Gunsch et al. present observations of aerosol concentration and composition at the University of Michigan Biological Station (UMBS) for July 2014. The authors use a combination of a high-resolution time-of-flight aerosol mass spectrometer, single particle aerosol time-of-flight mass spectrometer, and combination scanning mobility particle sizer spectrometer and aerodynamic particle size spectrometer to investigate the aerosol characteristics between 0.01 – 2.5 μm during this time period. The authors found four different air masses impacted the area during the time period: 2 air masses impacted by wildfires from Canada, 1 air mass impacted by cities south of UMBS, and 1 air mass from clean regions over Canada. The authors found an increase in particle number and mass, over the clean regime, for the air masses impacted by wildfires and cities; however, no matter where the air came from, it was always influenced by biomass burning. The paper provides important information about what influences the aerosol mass in a background, rural location, and is of value for *Atmospheric Chemistry and Physics* community; however, there are some concerns and some clarifications that need to be addressed first prior to publication.

Major Comments

1) I'm wondering why the AMS data was not used more to help further validate the results from the single particle mass spectrometer, or to support some of the authors' hypotheses. For example, either PMF (Ulbrich et al., 2009), "poor-man's PMF" (Aiken et al., 2009; Zhang et al., 2005), or triangle plots of different fragments (Cubison et al., 2011; Hu et al., 2015; Ng et al., 2010) would further support the evidence of OA being strongly influenced by biomass burning, anthropogenic emissions, and biogenic emissions/chemistry (e.g., page 10, Lines 3 – 5 and page 11, lines 20 – 22). Without this support, speculations that biogenic emissions and chemistry leading to the very high O/C ratios observed is hard to

interpret (page 11, lines 20 – 22). Other studies found high O/C ratios due to photochemical aging of the biomass burning emissions and aerosol (Liu et al., 2016; Zhou et al., 2017), which may have led to the high O/C instead of OA production from biogenic emissions. Also, cloud processing of the gases and OA may lead to high O/C ratios, along with the SO_4 (Sullivan et al., 2016). A discussion either including these various processes or a discussion that argues why one process over the others leads to the high O/C ratios is needed.

2) Throughout the Sections 3.2 – 3.4, in the comparisons of the different aerosol regimes, the aerosol number mode is mentioned. The biomass burning mode is the same as the background mode, and the urban mode is smaller than both the biomass burning and background mode; however, the authors discuss how chemistry and accumulation are occurring during transport of the biomass burning and urban air masses. A discussion about why the modes are similar (or smaller) while chemistry and accumulation is occurring is necessary for the readers to better relate these two possibly contradictory processes.

3) A more in-depth analysis of some of the aerosol characteristics would improve the results and the paper. For example, page 12, lines 6 – 11, the authors very briefly discuss an accumulation of SO_4 into the biomass burning particles; however, these particles are coming from wildfires in Canada. A discussion about where this SO_4 comes from would be beneficial, as recent studies have found a minor role for biomass burning emissions SO_4 and unclear for SO_2 emissions (Collier et al., 2016; Liu et al., 2016, 2017). As another example, the authors found little NO_3 in the air masses impacted by urban regions (page 14, lines 7 – 10 and Figure 3), which is surprising with the NO_x emissions and chemistry.

What would have led to the extremely low amounts, especially since other downwind sites have observed enhanced NO_3 (Jimenez et al., 2009)?

Minor Comments

1) Page 1, line 27: “The field site was also influenced . . .”. It is not clear in the abstract what the influences are before this line.
2) Page 2, line 21: Change leading primary particles to leading to primary particles
3) Page 2, line 22: Why is water included as a secondary species for aerosol?
4) Page 4, line 3: Any reason why Slowik et al. (2011) is not included in this comparison of SOA in Ontario?
5) Page 5, line 19: Please check the Jimenez and DeCarlo, 2017 citation. The website listed for this citation leads to Canadian Interagency Forest Fire Centre.
6) Page 11, line 1: superscript the minus sign
7) Page 11, line 12: Please change limit to limiting

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