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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Size-resolved aerosol composition and link to hygroscopicity at a forested site in Colorado" *by* E. J. T. Levin et al.

Anonymous Referee #2

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In the present paper Levin et al., have presented the size-resolved CCN measurements from a mountainous site in Colorado during the six-week BEACHON-RoMBAS campaign. The dataset reported appears to be of good quality and can make good contribution, as pointed out by Referee #1, in enhancing our understanding related to hygroscopicity of ambient biogenic SOAs. I believe that manuscript adequately meets the standards of Atmospheric Chemistry and Physics (ACP). I, however, have following comments, which Authors might want to address before manuscript is considered for publication in ACP. Comments:

1. I echo the suggestion raised by Referee #1 that conclusions are based on series of assumptions. In my opinion under certain scenario these assumptions may tend to bias the results considering that information is not available regarding the internal

mixing (and to an extent about external mixing) of the aerosols. Su et al., (2010) have demonstrated the spread of κ in the aerosol particle of same size to investigate the internal mixing. Hence, the lack of data about internal mixing could be crucial. In the present study no detailed discussion is presented about the assumption about internal mixing of the aerosol particles in the CCN active size range.

2. I wish authors could have given little more details about the measurement set-up (mostly flows through various instruments used) and estimated uncertainties in the CCNC calibration. As described by Rose et al., (2008) choice of Köhler model and calibration accuracy can strongly affect the estimated supersaturation; more so for the lower supersaturation. These uncertainties could be further crucial in estimating/calculating the κ by means of huge under- or over-estimations. At least authors could consider adding a line addressing this.

3. The density assumption/calculations are not quite clear to me. May be authors could consider elaborating the details at appropriate places in the revised manuscript.

4. Please note that on page 23826 line 17 what authors refer to cloud processing may not be true in this case as Fors et al., (2011) and Hao et al., (2013) reported their results under different scenario. Authors might reconsider this sentence.

5. Referring the Fig. 1 on certain occasions the agreement between CCN κ and AMS κ shows considerable disagreement. Do authors have any explanation? Not sure if AMS mass was too low during this period.

6. Fig. 3 y-axis can be scaled from 0.4 to 1

7. Please note that Jurányi et al have also reported similar low kappa values from Boreal forest measurements. Authors might consider having couple of sentences comparing their results.

8. Why very few measurement results are presented from TDCIMS measurements?

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