We thank the reviewers for their thorough evaluation and suggestions. We have carefully addressed the comments below.

RC2

Specific comments:

1. Line 16: Include a statement on the context/background of the research at the start of the abstract.
   We revised the abstract accordingly.

2. Line 59: Specify how the summer monsoon impacts O_3. The O_3 decreases after June due to the monsoon doing what? The current implication of the sentence is that the monsoon causes a peak in May-June O_3.
   We revised the sentence referring to the results from Lee and Park (2022) and Wie et al. (2018).
   "Photochemical O_3 production is largest during the summer, but the summer monsoon brings clean marine air masses into the Korean peninsula resulting in lower O_3 levels in July–August compared to May–June (Wie et al., 2018; H. M. Lee and R. Park, 2022). May–June also has additional contributions to O_3 from wildfires, stratospheric intrusions, and transport from China (H. M. Lee and R. Park, 2022)."

3. Lines 74-77: Restructure to first introduce O_3 sensitivity to VOCs versus NO_x and then the relevant trace gas ratio. Similarly, for NO_3^- You could also expand on why O_3 and NO_3^- are sensitive to these specific compounds earlier in the paragraph.
   Revised as follows:
   "Photochemical O_3 production takes place by oxidation of VOCs and CO in the presence of NO_x, and can be either NO_x- or VOC-limited depending on the concentrations of these precursors. Formation of $PM_{2.5}$ NO_3^-, which is a major component of wintertime secondary $PM_{2.5}$ in Korea and is mainly present as ammonium nitrate, can be either NO_x- or ammonia (NH_3)-sensitive again depending on the concentrations of these precursors. These dependences define chemical regimes that are important to identify for emission control strategies. O_3 sensitivity to NO_x versus VOCs can be diagnosed using formaldehyde (HCHO) to NO_2 ratios measured from satellites, where HCHO and NO_2 are proxies for VOCs and NO_x emissions (Duncan et al., 2010; Martin et al., 2004). Similarly, $PM_{2.5}$ NO_3^- sensitivity to NO_x versus NH_3 can be diagnosed using NH_3 to NO_2 ratios measured from satellites (Dang et al., 2023, 2024)."

4. Line 87-95: This paragraph would be a good place to highlight the purpose and novelty of your work in a brief statement.
   Revised as follows:
   "Here we analyze recent 2015−2023 trends in air quality in Korea by exploiting surface, airborne, and satellite observations to provide insights for the effectiveness of past regulation policies and future management."

5. Table 1: What about the temporal resolution or overpass time of the satellites? The LEO orbit satellites will be measuring at a specific time of day over South Korea, so they could be catching the daily min or max values. How does this compare to the times other measurements are available for? A discussion of how this might affect the differences in the results for different datasets is missing. This could be included in the relevant results sections or section 2.
   We included detailed descriptions on satellite overpass times and how the data are sampled in the text (Lines 125-131) and figures.

"OMI and TROPOMI make afternoon overpasses at 13:30 local time (LT). We make use of morning overpasses for MOPITT (10:30 LT) and IASI (9:30 LT). We use hourly daytime observations from GEMS (7:45−16:45 LT), GOCI (9:30−16:30 LT), and GOCI-II (8:15−17:15 LT). For annual trend analyses we use GEMS observations made between 12−14 LT for consistency with the overpass time of OMI and TROPOMI measuring the same gases. We find no significant differences in observed trends when using surface observations sampled at satellite overpass times and therefore use all hours of the day."

6. Line 149: Expand on how the topography affects the CO along the east coast
Revised as follows:
"Low VCDs over mountainous areas are due to surface elevation reducing the background column. This effect of surface elevation on VCDs is less apparent for shorter-lived species with weaker background contributions."

7. Line 158: The 2019 spike seems quite small – is it greater than the uncertainty of the data?
The retrieval uncertainties of CO from MOPITT and TROPOMI are $0.6\text{-}1\times10^{17}$ molec. cm^{-2}, and this spike is 0.9×10^{17} molec. cm^{-2}, which appears to be insignificant as pointed out and removed this sentence.

8. Line 165: Specify what the continuing motivation for SO_2 emission controls is
Revised as follows:
"However, there is continuing motivation for emission controls because SO_2 is a precursor to $PM_{2.5}$ sulfate (SO_4^{2-})."

9. Lines 174-177: Link the other studies' results back to your findings, e.g., are they consistent, what are the implications of the different sources of SO_2
We find consistent decreases in 2016-2022 surface SO_2 from our analysis, therefore revised as follows:
"J. Park et al. (2024) found that recent trends (2016–2022) in national mean surface SO_2 were driven by reductions in both domestic (25%) and Chinese (16%) emissions, explaining the 41% decrease shown in Figure 3d."

10. Line 218: Clarify why the transportation contribution may be a severe underestimate
VOC source apportionment studies over Korea listed in the reference report a 20-30% contribution from vehicle emissions. S. Song et al. (2019) explain the underestimate in the inventory through discrepancies in the source profile used which depends on car type, driving conditions, emission control technology, etc. We clarified the degree of underestimate as follows:
"More than half of anthropogenic VOC (AVOC) emissions according to CAPSS are from solvent use while transportation is responsible for less than 10%, although the latter may be underestimated by a factor of 2–3 according to source apportionment studies (S. Song et al., 2019; Y. Kim and G. Lee, 2018; Kwon et al., 2021)."

11. Line 250: "values in Korea are higher everywhere" is inconsistent with previous statements. Below 0.03 can be greater than 0.02.
Revised as follows:
"GEMS R_{GF} values in Korea are higher than 0.01 everywhere, indicating a more important role for AVOC emissions than in the US where these emissions have been strongly regulated for decades (Parrish et al., 2009; Warneke et al., 2012)."

12. Line 254: Although there is no significant trend in surface BTEX, can you comment on the higher values over 2019-2021? Is this within the data uncertainty or a significant signal?

For annual trend analyses we used observations with complete 2015-2023 records so that each year contains data from identical monitoring sites. We first clarified this in Section 2 (Lines 109-110) and in figure captions:

"For annual trend analyses we use observations from AirKorea sites that have continuous records from 2015 to 2023."

In the current Figure 5g we averaged data across all BTEX AirKorea sites, which resulted in large interannual variability and no significant trend. We thank the reviewer for pointing this out and corrected Figure 5g, which now shows a decreasing trend in BTEX. We revised the manuscript as follows:

"Figure 5g shows no significant trends in AVOC emissions and satellite observations of CHOCHO and HCHO, although surface BTEX decreased at $-5.0 \pm 3.9\%\ a^{-1}$ during 2015−2023."

13. Line 269: Can you comment on why the satellite data do not show the late afternoon rise?

Differences may arise from the flight track dependency of aircraft data (Kwon et al., 2021). We revised as follows:

"The aircraft data show a late afternoon rise in HCHO for which we have no explanation and might reflect sparse sampling (Kwon et al., 2021)."

Reference: Kwon et al., Elementa 2021 (https://doi.org/10.1525/elementa.2021.00109)

14. Line 279: This seems to be the only result for 2005-2014 in the paper. Is it relevant to the rest of the work? If not, I would suggest removing it.

We agree and revised as follows:

"We find that May–June 90^{th} percentile MDA8 O_3 calculated for individual AirKorea sites and then averaged across all sites shows an increase of 0.8 ± 0.9 ppbv a^{-1} during 2015–2023 (Figure 7b)."

15-16. Line 279: Can you comment on the O_3 change between 2019 and 2020? Lines 301-305: Link the US data back to your results, otherwise they just read as additional, slightly random, facts.

Thank you for pointing this out. We removed the US trends and revised the paragraph focusing on background O_3 and its affect during 2019-2020. Previous studies confirmed that changes in domestic NO_x or VOCs emissions during the COVID-19 pandemic did not have significant impacts on pollutant levels (S.-W. Kim et al., 2023; Koo et al., 2020). Instead, emission reductions in China during the lockdown reduced long-range transport and contributed to the observed changes in Korea. We revised the manuscript as follows (Lines 320-331):

"Reports of O_3 increases in Korea based on data from the AirKorea sites may be biased by the AirKorea sites being concentrated in the SMA, which has been mostly VOC-limited, but this is now changing as NO_x emissions decrease. Our analysis suggests that O_3 pollution in Korea is now poised to decrease everywhere in response to continued NO_x emission controls.

An additional challenge for Korea to meet its air quality standard is the high background originating from East Asia, estimated to be 55 ppbv (Colombi et al., 2023). During the COVID-19 lockdown in 2020 precursor emissions significantly dropped in China but not in Korea (Koo et al., 2020), which led to reduced long-range transport of O_3 and hence lower background levels over Korea (S. W. Kim et al., 2023). This could explain the large decrease in O_3 found between 2019 and 2020, especially in NO_x-limited areas which are more sensitive to background contributions than local emissions."

Reference: Koo et al., Sci Rep 2020 (https://doi.org/10.1038/s41598-020-80429-4)

17. Figures 2-9: It would be useful to see some measure of uncertainty or error on the line graphs, or a statement on the associated uncertainty in the main text.

Retrieval uncertainties of satellite observations vary depending on how the uncertainties are defined and therefore we only commented on this for the SO_2 VCDs which have largest uncertainties (Lines 189-190). For all other data we show the error standard deviations of the regression slopes (trends).

18. Additional detail on data analysis could be added to the supplement or processing scripts shared in a code availability section

We added details on how the surface and satellite data where sampled, averaged, and analyzed in the Supplement.

Technical corrections:

We have revised the manuscript as suggested.

1. Line 40: Clarify 'Subsequent atmospheric chemistry (of these trace gases?) produces'
2. Line 68-69: I would change 'would respond nonlinearly' to 'responds nonlinearly', as this a general statement
3. Line 79: clarify you are listing the relevant LEO instruments
4. Line 116: "We do not use them here." Be explicit that the O_3 measurements are what is not used. This sentence and the previous one could be combined for clarity: "(…) are inconsistent over Korea (Gaudel et al., 2018), therefore we do not use them here."
5. Line 135: Explain 'SMA' acronym in the main text
6. Line 144: 'plays an important role in driving ozone formation'
7. Line 169-170: I suggest rephrasing to "(…), consistent with OMI SO_2 hotspots previously identified for 2011-2016 (Chong et al., 2020)." for easier reading.
8. Line 182: replace 'accounting' with 'which account' for clarity
9. Line 184: add 'the potential' to match 'motivated by': "but also the potential to reduce PM2.5"
10. Line 185: replace 'diesel engines in 2016' with 'diesel engines since 2016'
11. Line 198: "CAPSS shows an increase"
12. Line 202: "additional information on the diurnal variation of NO_2"
13. Line 320: "and is at its minimum in summer"
14. Line 327: "$PM_{2.5}$ observations in Seoul show" (not shows)
15. Line 386: "(…) component not found to show"
16. Line 393: "for in terms of decreasing O_3"