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Atmospheric Chemistry and Physics Discussions

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

M. Yoshitomi et al.

o.wild@lancaster.ac.uk

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We are very grateful to the reviewer for their encouraging comments and for their careful reading of the paper. We have addressed each of the points raised, and have altered the text, tables and captions wherever appropriate to address the concerns raised. We believe that this has improved the paper, and are grateful to the reviewer for their time and effort in pointing out where improvements could be made.

Response to Comments:

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

We focus on a single model grid-square over Tokyo, but given the coarse model resolution this is representative of the wider Tokyo region, not simply the city itself. While urban conditions are not well represented at nighttime, we believe that the wider region is represented reasonably well at this model resolution (see comparison with other sites in Japan in Fig.4). However, we acknowledge the reviewer's concern here, and have replaced 'Tokyo' with 'the Tokyo region' in the title and elsewhere in the paper to avoid any implication that we focus solely on urban conditions.

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.

We have attempted to arrange the model evaluation in the simplest and clearest way by focusing first on remote conditions (Minamitorishima as an example) and then on suburban conditions (Tsukuba). Other sites fall between these extremes, and are described with reference to them. We then consider the impact of resolution, and end by focusing on Oki and Happo which lie upwind of Tokyo and where lagged correlations reveal transport from Asia. This last paragraph is probably the one that appears out of place to the reviewer, and we have therefore rewritten the first sentence to introduce the topic of the paragraph more clearly and to smooth the flow of the section. The following section then describes source contributions; the title of the section has been simplified to 'Source contributions over the Tokyo region' to emphasize the move beyond model evaluation.

It also would be nice to see timeseries for all sites and discuss if similar chemical environments show similar model performance.

We have compared timeseries at all sites, but have chosen to show selected sites (in

Fig 4, and also Figs 2–3) to avoid overwhelming the reader with extraneous detail. The comparisons for all sites are summarized in Tables 2 and 3. There is some variation in performance over the sites (this is evident in Fig 3), but the difference between sites with different chemical environments is generally larger than that between sites with similar environments, and this is clear from the biases and RMSE shown in Table 2. This is already noted in the final sentence of page 10409.

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?

Model data were sampled at the site locations, not interpolated; this detail has now been added in section 2. At T63 all sites were in different grid boxes; the text has been amended to state this explicitly.

Add standard deviation and correlation coefficients to Tables 2 and 3.

The full performance of the model is only clearly shown in the timeseries, probability distributions and distributions of the mean bias as shown for Minamitorishima and Tsukuba in Figs 2 and 3. Tables 2 and 3 are already busy, but we have added the standard deviation in each case.

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:O3 correlation could be an additional way of evaluating how well the model represents the chemical regime.

The inability to reproduce nighttime ozone in strong emission regions is a well-known feature of coarse resolution models, reflecting both chemical titration and deposition processes in the shallow urban nocturnal boundary layer. The improvement in performance at higher resolution is clearly evident in Fig 3, which also demonstrates that poor performance at night has relatively little impact on early afternoon ozone (see right-hand panel). Examining the CO:O3 correlation is valuable at larger distances

from the source region, but is less helpful very close to sources; the CO is defined almost entirely by emissions here while the O3 is governed by chemistry and deposition. Unfortunately we do not have CO measurements from Tsukuba during this period to explore this further.

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?

This is an important point, as it is clear that nonlinearities in ozone production will influence the results here, and hence that removal of Japanese sources provides only an estimate of Japanese contributions to ozone as it neglects the influence of Japanese precursor emissions on the chemical environment for ozone from other sources. It is thus likely that we underestimate the full contribution of Japanese sources. We do not attempt a full attribution of ozone here, and so cannot quantify this impact, but previous studies indicate errors of the order of 10%. We have already acknowledged the influence of this nonlinearity in the text, and reference studies that have attempted to quantify the effects, e.g., Wu et al., 2009.

Figure 6: the green line, is this the difference between total ozone and ozone from Japanese sources?

The green line is the difference; the caption of the figure has been altered to make this clearer.

Page 10414, line 15ff: Are for these results also only afternoon values analyzed?

The results presented here (and shown in Fig 9) reflect all data, not just afternoon values, and this explains the very large role that local Japanese sources play in controlling the variability seen here. We have also considered the afternoon values only to remove the diurnal cycle and demonstrate that Japanese sources contribute about 42% of the

day-to-day variability We have amended the text here to make this clearer, and have also added relevant information to the caption of Fig 9.

Please state the mean contribution during ozone episodes for Japanese local sources; the contribution for all other source regions is stated.

This data is presented in Table 6 for episodes greater than 80 ppb, but we have added the Japanese contribution for ozone values greater than 60 ppb (12.6 ppb) in the text on page 10414.

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.

The statements here were confusing and somewhat poorly phrased. Japanese sources clearly contribute more during episodes, but the contribution of distant sources can still be substantial under these conditions, as shown in Table 6. These two sentences have been rephrased to clarify them.

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 conditions. The O3 relationship, however is high for both months. Can the authors explain why this is?

The difference between the CO and O3 relationships for these sources principally reflects differences in lifetime. With a longer lifetime, surface CO is more influenced by boundary-layer transport; in contrast O3 in the boundary layer is much shorter-lived, and surface contributions are more greatly influenced by mid-tropospheric transport and subsequent subsidence. This greater dependence on subsidence over East Asia drives the higher correlation, even when patterns of horizontal advection lead to relatively low correlation for CO. The NA-Strat correlation, also driven by subsidence, shows similar behaviour. The explanation on page 10416 has been simpli-

fied to highlight the dependence of the correlation on subsidence processes here. Note that the timeseries of the vertical profiles of CO and O3 from Europe and North America are explored in more detail in Figure 11 of Wild et al. [2004, JGR, doi:10.1029/2003JD004501].

Figure 11: if the correlation maps for Chinese CO and O3 are similar and for other source terms maps for CO are shown, why not also show the maps for Chinese CO to be consistent?

We have chosen to show O3 to demonstrate that the relationships hold for chemically active species such as O3, not just for CO as previous studies have shown. While it would be nice to show both CO and O3 from all sources, we feel that the figure is already complex enough. We have altered the discussion on page 10417 to emphasize that we show examples of both.

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