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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Photochemical impacts of haze pollution in an urban environment" *by* M. Hollaway et al.

Anonymous Referee #2

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This manuscript assesses the impact of aerosol vertical distribution and composition on photolysis rates over Beijing using ground-based aerosol measurements from the Air Pollution and Human Health campaign and the offline Fast-JX photolysis code. The key result is that despite significant differences in aerosol composition between winter and summer, aerosols in Beijing tend to depress photolysis rates near the Earth's surface and enhance them above, with important implications for photochemical responses to PM2.5 mitigation strategies. The study is well-conceived and the manuscript is generally well-written. I recommend publication in Atmospheric Chemistry and Physics with minor revisions as detailed below.

Specific Comments:

Abstract: The abstract contains a lot of specific results, but some of the key outcomes

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of the paper get a bit lost in all of the numbers. In addition, the implications receive short shrift. There are many implications regarding potential PM2.5 mitigation strategies that are discussed in Sections 4 and 5 but are not reflected in the abstract.

Page 2, line 1: Please be specific about what is meant by “very high levels”

Page 2, line 29-30: the “)” is missing at the end of “(e.g. strong absorbers such as BC. . ...”

Page 3, lines 25-26: How were the extinction coefficients attributed specifically to anthropogenic aerosols?

Page 5, lines 23-24: Is “cloud cover” equivalent to “cloud fraction”? If so, the latter term might be clearer since that is (I believe) what is used in Fast-JX.

Page 6, Section 3.1: There is one instance in the entire record where Fast-JX fails to model a significant decrease in photolysis rates when one is observed – May 29th. Are the authors able to comment on what was special about this particular day?

Page 6, line 29: Please add “of each aerosol component” after “vertical profiles and contributions”

Page 7, lines 1-2: The wording here makes it seem as though NH4NO3 and organic aerosol make similar contributions as (NH4)2SO4.

Page 7, lines 2-4: I’m not sure I agree with the authors’ characterization of the vertical distribution. There is an enhancement of aerosol from 1-2 km that is not as large as in the boundary layer and above 3 km, but does not seem to be consistent with “high values . . . below 1 km which then decline rapidly with altitude before peaking again above 3 km”

Page 7, lines 14-16: Can the authors comment on why organic aerosol does not show the same vertical profile as the other aerosols? While the peak values are indeed within the same altitude range as the EPL, there is no layer-like feature in the OA.

Page 7, line 31: The word “substantial” has a typo

Page 7, lines 31-33: Why is the impact on J[NO2] larger than that on J[O1D]?

Page 7, line 33 – Page 8, line 2: The authors state that the surface layer is below the elevated levels of aerosol, but (NH4)2SO4 and BC are still clearly elevated at the surface according to the left panel of Figure 2. Please clarify.

Page 8, lines 6-7: Here the results in Figure 3 are attributed to “high levels of backscatter from the EPL”, but again the OA in Figure 2, 3rd panel does not seem to show a distinct layered structure like the other aerosols do. It seems more accurate to say “from the level of maximum OA” or something similar.

Page 8, lines 19-21: Can the authors elaborate on why the effects of scattering are less pronounced for J[O1D]?

Page 8, lines 31-34: I found the statement that “particulate matter confined mainly to the boundary layer is shown to produce significant impacts at altitude” to be confusing - in much of the preceding discussion, many of the features of Figure 3 were attributed to the EPL during each season, and during summer the EPL seems to dominate the photolysis rate response. I do not see in the analysis provided any example where there is aerosol confined mainly to the boundary layer on which to base this statement.

Page 9, line 18: It would be helpful to the reader to include a “(not shown)” in the first sentence of the paragraph.

Page 10, line 20: Please explain briefly what is meant by ‘photochemical limitation’.

Page 10, lines 24-30: It is clear that China has, in fact, implemented some emissions controls on aerosol precursors (see, for example, Wang et al., ERL, 2015, doi: 10.1088/1748-9326/10/11/114015 and Liu et al., ERL, 2016, doi:10.1088/1748-9326/11/11/114002) and satellite measurements show rapid decreases in NO2 and SO2 from 2011 onward. China’s clean air plans (including specific targets for Beijing) should at least be mentioned here. In addition, one of the key points of this study is

that the aerosol composition matters, but that aspect is missing here. The differences in aerosol composition between summer and winter appear to make it very unlikely that a single mitigation approach would have substantial impacts in both seasons. Yet the scenarios described here simply assume “reduction of aerosol composition” without regard to species.

Page 11, lines 5-14: Please see the previous comment regarding the lack of a discussion of aerosol composition. Given the focus on composition in this paper, it seems odd to treat aerosol as a singular component.

Page 11, lines 25-26: The wording here (“with contrasting results”) is very unclear.

Pages 11-13, Sections 4 and 5: There is some repetition between sections here that could be reduced.

Page 13, lines 15-16: I think it would be clearer and more impactful to explicitly say that reducing aerosols (which has a health benefit) would lead to more ozone at the surface (which has a negative health impact) – rather than simply “would offset the photochemical impacts demonstrated here”.

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