Atmos. Chem. Phys. Discuss., 14, C11602–C11605, 2015
www.atmos-chem-phys-discuss.net/14/C11602/2015/

Atmospheric Chemistry and Physics Discussions

ACPD

14, C11602–C11605, 2015

Interactive Comment

Interactive comment on "Biomass burning influence on high latitude tropospheric ozone and reactive nitrogen in summer 2008: a multi-model analysis based on POLMIP simulations" *by* S. R. Arnold et al.

Anonymous Referee #2

Received and published: 26 January 2015

GENERAL COMMENTS:

This manuscript explores the behaviour of tropospheric ozone and related gases in air dominated by biomass burning emissions in the Arctic region. Results from POLMIP simulations are examined, while further experiments are performed with a Lagrangian model to explain how differing composition and transport can explain the diversity of ozone production in POLMIP models. This analysis is a substantial and useful addition to the body of literature existing on this topic, as it examines the processes involved in much detail, and therefore provides further insight. The manuscript is well written, and suitable for publication in ACP following some amendments listed below:

- There is no mention of how photolysis is treated in the models. Certainly radiation-driven processes are important in the summer Arctic troposphere, and it would be good to describe how this is simulated in the models. Are "real-time" clouds, aerosols, surface albedos affecting the simulation? And how do factors such as the above behave during the period of study in that location (Mostly cloudy conditions or not? Air masses passing over ice? etc). Ideally there should be some analysis of the photolysis rates themselves - in addition to the concentrations and chemical fluxes - but if that is not possible, at least some discussion would be useful to the reader.

- Ozone-CO correlations/slopes can be very useful, but being positive is not always indicative of net ozone producing regions, especially over remote areas (as suggested by Voulgarakis et al. (2011) and later also discussed by Kim et al. (2013) and Zhang et al. (2014)). Examining the OH levels in comparison to the background and subsequent CO destruction fluxes would give an indication on the validity of this approach.

MINOR COMMENTS:

Page 24580, Lines 6-7: A little more justification of the choice of a 25-day lifetime is needed, for the more general readership.

Figures 1-4: It would be good to clearly label the axes, i.e. which one is the observations and which one is the model?

Page 24582, Lines 4-6: Worth mentioning that models typically underestimate CO in the northern extratropics, e.g. see Naik et al. (2013), Fig. 2 for a recent multi-model comparison.

Page 24583, Line 4: Please spell out "oVOC" as it is the first time it is encountered in the text.

Page 24583, Lines 4-7: Any ideas on why oVOCs show such a large variability? Is it a

ACPD

14, C11602–C11605, 2015

result of different emissions, or of atmospheric processing?

Figure 4: For SMHI-MATCH the bias appears positive (95%), but visually the figure suggests a negative bias.

Figure 5: “(k)” should not be bold.

Figure 6: I would suggest using consistent colouring for young/aged in Figures 5 & 6.

Figures 5 & 7: In those figures, some model names are different to previous figures.

Page 24587, Lines 22-24: I presume the authors here imply how future model developments regarding convection could affect the results. This should be more clearly stated.

Page 24589, Line 13: Please change “represented” to “be represented”.

Page 24590, Lines 10-13: It is not clear to me that this is the case. E.g. the CIFS model looks much more similar to TM5 next to it or CAM5 above it rather than to SMHI-MATCH.

Table 2: It would be useful to show OH on this table too, in order to get a sense of the variability between the models.

Page 24594, Lines 13-18: Is the factor of 2 arbitrary or based on the typical diversity range in the POLMIP models (e.g. from Emmons et al., 2014)? Please clarify.

Page 24595, Lines 3-6: I am not sure I understand, though I may be missing something here: All the lines in Figure 14c seem to be below the zero line, so I am not sure where one can see an enhancement of ozone.

Page 24595, Line 16: Please change “changes the rate” to “changes of the rate”.

Page 24596, Line 11: Please change “differences efficiency” to “differences in the efficiency”.

REFERENCES:

ACPD

14, C11602–C11605, 2015

Voulgarakis, A., Telford, P. J., Aghedo, A. M., Braesicke, P., Faluvegi, G., Abraham, N. L., Bowman, K. W., Pyle, J. A., and Shindell, D. T.: Global multi-year O3-CO correlation patterns from models and TES satellite observations, Atmos. Chem. Phys., 11, 5819-5838, doi:10.5194/acp-11-5819-2011, 2011. Kim, P. S., Jacob, D. J., Liu, X., Warner, J. X., Yang, K., Chance, K., Thouret, V., and Nedelec, P.: Global ozone–CO correlations from OMI and AIRS: constraints on tropospheric ozone sources, Atmos. Chem. Phys., 13, 9321-9335, doi:10.5194/acp-13-9321-2013, 2013.

Zhang, B., Owen, R. C., Perlinger, J. A., Kumar, A., Wu, S., Val Martin, M., Kramer, L., Helmig, D., and Honrath, R. E.: A semi-Lagrangian view of ozone production tendency in North American outflow in the summers of 2009 and 2010, Atmos. Chem. Phys., 14, 2267-2287, doi:10.5194/acp-14-2267-2014, 2014.

Naik, V., Voulgarakis, A., Fiore, A. M., Horowitz, L. W., Lamarque, J.-F., Lin, M., Prather, M. J., Young, P. J., Bergmann, D., Cameron-Smith, P. J., Cionni, I., Collins, W. J., Dalsøren, S. B., Doherty, R., Eyring, V., Faluvegi, G., Folberth, G. A., Josse, B., Lee, Y. H., MacKenzie, I. A., Nagashima, T., van Noije, T. P. C., Plummer, D. A., Righi, M., Rumbold, S. T., Skeie, R., Shindell, D. T., Stevenson, D. S., Strode, S., Sudo, K., Szopa, S., and Zeng, G.: Preindustrial to present-day changes in tropospheric hydroxyl radical and methane lifetime from the Atmospheric Chemistry and Climate Model Intercomparison Project (ACCMIP), Atmos. Chem. Phys., 13, 5277-5298, doi:10.51

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 24573, 2014.

ACPD

14, C11602–C11605, 2015