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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Surface gas pollutants in Lhasa, a highland city of Tibet: current levels and pollution implications" *by* L. Ran et al.

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Responses to referee #1:

The manuscript 'Surface gas pollutants in Lhasa, a highland city of Tibet: current levels and pollution implications' by L. Ran et al. presents measurements of CO, NO/NO2/NOx, SO2 and O3 made within the city of Lhasa. Data is interpreted in terms of meteorological influence, and comparison with available data from 1998 is shown. Especially the recent data seems to be of good quality, although information on measurement uncertainties is lacking. Since the data has been acquired at a location with very little available data sets, it is a valuable contribution which deserves to be published in ACP after addressing a few mainly minor issues. The manuscript is well



structured and of appropriate length. However, the analysis made is very descriptive, and a few interesting aspects could be discussed in more depth.

We highly appreciate the referee's instructive suggestions. We have addressed each comment as below and corresponding revisions have been made in the manuscript.

Specific comments:

1. P11789 L12: '. . .the content of oxygen in Lhasa is about 68% of that at the sea level': In absolute terms this is true, but the relative O2 content is still the same. Altitude dependent emissions of vehicles is an interesting aspect, but I doubt if this effect is really contributing much compared to the general increase in vehicle numbers.

According to the referee's suggestion, we added the term of 'absolute' in the revised manuscript. We made a great effort trying to obtain data regarding altitude dependent emissions of vehicles, but those above 3000 m could not be found. Distinct black smoke of vehicular exhausts can often be seen in Lhasa. However, we can exactly not tell how much incomplete combustion and an increase in vehicle numbers contribute to the increased emissions of vehicles, respectively. Thus, we revised corresponding sentence as:

At a high altitude above 3650 m, the absolute content of oxygen in Lhasa is about 68% of that at the sea level. Incomplete fuel combustion in the low oxygen-containing atmosphere (Chaffin and Ullman, 1994; Bishop et al., 2001; Nagpure et al., 2011), as well as the rapidly increasing vehicles undoubtedly enhances atmospheric pollutants of vehicular origin.

2. P11789 L12: 'The atmosphere in Lhasa was found to be photochemically active (Lin et al., 2008)': I am not sure if this sentence is needed or makes sense. If you decide to keep it, a few more words of the results of Lin et al. are needed. The atmosphere is probably everywhere photochemically active, but due to the altitude, photochemistry might even play a more important role.

Thank you. Lin et al. (2008) calculated the OH production potential over the Tibetan plateau and found the air there is photochemically active due to the strong solar radiation and relatively high ozone background. We have revised this sentence as:

With a high production potential of OH radicals over the Tibetan Plateau, where the solar radiation is strong due to the high altitude as well as the low latitude and the background concentration of ozone is high, the atmosphere in Lhasa was found to be very photochemically active (Lin et al., 2008).

3. P11790 L4: '. . .air quality in Lhasa was quite fine': A more quantitative statement or a brief comparison with data from other cities would be helpful.

Thank you for this helpful suggestion. A more quantitative comparison has been given in the revised manuscript as:

In comparison with many other cities in China (He et al., 2002 and the references therein), air quality in Lhasa was quite fine. The level of air pollutants in Lhasa has never exceeded the national air quality standards according to the reports of the local environment protection bureau.

4. P11790 L12: Delete 'Finally is the summary.'

We have deleted this sentence.

5. P11791 L6: Are the measurements ongoing, or is the presented data part of a campaign?

The observational data presented here was obtained from a one-year campaign. In view of the importance of the data, we have been trying to continue the measurements.

6. P11791 L20: Does the TE 49CPS have traceability to an ozone reference instrument?

Yes, the TE 49CPS is traceable to the NIST standard, the Standard Reference Photometer (SRP) in WMO World Calibration Centre for Surface Ozone, Carbon Monoxide



and Methane, Empa Dübendorf, Switzerland.

7. P11791 L23 and throughout the whole manuscript: Concentration in this context is the wrong terminology. It should be 'mole fraction' instead of concentration (see e.g. http://gaw.empa.ch/glossary/glossary.html). Please carefully revise terminology throughout the whole paper. The unit should also be nmol/mol (ppb might be acceptable) but not ppbv. All ppbv need to be replaced by ppb.

We thank the referee for this very helpful comment. We have revised this sentence as "Ambient mixing ratios of trace gases were recorded as 1-minute averages (ppb)." We have also corrected the unit from ppbv to ppb.

8. P11793 L19: 'are displayed' instead of 'were displayed'. This should be changed throughout the manuscript.

Thank you. We have revised the manuscript accordingly.

9. P11797 L11: Unit (USD?) is missing.

Thanks. The missing unit is RMB. We have added it to the manuscript.

10. P11797/8 and Figure 6: It is quite astonishing that CO remained almost constant between 1998 and 2012, whereas NO2 became a factor of almost 40 higher. Both species are indicators for relatively fresh emissions, and high CO levels in combination with very low NO2 as observed in 1998 seem to be unlikely. How can you exclude measurement artifacts? What were/are the measurement uncertainties? It would be helpful to provide measurement uncertainties together with the data. The analytical techniques were different in 1998 and 2012, and I would expect a very large measurement uncertainty for the NO2 data in 1998. The fact that CO and NO2 are do not show any significant correlation in 1998 potentially indicates that data quality of the 1998 NO2 measurements was not sufficient. Or do you have other explanations? Please address this in more detail.

The GFC/NDIR technique was used in the measurement of CO in both 1998 and 2012.

The quality control procedures in 1998 were similar to that in 2012. Due to the very low concentrations of NO2 and SO2 in Lhasa, no commercial on-line analyzers could meet the requirements of such measurements in 1998. The absorbent technique with a relatively long sampling time was thereby used at that time. The recorded data were finally corrected by reference standards in the laboratory. The measurement uncertainties are believed not to be the explanations for a high CO level together with a very low NO2 level in 1998. The more possible reason might be the difference in the emission sources at the two sites in different years. Biomass burning was dominant in 1998, while fossil fuel burning is dominant now.

11. P11801 L12: CO has not significantly increased (again: why?). Should it be O3 instead?

Thank you very much. It's O3 instead of CO. We have corrected this sentence.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 11787, 2014.