This paper is an extension of authors’ previous work on the detection of aerosol atmospheric rivers (AARs) with the MERRA-2 aerosol reanalysis. The study focuses on the characteristics of AARs derived from 18-years (1997-2014) of data record. The paper is generally well written, but some clarifications and elaborations are needed.

**General comments:**

- Numerous studies have used satellite remote sensing, ground-based networks, and global chemical transport models to characterize aerosol long-range transport across continents. How is this study related to previous studies? What new value will this study add? How can people use the generated datasets to advance sciences? These points need to be elaborated in the paper.

- Discussion of potential uncertainties associated with the analysis: MERRA-2 assimilates satellite observed aerosol optical depth (clear-sky radiance associated with it). This offers strong constraint to the AOD in MERRA-2. However, aerosol components (e.g., sulfate, dust, sea-salt, and carbonaceous aerosol) are not fully constrained. How will this lack of constraint in the aerosol components affect your detection and quantification of AARs? MERRA-2 aerosol vertical distribution is also not constrained by satellite-based lidar observations, which constitutes of another uncertainty. You could use CALIPSO lidar observations to evaluate the MERRA-2 aerosol extinction profiles in major AAR regions (e.g., those defined in Figure 5).

- An 18-years data record of AARs has been generated. But this study only analyzed climatology and seasonal variations of AARs. I strongly suggest that the authors analyze and present interannual variations of AARs in the paper.

**Specific comments:**
In the abstract: aerosol components DU, CA, SU, and SS are used but not defined.

Lines 56-58: not sure about what you mean here.

Lines 61-64: there have been numerous studies of aerosol intercontinental transport, based on satellites, global models, and even ground-based networks. What is a relationship between your study and these previous studies?

Line 70: here you define CA (carbonaceous aerosol) as a sum of organic and black carbon. Later when discussing CA AARs, you present organic carbon (OC) and black carbon (BC) separately. To be consistent, it is better to combine OC and BC.

Lines 94-95: no need to define DU, SU, SS and CA again. They are already defined in lines 69-70.

Lines 103-104: Given it assimilates satellite observations of aerosol optical depth, MERRA-2 should be able to “capture the global aerosol optical depth reasonably well”. This is obvious.

Figure 2: what do you mean SU emission here? Do you mean emitted sulfate or precursor gases (e.g., SO2 and DMS)? Direct emissions of sulfate are very small. Then what is unit for the sulfur emission? What is unit for carbonaceous aerosol (CA) emission?

Line 217: can “boreal forests” be referred to as “rainforests”?

Lines 252-254: But if satellite observations (e.g., AOD) captured the ash plume, the ash signal would be evident in the MERRA-2 product to some extent.

Lines 259-260: “in terms of AOD” - do you really need these four words?

Line 288: are you sure that MERRA-2 don’t account for biogenic sources like Carbonyl Sulfide and Dimethyl Sulfate? I cannot believe that an aerosol model dealing with sulfur cycle doesn’t account for DMS.
Lines 318-319 (and Figure 5A): There is a secondary peak at about 960 mb, which should be discussed. I assumed that the two peaks are associated with seasonal variations of trans-Atlantic dust transport.

Figure 5: why do you use aerosol mixing ratio? It is more suitable to show mass concentration?

Line 379: “component” should be “component”, right?

Lines 517-518: if CA AARs are associated with burning of boreal forests, can we still call them “rainforests”?

Lines 519-520: How can the presence of SU AARs all year around in the northern hemisphere be explained by the biomass burning and biogenic activities?