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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of meteorological variability on interannual variations of the springtime boundary layer ozone over Japan during 1981–2005" *by* J. Kurokawa et al.

Anonymous Referee #2

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Referee report:

Influence of meteorological variability on interannual variations of thee springtime boundary layer ozone over Japan during 1981-2005

by Kurokawa et al.

===== General comments:

This manuscript describes the roles of meteorological variability in springtime ozone concentration over Japan, particularly focusing on relationship with the past ENSO

events. This study appears to first suggest negative correlation between Japanese springtime ozone level and ENSO index. The authors interpret and discuss their model results very carefully. They mention well transport aspects of ENSO induced changes and their impacts on ozone in the context of surface pressure system, but hardly mention chemical aspects (O3 production in the source regions and loss during transport). I recommend for the authors to describe more about such chemical processes as much as possible. Plus, I'm a little concerned about discrepancy in Japanese O3 abundances between their simulation and surface observation. 20ppbv differences that they found between model and observation are severe for keeping validity of their discussions. If possible, the authors should compare their modeled O3 field with satellite measurements as well as surface measurements in remote sites around Japan.

Apart from the above described points, this study can be regarded as a significant addition to the current knowledge on the controlling factors of Japanese air quality. Given rapid growth of pollutants emissions in recent China, clarifying the mechanism of interannual changes in transport from the Chinese continent to Japan and the western Pacific region is of crucial importance.

The overall text is competently written, and reference to related previous studies is appropriate and adequate, several sentences seem to be tediously written, though.

The subject of this paper appears to be appropriate to the ACP. Changing process of tropospheric ozone is complex, and there are only a limited number of studies on meteorological roles in ozone change. Revealing mechanisms of ENSO impacts on tropospheric chemistry and subsequent ozone changes can help understanding impacts of future climate change on tropospheric chemistry in terms of chemistry climate interaction as well. However, I would like the authors to consider my questions and revise the manuscript before I recommend the publication of this paper. Details of my comments will be found in the following.

===== Major comments:

Introduction

*** B7558-L24: Why do you focus on the "springtime BL O3" ? Here, the authors should show clearly their motivation for concentrating on springtime O3, neither summertime nor annual O3.

And, why do you use BL concentration instead of surface one in this study ? Discussion using the modeled surface ozone would be more straightforward, I feel. The authors could compare their results with observations more directly as well.

2 Model description

*** B7560-L1-L2: The authors state that their chemistry and aerosol schemes are not applicable to the stratosphere. Does it mean that your model does not consider chemistry in the stratosphere ?

*** B7560-L15-: How about reproducibility of your RAMS-CMAQ simulation for meteorological fields like temperature, wind, precipitation, water vapor ?

*** B7561-L1: I didn't get the point. what do you mean by "inflow concentrations" ? I presume it is a parameter separate from boundary condition in CMAQ. Could you explain more about this in the text ?

*** B7562-L1:

3. Results and discussions

This section discusses the modeled BL ozone. Is it possible for you to mention the difference between BL and surface (model 1st layer) ozone in you model ? and, how does it affect your discussions in this section?

*** B7563-L5: "photochemical oxidants(Ox)": Presumably, this Ox includes oxidant species like PAN as well as O3 itself. If this is correct, the authors should use modeled Ox field instead of O3. In highly polluted area, O3 accounts for only 70 or 80% of Ox in some cases.

*** B7564-L21: "...are generally about 20 ppbv larger than observed..." The authors should take this discrepancy more seriously in fact. This can break the validity of the discussions on O3 anomaly in this manuscript. Underestimation of the NOx titration effect in urban area can be a candidate for the reason of this discrepancy as the authors suggest. This can be easily confirmed by comparing your model results only with the data selected for non-urban observation sites in the WCJ region.

The authors use BL O3 for their discussions. I presume that BL O3 is systematically larger than surface O3 in the model because of higher contributions from the stratospheric O3 transport at 1km altitude.

At least, I think, the authors should evaluate their model results further using any other observational data.

*** B7565

section 3.3

This section discusses the ozone anomaly in the context of pressure anomaly fields. Their focus seems to be only on transport processes like outflow from China. However, the authors should have described also anomalies in chemical processes involving O3 production intensity in the source regions (mainly China) and O3 loss (lifetime) in the downwind regions (around Japan and western Pacific). Please describe more about changes in chemical fields to clarify the roles of chemistry in their simulated ozone anomalies.

*** B7569-L23: "...by the hypothesis described in Sect. 3.3 (1) and (2)" I don't recognize well what this represents. Please be more specific here.

*** B7570-L8: "ENSO and their influences on tropospheric O3 (...:Koumoutsaris et al.,200 8)" Koumoutsaris et al. 2008 showed significant increases in O3 exported from Asia to the western Pacific including Japan in an ENSO phase (1998). This appears to be in the opposite direction to your results. Please describe this point and show your

interpretation.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7555, 2009.