

Interactive comment on “Variations of O₃ and CO in summertime at a rural site near Beijing” by Y. Wang et al.

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This review is by Owen R. Cooper, a co-editor for ACPD/ACP and the editor of this paper.

This paper describes the impact of the Asian summer monsoon on ozone production at a rural site in northeast China, showing that increased cloud cover associated with the moist monsoonal flow suppresses ozone formation. Overall the paper is well written and the data and model analyses support the conclusions. While the impact of cloud cover on ozone production has been well established in the literature, the results of this study are of general interest to the scientific community because it addresses ozone production in China, probably the world's fastest growing source of ozone precursors. I have not yet received the reviews from the two anonymous referees of this paper and

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their critiques of the manuscript, as well as your response to any concerns that they may have, will influence my final decision on whether or not the paper will be published in ACP. My current opinion is that the paper should be suitable for publication in ACP once important revisions have been made to the text, as described below.

The focus of the paper is that these results are of primary relevance to local authorities and the control of ozone pollution events within China. The policy of ACPD/ACP is that the journal scope is focused on studies with general implications for atmospheric science rather than investigations which are primarily of local interest. With its current focus the paper is not within the scope of the journal, and would have been rejected for being "out of scope" had it focused on a country (such as the USA or Germany) where much is already known about ozone seasonal cycles and the role of precursors and transport. But because the paper deals with China, where relatively little is known about the ozone distribution and trends, and where pollution levels are very high and rapidly increasing, the results have broader appeal. My major criticism is that the results are too focused on local issues and the context of the paper needs to be recast with an eye on broader implications for atmospheric chemistry. Following are my recommendations for shifting the focus of the paper:

In the Introduction please begin with a couple of paragraphs reviewing what is already known about surface ozone variability in China. Also highlight the limited measurements in comparison to Europe and North America, and the much shorter data records. Furthermore, please discuss the rapidly increasing emissions in China and the implications these have for the global ozone budget. Then go on to describe how the Miyun site fits into the picture. I'm assuming that: 1) this site fills a gap in our knowledge of summer ozone formation downwind of Beijing; and 2) understanding the factors that control ozone at this site can help constrain regional and global scale chemical transport models, which are important for quantifying the impact of China on the regional and global ozone budgets. It is fine to also state that the results will be beneficial for local ozone control strategies but this needs to be a secondary point.

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The following paper by Wild et al is relevant to your study.

Chemical transport model ozone simulations for spring 2001 over the western Pacific: Regional ozone production and its global impacts Author(s): Wild O, Prather MJ, Aki-moto H, et al. Source: JOURNAL OF GEOPHYSICAL RESEARCH-ATMOSPHERES Volume: 109 Issue: D15 Article Number: D15S02 Published: MAY 21 2004

Their springtime study shows how stagnation events allow for strong ozone production over east Asia, but these conditions tend to keep the ozone over Asia. In contrast cloudy frontal systems don't produce as much ozone over Asia, but export the precursors downwind where ozone is formed, having a greater impact on global ozone. In light of this study it would be very useful if you can use GEOS-CHEM to determine if the cloudy monsoonal conditions have large quantities of ozone precursors that can easily produce ozone once the cloudy air mass is exported from China. For example, under cloudy conditions is there still a lot of NO_x available for future ozone production or has most of it already been converted to HNO₃ or other oxidized species? This analysis will help to make your study more relevant to the broader scientific community.

My final recommendation for making the paper more appealing is to provide some context for the very high pollution levels found in rural China. For example your statement on line 11 of page 10406 describing a "modest peak" of 800 ppbv of CO comes as a surprise to someone accustomed to studying pollution episodes at rural sites in North America. A modest CO peak at a rural site in the eastern USA would be 200 ppbv, with 300 ppbv being very high. The pollution levels in China far exceed what is commonly found in the rest of the world and you need to make this point clear. I would like to see a figure comparing the CO percentiles (like your Figure 2) of ozone and CO at Miyun for June, July and August, averaged over the years 2005-2007 and compared to a rural site in eastern North America. Harvard Forest would be a good choice seeing as it is at a similar latitude (although I'm not sure if the Harvard Forest data are measured from the top of a tall tower which will make it difficult to compare the ozone data, but should be fine for CO, during well mixed daytime conditions).

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Minor Comments

Throughout the paper CO and O3 are described in units of ppbv, but described as concentrations, which is incorrect. All ppbv values need to be referred to as mixing ratios. Also all instances of ppb need to be replaced by ppbv. Also the brackets around [O3] and [CO] are unnecessary and need to be removed.

page 10399, line 1 change to "an understanding"

page 10399 line 9 change to "an elevation of 152 m"

page 10399 line 11-13 rearrange to: Mountains rise steeply to the north of the site, while the terrain to the south falls off gradually to about 90 m in a region characterized by a mix of agriculture and small villages.

page 10399 line 15 change to: "between relatively clean continental air"

page 10399 line 19 change to: "includes a suite"

page 10400 line 12 When CO is measured, is it concentrations or mixing ratios that are determined by subtracting the zero value from measured voltage. etc?

page 10401 line 8 change to "and run through 2006"

page 10402 lines 1-3 change to: "CO levels increased by about 300 ppbv from June to July, monthly mean O3 decreased by 17 ppbv"

page 10402 line 10 The Chinese air quality standard corresponds to 102 ppbv. Is this determined at standard temperature and pressure, or some other level of temperature and pressure?

page 10402 line 20-21 the trend is not increasing, but the CO is increasing, so change this sentence to say: "capturing the increase in CO from June to July"

page 10402, line 27 Please provide some details of the TRMM data, such as the type of instrument, the platform, orbit frequency and spatial resolution of the monthly average

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rainfall data.

page 10403 line 2 change to "East Asian summer monsoon"

page 10403 line 8 change to: "observations sampled at the site had less than"

page 1040 line 10 change to: "clean air was encountered only 15% of the time"

page 10403 lines 21-22 are the relative humidity values averaged over all hours, or just daytime hours?

page 10403 line 27 change to "influence on photolysis rates"

page 10404 line 26 change impressive to striking

page 10405 lines 20-22 How do you know that the model underestimates NO, do you have measurements that you can compare with the model.

page 10405 line 26 You state that the model demonstrates the significance of the radiative impact of monsoonal clouds on ozone and explains why obs. at other sites in east China do not show an O₃ max. in summer. But how do you know this? Have you, or other researchers actually conducted analyses to see if this really is the case, or is your statement just speculation? Please clarify.

page 10406 line 22 change to "Large intra-seasonal differences"

page 10407 line 1 change to "associated with the onset"

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 10397, 2008.

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