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Geoscientific Model Development Discussions

Interactive comment on "The Canadian atmospheric transport model for simulating greenhouse gas evolution on regional scales: GEM-MACH-GHG v.137-reg" *by* Jinwoong Kim et al.

Anonymous Referee #1

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Review of Kim et al.: The Canadian atmospheric transport model for simulating greenhouse gas evolution on regional scales: GEM-MACH-GHG v.137-reg

General comments: the authors have developed an integrated greenhouse gas transport system from the global to regional scales. In this study, the regional domain of interest they chose is Canada and the United States. Three experiments were set up and evaluated with various meteorological and CO2 data. These experiments are GLB90, GLB45, LAM. GLB90 is the global model output with 0.9 deg grid spacing, GLB45 is similar to GLB90 but with 0.45 deg grid spacing, and LAM is the regional

setup at the 10 km resolution driven with GLB45' meteorology and CO2. All of the experiments used the same surface fluxes from CT2016. That the main message of this study delivers is the high-resolution outperforms the simulations.

The content of the manuscript lies in the category of the model description papers of GMD and meets the requirement of this category. The manuscript is generally well written, and the analyses presented are fairly well, I would like to recommend for publication after addressing my concerns as follows.

1. The CT2016 posterior CO2 mixing ratios are used as a baseline in Figure 7. It seems to me that the transport difference causes much bigger discrepancy than those by grid spacing. Both of Carbon Tracker and GEM-MACH-GHG are an operational system, and this is a forefront work towards (regional) flux estimates. One recent study on the transport uncertainty (Schuh et al, 2019) demonstrates the large difference in CO2 mixing ratios between TM5 and Geos-Chem models using the same CT surface fluxes. They show that the inverse model flux estimates for large zonal bands can have systematic biases of up to 1.7 PgC/year due to largeâĂŘscale transport uncertainty. The difference between CT2016 and GEM-MACH-GHG models in Figure 7 implies the bias in the GEM-MACH-GHG transport. I would expect the authors to have a discussion on the possible strategies to improve GEM-MACH-GHG transport since it is an operational system as Carbon Tracker. Additionally, my personal experience of working with CT2016 and CT2017 is that CT2017 outperforms the posterior CO2 mixing ratios a lot over CT2016. I encourage the authors to add CT2017 results in Figure 7 as well.

2. The authors use site codes quite often in writing. It may cause difficulties and confusion to some readers who are not familiar with the geographic locations of those sites to follow the statements. I recommend the authors to add the codes of the stations in Figure 1 for the references.

3. In the manuscript, the authors gave the credit to LAM for resolving the complex

terrain better. This is an overstatement. I don't see clear improvement at the mountain sites (such as BAO, in Figure 9 and 10) or coastal sites (such as ESP, in Figure 9 and 10). I was puzzled by that because I also expected the outperformance from the high-resolution simulations. One of the reasons that the improvement of the high-resolution simulations is not the evidence is that the analyses/figures shown were averaged over a relatively long period (monthly or seasonally). The improvement should be more evident in a higher frequent timescale, such as daily or even hourly due to resolving faster physics. I recommend the authors to select a or a few cases and to show hourly/daily time series for that/those case(s), as Figure 15 in Agustí-Panareda, et al., 2019, by which the authors should be able to justify the statement better.

Specific comments:

1. P2/L12, add a more recent work from OCO2MIP (Crowell et al., 2019)

2. P16/L20, "from Figure 15, it can be concluded that higher horizontal resolution might help to enhance the performance of CO2 simulations even without using fluxes on a finer grid spacing " is an overstatement. I don't see the statement is true. Please clarify it. Even though the observation network is sparse, there are still a few sites in the domain of interest. I would like to see the same matrices from the observations overlaid to the model values in Figure 15.

3. P17/L28, "This is a promising result because it suggests that using night-time data in an inversion to estimate night time fluxes (e.g. Lauvaux et al., 2008) may be beneficial if a high-resolution model is used. " This can be demonstrated by using CT2017. CT2017 has assimilated nighttime data, but CT2016 doesn't.

4. P18/L33, add Feng et al, 2016, another study demonstrates that both of the high-resolution transport and fluxes are demanding for accurate CO2 simulations at the urban scale. It also links to the last paragraph of the conclusion.

5. P19/L7, add Díaz-Isaac et al., 2018, which demonstrate that the meteorological

IC/LBC is one of the big players in regional CO2 simulations.

6. Figure 1, add the codes of the observation sites.

7. Figure 7, it's a very dense figure. The panels should be enlarged. To make the results stand out, the authors should use lines instead of markers for presentation.

8. Figure 8, remove "Should replace STD by STDE" in the caption.

9. Figure 12, replace the greens with another color. The green numbers are not distinguishable in most of the cases. The figures should be enlarged.

10. Figure 14, can the author extend the time scale from 92 D to at least half year? It should be very interesting results from hourly to a half year time scale.

11. Figure 15, add "in" following "zoomed" in the caption.

Reference: Agustí-Panareda, A., Diamantakis, M., Massart, S., Chevallier, F., Muñoz-Sabater, J., Barré, J., Curcoll, R., Engelen, R., Langerock, B., Law, R. M., Loh, Z., Morguí, J. A., Parrington, M., Peuch, V.-H., Ramonet, M., Roehl, C., Vermeulen, A. T., Warneke, T., and Wunch, D.: Modelling CO2 weather - why horizontal resolution matters, Atmos. Chem. Phys., 19, 7347-7376, https://doi.org/10.5194/acp-19-7347-2019, 2019. Crowell, S., Baker, D., Schuh, A., Basu, S., Jacobson, A. R., Chevallier, F., Liu, J., Deng, F., Feng, L., McKain, K., Chatterjee, A., Miller, J. B., Stephens, B. B., Eldering, A., Crisp, D., Schimel, D., Nassar, R., O'Dell, C. W., Oda, T., Sweeney, C., Palmer, P. I., and Jones, D. B. A.: The 2015-2016 carbon cycle as seen from OCO-2 and the global in situ network, Atmos. Chem. Phys., 19, 9797-9831, https://doi.org/10.5194/acp-19-9797-2019, 2019. Díaz-Isaac, L. I., Lauvaux, T., and Davis, K. J.: Impact of physical parameterizations and initial conditions on simulated atmospheric transport and CO2 mole fractions in the US Midwest, Atmos. Chem. Phys., 18, 14813-14835, https://doi.org/10.5194/acp-18-14813-2018, 2018. Feng, S., Lauvaux, T., Newman, S., Rao, P., Ahmadov, R., Deng, A., Díaz-Isaac, L. I., Duren, R. M., Fischer, M. L., Gerbig, C., Gurney, K. R., Huang, J., Jeong, S., Li, Z., Miller, C. E., O'Keeffe, D., Patarasuk, R., Sander, S. P., Song, Y., Wong, K. W., and Yung, Y. L.: Los Angeles megacity: a high-resolution land-atmosphere modelling system for urban CO2 emissions, Atmos. Chem. Phys., 16, 9019-9045, https://doi.org/10.5194/acp-16-9019-2016, 2016. Schuh, A. E., Jacobson, A. R., Basu, S., Weir, B., Baker, D., Bowman, K., et al. (2019). Quantifying the impact of atmospheric transport uncertainty on CO2 surface flux estimates. Global Biogeochemical Cycles, 33, 484-500. https://doi.org/10.1029/2018GB006086

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