<u>Review</u>:

Zhang et al., Influence of convection on the upper tropospheric O_3 and NO_x budget in southeastern China

Summary

The authors examine two convective cases, 25 July, 2019 and 1 September, 2020 using ozonesonde, TROPOMI NO_2 and lightning data. For both cases, they discuss O_3 distributions resulting from convection, advection, and chemistry. They also examine the effects of varying LNO_x in local-scale models for quantifying O_3 production and for computing the air mass factors (AMFs) needed in NO_2 and LNO_x satellite retrievals. The study is valuable in showing the variability of O_3 in different convective environments, and because the inclusion of more accurate local LNO_x chemistry can potentially improve NO_2 retrievals.

This paper reads well, is well-organized and includes appropriate references. I have only a few general comments as well as technical corrections. If adequately addressed, I would recommend it for full publication.

General comments:

(1) Since LNO_x is a key component of this study, affecting both O_3 chemistry and NO_2 and LNO_x air mass factors, it would be helpful to show several examples of WRF-Chem vertical profiles of these constituents for the two cases under different PE assumptions near and downwind of convection. For example, NO_2, LNO_2 and LNO_x profiles could be discussed in the context of the O_3 profiles of Fig. 4 and the ΔAMFs of Fig. 6. If feasible, it also would be interesting to compare the profiles with the standard profiles from TM5 used in the TROPOMI data product to illustrate the importance of local effects.

(2) I find that giving the contributions of dynamic and chemical effects on O3 as percentages is confusing (abstract, text and conclusions) when the percentages are greater than 100 and the effects have opposite signs. For example, to summarize the life cycles of both cases, it would be clearer to say that the chemistry increases O_3 in both cases and that the magnitude of the effect is 5 – 10 times the magnitude of dynamic effects (rather than using the > 87% figure).

(3) A substantial part of UT NO_2 seen in the 2020 event is likely *not* produced by the flashes counted in the region in the hour(s) immediately prior to overpass. As seen in Fig 5a, b, increased $SCD_{tropNO2}$ is visible in regions where the cloud pressures are higher and cloud

fractions are lower. This is mentioned in lines 210 – 215 on page 12 (the relevance of Fig. S5 to this should also be made clearer). Some estimate of ambient NO_2 is needed so that it can be subtracted as a tropospheric background before the LNO_x is computed. Studies have shown backgrounds can be substantial (e.g. Allen et al. 2019; Bucsela et al. 2019).

A related issue is the relatively small estimate of 10% for the error introduced by the stratosphere (Allen et al., 2019 - not 2021). This error assumed a tropospheric background subtraction that partially cancels stratospheric errors. Without this subtraction, the error would be larger.

(4) Fig. 6 $a_{ii} – c_{ii}$ and $a_{iv} – c_{iv}$ d show a decrease in AMF_{LNOx} when LNO_x is enhanced at higher altitudes, in contrast to the behavior of AMF_{trop}, which is consistent with Fig. 7. Please include some words qualitatively discussing the behavior of AMF_{LNOx}.

(5) In Figure 5f, why wasn't the northern part of the region included in the LNO_x analysis (section 5.2)? There appear to be adequate flashes there, along with LNO_x and a high cloud fraction. The southern/southeastern regions include areas of low cloud fraction that could potentially contaminate the measurements with anthropogenic NO_2. Also, if winds are from the WNW, shouldn't the flash-counting window be displaced WNW of the LNO_x window?

Specific comments and technical corrections:

(1) Page 2, line 49: "We apply new a priori NO_2..."

(2) Page 3, line 57: "...near the airmass convection that developed on 25..."

(3) Page 3, line 64: "...the observed difference of more than 65%."

(4) Page 5, line 84: "...with a constant IC/CG ratio of 3:1 based on Wu et al..."

(5) Page 5, lines 81 – 85: Please add some detail on how the 3 datasets were merged. Was CNDLN used to estimate a DE for ENTLN and WWLLN?

(6) Page 6, line 105: Please add a similar equation for AMF_{trop}, since it is used in section 5.1

(7) Page 7, line 161: "The squall line on 1 September, 2020 was born..."

(8) Page 10, Fig. 4 caption: "The vertical distributions of the O_3 net production rate and tendency..."

(9) Page 10, line 198: Regarding "...less significant (< 1%)..." From Table 1, I estimate that for the 2020 case, changes in chemistry affect net O_3 production by 0.3% and ~3% during the life cycle and convective period, resp.

(10) P10, line 201: “...can certainly enhance the downwind ozone production on the scale of days...”

(11) Page 12, Fig. 5 caption: Please state white grid cells are for missing TROPOMI data (no2_scd_flag > 0 ?).

(12) Page 13, lines 218-219: “…middle troposphere (MT, 800 hPa to 400 hPa), upper troposphere (UT 400 hPa to 150 hPa)…”

(13) Page 13, line 220-221: “...Figure 6 shows that the AMF changes...”. Also, Beirle et al. (2009) noted a *decrease* in sensitivity in the UT due to the NO_2/NO_x branching ratio.

(14) Page 13, line 226: “...UT ΔAMF_{trop} > 20 % exists in Fig. $6b_i$ and b_{iii},”

(15) Page 14, Fig 6 caption:

“...is the AMF_{trop} with 500 mol NO per flash relative to 0 mol NO per flash”

“...is the AMF_{LNOx} with 700 mol NO per flash relative to 500 mol NO per flash”

Also, it would help to see the ovals overplotted on all figures for easier comparison.

(16) Page 15, Fig 7: Please label the x-axes in (b), (c) and (d)

Supplement:

(17) Figure S1, caption: “…WRF-Chem simulations for the 2019 and 2020 cases.”

(18) Figure S2: The times/dates in the legend of (a) are not correct

(19) Figure S3, caption: “…in Fig. 2 for 25 July, 2019.”