Biogeosciences Discuss., 10, C6805–C6808, 2013
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Biogeosciences Discussions
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Interactive comment on "Sub-grid scale representation of vegetation in global land surface schemes: implications for estimation of the terrestrial carbon sink" *by* J. R. Melton and V. K. Arora

Anonymous Referee #1

Received and published: 24 November 2013

I thank the authors for their response and look forward to reading the new version of their paper. Although we do not fully agree on all points, the authors have correctly addressed most of the requested modifications. I would however like to come back on two specific points.

FIRST POINT

***** 2.2 What is the fate of crops biomass? Normally crops should be harvested each year (otherwise, soil carbon could potentially build up to unrealistic high values). I assume that this is the case in CLASS-CTEM, right? If not, why? If yes, what hap-

pens with the crops biomass carbon (is it sent to the atmosphere immediately after the harvest)? These points should be addressed at the end of Section 2.1.

AU»During harvest, the crop biomass is transferred to the litter pool. Crops are harvested every year. We have added in more information about the treatment of crops due to harvesting. «AU *****

Then I recommend that the authors avoid the word “harvest”, which may lead various readers into thinking that some biomass carbon is directly taken out of the grid cell (to then be transferred, progressively or instantaneously, to the atmosphere). The authors should also specify that transferring 100% of crop biomass to litter is a modelling decision that overestimates the actual amount of litter input. Most readers will understand that DGVM must resort to such simplifications.

SECOND POINT

***** 2.8 Is the higher productivity of crops, compared to the natural vegetation they replace (page 16021, line 4 and page 16024, line 23), credible? This appears to contradict textbooks values, particularly for tropical and temperate forests (e.g., Tables 6.3 and 6.6 of Chapin et al., 2002). The authors need to discuss this point.

AU» Table 6.4 in Chapin et al. (2002) (assumedly the reviewer intended this table, not 6.3) shows the aboveground productivity of crops to be more than double that of a temperate grassland (530 g/m2/yr vs. 250 g/m2/yr), while the below-ground productivity of the grasslands (500 g/m2/yr) is much higher than the crops (80 g/m2/yr). It is unclear how the values in Chapin et al (2002) are derived (as there is no information given) so difficult to parse how relevant these figures are to our simulation outputs. In CLASS-CTEM, the greater productivity of crops is primarily a reflection of their enhanced Vcmax values, the C3 crops in particular. Our Vcmax value for C3 crops is on the upper end of other models (Rogers, 2013) but actually below the mean value from a compendium of measurements as gathered by Kattge et al. (2009). «AU *****

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Although our table numbers do differ (mine is really 6.3), our values do agree: total NPP for temperate grasslands is higher than for crops (750 vs. 610 g/m2/yr), which remains as an issue. The main issue with CLASS-CTEM simulated LUC, however, is rather when croplands replace temperate and tropical forests, for which NPP is much higher (1,550 and 2,500 g/m2/yr, respectively). A higher NPP for croplands than for the forests they replace is contrary to "common knowledge" (not only the values in Chapin et al. (2002), which by the way come from Saugier et al. (2001)). The authors should therefore, in the paper itself: 1) explain why this "common knowledge" is in fact inaccurate (i.e., crops do have a higher NPP than the forests and grasslands they have replaced, both in temperate and tropical regions); or 2) acknowledge this bias in the DGVM and highlight its possible role in overestimating soil-litter carbon following LUC, particularly under the mosaic approach.

FINAL REMARK

I come back on the previous two points mostly because I think they are key factors in the explanation of the strange impact of LUC under the mosaic approach (i.e., LUC causes soil-litter carbon to increase). The overestimation of crop NPP, the complete transfer of crop biomass to litter, and the lack of simulated degradation of the soil carbon pool (==> my former comment 2.4, along with the authors response) all combine to overestimate the amount of soil-litter input. Now, all these factors affect both the composite and mosaic approach, so why do they have a much stronger impact in the latter case? I do not know, but I would start by looking at soil respiration because the related parameter (Table A1) is much higher for crops than natural vegetation. Increased soil respiration may compensate the overestimation of soil-litter input to a stronger extent for the composite than for the mosaic approach, due to the differences in soil temperature and moisture between the two approaches. I hope that the authors will find the time to undertake a new study to shed more light on the intriguing issue they have uncovered in the current study.

Saugier et al. (2001). Estimations of global terrestrial productivity: Converging toward a

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single number? In Terrestrial Global Productivity, edited by Roy, Saugier, and Mooney.

Interactive comment on Biogeosciences Discuss., 10, 16003, 2013.