Review of "Global Soil Consumption of Atmospheric 1 Carbon Monoxide: An Analysis Using a 2 Process-Based Biogeochemistry Model" by Liu et al.

The paper presents a bottom-up model for the exchange of CO between soil and atmosphere. The CO soil exchange module is process-based and includes explicit gross soil consumption and production fluxes. This module uses soil moisture and temperature, and atmospheric CO mole fraction data.

The work has several main components: (1) validation of the model using data from four field measurement stations; (2) simulation of the past using a constant distribution of atmospheric CO; (3) simulation of the recent period using MOPITT data, and (4) simulation of the future using the same constant distribution from point (2). Additionally, sensitivity tests are used for determining the influence of various parameters on soil fluxes.

I find the paper interesting, and the subject fits very well into the ACP area of interest. However, I find the paper quite descriptive – I miss some discussion and conclusion on the meaning and significance of the results, and some outlook on the further use of this model. The paper is not very well organized, with some parts difficult to follow, and at some places I found it difficult to extract the relevant information from the text.

I recommend publication after the following points are addressed.
I include below only those issues from the access review that have not been solved.

Main concern: CO concentration and flux mismatch between MOPITT and constant function simulations
The CO soil deposition is largest flux and is strongly dependent on the atmospheric CO mole fraction. The constant CO mole fractions used in past and future model runs are not realistic, at least for the period 2000-2013, as shown by the large mismatch in magnitude with the MOPITT satellite data for the overlapping period (Fig . 5b).
Correspondingly, the annual fluxes calculated over 1901-2013 using the constant CO distribution (determined by the function shown at lines 263) are very different from the fluxes calculated using the CO MOPITT data for the shorter period 2000-2013.
The authors report both sets of incompatible fluxes as results. Assuming that the satellite measured values are close to the truth, then the fluxes calculated using the constant function, for the 20th and 21st centuries, are obviously wrong. However, these are reported as main results and the authors claim to "quantify global soil budget for the 20th and 21st centuries".

In my opinion these fluxes should not be reported in the actual form. They can however be used to study the variability and the relative magnitude of the components, and the relative variation in time. If the exercise is only for understanding the controls and the relative evolution (as the authors state in their reply to reviewer 2), this can be accepted as long as the limitations are made clear and the fluxes are not claimed to be solved.

Here are some ideas on what can be done:
- scale the CO distribution function in such a way that it matches the satellite observations for the overlapping period. This would need an assumption for the temporal evolution of CO during the 20th century

- report the results not in terms of fluxes, but in terms of net deposition velocities. This would require changes through the paper.
- keep the fluxes as they are, but do not discuss the absolute values of the fluxes but only the relative variations

Besides these, the authors should be careful with claiming that they quantify the soil budget for the 20th and 21st centuries.
This issue should be discussed thoroughly in Sect. 4.3.

Other general comments
- Abstract: please report the same values given in the paper, either averages or ranges.
- please try to organize the paper in smaller paragraphs to improve readability
- I find the Introduction up to line 60 difficult to read, because of the long list of references following each bit of information. Please consider rephrasing and reducing the references that keep being repeated.
- The summary of the experiments is given both in 2.1 and 2.5, but the two descriptions do not seem to match. In 2.1. it is mentioned that the purpose was to "investigate the impact of …atmospheric CO concentrations " but the sensitivity tests that are meant for this are not mentioned here.
- is Michaelis-Menten kinetics necessary? The net uptake means in principle that at least the uppermost layer of soil has lower concentration than the atmosphere, and this is much lower than kCO. Are the CO concentrations in the lower soil layer much larger, and if yes, why? Related to this, please show the CO soil profiles for some typical situations.
- please number the figures in the order they are discussed in text (e.g. Figs 7, 8) – or change the text to mention the figures in the right order.
- Section 4.2 should be reorganized and at least partly moved to results – see specific comments
- Section 4.3 discussed the model uncertainties and limitations, but ignores two of the major issues: (1) the CO concentrations mismatch between satellite and constant function, and (2) the overestimation of soil consumption at high temperatures.

Specific comments
- line 16: "constant spatially distributed" – can be understood wrongly as constant in space, which is not; I suggest to change to " constant in time, spatially distributed"
- line 20: "the largest sinks at 93 Tg CO yr-1" – is this from the 20th century or 2000-2013 simulation? Please specify. Same for the next phrase.
- lines 63 and 68: use the same units for the deposition velocity
- lines 61-68: the phrase is unclear, consider breaking it into smaller pieces. Also, quite some information is given on the studies listed, but not enough to actually understand what they did and what the differences are. Please consider either giving more details, or shortening this part.
- lines 62 – 66: the text seems contradictory: the uptake flux when using a constant deposition velocity globally (115-230 Tg) is smaller than when using the same deposition velocity in general and some area set to zero (300 Tg) - please explain or reformulate. "With different approaches" is too unspecific and does not really say anything.
- line 67: " using empirical approaches with higher probability for lower values" – unclear, higher probability of what? lower values for what?
- lines 68 – 70: which other substances? what are other deposition velocities? please give examples if you mention this.

- lines 75 – 91: a lot of this discussion on the thermal and photo degradation is irrelevant for this paper, especially the part on photo degradation which is not included in the model. Please reduce the irrelevant parts.
- lines 92 – 93: it is unclear to me what this phrase means. I think what the authors may intend to say is that *little attention has been paid to CO (including soil consumption and production) in global (chemistry?) models.* Please reformulate if true.
- lines 97-98: suggest replacing "oxidation from soil bacteria and microbes" by "oxidation by soil microbes". Bacteria are microbes.
- line 110: CO emission is abiotic, right?
- line 156: "determined by the mass balance" – unclear what this means: which mass balance, of what, between what? please clarify.
- line 162: this term represents all the consumption; remove "due to oxidation"
- line 171: "modeled as an anaerobic process" there is nothing in Eq. 2 that makes it anaerobic
- lines 186- 188: phrase unclear - I think the authors mean that Eq 2.2 will overestimate CO consumption because in reality CO consumption decreases at high temperatures, while in Eq 2.2 CO keeps increasing with temperature. Please reformulate.
- line 193: i do not understand what Pr(t,i) is
- line 198: should it be 20 cm SOC?
- lines 275 – 277: give some details on the scenarios and datasets
- line 297: is the reported correlation really *r*, or is it *r^2*? Also, a correlation coefficient *r* of 0.5 is usually not considered high correlation.
- lines 299 - 300: please compare the RMSEs reported to the CO fluxes, in order to give an impression on the relative errors.
- lines 318 – 319: "consume 42% and 58% of the total consumption, and produce 41% and 59% of total production" – please reformulate
- line 328: Table 3 does not show the deposition flux in mg/m2 day
- lines 329 – 343: I find the text in this paragraph somewhat misleading. The fluxes are presented as changing or increasing relative to the simulation for 20^{th} century, which suggests a temporal evolution, but in fact they are different mainly because of a different model setup, i.e different atmospheric CO concentrations. Consider using "different" and "larger" instead of "increasing".
- line 359: "the rate ranges of increasing of consumption…" – I think it should be something like "the ranges of the rates of increase in consumption…"; please explain what these ranges are, are they corresponding to the three scenarios?
- line 382: the references should be given in the method section, not here
- Sect 4.2: The information here belongs mostly to Results, please reorganize. Also, the section is hard to follow – there are many correlations mentioned without much coherence. Consider reformulating into a more focused way, with one paragraph per idea (e.g on annual time scales, the CO uptake is mostly correlated to X, Y, Z… and then comment more if needed on X). Try to separate the annual and monthly results.
- lines 398 – 400 and Table 5: The effect of the SOC on the gross uptake flux seems too large. In my understanding, the text tries to explain that SOC increases the gross production which makes more CO available, which in turn leads to an increase in the gross CO consumption. But, for an increase in SOC of 30%, the production increases by about 10Tg/year, and the consumption increases by 28 Tg/year! How can that be? Where are the extra 18 Tg/year coming from?
- line 406: "as CO flux" – do you mean "as does the CO flux"?
- line 415: what is 0.91? Is it r or r^2, or something else? The same for line 418.
- line 425: the same data limitation is when using any method, not only SCE-UA-R, correct? If yes, remove "using SCE-UA-R method"

- line 427: “with RMSE … day-1” – I think this info has no meaning here
- Table 6: what are the numbers? what are the units? These are not absolute values of fluxes.
- line 796: I suggest to replace “would happen inside” by “take place”
- Fig. 2: is this really volumetric soil moisture? The values do not seem realistic; I think typical values for water holding capacity for most soils are around 50 % and that would give the saturation. Please check the units.
- Fig. 2-d2: please use the same units as in a2, b2 and c2 figures.

Text comments
- line 60: should be “… consumption to be …”
- line 81: “formations” should be “formation”
- line 118: “are in Pihlatie” should be “is Pihlatie”
- line 219: I think “misfit” should be “mismatch”
- line 266 and through the paper: “transient” is used wrongly. If what is meant is “variable” then please do use “variable”. Transient does not mean variable, but something that disappears.
- Fig. 3: axes text too small; markers not visible in all figures (especially c2) , please consider using color markers
- Fig. 3 part 1, page 34: Remove from caption the explanation for c and d
- Fig. 3 part 2, page 35: Remove from caption the explanation for a and b
- Fig. 6: x labels not visible; some of the y labels cut
- Fig. 8: typo in legend: “producion”
- Fig. 9: typo in legend: “producion”