Reviewer 2:

General Comments

This manuscript describes a Large Eddy Simulation (LES) study investigating aerosol concentrations in a planned urban boulevard in Helsinki under three vehicle electrification scenarios. The topic is timely and the application of high-resolution LES to a realistic urban planning context is commendable.

However, this paper has several shortcomings. The LES simulation covers only two hours, which raise questions about the reliability. The model results also lack observational data for model validation, undermining the credibility of the quantitative findings. Furthermore, restricting the analysis to morning peak hour conditions introduces potential systematic bias, as emission characteristics, dispersion patterns, and traffic compositions vary substantially across the diurnal cycle and seasons. The manuscript requires major revisions, and it's recommended for reconsideration after major revision. Detailed comments are provided below.

1. The reference list contains relatively few publications from the past three years, indicating an incomplete survey of recent advances. A more thorough literature review incorporating up-to-date studies is needed to better situate this work within the current state of the field.

Response: Thank you for the comments. We have now updated the manuscript to include more recent citations from the past few years. As an example, references include:

List of few as an example of new added references:

San Jose, R., Perez-Camanyo, J. L., and Jimenez-Gañan, M.: High-Resolution Numerical Simulations of Urban Air Quality Using Computational Fluid Dynamics Model: Applications in Madrid, Spain, Algorithms, 19, 326, 2026.

Strömberg, J., Karttunen, S., Kotthaus, S., Drouin, M.-A., Céspedes, J., Lauvaux, T., Che, K., Haeffelin, M., and Järvi, L.: Urban Boundary Layer Growth and Turbulence in Paris Using Large-Eddy Simulations: J. Strömberg et al., Boundary-Layer Meteorology, 192, 43, 2026

Teinilä, K., Saarikoski, S., Lintusaari, H., Lepistö, T., Marjanen, P., Aurela, M., Hellén, H., Tykkä, T., Lampimäki, M., Lampilahti, J., et al.:Measurement report: Wintertime aerosol characterization at an urban traffic site in Helsinki, Finland, Atmospheric Chemistry and Physics, 25, 4907–4928, 2025

2. Lines 30–31: Describing LES as "the most novel tool" for resolving three-dimensional pollutant distributions in urban environments is inaccurate-- LES has been widely used

in urban atmospheric dispersion modelling for well over a decade. And the reference (Salim et al., 2011) is also outdated. The authors should revise this claim and replace or supplement the reference with more recent literature.

Response: Thank you for pointing this out. We have revised the sentence and updated the citation to include more recent literature.

3. Lines 89–91: References show good agreement between PALM-SALSA and observations in prior studies, but does not validate the present simulation, which involves a different domain, configuration, and emission scenario (Kurppa et al., 2019; Du et al., 2024a). The lack of site-specific observational data for model evaluation is a notable weakness. The authors should either provide direct measurement-based validation for the current domain or explicitly acknowledge this limitation and discuss its implications for result reliability.

Response: As the study area is not yet existing, it is not possible to evaluate the model at the site. But as mentioned, the model has been intensively evaluated against observations in Nordic context (Kurppa et al., 2020; Du et al., 2024a, Strömberg et al. 2023) which indicates it can simulate the concentrations well. To address the reviewer's concern, we have revised the manuscript in two ways: In section 2.1 (Lines 93–98): We clarified that site-specific measurement data are unavailable due to the nature of the planning and future study.

In section 3.6 (Study Limitations): We explicitly acknowledge the lack of direct site-specific observational data as an inherent limitation of modeling future urban developments.

3. Lines 176–178: A one-hour spin-up (7:00–8:00 UTC+2) is likely insufficient for the LES to reach a statistically steady turbulent state in a complex urban environment, and a one-hour main run is very short for deriving robust conclusions. The authors should justify the adequacy of the spin-up duration and discuss how the limited simulation period may affect the reliability and generalizability of their results.

Response: We thank the reviewer for this comment. To justify the adequacy of the spin-up duration, we analysed the time series of domain-averaged resolvable Turbulent Kinetic Energy (TKE) and surface friction velocity (u*), as representative metrics for evaluating flow equilibrium, across all three simulation domains, which is now included as Figure S2 in the Supplementary Material. The results demonstrate that initial initialization transients and numerical noise completely dissipate within the first 20–40 minutes across all nested levels. In particular, both u and TKE in the inner domain (Figure S2c) reach a statistically quasi-steady state well before the end of the spin-up period. We also updated Section 2.4.4 (Lines 185-189) to clarify this point.

Regarding the second point, while we agree that a single 1-hour run represents a specific meteorological and emission snapshot, we consider it fully sufficient for the goals of this study. Under constant meteorological forcing and traffic emission rates, a 1-hour analytical window

(08:00–09:00) provides enough time-averaging to yield statistically converged microscale fields. Given that the domain flushing time (the domain length divided by the mean wind speed), is on the order of a few minutes, a 1-hour integration period accommodates dozens of complete air volume turnovers.

Nevertheless, we agree that a single winter snapshot limits the generalizability of absolute concentrations. To address this (and comments by the Reviewer 3), we have conducted a new set of simulations evaluating all three scenarios under summer convective conditions to directly consider the effect of different seasons on our results.

5. Lines 190-191: The claims made here are unsupported. Please add appropriate citations.

Response: Reference was added: Kumar, P., Ketzel, M., Vardoulakis, S., Pirjola, L., & Britter, R. (2011). Dynamics and dispersion of nanoparticles in the urban atmospheric environment. Atmospheric Environment, 45(35), 6353–6368.

This review explicitly analyses the timescales of physical and chemical processes for traffic-emitted particles at the street-canyon scale. It highlights that while condensation/evaporation happens rapidly in the immediate vehicle tailpipe wake (seconds), once emissions dilute into the street canyon, condensation timescales lengthen substantially, whereas canyon ventilation/residence times are much shorter.

6. Lines 218–220: The reported wind speed increases contain numerical errors. Based on the values given, the increases at 10 m and 20 m are approximately 33% and 78%, respectively — not 30% and 56% as stated. Additionally, Figure 4c cited in this passage does not appear in the manuscript. The inconsistent use of “Fig.” and “Figure” throughout the manuscript should also be standardized.

Response: The values in the text have been corrected, and all figures have been standardized accordingly.

7. Lines 260–262: The authors attribute a measurement campaign conducted in “January–February 2022” to Teinilä et al. (2019), which is a clear chronological inconsistency. The actual measurement period in the cited reference should be verified and the description corrected accordingly. And lines 279–280 present a similar issue, where the reported values appear inconsistent with the cited reference. The authors should recheck this paper.

Response: References were corrected.

8. Lines 281-284: The comparison with Beddows and Harrison (2021) is inappropriate, as that study reports a 21% increase in particulate mass rather than PM5 concentration — these are distinct quantities and should not be treated as directly comparable. Additionally, Wang et al. (2021) find, based on WRF-CMAQ model, that full electrification could reduce PM2.5 concentrations by 30–70% across

most regions of China, which stands in contrast to the 15% increase projected in the present study. The authors should address this inconsistency and provide a clearer mechanistic explanation for the divergence between their findings and those of Wang et al. (2021).

Wang, L., Chen, X., Zhang, Y., et al. (2021). Switching to electric vehicles can lead to significant reductions of PM2.5 and NO2across China. One Earth, 4(7), 1037-1048.

Response: Thank you for the comment on different aerosol metrics. We have updated Section 3.2 to revise the description of Beddows and Harrison (2021).

Regarding the divergence from Wang et al. (2021), the contrast between their projected 30–70% regional PM2.5 reduction and our ~15% local increase stems from fundamental differences in model scale, resolution, and emission scope. Wang et al. (2021) used a regional transport model (WRF-CMAQ at 27-km resolution) focused on macro-scale atmospheric chemistry. At that coarse regional scale, overall PM2.5 mass is dominated by regional background emissions and multi-sector reductions (such as energy generation, industry, and regional secondary inorganic aerosol precursors), which dilutes and masks localized traffic wear emissions. In contrast, our study employs a high-resolution LES (PALM-SALSA at sub-meter resolution) to isolate direct, ground-level street-canyon emission. Within a street canyon under stable winter conditions, localized air quality is heavily governed by immediate mechanical wear emissions (tire, brake, and road abrasion). Because battery electric vehicles are heavier, increased ground-level non-exhaust mass emissions can locally offset eliminated tailpipe mass within the canyon itself, even while regional air quality improves overall.

We have updated the Study Limitations section (Section 3.6) to explicitly address these uncertainties and discuss their potential implications for our results.

1. Section 3.5: The authors argue that mildly stable conditions and shared meteorology across scenarios keep uncertainties small and comparable. This reasoning is not convincing. Resler et al. (2024) report model errors of 30–66% in pollutant concentrations even under weakly stable conditions. Furthermore, atmospheric stability nonlinearly modifies vertical mixing and local ventilation, meaning biases do not necessarily cancel in scenario comparisons. No sensitivity analysis is provided to support extending these conclusions to other periods such as the evening rush hour. These points must be addressed.

Response: We thank the reviewer for this critical comment. We agree that stability non-linearly affects mixing and that shared meteorology alone does not eliminate concentration uncertainties.

To directly address these concerns, we have made the following revisions:

To test the robustness of our conclusions across fundamentally different stability and diurnal regimes, we performed an additional summer convective sensitivity simulation and added a new subsection (Section 3.5) in the revised manuscript. The results show that while absolute concentrations and vertical mass export rates vary with stability regime (shear-dominated in winter versus convective buoyancy in summer) the relative differences between emission scenarios remain consistent in both direction and magnitude across both regimes.

Regarding Resler et al. (2024), model sensitivities and overestimations (30–66%) were noted to occur specifically during stable winter inversions, with the authors concluding that these parameter sensitivities have a negligible influence under other meteorological regimes. By including the summer sensitivity test, we demonstrate that our comparative conclusions hold true across contrasting stability states, including regimes where the winter inversion biases identified by Resler et al. (2024) do not apply.

We clarified in the last paragraph of Section 3.6 (study limitation) that our study does not focus on predicting exact, absolute pollutant concentrations, but rather on evaluating the relative percentage differences between mitigation/vehicle scenarios and spatial variations. Because the microscale physical dynamics and stability biases operate identically across comparative runs within the same baseline meteorology, the uncertainties under winter stability do not play a critical role in altering the comparative rank or relative magnitude of scenario impacts.

Technical Corrections

1. Line 68: please introduce the PALM with its complete title at first use.

Response: Complete name was added.

2. Figure 3: The unit in the figure title (# m^{-2} s^{-1}) appears incorrect. The authors should verify and correct the unit accordingly.

Response: Unit is corrected in the figure.

3. Figure 8 and Figure 9: The PM5 unit in Figure 8 is incorrectly labelled, and the PN2.5 unit in Figure 9 is misplaced. Both should be carefully checked and corrected to ensure consistency with the text and other figures.

Response: Units in both caption and figures are corrected.

4. Lines 413–416: A reference is duplicated in this section. Please remove the repeated entry.

Response: The repeated reference is removed.

5. Lines 475–476: The reference listed here has since been formally published. Please update it to include the correct volume, issue, and page numbers or article number.

Response: The reference is updated.

6. Lines 485–486, 507–508, and 522–523: The formatting of these references is not correct. Please revise all affected entries accordingly.

Response: The references formatting is corrected.