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Atmospheric Chemistry and Physics Discussions

Interactive comment on "3-D evaluation of tropospheric ozone simulations by an ensemble of regional Chemistry Transport Model" *by* D. Zyryanov et al.

Anonymous Referee #3

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This paper presents a comprehensive evaluation of 5 regional and 1 global chemistry transport models (CTMs) in simulating the vertical profiles and horizontal distributions of tropospheric ozone over Europe using observations from balloon soundings, commercial aircrafts and satellite measurements during June to August 2008. To my knowledge, it is indeed one of the first of its kind for performing a detailed 3-D evaluation of CTM simulated ozone at regional scales. The paper is well written. The major conclusion that the RMSE values are constantly growing with altitude while the correlation forms a C-shape structure with the lowest correlation found near 8 km is rather interesting. Having said that, I do have a few major and minor comments for the authors to consider when they revise their manuscript.

A. Major comments:

1. The authors put forward a few quite speculative reasons such as coarse horizontal resolution, uncertainties in long-range transport and emissions, and limitations of chemistry schemes to explain why RMSEs grow with altitude and why correlation appears to be the smallest near 8 km. I think that this is not enough. The authors need to do more diagnostics. One thing that the authors can do is to examine the model meteorology at one or two sites within the domain using atmospheric soundings to see how good or bad the model temperature and winds compare with the observations. Another thing that the authors can do is to perform some sensitivity experiments by changing the emissions rates of ozone precursors using one CTM. Since the ensemble of CTMs were run with various resolution and chemistry schemes but still showed more or less similar distributions in RMSE and correlations, deficiencies in model meteorology and uncertainties in emissions did appear to play important roles. I think that the suggested diagnostics may be able to sort those things out.

2. The authors suggest that the somehow poor performance of CTMs at some of the western stations may be related to the boundary conditions. Though it is a logic way to think this way, I am not convinced that the boundary conditions impact the western stations much more than the interior stations. I am not sure how far away the western stations are from the model boundaries in grid numbers but I think that nowadays both CTMs and meteorological models are pretty good at dealing with boundary discontinuities and noises. Even though the models produce some unwanted waves along the boundaries, the waves would eventually impact the interior stations as much as they impact the stations near the boundaries.

I think that the relatively poor performance at some of the western stations may be related to the land-sea contrast and mountainous terrain that induce mesoscale circulations. These circulations would control the transport and dispersion of the pollutants but the models may not resolve the circulations well as the authors briefly mentioned. I would like the authors to discuss more along this line. Since Frankfurt had nearly two

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vertical profiles per day during the study period, the authors may be able to compare the simulations and observations at different times of the day and see if the CTMs perform differently, which may indicate some model issues with representing day/night or early morning/late afternoon circulations.

3. Summertime corresponds to strong forcing at surface that tends to generate convective clouds and circulations. I am wondering if the authors ever looked at the satellites images and atmospheric soundings during the months to see how high the convective clouds might be (i.e., use satellite "measured" cloud top temperature and compare with the soundings to determine the cloud top). If the top of the clouds were located around 8 km, then we might be able to explain the C-shaped structure of correlation since numerical models still can't handle clouds good enough.

B. Minor comments:

1. The authors use "planetary boundary layer", "free troposphere", and "upper troposphere" to denote the three layers between 0-2 km, 2-8 km and 8-10 km in the vertical. I would suggest the authors to use "middle troposphere" instead of "free troposphere" since in meteorology, "free troposphere" refers to the layer above the planetary boundary layer which includes the upper troposphere.

2. Along this line, I am wondering what is the reason behind the classification of these three layers in the vertical. Is it due to changes in ozone concentrations or different dynamic and chemical processes? The authors need to make this clear in the beginning of the paper.

3. I am wondering how the model runs were set up, i.e., daily restart or continuous runs from June 1 through August 31, 2008? How often the boundary (meteorological and chemical) conditions were updated?

4. The authors used MOZART-IFS at times and IFS-MOZART at other times. Please be consistent throughout the paper. If the authors do need to use MOZART-IFS and

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IFS-MOZART separately, please explain the differences.

5 Acronyms should be defined upfront. I would suggest the authors to have a table in the introduction section with all the acronyms defined.

6. I would suggest to change ”associated to” to “associated with”, and “participate to” to “participate in” throughout the paper.

7. Please change “through” on Pages 3, 24, 25, and 26 to “trough” (i.e., ridge and trough not ridge and through).

8. Please improve the writing of the paper by paying attention to details. For example, on Page 3 Line 18, “during summer is well catched” should be “during summer is well captured”; on Page 5 Line 18, “It is proposed here is to conduct” should be “What is proposed here is to conduct”; on Page 16 Line 16, the second “either as” should be “or as”; on Page 21, Line 7, “on Fig. 4” should “in Fig. 4”; on Page 27 Line 1, “to catch to a full extent” should better be “to capture to a full extent”.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 28797, 2011.

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