Atmos. Chem. Phys. Discuss., 14, C3517–C3519, 2014
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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 #2

Received and published: 12 June 2014

This paper presents data analysis for the air quality in Lhasa, Tibet. The manuscript is well organized and the data set is highly valuable to our understandings of the atmospheric chemistry in that region. However, there are some issues need to be discussed in further details. Thus I'd like to recommend a major revision to this article. The followings are some specific comments that should be considered carefully in the revision.

1. The ambient levels of trace gases measured in this study (2012) are compared with those measured in 1998. This comparison is very interesting and provides direct evidences for the environmental changes occurred during the 15 years. However, as different methods were used in the two investigations, results from the comparative study seem ambiguous. I suggest address the differences in instrumentation clearly in the revision, particularly in terms of measurement uncertainty.

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2. The scientific argument for comparing the air quality measurements made in Lhasa with those from Beijing and NCP is very weak. The two cities (areas) are so different in many ways. I'd like to suggest a comparison with another high altitude urban area, such as Mexico City, in the world. Many papers in the ACP special issue for the Mexico Study can serve as your reference.

3. The seasonal differences in nighttime ozone levels were attributed to emission of NO in cold seasons [Sec 3.2]. I disagree with this argument. As shown in Figure 5, it is obvious that, in line with O3, high Ox levels were also observed in spring and summer. Thus, the NO-O3 titration was unlikely responsible for the observed phenomena.

4. It was indicated that the mixing ratio of ozone peaked in spring (Sec 3.2), and local photochemical production of ozone was suggested as the main cause responsible for the seasonal features (Sec 3.3). The two arguments are not exactly consistent with each other because the photochemical production of ozone should be higher in summer than in spring. The formation of spring ozone maxima could be much more complicated than the simple attribution discussed in this paper, and merits an in-depth analysis in the revision.

5. I think that readers will not appreciate the content of Table 1 where you compared your measurement with a "self-defined" air quality standard. Are you trying to propose a new national air quality standard? Any Epi or toxicological data available to support this "new standard"? What implications are given from a case against this new standard? Please note that the national air quality standard is not merely a set of "numbers".

6. As a scientific paper, you can setup your criteria for data screen and define some pollution events in your study. Then, I strongly suggest make a table of descriptive statistics straightforward to the "measurements", i.e. the mixing ratios of respective trace gases, for selected cases or periods.

7. How did you define "urban heat island intensity" (Sec 3.4)? Some data analysis or references are needed to support your simple statement.

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8. Again, information of measurement precision is needed to facilitate the comparison between two datasets (shown in Figure 6). This is particularly important to the discussion on the differences in the changes of CO and NO2.

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