

1 Response to RC1:

2 The Abstract still mentions “wintertime”. Please replace with specific months for consistency  
3 with the main text.

4 We have modified the text in the abstract, which now reads:

5 “... we find population-weighted annual mean urban NO<sub>2</sub> VCDs were lower in 2024 than 2019 in  
6 Europe (-13%) and Asia and Oceania (-17%), with seasonal decomposition indicating that  
7 annual changes are largely driven by concentration decreases during November-March.”

8 Line 52 (also 55, 75, 122): "x" -> "x". (I believe such issue would be finally addressed before  
9 your proofreading.)

10 We changed the remaining instances in the manuscript to "x".

11 The shaded peripheral areas in Fig. 3a–c appear strange.

12 We have remade this figure with the peripheral shading removed.

13 Many place names mentioned in the maintext are not labeled in the figures. For example,  
14 Tangshan Shi (Line 298), Ulaanbaatar (Line 299).

15 Tang Shan Shi is already included in Figure 6c. Despite this, we note that it is common practice  
16 to discuss and include results that may not be incorporated into a specific plot. Given the large  
17 number of cities (>11000) discussed in this paper, it is simply not possible to have every city that  
18 is discussed within our results to be labeled on a plot. We will be hosting a dataset with all of  
19 these values in a publicly-available Zenodo repository.

20 The colorbar in Fig. 1 (also Fig. 6,8,9,10,11) has too few tick marks and does not include the  
21 boundary values.

22 We have added additional tick marks and ensured that boundary values for each end of the  
23 colorbars are included.

24 The x-axis titles in Fig. 6c–d are not clear.

25 We have increased the size of the axis titles on each of the combination figures (Figs. 6,8-11).

26 Line 390 (cm-2%): Typo?

27 We have removed the “%”.

28 The authors are strongly encouraged to improve the overall quality and consistency of the  
29 figures in the Supplementary Material. Issues include misuse of "x" and "x", inconsistent unit  
30 formatting (e.g., "molec." vs. "Molec." in Fig. S11), and non-standard capitalization (e.g., "year"  
31 in Fig. S13). The use of chemical notation should also be standardized (e.g., "NO<sub>2</sub>" vs. "NO2")

32 We have addressed the following within the supplement:

- 33
- Re-made Figures S11 and S19 to match the style of Figure 3 within the main text without  
34 the peripheral gray area

- 35 • Consistent use of "x" instead of "X"
- 36 • Consistent use of "molec." Instead of "Molec."
- 37 • Consistent use of "Year" instead of "year"
- 38 • Consistent chemical notation for nitrogen dioxide (NO<sub>2</sub>)
- 39

40 Response to RC3:

41 The revised manuscript by Huber et al. improved upon the previous version and adequately  
42 addresses some of my concerns, in particular regarding the uncertainty analysis. However, my  
43 comment on the too short period for trend analysis, 6 years (including the COVID-19 lockdown  
44 period) is not addressed. Because the authors do not extend the trend analysis over the OMI and  
45 TROPOMI period (2005-2024), I strongly recommend to replace 'trends' in the title by 'column  
46 variability' or 'temporal variability', and to avoid the term 'trends' throughout the manuscript. This  
47 work is about the observed NO<sub>2</sub> variability over 6 years, and as such, it is a useful piece of work.

48 We have removed the word “trend” from the title and have changed the title to “Global NO<sub>2</sub>  
49 Changes between 2019 and 2024 as observed by TROPOMI in Urban Areas and Emerging  
50 Hotspots”.

51 Additionally, we have removed all instances of the word “trend” when discussing our results and  
52 conclusions. We only continue to use “trend” in the introduction of the manuscript, when  
53 introducing previous studies of satellite NO<sub>2</sub> over multi-decadal timeframes. Any references to our  
54 results do not use the word “trend”.

55 Furthermore, the reply to my comment on the seasonal variability is not quite satisfactory, e.g.  
56 what caused the winter spike in 2022 in Russian cities? On the other hand, sources like fires and  
57 soils have a strong seasonal variability, and their potential impact on the seasonal NO<sub>2</sub> column  
58 should be discussed.

59 We find the request to identify specific causes of the winter 2022 spike in Russian cities to be  
60 beyond the scope of this manuscript. We agree with the reviewer’s sentiment that: “models would  
61 be necessary to separate the different contributions”. Our work is not an attribution study; it is  
62 intended to highlight changes in NO<sub>2</sub> concentrations observed by TROPOMI, with data having  
63 undergone rigorous quality control before oversampling to monthly or annual values. TROPOMI  
64 did observe real increases in NO<sub>2</sub> column concentrations over Russian cities in some Winter  
65 months of 2022. Identifying the explicit cause of such individual changes goes beyond the scope  
66 of this work, and would be very difficult given that Russian air quality data is not publicly available.

67 We agree that fires and soils as an NO<sub>2</sub> source should be acknowledged. We have added the  
68 following text to the conclusions at line XXX of the revised manuscript to acknowledge the  
69 potential role these sources could play:

70 “Seasonal differences in photochemical lifetimes (i.e., longest in winter), boundary layer mixing  
71 (i.e., more vertical mixing in summer), chemical partitioning between NO and NO<sub>2</sub> (i.e., the fraction  
72 of NO<sub>2</sub> is largest in winter), meteorological variability, and contributions from additional emissions  
73 sources including soil NO<sub>x</sub> and fire emissions, can all modulate the magnitude and timing of  
74 observed NO<sub>2</sub> concentrations.”

75 Finally, there seems to be a contradiction in your replies 2 (lines 543-558) and 4 (lines 567-576).  
76 The claim that 'you are documenting which urban areas appear to be implementing effective  
77 control strategies and which are not' is contrasting your statement that 'it is important to recognize  
78 that atmospheric chemistry also influences the observed NO<sub>2</sub> variability...'. As a matter of fact,

79 the observed variability is a combination of many factors (meteorology, emissions, chemistry),  
80 and models would be necessary to separate the different contributions. This should be clearly  
81 stated in the revised manuscript.

82 We think this may have been a misinterpretation by the reviewer. We are using the term “appear”  
83 to suggest but not definitively conclude that control strategies have been effective. We show  
84 statistical significance to demonstrate when changes are likely due to an anthropogenic factor,  
85 but even statistically significant changes would never guarantee that the random (natural)  
86 variability is not a cause (e.g., a statistically significant P-value of 0.01 suggests a 1% chance  
87 random variability was a cause, not 0%). We believe the phrasing of these sentences already  
88 captures this.

89 In conclusion, I would recommend the publication of the paper after the above comments are  
90 carefully addressed.

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