

Interactive comment on “Three years of measurements of light-absorbing aerosols in the marine air at Henties Bay, Namibia: seasonality, origin, and transport” by Paola Formenti et al.

Anonymous Referee #3

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The paper presents results from a 3-year record of atmospheric Black Carbon measured at a coastal site in Namibia using an aethalometer instrument. The record is presented together with a variability analysis and connected to synoptic meteorology in the region and air-mass back trajectories. Further analysis of the results is used to derive BC contribution to aerosol optical depth and potential contribution of BC to cloud droplet number concentrations as an estimate of BC role in regional climate forcing. While it is clear that data documenting BC in areas of the World where information is scarce is very valuable, the paper is, to my view, not ready for publication : on one side, analysis of the observed variability is of limited scope and, in the other, the atmospheric relevance section goes way to far beyond reasonable interpretation of the

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actual measurements. Section 4. is not acceptable as it is : going from ATN measurements to Cloud Number Concentration using fixed proportionality factors derived from 2 previous studies without any independent way to control the estimate is scientifically questionable. This entire section should be removed unless additional evidences are provided (this applies to third bullet point in the conclusion as well). Other comments : Line 125, P.6 : why using Weingartner et al. when more suited correction procedures are now proposed in litterature ? Line 142, P.6 : on which basis is the 100 ng m⁻³ treshold chosen as a base for \hat{N} excess eBC mass \hat{Z} . This is misleading also when presenting results and figures : for example, it is not clear if 100 ng m⁻³ should be added to data in Figure 5 to actually get the actual measurements. Similarly, Table 2 (excess) and Table 1 (observed) are not directly comparable. I recommend not to use this 100 ng/m³ treshold consideration and stick to eBC, and not excess eBC. If any \hat{N} excess eBC \hat{Z} must be used, it should then be defined upon statistical analysis of the record as definition of background conditions is not so trivial. Line 160, P.7 : seasonal variability is more than just apparent in Figure 2. In fact, adding a Figure/Table with the actual monthly values would help (in fact, documenting diurnal variability would also help). Section 3.2.2 : I have difficulties connecting section 3.2.1 to section 3.2.2. If seasonal dependent synoptic circulation controls BC variability, this should also appear somehow in the back-trajectory analysis, which is not mentioned. I am surprised not to see any artefact in Figure 4 due to the lack of measurements during the first 6-month in 2015 (not explained). In this section, linking concentration values for each flow pathways to potential sources identified through relevant emission inventories may add useful information to the study. Table 2 : many other studies can actually be added to the table (also add your own results).

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