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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hindcast experiments of tropospheric composition during the summer 2010 fires over Western Russia" *by* V. Huijnen et al.

Anonymous Referee #1

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General comments

The paper examines the ability of the global MACC (Monitoring Atmospheric Composition and Climate) atmospheric composition forecasting system to predict evolution of several major compounds during large-scale pollution episodes caused by wildfires. It also investigates the influence of a priori emission information and data assimilation on the accuracy of the forecasts. The study focuses on the extreme air pollution event in Western Russia in summer 2010. The authors performed four different 4-day hindcast runs with and without using data assimilation and daily emissions from fires. The "background" scenario was based on the GFEDv2 monthly mean "climatological" emissions calculated from the years 2001–2006. The daily fire emissions taken from the Global Fire Assimilation System (GFAS) were derived from satellite fire

radiative power retrievals. The system performance is evaluated against various measurement data that were not used in the assimilation. It is found that both including GFAS emissions into the model and constraining initial conditions by assimilation results in significant improvements in the accuracy of hindcasts of aerosol optical depth (AOD) and carbon monoxide (CO). Smaller improvements are found in the cases of ozone (O3) and formaldehyde (HCHO). For NO2 columns, the mean bias strongly decreased against both SCIAMACHY and OMI data, while RMSE significantly increased with the SCIAMACHY data and insignificantly changed with the OMI data.

In my opinion, this study is important, and the results may present substantial interest for the atmospheric scientific community. The text is well written. However, the paper should be further improved before it can be recommended for publication in ACP.

Major concerns

1. The scientific goals of this study are not defined sufficiently clearly. It is said in the abstract that the "extreme event is used to evaluate the ability of the global MACC (Monitoring Atmospheric Composition and Climate) atmospheric composition forecasting system to analyze large-scale pollution episodes and to test the respective influence of a priori emission information and data assimilation on the results". These goals and questions formulated in Introduction mostly concern a specific modeling system and have technical character. I think that the authors should try to put this study into a broader scientific context and to emphasize its importance. In particular, the following questions could be addressed in Introduction:(i) What is the place of the MACC system among other similar modeling systems (if they exist)? (ii) Has the impact of fire emission estimates on atmospheric composition forecasting been already addressed in any other studies, (iii) Have any other modeling systems been evaluated against simultaneous satellite measurements of several species during an extreme air pollution event caused by wildfires? It would also be useful to provide a brief overview of previous studies attempting validation of fire emissions by comparing model results with measurements; (iv) What are possible practical applications of forecasting air pollution on

the global scale? Are such forecasts needed to provide better boundary conditions for regional models and/or to improve meteorological forecasts? Are there any examples of such applications?

2. In most cases, the difference between the error statistics of the Assim and Assim-GFAS runs is very small. This is a rather puzzling result, which may mean that the model does not add any important information to the forecasted characteristics in comparison with corresponding contribution of measurements. It also seems possible that the assimilation system is not well optimized. Since one of the goals of this study is to evaluate the MACC system, it would be important to demonstrate that the information from the model is combined with observations in an optimal way. My suggestion is to perform two more runs with the error covariances increased or decreased (globally) by, e.g. 50 percent. Such experiments would clarify the respective contributions of the model and observations to the forecasts and could help in identifying possible ways to further improve the MACC system performance.

3. The IASI measurements show high ozone columns over Kazakhstan which are not reproduced by the model. The authors discuss several possible reasons but recognize that the true reasons are not fully understood. It also remains unknown whether this feature is due to uncertainties in the IASI retrieval or due to some deficiencies in the model. I believe that the authors should put some more efforts in elucidating the origin of this puzzling ozone high. In particular, the respective OMI data for the upper troposphere (which are anyway used in the assimilation) could be considered for this purpose.

4. The accuracy of the hindcasts is evaluated in terms of the bias and RMSE. Another important metric which is used in most of forecasting studies is the correlation coefficient (for time series). Values of the correlation coefficient should be provided along with the bias and RMSE in order to facilitate using the results of this study for future references.

5. Time series are shown for CO and ozone but not for NO2 and HCHO. For HCHO, even the biases and RMSE are not reported. The missing figures and tables should be provided. My opinion is that scientific results should be presented in an objective way, even if some of them may not look "nice". The same comment also concerns Fig. 13, where an unspecified spatial smoothing is applied to the SCIAMACHY measurements. The original measurements should also be shown.

Other comments

Abstract, l.7.: "analyze": In my understanding, the paper discusses the ability of MACC to forecast rather than to analyze large-scale pollution episodes.

Introduction, p. 31853. The statement: "a range of observations were used in various studies to characterize the tropospheric composition during this episode ..." may be understood such that all of the four mentioned studies are purely observational (in contrast to the study by Huijen et al.), what is not true. The respective paragraph should be revised and extended to allow a reader to get a better and more accurate idea about the previous studies of the same episode.

p. 31857, l. 4,5: " It applies the same 60 level vertical discretization as the IFS, but the horizontal resolution is 3_ lon×2_ lat, globally". Is the horizontal resolution of IFS different?

p. 31859, l. 20. "23 percent": Where this critical value is taken from (reference)?

p. 31860, l. 18-21: "Furthermore, the CO and HCHO emissions are much higher than the monthly-mean GFEDv3.1 emissions. This is mainly caused by the different predominant soil type maps used in GFEDv3.1 and GFASv1.0". Can the authors justify the last statement, or is it simply a guess?

p. 31860: "the CO and HCHO emissions are much higher than the monthly-mean GFEDv3.1 emissions": It is mentioned on the page 31858 that the conversion factor was derived with a linear regression between the observed fire radiative energy and the dry matter burned in the GFEDv3.1 inventory. Do these facts imply that GFAS estimates are much smaller than the GFEDv3.1 data in some other regions. Are so large differences between GFAS and GFEDv3.1 typical, or these region and event are exceptional? Please comment.

p. 31860, l. 28: "The CO emissions are ∼25% higher than Konovalov et al. (2011)" According to Table 3 of the reviewed paper, CO emissions from fires in the considered regions during July and August are ∼ 13.3 Tg, while according to Table 4 in Konovalov et al. (2011) the fires in European Russia emitted about 12.8 Tg of CO in the same period. That is, the difference is actually less than 4 percent. This is an encouraging agreement. However, the considered regions are not quite identical. Please correct the discussion accordingly (including the conclusions).

ibid: "... higher than Konovalov et al. (2011)" => "... higher than in Konovalov et al. (2011)"

p. 31862, l. 28: "The RMSE for D+3 in run Assim-GFAS is slightly worse than the one in run Assim": I would not say that the difference between 0.77 and 0.52 (that is, almost 50 percent) is really a "slight" difference.

p. 31866, l. 16, 17.: "key spatial patterns are captured by the model, such as the north-south gradient in O3 columns over Western Russia". What are the other "key spatial patterns" captured by the model?

p. 31866, l. 23: "This is larges for run CNT" => "It is largest for the run CNT"

p. 31869: I do not think that the large difference in RMSE obtained with SCIAMACHY and OMI data is explained sufficiently; the corresponding discussion should be extended. In particular, can these differences relate to any differences in the retrieval algorithm? What is the potential impact of optically dense aerosols on the different satellite NO2 data products (see, e.g., Leitao et al., 2010)?

p. 31872, l. 22: "The increase in CO lifetime illustrates a reduction of the hydroxyl

radical (OH) concentration". Would not it be better to say that "the increase in CO lifetime is due to a reduction of the hydroxyl radical (OH) concentration"?

p. 31876, l. 8: "An important factor for the accuracy of the fire emissions was the development of a detailed soil map". I do not think that the importance of this factor is really demonstrated in this study. Did authors try using emissions obtained with some other soil map?

Reference:

Leitao J., A. Richter, M. Vrekoussis, A. Kokhanovsky, Q. J. Zhang, M. Beekmann, and J. P. Burrows, On the improvement of NO2 satellite retrievals – aerosol impact on the airmass factors, Atmos. Meas. Tech., 3, 475–493, 2010, www.atmos-meas-tech.net/3/475/2010/.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31851, 2011.