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Atmospheric Chemistry and Physics Discussions

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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 #2

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The manuscript submitted by F. Chouza and co-authors concerns the analysis of ozone measurements at the Table Mountain (TMO) California station. The objectives are multiple (1) to demonstrate the contribution of new measurements at low altitudes of the TMO lidar for the analysis of regional pollution (2) to analyze the respective role of stratospheric intrusions and regional pollution on ozone threshold exceedances (3) an evaluation of the performance of WRF-CHEM model ozone forecast for the analysis of pollution episodes.

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Discussion paper

The paper contains many interesting results based on a joint analysis of observation and modeling. It certainly deserves a publication in ACP. My main criticism will be on the readability of the objectives and the overall coherence of the paper. The authors are probably trying to meet too many objectives in the same paper. The evaluation of the WRF CHEM forecast seems to be both the most original objective considering the previous publications on the analysis of ozone measurements in the Los Angeles area and a good approach to ensure consistency in the paper. This could be improved by (1) a better presentation of the objectives in the introduction (2) add a discussion of model/measurement differences in section 3.1, otherwise the results presented will be difficult to use (3) Focus section 4 on the evaluation of model performance for the three case studies. The latter could be seen as examples to discuss the differences identified in section 3.2 using the 726 lidar profiles. Indeed the identification of the 3 high surface O3 drivers at TMF based on section 4 is not really new (see previous papers by Cooper, Langford, Lin) and the same results could be very useful if the discussions focus on the model forecast performances to represent theses processes.

Detailed remarks

Line 37 and 51: Relevant objectives are provided here and the introduction generally misses a list of these objectives in addition to the list of tools used.

Line 40-44 : While the representativity of a mountain top station to characterize PBL or free tropospheric air is a very important point, it is not clear how the paper address this question in section 3.1 or in section 4. If some results address this question they should be mentioned in section 3 or 4.

Line 175 : Scatter of WACCM stratospheric tracer on O3 exceedance days are a little hard to read in figure 3.b but it seems that the mean is always higher than the median or even than the upper interquartile of the stratospheric tracer for the overall data set. So is it true to say that the stratospheric intrusion plays a limited role based on this unique feature ? I am not sure I understand the differences between the black and color dots in Fig. 3. On the other hand it is true to say that the O3 exceedance days with RH>50 % are often observed. So interpretation of the WACCM forecast is not straightforward. May be the age of the stratospheric tracer could help.

Line 179 : Make a new section here because there is no obvious link between the analysis of the O3 exceedance days shown in Fig. 3 and the new question discussed in Fig. 4 where the wind and diurnal cycle dependency of surface and model are shown.

Line 190 : I agree it is a nice result of this study. However one could expect some hints about of the model difficulties to reproduce this diurnal cycle. Is a well known feature or specific to the TMO data set ?

Line 191 -201: The authors give a great importance to the distribution of ozone in the prevailing wind sector but why do you expect an ozone maxima for this sector ? Indeed according to the ozone distribution shown in figure 5, the ozone max are found west and south of TMO. So the TMO surface ozone max is obviously found for the western and southern wind sectors. I propose to simplify the discussion line 191-201 and use Figure 5 to explain why the observed ozone maxima are for the western and southern sectors. Then the second level of discussion is the comparison of the ozone/wind dependency for the observations and the forecast, which is quite good.

Figure 5 : add TMO position on this figure, trajectories are also hard to read and it is not clear why they are different for top, middle and bottom panel

Section 3.1 A summary about the main findings about this surface measurement/model comparison must be given at the end of the section: accuracy of model ozone field around TMO, biais in diurnal variation, accuracy of ozone titration in the model.

Section 3.2 It is a very interesting section and there is little published work where many lidar profiles are statistically compared with regional-scale CTM simulations. However the discussion is not very developed and the lidar vertical resolution is probably not the main driving factor. While large differences due to small time or spatial shift of the simulated ozone layers above TMO are expected near the tropopause or at the PBL top, it is unexpected to find 30-50 % differences at 7.5 km. A small discussion about possible reason for this overestimate of the forecast could be added. Was this feature already observed during model evaluation in the free troposphere ? Is it specific to TMO and why ? The differences seems also worse during nighttime (except in summer 2020) while the lidar accuracy is less for daytime observations. Do you have an explanation for this ?

Section 4.1. It is indeed an interesting case study to demonstrate the role of stratospheric intrusion for increasing surface ozone measurements at a mountain top station. However it is not a new result which has been reported in several publications. So a discussion is missing either to compare this new case study with previous ones (Bonasoni 2000, Trickl 2019, Knowland 2017) or to explain why the model did not forecast a surface ozone enhancement (line 285 and 300).

Line 308 Can the authors use their detailed spatio-temporal analysis of the measured and forecasted ozone field in Figures 9 and 10 in order to discuss the differences in the time series at TMO ? Can a model shift along BB' explain the observations ? Can such a shift explain why the model is too low at the surface or is the model mixing too low in the PBL ?

Figure 10c The dashed line above mountain top is not defined. Is it the PBL top like in figure 9 ? Add a vertical line in Figure 9 at the time corresponding to the horizontal/vertical cross-sections shown in Fig.10. Similar comment for fig. 13 and 11.

Section 4.2. This case is indeed interesting for the model assessment because the large scale upper tropospheric ozone feature are weaker than case 4.1 and small shift of the modeled ozone field will not easily explain the lidar/model difference.

Line 339. Does this kind of model overestimate correspond to the statistical positive bias between model and TMTOL near 3 km which is discussed in section 3.2 ?

Line 341 It is worth mentioning that the forecast daily maximum is earlier in the model as previously shown in section 3.1 using the surface observations. It seems to be a permanent feature of the forecast.

Line 378 What is the reason for the model performing less for this case study ?

Line 344 Is mixing too high in the model close to the ground ?

Line 413 Although the model/TMTOL differences are lower than 20% within the PBL, it must be stressed that there is a model positive bias which might be related to the large differences observed in the free troposphere between 4 and 7 km.

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