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Interactive comment on "Four years (2011–2015) of Total Gaseous Mercury measurements from the Cape Verde Atmospheric Observatory" *by* Katie A. Read et al.

Anonymous Referee #2

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The authors present measurements of mercury concentrations in 2011 - 2015 at a coastal site located at Cabo Verde archipelago. They used a standard, well established, quality controlled technique. The data are analysed mainly in terms of comparison with measurements at other sites around the Atlantic Ocean, seasonal variation, trends and their seasonal variation, differences between air masses of different origins, and diurnal variations.

The data are valuable and I recommend a publication of their analysis. Unfortunately, the presented analysis is rather superficial and at times flawed. The used statistical methods are not always described and the statistical significance of averages, trends and their differences is frequently not given. Consequently, probably insignificant dif-

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ferences are sometimes discussed at length. In addition, the discussion is at times muddled by using false references or misusing some. The major deficiency of the paper is, however, that a suite of other species is measured at Cabo Verde, such as O3, CO, nitrogen oxides, VOCs, and greenhouse gases, even BrO, but these data are completely ignored in the presented analysis. E.g. CO data could provide decisive information about the origin of high Hg concentrations from the African continent: whether it is biomass burning or small scale artisanal gold mining. The BrO data could provide important clues about bromine chemistry. The ancillary data could also help to identify air masses from the southern hemisphere. In addition, they could help (with backward trajectories) to explain the origin of events with extremely high and extremely low mercury concentrations. As it is, the mercury data are used far below their potential. I recommend the publication of the paper after substantial improvements, some of which are listed below.

Specific comments

Page 1, line 38: GEM has to be defined when introduced. GEM is Gaseous Elemental Mercury and as such not TGM. TGM is GEM + RGM.

Page 1, line 41-42: In the context of this paper, biomass burning is probably a very important Hg source. It may be initiated by man or nature and as such does not fit only the category of anthropogenic sources.

Page 2, line 38: The paper by Slemr et al (2013) as cited in references is about 222Rn calibrated terrestrial fluxes in southern Africa and not about trends. A paper by Slemr et al about trends would be Atmos. Chem. Phys. 11, 4779-4787, 2011.

Page 3, line 15: “Leipzig” instead of “Leibzig”

Page 3, line 30, to page 4, line 8: It should be mentioned that GEM is measured because RGM (or GOM) will be most likely captured by the salt deposited at the inlet tubing and the particle filter. The standard conditions of the reported mercury concentrations have also to be clearly stated.

Page 3, line 18-26: Information about diurnal circulation pattern (sea and land breeze) is needed because it is important for the interpretation of the diurnal cycle.

Page 5, line 16-18: Chemistry is not the only possible explanation, an influence of air masses from the southern hemisphere without any pronounced seasonal variation may be another (Slemr et al., Atmos. Chem. Phys. 15, 3125-3133, 2015).

Figure 3: What do the bars represent?

Page 5, line 24-26: Duncan et al. (2007) do not say anything about seasonal variation of specifically emissions from coal burning - they calculate only emissions from fossil fuels. In fact, emissions from coal burning tend to have a flat seasonal variation because of residential heating in winter and air conditioning in summer (Rotty, Tellus 39B, 184-202, 1987). The sentence needs rewording.

Section 3.2 about trends: The method of trend calculation is not clearly stated and neither is the significance of the discussed trends. Non-significant trends should not be discussed at all. The significance of the presented trends is probably low because only 4 years of measurements with numerous gaps are available. In addition, the unevenly distributed data gaps and pronounced longer term events with high GEM concentration, such as after December 2013 and before December 2015 could produce wrong trends. A defensible statistical method of calculation of trends and their significance is sorely needed. When stating averages and medians, the number of measurements they represent has always to be stated. Without the number of measurements no statistical tests for significance of differences can be made.

Page 6, line 8: The trend has to be given with 3 significant decimal numbers to be consistent with its standard deviation.

Page 7: For the regional classification of origin of air masses the reader has now to consult Carpenter et al. (2010). A reproduction of the figure from 5 from Carpenter et al. (2010) or even better a similar figure of trajectories for 2011 – 2015 would make the reading more comfortable.

Figure 5: The colour differences are not very pronounced, please improve.

Table 2: Number of measurements should be stated for each average.

Page 7, line 12-23: This discussion would make sense only if the average concentration for AFR air masses were significantly different from the other air masses. Statistical test for differences have thus to be presented. As mentioned before, biomass burning has to be considered in addition to the AGSM activity and their relative importance could be assessed by using the CO data.

Page 8, line 9-12: The statement that the method is "resistant to outliers" is not quite true because monthly averages themselves can be strongly influenced by outliers. Using monthly medians instead of monthly averages would remove this objection. It could even provide more certain results – see below.

Figure 6: The slopes and their uncertainty in green have to be substantially enlarged in final version to become readable.

Page 8, line 16-24: I think that this discussion is flawed. All the calculated trends including the one for AFR are significant at some numerical level, the significance for AFR being the lowest but still existent. In other words: the AFR trend is more uncertain than the other ones but still existing. What matters for the discussion are the differences between the AFR trend and other trends (their slopes), not the absence of the significant AFR trend at a preselected discreet significance level of 95% (why not 90% or any other number?). Because of the larger uncertainty of the AFR trend there is probably no significant difference between the trend slopes. If so, there is no difference to be discussed. The discussion also neglects that the AFR trend is based on the smallest number of monthly averages (the number of monthly averages for each trend should be given in Figure 6) one of which is with 1.6 ng m-3 extremely high.

Page 8, line 26-34: The discussion in this paragraph is muddled. In the first sentence measurement at Mace Head (Weigelt et al., 2015) are compared with those at Cabo Verde which is questionable because of geographical and temporal difference. The trend of -0.016 $\pm$ 0.002 ng m-3 yr-1 for subtropical maritime air masses is implausibly precise for a suggestion originating from a model by Soerensen et al. (2012). In fact this trend originates from the analysis of Weigelt et al (2015) specifically for subtropical maritime air masses, not from Soerensen et al. According to Weigelt et al. (2015) this trend did not changed with time and can thus be directly compared with measurements at Cabo Verde in 2011-2015. No levelling off was observed for these air masses in contrast to the statement in the last sentence of this paragraph. In the third sentence the trend is erroneously compared with seasonal variation.

Section 3.3 Short term variability: With statistically insignificant diurnal variation it does not make much sense to discuss the difference between a maximum and a minimum. To reveal a more distinct diurnal variation one should try to get rid of the day-to-day variation by e.g. normalizing the hourly data to a daily average. Because Br is also produced photochemically the diurnal variation should not be discussed solely in terms of OH chemistry. I wonder why BrO measurements at the station are not used in the interpretation of the mercury chemistry. As a coastal site, the diurnal variation may also be influenced by sea and land breeze.

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