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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.

Anonymous Referee #1

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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.

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Interactive Discussion
Discussion Paper

Scientific Issues

I do not have fundamental problems with their work. My comments serve to make some of the discussions more focused, and I suggest a few additional analyses.

1. Page 16339, line 25—What is meant by high latitudes? My first impression 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 Nino, I agree that it influences wildfire initiation; however, transport patterns also are affected. 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. 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. 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. 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. 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. 7. Page 16351, line 4—Are these 3 day running averages? 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. 9.

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

Technical Corrections

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Interactive Comment

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 16335, 2008.