

## ***Interactive comment on* “Contributions of regional and intercontinental transport to surface ozone in Tokyo” by M. Yoshitomi et al.**

### **Anonymous Referee #1**

Received and published: 16 May 2011

This is a generally well-written manuscript and the discussed analysis is well conducted. The methods applied are commonly used and results overall in agreement with findings from previous works. Given the increasing awareness of the importance of ozone pollution transport on local air quality this study discusses a timely issue, even more so given the tight air quality standards in Japan (hourly ozone of 60 ppbV). This study has the potential of making a valuable contribution to the field of intercontinental transport. I recommend this paper for publication, but would like to see the authors address the points raised below.

General: Given the coarse model resolution and the bad representation of the urban environment, I wonder if it would not be more suited for the study to focus on the regional influence of transported ozone instead of limiting the focus on the Tokyo region.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



Section 2: The model evaluation is confusing and lengthy to follow; a main reason is that the discussion jumps back and forth between sites and chemical environments. I suggest grouping the discussion and analysis dependent on the chemical environment similar to what is done in Table 2. It also would be nice to see timeseries for all sites and discuss if similar chemical environments show similar model performance. Were model data interpolated to the site locations or grid box results used? Is it only for T21 that sites are located in identical grid boxes or does this also occur for T63? Add standard deviation and correlation coefficients to Tables 2 and 3. The bad model performance for nighttime is making me wonder if the better agreement during daytime is happening for the wrong reason? Maybe a comparison of measured and modeled CO:O<sub>3</sub> correlation could be an additional way of evaluating how well the model represents the chemical regime.

Section 3: The source contribution analysis based on shutting off emission sources is impacted by non-linearities in ozone chemistry. The study by Wu et al. (referenced on Page 10412, line 4) states that this effect could exceed a factor of 2. Can the authors comment to what degree non-linearities might affect the results. Especially when shutting off all Japanese sources, this is a drastic change in the chemical regime. If all source contributions are summed up, how does this compare to the total ozone? Figure 6: the green line, is this the difference between total ozone and ozone from Japanese sources? Page 10414, line 15ff: Are for these results also only afternoon values analyzed? Please state the mean contribution during ozone episodes for Japanese local sources; the contribution for all other source regions is stated. Page 10415, lines 15-20: These two sentences read contradictory to me. In the first it is stated that during episodes contributions from Japanese sources are much larger than average and from inflow marginally smaller. And from this it is concluded that transport impacts attainment.

Section 4.1: Page 10416 and Figure 10: The EU-NA CO relationship is larger in April compared to February and the authors explain this by differences in synoptic condi-

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

tions. The O<sub>3</sub> relationship, however is high for both months. Can the authors explain why this is? Figure 11: if the correlation maps for Chinese CO and O<sub>3</sub> are similar and for other source terms maps for CO are shown, why not also show the maps for Chinese CO to be consistent?

---

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 10403, 2011.

Interactive  
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper