Review comment on #egusphere-2026-3032

This study utilizes a year-long surface O_3 measurement dataset from Leh in the Trans-Himalayan region, alongside satellite observations and model simulations, to analyze local O_3 characteristics. The O_3 observations reveal a unique double-peaked diurnal variation and strong daytime O_3 enhancement compared to other high-altitude sites. WRF-Chem simulations suggest that this region primarily serves as an O_3 sink, with O_3 levels strongly influenced by complex local circulation and transport processes. In addition, analysis of large-scale model outputs indicates that O_3 over Ladakh is mainly contributed by background CH_4 oxidation, stratospheric intrusions, and emissions from South Asia and other surrounding regions. Overall, this study provides a comprehensive analysis of O_3 dynamics and source attribution in the Ladakh region, which fits well within the scope of Atmospheric Chemistry and Physics. However, there are several weaknesses and areas requiring further clarification and justification. Therefore, I recommend a major revision for this manuscript. My specific comments are detailed below.

Major Comments

1. Section 3.1: The distinct double-peak O_3 diurnal pattern shown in Figure 2 may be closely linked to complex local mountain meteorology (e.g., upslope/downslope winds, valley breezes, or localized precipitation). Are there additional meteorological observations or nearby weather stations in the Leh region that could be analyzed to elucidate the interaction between O_3 variations and local wind dynamics? Furthermore, is the WRF-Chem model capable of capturing these fine-scale circulation patterns at the current grid resolution? The authors should provide further discussion and justification on these aspects.

2. Section 3.4: Using the satellite-derived $HCHO/NO_2$ ratio (FNR) as an indicator for the surface O3 formation regime over Ladakh comes with significant uncertainties. Both TROPOMI NO_2 and HCHO retrieval products contain intrinsic systematic biases, and TROPOMI HCHO is subject to substantial retrieval noise (low signal-to-noise ratio), particularly over pristine, high-altitude environments. Taking the ratio of these two products can severely compound these errors. While retaining this analysis is acceptable, the authors should explicitly discuss the limitations of using satellite FNR in this remote region. Additionally, I suggest providing spatial distribution maps of

the multi-year or seasonal mean TROPOMI NO_2 and HCHO columns over Ladakh so readers can assess data quality and signal levels.

Minor Comments

Lines 201–204: Given the complex topography over Ladakh, applying meteorological grid nudging (especially for horizontal winds) within the planetary boundary layer (PBL) can distort or artificially suppress fine-scale local circulations simulated by WRF dynamics. It is generally recommended to nudge only above the PBL height. Could the authors also clarify specify which reanalysis dataset (e.g., ERA5, NCEP FNL) was used for nudging?

Lines 205–206: The anthropogenic emission inventory used here (base year 2015) lags the target simulation period (2024–2025) by nearly a decade. Since the authors state that the Ladakh region has experienced rapid urbanization and increasing tourism, have they evaluated how these emission changes might impact model performance and source attribution?

Section 3.1: The observed double-peak diurnal pattern in O_3 is absent in certain months (e.g., afternoon dips do not occur in December, January, and May). Could the authors provide more explanation for these seasonal differences?

Line 318: The phrase "photochemical loss" reads awkwardly in this context; consider using the standard term "net chemical loss" or simply "chemical loss."

Line 340: The chemical tendencies reflect the daily variation of the chemical term rather than the total net budget of O_3. In addition, the magnitude of the chemical tendencies shown in Figure 3d appears unusual—one would expect it to oscillate around a balanced baseline. Please double-check these values and clarify the budget definition.

Section 3.4: Please clarify how the TROPOMI NO_2 and HCHO retrieval data were processed (e.g., spatial/temporal averaging windows). Were the satellite observations sampled strictly over the co-located ground site pixel or averaged over a wider regional domain?

Lines 424–429: Could the authors comment on why the model indicates the strongest stratospheric O_3 intrusion in June, even though the vertical velocity is relatively low during this period?

Figure 7c: Please clarify the mathematical definition and calculation methodology used to determine "O_3 build-up."

Lines 458–459: Please explicitly list the station names, coordinates, or mark their locations on a regional map.

Figure 8a: Please clarify how the dashed line was derived.

Line 472: Should "2024-2015" be "2024–2025"?

Section 3.6: Please provide a brief description of how O_3 source attribution is estimated in the CAM-Chem simulations.

Figure 9a: Please clarify how the CAM-Chem model output was combined with stratospheric O_3 estimates from CAMS reanalysis, as this methodology is currently omitted from the text.

Section 3.7: Given that emissions from other regions contribute substantially to O_3 levels over Ladakh, have the authors evaluated which specific upstream source regions show the largest transport impact?

Lines 570–580: The observed multi-year increases in TROPOMI NO_2 and HCHO columns could also be driven by upwind emission trends across South Asia (via regional transport) or climate variability (e.g., warmer temperatures enhancing biogenic VOC and soil NO_x emissions). Do the authors have access to local emission datasets, traffic statistics, or visitor numbers to substantiate the claim that these satellite trends are primarily driven by local anthropogenic activities?