

Interactive comment on “Trans-Pacific transport and evolution of aerosols and trace gases from Asia during the INTEX-B field campaign” by B. Adhikary et al.

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Notes: The reviewers' comments are included and the author responses are designated by »

Response to Reviewer #2

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General comments

This paper presents a detailed evaluation of the STEM model simulations with airborne and ground-based measurements obtained from the INTEX-B campaign. I feel this paper is strong in description but much need to be improved in analysis. Many discussions in the present paper are subjective and incomplete, and the authors often randomly move from one point to another point without building a clear connection. I would recommend publishing this paper in ACP only if the authors thoroughly revise the manuscript and present the scientific results in a more clear, concise, and well structured way.

» We thank the reviewers for their careful review of our paper. Both reviewers thought that the paper would be improved by a stronger focus on the scientific findings. We have extensively modified the paper with a major reorganization, eliminating some previous sections and adding some more focused discussions. This is reflected in the new title of the paper: “A Regional Scale Modeling Analysis of Aerosol and Trace Gas Distributions Over the Eastern Pacific during the INTEX-B Field Campaign”. The paper is focused on our investigation of the distributions of trace gases and aerosols over the Pacific and to estimate how anthropogenic, biomass burning and wind blown dust emissions from various geographical regions impact these distributions during the INTEX B experiment period. We also use the model to investigate the relative contributions of distant sources of pollutants relative to more local sources on the observations at the Mt. Bachelor Observatory, which obtained measurements in conjunction with the airborne observations during INTEX B. This is a topic of growing interest due to the changing emission patterns along the Pacific Rim. » We feel that the paper presents a comprehensive comparison of the model predictions with the extensive aircraft observations. We evaluated over 30 meteorological, trace gas and aerosol components as a function of altitude and present various statistics. The discussion has been sharpened. We feel that the paper has been strengthened based on the reviewers' comments. We feel that we have addressed all of the reviewers' comments in the revision. Below we

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address specific comments.

Specific comments:

1. I am not fully convinced that the overprediction of PAN is simply due to uncertainties in emission estimates and boundary conditions. I believe this is something more closely related with the SAPRC99 chemical mechanism used for gas-phase reactions in STEM. More PAN analogues are considered in SAPRC99 than other carbon-bond schemes. Did you compare the sum of all PAN analogues in SAPRC99 with the observations? Is this consistent with the observed PAN species during INTEX-B? I suggest the authors to check previous studies evaluating PAN predictions in other regional models (e.g., CMAQ) using the SAPRC99 scheme with aircraft measurements.

» Good points. We have expanded the discussion of PAN and looked into issues with the chemical mechanisms. The SAPRC99 does produce more PAN than say CMAQ in experiments over Asia. Also some recent studies are suggesting that the mechanisms lack a loss channel for PANs. The very long range transport (10 days) in this model domain show an amplification of the more active chemistry used in SAPRC99. This information and references have been added the paper.

2. Discussion on Figure2a and Figure5a: The authors need to bring more convincing evidence supporting the statements that CO and O3 at higher latitudes are due to European inflow.

» We have added new figures and discussion showing how emissions from various source regions impact the distributions.

3. Discussion on Fig.12c (Page16401,line20-25):The authors stated that "The model is able to capture the magnitude and variation of measured Co at Mt. Bachelor". However, there are a few episodes that the model predicts elevated CO while the observations show decreasing CO. In addition, can you elaborate what happened during April 16- 21?

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» We have redrawn the figure to show more clearly the CO comparison, its vertical structure and the source attribution. Much of the variability is captured, but there are some offsets in timing and magnitude, which is due in part to the resolution of the model and displacements in the flow fields. The most obvious miss is the predicted peak on 12 May, which is attributed in the model to CONUS emissions, but is not seen as out of the ordinary in the observations. The model attributes this to a biomass plume from North America, which is not obvious from the observations. At the start of the time series both the model and the observations show very small variability, but the model shows a systematic offset. We do not have a clear explanation for this, but as it was a period of prolonged high concentrations in the observations, it could be do to a biomass emission issue that is not captured in our model.

4. Throughout the manuscript, when discussing emission uncertainties, please be clear which emission inventory you are referring to. For example, page 16397, line1, SO2 emissions may be too low in Asian or NEI emissions? Since most C130 flights were operated in the coastal regions of western US, I think uncertainties and possible errors in the NEI data may play an important role. The authors stated in the manuscript a couple of times that they were using NEI 2001 instead of NEI 1999. Can you provide quantitative information on how much difference between these two inventories?

» We discuss this a bit more in the paper. The NEI emissions for SO2 decreased by about 12% from NEI 1999 to 2001. There continue to be underpredictions of SO2/sulfate even with more current inventories.

5. Page16396, discussion on Fig.9: The authors stated that" The model fail to capture the elevated sulfate levels above 2km that were observed by the C-130". Does the model fail to capture the plume transported or the production of sulfate in the plume? The elevated sulfate is due to transpacific transport or originates from surface sources in the western US?

» We have added discussion and analysis. The observed sulfate in the upper tropo-

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sphere is a real feature that has been also observed more recently in the 2008 ARCTAS experiment. The model predicts an enhancement at these altitudes and locations, but significantly underestimates it. The area of enhanced sulfate has significant contributions from sources in China, Russia, South Asia and North Asia. These are areas with highly uncertain emissions. So this remains an open question.

6. Page16388, line1: The ozone column data from OMI are climatological mean or dynamic data for the INTEX-B period?

» We downloaded dynamic data from the OMI website for ozone everyday during the INTEX-B period

7. Page16393, line8- 10 (Figure5a): Ozone hotspots appear in central Japan as well.

» Noted

Technical corrections 1. Much work needed to improve the quality of most figures included in this manuscript. All sub- figures should be labeled by a,b,c...,not just in the captions. Fonts are too small for many figures, which make the readers very difficult to see them.

» Done

2. The manuscript needs to be checked for English grammatical errors. Some of them are listed below. page16386, line5 , 3 dimensional→ three-dimensional? page16386, line8 ,in the recent past→in the recent decade? page16386,line18-19, delete "(Tang et al.,2003)" at the end of the sentence? page16387, line2,ageing→aging? page16387, line6, we used→used?

» Done

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 16381, 2009.

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