This paper reports a very interesting episode of high pollutant concentrations that developed during a period of high pressure over the region around Beijing. The study is generally well documented and described in this paper; however, the conclusions that are drawn as to the cause of the high concentrations are not necessarily correct. Some of what is concluded seem to be supported by the measurements but there are several factors that have not been sufficiently explored in the analysis.

Two of the main conclusions that are reported in the abstract and summary, and that are developed in the main text, are that it was mainly the high intensity of local pollutions that led to this particular haze event and that it was new particle formation that was the most important factor contributing to this haze.

Although neither of these conclusions are necessarily incorrect, I would argue that since the emissions from Beijing sources probably change very little from day to day or month to month, it was the meteorological factors that led to the haze event, not any change in the sources of emissions.

The meteorology is partially discussed but is in need of much more analysis that includes looking at the vertical structure of the temperature, humidity and winds using the local radiosonde information. The introduction of the surface maps was a good start, as was the use of the lidar to look at how the aerosols were distributed. This is not enough however to tell the complete story.

The vertical profile of temperature and dew point temperature allow you to calculate the potential temperature and hence the stability of the boundary layer. The boundary layer is defined by its thermodynamic structure and not by its aerosol population. Perhaps there were morning inversions also associated with the increased aerosol concentration?

The low wind speeds and shallow boundary layer were certainly the primary cause of intensified haze; however, the primary driving force of the decreased depth of the boundary layer and the outflow seen on the AOD maps was the larger scale circulation and high pressure over that region. This is the story that needs to be told.

High pressure means convergence aloft, divergence on the surface. This is the reason that as the high pressure moved over the region the boundary layer depth decreased, the concentration of particles increased, humidity increased and temperature decreased leading to deliquescence of the particles and visibility decreased. This is a classic pollution event seen often in the wintertime but is not restricted to just cold weather. Yes, winds were low because there were no strong pressure gradients; however, the divergence at the ground led to large scale outflow seen by the increase AOD over time in Fig. 7.

This is a very nice example that should be published but it has to be better documented with respect to the actual meteorological factors that led to the high pollution event. The authors are correct that this case can be quite instructional, especially for those forecasting such events since they are clearly tied to meteorology that can be forecast fairly accurately. If none of the authors are experience meteorologists, than I strongly recommend that they consult with one.

With respect to the claim that new particle formation is the most important contributor to the high particle concentrations, I have to ask that the authors do more research on the conditions that are necessary to bring about gas to particle conversion. A case of new particle formation is described by Dunn et al (2004) in Mexico City, an urban area similarly polluted as Beijing. They concluded that three criteria consistently characterize new particle formation events within the city: the events occur during daylight hours, while SO2 is elevated, and when particulate matter mass concentrations are at significantly lower values than their averages.

The criteria of elevated SO2 and low PM is critical to new particle formation. There has to be high SO2 vapor pressure and at the same time, very low available surface area to which the SO2 will preferentially diffuse, since that will have a higher probability than forming clusters of SO2 molecules that grow into particles. In the present case, on the days that are being called new particle formation days, there is high concentrations of PM and low SO2. The SO2 concentrations are actually increasing after the says that are being called new particle formation.

Reference

Dunn, M. J., J.-L. Jiménez, D. Baumgardner, T. Castro, P. H. McMurry, and J. N. Smith (2004), Measurements of Mexico City nanoparticle size distributions: Observations of new particle formation and growth, Geophys. Res. Lett., 31, L10102

Other comments

Page 16264: Line 7 – What were the objectives of the field experiment? Why only 8 days?

Page 16264: Lines 18&20 – The TEOM and meteorological sensors are not in the instrumentation table.

Page 16265: Line 1 – What was the model number of the CPC?

Page 16265: Line 10 – “Aerosol absorption coefficient was then calculated by the product of mass concentration of BC by specific absorption coefficient (6.6m2 g−1), which was from the manufacture guide.” This seems backward to me. The MAAP measures light attenuation, not BC. Why not use the absorption directly measured?

Page 16267: Line 5 – Suggest including the CN concentration on one of the time series plots.

Page 16269: Line 7- This is a weak argument. Maximum temperature were still in the upper 20's, more than enough to heat and grow the boundary layer. As I discuss in my opening paragraphs, larger scale forcing is the mot likely reason. The air temperature doesn't "sharply decrease".

Page 16270: Line 4 – The lidar backscatter signal doesn't actually give you the top of the boundary layer. The boundary layer is defined by its thermodynamic structure not the aerosol properties. The blak line here is only an estimate.

Page 16270: Section 3.2.4 – Unless the authors can present a more compelling argument than just the size distributions, I strongly suggest removing this entire section and any references to new particle formation in the text. The real story here is the development of the haze event due to meteorological factors.

Page 16272: Line 11 – The hygroscopic factor was introduced by Hegg et al back in the early 1990's. Please give credit to the ones that originated the idea.

Page 16272: Line 19 – But the average RH in Beijing is not 80%

Page 16272: Section 3.2.6 – This complete section is incomprehensible for me. No references for the equations that are introduced and no explanation for why it is in this manuscript. Unless its relevance can be explained and netter document, it would be better left out.