Atmos. Chem. Phys. Discuss., 11, C755–C757, 2011
www.atmos-chem-phys-discuss.net/11/C755/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A regional chemical transport modeling to identify the influences of biomass burning during 2006 BASE-ASIA" *by* J. S. Fu et al.

Anonymous Referee #1

Received and published: 11 March 2011

General comments: This paper describes a modeling study to identify the impacts of biomass burning in Southeast Asia on air quality in 2006. Two 2-day biomass burning episodes were analyzed using a regional chemical transport model and two fire emission databases. Influence of biomass burning on CO, O3 and PM2.5 was discussed. In general, description of modeling method and evaluation of model performance is very poor. The main problem however, is that a regional offline model system (WRF: meteorology, CMAQ: chemistry) was used to investigate the influence of biomass burning in this study (i.e. meteorological output is given as input to chemical model). Although the effects of biomass burning on atmospheric properties such as temperature and solar irradiation are mentioned in the introduction, there is no indication that the authors

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

took them into account. The offline model system used in this study cannot consider the radiative effects of biomass burning aerosols. Changes in the atmospheric properties due to biomass burning aerosols might have a significant effect on chemistry. Such studies normally use coupled models to include two-way impacts. There is no discussion about it in this manuscript.

Specific comments: Emission inventories: It is not easy to understand why authors used GFED inventory at all when the drawbacks were already known at the beginning (very coarse temporal resolution (8-day), limitation in spatial coverage of MODIS) while FLAMBE has a much better resolution (hourly).

Model evaluation : This section is very confusing. The parameters used for model performance analysis should be defined clearly. Authors used MFB and MFE for both O3 (15 and 35%, respectively) and PM2.5 (50 and 75%, respectively). It seems they were applied to CO as well (Figures 2 and S1) but this is not given in the text. In literature, MFB $\leq$ 60% and MFE $\leq$ 75% are recommended for particles (Boylan and Russell, 2006). For O3, USEPA recommends MNB $\leq$ 15% and MNG $\leq$ 35%. The definition, justification and correct references for the statistical parameters used for O3, CO and PM2.5 should be given. The names of the parameters in the text and in figures are not the same.

Model seems to underestimate ozone in the afternoon significantly (up to 35 ppb difference) and overestimate it at night (Figure S1). Some discussion about it is needed.

Modelled PM2.5 is too low -about 5-10 times lower than measurements. There must be some information what species are included in the model PM2.5 (primary, secondary). Are biogenic SOA precursors included in the emissions? Without any chemical composition it is not possible to explain such a big difference.

Technical comments: Figure 1: caption : Five (not Fiver)

Figure 2: Although nowhere mentioned, modeling period seems to be between 1 April

ACPD
11, C755–C757, 2011

and 31 May (Figure 2, Figure S1). However discussion is about 27-28 March. Is something wrong with time axis? Julian date (91-151) doesn't seem to match the episodes. The x-axis should be given in calendar date.

Figure 3: What is the height of CMAQ column? (top of 19 layers)

Line 3084: in text as AOT, in figure 4 as AOD.

Reference for model evaluation Morris 2005 cannot be found easily. More appropriate reference should be chosen.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 3071, 2011.