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Biogeosciences Discussions

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

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1. OVERVIEW

The authors investigate the differences between two approaches used to represent sub-grid cell vegetation heterogeneity in dynamic global vegetation models (DGVM), using CLASS-CTEM. For pre-industrial equilibrium simulations and for transient simulations that do not consider land use change (LUC), the two approaches give similar global-scale results, despite substantial differences in various regions. However, when LUC is included, the two approaches give very different results for the atmosphere-to-land global carbon flux over the 1959-2005 period.

The three main strengths of the manuscript are the comparisons of CLASS-CTEM results with many other estimates (Table 2, Fig. 2, and Fig. 4), the in-depth explanations for a given grid cell in Russia, and the overall presentation quality (especially the Figures). The main weakness is the lack of sufficient discussion of what the outcomes mean for modellers that do not use CLASS-CTEM (see point 2.1). I consider that the manuscript fits well within the scope of Biogeosciences and suggest that it be accepted for publication, provided that the authors address the comments below.

2. SPECIFIC COMMENTS

2.1 The relevance of the study for modellers that do not use CLASS-CTEM is not obvious, for two different reasons. In order to improve the manuscript, I suggest (but do not request) that the authors address this shortcoming.

2.1.1 Besides CLASS-CTEM, the authors do not provide any example of a modern DGVM that resort to the mosaic approach as defined in the manuscript. As for the two examples of modern DGVM (besides CLASS-CTEM) using the composite approach as defined in the manuscript, I would rather argue that they resort to the mixed approach. In LPJ-DGVM, "the grid cell is treated as a mosaic divided into fractional coverages of PFTs", for which "the physical environment is well mixed", e.g., they share the same soil moisture (quotes are from Sitch et al., 2003). In CLM 4.0, all "the fluxes to and from the surface are defined at the PFT level, as are the vegetation state variables", but the PFTs share a single soil column (quotes are from Oleson et al., 2010; see Section 1.1.2). The few other DGVM I know also resort to a mixed approach. How should these modellers interpret the results of the study? Should the mixed-approach DGVM be considered closer to the mosaic type (because PFT-specific fluxes are averaged over the grid cell) or the composite type (because the soil is the same for all the PFTs)?

2.1.2 Regardless of point 2.1.1, how should the users of other models react to the study? To my knowledge, not many models (if any) besides CLASS-CTEM offer the possibility to choose between different (composite, mosaic, or mixed) approaches. If

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the study had concluded that one of the two approaches tested was clearly better, then users of other models could reflect upon the appropriateness of switching to this better approach. (Or maybe that the best approach is actually a mixed one, not tested in the study?) I understand that the authors do not have sufficient evidence to take a clear position, but additional discussion of the merits of at least the two approaches tested would improve the manuscript. Here are a few points to consider, at the authors will. (a) The two approaches are briefly discussed in the Introduction (end of page 16006); this text could be moved to the discussion and expanded, with references to support the claims that are made. (b) If each approach is better suited to specific vegetation types, does it mean that DGVM should change their approach from one grid cell to another? And maybe even through time in the same grid cell, if the vegetation type changes? If yes, based upon which criteria? (c) From an ecological perspective, do composite structural and physiological attributes make sense? Plants function as individual units... (d) See point 2.7 below.

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 happens 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.

2.3 In Table 2, results from pre-industrial equilibrium simulations are compared with contemporary estimates, which is not very informative given all the changes (LUC, climate, CO2 concentration, etc.) that have occurred since 1861. Replace the "other estimates" by pre-industrial values; for example, some values are provided in Table 2 of Arora and Boer (2010).

2.4 At first, the comparison of CLASS-CTEM results with the Houghton et al. (2012) estimate casts doubt on the capacity of CLASS-CTEM to simulate LUC impacts (end of page 16016 and Fig. 4b). Moving here some of the text from the Discussion (page 16020, lines 14+), or at least stating clearly that changes in pasture area were not sim-

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ulated, could prove helpful. Urbanization (page 16020, line 18) is probably irrelevant, because global LUC datasets apparently do not account for it neither (Houghton et al., 2012; Section 5.2). I would however suggest discussing the role of soil carbon (see the end of Section 3.2.3 in Houghton et al., 2012): bookkeeping approaches apparently assume high soil carbon losses (likely included in the Houghton et al. (2012) data), whereas the results for CLASS-CTEM in Fig. 4b are for vegetation only.

2.5 I appreciate the effort of the authors to better explain their results through the use of the H index. However, I have three issues with the use of the H index.

2.5.1 The authors ask us to compare Figs. 6 and 3 to see the association between the H index and the differences between the two approaches. It is obvious that desertic regions have low H and small absolute differences between the approaches. But the association between high H and high negative values (higher results for the composite approach) appears clearly to me only for south-eastern China and a few pixels in Mexico. For the rest of the world, I do not see much because the results are too variable over space... It is even worse for Fig. 3 vs. Fig. 5a, in particular for the "US Prairies" (which are not clearly identified) example. If the authors want to convince readers, I suggest (but do not request) that they compute a global-scale value for some association indicator between the two elements.

2.5.2 Either I do not understand the definition of the terms in Equation (5), either the claim that the H index takes "a value of 0 if an entire grid cell is occupied with only a single PFT" is inaccurate. Please look at the mathematical exercise in supplement and respond accordingly.

2.5.3 Regardless of point 2.5.2, I am not sure whether the H index is relevant for grid cells that are mostly covered by bare ground. What would be the value of the H index in a grid cell that is 90% bare, but has the remaining 10% equally divided among the 9 PTFs? I have a sense that the H index would be high. Yet having a high H index in such a case would be a poor indicator of possible important absolute differences

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in LUC impacts between the composite and mixed approaches, because there is very little vegetation in the first place. Please address this minor point in your response only, not in the manuscript itself.

2.6 Once again, I appreciate the effort of the authors regarding the R_C index in order to better understand the results of Fig. 5b. But we now must look at three Figures (Figs. 5b, 6, and 7) simultaneously and, honestly, I almost do not manage to see anything out of it. Once again, I suggest (but do not request) that the authors provide a formal global-scale association indicator.

2.7 Are the results from the mosaic approach credible under LUC? The first element of doubt is the following. Based on Fig. 4c, the impacts of LUC when only climate change is accounted for are 14.3 PgC (4.1 minus -10.2) for the composite approach and 7.6 PgC (0.0 minus -7.6) for the mosaic approach. When CO2 is also accounted for, the impacts of LUC increase to 21.4 PgC for the composite approach, but *decrease* to 1.2 PgC for the mosaic approach (which is counter-intuitive, because CO2 fertilization should lead to more LUC-caused emissions). The second element of doubt is the following. For the specific grid cell analyzed, we see that the amount of cropland basically doubles from 30 to 60% between 1860 and 1940 (Fig. 8a). Yet during this time the amount of soil carbon slightly increases for the mosaic approach (Fig. 8e). This is counter-intuitive, because 1) conversion to cropland is believed to cause important losses of soil carbon (Houghton et al., 2012) and 2) the CLASS-CTEM parameter for soil respiration is much higher for crops than for natural vegetation (Table A1). I suggest (but do not request) that the authors consider discussing these observations, which are also related to points 2.8 and 2.9.

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.

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2.9 Could the lower LUC emissions under the mosaic approach result mostly from the build up of soil carbon following LUC, resulting itself from the cooler soil temperature of the new cropland (due to higher albedo?). This hypothesis is coherent with Fig. 8 and the related explanations (page 16022, lines 15+), but does it apply to the majority of LUC-affected grid cells or just to this single grid cell? In particular, what is the global impact of LUC on the soil carbon pool for each approach, both with and without CO2 fertilization? Please have a look at this hypothesis and respond accordingly.

3. TECHNICAL CORRECTIONS

3.1 Page 16008, line 20. Is the vegetation necessarily over snow? Or, in the case of crops and grasses, can the vegetation be buried by snow?

3.2 Page 16010, line 10. For more information on NBP versus NEP, the authors could refer readers to Chapin et al. (2006).

3.3 Page 16011, lines 17+. To clarify, modify the text to: “As in McGuire et al. (2001) and Arora and Boer (2010), we diagnose {E_LUC} according to [...]”.

3.4 Page 16011, last line. Explain how the “total 6 h precipitation amount was used to determine the number of wet half-hour timesteps”.

3.5 Page 16013, line 17. Although fire is not modelled explicitly, don’t these results indirectly include the impact of *some* biomass burning, i.e., the deforestation (permanent) fires that are responsible for a part of LUC? Please clarify accordingly.

3.6 Page 16016, lines 23+. The deforested biomass correspond to the change in H_V (or L) only, as explained in page 16010 around line 20, right? A reminder might prove useful.

3.7 Page 16016, line 28. The authors should briefly explain how they derived the 68.8 PgC value over the 1959-2005 period from Houghton et al. (2012). Unless I am mistaken, this value does not appear clearly in the Houghton et al. (2012) paper.

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3.8 Page 16020, line 19. The reference should rather be Ramankutty et al. (2007). The authors should specify that the results of that study are for Amazonia only. If the authors rather refer to the range of results in Fig. 1 of Ramankutty et al. (2007), then the authors should cite the original studies.

3.9 Table A1. In the third column: replace "co-efficient" by "coefficient"; if the coefficient is really unitless write it explicitly, otherwise provide the units. In the fourth and fifth columns, the units are incomplete: there must be a time dimension (per year?) associated with these rates.

3.10 Fig. 4a). In the Figure itself, the reference should be Le Quéré et al. (2013).

3.11 Fig 4c). In the Figure itself, put a minus sign in front of the two negative results.

3.12 Fig. 4, in the legend. For (a), it should be \tilde{F}_Ln instead of \tilde{F}_L (as stated on page 16016, line 6). For (c), please specify that the results are for \tilde{F}_L.

3.13 Fig. 8a). In the Figure itself, the legend line for "Broadleaf evergreen" goes through the text. I would also recommend a different choice of colour, in order to clearly highlight C3 crops (put in red?).

4. NEW REFERENCES

Chapin et al. (2006). Reconciling Carbon-cycle Concepts, Terminology, and Methods. Ecosystems 9, 1041-1050.

Chapin et al. (2002). Principles of Terrestrial Ecosystem Ecology. Springer.

Ramankutty et al. (2007). Challenges to estimating carbon emissions from tropical deforestation. Global Change Biology 13, 51-66.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C5954/2013/bgd-10-C5954-2013-supplement.pdf

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