Review of the manuscript by Xu et al, ACPD 2015

General comments

The manuscript aims at characterizing the submicron non-refractory particles in the southeastern US. It is mainly based on aerosol mass spectrometry measurements, but complements these with a set of 'ancillary measurements' to support the interpretation. Quantification of organic nitrates to the total organic aerosol is a main result of the manuscript. As it addresses relevant scientific questions within the scope of ACP, and the scientific methods and assumptions used are sufficiently valid, it should be accepted for publication after addressing some points described hereafter.

Main points:

1) A main point that should be clarified in the text is the origin and use of boundary layer height (BLH) data. The authors often refer to the BLH diurnal variation as an element that affects the diurnal pattern of the different chemical species observed at the ground. However, very little information is provided on the origin of these BLH data, and on the actual BLH diurnal variation these data show in the region under investigation. More details are definitely needed in the text on this aspect. At least a Figure with the typical BLH diurnal pattern in summer and winter in the region should be included in the supplementary material. This would (possibly) allow the reader to understand the use of the BLH data in generating the results of Figures 3d and 6d. In fact, to my understanding, at present these data are used in an incorrect way. The authors state they multiply the different chemical species loads at the surface (expressed as $\mu g/m^3$) by the BLH (see e.g. page 10513, line 25), obtaining units of $\mu g/m^2$. I do not see the physical reason for that. The concept of the BLH dilution effect is that, given a certain load of the chemical component X (expressed in µg), uniformly distributed within a well mixed atmospheric volume V given by [area(m^2) x BLH(m)], the concentration of X that is measured at the ground, i.e. X/V, would be higher when BLH is lower (i.e., in the morning/evening), and would be lower when BLH is higher, i.e. at midday. As the authors correctly describe in the text, this effect contributes explaining why, for example, a given component expected to increase in the central part of the day for photochemical processes, can exhibit a flat diurnal cycle (or even be observed to decrease in the central part of the day). So my point is that the authors give the right explanation to justify some of the BLH-driven diurnal patterns observed, but provide the wrong (or insufficiently explained) demonstration for that.

2) The authors often refer to the term 'Brown Carbon' and use 'Brown Carbon' data in the manuscript. As it can be inferred from the text, with this term they refer to the 'Brown-carbon light-absorption' (units m^{-1}). This should be clearly stated in the text and in the relevant Figure showing these data (Figure 7) in order to avoid potential confusion.

Additionally, the description of how Brown Carbon Absorption is derived should be improved (Page 10489 lines 11-15). In fact, a) it is not clear how the Black Carbon information from MAAP is combined to the Aethalometer one to derive the Brown Carbon Absorption, and, b) I doubt that, in the Aethalometer case, '*the measurements under seven wavelengths (i.e., 370, 450, 571, 590, 660, 880, and 950 nm) ~~were averaged~~ to represent the black carbon concentration'* as currently stated. Please explain better and give appropriate reference to the methods used to derive the Brown Carbon Absorption data used in the manuscript.

3) Although the manuscript is generally well written, some parts/sentences could be shortened, improving its readability. Another issue is the quantity and the specific choice of Figures to be included in the main text and in the supplementary material. Some times in the text the authors refer to Figures in the supplementary material as main points of their discussion. This makes the reading not straightforward as it is needed to switch between the main text and the supplementary material several times. To my opinion all the Figures necessary for the main discussion should be in the main text (Figure S14 for example) and the supplement should only provide the material for a deeper investigation/explanation of the results described in the main text.

Specific/technical comments

Title

Possibly, given the contents of the manuscript, the title should be modified as '*Aerosol characterization over the southeastern United States using high resolution aerosol mass spectrometry: spatial and seasonal variation of aerosol composition and sources with focus on organic nitrates*'

Page	Lines	Comment
10481	6	Acronyms should always be introduced first (not all the readers may know AL stands for Alabama)
	11	It would be useful to specify in this abstract what 'important' means here, please provide for example data of % contribution of HOA and COA.
10488	5	Please, specify the meaning of V mode and W mode
10489	5-25	It would be important to know the temporal resolution of the different datasets introduced in this paragraph.
10490	6	Shouldn't it be 'assumes' rather than 'represents'?
	19	Remove 'that'
10493	1	Shouldn't it be 'because they cover..' rather than 'that they cover..'?
10494	10-12	It would be useful to know how these averages were obtained (Average of daily averages? Averages of hourly averages? See also my comment above on the temporal resolution of the measurements)
	20-23	BLH also plays a role in the winter-to-summer difference here, not only emissions..
10495	19	Give reference to Fig. 2 here.
	22-23	I'm not convinced entrainment could play a role in this. In fact, it is more likely to act in the first part of the day, when BL grows and intercepts the residual layers aloft (e.g. Curci et al, Atmos. Chem. Phys., 15, 2629-2649, 2015)
10496	14	You introduce here Fig. 6 but Fig 5 has not been introduced yet
10497	15-16	I cannot see this 'clear lunch and dinner feature' here. This is only shown in 3 out of 6 plots and: a) it is not visible in JST_May, b) in RS-Jan the peak is in the morning at 5 a.m. Please explain this point better or rephrase.
10498	5	Why not referring to Isoprene-OA as IOA as done for all the other categories?
	8	Acronyms should always be introduced first (not all the readers may know IEPOX stands for Isoprene epoxydiols)
10499	3	It should be ' thought to be'
10501	7-14	Could these BBOA differences be also due to the fact that you compare PM1-related data to PM2.5-related ones? Please, comment.
	29	It should be 'For four (out of five)..'
10502	20	It would be useful to remind here that M stands for 'More' and L stands for 'Less'.
10503	5	It should be 'in the atmosphere..'
	8	Do you mean 'identification of MO_OOA aerosol sources'?
	11-13	Please, explain better, see also my comment on BLH effect above
10505	22	It should be 'is applied in this case..'
	24	Remove the text 'because estimated…RON', as it is redundant.
10506	3	It should be 'concentration estimated..'
	20	Molecular Weight (?)
10508	4	Is that from NaNO3 the only possible contamination?
	27	I just want to highlight here that a variability of 0.11-0.21 means a difference of about 100%!
10509	2	Remove 'are'
	12	Why do you consider GT_AUG to be in a transition month? Isn't August a summer month (particularly considering that relevant observation started on July 20)?
10510	2	It should be 'than in summer'
	23	Are you referring to Figure S16?
	24	It should be 'evidences'
10511	14	I would rather title as 'aerosol spatial variability', and use 'variability' instead of 'distribution' all over the paragraph

	18-19	Rephrase the sentence to refer to Fig 12 and then address the reader to figure S14 for deeper analysis (if you believe Fig 12 is more 'efficient' than Figure S14 in summarizing the results you are commenting here'….)
10512	19	It should be 'in summer compared to winter'
	25-27	I cannot see a 'POA' curve in Figure 5
10513	4	It should be 'leads'
	7-8	'robustness' is probably not the most suitable term here, you can rather test the 'validity/applicability' of your findings to a long-term record.
	24-26	See my main comment above on this matter. Please give details on how BLH data have been collected, ceilometer instrument used, BLH retrieval. Provide at least a Figure in the supplementary material showing the typical BLH in the region and its seasonality.
10515	28	It should be 'by the fact that OA sources...'

Figures

Figure 2: it would be useful to also have an additional panel showing the 'absolute' plot (as in Figure 5).

Figure 3: Axis Labels and Tick Labels are not readable at all, please increase the character size of all of them. Axis Limits should better be the same in all panels to allow a more direct and straightforward comparison.
For Figure 3d see my comments above. I think it can even be removed without loosing much of the manuscript significance. Otherwise, carefully describe its content as mentioned.

Figure 5: in the legend Isoprene-OA could conveniently be indicated as IOA for homogeneity.
Numbers at the top of panel b are not necessary as also shown in panel a. Possibly you could add error bars on panel a.
I cannot see POA line in the Figure.

Figure 6
All the comments for Figure 3 are still valid for this figure.

Figure 7
Please, put labels a) – j) in the plots. Axis Limits should better be the same in all panels to allow a more direct and straightforward comparison (there is no need to reach 70 in the Y axis of panel d, you can leave 'out of scale' values without loosing much information. Use 'Brown Carbon Absorption' rather than 'Brown Carbon' in the plot labels (see my comment above).

Figure 8
Please, put labels a) – j) in the plots. Axis Limits should better be the same in all panels to allow a more direct and straightforward comparison.

Figure 9
This Figure is not very readable at least in the printed copy I used. Please, try to improve readability/figure resolution.
Please, put labels a) – j) in the plots. Axis Limits should better be the same in all panels to allow a more direct and straightforward comparison

Figure 11
I think the way this Figure is organized is not optimal. You should rather show the AMS-IC value (Y) versus the relevant range coming from the NOX ratio method with the two RON values (e.g. similarly to the vertical lines of Figure 10 but as horizontal lines in the X axis). You can leave info on the two fit lines; correlation R is obviously the same and should not be repeated. The reason of the offset is not clear and should be better explained. Measurement errors are mentioned in the caption but not shown. Please, specify and/or show such errors.

Figure 12: In the caption it should be 'values are plotted versus the relevant distance of the measurement site from the GT one, where the…'