

Interactive comment on “Total gaseous mercury depletion events observed at Cape Point during 2007–2008” by E.-G. Brunke et al.

Anonymous Referee #1

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General comments

This article by Brunke et al., presents a clear and well written description of a fascinating phenomenon observed during a long series of high time resolution atmospheric Hg measurements at Cape Point in South Africa. The observations show that the atmospheric Hg⁰ concentration decreases to almost zero for several hours at a time. Such behaviour has never been seen outside of polar regions, and is unexpected, not least because it would seem that the mechanism in this instance is different to that which is believed to occur in the Arctic. The article merits publication as an important contribution to the available data on atmospheric Hg concentrations and because of the singularity of these 'depletion events', which until now were unknown at these latitudes. There is a severe lack of data regarding atmospheric Hg species concentrations in the

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southern hemisphere and the data presented here are valuable for this reason as well.

However, the data presented can only be described as surprising, as mentioned above and will no doubt be questioned within the mercury chemistry community – as can be seen already from the interactive discussion. It is possible that there will be some scepticism regarding these results, especially as the DEs observed seem not to be directly related to any specific variable, or combination of variables, in the meteorological or trace gas data from Cape Point. For this reason, I suggest that the authors provide a detailed explanation, and supporting evidence if possible, to demonstrate that their Tekran analyser was working correctly.

Specific comments

In the Abstract the authors mention DEs reported for urban plumes, (line 14, there is no reference) and then in the Introduction they state that DEs have so far only been observed in Polar Regions. This should be clarified.

As mentioned above I recommend that the authors add a section in which, possible interferences, malfunctions, changes in sensitivity etc., are discussed. The reason for this is that the data presented are unusual, unique in fact. Measurements of Hg⁰ have been performed at coastal sites in numerous places (mostly in the northern hemisphere admittedly), and there have been a number of oceanographic cruises during which Hg⁰ has been measured: but none of them have ever recorded anything similar to the DEs described here. The other factor that makes these results distinct from other phenomena regarding atmospheric Hg species, is the lack of link to any meteorological or atmospheric chemistry variables apart from low speed and possibly a change of air mass. Until now, for events in which the partitioning of Hg between Hg⁰, Hg^I and Hg^P has been observed to be different from the norm, it has been possible to find a link to another change, in either meteorological variables or atmospheric chemistry variables, even if an explanation was not immediately obvious. AMDEs, or high RGM concentrations for example have been shown to occur under conditions which are par-

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ticular, such as the simultaneous depletion of ozone in the Arctic or the maximum RGM in the MBL occurring around midday (maximum $h\nu$). Therefore I feel that the onus is on the authors to provide irrefutable evidence that the DEs are not instrumental artefacts, or the result of sampling or analysis errors. The EPA methods, nos. 1631 and 245.7 (<http://www.epa.gov/waterscience/methods/method/mercury/>), give some indications as to which contaminants can interfere or reduce the sensitivity of the CVAFS detection system. I believe that the shape and timing of the CVAFS peak can also indicate the presence of contaminants in the traps, the authors may have peak timings and areas from calibrations during DEs which would be relevant.

In the experimental the authors state that under the prevailing atmospheric conditions at Cape Point, they do not sample RGM and that the Hg reaching the gold traps is Hg^0 , however throughout the rest of the article they refer continuously refer to measurements of TGM which is misleading if it is Hg^0 which is being measured, as most readers, I think, would assume TGM to be $Hg^0 + Hg^{II}$. This also applies to the title, was total gaseous mercury depletion observed or was it elemental mercury depeltion?

It would be interesting to know where the air masses which showed low Hg^0 concentrations during the night (both on the 19th September, section 3.1), had been during the previous 24 hours.

Is the gold mining source of Hg^0 referred to (p20989) artisanal mining? I was under the impression that the gold refining industry used cyanidation leaching technology, which has quite low emissions.

The authors discuss the apparent preference for DEs to occur with certain wind directions and say this preference "... is not very pronounced and may disappear if the data below 5 m.s^{-1} is included." Does it?

Technical corrections

I am unsure about the use of the word quantitative in this case (Abstract). If there
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were production of RGM or Hg^P at the same time maybe that could be described as quantitative, perhaps 'total' is better here.

Some of the references in the introduction are not very recent, the recent UNEP report on the fate and transport of Hg, published by Springer as "Mercury Fate and Transport in the Global Atmosphere: Emissions, Measurements and Models", (Pirrone, N. and Mason, R. P. (eds.), Springer, 2009) contains some up to date estimates of emissions, residence time and obviously, references.

The use of Hg° is not really correct, it should be Hg^0 . P20983 line 17.

I am not sure what is meant when the airflow (p20987, line 27) is described as "appropriate".