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Biogeosciences Discussions

Interactive comment on "Topography induced spatial variations in diurnal cycles of assimilation and latent heat of Mediterranean forest" *by* C. van der Tol et al.

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General comment

The paper of Van der Tol et al. presents an issue of sure interest and actuality. At present, much of the data regarding interactions between forests and atmosphere are inferred from tower-based measures. Due to theorical limitations, actually we can count on reliable estimates based on eddy covariance technique concerning flat areas ecosystems, while a trustworthy technique was not yet reached to take into account advective fluxes that occur on sloping terrain. Unluckily, in Southern Europe, due to thousands of years of interactions with humans, forests are mostly limited on steep slopes, where soils are generally thinner and less water is available for plant growth. That was particularly evident in Slovenia, where the aridity due to the highly permeable calcareous bedrock, and the extensive practice of cattle grazing, led in the past to a wide destruction of forests and depletion of forest soils. At the time of the first world war, the Carso, a large plateau near the study area presented in this paper, appeared like a desert, and only in the last decades a dense forest cover, that can be mostly referred as Orno-ostryetum, was able to expand from the steep slopes and the dolines where some stunted plants were previously confined. The aim of the paper is to define the relative importance, on gas exchange of vegetation living on different slopes, of short-term effects local weather conditions induced by slope and by long-term effects on plants, adapted and acclimated to different conditions of exposure. To do this, the study uses measured data of gas exchange taken at leaf level, measures of sap flux density, isotope discrimination analysis, nitrogen content analysis and meteorological measures. The relative impact of short-term and long-term effects on vegetation is separated by a sensitivity analysis applied on the model, based on that of Leuning et al. (1995), used to simulate latent heat and carbon dioxide fluxes. As a result of the study, Authors infer that spatial modifications of vegetation characteristics are at least of equal importance of characteristics induced on local weather by the aspect of the slope. The paper is written clearly and fluently, with appropriate references. Figures are generally clear. A large amount of work was carried out for this study, including a field campaign, laboratory measures and a modelling effort. However, going into the details of the paper, it seems that conclusions are not supported by the presented data. This is due to a substantial deficiency in the statistical settings of the experiment. In a forest environment structure and composition and their interactions with microclimatic environment often have a larger influence on gas exchange and productivity than climate or slope alone. The two sites in which gas exchange measurements were taken had the same aspect (210°N), but age and stand structure were different. Critical is the scarcity of really independent data, in order to test the model used. For instance, what is shown as the result of a comparison between measured latent head

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and modeled latent heat, is actually a comparison between the outputs of two models: one is based on measures of sap flux density and a crude extrapolation to latent heat flux from a fixed amount of sapwood in the trees, the second has the same basis but uses the model derived from Leuning et al., (1995) for the extrapolations. Authors have not considered that differences found in the comparison among stands could be due to a combination of different variables: soil characteristics, vegetation height, age, composition, stand density, canopy structure, species composition, in addition to the considered constraints. A study like that of Van der Tol el al., is inherently difficult: it is vital to limit the variables to the contour and multiply independent samplings in order to refer correctly a result to a single variable.

Specific comments

-Title: I'm not sure that the term 'Topography' in the title (and in the text) is appropriate. It seems that this term in the text has many meanings: slopes and aspect, exposure, soil water availability, long-term local humidity and temperature. The use of names of cardinal points as names for the sites produces additional confusion.

-Page 1638: Authors assess that: 'Surface temperature is solved from energy balance'. In the energy balance soil heat fluxes and heat storage are neglected, therefore the equation is reduced to Rn=H+955;E. I cannot understand why - if the model of Leuning et al. needed the surface temperature - it was not measured, for instance with the same thermocouple wires used to build the Granier sensors. Alternatively, the radiative temperature of the canopy can be obtained from the measurements of a net radiometer with a separate channel for long wave radiation and the measure of the temperature of the sensor, applying the Stefen-Bolzman equation. It's really unclear how the net radiation without long wave radiation measures was estimated. Furthermore, neglecting heat storage and soil heat fluxes can lead to a considerable bias in the energy balance closure, varying with slopes and aspects.

-Page 1639: The aerodynamical conductance is calculated with the use of a wind function. The influence of stand structure is not taken into account as needed (Jarvis, 1986, Lindroth, 1993). A constant wind velocity was assumed, derived from measured values taken at only one measuring station. Effects that radiation intensity may have on convection were not considered.

-Page 1640: Four plots are mentioned, with names of cardinal points and it is stated that dominant species are Carpinus sp. and Quercus sp. Later on it is mentioned that photosynthesis was measured (without clarifying the number of sampled individuals) only on the plots South and East (sites with the same aspect, 210°N). It seems therefore that a relation with photosynthesis was built from these two plots only and then extrapolated to the other two sites by using nothing but the relation Photosynthesis/Nitrogen leaf content. Since this relation (Figure 6) seems to be based on two data only and since Vcm is essential in the model, the whole procedure doesn't seem solid. It would be preferable to show all the measured data to understand if the relations found for the two sampled species are similar, and possibly to perform a statistical analysis.

-Page1640: Sites with high vapor pressure deficit are distinguished. It would be interesting to know the measured values, the differences among sites and the instruments used to measure temperature and humidity.

-Page1640: Juniperus communis is a conifer. It has thracheids, not vessels. Only the more evoluted angiosperms have vessels, in some species forming a porous ring, in some others distributed in a diffuse porous ring.

-Page1640: Quercus is a genus, not a species

-Page1641: The idea is expressed that biochemical parameters are constant without water stress. That's absolutely wrong: plants modify nearly all the biochemical parameters throughout the day. For instance, the electron transport capacity varies throughout the day according to environmental constraints, and this is particularly evident in the relation with solar radiation and temperature.

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-Page1642: It's stated that leaf samples were collected from places predominantly sunlit and predominantly shaded. However, the selective criterion, used also in the model of Leuning et al. to define sunlit and shade leaves is not clear. Plants, although with different structural forms and different degrees of adaptation to the light environment, develop the largest part of the leaves in an intermediate condition of light, where full direct radiation, penumbral effects and diffuse radiation are combined.

- Page 1643: Authors states "maximum carboxylation capacity and electron transport capacity for each plot were calibrated from the leaf chamber photosynthesis measurements". The definition of the term "calibrated" is not clear, probably they mean fitted.

-Page 1644-1645. There is a single assumption that alone could weaken the whole paper. The sapwood area has a clear relation with the leaf area held by the plant; the ratio between these values is defined Huber value. The sapwood area and the same Huber value change in the plant as a function of the height in the plant and environmental conditions. The relation between stem cross sectional area (x) and sapwood area (y) was effectively modelled by Medhurst Beadle, 2003. They fitted that relation according to the hyperbolic function y=Ax/(A+x)+B, where A was 0.019 and B was 0.133 in a Eucalyptus nitens plantation. It can be expected that these coefficient values vary according to species, age, and height in the crown and probably with the aspect of the slope. Although some variables can have opposite effects, the assumption of a sapwood area of a fixed width of 2.5 cm is not acceptable. A direct measure of sapwood area along different degree of slope and aspect, combined with the sap flow density measure, could be in itself an interesting issue for publication.

-Page 1645. It is stated that the model of light transmission was tested with PAR sensors along horizontal as well as vertical transect on the southern slope, only horizontal in the northern slope. To quantify the intercepted radiation with single sensors of PAR is an obsolete method. The measurement of the wide spatial and temporal variability of light transmission in a forest canopy, and its variation throughout the day and the year need specific instruments. In the last few decades ceptometers were developed, that held several radiation sensors along a boom in order to integrate the measures along a fixed length. The 'plant canopy analyzer' that can record many parameters using five optical sensors is more than ten years old. The recent developments of digital photography, combined with dedicated software, actually allow synthetic measures of transmitted direct and diffuse radiation along the year. It is needed to make photographs toward the zenith, with a hemispherical objective. Azimuth has to be recorded as well. From the analysis of the photographs, radiation values collected in the open field and commercially available software, can be modelled, with a good approximation, the amount of direct and diffuse radiation transmitted during the day.

-Page 1650. At the beginning of the discussion session is expressed a concept on which worth to discuss. It's stated that:' The agreement between modelled and measured fluxes depends on the accuracy of four components: (1) measurements used for validation, (2) input variables, (3) parameter values and (4) the model description itself.' In this statement is not considered the importance of the statistical settings of the experimental design. A model increases its predictive value as a function of collected samples and their distribution.

-Page 1651. It is stated that both temperature and humidity decreased after having placed the sensors in a position sited at a higher height from the ground. This is true for humidity, but the displacement of temperature sensors to a higher height from the ground generally induces a narrower thermal excursion, while the decrease in average temperature (roughly 0.03°C m-1) is hardly perceived by vegetation.

-Figure 4. It's surprising that Ostrya carpinifolia, species typical of the consociation to which nearly all the other mentioned species belong, was not recorded. However, since gas exchange measures were taken on Quercus spp. and Fraxinus ornus only on South or Southwest exposed sites, how can the extrapolation to the northern slope be justified, where gen. Carpinus prevails, with a very different height and structure?

-Figure 6. It's well established that Nitrogen content, in a vertical profile in the canopy, varies little in terms of dry matter (gN gDM-1), but, since specific area density of leaves is definitely related to radiation, plants show an increasing nitrogen content per unit of surface at increasing height in the crown. Nitrogen content could be expressed also as gN m-2 of leaf area. In the picture the unit in which nitrogen content is expressed is not clear, maybe because of a typographical error.

-Figure 7. The names chosen for the sites here, that recall an aspect, are probably misleading. The plot 'East' is actually exposed to 210°N, between south and south-west. -The Greek letter Lambda as capital was referred to in the text as a measure of water use efficiency, here as latent heat.

-Figure 11: 'Difference between surface temperature...' It's not clear if canopy temperature, soil temperature or radiative temperature have been taken into account.

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