

## ***Interactive comment on “Chemical composition of atmospheric aerosols between Moscow and Vladivostok” by S. Kuokka et al.***

**S. Kuokka et al.**

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I found this paper to be generally thorough, well-written, and sound. The authors are to be commended on the large number of measurements they brought to bear on the atmospheric chemistry across Russia and back. The techniques and their application seem to be state of the art. I therefore have no problem in recommending this paper for publication. I do wish to comment on a few aspects of the manuscript, however. First is the writing style. Although it is generally very high, it varies a bit from place to place. My guess is that different sections were written, or at least drafted, by different authors. The most common thing I noticed was the Russian custom of omitting articles. I saw it mainly in section 2.1.

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The use of articles in the manuscript was checked.

In section 3.1, on trajectories, it might be noted that even though the authors generated back-trajectories for 100-m and 500-m starting heights (not arrival heights, as stated there), only the 100-m trajectories appear in the paper. Readers might look for both levels and wonder whether something was wrong with them when they couldn't find them. I also didn't see as much difference in the trajectories between the two legs of the trip as the authors made me expect to find. Except for the ones near Vladivostok, they seemed to be distinctly westerly. Perhaps the larger differences in the chemistry could be explained by differences in pathways farther out than 4 days, but it is hard to do that because trajectories longer than that become quite uncertain. While writing this, I just realized that I didn't remember anything about precipitation in the paper. If it didn't rain during the entire trip, it should be noted. If I have missed something, I am sorry.

There were only minor differences between the backtrajectories calculated for 100 and 500 m. For this reason only the 100 m trajectories are shown. This explanation has been added to the manuscript. The backtrajectories have been calculated to arrival heights.

The role of the precipitation was added to the manuscript (section 3.1).

A couple of comments on the elemental data. First, thank goodness that the authors used nitric and hydrofluoric acids to dissolve the entire silicate matrix. So many people try to skip this critical step, and it really limits the possible interpretations of the data. Second, I was very interested in the enrichments of As, Cd, Zn, Pb, and Sb in the Perm-Sverdlovsk area. They raise the possibility that this general area might have been part of the old "Central USSR" source deduced in earlier studies of Arctic Haze. Third, one must be careful when attributing sources to fine-particle Al, Si, etc. They could just as well have come from coal flyash as from soil. That is not to say that it had to be the case here, even with trajectories from China and Mongolia, but it needs to be kept in mind.

Because of the high temperatures and coarse-particle controls, coal-fired power plants will generally release finer-particle "soil" elements than soils and deserts will. I have a mixed reaction to the discussion of sources for the high-concentration region (section 3.5.1). On the one hand, the authors' explanation pointing to northeastern China and Beijing makes a lot of sense. On the other hand, their trajectories don't really get into the most polluted parts of this region, and certainly don't get near Beijing. Maybe this is another one of those cases of transport beyond the 4-day trajectories, which is certainly possible in this dry region.

"Near Beijing" was removed from the text.

I am a little puzzled by the behavior of sulfate vs. other things. In the low-concentration region, it did not correlate well with ammonium, nitrate, or black carbon. (It stayed high while they went low.) In the forest-fire region, it peaked along with the classical indicators such as potassium. I am guessing that both these cases are trying to tell us either that sulfate is more regional than those other species (which probably shouldn't surprise us too much), or that the two kinds of events are superimposed on a sulfaterich background of really distant aerosol. Maybe these two explanations are just two sides of the same coin, but I see the first as relating to a less-distant situation than the second. Perhaps the authors could offer some comment on sulfate in these cases. My last comment is a pet peeve: I find stacked bar graphs like figures 5 and 6 almost impossible to read other than for the sum of the constituents and maybe one or two of the major constituents. If that is how the authors want to use them, fine. If not, they should consider using line plots instead.

This is a good point made by the reviewer. We modified the discussion in sections 3.5.2 and 3.5.3 in order to explain the different behavior of sulphate vs. other things in line with suggestions by the reviewer.

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