Qeios

Peer Review

Review of: "Electric Vehicle Fleet Penetration and Air Pollution Mortality in Europe"

Elizabeth Chan[1]

1. Independent researcher

Overall, the manuscript tackles an important question and makes a commendable choice to focus on fleet composition rather than vehicle sales. However, in its current form, it reads more like an opinion piece than a research article, with substantial gaps in documentation and analytics that undermine the strength of its conclusions.

The manuscript is missing many essential references, especially in the introduction. The single citation provided does not substantiate the breadth of claims made. Several declarative statements, including the opening claim in the discussion and the second-to-last sentence of the conclusion, lack any citation or supporting analysis.

Methodologically, the manuscript overstates what the data show. The use of terms such as “prove” is an over-interpretation; the results are at best suggestive and consistent with the hypothesis that higher EV shares are associated with better air quality and lower mortality. Crucially, the analysis does not disentangle or elucidate the specific impacts of EV usage on PM2.5-attributable mortality in each country. The implicit assumption that a higher percentage of EVs leads to fewer PM2.5 deaths per 100,000 people is a causal leap absent strong support. Important confounders appear to be omitted, including population density, the number of vehicles per 100,000, baseline PM2.5 concentrations, fuel mix, and socioeconomic factors. Without considering these, the observed association cannot be interpreted as causal and may simply reflect broader cross-country differences.

The abstract needs clarification. For example, the third sentence is not clear; it is not evident what variables are being compared in the reported negative correlation. Please specify so readers can understand the claim.

doi.org/10.32388/VLFA9Z

With respect to the one-year offset between mortality data (2023) and fleet data (2024), I agree with the author that it is likely negligible and not a major concern.

Presentation of results requires revision, particularly Figure 2. The dashed linear regression line does not add insight and is likely misleading. It may imply more confidence than warranted by the strength of the correlation and even suggests the possibility of negative air pollution deaths per 100,000, which is not meaningful. If a regression line is retained, it should be constrained to nonnegative values and possibly truncated at the range of observed data (e.g., at Norway's data point). That said, the correlation itself appears weak: points in the 2–15% EV share range look essentially random, and the pattern does not support strong conclusions even if a statistical test yields significance. A more nuanced analysis (e.g., multivariate models with appropriate controls, sensitivity checks, or a panel design exploiting within-country changes) would be necessary to substantiate the claims.

In light of these issues, I recommend one of two paths: 1) Undertake a substantial methodological and analytical revision. This would include adding the missing literature citations, specifying the statistical framework, incorporating key confounders, performing additional analyses, and potentially tempering causal language, or 2) Reframe the piece as an op-ed that presents the argument qualitatively, supported by selective evidence, without claiming proof. As written, the manuscript's conclusions are not sufficiently supported by the data.

Declarations

Potential competing interests: No potential competing interests to declare.