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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Pairing FLUXNET Sites to Validate Model Representations of Land Use/Land Cover Change" *by* Liang Chen et al.

Anonymous Referee #2

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This manuscript by Chen et al. "Pairing FLUXNET sites to validate model representations of land use/land cover change" aims at evaluating the performance of CLM and Noah-MP LSMs in simulating the impacts of LULCC on surface energy balance. Authors rely on observations from paired FLUXNET sites for model validation. The manuscript contains new and significant research, especially efforts to utilize the FLUXNET observation in a paired scheme for LULCC analysis. Also, the choice of the LSMs are very well justified and results could potentially help inform future model improvement. Writing, especially methods and results, could be improved by adding sufficient details for an unfamiliar audience. In its current form the manuscript is very hard to follow, especially, if the reader is not familiar with all the LSM lingo. Also, excessive referring of key information by pointing the readers to tables does not help either.

To an extent figures should be self-explanatory, especially when not restricted by page limit. I am not a big fan of figure caption stating: "same as figure x", this caption is no better than a figure w/o caption.

Major comments:

1) Provide some details on how point-scale models were implemented as this directly relates to foot print at which FLUXNET towers and model operate. Based on the limited information, it is hard to make sense of the differences between model and observation shown in Figure 2. FLUXNET towers are typically have bigger footprint, in some stances > 1 km, that may vary between open and closed canopies. Were these differences in spatial scale between model and observation accounted? Figure 2c suggests otherwise and diverging patterns could be driven by the scale. See Desjardins et al., 1992; Baker et al. 2003, and Griebel et al., for details. 2) The inclusion of CLM-PFT and CLM-PFTCOL with CRUNCEP forcing makes no sense to me as you cannot directly compare the diurnal energy fluxes with other simulations and attribute the differences to LULCC. For direct comparison, all model simulations should be forced with similar climate forcings. At least, I will not try to use these simulations to explore mechanism as shown in Figure 3 and discussed between Line 231:248. 3) I do not see the point of including the FLUXNET data with energy balance closure correction when it is not being discussed after Figure 2. This only makes the figures crowded and confusing. Suggest comparing the uncorrected and corrected observations in the beginning, or may in the supplemental, and then using one of the two as a reference for further comparisons with model simulations [which you have already done for some figures]. 4) Considering the large difference in LE between some of the paired sites (in particular 3, 7, 12, and 15) I would suggest setting a threshold for inclusion. These differences in LE and H within paired sites are comparable to the corresponding changes under deforestation and cannot be overlooked. 5) As of now the analysis is mostly focused on validation with very little emphasis on the sources of over- and under-estimation in energy fluxes. The discussion section is very speculative and mostly hand waving.

Authors should put more emphasis on mechanistic model diagnosis that goes beyond forcing.

Minor Points:

L35: what do you mean by deficiencies over forest land-cover type? L58 which were associated? L130-133: I do not think this statement is supported by data, at least for some sites. L165-166 do PFT in CLM are the same as the land cover reported for FLUXNET sites? Figure 1: source of land cover? Figure 2: label each panel with “a”, “b”, and “c”. Also, in caption Table reference is missing. Note that the difference is calculated as closed-open canopy? Figures 5-10: DO NOT USE SAME AS. It is very difficult to flip pages back and forth in order to understand the figure. Figures 11 and 13 are very difficult to follow. Not sure what you mean observations or model also the arrows showing LC conversion. Also, instead of 1-7, why not directly label using actual simulation type?

Desjardins, R. L., Hart, R. L., MacPherson, J. I., Schuepp, P. H., & Verma, S. B. (1992). AircraftâĂŘand towerâĂŘbased fluxes of carbon dioxide, latent, and sensible heat.ÂăJournal of Geophysical Research: Atmospheres,Âă97(D17), 18477-18485. Baker, I., Denning, A. S., Hanan, N., Prihodko, L., Uliasz, M., Vidale, P. L., ... & Bakwin, P. (2003). Simulated and observed fluxes of sensible and latent heat and CO2 at the WLEFâĂŘTV tower using SiB2. 5.ÂăGlobal Change Biology,Âă9(9), 1262-1277. Griebel, A., Bennett, L. T., Metzen, D., Cleverly, J., Burba, G., & Arndt, S. K. (2016). Effects of inhomogeneities within the flux footprint on the interpretation of seasonal, annual, and interannual ecosystem carbon exchange.ÂăAgricultural and Forest Meteorology,Âă221, 50-60.

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