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# Acceptability of Hanoi's planned low-emission zone: Rankings, spatial heterogeneity, and entropy sensitivity

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**Abstract:** Municipal governments are introducing low-emission zones (LEZs) to improve environmental quality. Their success depends on both effective regulation and public acceptability. This study examines spatial variation in the pre-implementation acceptability of Hanoi's planned LEZ and whether association-based and entropy-based rankings address the same policy question. Of 685 survey respondents, 648 passed an outcome-blind response-quality screen. The study finds that acceptability was modestly positive (mean 3.286/5), although favourable item responses remained below 50%. Positive affect ($\rho$=0.687), value alignment ($\rho$=0.641), adaptability ($\rho$=0.603), and air-quality concern ($\rho$=0.584) showed the strongest Spearman associations. Positive affect, adaptability, and air-quality concern remained significantly associated with acceptability after adjustment for demographic and mobility characteristics. Acceptability differed across locality groups ($\varepsilon^2$=0.044); residence in a listed ward remained negatively associated with acceptability after adjustment for demographic and mobility characteristics ($\beta$=-0.310), but the association weakened after attitudinal variables were added ($\beta$=-0.088, p=0.194). Spearman and adjusted mutual-information rankings agreed ($r_s$=0.918), whereas entropy rankings changed with normalisation and coding. Targeted engagement should combine credible air-quality evidence with feasible mobility support. The findings indicate associations and are not representative of the population.

***Keywords:*** *Entropy weight, Hanoi, Low-emission zone, Mutual information, Public acceptability, Spatial heterogeneity, Transport equity.*

## 1. Introduction

Low-emission zones restrict or charge higher-emitting vehicles within a defined urban area. Their environmental and health outcomes vary with design, enforcement, fleet response, spatial displacement, and the availability of cleaner transport alternatives [1-4]. These technical conditions are inseparable from public acceptability. A policy that is considered effective but unfair, or beneficial in aggregate but infeasible for affected travellers, can encounter resistance, evasion, delayed implementation, or pressure for exemptions. Conversely, legitimacy, perceived co-benefits, and credible alternatives can create a basis for durable implementation [5-7].

Hanoi is an important setting in which to study this problem. At one urban monitoring site, mean $PM_{2.5}$ was 40.2 $\mu g/m^3$ between September 2019 and December 2020, and primary traffic accounted for 7.6% of the apportioned particle mass while also contributing to oxidative-potential exposure [8]. These figures provide environmental context rather than respondent-level exposure. Local evidence also indicates demand for improved air quality, although willingness to support environmental action depends on household and policy conditions [9]. Resolution No. 57/2025/NQ-HDND established the municipal basis for LEZ implementation, and the city later announced a staged inner-city plan [10, 11].

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The timing makes pre-implementation evaluation central to the research problem. The survey was conducted from 23 April to 8 May 2026, before the detailed operational plan was announced on 30 June 2026 and before initial measures began on 1 July 2026 [11]. Respondents therefore evaluated a proposed policy under uncertainty about its final restrictions, support measures, and enforcement. We use the term acceptability to describe this ex-ante evaluation, distinguishing it from post-implementation acceptance and observed behavioural responses. Identifying this baseline can reveal concerns that require engagement before experience with the policy changes attitudes or travel behaviour.

The Hanoi case also creates a question about spatial variation that a city-wide mean cannot capture. Residents in a listed ward may anticipate restrictions, enforcement, local benefits, or disruption more directly than respondents outside it. Locality can therefore shape evaluation through mobility needs, knowledge, trust, and the credibility of promised co-benefits, although residence is not treated as a causal exposure.

The emerging literature on LEZ acceptability shows that attitudes, norms, affect, trust, expected effectiveness, fairness, and mobility dependence can matter [7, 12-15]. Pre-implementation research additionally shows that support for accompanying measures and policy attributes varies across resident profiles [16, 17]. These findings justify examining both overall acceptability and spatial variation in anticipated exposure. They do not, however, resolve how analytical rankings should be interpreted.

Most rankings in acceptability research are linked to the outcome: coefficients or correlations quantify how perceptions covary with stated acceptability. A different tradition assigns "objective" weights using Shannon entropy. Conventional entropy-weight methods (EWM) give more weight to variables that are more dispersed after normalisation, without conditioning on acceptability [18, 19]. Confusing these outputs can redirect communication or mobility support towards constructs with more variable responses rather than perceptions associated with support. The interpretation of these rankings is therefore a policy issue, not a technical detour from pre-implementation evaluation.

Published LEZ studies have examined attitudes, trust, fairness, mobility dependence, and implementation context, primarily in Europe [7, 12-14, 20]. However, this evidence does not establish whether within-city differences in a pre-implementation Vietnamese setting reflect mobility composition, adjusted mean differences, or differences in attitudinal associations [20-22]. A mean difference cannot distinguish these explanations.

A second gap concerns method interpretation. LEZ studies generally use outcome-linked coefficients, whereas conventional EWM weights outcome-free dispersion [7, 12, 18-20]. The literature has not compared these estimands or tested the stability of EWM rankings under monotone reversal of adverse Likert items. We therefore compare methods and examine spatial differences in sample composition, adjusted differences in acceptability, and moderation without treating locality as randomly assigned.

This distinction motivates four research questions:

RQ1. What is the level and distribution of public acceptability of the planned Hanoi LEZ, and which respondent groups differ in acceptability?

RQ2. Which attitudinal constructs are most strongly associated with acceptability when assessed using monotonic correlation and entropy-based measures of statistical dependence?

RQ3. How and why do rankings from Spearman correlation, normalised mutual information, and conventional entropy weights diverge?

RQ4. How do acceptability, attitudinal profiles, and mobility composition differ across survey-locality groups, and does locality modify the leading attitudinal associations?

The paper makes three bounded contributions. Empirically, it profiles pre-implementation LEZ acceptability and its survey-locality heterogeneity in Hanoi. Methodologically, it distinguishes monotonic association, general statistical dependence, and outcome-free dispersion rather than calling all three "importance". It also evaluates the sensitivity of EWM to endpoint reversal and an alternative

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normalisation. Practically, it translates these evidence types into a decision-support framework for targeted engagement and monitoring. The cross-sectional design does not identify causal policy effects.

The remainder of the paper is organised as follows. Section 2 reviews evidence on LEZ acceptability, spatial heterogeneity, and objective weighting. Section 3 states the analytical framework and estimands. Sections 4 and 5 present the data, methods, and findings, followed by the discussion, limitations, and conclusion.

## 2. Literature Review

### *2.1. Public Acceptability of Restrictive Transport Policy*

Travel-demand-management research distinguishes general environmental concern from policy-specific beliefs. Problem awareness and personal norms can create motivation for intervention, while perceived freedom restrictions, fairness, and effectiveness shape whether a particular instrument is acceptable [5, 6, 23]. LEZ studies extend this framework to trust in authorities, affective reactions, vehicle dependence, expected local benefits, and distributional consequences. In the United Kingdom, attitudes, norms, emotions, and trust jointly accounted for variation in LEZ acceptability [7], and subsequent work quantified the additional importance of socio-psychological factors [12]. Evidence from Barcelona, Madrid, Thessaloniki, and other European settings similarly shows that support is heterogeneous and tied to perceived effectiveness, mobility burden, and political or institutional context [13, 14, 20, 24].

Acceptability can also change as implementation becomes more concrete. Recent evidence describes reluctant or conditional acceptance when health benefits coexist with burden, distrust, or perceived inequity [21]. Reinforcement of an existing LEZ can generate a different political response from the initial adoption [22]. The present study captures a pre-implementation moment and should not be extrapolated to later experience without longitudinal evidence.

Distributional concerns require particular attention. LEZ costs may fall unevenly across vehicle owners, lower-income households, workers with constrained schedules, and people living or travelling near a boundary. Spatially differentiated benefits and exposure can also produce environmental-justice trade-offs [25, 26]. For this reason, an average support score is insufficient. Residence, vehicle ownership, present travel mode, exposure frequency, and willingness to change mode are examined as policy-relevant dimensions of heterogeneity.

### *2.2. Evidence on Spatial and Preference Heterogeneity*

Heterogeneity appears in both stated preferences and observed responses. Distance from a proposed zone, dependent children, and travel constraints provided information beyond psychological predictors in a statistically well-powered acceptability study [12]. A Budapest study identified preference groups associated with car access, age, and employment, while a pre-implementation Grenoble study found six profiles distinguished by environmental effectiveness and social justice concerns [16, 17]. These findings indicate that accompanying measures cannot be designed from a city-wide mean alone.

Post-implementation evidence provides a complementary spatial warning. Mobility responses to London's expanded ultra-low-emission zone showed spatially varying patterns of decline and recovery [27]. Environmental justice assessments likewise show that exposure, benefits, and economic burdens can be distributed unevenly [25, 26]. However, most evidence comes from European settings and does not distinguish whether an ex-ante locality gap reflects differences in sample composition, attitudinal levels, or attitude-acceptability slopes. This unresolved distinction motivates the Hanoi analysis.

### *2.3. Objective Weighting and the Boundary of MCDM*

Objective weighting methods summarize different properties of a decision matrix. EWM uses normalised dispersion, CRITIC combines contrast intensity with conflict between criteria, and MEREC measures how removing a criterion changes the aggregate performance of defined alternatives [19, 28,

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29]. These approaches are valuable when a decision problem contains explicit alternatives, criteria, benefit or cost orientations, and a downstream aggregation or ranking rule. Their weights remain functions of the decision matrix and preprocessing choices rather than estimates of association with an external public-acceptability outcome.

The respondent-by-construct matrix used here does not define an alternative-ranking problem. Treating respondents as policy alternatives and then applying TOPSIS, VIKOR, or a hybrid random-weight ranking would produce a ranking of individuals that has no role in the research questions. Adding CRITIC or MEREC weights would likewise broaden the family of outcome-free summaries without resolving which perceptions covary with acceptability. We therefore retain EWM as the conventional method being evaluated, extend the checks on its sensitivity to normalisation and item polarity, and discuss CRITIC and MEREC as appropriate comparators for future studies that define genuine policy alternatives.

## 3. Analytical Framework

### *3.1. Policy Exposure and Spatially Differentiated Acceptability*

Spatial heterogeneity can enter LEZ acceptability through differences in exposure and proximity to benefits. Residents inside the proposed zone may anticipate compliance costs and enforcement more directly, but may also expect cleaner air and public-space benefits. The direction of a residential difference is therefore not predetermined. Mobility dependence and institutional evaluation provide two further pathways. Acceptability may be lower among travellers with few transport alternatives, while trust, transparency, exemptions, and expected delivery shape policy appraisal [5-7]. Locality should therefore be examined jointly with adaptability and governance rather than interpreted as a fixed preference.

These pathways motivate three complementary spatial tests. An omnibus comparison identifies whether locality groups have different acceptability or construct distributions. A sequential adjusted model assesses whether a binary inside/outside difference is robust to observed demographic and mobility composition and whether it attenuates after leading attitudinal correlates are added. Interaction models then test whether air-quality concern, adaptability, and positive affect have detectably different associations inside and outside the listed wards. This sequence separates level differences from mechanism differences and avoids basing the entire spatial interpretation on a single subgroup test.

The three empirical patterns are not mutually exclusive. Attenuation after adjustment for sample composition is compatible with a compositional explanation; further attenuation after adjustment for attitudes is compatible with evaluative alignment; and interactions are compatible with differences in slopes. Because residence, travel, and attitudes are jointly selected and measured concurrently, these signatures are diagnostic and require longitudinal or quasi-experimental confirmation.

### *3.2. Three Meanings of "importance" in Pre-Implementation Evaluation*

The word "importance" can imply three different policy decisions. An outcome-linked measure can help identify perceptions that accompany acceptability, a dependence measure can flag nonlinear or segmented relationships, and an outcome-free weight can identify variables that differentiate observations under a specified coding rule. These quantities are not interchangeable because they use different information. Mislabelling dispersion as a driver could direct engagement towards a variable that varies widely but has little relationship with acceptability. Table 1 summarizes these distinct analytical roles and the interpretations that each method does and does not support.

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**Table 1.**
Analytical roles of the three method families.

| Method family | Quantity estimated | Appropriate policy use | Invalid interpretation |
|---|---|---|---|
| Spearman correlation | Signed monotonic association with acceptability | Rank perceptions that increase or decrease with support | Causal effect |
| Normalised mutual information | Direction-free statistical dependence with acceptability | Screen nonlinear or segmented dependence | Positive or negative effect without a signed statistic |
| Entropy weight | Relative dispersion after coding and normalisation | Audit response heterogeneity in a defined criterion set | Predictor, driver, or determinant of acceptability |

Let $Y$ denote the public-acceptability composite, and let $X_j$ denote an attitudinal construct. Spearman's rank correlation, $\rho_j$, measures monotonic association. Its sign indicates direction, while $|\rho_j|$ supports an outcome-linked ranking. It is robust to monotone transformations and appropriate for ordered, non-normal scale scores, but it can understate non-monotonic dependence. Mutual information (MI) measures the reduction in uncertainty about one variable associated with knowledge of the other. We use symmetric uncertainty:

$$U(X_j, Y) = 2I(X_j; Y)/(H(X_j) + H(Y)),$$

where H denotes Shannon entropy and I denotes mutual information. This normalized measure is non-negative, has no direction, and can capture non-monotonic dependence [18, 30]. Bias from finite discretization is estimated by permutation.

The conventional EWM answers a different question. After min-max normalisation of $z_{ij}$, the method defines $p_{ij} = z_{ij}/\sum_i z_{ij}$, entropy

$$e_j = -(1/ln\, n) \sum_{i=1}^{n} p_{ij}\, ln\, p_{ij},$$

divergence $d_j = 1 - e_j$, and weight $w_j = d_j/\sum_j d_j$ [19]. No Y appears in these equations. A large $w_j$ means that the coded values provide greater cross-respondent contrast relative to other criteria. It does not mean that $X_j$ predicts, explains, or causes acceptability.

The coding problem follows directly from the normalisation. For a five-point variable reversed according to $x'_{ij} = 6 - x_{ij}$, min-max scaling gives $z'_{ij} = 1 - z_{ij}$. The new proportions are

$$p'_{ij} = (1 - z_{ij})/\sum_i (1 - z_{ij}),$$

which are generally not a permutation or constant multiple of $p_{ij}$. Consequently, $e'_j$, $d'_j$, and $w'_j$ can change even though reversal preserves order and information. Invariance requires specific distributional conditions and cannot be assumed. Our framework therefore treats EWM as an auxiliary measure of heterogeneity with explicit checks of coding sensitivity, while $|\rho|$ and NMI remain the outcome-linked rankings.

## 4. Data and Methods

### *4.1. Study Setting and Policy Timeline*

Hanoi is Vietnam's capital and a dense, highly motorised metropolitan area. The official LEZ announcement reported more than eight million road vehicles and identified traffic as a substantial source of direct emissions [11]. The detailed plan covers nine wards inside Ring Road 1: Hoan Kiem, Cua Nam, Ba Dinh, Giang Vo, Ngoc Ha, Tay Ho, O Cho Dua, Hai Ba Trung, and Van Mieu-Quoc Tu Giam. This setting makes both the anticipated mobility burden and the expected local air-quality benefit relevant to acceptability.

The policy timeline must be distinguished from the survey timeline. The survey closed on 8 May 2026, whereas the detailed plan was approved and announced in June 2026. The subsequent plan specified a first phase in Areas 1 and 2 of Hoan Kiem Ward from 1 July to 31 December 2026, expansion to designated areas of Hoan Kiem and Cua Nam in 2027, and expansion along Ring Road 1 in 2028–2029 [11]. Initial measures varied by vehicle type and time and did not impose a blanket ban on all

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gasoline vehicles across Ring Road 1. The survey is therefore analysed as an evaluation of the policy proposal framed by Resolution 57/2025, not as an evaluation of the later detailed operating rules.

*4.2. Survey Data and Analytic Samples*

The analytical workbook contains 685 respondent records, timestamped between 23 April and 8 May 2026, 47 complete Likert items, and demographic/mobility variables. The locality item recorded residence in one of the nine wards or in an "other" category; it did not identify the physical interview location. The workbook does not document the sampling frame, recruitment channel, response rate, or provenance of the item battery. This information was not available in the analytical dataset.

An outcome-blind response-quality audit was conducted before association testing. Because the 47 items include substantively opposing statements, an identical non-neutral answer to every item produces substantively incompatible endorsements. There were 122 invariant response strings: 85 consisted entirely of neutral responses and were retained because neutrality is not inherently inconsistent; 37 consisted of identical non-neutral responses and were excluded from the primary analysis. Thus, $n = 648$ for the primary estimates. Sensitivity analyses use the full sample ($n = 685$) and a strict sample excluding all invariant strings ($n = 563$). No respondent-level record is exported by the reproducible script.

The primary analytical sample included 481 respondents in the outside/other category and 167 respondents across the nine listed wards. Ward-specific counts were Tay Ho 72, Ngoc Ha 19, Hai Ba Trung 18, Ba Dinh 17, Hoan Kiem 14, Giang Vo 14, O Cho Dua 7, Cua Nam 3, and Van Mieu-Quoc Tu Giam 3. These are self-reported residence counts rather than population-calibrated ward samples. They identify the sampled locality field but do not support ward-level estimates of public opinion.

The primary outcome is the mean of three items measuring overall support, policy acceptability, and willingness to advocate for the LEZ. Explanatory items were grouped according to their wording and checked with Cronbach's alpha and item-rest correlations [31]. Preliminary broad groupings that did not meet the construct-coherence criteria were split before outcome analysis. The resulting multi-item scales had $\alpha = 0.737$-$0.924$; four conceptually narrow indicators remained single items. Higher scores indicate more of the attribute named in each construct label. Constructs expressed in adverse terms retain their original coding in the main analysis. Reliability does not establish unidimensionality or construct validity, and the absence of documented scale provenance prevents the present study from claiming a confirmatory measurement model. The more narrowly defined composites are therefore treated as transparent analytic indicators, and their item mappings are retained in the reproducibility files. The demographic, mobility, locality, and acceptability characteristics of the primary analytic sample are reported in Table 2.

**Table 2.**
Primary-sample profile and outcome ($n = 648$).

| Dimension | Category or statistic | n | % |
|---|---|---|---|
| Age | 18-25 years | 422 | 65.1 |
| | 26-35 years | 45 | 6.9 |
| | 36-50 years | 141 | 21.8 |
| | >50 years | 40 | 6.2 |
| Gender | Women | 395 | 61.0 |
| Employment | Students | 389 | 60.0 |
| Current mode | Motorcycle | 452 | 69.8 |
| | Public transport | 83 | 12.8 |
| | Car | 60 | 9.3 |
| | Walking/cycling | 53 | 8.2 |
| Intends to change mode | Yes | 388 | 59.9 |
| Survey-locality group | Outside/other | 481 | 74.2 |
| | Tay Ho | 72 | 11.1 |

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| | | | |
|---|---|---|---|
| | Other listed pilot wards | 95 | 14.7 |
| Acceptability | Mean (SD) | 3.286 (0.962) | |
| | Median [IQR] | 3.000 [3.000, 4.000] | |
| | Cronbach's α | 0.906 | |

Percentages refer to the primary analytical sample. "Other listed pilot wards" pools the eight listed wards other than Tay Ho because their individual primary-sample sizes were small. Residence is self-reported and not independently geocoded.

*4.3. Statistical Analysis*

We report medians and means for interpretability, but use rank-based primary inference because scale scores are bounded and discrete. For each construct, Spearman's $\rho$ is accompanied by a percentile bootstrap 95% confidence interval (2,000 respondent resamples). Family-wise p-values are adjusted with Holm's sequential procedure [32].

For NMI, each construct and the outcome are discretised by rank quantiles into five bins. Observed symmetric uncertainty is corrected by subtracting the mean under 1,999 permutations of Y, truncated at zero. Permutation p-values and bootstrap intervals are reported. Four- and six-bin specifications are sensitivity checks. The direction of association is assessed using Spearman's $\rho$.

Conventional entropy weights are computed across the 15 explanatory constructs, using 2,000 resample intervals. We calculate min-max weights under natural wording and after the five adverse constructs are mapped to $6 - x$. As a robustness check for normalisation, we also form $a_{ij} = |(x_{ij} - \bar{x}_j)/s_j|$ and apply the same proportion-entropy equations to $a_{ij}$. Absolute values are required because signed z-scores cannot define Shannon proportions. This alternative weights the standardised distance from the mean and is invariant to endpoint reversal. Cross-method rankings are compared with Spearman rank correlation and Kendall's $\tau$.

Subgroup differences are tested with Mann-Whitney tests for two groups and Kruskal-Wallis tests otherwise. Rank-biserial r or epsilon-squared effect sizes are reported, with Holm adjustment across nine comparisons. All constructs are then entered into a standardised ordinary least-squares model with HC3 heteroskedasticity-consistent intervals [33]. A second HC3 model includes age, gender, employment, zone-access frequency, gasoline-motorcycle ownership, car ownership, current mode, and mode-change intention. Income and education were not collected and could not be controlled. Holm adjustment is applied to the 15 focal construct coefficients in both models. These are conditional-association robustness models, not causal models. The analysis was conducted using Python 3.13.15 with seed 20260820; aggregate outputs and verification hashes are supplied.

*4.4. Exploratory Survey-Locality Analysis*

The locality analysis was specified before inspecting locality-outcome results and is exploratory. Because the samples from eight listed wards were small, the primary grouping is outside/other ($n = 481$), Tay Ho ($n = 72$), and the other listed wards pooled ($n = 95$). Inconsistent free-text responses for areas outside the listed wards were not geocoded or used for post hoc grouping.

Acceptability and 15 constructs are compared using Kruskal-Wallis tests, epsilon-squared, and 2,000-resample intervals. Significant omnibus tests are followed by pairwise Mann-Whitney comparisons and rank-biserial intervals. Regional composition is tested for 11 demographic and mobility variables using chi-square and corrected Cramer's V; sparse tables use 1,999 locality-label permutations. Holm adjustment is applied within each stated family.

Sequential HC3 models compare inside with outside/other residences. M0 is unadjusted, M1 adds observed demographic and mobility controls, and exploratory M2 adds air-quality concern, adaptability, and positive affect. The M2 set was selected because these terms retained Holm-adjusted associations in

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the same outcome data, so attenuation is not a prespecified mediation test. The same sequence is repeated for the full and strict samples.

Three M1-adjusted interaction models test whether the leading slopes of the attitude–acceptability associations differ by inside/outside residence. Their p-values are Holm-adjusted, and a nonsignificant interaction is not treated as equivalence. The adjustment set describes observed composition rather than a causal residence effect; all locality outputs are aggregated and recorded with verification hashes.

## 5. Results

### *5.1. Analytic Sample and Inferential Boundary*

The dataset contained 685 complete response records collected from 23 April to 8 May 2026, of which 648 (94.6%) remained after the outcome-blind screen. The 94.6% figure is analytic retention, not a response rate: the sampling frame, recruitment channel, and number invited or approached were not recorded, so participation and response rates cannot be calculated. Because selection probabilities and population weights are unavailable, all results describe the analysed respondents and do not provide population prevalence estimates or representative measures of opinion among Hanoi residents, residents of the pilot wards, or LEZ users.

### *5.2. Level and Heterogeneity of Public Acceptability (RQ1)*

Public acceptability was centred slightly above the scale midpoint (mean 3.286; median 3.000). The composite showed high internal consistency ($\alpha = 0.906$), but favourable responses did not constitute a majority: 41.2% gave favourable responses to the overall-support item, 46.0% rated the policy acceptable, and 41.0% gave favourable responses to the public-advocacy item. This pattern is better described as conditional or unsettled acceptability than broad consensus (Figure 1).

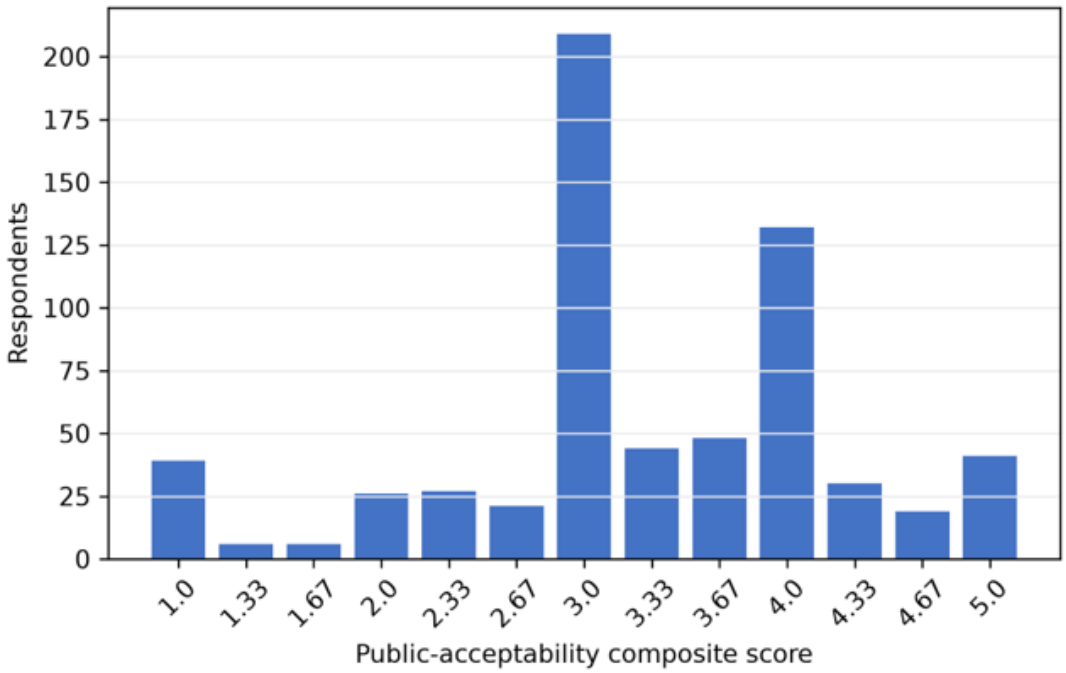



**Figure 1.**
Primary-sample distribution ($n = 648$) of the mean of three five-point public-acceptability items; bars show respondent counts.

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Three subgroup comparisons remained significant after multiplicity control. Respondents intending to change travel mode had higher acceptability (median 3.667, $n = 388$) than those without that intention (median 3.000, $n = 260$; rank-biserial $r = 0.266$, $p_{Holm} = 4.01 \times 10^{-8}$). Respondents classified outside the listed pilot wards had a higher median (3.333, $n = 481$) than those inside (3.000, $n = 167$; $r = 0.258$, $p_{Holm} = 2.89 \times 10^{-6}$). Differences between car-ownership groups were small ($\varepsilon^2 = 0.018$, $p_{Holm} = 0.0288$): respondents reporting more than two gasoline cars had a median of 2.667, compared with 3.333 among those reporting none, but the smallest categories contained few respondents. Age, gender, employment, access frequency, motorcycle ownership, and current mode did not meet the Holm-adjusted significance threshold. These comparisons are descriptive associations; mode-change intention may partly reflect the same favourable disposition measured by the outcome.

### *5.3. Outcome-Linked Association and Dependence (RQ2)*

Positive affect had the strongest monotonic association with acceptability ($\rho = 0.687$, 95% CI 0.631-0.737), followed by value alignment ($\rho = 0.641$, 0.579-0.697), behavioural adaptability ($\rho = 0.603$, 0.537-0.659), air-quality concern ($\rho = 0.584$, 0.519-0.638), perceived co-benefits ($\rho = 0.573$, 0.507-0.637), and governance legitimacy ($\rho = 0.566$, 0.496-0.634). Fairness and expected personal impact were moderately associated. Travel difficulty, policy rejection, perceived air-quality seriousness, and implementation risk had small monotonic associations or associations that were statistically inconclusive after Holm adjustment; see Table 3 and Figure 2.

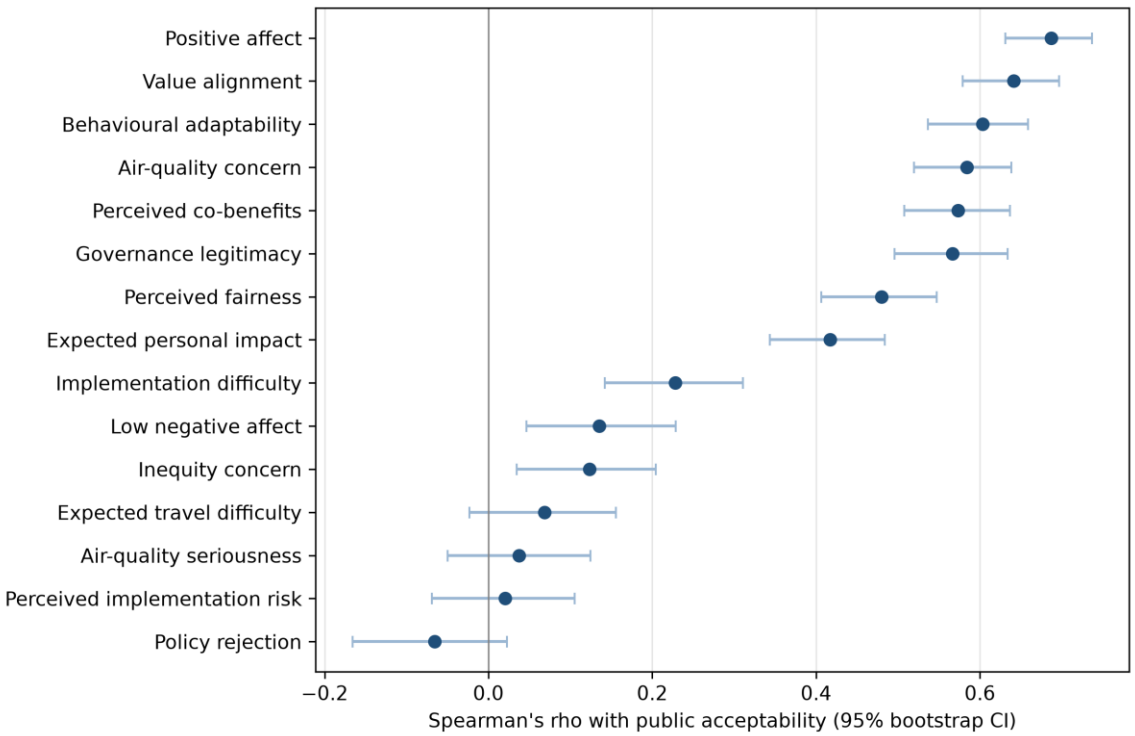



**Figure 2.**
Spearman associations of 15 constructs with primary-sample acceptability ($n = 648$). Points show $\rho$; intervals are 95% confidence intervals from 2,000 respondent bootstrap resamples.

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NMI identified the same leading constructs: positive affect (0.252), value alignment (0.224), adaptability (0.185), governance (0.174), and co-benefits (0.166). The ordering was stable with four or six bins, and the strict sample retained the leaders despite attenuation. All permutation tests met the Holm-adjusted significance threshold, but NMI has no sign and can reflect nonlinear dependence, discrete patterns, or the neutral-response cluster rather than universal substantive importance.

**Table 3.**
Primary-sample outcome-linked and dispersion rankings ($n = 648$); rank 1 denotes the largest method-specific value.

| Construct | ρ | Adj. NMI | Rank \|ρ\| | Rank NMI | EWM natural | EWM favourable | EWM abs.-z |
|---|---|---|---|---|---|---|---|
| Positive affect | 0.687 | 0.252 | 1 | 1 | 6 | 8 | 7 |
| Value alignment | 0.641 | 0.224 | 2 | 2 | 8 | 10 | 8 |
| Behavioural adaptability | 0.603 | 0.185 | 3 | 3 | 3 | 5 | 5 |
| Air-quality concern | 0.584 | 0.135 | 4 | 6 | 14 | 14 | 11 |
| Perceived co-benefits | 0.573 | 0.166 | 5 | 5 | 11 | 12 | 12 |
| Governance legitimacy | 0.566 | 0.174 | 6 | 4 | 12 | 13 | 9 |
| Perceived fairness | 0.480 | 0.134 | 7 | 7 | 7 | 9 | 14 |
| Expected personal impact | 0.417 | 0.101 | 8 | 9 | 15 | 15 | 2 |
| Implementation difficulty | 0.228 | 0.092 | 9 | 11 | 9 | 2 | 15 |
| Low negative affect | 0.135 | 0.111 | 10 | 8 | 5 | 7 | 1 |
| Inequity concern | 0.123 | 0.059 | 11 | 15 | 10 | 4 | 10 |
| Expected travel difficulty | 0.068 | 0.082 | 12 | 12 | 4 | 1 | 13 |
| Policy rejection | −0.066 | 0.099 | 13 | 10 | 2 | 6 | 3 |
| Air-quality seriousness | 0.037 | 0.061 | 14 | 14 | 1 | 3 | 6 |
| Perceived implementation risk | 0.020 | 0.074 | 15 | 13 | 13 | 11 | 4 |

**Note:** NMI is permutation-bias-adjusted symmetric uncertainty with five rank-quantile bins. EWM columns report ranks. "Favourable" reverses five adverse constructs using $6 - x$; "abs.-z" uses absolute z-score normalisation. Spearman, NMI, and absolute-z EWM ranks are reversal-invariant; min-max EWM ranks are not.

*5.4. Why the Entropy Rankings Diverge (RQ3)*

The $|\rho|$ and adjusted-NMI rankings were highly concordant ($r_s = 0.918$, $p = 1.42 \times 10^{-6}$; Kendall $\tau = 0.771$). In contrast, $|\rho|$ was not rank-correlated with natural EWM ($r_s = -0.189$, $p = 0.499$) or favourable EWM ($r_s = -0.396$, $p = 0.143$). Positive affect ranked first under both outcome-linked methods but sixth or eighth under min-max EWM, whereas air-quality seriousness moved from 14th to first under natural EWM. These methods therefore estimate different quantities.

The two min-max EWM rankings were only imperfectly concordant ($r_s = 0.750$). Reversal moved policy rejection from rank 2 to 6, travel difficulty from 4 to 1, implementation difficulty from 9 to 2, and inequity concern from 10 to 4. Absolute-z EWM was reversal-invariant but remained poorly concordant with natural EWM ($r_s = 0.139$), favourable EWM ($r_s = -0.207$), $|\rho|$ ($r_s = -0.200$), and NMI ($r_s = 0.004$). Removing endpoint sensitivity therefore did not create a normalisation-independent or outcome-linked ranking. Figure 3 compares the resulting construct rankings across the five method and normalisation variants.

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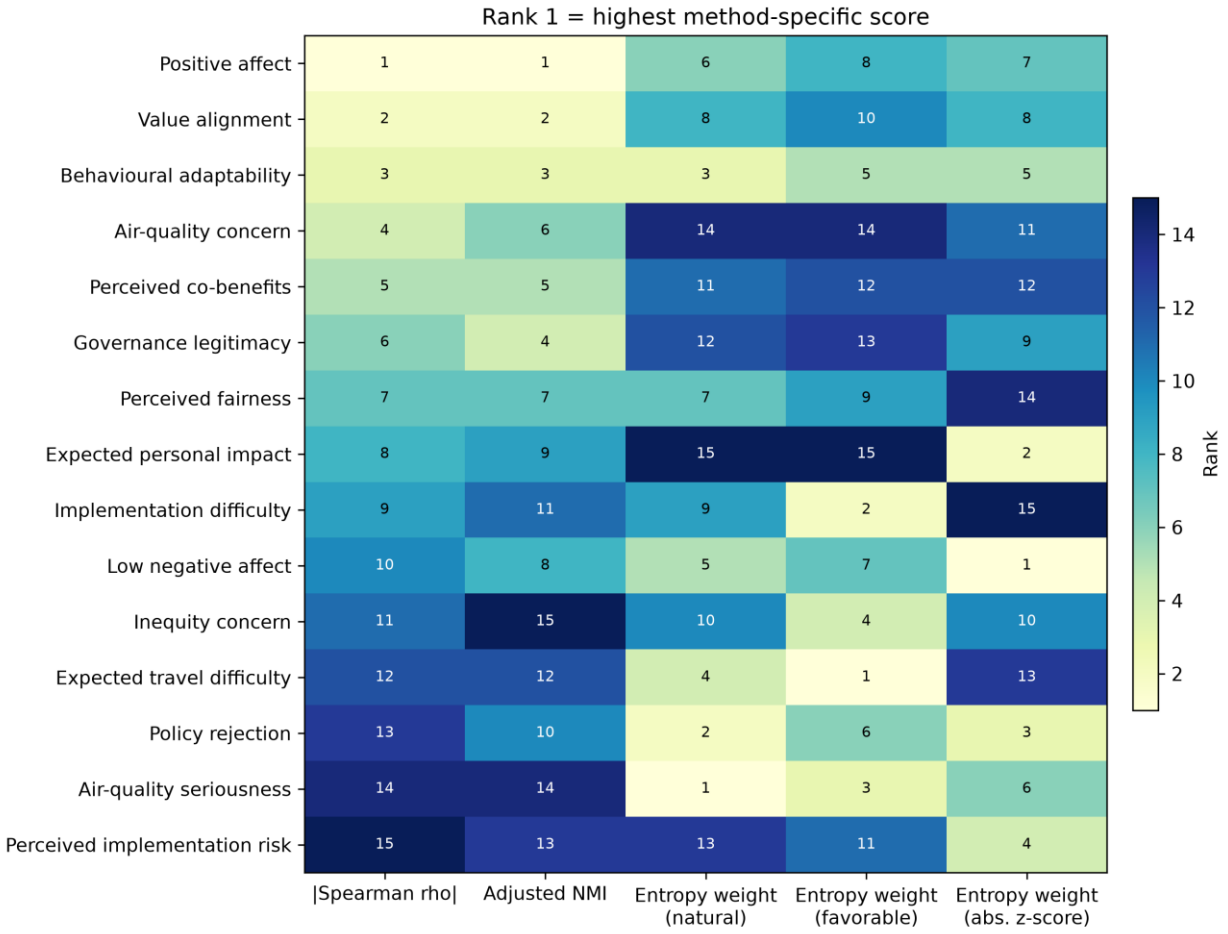



**Figure 3.**
Primary-sample construct ranks ($n = 648$); rank 1 denotes the largest method-specific value. EWM uses natural min-max, favourable min-max, and absolute-z normalisation.

*5.5. Conditional-Association Robustness Model*

The construct-only standardised HC3 model explained 62.0% of outcome variance (adjusted $R^2 = 0.611$). After Holm adjustment for 15 constructs, behavioural adaptability, air-quality concern, and positive affect retained conditional associations. The maximum VIF was 5.29 for governance legitimacy, so the coefficients represent associations conditional on the other overlapping evaluations rather than total effects.

After adjusting for demographic and mobility characteristics, the model identified the same three associations. The estimates were $\beta = 0.270$ for adaptability (95% CI 0.174-0.366, $p_{Holm} = 7.88 \times 10^{-7}$), $\beta = 0.278$ for air-quality concern (0.166-0.391, $p_{Holm} = 2.08 \times 10^{-5}$), and $\beta = 0.280$ for positive affect (0.131-0.429, $p_{Holm} = 0.00321$). Its $R^2$ was 0.633, and its adjusted $R^2$ was 0.609. The observed demographic and mobility composition therefore did not remove these associations, although unmeasured income, education, selection processes, and concurrent measurement prevent causal interpretation.

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*5.6. Survey-Locality Profiles, Composition, and Moderation (RQ4)*

Acceptability differed across the three survey-locality groups ($H(2) = 30.51$, $p_{Holm} = 3.78 \times 10^{-6}$, $\varepsilon^2 = 0.044$, 95% bootstrap CI 0.019-0.080). The outside/other group had a median of 3.333, compared with 3.000 in both Tay Ho and the other listed wards. Pairwise effects showed a larger outside-Tay Ho contrast (rank-biserial $r = 0.372$, 95% CI 0.248-0.486, within-outcome $p_{Holm} = 5.44 \times 10^{-7}$) than the outside-other-listed contrast ($r = 0.172$, 0.040-0.303, $p_{Holm} = 0.0136$). Tay Ho and the other listed wards did not differ significantly after adjustment ($p_{Holm} = 0.0684$).

Eight attitudinal constructs also differed by locality (Table 4 and Figure 4). Co-benefits, air-quality concern, adaptability, and governance had the largest effects ($\varepsilon^2 = 0.036$-0.038); values, positive affect, fairness, and personal impact had smaller effects. No Holm-adjusted difference was detected for measured travel difficulty, rejection, implementation risk or difficulty, air-quality seriousness, low negative affect, or inequity concern. The differences were concentrated in concern about air quality and favourable policy appraisal rather than in measured burden.

**Table 4.**
Constructs differing across survey-locality groups.

| Construct | Outside median | Tay Ho median | Other listed median | $\varepsilon^2$ [95% CI] | $p_{Holm}$ |
|---|---|---|---|---|---|
| Public acceptability | 3.333 | 3.000 | 3.000 | 0.044 [0.019, 0.080] | $3.78 \times 10^{-6}$ |
| Perceived co-benefits | 3.750 | 3.000 | 3.250 | 0.038 [0.015, 0.074] | $2.33 \times 10^{-5}$ |
| Air-quality concern | 3.750 | 3.125 | 3.500 | 0.038 [0.016, 0.071] | $2.33 \times 10^{-5}$ |
| Behavioural adaptability | 3.000 | 3.000 | 3.000 | 0.037 [0.016, 0.075] | $2.79 \times 10^{-5}$ |
| Governance legitimacy | 3.400 | 3.000 | 3.000 | 0.036 [0.013, 0.069] | $4.70 \times 10^{-5}$ |
| Value alignment | 3.500 | 3.000 | 3.000 | 0.031 [0.010, 0.067] | $1.96 \times 10^{-4}$ |
| Positive affect | 3.000 | 3.000 | 3.000 | 0.021 [0.005, 0.051] | 0.00385 |
| Perceived fairness | 3.000 | 3.000 | 3.000 | 0.017 [0.004, 0.044] | 0.0124 |
| Expected personal impact | 3.000 | 3.000 | 3.000 | 0.016 [0.003, 0.045] | 0.0148 |

**Note:** $n = 481$ outside/other, $n = 72$ Tay Ho, and $n = 95$ in the other eight listed pilot wards. P-values are Holm-adjusted across acceptability and 15 explanatory constructs. Equal medians can coexist with a significant rank-distribution difference because multi-item scales contain finer values and the tests use the full rank distributions.

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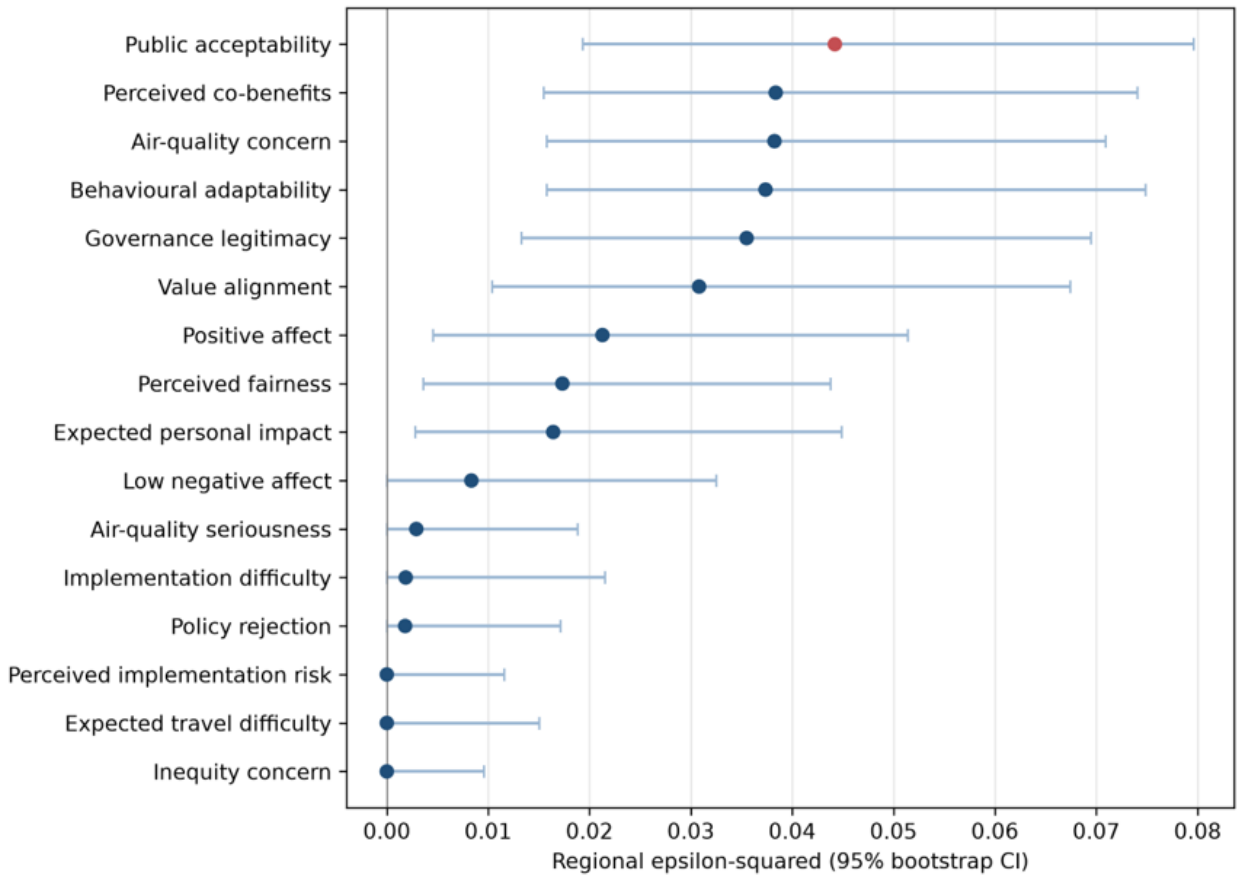



**Figure 4.**
Primary-sample survey-locality differences ($n = 648$: 481 outside/other, 72 Tay Ho, 95 other listed wards). Epsilon-squared compares the three groups; intervals use 2,000 group-stratified bootstrap resamples. Larger values indicate stronger separation; red denotes acceptability.

Locality groups also differed in weekly LEZ access ($V = 0.195$, $p_{Holm} = 1.66 \times 10^{-8}$), trip purpose ($V = 0.164$, permutation $p_{Holm} = 0.005$), and employment ($V = 0.156$, permutation $p_{Holm} = 0.005$). Daily access reached 53.7% in the other listed wards, 36.1% in Tay Ho, and 21.0% outside. Study trips and student respondents were also more prevalent in the two inside groups. Age, gender, current mode, vehicle ownership, mode-choice factor, mode-change intention, and preferred future mode did not meet the Holm threshold (Table 5).

Table 5.
Regional composition and sequential inside/outside models.

| Panel | Test or model | Estimate | 95% CI | Adjusted / inferential **p** |
|---|---|---|---|---|
| A | LEZ access frequency, corrected Cramer's V | 0.195 | - | $1.66 \times 10^{-8}$ |
| | Main trip purpose, corrected Cramer's V | 0.164 | - | 0.005 |
| | Employment, corrected Cramer's V | 0.156 | - | 0.005 |
| B | M0: inside listed wards, unadjusted β | −0.443 | [−0.628, −0.258] | $3.28 \times 10^{-6}$ |
| | M1: demographic and mobility adjusted β | −0.310 | [−0.506, −0.114] | 0.00194 |
| | M2 exploratory: M1 + outcome-informed attitudes β | −0.088 | [−0.222, 0.045] | 0.194 |

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Panel A reports Holm-adjusted values; sparse tables use 1,999-permutation inference. Panel B coefficients express standardised acceptability differences between inside and outside/other residence with HC3 intervals. M1 adjusts for demographic and mobility composition. M2 is an exploratory specification informed by the outcome data; the change in its coefficient is not a confirmatory or causal estimate of mediation.

The inside coefficient remained negative after demographic and mobility adjustment ($\beta = -0.310$, 95% CI $-0.506$ to $-0.114$, $p = 0.00194$). Its magnitude decreased in the outcome-informed M2 specification ($\beta = -0.088$, $p = 0.194$), with the same pattern in the full and strict samples. Interactions for concern, adaptability, and positive affect were small, and all $p_{Holm} = 1.000$.

## 6. Discussion

### *6.1. Substantive Interpretation*

The survey shows neither categorical rejection nor a settled mandate. Mean acceptability was slightly favourable, but item-level favourable shares remained below 50%. This conditional baseline leaves scope for credible implementation details, mobility support, or perceived burden to shift the views of neutral respondents.

Positive affect and value alignment led the bivariate rankings, consistent with evidence that LEZs are judged through values and emotion as well as instrumental beliefs [7, 12]. Adaptability, air-quality concern, and positive affect remained associated after observed demographic and mobility adjustments. Concern can supply a reason for intervention, whereas adaptability reflects perceived feasibility. Concurrent measurement nevertheless leaves open the possibility that general support influences favourable appraisal rather than the reverse.

Governance, co-benefits, fairness, and personal impact had substantial bivariate associations but no statistically significant unique associations after adjustment. Their overlap is compatible with a connected evaluative system, consistent with evidence on effectiveness, fairness, and trust [6, 23]. Communication about aggregate benefits should therefore be paired with credible alternatives and fair implementation.

### *6.2. Interpreting the Spatial Pattern*

The locality analysis clarifies the pattern of lower acceptability observed inside the listed wards. The largest contrast was between outside/other and Tay Ho, while the two inside groups did not differ reliably. Because eight wards were pooled and residence was not geocoded, this is an inside–outside pattern among the sampled respondents rather than a ranking of wards.

Locality differences appeared in concern, co-benefits, adaptability, governance, values, affect, fairness, and personal impact, but not in measured difficulty, rejection, risk, or inequity concern. The pattern is inconsistent with a simple burden-only account. It is compatible with weaker favourable appraisal within the listed area, although the direction of these relationships cannot be established.

Insider groups reported more frequent access, study trips, and student participation. Adjustment reduced the residence coefficient, and the outcome-informed attitude set reduced it further, but this attenuation following variable selection does not establish mediation. The interaction tests provided no evidence of different attitude slopes across the binary locality contrast. Verified residence, repeated observations, and actual travel behaviour are required to separate exposure, composition, appraisal, and reverse causation.

This pattern complements studies that identify distinct preference profiles and support needs before implementation [16, 17]. The Hanoi analysis adds a locality comparison, but its pooled wards and non-probability sample are less spatially resolved than designs based on verified distance or mobility traces. The comparison therefore supports targeted follow-up rather than general claims about spatial differences.

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*6.3. Methodological Implications*

The methodological result is not that one ranking is superior, but that each addresses a different decision question. Spearman and NMI agreed on constructs tied to acceptability, although NMI can capture unsigned, non-monotonic response patterns. It should therefore complement signed effect sizes. Likewise, a locality omnibus statistic identifies heterogeneity but requires sequential models to distinguish differences in composition, appraisal, and slopes.

Conventional EWM measures neither monotonic association nor general outcome dependence. Absolute-z normalisation removed sensitivity to endpoint reversal but produced another divergent ranking, revealing broader preprocessing dependence. CRITIC and MEREC can audit other properties when genuine alternatives and criterion directions exist, but they would remain outcome-free here. Survey applications should disclose coding, normalisation, reversal checks, and the precise quantity being estimated.

*6.4. A Three-Channel Decision-Support Framework*

The findings support three complementary dashboard channels that serve distinct purposes. An acceptability channel tracks the outcome, signed associations, adjusted estimates, and change over time. A dependence channel uses bias-corrected NMI to screen nonlinear patterns. A heterogeneity channel reports dispersion estimates accompanied by coding-sensitivity checks, spatial profiles, mobility composition, and effect-size intervals.

For Hanoi, air-quality communication should be paired with specific information on alternative routes, travel costs, and travel times. Engagement in the listed wards should address gaps in concern, co-benefit expectations, and governance confidence rather than assume a burden-only explanation. The higher proportions of daily users and study-related trips support engagement with frequent users and educational institutions. Repeated measurement should link verified residence with mode use, exemptions, enforcement, air quality, and distributional outcomes.

Applications in other Vietnamese cities should use the framework rather than directly transferring Hanoi's coefficients. Motorcycle dependence, public transport, street form, and enforcement capacity can change both baseline acceptability and adaptability. A common core instrument with documented local modules could support hierarchical, multi-city validation.

## 7. Limitations and Research Agenda

Several limitations constrain inference. First, the workbook does not identify a probability sampling frame, recruitment process, response rate, or population weights. Respondents aged 18-25 accounted for 65.1% of the primary sample, and students accounted for 60.0%. This concentration may distort both the acceptability distribution and observed mobility relationships, while adjustment cannot repair unknown survey selection. Post-stratification requires a documented recruitment mechanism and valid population margins. Income and education were unavailable, and employment is only a partial socioeconomic control. The estimates are therefore not population prevalence estimates for Hanoi, and external validity to other cities is untested.

The secondary air-pollution evidence also has a narrow role. The reported $PM_{2.5}$ concentration and source shares came from one urban site during 2019-2020 rather than the 2026 survey period [8]. No monitoring value was linked to a respondent or locality group. These data establish environmental context but cannot explain individual acceptability or regional differences.

Second, all measures are concurrent self-reports. Common-method variance, consistency motives, and reverse causation can inflate associations; mode-change intention is particularly close to the policy outcome. The sequential locality models are not a mediation design because temporal order, unmeasured confounding, and causal identification are absent. The attenuation after adding attitudes should therefore be used to generate mechanisms for longitudinal testing, not to prescribe attitude change as a proven intervention.

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Third, construct provenance, translation procedures, pilot testing, and formal validation information were absent. Internal consistency was high for the retained multi-item scales, but alpha is not evidence of unidimensionality, and single-item indicators contain more measurement error. A confirmatory ordinal measurement model, omega estimates, and tests of measurement invariance should precede strong cross-group comparisons. Such a model was not retrofitted because the scale-development record needed to distinguish confirmatory from exploratory specifications was unavailable. The comparisons of constructs across localities should consequently be interpreted as comparisons of transparent composite indicators.

Fourth, 17.8% of respondents used an invariant 47-item response string. The primary rule removed only logically contradictory non-neutral strings and retained all-neutral responses, but no rule can establish respondent intent. Sensitivity samples reduce, rather than eliminate, uncertainty about response styles. The binary locality coefficient followed the same sequence in the full and strict samples, which supports the robustness of the results to this rule but does not validate every individual response.

Fifth, subgroup and locality tests are exploratory, and some groups are small. Eight listed wards were pooled, while the outside free-text field was too inconsistent for defensible granular geocoding. The analysis therefore cannot estimate individual ward effects or spatial gradients. Multiple-testing control limits the risk of false positives but cannot remove uncertainty arising from small groups. Larger stratified samples should oversample each listed ward and frequent outside users, verify residence at an appropriate privacy-preserving resolution, and predefine the relevant spatial contrasts.

Sixth, NMI depends on discretization and can capture response clusters without revealing direction or mechanism. Permutation correction and alternative bin counts mitigate bias in finite samples but do not create a causal interpretation. Seventh, min-max entropy weighting is sensitive to endpoint reversal, while the absolute-z alternative, which is invariant to reversal, produced a substantially different ranking. Entropy weights should not be transported across instruments without harmonized coding, a declared normalisation, and sensitivity audits. Finally, the survey precedes implementation. Longitudinal or quasi-experimental evidence is needed to study behaviour, effectiveness, spillovers, and equity.

## 8. Conclusion

Pre-implementation public acceptability of Hanoi's planned low-emission zone was modestly positive but far from consensus. Positive affect, value alignment, behavioural adaptability, air-quality concern, perceived co-benefits, and governance legitimacy had the strongest bivariate associations. Adaptability, concern, and positive affect remained associated with acceptability in the adjusted model. Respondents intending to change modes were more favourable, while sampled respondents in the listed pilot wards were less favourable.

The spatial extension shows that this gap was concentrated in air-quality concerns, co-benefits, adaptability, governance, values, affect, fairness, and personal impact rather than measured difficulty, rejection, risk, or inequity concerns. It remained after demographic and mobility adjustments. Its attenuation in exploratory, outcome-informed M2 generates a hypothesis rather than confirming mediation. No moderation by locality was detected. These results support differentiated engagement intensity and local evidence, but not different causal mechanisms or ward-level rankings.

The comparison of methods changes the interpretation of entropy. Spearman correlation and normalised mutual information produced highly concordant outcome-linked rankings. Min-max entropy changed under endpoint reversal, whereas absolute-z entropy was invariant but yielded another divergent ranking. NMI can screen dependence, and EWM can flag normalisation-dependent dispersion, but neither estimates causal importance. Behavioural association, dependence, spatial composition, and dispersion should remain distinct and be validated with representative post-implementation data.

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## Transparency:

The authors confirm that the manuscript is an honest, accurate, and transparent account of the study; that no vital features of the study have been omitted; and that any discrepancies from the study as planned have been explained. This study followed all ethical practices during writing.


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