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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Interannual variability of long-range transport as seen at the Mt. Bachelor Observatory" *by* D. R. Reidmiller et al.

D. R. Reidmiller et al.

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General Comments This manuscript compares chemical measurements at Mt. Bachelor, WA during Spring 2005 with those during Spring 2006 and provides several reasons as to why values during 2005 exceed those from 2006. The authors consider differences in Asian emissions and transport pathways from Asia to the Mt. Bachelor area. Observed data, output from chemical transport and trajectory models, and satellite measurements are employed. The manuscript generally is well written, and the authors provide appropriate caveats when presenting their results. Except for a few notable exceptions, I believe that their findings are supported by the analyses, and that the manuscript will make an important contribution to our understanding of trans-Pacific transport processes.

REVIEWER 1. Page 16339, line 25: What is meant by high latitudes? My first im-

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pression is that the reference means north of about 60 deg. Did the authors make conclusions about the middle latitudes which are more appropriate for Mt. Bachelor? As to El Niño, I agree that it influences wildfire initiation; however, transport patterns also are affected.

RESPONSE 1. Yes, "high latitudes" refers to those north of 60 deg; we have changed the text in paragraph 7 of Sect. 1 to reflect this. We have also added text to the end of the same paragraph to address the reviewer's other two comments (comparison to mid-latitude sites and transport effects of El Niño).

REVIEWER 2. Page 16344, line 9: Are the TES evaluations referring to the entire vertical column or a specific layer. This was clearer in your earlier discussion of MOPITT.

RESPONSE 2. We have clarified this in the text (towards the end of paragraph 1 of Sect. 2.3). TES profiles are most sensitive to CO between ˜700 - 200 hPa. The aircraft profiles used to validate the TES retrievals were flown from the surface to ˜300 hPa.

REVIEWER 3. You used HYSPLIT with FNL input data which consists of 13 levels between 1000 and 20 mb. How many of these 13 levels are below your 2900 m altitude of study? Creating backward trajectories at 100 m intervals probably is overkill since the native data are at a much coarser resolution. My point is that FNL provides poor vertical resolution, and this impacts your ability to draw conclusions about this aspect, e.g., the ALRT discussion. The vertical resolution issue needs to be stated in the text. Note that it is common for high resolution models to have approximately 50 vertical levels.

RESPONSE 3. Three to four levels in the FNL data are below the ˜730 hPa pressure-altitude of MBO. We have added text to Sect. 2.4 clarifying that use of the GDAS data would have been ideal given its higher vertical and temporal resolution. However, large gaps exist in the GDAS dataset for the period of interest in this study (i.e., early April and all of May 2005). The method we employ, or techniques very similar to it, have been used widely with a good deal of success, so we feel confident in using this

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method as part of our suite of diagnostic tools (Stohl et al., 2002; Weiss-Penzias et al. 2004, 2006, 2007; Hafner et al. 2007, Wolfe et al. 2008).

REVIEWER 4. Page 16347 is very confusing to me and needs to be revised. What are you trying to eliminate and retain through the water vapor or time of day segregations? Please provide a physical basis for the approach. My understanding is that you end up using only the water vapor approach.

RESPONSE 4. No data have been eliminated, only segregated so as to understand the types of air masses we observe. At a mountaintop site, such as MBO, we are subject to strong diurnal changes in upslope/downslope flow. Segregating the data by water vapor has been shown to be a good indicator of boundary layer (large WV values) vs. free tropospheric (lower WV values) air. We have eliminated paragraph 1 of Sect. 3 as it is somewhat extraneous to the interannual variability of CO analysis. We have retained paragraph 2 of Sect. 3 (summarizing the results from MBO during INTEX-B) as this publication is part of the INTEX-B special issue and the lone work summarizing the results from the station during the campaign.

REVIEWER 5. Page 16350, lines 1-10: You state that MOPITT and TES sample different levels of the troposphere. As a result, you use an over-ocean box. How does using a box solve a vertical resolution problem? Please elaborate on this.

RESPONSE 5. We have clarified the text in paragraph 1 of Sect. 4.3 to relay our motivation for using an over-ocean box: to minimize issues of topography in the satellite retrievals while retaining some geographic representation.

REVIEWER 6. Page 16350, line 20 and many other locations: Since you describe vertical changes as well as changes in magnitude, it would be less confusing to reserve the words "higher" and "lower" only for altitude comparisons.

RESPONSE 6. We agree and have revised the entire manuscript to ensure that "lower" and "higher" are reserved for discussion of altitude, while other descriptors are used to

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indicate magnitude differences (i.e., "smaller" / "greater", etc.).

REVIEWER 7. Page 16351, line 4: Are these 3 day running averages?

RESPONSE 7. No; they are 3-day means with the datapoint in the plots representing the end-date taken from midnight local time to midnight local time three days later. We have clarified this point in the first line of Sect. 4.4.

REVIEWER 8. Page 16352, line 6 and Fig. 4c: I believe that "particularly strong" for May 2006 is an overstatement. It is stronger and of longer duration, but not by that much.

RESPONSE 8. We agree and have re-phrased this sentence to be less bold: "we can see that in early- to mid-May 2006 RGM enhancements were greater and of longer duration than for the same time in 2005."

REVIEWER 9. Same page, lines 10-12: In what ways were conditions particularly conducive to dust transport from Asia during May 2006?

RESPONSE 9. At the reviewers suggestion, we have added Fig. 5 which shows monthly mean omega fields from NCEP reanalysis during May of both years and more text (to the end of paragraph 2 in Sect. 4.4) to elaborate on this. The observations in May 2006 (when compared to May 2005) suggest more FT air (high RGM and O3) was reaching the summit of MBO. This, coupled with the air having much lower water vapor content indicates that drier air was being transported to the summit in May 2006, thus facilitating aerosol transport. We have added text to the second paragraph of Sect. 4.4 explaining how these plots of vertical velocity show that the more northerly location of the Pacific High coupled with the much weaker upward motions along the west coast of the U.S. in 2006 indicate that conditions during May 2006 were more conducive to subsidence. In addition, according to Fischer et al. (2008), late spring 2006 had a large amount of dust mobilization over the Gobi and Taklimakan desert regions and this resulted in higher PM concentrations over the western U.S. The average AOT over

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this region was much higher than in 2005 and the [Chinese Meteorological Association] CMA reported the largest number of dust storms during this spring. The CMA record extends from 2000 to 2006.

REVIEWER 10(a). Page 16352, line 19: Please explain the "downward sloping L-shapes". I don't see anything with that shape. It would be better to describe it differently. Also, you should explain to the reader how you infer subsidence from these figures.

RESPONSE 10(a).We have removed mention of the L shapes. We have added text to paragraph 1 of Sect. 5.1 to explain the figures more clearly.

REVIEWER 10(b). And, where over the Pacific does this subsidence tend to occur? Keep in mind that your FNL data do not provide detailed vertical resolution. Therefore, I am pretty skeptical about this whole approach. You need to convince me that it is valid.

RESPONSE 10(b). We have significantly revised this discussion. First, we agree that subsidence is not the sole transport mechanism responsible for bringing Asian emissions to the U.S. free troposphere and is not the only explanation for the patterns seen in Figure 6+7. Emissions in the Asian BL can be lofted into the free troposphere. These pollutants can then be horizontally advected across the Pacific and reach the free troposphere along the west coast of North America. Pollutants at higher elevation will be transported more rapidly due to faster winds aloft. Subsidence may bring the pollutants to lower elevations over North America, but this depends on the specific meteorological situation. However this does not change the result that the ALRT transport index is well correlated with tracers emitted in the Asian boundary layer. This is similar to previously published work (see Sect. 2.4 for references).

REVIEWER 11. Page 16353, lines 9-11: How and why are time differences related to strongly subsiding air? I do not understand the rationale for this statement. In general, models do a fair job of depicting subsidence because it occurs on a large scale and

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has relatively small magnitudes when compared to strongly ascending air which often occurs in deep convection and which the models tend to mishandle in time and space.

RESPONSE 11. We agree; the relevant text in Sec. 5.1 has been modified.

REVIEWER 12. Subsidence discussions: Your methods of inferring subsidence generally are indirect. Why don't you go to the http://www.cdc.noaa.gov site and prepare spatial fields of omega from the re-analysis data? That would provide a much more convincing argument that there was weaker subsidence during 2006.

RESPONSE 12. We have done as the reviewer suggests and made vertical velocity fields from the NCEP reanalysis (Fig. 5). In Fig. 5b,d we show omega fields for May of 2005 and 2006 to make the point that on average, vertical velocity was more positive in May 2006. (text has been added to the end of paragraph 2 in Sect. 4.4). In contrast, Fig. 5a,c show the omega fields for 1 March - 30 April were more positive in 2005. Thus, it follows that the mean synoptic condition would be more conducive to subsidence of lower FT air in March and April of 2005 than for the same time in 2006. Text has been added as paragraph 2 of Sect. 5.2.

REVIEWER 13. Page 16358, discussion of Fig. 11: I used the CDC web site to examine the flow patterns that you describe in the figure. In general, the flow was stronger during 2005, and the path was shorter during 2005. You can see if you agree with me on this. However, I would describe the flow during both years as originating more on the central coast of Asia instead of Southeast Asia. You need to provide some additional information to convince me that Southeast Asia is the origin of the Mt. Bachelor enhancement.

RESPONSE 13. We do agree with the reviewer on his/her interpretation of the generalized flow patterns over the Pacific in 2005. However, we have added Fig. 12 which shows FT winds from NCEP reanalysis for the period from 29 March - 5 April. Superimposed on this figure is a red box outlining the "India and Thailand" region Yurganov et al. (2008) used in their analysis of MOPITT- and AIRS-derived CO burden anoma-

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lies to show significant anomalies in early spring 2005 due to wildfires in the region. The vector winds show a strong shift from pure easterlies to strong southwesterlies between 150 - 170 deg E which allowed for rapid transport of these emission to the west coast of the U.S. Figure 12b shows that this feature is a climatological anomaly, so the anomalous wildfire emissions from SE Asia, coupled with the anomalous transport pattern, led to the observed CO enhancement. The earlier time of this NCEP wind field vs. the NAAPS aerosol transport model implies that the wildfire emissions from this region began at least in late March and lasted through mid-April. This is reflected in the CO timeseries at MBO (Fig. 4a) which shows CO beginning to increase in late March, peaking in early April and only gradually declining from mid-April through early May. Corresponding text has been added as paragraph 4 of Sect. 7.

REVIEWER 14. Finally, climatological isentropes slope toward higher altitudes as one goes from the tropics to the poles. So, mentioning isentropes does not bolster your argument about subsidence. I believe you should omit this sentence. To summarize, this discussion is the weakest of the entire manuscript. It needs to be placed on a more solid meteorological foundation by presenting more conclusive data from your study period.

RESPONSE 14. We have eliminated the mention of isentropes. We have also employed the reviewer's recommendations of putting the study period in a more solid meteorological context by including NCEP reanalysis of vertical velocity (Figs. 5 with accompanying text in Sect. 4.4 and 5.2) and horizontal winds (Fig. 12 and Sect. 7).

Technical Corrections

REVIEWER The manuscript generally was well written, with good grammar. I could not find any misspelled words. I do suggest that Fig. 11 be made larger. I had to use a magnifying glass to view it.

RESPONSE Despite attempts, we were unable to enlarge all 6 panels of this figure while retaining a good degree of resolution, so we must keep it as is. Hopefully, it will

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appear as large as possible in the final manuscript.

** We thank this reviewer for his/her thorough analysis of our manuscript. Their comments have enhanced the scientific quality of our work and we are grateful.

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