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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

Interactive comment on "Evaluation of GEOS-5 sulfur dioxide simulations during the Frostburg, MD 2010 field campaign" *by* V. Buchard et al.

Anonymous Referee #2

Received and published: 16 October 2013

This paper presents a modeling study with the GEOS-5+GOCART global model that focuses on the sensitivity of surface level SO2 and sulfate to anthropogenic SO2 emissions, particularly the injection height of these emissions. Model results are evaluated using surface measurements from a monitoring network in the continental US, and aircraft and surface remote sensing measurements from a short duration field campaign over north-eastern Maryland. The main conclusion is that emitting SO2 from energy sources at 100-500 m rather than in the lowest model layer greatly reduces the strong high bias of the surface SO2 concentrations. However, this change had little impact on surface concentrations of sulfate, which have a weaker high bias, suggesting that sulfate removal may too slow in the model. The subject matter is appropriate for ACP, and the paper is concise and generally well written. It should be acceptable for publication

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in ACP after some revision.

My main concern is the lack of technical discussion about the appropriate injection height for SO_2 emissions from fossil fuel power plants. The elevation range used (100-500 m) is given without any justification or discussion. Plume rise of emissions from large point sources has been studied for many decades, and it is incorporated in the emission modules of several mesoscale to regional scale models (e.g., EPA CMAQ, WRF-Chem). Although information needed to estimate plume rise and injection height is not available (to my knowledge) for global emissions datasets, it is available for the US. The authors might consider it outside the scope of their study to incorporate such information, although it would certainly strengthen their results. Some technical discussion of injection heights is definitely needed.

Specific Comments

As noted by Referee 1, there are two differences between the control and revised run SO_2 emissions: magnitudes and injection heights. If there are appreciable differences in magnitudes, then the authors should perform a third simulation in which only one of these emissions differences was applied. Some of the discussion suggests that the emission magnitudes do not differ substantially. If this is the case, the third run is less important, but the authors should provide some quantitative comparison of the two emissions data sets. E.g., give the annual emissions for the entire globe, for the continental US (or the area in Figs. 1 and 2), and for the portion of the US where most of the monitoring sites (see Fig. 6) are located. Spatial correlation coefficients for the two emissions datasets might also be provided. Also, Figs. 1 and 2 should be revised to allow visual comparison.

STDV statistics. Like Referee 1, I was not sure that I understood how this is calculated. If STDV is simply the standard deviation of a simulation's results (hourly concentrations at the observation sites), then the importance of these statistics to the analysis is unclear. Please clarify both how STDVs are calculated and their importance. Also, in Fig.

6 and 8, would showing RMS rather than STDV results in the middle rows be more useful?

Section 3. Have the authors looked into seasonal and diurnal differences in the simulated surface SO2 bias? If these differences are small, then this could be stated in one or two sentences. If they are large, more discussion would be informative.

Tables 1-2 (number of points) and Figures 10-11 suggest that January 2010 results are missing for the control run. Please explain, and include run duration information (including spin-up) at top of p. 21769). If this is correct, is it appropriate to use different time periods for the Table 1-2 and Figure 5-8 results?

P. 21774, L. 1-3. I concur with the comment by P. Castellanos on sulfate removal rate. The authors should at least provide numbers for emissions increases and/or observed surface sulfate decreases from 2005 to 2010, and compare these to the (normalized) model bias for sulfate.

The model is global, so why limit evaluation to continental US? European observations (EMEP) could also be included. Also, does the injection height change have much impact on the SO2 and sulfate global lifetimes?

Section 4.1. Since Piney Run Station is "in a mountain valley", it is not ideal for evaluation of 25 km resolution model results, which probably do not resolve flow details at scales below about 50 km. Add some discussion about the topography here. What are the valley bottom and ridge top elevations and the valley width? Is the actual terrain much more complex than that used by the model? Also, consider showing surface SO2 on Nov. 8-9 separately from Fig. 11, with an expanded time axis.

Section 4.2 Add more discussion of the meteorology on and preceding Nov. 8 and 9. Was the weather generally clear, or were there low clouds or precipitation in the area (which would strongly affect SO2 concentrations), and if so, how well did the model simulate them? What were the wind speeds and direction below 1 km (where most of the SO2 was found) at the MFDOAS site on these days, and were there any shifts on Nov. 9 that might explain the MFDOAS downward trend?

Section 4.3. Please be more specific about the “major features” that the simulation captures. For example, the model values appear to drop off more rapidly with height (from within to above the PBL) than the measurements, and model is too low at ∼25-35 minutes. If the aircraft recorded air temperature, then how did the model's inversion heights compare to the observed. You compare control and revised run results in nearly all the figures, so why not show the control run SO2 in the rightmost (line) plots of Fig. 13, and discuss it in text?

Minor Comments

P. 21766, L. 15-16. I did not see any substantial discussion of “mixing processes in the model”.

P. 21767, L. 1-3. SO2 oxidation is quick only in the presence of clouds, and so is highly variable.

P. 21767, L 19. Change to “. . . SO2 losses due to oxidation and dry/wet removal” ?

P. 21768, L. 5. Change to “Representation of Aerosols and Sulfur Gases in . . .” ?

P. 21768, L. 18-21. Please give some information about the model's vertical resolution in the lowest 1 km, such as the number of vertical levels here.

P. 21769, L. 19. Change to “. . . over the US in 2007 (from Streets et al., 2009)” ?

P. 21770, L. 19-23. These two sentences would seem to fit better on P. 21769 after L. 17.

P. 21771, L. 3-4. Mention that Lee et al. results are for 2006.

P. 21771, L. 13-14. Differences in clouds and precipitation in between 2006 and 2010 could also be a factor here, as well as less SO2 dry deposition in the revised run

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because of the elevated emissions.

P. 21772, L. 5-6. Please state here that throughout the paper, “log” means natural (or base 10) logarithm.

P. 21772-3 or Tables 1-2. Please give the means and STDVs of the observations, as they help to put the model-observation comparison statistics (those in ppb and ug/m3 units) and model STDVs into perspective.

Figures. Axis labels and numbers could be larger on many of them.

Fig. 3. Choice of contour levels (which leave 80% of globe as white) could be improved.

Fig. 5. In the caption, add some explanation of the coloring (representing the PDF).

Fig. 9. It would be more informative to show topography (elevation contours) on the map rather that state outlines and rivers.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 21765, 2013.

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