One Land, Many Promises: Assessing the Consequences of Unequal Childhood Location Effects

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Abstract

This paper estimates the causal effects of childhood residential location on the adult income of native-born Israeli children and the children of immigrants from the former Soviet Union, and studies the consequences of location effect heterogeneity on the design and effectiveness of neighborhood recommendation policies. The causal effects of childhood location contribute substantial variability to the adult earnings of Israeli children. While the place effects of high-income immigrants and high-income natives are strongly correlated, location effects for low-income immigrants are uncorrelated with location effects for low-income natives. Guided by these findings, we develop a policy-targeting framework that aims to recommend the top locations in Israel while incorporating the constraint that the policymaker cannot make ethnicity-dependent location recommendations. Using empirical Bayes tools, we find that targeting policies based on pooled population-wide averages yield inferior outcomes for immigrants. Robust targeting strategies designed to perform well against the least favorable sorting patterns reveal a set of 10 cities that are likely to benefit children of both groups.

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1 Introduction

A growing body of literature finds that childhood locations have a significant and long-lasting effect on outcomes in adulthood (see Chyn and Katz, 2021, for a review). This evidence is the basis for "moving to opportunity" policies that aim to encourage low-income housing voucher recipients to move to high-opportunity neighborhoods (Katz et al., 2001; Bergman et al., 2019). These policies often provide a single unified recommendation about where people should move based on a ranking of pooled neighborhood-level estimates. In the absence of prior knowledge about recipients' behavioral responses, the effectiveness of such unified policies relies on the assumption that neighborhood effects are universally beneficial and comprised of limited heterogeneity. As evidence of heterogeneity grows (e.g., Chetty et al., 2018), there is increasing uncertainty about whether families who follow the policy recommendations will ultimately benefit.

In this paper, we study this question in two steps. First, we provide evidence that childhood location effects vary substantially for low-income children from different backgrounds. Using a comprehensive administrative dataset from Israel, we establish that, similar to Chetty and Hendren (2018a,b), one's place of birth contributes substantial variability to the adult earnings of both native-born and immigrant children. However, the correlation between these effects for low-income immigrant and native-born children is close to zero, suggesting that places that boost the income of one group do not necessarily benefit the other. Based on these findings, we then study the implications of this heterogeneity for the outcomes of potential recipients of neighborhood recommendation policies and propose an alternative unified recommendation policy that accounts for heterogeneity.

We begin by revisiting the benchmark estimates of childhood location effects from Chetty and Hendren (2018a,b) in Israel and separately estimate effects for immigrants from the former Soviet Union and native-born children. Focusing on children born between 1980 and 1991, we estimate the effects on income rank at age 28 for each city and regional council.[1] By exploiting the mass migration wave from the former Soviet Union to Israel between 1989 and 2000, during which 1 million immigrants arrived and spread throughout the country, we identify childhood location effects in most major cities for both groups.

Causal location effects are identified by leveraging variations in children's exposure time to different cities during childhood due to household moves at different ages. This strategy combines variations in the timing of moves across locations within Israel and

[1] A regional council is a group of small localities, such as small towns or *kibbutzim*, that are geographically close and share the same local governing council. Cities and regional councils are the smallest local government units in Israel.

the age at which children migrated from the former Soviet Union. This strategy does not require families to sort randomly, but rather assumes that among families with the same sequence of location choices, the child's age at arrival is unrelated to unobserved components that affect potential outcomes. To support this, we demonstrate that conditioning on these sequences balances observable family characteristics across children's ages upon arrival to each city, which suggests unconfounded comparisons. Our model also assumes that location effects are linear with exposure time. We justify this by demonstrating that the standard diagnostics in the literature that indicate a linear relationship between exposure time and mean outcomes of children who spent their entire childhood in the same location also hold in Israel (Chetty and Hendren, 2018a; Deutscher, 2020; Laliberté, 2021) and provide additional evidence that the location effects themselves are linear with exposure time.

Childhood location effects vary substantially for both native-born and immigrant children. To quantify the extent of across-city heterogeneity, we estimate the standard deviation of location effects for natives and immigrants, adjusting for sampling error. For a child with parents at the 25th percentile of the national income distribution, a one standard deviation increase in city quality for a single year for both groups boosts income at age 28 by 0.44% per year, compared to the mean. Extrapolating over 18 years of childhood, growing up in one standard deviation better city from birth would increase a child's yearly income in adulthood by 8%.

Childhood location effects also vary substantially within cities across immigration groups, with a pattern that differs by household income. We find that the correlation between the location effects of immigrants and natives among low-income families at the 25th percentile of the income distribution is close to zero, while there is a strong positive correlation between the location effects of immigrants and natives among high-income families. This result implies that there is no single "promised land" for low-income families, i.e., places that generate high adult income for one group do not generally boost income for the other. We show that this zero correlation is not driven by differences in high school attendance patterns within locations, within-city heterogeneity in neighborhood effects, or mismeasurement of immigrant parental income. In contrast, our findings suggest that lack of social integration and assimilation—measured by the location effects on intermarriage rates—serve as a plausible explanation.

Large, diverse cities with a substantial share of both immigrants and natives are more likely to benefit immigrants. This finding aligns with the literature, which emphasizes the importance of geographic concentration of immigrants and refugees on their outcomes (Edin et al., 2003; Beaman, 2012; Abramitzky et al., 2020). In contrast, places with higher municipality welfare expenditure per capita are more likely to be detrimental to native-born children, while those measures are less predictive of low-income immigrant location effects. Previous literature has emphasized the relationship between poverty-related covariates and location effects, using these characteristics to target housing policy (Katz et al., 2001). Our findings suggest that such targeting strategies may not be useful for immigrants in the Israeli context.

Motivated by these findings, we next study the consequences of heterogeneity for the policy implemented in the Creating Moving to Opportunity (CMTO) experiment (Bergman et al., 2019), which provided housing voucher recipients with recommendations regarding where to move based on tract-level upward mobility estimates. We focus on a unified policy that provides the same recommendations to all groups. Although the literature suggests that the optimal policy should ideally be personalized and based on group identity (Chan and Eyster, 2003; Cowgill and Tucker, 2019; Rambachan et al., 2020; Ellison and Pathak, 2021), this restriction is motivated by legal and moral constraints in many countries, where it is unacceptable to base public programs on ethnic identity or promote segregation.

Using a decision-theoretic framework, we start by evaluating the policy considered in Bergman et al. (2019), which ranks locations based on a pooled average estimate of city quality. Such policy results in lower weights on the gains for minority groups and produces inferior outcomes for such groups. These unequal outcomes arise from two sources: First, the decision-maker's inability to target treatment by ethnic group ex-ante, which prevents the policy from leveraging the heterogeneity in location effects across groups, and second, the decision-maker's ambiguity regarding which households will respond to each particular policy recommendation and how. With treatment effect heterogeneity, some compliance behavior with the policy may dilute its effectiveness if the gains for households that respond are very different from the overall average effect.

We suggest an alternative targeting policy, the *minimax* strategy (Wald, 1950), which provides a list of recommended locations that are optimal under the least favorable compliance scenario. We show that this robust policy can generate substantial advantages for minority groups and achieve more equitable outcomes. With the minimax policy, we can pinpoint at least 10 cities that offer benefits for both groups, in which the worst-case outcome for either group is 40% better than under the city-level average policy. Also, we can ensure that, on average, no more than 10% of the recommended cities would yield outcomes inferior to those resulting from the current status quo sorting patterns.

This paper contributes to several strands of the literature. First, we add a new perspective to the vibrant discussion of the challenges that might arise from the neighborhood recommendation policies proposed in the CMTO experiment. So far, the literature has focused primarily on issues of identification (Heckman and Landersø, 2021; Eshaghnia, 2023), measurement (Chen, 2023; Aliprantis et al., 2024), and inference (Andrews et al., 2022; Mogstad and Torsvik, 2021; Mogstad et al., 2024), where the latter work emphasizes the ramifications of ranking locations based on noisy estimates rather than their true values. Although Chetty et al. (2018) and Chetty et al. (2020) acknowledge the potentially multifaceted nature of locations, the literature has not considered the complications it generates. As a result, analysis of both existing and proposed mobility policies behaves as if there is a single ladder of location effects. In the CMTO, for example, there is no guarantee that all recommended places are indeed beneficial for all participants. While Mogstad et al. (2024) note this concern regarding the risk of forming policy based on noisy estimates, similar logic applies if the signal varies. In this paper, we directly address the policy implications of location effect heterogeneity by modeling uncertainty from both heterogeneity and unknown compliance, along with uncertainty driven by measurement error.

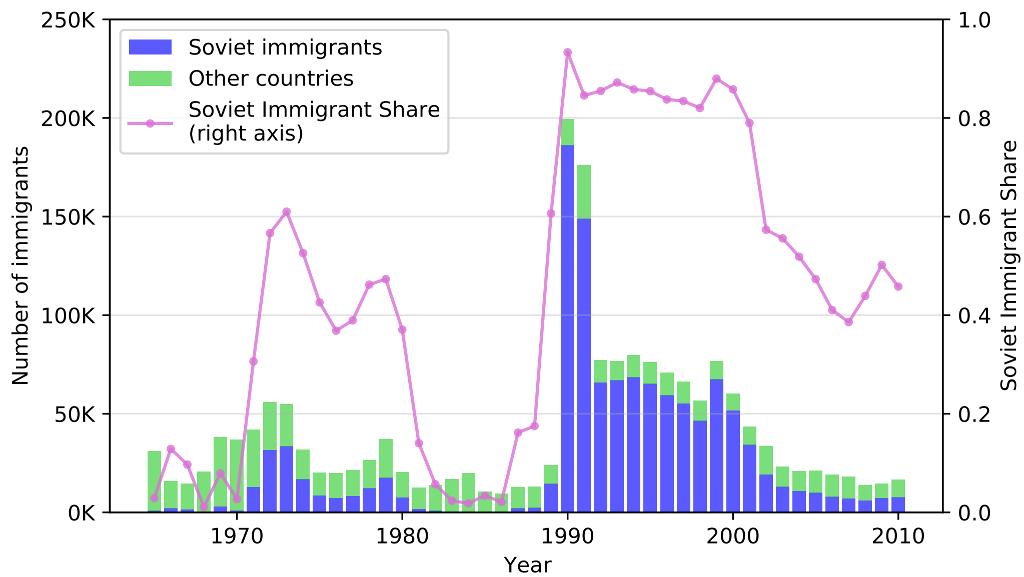
Methodologically, our work relates to a growing literature on empirical Bayes ranking and prediction methods that use shrinkage estimates to identify the value added of schools, teachers, hospitals, and discriminatory firms (Chetty et al., 2014a; Abdulkadiroğlu et al., 2020; Abaluck et al., 2021; Kline et al., 2022). Recent work in econometrics has emphasized that such tasks are analogous to multiple testing problems, in which decisions result from constraints on various sorts of error rates (Gu and Koenker, 2020; Kline and Walters, 2021; Kline et al., 2023; Mogstad et al., 2024). We add to this literature by modeling the risk a decision-maker faces, distinguishing between the risk stemming from effect heterogeneity, unknown behavioral responses, and statistical noise. As such, our paper contributes to the literature on optimal statistical treatment rules (Manski, 2004; Kitagawa and Tetenov, 2018; Manski, 2021). Similar to Christensen et al. (2022), our model departs from classic approaches to these problems by considering how optimal decisions depend on partially identified parameters. In our setting, location effects are point-identified (so the decision-maker faces only statistical uncertainty), while household compliance patterns are not, which creates ambiguity regarding the final allocation of true payoffs across families.

This paper also extends a growing literature in economics on algorithmic bias and fairness (Kleinberg et al., 2018; Cowgill and Tucker, 2019; Rambachan et al., 2020; Liang et al., 2021) and the equity-efficiency tradeoffs of affirmative action programs (Lundberg, 1991; Chan and Eyster, 2003; Ellison and Pathak, 2021). Papers in both strands conclude that the optimal policy should exploit all available information, including group identity variables. Instead, we explore the possibilities for a policy conditional on a suboptimal restricted algorithm, which, to our knowledge, has been studied less. Our model demonstrates that we can improve the fairness of the restricted policy by modeling the uncertainty generated by such restrictions using a decision-theoretic framework. This approach can be extended to other settings with anti-discriminatory laws or group-directed treatments.

2 Historical Context

In 1989, the Soviet Union relaxed its emigration restrictions, triggering one of the most significant human movements of the late 20th century. Prior to this relaxation, restrictive emigration laws and tight governmental controls made it nearly impossible for Soviet residents to leave the country. As the USSR disintegrated, these legal barriers dissolved, and approximately 7 million Soviet residents left the Soviet Union between 1989 and 2000 (Abramitzky et al., 2022). Among them, more than 1 million Jewish immigrants arrived in Israel, increasing Israel's population by 20%.

Figure 1: Annual number of Soviet immigrants and other countries to Israel



Note: This figure displays the number of migrants to Israel between 1965 and 2019 arriving from the Soviet Union and other countries. On the right axis, the pink line displays the fraction of Soviet immigrants. Source: the Israeli Central Bureau of Statistics.

Figure 1 presents the number of Soviet immigrants entering Israel by year. The bulk of the migration wave—over 300 thousand immigrants—arrived in a relatively short time span, between 1989 and 1991, and accounted for 7% of the Israeli population prior to the immigration. This peak was followed by a steady influx of 60,000 per year throughout the decade, totaling over 1 million—one-fifth of Israel's 1989 population.

Soviet Jews received full Israeli citizenship upon arrival, granting them unrestricted access to social services, education, healthcare, and social security (Buchinsky et al., 2014). They faced no formal labor market restrictions and could settle anywhere in Israel. The government provided support, including a modest one-year grant ("absorption basket"), free Hebrew classes, and local integration centers.

This migration wave provides several favorable features for studying the effect of childhood location of residence on children's long-run economic outcomes. First, it was large and unrestricted, with entire families immigrating together, enabling causal identification separately for Soviet immigrants across multiple locations. Second, as citizens, immigrants faced no regulatory barriers compared to natives, ensuring institutional factors do not explain any immigrant-native gaps.

3 Empirical Model

3.1 Conceptual Framework

Consider a population of children indexed by i and a set of locations indexed by $j \in \{1, ..., J\}$. Let $Y_i(e)$ denote child i's potential adult income as a function of the number of years of exposure to each location, represented by the vector $e = (e_1, ..., e_J)'$. We assume that childhood locations affect children's long-run outcomes from birth to age 18, with e_j representing the number of years of exposure to city j before age 18 such that $\sum_j e_j = 18$.[2] Throughout the paper, we assume that potential outcomes follow an additive stricture:

$$Y_i(e) = \sum_{j=1}^{J} \theta_{ij} \cdot e_j + \xi_i, \tag{1}$$

where θ_{ij} represents the contribution to adult income of an extra year in city j to child i, and ξ_i is the error term, accounting for all other age-, time-, or location-dependent shocks beyond the variation by exposure time and childhood city that affect children's long-run outcomes, such as time-invariant and time-varying parental investments, moving costs, or age-specific shocks. This model rules out location effect heterogeneity by child's age or complementarity or substitutability between time spent in different places. The observed outcome for child i is given by $Y_i = Y_i(E_i) = \sum_j \theta_{ij} E_{ij} + \xi_i$, where $E_i = (E_{1i}, ..., E_{Ji})'$ represents child i's realized years of exposure to each city from birth to age 18.

[2] In Appendix Section D, we present evidence suggesting that in Israel, parents' moves after children are over 18 are inconsequential for children. This finding aligns with Israeli institutions, whereby most individuals enlist in the army immediately after high school.

3.2 Identification Strategy and Research Design

The ideal experiment would randomly send children to different places at different ages. Absent such an experiment, we exploit a quasi-experimental design on the entire population, following Chetty and Hendren (2018b). We identify location effects by exploiting the variation in children's exposure time to different cities during childhood due to household moves at different ages. Our strategy combines variation in the timing of moves across locations within Israel with variation in the age at which children migrated to Israel from the former Soviet Union. To build intuition, consider the following example. Among all native-born families that moved from city j to city l and are of the same income level, some children arrived at younger ages, and some arrived at older ages. Then, if among that narrow group, the moving decision is unrelated to the child's age at the move, we can infer the effect of growing up in city j compared with city l: $\theta_{j\mathcal{N}p} - \theta_{l\mathcal{N}p}$ by comparing the outcomes of children who spent different time spans in each city.

Formally, consider all the families with the same family income rank $p(i) = p$ who moved once or twice between places when the child was younger than 18 years old, where $o(i)$ is child's origin location, which is always the USSR for immigrants, $d(i)$ is the destination location, and if the family moved twice, $d_2(i)$ is the second destination, which equals zero otherwise. We assume:

Assumption A1 *(Selection on observables)*

$$\xi_i \perp\!\!\!\perp E_i \mid (o(i), d(i), d_2(i), p(i))$$

Assumption A1 imposes important restrictions on the economic environment. It requires that among children with the same set of childhood places and parental income, the time spent at each location is not systematically correlated with unobserved inputs that determine human capital. Importantly, it does not preclude systemic spatial sorting that correlates with the location effects of the origin and destination locations. For example, we find in Section 6 that immigrants are more likely to reside in cities with high long-run effects on children's income in adulthood.

Note that Equation (1) with Assumption A1 imply that among the families that moved between origin location o to destination location d (using the abbreviation $o \to d$) when the children were at different ages, we have:

$$\frac{\mathbb{E}[Y_i(e)|o \to d, p(i) = p, a_i = a] - \mathbb{E}[Y_i(e)|o \to d, p(i) = p, a_i = b]}{a - b} = \frac{\mathbb{E}[Y_i(e)|o \to d, p(i) = p, a_i = a] - \mathbb{E}[Y_i(e)|o \to d, p(i) = p, a_i = c]}{a - c},$$

that is, differences in outcomes among same-location movers are, on average, proportional to their time spent. This equation echoes the difference-in-difference logic in movers design regressions with binary treatments (Hull, 2018). While in long panel datasets with binary treatments, the identification assumption requires no pretend, in this context, this model requires that differences in outcomes between families with different years of exposure follow the same average trend. Therefore, among all these families, we can identify the contribution of time spent in each place as the slope coefficient on exposure time within each group.

3.3 Empirical Implementation

Each child i belongs to a group $g(i) \in \{\mathcal{N}, \mathcal{I}\}$, either natives ($\mathcal{N}$) or immigrants ($\mathcal{I}$). Building on the identification strategy mentioned above, we estimate the childhood location effects of each city in Israel separately for immigrants and native-born Israeli children who moved between places in Israel when the children were young. To maximize sample size, we exploit variation in children's exposure time to different locations in Israel among all families that experienced up to two moves when the child was young.[3] For immigrants, we consider two groups. The first includes families that moved once to Israel when the child was at age a_i, settled in city j, and stayed there until the child grew up. For these families, the exposure variable is $E_{ij} = 18 - a_i$ for the first city of residence j and zero otherwise. The second group consists of immigrants who moved twice. First, immigrated to Israel when the child was at age a_i and settled in city $d(i)$, then moved to city $d_2(i)$ when the child was at age a_{2i}. For these families, exposure is given by:

$$E_{ij} = \mathbb{1}\{j = d(i)\}(a_{2i} - a_i) + \mathbb{1}\{j = d_2(i)\}(18 - a_{2i})$$

Similarly, for natives, our analysis includes families who moved once or twice between cities in Israel before the child turned 18, with a_i denoting the child's age at the first move from origin city $o(i)$ to destination city $d(i)$, and a_{2i} the child's age at the second move to destination city $d_2(i)$, which equals zero if that child moved only once during childhood. Therefore, their exposure variable is given by

$$E_{ij} = \mathbb{1}\{j = o(i)\}a_i + \mathbb{1}\{j = d(i)\}(a_{i2} - a_i) + \mathbb{1}\{j = d_2(i)\}(18 - a_{2i}).$$

Given these building blocks, we estimate the following OLS regression for children whose families moved between cities in Israel or immigrated to Israel before the child

[3] Adding those who moved twice increases the immigrant sample size by 23% and native-born sample size by 13%. For more details, see Appendix Section C.

turned 18:

$$Y_i = \sum_{g' \in \{\mathcal{N},\mathcal{I}\}} \sum_{j=2}^{J} \left(\underbrace{(\alpha_{jg'} + \eta_{jg'} p(i))}_{\theta_{jg'p}} E_{ij} + x_i' \gamma_{g'} \right) \mathbb{1}\{g(i) = g'\} + \epsilon_i, \tag{2}$$

where our main parameters of interest are the city-level slope coefficients on years of exposure, E_{ij}. We estimate heterogeneous location effects, allowing them to vary linearly by parental income rank, following earlier work indicating that a linear relationship between parental income rank and location effects provides a good empirical approximation (Chetty et al., 2014b).[4] The intercept α_{jg} measures the effect of spending one more year in city j for a child of group g whose parental income is at the lowest percentile in the national income distribution, and the slope η_{jg} measures the one-year return to parental income in location j for a child who belongs to group g. Therefore, the total one-year location effect in city j for a child in group g with parental income p is θ_{jgp}.

In Equation (2) x_i includes fixed effects for sequences of location choices at the $o(i)$-$d(i)$-$d_2(i)$ level for native-born children and at the $d(i)$-$d_2(i)$ by birth cohort level for immigrants.[5] By including the sequence of location choice fixed effects, location effects are identified only from variation in the timing of moves rather than variation between families that moved between different places. We measure children's outcomes at a fixed age and, therefore, in different calendar years. Therefore, we add the birth-cohort fixed effects to account for fluctuations in labor market conditions over time. Lastly, x_i includes year of birth fixed effects interacted with parental income rank, where we control for the sequences of location fixed effects and parental income in an additively separable way due to the sample size restriction we face. Note that in this model, location effects are identified only in relative terms. Therefore, in our analysis, we set the base-level location of immigrants to be the former Soviet Union and the base-level location of native-born children to be Jerusalem.

Immigrants' origin location is grouped at the USSR level. Therefore, for this population, Assumption A1 requires the age of migration not to be correlated with the origin neighborhood within the USSR. This could be violated if the timing of when families left the Soviet Union varied across origin neighborhoods. In Appendix section D, we provide a list of robustness tests for the model assumptions. In particular, we show that results are robust to the inclusion of family fixed effects, which, for immigrants,

[4] In Appendix Figure A.1, we present the relationship between children's income rank at ages 28-30 and parental income rank by immigration group and within a few selected cities. This suggests that the relationship between children and parental income rank is approximately linear in Israel as well.

[5] For immigrants, we interact the sequence of location choices' fixed effects with the child's year of birth to account for the potential correlation between parents' cohorts and children's age at arrival. We thereby compare immigrant families that moved at different years within cohorts.

addresses the mentioned-above concern.

Estimation results in two vectors for every immigration group $g \in \{\mathcal{I}, \mathcal{N}\}$: one for location effect intercepts, $\hat{\alpha}_g = (\hat{\alpha}_{1g}, ..., \hat{\alpha}_{Jg})'$ and another for parental income rank slopes, $\hat{\eta}_g = (\hat{\eta}_{1g}, ..., \hat{\eta}_{Jg})'$, and their corresponding variance-covariance matrix, which is clustered by family id. The full estimated location effects vector is represented by the stacked vector $\hat{\theta} = (\hat{\alpha}'_{\mathcal{I}}, \hat{\eta}'_{\mathcal{I}}, \hat{\alpha}'_{\mathcal{N}}, \hat{\eta}'_{\mathcal{N}})'$, and its corresponding variance is represented by the matrix Σ. We are interested in studying the joint distribution of θ and measuring the heterogeneity in location effects across immigration groups.

3.4 Variance Components

Having estimated $\hat{\theta}$, our central objective is to study the heterogeneity in location effects both across cities and within cities by immigration group and parental income. We measure the heterogeneity across and within cities by studying the variance-covariance matrix of θ_j, denoted by Ω. For every group g, the diagonal elements of Ω give the variance of the elements of θ_j. For example, the variance of α_{jg} is

$$\sigma^2_{\alpha g} = \sum_{j=1}^{J} \frac{n_j}{N} (\alpha_{jg} - \sum_{l=1}^{J} \frac{n_l}{N} \alpha_{lg})^2 \tag{3}$$

where n_j is the number of children residing in city j during childhood for at least one year, and $N = \sum_{j=1}^{J} n_j$. The off-diagonal elements of Ω are the covariances of elements in θ_j with either the other group's parameter or the within-group relationship between the slope and the intercept.

We observe only noisy estimates of the location effects $\hat{\theta}_j$, rather than the location effects themselves, θ_j. Therefore, the sample variance, $\sum_{j=1}^{J} \frac{n_j}{N}(\hat{\alpha}_{jg} - \sum_{l=1}^{J} \frac{n_l}{N} \hat{\alpha}_{lg})^2$, of α_{jg}, or each of the other elements in θ_j, is over dispersed. The standard approach to bias-correct the estimate of Equation (3) is to subtract from the sample variance the mean squared of the standard errors (Chetty et al., 2014a; Chetty and Hendren, 2018b; Rose et al., 2022; Kline et al., 2022). As detailed in Appendix Section E, we use a variant of that estimator, which accounts for the correlation of the $\hat{\theta}_j$ across different j arising from our estimation procedure.

4 Data

We use administrative data collected by the Israeli Central Bureau of Statistics (CBS). The data cover the entire population of registered Israeli citizens born between 1950-1995 and their parents. The data comprise four primary sources: tax records from the Tax Authority for the years 1995–2019 with employer-employee and self-employment tax information; education records from the Ministry of Education, including school identifiers and city; civil registry records providing demographics including gender, birth year, immigration date, and origin, family links (parents, siblings, spouse, children), and annual location of residence; and the 1983 and 1995 censuses, which include city of residence. The next section details sample construction and key variables. Further details are in Appendix B.

4.1 Sample Selection and Variable Definitions

The main sample consists of all children born in the years 1980-1995. Using the location of residence of both the child and the parents, we define the primary parent as the one who shares an address with the child for the majority of the years. If a location value is missing for a certain year, we fill in the location of residence using the child's school location only if the school is in the same location as the child's location of residence in year $t-1$.[6] We enrich the location data using the city information available from the 1995 census. Specifically, we use the answers to two questions: "When did you move to your current city?" and "Where did you live 5 years ago?". Using these variables, we construct location information starting from 1995 and, for a subset, from 1990.[7] For the rest of this paper, our unit of location is a city or regional council,[8] which represent the units of local government.[9]

For every parent in the sample, we construct the following variables: Parents' income, which is the total gross income at a household level, measured in 2016 Israeli shekels (1 ILS $\sim$ \$0.28). In years when the family has no recorded earnings, the family's income is coded as zero. To derive an approximation of parents' resources during childhood, we calculate the average earnings over the years 1995-2016. This time frame is selected to balance between potential attenuation biases that may arise from measuring parental income over too short a period and the risk of doing so too late in life when income tends to be more volatile (Mazumder, 2005). We exclude families with less than 4 years of earnings, which accounts for 1.5% of parents. Finally, we work with a parents' percentile rank variable, defined as the parental income rank in the national population that satisfies the restriction of having at least 4 years of earnings in 1995-2016. To account for the unbalanced structure of the child's age at which parents have earnings, we calculate each income rank within children's cohorts and, therefore, compare parents' earnings for children at the same ages. Our main outcome for children is their income at age 28 and calculate the percentile rank within

[6] Thereby, differences between school locations and location of residence are not accounted as moves.

[7] Response rate to the questions in 1995 is around 20%.

[8] A regional-council locality is a group of small localities such as small towns or kibbutzim that are geographically close to each other and share the same local governing council

[9] Our data agreement usage restricts us to estimate location effects only at the city level or bigger geographic unit.

the child's cohort to account for differences in calendar year labor market conditions.

We examine two primary populations. The first group consists of immigrants from the former Soviet Union (FSU) who arrived in Israel between 1989 and 2000. We identify the children of immigrants based on their parents' birth country and year of immigration.[10][11] For each immigrant child, we calculate a_i, the age of the child when the family immigrated to Israel. We then designate the first city or regional council of residence as their initial destination location and record any other cities where the family lived during the child's childhood had they moved.

The second group in our analysis is the native-born, which includes all non-Arab individuals born in Israel (including families from older immigration waves).[12] Similarly to the immigrants, for every family we record all the cities in which the families lived during their children's childhood. In some cases, we refer to families who reside in a single location throughout the child's childhood as permanent residents or stayers and the subset of families that are not permanent residents as movers.

Lastly, our objective is to measure the childhood location effects separately for every city and immigration group. We restrict attention to cities with at least 100 individuals in every group. These requirements narrow our analysis to 98 cities and regional councils out of 253.

4.2 Summary Statistics and the Immigrant-Native Income Gap

Table 1 presents the number of children and mean income of parents and children in Israeli shekels (\$1 $\sim$ 3.4 ILS) of natives whose families moved between locations either once or twice and Soviet immigrants whose families either moved to Israel and stayed in the same city or immigrated to Israel and then moved between cities in Israel before their child turned 18. For a more detailed comparison of families that moved once and twice, see Appendix Section C.

The 98 selected cities are the largest cities and regional councils in Israel and, therefore, represent most Israeli citizens, covering 88% of immigrants and 81% of native-born families that move between cities in Israel. Parental income in these cities is slightly lower than in the full city sample for both groups, while children's income is slightly

[10] Around 10% of the immigrants during this period had a missing country of origin before immigration. In such cases, we classified them as FSU immigrants as well, since immigrants from the former Soviet Union accounted for 90% of arrivals during these years (See Figure 1).

[11] For immigrants who arrived in Israel before 1995, who are the majority of immigrants, the data does not record the exact country of origin within the Soviet Union.

[12] Approximately 20% of the Israeli population are Arab citizens; however, since Jews and Arabs in Israel are geographically segregated, over 70% of the Israeli Arabs live in cities and villages that are 100% Arabs. As a result, there is very little overlap between the two groups, which would restrict our ability to compare them.

higher. Notably, immigrant parental income is 55% that of natives, echoing the results in Cohen-Goldner and Paserman (2011), Goldner et al. (2012), and Arellano-Bover and San (2023) of a large immigrant-native wage gap. Arellano-Bover and San (2023) show that this gap persisted for 27–29 years after arrival. However, by age 28, second-generation immigrants closed most of the gap, earning 95–96% of their native-born peers' income.

Appendix Figure A.2 plots the geographic distribution of immigrants across Israel, both as their share of the total immigrant population (Panel (a)) and as their share within each locality (Panel (b)). As expected, major cities like Haifa, Tel Aviv, Jerusalem, and Be'er Sheva absorbed the largest number of immigrants. However, Panel (b) illustrates that immigrants settled not only in large urban centers but also across the country, making up a significant portion of residents in many localities.

Table 1: Descriptive statistics

	All cities		98 cities	
	Immigrants (1)	Native-born (2)	Immigrants (3)	Native-born (4)
(A) Children				
Income age 28	67,108	70,741	68,191	71,701
Rank age 28	52.5	53.6	53.2	54.2
(B) Parents				
Parents' income	131,670	235,981	129,997	233,095
Rank parents	45.7	63.3	44.8	63.1
Num. of children	156,269	116,572	138,664	95,500

Note: This table presents the mean children's income and income rank at age 28 and the mean parental income and parental income rank between the years 1995 and 2016 among immigrants and natives in our main sample of movers. All income variables are measured in Israeli shekels (1 US $ ≈ 3.4 ILS). For immigrants, the sample includes all immigrants who either arrived in Israel and stayed in the same city or arrived in Israel and then moved again between cities in Israel before the child turned 18. For natives, the sample includes all families that moved either once or twice between cities in Israel before the child turned 18. Columns 1-2 present the statistics for all families, and columns 3-4 present the statistics for families in our selected sample of 98 cities and regional councils. Panel (A) displays children's mean income and mean income rank at age 28. Panel (B) displays mean parental income and parental income rank at the national distribution.

5 Estimates of Location Effects

5.1 Across-city Heterogeneity

Table 2 presents estimates of the distribution of causal location effects. Panel (i) reports the mean and standard deviation of α_{jg} and η_{jg} for immigrants and natives. As noted in Section 3.2, the cardinal value of location effects is not identified; therefore, for natives, they measure the effect of spending one more year in city j compared to one more year in Jerusalem and for immigrants, they measure effects compared to one more year in the former Soviet Union. To summarize the full one-year effect of each city, panel (ii) presents the same statistics for $\theta_{jpg} = \alpha_{jg} + \eta_{jg} \times p$, for $p = 25$ and $p = 75$, which we refer to as the location effects of low- and high-income families, respectively. Columns (1)-(3) display the main statistics for all cities in Israel that satisfy the sample restrictions separately for each immigration group, and columns (4)-(6) present them for the set of overlapping locations, where effects are estimated for both groups. Column (7) reports a Wald test statistic and corresponding p-value for the null hypothesis of no location effect heterogeneity across these overlapping cities.

The causal effects of cities vary substantially for both immigrants and natives. A total of 155 cities and regional councils satisfy the sample restriction for natives, and 99 cities and regional councils satisfy the sample restriction for immigrants. The average intercept α_{jg} of natives is 0.23, which implies that an extra year spent in the average city rather than Jerusalem boosts age-28 income for native-born children in the lowest income rank by 0.23 ranks. The corresponding estimate for immigrant children is 0.05 income ranks relative to staying one more year in the Soviet Union. Note that because the estimates are in relative terms, η_{jg} can obtain negative values.[13]

Panel (ii) summarizes the distribution of location effects for low- and high-income families, separately by immigrant status. For both groups, mean location effects for families in the 25th percentile are positive, and for immigrants, they are statistically distinguishable from zero.[14] For every year spent in the average city, low-income immigrant (native) earnings rank at age 28 increases by 0.138 (0.143) compared to the effect of spending one more year in the USSR (Jerusalem). This rank increase is equivalent to a 307 (264) Israeli shekels increase, which amounts to 90 (77) US dollars.

[13] In our fixed-effect model, we control for the interaction of parents' rank and children's birth cohorts. We test whether η_{jg} is always positive by computing the estimated coefficients of parental income rank and child-birth cohorts and add that number to $\hat{\eta}_{jg}$. Bai et al. (2022)'s testing procedure suggests that we can reject the null that η_{jg} is always negative, with p-values > 0.01, and cannot reject the null that it is always positive, with p-values > 0.99.

[14] This result suggests that immigrating earlier increases child income among immigrant children, in line with a large literature (Alexander and Ward, 2018; Connolly et al., 2023) on the effect of age of migration on children.

Table 2: Variation in location effects on adult income rank at age 28

	All cities			Overlap cities			
	# of cities	Mean	Std.	# of cities	Mean	Std.	χ^2 test $H_0 : \theta_j = \theta_1 \forall j$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(i) By α and η							
Natives							
Cons. (α)	155	0.228	0.256	98	0.212	0.227	160.4
		(0.121)	(0.050)		(0.121)	(0.048)	[0.0001]
Rank-parents (η)	155	-0.003	0.002	98	0.003	-0.003	161.2
		(0.001)	(0.001)		(0.001)	(0.001)	[0.0001]
Immigrants							
Cons. (α)	99	0.055	0.146	98	0.075	0.142	144.3
		(0.045)	(0.058)		(0.046)	(0.065)	[0.0016]
Rank-parents (η)	99	0.003	0.003	98	0.003	0.003	221.8
		(0.001)	(0.000)		(0.001)	(0.000)	[0.0000]
(ii) Total city effect							
Natives							
θ_{25}	155	0.143	0.263	98	0.130	0.186	152.5
		(0.110)	(0.039)		(0.110)	(0.042)	[0.0004]
θ_{75}	155	-0.026	0.312	98	-0.035	0.164	148.2
		(0.111)	(0.039)		(0.111)	(0.043)	[0.0008]
Immigrants							
θ_{25}	99	0.138	0.155	98	0.148	0.160	157.9
		(0.032)	(0.048)		(0.033)	(0.051)	[0.0001]
θ_{75}	99	0.302	0.237	98	0.295	0.249	250.0
		(0.036)	(0.034)		(0.037)	(0.036)	[0.0000]

Note: This table presents estimates of the distribution of causal effects of Israeli cities on income rank at age 28, separately for immigrant and native children. Columns (1)-(3) show estimates for all available cities, while columns (4)-(6) display estimates for cities with sufficient samples to estimate effects for both immigrants and natives. Estimates come from OLS regressions of child income rank on years of exposure to each location and interactions of years of exposure with parental income rank controlling for location sequences fixed effects and birth-cohort fixed effects interacted with parents' income rank. Panel (a) reports estimates of the distributions of location-specific intercepts (α) and slope coefficients on parental income rank (η). Columns (2) and (5) show the mean of each estimated parameter, and columns (3) and (6) show standard deviations, computed as the square root of the standard deviation of the bias-corrected variance of parameters across locations. Panel (b) displays corresponding distributions of location effects for children in the 25th and 75th percentiles of parental income distribution, computed as the sum of the location intercept and the parental income slope multiplied by the relevant percentile. Column (7) shows test statistics and p-values from chi-squared tests of the null hypothesis that all locations are identical. Standard errors for all variance estimators are based on the asymptotic variance, assuming that location effects are drawn from a normal distribution.

However, comparing the mean effects of natives and immigrants reveals heterogeneity in location effects with respect to parental income. While the effect of the average city on child's income for immigrants is always greater than zero, for low-income natives, the average city is better than Jerusalem, but for high-income, it's as good as Jerusalem.

The standard deviations of α_{jg}, η_{jg}, θ_{jg25}, and θ_{jg75}, presented in column (3), imply substantial across-city variation in location effects in Israel among different immigration and income groups. For families at the 25th (75th) percentile, the location effect standard deviation is 0.186 (0.164) for natives and 0.160 (0.249) for immigrants—comparable to US county-level estimates (Chetty and Hendren, 2018b). Interestingly, locations are more consequential for high-income immigrants than for high-income natives. The variance of the location effects for high-income immigrants is 40% larger than the variance for natives.

Moving at birth to a city with a one standard deviation higher location effect for natives (immigrants) increases children's income rank at age 28 by $0.18 \times 18 = 3.24$ ($0.16 \times 18 = 2.88$) ranks. To assess the monetary impact, we rescale the one-year location effects on rank to money value. Regressing children's income on their rank among children who spent their entire childhood in the same city shows that each percentile rank increase adds 1,530 shekels ($\approx$ \$450) for families at the 25th percentile and 1,689 shekels ($\approx$ \$490) for those at the 75th percentile. Therefore, a one standard deviation better city at birth increases the income of native-born (immigrant) children from the 25th percentile by 4,957 (4,406) ILS, or \$1,458 (\$1,295), which is around 8% of the mean income of children with parents with below median income.[15] For comparison, the return to a matriculation certificate in Israel is 13% (Angrist and Lavy, 2009). Thus, moving at birth to one standard deviation better city yields 61% of the gains from earning this credential.[16]

Columns (5)-(6) display the same statistics for the 98 cities and regional councils for which we estimate location effects for both immigrants and natives. For the remainder of the paper, we use this sample to study location effects in Israel and how they vary between immigrants and natives. The estimates of the first two moments in columns (2) and (3) are not qualitatively different from those in columns (5) and (6), suggesting that this is not a special subset of cities. Finally, column (7) presents the χ^2 test statistic and corresponding p-value for the null hypothesis of no location effect heterogeneity across these cities. For all city-level parameters, we reject this null at conventional significance levels.

5.2 Immigrant-Native Differences in Childhood Location Effects

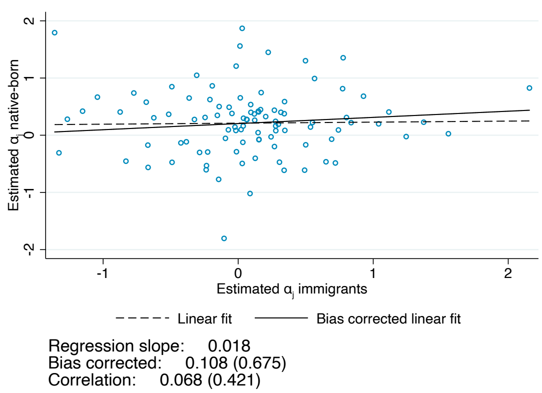
Location effects vary substantially between native and immigrant children at the same income level. This is illustrated in Figure 2, which presents scatter plots and observation-weighted regression lines of the estimates of effects for natives against the corresponding effects for immigrants, separately by income group. Figure 2a displays the relationship between immigrants' and natives' intercepts α_{jg}—i.e., between the location effects on families from the lowest income percentile; Figure 2b displays the relationship between the slopes η_{jg}—i.e., between the city returns to parental income; and Figures

[15]Average age-28 income for immigrant children from below-median-income families is 59,670 shekels, compared to 77,111 above the mean. For natives, these figures are 57,884 and 73,448 shekels, respectively.

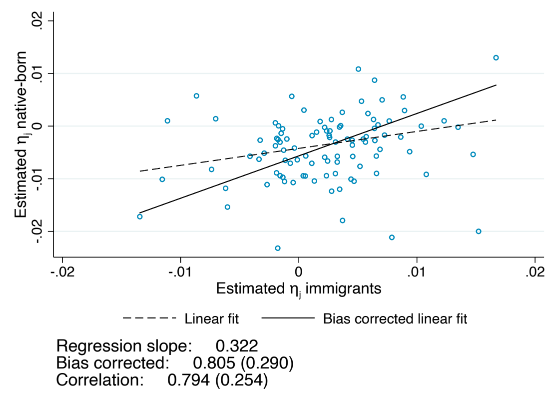
[16]A matriculation certificate requires passing standardized national exams in the final two years of high school and is a key determinant of future labor market outcomes, as most post-secondary institutions require it for admission.

2c and 2d display that relationship for total one-year location effects for families in the 25th and 75th percentiles of the national income distribution. Dashed lines are the naive attenuated regression lines, while solid lines are the biased corrected regression lines, with slopes estimated as the ratio between the covariance and the bias-corrected variance of immigrants' location effects. Table 3 presents the corresponding estimates, together with the mean and standard deviation of within-city immigrant-native location effects gaps.[17]

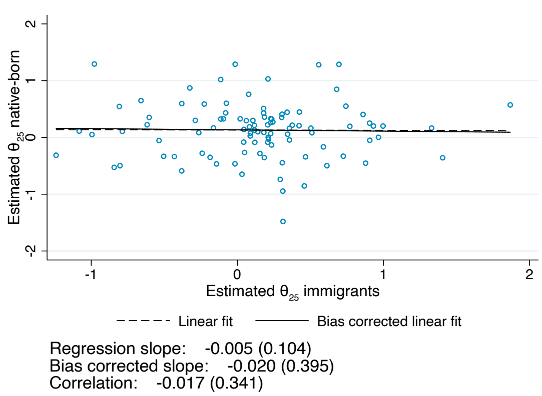
Figure 2: The relationship between location effects for immigrants and natives



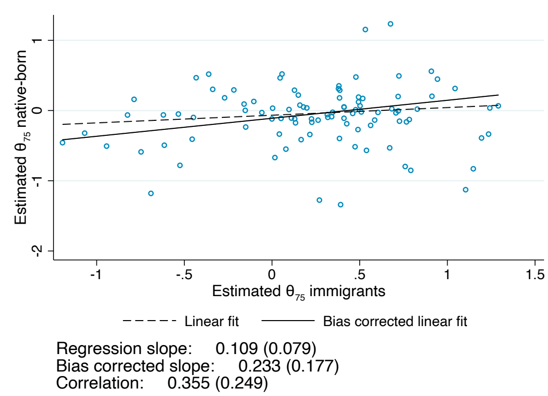
(a) α (intercept)



(b) η (slope)



(c) Total effect, θ_{25}



(d) Total effect, θ_{75}

Note: These figures display scatter plots and observation-weighted regression lines for immigrants' and natives' location effects. Panel (a) plots the estimated intercepts α_{jg}, panel (b) the estimated slopes η_{jg}, panel (c) the one-year location effect for families at the 25th percentile of the income distribution, and panel (d) the one-year location effect for families in the 75th percentile of the income distribution. The dashed line shows the naive regression line of $\hat{\theta}_{\mathcal{N}}$ on $\hat{\theta}_{\mathcal{I}}$ and the solid line the bias-corrected regression line with slope $Cov(\theta_{\mathcal{N}}, \theta_{\mathcal{I}})/Var(\theta_{\mathcal{I}})$ using the estimates in Table 3.

[17]The full correlation matrix of $(\alpha_{j\mathcal{N}}, \eta_{j\mathcal{N}}, \alpha_{j\mathcal{I}}, \eta_{j\mathcal{I}})'$ is reported in Appendix Table A.1.

The scatter plot and regression lines of the intercept in Figure 2a reveal substantial heterogeneity between immigrants and natives with the lowest parental income. Places that benefit low-income immigrants are not necessarily places that benefit low-income natives ($corr = 0.07$). In contrast, Figure 2b suggests much less heterogeneity in location effects as parental income increases. Places with high returns to parental income for immigrants tend to have high returns to parental income for natives. Combining these findings, Figure 2c shows no relationship between location effects for immigrants and native families at the 25th income percentile ($corr = -0.02$). However, for families in the 75th percentile (Figure 2d), the location effects of immigrants and natives are strongly correlated. A correlation coefficient of 0.36 suggests that locations with one standard deviation higher effects for high-income immigrants have %35 of a standard deviation higher effects for natives.

The standard errors of the correlation coefficients, calculated via the delta method, suggest that these correlations are imprecisely estimated. At the same time, the correlation is a highly nonlinear function for which the delta method approximation may be inaccurate. Therefore, we report in the square brackets of column 3 of Table 3 the bootstrapped equal-tailed 90% confidence intervals assuming normally distributed location effects. These intervals allow one-sided tests of whether each correlation coefficient equals 1. For low-income families—either at the bottom of the distribution or the 25th percentile—we can decisively reject correlations stronger than 0.4. In contrast, for η_{jg} (the return to parental income), the correlation is 0.80, and we cannot reject the null that it equals 1.

The last three columns of Table 3 report the mean and standard deviation of the immigrant-native location effect gap and the p-value for the test for within-city immigrant-native heterogeneity. First, column (5) reveals substantial heterogeneity in the city-level location effects immigrant-native gap. the standard deviation of this gap is 0.24 for families at the 25th and 75th income percentiles—50%-33% higher than the standard deviation of the effects themselves for low-income families. That is, moving at birth to a city with one standard deviation higher gap implies moving to a city that increases the adulthood income for one group by 8,598 ILS ($\approx \$2,623$) more than the other, which is more than 14% of the mean income at age 28 for children from a below-median-income family.

Column (6) presents p-values for the null hypothesis of no within-city differences in location effects. In line with our findings, we can decisively reject the null of no within city heterogeneity, except for the slope coefficients η_{jg}.

Table 3: Differences in location effects between immigrants and natives

	Covariance	Correlation	Implied OLS coefficient	Difference: Mean	Difference: Std.	Difference: χ^2 test $H_0: \theta_{j\mathcal{N}} - \theta_{j\mathcal{I}} = c\ \forall j$
	(1)	(2)	(3)	(4)	(5)	(6)
α	0.002	0.068	0.108	-0.137	0.259	142.1
	(0.013)	(0.421)	(0.675)	(0.130)	(0.077)	[0.002]
		[-0.524, 0.408]	[-0.551, 0.677]			
η	0.000	0.794	0.805	0.006	0.002	110.2
	(0.000)	(0.254)	(0.290)	(0.001)	(0.001)	[0.189]
		[0.403, 1]	[0.474, 1.412]			
θ_{25}	-0.001	-0.017	-0.020	0.018	0.248	150.2
	(0.010)	(0.341)	(0.395)	(0.115)	(0.062)	[0.0006]
		[-0.564, 0.355]	[-0.539, 0.374]			
θ_{75}	0.015	0.355	0.233	0.330	0.245	149.5
	(0.011)	(0.249)	(0.177)	(0.117)	(0.055)	[0.0006]
		[-0.088, 0.729]	[0.057, 0.589]			

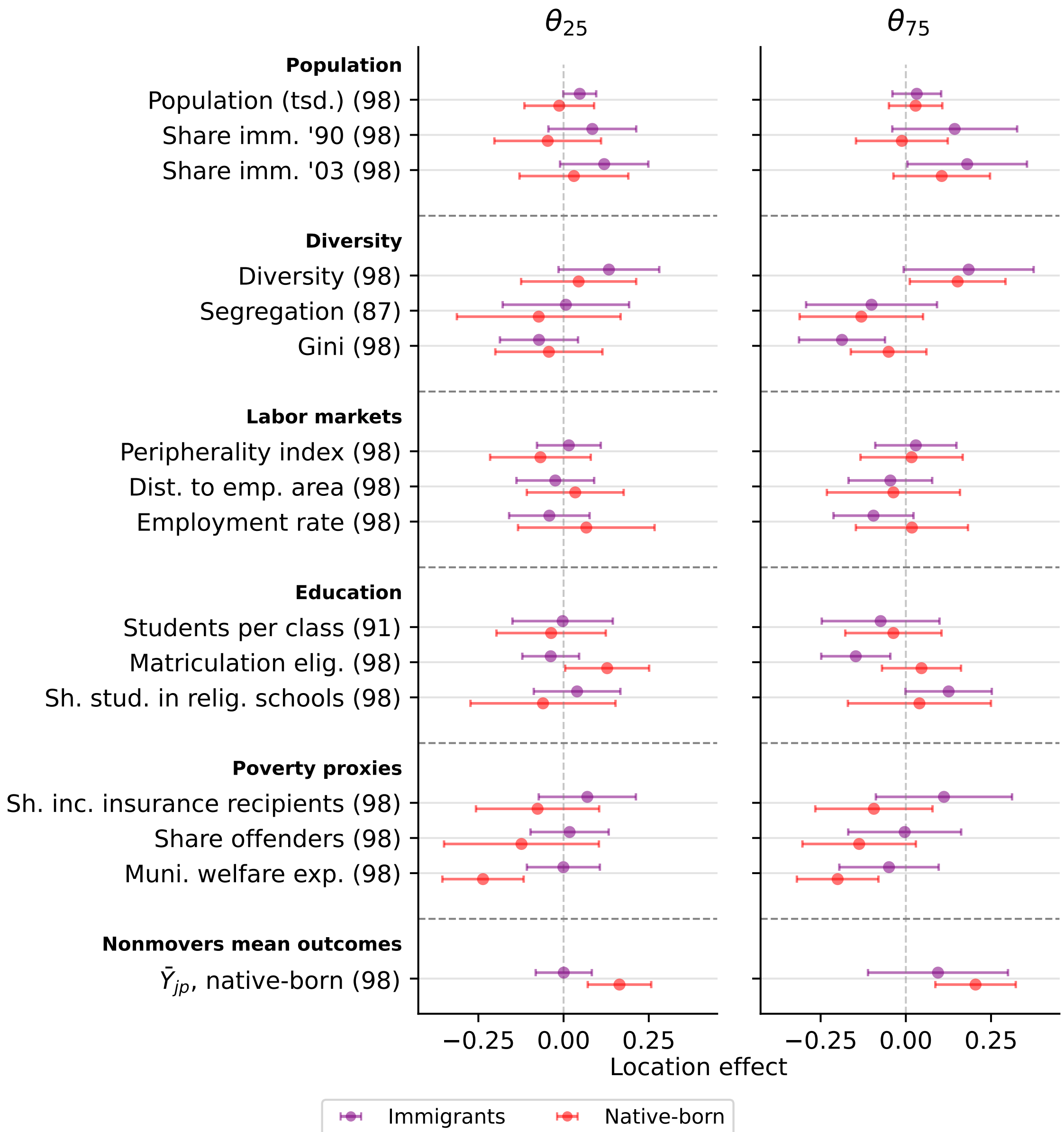
Note: This table reports the relationship between location effects of immigrants and location effects of natives and tests for within-city heterogeneity. Column (1) presents the covariance between the location effects of immigrants and natives, column (2) presents the bias-corrected correlation, which is the covariance divided by the standard deviation of immigrants times the standard deviation of locals, and column (3) presents the implied OLS coefficient, which is the covariance divided by the variance of immigrants. Column (4) presents the mean within-city gap between immigrants and natives, column (5) presents the standard deviation of the within-city gap, and column (6) presents test statistics and p-values from chi-squared tests of the null that location effects don't vary within cities. Location effect estimates come from OLS regressions of child income rank on years of exposure to each location and interactions of years of exposure with parental income rank, controlling for location sequences fixed effects and birth-cohort fixed effects interacted with parents' income rank. The first row reports estimates of the location-specific intercepts (α), the second row reports the estimates of the slope coefficients on parental income rank (η), and the last two rows report location effects for children in the 25th and 75th percentiles of parental income distribution, computed as the sum of the location intercept and the parental income slope multiplied by the relevant percentile. Standard errors of the variance and covariances are based on the asymptotic variance, assuming location effects are drawn from a normal distribution. Standard errors of the correlations and OLS slopes are calculated using the delta method. Square brackets display parametric bootstrapped equal-tailed confidence intervals.

6 Predictors of Location Effects

Next, we explore the characteristics of cities with high long-run effects on children's income by estimating the linear relationship between effects and characteristics at the city level. While this section focuses on describing the predictors of location effects, the next subsection discusses the possible explanations for the zero correlation between the location effects of immigrants and natives. Throughout this section, within each group, immigrants and natives, we demean the effects and the characteristics and divide them by the sample standard deviation. For most locality-level characteristics, we rely on data from the early 2000s collected from various sources. Detailed definitions of the variables and information about their sources can be found in Appendix Section B.2.

Figure 3 plots the coefficients from Weighted Least Squares (WLS) regression of the

Figure 3: Relationship between location effects and city characteristics



Note: This figure plots the relationship between city-level covariates and the total location effect of yearly exposure for high- and low-income families whose income rank is in the 25th (left panel) and 75th percentile (right panel) of the income distribution. Each relationship is estimated with a feasible generalized least squares regression, reweighting observations by the inverse of the Cholesky decomposition matrix of Σ, the variance of $\hat{\theta}$, with location effects as the outcomes. Covariates are standardized to have a mean of zero and a standard deviation of one in the sample. In each panel, the first column plots the coefficients from regressions of effects on each covariate alone, and the second column plots the coefficients of a multivariate regression with all the characteristics simultaneously. Bars indicate 95% confidence intervals based on robust standard errors. Appendix Section B.2 provides a complete description of covariates definitions. The number of cities in each regression is in parentheses. Cases with fewer localities than the full sample (98) are due to missing values or in the case of segregation, because values cannot be calculated for cities that do not have sub-areas (see Appendix Section F).

one-year location effect for immigrants and natives from the 25th and 75th percentiles of the national income distribution on city characteristics, reweighting by city size. The predictors for α_{jg} and η_{jg}—the location effects on families at the bottom of the income distribution, and the return to parental income— are presented in Appendix Figure A.3.

Population and diversity: The first rows in Figure 3 suggest that larger cities with large immigrant shares are associated with larger long-run causal effects on children of immigrants. Findings on the effects of the geographic concentration of ethnic groups on their economic outcomes are mixed. Ghettos—mostly of the Black population in the US–have been found to have negative, lasting effects (Massey and Denton, 1993; Cutler and Glaeser, 1997; Chyn et al., 2022a,b). In contrast, studies on refugees suggest a more nuanced relationship. On the one hand, aligned with our findings, a handful of papers find that larger enclaves improve refugees' labor market outcomes through networks and social support (Edin et al., 2003; Beaman, 2012). Interestingly, in these papers, the shares of immigrants in the city were at most 10%. On the other hand, in a recent study of large Jewish enclaves in New York from the beginning of the 20th century, Abramitzky et al. (2020) find that Jewish immigrants who left the enclave saw earnings gains for themselves and their children. In that setting, the Jewish enclaves were huge, comprising over 60% of Jews.

Inspired by this, we also estimate the relationship between diversity— measured with the entropy index– and location effects.[18] This index achieves its maximum value when city level immigrant share equals half and its lowest value when it is zero or one. The city-level diversity index positively predicts low-income immigrants' location effects and high-income location effects.

A positive correlation between group share and location effect could also reflect sorting, whereby immigrants are more likely to locate in places that benefit their children in terms of long-run economic outcomes. In the US, Chetty and Hendren (2018a) suggest that low-income families are less likely to reside in areas with large long-run effects on children. Our findings suggest that this is also the case in Israel for low-income native families but not for immigrant families.[19] Generally, our results call for more causal research to disentangle peer effects from sorting, with particular emphasis on

[18] Diversity is defined as: $-(\pi_{j\mathcal{I}} \ln(\pi_{j\mathcal{I}}) + (1-\pi_{j\mathcal{I}})\ln(1-\pi_{j\mathcal{I}}))$, where $\pi_{j\mathcal{I}}$ is the share of immigrants in city j.

[19] Abramitzky et al. (2021) find somewhat different results among immigrants in the US. They show that immigrants are more likely to reside in areas with high mobility rates. Mean area mobility rates probably reflect the mobility rates of natives, which suggests that immigrants live in places with high native-born long-run outcomes. Nevertheless, further evidence is required to compare their findings and ours since they don't estimate causal location effects but rather mean outcomes conditional on parental earnings.

the differences between immigrants and natives.

In contrast to evidence from the US, we find no relationship between low-income location effects and segregation, measured using Theil (1972) index and city Gini coefficient. Cities with higher income inequality are associated with high location effects, especially for immigrants.

Labor market: Several studies have posited that local labor markets influence children's future income by providing access to labor market opportunities (Wilson, 1987; Garin and Rothbaum, 2022). However, Figure 3 finds that employment rates and proximity to employment centers and Tel Aviv, Israel's economic hub, are not predictive of location effects. One possible explanation for that is Israel's small size, with essentially one major employment center around the Tel-Aviv metropolitan area.[20]

Education: In the next panel in Figure 3, we study the relationship between location effects and education inputs and outputs. For low-income families, cities with high rates of matriculation certificate attainment are associated with high location effects for natives but not immigrants. In Section 8, we investigate the role of high schools in further detail.

Poverty proxies: Figure 3 shows that municipality welfare expenditure per capita negatively predicts native-born children's location effects for all family incomes. The point estimates for the share of families receiving income insurance and the crime rate are also negative, although not precisely estimated. In our data, these are our best proxies for city poverty rates.[21] Interestingly, municipality welfare expenditure per capital is *not* predictive of immigrant location effects, further emphasizing the heterogeneity in our data. A negative relationship between location effects and poverty rates has also been found in the US and was one of the first measures the literature used for targeting housing policy (Katz et al., 2001). Their weak predictive power for immigrants suggests that using such a targeting policy would not be useful for immigrants in Israel.

City-level mean child rank conditional on parental income: Previous research has emphasized that observable mean child rank conditional on parental income rank is strongly predictive of location effects and suggests using these statistics for policy targeting (Chetty and Hendren, 2018b; Bergman et al., 2019). We demonstrate here

[20]See Appendix Figure A.4, which plots the number of workers across Israel's primary employment centers, as measured in the 2008 census.

[21]Unfortunately, there are no official records of poverty rates at the city level.

that due to the high heterogeneity in location effects, it is not predictive of the benefits for all groups. [22][23]

The last panel of Figure 3 shows that, in line with our evidence for heterogeneity, the native-born nonmovers' permanent residents' upward mobility rates, $\bar{Y}_{jp}$, are strongly predictive of natives' effects, with a point estimate of the same scale as in Chetty and Hendren (2018a). However, they have very little predictive power for low-income immigrants' location effects. These measures serve as the main instrument for guiding housing voucher policy (Bergman et al., 2019). Their weak predictive power for low-income immigrant place effects hints at the potential risk that may arise when using them to guide policy. We further discuss this risk in Section 10.

To sum up, our analysis provides two new facts about the type of cities that benefit immigrants and natives in Israel. First, unlike natives, low-income immigrants do benefit from populated cities, especially if these cities are diverse and have a high immigrant share. Second, previous literature emphasizes the relationship between poverty rates and upward mobility rates with causal location effects and uses them for neighborhood recommendations policies. Our findings hint at the possible costs to immigrants from guiding mobility policies based on such measures. In Section 10, we formally model the caution policymakers should undertake when designing policies in the face of heterogeneity.

7 Possible Mechanisms

There are several plausible explanations for the lack of correlation between the location effects of immigrants and natives. For instance, immigrants and natives might attend schools of differing quality or reside in different neighborhoods within cities. Other possible explanations include the mismeasurement of immigrants' parental income or the possibility that the lack of income rank correlation reflects low rates of social interaction.

In the following section, we directly test the first three explanations and find that they do not account for the zero correlation between the location effects of immigrants and natives. Although we are unable to fully disentangle the last explanation, we provide evidence suggesting that cities promoting social integration—measured by intermarriage rates—are more likely to exhibit high location effects for high-income

[22]Note that because all the immigrants are movers and included in our analysis, we can't compute the equivalent index for immigrants.

[23]We construct these estimates by running the following regression: $Y_i = a_{j(i)} + b_{j(i)}p(i) + u_i$, by city only on the sample of native-born children whose parents stayed in the same city throughout their childhood, where $p(i)$ is parental income rank and Y_i is child's income rank at age 28. The opward mobility rate of city j is then $\bar{Y}_{jp} = \hat{a}_j + \hat{b}_j p$.

immigrants, but not for low-income immigrants.

High school fixed effects: We approximate the role of high schools in explaining variability in location effects by comparing variance components from Equation (2) with those from a similar regression that also controls for high school fixed effects.[24] To avoid dropping observations from our original location effects model, we group high schools with fewer than five observations into one category.[25] This model is identified from cities with multiple schools and from schools that accept children from several local surrounding cities.

Appendix Table A.2 reports variance components for immigrants and natives with and without controlling for high-school fixed effects. The standard deviation of low-income native (immigrant) location effects declines from 0.18 (0.16) at baseline to 0.13 (0.10) when controlling for high-school fixed effects. Thus, high school effects explain $1 - \frac{0.13^2}{0.18^2} = 41\%$ ($1 - \frac{0.10^2}{0.16^2} = 37\%$) of the variation in location effects. However, the variance of the immigrant-native within-city gap is three times larger, and although the correlation coefficient becomes much noisier, the point estimate is more negative. This suggests that the zero correlation between immigrants and natives is not caused by schools. If anything, high schools in Israel act as equalizers. For high-income families, the drop in the correlation is even more striking, as, without the high school fixed effects, the correlation was strongly positive.

Neighborhood reweighting: The immigrant-natives within city heterogeneity might reflect differences in within-city sorting rather than heterogeneity in the effects themselves. To test that, one could estimate location effects at the neighborhood level and use these estimates to reconstruct the city-level effects as the equally weighted average of neighborhood effects.[26] This approach was taken in Card et al. (2022) to estimate industry-level wage premia as the average firm effects. We follow a similar approach. We estimate Equation (2) at a city level but reweight our regression inversely by the number of observations in each origin-destination(s) neighborhood cell, thereby equalizing the influence of each neighborhood on the aggregated city-level location effect.[27]

Appendix Table A.3 presents the results, with the first two rows showing the standard deviation and Wald test for no immigrant-native differences. Two key findings emerge.

[24] Ideally, we would have estimated high school exposure effects but used fixed effects due to computing limitations.

[25] As a result, there are 10 high schools in this grouped category.

[26] Unfortunately, we can't estimate the distribution of location effects at a neighborhood level due to our data agreement restrictions.

[27] For more information on how we build the geographic units of neighborhoods, see Appendix F.

First, columns 1 and 3 show that the standard deviation of the reweighted estimates is 2 to 4 times larger than the unweighted estimates,[28] Second, we find that even after accounting for the differences in the spatial distributions of immigrants and natives, there is still substantial within-city heterogeneity. The correlation between the location effects of low-income immigrants and natives remains zero for low-income families and strongly positive for high-income families, aligned with our baseline estimates in Table 3. Similarly, in Columns 2 and 4, we report the χ^2 test statistic and p-value for the test of no heterogeneity and decisively reject the null. These results suggest that although the differential spatial distributions matter for the magnitude of city-level location effects, they do not explain the disparities between immigrants and natives.

Parental income of immigrants not reflecting earnings potential: Immigrants face earnings penalties due to frictions such as language barriers, cultural differences, and lack of networks and information. Arellano-Bover and San (2023) estimate an immigrant-native earnings gap on arrival of 50%, which was fully closed only after 27-29 years. Therefore, if the heterogeneity in location effects with respect to parental income is driven by heterogeneity in skills, then immigrant parents' income rank is lower than their skill or ability rank would suggest. Therefore, we might classify high-earning-potential parents as low-earners. By doing so, when we compare the location effects of immigrants and natives, we do not compare families with the same set of skills.[29]

To accommodate this, Appendix Table A.4 displays the correlation matrix of high- and low-income families when estimating Equation 2, but instead of calculating parents' income rank in the national distribution, we do it within immigration groups and therefore rank parental income among comparable individuals. The standard deviation and correlations remain qualitatively similar to those in Table 2, suggesting that the negative relationship between low-income immigrants and natives is not due to misclassifying immigrants' income potential.

The role of assimilation: A growing literature emphasizes the role of social interactions on children's long-term economic outcomes Chetty et al. (2022a,b). Therefore, a possible explanation for the lack of correlation in location effects among low-income immigrant and native families—and the high correlation among high-income families—might be a lack of social interaction between the two groups among low-income families and higher assimilation rates among high-income families.

[28] This increase in variance after accounting for sorting aligns with Card et al. (2022), where the reweighted firm premium exceeds the non-reweighted in wage equations.

[29] Although we do compare families with the same resources in childhood.

To test that, we follow a long line of research in the social sciences that approximate social assimilation with intermarriage rates of immigrants and natives (Angrist, 2002; Meng and Gregory, 2005). As detailed in Appendix Section G, we proceed in two steps. First, we estimate the causal effect of each location in Israel on the intermarriage probability between immigrants and natives. In the second stage, we regress the income-rank location effects on the posterior mean estimates of intermarriage location effects. Interestingly, we find that while intermarriage effects are not predictive of immigrants' income-rank location effects for low-income families, they are predictive of high-income location effects. This result might suggest that a lack of assimilation and social interaction between low-income immigrants and natives could explain the absence of income-rank correlations.

8 Robustness and Research Design Validation

Research design validation: In Appendix Section D, we conduct several tests aimed at validating our research design and supporting our identification strategy. In Appendix Figures D.1 and D.2, we depict a balancing exercise for both immigrants and natives of the relationship between the age at move and age at arrival in Israel and parents' years of schooling, as measured in the 1995 census. For native-born children, we also estimate for the relationship between parents' earnings growth when the child was young and the child's age at move. We find no statistically significant relationship between age at move and family characteristics conditional on the sequence of location choice fixed effects and parents' income rank.

Linear location effects: The credibility of our approach also depends on a functional form assumption in which location effects are linear with the years of exposure. In Appendix Section D, we provide several specification tests for that functional form assumption. Appendix Figure D.5 shows that, similar to findings in the US (Chetty and Hendren, 2018a), Australia (Deutscher, 2020), and Canada (Laliberté, 2021), the relationship between years of exposure and the mean outcomes of children who spent their entire childhood in the same location is approximately linear. These figures suggest that in Israel, the last age at which locations affect outcomes (age A in our model) is approximately 18, a finding that aligns with the Israeli institutions where most individuals enlist in the army immediately after high school. Additionally, in Appendix Figure D.7, we provide a unique validation test, utilizing test scores realized in childhood, before age 18. The Figures suggest no relationship between mean test scores and children's test scores in that same exam for children moving after the age the exam was taken. Lastly, in Appendix Figure D.8, we provide evidence that not only the relationship between mean outcomes and exposure time is linear but that the relationship between location effects themselves and exposure time is well approximated by a linear function.

Change in natives' location effects: Derenoncourt (2022) finds that childhood location effects for incumbents could change due to a large migration wave. We test this hypothesis by estimating Equation (2) separately for older cohorts born between 1980-1987 and younger cohorts born between 1988-1991.[30] Then, in Figure A.5, we estimate the correlation between the location effects of older and younger cohorts, where a weak correlation would indicate that location effects have changed. While cutting the sample in half increases sample uncertainty, all the bias-corrected correlations we estimate suggest no change in location effects across the two groups.

Robustness checks and sensitivity: Appendix Section C estimates location effects using only one-time movers. This yields similar qualitative results despite the smaller sample size, which allows us to identify location effects for only 92 cities. Appendix Table A.5 shows that our findings—and in particular, the variance-covariance matrix of the location effects of immigrants and natives—are robust to alternative measures of income such as earnings and log earnings (excluding zeros). Lastly, our current approach reweights cities based on the total number of families. Appendix Table A.6 shows that results are robust to reweighting cities by the total number of movers (i.e., the total number of individuals who are included in our main regression sample) and to reweighting by city-level group size.

9 The Distribution of Childhood Location Effects

Next, we extend our model and estimate the joint distribution of immigrant-native location effects. We use this extended model for two tasks. First, to form the posterior mean effect of each city, which is the best location effect forecast that minimizes the mean squared error (James and Stein, 1961). Second, in Section 10, we exploit the joint distribution for a housing policy exercise, in which we generate predictions for other features of the joint distribution. In the following section, we briefly describe the model. For a more detailed discussion, see Appendix Section H.

9.1 Model and Estimation

As detailed in Appendix Section H, we find that each marginal distribution of θ_{jgp} is well approximated by a normal distribution by applying the log-spline estimator of

[30] An ideal test for this hypothesis would be to compare our current cohorts to cohorts who were children before the migration wave from the former Soviet Union, similar to Derenoncourt (2022). Unfortunately, there is no micro-level location data available for these years.

Efron (2016). Therefore, following the standard approach in the literature, we assume that location effects and their estimates are normally distributed.

To improve the predictive power of our model, we allow mean location effects to vary linearly with a few city-level covariates z_j that were found to be predictive of location effects in Section 6.[31] Formally, we denote $z_j \in \mathbb{R}^p$ the vector of p covariates of city j (including a constant one for the intercept) where $z = (z_i, .., z_J)'$ is the corresponding $J \times p$ matrix. z_j includes the following covariates. First, following our findings from Section 6, it includes the city-level diversity, population size, and locality welfare expenditure per capital. Since the diversity index is a function of the group shares of both immigrants and natives, it allows location effects to correlate with the mobility patterns of both groups. Lastly, following the recent literature, our analysis additionally controls for the first principle component of the noise variance-covariance matrix of $\hat{\theta}$ (Chen, 2023).

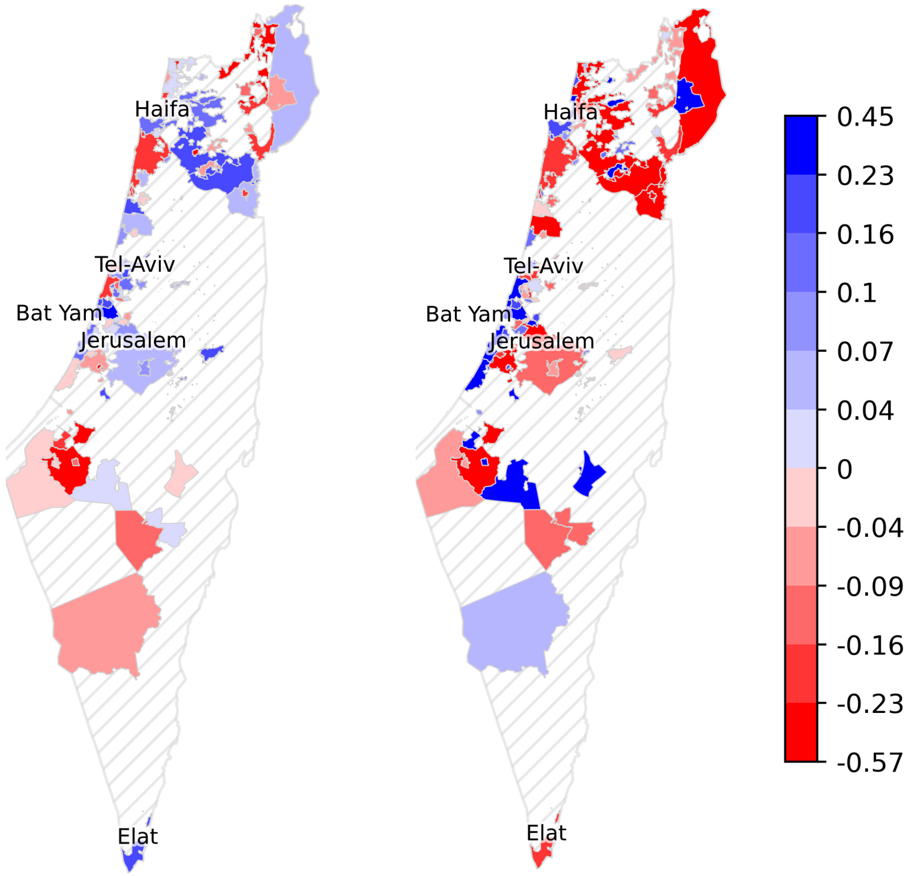
We estimate the model in two steps. First, we run a weighted least squares regression of $\hat{\theta}_{gp}$ on z separately for every group $g \in \{\mathcal{N}, \mathcal{I}\}$. Then, similar to Section 3.4, we estimate the city-size-weighted unbiased variance component by method of moments, accounting for the sampling error. As reported in Appendix Table H.1, our parsimonious extended model provides a good fit with high predictive power to each group's location effects, explaining between %34-%40 of the variation of low-income location effects.

Using the estimated hyperparameters from column (2) in Appendix Tables H.1 and H.2 as prior, we estimate the posterior mean effect of each location, shrinking each of the estimated location effects $\hat{\theta}$ toward the linear prediction of $\hat{\theta}$ on z. Even if the true location effects are not normally distributed, the posterior mean yields a prediction of θ that reduces the mean squared error at the cost of increased bias (James and Stein, 1961).

The location effects of low-income families: Figure 4 plots the demeaned posterior mean of location effects across cities and regional councils in Israel for immigrants and natives in the 25th percentile of the national income distribution. Each effect describes the group's specific annual effect on income rank at age 28 compared with the annual effect of the average city. In Appendix Table I.1, we provide the full lists of all 98 cities

[31] Analogous to correlated random effects model, the ideal model would incorporate the relationship between the full origin-destination network and location effects. Due to limited access to computing power with the microdata, we take a simplified approach that allows only the mean to vary linearly with a few main components of the mobility network. While this is suboptimal, this approach can be rationalized by a decision that approximates the constrained oracle who only has access to the estimates and few features (Chen, 2023).

Figure 4: Posterior mean location effects, low-income families



(a) Natives, θ_{25} (b) Immigrants, θ_{25}

Note: These maps plot children's posterior mean effects of year-long exposure to cities and regional councils in Israel for children's income rank at age 28 of low-income families whose parents are on the 25th percentile of the national income distribution. Figure (a) displays the effects for native-born children and Figure (b) the effects for immigrant children. The maps are constructed by grouping cities into 12 equally sized groups in which the darker blue the area the larger its effect compared to the mean, and the darker red the area the smaller the effect compared to the mean.

and regional councils posterior means.[32]

The posterior means are highly variable both across cities and within cities across groups. The posterior mean effect of spending one more year in the worst city ranged between -0.42 lower yearly income rank at age 28 and 0.43 higher income rank, which is approximately a change of 642 ILS per year ($\approx$ 189 US dollars). Comparing immigrants and natives, it is apparent that there are significant differences between the cities that benefit one group and the cities that benefit the other group. Among low-income

[32]The corresponding Figure and full list of effects on high-income families are in Appendix Figure A.6 and Appendix Table I.2.

families, many of the northern Israeli cities are found to be places that benefit natives but not immigrants. Southern cities on the coastline of Israel, which have a high immigrant share, are among the best cities for immigrants but are only as good as the average for natives.

10 Policy in the Face of Heterogeneity

Evidence on the importance of childhood residential location for children's long-term outcomes is the main motivation behind "moving to opportunity" policies, in which policymakers aim to motivate low-income housing voucher recipients to move to high-opportunity neighborhoods. Less recent literature emphasizes selecting areas for public housing based on their poverty rates (Katz et al., 2001), while more recent studies suggest targeting locations based on children's outcomes in adulthood conditional on parental income (Bergman et al., 2019). We find that location effects in Israel exhibit substantial heterogeneity, whereby the places that benefit low-income immigrants and native-born children are not necessarily the same places. Suppose we wanted to generate a list of recommended cities that provide the highest mobility for low-income children to inform housing policy in Israel, similar to Bergman et al. (2019). How does the treatment effect heterogeneity we documente affect the outcomes and design of the optimal policy? In this paper, we restrict attention to a model that maps closely to the selection of top places used in the CMTO experiment. We focus on a partial equilibrium analysis and start with a simplified model that abstracts from capacity and budget constraints. In Appendix Section J, we provide an extension that incorporates capacity constraints and restrictions on possible behavioral responses.

10.1 Setup

Consider a decision-maker whose task is to provide us with a single list of the top K cities in terms of their long-run effects on children's income in adulthood. Since public housing programs target low-income families, we restrict attention to a policy that takes into account only the long-run effects on low-income children. As such, hereafter, to ease notation, we drop the parental income rank p subscript.

We assume that the decision-maker faces two main restrictions. First, due to ethical or legal considerations, the decision-maker is restricted to a decision rule that provides the same list to all groups, regardless of immigration status.[33] This decision rule is described by the vector $\delta = (\delta_1, ..., \delta_J)'$, where $\delta_j \in \{0, 1\}$ indicates whether city j is selected by the policymaker. For example, in Bergman et al. (2019), the authors prespecified a list of neighborhoods that promote upward mobility. Then, in an experiment on housing voucher recipients, they recommended families in the treatment

[33] For example, in the US, court rulings have disallowed race-based housing policies (Tegeler, 2009).

group move to one of the neighborhoods on their list. Therefore, restricting the policy to be unified in this context implies precluding the possibility of providing different recommendations to different groups. Another example could be a decision-maker who wants to choose K locations for new public housing units for low-income families. Here, a unified decision rule implies that housing agencies cannot restrict access to an existing housing unit based on group characteristics.

Second, we assume that our decision-maker faces uncertainty regarding the true value of θ, and therefore can not form the decision δ based on the true location effects θ. While θ is unknown, we assume that instead, the decision-maker knows the distribution of θ and observes the estimates of θ and their variance, which we collect in the array $\mathcal{Y} = (\hat{\theta}, \Sigma)$. As a result, the decision-maker forms decisions by minimizing the expected, rather than the true, loss, where the expectation is based on the posterior distribution of θ given the evidence $\mathcal{Y}$.

To evaluate the performance of different selection criteria and compare the gains for each group, we assume the decision-maker evaluates the benefit of a decision rule relative to a first-best policy under full certainty about θ. The top K cities with the highest location effects on each group $g \in \{\mathcal{N}, \mathcal{I}\}$ are selected by the *oracle* rule:

$$\delta^*_{jgK} = \mathbb{1}\{\theta_{jg} \in \{\theta^{(1)}_{jg}, \theta^{(2)}_{jg}, ..., \theta^{(K)}_{jg}\}\}$$

where δ^*_{jgK} is the first-best policy and $\theta^{(l)}_{jg}$ is the l'th order statistic of the location effects of group g—i.e., the l'th largest value of θ_{jg}.

We define $\theta^*(\delta^*_{jgK}, K) \equiv \frac{1}{K}\sum_{j=1}^{J} \mathbb{E}[\theta_{jg}\delta^*_{jgK}]$ as the group g's expected long-run effect of selected cities under the first-best. Equipped with these definitions, the decision-maker values the return to each city in comparison with the expected first-best value:

$$\vartheta_{jgK} = \theta^*(\delta^*_{jgK}, K) - \theta_{jg}. \tag{4}$$

Equation 4 describes the *regret* of not using the first-best policy (Savage, 1954; Manski, 2004). It reflects the loss experienced by the decision-maker not bound by ethical or legal constraints.

10.2 Benchmark: Selection Based on the Average Effect

We start with a model that rationalizes Bergman et al. (2019)'s selection criteria, in which the goal of the decision-maker is to choose the cities with the highest city-level location effects on the full population. Formally, the decision-maker would like to choose the list of selected cities, δ, by minimizing the following loss function:

$$\mathcal{L}(\vartheta, \delta, \pi^0) = \sum_j \delta_j (\pi^0_{j\mathcal{I}} \vartheta_{j\mathcal{I}K} + (1 - \pi^0_{j\mathcal{I}}) \vartheta_{j\mathcal{N}K}) \tag{5}$$

subject to $\sum_{j=1}^{J} \delta_j = K$, where $\pi^0_{j\mathcal{I}} = \frac{n_{\mathcal{I}j}}{n_{\mathcal{I}j} + n_{\mathcal{N}j}} \in [0, 1]$ is the share of immigrants in city j in the data and n_{gj} the number of group $g \in \{\mathcal{N}, \mathcal{I}\}$ families in city j. This loss function implies that the decision-maker would like to rank places based on the pooled city-level population mean effect, which describes how people sort within cities under the status quo:

$$\bar{\vartheta}_{jK} = \pi^0_{j\mathcal{I}} \vartheta_{j\mathcal{I}K} + (1 - \pi^0_{j\mathcal{I}}) \vartheta_{j\mathcal{N}K}, \tag{6}$$

and select the cities with the lowest $\bar{\vartheta}_{jK}$. Since immigrants are a minority group, the average city index assigns a small weight to their losses, disproportionally favoring the native-born group. With zero correlation between immigrants' and natives' location effects, by construction, places with low $\bar{\vartheta}_{jK}$ are more likely to be beneficial for natives but not necessarily beneficial for immigrants.

The decision-maker does not observe location effects θ_{jg} directly. Instead, we assume she treats the joint distribution from Section 9 as a prior and makes decisions based on the vector of location effects and their variance matrix $\mathcal{Y}$.Therefore, the decision-maker minimizes the expected loss—i.e., the Bayes risk—by choosing δ to minimize:

$$\begin{aligned}\mathcal{R}(\delta; \pi^0) &= \mathbb{E}[\mathcal{L}(\vartheta, \delta, \pi^0) | \mathcal{Y}] \\ &= \sum_j \delta_j (\pi^0_{j\mathcal{I}} \mathbb{E}[\vartheta_{j\mathcal{I}K} | \mathcal{Y}] + (1 - \pi^0_{j\mathcal{I}}) \mathbb{E}[\vartheta_{j\mathcal{N}K} | \mathcal{Y}]),\end{aligned}$$

where the expectation is taken over the posterior distribution of location effects given the evidence $\mathcal{Y}$, and $\mathbb{E}[\vartheta_{jgK} | \mathcal{Y}]$ is the posterior mean of the regret of city j and group $g \in \{\mathcal{N}, \mathcal{I}\}$. Therefore, the Bayesian decision-maker ranks locations by their posterior expected regret:

$$\mathbb{E}[\bar{\vartheta}_{jK} | \mathcal{Y}] = \pi^0_{j\mathcal{I}} \mathbb{E}[\vartheta_{j\mathcal{I}K} | \mathcal{Y}] + (1 - \pi^0_{j\mathcal{I}}) \mathbb{E}[\vartheta_{j\mathcal{N}K} | \mathcal{Y}],$$

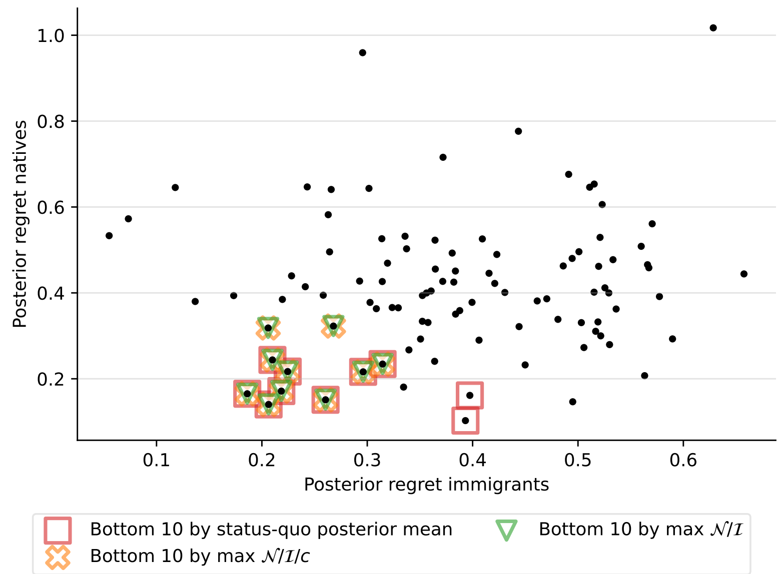
and the optimal decision rule takes the following form:

$$\delta_{jK} = \mathbb{1}\{\mathbb{E}[\bar{\vartheta}_{jK} | \mathcal{Y}] \leq \kappa_K\},$$

with κ_K being the value of the Kth lowest posterior mean $\mathbb{E}[\bar{\vartheta}_{jK} | \mathcal{Y}]$. In what follows, we refer to this policy as targeting based on the *average status quo* sorting patterns.

Figure 5 displays a scatter plot of the posterior mean of ϑ for immigrants and natives,

Figure 5: Selected Cities Under the Average and Minimax Policies, $K = 10$



Note: This figure displays the scatter plots of immigrants' and natives' posterior mean regret for families at the 25th percentile of the income distribution. Black dots are the posterior mean regret for immigrants and natives. Red squares present the cities selected by the status-quo mean policy; Green rectangles present the cities selected by the minimax ($\mathcal{N}/\mathcal{I}$) policy; Orange Xs present the cities selected by the minimax ($\mathcal{N}/\mathcal{I}$) policy.

where the red squares represent the cities selected under the mean status-quo policy when $K = 10$. We can see that while the regret for natives is bounded below 0.3 ranks per year, the regret for immigrants could be 50% larger.

10.3 Accounting for Unknown Behavioral Responses

We now turn to explore alternative policies that strive to avoid harming any of the groups that are being treated. Harm arises in our setting for two reasons. First, due to the inability of the decision-maker to provide personalized recommendations ex-ante, together with the lack of information regarding which families will actually follow through and move to the recommended locations ex-post. Take-up uncertainty is a built-in restriction in the literature where suggested policies are primarily based on estimates of location effects but not on estimates of demand elasticities that incorporate information on recipients' compliance. This shortcoming was raised in Mogstad et al. (2024), who point out that there is no guarantee that families who received a recommendation in the CMTO experiment will sort into places whose location effects are indeed high.[34]

[34]Similar concern was also raised in Pope and Sydnor (2011), who study statistical decision rules under anti-discrimination policies and note that the economic efficiency of such rules depends on

In this section, we propose a possible remedy by acknowledging the compliance uncertainty the decision-maker faces.

10.3.1 Who Shows Up?

We start with a simplified toy model. Consider a scenario in which the decision-maker is uncertain about the identity of the families that move to each recommended location. Such a scenario could arise, for example, if the decision-maker's task is to select a list of K cities for new public housing units, with one unit in each chosen city. The loss function in this model mirrors Equation (5), but the weights on each city represent the probability that an immigrant or native family will eventually move into the housing unit built in city j. This probability, denoted by $\pi_{j\mathcal{I}}$, is therefore a function of families' preferences, constraints, information, and responses to the policy, all of which are unknown.

Facing this uncertainty, the decision-maker can take several paths. Analogous to how the decision-maker handles uncertainty with respect to each location effect θ, she can form a prior distribution on $\pi_{j\mathcal{I}}$ based on her beliefs. One justification for the decision rule in Equation (6) is that the decision-maker's prior reflects a belief that public housing recipients sort according to the status quo—that is, similar to existing sorting patterns within each city—regardless of the policy they face. We opt for a different approach, acknowledging our ignorance regarding family sorting behaviors. Our goal is to devise a policy that is robust to the least favorable compliance scenario: this is the *minimax* strategy (Wald, 1950), which was axiomatized by Gilboa and Schmeidler (1989).

Formally, given the vector of location effects θ, for every list of recommended cities δ, the least favorable compliance pattern implies that the worst-case regret is

$$\mathcal{L}^{(\mathcal{N},\mathcal{I})}(\vartheta,\delta) = \max_{\pi} \mathcal{L}(\vartheta,\delta,\pi) = \sum_j \delta_j \max_{\pi_j} \left\{ \pi_{j\mathcal{I}}\vartheta_{j\mathcal{I}K} + (1-\pi_{j\mathcal{I}})\vartheta_{j\mathcal{N}K} \right\}. \tag{7}$$

If the decision-maker knew the location effect of each city, she would minimize (7). However, with uncertainty regarding the true value of θ, the decision-maker chooses δ to minimize the following expected maximum loss:

$$\mathcal{R}^{(\mathcal{N},\mathcal{I})}(\delta) = \mathbb{E}\left[\mathcal{L}^{(\mathcal{N},\mathcal{I})}(\theta,\delta)\middle|\mathcal{Y}\right] = \sum_j \delta_j \mathbb{E}\left[\max_{\pi_j} \left\{ \pi_{j\mathcal{I}}\vartheta_{j\mathcal{I}K} + (1-\pi_{j\mathcal{I}})\vartheta_{j\mathcal{N}K} \right\} \middle|\mathcal{Y}\right] \tag{8}$$

subject to $\sum_{j=1}^{J} \delta_j = K$, where the expectation is taken over the posterior distribution of θ given the evidence $\mathcal{Y}$. Decisions motivated by optimizing objective functions

individuals' behavioral responses.

involving both parameters that are not identified ($\pi(\delta)$) and parameters that are point identified (θ) are sometimes referred to in the literature as robust Bayes decisions (Giacomini et al., 2021; Christensen et al., 2022). They are equivalent to a zero-sum game with nature, where nature knows the true location effects and, for every choice of recommended list of cities δ, it chooses the worst behavioral response π. By minimizing the maximum *regret*, the decision-maker tries to achieve the oracle's first-best solution without violating the horizontal equity constraint.

Minimizing the objective function in Equation (8) yields the following decision rule, in which the optimal policy is to rank locations based on their expected within-city posterior maximum regret:

$$\delta_{jK}^{(\mathcal{N},\mathcal{I})} = \mathbb{1}\{\mathbb{E}[\max\{\vartheta_{j\mathcal{I}K}, \vartheta_{j\mathcal{N}K}\}|\mathcal{Y}] \leq \kappa_K\}, \tag{9}$$

where κ_K is the maximum value such that there are exactly K cities with $\mathbb{E}[\max\{\vartheta_{j\mathcal{I}K}, \vartheta_{j\mathcal{N}K}\}|\mathcal{Y}] \leq \kappa_K$. Since this policy arises under uncertainty with respect to group identity, we refer to it as *minimax over $(\mathcal{N},\mathcal{I})$*.

Figure 5 displays the posterior mean regret for immigrants and natives from each selected city, where the green rectangles mark the cities selected under the minimax $(\mathcal{N},\mathcal{I})$ policy. Unlike the cities selected under the mean status-quo policy, cities selected under $(\mathcal{N},\mathcal{I})$ decision rule identify places that provide relative benefits (bounded regret) for both groups.

Connection to welfare economics: The spectrum of objectives between that implied by the observation-weighted loss function in Equation (5) and the minimax loss function in (7) map to the familiar social welfare criteria. At one extreme, Equation (5) can be thought of as a utilitarian social welfare function that linearly aggregates benefits across different groups, with each group's population share serving as the decision-maker's social welfare weights. At the other extreme is the minimax decision loss function in Equation (7), which is equivalent to a Rawlsian decision-maker with extreme equity preferences. The range of social preferences between the linear and the Rawlsian utility functions depends on the marginal rate of substitution between the two groups and reflects the decision-maker's attitudes towards equity.

10.3.2 Who Shows Up and Where Do They Go?

Next, we consider an example of a decision-maker who faces uncertainty not only regarding the identity of each housing recipient but also regarding families' location choices. This model is directly inspired by Bergman et al. (2019)'s experiment. In this framework, the decision-maker recommends housing voucher recipients to relocate to one of the top K cities that provide high opportunities for low-income children.

Then, given the recommended list δ, each family $g \in \{\mathcal{N}, \mathcal{I}\}$ sorts into cities according to the function $\pi_{jg}(\delta) \in [0,1]$, such that $\sum_{j=1}^{J}(\pi_{j\mathcal{N}}(\delta) + \pi_{j\mathcal{I}}(\delta)) = 1$. Hence, the decision-maker seeks to minimize the following loss function:

$$\mathcal{L}(\vartheta, \delta, \pi(\delta)) = \sum_{j=1}^{J} \Big[\delta_j (\pi_{j\mathcal{I}}(\delta)\vartheta_{j\mathcal{I}K} + \pi_{j\mathcal{N}}(\delta)\vartheta_{j\mathcal{N}K}) + (1-\delta_j)(\pi_{j\mathcal{I}}(\delta)\vartheta_{j\mathcal{I}K} + \pi_{j\mathcal{N}}(\delta)\vartheta_{j\mathcal{N}K}) \Big] \tag{10}$$

subject to $\sum_{j=1}^{K} \delta_j = K$. To avoid a degenerate minimax solution, we restrict attention to behavioral responses that satisfy full compliance, in which, given a selected list of recommended cities, recipients follow the recommendation and move to one of the cities on the list. We justify this approach following the findings of the CMTO experiment. First, the CMTO experiment suggests that Bergman et al. (2019) were able to build a technology that induces substantial compliance, which increased the share of families moving to recommended places by more than 38%. Second, Bergman et al. (2019) find that the sorting pattern of the control group in the CMTO experiment aligns with the sorting pattern in the status quo, absent the experiment. Therefore, the second part of the loss function in Equation (10) is likely constant, and consists of the share of non-compliers and the expected regret from the status quo sorting patterns.[35] Therefore, we proceed by restricting attention to the decision-maker who would like to minimize the following loss function:

$$\mathcal{L}(\vartheta, \delta, \pi(\delta)) = \sum_{j=1}^{J} \delta_j (\pi_{j\mathcal{I}}(\delta)\vartheta_{j\mathcal{I}K} + \pi_{j\mathcal{N}}(\delta)\vartheta_{j\mathcal{N}K}) \tag{11}$$

subject to $\sum_{j=1}^{J} \delta_j = K$. We consider a decision-maker who seeks a policy that is robust to the least favorable location choices. For a given list of cities δ and location effects θ, the loss under such sorting behaviour is

$$\mathcal{L}^{(\mathcal{N},\mathcal{I},city)}(\vartheta, \delta) = \max_{\pi(\delta)} \mathcal{L}(\vartheta, \delta, \pi(\delta)) = \max_{\pi(\delta)} \left\{ \sum_{j=1}^{J} \delta_j (\pi_{j\mathcal{I}}(\delta)\vartheta_{j\mathcal{I}K} + \pi_{j\mathcal{N}}(\delta)\vartheta_{j\mathcal{N}K}) \right\},$$

[35] Formally, if we denote $\delta = 0$ as the no-policy where no recommendation is made and represent the regret from the no-policy as $\mathcal{L}(\vartheta, 0) = \sum_{j=1}^{J}(\pi_{j\mathcal{I}0}\vartheta_{j\mathcal{I}K} + \pi_{j\mathcal{N}0}\vartheta_{j\mathcal{N}K})$, where π_{jg0} is the share of group g families in city j in the status quo out of the entire population. Then, the loss function of the decision-maker is:

$$\mathcal{L}(\vartheta, \delta, \pi(\delta)) = \omega \sum_{j=1}^{J} \delta_j (\pi_{j\mathcal{I}}(\delta)\vartheta_{j\mathcal{I}K} + \pi_{j\mathcal{N}}(\delta)\vartheta_{j\mathcal{N}K}) + (1-\omega)\mathcal{L}(\vartheta, 0),$$

where ω is the share of compliers. In this model, non-compliers will not affect the optimal policy of choosing K locations.

where $\pi(\delta) = (\pi_{1\mathcal{N}}(\delta), \pi_{1\mathcal{I}}(\delta), ..., \pi_{J\mathcal{N}}(\delta), \pi_{J\mathcal{I}}(\delta))'$. This loss arises when compliers belong to the immigration group with the highest regret and sort to the worst recommended city. Hence, the robust Bayesian policy aims to select the δ that minimizes the expected loss given the evidence $\mathcal{Y}$:

$$\mathcal{R}^{(\mathcal{N},\mathcal{I},city)}(\delta) = \mathbb{E}[\mathcal{L}^{(\mathcal{N},\mathcal{I},city)}(\vartheta, \delta)|\mathcal{Y}]$$

subject to $\sum_j \delta_j = K$. This objective function yields a decision rule in which the optimal policy is to rank lists of locations of size K based on their expected maximum regret across all cities on that list and across all groups:

$$\delta_K^{(\mathcal{N},\mathcal{I},\text{city})} = \arg\min_{\delta} \mathbb{E}[\max(\{\vartheta_{j\mathcal{N}K}, \vartheta_{j\mathcal{I}K}\}_{j\in S(\delta)})|\mathcal{Y}], \tag{12}$$

where $S(\delta) = \{j : \delta_j = 1\}$ is the set of recommended cities. Under this decision rule, the decision-maker evaluates the posterior expectation of the maximum regret across all selected locations and across immigrants and natives and chooses the list that attains the lowest worst-case regret. Therefore, hereafter we refer to this policy as *minimax over* $(\mathcal{N},\mathcal{I},city)$.

Figure 5 displays the posterior mean regret for immigrants and natives from each selected city, where the orange Xs are the cities selected under the minimax $(\mathcal{N}, \mathcal{I}, city)$. In our setting, the cities selected under the minimax $(\mathcal{N}, \mathcal{I}, city)$ decision rule are the same cities as those selected under the minimax $(\mathcal{N}, \mathcal{I})$ policy, therefore it identifies the places that provide relative benefits (bounded losses) for both groups.

10.4 Evaluation of Each Policy

We evaluate the expected benefits and tradeoffs from each of the policies mentioned above via a simulation exercise. We simulate the location effects for low-income families of the 98 cities implied by the mixing distribution in Appendix Table **??**. In the first step, we use the simulation to compute $\theta^*(\delta_g^*, K)$. For every draw, number of selected cities K, and group, we calculate the optimal first-best policy δ_{jgK}^* and compute $\theta^*(\delta_g^*, K)$ as the across simulations average effect of selected cities under the first-best policy. Then, in the next step we compute the regret values ϑ_{jgK} as the difference between each draw of θ_{jg} and $\theta^*(\delta_g^*, K)$.

For every simulated draw and for a grid of values of K, we estimate the mean status-quo average policy and the minimax policies. As detailed in Appendix J, empirical Bayes posterior expectations of the maximum are computed by numerical integration. Lastly, for every policy, we compute the expected outcomes for each group by averaging across bootstrap draws.

10.4.1 Expected Benefits

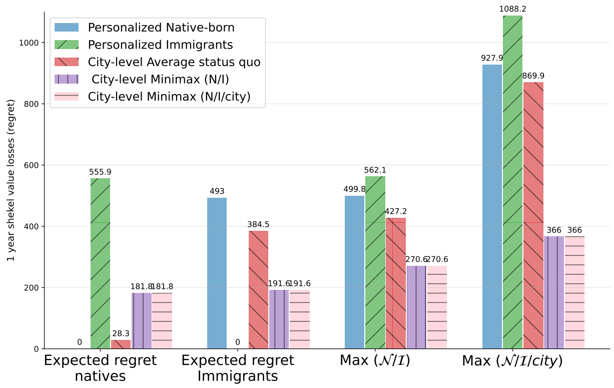
Figures 6a and 6b illustrate the costs and benefits of employing each of the three policies described in Section 10 for a given $K = 10$. Values are the shekel value of the regret associated with an additional year of exposure. That is, they reflect the expected money lost from not residing in the expected group-specific first-best city for one year. The first two groups of bars report the expected true regret of immigrants and natives from the selected cities; the third, $max(\mathcal{N}/\mathcal{I})$, reports the expected true within-city maximum regret $\frac{1}{K}\sum_{j=1}^{J}\max\{\vartheta_{j\mathcal{N}K}, \vartheta_{j\mathcal{I}K}\}\delta_j$ across immigrants and natives in selected cities; and the forth, $max(\mathcal{N}/\mathcal{I}/city)$, reports the true expected maximum regret across all selected cities, immigrants, and natives. In subfigure 6a, we evaluate each of the policies described above when the decision-maker knows θ, and in subfigure 6b, we consider a decision-maker who doesn't know θ but relies on our estimated location effects and their standard errors $\mathcal{Y}$, as described in the previous section.

As a result of the lack of correlation between location effects for immigrants and natives, policy recommendations based on the effects on one group generate substantial regret for the other. This can be seen in the blue and green bars, which present the personalized first-best policy in which the decision-maker recommends the top 10 locations based on the returns of only one of the groups, either immigrants or natives. By construction, under full information on location effects (subfigure 6a), the first-best policy of each group generates no regret. Since location effects are not correlated, the average one-year regret for immigrants sent to the top locations for natives provides 555.9 ILS ($\approx$ \$163) lower income in adulthood compared with the first-best. Similarly, sending native-born children to the top 10 immigrant places implies sending them to places that generate 493 fewer shekels ($\approx$ \$145) in adulthood per year, compared to the first-best.

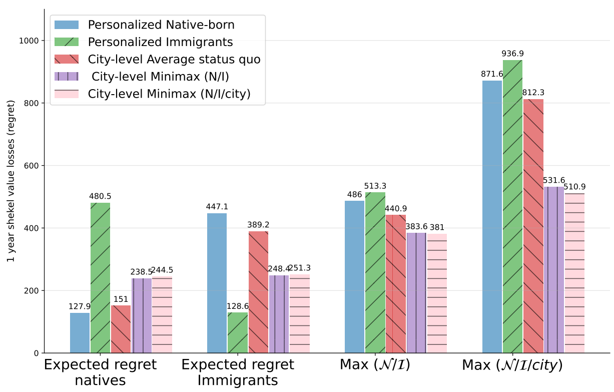
When θ is unknown (subfigure 6b), the policy is based on empirical Bayes shrinkage of functions of the estimated location effects, whereas in the personalized policy, the places are ranked according to the posterior mean of each location effect for every group. The blue and green bars show that for immigrants (natives), the average recommended location under that feasible first-best generates 128 (127) ILS fewer shekels in adulthood than the group's first-best. This loss is driven entirely by noise uncertainty that the former literature was worried about (Mogstad et al., 2024). Nevertheless, the losses from providing the personalized policy of one group to the other under uncertainty are similar to those when the decision-maker faces full certainty.

Policies based on the status quo mean effects place more weight on the gains of natives and, therefore, provide much weaker gains for immigrants. This is illustrated in the red bars, which show that the status-quo average policy nearly attains the first-best

Figure 6: Targeting trade-offs from choosing the top 10 cities



(a) Known θ (oracle)



(b) Unknown θ

	% better than the status-quo	
	Known θ (oracle) (1)	Unknown θ (2)
i) Personalized policy		
Native-born	0.761	0.786
Immigrants	0.501	0.591
ii) City-level policy		
Av. status-quo	0.836	0.831
Minimax ($\mathcal{N}/\mathcal{I}$)	1.000	0.885
Minimax ($\mathcal{N}/\mathcal{I}$/city)	1.000	0.887

(c) Share worse than the status-quo

Note: These plots present the expected regret from different policies that aim to select the top 10 places in Israel based on the location effects of children whose parents are in the 25th percentile of the income distribution. Regret is defined as the difference between the one-year location effects and the average benefits from the top 10 cities for each group, immigrants and natives. Regret is in shekel values (1 US \$ $\approx$ 3.4 ILS). Subfigure (a) displays results from policies based on the true location effects, and subfigure (b) displays the results from policies based on the expected values of location effects, where the expectation is taken over the posterior distribution of location effects conditional on the estimated location effects and standard errors, and the distribution of location effects from Table H.1 is treated as prior. The first two groups of bars report the expected regret of each policy for every group, immigrants and natives. The $Max(\mathcal{N}, \mathcal{I})$ bars report the expected within-city immigrant-natives maximum regret among selected cities, and the $Max(\mathcal{N}, \mathcal{I}, city)$ bars report the expected maximum regret across all selected cities and immigrants and natives. The blue and green bars report the results from a policy that ranks locations based on the regret of each group. The red bars report the policy that ranks locations based on the city-level average regret as described in Equation (6). The purple bars report the minimax ($\mathcal{N}/\mathcal{I}$) policy that ranks locations based on the city-level immigrant-native maximum regret as described in Equation (9). The pink bars report the minimax ($\mathcal{N}/\mathcal{I}$/city) policy that ranks lists of 10 cities based on maximum regret across all the cities and groups in the list, as described in Equation (12). Subplot (c) presents the share of selected places that provide benefits that are higher than that under the status quo sorting patterns of both immigrants and natives.

outcomes for natives. Under full information about location effects, the status-quo average policy generates only 28 fewer shekels (< \$10) for natives compared with the first-best. Likewise, the Bayesian decision-maker attains 151 fewer shekels per year (≈\$45), which is only 18% more than the feasible first-best. Accordingly, the losses for immigrants from the status quo average policy are almost as bad as the personalized recommendation that is based only on the gains of natives. They generate 78% of the losses for immigrants from the natives' oracle personalized recommendation and 82% of the losses under the Bayesian natives' personalized recommendation. In the table right next to these figures, we show the share of selected places that are better than the status quo mean. Column 1 shows that even with full information on the location effects, under the status-quo average recommendation, almost 2 out of the 10 selected cities will end up with outcomes lower than the expected outcome under the status-quo sorting patterns for either group.

While the status-quo average policy generates losses for the minority group, a minimax regret analysis shows that it is possible to avoid extreme adverse outcomes for both groups. The purple and pink columns report the average regret under the minimax $(\mathcal{N}, \mathcal{I})$ and $(\mathcal{N}, \mathcal{I}$, city) policies described in Equations (9) and (12), in which the decision-maker is ambiguous with respect to future behavioral responses. The second row reports results for ranking places based on maximum group regret, and the third row reports results for ranking based on the maximum regret across all the cities and groups. Under the feasible case in which θ is unknown (subfigure 6b), we can attain a more equal allocation that generates substantial improvements for immigrants. Comparing the minimax policies with the average status-quo policy, immigrants' regret can be reduced by almost half, at the cost of a 57% increase in regret for natives. The expected maximum regret across the immigration groups (the value of max $(\mathcal{N}, \mathcal{I})$) drops by 15%, and, remarkably, the expected maximum regret across groups and cities (the value of max $(\mathcal{N}, \mathcal{I})$) —i.e., the worst-case scenario—drops by 45%. Lastly, column 2 in the adjacent table shows that the minimax policies cannot provide full insurance against cities that are worse than the status quo, as in column 1. Still, they ensure that at most 1.1 of the cities will not be beneficial to any groups.

The gains from employing more equitable policies (i.e., the minimax policies) are less pronounced when the location effects of both groups are positively correlated. To illustrate this phenomenon, Appendix Figures A.7a and 6b report the expected regret from such policies for high-income families who exhibit a strong correlation between the location effects of immigrants and natives. For immigrants, the city-level average posterior mean policy provides only 54% higher regret than the feasible first-best. This is much lower than the costs of low-income families, for which the city-level mean policy generates losses that are almost 3 times larger than the feasible first-best. As a result, minimax policies provide much lower improvements for immigrants than under the scenario of zero or negative correlation. The improvement in the share of places that are worse than the status quo is also marginal, and the average policy can ensure that more than 96% of the recommended cities will be better for both groups compared with the status quo.

10.5 Trade-offs

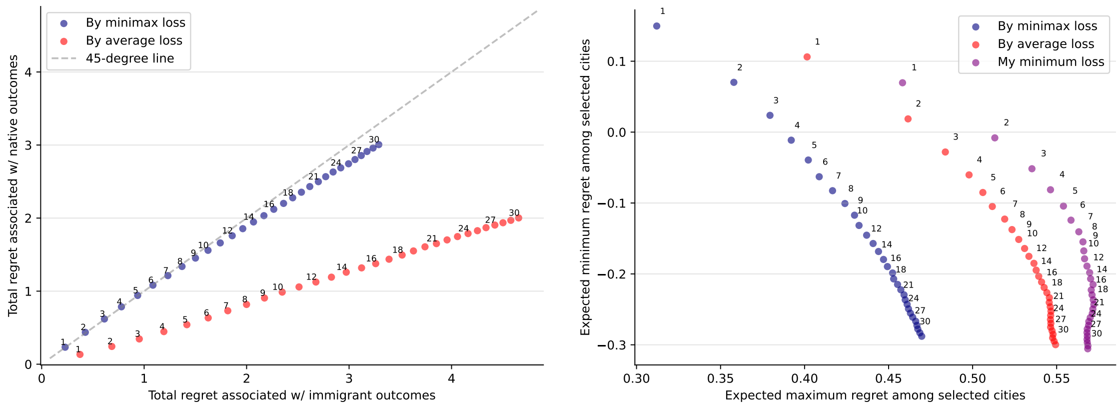
The trade-off faced by the decision-maker is visualized in Figure 7 where we simulate losses for a grid of values of K and compute the mean status-quo average policy and the minimax policies. Figure 7a plots the expected total regret of immigrants and natives from the status-quo average policy described in Equation (6) and the minimax ($\mathcal{N}/\mathcal{I}$) policy that ranks cities based on the within-city maximum regret described in Equation (9). Each dot corresponds to the sum of regret in selected cities by varying K between 1 and 30, and the corresponding K is displayed right next to the dot.

For every K, a policy based on the status-quo average is clearly advantageous for natives but provides limited benefits for immigrants, which is reflected by the red curve lying below the 45-degree line. In contrast, for every K, the minimax strategy that ranks places based on the expected maximum regret lies very close to the 45-degree line, which implies a more equitable outcome. We can see that as K increases, total regret increases, as it is harder to attain equal outcomes since there are fewer and fewer places that benefit both groups equally. While for a given K, the regret of native-born children under the minimax policy is higher than under the status-quo average, for every value of native-born children regret, there exists a minimax strategy that attains the same level of regret, together with lower losses for immigrants.

The ambiguity of the decision-maker regarding compliance is reflected by her inability to determine the relative weights of each group's gains and how to aggregate these gains into a unified social objective. Under the worst-case scenario, which the minimax decision rule is trying to guard against, families that comply with the treatment will sort into the least favorable places, maximizing the regret. In contrast, under the best-case scenario, it could also be possible that families who comply would be the ones that benefit the most, therefore further minimizing the regret of any given policy.

The relationship between the choice of K and outcomes under the optimistic and pessimistic scenarios are presented in Figure 7b, which plots the expected maximum worst-case regret against the expected minimum best-case regret for the minimax (blue dots) and status-quo mean (red dots) policies for values of K between 1 and 30. To assess the performance of an optimistic decision-maker when facing heterogeneity, we also analyze the policy that ranks places based on the posterior expectation of the minimum regret, $\mathbb{E}[\min(\{\vartheta_{j\mathcal{N}K}, \vartheta_{j\mathcal{I}K}\}_{j\in S(\delta)})|\mathcal{Y}]$, plotted by purple dots. As noted

Figure 7: Regret under minimax and status-quo average targeting policies



(a) Total regret of immigrants and natives (b) Maximum and minimum regret

Note: These figures evaluate the regret losses of immigrants and natives from the average status-quo and minimax ($\mathcal{N}/\mathcal{I}$) policies. The regret, ϑ_{jgK}, of each city and group, immigrants and natives, is the difference between the location effect and the average location effects of the group-specific top K cities based on the true location effect θ. Blue dots plot outcomes generated by the minimax ($\mathcal{N}/\mathcal{I}$) policy that ranks cities based on the within-city group maximum regret described in Equation (9). Red dots plot outcomes generated by the status-quo average policy described in Equation (6), which ranks locations based on the within-city average regret, using the status quo group shares. In all figures, curves are generated by varying K, the number of selected cities, between 1 and 30, and the K is printed next to each dot. Figure 7a plots the total regret of immigrants and natives from policies that select the top K Israeli cities. Total regret is the sum of regret over selected cities, separately by immigration group, and the dashed line is the 45-degree line. Figure 7b plots the expected maximum regret among selected cities against the expected minimum regret. Purple dots display outcomes from an optimistic policy that ranks places based on posterior expectation of the minimum regret between immigrants and natives $\mathbb{E}[\min(\{\vartheta_{j\mathcal{N}K}, \vartheta_{j\mathcal{I}K}\}_{j \in S(\delta)})|\mathcal{Y}]$.

by Hurwicz (1951), ranking places based on a convex average of the minimum and maximum loss, known as the α-minimax decision rule, reflects continuous types of decision-makers with different levels of pessimism.

Figure 7b shows that in the face of heterogeneity, guarding against the least favorable scenarios could also promise substantial gains in states of the world that are less pessimistic. First, although for every K, the average status quo policy achieves lower values of minimum regret compared with the minimax policy, the worst- and best-case curves generated by the minimax decision rule outperform those of the average status quo decision rule. Interestingly, we find a similar pattern with the optimistic decision rule. By construction, for every K, the optimistic decision rule attains the lowest regret at the cost of risking with regret levels that are 2-4 times higher if families sort to the least favorable places. In contrast, the pessimistic minimax decision rule provides the lowest worst-case losses while maintaining close to zero (and lower) regret levels in the state of the world in which families sort into the places that benefit them the most.[36]

10.5.1 Selected Israeli Cities

Table 4 displays the top 15 cities sorted according to their within-city posterior maximum regret across immigrants and natives (i.e., according to the minimax $(\mathcal{N}, \mathcal{I})$ targeting policy) when $K = 10$. The regret is in shekel values (1 US $\approx$ 3.4 ILS), and represents the lost earnings at age 28 from spending one year in city j, compared with the average city selected under the first-best policy that allows for personalized recommendations and knows the true location effects.

The regret from selecting the top 10 leading cities is bounded for both groups, which implies that it is possible to identify at least a few places that provide substantial opportunities for both immigrants and natives. This is evident in columns 1-3, which report the posterior mean regret of each immigration group and the city average, weighted by the status quo within city shares. Compared with the personalized first-best, an additional year spent in any of these cities generates, on average, 391-483 lower ILS earnings (115-143 US $) for natives and lower 162-426 ILS earnings (47-126 US $) for immigrants. Column (4) reports the posterior expectation of the maximum regret, ϑ_{jg5}, of each group $g \in \{\mathcal{N}, \mathcal{I}\}$. Even in the least favorable scenario, children in the top five cities have incomes only 421-547 ILS lower (equivalent to approximately 123-160 US $) than those in their average optimal city.

The top 10 cities based on the minimax $(\mathcal{N}, \mathcal{I})$ targeting policy are likely to provide returns that are higher than those expected under the status quo sorting pattern. In column 5, we report the posterior probability that either the location effects of immigrants or natives fall below the average location effect under the status quo sorting. This posterior probability is the analog of the false discovery rate (Benjamini and Hochberg, 1995) in multiple hypothesis testing settings in which, for each city, we test the null that both the effects of immigrants and the effects of natives are greater than the expected value under the status quo sorting patterns. By averaging the first 10 values in column 10, we conclude that when selecting the top 10 cities following the minimax $(\mathcal{N}, \mathcal{I})$ targeting policy, we should expect that at most 1 of these 10 cities would generate outcomes worse than the status quo for either of the two ethnic groups.

The last column indicates whether the city is selected under the model that allows for ambiguity with respect to both ethnicity and sorting patterns within the recommended list. When $K = 10$, the list of top 10 recommended cities coincides with the list of cities selected using the minimax $(\mathcal{N}, \mathcal{I})$ targeting policy, in which ambiguity regarding

[36] The optimistic decision rule attains negative regret levels because we defined the first-best value as the expected average values across all selected cities, while the optimistic scenario outperforms it and sends all housing recipients to the single best place.

Table 4: Top Israeli cities selected based on minimax criterion, $K = 10$

Loc. name	Posterior mean: Native-born (1)	Imm. (2)	Average (3)	$\mathbb{E}[\max\{\vartheta_{\mathcal{N}}, \vartheta_{\mathcal{I}}\}\|\mathcal{Y}]$ (4)	Worse than status-quo (5)	Selected by minimax ($\mathcal{I}/\mathcal{N}$/city) (6)
Qiryat Gat	391.7	162.1	237.2	421.7	0.039	Yes (749.4)
Ma'alot-tarshiha	399.6	339.5	368.3	499.7	0.103	Yes (749.4)
Karmi'el	477.8	212.5	316.7	503.1	0.108	Yes (749.4)
Rishon Leziyyon	512.3	208.6	265.4	517.0	0.066	Yes (749.4)
Yavne	354.2	435.0	425.0	526.8	0.118	Yes (749.4)
Mateh Binyamin	400.1	416.6	414.7	532.6	0.126	Yes (749.4)
Bat Yam	536.4	108.8	244.3	537.1	0.090	Yes (749.4)
Arad	401.2	430.0	417.9	542.8	0.142	Yes (749.4)
Ramla	451.4	439.9	443.0	544.7	0.106	Yes (749.4)
Ashqelon	483.0	426.5	446.3	546.9	0.086	Yes (749.4)
Ra'annana	537.2	321.0	350.4	575.5	0.213	
Qarne Shomeron	478.1	371.9	388.4	586.3	0.246	
Holon	587.9	217.1	282.7	590.2	0.187	
Be'er Sheva	574.9	407.0	458.4	596.8	0.144	
Tel-Aviv	487.1	564.4	554.6	600.9	0.142	

Note: This table reports a list of 15 Israeli cities sorted by within-city posterior immigrant-native maximum regret. Regret is defined as the difference between one-year location effects and the average benefits from the top cities of each group. It represents the lost earnings at age 28 from spending one year in city j, compared with the average city selected under the first-best policy that allows for a personalized recommendation. Columns 1-3 report the posterior mean regret of native-born children, immigrants, and the average. Column 4 reports the posterior maximum regret across immigrants and natives. Column 5 reports the posterior probability that the location effects of immigrants or the location effects of natives are lower than the average effect under the status quo sorting patterns. Column 6 reports which cities are selected as the top 10 cities based on the minimax ($\mathcal{N}/\mathcal{I}/city$) policy that ranks lists of 10 cities based on their posterior maximum regret across all cities and groups, where the posterior maximum regret of the selected list is presented in parentheses.

behavioral responses was ignored. In parentheses, we present the posterior expectation of the maximum regret across all selected cities and across both immigrants and natives ϑ_{jg10}. Consistent with Jensen's inequality, the expected maximum value across all cities exceeds the average values of the top 10 cities found in column 4.

10.6 Model Extension and Robustness

Model Extension: The stylized models depicted in Section 10.3 provide a clear, easy-to-interpret closed-form decision rule for the minimax decision-maker who seeks to be robust against the least favorable sorting scenario. Nevertheless, these models were derived under simplified assumptions that might not hold in reality. First, the minimax decision-maker behaves as if all families might sort into a single worst place—a phenomenon that is rejected by the data. To illustrate this, Appendix Figure A.8 plots the distribution of location choices of families if they follow the minimax ($\mathcal{N}/\mathcal{I}/city$) strategy and face the minimax ($\mathcal{N}/\mathcal{I}/city$) decision-maker. This figure shows that the minimax behavior assumes that families sort into a small set of places, which might not be reasonable. Second, the models in Section 10.3 do not take into account capacity constraints and other restrictions that might arise in real-world problems.

To account for these concerns, Appendix Section K describes an extended model that restricts sorting probabilities to better align with the spatial distribution in the data while maintaining compliance uncertainty. We show that when location choices are restricted to align closely with the status quo spatial distribution, decisions are similar to those of the average status quo policy. In contrast, with more ambiguity regarding location choices, optimal policy aligns with the minimax decision rule and offers more equal outcomes to both groups. On that scale of ambiguity, the choice between these possible models depends on the decisionmaker's information and uncertainty.

The restrictions of location choices discussed in this section reflect the importance of careful contemplation of the information set and social objectives decision-makers might have. Previous literature has focused ultimately on how noise affects decision (Mogstad et al., 2023; Andrews et al., 2024). This extension emphasizes that statistical uncertainty alone could result in various types of policies, depending on the uncertainty decision-makers face on other dimensions.

Normalization: Regret normalization provides us with a measure that compares each policy with the non-restricted optimal one. Also, it enables us to overcome the consequences of our estimation procedure, in which only relative effects, rather than exact levels, are identified.[37] To assess the sensitivity of the results to the regret normalization, Appendix Table A.7 replicates Table 4 while normalizing the value of each place in comparison with that expected under the status quo sorting patterns. Selecting the top 10 Israeli cities using the mean status quo normalization yields the same list as in Table 4, although the within-list ranking is different.

11 Conclusion

This paper studies the heterogeneity in the causal effects of Israeli cities on children's income in adulthood. Our exploration into the nuanced association between childhood location effects of natives vs. immigrants in Israeli cities has demonstrated that cities that benefit one group are not necessarily the cities that benefit the other groups.

While the literature on school value-added and hiring policies has emphasized the importance of taking into account match effects and treatment effect heterogeneity (e.g., Biasi et al., 2021; Bates et al., 2024), neighborhood recommendation policies tend to treat places as an ordered treatment, usually proportional to mean poverty

[37] A decision-maker who seeks to minimize regret is concerned not only about the outcome she receives but also about the outcome she would have received had she chosen differently. One of our motivations for using regret normalization was a discussion we had with Israeli government officials, who expressed their disappointment after finding the excess heterogeneity in location effects in Israel and raised the concern that the first-best personalized policy will not be politically feasible.

rate or mean earnings (Katz et al., 2001; Bergman et al., 2019). Our findings suggest that such policies can disproportionately harm minorities.

We discuss the trade-offs policymakers face when implementing a unified policy that cannot be conditioned on individual characteristics when uncertainty with respect to the true effects and future compliance patterns is present. While the literature has primarily focused on the perils of forming policy based on noisy estimates, we demonstrate that uncertainty driven by heterogeneity should also be taken into account in cases where there are substantial treatment effects heterogeneities. Nevertheless, Using a decision-theoretic framework, we show that by acknowledging the ambiguity with respect to individuals' sorting, it is possible to find at least 10 cities in Israel that are beneficial to both groups.

Our model demonstrates that we can improve fairness also in restricted unified policies. We think that such framing could be useful in various other settings where the treatment is not directed to individuals but to predefined groups.

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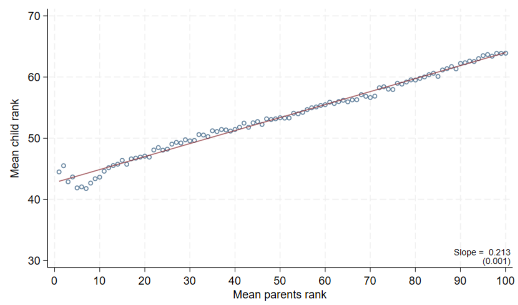
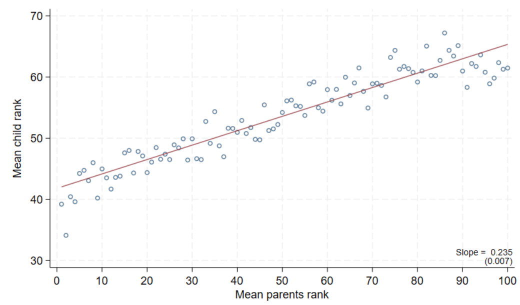
A Additional Figures and Tables

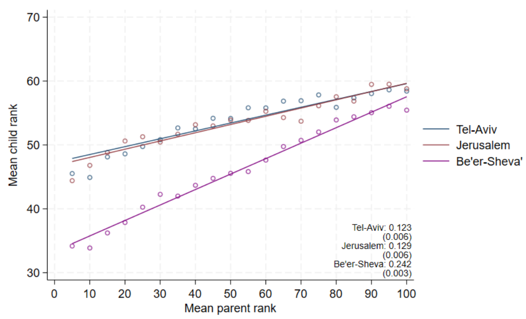
Figure A.1: Relationship between parental income rank and child mean income rank at ages 28-30



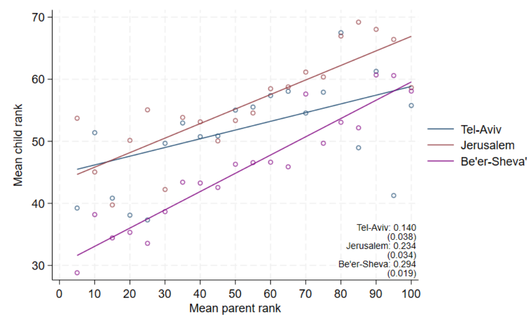
(a) All cities - natives



(b) All cities - immigrants



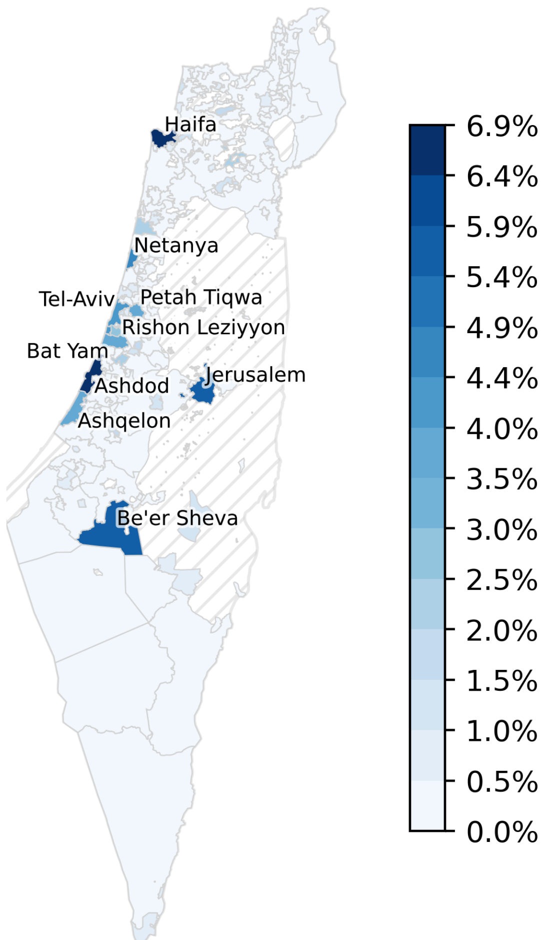
(c) Selected cities - natives



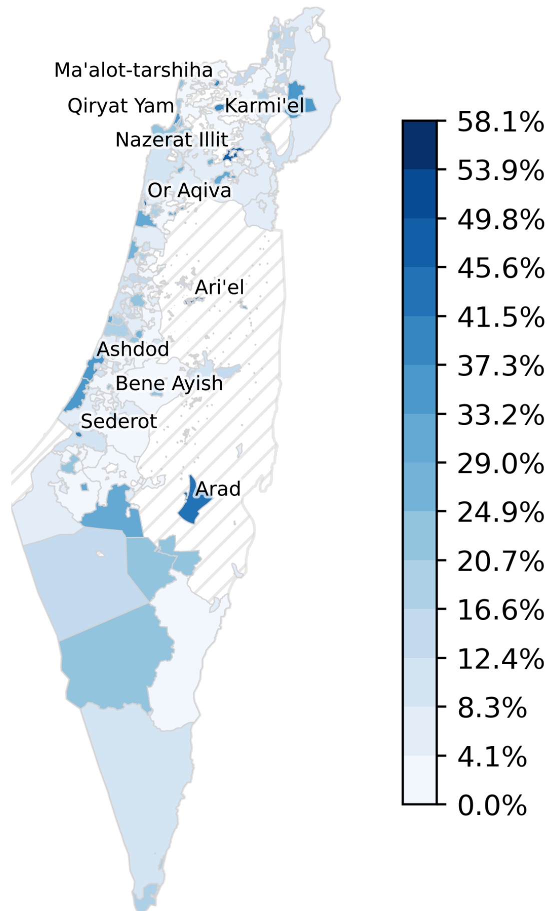
(d) Selected cities - immigrants

Note: This figure displays the relationship between parental income rank and mean child income rank of children of immigrants from the Former Soviet Union and native-born Israeli children at ages 28-30. Panels (a) and (b) display the relationship in all the Israeli cities, and panels (c) and (d) display this relationship among children who lived in Tel-Aviv, Be'er-Sheva' and Jerusalem from birth to age 18.

Figure A.2: Immigrants' spatial distribution



(a) Across city share

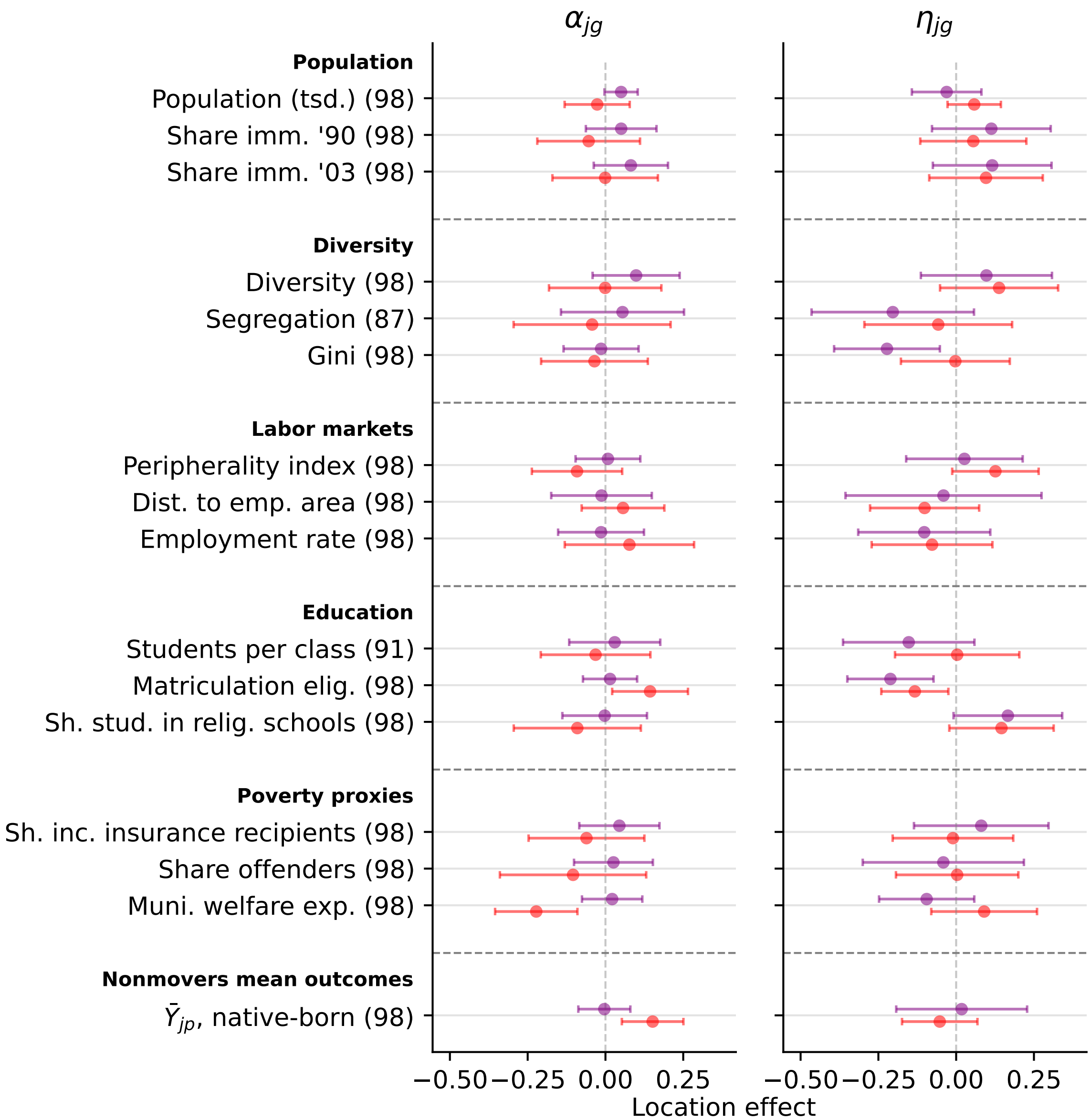


(b) Within city share

Note: This map presents the geographic distribution of immigrants across Israel. Panel (a) maps the share of immigrants in each location out of the immigrant population. Panel (b) maps the share of immigrants out of the whole population within each city. Location names are attached to the cities with the ten largest values. The values are grouped into 15 equally sized bins and collared accordingly. Source: The annual Local Authorities in Israel report of the Central Bureau of Statistics 2003.

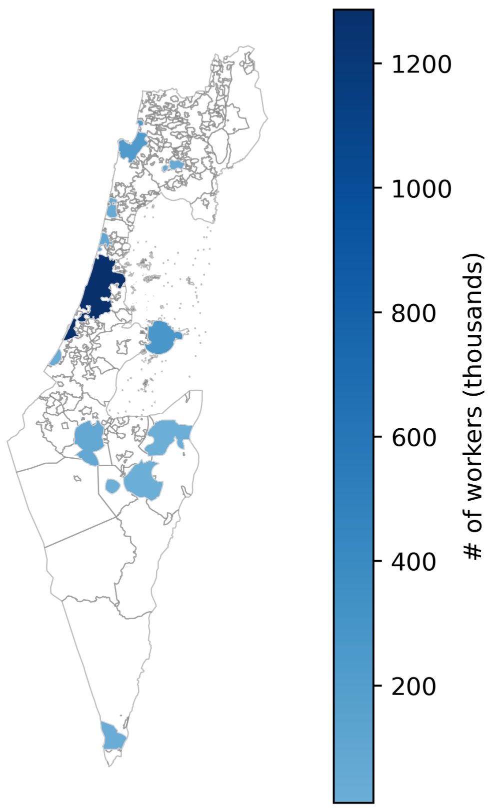
vides a complete description of covariates definitions. The number of cities in each regression is in parentheses. Cases with fewer localities than the full sample (98) are due to missing values, or in the case of segregation, where values cannot be calculated for cities that do not have sub-areas (see Appendix Section F).

Figure A.3: Predictors of location effects (α_{jg} and η_{jg})



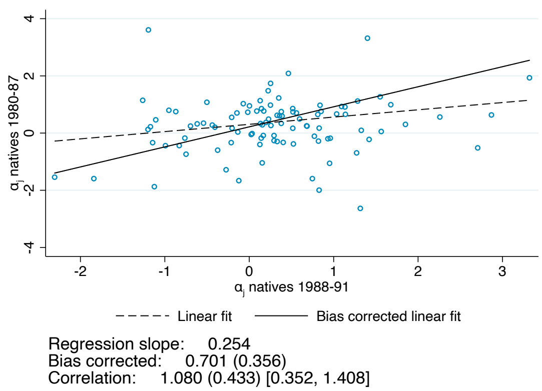
Note: This figure plots the relationship between city-level covariates and the parameters governing the location effects, the intercept α (left panel), and the slope η (right panel) for immigrants and natives. Each relationship is estimated with a feasible generalized least squares regression, reweighting the observations by the inverse of the Cholesky decomposition matrix of Σ, the variance of the estimated location effects, and with the location effects as the outcomes. Covariates are standardized to have a mean of zero and a standard deviation of one in the sample. In each panel, the first column plots the coefficients from regressions of effects on each covariate alone, and the second column plots the coefficients of a multivariate regression with all the characteristics simultaneously. Bars indicate 95% confidence intervals based on robust standard errors. Appendix Section B.2 provides a complete description of covariates definitions. The number of cities in each regression is in parentheses. Cases with fewer localities than the full sample (98) are due to missing values, or in the case of segregation, where values cannot be calculated for cities that do not have sub-areas (see Appendix Section F).

Figure A.4: Employment center, census 2008

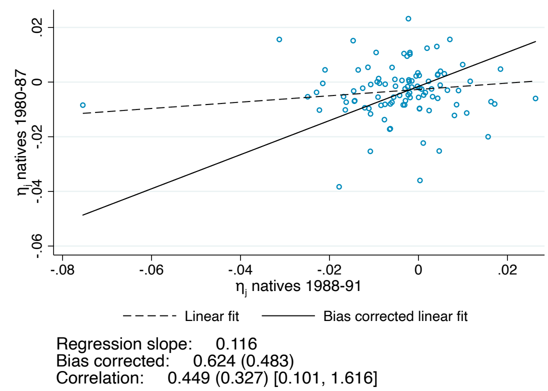


Note: This figure displays the number of workers (in thousands) in the major employment centers from the 2008 census.

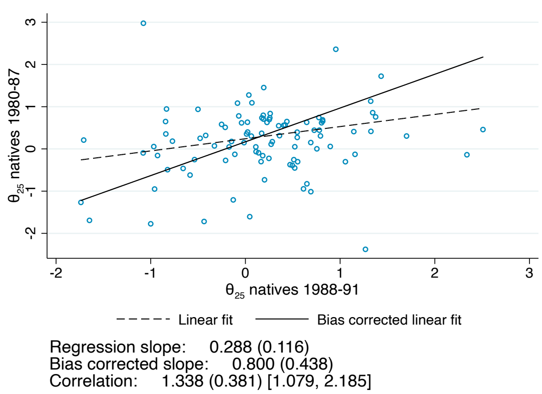
Figure A.5: The relationship between location effects of natives born in 1980-1987 and 1988-1991



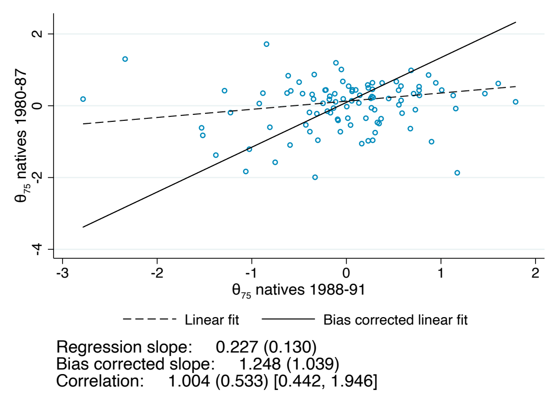
(a) α (intercept)



(b) η (slope)



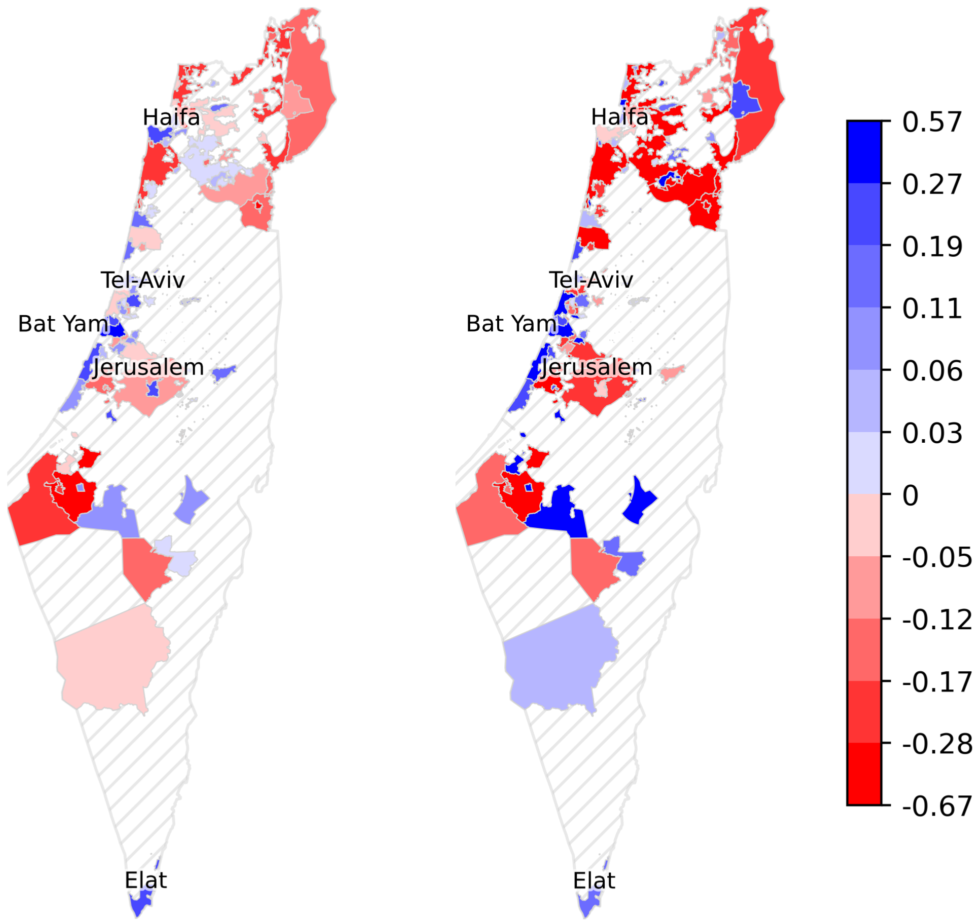
(c) Total effect, θ_{25}



(d) Total effect, θ_{75}

Note: These figures display the scatter plots and observation-weighted regression lines of location effects for native Israeli children born in years 1980-1987 and 1988-1991. Panel (a) the estimated intercepts α_{jg}, panel (b) plots the estimated slopes η_{jg}, panel (c) the total one-year location effect for families in the 25th percentile of the income distribution, and panel (d) the total one-year location effect for families in the 75th percentile of the income distribution. The dashed line is the naive regression line and the solid line is the bias-corrected regression. Square brackets display parametric bootstrapped equal-tailed confidence intervals

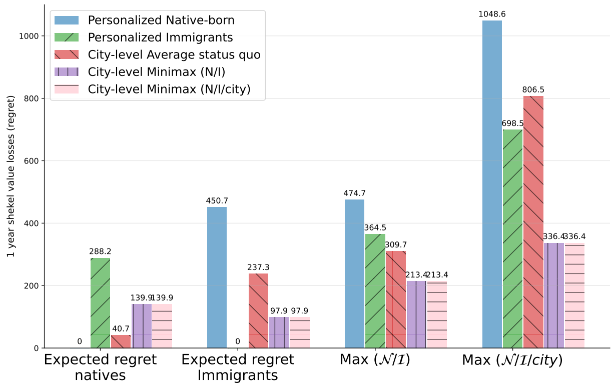
Figure A.6: Posterior mean location effects, high-income families ($p = 75$)



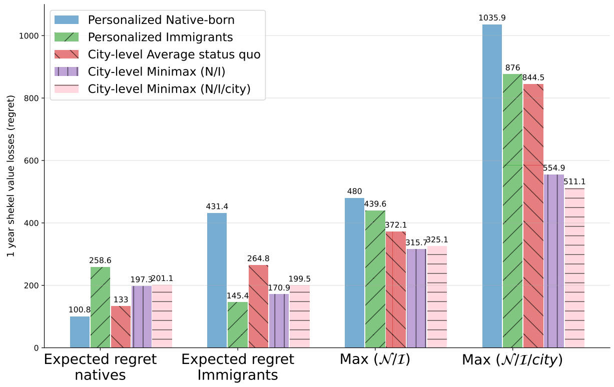
(a) Natives, θ_{75} (b) Immigrants, θ_{75}

Note: These maps plot the children's posterior mean effect of year-long exposure to cities and regional councils in Israel on children's income rank at age 30 for immigrants and native-born children. Figures (a) and (b) display the location effects of low-income families whose parents are at the 25th percentile of the national income distribution, and figures (c) and (d) display the location effects of high-income families whose parents are at the 75th percentile of the income distribution. The maps are constructed by grouping cities into 12 equally sized groups where the more blue the area, the greater its effect from the mean, and the more red the area, the smaller the effect compared to the mean.

Figure A.7: Targeting trade-offs from choosing the top 10 cities for high-income families



(a) Known θ (oracle)



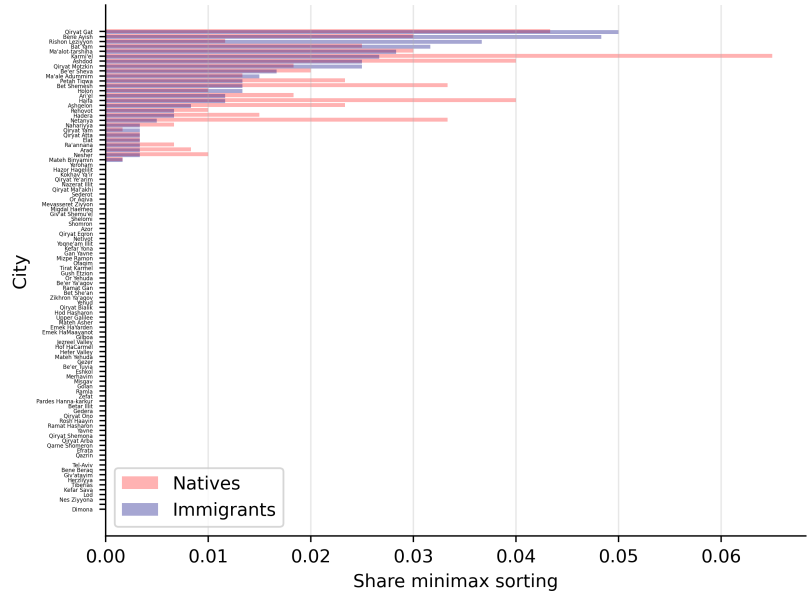
(b) Unknown θ

	% better than the status-quo	
	Known θ (oracle) (1)	Unknown θ (2)
i) Personalized policy		
Native-born	0.914	0.917
Immigrants	0.903	0.935
ii) City-level policy		
Av. status quo	0.975	0.963
Minimax ($\mathcal{N}/\mathcal{I}$)	1.000	0.979
Minimax ($\mathcal{N}/\mathcal{I}$/city)	1.000	0.981

(c) Share worse than the status-quo

Note: These plots give the expected regret from different policies that aim to select the top 10 places in Israel based on the returns for children whose parents are in the 75th percentile of the income distribution. Regret is defined as the difference between the one-year location effects and the average benefits from the top cities for each group, immigrants and natives. Regret is in shekel values (1 US \$ $\approx$ 3.4 ILS). It represents lost earnings at age 28 from spending one year in the average selected city, compared with the average selected city under the first-best policy. Subfigure (a) displays results from policies based on the true location effects, and subfigure (b) displays the results from policies based on the expected values of location effects, where the expectation is taken over the posterior distribution of location effects conditional on the estimated location effects and standard errors, and the distribution of location effects from Table **??** is treated as prior. The first two groups of bars report the expected regret of each policy for every group, immigrants and natives. The $Max(\mathcal{N},\mathcal{I})$ bars report the expected within-city immigrant-natives maximum regret among selected cities, and the $Max(\mathcal{N},\mathcal{I},city)$ bars report the expected maximum regret across all selected cities and immigrants and natives. The blue and green bars report the results from a policy that ranks locations based on the regret of each group. The red bars report the policy that ranks locations based on the city-level average regret as described in Equation (6). The purple bars report the minimax ($\mathcal{N}/\mathcal{I}$) policy that ranks locations based on the city-level immigrant-native maximum regret as described in Equation (9). The pink bars report the minimax ($\mathcal{N}/\mathcal{I}$/city) policy that ranks lists of 10 cities based on maximum regret across all the cities and groups in the list, as described in Equation (12). Subplot (c) presents the share of selected places that provide benefits that are higher than the status quo sorting for both immigrants and natives.

Figure A.8: The distribution of the least favorable sorting patterns



Note: This figure plots the probability that a housing voucher recipient family chooses to move to each Israeli city if families follow the minimax strategy and are facing the optimal policy of the minimax decision-maker. Red bars display the sorting probabilities of native families, and blue bars display the sorting probabilities of immigrant families.

Table A.1: Variance component of the intercept and slope $(\theta_{jI}, \eta_{jI}, \theta_{jL}, \eta_{jL})$

	Natives		Immigrants	
	Cons. (1)	Rank-parents (2)	Cons. (3)	Rank-parents (4)
Natives				
Cons.	0.227 (0.048)			
Rank-parents	-0.709 (0.160) [-0.885, -0.277]	0.003 (0.001)		
Immigrants				
Cons.	0.068 (0.421) [-0.524, 0.708]	-0.351 (0.422) [-1.094, 0.145]	0.142 (0.065)	
Rank-parents	-0.190 (0.289) [-0.717, 0.324]	0.794 (0.254) [0.403, 1.203]	0.072 (0.399) [-0.255, 1.247]	0.003 (0.000)

Note: This table reports the standard deviation in diagonal and correlation in off-diagonal of the vector of intercepts and slopes of the location effects of immigrants and native-born Israeli children. All variance components are weighted by the total number of city residents. Standard errors of the variance and covariances are based on the asymptotic variance, assuming location effects are drawn from a normal distribution. Standard errors of the correlations and standard deviations are calculated using the delta method. Square brackets display parametric bootstrapped equal-tailed confidence intervals.

Table A.2: Variance components and correlations, robustness to school fixed effects

	Baseline (1)	w/ school FE (2)	% explained variance (3)
(i) Low-income families (θ_{25})			
Std. Natives	0.186	0.128	0.311
	(0.042)	(0.059)	
Std. Immigrants	0.160	0.101	0.368
	(0.051)	(0.052)	
Std. Difference	0.248	0.545	
	(0.062)	(0.154)	
Immigrants-native corr	-0.017	-0.154	
	(0.341)	(0.673)	
	[-0.564, 0.355]	[-1.101, 0.711]	
(ii) High-income families (θ_{75})			
Std. Natives	0.164	0.097	0.40
	(0.043)	(0.036)	
Std. Immigrants	0.249	0.179	0.281
	(0.036)	(0.057)	
Std. Difference	0.245	0.473	
	(0.055)	(0.166)	
Immigrants-native corr	0.355	-0.076	
	(0.249)	(0.476)	
	[-0.088, 0.729]	[-0.835, 0.673]	

Note: This table reports the bias-corrected variance components of year-long exposure location effects of native-born and immigrants from high- and low-income families. Column (1) reports our baseline estimates of location effects. Column (2) reports the variance components from a model with high school fixed effects. Column (3) reports the share of variance explained by high school fixed effects. Standard errors are calculated via the delta method.

Table A.3: Variance component, equal neighborhood weights

	Low-income families, θ_{25}		High-income families, θ_{75}	
	Std.	χ^2 test $H_0 : \theta_{jI} - \theta_{jN} = c\ \forall j$	Std.	χ^2 test $H_0 : \theta_{jI} - \theta_{jN} = c\ \forall j$
	(1)	(2)	(3)	(4)
Natives	0.9563	189.4	0.8924	202.1
	(0.2174)	[0.0000]	(0.1959)	[0.0000]
Immigrants	0.5040	464.0	0.5360	286.7
	(0.0749)	[0.0000]	(0.0559)	[0.0000]
Immigrants-native corr	0.0556		0.3098	
	(0.2185)		(0.1618)	

Note: This table reports the bias-corrected variance components of year-long exposure location effects of native-born and immigrants from high- and low-income families. Location effects were estimated by exploiting differences in the exposure time to cities in Israel using families who moved between cities and for immigrants exploiting the variation in the age of arrival to the country. This table reports the robustness exercises reweighting the regression by the origin-destination number of observations. Columns (1) and (2) present the results for families from the 25th percentile of the income distribution, and columns (3) and (4) present the results for families from the 75th percentile of the income distribution. The first two rows in columns (1) and (3) display the standard deviation of location effects, while the third row presents the correlation between natives' and immigrants' location effects estimate as the covariance divided by the standard deviations. Standard errors are calculated via delta method.

Table A.4: Heterogeneity in location effects, within group parents income rank

	Low-income families, θ_{25}		High-income families, θ_{75}	
	Natives	Immigrants	Natives	Immigrants
	(1)	(2)	(3)	(4)
Natives	0.158		0.193	
	(0.052)		(0.041)	
Immigrants	-0.116	0.186	0.488	0.248
	(0.338)	(0.026)	(0.215)	(0.025)
	[-0.689, 0.439]		[0.064, 0.882]	

Note: This table reports the standard deviation in diagonal and correlation in off-diagonal of immigrants' and natives' location effects on children's income rank at age 28. Columns 1-2 display the correlation matrix for low-income families from the 25th percentile of the within-group income distribution, and columns 3-4 display the correlation matrix for high-income families at the 75th percentile of the within-group income distribution. Standard errors of the variance and covariances are based on the asymptotic variance, assuming location effects are drawn from a normal distribution. Standard errors of the correlations and standard deviations are calculated using the delta method. Square brackets display parametric bootstrapped equal-tailed confidence intervals.

Table A.5: Heterogeneity in location effects, earnings and log earnings

	Low-income families, θ_{25}		High-income families, θ_{75}	
	Natives (1)	Immigrants (2)	Natives (3)	Immigrants (4)
A) Earnings				
Natives	169.53 (300.71)		262.42 380.80)	
Immigrants	-0.75 (1.48) [-3.12, 0.96]	367.35 (61.53)	0.35 (1.18) [-0.81, 1.16]	557.07 (59.98)
B) Log earnings				
Natives	0.0181 (0.0081)		0.0179 (0.0068)	
Immigrants	0.0284 (0.4008) [-0.614, 0.324]	0.0268 (0.0042)	0.5638 (0.3220) [0.122, 1.138]	0.0350 (0.0043)

Note: This table reports the standard deviation in diagonal and correlation in off-diagonal of immigrants' and natives' location effects on children's earnings at age 28 measured in Shekels (Panel A, 1 US \$ $\approx$ 3.4 ILS). Columns 1-2 display the correlation matrix for low-income families from the 25th percentile of the within-group income distribution, and columns 3-4 display the correlation matrix for high-income families at the 75th percentile of the within-group income distribution. Standard errors of the variance and covariances are based on the asymptotic variance, assuming location effects are drawn from a normal distribution. Standard errors of the correlations and standard deviations are calculated using the delta method. Square brackets display parametric bootstrapped equal-tailed confidence intervals.

Table A.6: Heterogeneity in location effects, robustness to city level weights

	θ_{25}		θ_{75}	
	Natives (1)	Immigrants (2)	Natives	Immigrants
(i) Total # of movers weights				
Natives	0.163		0.171	
	(0.048)		(0.042)	
Immigrants	-0.067	0.264	0.429	0.339
	(0.253)	(0.043)	(0.201)	(0.042)
	[-0.532, 0.383]		[0.070, 0.825]	
(ii) Group # of movers weights				
Natives	0.173		0.198	
	(0.050)		(0.041)	
Immigrants	-0.081	0.205	0.468	0.267
	(0.306)	(0.026)	(0.217)	(0.026)
	[-0.622, 0.457]		[0.025, 0.879]	
(iii) Group # of residents weights				
Natives	0.183		0.163	
	(0.045)		(0.042)	
Immigrants	-0.180	0.190	0.470	0.248
	(0.299)	(0.023)	(0.268)	(0.023)
	[-0.732, 0.360]		[-0.005, 0.970]	

Note: This table reports the standard deviation in diagonal and correlation in off-diagonal of the location effects of immigrants and natives on children's income rank at age 28 for different reweighting schemes. Columns 1-2 display the correlation matrix for low-income families from the 25th percentile of the within-group income distribution, and columns 3-4 display the correlation matrix for high-income families at the 75th percentile of the within-group income distribution. In panel (i), cities are reweighted by the total number of movers; in panel (ii), cities are reweighted by the number of each group's movers; and in panel (iii), cities are reweighted by each group's total number of residents. Standard errors of the variance and covariances are based on the asymptotic variance, assuming location effects are drawn from a normal distribution. Standard errors of the correlations and standard deviations are calculated using the delta method. Square brackets display parametric bootstrapped equal-tailed confidence intervals.

Table A.7: Top selected Israeli cities, $K = 10$, status-quo sorting normalization

Loc. name	Psterior mean Native-born (1)	Immigrants (2)	Average (3)	$\mathbb{E}[\min\{\vartheta_{\mathcal{N}}, \vartheta_{\mathcal{I}}\}\vert\mathcal{Y}]$ (4)	Worse than status-quo (5)	Selected by maximin ($\mathcal{I}/\mathcal{N}$/city) (6)
Qiryat Gat	356.6	526.1	470.7	312.5	0.023	Yes (93.4)
Karmi'el	270.5	475.7	395.1	233.1	0.072	Yes (93.4)
Rishon Leziyyon	236.0	479.6	434.0	226.4	0.033	Yes (93.4)
Ma'alot-tarshiha	348.7	348.7	348.7	220.6	0.083	Yes (93.4)
Bat Yam	211.9	579.4	462.9	209.9	0.046	Yes (93.4)
Yavne	394.2	253.2	270.5	183.2	0.111	Yes (93.4)
Mateh Binyamin	348.3	271.5	280.4	181.9	0.109	Yes (93.4)
Ashqelon	265.3	261.7	263.0	173.9	0.075	Yes (93.4)
Ramla	296.9	248.3	261.4	171.6	0.093	Yes (93.4)
Arad	347.1	258.2	295.4	170.9	0.129	Yes (93.4)
Ra'annana	211.2	367.1	345.9	157.0	0.162	
Holon	160.4	471.1	416.1	155.3	0.113	
Be'er Sheva	173.4	281.1	248.2	136.7	0.091	
Qarne Shomeron	270.2	316.3	309.1	136.4	0.214	
Mevasseret Ziyyon	243.0	288.2	283.2	119.3	0.230	

Note: This table reports the list of the top 15 Israeli cities sorted by the within-city posterior immigrant-native minimum location effect, where the location effects of both groups are normalized in comparison to the expected effect under the status-quo sorting patterns of each group. Location effects are in shekel value (1 US \$ ≈ 3.4 ILS) and represent earnings returns at age 28 from spending one year in city j, compared to the average returns under the status-quo sorting patterns. Columns 1-3 report the posterior mean of native-born children, immigrants, and the average. Column 4 reports the posterior minimum location effect across immigrants and natives. Column 5 reports the posterior probability that the location effects of immigrants or the location effects of native-born are lower than the average effect under the status quo sorting patterns. Column 6 reports which cities are selected as the top 10 cities based on the minimum (N/I/city) policy that ranks lists of 10 cities based on their posterior minimum location effect across all cities and groups, where the posterior minimum of the selected list is presented in parentheses.

B Data and Definitions

B.1 Data Construction

This appendix provides a general overview of the data construction and restrictions. Our data construction starts with base demographics data, which contains the entire population of Israel born between 1950 and 1995, their years of birth and death, fake identifiers of individuals and their parents, and the country of birth of both parents and the child. To this file, we merge the annual population registry files which contain the locality of residence at the city and statistical area level of children and both parents separately (available for years 1995, 1999-2019), the immigration year and country of origin of the child and each parent, and tax records files for the years 1995 to 2019. In addition, we supplement the data with parental incomes available starting from 1983. We correct all income values for inflation to prices of 2016, sum the total earnings from all sources (employed or self-employed earnings), and define the ranks of children and parents within the income year relative to the entire sample population. To define the immigrants for our analysis, we follow the rule that if at least one parent was born in the USSR and immigrated starting from 1989, we define the child as an immigrant. In this process, we drop anomaly observations for what we believe are data input mistakes in the administrative records, which include individuals with birth dates after their death dates (≈ 450 observations), very early birth years of parents, below 1950 (10 observations), non-matching single parent identifiers to tax records (113 observations), and negative parental earnings found only in the year 1983 (≈ 950 observations).

To this file, we merge school identifiers and localities at the city level annually for the period 1995-2016. Next, we construct the geographic mobility variables. We use the registry data as the main source for locality information at the annual level. Since we observe both the mother's, father's, and child's locality each year, we define the single series of locations as the parent's location for the parent who shares most years at the same location with the child. We define a move in every year in which the locality in the series is different from the locality in the previous year. Since the registry data is missing the years 1996-1998, we use the school locality information to accurately pinpoint any move that happens in the missing years' span. That is, if we identify a change in locations during missing locality information, we use the school locality data, in cases that it matched either the origin or the destination, to establish the correct move age. We enrich the location data using the city information available from the 1995 census. Specifically, we use the answers to two questions: "When did you move to your current city?" and "Where did you live 5 years ago?". Using these variables, we construct location information starting from 1995 and, for a subset, from 1990.

After defining for every child the origins, destinations, and move ages for all moves, we count the number of children that are associated with origin and destination before the age of 18 and restrict attention to localities that have at least 5 children in a location as an origin, and at least 5 children in a location as a destination, and with at least 100 children in the location at the total, regardless whether the location is an origin or destination. This results in 98 localities out of the total 256 localities and regional councils.

B.2 City Level Data

In this Appendix, we provide detail on the locality-level variables used in Section 6.

- **Gini index for inequality** calculated at the locality level, taken from the annual Local Authorities in Israel report of the Central Bureau of Statistics (1998), is measured based on the gross earnings of employees in 1998, using administrative records from The National Insurance Institute records.

- **The Theil (1972) index for segregation** constructed using the 2000 tax records data for earnings, combined with the population registry for the city of residence. The segregation index is calculated in the following way. For every group g[38] we denote π_g the share of individuals in a given city. Let $s = 1, .., N$ index the statistical areas in each city. Analogously, we calculate in every statistical area π_{gs}, the group share in statistical area s. For every city and every statistical area, we measure the entropy index $E = \sum_g \pi_g log\frac{1}{\pi_g}$, and $E_s = \sum_g \pi_{gs} log\frac{1}{\pi_{gs}}$. The degree of segregation in every city is defined as:

$$H = \sum_s (\frac{pop_s}{pop}\frac{E - E_s}{E})$$

  where pop_s is the total population of statistical area s and pop denotes the total population of that city. The segregation index H ranges between 0 and 1. When it equals 1 there is no heterogeneity within statistical areas indicating a high level of segregation. When $H = 0$ then all the statistical areas in the city have the same group shares as the city as a whole. Note that we cannot calculate segregation values for localities for which we do not observe sub-cities or sub-localities.

- **Diversity**: similarly to the segregation index, we construct a diversity index based on the entropy index. However, instead of focusing on the discrepancy between the entropy of the city, denoted earlier as E, and that of its individual statistical areas (which results in a measure of segregation), we calculate the

[38] We calculate this variable for two sets of population groups: immigrants and non-immigrants, and the different ethnic groups in Israel.

entropy directly at the city level. Specifically, for each city, we calculate its diversity as:

$$E_d = \sum_g \pi_g \log \frac{1}{\pi_g}$$

In this equation, E_d represents the diversity index for each city, π_g is the proportion of group g in the city, and the summation is performed over all groups g in the city.

The diversity index E_d ranges between 0 and $\log G$, where G is the number of groups. When $E_d = 0$, it means there is no diversity, indicating that the city is entirely composed of a single group. Conversely, when $E_d = \log G$, it means there is maximum diversity, indicating that each group is equally represented in the city.

By constructing the diversity variable in this way, we are able to capture the variety and evenness of group representation in each city, which is the essence of diversity.

- **Share criminal offenders**: the proportion of individuals within the locality who have been charged with a serious crime for which the potential punishment is imprisonment, taken from Fogel (2006), based on police records of 2002.

- **Municipality welfare expenditure**: the total government expenditure on welfare payments per capita, taken from the annual Local Authorities in Israel report of the Central Bureau of Statistics (1998), based on administrative records from The National Insurance Institute records.

- **High-school Bagrut eligibility**: the proportion of 12th-grade students of the years 1999 and 2000 who were eligible for a Bagrut certificate, taken from the annual Local Authorities in Israel report of the Central Bureau of Statistics (2000), based on administrative records from the Ministry of Education.

- **Distance to an employment center**: we use the Central Bureau of Statistics definition of central employment hubs, identified using a spatial interpolation model (Inverse Distance Weighted) to locate the largest concentrations of employees. The processing was based on workplace data from the 2008 population census, and it results in about 11 main employment hubs in Israel: Tel-Aviv, Haifa, Jerusalem, Hadera, Netanya, Be'er Sheva, Ashkelon, Eilat, Nazareth, Dimona, and Arad. We calculate the distance of each locality center to the nearest employment hub border.

- **Peripherality index**: we use the peripherality score of Israeli localities based on their geographic proximity to major population centers. Developed by the Central Bureau of Statistics, it is based on factors like distance from markets, employment hubs, and the Tel Aviv district. The index averages "potential accessibility" which measures the ease of accessing opportunities from a location with a weight of $\frac{1}{3}$, and proximity to the border of Tel Aviv with a weight of $\frac{1}{3}$. Large localities' scores are averages of their sub-localities, adjusted for population size.

- Based on the 1995 census, we also use the following shares:
  - **Within-city immigrants shares**: the proportion of immigrants out of the city population, capturing the density of immigrants within a city.
  - **City share of immigrants**: the proportion of immigrants out of the immigrant population in Israel who live in the city, as a measure for the dispersion of immigrants across Israel.

C One Move vs. Multiple Moves

Our approach diverges from the traditional literature by including not just one-time movers but also those who move twice (Chetty and Hendren, 2018a,b). In this appendix section, we provide a detailed description of data by group and by number of moves.

Tables C.1 and C.2 provide a description of the sample of immigrants and natives, accordingly. First, we note that values are similar when comparing the full sample of cities (left panels) and the analysis sample of 98 cities (right panels) for both groups. Among the immigrants, those who moved after arrival to Israel had higher parental earnings. This is the case for native parents as well, where one-time movers have higher incomes than both stayers and two-time movers. Comparing the two groups, we note that native children generally outearn their immigrant counterparts, although these differences are quite small. These small differences at the children's generation level are notable, especially given the gap in parental earnings, as native children's parents boast significantly higher incomes compared to the parents of immigrant children. When it comes to educational outcomes, native children are slightly more likely to have attained a Bachelor's degree by age 27 than immigrant children. Lastly, immigrant children tend to have much higher intermarriage rates, which primarily reflects their smaller proportion in the population.

In Table C.3, we recreate the results from Table 2 of the across-city heterogeneity estimates, restricting the sample to the single move population. First, note that removing the two-time movers decreases the number of localities that are included in the analysis to 92 (from 98). Second, we also find that the standard deviations using the overlap cities are all qualitatively similar to, and have overlapping confidence intervals with those found in our main analysis in Table 2. Lastly, the standard errors of most estimates in the overlap cities' sample (right panel), are smaller in our main analysis using both types of movers.

Lastly, we estimate the within-city heterogeneity, as done in Table 3, on the one-time movers' sample. The results are presented in Table C.4. We find that the covariance structure of the location effects in the one-time movers sample and twice movers sample is qualitatively the same.

Table C.1: Sample descriptive statistics, immigrants

	All cities			98 sample cities		
	Stayers (1)	Movers (2)	All (3)	Stayers (4)	Movers (5)	All (6)
(A): Children						
Income at 28	66,926	67,847	67,108	68,111	68,536	68,191
Rank at 28	52.68	51.97	52.54	53.43	52.45	53.24
(B): Parents						
Parents income	125,859	152,698	131,670	124,521	150,317	129,997
Parents rank	45	48.11	45.7	45.2	47.9	44.8
Num. of children	125,959	30,310	156,269	112,472	26,192	138,664

Note: This table presents the means of immigrant children (Panel A) and their parents (Panel B). We present the statistics for stayers, migrants who were either born in Israel or immigrated and did not move within Israel, and movers, who moved within Israel up to two times. In the left panel, we include all cities in our sample, and in the right, we include only the 98 cities for which we can estimate effects for both immigration groups and hence are included in our main analysis. Income variables are measured in Israeli Shekels (≈ 3.4$).

D Research Design Validation

The credibility of our approach depends on the functional form assumptions regarding the relationship between location effects and exposure time and the identification assumption that allows us to identify location effects by estimating Equation 2. In this section, we provide a series of specification tests aimed at validating our research design and supporting our identification strategy.

D.1 Balance Test

The identification assumption requires that exposure time and move age are not systematically correlated with time-invariant factors, such as ability, or time-varying factors, such as

Table C.2: Sample descriptive statistics, natives

	All cities			98 sample cities		
	1 move (1)	2 moves (2)	Stayers (3)	1 move (4)	2 moves (5)	Stayers (6)
(A): Children						
Income at 28	70,951	69,196	66,754	71,964	69,757	68,266
Rank at 28	53.38	51.06	53.09	53.86	51.36	53.86
(B): Parents						
Parents income	242,214	198,267	199,978	239,465	194,519	201,033
Parents rank	64.1	58.4	58.9	64.0	57.9	59.3
Num. obs	101,562	13,758	610,945	83,346	11,260	492,104

Note: This table presents the means of native-born children (Panel A) and their parents (Panel B). We present the statistics for one and two-time movers up to age 18, as well as stayers, natives who did not move within Israel since birth and up to age 18. In the left panel, we include all cities in our sample, and in the right, we include only the 98 cities for which we can estimate the effects for both immigration groups and hence are included in our main analysis. Income variables are measured in Israeli Shekels (≈ 3.4$).

parents' investments, that affect the child's income in adulthood. Figures D.1, D.2, D.4, and D.3 provide our first test for these assumptions.

Figure D.1 presents the relationship between native-born children's age when the family moved for the first time and parents' characteristics. In sub-figures D.1b and D.1a, the blue dots and error bars represent the raw relationship between the age at the time of the move and parents' education for native-born Israeli children and the gray ones present the same relationship after controlling for the set of covariates mentioned in Section 3.2. Parents' years of schooling are obtained from the 1995 census, which is available for 20% of the population. Consistent with findings from Heckman and Landersø (2021), we find that the more educated the parents, the more likely they are to move when their children are younger. However, after controlling for x_i, we find that this relationship disappears, supporting our identification assumption of no systematic relationship with time-invariant characteristics.

Sub-figure D.1c displays the relationship between the age of move and parents' earnings growth when the child was between ages 0-5. To measure this variable, we use an earnings dataset that is available only for salary-employed workers in the years 1986-1995. Thus, since we don't use this dataset to construct our parental income, we avoid mechanical correlation between parents' rank and wage growth up to age 5.[39] This figure reveals that families that moved when the children were in older ages

[39] We don't use the salary-employed earning data for the years 1986-1995 because it does not include information on earnings from self-employment.

Table C.3: Heterogeenity of location effects, single move

	All cities			Overlap cities			
	Cities	Mean	Std.	Cities	Mean	Std.	χ^2 test $H_0 : \theta_j = \theta_1 \forall j$
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
(i) By α and η							
Natives							
Cons.	142	0.154	0.214	92	0.180	0.215	143.9
		(0.131)	(0.065)		(0.123)	(0.053)	[0.0004]
Rank-parents	142	-0.004	0.003	92	-0.003	0.003	150.7
		(0.001)	(0.001)		(0.001)	(0.001)	[0.0001]
Immigrants							
Cons.	93	0.640	0.259	92	0.650	0.260	203.8
		(0.078)	(0.044)		(0.047)	(0.044)	[0.0000]
Rank-parents	93	-0.005	0.003	92	-0.005	0.003	204.2
		(0.001)	(0.000)		(0.001)	(0.000)	[0.0000]
(ii) Total city effect							
Natives							
P_{25}	142	0.056	0.178	92	0.1274	0.1768	134.4
		(0.115)	(0.055)		(0.1109)	(0.0465)	[0.0026]
P_{75}	142	-0.138	0.186	92	-0.0153	0.1806	131.2
		(0.112)	(0.055)		(0.1107)	(0.0417)	[0.0046]
Immigrants							
P_{25}	93	0.521	0.235	92	0.5977	0.2358	193.1
		(0.065)	(0.042)		(0.0329)	(0.0421)	[0.0000]
P_{75}	93	0.284	0.273	92	0.4053	0.2743	187.0
		(0.066)	(0.042)		(0.0388)	(0.0423)	[0.0000]

Note: This table presents the estimated bias-corrected standard deviation of the location effect of immigrants and locals. Columns 1-3 present the counts, means, and standard deviation of all the cities that had at least 100 children, and columns 4-6 present the counts, means, and standard deviation for the set of 92 overlapping cities for which we have estimates for both immigrants and locals. Panel (i) displays the θ_{jg} and η_{jg} estimates, and panel (ii) displays the total location effect for families in the 25th and 75th percentile. Column 7 presents a χ^2 test statistic and associated p-value of the null of no location effect heterogeneity across cities. Standard errors for all variance estimators are based on the asymptotic variance, assuming the location effects are drawn from a normal distribution.

experienced higher wage growth when the child was between 0-5 years old. However, after controlling for our set of fixed effects and controls, we find that there is no statistically significant relationship between the age of move and parents' earnings growth at younger ages. This suggests that time-varying components are also not systematically correlated with the child's age at the move after adjusting for location choices and parents' income rank.

Next, in Figure D.2, we examine the relationship between the child's age at arrival to Israel and the parents' year of schooling. In sub-figures D.2b and D.2a, the sample is restricted to all the immigrants who arrived in Israel before 1995 and answered the demographic survey in the 1995 census. Similar to the natives, in the general population of immigrants, a child's age of arrival is negatively correlated with parents' years of schooling, but this relationship disappears after adding controls.[40]

[40] We do not present the wage growth balance because, for most of the immigrant families, we do not observe parental wages when the children are as young as 5 years old.

Table C.4: Differences in location effects between immigrants and natives, single move

	Covariance	Correlation	Implied OLS coefficient	Difference: Mean	Difference: Std.	Difference: χ^2 test $H_0: \theta_{jI} - \theta_{jL} = c\ \forall j$
	(1)	(2)	(3)	(4)	(5)	(6)
α	-0.0047	-0.0839	-0.069	0.5189	0.3571	156.6
	(0.0180)	(0.3213)	(0.266)	(0.1346)	(0.0797)	[0.0000]
		[-0.6407, 0.4661]	[-0.505, 0.335]			
η	0.0000	0.6399	0.542	-0.0013	0.0025	118.3
	(0.0000)	(0.2040)	(0.187)	(0.0015)	(0.0010)	[0.0338]
		[0.2605, 0.9901]	[0.235, 0.923]			
P_{25}	-0.0105	-0.2528	-0.190	0.486	0.340	159.3
	(0.0143)	(0.3396)	(0.256)	(0.118)	(0.072)	[0.0000]
		[-0.845, 0.320]	[-0.643, 0.180]			
P_{75}	0.0014	0.0286	0.019	0.420	0.341	142.6
	(0.0142)	(0.2862)	(0.189)	(0.119)	(0.075)	[0.0006]
		[-0.511, 0.575]	[-0.322, 0.356]			

Note: This table reports the relationship between the location effects of immigrants and the location effects of locals and the test for within-city heterogeneity. Column (1) presents the covariance estimate, column (2) presents the bias-corrected correlation, which is the covariance divided by the standard deviation of immigrants times the standard deviation of locals, and column (3) presents the implied OLS coefficient, which is the covariance divided by the variance of immigrants. Column (4) presents the mean within-city gap between immigrants and locals, column (5) presents the standard deviation of the within-city gap, and column (6) presents the chi^2 test statistic Nd associated p-value of the null of no demeaned within-city gap heterogeneity. Standard errors of the variance and covariances are based on the asymptotic variance, assuming location effects are drawn from a normal distribution. Standard errors of the correlations and OLS slopes are calculated using the delta method. Squared brackets display parametric bootstrapped equal-tailed confidence intervals.

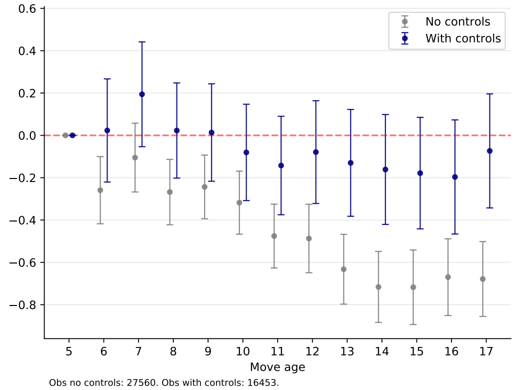
In Figures D.4 and D.3, we repeat the exercise, now assessing whether the age of the child at the second move is related to the parents' characteristics. Interestingly, we find that among the families that moved twice, there is a very weak relationship between the child's age at the second move and the parents' education also in the uncontrolled model.

D.2 Uncovering the Functional Form

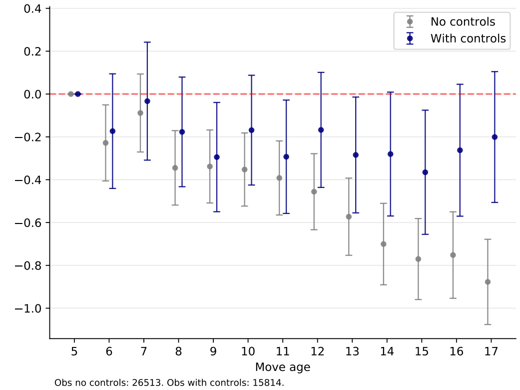
Moving to cities with higher mean outcomes - To test whether the effects of childhood location follow a linear relationship as modeled in Equation 1, and to identify the age A at which childhood location no longer impacts children's outcomes, we proceed in two steps. In the first step, following the benchmark diagnostics in the literature (Chetty and Hendren, 2018a), we leverage a split sample methodology. We test whether relocating at an earlier age to a city with higher city-level mean permanent residents outcomes increases the child's long-run outcomes. To minimize the impact of measurement errors that could attenuate the results, we sidestep heterogeneity related to parents' rank.

For the non-immigrants, we estimate for every city j the city-level mean income rank at

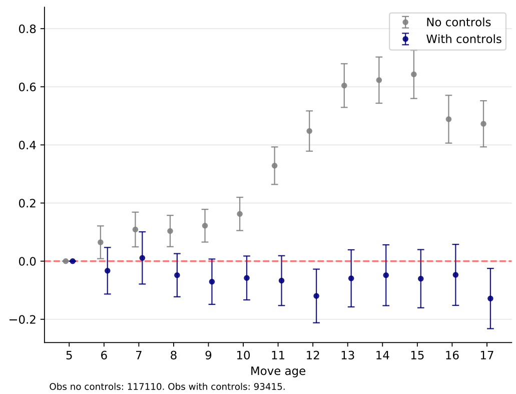
Figure D.1: Relationship between age at first move and parents' characteristics, native-born children



(a) Mother years of schooling



(b) Father years of schooling

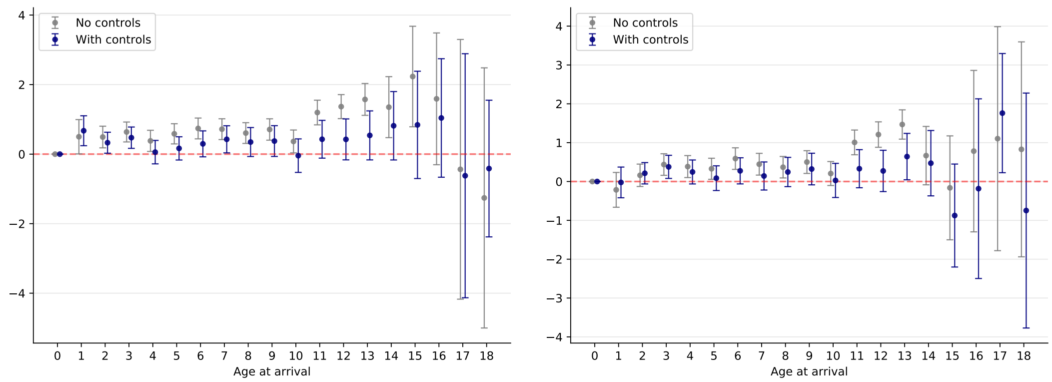


(c) Wage growth, ages 0-5

Note: This figure presents balance test results for the non-immigrant sample, showing the relationship between the child's age at the first move between cities in Israel and parents' characteristics. Sub-figures D.1a and D.1b present parents' years of schooling, as recorded in the 1995 census. Sub-figure D.1c presents the relationship between the age of move and parents' earnings growth when the child was aged 0-5. Controls include origin-destination fixed effects, birth-year fixed effects, and parents' income rank interacted with the year of birth. Confidence intervals are constructed using family levels clustered standard errors.

age 28 $\bar{Y}_j$ among the children whose families did not move between cities until the age of 30. Then, in our main analysis sample of natives who moved only once between cities in childhood, we study whether moving one year earlier from origin o to destination d shifts outcomes in the same direction as the difference between origin and destination city mean outcomes and how this relationship varies by the age of the child. Formally, on the sample of native-born children to families who moved once, we run the following

Figure D.2: Relationship between age of arrival to Israel and parents' characteristics, immigrants



(a) Father years of schooling

(b) Mother years of schooling

Note: This figure presents the relationship between the child's age at the move to Israel and the parents' education. Sub-figure D.2a presents the father's years of schooling, and D.2b presents the mother's years of schooling from the 1995 census. Controls include origin-destination-birth-year fixed effects and parents' income rank interacted with the year of birth. Confidence intervals are constructed using family levels clustered standard errors.

regression:

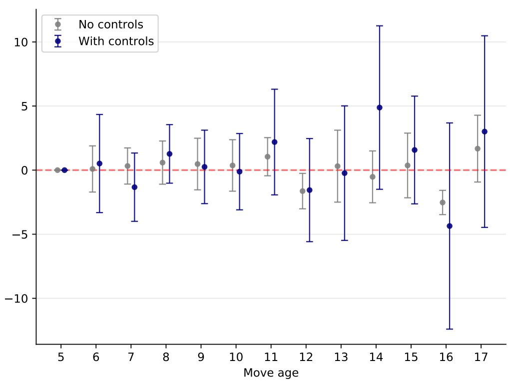
$$Y_i = \sum_{m=1}^{30} \beta_m \mathbb{1}\{m(i) = m\} \Delta_{o(i)d(i)} + x_i'\gamma + \epsilon_i, \tag{13}$$

where $o(i)$ is the origin city of child i, $d(i)$ is the destination city of child i, x_i includes all the controls and fixed effects described in section 3.2 which are origin-destination-second destination fixed effects, and cohorts dummies interacted with parents' income rank, and $\Delta_{od} = \bar{Y}_d - \bar{Y}_o$. Our parameters of interest are β_m coefficients, which, under our identification assumption, measure the effect of moving at age a to a destination city d, which has one percentile rank higher children income rank in age 28 than in the origin city o.

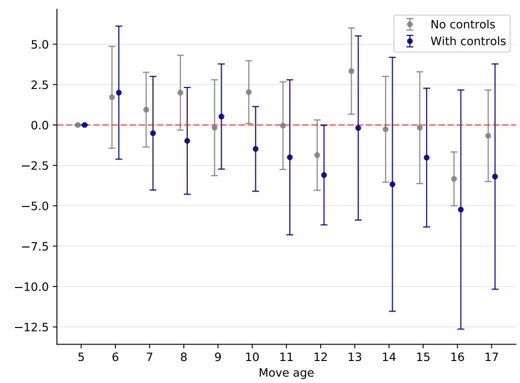
For immigrants, we conduct a similar exercise, with the adjustment for the fact that immigrants include both children who were born in Israel and children who arrived in Israel from the USSR at different ages. We calculate $\bar{Y}_j$ among the children of immigrants who were born in Israel and stayed in the same city up until age 17. Because our main analysis sample includes all immigrants, including those who were born in Israel,[41] we randomly split the sample of immigrants who were born in Israel into two random samples, and within each sample $s \in \{1, 2\}$, we calculate their mean outcome $\bar{Y}_{js}$. Then, on the sample of immigrant children who stayed in the same city

[41] They are included with a moving age of zero, that is, with exposure of 17 years to their location.

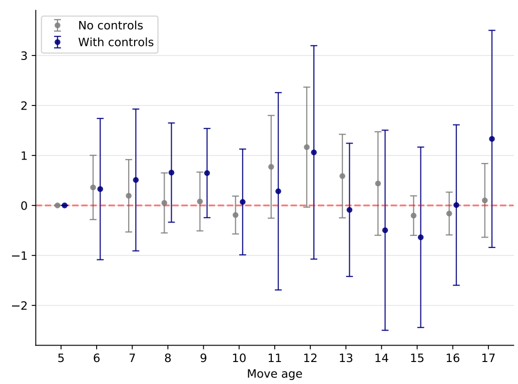
Figure D.3: Relationship between age at second move and parents' characteristics, native-born children



(a) Mother years of schooling



(b) Father years of schooling



(c) Wage growth ages 1-5

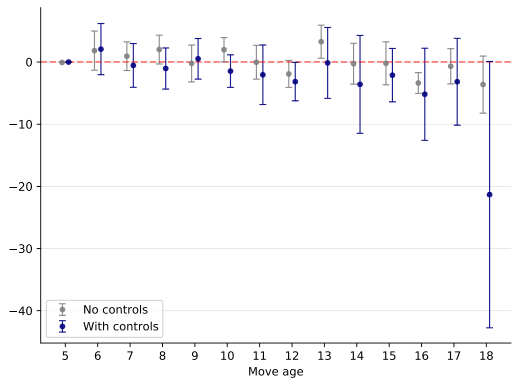
Note: This figure presents balance test results for the non-immigrant sample, showing the relationship between the child's age at the second move between cities in Israel and parents' characteristics. Sub-figures D.3a and D.3b present parents' years of schooling, as recorded in the 1995 census. Sub-figure D.3c presents the relationship between the age at the second move and parents' earnings growth when the child was aged 0-5. Controls include origin-first destination-second destination fixed effects, birth-year fixed effects, and parents' income rank interacted with the year of birth. Confidence intervals are constructed using family levels clustered standard errors.

until age 18, we run the following regression:

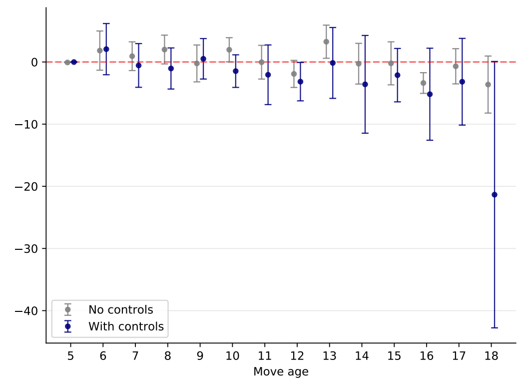
$$Y_i = \beta_0 \mathbb{1}\{a(i) = 0\}\bar{Y}_{j(i)s'(i)} + \sum_{a=1}^{17} \beta_a \mathbb{1}\{a(i) = a\}\bar{Y}_{j(i)} + x_i'\gamma + \epsilon_i, \qquad (14)$$

where $s'(i)$ is the random sample that does not include child i. Our parameters of interest are the β_a coefficients, which measure the effect of arriving in Israel at age a to a city with one percentile rank higher average income of immigrants born in Israel. Note that our cohort restriction, together with the fact that the immigration wave is

Figure D.4: Relationship between age when the family moved between cities in Israel and parents' characteristics, immigrants



(a) Father years of schooling



(b) Mother years of schooling

Note: This figure presents the relationship between the child's age at the move to Israel and the parents' education. Sub-figure D.4a presents the father's years of schooling, and D.4b presents the mother's years of schooling from the 1995 census. Controls include origin-destination-birth-year fixed effects and parents' income rank interacted with the year of birth. Confidence intervals are constructed using family levels clustered standard errors.

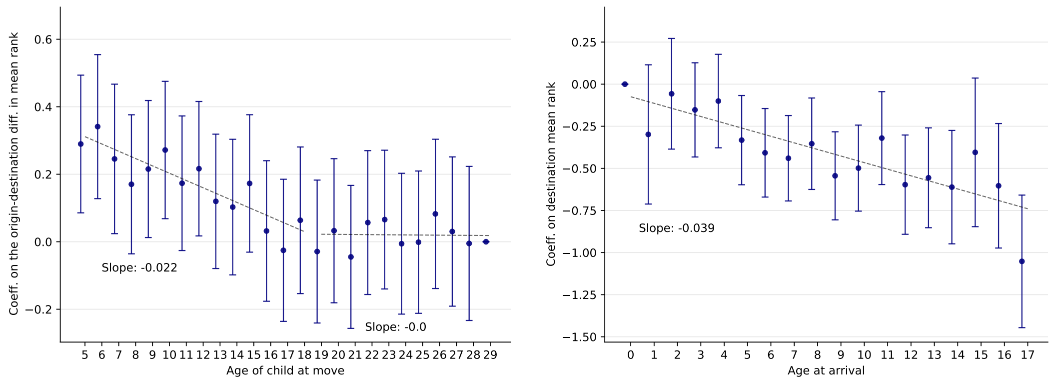
restricted to the years 1989-2000, implies that we have only a few observations with an immigration age above 17.

Figure D.5 plots the estimates and confidence intervals of β_m from both Equations 13 and 14, with the earnings rank at age 28 as the outcome.

The estimates in Figure 13 on the non-immigrants exhibit two key patterns: First, the estimates of β_m decline steadily with ages $m < 18$ at a linear rate, suggesting that the age of arrival to a city with higher mean outcomes is approximately linearly related to children's income rank at age 28. Such a pattern was also found in the US (Chetty and Hendren, 2018a), Australia (Deutscher, 2020), and Canada (Laliberté, 2021). Second, we find that for ages $m \geq 18$, the effect of parents moving to a location with one percentage higher outcome of permanent residents in the destination compared to origin does not change with the age of the child when moving. Such a result is in line with the institutional setting in Israel in which most of the children enlist in the Israeli army at ages 18-19 and, therefore, are significantly less exposed to the local institutions and peers.

Similarly, the pattern in Figure D.5 among immigrants displays a similar pattern where b_a declines steadily with age. Regressing $\hat{b}_m$ of m for $m \leq 18$ among native-born children, we estimate a slope of 0.022. That is, the income rank at age 28 of children who moved gets similar to the mean income rank of permanent residents at a rate of 2.2% per year until age 18. Repeating the same exercise for immigrants, we estimate

Figure D.5: Childhood exposure effects on earnings rank in adulthood



(a) Natives (b) Immigrants

Note: This figure presents the exposure effect coefficients on earnings rank measured at age 28, against the child's age at the time of move. In dashed lines are the linear piece-wise fitted lines with the corresponding slope. Panel (a) presents the effects on native-born Israeli children, estimated via Equation 13, and Panel (b) presents the effects on immigrants from the former Soviet Union, estimated via Equation 14. Earning ranks are defined as percentiles within cohort. Controls include origin-destination-birth-year fixed effects and parental income rank interacted with the year of birth. Confidence intervals are constructed based on family-level clustered standard errors.

a larger slope of 0.039. There could be several explanations for why the exposure slope among natives is larger than the exposure slope among immigrants. Following our findings above, we find that the location effects of immigrants are larger than the location effects of native-born. Therefore, it is likely that our exposure-sloped results express that to some degree. Second, it is also possible that sampling error in $\bar{Y}_j$ attenuates our estimated slope more severely for natives since their sample is smaller while the regression includes more controls.

The kink at age 18 motivates a piece-wise regression to estimate the slope coefficients on the individual-level data formally. For natives, we use the following specifications:

$$\begin{aligned} Y_i =& (\gamma_{below} + \beta_{below} m(i))\Delta_{o(i)d(i)}\mathbb{1}\{m(i) \leq 18\} \\ &+ (\gamma_{above} + \beta_{above}(30 - m(i)))\Delta_{o(i)d(i)}\mathbb{1}\{m(i) > 18\} + x_i'\gamma + \epsilon_i, \end{aligned} \tag{15}$$

where the variable definitions and controls are identical to the ones used in Equation 13. The coefficients of interest are β_{below} for the slope below age 18 and β_{above} for the slope above age 18, which we expect to be zero. The corresponding regression equation for immigrants is the following, where we only estimate the slope up to age 18, due to our data constraints:

$$Y_i = \gamma_{below} + \beta_{below} m(i)\bar{Y}_{j(i)s'(i)} + x_i'\gamma + \epsilon_i, \tag{16}$$

where definitions follow Equation 14.

Table D.1 presents the estimated slopes for natives and immigrants in columns (1) and (3). First, we find a small and non-significant coefficient for the slope above age 18 for locals. Additionally, we find a slope of 0.022 and 0.033 for natives and immigrants up to age 18, respectively. These estimates are comparable yet smaller than the effects found in previous studies conducted in the US (0.035 at the county level in Chetty and Hendren (2018a)), Australia (0.033 in Deutscher (2020)), and Canada (0.042 in Laliberté (2021)).

Table D.1: Relationship between years of exposure in childhood city and mean outcomes and posterior location effects

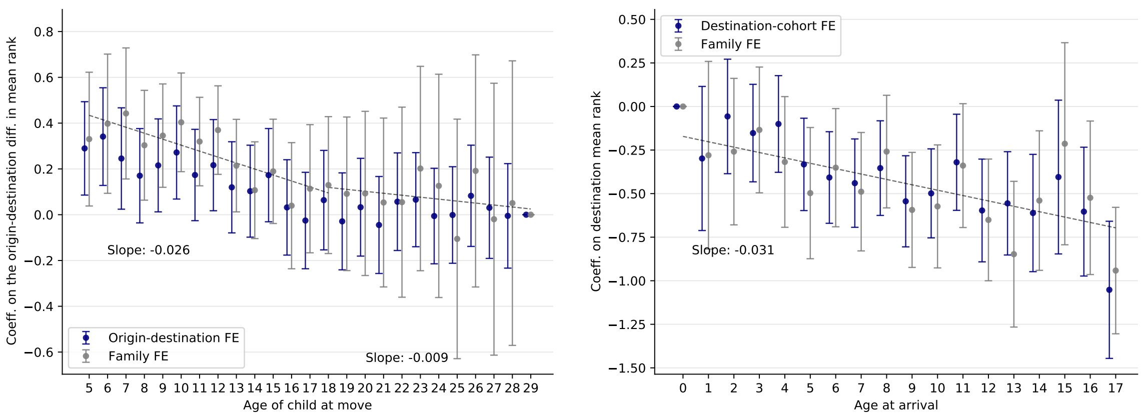
	Mean outcomes				Posteriors	
	Natives (1)	Natives (2)	Immigrants (3)	Immigrants (4)	Immigrants (5)	Natives (6)
$\Delta\times$ below 18	-0.022	-0.019	-0.033	-0.027	-1.433	-0.743
	(0.005)	(0.011)	(0.006)	(0.013)	(0.295)	(0.204)
$\Delta\times$ above 18	0.006	0.005				
	(0.006)	(0.008)				
Family fixed effect	No	Yes	No	Yes	No	No
Obs.	95,500	70,549	138,664	110,462	138,664	95,500

Note: This table reports the linear slope coefficients between location effects or posterior means and the age of move. Columns (1) and (3) present the effects estimated via Equations 15 and 16, accordingly. In columns (2) and (4) we add family fixed-effects to the estimation. Immigrants columns do not have coefficients for moves above age 18 due to our sample restriction. Columns (5) and (6) present the slope coefficients between the posterior means and the arrival age for immigrants and the age of move for locals. Standard errors in parenthesis are clustered at the family level.

Robustness to family fixed effect - Our key identifying assumption is that the potential outcomes of children who move to better vs. worse cities do not vary with the age of move or the age of immigration. If families with higher mean ability move to cities when the children are younger, then Assumption A1 is violated. To test this, we control for differences in family-level factors by including family fixed-effects when estimating Equations (13) and (14). The inclusion of family fixed effects implies sibling comparisons.

Figure D.6 displays the estimates from Equations (13) and (14), with and without family fixed effects on child income rank at age 28. The blue dots and confidence intervals replicate the estimates from Figure D.5, without controlling for family fixed effects. The gray dots and confidence intervals present the estimates with family-fixed effects. The linear decline in the estimated values of β_m for locals and β_a for immigrants until age 18 is very similar to that in the baseline specification, although noisier. Siblings who moved to a city with high outcomes at younger ages have better outcomes than their older siblings. Table D.1 presents in columns (2) and (4) the corresponding slopes estimated with family fixed effects. We find coefficients of 0.19 for natives and 0.026 for immigrants, values close to the estimates above without family fixed effects, presented in the same table in columns (1) and (3).

Figure D.6: Robustness to family fixed effects



(a) Natives

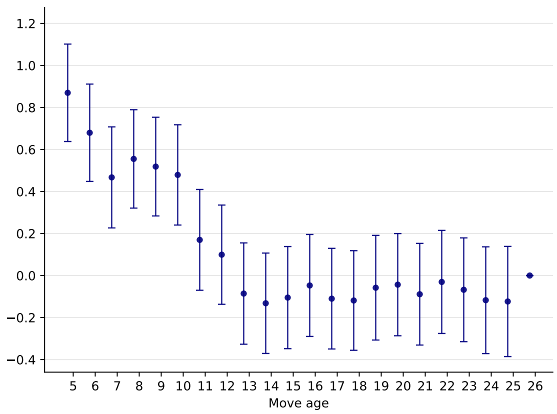
(b) immigrants

Note: This figure presents the exposure effect coefficients on earnings rank measured at age 28 against the child's age at the time of move. In dashed lines are the linear piece-wise fitted lines with the corresponding slope. Panel (a) presents the effects on native-born Israeli children, estimated via Equation 13, and Panel (b) presents the effects on immigrant children, estimated via Equation 14. In grey, we present the results of a regression with family fixed effects instead of the origin-destination-birth-year fixed effects, which implies sibling comparison. In blue, we present the results of the regressions as presented in Figure D.5 for comparison. Earning ranks are defined as percentiles within year. Confidence intervals are constructed based on family-level clustered standard errors.

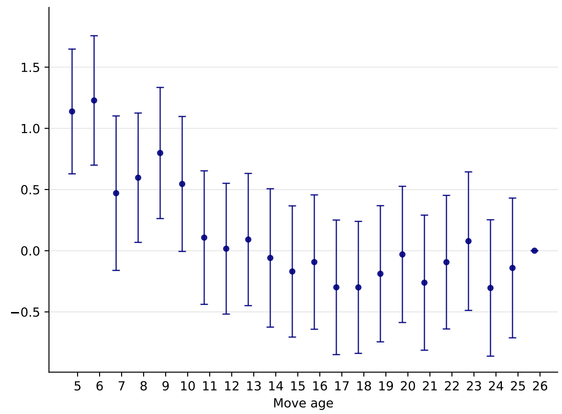
Placebo test using outcomes realized in childhood - In Appendix Figure D.7, we exploit outcomes that are realized before age 18 as a placebo test. If the relationship between child's outcomes and city-level mean outcomes we estimate is driven by selection, that is, that higher ability children move to higher mean outcome cities at younger ages, then we should expect to see the same linear pattern of decreasing impacts until age 18 for every outcome, regardless of its realization year. Otherwise, we expect the kink in the effects from which values flatten out to appear sooner, around the age of realization.

For this purpose, we run Equation 13 utilizing children's scores in the 5th-grade national standardized school evaluation exams, called *Meitzav*, administered by the

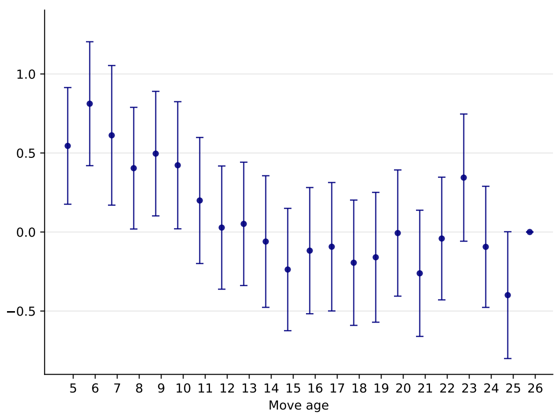
Figure D.7: Childhood exposure effects on 5th grade standardized exam scores of natives



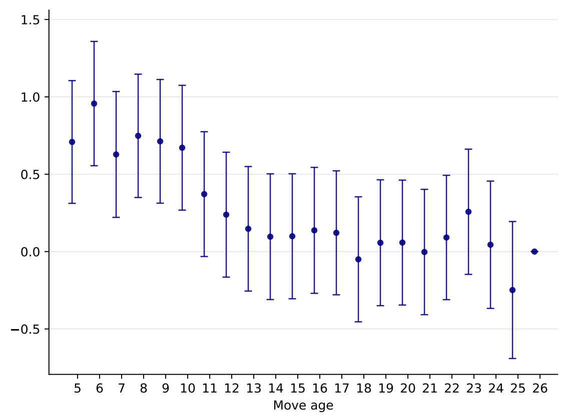
(a) Math



(b) Science



(c) English



(d) Hebrew

Note: This figure presents the exposure effect coefficients on the national standardized exam scores in the 5th grade, the *Meitzav*, which mimics the PISA exam administrated by the OECD. The different panels display the results using students' scores in different key subjects: mathematics (a), science (b), English (c), and Hebrew (d), measured in standard deviations. This exam is administered at a random representative sample of schools, which in our sample amounts to a tenth of the children in the full analysis sample. We present the effects on non-immigrants, estimated via Equation 13. Confidence intervals are constructed based on family-level clustered standard errors.

Israeli Ministry of Education.[42] The *Meitzav* exams cover several key subjects, including mathematics, science, English, and Hebrew or Arab language skills, and closely mirror the PISA exam administrated by the OECD. This exam is administered at a random representative sample of schools, which in our sample amounts to a tenth of the children in the full analysis sample. Since the exam is taken when the child is at age 11, the

[42] "Meitzav" is a Hebrew acronym translating to "School Efficiency and Growth Measures".

estimates in years 11-18 serve as a placebo test as we expect to find null effects in this age span. Note that because children who immigrated after the age of 11 never took the exam, we can perform this exercise only among the non-immigrants. The results in Figure D.7 validate our assumption, as we find that the effect of moving to a city with higher mean grades in the Meitzav exam declines with the age of move up to age 11 and stabilizes thereafter.

Test for linearity - The uncontrolled mean outcomes used in the above regressions consist of both location effects and the mean ability $\bar{Y}_j = \theta_j + \bar{\xi}_j$. That is, the relationship estimated in Figure D.5 could reflect either linearity with respect to location effects or a linear relationship between the age of move and location effects along with a selection bias. Therefore, the interpretation of the estimates in the above exercises is mixed. On the one hand, it does tell us that moving to a place with higher mean outcomes of permanent residents is approximately linear with the years of exposure to the location. However, it is less clear if the location effects θ_{jg} themselves are linearly related to the years of exposure. If $\bar{\xi}_o - \bar{\xi}_d = 0$, then the slope $\beta_{m+1} - \beta_m$ with respect to child's age should be approximately $\frac{1}{A} \approx 0.055$ for $A = 18$. Otherwise, it is the sum of $\frac{1}{A}$ and the relationship between nonmovers' ability and location effects.

To address this concern, we conducted an additional exercise. We regress the estimated location effects of movers from Equation (2), i.e., the estimated location effects residualized of selection based on x_i, on children's long-run outcomes, allowing separate coefficient for each age at the time of the move. If the relationship is approximately linear, we should expect a constant rate of change in the age-specific coefficients by the age at move.

Since Equation (2) is estimated on the same sample, we expect a mechanical correlation. To avoid this, we run Equation (2) on two random splits $s(i) \in \{1, 2\}$ of our analysis sample. Given our findings above, we run these regressions using age $A = 18$ as the last age at which places affect outcomes. Therefore for every group g, city i, and sample split s, we estimate $\hat{\theta}^s_{jgp}$. Then, to test for the linearity assumption on the sample of natives, we run:

$$Y_i = \sum_{m=1}^{18} \beta_m \mathbb{1}\{m(i) = m\} \hat{\Delta}^{s'(i)}_{o(i)d(i)p(i)} + x_i'\gamma + \epsilon_i \tag{17}$$

where $s'(i)$ is the random sample that does not include child i, $\hat{\Delta}^s_{od} = \hat{\theta}^{*s}_{dgp} - \hat{\theta}^{*s}_{ogp}$, and $\hat{\theta}^{*s}_{dgp}$ are the Empirical Bayes (EB) posterior mean of θ_{dgp} we estimate in Section 9.

Similarly, we estimate the equivalent regression on the sample of immigrants:

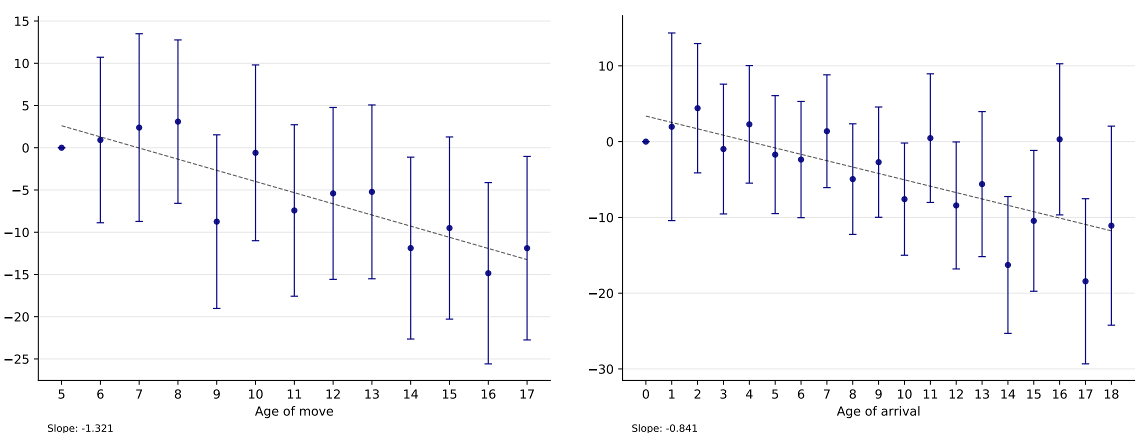
$$Y_i = \sum_{a=1}^{18} \beta_a \mathbb{1}\{a(i) = a\} \hat{\theta}^{*s}_{j(i)g(i)} + x_i'\gamma + \epsilon_i \tag{18}$$

where the base-level age is the immigrants who were born in Israel.

If Equation 2 captures the true functional form of location effects and outcomes, then for immigrants, the coefficient β_a should give us the *number* of θ_j accumulated in location j each year of exposure as θ^* is the effect of spending one year in city j. Thereby, the slope $\beta_{a+1} - \beta_a$, with respect to exposure time should be 1 on average. Accordingly, for natives, β_m gives the equivalent sum of location effects of the destination location relative to the origin.

Figure D.8 presents the main results visually. We observe a linear relationship between the posterior means and the age of move for both native-born and immigrants, with fitted slopes of 1.321 and 0.841, respectively. In Table D.1, we present the formally estimated slopes in column (5) for immigrants and column (6) for locals, with values of 1.433 and 0.743, respectively. Importantly, in support of the linear effect assumption, we cannot reject the null that these coefficients are different than 1.

Figure D.8: Relationship between age of arrival/move and posterior mean



(a) Native-born (b) Immigrants

Note: This figure displays the relationship between the years of exposure and the posterior mean of location effect estimated via Equations (17) for immigrants and (18) for native-born Israeli children. We present the β_a coefficients for each arrival age for immigrants in Panel (a) and β_m coefficients for each moving age in Panel (b), with the confidence intervals based on robust standard errors as vertical lines. The dashed lines represent the linear fitted lines, and their slopes are at the bottom left of the figure. The baseline coefficient is the lowest age of arrival/move.

E Estimation of Variance Componenets

For every city $j \in \{1, .., J\}$ in Israel, we denote θ_{jgp} the effect of spending one more year in the city j for an individual who belongs to group $g \in \{I, L\}$, with parent income rank p. We model the location effects in each city j to be:

$$\theta_{jgp} = \alpha_{jg} + \eta_{jg} p,$$

where θ_{jg} measures the returns to the city j for individuals who belong to group g with parents at the lowest income rank, and η_{jg} measures the returns to parents rank in city j. Therefore, for every group g, we end up with a vector of size J of location effects intercepts $\alpha_g = (\alpha_{1g}, ..., \alpha_{Jg})'$, and a vector of size J parents ranks slopes $\eta_g = (\eta_{1g}, ..., \eta_{Jg})'$. Finally, we denote $\theta \in \mathbb{R}^{4J}$ the stacked vector that describes the location effects in Israel $\theta = (\alpha'_{\mathcal{N}}, \eta'_{\mathcal{N}}, \alpha'_{\mathcal{I}}, \eta'_{\mathcal{I}})'$.

We estimate θ by running equation (2) which results with a $4 \cdot J$ vector of estimated location effects $\hat{\theta} = (\hat{\alpha}'_{\mathcal{N}}, \hat{\eta}'_{\mathcal{N}}, \hat{\alpha}'_{\mathcal{I}}, \hat{\eta}'_{\mathcal{I}})'$, and $\Sigma = \mathcal{V}(\hat{\theta})$, the sampling variance of $\hat{\theta}$. In this appendix section, we provide a detailed explanation of how we estimate the variance of θ and its standard error.

E.1 Method of Moments Variance Component Estimate

We denote $\Omega \in \mathbb{R}^{4\times 4}$ the variance covariance matrix of $\theta_j = (\alpha_{j\mathcal{N}}, \eta_{j\mathcal{N}}, \alpha_{j\mathcal{I}}, \eta_{j\mathcal{I}})$. The maximum likelihood variance of elements of θ_j:

$$\sigma_g^2 = \sum_{j=1}^{J} \pi_{jg}(z_{jg} - \sum_{j=1}^{J} \pi_{jg} z_{jg})^2 \tag{19}$$

where z_{jg} represents either the intercept term θ_{jg}, or the slope on parents rank η_{jg}, and $\pi_{jg} = \frac{n_{jg}}{N_g}$ are group-specific observation share , where n_{jg} is the number of children of group g residing during childhoods in city j for at least one year, and $N_g = \sum_{j=1}^{J} n_{jg}$. Note that we can write (19) also as:

$$\begin{aligned}
\sigma_g^2 &= \sum_{j=1}^{J} \pi_{jg} z_{jg}^2 - \left(\sum_{j=1}^{J} \pi_{jg} z_{jg} \right)^2 \\
&= \sum_{j=1}^{J} (1 - \pi_{jg}) \pi_{jg} z_{jg}^2 - 2 \sum_{j=1}^{J} \sum_{k=j+1}^{J} \pi_{jg} \pi_{kg} z_{jg} z_{kg} \\
&= S_{ML}.
\end{aligned}$$

Let $z_g = (z_{1g}, ..., z_{Jg})'$. Then we can represent (19) as a quadratic form:

$$S_{ML} = z'\tilde{A}z$$

where

$$\tilde{A} = \begin{pmatrix} (1-\pi_{1I})\pi_{1I} & -\pi_{1I}\pi_{2I} & \cdots & -\pi_{1I}\pi_{2I} \\ -\pi_{2I}\pi_{1I} & (1-\pi_{2I})\pi_{2I} & \cdots & -1 \\ \vdots & \vdots & \ddots & \vdots \\ -\pi_{JI}\pi_{1I} & -\pi_{JI}\pi_{2I} & \cdots & (1-\pi_{JI})\pi_{JI} \end{pmatrix}.$$

To get an unbiased estimator for the variance, we multiply by $\frac{N_I}{N_I - 1}$:

$$S_U = \frac{N_I}{N_I - 1} S_{ML}$$

And therefore work with $A = \frac{N_I}{N_I - 1}\tilde{A}$, and get $S_U = z'Az$.

Respectively, the unbiased estimate of the covariance of z_j and z_j', elements of θ_j, can be written as:

$$\sigma_{zz'} = z'Az.$$

Lastly, note that using the representation above, we can represent any of the elements of Ω as a quadratic form as a function of θ. For example, the variance of $\alpha_{j\mathcal{N}}$ can be written as

$$\sigma_I^2 = \theta' B \theta$$

where

$$B = \begin{pmatrix} A & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix}$$

and the covariance between $\alpha_{j\mathcal{N}}$ and $\alpha_{j\mathcal{I}}$ can be written as

$$\sigma_I^2 = \theta' B \theta$$

where

$$B = \begin{pmatrix} 0 & 0 & A & 0 \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \\ 0 & 0 & 0 & 0 \end{pmatrix}.$$

Since we do not observe θ but its noisy estimate $\hat{\theta}$ and its sampling variance Σ, an unbiased estimate for the variance component is therefore:

$$\widehat{\theta' B\theta} = \hat{\theta}' B\hat{\theta} - Tr(B\Sigma)$$

where $Tr(\cdot)$ is the trace operator.

F Neighborhood grouping

We construct the geographic unit of neighborhoods based on the geographic unit "statistical area". Statistical areas are defined in the census as areas of 3,000-5,000 inhabitants, similar to a census tract in the US which is aimed to have around 4,000 inhabitants. This allows us to check for whether intra-city segregation drives our results of location effects heterogeneity, as described in Section 8. The city divisions, along with the 2008 statistical area codes that comprise them, are as follows:

- **Tel-Aviv:** North Tel Aviv (111-235), Center Tel Aviv (311-625), South Tel Aviv (811-947), Jaffa (711-747)
- **Haifa:** North-West Haifa and the shore (111-434), North-East Haifa (511-644), South Haifa (711-945)
- **Jerusalem:** East Jerusalem (611-741, 1411-1614, 2100-2999), Jerusalem Center (811-864, 1011-1044, 1211-1355), South Jerusalem (1111-1147, 1621-1644), North Jerusalem (111-543, 911-934)
- **Be'er Sheva:** Be'er Sheva Old (111-314), Be'er Sheva New (411-645)
- **Natanya:** Natanya East + Natanya North (111-355), Natanya South (411-534)
- **Petach Tiqva:** Petach Tiqva West (111-324), Petach Tiqva East (411-524)
- **Rishon Lezion:** Rishon Lezion East (111-427), Rishon Lezion West (511-625)
- **Ashdod:** Ashdod North (111-244), Ashdod South (311-434)

In less dense areas, we group localities according to their regional councils. Regional councils in Israel, or "Mo'atzot Ezoriyot" in Hebrew, serve as the administrative bodies for a group of smaller, geographically close communities, typically in rural settings. The overarching governance provided by regional councils enables us to include smaller villages and towns in our analysis as single units due to their shared administrative bodies, geographic proximity, and usually also education institutions. The regional councils that are included in our sample are the following: Upper Galilee, Mateh Asher, Emek HaYarden, Emek HaMaayanot, Gilboa, Jezreel Valley, Hof HaCarmel, Hefer Valley, Mateh Yehuda, Gezer, Be'er Tuvia, Eshkol, Merhavim, Misgav, Golan, Shomron, Mateh Binyamin, and Gush Etzion.

G Effect on inter-marrige

We evaluate the role of cultural assimilation in location effect heterogeneity by examining whether places that causally increase the probability of Russian-Israeli intermarriage also affect children's long-run economic outcomes. We begin by estimating Equation 2 using an intermarriage indicator. Specifically, for immigrants, the indicator equals one if the child married a non-Russian immigrant, and for natives, it equals one if the child married a Russian immigrant.

In Table XX, which reports the variance components of the intermarriage location effects, we document two interesting findings. First, we find no evidence of causal location effects for native-born children. Second, for immigrants, we find that places do affect the likelihood of intermarriage, although these effects do not vary by parental income. Therefore, our final measure for intermarriage location effects is the slope parameter on years of exposure in each city—similar to Equation **??** but without the interaction with parental income.

Lastly, in Table G.1, we present the coefficient from a weighted least squares regression of income rank location effects on the posterior mean intermarriage location effects. Columns (1), (3), (5), and (7) present the results with no controls, while the other columns additionally control for diversity index, population size, and per capita local welfare expenditure. We find that while intermarriage effects are not strongly predictive of income-rank location effects for low-income families, they are predictive of high-income location effects.

Table G.1: The relationship between intermarriage and child income ranks location effects

	Immigrants				Natives			
	θ_{25} (1)	θ_{25} (2)	θ_{75} (3)	θ_{75} (4)	θ_{25} (5)	θ_{25} (6)	θ_{75} (7)	θ_{75} (8)
Inter-marrige effect	0.17	0.11	0.59	0.52	-0.07	-0.12	0.28	0.21
	0.18	0.18	0.20	0.20	0.18	0.18	0.17	0.16
Controls	No	Yes	No	Yes	No	Yes	No	Yes
# of cities	98	98	98	98	98	98	98	98

H The Joint Distribution of Location Effects

For evrey city j, let θ_g be the $J \times 1$ vector of location effects of group $g \in \{\mathcal{N}, \mathcal{I}\}$, with the corresponding $J \times 1$ vector $\hat{\theta}_g$ of estimated location effects. We assume that the estimated location effect follows a normal distribution: $\hat{\theta}_g \sim \mathcal{N}(\theta_g, \Sigma_g)$, which can be justified by central limit theorem with a growing number of families in each city. Our goal is to estimate the joint distribution of location effects while allowing the mean location effects to vary linearly with a few city-level covariates z_j.

Abstracting from the p subscript for simplicity, our model is described by:

$$\begin{aligned} \theta_{jg} &= z_j'\beta_g + \nu_{jg} & \qquad \nu_j | z_j, \Sigma &\overset{iid}{\sim} G \\ \hat{\theta}_{jg} &= \theta_{jg} + u_{jg} & \qquad U_g | z_j, \Sigma &\sim \mathcal{N}(0, \Sigma) \end{aligned} \tag{20}$$

for $g \in \{\mathcal{N}, \mathcal{I}\}$, where $\nu_j = (\nu_{j\mathcal{N}}, \nu_{j\mathcal{I}})'$, $U_g = (u_{1g}, ..., u_{Jg})$, σ is the $2J \times 2J$ sampling error covariance matrix with Σ_g on the diagonal and zeros in the off-diagonal, and z_j is a $p \times 1$ vector of city level covariates, with $z = (z_1, ..., z_p)$ the $J \times p$ design matrix. In this model, the prior distribution of the demeaned location effects $\nu_{jg} = \theta_{jg} - z_j'\beta_g$ is independent of z_j and Σ. To estimate this model, one needs to estimate $\beta = (\beta_{\mathcal{N}}, \beta_{\mathcal{I}})'$ and G, the distribution of ν_j.

Throughout this paper, we estimate the prior distribution of immigrant-native location effects by taking a two-step approach. First, we estimate β by city-size weighted least squares regression and form each group's $J \times 1$ residual $r_{jg} = \hat{\theta}_{jg} - z_j'\beta_g$. Then, in the second step, we estimate the joint distribution of $r_j = (r_{j\mathcal{N}}, r_{j\mathcal{I}})'$.

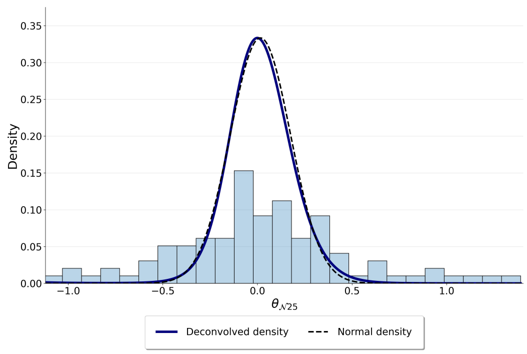
Choice of z_j: Tables H.1 and H.2 report the estimates of β and the variance component using diffrent covariates z_j. Our preferred model, which is presented in Column (2), is chosen as the one that maximizes the explanatory power of location effects of both immigrants and natives.

H.1 Log-spline Estimator

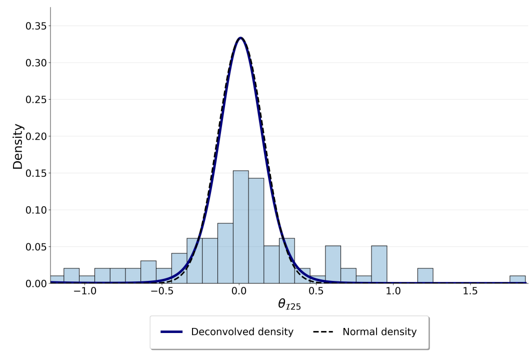
We start by estimating the marginal distribution of each ν_{jg} nonparametrically using the empirical Bayes deconvolution estimator from Efron (2016). Under this approach, the prior distribution is assumed to belong to an exponential family, estimated flexibly by a fifth-order spline. The spline parameters are estimated via penalized maximum likelihood, where the log-likelihood is weighted by the total number of residents in each city. Following the approach taken in Kline et al. (2024), the penalization parameter is chosen to match the mean zero and method of moments variance component estimate of ν_j: $r_j'Br_j - Tr(B\tilde{\Sigma})$ described in Appendix Section E.

Appendix Figure H.1 plots the marginal deconvolved distribution of r_{jg} separately for every $g \in \{\mathcal{N}, \mathcal{I}\}$ and low and high-income families (solid blue line) together with the density of a normal distribution with the same mean and variance (dashed line). Results strongly suggest that each marginal distribution is well approximated by a normal distribution.

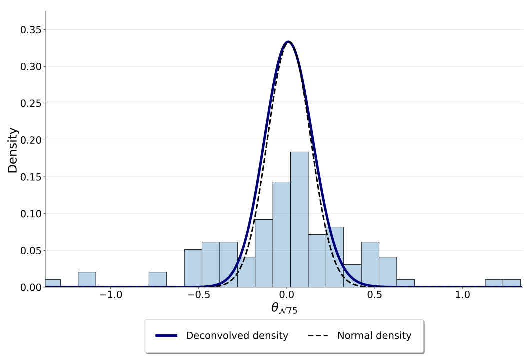
Figure H.1: Deconvolved density of childhood location effects of immigrants and natives



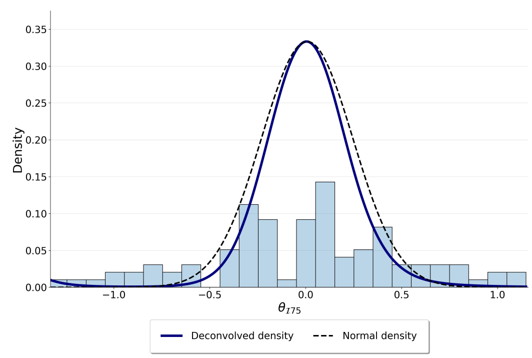
(a) θ_{25} Natives



(b) θ_{25} immigrants



(c) θ_{75} Natives



(d) θ_{75} immigrants

Note: These figures display the log-spline estimates of the distribution of the residuals of childhood location effects of immigrants and natives. The residuals are the difference between the estimated location effects $\hat{\theta}$ and the $z_j'\beta$, where β, presented in panel (i) of Tables H.1 and H.2. Panel (a) presents the distribution for natives from the 25th percentile of the national income distribution. Panel (b) presents the distribution for immigrants from the 25th percentile of the national income distribution. Panel (c) presents the distribution for locals from the 75th percentile of the national income distribution. Panel (c) presents the distribution for immigrants from the 75th percentile of the national income distribution. The solid blue line shows the estimated deconvolved density following Efron (2016) penalized log-spline estimator with a natural cubic spline with five knots. The parameters of the deconvolved density were chosen to match the mean zero and variance from Tables H.1 and H.2. Histograms show the estimated location effects. Dashed lines show the density of normal distribution with the same mean and variance.

H.2 Normal Prior

Following the previous results, we assume that G follows a mean zero normal distribution with 2×2 covariance matrix Ω. With normally distributed signal and noise, the joint distribution of the estimated location effects is given by:

$$\hat{\theta}|\beta, \Omega, \Sigma, z \sim \mathcal{N}(\mu(z), V)$$

where $\mu(z) = (z'\beta_{\mathcal{N}}, z'\beta_{\mathcal{N}\mathcal{I}})'$, $V = \check{\Omega} + \tilde{\Sigma}$, $\check{\Omega} = \Omega \otimes I_J$, and I_J is $J \times J$ unit matrix. Lastly, the posterior distribution we exploit for decision-making is given by

$$\theta|\hat{\theta}, \Sigma, \Omega, \mu_\theta(z), z \sim N(\theta^*(z), (\check{\Omega}_\nu^{-1} + \Sigma^{-1})^{-1})$$

where

$$\theta^*(z) \equiv \mathbb{E}[\theta_{jg}|\hat{\theta}, z] = (\check{\Omega}_\nu^{-1} + \tilde{\Sigma}^{-1})^{-1} \left(\check{\Omega}_\nu^{-1}\check{\mu}_\theta(z) + \tilde{\Sigma}^{-1}\hat{\theta}\right),$$

and β and Ω are estimated via weighted least squares and method of moments as described above.

Estimation results: Appendix Tables H.1 and H.2 report the estimated hyperparameters of this extended model for families in the 25th and 75th percentiles of the income distribution. As shown in panel (i), the coefficients on z_j closely match out findings from Section 6.

Our parsimonious extended model provides a good fit with high predictive power for each group's location effects. The first two rows of panel (iii) report $\sqrt{\beta_g^2 \mathbb{V}(z) + \sigma_g^2}$—the total standard deviation of location effects—suggest that the variance of the location effects of immigrants and natives qualitatively aligns with the method of moments estimates in Table 2. The last row in panel (iii) reports the total correlation between $\theta_{j\mathcal{N}p}$ and $\theta_{j\mathcal{I}p}$ which is the ratio of the total covariance between immigrants and natives ($\mathbb{C}ov(\theta_{j\mathcal{I}p}, \theta_{j\mathcal{N}p}) = \mathbb{C}ov(\mu_{\mathcal{N}}(z_j), \mu_{\mathcal{I}}(z_j)) + \rho\sigma_{\mathcal{N}}\sigma_{\mathcal{I}}$) divided by the product to the standard deviation reported in panel (vi). In line with our findings in Table A.6, we find a small negative correlation between the location effects of low-income immigrants and natives and a stronger positive correlation of 0.18 among high-income families. Lastly, to assess the predictive power of z_j, we calculate the R^2 of each group as the share of variation in θ explained by variation in z_j, $\frac{\beta_g^2 \mathbb{V}(z)}{\beta_g^2 \mathbb{V}(z) + \sigma_g^2}$. For low-income families, z_j explains between 35-40% of the variation.

Table H.1: Location effect hyperparameters ($p = 25$)

	(1)	(2)	(3)	(4)	(5)
(i) Mean					
Intercept (I)	0.122	-0.081	0.004	0.044	-0.057
	(0.082)	(0.171)	(0.126)	(0.121)	(0.167)
Intercept (N)	0.408	0.354	0.378	0.376	0.351
	(0.072)	(0.147)	(0.105)	(0.103)	(0.147)
Diversity (I)		0.433			0.457
		(0.240)			(0.251)
Diversity (N)		0.115			0.113
		(0.240)			(0.239)
Welf. expnd. (I)	-0.006	-0.005	-0.005	-0.003	-0.002
	(0.012)	(0.012)	(0.012)	(0.016)	(0.015)
Welf. expnd. (N)	-0.054	-0.053	-0.053	-0.054	-0.053
	(0.014)	(0.014)	(0.014)	(0.014)	(0.014)
1st PCA of Σ (I)	0.694	0.867	0.792	0.732	0.823
	(0.100)	(0.144)	(0.117)	(0.134)	(0.160)
1st PCA of Σ (N)	-0.721	-0.741	-0.770	-0.770	-0.741
	(0.148)	(0.151)	(0.171)	(0.169)	(0.148)
Pop. size (I)	0.005	0.004	0.005		
	(0.002)	(0.002)	(0.002)		
Pop. size (N)	-0.000	-0.000	-0.000		
	(0.003)	(0.002)	(0.003)		
Share imm. (I)			0.565	0.568	
			(0.298)	(0.312)	
Share imm. (N)			0.145	0.145	
			(0.310)	(0.309)	
(ii) Std. devs. and corr.					
$\sigma_{\mathcal{N}}$	0.158	0.157	0.157	0.157	0.157
	(0.0500)	(0.0493)	(0.0494)	(0.0499)	(0.0495)
$\sigma_{\mathcal{I}}$	0.151	0.138	0.140	0.147	0.144
	(0.0553)	(0.0550)	(0.0546)	(0.0552)	(0.0550)
ρ	-0.029	-0.079	-0.071	-0.072	-0.082
	(1.039)	(1.407)	(1.177)	(1.288)	(1.313)
(iii) Total std. and corr.					
Natives	0.206	0.204	0.205	0.205	0.205
	(0.041)	(0.041)	(0.041)	(0.041)	(0.041)
Immigrants	0.168	0.171	0.171	0.173	0.173
	(0.055)	(0.053)	(0.053)	(0.054)	(0.053)
Correlation	-0.041	-0.114	-0.117	-0.145	-0.141
	(0.6453)	(0.4970)	(0.5011)	(0.5242)	(0.4696)
	[-0.891, 0.756]	[-0.976, 0.585]	[-1.005, 0.580]	[-1.091, 0.546]	[-1.052, 0.539]
R^2 Natives	0.412	0.408	0.413	0.413	0.413
R^2 Immigrants	0.192	0.349	0.330	0.278	0.307
# of cities	98	98	98	98	98

Note: This table reports the estimated parameters and standard errors of the joint distribution of native-born and immigrant location effects, assuming they are drawn from a normal distribution. Panel (i) reports the mean native (N) and immigrant (I) location effects, a linear function of city-level covariates. Panel (ii) reports the standard deviation (σ) of natives and immigrants and the correlation (ρ) of the random effect, and panel (iii) reports the implied total standard deviation, defined as $\sqrt{\beta_g^2 \mathbb{V}(z) + \sigma_g^2}$ for every group $g \in \{\mathcal{I}, \mathcal{N}\}$, and the implied correlation, which is the ratio between $\mathbb{C}ov(z_j' \beta_{\mathcal{I}}, z_j' \beta_{\mathcal{I}}) + \rho \sigma_I \sigma_N$ and the product of the implied standard deviation of immigrants and natives. R^2 is defined as the group specific ratio between variance share $\frac{\beta_g^2 \mathbb{V}(z)}{\beta_g^2 \mathbb{V}(z) + \sigma_g^2}$. Panel (i) was estimated by a city-size weighted least regression. In panel (i), robust standard errors are reported in parenthesis, and in panels (ii)-(iii), parentheses report the parametric bootstrapped standard errors, and square brackets report parametric bootstrapped equal-tailed confidence intervals.

Table H.2: Location effect hyperparameters ($p = 75$)

	(1)	(2)	(3)	(4)	(5)
(i) Mean					
Intercept (I)	0.340	0.060	0.168	0.196	0.076
	(0.101)	(0.197)	(0.145)	(0.147)	(0.195)
Intercept (N)	0.144	-0.047	0.057	0.077	-0.037
	(0.065)	(0.100)	(0.079)	(0.082)	(0.102)
Diversity (I)		0.599			0.614
		(0.314)			(0.322)
Diversity (N)		0.401			0.413
		(0.184)			(0.183)
Welf. expnd. (I)	-0.017	-0.016	-0.016	-0.014	-0.014
	(0.018)	(0.018)	(0.018)	(0.021)	(0.020)
Welf. expnd. (N)	-0.037	-0.035	-0.036	-0.035	-0.034
	(0.012)	(0.012)	(0.012)	(0.013)	(0.012)
1st PCA of Σ (I)	0.671	0.923	0.825	0.779	0.893
	(0.147)	(0.216)	(0.180)	(0.181)	(0.215)
1st PCA of Σ (N)	1.330	1.440	1.394	1.426	1.468
	(0.819)	(0.822)	(0.822)	(0.848)	(0.844)
Pop. size (I)	0.003	0.003	0.003		
	(0.004)	(0.004)	(0.004)		
Pop. size (N)	0.002	0.002	0.002		
	(0.002)	(0.002)	(0.002)		
Share imm. (I)			0.834	0.836	
			(0.407)	(0.418)	
Share imm. (N)			0.413	0.417	
			(0.262)	(0.263)	
(ii) Std. devs. and corr.					
$\sigma_{\mathcal{N}}$	0.131	0.117	0.124	0.126	0.119
	(0.0462)	(0.0456)	(0.0462)	(0.0464)	(0.0457)
$\sigma_{\mathcal{I}}$	0.245	0.229	0.229	0.232	0.231
	(0.0494)	(0.0501)	(0.0498)	(0.0498)	(0.0496)
ρ	0.267	0.129	0.171	0.187	0.139
	(0.887)	(0.942)	(0.842)	(0.724)	(0.815)
(iii) Total std. and corr.					
Natives	0.217	0.221	0.219	0.220	0.221
	(0.032)	(0.030)	(0.031)	(0.031)	(0.030)
Immigrants	0.254	0.257	0.258	0.257	0.257
	(0.047)	(0.044)	(0.043)	(0.044)	(0.044)
Correlation	0.213	0.167	0.190	0.181	0.161
	(0.2109)	(0.1935)	(0.1957)	(0.1972)	(0.1919)
	[-0.193, 0.641]	[-0.194, 0.565]	[-0.177, 0.589]	[-0.189, 0.585]	[-0.201, 0.551]
R^2 Natives	0.636	0.720	0.679	0.672	0.710
R^2 Immigrants	0.070	0.206	0.212	0.185	0.192
# of cities	98	98	98	98	98

Note: This table reports the estimated parameters and standard errors of the joint distribution of native-born and immigrant location effects, assuming they are drawn from a normal distribution. Panel (i) reports the mean native (N) and immigrant (I) location effects, a linear function of city-level covariates. Panel (ii) reports the standard deviation (σ) of natives and immigrants and the correlation (ρ) of the random effect, and panel (iii) reports the implied total standard deviation, defined as $\sqrt{\beta_g^2 \mathbb{V}(z) + \sigma_g^2}$ for every group $g \in \{\mathcal{I}, \mathcal{N}\}$, and the implied correlation, which is the ratio between $\mathbb{C}ov(z_j' \beta_{\mathcal{I}}, z_j' \beta_{\mathcal{I}}) + \rho \sigma_I \sigma_N$ and the product of the implied standard deviation of immigrants and natives. R^2 is defined as the group specific ratio between variance share $\frac{\beta_g^2 \mathbb{V}(z)}{\beta_g^2 \mathbb{V}(z) + \sigma_g^2}$. Panel (i) was estimated by a city-size weighted least regression. In panel (i), robust standard errors are reported in parenthesis, and in panels (ii)-(iii), parentheses report the parametric bootstrapped standard errors, and square brackets report parametric bootstrapped equal-tailed confidence intervals.

I Full List of Location Effects

Table I.1: Forcast of location effects for low-income families (p=25)

Loc. Code	Name	Posterior mean immigrants (1)	Posterior mean natives (2)	Share immigrants (3)
246	Netivot	0.431	-0.014	0.223
31	Ofaqim	0.417	0.007	0.299
7600	Akko	0.371	-0.044	0.277
4100	Qazrin	0.304	0.080	0.336
2660	Yavne	0.250	0.162	0.123
2630	Qiryat Gat	0.226	0.340	0.327
1034	Qiryat Mal'akhi	0.224	-0.066	0.220
1063	Ma'alot-tarshiha	0.220	0.224	0.479
73*	Mateh Binyamin	0.220	0.174	0.115
2560	Arad	0.219	0.165	0.419
2100	Tirat Karmel	0.207	-0.052	0.197
9100	Nahariyya	0.206	0.084	0.212
8500	Ramla	0.187	0.159	0.269
7700	Afula	0.181	0.100	0.302
6300	Giv'atayim	0.178	0.116	0.065
1139	Karmi'el	0.169	0.307	0.393
3640	Qarne Shomeron	0.169	0.203	0.155
7100	Ashqelon	0.166	0.167	0.351
5000	Tel-Aviv	0.163	0.077	0.127
1015	Mevasseret Ziyyon	0.151	0.185	0.111
8300	Rishon Leziyyon	0.147	0.310	0.187
2400	Or Yehuda	0.140	0.064	0.155
72*	Shomron	0.137	0.191	0.080
6200	Bat Yam	0.131	0.375	0.317
8700	Ra'annana	0.131	0.236	0.136
874	Migdal Haemeq	0.130	-0.079	0.289
1137	Qiryat Ye'arim	0.128	0.043	0.192
812	Shelomi	0.128	-0.422	0.202
99	Mizpe Ramon	0.118	0.085	0.245
3780	Betar Illit	0.113	0.189	0.087
9000	Be'er Sheva	0.106	0.180	0.306
76*	Gush Etzion	0.098	0.253	0.144
6600	Holon	0.097	0.304	0.177
1020	Or Aqiva	0.096	-0.011	0.437
240	Yoqne'am Illit	0.093	0.230	0.264
6400	Herzliyya	0.087	0.110	0.103
3650	Efrata	0.084	0.285	0.139
2800	Qiryat Shemona	0.079	0.024	0.183
7000	Lod	0.073	0.154	0.350
9600	Qiryat Yam	0.070	0.181	0.358
1066	Bene Ayish	0.066	0.285	0.581
6700	Tiberias	0.065	0.107	0.181
9400	Yehud	0.062	0.172	0.070
2500	Nesher	0.061	0.198	0.316
70	Ashdod	0.058	0.312	0.366
2530	Be'er Ya'aqov	0.058	0.148	0.147
9500	Qiryat Bialik	0.048	0.203	0.232
38*	Eshkol	0.048	0.112	0.075
4000	Haifa	0.046	0.304	0.272
1031	Sederot	0.040	0.110	0.392
8200	Qiryat Motzkin	0.035	0.344	0.220
8400	Rehovot	0.022	0.174	0.203
168	Kefar Yona	0.020	0.128	0.086
3616	Ma'ale Adummim	0.014	0.303	0.149
1061	Nazerat Illit	0.007	0.122	0.521
1*	Upper Galilee	0.007	-0.141	0.134
26*	Mateh Yehuda	0.007	0.187	0.042
2034	Hazor Hagelilit	0.005	0.055	0.116
6100	Bene Beraq	-0.004	0.009	0.065
6900	Kefar Sava	-0.009	0.214	0.141
3570	Ari'el	-0.020	0.234	0.431
565	Azor	-0.040	0.161	0.105
831	Yeroham	-0.045	0.070	0.229
8600	Ramat Gan	-0.045	0.275	0.102
1224	Kokhav Ya'ir	-0.058	0.422	0.186
7900	Petah Tiqwa	-0.060	0.251	0.212

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Table I.1: Forcast of location effects for low-income families (p=25) (cont.)

Loc. Code	Name	Posterior mean immigrants (1)	Posterior mean natives (2)	Share immigrants (3)
7400	Netanya	-0.061	0.236	0.274
7200	Nes Ziyyona	-0.065	0.129	0.083
3611	Qiryat Arba	-0.069	-0.213	0.186
2640	Rosh Haayin	-0.071	0.250	0.069
9700	Hod Hasharon	-0.084	0.304	0.071
681	Giv'at Shemu'el	-0.086	0.140	0.081
2620	Qiryat Ono	-0.095	0.163	0.071
6800	Qiryat Atta	-0.095	0.271	0.223
469	Qiryat Eqron	-0.096	0.212	0.124
8000	Zefat	-0.109	0.055	0.195
9300	Zikhron Ya'aqov	-0.110	0.202	0.077
2650	Ramat Hasharon	-0.122	0.219	0.038
2610	Bet Shemesh	-0.124	0.231	0.247
16*	Hefer Valley	-0.126	0.181	0.048
30*	Gezer	-0.126	0.219	0.030
6*	Emek HaYarden	-0.136	-0.061	0.099
71*	Golan	-0.136	0.201	0.059
2600	Elat	-0.136	0.336	0.180
9*	Jezreel Valley	-0.140	0.294	0.062
166	Gan Yavne	-0.144	0.121	0.108
7800	Pardes Hanna-karkur	-0.146	0.141	0.195
15*	Hof HaCarmel	-0.147	-0.031	0.119
56*	Misgav	-0.158	0.280	0.044
6500	Hadera	-0.163	0.376	0.303
4*	Mateh Asher	-0.165	0.174	0.000
2200	Dimona	-0.181	0.174	0.231
9200	Bet She'an	-0.182	-0.013	0.074
8*	Gilboa	-0.223	0.303	0.071
33*	Be'er Tuvia	-0.251	0.106	0.014
7*	Emek HaMaayanot	-0.251	0.176	0.052
2550	Gedera	-0.299	0.158	0.116
42*	Merhavim	-0.376	-0.268	0.000

Note: This table presents the posterior mean location effects on childrenwith parents' income from the 25th percentile. Columns (1) and (2) present the predicted location effects on immigrant and native-born children, accordingly. The table is sorted according to column (1). We list each location with its name and location code, where an asterisk marks regional council codes as detailed in Section F. Colum 3 presents the share of immigrants in the city in the year 2003.

Table I.2: Forcast of location effects for high-income families (p=75)

Loc. Code	Name	Posterior mean immigrants (1)	Posterior mean natives (2)	Share immigrants (3)
7600	Akko	0.542	-0.076	0.277
2400	Or Yehuda	0.466	-0.031	0.155
31	Ofaqim	0.413	0.001	0.299
246	Netivot	0.374	-0.084	0.223
72*	Shomron	0.362	0.007	0.080
4100	Qazrin	0.342	-0.148	0.336
6200	Bat Yam	0.338	0.267	0.317
3640	Qarne Shomeron	0.316	-0.045	0.155
1034	Qiryat Mal'akhi	0.287	-0.155	0.220
8500	Ramla	0.286	0.050	0.269
2660	Yavne	0.281	-0.015	0.123
7700	Afula	0.275	-0.014	0.302
9600	Qiryat Yam	0.271	0.049	0.358
2630	Qiryat Gat	0.267	0.168	0.327
812	Shelomi	0.226	-0.509	0.202
2100	Tirat Karmel	0.222	-0.223	0.197
5000	Tel-Aviv	0.212	-0.028	0.127
70	Ashdod	0.207	0.136	0.366
1066	Bene Ayish	0.203	-0.022	0.581
1015	Mevasseret Ziyyon	0.199	-0.016	0.111
73*	Mateh Binyamin	0.196	0.086	0.115
8300	Rishon Leziyyon	0.195	0.150	0.187

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Table I.2: Forcast of location effects for high-income families (p=75) (cont.)

Loc. Code	Name	Posterior mean immigrants (1)	Posterior mean natives (2)	Share immigrants (3)
1020	Or Aqiva	0.194	-0.177	0.437
3650	Efrata	0.194	-0.037	0.139
6300	Giv'atayim	0.179	-0.006	0.065
1031	Sederot	0.178	-0.113	0.392
1137	Qiryat Ye'arim	0.175	-0.216	0.192
8700	Ra'annana	0.175	0.074	0.136
2600	Elat	0.173	0.129	0.180
2560	Arad	0.163	0.028	0.419
9400	Yehud	0.161	-0.050	0.070
1063	Ma'alot-tarshiha	0.155	-0.006	0.479
6600	Holon	0.145	0.179	0.177
2200	Dimona	0.138	-0.052	0.231
6700	Tiberias	0.132	-0.077	0.181
99	Mizpe Ramon	0.130	-0.097	0.245
9000	Be'er Sheva	0.094	0.022	0.306
2800	Qiryat Shemona	0.084	-0.205	0.183
76*	Gush Etzion	0.063	0.002	0.144
7000	Lod	0.059	0.030	0.350
7100	Ashqelon	0.058	0.034	0.351
240	Yoqne'am Illit	0.054	-0.004	0.264
6100	Bene Beraq	0.050	-0.021	0.065
6400	Herzliyya	0.050	0.009	0.103
7400	Netanya	0.029	0.076	0.274
9100	Nahariyya	0.024	-0.085	0.212
2640	Rosh Haayin	0.021	-0.010	0.069
38*	Eshkol	0.021	-0.270	0.075
2034	Hazor Hagelilit	0.007	-0.237	0.116
7900	Petah Tiqwa	0.006	0.087	0.212
2530	Be'er Ya'aqov	0.004	-0.109	0.147
1224	Kokhav Ya'ir	0.000	0.089	0.186
26*	Mateh Yehuda	-0.006	-0.085	0.042
2500	Nesher	-0.011	-0.012	0.316
30*	Gezer	-0.014	-0.038	0.030
8000	Zefat	-0.014	-0.155	0.195
8200	Qiryat Motzkin	-0.019	0.128	0.220
166	Gan Yavne	-0.019	-0.062	0.108
9500	Qiryat Bialik	-0.023	0.044	0.232
874	Migdal Haemeq	-0.023	-0.218	0.289
1139	Karmi'el	-0.027	0.083	0.393
168	Kefar Yona	-0.028	-0.156	0.086
565	Azor	-0.029	-0.071	0.105
1*	Upper Galilee	-0.030	-0.341	0.134
3616	Ma'ale Adummim	-0.035	0.088	0.149
7200	Nes Ziyyona	-0.039	-0.099	0.083
2620	Qiryat Ono	-0.042	-0.092	0.071
2650	Ramat Hasharon	-0.047	-0.046	0.038
3780	Betar Illit	-0.051	-0.038	0.087
1061	Nazerat Illit	-0.061	-0.116	0.521
3611	Qiryat Arba	-0.064	-0.382	0.186
831	Yeroham	-0.067	-0.182	0.229
3570	Ari'el	-0.073	0.022	0.431
2610	Bet Shemesh	-0.098	0.166	0.247
71*	Golan	-0.100	-0.187	0.059
9300	Zikhron Ya'aqov	-0.100	-0.046	0.077
6800	Qiryat Atta	-0.103	0.034	0.223
6900	Kefar Sava	-0.111	0.048	0.141
6500	Hadera	-0.116	0.054	0.303
8600	Ramat Gan	-0.118	0.050	0.102
9700	Hod Hasharon	-0.126	0.035	0.071
9200	Bet She'an	-0.139	-0.355	0.074
7800	Pardes Hanna-karkur	-0.163	-0.046	0.195
4*	Mateh Asher	-0.164	-0.224	0.000
16*	Hefer Valley	-0.166	-0.052	0.048
8*	Gilboa	-0.168	-0.139	0.071
681	Giv'at Shemu'el	-0.184	-0.100	0.081
8400	Rehovot	-0.192	0.007	0.203
9*	Jezreel Valley	-0.196	-0.034	0.062
469	Qiryat Eqron	-0.200	-0.081	0.124
6*	Emek HaYarden	-0.220	-0.333	0.099
56*	Misgav	-0.225	-0.096	0.044
4000	Haifa	-0.253	0.086	0.272
15*	Hof HaCarmel	-0.258	-0.215	0.119

Continued on next page

Table I.2: Forcast of location effects for high-income families (p=75) (cont.)

Loc. Code	Name	Posterior mean immigrants (1)	Posterior mean natives (2)	Share immigrants (3)
33*	Be'er Tuvia	-0.351	-0.196	0.014
7*	Emek HaMaayanot	-0.365	-0.224	0.052
2550	Gedera	-0.410	-0.161	0.116
42*	Merhavim	-0.479	-0.542	0.000

Note: This table presents the posterior mean location effects on childrenwith parents' income from the 75th percentile. Columns (1) and (2) present the predicted location effects on immigrant and native-born children, accordingly. The table is sorted according to column (1). We list each location with its name and location code, where an asterisk marks regional council codes as detailed in Section F. Colum 3 presents the share of immigrants in the city in the year 2003.

J Estimation of Minimax Decision Rules

We study several neighborhood recommendation policies in Section 10. In this appendix section, we provide further detail on how we estimate the policies of the minimax decision maker. We start by describing the benchmark models from Section 10.3 that put minimal restriction on location choices, and then turn to an extension that restricts to location choices that align partially with status quo sorting patterns.

J.1 Decision Rules Under Ambiguity

To account for the uncertainty of the decision maker regarding behavioral responses, we consider a minimax decision rule that seeks to be robust against the least favorable behavioral responses. First, the minimax decision rule in a model with uncertainty only regarding who shows up to each selected city is given by:

$$\delta_{jK}^{(\mathcal{N},\mathcal{I})} = \mathbb{1}\{\mathbb{E}[\max\{\vartheta_{j\mathcal{N}K}, \vartheta_{j\mathcal{I}K}\}|\mathcal{Y}] \leq \kappa_K\}, \tag{21}$$

which ranks locations based on their expected within-city posterior maximum regret, and where κ_K is the maximum value such that there are exactly K cities with $\mathbb{E}[\max\{\vartheta_{j\mathcal{I}K}, \vartheta_{j\mathcal{N}K}\}|\mathcal{Y}] \leq \kappa_K$. We refer this decision rule as *minimax over* $(\mathcal{N},\mathcal{I})$.

We consider a second minimax decision rule, which results from a model in which the decision maker faces uncertainty regarding who shows up and where they go. This decision rule has the following form:

$$\delta_K^{(\mathcal{N},\mathcal{I},\text{city})} = \arg\min_{\delta} \mathbb{E}[\max(\{\vartheta_{j\mathcal{N}K}, \vartheta_{j\mathcal{I}K}\}_{j\in S(\delta)})|\mathcal{Y}], \tag{22}$$

where $S(\delta) = \{j : \delta_j = 1\}$ is the set of recommended cities. Under this decision rule, the decisionmaker evaluates the posterior expectation of the maximum regret across all the selected locations and across immigrants and natives and chooses the list that attains the lowest worst-case regret. Therefore, hereafter, we refer to this policy as *minimax over* $(\mathcal{N},\mathcal{I},$*city*$)$.

Estimation: Estimation of the policies in Equations (21) and (22) requires computing the posterior expectation of the maximum. We compute each expectation using numerical integration. We reduce the complexity of this computation in the following way. For simplicity, we present here the posterior expectation of the maximum across immigrants and natives. The derivation below can be generalized easily to the posterior expectation in Equation 22. let $W = \max\{\vartheta_{j\mathcal{N}}, \vartheta_{j\mathcal{I}}\}$. Then the CDF of W given $\mathcal{Y} = y$:

$$\begin{aligned} F_W(t|\mathcal{Y} = y) &= Pr(W \leq t|\mathcal{Y} = y) = Pr(\max\{\vartheta_{j\mathcal{N}}, \vartheta_{j\mathcal{I}}\} \leq t|\mathcal{Y} = y) \\ &= \int_t^{\infty}\int_t^{\infty} dG(\vartheta_{j\mathcal{N}}, \vartheta_{j\mathcal{I}}|\mathcal{Y} = y), \end{aligned}$$

Where $G(.|\mathcal{Y})$ is the posterior CDF of location effects, which, with a normal prior, follows a normal distribution. Therefore, the posterior expectation can be written as:

$$E[W|\mathcal{Y} = y] = -\int_{-\infty}^{0} F_W(t|\mathcal{Y} = y)dt + \int_0^{\infty}(1 - F_W(t|\mathcal{Y} = y))dt.$$

where we compute the posterior maximum by plugging in $\hat{G}(.|\mathcal{Y})$, and commuting numerically $\hat{F_W}(t|\mathcal{Y} = y)$ and $E[W|\mathcal{Y} = y]$ over a one-dimensional grid.

K Restricted Choice Model

Let $\pi_{jg0} \in (0, 1)$ be the status quo share of group $g \in \{\mathcal{N}, \mathcal{I}\}$ individuals who live in city j in the absence of any policy such that $\sum_j(\pi_{j\mathcal{N}0} + \pi_{j\mathcal{I}0}) = 1$. We impose the following restrictions on the location choice probabilities of natives, $\pi_{\mathcal{N}}(\delta) = (\pi_{j\mathcal{N}}(\delta), ..., \pi_{J\mathcal{N}}(\delta))'$, and immigrants, $\pi_{\mathcal{I}}(\delta) = (\pi_{j\mathcal{I}}(\delta), ..., \pi_{J\mathcal{I}}(\delta))'$. First, we rule out sorting probabilities that deviate substantially from the status-quo sorting patterns by considering choice probabilities $\pi(\delta) = (\pi_{\mathcal{N}}(\delta)', \pi_{\mathcal{I}}(\delta)')'$ whose distance from the status quo sorting $\pi_0 = (\pi'_{0\mathcal{N}}, \pi'_{0\mathcal{I}})'$ is bounded. To measure the distance between $\pi(\delta)$ and π_0, we use the Total Variation distance function, which gives the largest absolute difference between the probability distributions across all cities:

$$TV_{\pi_0}(\pi(\delta)) = \sup_{(j,g)\in\{1,..,J\}\times\{\mathcal{N},\mathcal{I}\}} |\pi_{jg}(\delta) - \pi_{0jg}|.$$

With this metric, $TV_{\pi_0}(\pi) = 0$ implies that all the families comply according to the status quo distribution, while as $TV(\pi) \to 1$, all the families belong to a single immigration group and sort into a single place. Our aim is to examine the optimal recommendation policy under a hypothetical bound on the tendency of families to deviate from the status-quo shares:

$$TV(\pi) \leq a,$$

where $a \in [0,1]$ represents the degree of unexpected sorting behavior. This restriction, together with the logical bound of $\pi_{jg}(\delta) \in [0,1]$ and the restriction of $\pi_{jg}(\delta) = 0$ if $\delta_j = 0$ we describe in Section 10.3, implies that location choices are set identified and satisfy for every city j with $\delta_j = 1$ and for every $g \in \{\mathcal{N}, \mathcal{I}\}$:

$$\pi_{jg}(\delta) \in [\max\{\tilde{\pi}^{\delta}_{jg0} - a, 0\}, \min\{\tilde{\pi}^{\delta}_{jg0} + a, 1\}], \quad \text{with} \quad \tilde{\pi}^{\delta}_{jg0} = \frac{\pi_{jg0}}{\sum_j (\pi_{j\mathcal{N}0} + \pi_{j\mathcal{I}0})\delta_j}, \tag{23}$$

where $\tilde{\pi}^{\delta}_{jg0}$ is the status quo shares normalized to sum to one across selected cities. These restrictions ensure that location choices approximately follow the status quo distribution while maintaining ambiguity regarding compliance ("Where do they go?") and families' group affiliation ("Who shows up?") governed by the parameter $a > 0$.

With uncertainty regarding location choices and families' identity, the minimax decision-maker would like to choose δ that is robust to the least favorable behavioral responses. For any given δ, the maximum regret can be written as:

$$\begin{aligned} \mathcal{L}^{max}_{R}(\vartheta, \delta) = &\max_{\pi(\delta)} \mathcal{L}(\vartheta, \delta, \pi(\delta)) \\ &\text{s.t} \quad \text{Equations (23)}, \\ &\sum_j (\pi_{j\mathcal{N}}(\delta) + \pi_{j\mathcal{I}}(\delta)) = 1, \end{aligned} \tag{24}$$

where $\mathcal{L}(\vartheta, \delta, \pi(\delta))$ is defined in Equation (11) and the decision-maker, therefore, chooses the δ that minimizes

$$\mathcal{R}^{\mathcal{N},\mathcal{I},city}_{R}(\delta) = \mathbb{E}[\mathcal{L}^{max}_{R}(\vartheta, \delta)|\mathcal{Y}] \tag{25}$$

subject to $\sum_j \delta_j = K$. Similar to the policy in Equation (12), the decision rule in Equation (25) ranks lists of size K places based on the expected maximum regret under the least favorable compliance. Unlike the unrestricted model, here the decision-maker assumes that there is a distribution of families across all recommended cities, ruling out the possibility that all families sort to a single least-beneficial location. The smaller the value of a, the more location choices align with the status-quo sorting pattern. When $a = 0$, lists of places are ranked based on the posterior average status quo regret $\sum_j \delta_j \mathbb{E}[\tilde{\pi}^{\delta}_{j\mathcal{N}0}\vartheta_{j\mathcal{N}10} + \tilde{\pi}^{\delta}_{j\mathcal{I}0}\vartheta_{j\mathcal{I}10}]$. In contrast, when $a \to 1$, the decision-maker faces more uncertainty regarding families' behavioral responses, and the optimal decision approaches the one reported in Equation (12).

In the following Section K, we show that these location choices can be microfunded by a discrete choice model with additively separable components of compliance and private evaluation, and where their private valuations are not restricted to follow a particular distribution or correlation structure across cities. This model implies a rational behavior accompanied by flexible heterogeneity in compliance responses.

Estimation: Motivated by Christensen et al. (2022) and as detailed in Appendix Section J, we estimate the decision rule implied by Equation (25) using a bootstrap implementation. Given a value of $a \in [0, 1]$, we estimate the bootstrap average maximum risk:

$$\mathcal{R}_R^{*\mathcal{N},\mathcal{I},city}(\delta) = \mathbb{E}^*[\mathcal{L}^{max}(\vartheta, \delta)|\mathcal{Y}], \tag{26}$$

where the expectation operator $\mathbb{E}^*$ denotes the expectation with respect to S bootstrap draws from the posterior distribution of θ given $\mathcal{Y}$, and in each bootstrap simulation we solve Equation (24) by linear programing. The minimax bootstrap decision rule is then the policy that attains the minimal expected maximum risk:

$$\delta_{K,R}^{*\mathcal{N},\mathcal{I},city} = \arg\min_{\delta} \mathcal{R}_R^{*\mathcal{N},\mathcal{I},city}(\delta).$$

Resutls: Table K.1 reports the top 10 selected cities and the posterior mean regret for both immigrants and natives for values of $a = 0.005$ and $a = 0.9$. When location choices are restricted to align closely with the status quo distribution ($a = 0.005$), the selected cities provide significant benefits (low regret levels) for native-born families, since they constitute the majority in each city according to the status quo. In contrast, for $a = 0.9$, with fewer restrictions on location choices, the selected cities offer more equal outcomes for both groups. The average regret for each city does not exceed 483 lost shekels per year compared with the oracle first-best policy.

K.1 Microfoundation

This section mircofounds the model of location choices presented above. Let $D_i \in \{1, ..., J\}$ be the random variable indicating the location choice of individual i to one of the J Israeli cities. Restricting attention to preferences that are consistent with the following choice model

$$D_i = \arg\max_{j\in\{1,...,J\}} U_{ij} - (a_{ij} + b_{ij}\delta_j), \qquad \text{for every } g \in \{\mathcal{N}, \mathcal{I}\}$$

where $U_i = (U_{i1}, ..., U_{iJ})$ is individual i's private evaluation, whose pdf is given by $f(u|g, \delta)$, conditional on immigration group $g \in \{\mathcal{I}, \mathcal{N}\}$ and policy δ. We do not require that U_i follows a specific distribution and allow U_{ij} and U_{ik} to be dependent for $j \neq k$.

The share of families of group $g \in \{\mathcal{I}, \mathcal{N}\}$ who choose to move to location j after

Table K.1: Top 10 Israeli cities selected based on the restricted minimax criterion

$\alpha = 0.001$			$\alpha = 0.9$		
Loc. name	Post. mean imm. (1)	Post. mean natives (2)	Loc. name	Post. mean imm. (3)	Post. mean native (4)
Kokhav Ya'ir	826.1	37.5	Qiryat Gat	391.7	162.1
Qiryat Gat	391.7	162.1	Ma'alot-tarshiha	399.6	339.5
Bat Yam	536.4	108.8	Karmi'el	477.8	212.5
Hod Hasharon	865.8	217.2	Rishon Leziyyon	512.3	208.6
Rishon Leziyyon	512.3	208.6	Yavne	354.2	435.0
Qiryat Motzkin	683.9	156.7	Mateh Binyamin	400.1	416.6
Jezreel Valley	951.2	233.4	Bat Yam	536.4	108.8
Gilboa	1078.4	218.6	Arad	401.2	430.0
Holon	587.9	217.1	Ramla	451.4	439.9
Misgav	978.0	254.3	Ashqelon	483.0	426.5

Note: This table reports the list of 10 selected cities by the restricted minimax ($\mathcal{N}/\mathcal{I}/city$) decision-maker depicted in Section 10. Columns 1-2 report the posterior mean regret of the selected 10 cities when $a = 0.01$, which reflects a prior with little deviation from the status-quo sorting patterns, and columns 3-4 report the posterior mean regret of the selected 10 cities when $a = 0.9$, which reflects very few restrictions on sorting patterns. Regret is defined as the difference between the one-year location effects and the average benefits from the top cities for each group, immigrants and natives. It represents the lost earnings at age 28 from spending one year in city j, compared to the average city selected under the first-best policy that allows for a personalized recommendation.

observing recommendation δ can be written as:

$$\pi_{jg}(\delta) = \int \mathbb{1}\{U_j - (a_{gj} + b_{ij}\delta_j) \geq U_k - (a_{gk} + b_{ik}\delta_k) \text{ for all k}\} f(u|g,\delta)du,$$

where we set a_{gj} such that the location choice probabilities of individuals who face no recommendation (i.e., $\delta_j = 0$ for all j, represented at $\delta = 0$) equal to the status-quo sorting probabilities:

$$\tilde{\pi}^{\delta}_{jg0} \equiv \pi_{jg}(0) = \int \mathbb{1}\{U_j - a_{gj} \geq U_k - a_{gk} \text{ for all k}\} f(u|g,0)du.$$

To see how this choice model is equivalent to the one in Section 10 let $\bar{b}_{gj}$ and $\underline{b}_{gj}$ be the lower- and upper-bounds of b_{ij} that satisfy the following conditions. If $\tilde{\pi}^{\delta}_{jg0} - a > 0$, $\underline{b}_{gj}$ satisfies

$$a = \tilde{\pi}^{\delta}_{jg0} - \int \mathbb{1}\{U_j - (a_{gj} + \underline{b}_{gj}\delta_j) \geq U_k - (a_{gk} + \underline{b}_{gl}\delta_k) \text{ for all k}\} f(u|g,\delta)du,$$

while if $\tilde{\pi}^{\delta}_{jg0} - a \leq 0$, $\underline{b}_{gj} \to -\infty$. Accordingly, if $\tilde{\pi}^{\delta}_{jg0} + a < 1$, $\bar{b}_{gj}$ is the value that satisfy

$$a = \int \mathbb{1}\{U_j - (a_{gj} + \bar{b}_{gj}\delta_j) \geq U_k - (a_{gk} + \bar{b}_{gl}\delta_k) \text{ for all k}\} f(u|g,\delta)du - \tilde{\pi}^{\delta}_{jg0},$$

and $\bar{b}_{gj} \to \infty$, otherwise.