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Discussions

Interactive comment on "Modeling anthropogenically-controled secondary organic aerosols in a megacity: a simplified framework for global and climate models" *by* A. Hodzic and J. L. Jimenez

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Review of "Modeling anthropogenically-controlled secondary organic aerosols in a megacity: a simplified framework for global and climate models" by A. Hodzic and J. L. Jimenez

This paper describes a simple approach of treating anthropogenic SOA and SOA associated with biomass burning in 3D models. For these SOA contributions, a parent SOA precursor is co-emitted with CO (with a constant emission ratio to CO) and oxidized to

form SOA with a first-order reaction with OH. The authors also explore possible effects of anthropogenic species on biogenic SOA and conclude that while these effects can increase the biogenic SOA mass, they cannot reproduce the measured diurnal cycle of SOA.

This paper provides a new alternative approach for modelling anthropogenic and biomass burning SOA, and the paper fits within the GMD goals. I believe that it should be published once the following comments have been addressed.

General comments

1. I agree with referee #1 that there are weaknesses in the conclusions regarding the enhancement of anthropogenic species on biogenic SOA. Using the latest NOx-dependent yields of biogenic SOA precursors that show larger yields in high-NOx conditions could be important. This should at least be discussed in the paper.

I realize that the authors used SO2 as a proxy for aerosol acidity to be consistent with Spracklen et al. (2011). However, I would guess that SO2 and aerosol acidity are only weakly correlated with aerosol acidity near Mexico City. Since the model already predicts sulfate, ammonia and nitrate, it explicitly predicts inorganic aerosol acidity. While organics can modify this acidity, the inorganic acidity should be a much better predictor of aerosol acidity than SO2.

I think it is unlikely that the effect of anthropogenic pollution on biogenic SOA could account for the diurnal cycle in organic mass; however, I feel that the above approaches would be an improved approach. These issues must at least be discussed in the revised paper.

2. It was my impression that the IVOC distribution and the aging rates in Robinson et al. (2007) were first guesses with significant room for improvement. Thus, the Robinson approach could also easily be tuned to better match MILAGRO observations too. This should be discussed in the paper.

Furthermore, the authors are already using a VBS approach for biogenic species, but is not discussed in their comparison of the number of species with the Robinson and Shrivastava formulations. Thus, I believe the computational benefits of the current approach are overstated relative to these previous works. I discuss this issue more in the specific comments.

Specific comments

Page 872, lines 9-10: Multiple size bins is not necessary for aerosol mass predictions (even though it is used in this paper).

Page 873, lines 1-20: How is the OA/delCO enhancement corrected for biogenic SOA?

Page 880, line 3: Why is SO2 used as a surrogate for acidity? See general comment.

Page 881, lines 9-19: In the discussion of the necessary number of species in the new model formulation, there is no mention of the biogenic species. You are already using a 4-bin VBS approach for this. Therefore, there is an additional 40 (9*4+4) species (58 total when including the 18 ASOA/BBSOA species), which gives you slightly MORE species than the Shrivastava approach where biogenic and anthropogenic organics are lumped together by volatility. Thus, I feel that the computational benefits of the current model configuration is overstated.

Page 884, line 13: Please also cite Riipinen et al. (2011) and Pierce et al. (2011), which also show low volatility SOA using different methods than the other articles cited.

Riipinen, I., Pierce, J. R., Yli-Juuti, T., Nieminen, T., Hakkinen, S., Ehn, M., Junninen, H., Lehtipalo, K., Petaja, T., Slowik, J., Chang, R., Shantz, N. C., Abbatt, J., Leaitch, W. R., Kerminen, V.-M., Worsnop, D. R., Pandis, S. N., Donahue, N. M., and Kulmala, M.: Organic condensation: a vital link connecting aerosol formation to cloud condensation nuclei (CCN) concentrations, Atmospheric Chemistry and Physics, 11, 3865-3878, 2011.

Pierce, J.R., Riipinen, I., Kulmala, M., Ehn, Petaja, T., Junninen, H., Worsnop, D.R.,

Donahue, N.M.: Quantification of the volatility of secondary organic compounds in ultrafine particles during nucleation events, Atmospheric Chemistry and Physics Discussions, 11, 14495-14539, doi:10.5194/acpd-11-14495-2011, 2011.

Page 887, line 2: Does Spracklen et al. (2011) suggest that 90% of the predicted mass is biogenic origin near Mexico City or globally? Is it realistic to assume these two values would be similar? Please make this discussion more clear.

Page 888, lines 4-5: “The difference is even larger for high NOx conditions and low organic mass (∼7 times).” What curve/panel are you referring to here?

Writing-related comments

Title: “anthropogenically-controlled” doesn’t need a hyphen because anthropogenically is an adverb, which already implies that it modifies controlled. It is not a joint adjective such as “human-generated SOA”.

Page 879, line 17, first sentence: It would be more clear to say “A similar approach as for anthropogenic SOA is adopted for biomass burning SOA formation”. The previous paragraph discusses POA aging, not the ASOA formation via ratio w/ CO, so the way the first sentence was written was confusing.

Page 884, line 22: “observation]based”

Page 888, line 8: There should be a period after Pandis et al. (1991), not a comma.

Page 890, line 15: parameters should be parameter.

Interactive comment on Geosci. Model Dev. Discuss., 4, 869, 2011.