

Interactive comment on “Contributions of pollutants from North China Plain to surface ozone at the Shangdianzi GAW station” by W. Lin et al.

Anonymous Referee #1

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This manuscript of Lin et al. reports 3-years records of ozone and related trace gases measured at Shangdianzi station in the North China. The data should be very important for the scientific community because rare of works have been done in this polluted region. The paper is written in a compact format, overall structure is good, and figures are clear and well produced. However, the referee has a major concern on the methodology that used to address the main topic, i.e. the regional contribution of ozone from the North China Plain. The paper can be published in ACP, if this major concern and other comments were appropriately addressed.

The major concern: The authors use the surface measured winds to classify the ozone data into two contrast parts: natural background and polluted background, and further try to quantify the regional contribution from the North China Plain. This method has a

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large uncertainty because the surface winds at complex topography are not appropriated to address long-range transport issue. The Fig.1 shows that the site locates in a valley with a lot of mountains surrounded, therefore the wind should be large affected by local topography, and by the thermal driven mesoscale processes, like mountain-valley breezes, which can be clearly seen from Fig 2. So the classified results based on surface winds can only represent a relative small scale but not a regional scale as large as 1000 x 1000 km² that defined by the author. In another word, the results here can probably only reveal the impact from Beijing urban area, which is just located in the southwest of the site about 100km. However, the referee strongly suggests that author addressing the regional contribution from the North China Plain and comparing it with that from Beijing. Back trajectory analysis could be an appropriate methodology, or a method combining large scale wind data (like NCEP or ECMWF reanalysis) with the local winds could also be a choice. The referee find that the back trajectory analysis has been adopted in a sister paper using the same dataset by Meng et al., (ACPD, 8, 9405-9433, 2008).

In fact, if only using the local wind data some results presented in the current paper seem to be duplicated. For example, the seasonal difference of ozone between SW and NE has also been shown in Fig.7. The patterns of Figs. 10 and 11 are similar with Fig 3, maybe because the winds have strong diurnal variation with SW in the afternoon and NE in early morning and thereafter the difference between SW and NE somewhat reflects the range of O₃ diurnal change in Fig.3.

Other minor points:

Section 3.1 contains too much discussion comparing the observed results with those from a remote mountainous site Waliguan, which is totally different site from the Shang-dianzi. It should be better to make some comparison with previous works done in this area and also other similar polluted region in North Hemisphere, like Northeast US etc.

Section 3.2 including Fig. 5 is not useful since it only show a common frequency

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distribution of ozone. The referee suggests deleting this part or making it for different seasons and putting it in Section 3.1 when discussing the seasonal differences.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 9139, 2008.

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