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Earth System Dynamics Discussions

Interactive comment on "Disequilibrium of terrestrial ecosystem CO_2 budget caused by disturbance-induced emissions and non-CO_2 carbon export flows: a global model assessment" *by* Akihiko Ito

Anonymous Referee #1

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General comments: Ito presents an interesting and comprehensive study on the net effects of Minor Carbon Flows (MCFs) on the regional and global carbon (C) balances. Although this study is highly important for research on the global carbon (C) cycle and climate change, many uncertainties remain unaddressed. Uncertainties play an important role in this study because the individual effects of the MCFs are much smaller compared to the GPP and respiration fluxes. I believe that sensitivity and ensemble simulations are not enough to address the various large uncertainties related to the methods and models that quantify the MCFs. I would urge for a more detailed compar-

ison of the results to existing observations and to other studies that have addressed certain MCFs in more detail in the past.

Major comments:

1) As indicated in this study, many uncertainties remain in the simulation of MCFs. There have been several studies in the past that have focused on individual MCFs in great detail and tried to address these uncertainties. This study, however, simulates the MCFs in a much simpler way. Although this is understandable, the question that remains is: Does the combination of MCFs in a single framework lead to new insights in the global C cycle, or does it pose more uncertainties and might even lead to the misinterpretation of their net effect? I feel that this issue has not been fully addressed by Ito.

2) Secondly, it remains unclear to me if including the MCFs leads to a better or worse representation of net C fluxes between land and atmosphere and the C stored in biomass and soil. This study indicated that the VISIT model has been validated with various datasets at field, regional and global scales on ecosystem CO2 exchange. I am curious how the inclusion of MCFs changes the performance of the model. For example, do we see a better spatial variability in biomass and soil C stocks after including MCFs?

3) The various uncertainties in parameter estimation of the MCFs such as for the POC and agriculture MCFs are not quantified. For example, in line 21-22 on page 7, the implications of a constant harvest index for crop yields is not quantified. In relation to this, the study of Hay (1990) shows that for Europe the harvest index increased substantially since the 1900s. Furthermore, in line 19-20 on page 8 constant factors have been used for C emissions by decomposition, sedimentation and export to rivers. These factors are very uncertain. Several previous studies (Berhe et al, 2007; Van Oost et al.,2012) show for example that colluvial and alluvial reservoirs play a crucial role in C burial and that the fraction of C emitted to the atmosphere as a result of

erosion as indicated by the studies of Lal et al. might be overestimated.

4) Land use change emissions are proven to be highly uncertain, while being the largest contributor to C emissions amongst the MCFs. For example, the study of Fuchs et al. (2016) shows that gross land use change leads to considerable differences in C emissions compared to net land use change. Without taking such issues into account it is difficult to assess the overall uncertainty of land use change. I think that the author should go deeper into these methodological uncertainties related to the MCFs, especially for land use change.

Minor Comments:

L 20, page1: The author finds that including MCFs in the global C budget reduces the land C storage due to the smaller residence time. This might be seen as contrasting to the fact that land is a net C sink, which remains unexplained for a large part. Thus, the attempt to capture all the major mechanisms of the C cycle leads to even more uncertainty. This is something that needs to be addressed in the paper.

L 23, page 1: Instead of aggregating results per cropland fraction it would be more interesting to see the results per land cover type (forest, grass, crop).

Why did the author use the VISIT model? What would be the difference in results if a global land surface model would be used instead? For example, the ORCHIDEE land surface model, which simulates explicitly and in great detail the various ecosystem processes described in this study, and has the possibility to be coupled to the atmosphere and ocean models.

L 1, page 6: Why are human-prescribed fires not considered? In previous studies it is shown that population density and crop fraction are important drivers of burnt area (Lasslop and Kloster, 2017).

It would be interesting to compare methane emissions from the model to observations.

Page 6, section 2.2.4: How is the wetland fraction determined in the model and is that

comparable to observed wetland distribution globally?

How is crop harvest simulated in the tropics? And is crop irrigation included?

In section 2.2.8, how are the individual erosion parameters calculated? Previous studies have shown that erosion rates can be highly uncertain when applying the RUSLE model on the global scale using coarse resolution input data. Including the L and P factors in the RUSLE at the global scale can also contribute to large uncertainties as they are local-scale dependent.

L 17, page 8: “The carbon of Fpoc is extracted from the litter pool.” Why is the SOC of the topsoil not taken into account? This could produce biases in C erosion rates, especially for cropland.

L 25, page 8: How would neglecting riverine lateral fluxes (POC and DOC) contribute to the uncertainty of MCFs?

L 21, page 9: Why is Fpoc classified as biogeochemical flow and not anthropogenic? Fpoc is the result of human-induced erosion, as far as I understand.

L 5, page 10: Why is only the erodibility perturbed randomly and not the other RUSLE factors that maybe be more sensitive such as erosivity?

L 7-15, page 11: It would be useful to compare these results to the findings of other studies that quantified one or more of the individual MCFs at the global scale.

L 15, page 11: I find it surprising that the DOC export has a larger effect on SOC stocks in comparison to POC export. What could be the possible cause of this?

L 8, page 36: Soil erosion rates have been compared to the findings of Chappell et al. (2016) only, however, Chappell et al. did not calibrate their erosion model for other land cover types than cropland. It would be useful to compare the results also to the study of Naipal et al. (2018), who estimated gross SOC erosion for the period 1850-2005.

Page 37, figure 6: Why is Fap negative in some regions?

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