

***Interactive comment on* “Characteristics of trace gaseous pollutants at a regional background station in Northern China” by Z. Y. Meng et al.**

Anonymous Referee #2

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This paper reports results of trace gases measured in the North China during 2004–2006. The seasonal cycle, diurnal variation and long range transport of air pollutions were investigated using some general data analysis techniques. However, in current form this paper is not well-organized and well-presented, and there are no any new/important scientific findings. Therefore, the referee cannot recommend it to be published in ACP. My main comments are listed below:

1. This manuscript looks more like a data summary/report but not a scientific article. In many paragraph in Section 3 (e.g. Page 9409–9410, page 9412 and 9417), the authors tried to repeat all results shown in tables and figures but without a more summarized discussions or conclusions.

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2. About the influence of biomass burning activities in summer, is there any evidence (like chemical signature of CO/NO_x and CO/SO₂ ratio, or satellite data) supporting this conclusion? For this issue, the referee actually doubts about the quality of CO data measured in summer 2006. Fig 2 shows a broad summertime peak of CO, but very low NO_x and moderate O₃ concentration. These results cannot suggest a possible source like biomass burning, which could also cause a high O₃ and NO_x. Meanwhile, Fig 4 shows that CO was about 700–800 ppbv in summer but almost without any diurnal change. This pattern is a little strange for such kind of rural station downwind of the polluted Beijing area. The referee believes that the local wind should have strong diurnal variation because of the complex terrain there.

3. For the long-range transport part (Section 3.4), why only one year 2006 was chosen for the trajectory analysis? Do the authors think the year 2006 is more climatically representative? However, the seasonal patterns of trace gases (Fig 2) do not support this point but show an abnormal seasonal cycle of O₃ and CO in 2006 compared with other years. So if the authors want to address a general long-range transport characteristics using trajectory cluster analysis, it would be better choosing a representative year. Otherwise, they should focus on the possible impact of change in long-range transport to the abnormal seasonal patterns of some species.

4. About the cluster analysis, is it really necessary to classify the trajectories into 14 categories? The authors should consider the statistical criterion to choose appropriate cluster numbers. In fact, some trajectory categories show similar pathways (refer to Fig. 5) and the trace gases concentrations neither show significant difference (Table 6).

5. About the wind rose analysis and diurnal change part in the discussion, the authors should be aware that the same results have been included in another paper in ACPD (by Lin et al., 8, 9139–9165, 2008).

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 9405, 2008.

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