

Interactive comment on “Simulations of Organic Aerosol Concentrations during Springtime in the Guanzhong Basin, China” by Tian Feng et al.

Anonymous Referee #2

Received and published: 3 May 2016

This manuscript is clearly written and on an important topic on SOA simulation. It is easy to follow and some of its conclusions are interesting. But I am not happy respecting to following points. 1) The model and the method used in this application are nearly the same as in Li et al. (2011b), and one major conclusion is obvious that NT-SOA produces higher SOA than T2-SOA, because the total amount of material (POA+SVOC+IVOC) introduced in the NT-SOA module is 7.5 times the particle-phase POA emissions. Besides, many studies have already shown that VBS produces higher SOA than the traditional 2-product SOA module. 2) It does not clearly show whether diurnal (time) variation in SOA concentration is improved by including VBS than the traditional 2-product SOA module. NT-SOA module contains more precursors and production processes, is it good or not if “The diurnal variations from the two models agree well with each other, with peak occurrence during noontime, caused by the enhanced photo-

Printer-friendly version

Discussion paper



chemical activities.” as shown in Figure 10? 3) The model performances are evaluated against surface observed O₃, PM_{2.5}, EC and OA, but the importance is not clearly seen to compare with satellite derived AOD. Contribution of OA or SOA to AOD is not significant, and it is not clearly stated in the manuscript how the model calculates AOD, and AOD values depend on aerosol number and size distribution, mixing state and air humidity, which are beyond the scope of this study. So good agreement between simulated and satellite retrieved AOD does not imply the model simulates PM_{2.5} and OA well.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-224, 2016.

[Printer-friendly version](#)

[Discussion paper](#)

