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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Modeling the aging process of black carbon during atmospheric transport using a new approach: a case study in Beijing" *by* Yuxuan Zhang et al.

Anonymous Referee #1

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This paper shows a new and innovative method of trying to represent observed changes in the 'coating' of black carbon, as detected as changes in the Dp/Dc metric produced by the DMT SP2. This works by combining an emission field with a Langrangian transport model and an empirical parameterisation for the evolution of black carbon in the atmosphere. While this 'k' parameter is tuned to the measurements, the model shows an impressive correlation with the observations, giving confidence that there is value in the technique. The authors then go on to make estimates of the contributions from inside and outside the city to the measured concentrations and properties.

While not an all-encompassing process model, this does present an intriguing new way of looking at the data, that could conceivably bridge the gap between a highly detailed process model such as PARTMC-MOSAIC and Eularian chemical transport models like WRF-CHEM. It is also good that it provides another new perspective on the phenomenon of pollution 'building up' in Beijing, offering further evidence that this is driven by regional transport and transformation rather than local sources and processes.

While the main innovation with this paper is technical, the authors do use it to interpret air quality sources and phenomena, so I would say this is in-scope for ACP (as opposed to AMT or GMD). While I am not completely convinced of the results (see below), I think that the novelty of the technique alone means that it deserves to be discussed within the scientific literature. Overall, the quality of English is good, but I do have certain issues regarding how the results are interpreted and presented. As such, I recommend publication after revisions.

General comments:

While the earlier sections of the paper were well written, I was not impressed with how the paper interpreted the implications of the results. Sections 4 and 5 in particular seem to just largely restate what was already said in section 3 in different ways, so this entire portion of the paper could do with rewriting and sharpening up. But more generally, I would question what the key implications of this paper are; the conclusions seem to work off the reduction of Babs being a regulatory motivation, however most evidence supports BC being a more important metric for human health, which while related, is more directly related to emissions rather than processing. While the evolution of the mixing state of BC is important to consider for wet removal and radiative transfer, I would say it is debatable as to whether reducing the coatings (as opposed to overall BC) should be considered an objective for policymaking (while BC is recognised as a global climate forcing agent, the majority of this is likely from biomass burning). It is my opinion that the conclusions of this paper are more applicable to process-level atmospheric science, except for the part where the contributions to bulk BC are discussed.

I would recommend the discussion and conclusions be reframed accordingly.

Specific comments:

I found this paper extremely abstract and hard to read in places regarding certain quantities and it took me multiple reads before I think I began to understand what was going on. It particular, I would have appreciated a more intuitive explanation of what things like 'k' and 'EEI' physically represent (along with many others).

More discussion should be given to the possible mechanisms for the increase in Dp/Dc, i.e. coagulation and secondary aerosol formation. Given that there are already other models out there that consider these processes, is it possible to compare the value of the 'k' parameter to equivalent timescales in other models?

Ultimately, while good correlation between measurement and model is reached, it is possible that other yet-to-be-identified factors may be responsible for this agreement rather than the 'k' parameter being a good representation of ageing, which is the working hypothesis. The authors should spend more time discussing how best to further test the robustness of this model. In particular, a major limitation of this work is that it is restricted to autumn/winter datasets. During the summer in Beijing, there is typically much more photochemistry (reflected in high ozone concentrations) but BC concentrations and coating thicknesses are both reduced according to Liu et al. (https://www.atmos-chem-phys-discuss.net/acp-2018-1142/). Can this result be reconciled with this model? Would it mean that the 'k' value would need to have a seasonal dependence if this model were to be extended to other months?

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