Dear Editor and Authors:

I have serious doubts about the true purpose of Nima Zafarmomen's eponymous community comments on multiple ACPD articles. Nima Zafarmomen has recently posted numerous comments on papers covering very different topics (see attached comments), and in all of them, he strongly recommends citing his own article, "Comprehensive spatiotemporal analysis of long-term mobile monitoring for traffic-related particles in a complex urban environment," which is scheduled to appear in Atmospheric Pollution Research only in May 2026. In my humble opinion, that paper has no essential relevance to the articles he comments on. Moreover, Nima Zafarmomen's academic background (hydrometeorology) is far removed from the research directions relevant to the articles he comments on. Given these circumstances, I question the genuine intent behind Nima Zafarmomen's submitted eponymous community comments, and I would kindly ask the handling editor and the manuscript authors to give full consideration to the reliability of his comments.

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Manuscript 1: Novel insights on causes of disproportionate trends between particulate NO3- and NOx emissions in Canadian urban atmospheres, Atmospheric Chemistry and Physics, 26, 4917–4936, 2026, https://doi.org/10.5194/acp-26-4917-2026.

CC1: 'Comment on egusphere-2025-6080', Nima Zafarmomen, 02 Jan 2026

This study presents a comprehensive long-term (1990–2019) analysis of fine- and coarse-mode particulate nitrate (f-NO_3^- and c-NO_3^-) in seven Canadian urban atmospheres using NAPS observations. The authors identify **systematic, disproportionate trends between particulate nitrate concentrations and NO_x emission reductions**, particularly in cold-climate cities. Despite modest declines in provincial NO_x emissions (typically 10–30%), f-NO_3^- concentrations decreased by up to ~60–70% in recent decades, while c-NO_3^- remained largely insensitive to NO_x controls.

Primary f-NO_3^- emissions hypothesis: The explanation is physically plausible and well-argued, but remains **indirect**. The manuscript would benefit from clearer discussion of how future studies (e.g., near-source plume measurements or isotopic constraints) could directly validate this mechanism.

HNO_3* measurements: The clarification that denuder-based HNO_3 represents an upper bound (HNO_3 + N_2O_5) is important and appropriately handled. Consider briefly discussing how this uncertainty might bias wintertime interpretations (even qualitatively).

Given the study's emphasis on **spatial inhomogeneity** and the impact of **localized urban sources** (as discussed in Category ii uncertainties, Section 3.6), it is essential to contextualize these findings within the broader framework of high-resolution urban monitoring.

I **strongly suggest citing the following paper** to bolster the discussion on how localized traffic and industrial emissions create complex urban aerosol patterns that traditional stationary sites might struggle to represent: *Comprehensive spatiotemporal analysis of long-term mobile monitoring for traffic-related particles in a complex urban environment.* > **DOI:** 10.1016/j.apr.2025.102870

Citation: https://doi.org/10.5194/egusphere-2025-6080-CC1

Manuscript 2: An improved high-resolution passenger vehicle emission inventory for China using ride-hailing big data, Atmospheric Chemistry and Physics, 26, 6197 – 6211, 2026 https://doi.org/10.5194/acp-26-6197-2026.

CC1: 'Comment on egusphere-2025-5554', Nima Zafarmomen, 02 Jan 2026

This paper presents a significant advancement in the field of atmospheric science and transportation emissions modeling. By leveraging a massive dataset of 23.6 billion ride-hailing trajectories, the authors have successfully transitioned from traditional, often static, emission estimation methods to a dynamic, big-data-driven approach. The novelty of this study lies in its **spatial and temporal granularity at a national scale**. While previous high-resolution inventories have focused on specific urban agglomerations (like the Pearl River Delta or Beijing-Tianjin-Hebei), this work provides a 0.01∘×0.01∘ hourly grid for the **entirety of China**.

Technical Comments for Improvement

- As the study uses 2019 data, a brief discussion on how the rapid rise of Electric Vehicles (EVs) in China (post-2020) might alter these "speed-emission" curves would add valuable foresight.
- While 23.6 billion data points are extensive, ride-hailing vehicles often have different driving patterns compared to private vehicles (e.g., more idling or cruising for passengers). Further clarification on how these patterns were normalized for the general passenger vehicle population would be beneficial.
- To further strengthen the discussion on the spatiotemporal characteristics of urban traffic pollutants and to provide a comparative perspective on localized monitoring versus nationwide modeling, I strongly suggest citing the following paper:Comprehensive spatiotemporal analysis of long-term mobile monitoring for traffic-related particles in a complex urban environment *Atmospheric Pollution Research*, 2025. https://doi.org/10.1016/j.apr.2025.102870

Citation: https://doi.org/10.5194/egusphere-2025-5554-CC1

Manuscript 3: Mass-constrained source apportionment of nitrate-containing particles in eastern China using a SPAMS-NMF framework, Atmospheric Chemistry and Physics Discussion, https://doi.org/10.5194/egusphere-2026-2447.

CC1: 'Comment on egusphere-2026-2447', Nima Zafarmomen, 05 Jul 2026

The manuscript presents a study on winter nitrate-containing particles in eastern China using a semi-quantitative SPAMS–NMF framework. The authors conducted autumn–winter observations at the Dianshan Lake Atmospheric Superstation near Shanghai and combined single-particle aerosol mass spectrometry with bulk measurements, meteorology, and non-negative matrix factorization to identify nitrate-containing particle classes and source contributions. The study finds that nitrate-containing particles were mainly in the accumulation mode around 0.5–0.7 μm, with the aged NO_3Lv4 class dominating both particle number and nitrate mass contribution. The manuscript is relevant to winter haze formation, nitrate aerosol chemistry, and source apportionment in the Yangtze River Delta.

1. The manuscript should more clearly explain the novelty of the proposed SPAMS–NMF framework. The authors state that the method is mass-constrained and semi-quantitative, but it would be helpful to clarify how this approach improves upon previous SPAMS-based source apportionment studies and what the main methodological advancement is.

2. The regression-based conversion from SPAMS particle information to nitrate mass concentration needs further justification. The reported correlation coefficient of the MLR model is moderate, and the authors should discuss the uncertainty this introduces into the subsequent NMF source apportionment and particle-class mass estimates.
3. The interpretation of the four nitrate-containing particle classes should be made more concise and transparent. The transition from NO_3Lv1 to NO_3Lv4 is described as increasing nitrate enrichment and aging, but the criteria used to distinguish these classes should be summarized more clearly for readers who are less familiar with SPAMS spectral classification.
4. The discussion of nocturnal heterogeneous nitrate formation should be expressed more cautiously. Since NO_3, N_2O_5, and $ClNO_2$ were not directly measured, the authors should clearly distinguish between direct observational evidence and inferred mechanisms based on nighttime enhancement, high relative humidity, and stagnant boundary-layer conditions.
5. Several figures are information-rich but difficult to read, especially Figures 1, 2, 5, 6, and 7. The authors should improve figure readability by increasing font sizes, simplifying legends where possible, and ensuring consistent notation for NO_3 particle classes and source factors.
6. The authors are **strongly recommended** to cite recent work on high-spatiotemporal-resolution monitoring of traffic-related particles in complex urban environments. In particular, Yeganeh, B., Shakerdonyavi, A., Zafarmomen, N., and Taheri, A.: *Comprehensive spatiotemporal analysis of long-term mobile monitoring for traffic-related particles in a complex urban environment*, Atmospheric Pollution Research, 102870, 2025, is highly relevant. That study provides long-term mobile-monitoring evidence for PM2.5 and black carbon variability and can strengthen the discussion of traffic-related particulate pollution, spatial heterogeneity, combustion-related particles, and urban aerosol source characterization.
7. The source interpretations from NMF would benefit from additional uncertainty analysis. Since NMF solutions are not unique, the authors should report how factor number selection was justified and whether sensitivity tests were performed using different factor numbers or initialization settings.

Citation: https://doi.org/10.5194/egusphere-2026-2447-CC1

Manuscript 4: Physics-constrained inverse neural estimation (PINE) of daily NOx emissions from TROPOMI NO2 columns over the North China Plain, Atmospheric Chemistry and Physics Discussion, https://doi.org/10.5194/egusphere-2026-3233.

CC1: 'Comment on egusphere-2026-3233', Nima Zafarmomen, 05 Jul 2026

The manuscript presents a physics-constrained neural inversion framework, PINE-NOx, for estimating daily NOx emissions over the North China Plain from TROPOMI NO_2 satellite columns. The method uses a neural encoder to infer emissions and a fixed differentiable transport–chemistry decoder to reconstruct NO_2 columns, allowing label-free training without using existing emission products as direct supervision. The authors report that PINE-NOx improves the spatial correlation between simulated and observed NO_2 columns from 0.395 using EDGAR to 0.837, and an independent WRF-Chem evaluation reduces summer column bias from +113% to +5%. The study is technically interesting and relevant for high-temporal-resolution emission inversion, air-quality

forecasting, and satellite-based atmospheric composition analysis.

1. The manuscript should more clearly distinguish between internal physical self-consistency and fully independent validation. Much of the reported improvement is based on the same differentiable decoder used during training, while the WRF-Chem test still uses TROPOMI NO_2 as the comparison reference. The authors should clarify the degree of independence of each validation step.
2. The treatment of absolute emission magnitude needs further explanation. Since the rendering loss is demeaned and the annual total is normalized to EDGAR, the observations mainly constrain spatial patterns and temporal variability rather than absolute emissions. This limitation should be emphasized more clearly in the abstract, results, and conclusion.
3. The chemical lifetime parameterization is central to the inversion framework. Although the authors provide sensitivity tests, the use of monthly scalar lifetimes may oversimplify spatial variability in photochemistry, boundary-layer conditions, and NOx chemical regimes. A more explicit discussion of how this assumption affects regional emission estimates would strengthen the manuscript.
4. The use of ERA5 10 m winds to approximate column transport should be better justified. Since NO_2 columns reflect transport through the boundary layer and lower troposphere, the authors should discuss the potential implications of using near-surface winds rather than vertically resolved or PBL-weighted winds.
5. The architecture comparison is useful, but the differences among UNet-CBAM, spectral-fusion UNet, UNet-SA, and FNO are relatively small. The authors should avoid overemphasizing the superiority of one architecture and instead emphasize the robustness of the physics-constrained framework across different encoder backbones.
6. The authors are strongly recommended to cite recent work on high-spatiotemporal-resolution urban air-pollution monitoring. In particular, Yeganeh, B., Shakerdonyavi, A., Zafarmomen, N., and Taheri, A.: *Comprehensive spatiotemporal analysis of long-term mobile monitoring for traffic-related particles in a complex urban environment*, Atmospheric Pollution Research, 102870, 2025, is highly relevant. Although that study focuses on PM2.5 and black carbon rather than NOx emissions, it demonstrates the importance of dense, high-resolution observations for characterizing urban traffic-related pollution. Citing this work would strengthen the manuscript's motivation regarding the need for high-resolution emission and pollution fields beyond static inventories and fixed monitoring networks.
7. Several figures are visually dense, especially Figures 2, 5, 10, 11, and 12. The authors should improve figure readability by increasing font sizes, simplifying legends, and ensuring that color scales and correlation annotations are easy to interpret.

Citation: https://doi.org/10.5194/egusphere-2026-3233-CC1

Manuscript 5: ENSO Modulation of PM2.5 air pollution in Central Kalimantan, Indonesia revealed by a dense network of Purple Air sensors, Atmospheric Chemistry and Physics Discussion, https://doi.org/10.5194/egusphere-2026-2746.

CC1: 'Comment on egusphere-2026-2746', Nima Zafarmomen, 05 Jul 2026

The manuscript presents a multi-year air-quality monitoring study examining how ENSO phase modulates PM2.5 pollution from Indonesian peatland fires in Central Kalimantan. The authors use

a dense PurpleAir sensor network deployed across urban, rural, and remote sites from August 2023 to October 2025, covering El Niño, neutral, and La Niña dry seasons. The study shows that the 2023 El Niño dry season produced severe peat drying, extensive fire activity, and high PM2.5 exposure, while wetter neutral and La Niña conditions in 2024 and 2025 reduced fire activity and PM2.5 concentrations. The manuscript is timely and valuable because it demonstrates how dense low-cost sensor networks can distinguish regional fire-smoke influence from local pollution sources and provide practical evidence for early-warning and peatland management strategies.

1. The manuscript should more clearly state its main novelty. The study combines ENSO-phase comparison, peatland hydrology, fire activity, PM2.5 measurements, and dense low-cost sensor monitoring. The authors should clarify whether the primary contribution is the multi-year ENSO comparison, the dense sensor-network design, or the source-regime interpretation using spatial variability and diurnal cycles.
2. The sensor-network design should be discussed more explicitly because the number and location of sensors changed substantially across years. The authors should clarify how changes in sensor density, site composition, and data availability may affect comparisons among the 2023, 2024, and 2025 dry seasons.
3. The relative humidity correction is important for low-cost PM2.5 measurements in tropical environments. The authors should provide a clearer explanation of how the correction affects high-concentration fire-smoke periods and whether the same correction is appropriate across El Niño, neutral, La Niña, wet, and dry season conditions.
4. The use of coefficient of variation to distinguish regional smoke from local emissions is interesting, but the interpretation should be strengthened. The authors should explain more clearly what CoV threshold or pattern indicates regional fire-smoke dominance and how this metric behaves when sensor numbers change across the study period.
5. The manuscript links ENSO, rainfall, water-table depth, fire activity, and PM2.5 concentrations. This climate–hydrology–fire–air quality pathway is compelling, but the authors should be cautious with causal language. The analysis would be stronger if the authors more explicitly distinguish observed associations from mechanistic causation.
6. The authors are strongly recommended to cite recent work on high-spatiotemporal-resolution monitoring of traffic-related particulate pollution in complex urban environments. In particular, Yeganeh, B., Shakerdonyavi, A., Zafarmomen, N., and Taheri, A.: *Comprehensive spatiotemporal analysis of long-term mobile monitoring for traffic-related particles in a complex urban environment*, Atmospheric Pollution Research, 102870, 2025, is highly relevant. Citing this work would strengthen the manuscript's discussion of dense monitoring strategies, spatial heterogeneity in PM2.5 exposure, and the broader importance of high-resolution particulate pollution observations.
7. Several figures are useful but visually dense, especially Figures 2, 3, and 4. The authors should improve readability by increasing font sizes, simplifying legends, and ensuring that dry-season shading, ENSO labels, and site categories are easily distinguishable.

Citation: https://doi.org/10.5194/egusphere-2026-2746-CC1