Atmos. Chem. Phys. Discuss., 9, C6412–C6414, 2009
www.atmos-chem-phys-discuss.net/9/C6412/2009/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Impact of Chinese SO_2 emissions on submicron aerosol concentration at Mt. Tateyama, Japan" *by* K. Osada et al.

K. Osada et al.

kosada@nagoya-u.jp

Received and published: 30 October 2009

We thank Dr. Porch for very constructive comments with respect to the overall clarity of the article. Modified words or sentences are highlighted as yellow in the text.

Comment: This is an excellent paper describing trends over a ten year period of submicron aerosol concentrations at a site on a mountain in Japan and interpretations using meteorological and chemical modeling. The most surprising result is the relative lack of significant trends in nighttime aerosol observations during the summer and autumn when aerosol concentrations were the highest (Fig 4). This would seem to dampen recent speculation regarding a rise in global background aerosol (global dimming). The observed increasing trends in winter are very interesting and deserve more discussion and analysis.

Response: We added additional discussion and analyses, including that of the local meteorological situation, to the revised manuscript.

Comment: The authors state that the models predict that the influence of Chinese SO2 to sulfate emissions are high (60 to 80%) in winter-spring and low (20 to 80%) in other seasons. This statement deserves more discussion. The implication is that this is a wind direction effect, but this seems to conflict with the statement that "During the winter monsoon period (November-April), strong northwesterly winds prevailed with frequent snowfalls with rimed ice."

Response: We added additional discussion and a new figure related to backward trajectories to the revised manuscript.

Comment: The lack of trend in rainfall in Fig. 5 is interesting, but much more discussion is required to understand the possible effect of meteorological parameters. For instance, snowfall trends may be different than rainfall trends. Also, boundary layer trends derived from atmospheric temperature profiles may be important. Since the sampling site is at 2450 m a.s.l. trends (or lack thereof) in the strength and depth of mixing both over the source regions of China and at the receptor need more discussion. It is also possible that daytime ground heating in China can mix the SO2 above the nighttime inversion and the nighttime inversion can act as a smooth lower boundary so that the particles can more easily flow directly to the sampling site. These effects may (or may not be) included in the modeling, but more discussion is required.

Response: We added further discussion and analysis including that related to the local meteorological situation such as the type of precipitation at the foot of the mountain. We also added discussion of the limitations of our model systems in reproducing regional atmospheric stability and vertical motions related to changes in land use and light-absorbing aerosol concentrations in China.

Minor editorial comments: 1. pg 16529 (last paragraph) ". . .increasing trend in aerosol data." to ". . .increasing trend in aerosol concentrations."

Response: We corrected this point as suggested.

Minor editorial comments: 2. pg 16532 (1st paragraph): the summer eruption of the Miyakejima Volcano is mentioned but hard to see effect in (Fig. 2) maybe an arrow could be included in figure.

Response: The effects of SO2 emissions from the Miyakejima Volcano are not so large on total and SO42- aerosols at Mt. Tateyama. We added descriptions of the effects from the Miyakejima Volcano to the revised manuscript.

Minor editorial comments: 3. pg 16535 (2nd paragraph): the reference to Fig. 7 needs to be read very carefully to separate which data are observed (I think the red) and which are simulated results (I think blue). It would help if this were clarified in the text and included in the figure caption.

Response: We apologize for your inconvenience related to Fig. 7. Considering the overall clarity and structure, we changed the old Fig. 7 to the new Fig. 8 in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16527, 2009.