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Discussions

Interactive comment on "Long-term trends of surface ozone and its influencing factors at the Mt. Waliguan GAW station, China – Part 1: Overall trends and characteristics" *by* W. Y. Xu et al.

Anonymous Referee #1

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review of acp-2015-560

Title: Long-term trends of surface ozone and its influencing factors at the Mt. Waliguan GAW station, China, Part 1: Overall trends and characteristics Author(s): W. Y. Xu et al.

The manuscript presents a comprehensive description and analysis of the long-term time series of surface ozone observations at the high-altitude measurement site Mount Waliguan in China. The ozone data from Mt. Waliguan represents a very valuable dataset as it is one of the very few perennial time series that is available in pristine environments in Asia. The analysis and presentation is sound and the topic is within the

scope of Atmospheric Chemistry and Physics. There are only a few minor comments that should be considered before publication.

General comments:

Unfortunately, the paper remains a bit vague and less conclusive in some parts but refers to an accompanying paper that is still not yet available. As the present paper seems to lay the foundation for future analysis, the experimental section should be more elaborated on (see comments below) because a sound quality assurance of the data is key when looking at trends etc. Moreover, the findings should be more discussed in relation to other available time series at elevated Northern hemisphere measurement sites. This is already the case for the trends in Chapter 3.3 but could be extended to the sections where diurnal and seasonal cycles are presented.

The authors often refer to ozone concentrations but use ppb units. Concentrations cannot be given in ppb as numbers in ppb refer to mole fractions or mixing ratios.

The order of the Figures does not correspond with the appearance in the text. The references to Figs. 4 and 7 come earlier than the one to Fig. 3. Please reorder the Figures.

Specific comments:

Abstract:

the abstract is rather long; I suggest shortening it, e.g. by deleting "using a modified Mann–Kendall test and the Hilbert–Huang Transform analysis for the trend and periodicity analysis, respectively." and "Analysis suggests that there is a season-diurnal cycle in the three-dimensional winds on top of Mt. Waliguan. Season dependent daytime and nighttime ranges of 6 h were determined based on the seasondiurnal cycle in the three-dimensional winds and were used to sort subsets of ozone data for trend analysis."

Line 22: replace "increasing trend" by "positive trend"

Line 24: delete "relatively"

Lines 25-26: shorten the sentence to "Spectral analysis identified four episodes with different positive trends, with the largest increase . . ."

Main text:

Page 30990, line 14: reference to Lin, 2015 is missing

Page 30991, lines 4-5: "there are a few representative sites . . ."; does this statement refer to the situation in China? Which are the other stations? To my knowledge, the China Meteorological Administration also operates a remote measurement station at Shangri-La at nearly the same elevation than Mt. Waliguan. Are surface ozone observations available from the Shangri-La station?

Section 2.1 Sites and measurements

This part needs some elaboration. Duplicate ozone measurements seem to be available for most of the time. The authors state that data were used if the two analyzers agree within 5ppb. A quality control criterion of matching data within 5ppb is pretty lax and well above the data quality objectives for key GAW goals (see e.g. the GAW report #209 "Guidelines for Continuous Measurements of Ozone in the Troposphere"; available at http://www.wmo.int/pages/prog/arep/gaw/gaw-reports.html). How was the data flow implemented in detail? Was there one master and one backup instrument? How did they compare? Did you experience e.g. a steady bias, a perfect match, a difference as function of daytime, season, temperature, humidity . . .? Or random differences? What happened when the master instrument didn't record data but data from the backup analyzer were available? Were the data from the backup instrument used to fill the gaps? Have the backup data been corrected based on a long-term master-backup comparison? How many gaps were filled? Maybe an additional figure could help just showing a time series that illustrates which analyzer provided when data for the final data set used for the analysis. The authors mention that a TE49i model is used

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since 2011. Did this analyzer become the master instrument? How was it ensured that there is a smooth transition when changing the master instrument? Did the old and the new master run in parallel for a certain time? All these things are important information because the observed trends are small and could be also biased by some instrumental artefacts. When looking at Fig. 4a, there seems to be a discontinuity with slightly elevated ozone mole fractions for approximately the last two years. This step roughly coincides with the implementation of the TE49i analyzer. Can the authors comment on that?

Page 30993, lines 10-11: "Surface ozone data are recorded every 5 minutes . . .". I assume that this statement is misleading as the used ozone analyzers record data in 10 sec intervals. I suppose that the authors want to say that 5 min averages are recorded on the data acquisition. If this is the case, why not saying "Surface ozone data are recorded as 5-minute averages and corrected . . ."

What was the sampling height above ground for the ozone observations?

Page 30996, lines 4.5: rephrase the last sentence that it reads "The nighttime window also covers 6 h and is considered to be offset by 12 h to the daytime window."

Section 2.3: Did you use hourly averages for the analysis?

Section 2.3: Which software was used for the statistical analysis? Matlab? R? Did you use specific add-ons (packages)?

Page 31000, line 22: replace "Past researches" by "Previous studies"

Page 31001, lines 10-11: How does the long-term time series of 10Be/7Be look like. Is it possible to draw any conclusions on changes in STE strength?

Page 31001, line 20: "total ozone column", remove the ","

Page 31001, line 24: mention once more "based on zonal wind information".

Page 31003, lines 6-8: this is mainly true for remote locations.

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Page 31006: replace "Ds" by "DS" at various locations.

Summary: the concluding chapter only summarizes the findings presented above. I would like to see some outlook beyond. What will be looked at next? What are the implications of the findings? What does it e.g. mean for efforts to reduce maximum ozone levels in urban agglomerations (e.g. if ozone input due to STE is getting stronger)? Can the results somehow be generalized? What does it mean for the Asian outflow towards the Western US? Is the observed trend in Western US maybe caused by changes in STE input rather than increasing ozone precursor emissions in Asia?

References

Add urls to the Zellweger et al. audit reports, if online available.

Figures:

Figs. 2 and 3: is it confusing to have two different sets of white dots and dashed lines in Figs. 2 and 3. Since the differences in the seasonal-diurnal variations are discussed in Section 3.1, I suggest to add the daytime range based on the zonal wind (white dots from Fig. 2) in Fig. 3 and to draw the white dots in Fig. 3 based on minimum ozone in a different color. This makes it easier for the reader to compare the different features. The +/- 3h band is maybe even not needed here.

Fig. 5: add on the right.hand side of panels a5 to b5 "all", "daytime", nighttime"; add below the x-axis of the c-row "all", "spring", "summer", . . .

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