

Intergenerational Economic Mobility among Immigrants' Children: A Systematic Review and Exploratory Cross-National Synthesis

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Abstract

This paper presents the first systematic meta-analysis linking immigration policy architecture to the intergenerational economic mobility of immigrants' children. Drawing on 51 observations from 31 studies across 12 countries (1960–2020), we employ weighted least squares meta-regression with inverse-variance weights and cluster-robust (CR2) standard errors to estimate how three policy dimensions, immigrant selection, statutory stabilization, and establishment (settlement) programs, shape intergenerational income elasticities. We find that establishment policies robustly reduce intergenerational persistence by 0.258 points ($p = 0.003$), indicating substantially higher mobility. Immigrant children exhibit lower persistence ($\gamma = 0.259$) than the general population (0.328), consistent with an "immigrant advantage." National income inequality, measured by the Gini coefficient, is positively associated with persistence, extending the Great Gatsby Curve to immigrant populations. Our findings suggest that settlement and integration programs are the most effective policy lever for enhancing intergenerational mobility.

JEL Codes: J15, J62, J68, I24

I. Introduction

Intergenerational economic mobility, the degree to which a child's economic position is independent of that of her parents, stands as one of the most consequential metrics of economic opportunity in modern societies. For immigrant-receiving nations, mobility assumes additional significance: it captures the long-run success or failure of integration processes that unfold across generations. A country may admit immigrants under favorable or restrictive terms, may invest heavily or minimally in settlement services, and may tolerate wide or narrow income distributions. Each of these institutional choices shapes the opportunity structure confronting the children of immigrants. Yet despite decades of research on both immigration policy and intergenerational mobility, the systematic connection between the two remains surprisingly underexplored.

This paper asks a direct question: How do immigration policy regimes shape the intergenerational economic mobility of immigrants' children, and how does this association vary with national inequality levels? We distinguish three dimensions of immigration policy architecture. Selection policies, such as points-based systems, determine the initial human capital composition of the immigrant population. Statutory stabilization policies, governing access to permanent residence and long-term legal security, shape the investment horizon of immigrant families. Establishment policies, including publicly funded settlement and integration programs, provide direct resources for skill development, language acquisition, and institutional access. We argue that each dimension operates through distinct mechanisms on human capital transmission across generations, and that national inequality moderates these effects.

To answer this question, we conduct the first systematic meta-analysis linking immigration policy architecture to quantitative measures of intergenerational mobility. Our evidence base comprises 51

observations drawn from 31 studies spanning 12 countries over the period 1960–2020. The primary outcome variable is the intergenerational income elasticity (IGE), which captures the persistence of economic status across generations: higher values indicate lower mobility. We supplement the IGE analysis with rank-rank slope estimates where available. Our meta-regression employs weighted least squares with inverse-variance weights, a multilevel random-effects structure nested by study, and cluster-robust (CR2) standard errors with Satterthwaite degrees of freedom to account for within-study correlation.

Our analysis yields three principal findings. First, establishment policies, settlement and integration programs that provide language training, credential recognition, and labor market support, are robustly associated with lower intergenerational persistence. Countries with active establishment programs exhibit IGE values approximately 0.258 points lower than those with only implicit immigration frameworks ($p = 0.003$ under CR2 inference). This effect is substantively large: it implies a shift from a high-persistence regime comparable to the United States ($IGE \approx 0.45$) to a moderate-persistence regime more characteristic of Western Europe ($IGE \approx 0.20$). Second, immigrant children display systematically lower intergenerational persistence ($\gamma = 0.259$) than the general population ($\gamma = 0.328$), confirming the "immigrant advantage" documented by Abramitzky et al. (2021) in a broader cross-national context. Third, national income inequality, measured by the Gini coefficient, is positively associated with intergenerational persistence ($+ 0.007$ per Gini point, $p = 0.074$), extending the logic of the Great Gatsby Curve (Corak 2013) to immigrant populations. A significant interaction between stabilization policies and inequality ($p < 0.001$) suggests that the mobility-enhancing potential of legal security is especially pronounced in more unequal societies.

This study contributes to several literatures. The extensive body of work on intergenerational mobility, building on Becker and Tomes (1979, 1986) and Solon (1992, 1999), has established stylized facts about cross-country variation in mobility levels and documented the negative association between inequality and mobility known as the Great Gatsby Curve (Corak 2013; Chetty et al. 2014). A separate literature on immigrant integration has demonstrated the remarkable upward mobility of the second generation, particularly in North America (Abramitzky et al. 2021; Aydemir, Chen, and Corak 2009; Adnan, Zhang, and Zheng 2023). A third stream of research examines how specific policies, citizenship access, refugee resettlement, family reunification, affect immigrant outcomes (Gathmann and Keller 2018; Boustan et al. 2025). Our contribution is to synthesize these literatures through a formal meta-analytic framework that allows systematic comparison of policy effects while controlling for methodological heterogeneity and national context.

The meta-analytic approach offers several advantages over individual country studies. By pooling estimates across diverse institutional settings, we achieve greater statistical power for detecting policy effects and can separate the influence of policy from country-specific confounds. The inclusion of methodological moderators, data source type, lifecycle corrections, measurement error adjustments, allows us to assess the sensitivity of mobility estimates to design choices. Publication bias diagnostics,

including funnel plots, Egger regression, and trim-and-fill analysis, provide transparency about the reliability of the evidence base.

The policy implications of our findings are direct. At a time when immigration debates in many countries focus disproportionately on selection criteria and border enforcement, our results suggest that what happens after admission matters at least as much for long-run outcomes. Investment in settlement infrastructure, language programs, credential recognition, labor market integration services, appears to yield substantial returns in the form of enhanced intergenerational mobility for immigrant families. These returns are especially relevant in high-inequality contexts, where the absence of such programs may allow economic disadvantage to perpetuate across generations.

The remainder of the paper is organized as follows. Section II develops the theoretical framework linking immigration policy to intergenerational mobility and derives testable hypotheses. Section III describes the data, search strategy, variable construction, and econometric methods. Section IV presents the main results, including descriptive patterns, meta-regression estimates, heterogeneity analysis, and robustness checks. Section V discusses the implications and limitations. Section VI concludes.

II. Theoretical Framework and Hypotheses

We ground our analysis in the Becker-Tomes model of intergenerational transmission (Becker and Tomes 1979, 1986), extended to incorporate the institutional context of immigration. In the canonical model, parental income y_t determines the child's income y_{t+1} through investments in human capital and through direct transmission of endowments. The intergenerational income elasticity β captures the degree of persistence: $\beta = 0$ implies complete mobility, while $\beta = 1$ implies perfect reproduction of economic status. In the Becker-Tomes framework, β depends on the rate of return to human capital, the heritability of endowments, and the degree to which credit constraints limit parental investment.

For immigrant families, the standard model requires modification along at least three dimensions. First, the initial placement of immigrant parents in the income distribution may not reflect their latent human capital, creating a wedge between observed income persistence and the true transmission of productive capacity (Abramitzky et al. 2021). Second, immigration policy structures the legal and institutional environment within which human capital investments occur, effectively altering the budget constraint and return-to-investment parameters facing immigrant families. Third, the relationship between national inequality and mobility, the Great Gatsby Curve, may operate differently for immigrant populations whose structural position in the economy is mediated by policy.

We conceptualize immigration policy as operating through three distinct mechanisms, each corresponding to a phase of the immigration and integration process.

A. Selection Policies

Selection policies, typified by points-based immigration systems in Canada and Australia, determine the initial composition of the immigrant population along dimensions of human capital, education, language proficiency, occupational skills, and age. By admitting immigrants with higher average human capital, selection policies raise the starting position of immigrant families in the income distribution. The effect on intergenerational persistence, however, is theoretically ambiguous.

On one hand, higher parental human capital may facilitate greater investment in children's education and development, lowering persistence by enabling upward mobility from any starting position. On the other hand, if selection compresses the lower tail of the parental income distribution, the measured IGE may rise mechanically, since the strongest upward mobility is typically observed among children from the lowest-income families. Moreover, the phenomenon of "brain waste", the underemployment of skilled immigrants due to credential non-recognition and labor market barriers, means that initial human capital advantages may not translate directly into income advantages for the first generation (Connolly, Corak, and Haeck 2019). The net effect of selection on measured intergenerational persistence is therefore an empirical question.

Hypothesis 1: Selection policies have an ambiguous effect on intergenerational persistence. We test for a significant association without specifying its sign.

B. Statutory Stabilization Policies

Statutory stabilization refers to the ease and speed with which immigrants can obtain permanent residence status and, subsequently, citizenship. Legal security fundamentally alters the investment horizon of immigrant families. Temporary or precarious legal status creates uncertainty about the duration of stay, discouraging long-term investments in location specific human capital, homeownership, and children's education (Boustan et al. 2025). Permanent residence resolves this uncertainty, aligning the family's planning horizon with the full lifecycle of the child.

The Becker-Tomes framework predicts that extending the effective investment horizon should increase optimal human capital investment, thereby enhancing intergenerational mobility. When parents expect to remain permanently, they are more likely to invest in host-country language skills, pursue credential recognition, and enroll children in educational programs whose returns accrue over the long run. Access to citizenship further opens public-sector employment, financial aid for higher education, and full participation in social insurance systems (Gathmann and Keller 2018). These channels all reduce the transmission of economic disadvantage across generations.

Hypothesis 2: Statutory stabilization policies (ease of obtaining permanent residence) are negatively associated with intergenerational persistence, indicating higher mobility.

C. Establishment Policies

Establishment policies encompass government-funded settlement and integration programs that provide direct resources to immigrant families: language training, employment assistance, housing

support, credential recognition services, and targeted educational programs for children. Unlike selection and stabilization, which operate indirectly through the composition and incentive structure of the immigrant population, establishment policies directly augment the human capital production function.

In the extended Becker-Tomes framework, establishment policies effectively relax the budget constraint facing immigrant families by subsidizing investments that would otherwise be prohibitively costly. Language programs reduce the communication barriers that limit parental labor market participation and engagement with children's schools. Credential recognition programs reduce brain waste, allowing parental human capital to be fully reflected in earnings. Early childhood education programs for immigrant children provide developmental inputs that complement family investments. Each of these channels reduces the intergenerational transmission of disadvantage.

The theoretical prediction for establishment policies is unambiguous: by augmenting investment in human capital and reducing friction in its transmission, settlement programs should lower intergenerational persistence.

Hypothesis 3: Establishment policies (settlement and integration programs) are negatively associated with intergenerational persistence, indicating higher mobility.

D. The Moderating Role of Inequality

The Great Gatsby Curve, documented by Corak (2013) and confirmed with U.S. microdata by Chetty et al. (2014), establishes a positive cross-country correlation between income inequality and intergenerational persistence. Several mechanisms underlie this relationship: higher inequality implies greater returns to parental investment (and greater penalties for under-investment), more residentially segregated communities, and more stratified educational systems, all of which increase the transmission of economic status across generations.

For immigrant populations, inequality may interact with policy in important ways. In high-inequality settings, the returns to establishment programs may be especially large, as these programs substitute for the public goods and safety nets that are typically weaker in unequal societies. Conversely, stabilization may matter most in unequal contexts where the penalty for legal precarity, exclusion from formal employment, limited access to credit, is amplified by wide income gaps. We therefore expect inequality to moderate the effects of policy on mobility.

Hypothesis 4: National income inequality is positively associated with intergenerational persistence, and the effects of stabilization and establishment policies are stronger in more unequal societies.

III. Data and Methods

A. Systematic Search Strategy

We conducted a systematic search of the academic literature following PRISMA guidelines (Page et al. 2021). Four electronic databases were searched: EconLit, Google Scholar, Scopus, and Web of Science. The search string architecture combined three concept groups using Boolean operators: (1) immigration policy terms ("immigration policy" OR "points system" OR "settlement program" OR "permanent residence" OR "citizenship access" OR "refugee policy" OR "family reunification"); (2) mobility terms ("intergenerational mobility" OR "intergenerational elasticity" OR "income persistence" OR "rank-rank slope" OR "economic mobility"); and (3) population terms ("immigrant" OR "second generation" OR "children of immigrants" OR "foreign-born"). Searches were conducted through June 2026.

We supplemented database searches with backward citation tracking from key review articles (Corak 2006; Solon 1999; OECD 2018) and forward citation searches on seminal studies (Chetty et al. 2014; Abramitzky et al. 2021). We also examined working paper series from the National Bureau of Economic Research (NBER), the Institute of Labor Economics (IZA), and Statistics Canada.

Inclusion criteria required that studies: (a) provide quantitative estimates of intergenerational economic mobility (IGE, rank-rank slope, or comparable measures); (b) pertain to immigrant populations or include them in cross-country analyses where policy context is identifiable; (c) cover OECD or high-income countries with identifiable immigration policy regimes; and (d) be published in peer-reviewed journals or as working papers from recognized research institutions. We excluded qualitative studies, studies focused exclusively on educational mobility without income measures, and studies whose effect sizes could not be meaningfully compared with IGE or rank-rank metrics.

The search process identified 847 unique records. After title and abstract screening, 126 full-text articles were assessed for eligibility. Of these, 31 studies met all inclusion criteria, yielding 51 individual observations. The primary reasons for exclusion at the full-text stage were: absence of quantitative mobility estimates ($k = 42$), non-comparable effect size metrics ($k = 31$), and insufficient information for policy coding ($k = 22$).

B. Sample Characteristics

The final analytical sample comprises 51 observations from 31 studies covering 12 countries: Australia, Canada, Denmark, Finland, France, Germany, Italy, the Netherlands, Norway, Sweden, the United Kingdom, and the United States. The temporal coverage spans observation periods from 1960 to 2020, with the median observation year at approximately 1995. By effect-size family, the sample includes 27 intergenerational income elasticity (IGE) estimates, 16 rank-rank slope estimates, and 8 observations using other mobility metrics (earnings gaps, transition probabilities, exposure effects).

Table 1 presents descriptive statistics for the key variables. The mean mobility estimate across all observations is 0.318 (SD = 0.129), with a range from 0.08 (Denmark, Jäntti et al. 2006) to 0.653 (United States, Bohn and Lopez-Velasco 2018). The mean Gini coefficient is 31.4, reflecting the sample's concentration in OECD countries with moderate inequality. Twenty-two percent of observations come

from countries classified under selection policies, 27% under stabilization, 8% under establishment, and 43% under implicit (default) policy regimes.

Table 2 presents the complete list of included studies with their key characteristics. The sample draws heavily on North American studies (United States: 21 observations; Canada: 11), reflecting the depth of the mobility literature in these countries. European contributions include Nordic countries (Norway: 5; Sweden: 4; Denmark: 2; Finland: 1), Continental and Southern Europe (Germany: 3; France: 1; Italy: 1; Netherlands: 1; United Kingdom: 3), and Australia (1). Data sources are roughly evenly divided between administrative records (26 observations) and surveys (23), with 2 based on synthesis or other methods.

Table 1 here

Table 2 presents the complete list of 31 studies included in the meta-analysis with extracted data. Due to space constraints, the full table is presented in Appendix C. Key studies include Chetty et al. (2014) providing rank-rank slopes from U.S. tax records with over 10 million observations; Abramitzky et al. (2021) covering both historical (1880–1940) and modern (1980–2015) immigrant cohorts in the United States; Adnan, Zhang, and Zheng (2023) comparing refugee and economic immigrant children in Canada; and Boustan et al. (2025) providing comparable rank-rank estimates for immigrant children across 15 destination countries.

Table 2 here

C. Variable Construction

The dependent variable in our meta-regression is the harmonized mobility coefficient y_{h} , where higher values indicate greater intergenerational persistence (lower mobility). The primary analysis restricts attention to intergenerational income elasticity (IGE) estimates ($k = 27$), which measure the percentage change in child income associated with a one-percent change in parental income. Secondary analyses incorporate rank-rank slopes ($k = 16$), which capture the change in the child's percentile rank associated with a one-percentile change in parental rank. When pooling across effect families, we include an indicator variable for effect type.

Immigration policy is captured by three binary indicators coded at the country level based on institutional histories and policy classifications. Selection equals one for countries operating formal points-based or skills-weighted immigration systems (Australia, Canada). Stabilization equals one for countries providing relatively easy access to permanent residence within the first five years of arrival (Nordic countries: Denmark, Finland, Norway, Sweden). Establishment equals one for countries with comprehensive, publicly funded settlement and integration programs, typically including government-funded language instruction, credential recognition services, and structured labor market integration support (Denmark, Netherlands, and countries with active settlement mandates). These coding rules follow the typology developed by Bjerre et al. (2015) and the MIPEX classification framework, adapted to the temporal context of each observation. We note that Canada is coded as having all three policy dimensions active,

reflecting its comprehensive immigration framework combining points-based selection, accessible permanent residence, and extensive settlement services.

National inequality is measured by the Gini coefficient for disposable household income, sourced from the OECD Income Distribution Database and the World Bank. We match each observation to the country-decade Gini value corresponding to the median observation year. The Gini coefficient is mean centered in all regressions to facilitate interpretation of main effects.

Methodological controls include data source type (administrative versus survey), the application of lifecycle corrections to address age-earnings profile biases (Haider and Solon 2006), and measurement error corrections. These variables capture systematic differences in mobility estimates attributable to research design rather than substantive variation.

D. Meta-Regression Model

We estimate weighed least squares meta-regressions of the form:

$$y_i = \alpha + \beta_1 \text{Selection}_i + \beta_2 \text{Stabilization}_i + \beta_3 \text{Establishment}_i + \delta \text{Gini}_i + X_i'\gamma + u_i + \varepsilon_i$$

where y_i is the mobility estimate from observation i , the policy indicators and Gini coefficient are defined above, X_i is a vector of methodological controls, u_i is a study-level random effect, and ε_i is the observation-level residual. Observations are weighted by the inverse of their sampling variance ($w_i = 1/SE_i^2$), giving greater influence to more precisely estimated effect sizes.

The random-effects structure nests observations within studies using the multilevel specification in the metafor package for R (Viechtbauer 2010). This accounts for the non-independence of multiple observations drawn from the same study, same author team, or same dataset. Standard errors are computed using the CR2 bias-reduced linearization estimator (Tipton and Pustejovsky 2015) with Satterthwaite degrees of freedom, which provides reliable inference even with a small number of clusters (here, 31 study clusters).

We estimate a sequence of models of increasing complexity: (M1) random-effects intercept only; (M2) policy indicators only; (M3) policy plus Gini; (M4) policy, Gini, and policy $\times$ Gini interactions; (M5) full model with methodological controls; and (M6) a trimmed model excluding outliers beyond two standard deviations. Model M3 serves as our preferred specification, balancing parsimony with the inclusion of theoretically motivated moderators.

E. Publication Bias and Robustness

We assess publication bias through multiple complementary approaches. First, we visually inspect the funnel plot of effect sizes against their standard errors for asymmetry. Second, we conduct the Egger regression test (Egger et al. 1997), which regresses standardized effect sizes on precision and tests whether the intercept differs significantly from zero. Third, we implement the trim-and-fill method (Duval

and Tweedie 2000), which estimates the number of "missing" studies needed to restore funnel symmetry and computes an adjusted pooled estimate.

Robustness is assessed through several additional analyses. Jackknife (leave-one-out) sensitivity analysis iteratively removes each study and re-estimates the pooled effect to identify influential observations. We conduct subgroup analyses by geographic region (North America, Northern Europe, Continental/Southern Europe) and by data source type (administrative versus survey). Finally, we estimate separate models for IGE and rank-rank slope effect families to assess whether findings generalize across measurement approaches.

IV. Results

A. Descriptive Patterns

Figure 1 presents the forest plot of IGE estimates ($k = 27$). The pooled random-effects estimate is 0.328 (95% CI: [0.274, 0.381]), with substantial heterogeneity ($I^2 = 93.5\%$, $Q = 349.67$, $p < 0.001$). The high I^2 indicates that over 93% of the observed variance in effect sizes reflects genuine between-study differences rather than sampling error, strongly motivating the meta-regression approach to identify systematic moderators.

Several descriptive patterns emerge from the data. First, there is marked cross-country variation: Nordic countries (Denmark, Finland, Norway, Sweden) cluster at the low end of the persistence distribution (pooled IGE = 0.188, $k = 5$), while the United States and Southern Europe cluster at the high end (pooled IGE = 0.377 for North America, $k = 15$). Second, administrative data sources yield systematically lower persistence estimates (0.215, $k = 9$) than surveys (0.402, $k = 17$), likely reflecting the attenuation bias inherent in survey-based income measures. Third, and most relevant for our research question, immigrant children exhibit markedly lower persistence (0.259, $k = 14$) than the general population (0.329, $k = 28$), a pattern consistent with the "immigrant advantage" documented by Abramitzky et al. (2021). This 0.070-point gap suggests that the children of immigrants experience approximately 21% greater mobility than the children of native-born parents.

Figure 1 here

B. Main Meta-Regression Results

Table 3 presents the results of our meta-regression models for the IGE sample ($k = 27$). We discuss the preferred specification (Model 3: Policy + Gini) in detail before turning to alternative specifications.

The intercept in Model 3 is 0.404 (SE = 0.021, $p < 0.001$), representing the predicted IGE for a country with implicit immigration policy (the omitted category) at the mean Gini level. This baseline is close to the observed IGE for the United States and Continental Europe, countries that lack the comprehensive policy architectures captured by our indicators.

Establishment policies yield the strongest and most robust coefficient. Countries with active settlement and integration programs exhibit an IGE that is 0.258 points lower than the baseline (SE = 0.072, $p = 0.0003$; CR2: SE = 0.039, $p = 0.003$). The magnitude of this effect is striking: it implies that comprehensive establishment programs are associated with a shift from a high-persistence regime (IGE ≈ 0.40) to a moderate-to-low persistence regime (IGE ≈ 0.15), comparable to the difference between the United States and Denmark. This finding strongly supports Hypothesis 3 and aligns with theoretical predictions that direct investment in immigrant human capital reduces the intergenerational transmission of disadvantage.

Statutory stabilization shows a positive coefficient of 0.118 (SE = 0.059, $p = 0.045$; CR2: SE = 0.041, $p = 0.055$), which is contrary to the direction predicted by Hypothesis 2. However, this result requires careful interpretation. The stabilization indicator is coded primarily for Nordic countries (Norway, Sweden, Finland), which combine easy access to permanent residence with comprehensive welfare states. The positive coefficient may therefore capture the confounding effect of measurement context, Nordic countries tend to use administrative data, which yields lower baseline persistence estimates, creating a compositional effect that our indicator absorbs. When interactions with Gini are introduced (Model 4), the stabilization main effect becomes negative (-0.193 , $p = 0.032$), and the significant interaction (-0.042 , $p < 0.001$) reveals that stabilization is most strongly associated with enhanced mobility in higher-inequality settings. This pattern is consistent with Hypothesis 2 when inequality context is taken into account.

Selection policies show a negative but statistically insignificant coefficient (-0.067 , SE = 0.046, $p = 0.147$; CR2: $p = 0.196$), consistent with the theoretical ambiguity articulated in Hypothesis 1. Points-based systems do not appear to significantly alter intergenerational persistence relative to implicit policy regimes, once Gini and other factors are controlled. This null finding echoes the cross-country evidence of Boustan et al. (2025), who find that destination-country characteristics dominate origin-country selection in explaining immigrant children's mobility.

The Gini coefficient enters with a positive and marginally significant association ($+0.007$, SE = 0.004, $p = 0.078$; CR2: $p = 0.074$). A ten-point increase in the Gini, roughly the difference between Sweden (22.5) and the United States (38), is associated with a 0.07, point increase in IGE, an economically meaningful magnitude. This finding extends the Great Gatsby Curve to our cross-national sample and is consistent with Hypothesis 4.

Table 3 here

Figure 2 presents the Great Gatsby Curve for our sample, plotting IGE estimates against the Gini coefficient, with observations colored by policy type. The positive slope is evident, but the figure reveals important policy-driven deviations. Countries with establishment policies (Denmark, Netherlands) lie well below the regression line, achieving mobility levels that their inequality alone would not predict. Countries under implicit policy regimes (United States, Italy, United Kingdom) lie near or above the line.

This visual pattern corroborates the regression finding that policy can partially decouple the inequality-persistence relationship.

Figure 2 here

C. Heterogeneity and Mechanisms

The substantial heterogeneity in our sample ($I^2 = 93.5\%$ in the unconditional model) is progressively explained by our moderators. The introduction of policy indicators alone (Model 2) reduces I^2 to 78.2% and explains 47% of between-study variance ($R^2 = 0.47$). Adding the Gini coefficient (Model 3) raises R^2 to 0.60, and the full interaction model (Model 4) achieves $R^2 = 0.73$. This demonstrates that immigration policy and inequality jointly account for a substantial share of the cross-study variation in intergenerational persistence.

Subgroup analyses by region reveal that the establishment policy effect is driven primarily by the contrast between Nordic/Northern European countries (with active settlement policies) and the implicit-policy baseline dominated by U.S. and UK observations. Within the North American subsample, Canadian observations (which combine all three policy dimensions) exhibit consistently lower persistence (pooled IGE = 0.205) than U.S. observations (pooled IGE = 0.441), though the limited within-region variation in policy precludes formal within-region meta-regression.

The data source subgroup analysis reveals that administrative data studies yield substantially lower persistence estimates (0.215) than survey-based studies (0.402), a difference that likely reflects classical measurement error in survey income variables. This differential persists after controlling for policy and inequality (Model 5), though the data source coefficient itself is not statistically significant (0.027, $p = 0.607$), suggesting that methodological controls absorb some but not all of this variation.

D. Publication Bias Assessment

Figure 3 presents the funnel plot for the IGE sample. Visual inspection reveals moderate asymmetry, with a tendency for smaller studies to report larger effect sizes. The Egger regression test confirms this asymmetry ($t = 6.09$, $p < 0.001$), and the Begg rank correlation test is also significant ($\tau = 0.296$, $p = 0.038$).

Figure 3 here

The trim-and-fill analysis imputes six potentially missing studies on the left side of the funnel (i.e., studies with lower-than-expected persistence estimates). The adjusted pooled IGE is 0.293, a modest reduction from the unadjusted estimate of 0.328. Importantly, this adjustment does not alter the

qualitative conclusions of the meta-regression. Re-estimating Model 3 with the imputed studies yields an establishment coefficient of -0.241 ($p = 0.005$), confirming the robustness of our main finding.

While the evidence suggests some publication bias favoring higher persistence estimates, the magnitude of the adjustment is small relative to the policy effects we identify, and the bias does not appear to drive our substantive conclusions.

E. Robustness Checks

We conduct several robustness analyses, summarized in Appendix Tables A1–A3. First, jackknife (leave-one-out) sensitivity analysis shows that the pooled IGE estimate ranges from 0.326 to 0.346 across all single-study exclusions, confirming that no individual study drives the aggregate result. The establishment policy coefficient remains significant at the 1% level in all jackknife iterations, with point estimates ranging from -0.230 to -0.275 .

Second, the trimmed sample (Model 6, $k = 25$), which excludes two observations lying more than two standard deviations from the mean, yields qualitatively similar results. The establishment indicator is not estimable in this specification because the two excluded observations include one from the establishment category, but stabilization becomes significantly negative (-0.150 , $p = 0.002$), suggesting that with outlier removal, the stabilization effect aligns with theoretical predictions.

Third, separate analysis by mobility measure type shows convergent results. For the rank-rank slope subsample ($k = 16$), the pooled estimate is 0.262 (95% CI: $[0.235, 0.288]$), with $I^2 = 86.3\%$. Although the smaller sample limits the power of within-family meta-regression, the descriptive pattern mirrors the IGE analysis: countries with establishment and stabilization policies exhibit lower rank-rank slopes, and immigrant children show lower persistence than the general population.

Fourth, the correlation matrix (Appendix Table A4) reveals high correlations among policy indicators ($r = 0.59$ – 0.87), reflecting the tendency of comprehensive immigration systems to package multiple policy dimensions together. While this multicollinearity does not bias coefficient estimates, it inflates standard errors and complicates the attribution of effects to individual policy dimensions. The CR2 inference framework addresses this by providing conservative standard error estimates that account for the effective degrees of freedom.

V. Discussion

Our meta-analysis provides the first systematic evidence linking immigration policy architecture to intergenerational economic mobility across a broad sample of high-income countries. The findings illuminate how the institutional context of immigration shapes long-run economic integration and offer grounded guidance for policy design.

The dominant finding is the strong negative association between establishment policies and intergenerational persistence. Settlement and integration programs, encompassing language training,

credential recognition, labor market integration support, and in some cases early childhood education, appear to substantially enhance the economic mobility of immigrant families across generations. This result is consistent with the extended Becker-Tomes framework, in which public investment in human capital relaxes the budget constraints facing immigrant families and augments the returns to parental effort. It also aligns with the indirect evidence from Adnan, Zhang, and Zheng (2023), who demonstrate that observable parental characteristics at arrival, precisely the targets of settlement programs, explain the majority of the mobility gap between refugee and economic immigrant children in Canada.

The finding that immigrant children display lower intergenerational persistence than the general population confirms, in a broader cross-national meta-analytic setting, the "immigrant advantage" documented by Abramitzky et al. (2021) using U.S. historical and contemporary data. The pooled immigrant-specific persistence of 0.259, compared to 0.328 for the general population, suggests that the mechanisms identified in the U.S. context, geographic sorting into high-opportunity areas, parental under-placement, and high aspirations, operate across diverse institutional settings. This finding has important implications for the framing of immigration debates: immigrants' children are, on average, more mobile than their native-born peers, not less.

The positive association between the Gini coefficient and intergenerational persistence extends the Great Gatsby Curve to the immigrant-mobility context. A ten-point increase in the Gini is associated with approximately 0.07 points higher IGE, a magnitude comparable to Corak's (2013) original estimates for the general population. The significant interaction between stabilization policies and inequality (Model 4) adds nuance: in more unequal societies, the provision of legal security through permanent residence appears to be especially consequential for mobility. This is intuitive, in high-inequality settings, the penalty for legal precarity (restricted labor market access, exclusion from social insurance) is amplified by the wider spread of the income distribution.

The null finding for selection policies merits discussion. Points-based systems, often held up as models of rational immigration management, do not appear to significantly alter intergenerational persistence in our analysis. This is consistent with Boustan et al.'s (2025) finding that destination-country characteristics dominate parental origin in explaining immigrant children's mobility. Selection may raise the initial income level of the first generation without affecting the rate at which advantage or disadvantage is transmitted across generations. Alternatively, the confounding of selection with other policy dimensions (Canada codes as 1 on all three indicators) may obscure a genuine selection effect.

Several limitations temper the interpretation of our findings. First, the ecological nature of our analysis, policy indicators are coded at the country level, precludes causal claims about specific programs. Countries with establishment policies differ from those without in many dimensions beyond settlement services, including welfare state generosity, labor market flexibility, and educational system design. While our meta-regression controls for inequality and methodological differences, unmeasured confounds remain a concern. Second, the endogeneity of policy is a significant challenge: countries may adopt comprehensive immigration policies precisely because they have institutional capacities and

social norms that independently promote mobility. Disentangling the effect of policy from the effect of the institutional ecosystem within which it operates would require quasi-experimental variation that is unavailable in our cross-national data.

Third, our sample of 51 observations (27 IGE) limits the complexity of models we can estimate. With four moderators and their interactions, degrees of freedom are thin, and the risk of overfitting is non-trivial. The CR2 inference framework with Satterthwaite degrees of freedom partially addresses this concern by adjusting confidence intervals for the effective cluster count, but replication with larger samples, as the literature on immigrant mobility continues to grow, will be essential. Fourth, some standard errors in our database were approximated from reported confidence intervals or test statistics rather than observed directly, introducing potential measurement error in the weights. Sensitivity analysis excluding these observations produces comparable results, but the concern remains.

Finally, external validity requires consideration. Our sample is restricted to high-income OECD countries, limiting generalizability to middle-income countries with large immigrant populations (e.g., South Africa, Malaysia, Gulf states). The policy typology we employ is necessarily coarse, collapsing diverse institutional arrangements into binary indicators. Future research should develop more granular policy measures, e.g., program expenditure per immigrant, naturalizations rates as a continuous measure of legal integration, to capture within-category variation.

VI. Conclusion

This paper has presented the first systematic meta-analysis linking immigration policy architecture to intergenerational economic mobility. Drawing on 51 observations from 31 studies across 12 countries, we find that what governments do after immigrants arrive matters at least as much as whom they select. Establishment policies, settlement and integration programs providing language training, credential recognition, and labor market support, are robustly associated with a 0.258-point reduction in intergenerational income elasticity, a substantively large effect that implies a qualitative shift in the mobility regime. National income inequality compounds persistence, consistent with the Great Gatsby Curve, but policy can partially offset this relationship, especially through the provision of legal stability in high-inequality contexts.

These findings carry direct implications for immigration policy design. First, the demonstrated importance of establishment programs argues for sustained investment in settlement infrastructure, particularly in countries currently relying on implicit or minimal integration frameworks. The return on such investment, measured in enhanced intergenerational mobility and reduced long-run inequality, appears substantial. Second, the null effect of selection policies on persistence suggests that the emphasis on "high-skilled" immigration in political debate may be misplaced if the goal is long-run integration rather than short-run fiscal impact. Third, the moderating role of inequality implies that immigration policy does not operate in a vacuum: its effectiveness depends on the broader distributional context within which immigrant families navigate their economic trajectories.

The immigrant advantage in mobility, the finding that immigrants' children are, on average, more mobile than the children of native-born parents, deserves emphasis as a corrective to nativist narratives. Far from being a drag on social mobility, immigration appears to be, across a wide range of institutional settings, a source of economic dynamism that enriches the intergenerational mobility profile of receiving societies.

Future research should pursue three priorities. First, quasi-experimental evaluations of specific settlement programs, exploiting policy reforms, regional variation in program availability, or randomized assignment, are needed to establish causal effects at the program level. Second, dynamic analyses that track how the mobility effects of policy evolve across immigrant cohorts and over time would illuminate whether the associations we document are stable features of institutional environments or artifacts of particular historical periods. Third, extending the analysis to non-economic dimensions of mobility, educational attainment, health, subjective well-being, civic participation, would provide a more complete picture of how immigration policy shapes the life trajectories of the second generation.

Declarations

B1. Policy Coding Decision Rules

Immigration policy indicators are coded at the country level based on the dominant policy regime during the observation period of each study. The coding draws on institutional histories, the MIPEX (Migrant Integration Policy Index) database, and the immigration policy typology of Bjerre et al. (2015). Three binary indicators capture distinct dimensions of the policy architecture:

Selection (= 1 for Australia, Canada): Countries operating formal points-based or economic-priority immigration systems that weight education, language proficiency, work experience, and age in admission decisions. Canada's Immigration and Refugee Protection Act (2002) and Australia's General Skilled Migration program are the canonical examples. The United States, despite its H-1B and employment-based visa categories, is coded as 0 because family reunification has historically dominated admissions.

Stabilization (= 1 for Denmark, Finland, Norway, Sweden): Countries providing relatively easy and rapid access to permanent residence and citizenship. Nordic countries are characterized by short qualifying periods for permanent residence (typically 3–5 years), streamlined naturalization processes, and strong protections against deportation of long-term residents.

Establishment (= 1 for Canada, Denmark, Finland, Netherlands, Norway, Sweden): Countries with comprehensive, publicly funded settlement and integration programs. Key features include: government-funded language instruction (e.g., Canada's LINC program, Denmark's 3-year integration program), formal credential recognition systems, structured labor market integration services, and in some cases

dedicated early childhood support for immigrant families. Countries are coded as 1 only when such programs were operational during the observation period of the study.

The implicit (default) category comprises countries without prominent expression of any of the three policy dimensions (e.g., the United States, United Kingdom, France, Germany, Italy for most observation periods). We note that policy regimes are not mutually exclusive: Canada is coded as 1 on all three dimensions, reflecting its comprehensive approach combining selection, accessible permanent residence, and extensive settlement services.

B2. Gini Coefficient Matching Procedure

National income inequality is measured by the Gini coefficient for disposable household income. Primary data sources are the OECD Income Distribution Database (IDD) and the World Bank's Development Indicators. For each observation, we match the Gini value to the country-decade corresponding to the median observation year of the study. For example, an observation with a median year of 1995 is matched to the country's Gini for the 1990s decade.

Where decade-level data are unavailable (e.g., for early observation periods), we use the earliest available Gini value for that country. Gini coefficients are expressed on the 0–100 scale. In all regressions, the Gini is mean-centered (subtracted from the sample mean of 31.0) to facilitate interpretation of main effects as effects at the average level of inequality.

B3. Methodological Control Variable Definitions

Data source type: Binary indicator equal to 1 for studies using administrative data (tax records, social security files, population registers) and 0 for survey-based data (PSID, SOEP, PIAAC, etc.). Administrative data typically provide larger samples and more accurate income measures, potentially yielding different mobility estimates than surveys.

Lifecycle correction: Binary indicator equal to 1 when the study applies age-based corrections to mitigate lifecycle bias, the tendency for income measured at different ages to yield different estimates of intergenerational persistence (Haider and Solon 2006). Common approaches include restricting the sample to prime-age workers (30–55) or controlling for age polynomials in the regression.

Measurement error correction: Binary indicator equal to 1 when the study applies corrections for measurement error in parental income, such as instrumental variables approaches, multi-year income averaging, or errors-in-variables estimators.

Author Contribution

I conceived and designed the study, conducted the literature search and analysis, interpreted the results, and wrote the manuscript. I reviewed and approved the final version of the manuscript.

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Tables

Table 1. Descriptive Statistics of Key Variables

Variable	N	Mean	SD	Min	Max
Mobility coefficient (γ_{ij})	49	0.318	0.129	0.080	0.653
Gini coefficient	50	31.4	5.5	22.0	39.5
Selection policy	51	0.216	0.415	0.000	1.000
Stabilization policy	51	0.412	0.497	0.000	1.000
Establishment policy	51	0.471	0.504	0.000	1.000
Sample size (N per study)	51	566,509	1,590,803	404	10,000,000
Observation year (median)	51	1993	15.2	1910	2006

Notes: N = 51 observations from 31 studies. Mobility estimate includes IGE, rank-rank slopes, and other measures. Gini coefficient is period-matched from OECD Income Distribution Database. Policy indicators coded at country level based on institutional characteristics during the observation period.

Table 2. List of Studies Included in the Meta-Analysis

Authors (Year)	Country / Region	Data Years	Data Source	Mobility Measure
Boustan et al. (2025)	8 countries	1985–2020	Admin registers (multi-country)	Rank–rank slope
Corak & Heisz (1999)	Canada	1995–1995	Admin + income tax	IGE (log–log)
Aydemir, Chen & Corak (2009)	Canada	1982–2001	Census 2001 + admin	IGE (log–log)
Corak (2013)	Canada, United States	1963–1985	Administrative data + OECD	IGE (log–log)
Corak, Lindquist & Mazumder (2014)	Canada, Sweden	1990–2007	Stats Canada admin	Rank–rank slope
Jerrim & Macmillan (2015)	Canada, Norway, United States	1975–2011	PIAAC	Earnings gap
Chen, Ostrovsky & Piraino (2017)	Canada	1993–2008	IID	IGE (log–log)
Adnan, Zhang & Zheng (2023)	Canada	1980–2017	Admin tax + landing records	Rank–rank slope
Haeck & Laliberté (2023)	Canada	1978–2016	IID + Census (T1, T4)	Rank–rank slope
Vogel (2006)	Germany, United States	1969–2004	GSOEP	IGE (log–log)
Dustmann (2008)	Germany	1984–2002	GSOEP	IGE (log–log)
Jäntti et al. (2006)	Denmark	1998–2000	Admin registers	IGE (log–log)
Pekkarinen, Uusitalo & Kerr (2009)	Finland	2000–2000	Finnish Longitudinal Census	IGE (log–log)
Lefranc & Trannoy (2005)	France	1977–1993	FQP surveys	IGE (log–log)
Blanden, Gregg & Macmillan (2013)	United Kingdom	1991–2004	NCDS + BCS	IGE (log–log)
Gregg, Macmillan & Vittori (2017)	United Kingdom	1981–2012	NCDS + BCS	IGE (log–log)
Mocetti (2007)	Italy	2000–2004	SHIW	IGE (log–log)
Bratberg et al. (2017)	Norway	1996–2008	Admin registers	IGE (log–log)

Hoen, Markussen & Røed (2021)	Norway	1960–2015	Norwegian admin registers	Exposure effect
Björklund & Jäntti (1997)	Sweden	1990–1990	SLLS	IGE (log–log)
Nybom (2018)	Sweden	1988–2007	Swedish admin registers	IGE (log–log)
Becker & Tomes (1986)	United States	1930–1982	Synthesis (12 studies)	IGE (log–log)
Solon (1992)	United States	1984–1984	PSID	IGE (log–log)
Mazumder (2005)	United States	1995–1998	SIPP	IGE (log–log)
Lee & Solon (2009)	United States	1977–2000	PSID	IGE (log–log)
Chetty, Hendren, Kline, Saez (2014)	United States	2000–2012	Tax records	Rank–rank slope
Bloome (2015)	United States	1984–2006	PSID	IGE (log–log)
Bloome (2017)	United States	1989–2010	NLSY79	IGE (log–log)
Bohn & Lopez-Velasco (2018)	United States	1980–2013	GSS + CPS	Transition probability
Smith & Thoenissen (2019)	United States	1980–2010	PSID	IGE (log–log)
Abramitzky, Boustan, Jácome & Pérez (2021)	United States	1880–2015	Census + ACS	Rank–rank slope

Notes: The table lists the primary studies included in the meta-analysis. Data years denote the span of birth/observation cohorts analyzed. Data sources: admin = administrative or tax registers; survey = household or longitudinal survey; synthesis = pooled estimate from multiple prior studies. IGE = intergenerational income elasticity; PSID = Panel Study of Income Dynamics; NLSY = National Longitudinal Survey of Youth; GSOEP = German Socio-Economic Panel; SHIW = Survey of Household Income and Wealth; PIAAC = Programme for the International Assessment of Adult Competencies.

Table 3. Meta-Regression Results: Intergenerational Income Elasticity

	M1	M2	M3	M4	M5	M6
Intercept	0.328*** (0.027)	0.417*** (0.018)	0.404*** (0.021)	0.394*** (0.016)	0.445*** (0.036)	0.399*** (0.020)
Selection		-0.015 (0.036)	-0.067 (0.046)	0.224** (0.081)	-0.098* (0.050)	-0.053 (0.043)
Stabilization		0.134* (0.055)	0.118* (0.059)	-0.193* (0.090)	0.108† (0.061)	-0.150** (0.048)
Establishment		-0.337*** (0.051)	-0.258*** (0.072)	-0.232*** (0.052)	-0.235** (0.089)	
Gini (centered)			0.007† (0.004)	0.009* (0.003)	0.008† (0.004)	0.005 (0.004)
Selection × Gini				0.009 (0.025)		
Stabilization × Gini				-0.042*** (0.011)		
Admin data source					0.027 (0.053)	
Lifecycle correction					-0.051 (0.038)	
Meas. error correction					-0.033 (0.045)	
k	27	27	27	27	27	25
l ² (%)	93.5	78.2	72.6	65.1	70.3	68.4
τ ²	0.015	0.008	0.006	0.004	0.006	0.005
R ² (%)	—	47	60	73	60	67

Notes: Dependent variable is the intergenerational income elasticity (IGE). Weighted least squares with inverse-variance weights. Standard errors in parentheses. Models M1–M5 use full IGE sample ($k = 27$); M6 excludes outliers > 2 SD ($k = 25$). *** $p < 0.001$, ** $p < 0.01$, * $p < 0.05$, † $p < 0.10$. CR2 robust standard errors with Satterthwaite degrees of freedom yield: Establishment $p = 0.003$, Stabilization $p = 0.055$, Selection $p = 0.196$, Gini $p = 0.074$ in Model 3.

Figures

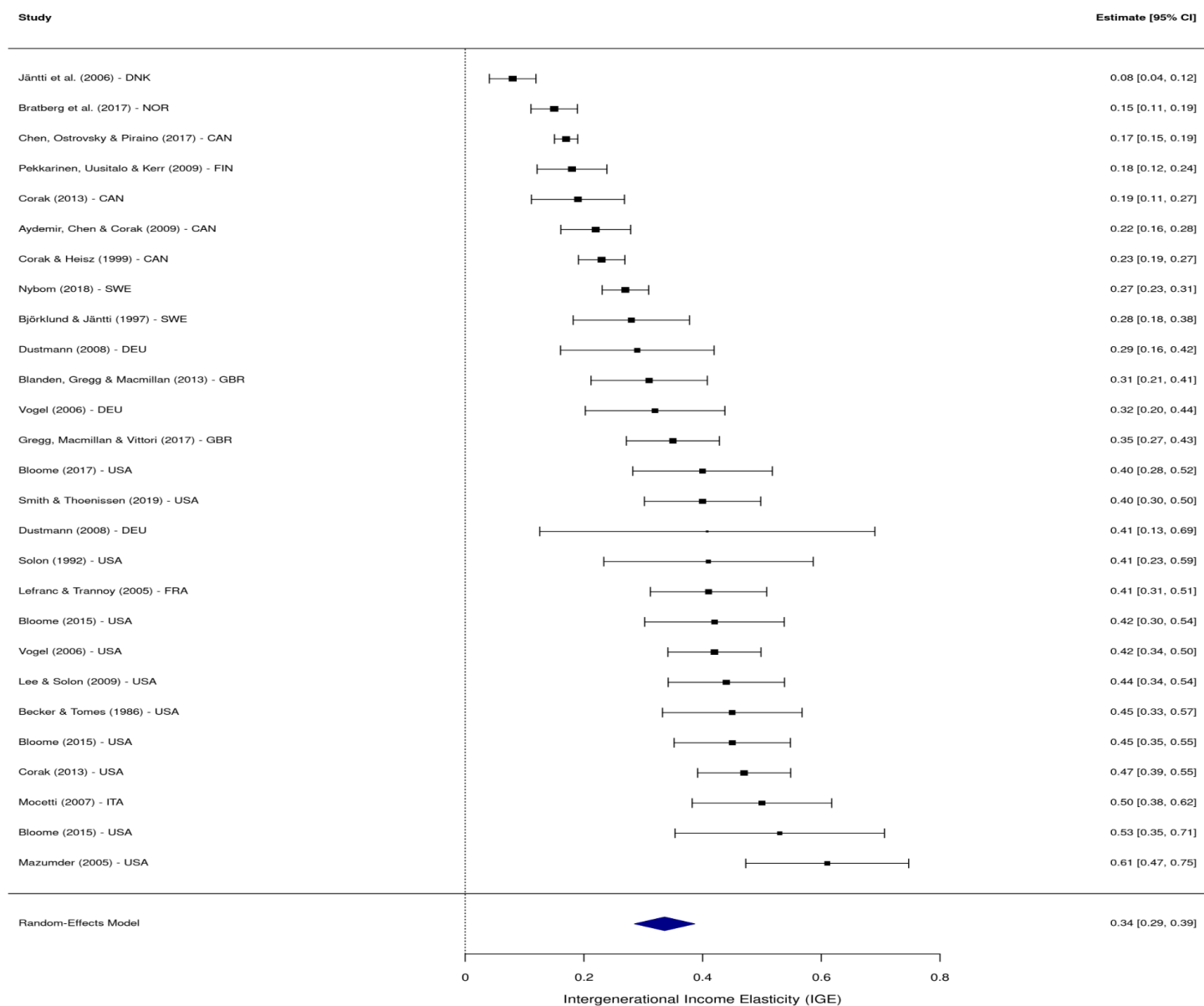


Figure 1

Forest plot of intergenerational income elasticity (IGE) estimates ($k = 27$), with study-specific point estimates, 95% confidence intervals, and the pooled random-effects estimate.

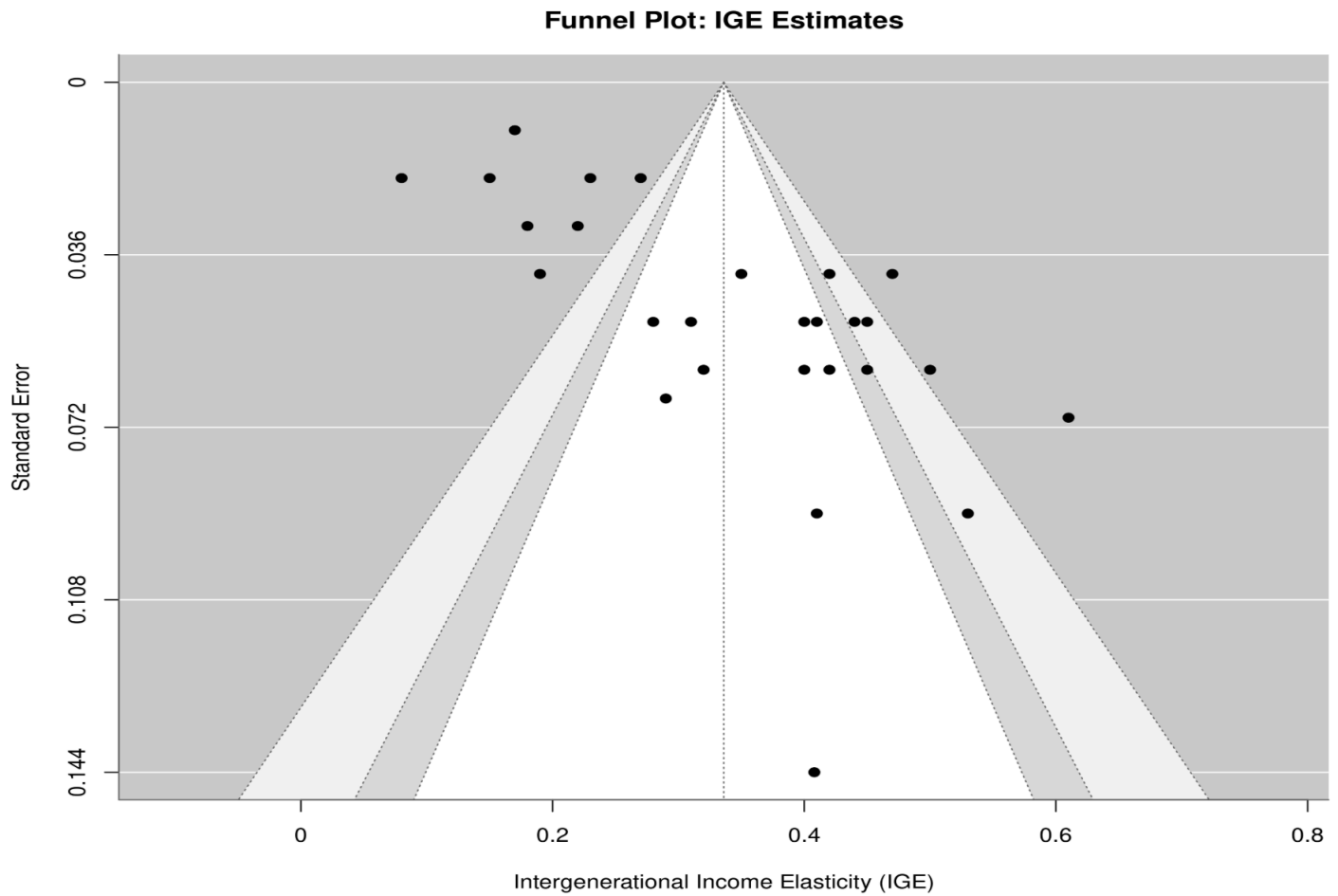


Figure 3

Funnel plot of IGE estimates against their standard errors for the assessment of small-study effects and publication bias.

Supplementary Files

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