Review of acp-2018-1142
Liu et al. "Contrasting physical properties of black carbon in urban Beijing between winter and summer"

Summary:
The authors present a study investigating mixing state and composition of black carbon (BC)-containing particles collected in Beijing over two seasons. They have included comprehensive measurements and/or calculations of the physical, optical, and chemical properties associated with these particles. They utilize this information to provide detailed discussion about the sources of these BC-containing particles and how different sources yield different aerosol properties (Figure 9 is especially nice in this regard!). I recommend that this manuscript be promoted from discussion paper to publication in *Atmospheric Chemistry and Physics* once the authors have sufficiently addressed comments from all reviewers.

General comments:

- The authors appear to be "loose" with statistics throughout the manuscript. For example, in lines 258, 266, 294, and 384 (and potentially elsewhere), differences between properties etc. are described as "significant" or "not significant", but no statistical metric indicative of significance testing (e.g., *p*-value) has been reported. Moreover, in lines 269 and 392 (and again, potentially elsewhere), reference is made to correlation/anti-correlation, but there is no metric reported. I suggest that if the authors would like to use this language, they should support this with some metric.
- Section 3.1: The authors appear to move back and forth between calculations representing all BC particles over a given time window (e.g., mass median diameter, volume-weighted coated BC size) and calculations for single particles (e.g., scattering enhancement, mass absorption cross section). Consequently, I found this section challenging to read. Could the authors please re-write this section for clarity?
- Section 4.6: The authors switch back and forth between symbolic and text representations for the different BC types (e.g., Line 446, 458, 459), which is a little distracting. Please correct for consistency one way or the other.
- Something that is not clear to me is how the authors "combine[d] two online source apportionment methods". Is this the point of Figure 10? Given that this is a key highlight in the abstract, the demonstration of this in the manuscript is weak (or at best, under-emphasized). The authors should address this concern with their revisions.

Specific comments:

- Lines 155-157: I am not questioning the authors' approach (i.e., the "why"), but I don't quite follow the "what". Could the authors please clarify this? The sentence beginning with "the fraction of 10%" is especially confusing to me.
- Line 171: I believe this should be "cm^{-3}"
- In 224: In my experience, it is "low-volatility" or "lower-volatility" organics, rather than "less-volatile" organics. (If the authors want to keep their terminology based on what they are more used to, this is fine)
- Lines 264-265: Does distance or time matter more? A longer transport pathway does not necessarily mean that something is more aged. Also, I thought that all of the back trajectories were limited to 24 hours.
- Lines 337-340: These source terms were defined previously but without the "BC". Please update the manuscript for consistency.
- Lines 368-371: Not a comment to address, but I learned that levoglucosan can be present in coal combustion!

- Line 396: How well does this linear model work? I'm not seeing anything demonstrating the quality of fit (e.g., R^2) or even a plot.
- Figure 6: I suggest that the authors "jitter" either winter or summer to make the error bars clearer (e.g., shift one of them ½ hour to the left or right along with a note in the caption)