Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., author comment AC3
https://doi.org/10.5194/acp-2021-690-AC3, 2021

Reply on RC1

Andrew O. Langford et al.

Author comment on "The *Fires, Asian, and Stratospheric Transport*–Las Vegas Ozone Study (*FAST*-LVOS)" by Andrew O. Langford et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-690-AC3, 2021

Abstract: "and illustrate some of the challenges facing air quality managers tasked with O3 attainment in the SWUS during late spring and early summer": A few of these issues could be listed here. What are the main findings of Zhang et al.? *This information will be added to the abstract.*

P. 3, line 66: Reference for "stratosphere"! *I moved the citation of the comprehensive Jaffe et al. review paper cited after "wildfires" in the submitted manuscript to the end of the sentence.*

P. 3, lines 66-67: The mountains (specify altitude ranges) are certainly higher and more susceptible to intrusions (Elbern et al., Atmos. Environ., 31, 3207–3226, 1997; Trickl et al., ACP 2010; ACP 2020). However, there are no cities up there. What are typical altitudes in the states mentioned, what is the chance of intrusions to reach the valleys? *Direct descent of stratospheric intrusions to the lower lying major population centers of the Intermountain West is indeed unlikely, but shallower intrusions can be entrained by the unusually deep boundary layers that form in this arid region. This question was the primary motivation for the FAST-LVOS campaign and is discussed a few lines further down.*

P. 3, Figure 1: Specify length of boxes in capture in km. Figures 4,5, others: Specify "alt" in captions (at least once). *Done*

P. 5, line 118: "Deep STT refers to those intrusions …": This is the case in the Zu¨rich modelling studies, but not in general. Please, rephrase! We have removed the restrictive definition of deep STT.

P. 5, line 122: "Clean or polluted" *Changed*.

P. 6, line 155: A lower-lying site implies the advantage of a coverage of a larger vertical range. *The key point here is that the upgraded lidar has a larger vertical range than the version deployed previously.*

P. 7: Figure caption: Add information to identify the panels. It isn't clear what additional information is being requested here. The panels are already labeled (a)-(d) and referred to by these labels in the caption.

P. 9, line 231: CO concentrations in the lowermost stratosphere, relevant for the

intrusions, are not that low (Trickl et al., ACP 2014); "much" is perhaps too strong! *The "much" has been deleted.*

P. 9, line 236: Please, add reference for low concentrations of both in the marine PBL. *A recent reference (Clark et al., 2015) has been added.*

P. 12, line 335: Add date to "until the end of the campaign", which reduces the effort for the readers to look for the date elsewhere. *The date has been added.*

P. 17, Fig. 6: g/kg is a terrible unit that is unfortunately frequently used in the H2O community. These numbers are difficult to understand since they do not directly reflect the ideal gas law. The volume mixing ratio is better. RH is also more meaningful for judging dryness. It would be helpful to have a second time scale above the top panel. *We agree that RH would be preferable if we only were considering surface data, but we use g/kg to facilitate semi-quantitative comparisons between the surface, aircraft, and ozonesonde measurements. A second scale will be added to the figure.*

P. 17, lines 424-425: Add C and D (etc.) in the brackets specifying the times. Please, add this also elsewhere, wherever it makes sense. *The lines have been revised.*

P. 18, line 431: Is "unique" needed here? If this is the case, briefly specify why (e.g., lowest angle if this is special for ozone lidars). Writing "unique" is dangerous anyway. *Although the vertical scanning capabilities of the TOPAZ lidar are, in fact, unique among ozone lidars, the word has been changed to "vertical".*

P.19, Fig. 7: Please, add time scale above the top panel.

P. 20, line 467: "performance" is misleading or ambiguous. What I see are typical data products. *The word "performance" has been changed to "output".*

P. 21, lines 474-475: Is the resolution of RAQMS sufficient for reproducing intrusions (e.g., Roelofs et al., JGR 2003)? If this is not the case, the subsequent finer model will miss also it. Please, give more information in Sec. 3.4. *The RAQMS forecasts have 1°X1° horizontal resolution and are more than adequate to resolve most intrusions. We have added this information to Sec. 3.4.*

P. 25, line 535: Are there RH data from the balloon ascents? 0-5 % RH is typical of intrusions. The RS92 sonde is rather quantitative at low RH, the RS41 sonde has a slight positive bias of about 3 % RH intrusions. I not so sure about other sensors, possibly used onboard the aircraft. The impact of Asian contributions on the humidity would be an interesting topic. *We show semi-quantitative examples of the sonde and aircraft (specific) humidity profiles in Fig. S3. We hope to examine some of the Asian transport events in more detail in a future publication.*

P. 25, lines 558-563: Is there prefrontal advection? This means rising air (as verified by the rise of the trajectory), polluted if pollution sources are present. Is this what is named synoptic forcing? *The sentence has been revised to clarify the influence of the synoptic winds on the low-level flow. The lifting of the airmass is probably orographically forced.*

P. 27, line 580: How dry? *The RH values (3-8%) were added to the text.*

P. 29, line 628-629: This observation is important. However, I do not understand why the situation before the arrival of the intrusion could influence the

composition of the intrusion itself. What is the role of PBL formation? Do night-time intrusions penetrate deeper (see Trickl et al., ACP 2020)? Did the dry layer move out of the observational area? *These questions miss the main point of the LVOS and FAST-LVOS studies. A major finding of the first LVOS campaign (described at length in Langford et al. 2017) was that the highest surface ozone occurred not when stratospheric intrusions descended all the way to the surface, but rather when stratospheric intrusions (or transported pollution layers) were entrained by the convective boundary layer and added to the photochemically produced ozone already there.*

P. 29, line 635: You need a high-resolution model for this purpose. Still, there is the issue of numerical diffusion. I am not sure if the models can quantify entrainment. Observations indicate a very small vertical exchange across the top of the PBL. I suggest to reformulate this paragraph slightly. *The word "shows" has been changed to "suggests".*

P. 30, line 653: This is really spectacular and normally not that clearly visible! Is this penetration perhaps caused by the fast descent? *This is unclear since the filament is not resolved by any of the models. We also plan to examine this event in more detail in a future publication.*

P. 35, line 708: Please, explain the role of the Mauna Loa data for the situation in Nevada. *These measurements are thought to represent the background concentrations in the free tropospheric air reaching the U.S. West Coast. This sentence has been added to the text.*

P. 38, line 775: "As expected": why? *As we state in line 772, these two sites routinely measure the highest O_3 concentrations in the LVV. Nevertheless, we have removed the "as expected" to prevent confusion.*

Sec. 10: Does one learn anything from a comparison with the 2013 campaign? Does it sense to add a table with some statistics, here or in the preceding chapters? *Several tables included in earlier versions of the manuscript were removed to shorten this very long paper.*