Authors Response to Reviewer 2 comments on

Flow Climatology for Physicochemical Properties of Dichotomous Aerosol over the Western North Atlantic Ocean at Bermuda

By J. L. Moody et al.

We thank Reviewer 2 for helpful comments on the submitted version of the subject manuscript and respond to each point below. The reviewer's comments are presented in italics followed by our response to each. Unless otherwise noted, line numbers refer to those in the original manuscript.

The manuscript by Moody et al. is a well written paper summarizing 3-yr long observations of aerosol composition and optical properties in Bermuda. The authors have performed a comprehensive analysis on the relations of aerosol composition and flow climatology and also placed the observations in the context of previous measurements in the Atlantic. The analysis is valuable for the community and I recommend publishing the manuscript after the following concerns are addressed: 1. P. 22402, line 7- the term 'light absorbing carbon' includes BC and BrC (brown carbon); I'd recommend removing this terminology for BC.

We have clarified the text as the follows: AAE values of about 1.0 indicate relatively little variability in absorption as a function of wavelength and are generally associated with urban pollution plumes, in which absorption is dominated by combustion-derived light-absorbing carbon.

2. Why are the mass scattering and absorptions efficiencies calculated with only the mass of SO4? Why not consider total mass when making the scatter plots of scattering or use mass of EC or dust when considering absorption?

Our rationale for adopting nss SO_4^{2-} as a proxy for all scattering aerosol constituents associated with each flow regime was explained in the response to a similar comment/concern raised by Reviewer 1. Briefly, this approach was necessitated by the large number of samples for which concentrations of one or more relevant analytes was below the detection limit(s). As noted the text has been revised to describe a modified mass scattering efficiency (mMSE).

3.P. 22410, line 13: supermicron nitrate concentrations appear to have been high (even higher than supermicron nss sulfate) and should be mentioned among the other compounds contributing to scattering.

The composition and associated scattering of super-μm diameter aerosol at Bermuda is dominated by inorganic sea-salt species (Table 1) and water. In addition, based on the pH dependence of HNO_3 phase partitioning (see last paragraph of section 2.3), virtually all NO_3^- is associated with the super-um size fractions consistent with results reported in Table 1. Consequently, NO_3^- does not contribute significantly to scattering under these conditions. In response to the reviewer's comment, the following has been added to the text in section 3.5, paragraph 2:

"Super-μm-diameter aerosol at Bermuda is composed primarily of inorganic sea-salt species (Table 1) and associated water, which dominate scattering by that size fraction. Because virtually all NO_3^- , which originates from secondary sources, is also associated with the super-um size fraction (Table 1), NO_3^- has relatively minor influences on scattering."

4. The paper is rather a long manuscript and some of the data presented in figures and tables are repetitive. I recommend removing Table 4, especially considering that these efficiencies are based on scattering vs. sulfate mass only. Data in Tables 2-3 and 5 also can be included in supplementary.

While the paper is admittedly long, moving tables and figures to supplementary files seems problematic since each figure and table is clearly cited in the text, and refers to information that is more than ancillary, but in fact directly describes important elements of the dataset. We have tried to add a few words better linking the information content to the overall conclusion of the paper, that there are significant differences in aerosol physiochemical observations related to aerosol source region.