

Author's response to referee 1:

Thank you very much for your supportive and precious comments! You helped us significantly improve this study.

1) Comments: Abstract The time step of the simulation in this study is monthly. Time step of the calculation is very important. So, please explicit describe about the time step of the calculation even in abstract.

Response: We have added the information in Abstract: "We develop a process-based biogeochemistry model to quantify CO exchange between soils and the atmosphere with a 5-minute internal time step at the global scale. The model is parameterized using CO flux data from the field and laboratory experiments for eleven representative ecosystem types. The model is then extrapolated to the global terrestrial ecosystems using monthly climate forcing data." From line 10 to line 15.

2) Comments: Page 2 line 31 I do not think that this sentence is necessary.

Response: Deleted.

3) Comments: Page 5 line 118 are in -> is

Response: Corrected. "The first study to report long-term and continuous field measurements of CO flux over grasslands using a micrometeorological eddy covariance (EC) method is Pihlatie et al. (2016)." From line 93 to line 95.

4) Comments: Page 5 line 127 withan-> with an

Response: Corrected. "A set of century-long simulations of 1901-2100 were also conducted using the atmospheric CO concentrations estimated with an empirical function (Badr & Probert, 1994; Potter et al., 1996)". From line

5) Comments: Page 6 line 139 Please spell out TEM at the first place.

Response: We have mentioned at the first place of introduction. "To improve the quantification of the global soil CO budget for the period 2000-2013 and CO deposition velocity for the 20th and 21st

centuries, this study developed a CO dynamics module (CODM) embedded in a process-based biogeochemistry model, the Terrestrial Ecosystem Model (TEM) (Zhuang et al., 2003, 2004, 2007)."

From line 96 to line 99.

6) Comments: Page 6 line 157 Vertical grid of 1cm can be used for simulation of CO₂ and CH₄ diffusion processes in soil, but, I consider that 1cm is still not so finely-gridded for the simulation to model soil CO consumption because of rapid CO consumption in soil (especially very active soil to consume CO). Some soils are strong consumers of CO and these soils absorb CO within 2-3cm of top soil layer. Though this comment does not deny robustness of the results of this study, I recommend that these technical aspects should be mentioned in the Discussion section (as in line 376) to be kind for readers who may study soil CO consumption. Furthermore, authors properly used implicit (Crank-Nicolson) method in order to be independent from time-step which must be set as short as possible in case of explicit method because the vertical grid must be finer for soil CO consumption for explicit method.

Response: Thanks much for your suggestions. In this revision, we have tested the model using 3, 15, 30, 300, 3000 thin layers to examine the influence of layer thickness. It turned out that we have chosen the proper layers division and more layers will need much more computing time, but not show further improvement. We have summarized these tests in Figure 12 and Section 4.3, line 452 to 460.

7) Comments: Page 7 line 160 "i" and "t" should be italic.

Response: Corrected. "Where $C(t, i)$ is the CO concentration in layer i and at time t , units are mg m⁻³." Line 142.

8) Comments: Page 12 line 297 Figure 3 (a2,b2,c2,d2) I cannot understand which observations in Table 1 were plotted in the sub-figures.

Response: We added information to indicate the site being used in Figure 3 caption.

9) Comments: Page 12 line 305- Page 13 Direction of consumption and production or net flux is misleading. I felt that minus expression of values is difficult to see through.

Response: Thanks for pointing this out. We have changed all values presented as ranges like “From -180 to -197, 34 to 36 and -145 to -163 Tg CO yr⁻¹”.

10) Comments: Page 16 Model Uncertainties and Limitations I consider that CO concentrations at soil surface (environmental CO concentration for soil) is a little different from CO data from MOPITT. Boundary-layer processes are also complex. A comment about this point is necessary.

Response: We have revised Section 4.3 to address your comments. “. Third, the derived CO surface concentration is lower than MOPITT CO surface concentration, which will lead to overestimation of CO deposition velocity during 1901-2100.” From Line 450 to 452.

11) Comments: Table2-6 Values were centered but should be formatted to be easily understood. For example, Earea/R (K) in Table 2 In place of 8801 14165, 8801 14165 is better.

Response: We have now centered all values and names of parameters.

12) Comments: Figure2 Units of soil moisture are different among (a2), (b2) (c2) and (d2). My concern is about the highness of the volumetric soil moisture. The soil moistures in (a2) (d2) are too high? Volumetric soil moisture contents (a2) are too high, as high as 80%. Normal soils have no capacity to hold such high moistures. The units of the soil moisture contents are all volumetric (m³/m³) ? Please check and if the shown volumetric soil moistures are correct, please mention reasons.

Response: Thank you for pointing this out. In this revision, we traced back to Nakai et al. (2013) and found that our units and values are the same as they presented. The reason why the values were so high is that the volumetric soil moisture (VSM) was converted from the water content reflectometry (WCR) probe output period using an empirical calibration function of Bourgeau-Chavez et al. (2012) for 5cm-30cm layer. Although Bourgeau-Chavez et al. (2012) provided calibration functions for each soil horizon (i.e., dead moss, upper duff, lower duff, and mineral soil), some of them resulted in values greater than 100% VSM in Nakai et al. (2013) study. The model estimated high VSM (close to 80%) is

due to top 10 cm moss in the model which has a saturation VSM of 0.8. We added the discussion on Figure 2 caption in this revision. From line 482 to 485.

13) Comments: Figure 3 (c2) and (d2) Why authors showed over-scale (CO emission) graph? The y-axis of (c2) should be -10 to 2

Response: We have corrected the Y-axis's range to -10 to 2 in Figure 3 (c2).

14) Comments: Figure 4 Please write clear the meaning of "global land surface". Global land surface includes Antarctic area? Normal readers think that global average temperature is about 15C but the shown temperature (a) is between 7.5-9C.

Response: We have added extra information in caption of Figure 4 and Figure 6: "Global land surface (excluding Antarctic area and ocean area)"

15) Comments: Figure 6 Why SOC increases sharply before 2100?

Response: In this revision, we have fixed this problem. The fixed values of SOC is showed in figure 6. We also rerun the model to remove the influence of odd SOC to future prediction of CO dynamics.