

Interactive comment on “New insights into the atmospheric mercury cycling in Central Antarctica and implications at a continental scale” by H. Angot et al.

Anonymous Referee #1

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General comments The manuscript is well written and presents a novel dataset of atmospheric mercury and mercury in snow interstitial air covering slightly more than one annual cycle. Mercury measurements from Antarctica is generally scarce in particular good data covering more than the summer season, so this manuscript will definitely be a longed for addition to the pool of mercury data for the scientific community, both for experimentalists and modelers. The references used are recent and relevant. The structure of the result and discussion chapter is complicated. The text jumps between different environmental compartments (atmosphere and snow interstitial air) and seasons, which makes it ponderous to follow the discussion. The authors should consider re-arranging the sections within the results and discussion chapter. Scientific com-

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ments Line 65-70: The manuscript text says that the Antarctic plateau was first considered to be chemically- inactive for atmospheric species including Hg and a paper from 2008 is cited. The manuscript text further says that it turned out to be highly active, now citing a paper from 2001 and 2007. To me there is a lack of logic in this argument. Line 131: Was mercury saturated manually? I assume you mean manually injecting saturated mercury. Line 144-146: QA/QC; Interesting that you use internal standard on the 2600, what was used as internal standard? Why not use commercially available control samples? Line 202-204: This was an elegant way of defining seasons! Line 211-212: Troll can hardly be called a coastal site as it is situated almost 250 km from the coast and at almost 1500 masl. However, due to its location Troll experiences air from both the Antarctic plateau and the southern Ocean, but it is not a coastal site. Line 259-263: I like the figure showing the vertical distribution of Hg(0) concentrations, however I think it could also be interesting for the audience to present a figure showing time series of Hg(0) in SIA as well. The atmospheric Hg(0) time series seems well covered in other figures. Line 356 and throughout the paragraph: I am not quite sure but I believe it is general consensus regarding the use of the term “depletion event” being oxidation of Hg resulting from bromine photochemistry and high correlation with tropospheric ozone depletions. The multi-day low concentrations described in chapter 3.5 are a different mechanism and should consequently be called something else. Line 391 and throughout chapter 3.6: A mechanism for reduced Hg(0) during winter is proposed, and it is also mentioned that this reductions are not observed at Troll or Neumayer. Any thought on why this reaction mechanism do not occur at Troll or Neumayer? Do you believe this reaction mechanism occur throughout the Antarctic plateau? Additionally, as you have O₃ measurements it would be interesting to have them presented in fig 12 alongside Hg(0) since O₃ is suggested to be involved in the Hg(0) wintertime decrease. The authors should perhaps also have a look at a very recent paper by Nerentorp Mastromonaco et al., 2016 (Atmospheric Environment) where winter depletions are present and to check whether this may have any relevance for the winter decreasing trend observed in this study.

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Technical comments: Line 412: Several characters in the equation disappear in print
Figure 6: It is difficult to tell the difference between summer and spring/fall colours in the figure and I would very much like to see something similar to box and whisker plots to be able to tell the concentration distribution at each height at each season. Figure 9: Hg(0) concentrations, which measurement height do the results represent? Figure 11: Same comment as above, please indicate at which height the Hg(0) measurements are from. Figure 12: same comment as above.

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