisture, such as pointed out by the reviewer. For brevity, we don't plot VPD due to its minor impact on LAI in this ecosystem. The following text has been revised: "The scatter plot shows that the grass LAI is correlated with the total precipitation of 15 days before, and a threshold value close to $LAI=2$ may be identified for increasing precipitation values. For higher precipitation values soil moisture is not limiting vegetation growth, and the grass LAI variability is due to the control of other atmospheric factors. Indeed, irradiance (i.e. PAR) affects significantly LAI, as can be noted by Figure 11.b, where the mean 15-day values of LAI are plotted versus the mean 15-day values of PAR for the aggregated 15-day precipitation values greater than 30 mm"

14) Comment: 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

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"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.

Response: sorry, the text was not clear. We have since revised the text. However, the result of 99% was only for the period with observed data available without any gap filling. Now that the gap filling in the Figure 9.b is removed so that the text is more clear now. The new Table 5 also reports statistical index of model performance distinguishing calibration and validation periods.

15) Comment: 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.

Response: The discussion has been added. The following text has been added: "...Even with the extreme dry conditions the WV species didn't wilt and LAI was still high (> 3), showing moderate changes throughout the year. This is not surprising as the typical shrubs and trees of Mediterranean water-limited ecosystems are known to be highly tolerant to water content fluctuations and are usually slow to limit their water losses (Larcher, 1995). For instance, literature values indicate that the *Olea* can tolerate leaf water potentials as extreme as -2 to -3 MPa (Lo Gullo et al., 2003) and -6 MPa (Sakcali and Ozturk, 2004), and the types of *Quercus* in these regions can tolerate -2.5 to -6 MPa (Tognetti et al., 1998; Sakcali and Ozturk, 2004) without significant drop of leaf conductance, while minimal water potential values of these resistant shrubs may reach -8 MPa (Larcher, 1995)"

16) Comment: In the last paragraph on page 236, the authors conclude that climate

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

Response: We have removed that paragraph.

17) Comment: 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.

Response: we agree with the reviewer. We adjusted the specific leaf areas of the dead biomass, so that they are close to the specific leaf areas of the green biomass now.

18) Comment: 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.

Response: We thank the reviewer for the suggestion. With the objective of using physically based parameters (such as suggested by the reviewer in comment 2), we set the saturated soil moisture equal to the measured porosity (53%). Then the model is further slightly re-adjusted for obtaining the results of the previous model version.

Technical corrections

19) Comment: 220, 4: Should be "competing for water"

Response: thanks. Change made.

20) Comment: 220, 6-8: I would rephrase the sentence to: "An extensive field cam-

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paign (...) was performed with the objective ..."

Response: thanks. Change made.

21) Comment: 221, 2: Should be "climate variation, fires, etc."

Response: correction made.

22) Comment: 221, 21-24: This sentence needs rephrasing.

Response: the sentence has been rephrased.

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

Response: Sentence revised.

24) Comment: 223, 17: I believe "Dancus cerota" would be "Daucus carota".

Response: thanks. Correction made.

25) Comment: 226, 2: First use of the greek letter q". Please give a definition.

Response: Definition added.

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

Response: we respectfully disagree. We prefer to leave these rows here (in the section "field measurements"), because this part describes vegetation characteristics of the site and the LAI observations.

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

Response: sentence split and rephrased.

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

Response: correction made.

29) Comment: 232, 14-15: cg and cd should also be explained here.

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Response: Such as reported in the manuscript, "Model parameters are defined in Table 1" for reducing manuscript length.

30) Comment: 234, 3: Please explain what "rmse" means (root mean square error?).

Response: addition made.

31) Comment: 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.

Response: addition made.

32) Comment: 235, 6+13: Should be "could not" and "does not" instead of "couldn't" and "doesn't". Please check for similar occurrences.

Response: correction made.

33) Comment: 236, 2-3: Should either be "in these ecosystems" or "in this ecosystem".

Response: correction made.

34) Comment: 236, 18-20: This sentence needs rephrasing.

Response: correction made.

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

Response: addition made.

36) Comment: 238, 2: Should be "Finally, the effect of VPD on stomata opening was modeled following Jarvis (1976)";

Response: thanks, correction made.

37) Comment: 246: Please mention the averaging depth for the given soil moisture in

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the caption.

Response: addition made.

38) Comment: 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.

Response: The new Table 4 provides Statistical index of model performance. The ranges of panels c) and d) are the same of panels a) and b) for a better comparison, and for highlighting the different magnitude of each energy balance component.

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

Response: addition made. Panel (c) changed according to reviewer suggestion of comment 12.

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

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