

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

**Anonymous Referee #2**

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Review of the manuscript "Measurements of greenhouse gases and related tracers at Bialystok tall tower station in Poland" submitted by Popa et al.

General comments:

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

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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.

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 SF<sub>6</sub> measurements in the same way to argue for the rural character of their sites. 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:

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

Page 2591, line 12-13: For detailed description of the measurement setup the authors

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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.)

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.

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 non-representative data? How frequently is the station covered by urban plume? Never?

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.

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

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 CO<sub>2</sub> and O<sub>2</sub>/N<sub>2</sub>) or 3 h (for CH<sub>4</sub>, CO, N<sub>2</sub>O and SF<sub>6</sub>). The in-situ measurements are (quasi-)continuous, are not they? Why so wide time range has been chosen?

Page 2607, line 6-17: The steeper than global trend may be caused for different rea-

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sons 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.

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

Page 2608, line 23 – page 2609, line 2: It is only a speculation. What are the different sources and sinks? N<sub>2</sub>O has hardly any sink in the troposphere or in the soil. Is there any correlation between the N<sub>2</sub>O mixing ratio increase and snow melting? The denitrification process is strongly temperature dependent. It produces very little N<sub>2</sub>O 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.

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

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?

References:

Henne et al. (2008) is a potentially important reference on page 2592, but hardly accessible. Bibliographic reference to Sturm et al. (2006) is not complete. Thompson

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et al. (2009) has already been published in AMT (see above for reference). Vermeulen et al. (1997) is hardly accessible by the readers. Vermeulen et al. (2004) is hardly accessible by the readers. 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. Vermeulen et al. (2006) has not been accepted for publication, it remained in the discussion phase. Worthy et al. (2003) is hardly accessible by the readers.

Figures:

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

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 CO<sub>2</sub> 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!

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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