esd-2013-45: Explaining the seasonal cycle of the globally averaged CO$_2$ with a carbon cycle model
g.alexandrov@ifaran.ru

point-by-point reply to the comments

I highly appreciate all the comments and address them in the revised version of the manuscript as follows.

I moved the material about the comparison at a site level from Appendix to the main text.

I addressed the concerns about relationship between the rate of change of global average CO2 concentration, as derived from the NOAA surface data, and the global averaged surface fluxes by adding Appendix 2 titled "Is it reasonable to infer world monthly CO2 fluxes from globally averaged monthly CO2 concentrations?", where I compare global total monthly fluxes derived from global average CO2 concentrations to the global total monthly fluxes derived from 12 inverse models (TransCom 3 experiment).

> Although modified MONTHLYC model fits the fluxes provided by the TransCom 3 experiment even better than fluxes derived from global average concentrations (Figure 9), I would not like to keep silent about the fact that the same fluxes could be obtained with much more simple and transparent method, unless this desire will prevent publication of the article.
>
> Frankly speaking, I do not understand why "global atmospheric concentrations and global fluxes are not simply related, except on time scales much longer than the mixing time of the atmosphere". At regional scale, where the average concentration depends not only on the surface flux but also on the atmospheric transport, atmospheric concentrations and surface fluxes are not simply related. But how atmospheric transport can change the carbon balance of the whole atmosphere? Of course, it is possible that current observation network is not adequate for measuring global average atmospheric concentrations on monthly basis. But how can we know this?
>
> I rely on Editor expertise in such question as whether to keep or to remove all the information on the possibility to infer world total monthly fluxes from globally averaged monthly concentrations.

The limitations in the analysis presented in the paper are now stated more clearly in the manuscript itself on the page 12.

The un-tuned model output is now presented for comparison on the Figure 7.

"since the CO2 seasonal cycle reflects not just local ecosystem dynamics but regional-to-hemispheric scale fluxes" (Reviewer #2), I added comparison to regional fluxes provided by TransCom 3 experiment (Figure 8). I hope that Figure 8 and the text on the page 12 may partly answer the question about possible "cancellation between different geographic regions". There is no cancellation between the northern and southern hemispheres, because the amplitude of the seasonal cycle in the ecosystems located to south of 25N is very small. I also added some words about the obvious limitation of the MONTHLYC model: to reproduce the seasonal cycle of regional fluxes model coefficients should be set on regional basis.

The major direction for the work needs to determine whether substrate limitation accounts at regional scales for discrepancies between models and observations was proposed by Reviewer #1: developing a model for microbial priming of soil organic matter decomposition. I said about this direction at the page 13.

I again express my gratitude to Editor and Reviewers for their help in improving this manuscript and for their advice on the directions for further research.