

Interactive comment on “Atmospheric tar balls: aged primary droplets from biomass burning?” by A. Tóth et al.

Anonymous Referee #1

Received and published: 17 March 2014

GENERAL REMARKS

The manuscript provides interesting results from an innovative approach of generating tar balls in the laboratory. Tar balls form a sub-fraction of biomass burning products and may contribute to the radiative effects of carbonaceous particles in the atmosphere. However, the formation process of tar balls is still not fully understood and as of today there is no established methods available for producing tar balls in the laboratory. In this respect the manuscript makes a significant contribution to an important research area. However, in its current form, the manuscript is not acceptable for publication in ACP and requires major revisions before re-consideration. Topics to be addressed with more detail before publication are the following:

1/ The introduction requires an update of the cited literature. In the meantime, another IPCC report was published and the assessment of the role of black carbon in the climate system by Bond et al. (Bond et al., 2013) became available. Both documents may contribute new details to the radiative effects of biomass burning products and thus a thorough discussion is recommended.

2/ The manuscript claims a good agreement between the properties of tar balls produced with the proposed method, and tar balls collected in the atmosphere. However, the degree of agreement is not presented but only mentioned (e.g., Section 3.1, 3rd paragraph; Section 3.2, last sentence). As an example, in Section 3.1, the optical diameter of produced particles and their good agreement with atmospheric particles is mentioned. However there is no proof for the claimed agreement. Here it is strongly recommended to present size distributions of particles produced in the lab and collected in the atmosphere. Data are obviously available, otherwise average diameters and diameter ranges couldn't be stated.

3/ Same point as above but now, even more serious, about the ageing of particles described in the entire fourth paragraph of Section 3.1. This paragraph is entirely descriptive and lacks a justification of arguments. It presents results only in the last sentences whereas the majority of the paragraph describes knowledge from literature. Although this paragraph discusses an important point of the study, namely the simulation of particle ageing by the applied "heat shock", a justification of this approach as an ageing simulator by intercomparison of results is missing.

4/ The description of the experimental methods is incomplete and could be better arranged. The first paragraph of the results section presents the rationale for the taken approach. It would be better placed at the beginning of the experimental section. With this paragraph at the beginning of Section 2, the complete approach can be easily described. A more quantitative description of the production of tar (including a sketch of the used apparatus) followed by the current version of the particle generation approach and the description of the TEM methodology would present a straightforward storyline

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

of the study.

5/ The presentation of the chemical composition of particles in Section 3.2 requires more detail. Since obviously at least 15 particles have been analysed, it would be good to see all results and not only the average values and their range. And is there any information on the chemical composition of particles previous to the “heat shock” ageing? This section also requires discussion of data from the literature.

6/ The nomenclature used in the manuscript deviates significantly from the recommended terminology for reporting BC data (Petzold et al., 2013). An adaptation of the recommended terminology is suggested.

MINOR COMMENTS

1/ Abstract: The abstract is more written like a summary; rewriting including more details on results is recommended.

2/ Page 33092, line 21: the sentence “. . . radical polymerisation with OH radical . . .” is not clear, please rephrase.

3/ Page 33093, line 13: replace “glass tube of 200 mm long” by “of 200 mm length”.

4/ Page 33095, line 26: remove one word “that”.

5/ Page 33097, line 22: It suggested to rephrase “. . . exclusion of flame processes.”

6/ Fig. 2: It would be beneficial to show the TEM images with the same scale to allow for an easy intercomparison of particle sizes.

REFERENCES

Bond, T. C., Doherty, S. J., Fahey, D. W., Forster, P. M., Bernsten, T., DeAngelo, B. J., Flanner, M. G., Ghan, S., Kärcher, B., Koch, D., Kinne, S., Kondo, Y., Quinn, P. K., Sarofim, M. C., Schultz, M. G., Schulz, M., Venkataraman, C., Zhang, H., Zhang, S., Bellouin, N., Guttikunda, S. K., Hopke, P. K., Jacobson, M. Z., Kaiser, J. W., Klimont, Z.,

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Lohmann, U., Schwarz, J. P., Shindell, D., Storelvmo, T., Warren, S. G., and Zender, C. S.: Bounding the role of black carbon in the climate system: A scientific assessment, *J. Geophys. Res.*, 118, 5380–5552, doi: 10.1002/jgrd.50171, 2013.

Petzold, A., Ogren, J. A., Fiebig, M., Laj, P., Li, S.-M., Baltensperger, U., Holzer-Popp, T., Kinne, S., Pappalardo, G., Sugimoto, N., Wehrli, C., Wiedensohler, A., and Zhang, X.-Y.: Recommendations for reporting “black carbon” measurements, *Atmos. Chem. Phys.*, 13, 8365–8379, 2013.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 13, 33089, 2013.

ACPD

13, C12921–C12924,
2014

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

C12924

