Does Mask Usage Correlate with Excess Mortality? Findings from 24 European Countries.

A Comment on Tausk and Spira (*BMC Public Health*, 2025)

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Abstract

Tausk and Spira (2025a) report a positive cross-country association between average mask usage and excess mortality across 24 European countries during 2020–2021. I re-examine that association using a reconstructed country-year panel. The positive cross-sectional pattern is preserved in the reconstructed data, but it is not stable to two minimal re-specifications: comparing changes within countries across the two years, and adding end-of-2021 vaccination coverage to the within-country specification. The first step reverses the sign of the mask coefficient; the second attenuates the differenced mask coefficient toward zero. A causal interpretation of any of these coefficients would require restrictive and, in this setting, arguably implausible assumptions. Because the published positive association is sensitive to routine re-projections of the same data, it is too fragile to support the policy interpretation that has been placed on it. The episode illustrates a more general point: cross-country regressions that relate country-level averages to health outcomes are vulnerable to sign reversals when aggregation obscures heterogeneity and timing.

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1 Introduction

Tausk and Spira (2025a) report a striking positive cross-country association: among 24 European countries, higher average mask usage during 2020–2021 is associated with higher excess mortality. The article treats this pattern as informative about population-level masking and describes the setting as "an ideal basis for a natural experiment." The finding was published in *BMC Public Health* and has attracted considerable attention, with roughly 30,000 accesses on the journal webpage. A subsequent correction (2025b) softens the causal language of the original, clarifies the modelled nature of the mask-usage series, and has received approximately 300 accesses.[1]

This comment examines the published correlation and regression result underlying that claim; the exact original rows and corresponding numerical targets are identified in the Results section. I first reproduce the positive cross-sectional pattern in the reconstructed panel used for all subsequent specifications and then ask whether the same empirical relationship is stable when the time dimension of the data is used. When the data are differenced within countries between 2021 and 2020, the sign of the estimated coefficient reverses, turning the positive cross-sectional slope into a negative within-country slope. Because the two specifications draw on fundamentally different sources of variation, the reversal is informative about the fragility of the original estimand rather than about the effectiveness of masks. A second robustness step adds end-of-2021 full-vaccination coverage to the within-country specification, which attenuates the mask coefficient toward zero. The original positive association is fragile to two routine operations on the same data: netting out country-invariant levels, and controlling for the dominant time-varying driver of 2021 mortality. Section 3 reports the magnitudes and inference.

The finding belongs to a well-known family of problems. Since Robinson's (1950) classic demonstration that correlations between group means can differ in sign from individual-level correlations, it has been understood that aggregation is not neutral. In this setting, collapsing two years of data into a single cross-section discards the within-country timing of both mask adoption and epidemic waves. The resulting regression combines persistent between-country differences with the common shift between 2020 and 2021 that affected mask use and mortality simultaneously. Differencing removes country-invariant levels and uses within-country changes as a more diagnostic, although still non-causal, source of variation.

The comment proceeds as follows. Section 2 describes the data and empirical approach. Section 3 links the original correlation and standardized-coefficient results to their counterparts in the reconstructed analysis data, presents the first-differences reanalysis, and reports the first-differences specification with vaccination coverage. Section 4 discusses identification failures in the original design, the implications of the sign reversal for policy interpretation, and broader lessons for research practice. Section 5 concludes.

A related comment by Cerqueira-Silva et al. (2026) raises methodological concerns using higher-frequency variation. My contribution differs in emphasis. I treat the fragility of the headline result as an identification problem created by the choice of estimand and by temporal aggregation, and I discuss the implications for how observational associations are communicated in policy-relevant

[1] I note this gap between the two documents as a fact about their circulation, without drawing further inferences about intent or responsibility.

outlets.

2 Data and empirical approach

2.1 Data

The empirical exercise uses the same 24-country European sample as Tausk and Spira (2025a) and reconstructs two annual observations per country ($t \in \{2020, 2021\}$). Mask usage is measured from the Institute for Health Metrics and Evaluation (IHME) COVID-19 projections, whose mask-usage series is derived from the COVID-19 Trends and Impact Survey fielded on Facebook (n.d.); the series reports the modelled share of respondents who report always wearing a mask when leaving home. Excess mortality in the reconstructed panel is the Our World in Data (OWID)-processed cumulative excess-mortality series based on the Economist model (n.d.). Country-year values are obtained by aggregating within each year. End-of-2021 full-vaccination coverage, used only in the extended specification of Section 3, is taken from the dataset accompanying the original article (2025a). European vaccination rollout began on 27 December 2020, so the end-of-2021 level is numerically equivalent to the first difference 2021–2020 of the vaccination variable. This common country-year structure keeps the cross-sectional comparison and the first-differences reanalysis on the same reconstructed data.

2.2 Empirical specifications

I estimate a sequence of regression specifications that use the same reconstructed analysis data but differ in variance convention and in the source of identifying variation. The first regression follows the cross-sectional approach of the original study. It uses two-year country averages and estimates

$$\overline{\text{ExcessMortality}}_i = \alpha + \beta\,\overline{\text{MaskUsage}}_i + u_i. \tag{1}$$

Definitions of the time-aggregated variables follow the Supplementary Materials of the original article (2025a). Identification of a causal β in (1) requires that $\overline{\text{MaskUsage}}_i$ be mean-independent of every other country-level determinant of excess mortality over 2020–2021 that is absorbed into u_i. Plausible violations include age structure, long-term care residence, baseline health-system capacity, the timing and severity of the early 2020 wave, and the extent of mandated versus voluntary mask adoption.

The second specification uses within-country variation over time. Because the panel has only two periods, I estimate the coefficient directly from first differences:

$$\Delta\text{ExcessMortality}_i = \kappa + \beta\,\Delta\text{MaskUsage}_i + \Delta\varepsilon_i, \tag{2}$$

where Δ denotes the change between 2021 and 2020. This regression removes all time-invariant country characteristics by construction and, through the intercept κ, absorbs the common year shock.[2] Identification of a causal β in (2) requires two assumptions. Additivity: the level equation

[2]With $T = 2$ the first-differences estimator is numerically identical to the within estimator from a two-way fixed effects model. Following Wooldridge (2010), write the levels equation as $\text{ExcessMortality}_{it} = \alpha_t + \beta\,\text{MaskUsage}_{it} + c_i + u_{it}$ for $t \in \{2020, 2021\}$, where α_t is a year effect and c_i is an unobserved, time-constant country effect that may be correlated with MaskUsage_{it}. Differencing across the two periods yields $\Delta\text{ExcessMortality}_i = (\alpha_{2021} - \alpha_{2020}) + \beta\,\Delta\text{MaskUsage}_i + \Delta u_i$. Thus c_i drops out by construction, and the differenced equation contains an intercept equal to the common year change. OLS applied to (2) is consistent for β under the orthogonality condition $\text{E}(\Delta\text{MaskUsage}_i \cdot \Delta u_i) = 0$. Demeaning each variable by its country mean and its year mean produces the same residual variation as differencing when $T = 2$, and so the same point estimate.

for excess mortality admits a country fixed effect that enters additively rather than through a nonlinear interaction with time. Strict exogeneity of $\Delta\text{MaskUsage}_i$ with respect to $\Delta\varepsilon_i$: no time-varying driver of excess mortality between 2020 and 2021 is correlated with the within-country change in mask usage. Additivity is not innocuous when the mapping from infection dynamics to mortality is nonlinear. Strict exogeneity is also doubtful: in this sample, countries with larger increases in mask usage tended to have higher end-of-2021 vaccination coverage (Pearson correlation 0.52), and vaccination is an important time-varying determinant of 2021 mortality.

The third specification extends (2) by including end-of-2021 full-vaccination coverage V_i as a time-varying covariate:

$$\Delta\text{ExcessMortality}_i = \kappa' + \beta\,\Delta\text{MaskUsage}_i + \gamma\,V_i + \eta_i. \tag{3}$$

For comparability, I first report the conventional HC1 heteroskedasticity-consistent standard error and then switch to HC3, the small-sample-preferred variant for $n = 24$; all tests are two-sided.

3 Results

I begin by anchoring the analysis to the same reconstructed data used in the robustness exercises. As a computational check, the replication package reproduces the published cross-sectional targets within rounding: the original Table 2 Spearman value is reproduced as $\rho = 0.4775$ ($p = 0.0183$), and the original Table 3 standardized mask coefficient is reproduced as 0.5157 ($p = 0.0012$). Table 1 then reports the published original result and its counterpart in the reconstructed analysis data, showing that the analysis panel reproduces the original positive cross-sectional association with only small numerical differences. The "Original paper" column reports the published cross-sectional targets, reproduced from the dataset accompanying the original article. The "Reconstructed data" column uses the country-year panel constructed for this comment from the same 24 countries. I use the reconstructed panel for the main analysis because the first-differences specifications require separate 2020 and 2021 observations for each country, whereas the original published targets are cross-sectional summaries. Small differences between the two columns therefore reflect differences in the excess-mortality measure, statistic, and data structure, not a change in country sample. The original Table 2 statistic is a Spearman rank correlation between mask usage and the Per Levitt age-adjusted (PLAA) excess-mortality measure. In the reconstructed analysis data, the corresponding Spearman correlation is 0.463 ($p = 0.023$), close to the original 0.477 ($p = 0.018$). For OLS, standardizing the bivariate slope gives 0.517, essentially the original Table 3 standardized mask coefficient of 0.52, although Table 3 is multivariate and uses PLAA. The unstandardized coefficient reported in columns (1) and (2) of Table 2 is 0.121 because mask usage is expressed in percentage points; its p-value is 0.005 under HC1 and 0.018 under HC3. The link between the bivariate correlation and the OLS coefficient is mechanical: in an OLS regression with an intercept, the standardized slope equals the Pearson correlation, and the unstandardized slope equals $r_{XY}s_Y/s_X$, where r_{XY} is the Pearson correlation between mask usage X and excess mortality Y, and s_X and s_Y are their sample standard deviations.

Table 1: Original cross-sectional results and counterparts in the reconstructed analysis data.

Quantity	Original paper	Reconstructed data
Spearman rank correlation (Table 2, PLAA row)	$\rho_{\text{mask,PLAA}} = 0.477$ $p_\rho = 0.018$	$\rho_{\text{mask,EM}} = 0.463$ $p_\rho = 0.023$
Std. mask coefficient (Table 3, Mask usage row)	$\beta^{std}_{\text{mask}} = 0.52$ $p_\beta = 0.0012$	$\beta^{std}_{\text{mask}} = 0.517$ $p_{\beta,\text{HC3}} = 0.018$

Notes: PLAA denotes the Per Levitt age-adjusted excess-mortality measure used by Tausk and Spira (2025a); EM denotes excess mortality in the reconstructed analysis data. The original-paper column reports published cross-sectional targets, reproduced from the dataset accompanying the original article. The reconstructed-data column uses the reconstructed country-year panel that is also used in all subsequent specifications. The original data are not used as the main analysis panel because they do not provide the separate 2020 and 2021 observations needed to compute first differences. Exact checks against the published Table 2 and Table 3 values are included in the replication package.

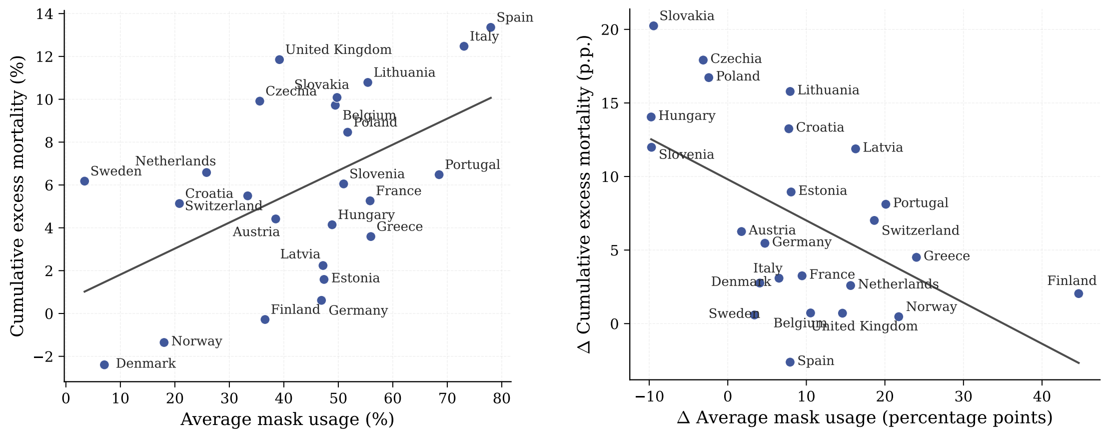
Table 2 then moves step by step from the positive cross-sectional association in the reconstructed analysis data to the robustness exercises. Columns (1) and (2) use the same cross-sectional OLS point estimate but differ in the variance estimator: HC1 in column (1), then HC3 in column (2). The estimated coefficient is $\hat{\beta} = 0.121$ in both columns and remains positive and statistically significant. Column (3) applies the HC3 convention to the first-differences regression. The estimated coefficient is $\hat{\beta} = -0.279$ ($p = 0.010$), so the sign reverses. Column (4) adds end-of-2021 vaccination coverage. The mask coefficient attenuates to -0.099, with its p-value rising from 0.010 to 0.098 (significant at the 10% but not the 5% level), while the vaccination coefficient is -0.445 and highly significant ($p < 0.001$). The Pearson correlation between $\Delta\text{MaskUsage}_i$ and V_i is 0.52 (bootstrap 95% confidence interval (CI) $[0.25, 0.73]$).

Table 2: Stepwise cross-sectional and first-differences reanalysis.

	Cross-section		First diff.	FD + vax
	(1) HC1	(2) HC3	(3) HC3	(4) HC3
Mask usage	0.121^{***} (0.043)	0.121^{**} (0.051)	-0.279^{***} (0.108)	-0.099^{*} (0.060)
Standardized coefficient	0.517	0.517	-0.530	-0.188
Vaccination				-0.445^{***} (0.103)
Observations	24	24	24	24
Countries	24	24	24	24
R^2	0.27	0.27	0.28	0.60

Notes: Columns (1) and (2) regress the time-aggregated excess-mortality measure on time-aggregated mask usage, one observation per country; the point estimate is unchanged and only the heteroskedasticity-consistent variance estimator changes. Column (3) regresses the change in excess mortality (Δ2021–2020) on the change in mask usage, removing all time-invariant country characteristics by construction. With $T = 2$, this estimator is numerically identical to a two-way fixed effects specification with country and year indicators. Column (4) adds end-of-2021 full-vaccination coverage, taken from the dataset accompanying Tausk and Spira (2025a). The HC1 standard error for the first-differences mask coefficient is 0.082; column (3) reports HC3 after the variance-convention step. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Figure 1 illustrates the two projections of the same data. Panel (a) plots the cross-sectional association in two-year averages and confirms the upward slope. Panel (b) plots the within-country association using the change between 2021 and 2020, showing a clear downward slope.



(a) Cross-sectional association (2020–2021 averages).

(b) Within-country association in first differences (Δ 2021–2020).

Figure 1: Mask usage and excess mortality across 24 European countries, 2020–2021. The fitted line in panel (b) is the OLS regression in differences, which is numerically identical to the two-way fixed effects estimator when the panel has two periods (see footnote 1).

Three comments on Table 2. First, the cross-sectional slope in columns (1) and (2) combines persistent between-country differences with the shared time component that shifted both mask use and mortality between 2020 and 2021. The regression on averages does not distinguish these sources of variation. By contrast, the first-differences coefficient in column (3) is estimated from changes within the same country across the two years; differencing mechanically removes all time-invariant country characteristics, and the intercept absorbs the common year shock. Second, the attenuation of the mask coefficient from column (3) to column (4) is what one would expect when the dominant time-varying driver of 2021 mortality is added to a first-differences specification. It does not reverse the argument of the comment, which turns on the instability of the cross-sectional slope under two related re-specifications rather than on any particular magnitude of the first-differences coefficient. Third, I do not interpret any coefficient in Table 2 as a causal effect of masking. Such an interpretation would require restrictive assumptions about cross-country comparability, additive country effects, and time-varying confounding. The pattern across columns instead shows that the sign and magnitude of the association depend strongly on whether the analyst uses the time structure of the data and on whether vaccination is included.

4 Discussion

4.1 Identification failures in the original design

The original article describes its setting as “an ideal basis for a natural experiment” (2025a). From an econometric standpoint, this characterization is incorrect. A natural experiment requires that the variation used to estimate the effect is plausibly independent of potential outcomes. Cross-country differences in mask compliance do not satisfy this condition. They reflect mandates, enforcement, social norms, risk perception, and the evolving severity of the epidemic itself (2021; 2021).

A joint-determination representation clarifies the problem. Both mortality and mask usage load on epidemic severity:

$$\text{Mortality}_{it} = f(\text{Severity}_{it}, \text{Healthcare}_{it}, \text{Demography}_{it}, \ldots) + u_{it}, \tag{4}$$
$$\text{MaskUsage}_{it} = g(\text{Severity}_{it}, \text{Mandates}_{it}, \text{RiskPerception}_{it}, \ldots) + v_{it}. \tag{5}$$

Countries hit harder by the pandemic adopted more protective behaviour. When one regresses mortality on mask usage without accounting for this joint determination, the coefficient captures the behavioural response to the epidemic rather than any direct protective effect of masking. Time aggregation compounds the problem by discarding the timing information that could help distinguish mask adoption that preceded epidemic waves from adoption that was triggered by them.

The cross-sectional and first-differences specifications in Table 2 answer different questions. The cross-section asks whether countries with higher average mask usage had higher average excess mortality; the first-differences specification asks whether countries where mask usage increased more between 2020 and 2021 also saw excess mortality increase more. These are distinct estimands, and the fact that they have opposite signs tells us something important about the original result. The positive cross-sectional slope is consistent with reverse causality from mortality conditions to mask adoption. Countries that experienced more severe pandemic conditions adopted masks more intensely and also suffered higher mortality; both outcomes respond to the same underlying forces. By averaging over two years, the cross-sectional regression can capture this joint response and attribute it to masks. That the sign reverses when time-invariant country characteristics and the common year shock are removed suggests that the positive cross-sectional association may be an artefact of aggregation rather than a feature of the underlying relationship. For policy evaluation the result is not wrong in a narrow statistical sense, but it is misleading in the sense that matters for public discourse.

The original study also suffers from small-sample fragility. With 24 countries, degrees of freedom are severely constrained. The original workflow combines bivariate screening for covariate inclusion and extensive specification search across a large number of regressions. In such a small sample, specifications that draw on the same between-country variation can produce similar coefficients even when the association is driven by a handful of influential observations or by shared confounders. That many variants point in the same direction does not establish robustness if all variants exploit the same problematic source of variation.

4.2 *The role of vaccination*

Column (4) of Table 2 adds end-of-2021 full-vaccination coverage to the first-differences specification. This is a natural covariate in this context. Vaccination rollout was the dominant positive intervention of 2021 in the 24-country sample; it began on 27 December 2020 and accelerated through the year, with substantial cross-country variation in pace and final coverage. Vaccination is also associated with lower excess mortality over 2021, and the specification in column (4) estimates $\hat{\gamma} = -0.445$ ($p < 0.001$). In this sample, countries with larger increases in mask usage also tended to have higher end-of-2021 vaccination coverage (Pearson correlation 0.52), so the first-differences specification without vaccination is exposed to a standard omitted-variable problem. Once vaccination is added, the mask coefficient falls to -0.099 and its p-value rises from 0.010 to 0.098, crossing the 5% threshold while remaining below 10%.

The interpretation of column (4) is not that the first-differences specification delivers a cleaner causal estimate than the cross-section. The first-differences coefficient in column (3) is also vulnerable to bias; the second assumption of Section 2 is doubtful precisely because of vaccination. The point is weaker and more robust: the cross-sectional coefficient reported in (2025a) does not survive two related and minimal re-specifications of the same data, one that removes time-invariant country differences and one that adds the dominant 2021 driver. A causal reading of the published positive association would require assumptions that are difficult to defend in this two-year cross-country setting.

4.3 The published correction

A correction to the original article was published later in 2025 (2025b). The correction softens, but does not eliminate, the causal framing of the original. Several passages in the Methods, Results, and Discussion are reworded into weaker formulations; the IHME mask-usage series is now described as a modelled quantity derived from Facebook-based survey responses, with the underlying survey methodology references added (2020; 2021); and two sentences are added to the Discussion stating that “increased mask usage did not necessarily lead to higher COVID-19 mortality” and that “mask adoption rates in each country were primarily shaped by governmental mandates and individual choices.”

Three features of the original article are not changed by the correction. First, the cross-sectional design, the 24-country sample, and the published numerical estimates are unaltered: the central result remains the positive cross-sectional slope in column (1) of Table 2. Second, the interpretation in the corrected Discussion is still that this slope is consistent with mask usage having been ineffective or counterproductive in community settings, an inference that requires a causal reading. Third, several causal formulations remain in the corrected text, even after the wording revisions described above. The Methods abstract still describes the 2020–2021 period as “providing an ideal basis for a natural experiment.” The Discussion still states that “we retrospectively examined the impact of population-level mask usage on excess mortality across Europe” and proposes a specific causal mechanism, namely that “mask use may lead to increased mortality in COVID-19 patients by promoting the re-inhalation of virions.” The corrected Conclusions still call for further inquiry into “the effectiveness and safety of masks” and frame the open question as whether masks “effectively prevent respiratory viral transmission, have no significant impact, or potentially cause adverse effects.” Each of these formulations is causal in content, even where the surface verbs of individual sentences have been softened. The corrected title (“Does mask usage correlate with excess mortality?”) frames the research question as correlational, while the language of the body of the article continues to be that of a study of effects.

The two issues that motivate this comment, the within-country re-projection that reverses the sign and the attenuation of the first-differences coefficient once vaccination is included, are not discussed in the correction.

4.4 Broader implications for research practice

This case study illustrates three lessons that extend beyond the specific debate about masks and mortality. First, temporal aggregation is not a neutral data-processing step. Collapsing time discards within-unit variation and acts as a low-pass filter that attenuates the high-frequency dynamics in which policy responses and epidemic feedback typically unfold (1995). Specifications that exploit within-unit variation over time, whether through differencing or fixed effects, are the appropriate default for settings in which behaviour and outcomes co-evolve. Cross-sectional averages should be treated as descriptive summaries rather than inputs to policy evaluation.

Second, the language of causal inference should be used with care. Terms such as "natural experiment," "effect," and "impact" have precise meanings in econometrics and epidemiology, and applying them to designs that lack the corresponding identification requirements invites causal interpretations that the evidence cannot support (2020).

Third, robustness checks should prioritise re-specifications that alter the source of identifying variation rather than merely adding or removing controls. If the estimated association reverses sign when the analyst moves from cross-sectional averages to within-country differences, or if it is substantially attenuated when a single first-order time-varying covariate is included, the reduced-form relationship is not stable in a way that is operationally useful for policy. Recent retractions of observational COVID-19 analyses illustrate how fragile inference can survive peer review and later collapse under methodological scrutiny (2021; 2022).

5 Conclusion

The positive cross-country association between mask usage and excess mortality reported by Tausk and Spira (2025a) reverses sign when the data are differenced within countries between 2021 and 2020, that is, when the regression compares how each country's mask usage and excess mortality moved between the two years. The resulting negative first-differences coefficient itself attenuates toward zero once end-of-2021 vaccination coverage is included, with its p-value rising from 0.010 to 0.098. These two results together show that the published cross-sectional coefficient is not a stable reduced-form relationship. It is consistent with a time-aggregation problem in which persistent between-country differences and shared pandemic shocks are projected onto mask usage, and it does not survive the inclusion of the dominant time-varying driver of 2021 mortality. A causal interpretation of either the cross-sectional or the first-differences coefficient would require restrictive assumptions that this design is unlikely to satisfy; the reversal and attenuation therefore indicate that the original finding should not be exported to policy discussions as evidence about the effect of masking.

Data availability

The analysis combines the replication dataset accompanying Tausk and Spira (2025a); mask usage from IHME's COVID-19 projections (n.d.); excess mortality from the OWID-processed series based on the Economist model (n.d.); and, for column (4) of Table 2, end-of-2021 full-vaccination coverage from the dataset accompanying the original article (2025a). All code and processed data used to produce the tables and figures in this comment are available as a replication package at the ZBW Journal Data Archive for the *Journal of Comments and Replications in Economics*.

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Author contributions

V.R. designed the study, conducted the empirical analysis, and wrote the manuscript.

Competing interests

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Use of AI-assisted tools

AI-assisted tools were used for language editing (grammar and clarity). The analysis, code, and all analytical content are the author's responsibility.

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