Biogeosciences Discuss., 6, C6–C10, 2009
www.biogeosciences-discuss.net/6/C6/2009/

Biogeosciences Discussions

Interactive comment on "Improved understanding of drought controls on seasonal variation in Mediterranean forest canopy CO_2 and water fluxes through combined in situ measurements and ecosystem modelling" *by* T. Keenan et al.

Anonymous Referee #1

Received and published: 8 March 2009

The paper examines effects of soil moisture deficit on canopy carbon and water fluxes, looking specifically at processes beyond stomata control. It is generally well written and has clearly stated objectives. But before the paper can be accepted for publication a number of issues must be addressed and further clarifications are required

What concerns me most about the work is its focus on effects of soil moisture deficit on stomata conductance and photosynthetic capacity - and then to base this effort on a simulated soil water balance with no attempt of its evaluation. Moreover, calculation of the water balance is seemingly simplified using a number of assumptions regarding

runoff and drainage. It is unclear whether/how e.g., rooting depth and the total volume of soil exploited by vegetation is considered (Table 1 suggests maybe yes, but the text certainly does not allow to judge how the soil water balance calculations were done exactly – please clarify). To my knowledge, the study sites all have soil water measurements although perhaps not on a continuous basis. Even if these data were not easily accessible via the Fluxnet web interface: what prevented you from either (a) digging into the published papers from these sites or (b) approaching the site PIs about availability of soil moisture data? For four locations this surely wouldn't have been an undue effort.

You may, of course, retort that measurements of soil water are not necessarily representative for the entire flux site because of the well-known spatial heterogeneity that is a problem for all below-ground measurements (soil moisture, heat flux, etc). There is a point to be made here, and I do not wish to say that your modelled soil moisture values shouldn't be used. But evaluation against measurements will demonstrate whether you capture seasonality, degree and speed of drying/rewetting – and this can normally be done even with data from a limited number of soil sensors. Without demonstrating that the modelled soil moisture patterns are reasonable the overall analysis is weak.

The analysis seeks to demonstrate that reduced conductance cannot fully explain effects of soil moisture deficit on canopy assimilation; it is required to introduce an additionally reduced photosynthetic capacity into the models. Can you comment how this goes together with the notion of stomata optimising carbon gain at a certain water loss? You seem to suggest that carbon gain is actually reduced further than suggested by stomata closure alone? Wouldn't that mean that the plant actually maintains the stomata more open than necessary, and the optimum theory wouldn't hold? Maybe I am missing the point here, but further discussion along these lines would be useful.

page 2288/line 6, "Recent studies have suggested..." It is certainly true that often stomata closure is being looked at the chief cause of reduced photosynthesis under drought conditions. However, here (as in some other places in the text) the authors seem to forget that effects of severe soil water deficit e.g., on Rubisco activity have been intensively studied for decades. They also have been addressed in previous (often plant-based or site-specific) modelling studies, although it is true that only a small number of studies that are applicable over larger regions have focussed on this issue.

Introduction/general: Soil water deficit may indeed become an increasingly frequent feature of Mediterranean ecosystems in future. However, vegetation response via reduced photosysnthesis and transpiration is only one aspect of the overall ecosystem response; eventually, existing vegetation types will be replaced by other growth forms, including desertification being discussed for some areas (also, vegetation changes can be amplified by transiently enhanced uncontrolled fires).

Page 2291/canopy conductance: the assumption of negligible soil evaporation is only valid when soils are dry. Soil evaporation is driven by radiation with the slope of the relationship becoming very low when the top soil layer dries. Therefore, assuming surface conductance being equal to canopy conductance is not always true when the canopy is dry but the soil sufficiently well watered. Also, the difference between forest canopy transpiration and conductance, transpiration & conductance of the entire vegetation (including understorey), and total evapotranspiration and surface conductance it is not clearly made in the manuscript (there is some vague mentioning of understorey vegetation in the discussion later-on). This must be addressed at least in form of discussion/uncertainties.

Page 2293, line 12: eddy flux measurements do not observe net photosynthesis. Presumably you used the gap-filled & portioned fluxe time series that are available as Fluxnet level 4 data. In seasonally dry ecosystems it is crucial to account for effects of soil water deficit and of rewetting on soil respiration. If that is not done well, the derived canopy assimilation rate will be wrong. Have you uncritically taken the provided Fluxnet GPP data (derived from standardised methods)? Have you checked whether the seasonality of respiration responds to temperature as well as soil water? I don't

want to imply that the Fluxnet data is wrong (and the standardising methods to my knowledge certainly DO have a soil water dependency of respiration), but it would still be advisable to cross-check the downloaded data (e.g., visually, by plotting time series of respiration together with meteorology; or by comparing to original papers from the sites, etc) before using it for your modelling purposes. And since only four sites are involved this could have been done easily.

What kind of photosynthesis parameterisations were done in GOTILWA and ORCHIDEE to be applied at the sites? Did you have to specify values of Jmax, Vmax for leaves or canopy? If so, how were these derived? How did you determine values of other site-level variables (page 2298; e.g., growth/maintenance respiration, allocation patterns, fractional cover of a PFT)? From published literature?

Page 2300: reference should be to fig. 3a and 3b (not 2a and 2b).

Fig 3 and related analysis: I may have overlooked something: in the Figure it looks like m and Gs0 were determined on a half-hourly (or hourly) basis, no? What is the rationale of doing so? I would expect changes in slope m, or offset Gs0 in response to changes in soil water deficit to be visible on a day-to-day basis (rather from one half hour to the next).

Fig. 4: how was An normalised? And can you be more specific as to why you have chosen An only within a certain range of Ci/Ca (ca. 0.6 to 0.8)? Presumably, you wanted to select periods when you could be sure that stomatal limitation would be small (and therefore other limitations are visible)? During periods of severe soil water deficit I would have expected that a Ci/Ca of 0.6 and above is only found in the morning hours, when vpd is still very low, is that so? In Figure 1 it would be helpful, to include calculated diurnal course of Gc in addition to the shown An and Ea.

Fig 5: show the range of golden days values of An and Ea from the measurements; can you also indicate st.dev of the model values?

Table 3: I am not very well versed in the use of MEF. But in many cases, values appear to be negative (which indicates relatively poor model-observation match) even in cases when r2 are quite good. In the text overall you do not discuss MEF a lot; but if MEF is indeed more sensitive than r2, I would have liked to see the results on these values being used in more detail.

In the end it was not clear to me why two models have been compared, what was the exact purpose of this comparison, and why were these two models chosen. This could be made clearer – For instance, is there a generally better performance to be expected from models that represent tree cohorts (like GOTILWA), and if so, why? Obviously, one advantage of Orchidee is the extrapolation to larger regions but other dynamic vegetation models that are based on forest gap dynamics can do as well. Is GOTILWA applicable for larger regions?

Interactive comment on Biogeosciences Discuss., 6, 2285, 2009.