Dear Editor and Referees:

Thank you for your comments. Your comments help specify weakness and unclarity in the manuscript and are very effective for me to improve the manuscript.

First of all, importantly, I re-calculated carbon flows by the VISIT model and improved accuracy of the mass balance of carbon budget, using the input data same to the first manuscript. Also, 146 parameter ensemble simulations (i.e. increased from 128 of the first submission) were conducted again. Additionally, consideration of leap years was revised to remove small but noisy 4-year-cycle fluctuations in the time-series of carbon stocks. As a result, all figures and tables were updated, and results became more convincing.

Both referees were interested in uncertainties in the present simulations of minor carbon flows. In the previous manuscript, the simulated results were compared with a few datasets and values in the literature. As recommended by the referees, I compared the simulated results with a larger number of data in a more comprehensive manner.

The manuscript is under revision on the basis of your comments. Otherwise, I try to justify my research strategy in the light of present data availability and time limitation. My point-by-point reply to your comments is presented below. I hope this revision is satisfactory for being accepted for publication.

Anonymous Referee #1

[Comment 1-1] ***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 comparison of the results to existing observations and to other studies that have addressed certain MCFs in more detail in the past.***

[Reply 1-1]

Thank you for this comment. I agree to include a more comprehensive comparison with existing observations and other studies on MCFs. The revised manuscript has an additional table (tentative one is seen as Table A1, below) for this purpose; more estimates and remarks will be included.

Table A1. Summary of previous estimates of minor carbon flows (MCFs) [tentative].

MCFs	References	(Pg C yr^{-1})
F_{BB}	Wiedinmyer et al. (2011): FINN	2.175
	van der Werf et al. (2012): GFED4s	2.2
	van Marle et al. (2017): BB4CMIP6	1.8964
F_{LUC}	Le Quére et al. (2018): GCP 2018 models	1.4 ± 0.6
	Le Quére et al. (2018): GCP 2018 bookkeeping	1.3 ± 0.7
F_{DOC}	Meybeck (1993)	0.199
	Dai et al. (2012)	0.17
	Cai (2011)	0.25
F_{POC}	van Oost et al. (2007)	0.25
	Regnier et al. (2013)	0.1 ± >0.05
	Galy et al. (2015)	0.157 (0.107–0.231)
	Naipal et al. (2018)	0.16 ± 0.06
F_{CH4}	Fung et al. (1991)	0.13875
	Saunois et al. (2016): GCP synthesis	0.135
F_{BVOC}	Guenther et al. (2012): MEGAN	0.95832
	Sindelarova et al. (2012): MEGAN	0.76
F_{AP}	Bondeau et al. (2007): LPJmL	2.2
	Ciais et al. (2007)	1.29
F_{WH}	Winjum et al. (1998)	0.98
	Pan et al. (2011)	0.189

Major comments:

[Comment 1-2] ***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.

[Reply 1-2] This is a great comment. I'm convinced that this is one of the early attempts to include MCFs into a single consistent model framework, and in this sense, that this study carries novel implications such as interactions between MCFs and effects on net ecosystem production. On the other hand, I agree that these points were not adequately declared in the previous manuscript. The sparse observational data and simple parameterization of MCFs can bring additional uncertainties, which need to be recognized, into our global carbon accounting. However, I believe such attempts would lead to deeper understanding and more accurate evaluation of carbon budget. The revised manuscript includes these statements.

[Comment 1-3] ***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?***

[Reply 1-3] Thank you for this comment. In the light of uncertainties, it may be too early to conclude that including MCFs surely improves quantitative accuracy of terrestrial carbon cycle models. At least several aspects, the model encapsulating MCFs performed better than the old, no MCF one. For example, the net biome production in 1990–2009 (1.13 Pg C yr^{-1} of EXALL vs 2.78 Pg C yr^{-1} of EX0) was estimated more closely to the GCP 2018 one (1.22 Pg C yr^{-1}). It is an excellent idea to compare the spatial distribution of biomass and soil carbon stocks with observation-based data. The revised manuscript, maybe as a supplementary figure, includes the comparison of vegetation and soil carbon stocks.

[Comment 1-4] ***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.

[Reply 1-4] Thank you for this insightful comment. In this study, the range of uncertainties in each MCFs was assessed by parameter-ensemble simulations. I know that other uncertainties (e.g., methodological uncertainties among different schemes) could substantially affect the results, but considering them should increase computational cost too much. Instead, in this study, I assumed a wide range of parameter uncertainties to harvest the uncertainty range as much as possible (Supplementary Figure S2). For example, transition from the low members to high members may roughly indicates the effects increased harvest index. I appreciate your suggestions on the temporal change in harvest index and reservoir effects on POC export. I found a several papers on the reservoir effect on riverine export (e.g., Mendonça et al., 2017, Nature Communication, 8, doi:10.1038/s41467-41017-01789-41466). The revised manuscript mentions about these effects and associated uncertainties. Straightforward assessments of these factors need further data collection, revised parameterization, and systematic simulations, which would be done in forthcoming studies.

[Comment 1-5] ***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.***

[Reply 1-5] Thank you for this comment. I could not find Fuchs et al. (2016) but Fuchs et al. (2015, Global Change Biology, 21, 299–313). I completely agree with your statement that there remain tremendous uncertainties in the present estimation of C emissions (and uptakes) associated with land-use change. In this study, we used gross land-use change derived from the land-use transition matrix (Hurtt et al., 2011, Climatic Change, 109, 117–161).

As shown in Supplementary Figure S7, existing biome models and inventories differ widely in the historical land-use emissions. The range of parameter-ensemble VISIT simulations was roughly comparable with that among biome models, but still I agree that methodology-related uncertainties should be considered. Therefore, I conduct additional simulations in which net land use change, on the basis of cropland data by Ramankutty and Foley (1999, Global Biogeochemical Cycles, 13, 997–1027), is used. I discuss methodological uncertainties by comparing the results.

Minor Comments:

[Comment 1-6] ***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.***

[Reply 1-6] Thank you this insightful comment. I agree that the shorter mean residence time (MRT) and net carbon sink seems contrasting. Clearly, the net carbon sink was not caused by elongation of MRT. The MRT of carbon stocks became longer in EX0, implying the it was not primarily related to MCFs or certain uncertainty. Here, MRT (inverse of turnover rate) of vegetation and soil carbon was approximately calculated by the following, assuming a ‘relaxed’ steady state (cf. Carvalhais et al., 2014, Nature, 514, 213–217).

MRT (vegetation, yr) = Biomass C stock / NPP

MRT (soil, yr) = Soil C stock / heterotrophic respiration

Here, increases in NPP and heterotrophic respiration could largely account for the apparent shortening of MRT. The historical elevation of atmospheric CO_2 concentration and temperature rise resulted in enhanced NPP and heterotrophic respiration, in the model simulation. In the revised manuscript, I discuss the point with a caution to the definition of MRT.

[Comment 1-7] ***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).***

[Reply 1-8] Thank you for this comment. As you recommended, I aggregated the MCFs per land cover types (Figure A1a) and would be included in the revised manuscript. The new figure clarified the difference among the land cover types, which seems different from the aggregation by cropland fraction (Figure A1b) and precipitation (Figure A1c).

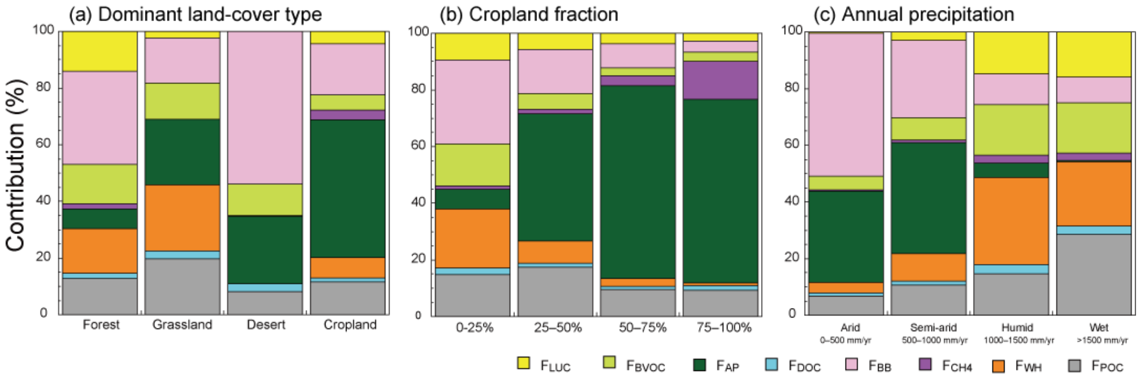

Figure A1. Relative contribution of MCFs to the terrestrial carbon budget simulated by EXALL in 2000–2009. (a) aggregated by dominant land-cover type, (b) aggregated by cropland fraction within grid cells, and (c) aggregated by annual precipitation

[Comment 1-8] ***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.***

[Reply 1-8] This is a good comment. In this study, I used the VISIT model, because I have developed the model from scratch and therefore know every detail. This is important to implement the MCFs into a terrestrial model in a biogeochemically consistent and practically efficient manner. Another advantage is the low computational cost of the model (less than 2 days for a whole simulation with a single CPU), allowing us to conduct >100 ensemble simulations with multi-CPU machine in a few weeks. Moreover, the VISIT model has already been coupled with an Earth System Model, which are going to make contributions to CMIP6. I acknowledge that there are many terrestrial models (e.g., ORCHIDEE, CLM, LPJmL, JULES, etc.), which have great details and sometimes their codes are available as open-source. I expect that the present study demonstrates the importance of MCFs and facilitate similar studies by other models.

[Comment 1-9] ***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.***

[Reply 1-9] Thank you for this comment. I checked Lasslop and Kloster (2017, Environmental

Research Letters, 12, 115011) and associated papers. One possible justification is that the present model has already simulated extensive global burnt area (around 600 Mha per year) comparable with that by satellite observation including both wild and human-caused fires. However, I agree that human impacts on fire regime is significant and related to population and land-use. As demonstrated in Supplementary Figure S10, the simulated biomass burning did not capture the decreasing trend after 1998. By using an updated fire scheme (it is beyond the scope of this study), I would like to include human impacts on fire regime. Comparison with the simulated emissions with observed atmospheric concentrations of methane (and carbon monoxide, black carbon etc.) should be effective for model validation.

[Comment 1-10] ***Page 6, section 2.2.4: How is the wetland fraction determined in the model and is that comparable to observed wetland distribution globally?***

[Reply 1-10] Thank you for this comment. I am sorry about the largely simplified description of methane simulation, although it was fully described in Ito and Inatomi (2012). The wetland fraction for each grid was determined by the Global Lake and Wetland Dataset (Lehner and Döll, 2004, Journal of Hydrology, 296, 1–22). I applied this observation-based map through the simulation period. The uncertainty of wetland and inundation maps would be addressed in the wetland methane model intercomparison project (e.g., Poulter et al., 2017, Environmental Research Letters, doi: 10.1088/1748-9326/aa8391). The revised manuscript includes the description and reference of wetland map.

[Comment 1-11] ***How is crop harvest simulated in the tropics? And is crop irrigation included?***

[Reply 1-11] In the tropics (annual mean temperature > 20°C and lowest monthly temperature > 10°C), a generic multiple cropping system was assumed; crop harvest occurs through the year round at a constant rate. Crop irrigation was considered only in an implicit manner. Namely, water stress factor on maximum photosynthesis rate was relaxed in croplands, assuming the effect of irrigation. On the other hand, hydrological budget of irrigated water was not considered.

[Comment 1-12] ***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.

[Reply 1-12] Thank you for this comment. I found several recent studies explored soil erosion at the global scale (e.g., Naipal 2018; Borrelli et al., 2018, Nature Communications, 8, doi:10.1038/s41467-017-02142-7; Xiong et al., 2019, Geoderma 343, 31–39). The RUSLE is a simple model of soil displacement by water erosion and then has been used widely. In the present study, as explained in my previous study (Ito, 2007, Geophysical Research Letters, L09403), slope factors (L and S) were calculated using a 1km-mesh topography data (GTOPO30 and HYDRO1k); rainfall factor (R) was calculated using an empirical parameterization by Renard and Freimund (1994, J. Hydrol., 157, 287– 306) every year; soil erodibility factor (K) was calculated on the basis of soil composition (organic matter, clay, silt, and sand) with a parameterization by Torri et al. (1997, Catena, 31, 1–22); vegetation coverage (C) and management protection (P) factors were derived from look-up tables for each of the land cover types from Yang et al. (2003, Hydrol. Processes, 17, 2913– 2928) and Morgan (2005, Soil Erosion and Conservation, 3rd ed). The locality effect of L could be ameliorated by using a fine-mesh topography data. On the other hand, the P factor could be heterogeneous due to farm-by-farm difference in soil management such as mulching and contour farming. It is, however, difficult to determine P value for each farm and to obtain a spatially representative value for each 0.5° grid, although ongoing development of high-resolution remote sensing and AI-based categorization would make it possible in the future. At this stage, I conventionally estimated P at each grid from the cropland fraction and whether developed or developing country. In relation to soil degradation and conservation, future studies would estimate the P factor in a more realistic manner. The revised manuscript describes how F_{POC} was estimated using the RUSLE and discusses the potential uncertainties.

[Comment 1-13] ***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.***

[Reply 1-13] Thank you for this comment. You are right, because soil erosion can occur not only in litter but also in SOC layer in a real world. My assumption of F_{POC} extraction from litter pool is only for simplicity, because I did not have enough data on the fractional contribution of litter and SOC to eroded carbon. I will explain this in the revised manuscript and in a forthcoming study, I would like to address this issue on the basis of a synthesis of soil erosion.

[Comment 1-14] ***L 25, page 8: How would neglecting riverine lateral fluxes (POC and DOC) contribute to the uncertainty of MCFs?***

[Reply 1-14] Thank you for this comment. The present model does not explicitly simulate lateral carbon flows between grids such as POC and DOC transport by rivers. As a result, carbon export in one upstream grid and deposition in another downstream grid were not considered. I do not think the magnitude of the effect exceed 1 Pg C per year, but at least in certain areas, this process would affect net carbon budget.

[Comment 1-15] ***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.***

[Reply 1-15] Thank you for this comment. As you pointed out, soil erosion (F_{POC}) has been largely enhanced by human activities, and in my model simulation, the flow was separately evaluated for croplands and natural ecosystem. The term, 'biogeochemical' flow, is a conventional one and does not indicate a 'natural' flow. Indeed, other 'biogeochemical' flows such as F_{CH4} (including paddy emission) were more or less affected by human activities, but in more indirect manners than anthropogenic flows such as land-use and harvests. The revised manuscript explains the definition of 'biogeochemcial' flow in a clearer manner.

[Comment 1-16] **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?**

[Reply 1-16] Thank you for this comment. Other factors such as precipitation, slope and its length, vegetation cover, and management are influential to F_{POC} and should have their own uncertainties, and I agree that it is desirable to assess them in a systematic manner. In this study, I chose the erodibility as a representative one for simplicity, with a sufficient width of perturbation. As shown in the result of ensemble simulation (Supplementary Figure S2), the simulated F_{POC} ranged widely from 0.1 to 1.4 Pg C yr^{-1}. Elsewhere, I would like to assess the effect of erosion-related factors using multiple input data.

[Comment 1-17] ***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.***

[Reply 1-17] I agree to compare the results with other global-scale model studies that quantified one or more MCFs. The results will be summarized into a new table (cf. Table A1).

[Comment 1-18] ***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?***

[Reply 1-18] Thank you for this comment. One reason for the stronger impact of DOC export is that a large part of POC export occurred in croplands and ecosystems on steep slopes. In contrast, DOC export occurred in a vast extent of ecosystems especially in humid tropical area (Figures 6g and 6h of the discussion paper). The impacts of DOC and POC exports were much smaller than those by land-use change and biomass burning, and the difference of the impacts (about 2 Pg C) could be, at least partly, caused by such spatial patterns.

[Comment 1-19] ***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.***

[Reply 1-19] Thank you for this comment. In the revised manuscript, the result would be compared with that in Naipal et al. (2018).

[Comment 1-20] ***Page 37, figure 6: Why is Fap negative in some regions?***

[Reply 1-20] Thank you for this comment. The negative Fap in the previous manuscript was due to an inaccurate mass balance calculation in croplands, and it was corrected in the second version. As a result, all croplands take positive (i.e. net carbon export) Fap values.

Anonymous Referee #2

Main comments:

[Comment 2-1] ***This study estimated the influences of eight minor disturbances (MCFs) on global land carbon budget over the historical period 1901-2016 using a process-based terrestrial ecosystem model VISIT. Carbon contributions from minor disturbances like CH4, BVOC, and carbon loss by water (or river) erosion were often ignored in the past modeling studies, but have been evaluated in this study within one model framework. Results from a group of sensitivity modeling experiment show notable contributions from MCFs to land carbon sink and storage, which is mostly due to land use change, fires, and wood harvest.***

The author also find BVOC has a comparable contribution. This study helps improve understanding of land carbon cycle and shows the importance of the MCFs on the land carbon budget. Overall, the manuscript is well written and could be acceptable for publication in ESD after some minor revisions. Please see my minor comments as below.

[Reply 2-1] Thank you for this encouraging comment.

Minor comments:

[Comment 2-2] ***(1) Line 17 in the abstract: It is unclear the net biome production was estimated for which period?***

[Reply 2-2] Thank you. The net biome production was estimated for the same period of the previous sentence (i.e. the 2000s). I added "in the same period" after "net biome production".

[Comment 2-3] ***(2) Page 11, Lines:29-30: does it mean that NEP dominate the trend of NBP? As from other estimates that trend in land-use change emission is relatively small. How are the fires from, e.g. FEEDs?***

[Reply 2-3] Thank you for this comment. In this study, the simulated NEP and NBP show comparable linear trends. The simulated FLUC shows a clear decreasing trend after 2000 and F_{BB} shows a moderate increasing trend. Such compensation may explain a part of the small trend associated with the MCFs. However, a recent study (Andela et al., 2017) indicate a decreasing trend in global burnt area, implying the necessity of further improvement of temporal trends in MCFs. The revised manuscript includes more comparison with observations (Table A1) and discussion about it.

Table A1. Summary of previous estimates of minor carbon flows (MCFs) [tentative].

MCFs	References	($Pg\ C\ yr^{-1}$)
F_{BB}	Wiedinmyer et al. (2011): FINN	2.175
	van der Werf et al. (2012): GFED4s	2.2
	van Marle et al. (2017): BB4CMIP6	1.8964
F_{LUC}	Le Quére et al. (2018): GCP 2018 models	1.4 ± 0.6
	Le Quére et al. (2018): GCP 2018 bookkeeping	1.3 ± 0.7
F_{DOC}	Meybeck (1993)	0.199
	Dai et al. (2012)	0.17
	Cai (2011)	0.25

F_{POC}	van Oost et al. (2007)	0.25
	Regnier et al. (2013)	0.1 ± >0.05
	Galy et al. (2015)	0.157 (0.107–0.231)
	Naipal et al. (2018)	0.16 ± 0.06
F_{CH4}	Fung et al. (1991)	0.13875
	Saunois et al. (2016): GCP synthesis	0.135
F_{BVOC}	Guenther et al. (2012): MEGAN	0.95832
	Sindelarova et al. (2012): MEGAN	0.76
F_{AP}	Bondeau et al. (2007): LPJmL	2.2
	Ciais et al. (2007)	1.29
F_{WH}	Winjum et al. (1998)	0.98
	Pan et al. (2011)	0.189

[Comment 2-4] ***(3) Page 12, Line 7: How the mean residence time (MRT) was calculated? What are the assumptions were used to calculated the MRT for each C pool? Also, why MRT was decreased in the Fig. 4?***

[Reply 2-4] Thank you for this comment. In this study, MRT at a non-equilibrium state was calculated approximately as

MRT (vegetation, yr) = Biomass C stock / NPP

MRT (soil, yr) = Soil C stock / heterotrophic respiration

Such approximation (assuming an equilibrium state at each year) has been adopted in previous studies. The revised manuscript includes the explanation how to calculate MRTs. The decadal decreasing trends in MRT was largely attributable to enhanced respiration rates due to climate change. Because this trend occurred also in EX0, it is not mainly caused by MCFs.

[Comment 2-5] ***(4) Page 16, section 4.5: It is good to see the uncertainty assessment. Because this study is based only one model (i.e., VISIT), and the single-model simulation may cannot avoid propagating the uncertainty of other processes to the minor C flows. For example, the uncertainty in C partitioning among vegetation, litter and soil pools may affect the simulations of FBB and FCH4 in this study. A further discussion on this point is necessary.***

[Reply 2-5] This is an important comment, and I agree to include further discussion on estimation uncertainty. This study used a single model (VISIT) aiming at conducting in-depth analyses on MCFs. Based on model intercomparison studies (e.g., Ito et al., 2016, 2017; Tian et al., 2015), the possible range of uncertainties and their propagation to MCF estimation will be discussed.

[Comment 2-6] ***(5) Page 14, Line 31: delete "(" or add a ")" after ": : :Chapin et al. (2006)".***
[Reply 2-6] Thank you. Corrected (i.e., ")" added after Chapin et al. (2006)).

[Comment 2-7] ***(6) Fig.3 d and e: Impacts of MCFs on NEP is offset by emissions from MCFs?***
[Reply 2-7] Figure 3d represents the effect of MCFs on NEP leading to higher CO2 uptake by terrestrial ecosystems. Figure 3e represents the difference between NEP and NBP including the MCFs. Indeed, when comparing Figure 3a and 3c, the effects would offset each other: a large fraction of carbon uptake by vegetation was lost by MCFs.

[Comment 2-8] ***(7) Fig. 6f: For the CH4 emission (FCH4), have you compared the FCH4 in this study with some other estimates? Why does the East Asia show much higher values in comparison with any other regions? In line 23, you have also mentioned that FCH4 in Asia was mostly from paddy field, could you show more details?***
[Reply 2-8] Thank you for this comment. The high methane emissions in East Asia are largely attributable to a vast extent of paddy fields, i.e. natural wetlands. In my paper (Ito and Inatomi, 2012), the model-estimated methane emissions were compared with previous studies. Also, for wetland emissions in 2000–2012, the VISIT model estimation was compared with other models (Poulter et al., 2017), suggesting validity of the model. Additionally, in my recent work, the model-estimated methane emissions from East Asian paddy fields were compared with inventory (EDGAR 4.3.2) value. The revised manuscript includes some more details about the methane emission in East Asia.

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