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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.

Anonymous Referee #1

Received and published: 11 June 2014

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.

Specific comments:

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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.

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.

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.

P11790 L12: Delete 'Finally is the summary.'

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

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

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.

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

P11797 L11: Unit (USD?) is missing.

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

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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.

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

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