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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "The impact of Los Angeles basin pollution and stratospheric intrusions on the surrounding San Gabriel Mountains as seen by surface measurements, lidar, and numerical models" *by* Fernando Chouza et al.

**Anonymous Referee #1**

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Chouza et al. present an interesting study comparing ozone lidar measurements from the Table Mountain Facility with model simulations from the NCAR WRF-Chem and WACCM models. The study includes an overview of the measurements from May 2019 through September 2020, and presents three case studies from this period describing a stratospheric intrusion event, a regional pollution transport event, and a third event with both influences. The analysis is thorough and compelling, but I think the authors should also use measurements from the extensive network of ozone monitors in the



Los Angeles area to provide more context for their results. This would better link these mountaintop measurements to the issue of regional ozone attainment they invoke in both the introduction and conclusions.

I have a number of minor comments and suggestions for improved readability.

P2, L46. "...near-surface measurements carried out at..."

P3, L63. "...northwest of Wrightwood..", "The site hosts..."

P3, L64-67. It is never clearly stated that the Chen study was based on DOAS. Perhaps start with "Differential Optical Absorption Spectroscopy (DOAS) measurements by Chen et al..."

P3, L67. "Similar conclusions have been reached..."

P4, Table 1. Is the temporal resolution of the TMTOL really 1 hour or is that just the integration time used for the comparisons?

P4, L86 and Figure 2. Is there an explanation for why the TMTOL values are consistently higher than the UAV values below 75 ppb? Did the UAV carry a standard ECC ozonesonde? Also, what is the relevance of showing the "Days since first validation"? Is this meant to somehow account for the one outlier?

P7, L164. Would the authors care to comment on the clear seasonal shift between 2012-2014 (highest O3 in May and June) and 2017-2020 (highest O3 in July and August) in Figure 3a?

P8, L181 and Figure 4. Are the seasons defined here as Fall (SSO), W(DJF), Sp(MAM), and Summer (JJA) or by equinox/solstice?

P8, L183 and below. "forecast" is the preferred form of the verb-not "forecasted".

P8, L186. Are the shifts in wind direction consistent with the lower resolution of the model topography?

P8, L195. What was the resolution of the meteorology used for the HYSPLIT back trajectories?

P8, L206. “The trajectories corresponding to the 210-240° prevailing winds. . .”

P8, L207. “. . .with respect to the 250-300° back trajectories. . .”

P9, Figures 3b-3d. These plots are confusing. What statistics are represented by the box-and-whisker plots? I assume the shading refers to 2019 and 2020, but this is not explicitly stated. What do the dark gray diamonds represent? By the way, the plotted symbols are time series and not “scatter plots”.

P11, L211. The discussion here skips back from Figures 5 and 6 to Figure 4 without warning. It took me a while to figure out the authors were referring to Figure 4 in the paragraph that followed. Perhaps revise to “During summer 2019, PM10 observations and forecast (third and sixth rows of Figure 4). . .”.

P11, L213. “A difference is observed for ENE. . .”

P11, L229. “. . .focused on the free troposphere. . .”

P11, L214. Were there any significant wildfire influences in the Fall of 2019?

P11, L217. The Met One 212 has a lower size cut at 0.3 $\mu$m and thus may be excluding the smaller particles in the model analysis.

P12, 13. Figures 5 and 6. The gray back trajectories are hard to see in some of the plots. Perhaps use heavier white or magenta lines?

P16, L279+. In my opinion, it would be better to switch sections 4.1 and 4.2 and describe the pollution transport event first since this is the more typical event. That would help to put the TMTOL and ceilometer measurements from Figure 9 in better context.

P16, L284. This sentence is awkward and could perhaps be phrased better.



P17, L295. The WRF-Chem RH isn’t shown.

P18, F9 caption. (a) The CO scale is missing. (b) Please note that the PBL height is from the ceilometer measurements in (c). The ozonesonde profile mentioned in the caption does not appear to be in the plot. (d) The plot is already complicated, but it would be useful to see the WRF-Chem RH since it is mentioned in the text.

P19, L340. Again, it would be useful if the TMF surface measurements were compared with the regulatory measurements, particularly those from particularly Phelan and Crestline.

P21, L357. “. . .the closest time to the ozonesonde. . .”

P23, L281. The high aerosol content of the irruption is not obvious from the ceilometer measurements.

P24, L396. This case can be contrasted with that described in Langford et al 2012* where the descent of a deep SI also caused surface O3 to decrease in the Los Angeles Basin. In that case, however, the surface RH also decreased as drier air from aloft displaced local pollution.

P24, L406. “This result,. . .”

* Langford, A. O., J. Brioude, O. R. Cooper, C. J. Senff, R. J. Alvarez, R. M. Hardesty, B. J. Johnson, and S. J. Oltmans (2012), Stratospheric influence on surface ozone in the Los Angeles area during late spring and early summer of 2010, J. Geophys. Res., 117, D00V06, doi:10.1029/2011JD016766.

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