**Biogeosciences
Discussions**

Interactive
Comment

Interactive comment on "Effect of mosaic representation of vegetation in land surface schemes on simulated energy and carbon balances" *by* R. Li and V. K. Arora

Anonymous Referee #2

Received and published: 1 September 2011

General: The authors raise some interesting points about uncertainty in modelled terrestrial C balances caused by different representations of ecosystem heterogeneity. However they need to check their model results more carefully against observations, particularly those for crops and grasses, before publication, because in some cases these results are quite unrealistic as noted below.

Introduction Composite approaches can also involve separate calculations of energy and water balances for each PFT competing for irradiance and water in a common environment. This should be clearly stated in the Introduction.

Methods What are the 9 PFTs in CTEM, and how were they allocated to each of the 4

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

sites in the study? These are given in Sec. 4.1 as it turns out, but I was looking for them in the Methods. What are the key attributes of each PFT, and what were their individual and aggregated values? How long were the spinup runs? Are CO2 and energy fluxes to be tested against any measurements?

Results Sec. 4.1. Fig. 2: Why are the mid-season Rn fluxes so much lower for boreal grasses than conifers? I take the point about the different albedos, especially with snow cover in winter. However the eddy covariance measurements I have seen indicate a reduction of only ∼5%, possibly because the effect of the lower albedo on SW exchange is partially offset by the effect of greater warming on LW exchange. The reasons for the 30% reduction in Rn modelled here need to be better explained. Similarly, the large reduction in LE modelled for boreal grasses vs. conifers is inconsistent with EC measurements which indicate that as long as the soil is moist, grasses can transpire rapidly (LE up to 400 W m-2), while conifers maintain a stable but relatively low LE (max. ∼200 W m-2) due to their hydraulic architecture. These differences need to be better justified, preferably with comparison against EC measurements for the modelled region. Fig. 3: Why does grass SWC rise earlier in spring than needleleaf SWC? The LAIs in Fig. 5b are both essentially zero during this period. Do wood surfaces also intercept radiation? Fig. 4: The NPP for conifers looks OK, but that for grass is much too low, unless during a drought. Note that most grass NPP is below ground. Are these differences in the mosaic vs. composite NPPs just caused by those in Rn? These don't seem enough to cause such large differences, such as a tripling of grass NPP. What about checks against some measured values? The reasons for the different NPPs in each approach need to be explained. Part of the problem with grass NPP may be the time course of its LAI, which in the field attains peak values in mid-July and declines thereafter, not as modelled in Fig. 4b. CTEM grass phenology needs to be revised. The seasonal variation in conifer LAI appears to be too large. Again, comparison with measurements would quickly reveal these inconsistencies. Are decomposition rates really lower in grasslands than in woody ecosystems? One of the reasons for the accumulation of SOC in grassland soils is their relatively large below-ground NPP and

hence litterfall, rather than low decomposition rates. Fig. 5: Are the differences in Rn between mosaic and composite approaches for the two boreal sites just due to the seasonal differences in albedo between deciduous and evergreen needleleaf trees? If so, this should be more clearly stated. If not, then the cause of these differences should be clarified. How can grass NPP in Fig. 5a rise in spring before grass LAI in Fig. 5b?

Sec. 4.2 Fig. 6: The time course of crop LAI is completely unrealistic, as noted earlier for grasses, so that NPP is much too low, certainly for the mosaic approach. These are determinate PFTs that grow their leaves early in the season, and then their reproductive organs later so that LAI rises until anthesis, and later declines during grain fill. I really don't see how the comparatively small differences in tree vs. crop Rn could drive these large differences in NPP. All the EC data I have seen shows that net CO2 exchange is fairly insensitive to Rn, partially because of the different PAR fractions in direct vs. diffuse components. The explanation given in the paper is just not plausible.

Sec. 4.3 Is the seasonality of evergreen vs deciduous albedo affected by that of LAI? Note that the seasonality of NPP in these tropical forests is strongly affected by rooting depth and hence water uptake during the dry season. This seasonality looks very pronounced for the latitude of the site. What was the rooting depth used here, 4.1 m? Can NPP be checked against any measurements?

Discussion Table 4: Peak values of LAI in the model may compare with measurements, but the seasonality, particularly of grasses, does not. Values of phytomass in the model must be compared with those measured at a similar forest age, otherwise the comparison does not mean very much. Also, comparisons of phytomass are not a substitute for those of NPP.

The authors might try to quantify uncertainties in modelled C balances between mosaic and composite approaches at regional or even continental scales. What does this uncertainty mean for global terrestrial C exchange?

Interactive comment on Biogeosciences Discuss., 8, 5849, 2011.