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Atmospheric Measurement Techniques Discussions

Interactive comment on "Measurements of greenhouse gases and related tracers at Bialystok tall tower station in Poland" *by* M. E. Popa et al.

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General comments:

R: The better understanding of the regional and global budget of greenhouse gases and to feed the inverse models (allocating the sources and sinks, potentially testing the emission inventories reported) require, among others, high quality tall tower measurements. These are rare yet, and the most of the existing towers are only gathering experience. Exchange of information and experience is essential for the development. Therefore, any paper on this topic is valuable for the scientific community. Popa and her coauthors give a fairly comprehensive description of the set up and operation of their monitoring site. I think, this part of the paper is essentially acceptable as it is.

However, evaluation of the available data is somewhat weaker, the explanations of the phenomena revealed are rather speculative without solid scientific background or supportive data. Taking into account the shortness of the available data series it is partly understandable, but some improvement would be desirable anyway. Reading the manuscript I faced references to hardly accessible papers, reports that I do not like but their acceptance depends on AMT publication policy.

A: We addressed the reviewer's general comment, by extending the data analysis and the comparisons with previous works. However, we maintained the focus of this AMT paper on the technical setup, the description of the data series and the general features, without aiming to make a thorough interpretation. In-depth analyses will require dedicated studies, with more powerful methods including modeling, and will be prepared for different journals.

We referenced where available peer reviewed literature. Unfortunately, some of the information that was essential during the design, setup and operation of our measurement station is only available in hardly accessible papers. However, most of these papers are freely available on-line. In those cases, we included the url addresses in the reference list (see specific comments).

R: Tall tower publication from Europe is rare yet. As far as the reviewer knows, it is the third of such kind (not taking into account the papers in preparation), after HUN (Haszpra et al., 2008) and OXK (Thompson et al., 2009). Therefore, it is important to compare the methods applied and the data measured. OXK is mentioned in the paper, but HUN – being also on the eastern edge of the CHIOTTO network, not very far from BIK, having relatively long time series – is missed. Its inclusion would also be reasonable because both Haszpra et al. (2008) and Popa et al. (2009) use the SF6 measurements in the same way to argue for the rural character of their sites.

A: SF6 is not used in our paper as an argument for the rural character of the site. Our data shows that there are no significant SF6 sources in the area, but this cannot be

extended to the other species. In fact, we observe features that indicate significant anthropogenic influences, for example the large CO signals (especially during winter), and the diurnal cycles with morning and evening peaks and vertical gradients.

We extended the comparisons with other studies, including Haszpra et al., 2008.

I would also recommend to have a look at NOAA's BAL site located also in Poland and having data series longer than a decade, although they are based on flask samples.

Haszpra et al., 2008: Trends and temporal variations of major greenhouse gases at a rural site in Central Europe. Atm. Envir., 42,. 8707-8716. doi:10.1016/j.atmosenv.2008.09.012

Thompson et al., 2009: In-situ measurements of oxygen, carbon monoxide and greenhouse gases from Ochsenkopf tall tower in Germany. Atmos. Meas. Tech., 2, 573–591, 2009 www.atmos-meas-tech.net/2/573/2009/

NOAA: http://www.esrl.noaa.gov/gmd/ccgg/iadv/

Specific comments:

1. Page 2590, line 6-7 and line 22-23: The order of the references should be corrected.

A: The correction has been done.

2. Page 2591, line 12-13: For detailed description of the measurement setup the authors refer to Popa (2008). This potentially very important and useful publication is a PhD thesis, and practically inaccessible by the readers. I do hope that the work is publicly accessible in electronic form, otherwise the reference is useless and it should be removed. Please, specify the URL in the reference list! (See a list of other problematic references below.)

A: The reference contains much more technical details, which could find space in a PhD thesis, but not in an article of reasonable dimensions. The PhD thesis is freely available on-line: http://www.db-

thueringen.de/servlets/DerivateServlet/Derivate-14971/Popa/Dissertation.pdf, and: http://www.bgc-jena.mpg.de/bgc-systems/dissertations/Popa_thesis.pdf We added the url to the reference.

3. Page 2591, line 14-15: The authors mention the comparison with data from other stations. In addition to OXK HUN and NOAA's BAL could (should?) also be used for comparison. It could reveal if the trends based on the short data series measured at BIK fit to the longer term trends measured at HUN and BAL.

A: We extended the comparisons with previous works, including Haszpra et al., 2008.

4. Page 2592, line 7: Although the prevailing circulation pattern may certainly be westerly at BIK the nearby big city in the opposite direction might cause pollution episodes occasionally. Is the data series checked and filtered for episodic, regionally nonrepresentative data? How frequently is the station covered by urban plume? Never?

A: The submitted data are not filtered according to representativeness. Representativeness flagging is a sensitive issue, and the data user has to do it according to the purpose and method of data use. For certain calculation (trends, seasonal and diurnal cycles) we used trimmed averaging (which exclude the highest and lowest certain percent of the data) and robust fitting methods (which down-weight the outliers) – these methods reduce the influence of local pollution events.

Being situated inside the continent, the station is indeed most often affected by local and regional emissions. We mostly do not see polluted / clean differences, but rather a gradual scale - more or less polluted air masses. Also, the degree of pollution depends on the sampling height in question. There are periods when the lower sampling levels receive strong anthropogenic influence, while the highest sampling levels see cleaner polar or free troposphere air.

5. Page 2592, line 9: Influence area calculation refers to a paper (Vermeulen et al., 2006) that was not accepted for publication, it could not be upgraded from discussion

phase. In principle, such a paper might contain inappropriate methods or false interpretation for what the reviewers/editors rejected, although the manuscript has remained publicly accessible as a late discussion paper. It is a question to AMT editor if such a publication is acceptable for reference.

A: We replaced the reference for the COMET model with Vermeulen et al., 1999.

6. Page 2596, line 17: Why is the lower wind monitoring level not co-located with the other measurements?

A: The lower wind sensor was added later, and was initially meant to be installed at 90 m, at the same level with the air intake. The installation at 90 m was not possible (due to technical reasons related to the tower structure) and the nearest location where the installation could be done was the 75 m level.

7. Page 2600, line 4-6: Only those flasks were selected for which the nearest in-situ measurement sampling time from 300 m height was different by at most 2 h (for CO2 and O2/N2) or 3 h (for CH4, CO, N2O and SF6). The in-situ measurements are (quasi-)continuous, are not they? Why so wide time range has been chosen?

A: In-situ measurements are indeed quasi-continuous, but the air coming from different heights is measured alternately. A measurement series of 15-18 min (for CO2 and O2/N2) or 42 min (for GC) is performed from a certain sampling height, after which the system will switch to the next sampling height or gas cylinder. Typically the time interval between two measurement series of air from 200 m varies between 1 h and 5 h. Moreover, some of the measurements are eliminated following quality check. As a consequence, there are flasks for which no good measurement from the height of 200 m was available, in the 2 h or 3 h interval.

8. Page 2607, line 6-17: The steeper than global trend may be caused for different reasons that should be discussed in the paper. The data series starts in August when the mixing ratio is the lowest in the year and it ends in December when

it is the highest. Taking into account that the data series is only a bit longer than 3 years this fact may distort the trend alone. (It may also influence the trends of other GHGs measured.) However, there are also recent changes in the methane mixing ratio trend: The growth rate has increased significantly since 2007. The station might catch this signal. See e.g. Dlugokencky et al., 2009: Geophysical Research Letters 36, L18803, doi:10.1029/2009GL039780, or the newest WMO GHG Bulletin no. 5 (http://www.wmo.int/pages/prog/arep/gaw/ghg/GHGbulletin.html In the comparison the same period has to be used for all sites in the comparison.

A: The computed trend is indeed dependent on the time interval used, even if we use data subsets that start and end at the same time of the year. For example, if the interval 25-Dec-2005 to 24-Dec-2008 is considered, the trend resulted will be 16.2 +/- 5.1 ppb / year; for the interval 1-Aug-2005 to 1-Aug-2008, the trend will be 8.3 +/- 5.9 ppb / year. This is mainly due to the large variability (including inter-annual) in CH4 mixing ratios, combined to a short data series. This imprecision in trend estimate is however reflected in the large uncertainty assigned to our results.

We extended the discussion in the text, taking into account the reviewer's comments.

9. Page 2608, line 7-9: The main sink for methane is also the reaction with OH!

A: The main sink for both CO and CH4 is the reaction with OH. The CH4 minimum during summer is largely due to the seasonality if OH radical concentrations. However the OH seasonality affects CO atmospheric concentration much more than it affects CH4. This difference is related to the higher reactivity (and faster atmospheric turnover) of CO compared to CH4. The lifetime of CH4 is on the order of 10 years, while the CO lifetime decreases during summer to few months. We introduced this explanation in the text.

10. Page 2608, line 23 – page 2609, line 2: It is only a speculation. What are the different sources and sinks? N2O has hardly any sink in the troposphere or in the soil. Is there any correlation between the N2O mixing ratio increase and snow melting? The

denitrification process is strongly temperature dependent. It produces very little N2O below zero, if any. The offset between the lowest level and the other levels seems rather constant throughout the year while the soil emission certainly has a remarkable seasonal variation. Is there any other source there? This section should be rewritten.

A: This section has been rewritten, taking into account the reviewer's comments.

11. Page 2611, line 24: The order of the references should be corrected.

A: The correction has been done.

12. Page 2612, line 4-10: In many cases the diurnal variation in the vertical mixing of the atmosphere is the main governing factor for the diurnal variations of the trace gases. I can imagine that it is also the case at BIK. Has it been investigated? Can it be excluded or its contribution estimated?

A: The vertical mixing is at least a partly responsible for the diurnal variations for each species (as mentioned at the beginning of Sect. 3.4.2). However, if vertical mixing were the only cause for the diurnal cycles of CH4 and N2O, and if the sources of these species were constant, then the night time increase would have been monotonous, similar to the one of CO2. The existence of a similar midnight dip for CH4 and N2O suggests that there are other contributing factors to the diurnal variations, which have similar temporal evolution for the two species. The paragraph has been rewritten.

References: 13. Henne et al. (2008) is a potentially important reference on page 2592, but hardly accessible.

A: We replaced the reference with Henne et al., 2009, published in the mean time in ACPD.

14. Bibliographic reference to Sturm et al. (2006) is not complete.

A: The reference has been updated.

15. Thompson et al. (2009) has already been published in AMT (see above for refer-

ence).

A: The reference has been updated.

16. Vermeulen et al. (1997) is hardly accessible by the readers.

A: The paper is accessible online at: http://www.ecn.nl/publicaties/default.aspx?nr=ECN-C–96-088 The url has been added to the reference.

17. Vermeulen et al. (2004) is hardly accessible by the readers.

A: The repport is accessible on-line at: http://ccu.jrc.ec.europa.eu/Pubblications/IM_WS_repc The url has been added to the reference.

18. Vermeulen et al. (2009) is in preparation and it has not even submitted to any journals yet. I do not think that it can be accepted as a reference.

A: The papers that are still in preparation have been removed.

19. Vermeulen et al. (2006) has not been accepted for publication, it remained in the discussion phase.

A: The reference Vermeulen et al. (2006) has been replaced with Vermeulen et al. (1999).

20. Worthy et al. (2003) is hardly accessible by the readers.

A: Worthy et al., 2003 is the first description of a gas chromatographic measurement method widely used today. This paper was an important source of information during the setup of our measurement station. Although we are aware that the paper is hardly accessible, we think it's fair and important to acknowledge its contribution.

Figures:

21. Figure 5: The symbols cover each other. A better representation would be desirable. What is the time resolution of the data in this figure?

A: The figure was redone with smaller symbols and better resolution. Each point represents one measurement, 3 min (for CO2 and O2) or 14 min (for GC species). The interval between two consecutive measurement series (one measurement series is 3 -6 consecutive measurement points) from the same height varies typically between 1 hr and 5 hr.

22. Figure 8: In the case of each GC measurements we see a surprising dip at midnight. It may not have any atmospheric reason because this dip does not appear in the CO2 record. The gases measured by the GC have different sources, therefore it is difficult to imagine any common reason but the instrument itself. Might this dip be an instrument artifact originated from some sort of regular maintenance/calibration/reset/etc. scheduled to midnight? The striking dip should be explained in the text!

A: There is no event (calibration, maintenance, reset etc) with a 24 hours (or multiples) periodicity in the measurement routine at Bialystok – this was avoided intentionally. Also, the probability for each sampling height to be measured at / around midnight is the same – thus an instrumental artifact would be seen equally in all sampling heights. Based on these, and on our detailed quality check, we are convinced that the midnight dip is not due to any instrumental artifact.

The CO2 diurnal cycle shows that the midnight dip in the GC species is not due to changes in vertical mixing. We interpret this feature (Sect. 3.4.2, paragraph 3) as a reduction of anthropogenic emissions (e.g. industry, biomass burning, transportation) between the evening and morning peaks. This would be consistent with the larger dip observed in CO, which has in larger proportion anthropogenic sources.

Taking into account the merit of the paper and the fact that its weaknesses can be corrected I recommend the paper for publication after revision.

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