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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "CO source contribution analysis for California during ARCTAS-CARB" *by* G. G. Pfister et al.

**Anonymous Referee #1**

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This manuscript provides model estimates of tagged CO tracers across the western USA. This type of information is useful for understanding the relative impacts of local and upwind sources of pollution on a particular area. Overall the paper is well written and the model analysis is thorough, but in its current form I find the results a little thin in terms of providing a significant advancement in scientific understanding. As described below I think the authors can easily expand the study to make it more informative.

The authors state that to the best of their knowledge their study is the first to use tagged CO tracers with a regional scale CTM. However, the recent paper by Huang et al. [2010] uses a nested global CTM (12 km resolution over California) to simulate the transport of tagged CO-tracers from various source regions to California during ARCTAS-CARB. They show that during pollution transport episodes CO from China



can dominate surface CO in California, with stronger impact in northern California and less impact in southern California. While the Huang paper focuses on ozone over California, it is very similar to Pfister et al. in terms of model set-up and general conclusions, although Pfister et al. do show more detail regarding CO sources and impact regions.

I have two suggestions for making this study more useful to the scientific community, both of which deal with the impact of Asian CO on the US surface:

1) The study focuses on a single 25-day period, because of the availability of aircraft measurements for comparison. Still, the reader is left wondering how representative the results are for other times of year. The authors could run their model for the full year of 2008 and show figures similar to Figure 8 for all four seasons. In my opinion this would provide a significant step forward from the conclusions of Huang et al. [2010]. We need high resolution simulations of how foreign emissions impact the surface of North America, estimates that up until now have mainly been provided by coarse resolution global CTMs. The high resolution WRF output can provide more details of the impact regions especially the impact of Asian emissions on high elevation vs. low elevations sites. Given that the authors have already provided a lot of model verification in the current version of the paper, I don't think they need to provide any additional verification.

2) The authors could focus more on the processes that bring background, and especially Asian CO to the surface. Parrish et al. [2010] and Huang et al. [2010] discuss downward mixing of background and Asian CO into the northern Sacramento Valley. How do the Pfister et al. results compare? Judging by Figure 8 there seems to be an enhancement of Asian CO in the Sacramento Valley. And I'm surprised that even though the Asian CO tracer should have greater mixing ratios in the free troposphere than in the marine boundary layer, there does not seem to be a greater impact of Asian CO on the mountains of southern California and the rest of the high elevation regions desert southwest. Instead there is more Asian CO in the marine boundary layer west

of southern California. Why? The authors suggest that the Sierra Nevada act as a barrier diverting inflow towards the north and northeast, but I just don't think this is the case. If they were such a barrier to flow (like the Tibetan plateau) this phenomenon would be well known and would be evident in rain fall patterns, storm development and incredible wind speeds around the mountains. Isn't the CO pattern more likely due to the fact that during summer the transport across the desert southwest is southerly, due to the widespread summer monsoon, which keeps the Asian CO further north? Monthly average wind vector plots at 700, 500 and 250 hPa would be helpful. Also, in springtime is there a stronger impact of Asian CO on the high elevation terrain of the desert southwest? Stohl et al. [2002] show that during springtime the strongest Asian CO plume, on average is found in the mid troposphere at 35 N latitude. How does this compare to summer 2008? Does the Asian plume enter North America at higher latitudes in summertime? Furthermore, there is a discrepancy between Holzer and Hall [2007] and Liang et al [2004] regarding the relevance of low altitude transport of Asian emissions to the western US. How do your results compare?

Other comments: Some reference needs to be made to the findings from the ITCT campaign that occurred in the same region in spring 2002.

page 3629 lines 8-10 Are these CO mixing ratios at the surface? Also, here and throughout the paper, seeing as the model CO values are reported in units of ppbv, CO values need to be described as mixing ratios and not concentrations.

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