Review of "Three years of measurements of light-absorbing aerosols in the marine air at Henties Bay, Namibia: seasonality, origin, and transport" by Formenti et al.

The authors present the results of three years of aethalometer measurements at a coastal site in Namibia. They provide a statistical analysis and relate the observed concentrations to airmass trajectories and synoptic meteorology. From their measurements of aerosol light attenuation, they derive estimates of black carbon concentration, aerosol optical depth, and cloud droplet number concentrations, and consider their effects on the climate system.

Given the scarcity of measurements in this remote region, this is a valuable dataset, which deserves publication. I feel, however, that some of the interpretations stretch the reliability of the estimates beyond their limits and should either be removed or provided with a lot more caveats, based on quantitative assessment of the uncertainties. I therefore recommend a major revision of the manuscript. Specific comments follow:

1) The paper is based on aethalometer measurements, which are converted to equivalent black carbon (eBC) concentrations. The conversion of the measured attenuation (ATN) to eBC is a highly uncertain process, which has been the subject of numerous studies and considerable controversy. In their discussion of the attenuation cross section (l. 107 – 123), the authors mix attenuation and absorption cross sections, which are not the same thing.

2) For the actual conversion of ATN to eBC, the authors use the correction of Weingartner et al. (2003). This is the oldest and probably least accurate formulation. There are numerous later papers (e.g., Virkkula et al., 2007; Schmid et al., 2006; Arnott et al., 2005; Collaud Coen et al., 2010; Saturno et al., 2017), which improve on the Weingartner approach. Especially Collaud Coen et al. (2010) and Saturno et al. (2017) provide evaluations of the earlier approaches. The authors should justify the choice of their correction techniques and provide a quantitative assessment of the resulting errors. They should also avoid given numbers to inappropriate precision, e.g., the value of 986 ng m^{-3}, which implies an accuracy of three digits but is probably uncertain to a factor of two.

3) The authors present a detailed analysis of airmass transport patterns. The value of this discussion would be considerably enhanced if these patterns would be related to source distributions. For example, show maps of emissions from biomass burning and power productions. Several gridded inventories are available, which could be used for this purpose.

4) In section 4, the authors calculate a mean AOD value from their eBC concentration estimates. For this purpose, they use values of the mass fraction of eBC to total aerosol mass (PM2.5 ?), aerosol mass extinction efficiency, and boundary layer thickness. All of these parameters vary by factors of 50% to 200%. The authors must provide uncertainty estimates of the individual parameters and use quantitative error propagation to provide an uncertainty estimate of their AOD estimate.

5) The estimated AOD due to the eBC associated aerosol is only 0.01, while the measured AOD at the site is 0.2 to 0.4. This requires most of the aerosol to be outside of the boundary layer. That is of course possible in principle, but should be justified. What data are there in the literature that show elevated aerosol layers of this AOD in the study region?

6) The biggest stretch comes when the eBC data are used to estimate cloud droplet concentrations (CDNC). Here, two parameters are used, the CN to BC ratio from Andreae et al. (1995) and the CNDC/AMNC ratio of Hegg et al. (2012). The former number was obtained with fairly ancient instrumentation and the paper actually states *"the ratio of CN to black carbon varies between 2 and 14 cm^{-3} (ng C m^{-3})$^{-1}$. The highest ratio was found in one episode on 18 March, close to the African continent during the more remote episodes the ratios were well below 10 cm^{-3} (ng C m^{-3})$^{-1}$. Measurements over Africa during the dry season also showed ratios of about 2-4 cm^{-3} (ng C m^{-3})$^{-1}$ …"*. In other words, the uncertainty of this parameter is about a factor of seven at face value, given the accuracy of the measurements at the time, probably worse. Note that this parameter also refers to CN, not to "AMNC", the accumulation mode number concentration, which is the variable used by Hegg et al. AMNC is typically much smaller than CN, by an unknown and variable factor. So, AMNC could well have been 200, instead of the 2900 cm^{-3} inferred by the authors. Furthermore, assuming the AMNC were as high as the authors estimate, the would be well above the range considered by Hegg et al., and in the range where the CDNC has already saturated because of water vapor limitations. It is simply not legitimate to extrapolate this linear relationship into a range where it does not apply. In conclusion, the uncertainty of this estimate is so large that it probably exceeds one order of magnitude, and this section really should be removed.