Anonymous Referee #1

We thank the reviewer for his/her thorough review and highly appreciate the comments and suggestions, which significantly contributed to improving the quality of the publication. Please find below a detailed response to the each of the comments.

Phister et al. (2009) analyzes the transport of pollution across the Pacific to the US between2000 and 2006 and provides a quantitative estimate of the interannual variability of transpacific transport, whether it is due to changes in emissions and meteorology.

This paper is a nice addition to the current collection of papers on transpacific transport.

I recommend this paper to be published in ACP after the following minor comments are addressed.

1. Proper reference of past literature. This paper focuses on understanding the drivers of the interannual variability of transport of pollution across the Pacific. There have been dozens of paper focusing on understanding transpacific transport in the past decade. Of particular relevance to this study, a number of papers focused on understanding the interannual variability of transport of pollution across the Pacific, e.g. Liu et al. (2005), Liang et al. (2005), Reidmiller et al. (2009). These papers should be properly accredited. In addition, how is the interannual variability discussed in this work compared with that from Reidmiller et al. (2009)?

Reidmiller et al.: Interannual variability of long-range transport as seen at the Mt. Bachelor observatory, Atmos. Chem. Phys., 9, 557-572, 2009.

Liang, Q., L. Jaeglé, and J. M. Wallace: Meteorological indices for Asian outflow and transpacific transport on daily to interannual timescales, J. Geophys. Res., 110, D18308, doi:10.1029/2005JD005788, 2005.

Liu, J., D. L. Mauzerall, and L. W. Horowitz, Analysis of seasonal and interannual variability in transpacific transport, J. Geophys. Res., 110, D04302, doi:10.1029/2004JD005207, 2005.

Liu et al. (2005) and Liang et al. (2005) are now referenced in the Introduction

The interannual variability from MOPITT and MOZART illustrated in Figure 3 (revised Figure 5) and Table 2 show that total CO and Asian CO was higher in 2005 compared to 2006. This is in agreement with the findings by Riedmiller at al. (2009) and a brief discussion has been added in Section 3.1. Adding a detailed comparison to the Mt. Bachelor data and the results discussed by Riedmiller et al. goes beyond the focus of our study, which is on a larger spatial and temporal scale. Mt. Bachelor data have also been available since 2004 only. Following suggestions from reviewer 2, however, additional comparison of MOZART results to long-term measurements at four NOAA surface sites has been added in support of the evaluation of model simulations (Sections 2.4 and 3.1; Figures 3 and 5).

However, we present results here to address the reviewer's comment in more depth. The figure shown below illustrates time series of CO observations at Mt. Bachelor for 2004-2006 as well as accompanying model simulations from MozVar and MozConst. The observed monthly means and standard deviations are derived from hourly observations and compared to monthly mean model data that have been mapped to the location and altitude of Mt. Bachelor (43.98N, 121.69W, 2.7 km asl). For a better representation of the large-scale patterns and to consider the coarse model resolution, we also compare to model results that are averaged over a 10 deg

x 10 deg region surrounding the measurement site. It is interesting to note, that while the model generally underestimates CO levels at Mt. Bachelor, it predicts well the magnitude at Cheeka Peak (as shown in the reply to Reviewer 2).

Both observations and model values show a decline in CO from 2005 to 2006, even though the modeled decline is clearly less than the observed value. The difference in observed CO between April 2006 and 2005 is up to about -25%, while model values interpolated to Mt. Bachelor differ by -1.3%, and model values for the larger region by -3.3%. As was also concluded by Reidmiller et al., a large part of this decline can be attributed to reduced Asian contributions (-6% in Asian CO, -4% in SE Asian CO). The percentage change in our simulations compare well with the GEOS-Chem results discussed in Reidmiller et al., who estimate a decline of -6% in total CO and -7% in Asian CO.

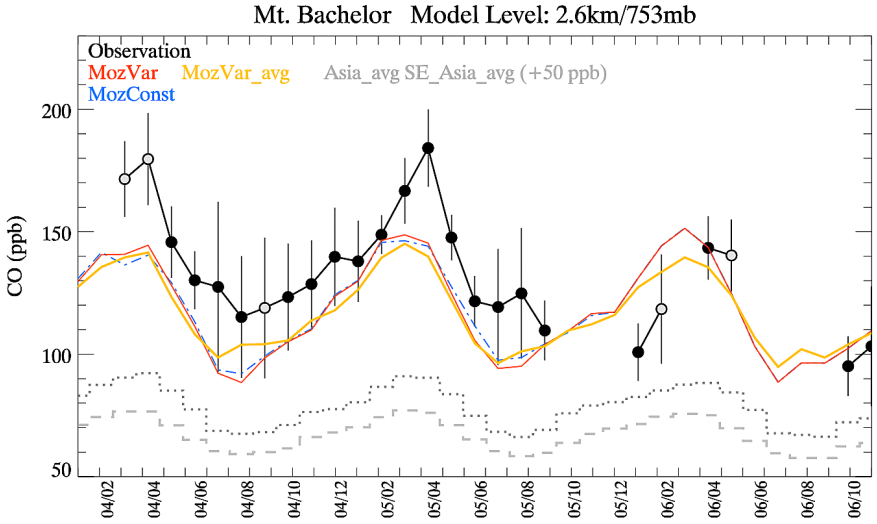

Figure: Time series for 2004-2006 for CO at Mt. Bachelor. Monthly mean and standard deviation of hourly observations is shown in black with those monthly averages where the temporal coverage is less than 75% highlighted as gray circles. Modeled CO mapped to the location and altitude of the site is shown in red (MozVar) and blue (MozConst). MozVar results averaged over a 10deg x 10deg region surrounding the site are plotted in yellow. In addition to total CO we also show tagged Asian and SE-Asian CO for the larger region (gray lines; an offset of 50 ppb is added for displaying results on the same scale).

We like to acknowledge Dan Jaffe for providing the Mt. Bachelor observations.

I found section 3.3 difficult to follow. I am not sure it is meaningful to derive the trend by adding the trend from MozConst to an assumed trend. The two components might not be linearly stackable as change in emissions will also result in changes in relative composition of pollution outflow from Asia and the subsequent transport pathway, etc. In addition to the impact of "natural" variability, trend analysis is subject to great uncertainties caused by other factors, such as the timing and length of the temporal period. These are not discussed in the manuscript. Since this section is peripheral to the overall analysis, I would suggest it

to be removed.

The reviewer does have a point in that Section 3.3 represents a simplified analysis and is not fully integrated with the overall scope of the paper. An in depth trend analysis would go beyond the bounds of this paper, and for this reason we agree to omit Section 3.3. Since the potential impact of a "natural variability" on trend detection, however, is a highly important statement to make, we emphasize this point more strongly in the Conclusions.

3. Page 17819, Line 24-25 & Page 17834, Line 21, 27: Based on the context of this study, it is probably better to use “meteorology” instead of “transport pathways”.

This has been changed.

4. Page 17823, Line 10-12. “. . . such as. . .“ should be deleted. Or if there are any other inventory that the authors want to reference, a brief comparison should be included.

"such as" has been taken out and instead the sentence now reads "These emissions are within the range of Zhang et al. [2009] ....."

5. Page 17828, Line 26-27: Should be “other than for Asia”?

Thanks, this has been corrected.