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Interactive comment on "Coupling the Canadian Terrestrial Ecosystem Model (CTEM v. 2.0) to Environment and Climate Change Canada's greenhouse gas forecast model" *by* Bakr Badawy et al.

Anonymous Referee #1

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This paper describes the coupling between the Canadian Terrestrial Ecosystem Model (CTEM v2.0) and Environment and Climate Change Canada's greenhouse gas forecast model (GEM-MACH-GHG). The radiation, surface temperature, and precipitation fields etc. calculated from GEM-MACH-GHG feed to CTEMv2.0 every 30 minutes, and the net biosphere fluxes from CTEM v2.0 are used as surface boundary conditions to drive CO2 simulations by GEM-MACH-GHG. The ultimate goal of this coupling is to do carbon cycle data assimilation to constrain biosphere model parameters and surface carbon fluxes. The authors tested the performance of such coupling by evaluating the

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meteorology fields, and carbon fluxes including gross primary production and net terrestrial biosphere fluxes. At last, they tested the impact of using net biosphere fluxes from the coupled system as priors on atmospheric flux inversion with GEOS-Chem 4D-Var system. The paper is well structured. However, the rationale of such coupling is not well described, the evaluation of the model performance can be further improved, and some conclusions in the paper are not supported by the results. My detailed comments are below.

1. It is not clear to me why it is necessary to couple CTEM v2.0 with GEM-MACH-GHG for the purpose of carbon cycle data assimilation. CTEM v2.0 and GEM-MACH-GHG can run in parallel, and the GEM-MACH-GHG read in the fluxes from CTEM-v2.0 every 30 minutes. In that case, CTEM v2.0 can use the best possible meteorology fields it can get. As shown in this paper, the CTEM v2.0 forced by CRUNCEP performs better. I don't see the benefits of having consistent meteorology between CTEM v2.0 and GEM-MACH-GHG. The errors in meteorology affect CTEM v2.0 and GEM-MACH-GHG in quite different ways. The rational discussed in the introduction is not convincing. I would recommend adding more discussions about the benefits of coupling these two models together. If the authors can give a specific example, it would be clearer.

2. It is not clear whether the energy fluxes (e.g., latent heat flux) and water fluxes (e.g., evaporation) required running GEM-MACH-GHG is from the CTEM v2.0 or from somewhere else. I would recommend adding descriptions whether the coupling is one-way or two-way.

3. The left panels and right panels in Figure 1 are basically the same. I would recommend either plotting only one year or averaging over two years. The same applies for Figures 3, 5, and 7.

4. As discussed in the paper, precipitation from reanalysis product is not the best product available. I would recommend using Global Precipitation Climatology Project (GPCP) or CPC Merged Analysis of Precipitation (CMAP) precipitation as validation

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data set.

5. In Figure 7, the authors compared model simulated GPP to an up-scaled GPP product based on flux towers (B10). But the B10 data are over different periods. The FLUXCOM that is from the same research group as B10 has GPP product over 2009 and 2010. I would recommend comparing the model simulated GPP to the FLUXCOM GPP over the same time period.

6. Figures 10 and 11 are not very informative. The differences shown in Figure 10 are a convolution of transport errors and the errors in the surface fluxes. Figure 11 uses the same transport model, so the differences are only due to surface fluxes, which have been discussed in Figure 9.

7. Tables 4 and 5 list the RMS and bias statistics of posterior CO2 relative to independent CO2 observations from TCCON and HIPPO campaigns. I would recommend adding one plot showing time series comparison between model simulated CO2 and TCCON, and one latitudinal plot showing the comparison between model simulated CO2 and HIPPO data, which may be more informative than the final statistics. I would also recommend adding a figure showing the comparison between posterior CO2 and the CO2 flask data assimilated in the flux inversion system, which will show how well the inversion system fits the assimilated data.

8. The inversions use the 3D CO2 fields from CarbonTracker as initial CO2 fields in the inversion. This is risky since the transport models between TM5 used in the CarbonTracker and GEOS-Chem is different. The initial fields works for the CarbonTracker does not necessarily the best for GEOS-Chem.

9. Some descriptions in the paper are not well justified by the results. For example, in the last paragraph in section 4.2.1: “CTEM-GEM flux estimates are within the range of the other estimates from TEMs used as a priori estimates in flux inversions (i.e., BEPS) or measurement-constrained fluxes (i.e., CT2013 B).”. This is not well justified since CTEM-GEM apparently has large differences with other fluxes over the tropics due to

the bias in precipitation. This further reinforces my first point that it is not necessary to have consistent meteorology between CTEM and GEM to do carbon cycle data assimilation. It is important to have best meteorology.

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