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RANDOMIZED TO OPPORTUNITY: MEASURING PLACE EFFECTS FOR REFUGEE CHILDREN

N. Meltem Daysal
Joseph J. Doyle Jr.
Naeim S. Samandari
Emil A. L. Simonsen
Mircea Trandafir

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N. Meltem Daysal, Joseph J. Doyle Jr., Naeim S. Samandari, Emil A. L. Simonsen, and Mircea Trandafir
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ABSTRACT

Area-level measures of economic opportunity are increasingly used to guide place-based policies. We study whether these measures predict long-run outcomes for a vulnerable population: over 12,000 refugee children quasi-randomly assigned to municipalities under Denmark's 1986–1998 dispersal policy. We focus on predetermined measures of opportunity available when children arrive and validate them using a mover-exposure design among permanent residents. Moving from a low- to a high-opportunity area (a two-standard-deviation increase) raises adult income rank by 3 percentile points (7.5 percent) and employment by 4 percentage points. It also reduces criminal convictions by 14 percent and teen parenthood by 19 percent, with more modest effects on educational attainment. Economic opportunity predicts crime and teen parenthood more strongly than other area characteristics, including local crime rates. For adult income, area-level inequality is a particularly strong predictor. These findings suggest that social mobility in the areas where refugee children grow up meaningfully shapes their long-term wellbeing.

N. Meltem Daysal
University of Copenhagen
Department of Business and Economics
and CEBI, CESifo, IZA and RFBerlin
meltem.daysal@econ.ku.dk

Joseph J. Doyle Jr.
Massachusetts Institute of Technology
MIT Sloan School of Management
and NBER
jjdoyle@mit.edu

Naeim S. Samandari
University of Copenhagen
Department of Economics,
and Center for Economic Behavior
and Inequality (CEBI)
and The ROCKWOOL Foundation
nsa@econ.ku.dk

Emil A. L. Simonsen
University of Copenhagen
Department of Economics
els@econ.ku.dk

Mircea Trandafir
The ROCKWOOL Foundation
and IZA
mt@rff.dk

1 Introduction

There is a growing literature examining the extent to which the places where children grow up shape later-life outcomes. In particular, measures of intergenerational income mobility vary substantially across cities, and higher mobility is associated with better long-run outcomes for children. Evidence from randomized housing mobility experiments, most notably the Moving to Opportunity experiment, and from mover-based research designs suggests that moving low-income families with young children to higher-opportunity areas raises educational attainment and adult earnings (Chetty et al., 2026a; Chetty and Hendren, 2018a,b; Chetty et al., 2016). These findings have spurred a broader policy agenda organized around creating moves to opportunity, with mobility programs and opportunity maps increasingly used to reduce spatial inequality (Chyn and Daruich, 2025; Chyn and Katz, 2021). More broadly, this literature motivates efforts to improve area conditions as a way to promote child development.

Meanwhile, the global refugee population has reached unprecedented levels, with 2023 marking a historic peak (UNHCR, 2024). Children are particularly affected: although they comprise roughly 30 percent of the world's population, they account for 41 percent of all refugees, and more than 1.9 million children were born into refugee status between 2018 and 2022 (UNHCR, 2024). Refugees are forced to flee their countries because of persecution, conflict, or violence. They differ from economic migrants, who typically relocate voluntarily and are admitted after meeting selective criteria such as minimum levels of education, language skills, and work experience. Once refugees gain admission to a host country, a central policy question is where they should be settled and, relatedly, how settlement location shapes their integration (Boustan et al., 2025; Foged et al., 2024a; Arendt et al., 2022).

This paper asks whether growing up in higher-opportunity areas improves long-run outcomes for refugee children, a population that is policy-relevant, growing rapidly, and potentially subject to different mechanisms than those highlighted in prior work. Answering this question is difficult for at least two reasons. First, data on refugees and their children are scarce: immigration status and admission class are typically missing from administrative datasets, and studying long-run integration requires following children far enough into adulthood to observe meaningful socioeconomic outcomes. Second, refugees often sort across locations within host countries, and those settlement choices may be correlated with observed and unobserved determinants of later success, making it difficult to isolate causal place effects.

We address these challenges by exploiting Denmark's 1986–1998 refugee dispersal policy, under which refugee families were assigned quasi-randomly across municipalities (Foged et al., 2024a; Damm and Dustmann, 2014). We combine this policy variation with population-wide administrative records covering 1986 to 2022 for more than 12,000 refugee children. This setting allows

us to examine a broad set of long-run outcomes. The longitudinal data also allow us to consider two complementary measures of local economic opportunity. Our primary measure, Realized Opportunity, is a predetermined municipality-level measure of intergenerational mobility, constructed from earlier cohorts of permanent residents and observable at the time refugee families arrive. Our secondary measure, Ongoing Opportunity, is a contemporaneous measure that captures the opportunities facing the cohorts growing up alongside refugee children, and it allows us to draw more direct comparisons with the existing literature.

We begin by documenting substantial variation in economic opportunity across municipalities among more than five million local residents in Denmark. We validate these opportunity measures using a mover-exposure design following Chetty and Hendren (2018a,b), comparing siblings exposed to different places for different lengths of time. The resulting estimates show that native-born children who move to higher-opportunity areas at younger ages have higher adult incomes than their older siblings. This within-family age gradient supports the interpretation of these measures as capturing meaningful differences in local childhood environments, under the assumption that move timing is unrelated to siblings' potential outcomes.

We then turn to the refugees who were quasi-randomly assigned across municipalities and ask whether such opportunity extends to them. We find that refugee children placed in municipalities with higher opportunity measures have better long-run outcomes: a two-standard-deviation increase in Realized Opportunity raises refugee children's income rank at ages 26 to 30 by about 3 percentile points, or 7.5 percent relative to the mean. The same increase in Realized Opportunity raises employment at age 26 by 4.0 percentage points (5.8 percent), while educational gains are more modest and concentrated in vocational attainment. Assignment to a higher-opportunity area also reduces a range of adverse outcomes: the total number of criminal convictions by age 30 falls by 14 percent and teen parenthood falls by 19 percent.

The registry data also allow us to explore potential mechanisms. We consider four related mechanisms: whether higher-opportunity areas improve refugee parents' economic integration, whether they facilitate better integration for children who arrive at younger ages, whether particular dimensions of opportunity are especially relevant for refugees, and whether Realized Opportunity mainly reflects underlying municipality characteristics such as labor market conditions or inequality. We find that assignment to higher-opportunity areas improves refugee parents' outcomes, and these gains can account for up to 20 percent of the estimated effect on children's adult income rank. Our results also suggest that refugee children benefit from higher-opportunity areas whether opportunity is measured using native Danes or immigrants, indicating that the relevant features of place are present in both the broader host society and immigrant communities. By contrast, gender-specific opportunity appears to matter, with boys' outcomes more closely related to boy-specific measures and girls' outcomes to girl-specific ones.

We find little evidence that the effects vary systematically by arrival age: refugees who arrive at age 17 benefit from assignment to a higher-opportunity area about as much as those who arrive as infants. The flat gradient is informative about mechanisms, as it suggests that the benefit of arriving in a higher-opportunity area runs through channels available at any age, such as local labor markets, rather than through early childhood investments like language acquisition or social integration.

Finally, we show that Realized Opportunity is a particularly strong predictor across a range of child wellbeing measures in horse-race regressions against the most commonly used area measures from earlier work. For adult income in particular, local *income inequality* emerges as especially important, consistent with the negative relationship between social mobility and inequality captured by the Great Gatsby curve (Britto et al., Forthcoming; Corak, 2013). Given the substantial attention paid to income inequality, it is notable that city-level income disparities have lasting consequences for adult income within a causal framework.

Our paper makes several contributions to the existing literature. First, we add to the growing evidence on the role of place in intergenerational mobility using a causal framework that exploits the random assignment of children across municipalities. The setting allows us to observe a wide range of outcomes, including income, education, crime, family formation, and fertility. A key innovation is our primary measure of area quality, Realized Opportunity, which captures the social mobility a neighborhood has already produced for children who grew up there rather than the mobility current children will experience. Realized Opportunity thus avoids reflection bias, in which existing residents and children moving into the neighborhood affect one another (Manski, 1993). It also provides actionable information when refugees arrive or when policies aim to encourage moves to opportunity.[1]

In addition, our paper adds to the literature on refugee integration, including work on dispersal policies, by focusing on refugee children. Prior work has concentrated mainly on adults, with calls for more research on child outcomes (Bahar et al., 2026). In particular, the Danish dispersal policy we study has previously been used to show that local crime rates predict criminal behavior among refugee youth (Dustmann et al., 2023; Damm and Dustmann, 2014), and that areas with a higher share of co-nationals have a negative impact on short-run educational outcomes and longer-run crime outcomes (Damm et al., 2025). Our paper shifts attention to the role of social mobility in shaping the long-run outcomes of refugee children. We find that economic opportunity is an independent and often stronger predictor of labor market and crime outcomes than local employment and crime rates themselves.

These results speak to ongoing policy debates about how refugee settlement policies should be

[1] Chetty et al. (2026c) note that a limitation of their estimates of the impacts of area social mobility is that they come with a lag, since children must grow up for their earnings to be observed. They further find that the optimal forecast of outcomes for children born this year places substantially more weight on mobility estimates for cohorts born 30 years ago than on poverty rates measured this year. Our Realized Opportunity measure fits both observations.

designed and how governments should target place-based interventions. They suggest that area-level measures of economic opportunity, especially when available at the time of assignment, may provide useful guidance for allocating vulnerable children across locations. More broadly, the findings indicate that where refugee children grow up has lasting consequences for their economic and social trajectories, and residential assignment policies and place-based improvements may function as long-run human capital investments.

The remainder of the paper is organized as follows. Section 2 reviews the related literature and provides background on the dispersal policy. Section 3 describes the data and Section 4 details the empirical strategy. Section 5 reports the results, and Section 6 offers conclusions.

2 Background

2.1 Literature

A growing body of literature documents large differences in income among people from similar backgrounds who grew up in different neighborhoods (Chetty et al., 2014). The central challenge for this literature has been to establish that these differences reflect causal place effects rather than selection into different neighborhoods (for an overview see, e.g., Katz, 2026; Chyn and Katz, 2021).

The first strand of papers exploited the Gautreaux program (1976–late 1990s), which placed roughly 7,000 low-income Black households from Chicago public housing into predominantly white, more affluent suburbs or majority-Black city neighborhoods. Initial studies reported higher employment for adult women and better educational outcomes for children placed in the suburbs (DeLuca et al., 2010; Mendenhall et al., 2006; Keels et al., 2005; Rosenbaum, 1995; Popkin et al., 1993), but the allocation to neighborhoods was not purely random. Using arguably exogenous variation in placement, Chyn et al. (2025) confirm the large effects on children's lifetime earnings, wealth, and marriage probability, but not the initial findings of adult labor-market gains. A "good" neighborhood in this literature is one that is more suburban and affluent.

A second strand of papers exploited the Moving to Opportunity (MTO) demonstration, which randomized 4,600 households across five U.S. cities in 1994–1998 into a low-poverty voucher, a traditional Section 8 voucher, or a control group.[2] For adults randomized to the low-poverty group, the experiment produced persistent mental and physical health gains but no effects on economic self-sufficiency, both in the short run and over a 10–15-year horizon (Ludwig et al., 2013; Kling et al., 2007).[3] For children randomized to the low-poverty group, the effects depend on age at move:

[2] The vast majority of MTO households were headed by individuals from African American or Hispanic minority groups (Chetty et al., 2016).

[3] Aliprantis and Richter (2020) argue that the lack of effects on adult labor outcomes masks substantial gains for the small subpopulation of compliers at the very low end of the distribution of neighborhood quality. Alternatively,

those moved before age 13 see large gains in adult earnings and college attendance, while older children show null or slightly negative effects, consistent with disruption costs (Chetty et al., 2016). The effects also vary by gender, with female youth benefiting and teenage boys experiencing adverse effects (Clampet-Lundquist et al., 2011; Kling et al., 2007). A "good" neighborhood here is one with a lower poverty rate.

The fact that neighborhoods seem to affect children but not adults motivated a subsequent line of research into the effects of growing up in "better" neighborhoods. This line was initiated by Chetty and Hendren (2018a,b), who use administrative data and a mover-exposure design that compares adult outcomes of similar children who moved into the same neighborhood at different ages. They estimate that each additional year of childhood exposure to a one-standard-deviation-better area raises adult earnings by roughly 0.5 percent, with the effect linear in exposure time. The recent Opportunity Atlas (Chetty et al., 2026c) extends this approach to the U.S. census-tract level and concludes that roughly 62 percent of the observed variation in children's outcomes across tracts reflects causal place effects. Others argue that there may be differential selection of areas by child age and point to family background playing a dominant role in child outcomes rather than area effects (Cholli et al., 2024b; Eshaghnia, 2023; Landersø and Heckman, 2022).[4] The mover design has been used to estimate place effects on education and/or income in Canada (Laliberté, 2021), Australia (Deutscher, 2020), Africa as a whole (Alesina et al., 2021), Brazil (Britto et al., Forthcoming), and South Africa (Carrillo et al., 2023). International applications of this design generally point in a similar direction, finding that childhood neighborhoods matter for later education and income. However, a Dutch replication finds effects on educational tracking at ages 14 and 19 but no effect on bachelor's degree attainment at age 28 in specifications that explicitly control for pre-move educational decisions at ages 14 and 19. Moreover, the apparent effect on adult income disappears when family fixed effects are included (Beekers, 2024). Meanwhile, Chetty et al. (2026b) show that changes in upward mobility in the United States are closely linked to changes in childhood environments within race- and class-specific social communities, with parental employment rates among peers playing a central predictive and causal role. A "good" neighborhood in this literature is one with higher upward mobility for the children of permanent residents, i.e., where the children of permanent residents grow up to have higher adult incomes, conditional on parental income. Recent work has also begun to examine why local opportunity changes over time.

A related strand of literature uses policy interventions and natural experiments that create plausibly exogenous variation in both moves and destination quality. One set of papers compares displaced children with non-displaced children after public-housing demolitions in Chicago or through the

Clampet-Lundquist and Massey (2008) document neighborhood effects on adult economic outcomes due to the cumulative time spent in a neighborhood.

[4] Eshaghnia (2023) runs a placebo test on Danish data and finds that pre-determined birth outcomes correlate with the age at move, pointing to differential selection by age.

nationwide HOPE VI program (Chetty et al., 2026a; Haltiwanger et al., 2024; Chyn, 2018). These papers find that moves to less disadvantaged neighborhoods lead to higher earnings, employment, and college attendance, and lower criminal activity for displaced children, with effects varying by age at move.[5] Similar to the MTO and Gautreaux studies, there are no significant effects on the economic outcomes of displaced adults. These results are also confirmed in the context of Chile, where children displaced by the 1979–1984 slum-clearance program to low-opportunity areas had lower educational attainment, earnings, and labor-market formality in adulthood than those displaced to high-opportunity areas (Rojas-Ampuero and Carrera, 2025). Displaced adults also experienced worse health outcomes (Rojas-Ampuero, 2026). Forced relocations driven by natural disasters reinforce these findings. The 1973 Heimaey volcanic eruption in Iceland led to higher long-run earnings and education for displaced children (Nakamura et al., 2022), while Hurricane Katrina led to income gains and lower mortality for displaced adults in the U.S. (Deryugina and Molitor, 2020; Deryugina et al., 2018). Kawano et al. (2024) use the quasi-random reassignment of U.S. army families to neighborhoods and find that more time in a better location during childhood improves education and adult earnings, with effects largest for older ages at exposure. Carrillo et al. (2023) study the forced relocation of Black households to "homeland" areas under apartheid in South Africa and find that changes in childhood environment account for approximately 70 percent of the migration-induced decline in schooling. Finally, both children and adults assigned to better neighborhoods through the quasi-random allocation of families to social housing in Copenhagen experienced better outcomes across education, labor markets, crime, and mental health (Billings et al., 2026, 2024; Boje-Kovacs et al., 2023). A "good" neighborhood in this literature is one with lower deprivation, measured by some combination of income, employment, education, and crime.[6]

The strand of literature most similar to our analysis uses the variation in neighborhood quality induced by the quasi-random dispersal of refugees within the destination country. Much of the prior work focuses on refugee adults and on identifying specific neighborhood characteristics that matter for outcomes, rather than on estimating "neighborhood effects." Earlier studies found that the size and skill composition of the co-national community in the assigned municipality raise subsequent earnings and employment, presumably through job-search networks (Battisti et al., 2022; Martén et al., 2019; Damm, 2014; Beaman, 2012; Damm and Rosholm, 2010; Damm, 2009; Edin et al., 2003).[7] Later studies document that better local labor market conditions, such as a higher employment rate in the assigned municipality, also raise the subsequent employment of refugees (Aksoy

[5] Jacob (2004) finds that the first move after the Chicago demolitions is to largely similar neighborhoods and schools, resulting in no short-run effects on educational outcomes.

[6] A separate set of papers identifies city- and county-level place effects from historical shocks, such as the Great Migration or the placement of railroads, rather than individual moves (Chyn et al., 2026; Baran et al., 2024; Derenoncourt, 2022).

[7] At the same time, Eckert et al. (2022) find that adult refugees settled in Copenhagen experience a big-city effect for their labor market outcomes that does not appear to operate through co-national networks.

et al., 2023; Müller et al., 2023; Azlor et al., 2020; Hamad et al., 2020; Godøy, 2017; Åslund et al., 2010; Åslund and Rooth, 2007). Finally, refugee adults allocated to more deprived neighborhoods in terms of income or social composition also end up with worse physical and mental health, including elevated cardiovascular risk (Hasager and Jørgensen, 2026; Kim et al., 2025, 2023; Schofield et al., 2024; Biddle and Bozorgmehr, 2024; and the studies cited in the review article by Biddle et al., 2023).

A second, smaller wave of papers focuses on the effects of growing up in neighborhoods with different conditions induced by dispersal policies on the short- and long-run outcomes of refugee children. Exposure to higher crime in the neighborhood raises the risk of later criminal convictions for boys and of teen motherhood for girls (Dustmann et al., 2023; Damm and Dustmann, 2014). The evidence on the presence of co-nationals among refugee children is more mixed: exposure to high-skilled co-national networks appears beneficial in Sweden (Åslund et al., 2011), while co-national exposure in Denmark worsens short-run educational outcomes and increases later crime (Damm et al., 2025). Relatedly, Hassan et al. (2023) study refugee peer effects in Danish schools and find that refugee children sort into lower-performing schools, but that exposure to refugee peers has no detectable effect on native students' test scores. Meanwhile, growing up in a more deprived neighborhood appears to worsen mental health in the short and long run (Schofield et al., 2024; Foverskov et al., 2022). A "good" neighborhood in the refugee-dispersal literature is generally defined by some combination of co-national network quality, less crime, and less overall deprivation.

As noted in the introduction, one of our contributions to this line of research is to examine whether economic opportunity provides a broader measure of place-based advantage for refugee children. In parallel work, Razavi et al. (2026) investigate a related area measure that combines social mobility with refugee parental income. Our preferred measure is predetermined and does not incorporate the improvement in refugee parental income that may result from settling in a higher-opportunity area. Instead, we study parental income as an outcome. Our results suggest that economic opportunity is particularly predictive across a wide set of domains: notably, it predicts later crime more strongly than the local crime rate itself and later income more strongly than mean earnings in the area. Relatedly, Aloni and Avivi (2025) estimate childhood location effects separately for natives and the children of immigrants in Israel and find the two are uncorrelated among low-income families, underscoring that the quality of a place can be group-specific.

2.2 Danish Dispersal Policy

Denmark was the first country to sign and ratify the 1951 United Nations Convention on the Status of Refugees, and, in 1956, the Danish Refugee Council (DRC) was created (Gammeltoft-Hansen and Madsen, 2021). The DRC's mandate was to support asylum seekers in their applications for

refugee recognition and residence permits in Denmark. Following a marked increase in refugee inflows in the early 1980s, the Danish government introduced a dispersal policy in 1986 for applicants whose asylum claims had been approved (i.e., recognized refugees). Implementation of this policy was delegated to the DRC.

The dispersal policy in place from 1986 to 1998 was implemented in two steps (Damm and Dustmann, 2014). Prior to 1986, refugees were concentrated mainly in Denmark's largest cities, and the aim of the new policy was to prevent further clustering there (Hasager and Jørgensen, 2026). The central goal was to allocate newly admitted refugees across the country's 275 municipalities, nested within counties, in proportion to each locality's population. These municipalities are best compared to incorporated towns or cities, or to small counties, in the United States.

Upon arrival and application for asylum, refugees were initially placed in Red Cross reception centers while their cases were adjudicated. These facilities, both historically and today, are generally located in sparsely populated areas. Until 2013, residents of these centers were not permitted to work, implying that most asylum seekers likely had limited contact with the wider Danish population before receiving official refugee status. By the late 1990s, the average processing time for an asylum decision exceeded one year (Foged et al., 2024b).

Once asylum was granted, refugees were transferred within 10 days to temporary accommodation in one of Denmark's 15 counties. The relevant local DRC office then assigned them to a municipality within that county and assisted them in obtaining permanent housing. Throughout the policy period, most refugees entered permanent housing within 18 months; only 0–4 percent did not do so within that timeframe (Damm and Dustmann, 2014).

The DRC made county and municipality placement decisions without meeting refugees face to face. After asylum approval, refugees completed a short questionnaire with basic information, including nationality and household size, which the DRC could use to inform assignments. Our study benefits from observing the variables that would have been available to the DRC, and conditional on those variables, the dispersal of families across municipalities is plausibly exogenous. We also observe parental education of refugees, which would not have been known to the DRC at the time of allocation, providing another check on the effective random assignment of refugees across areas.

Although refugees were not legally required to remain in the assigned municipality, they faced incentives to do so during the first 18 months after arrival. In particular, the 18-month introduction program—including Danish language courses, cultural orientation, and job training—was offered only in the municipality of assignment, even though access to means-tested social benefits and the ability to work were not contingent on staying (Damm, 2005). Moreover, the council evaluated requests for reassignment only after the initial placement had been made. We consider the mobility of refugees in more detail when mechanisms are explored below.

3 Data and Opportunity Measures

We use population-level administrative data from Denmark covering the period 1986–2022. The data contain individual-level records with unique personal identifiers, which allow us to link family members and follow individuals over time. We obtain refugee status from the Residence Basis Register, demographic characteristics and annual municipality of residence from the Population Register, labor market income from the Income Register, educational attainment from the Education Register, and criminal convictions from the Crime Registers.

3.1 Analysis Sample

We identify refugees using two approaches, depending on the sub-period. From 1986 to 1992, asylum status is not directly observed in the registers, so we follow Damm and Dustmann (2014) and impute refugee status. Specifically, we include children who arrived in Denmark within one year of their parents' arrival, whose parents originated from one of eight major refugee-sending countries (Lebanon, Iran, Iraq, Somalia, Sri Lanka, Vietnam, Afghanistan, or Ethiopia), and whose parents were not married either to a person from a non-refugee-sending country or to a person from a refugee-sending country who had arrived more than one year earlier.[8] For 1993–1998, our sample includes children of all refugees granted asylum in the Residence Basis Register, excluding individuals from the former Yugoslavia. Refugees from the former Yugoslavia were initially granted provisional asylum and were subject to non-random placement under the Bosnian program that began in 1993.[9]

We follow Damm (2009) and define the municipality of assignment as the municipality in which the refugee family resided in the year after receiving a residence permit, because some families initially spent several months in temporary housing at the county level before being placed in a permanent municipality.[10] Our sample includes 215 municipalities, and the average municipality has approximately 35,000 residents. The main analysis focuses on refugee children who were aged 0–17 at arrival and remained in Denmark for at least 15 years after arrival.[11] These restrictions leave us with a sample of 12,415 refugee children, and for some outcomes we have slightly fewer observations due to data availability.[12]

[8] Refugees from these eight countries accounted for more than 86 percent of all permanent residence permits issued in Denmark to refugees between 1985 and 1997 (Damm and Dustmann, 2014). Appendix Table C1 reports the countries of origin and their shares in our analysis sample. More than 70 percent of the children came from the top four countries: Lebanon, Iraq, Iran, and Somalia.

[9] Under the Bosnian program, refugees from the former Yugoslavia were accommodated in designated reception centers and purpose-built refugee villages rather than dispersed according to the standard proportional allocation across municipalities, making their placement non-random with respect to municipal characteristics.

[10] On average, families entered permanent housing six to seven months after county assignment, and between 96 and 100 percent did so within the 18-month introduction-program period (Damm and Dustmann, 2014).

[11] We show that inclusion in the analysis sample is not related to our measures of opportunity below.

[12] We observe child income rank for 10,474 children. There are three reasons for this smaller number of observations:

3.2 Measures of Economic Opportunity

Construction of Opportunity Indices. We construct two indices of economic opportunity. Our primary measure, Realized Opportunity, is predetermined with respect to refugee placement: it is estimated from cohorts that had reached adulthood before the refugee families in our sample arrived and captures what a municipality may have historically delivered to children from low-income families. Our supplemental measure, Ongoing Opportunity, is contemporaneous: it captures the prospects of the cohort growing up alongside the refugee children. However, it is not observed at the time of dispersal and could reflect spillover effects of refugees on the outcomes of native-born peers (Manski, 1993).

To construct Realized Opportunity, we follow Chetty and Hendren (2018a) and focus on permanent residents—families that remained in the same municipality throughout the period over which parental income is measured—and their children who turned 30 each year. For each child i, we compute (a) the child's percentile rank within the national labor-income distribution of their year-of-birth cohort, averaged over ages 26–30, and (b) the parent's (primary earner's) percentile rank within the national labor-income distribution, averaged over the years when the child was aged 24–28.[13] Each child's municipality is determined by the mother's place of residence over the parental-income averaging period. If maternal information is missing, we instead use the father's place of residence. We then estimate the following linear model for local child i living in municipality $m(i)$ and born in year-of-birth cohort $s(i)$ via OLS, suppressing the i subscripts on m and s for clarity:

$$y_i = \alpha_{ms} + \psi_{ms}\, p_i + \varepsilon_i, \tag{1}$$

where y_i is the child's income rank and p_i is the parent's income rank.

The index is allowed to vary at the municipality-by-birth-cohort level. Specifically, for each cell (m, s), we recover the fitted intercept $\hat{\alpha}_{ms}$ and slope $\hat{\psi}_{ms}$ and compute the predicted adult income rank for a child whose parents were at the 25th percentile of the national distribution:

$$\hat{y}_{25,ms} = \hat{\alpha}_{ms} + 25\,\hat{\psi}_{ms}. \tag{2}$$

By evaluating the rank-rank relationship at a common point in the parental income distribution, this index isolates variation in upward mobility stemming from the bottom of the parental income

(i) some children had not reached age 30 by the final year for which we have income data, 2022; (ii) some children emigrated before age 30 but more than 15 years after arriving in Denmark; and (iii) some children died before age 30. Similar reasons apply to other outcomes. We provide tests of selective sample attrition below.

[13] The age range for parental income was chosen because the Income Register begins in 1980, which implies that age 24 is the earliest age at which we can measure parental income for children observed at age 30 in 1986. This constraint, combined with the desire to maintain consistency across cohorts, motivated the chosen parental income window.

distribution (the intercept), and the intergenerational income correlation (the slope), rather than differences in parental resources across areas.

For the refugee sample, the index varies at the municipality-by-year-of-arrival level: for each arrival year, we construct it from the cohort of local permanent residents who turned 30 in that year, restricting attention to municipality-cohort cells with at least 10 observations. Panel (a) of Appendix Figure A1 provides a graphical illustration of how Realized Opportunity is estimated, using 1986 as an example year. In addition, we apply empirical Bayes shrinkage to adjust for sampling variation arising from estimation uncertainty, which would otherwise disproportionately affect opportunity estimates for smaller municipalities (Chetty et al., 2026c). Finally, we standardize Realized Opportunity to have mean zero and standard deviation one within the analysis sample, so that the estimated coefficients are interpretable as the effect of a one-standard-deviation increase in local opportunity.

We construct Ongoing Opportunity to facilitate comparison with the literature using an analogous methodology, with two differences. First, we use the cohort of local permanent residents born in the same year as the incoming refugee children. Second, we measure parental income when the child is between the ages of 15 and 19 because parental income data are available for these earlier years rather than ages 24–28, which was the age range used to measure Realized Opportunity. Panel (b) of Appendix Figure A1 illustrates the estimation of Ongoing Opportunity for the 1975 birth cohort. This index varies in the estimation sample at the municipality-by-birth-cohort level rather than the municipality-by-year-of-arrival level, and it draws on future income that is unobserved at the time of refugee placement.[14]

Descriptive Statistics. To characterize how the economic opportunity measures vary across Denmark, we begin by presenting the rank-rank relationship linking parents' and children's incomes among permanent residents. Figure 1 shows a binned scatterplot of this relationship, aggregated across Denmark for the cohort that was age 30 in 1992. The figure plots mean child rank within bins of the parental income distribution, together with a 95 percent confidence band (shaded) and the best-fitting linear trend (green line). The intercept of about 46 implies that children born at the bottom of the distribution can expect to rise nearly to the median. The estimated national slope in Figure 1 is 0.134, implying that a one-percentile increase in parental rank is associated with a 0.134-percentile increase in child rank. The tight confidence intervals and near-linearity of the relationship suggest that the single-parameter summary, $\hat{y}_{25,ms}$, captures the intergenerational association well across most of the distribution.[15] Appendix Figure A3 presents an analogous binned scatterplot,

[14] Since we measure parental income at ages 15–19 for Ongoing Opportunity rather than at ages 24–28 as for Realized Opportunity, the Ongoing Opportunity results also serve as a robustness check on the parental income averaging window.

[15] The bottom vigintile falls below the line of best fit, and when we restrict the analysis to parents at or below the 40th percentile, the estimated slope increases to 0.256 (Appendix Figure A2).

in which parental income is measured at ages 15–19, corresponding to the methodology used for Ongoing Opportunity. We show this figure for the 1991 birth cohort, the earliest cohort for which the necessary data are available under this definition. The estimates are similar, with an intercept of 45 and a national slope of 0.164.

Appendix Table A1 places our Danish estimates in context with estimates from other countries. Denmark has higher social mobility than the other countries in the table. The estimated intercept is higher, as other countries in Europe tend toward an intercept of 40, while the estimated intercept in the U.S. is closer to 34 (Britto et al., Forthcoming; Beekers, 2024; Deutscher and Mazumder, 2023; Acciari et al., 2022; Corak, 2020; Bratberg et al., 2017; Chetty et al., 2014). The slope is also lower in Denmark than in other countries, which tend toward 0.2, with the U.S. closer to 0.33. Meanwhile, Brazil stands out for its lower social mobility measures, with an intercept of 23 and a slope of 0.5. Nevertheless, parental income remains a strong predictor of child income even in Denmark.

We next turn from national-average measures of intergenerational mobility to spatial variation in Realized Opportunity. Appendix Figure A4 presents two maps of Denmark illustrating variation in Realized Opportunity among permanent residents, averaged across municipalities over 1986–1998. Panel (a) shows that Realized Opportunity ranges from 44.8 to 57.4 across municipalities, a spread similar to that in the Netherlands and smaller than in the United States (Beekers, 2024; Chetty and Hendren, 2018a). Panel (b) reports the share of refugee children dispersed to each municipality between 1986 and 1998. Larger shares were assigned to Denmark's five largest cities (Copenhagen, Aarhus, Odense, Aalborg, and Esbjerg), which is unsurprising given that the dispersal policy allocated refugees roughly in proportion to municipal population. Even so, refugee children were dispersed broadly across municipalities, and the correlation between dispersal share and Realized Opportunity is modestly negative at −0.24.

To better understand what drives variation in Realized Opportunity, it is useful to decompose the measure into the two components implied by Equation (1): the intercept term, $\hat{\alpha}_{ms}$, and the slope term, $\hat{\psi}_{ms}$ (Chetty et al., 2014). The intercept captures absolute mobility, that is, where children from the bottom of the parental income distribution can expect to end up. The slope captures relative mobility, that is, how strongly children's ranks increase with parental rank. Realized Opportunity is the sum of these two components evaluated at a parental income rank of 25. Appendix Figure A5 shows that both components vary across municipality-years. Appendix Figure A6 maps the decomposition: Panel (a) shows Realized Opportunity, Panel (b) the intercept, and Panel (c) the slope multiplied by 25. Consistent with the histograms, the spatial distributions of the intercept and slope do not fully overlap. The unweighted correlation between the two components is −0.69, and the population-weighted correlation is −0.71.

In terms of variation over time, Appendix Figure A7 plots the evolution of Realized and Ongoing Opportunity across the 1986–1998 arrival cohorts in our refugee sample. Both measures de-

cline over this period, with the sharpest decreases occurring between 1986 and the early 1990s, followed by a modest rebound. This pattern suggests that absolute mobility for children growing up in Denmark declined over the period we study, mirroring developments documented in the United States (Chetty et al., 2017). The two measures are correlated with one another. Appendix Figure A8 presents a binned scatterplot of Ongoing Opportunity against Realized Opportunity, with both measures standardized to have mean zero and standard deviation one. The estimated slope is 0.626 (S.E. = 0.05).

Finally, Appendix Figure A9 illustrates the role of empirical Bayes shrinkage. Maps of Realized Opportunity with and without shrinkage show nearly identical spatial patterns, while shrinkage narrows the range across municipalities from 44.8–57.4 to 46.0–53.7. Appendix Figure A10 shows the distributions of Realized Opportunity (Panel (a)) and Ongoing Opportunity (Panel (b)) experienced by refugee children, again with and without shrinkage. With shrinkage, the mean (standard deviation) of Realized Opportunity is 49.12 (2.55), while the corresponding values for Ongoing Opportunity are 48.81 (3.13). The figure shows that shrinkage modestly compresses both distributions and that Ongoing Opportunity is more dispersed than Realized Opportunity.

Overall, the evidence shows that economic opportunity varies substantially across Danish municipalities, even in a relatively high-mobility setting. Realized Opportunity exhibits meaningful spatial variation, and that variation reflects differences in both absolute and relative mobility. Ongoing Opportunity is closely related to Realized Opportunity but remains distinct. This variation is central to our analysis, where we ask whether refugee children assigned to higher-opportunity municipalities experience better long-run outcomes.

3.3 Outcome Variables and Additional Covariates

Outcomes. Our primary outcome is the child's percentile rank in the national labor-income distribution, averaged over ages 26–30. We construct income rank within the national distribution of each birth cohort, so the measure is directly comparable across cohorts and corresponds to the same object used to construct the opportunity measures for permanent residents.[16] In addition to income rank, we examine two complementary labor market outcomes: employment at age 26, defined as having positive labor income, and mean annual labor income over ages 26–30.

One way income can be affected is through education. We measure educational attainment at

[16]Labor income is defined as gross market income, comprising earnings, self-employment income, and capital income. For our income-rank variables, we winsorize income at the 0.5th and 99.5th percentiles. Since the income distributions contain mass points at zero, representing shares larger than the 0.5th percentile, we assign all individuals with zero income in each year the midpoint between zero and the lower cutoff for positive incomes. We recode negative incomes reported in any year to zero, following Cholli et al. (2024a). All income amounts are deflated to 2015 Danish kroner using the consumer price index published by Statistics Denmark and converted to 2015 US dollars using a purchasing power parity exchange rate of $100 to 776 DKK (Cholli et al., 2024a).

age 30 based on the highest completed degree. We classify attainment into four mutually exclusive categories: compulsory schooling only (primary education up to 9th grade), upper secondary (high school), vocational training, and academic education, defined as any post-secondary education beyond high school that is not vocational. We additionally examine years of completed schooling as a continuous summary measure.

A benefit of the Danish context is that we can observe a wide range of welfare-relevant outcomes to assess child wellbeing beyond labor market outcomes. We measure criminal offending using convictions accumulated by age 30. Outcomes include an indicator for any conviction and the total number of convictions, both overall and by offense category: property crime, violent assault, and other offenses. Traffic offenses are excluded from all crime measures. Criminal justice involvement is indicative of problematic behavior that is particularly policy relevant due to the inherent externalities involved.

Finally, we also examine effects on family formation and fertility. In particular, we study teen parenthood by age 19, parenthood by age 30, teen marriage, partnership status at age 30, divorce by age 30, and whether the individual is partnered with a Dane at age 30 as a measure of integration.

Baseline Covariates. Our baseline specification includes characteristics observed by the Danish Refugee Council: indicators for parental age, gender, and marital status; the number of children aged 0–2 and 3–17; and child age and gender. We additionally control for parental education, grouped into four categories: unknown, high school or below, vocational, and academic. We measure child and parental characteristics at the time of arrival. Parental characteristics are measured for the first-arriving parent. If both parents arrived at the same time, we use the father's characteristics.

Municipality Characteristics. We construct a rich set of observable characteristics for each municipality and arrival year, largely based on measures directly related to our outcomes or inspired by the literature. These characteristics include: log population; the share of residents who are non-Western immigrants; the share of residents who are co-nationals of the arriving family (including immigrants and their descendants); the share of adults aged 18–60 with at least a high school degree; the employment rate among adults aged 18–60; mean annual labor earnings among adults aged 18–60; the poverty rate (following the OECD (2024) definition, measured as the share of households with incomes below half the national median household income); the Gini index of labor earnings among adults aged 18–60; the crime rate, defined as the average number of criminal convictions among residents aged 15–25; and the share of births to mothers aged 19 or younger. We use these characteristics to assess the quasi-random assignment of refugees to municipalities. In addition, we use them as competing predictors in the horse-race specifications that assess which area attributes are most predictive of refugee children's long-run outcomes.

Descriptive Statistics. Appendix Table C2 presents descriptive statistics for the analysis sample. The average age at arrival was 7.4 years for children and 36.8 years for parents. In 18.7 percent of cases, a parent held an academic degree (bachelor's or higher) before arrival, and 90.7 percent of children arrived with married parents. The average family included 0.46 children aged 0–2 and 2.98 children aged 3–17. Overall, refugee children in our sample appear to come from socioeconomically disadvantaged, traditional families, as reflected in their parents' relatively low levels of education, the high marriage rate, and comparatively large family size. On average, 69.1 percent of refugee children were employed at age 26, and mean annual labor income at ages 26–30 was $19,650. This corresponds to an average child income rank of 38.8 in the national distribution of their birth cohort, compared with a mean parental rank of 13.5 among parents of children in the same birth cohorts. The average refugee child in the sample thus climbed substantially higher in the income-rank distribution compared to their parents. We next turn to our empirical strategy to examine whether refugee children assigned to municipalities with higher economic opportunity experience better long-run outcomes.

4 Empirical Strategy

4.1 Opportunity Effects among Locals: Mover-Exposure Design

We first assess the predictive power of opportunity measures among locals as a form of validation, and the estimates are of interest in their own right. The exercise follows the logic of Chetty and Hendren (2018a,b): if the opportunity measures capture causal features of childhood environments, then children who move to higher-opportunity municipalities at younger ages should experience larger gains in adulthood than otherwise similar children who make the same type of move at older ages.

For this analysis, we study non-refugee children aged 0–22 and, following Chetty and Hendren (2018a), define movers as children whose parents moved exactly once across municipal boundaries before the child turned 23 during the period 1986–2014. Moves are therefore defined based on the parents' addresses. We relate child income rank at ages 26–30, y_i, to the shrinkage-adjusted, standardized change in Realized Opportunity, Δ_{odt}, between origin municipality o and destination municipality d for a child moving at age a in calendar year t, through the following specification:

$$y_i = \omega_{ot} + \gamma\,\Delta_{odt} + \sum_{\substack{a=0 \\ a\neq 17}}^{22} \upsilon_a\,\mathbf{1}\{a_i = a\} + \sum_{\substack{a=0 \\ a\neq 17}}^{22} \eta_a\,\mathbf{1}\{a_i = a\}\,\Delta_{odt} + \theta\mathbf{X}_i + \varepsilon_i, \tag{3}$$

where ω_{ot} are origin-by-year-of-move fixed effects and $\mathbf{X}_i$ is a vector of child and parental characteristics. The coefficients of interest, η_a, capture how the effect of moving to a higher-opportunity municipality varies with age at move relative to moving at age 17. With family fixed effects, identi-

fication of η_a comes from comparing siblings whose parents move at the same time but at different ages, so that differences in siblings' ages generate differences in their length of exposure to the destination municipality. The identifying assumption is that this within-family variation in age at move is not endogenously related to children's potential outcomes. We estimate Equation (3) using both Realized and Ongoing Opportunity. When using Ongoing Opportunity, variables are indexed by birth cohort c rather than year of move t, but the methodology is otherwise identical.

4.2 Opportunity Effects among Refugees: Quasi-Random Dispersal

Our main analysis exploits the quasi-random dispersal of refugees across municipalities to examine whether assignment to higher-opportunity areas causally improves children's long-run outcomes. For each outcome, we estimate a specification of the form:

$$y_{imoT} = \beta_0 + \beta_1 \, \hat{y}_{25,ms(T)} + \theta \, \mathbf{X}_{iT} + \kappa_T + \mu_o + u_{imoT}, \tag{4}$$

where y_{imoT} denotes an outcome for refugee child i assigned to municipality $m(i)$ arriving in year $T(i)$ from country of origin $o(i)$ (e.g., income rank at ages 26–30, years of schooling, or the number of criminal convictions up to age 30); $\hat{y}_{25,ms(T)}$ is our constructed measure of economic opportunity in the municipality of assignment, standardized to have mean zero and standard deviation one; $\mathbf{X}_{iT}$ is a set of child and parental characteristics measured at arrival; κ_T are year-of-arrival fixed effects; and μ_o are country-of-origin fixed effects. Standard errors are clustered at the municipality level.[17]

The key identifying assumption is that, conditional on the characteristics available to the Danish Refugee Council at the time of placement, municipality assignment is orthogonal to unobserved determinants of child outcomes. We provide supportive evidence on this in Section 5.2 through balance tests. The coefficient β_1 is an intent-to-treat (ITT) estimate: it captures the effect of being assigned to a municipality with one-standard-deviation higher economic opportunity regardless of the family's subsequent residential choices. We examine mobility below, where we show that the opportunity measure in the assigned municipality strongly predicts cumulative opportunity exposure over childhood. As a result, the ITT estimates likely underestimate the impacts of area characteristics while providing policy-relevant estimates of the impacts of the dispersal policy on child outcomes.

[17] In robustness checks, we show that results are similar when using a clustered bootstrap that accounts for sampling variation due to the opportunity measures being generated regressors (Section 5.6).

5 Results

5.1 Opportunity Measures and Local Outcomes

Before turning to causal place effects for refugee children, it is useful to test whether our opportunity measures predict economic outcomes in the broader Danish population using the mover-exposure design. Appendix Table B1 reports descriptive statistics for the mover sample. On average, movers reach an income rank of 52 at ages 26–30, and their parents' average income rank is 51. This suggests that movers are broadly representative of the national income distribution. Appendix Figure B1 shows the distributions of changes in Realized and Ongoing Opportunity between origin and destination municipalities, where the ranges are approximately 20 points.

Figure 2 presents the estimated exposure effects by age at move. To place the results in the context of the literature, Panel A reports results for moves with varying Ongoing Opportunity, while Panel B reports results for Realized Opportunity. Both panels point to the same conclusion: children who move to higher-opportunity municipalities at younger ages fare better in adulthood than their older siblings. For Ongoing Opportunity, the age gradient through age 17 has a slope of −0.12 (S.E. = 0.016) without family fixed effects and −0.093 (S.E. = 0.035) with family fixed effects. Using within-family variation, the estimated gradient implies that moving as an infant to an area one standard deviation higher on the opportunity measure would result in an income rank that is 1.7 percentile points higher than moving at age 18. For comparison, among studies that estimate similar specifications controlling for family fixed effects, Chetty and Hendren (2018a) estimate a gradient with respect to the change in the opportunity index of −0.044 (S.E. = 0.008) for moves occurring up to age 23 in the United States. In the Netherlands, Beekers (2024) estimates a statistically insignificant gradient of 0.0062 (S.E. = 0.0108) for moves before age 28. As in Denmark and the United States, the gradient in Australia is steeper among older children: Deutscher (2020) estimates a gradient of −0.028 (S.E. = 0.006) for moves after age 11 up through age 24.

Results for Realized Opportunity are more muted, as they do not incorporate contemporaneous income shocks that affect locals and movers. The estimated gradients through age 17, with and without family fixed effects, are similar at −0.05, although the standard errors are smaller without the fixed effects (S.E. = 0.015 vs. 0.040). The estimates imply that moving as an infant to an area one standard deviation higher on this opportunity measure would result in an income rank that is 0.9 percentile points higher than moving at age 18. Taken together, the opportunity measures appear to capture meaningful differences in local childhood environments.

5.2 Assessing Quasi-Random Assignment

In this section, we assess whether refugee children were quasi-randomly distributed across neighborhoods using a series of balance tests. In addition, we examine the potential for sample selection bias stemming from differential mobility or mortality between refugees assigned to high- and low-opportunity areas.

Table 1 and Appendix Table C3 report the results of balance tests in which we regress the municipality characteristics indicated in the column, measured at the time of refugee children's arrival, on year-of-arrival and country-of-origin fixed effects, as well as two sets of refugee family characteristics predetermined at the time of assignment.[18] The first set, consisting of parental age, child age, and the number of children aged 0–2 and 3–17, was observed by the DRC at the time of placement, whereas the second set, parental education, was not. Standard errors are clustered at the household level. The two joint F-tests reported at the bottom of the tables assess whether (i) parent and child characteristics observed by the DRC at allocation or (ii) parental education, which was unobserved by the DRC at the time of allocation, jointly predict placement.

The balance tests are broadly consistent with quasi-random assignment. In particular, parental education, which was not observed by the DRC at the time of allocation, is not meaningfully related to either Realized Opportunity or the broader set of municipality characteristics, including population size, immigrant shares, local education, labor-market conditions, poverty, inequality, crime, and teen motherhood. The estimated education coefficients are uniformly small, and the corresponding p-values are consistently above conventional significance thresholds even without adjusting for multiple hypothesis testing, providing little evidence that parental education is related to assignment across municipalities. Observable family characteristics are more predictive of some individual municipality attributes. In a few cases, these associations are economically noticeable. For example, having a male parent is associated with about a 10 percent lower co-national share (0.027 percentage points compared to a mean of 0.29 percent), while having married parents is associated with about a 10 percent higher co-national share. However, these same observable characteristics are much less predictive of Realized Opportunity itself. For example, having married parents is associated with only a 0.35 percentage point reduction in non-standardized Realized Opportunity, which is about 0.7 percent of the mean. Overall, these results are consistent with earlier work finding quasi-random refugee placement based on similar balance tests (Hasager and Jørgensen, 2026; Foged et al., 2024a; Damm and Dustmann, 2014), and with the institutional descriptions of the Danish dispersal policy outlined in Section 2.2.

Finally, we turn to the possibility of selective sample attrition. Since our analysis is restricted to

[18] In the balance regressions where Realized Opportunity is the dependent variable, we use the non-standardized measure. Table 1 includes the largest sample for which outcomes are observed. Column (12) in Appendix Table C3 reports results for the somewhat smaller sample where we observe the income rank of the refugee children.

refugee children who are observed in Denmark for at least 15 years after arrival, differential outmigration or mortality across high versus low opportunity areas could induce sample selection bias. The results in Appendix Table C4 show no statistically significant relationship between Realized Opportunity and inclusion in the analysis sample, mitigating concerns that selective attrition biases our estimates.

5.3 Effects of Area Economic Opportunity on Refugee Children's Socioeconomic Outcomes

Income and Education. Table 2 presents the effects of Realized Opportunity on refugee children's income rank at ages 26–30. The mean income rank achieved by the refugees is about 39, which is lower than the intercept among natives. This reflects the disadvantaged nature of the refugee families considered here. Column (1) reports estimates from a parsimonious specification with only year-of-arrival fixed effects and implies that a one standard deviation increase in Realized Opportunity raises adult income rank by 1.78 percentile points. Columns (2)–(5) successively add country-of-origin fixed effects, parental characteristics observed by the DRC, child characteristics, and parental education. Including country-of-origin fixed effects lowers the coefficient to 1.59, adding demographics results in a coefficient of 1.45, and adding parental education results in a similar coefficient of 1.46. This stability is consistent with the quasi-random assignment generating exogenous variation in Realized Opportunity exposure across refugee children.

The controls show that children from larger families have lower income rank, male refugees have higher income rank than female refugees (by 7 percentile points), and children of refugee parents who had an academic education rise modestly higher in the income distribution compared to those whose parents had a vocational degree, with an estimate of 3.4 percentile points. Arriving in Denmark as a younger child results in higher income ranks, a subject we return to below.

Table 3 turns to effects on education, employment, and alternative measures of income. All estimates are based on the specification with the full set of controls. We find that Realized Opportunity has only modest effects on education. A one standard deviation increase in Realized Opportunity results in only 0.07 more years of school (S.E. = 0.049). The largest proportional effect is for the probability of completing a vocational degree, which increases by an estimated 1.1 percentage points (about 3 percent of the mean). The relationship is stronger for labor-market outcomes, with estimates consistently pointing to gains in employment and earnings. In particular, a one standard deviation increase in Realized Opportunity is associated with a 2.0 percentage point higher probability of employment at age 26 (2.9 percent of the mean) and an $863 increase in annual earnings over ages 26–30 (4.4 percent of the mean). We view these as substantial, especially when considering a move from a low opportunity area to a high one: the estimates imply that moving from the 10th

to the 90th percentile of the opportunity distribution would increase earnings by $2,205, or 11.2 percent of the mean.

Effects on Crime. We can go beyond education and income and assess other barometers of child wellbeing, such as criminal justice involvement. Again, the means are noteworthy: 36 percent of refugee children are convicted of a crime by age 30, and the average child has 1.6 convictions. This includes 13 percent convicted of a violent crime. Among locals, the share committing a crime among these birth cohorts averages 18.6 percent.

Columns (1) to (4) of Table 4 show little evidence that being placed in areas with higher Realized Opportunity affects offending at the extensive margin. By contrast, the estimates indicate meaningful effects on the intensive margin. A one standard deviation increase in Realized Opportunity reduces the total number of criminal convictions by age 30 by 0.11 convictions, or 6.9 percent relative to the sample mean.[19] This effect is driven primarily by property crime, for which the corresponding estimate implies a reduction of about 8 percent.

Effects on Family Formation and Fertility. Finally, Table 5 examines whether better economic opportunities affect family formation and fertility. During our study period, 3 percent of refugees are recorded as having a child as a teen, and 6 percent marry as a teenager. Meanwhile, by age 30, 51 percent are married or cohabiting, 5 percent are divorced, and 11 percent are partnered with a local Dane, an outcome of interest as a measure of social integration. We find little evidence that placement in areas with higher Realized Opportunity alters patterns of partnership formation or relationship stability. The results instead suggest that higher Realized Opportunity in the area affects fertility. In particular, a one standard deviation increase in Realized Opportunity is associated with a 0.3 percentage point reduction in the probability of teen parenthood and a 1.5 percentage point reduction in the probability of being a parent by age 30. These estimates correspond to 9.3 percent and 3.2 percent of the sample means, respectively. It is noteworthy that we do not find a meaningful relationship between area opportunity level and partnering with a Dane, suggesting that this form of integration is not driving the income and crime results.

5.4 Potential Pathways

This section explores several potential pathways through which higher local economic opportunity may affect refugee children's long-run outcomes. We consider four sets of mechanisms: whether higher opportunity areas improve parents' economic integration, whether they facilitate better in-

[19] For comparison, Damm and Dustmann (2014) find that a one standard deviation increase in the youth crime rate in the assigned municipality at the time of assignment increases the number of crimes committed by men by age 21 by 0.106 (2 percent relative to the sample mean), with no effect among women.

tegration for children who arrive at younger ages, whether particular dimensions of opportunity are especially relevant for refugees, and whether Realized Opportunity mainly reflects underlying municipality characteristics such as labor market conditions or inequality. To keep the discussion focused, we center this section on our main outcome, child income rank at ages 26 to 30. Comparable estimates for a subset of secondary outcomes are reported in Appendix C3 and described below.

Parental Economic Integration. A natural pathway through which local economic opportunity may affect children is by improving the economic outcomes of their parents. We explore this channel in three steps. First, we ask whether refugee parents assigned to higher-opportunity municipalities achieve better labor-market outcomes themselves. To do so, Appendix Figure C1 plots average parental income rank against the Realized Opportunity of the municipality to which the family was assigned. Parental income is measured when the child is aged 24–28, and the fitted line is based on the specification with the full set of controls. The positive slope indicates that parents assigned to higher-opportunity municipalities climb higher in the income distribution. Quantitatively, a one standard deviation increase in Realized Opportunity raises parental income rank by about 0.989 percentile points. This is about two-thirds of the effect on child income rank.

Second, we ask how strongly parental income rank predicts children's later outcomes within the refugee population. Appendix Figure C2 shows a binned scatterplot of refugee children's adult income rank, averaged over ages 26–30, against parental income rank. Income rank is measured within the national distribution of the child's birth cohort. As expected, the parental rank distribution is heavily concentrated near the bottom, reflecting relatively weak labor-force attachment among recently arrived refugees. The figure shows a positive correlation within the refugee families: the estimated slope is 0.308 (S.E. = 0.02), with an intercept slightly above 35. Appendix Table C5 further shows that parental income rank is strongly related not only to children's adult income rank but also to their criminal convictions and likelihood of teen parenthood.

If the intergenerational correlation of income provides an upper bound on the causal effect of parental income on children's adult earnings,[20] we can begin to assess how much of the effect of area Realized Opportunity on child income rank could plausibly operate through parental earnings. Our estimates imply that a one standard deviation increase in Realized Opportunity raises parental rank by about one percentile point, and the intergenerational income correlation among refugee families implies that a one percentile increase in parental rank is associated with about a 0.31 percentile increase in child income rank. This means that the parental income channel would imply an increase in child income rank up to approximately 0.3 percentile points, or about 20 percent of the

[20] The intergenerational correlation would be biased upward if income is positively related to ability or other factors that influence earnings.

estimated effect of area opportunity on child income shown in Table 2. An analogous calculation for the other outcomes suggests that parental income explains only a modest share of the estimated effects, accounting for less than 10 percent of the reductions in criminal convictions and teen parenthood.[21] Overall, the evidence suggests that improvements in parents' economic outcomes are part of the story, but they account for only a limited share of the overall place effect on refugee children.

Age at Arrival and Integration. Another potential mechanism is that higher-opportunity areas may facilitate better integration into the host society. Although we do not directly observe social integration, we provide suggestive evidence on this channel using age at arrival. Prior research shows that children who arrive at younger ages tend to have better long-run outcomes, in part because they acquire language skills more easily and integrate more successfully into school (Bleakley and Chin, 2010, 2004). If higher opportunity areas operate partly by supporting these forms of integration, one might expect both a strong relationship between age at arrival and later outcomes, and larger effects of Realized Opportunity for children who arrive younger.

Figure 3 speaks to these two predictions. Panel (a) presents coefficients from regressions of income rank at ages 26–30 on age-at-arrival indicators, estimated with and without family fixed effects. In both specifications, refugee children who arrive at younger ages have much better outcomes in adulthood. Arriving at age 0 rather than age 17 is associated with an increase in early adulthood income rank of 22.7 percentile points without family fixed effects and 18.7 percentile points with family fixed effects. Arriving after age 17 is associated with worse outcomes, although the sample is restricted to those who arrive with their parents and young adults who arrive with their parents may not be representative of other young adults. The pattern from age 0 to 17 shows that earlier arrival is strongly associated with better long-run integration, consistent with prior evidence on immigrant children.

Panel (b) then asks whether the effect of Realized Opportunity varies with age at arrival. The interactions between age-at-arrival indicators and Realized Opportunity are flat in both specifications. The slope is imprecisely estimated, however, and does not rule out the gradient found for locals in Figure 2. Combined with the main results, the lack of an age gradient implies that arriving in a higher-opportunity area is beneficial regardless of age at arrival. This differs from the pattern among Danish children who move between municipalities, where the same measure points to larger gains for younger movers: in Figure 2, moving at birth rather than at age 18 to a municipality with one-standard-deviation higher Realized Opportunity is associated with a gain of about 0.9 percentile points within families. The two designs identify different and complementary objects, however.

[21] For these two child outcomes, we measure parental income at child age 15–19 to ensure that it does not capture parental resources after the child outcomes are realized. Appendix Figure C3 shows that the relationship between parental income at those ages and Realized Opportunity is weaker: a one standard deviation increase in Realized Opportunity increases parental income rank at child age 15–19 by only 0.370 percentile points.

The mover-exposure design recovers the gradient of place effects in exposure time and, by design, does not estimate the level effect that applies to all ages. The dispersal design recovers that level effect directly.

The absence of a detectable gradient is in contrast with previous estimates from mover designs (e.g., Laliberté, 2021; Deutscher, 2020; Chetty and Hendren, 2018a), which typically find that local children who move at younger ages benefit more from areas with better opportunity, but consistent with mover estimates from the Netherlands (Beekers, 2024) and with evidence from public housing project demolitions (Haltiwanger et al., 2024). One explanation for the lack of a gradient in the current context is that refugee children are a different population from local movers. An older local mover carries the networks and often continued ties of the area where they grew up, so the destination's effect is partly diluted; an older refugee child has no such origin in Denmark. The lack of an age gradient does not imply that opportunity is unimportant for refugee children who arrive young; it implies that the effect operates through the intercept of the exposure relationship rather than its slope.

In summary, while younger arrival is strongly associated with better adult outcomes overall, there is little evidence that the effects of Realized Opportunity are systematically larger for children who arrive younger. Appendix Figures C5 and C6 and Appendix Table C6 show a similar pattern for the secondary outcomes. The estimated effects of Realized Opportunity on criminal convictions and teen parenthood do not exhibit a monotonic decline with earlier arrival. If anything, the reductions in criminal convictions are largest for those arriving at ages 3–10 and 19–21, and the teen parenthood effect is concentrated among those arriving at ages 15–16. Thus, while age at arrival is strongly associated with later outcomes in levels, there is little evidence across outcomes that higher Realized Opportunity is systematically more beneficial for children who arrive younger. This pattern suggests that early-life human capital formation, such as language acquisition, and social integration are unlikely to be the main channels through which living in an area with greater Realized Opportunity benefits refugee children. Instead, the benefits appear to run through channels available at any age at arrival, such as local labor market opportunities. Consistent with this interpretation, refugee parents also gain from assignment to higher-opportunity areas, and the effects for children are concentrated in employment and earnings rather than educational attainment.

Different Dimensions of Opportunity. We next ask which dimensions of Realized Opportunity are most closely related to refugee children's adult outcomes in order to begin understanding the pathways that allow local economic opportunity to affect child wellbeing. We begin by asking whether the relevant opportunity for refugees is more closely tied to the social mobility of natives or of immigrants and descendants already living in the area. This distinction is informative about the type of environment that may matter. If refugee children benefit primarily from integration into

the broader host society, the mobility of natives may be especially relevant. If instead they benefit mainly from opportunities available within immigrant communities, mobility among immigrants and descendants may be more informative. To examine this, we construct separate Realized Opportunity measures for natives and for immigrants and descendants. Columns (1) to (3) of Appendix Table C7 show that both measures positively predict refugee children's adult income rank when entered separately, and both remain significant when included jointly. At the same time, the native-based measure is somewhat larger and more precisely estimated. Panel (a) of Appendix Figure C4 shows that the two measures are positively related, but far from identical, with an estimated slope of 0.49. Taken together, these results suggest that refugee children benefit from local environments that matter for both natives and immigrants.

We next ask whether the relevant opportunity differs by child gender. Columns (4) and (5) of Appendix Table C7 report results using separate Realized Opportunity measures for boys and girls. Column (4) reports regressions of boys' income rank at ages 26 to 30 on both the boys' and girls' measures, while column (5) reports the analogous regression for girls. For boys, only the boys' Realized Opportunity measure is statistically significant. For girls, only the girls' measure is significant. Panel (b) of Appendix Figure C4 shows that the own-gender and opposite-gender measures are positively correlated, but again not identical, with an estimated slope of 0.35 and a standard error of 0.09. This pattern suggests that the opportunity embedded in a place is not fully generic, but instead differs in ways that are relevant for boys and girls separately.

Finally, we ask which components of Realized Opportunity are most predictive of refugee children's outcomes. Column (6) of Appendix Table C7 decomposes Realized Opportunity into the intercept term (absolute mobility), $\hat{\alpha}_{ms}$, and the slope term (relative mobility), $\hat{\psi}_{ms}$. Both components are positively and significantly associated with refugee children's adult income rank, with a somewhat larger coefficient on the intercept term. Panel (c) of Appendix Figure C4 shows that the two components are strongly negatively correlated, with an estimated slope of -0.76 and a standard error of 0.05. These results suggest that both the overall level of upward mobility available to disadvantaged children and the degree of intergenerational persistence in a place are relevant for refugee children's later outcomes.

Realized Opportunity and Related Municipality Characteristics. Realized Opportunity is a constructed measure of local economic opportunity. As such, it may be related to underlying municipality characteristics that shape children's long-run prospects. Examining these relationships can therefore shed light on the kinds of places that generate higher opportunity and help clarify which local characteristics may lie behind the effects documented above.

Table 6 begins by examining how Realized Opportunity is related to a broad set of municipality characteristics in the sample of refugee children. These characteristics capture demographic com-

position, educational attainment, labor market conditions and earnings, inequality, and measures of risky behavior, all measured at the time of arrival to match the measurement of Realized Opportunity. Column (1) reports coefficients from bivariate regressions of Realized Opportunity on each municipality characteristic. Realized Opportunity is higher in smaller municipalities and in areas with lower shares of non-Western immigrants and co-nationals. Higher-opportunity municipalities also tend to have stronger labor market conditions, including higher employment rates, higher mean earnings, and lower poverty rates, and they exhibit lower inequality as measured by the Gini coefficient. For example, a standard-deviation (5.2 percentage points) increase in the poverty rate of the municipality is associated with a reduction in Realized Opportunity by 1.8 points, or 0.7 of a standard deviation of Realized Opportunity across municipality-years. By contrast, they do not differ markedly in terms of crime rates or teen motherhood rates.

Column (2) includes all municipality characteristics simultaneously to assess which remain predictive of Realized Opportunity conditional on the others. In this multivariate setting, measures of earnings potential and inequality remain statistically significant predictors, although the individual coefficients are harder to interpret because many of these characteristics are closely related. Column (3) complements this exercise using a post-LASSO procedure to identify the subset of municipality characteristics that best predict Realized Opportunity.[22] The post-LASSO results point to the (log) population, local employment rate, mean earnings and the Gini index, suggesting that size, labor-market strength, income and inequality are among the most important correlates of local economic opportunity. This pattern is consistent with the broader idea that areas with greater upward mobility also tend to be more equal (Britto et al., Forthcoming; Corak, 2013), and with recent work showing a strong relationship between changes in local social mobility and parental employment rates (Chetty et al., 2026b).

A natural next question is whether these underlying municipality characteristics account for the effects of Realized Opportunity on refugee children's outcomes. To address this question, Table 7 compares the predictive power of Realized Opportunity with that of other municipality characteristics in a series of horse-race specifications. Specifically, we regress child income rank at ages 26–30 on Realized Opportunity and, one at a time, each additional municipality characteristic.[23] All characteristics are standardized to facilitate comparison across coefficients.

The results show that Realized Opportunity remains statistically significant in all but one specification, namely the regression that includes the Gini index. In addition, the coefficient on Realized

[22] All variables were standardized prior to the LASSO estimation, but the variables reported in the table are not standardized.

[23] Appendix Table C8 documents the effects of these municipality characteristics on child income rank without controlling for Realized Opportunity. Several municipality characteristics have meaningful predictive power when entered on their own. However, in the horse-race regressions, most lose explanatory power once Realized Opportunity is included, suggesting that the opportunity measure absorbs much of the relevant variation in local conditions.

Opportunity is larger than that on the competing municipality characteristic in 8 of the 10 comparisons, with poverty and the Gini index as the main exceptions. Appendix Table C9 considers the predictive power of Realized Opportunity, poverty, and the Gini index jointly. When Realized Opportunity is entered alongside poverty, both coefficients remain sizable and statistically significant, although the coefficient on Realized Opportunity declines from 1.447 to 0.663. When Realized Opportunity is entered alongside the Gini index, the attenuation is more substantial: the coefficient on Realized Opportunity falls to 0.521 and is no longer statistically significant, while the coefficient on the Gini index is larger in absolute value. When all three variables are included together, the coefficient on Realized Opportunity falls further to 0.330, the coefficient on poverty to −0.635, and only the Gini index remains statistically significant, with a coefficient of −1.038. Taken together, these patterns indicate that economic opportunity is a strong predictor of adult income, and inequality captures an especially important part of what makes some places more conducive to upward mobility. This is consistent with the negative relationship between social mobility and inequality known as the Great Gatsby curve. The result echoes evidence from the U.S. that persistent lower-tail inequality raises early nonmarital childbearing among low-SES young women and high school dropout among low-SES boys, patterns attributed to a reduction in perceived returns to human capital (Browman et al., 2019; Kearney and Levine, 2016, 2014). Our setting using the quasi-random assignment of children across municipalities provides novel evidence that inequality in the area where children grow up is itself a powerful predictor of later-life outcomes.

Related to inequality is the notion of social connectedness, the degree to which lower-income children interact with higher-income peers. In the spirit of Chetty et al. (2026b) and Katz (2026), we consider a measure of the degree to which different income groups mix within schools in each municipality.[24] This measure of within-school income mixing is strongly correlated with Realized Opportunity, and children assigned to areas with higher income mixing attain higher income ranks as adults ($p < 0.1$). In a horse-race regression with Realized Opportunity, however, this mixing measure is not statistically significant.

Beyond income rank, the estimated effects of area opportunity levels are especially pronounced for criminal justice involvement and teen parenthood. Appendix Tables C10 and C11 show that Realized Opportunity is not only statistically significant for these outcomes, but also compares favorably in magnitude with the other municipality characteristics we consider. Its predictive power is most striking for criminal convictions, where it dominates all competing municipality characteristics, including the local crime rate itself (Appendix Table C10).[25] Realized Opportunity also remains

[24] Among children whose parents are at or below the 25th percentile of the parental income distribution, we calculate the average share of students in the same school cohort whose parents are at or above the 75th percentile of the income distribution among parents with children in that cohort. School cohorts are measured in 8th grade (approximately age 14) of the Danish primary school system, and the measure is aggregated to the municipality level based on the location of each school.

[25] In contrast, Dustmann et al. (2023) find that area employment and poverty rates at the time of arrival do not

among the strongest predictors of teen parenthood (Appendix Table C11). These patterns are consistent with local economic opportunity capturing an important dimension of child wellbeing that extends beyond income alone.

5.5 Heterogeneous Effects

Appendix Table C12 examines heterogeneity by gender, parental education, and region of origin. We find that the effects on child income rank are positive in every subgroup and economically meaningful throughout, ranging from roughly 2.5 percent to 5.4 percent of subgroup means. While the point estimates are somewhat larger for girls, children with highly educated parents, and children from the Middle East, the positive effects are sufficiently broad that the main result does not appear to be driven by any single subgroup. In contrast, the effects on adverse outcomes are less uniform. The crime effects are largest for boys, children with highly educated parents, and children from Africa, for whom criminal convictions fall by about 7, 13, and 18 percent, respectively. The teen parenthood effect is concentrated among girls and among children from the Middle East, with relative declines of about 8 and 12 percent. These patterns are broadly consistent with the fact that adverse outcomes such as crime and teen parenthood are concentrated in particular groups to begin with. Taken together, the results suggest that higher-opportunity areas appear to generate broadly shared gains in adult economic status, while the effects on crime and fertility are concentrated on margins where underlying risk is already greatest.

5.6 Robustness Checks

In this section, we assess the robustness of the main findings to alternative choices regarding the measurement of opportunity, sample definition, and statistical inference. To focus the discussion, we report results for our main outcome, child income rank at ages 26 to 30, together with a set of secondary outcomes for which Realized Opportunity has statistically significant effects in the baseline analysis.

Role of Geographic Mobility. We have thus far interpreted our results as reflecting children's exposure to better local environments due to the initial area assignment. A potential concern is that refugee families may leave their assigned municipality soon after placement. If mobility of this kind is common, the opportunity level in the municipality of assignment may be a noisy proxy for the opportunity level children actually experience while growing up. In that case, our baseline estimates should be interpreted as intent-to-treat effects of assignment rather than effects of realized exposure.

attenuate the positive relationship between local gang crime rates and later criminal activity.

We shed light on this issue in two ways. First, Appendix Figure C7 shows the fraction of refugee children who remain in the municipality of assignment over the first 15 years after arrival. For most of this period, a majority of refugee children continue to live in their assigned municipality, and even after 15 years roughly half remain there. On average, children in our sample spend 10.23 years in the municipality of assignment. These patterns indicate that initial assignment has substantial persistence.

Second, Appendix Figure C8 relates opportunity in the municipality of assignment to cumulative exposure to Realized Opportunity, constructed as the weighted average of Realized Opportunity (measured in each municipality in the year of assignment) across the municipalities where the child resides up to age 25, with weights given by the share of time spent in each municipality. The figure plots a binned scatterplot together with a fitted line from a regression that controls for year of arrival, country of origin, and child and parental characteristics. The estimated slope is 0.73 (S.E. = 0.028), indicating a strong relationship between assignment and exposure to opportunity. Thus, although opportunity at assignment does not fully capture the opportunity children ultimately experience, it remains a strong predictor of actual exposure.

Under the assumptions of independence, monotonicity, and the exclusion restriction, the intent-to-treat estimates can be re-scaled by the first-stage relationship between assignment and cumulative exposure to estimate a local average treatment effect (LATE). Independence is plausible given the quasi-random dispersal policy, and we find no evidence against monotonicity: assignment to higher-opportunity municipalities does not induce movement toward lower-opportunity ones. For the exclusion restriction, Schmandt et al. (2025) note that random assignment to places does not by itself identify the causal effect of any single area characteristic because places differ along many correlated dimensions. This concern is less relevant in our setting because our object of interest is the composite measure of area opportunity itself rather than any particular underlying mechanism. Like inequality, economic opportunity is an influential measure that is intended to summarize the overall childhood environment associated with a place, not to isolate the contribution of schools, peers, labor markets, or other individual channels. Under these assumptions, the estimated effect of Realized Opportunity on child income rank increases from 1.46 to 2.0 percentile points.

Measurement of Opportunity. We next assess the robustness of our results to alternative measures of local economic opportunity. First, Ongoing Opportunity is more strongly associated with child outcomes than Realized Opportunity. As shown in Appendix Figure C9, the coefficient on Ongoing Opportunity is 1.824 (S.E. = 0.304), compared with 1.463 (S.E. = 0.331) for Realized Opportunity in the equivalent regression reported in column (5) of Table 2. This is unsurprising because Ongoing Opportunity includes common shocks for locals and refugees, and it demonstrates that local opportunity has a strong association with child outcomes.

Appendix Table C13 examines how sensitive the results are to the timing of the Realized Opportunity measure. Panel A reproduces the baseline estimates, in which Realized Opportunity is measured in the year of arrival, for comparison.[26] Panel B instead measures Realized Opportunity in the year after arrival, $T + 1$, while Panel C uses the year before arrival, $T - 1$. Panel D includes all three measures simultaneously to assess which timing is most informative once the others are held constant. For child income rank, Realized Opportunity measured in the year before arrival, the year of arrival, and the year after arrival each predicts later outcomes when entered separately, but the arrival-year measure remains the strongest predictor when all three are included jointly. For the secondary outcomes, the coefficient on Realized Opportunity measured at arrival is generally the largest in magnitude, although the estimates become less precise in the joint specification. This pattern indicates that the main results are not sensitive to modest shifts in timing, and that the arrival-year measure captures the most informative summary of local opportunity at the time of assignment.

In Panel E, we assess the sensitivity of the results to applying empirical Bayes shrinkage when constructing the opportunity measures. The estimated effects based on Realized Opportunity without shrinkage are essentially unchanged, indicating that the main findings are not driven by the shrinkage procedure.

Cost of Living. Our measure of child income rank does not take into account differences in cost of living across municipalities. In Appendix Table C14, we assess whether our findings are robust to adjusting for local housing costs, a proxy for differences in the cost of living across municipalities. We use public housing valuations for 1987–2016 to calculate the average price per square meter in each municipality and year. We then construct a municipality-year housing price index by dividing these prices by the corresponding value in Copenhagen in 2015. Column (1) shows the results when child adult income is deflated using this housing price index before calculating the child's rank in the national income distribution. Column (2), for comparison, shows the results for the same sample when child adult income is deflated using the consumer price index, as in the rest of the paper.[27] The results are similar to the main results: adjusting for local housing costs reduces the point estimate on Realized Opportunity relative to the CPI-deflated specification, but the estimate remains statistically significant. Moreover, a formal test does not reject the null hypothesis that the two coefficients are equal.

[26] The results in Panel A for Realized Opportunity measured in the year of arrival (T) may differ slightly from the main results because the estimation samples differ. Specifically, Realized Opportunity is not defined for years before 1986, and the estimation sample is restricted to observations for which Realized Opportunity is defined in the year of arrival (T), the year after arrival ($T + 1$), and the year before arrival ($T - 1$).

[27] The sample in Appendix Table C14 is smaller because public housing valuations are available only for 1987–2016, so we lose the years 2017–2022, for which we otherwise have income data.

Sample Composition. Panel A of Appendix Table C15 assesses whether the main results are driven by Denmark's largest cities. Excluding refugee children assigned to the five largest municipalities (Copenhagen, Aarhus, Odense, Aalborg, and Esbjerg) leaves the estimate for child income rank largely unchanged. For the secondary outcomes, the estimated effect on criminal convictions becomes roughly 40 percent larger in absolute value. These results therefore suggest that the baseline findings are not driven by these larger urban areas alone.

Inference. In Panel B of Appendix Table C15, we address the fact that Realized Opportunity is a generated regressor and that conventional standard errors may understate uncertainty by failing to account fully for estimation error in the opportunity measure. To do so, we re-estimate the standard errors in our main specifications through a clustered bootstrap. This alternative inference procedure does not materially affect the results. If anything, the clustered bootstrap standard errors are 13 to 25 percent smaller than the conventional clustered standard errors.[28]

Overall, we show that the estimated effects are stable across alternative opportunity measures, modest shifts in timing, sample restrictions, and inference procedures. Moreover, the strong relationship between assigned and experienced Realized Opportunity exposure indicates that the assigned municipality is an informative proxy for the environments children experience during childhood. Taken together, these results suggest that the main findings are not an artifact of particular measurement or specification choices.

6 Conclusion

This paper studies whether growing up in a higher-opportunity area improves the long-run outcomes of refugee children. To address this question, we first construct municipality-level measures of economic opportunity using the income mobility of permanent residents who had already reached adulthood at the time of refugee placement (Realized Opportunity). We then exploit the quasi-random assignment of refugee families across municipalities under Denmark's 1986 to 1998 dispersal policy and estimate whether refugee children assigned to municipalities with higher opportunity experience better outcomes in adulthood.

We find that refugee children assigned to municipalities with higher Realized Opportunity have higher income rank in adulthood, higher employment and earnings, modestly better educational outcomes, and fewer criminal convictions and teen births. Our analysis of potential mechanisms suggests that parental economic integration accounts for up to 20 percent of the estimated effect on children's adult income rank, while the absence of differential effects by age at arrival suggests that

[28] That the results are qualitatively unchanged after accounting for estimation error in Realized Opportunity is not surprising, as the measures are constructed among the much larger local population, limiting the amount of uncertainty.

the opportunity channel does not operate primarily through language acquisition or early school integration. Further, refugee children appear to benefit from local environments that generate opportunity for both natives and immigrants, and the relevant opportunity differs by gender in ways that are consistent with gender-specific labor market trajectories. A natural question is whether Realized Opportunity simply proxies for other municipality characteristics. For crime and teen parenthood, Realized Opportunity is a stronger predictor than other commonly studied characteristics, including the local crime rate itself. For adult income, local inequality is a particularly strong predictor and substantially attenuates the coefficient on Realized Opportunity in horse-race regressions. This suggests that lower local inequality may be an important channel through which higher-opportunity areas benefit the children growing up in them.

Our results have two broader implications. First, they add to the evidence that place matters for children's long-run outcomes among vulnerable children, using a novel source of plausibly exogenous variation in area exposure. Second, they suggest that refugee settlement policy can shape long-run integration not only through immediate labor-market opportunities for adults, but also through the local environments where children grow up. Because Realized Opportunity is observed at the time of assignment, it could in principle be incorporated into dispersal decisions to improve child wellbeing.

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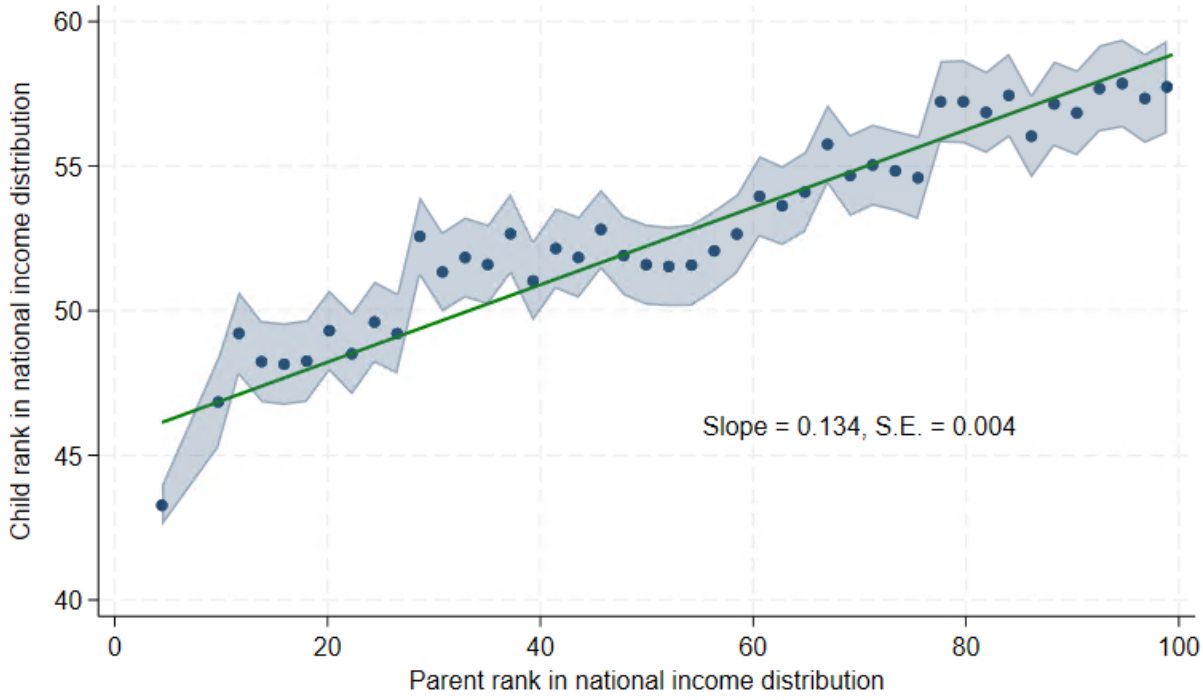
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Figure 1: Mean Child Income Rank versus Parent Income Rank for Permanent Residents

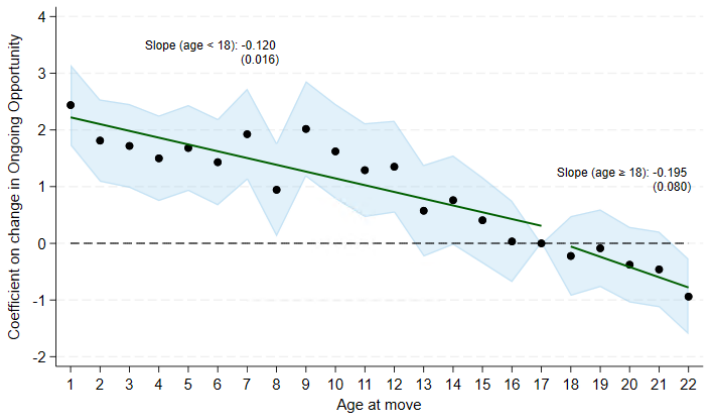


Notes: This figure presents a binned scatterplot of the relationship between parental rank and child rank in the national income distribution for sons and daughters in Denmark who were aged 30 in 1992. Child income is measured as labor income averaged over ages 26–30. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 24–28. The sample is restricted to "permanent residents," i.e., parents who lived in the same municipality throughout the parental income-averaging period. We implement the binned-scatter method of Cattaneo et al. (2024) with IMSE-optimal bins and 95 percent confidence bands that capture uncertainty in the estimated conditional mean function. The figure also overlays the OLS fit to the individual-level data. The reported standard error for the slope is heteroskedasticity-robust.

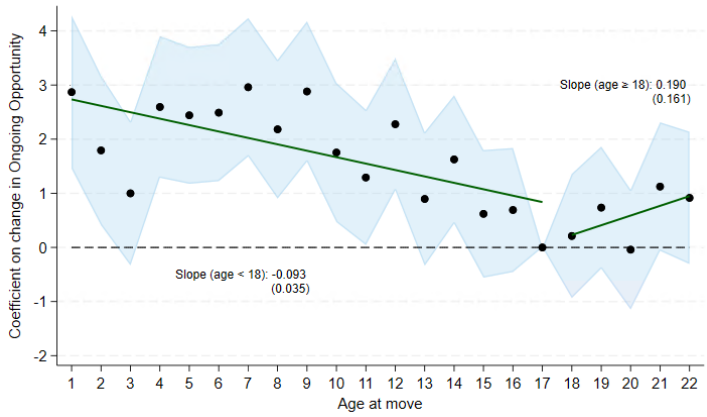
Figure 2: Age-Exposure Effects for Non-Refugee Movers

Panel A: Ongoing Opportunity

(a) Without Family Fixed Effects

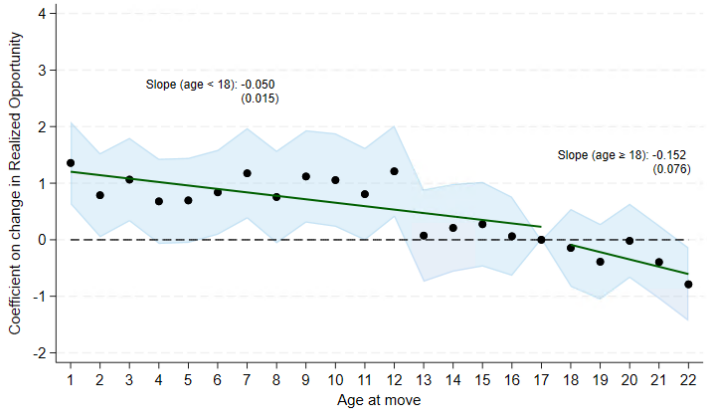


(b) Including Family Fixed Effects

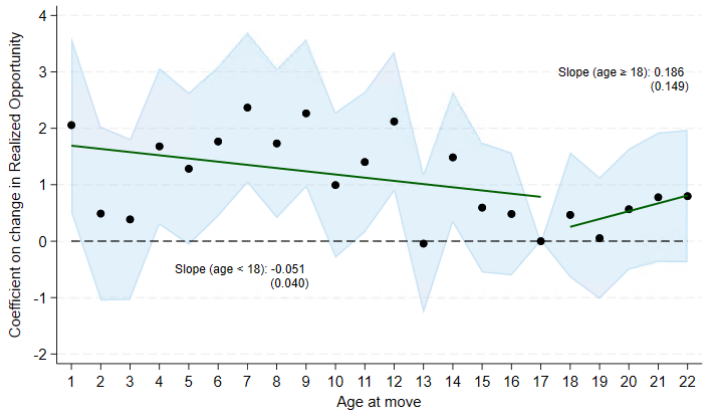


Panel B: Realized Opportunity

(c) Without Family Fixed Effects



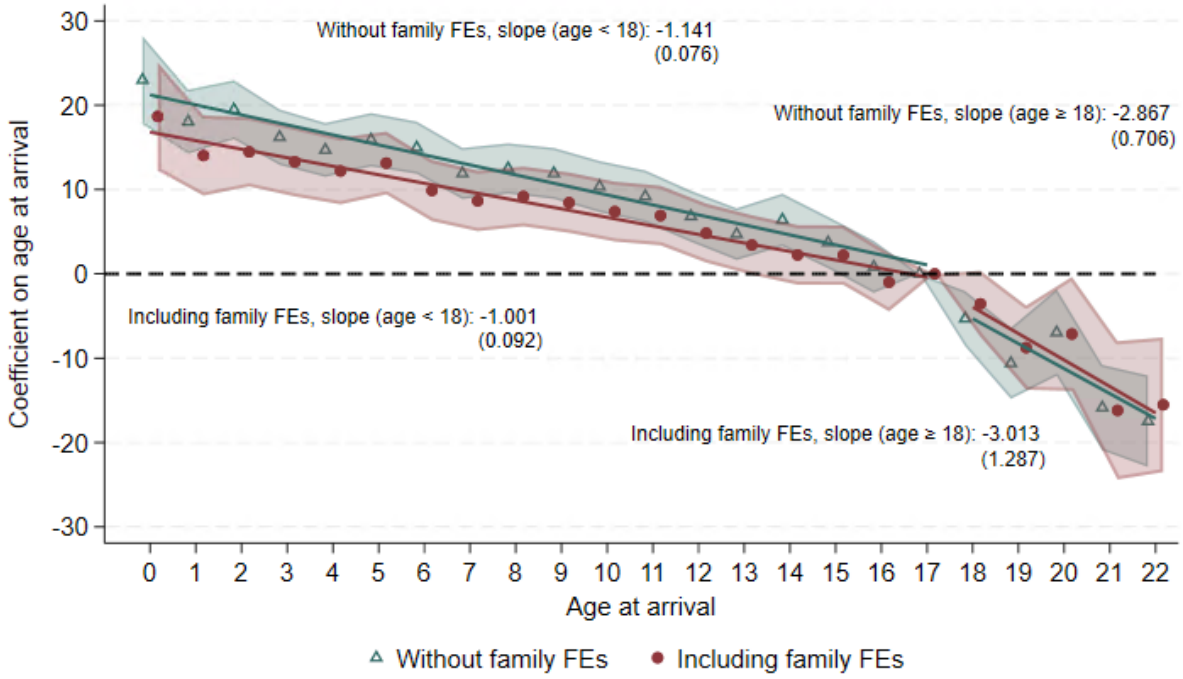
(d) Including Family Fixed Effects



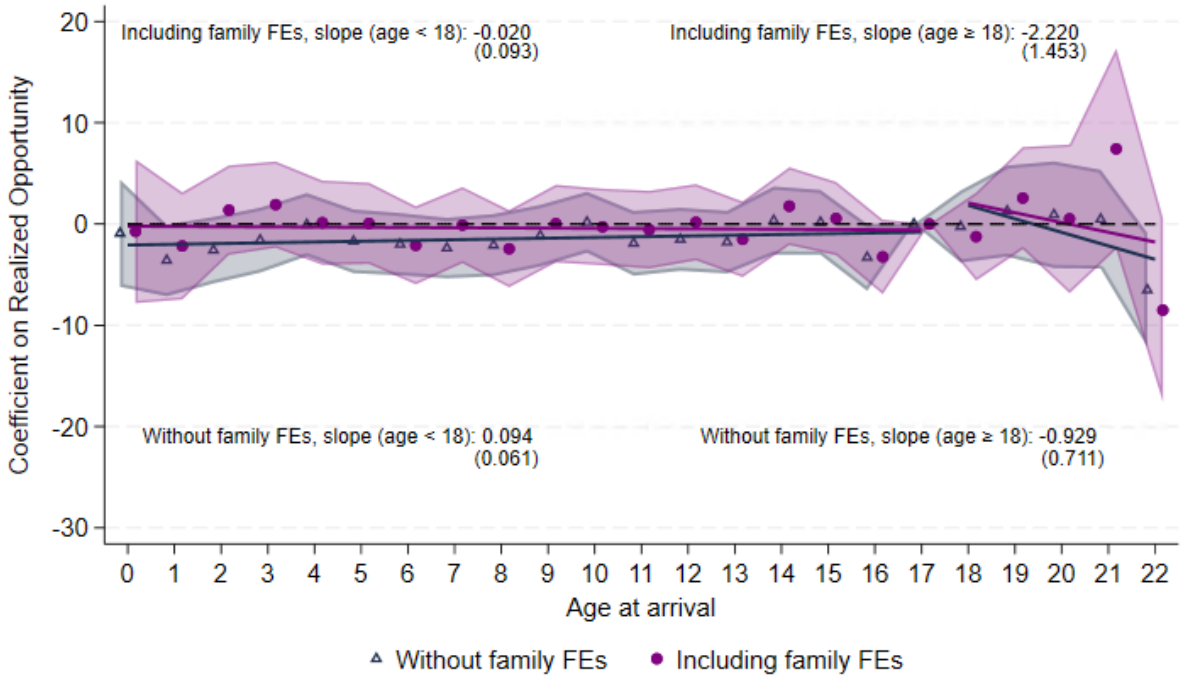
Notes: These figures present age-exposure effects, defined as the coefficients on the interaction between the standardized change in Ongoing or Realized Opportunity and age at move. The coefficients are estimated from regressions of child income rank at ages 26–30 on the change in Ongoing or Realized Opportunity, age at move, and their interaction, controlling for child characteristics (age at move and gender) and parental characteristics (age at move, gender of the household head, marital status at move, number of children at move, and education level at move). Panel A presents the results for Ongoing Opportunity, while Panel B presents the results for Realized Opportunity. The specifications in panels (a) and (c), shown on the left, do not include family fixed effects and instead include origin-by-year-of-move fixed effects, whereas those in panels (b) and (d), shown on the right, include family fixed effects. All regressions also control for ancestry-country fixed effects. The sample includes non-refugees aged 0–22 whose parents moved once from one municipality to another before the child turned 23 between 1986 and 2014. The interaction coefficient at age 17 is normalized to zero. The reported slope coefficients and standard errors are estimated using microdata, with age entered as a continuous variable in the regressions. The opportunity measures are standardized and estimated using empirical Bayes shrinkage. Standard errors are clustered at the household level.

Figure 3: Age-at-Arrival and Child Income Rank

(a) Child income rank and Age-at-Arrival



(b) Effects of Realized Opportunity by Age-at-Arrival



Notes: Estimates stem from regressions of child income rank at ages 26–30 on age at arrival, Realized Opportunity, and other child and parental characteristics. The sample consists of refugee children who arrived in Denmark at ages 0–22 within one year of their parents' arrival during 1986–1998 and were subject to the dispersal policy. Child and parental characteristics are measured at arrival. The coefficient for age 17 is normalized to zero. Panel (a) reports the coefficients on the age-at-arrival indicators. Panel (b) reports the coefficients on the interaction between the age-at-arrival indicators and Realized Opportunity. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. The reported slope coefficients and standard errors are estimated using microdata, with age entered as a continuous variable in the regressions. 95 percent confidence intervals are reported, using standard errors clustered at the household level.

Table 1: Balancing Tests for Refugee Children

Dependent var.:	(1) Realized Opportunity
Unobserved at Time of Allocation	
Unknown educ., parent	-0.144
	(0.123)
Basic educ., parent	-0.173
	(0.130)
Academic educ., parent	-0.171
	(0.138)
Observed at Time of Allocation	
Age, parent	-0.004
	(0.005)
Male, parent	0.054
	(0.093)
Married, parent	-0.345**
	(0.137)
No. of children, ages 0-2	-0.053
	(0.072)
No. of children, ages 3-17	0.017
	(0.027)
Age, child	-0.008
	(0.006)
Male, child	0.090**
	(0.043)
Observations	12,415
Households	5,555
Unobserved chars.: $Pr > F$	0.560
Observed chars.: $Pr > F$	0.067
Mean of dep. var	49.13
SD of dep. var.	2.545

*Notes: This table presents balance tests in which municipality-level Realized Opportunity is regressed on child and parental attributes for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. All variables are measured at arrival. Realized Opportunity is the predicted child income rank at ages 26–30 for children whose parents are at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2). $Pr > F$ reports the p-value from an F-test of the joint insignificance of the indicated set of variables. The educational-attainment dummies are basic education, academic education, and unknown education, with vocational education as the omitted reference category. Realized Opportunity is measured with empirical Bayes shrinkage. The regressions control for year-of-arrival and country-of-origin fixed effects. Standard errors are clustered at the household level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Table 2: Effects of Opportunity on Income Rank

Dependent var.:	(1) Income rank at 26-30	(2) Income rank at 26-30	(3) Income rank at 26-30	(4) Income rank at 26-30	(5) Income rank at 26-30
Realized Opportunity	1.775***	1.593***	1.563***	1.451***	1.463***
	(0.478)	(0.374)	(0.359)	(0.333)	(0.331)
Parental characteristics observed by DRC					
Age, parent			-0.259***	0.008	0.008
			(0.041)	(0.042)	(0.044)
Male, parent			2.612***	0.956	0.878
			(0.964)	(0.924)	(0.936)
No. of children, ages 0-2			-0.561	-2.433***	-2.380***
			(0.543)	(0.518)	(0.523)
No. of children, ages 3-17			-1.622***	-1.095***	-1.085***
			(0.154)	(0.168)	(0.169)
Married			0.427	0.231	0.021
			(0.929)	(0.897)	(0.914)
Child characteristics					
Age, child				-1.143***	-1.141***
				(0.072)	(0.072)
Male, child				7.002***	7.041***
				(0.584)	(0.588)
Parental characteristics unobserved by DRC					
Unknown educ.					0.530
					(0.909)
Basic educ.					0.661
					(1.024)
Academic educ.					3.373***
					(1.291)
Observations	10,474	10,474	10,474	10,474	10,474
R-squared	0.006	0.048	0.067	0.102	0.103
Clusters	213	213	213	213	213
Mean dep. var	38.79	38.79	38.79	38.79	38.79
Country-of-origin FE	No	Yes	Yes	Yes	Yes

*Notes: This table reports regression estimates of child income rank at ages 26–30 on Realized Opportunity in the municipality of assignment, with various sets of controls, for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Child and parental characteristics are measured at arrival; parental characteristics refer to the first-arriving parent. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. Each column reports results from a separate regression and all regressions control for year-of-arrival fixed effects. Robust standard errors, clustered at the municipality level, are reported in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10.*

Table 3: Effects of Opportunity on Education and Labor Market Outcomes

Dependent var.:	(1) Years of schooling	(2) Pr(Compulsory school)	(3) Pr(High school)	(4) Pr(Vocational degree)	(5) Pr(Academic degree)	(6) Pr(Employed at 26)	(7) Earnings at 26-30
Realized Opportunity	0.071	-0.012	0.001	0.011*	0.002	0.020***	0.863***
	(0.049)	(0.007)	(0.004)	(0.006)	(0.007)	(0.005)	(0.196)
Observations	10,454	10,565	10,565	10,565	10,565	10,467	10,474
R-squared	0.172	0.124	0.021	0.028	0.092	0.064	0.082
Clusters	212	212	212	212	212	213	213
Mean dep. var	12.32	0.393	0.104	0.353	0.139	0.691	19.65

*Notes: This table presents regression estimates of child outcomes—education, employment, and income—on Realized Opportunity in the municipality of assignment for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Child and parental characteristics are measured at arrival; parental characteristics refer to the first-arriving parent. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. Column-specific sample sizes reflect outcome-specific data availability and the age at which the outcome is measured. Education is measured at age 30; employment is defined as having positive labor income at age 26; and earnings at ages 26–30 are calculated as average labor income over those years and are measured in thousands of USD. Robust standard errors, clustered at the municipality level, are reported in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10.*

Table 4: Effects of Opportunity on Crime

	Pr(crime)				Number of crimes			
Dependent var.:	(1) All crime	(2) Property crime	(3) Violent crime	(4) Other crime	(5) All crime	(6) Property crime	(7) Violent crime	(8) Other crime
Realized Opportunity	-0.005	-0.007	-0.007	0.001	-0.113***	-0.058***	-0.014	-0.041
	(0.007)	(0.006)	(0.004)	(0.003)	(0.042)	(0.020)	(0.010)	(0.025)
Observations	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415
R-squared	0.194	0.116	0.125	0.036	0.135	0.081	0.097	0.121
Clusters	215	215	215	215	215	215	215	215
Mean dep. var	0.365	0.260	0.131	0.0643	1.637	0.683	0.251	0.703

*Notes: This table presents regression estimates of child criminal conviction outcomes on Realized Opportunity in the municipality of assignment for refugee children who arrived in Denmark between 1986 and 1998 at ages 0–17, immigrated within one year of their parents' arrival, and were subject to the dispersal policy. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Child and parental characteristics are measured at arrival; parental characteristics refer to the first-arriving parent. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. Column-specific sample sizes reflect outcome-specific data availability and the age at which the outcome is measured. Crime outcomes are measured up to and including age 30. Robust standard errors, clustered at the municipality level, are reported in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Table 5: Effects of Opportunity on Family Formation and Fertility

Dependent var.:	(1) Pr(Teen parenthood)	(2) Pr(Teen marriage)	(3) Pr(Parent at 30)	(4) Pr(Partnered at 30)	(5) Pr(Divorced at 30)	(6) Pr(Partnered with Dane at 30)
Realized Opportunity	-0.003*	-0.002	-0.015***	0.003	0.001	0.003
	(0.002)	(0.003)	(0.006)	(0.005)	(0.003)	(0.003)
Observations	12,415	12,166	12,042	11,009	11,009	11,009
R-squared	0.056	0.118	0.135	0.072	0.026	0.048
Clusters	215	214	215	212	212	212
Mean dep. var	0.0322	0.0574	0.465	0.515	0.0540	0.112

*Notes: This table presents regression estimates of family formation outcomes on Realized Opportunity in the municipality of assignment for refugee children who arrived in Denmark between 1986 and 1998 at ages 0–17, immigrated within one year of their parents' arrival, and were subject to the dispersal policy. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Child and parental characteristics are measured at arrival; parental characteristics refer to the first-arriving parent. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. Column-specific sample sizes reflect outcome-specific data availability and the age at which the outcome is measured. Teen parenthood is defined as becoming a parent at age 19 or earlier; teen marriage is defined analogously. Robust standard errors, clustered at the municipality level, are reported in parentheses. Significance levels: *** $p<0.01$, ** $p<0.05$, * $p<0.10$.*

Table 6: Correlation of Realized Opportunity with Other Municipality Characteristics

Specification:	Bivariate	Multivariate	Post-LASSO OLS	Variable Mean / S.D.
	(1)	(2)	(3)	(4)
Dependent var.:	Realized Opportunity	Realized Opportunity	Realized Opportunity	
log(Pop.)	-1.263***	-0.191	-0.206*	10.902
	(0.100)	(0.157)	(0.108)	[1.224]
% non-Western immig.	-0.611***	-0.137		2.506
	(0.159)	(0.094)		[1.797]
% co-nationals	-2.601***	-0.167		0.289
	(0.432)	(0.172)		[0.235]
% with high-school	-0.063	-0.031		59.097
	(0.047)	(0.042)		[6.872]
Employment rate	0.435***	0.201***	0.163***	77.204
	(0.028)	(0.058)	(0.046)	[4.728]
Mean earnings	0.440***	0.280***	0.169***	28.806
	(0.061)	(0.081)	(0.038)	[3.638]
Poverty rate	-0.346***	0.126***		20.934
	(0.035)	(0.036)		[5.163]
Gini coefficient	-0.646***	-0.295***	-0.297***	33.726
	(0.090)	(0.070)	(0.063)	[2.683]
Crime rate	-0.339	0.174		2.522
	(0.354)	(0.154)		[0.744]
% teen births	-0.256	0.089		1.826
	(0.188)	(0.083)		[0.995]
Observations	12,415	12,415	12,415	12,415
Municipalities	215	215	215	215
Mean dep. var	49.13	49.13	49.13	
R-squared		0.698	0.683	
F-stat		43.83	84.91	
$Pr > F$		0.00	0.00	

*Notes: This table presents regression estimates of Realized Opportunity on municipality-level characteristics in the main sample of refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2). Realized Opportunity is measured with empirical Bayes shrinkage. Column (1) reports coefficients from bivariate regressions, and column (2) reports results from a multivariate regression. Column (3) presents results from a post-LASSO OLS procedure, in which LASSO is first used to select the most predictive variables, with the Bayesian Information Criterion (BIC) used to determine the penalty parameter, λ. The selected variables are then included in a subsequent OLS specification. Column (4) reports the mean and standard deviation of each municipality characteristic. The regressions include controls for year-of-arrival fixed effects. Robust standard errors, clustered at the municipality level, are reported in parentheses beneath each regression coefficient. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Table 7: Horse Race Regressions – Income Rank

Dependent var.:	(1) Income rank at 26-30	(2) Income rank at 26-30	(3) Income rank at 26-30	(4) Income rank at 26-30	(5) Income rank at 26-30	(6) Income rank at 26-30	(7) Income rank at 26-30	(8) Income rank at 26-30	(9) Income rank at 26-30	(10) Income rank at 26-30	(11) Income rank at 26-30
Realized Opportunity	1.447***	0.980***	1.768***	1.424***	1.417***	1.299***	0.964**	0.663*	0.521	1.532***	1.504***
	(0.327)	(0.363)	(0.352)	(0.333)	(0.310)	(0.477)	(0.417)	(0.391)	(0.443)	(0.286)	(0.320)
log(Pop.)		-0.720									
		(0.466)									
% non-western immig.			0.931**								
			(0.465)								
% co-nationals				-0.109							
				(0.401)							
% with high-school					-0.241						
					(0.482)						
Employment rate						0.215					
						(0.529)					
Mean earnings							0.762				
							(0.481)				
Poverty rate								-1.206**			
								(0.569)			
Gini index									-1.365***		
									(0.440)		
Crime rate										0.780**	
										(0.317)	
% teen births											0.583
											(0.417)
Observations	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474
R-squared	0.103	0.104	0.104	0.103	0.103	0.103	0.104	0.104	0.104	0.104	0.104
Clusters	213	213	213	213	213	213	213	213	213	213	213
Mean dep. var	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79

Notes: This table presents regression estimates of child income rank (averaged over ages 26–30) on Realized Opportunity and other municipality-level characteristics for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. Column-specific sample sizes reflect outcome-specific register availability and the age at which the outcome is measured. All explanatory variables are measured at the time of arrival. All regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Randomized to Opportunity: Measuring Place Effects for Refugee Children

Online Appendix

N. Meltem Daysal
University of Copenhagen, CEBI, CESifo, IZA-LISER, RFBerlin

Joseph J. Doyle, Jr.
MIT Sloan School of Management, NBER

Naeim S. Samandari
University of Copenhagen, CEBI, ROCKWOOL Foundation

Emil A. L. Simonsen
University of Copenhagen

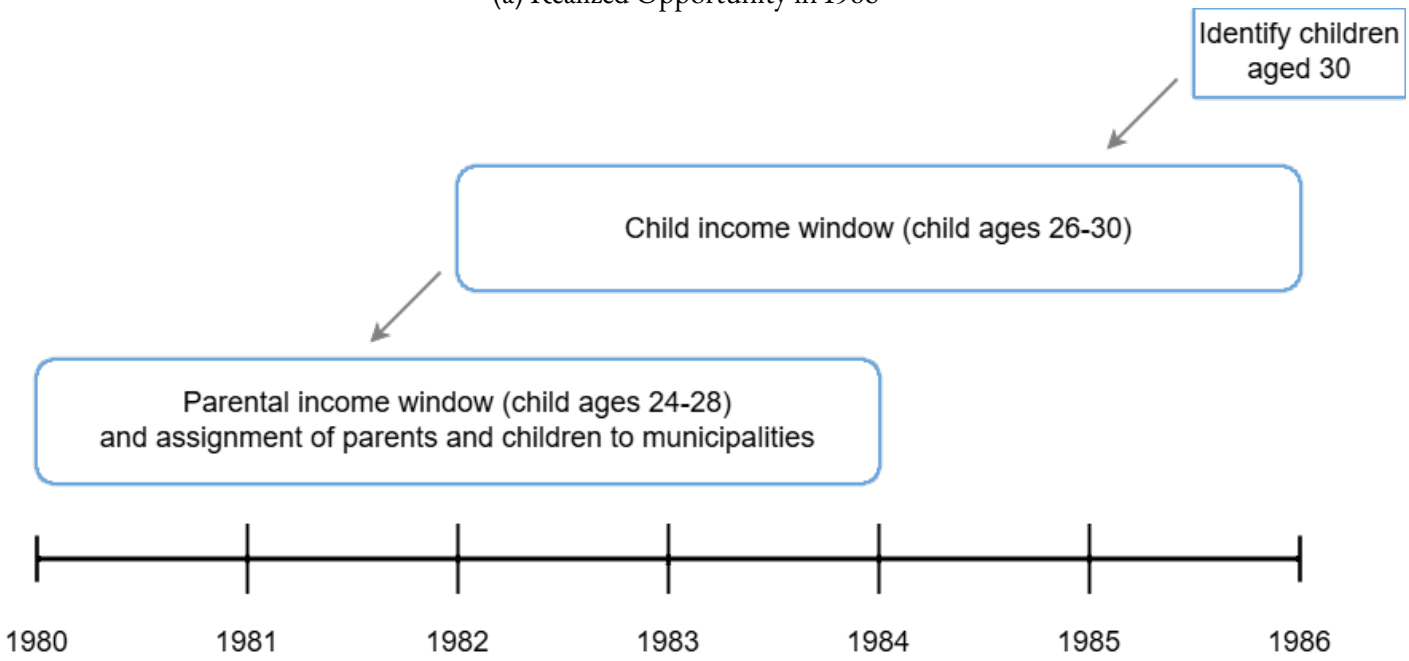
Mircea Trandafir
ROCKWOOL Foundation, IZA-LISER

Appendix A

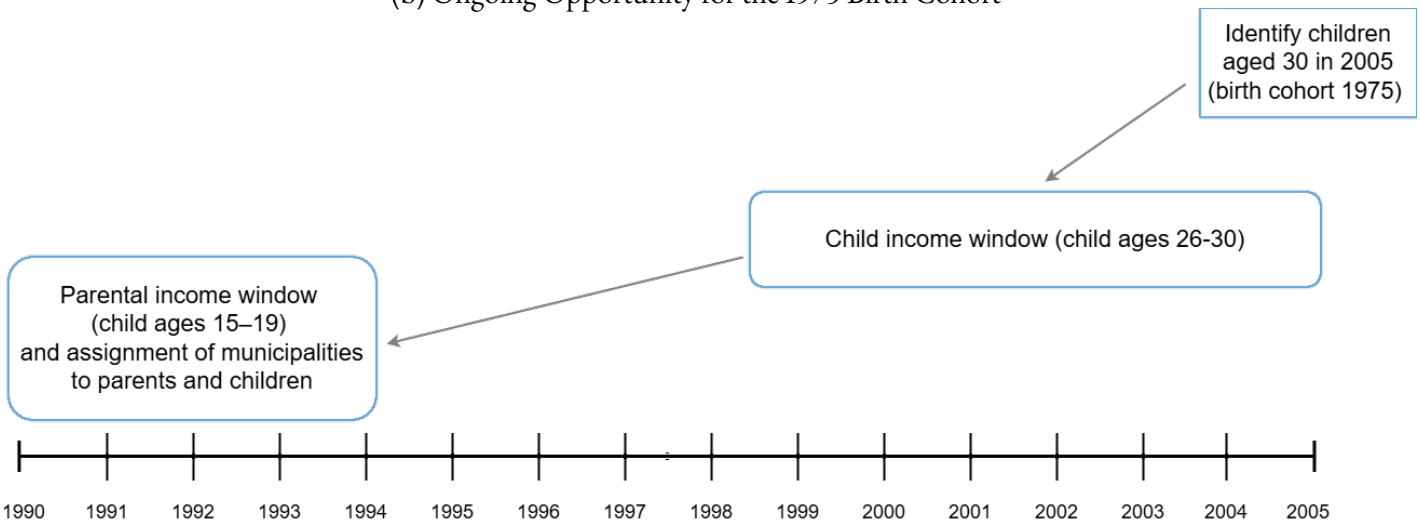
Descriptive Statistics on Measures of Economic Opportunity

Appendix Figure A1: Timeline Graphs Illustrating the Definitions of Realized and Ongoing Opportunity

(a) Realized Opportunity in 1986

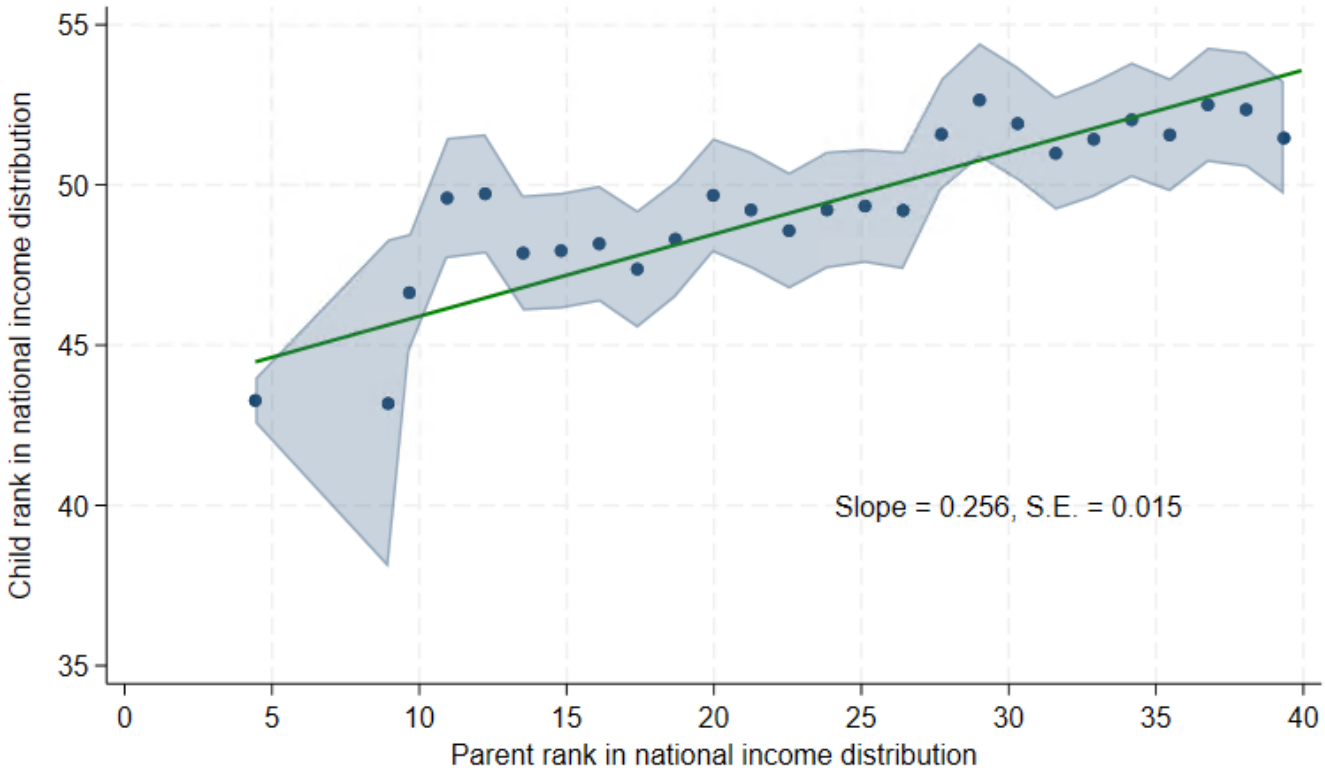


(b) Ongoing Opportunity for the 1975 Birth Cohort



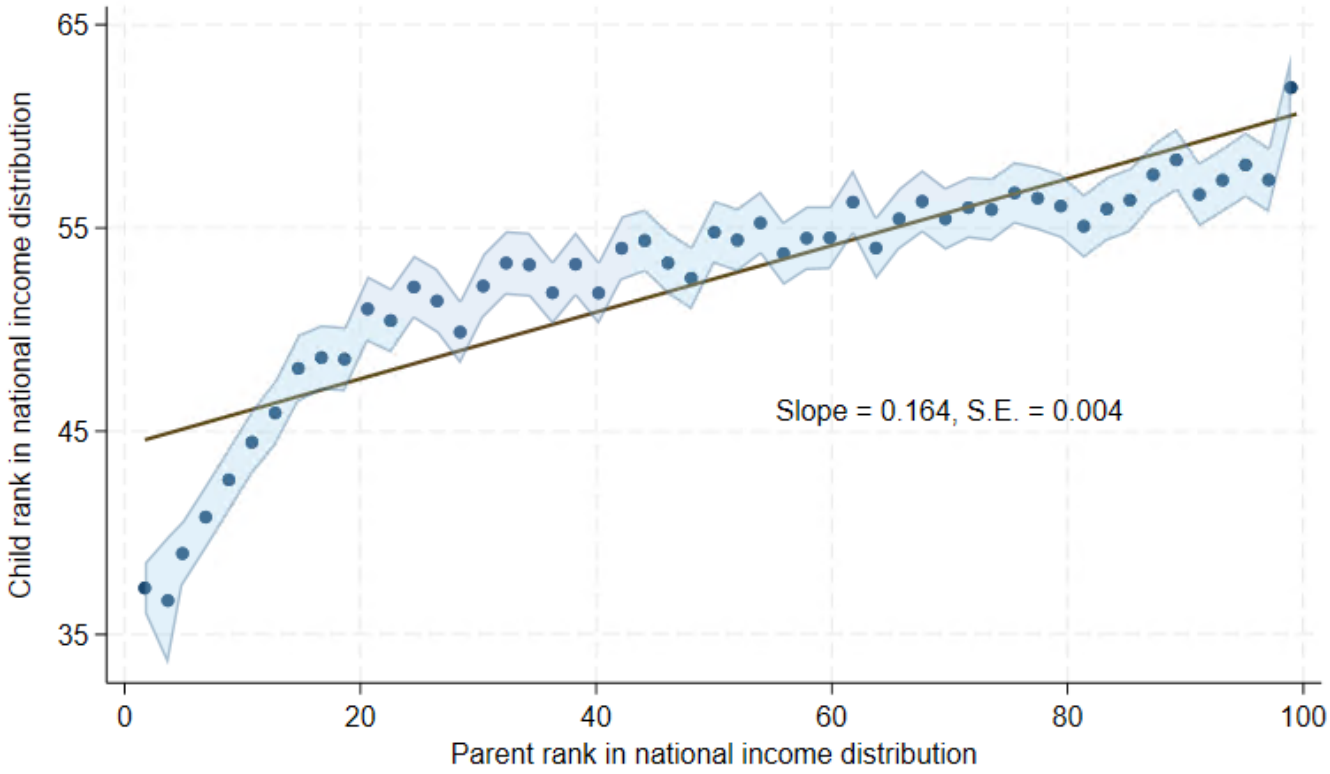
Notes: This figure illustrates the timelines used to define Realized and Ongoing Opportunity. Panel (a) shows how Realized Opportunity is estimated using data for 1986. Panel (b) shows how Ongoing Opportunity is estimated for the 1975 birth cohort, which turned 30 in 2005. The two measures are estimated analogously for other years and birth cohorts, respectively.

Appendix Figure A2: Income Rank vs. Parental Income Rank (40th Percentile or Below)



Notes: This figure presents a binned scatterplot of the relationship between parental income rank and child income rank in the national income distribution for sons and daughters in Denmark who were aged 30 in 1992 and whose parents were at or below the 40th percentile of the national income distribution. Child income is measured as labor income averaged over ages 26–30. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 24–28. The sample is restricted to "permanent residents," defined as parents who lived in the same municipality throughout the parental income-averaging period. We implement the binned-scatter method of Cattaneo et al. (2024) with IMSE-optimal bins and 95 percent confidence bands that capture uncertainty in the estimated conditional mean function. The figure also overlays the OLS fit to the individual-level data; the reported standard error for the slope is heteroskedasticity-robust.

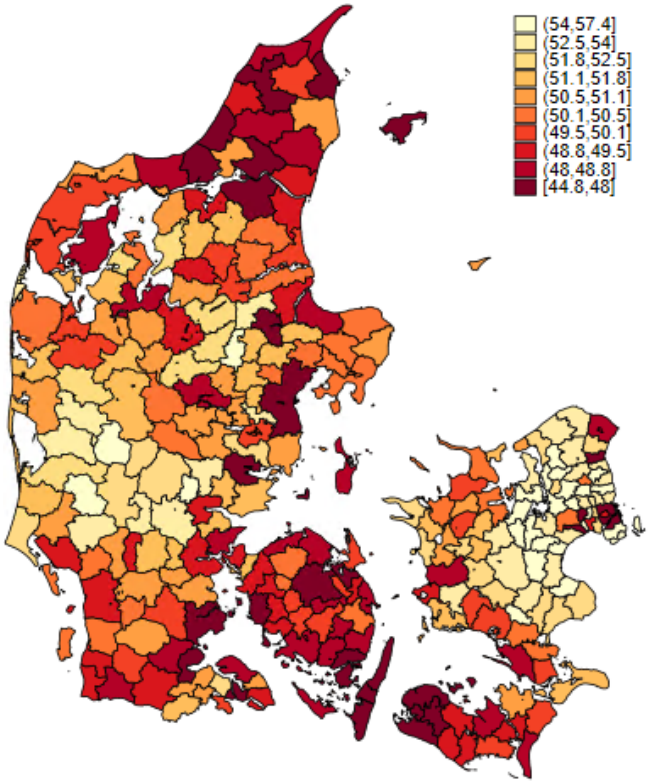
Appendix Figure A3: Child versus Parental Income Rank for Permanent Residents, Ongoing Opportunity



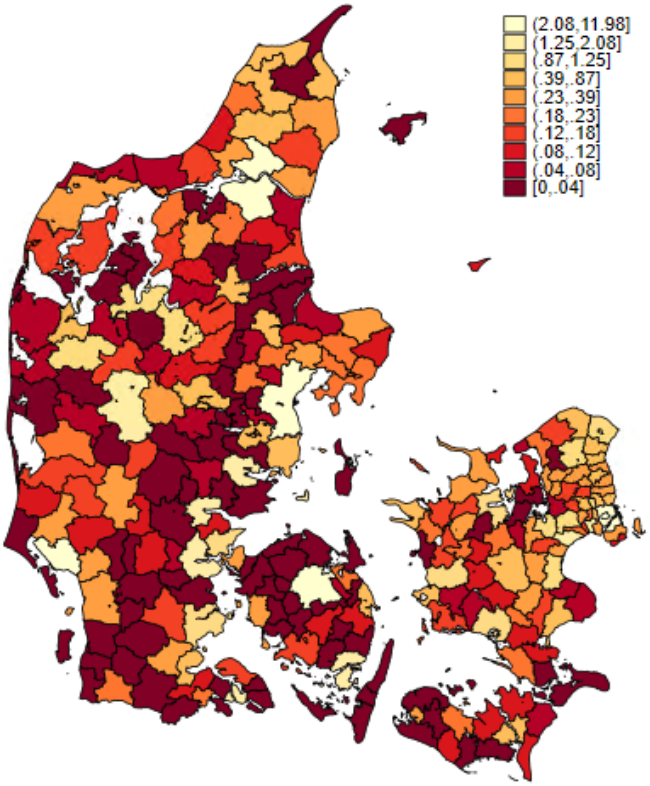
Notes: This figure presents a binned scatterplot of the relationship between parental rank and child rank in the national income distribution for sons and daughters in Denmark born in 1991. Child income is measured as labor income averaged over ages 26–30. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 15–19. The sample is restricted to "permanent residents," defined as parents who lived in the same municipality throughout their income-averaging period. We implement the binned-scatter method of Cattaneo et al. (2024) with IMSE-optimal bins and 95 percent confidence bands capturing uncertainty in the estimated conditional mean function. The figure also overlays the OLS fit to the individual-level data; the reported standard error for the slope is heteroscedasticity-robust.

Appendix Figure A4: Mean Income Ranks for Children of Permanent Residents and Share of Refugees

(a) Parents at the 25th percentile

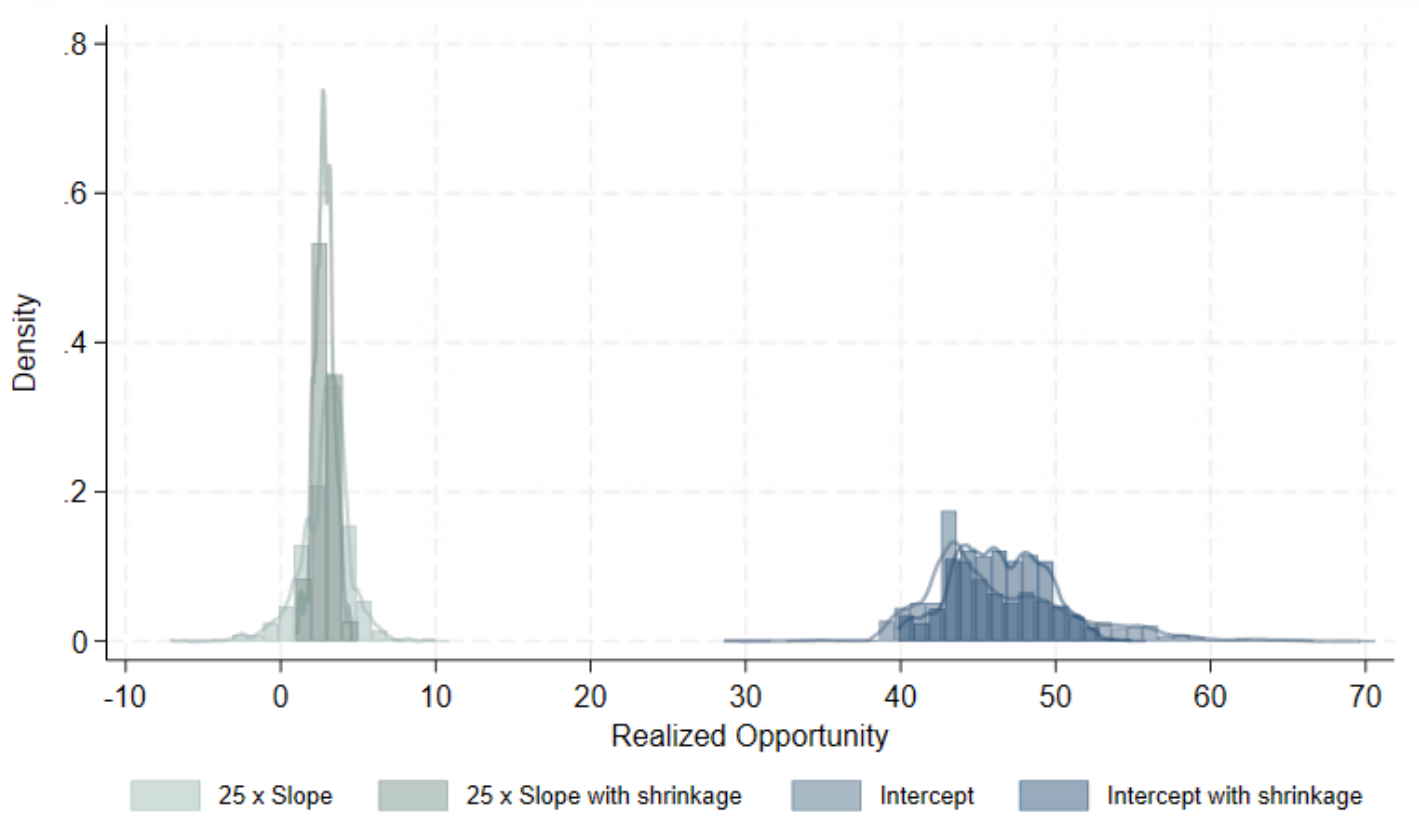


(b) Pct. of refugee children dispersed



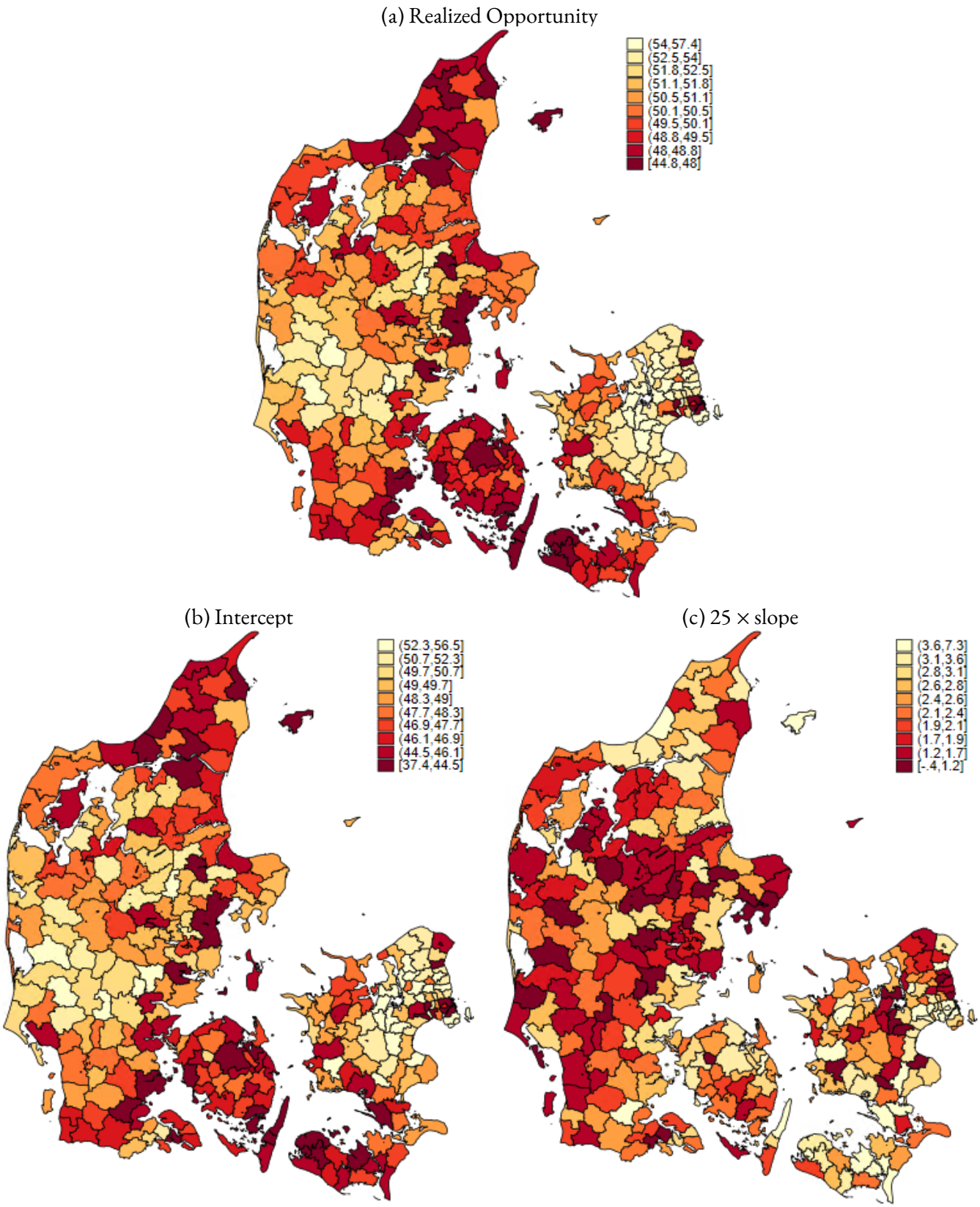
Notes: This figure displays two maps of Denmark's municipalities under the pre-2007 municipal boundaries. Panel (a) shows the predicted national income rank, averaged over 1986–1998, at ages 26–30 for children whose parents were at the 25th percentile of the national income distribution ("Realized Opportunity"). Panel (b) shows, by municipality, the share of refugee children aged 0–17 at arrival who immigrated within one year of their parents' arrival and were subject to Denmark's 1986–1998 dispersal policy. The unweighted correlation between the two variables is -0.24.

Appendix Figure A5: Histograms of the Slope and Intercept from Realized Opportunity



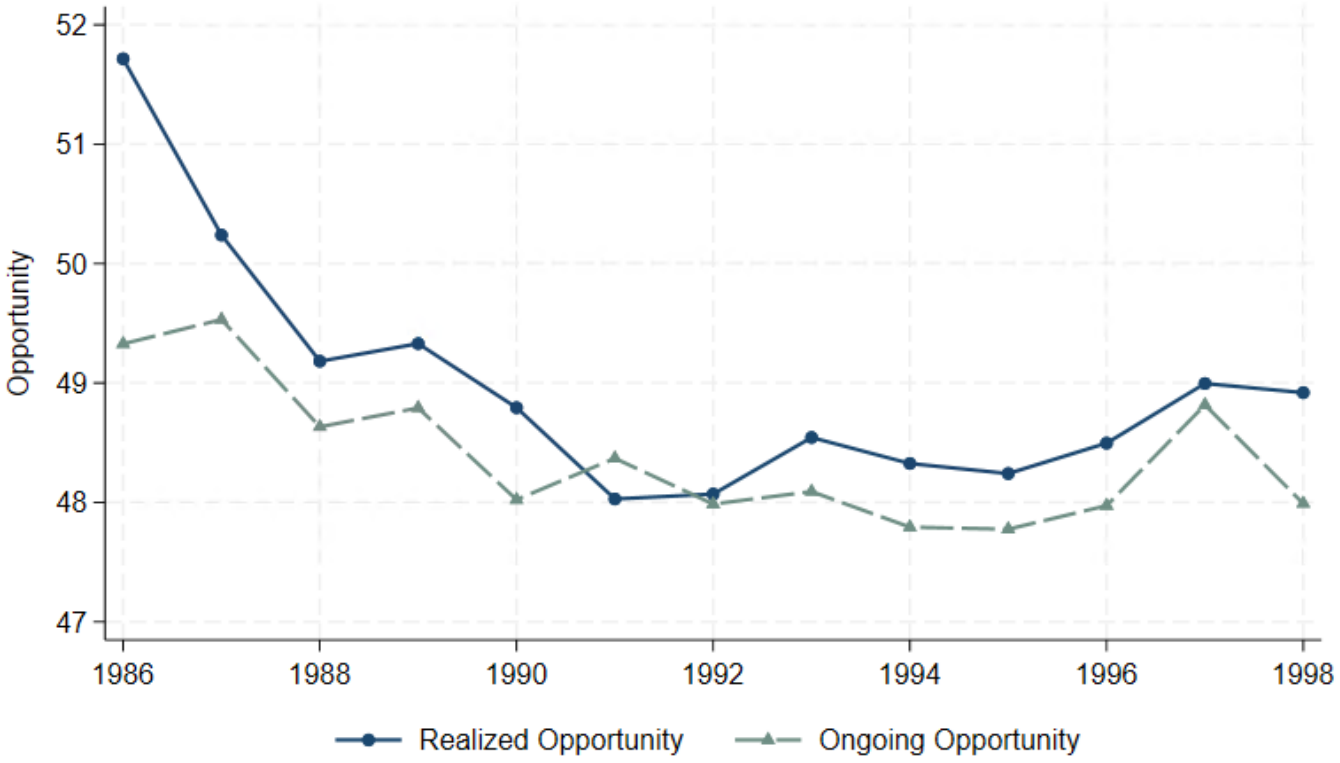
Notes: This figure presents histograms with kernel density overlays of the slope and intercept from Realized Opportunity, without and with empirical Bayes shrinkage, in the sample of refugee children whose parents arrived in Denmark during 1986–1998 and were subject to the dispersal policy. The sample is restricted to refugee children who were aged 0–17 at arrival.

Appendix Figure A6: Maps of Realized Opportunity Decomposed into Intercept and Slope



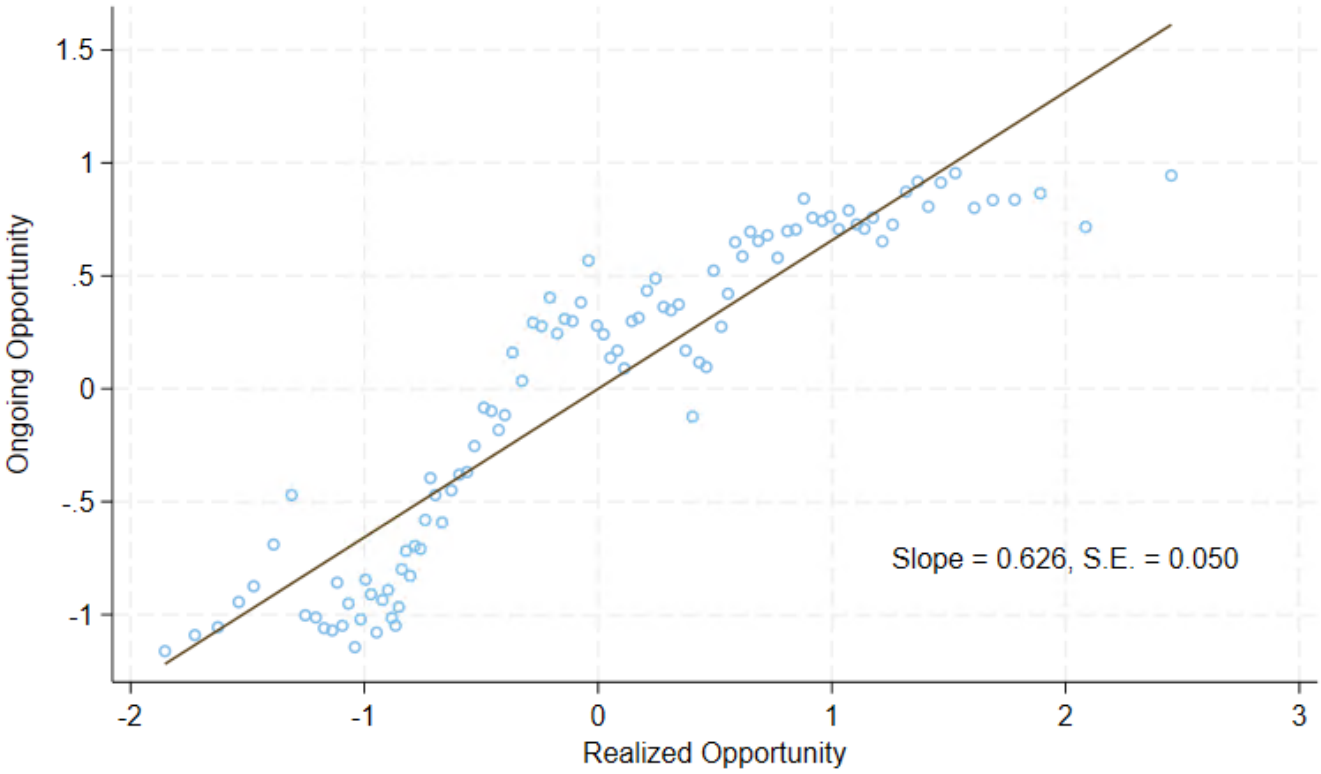
Notes: This figure displays three maps of Denmark's municipalities. Panel (a) shows the geographic distribution of Realized Opportunity, averaged over 1986–1998 ($\hat{y}_{25,ms} = \hat{\alpha}_{ms} + 25\,\hat{\psi}_{ms}$). Panel (b) shows, by municipality, the intercept ($\hat{\alpha}_{ms}$), and panel (c) shows 25 times the slope ($25\,\hat{\psi}_{ms}$), both from Realized Opportunity. The unweighted correlation between the slope and intercept is −0.692, and the population-weighted correlation is −0.715. All measures in this figure are estimated without empirical Bayes shrinkage.

Appendix Figure A7: Realized and Ongoing Opportunity Over Time



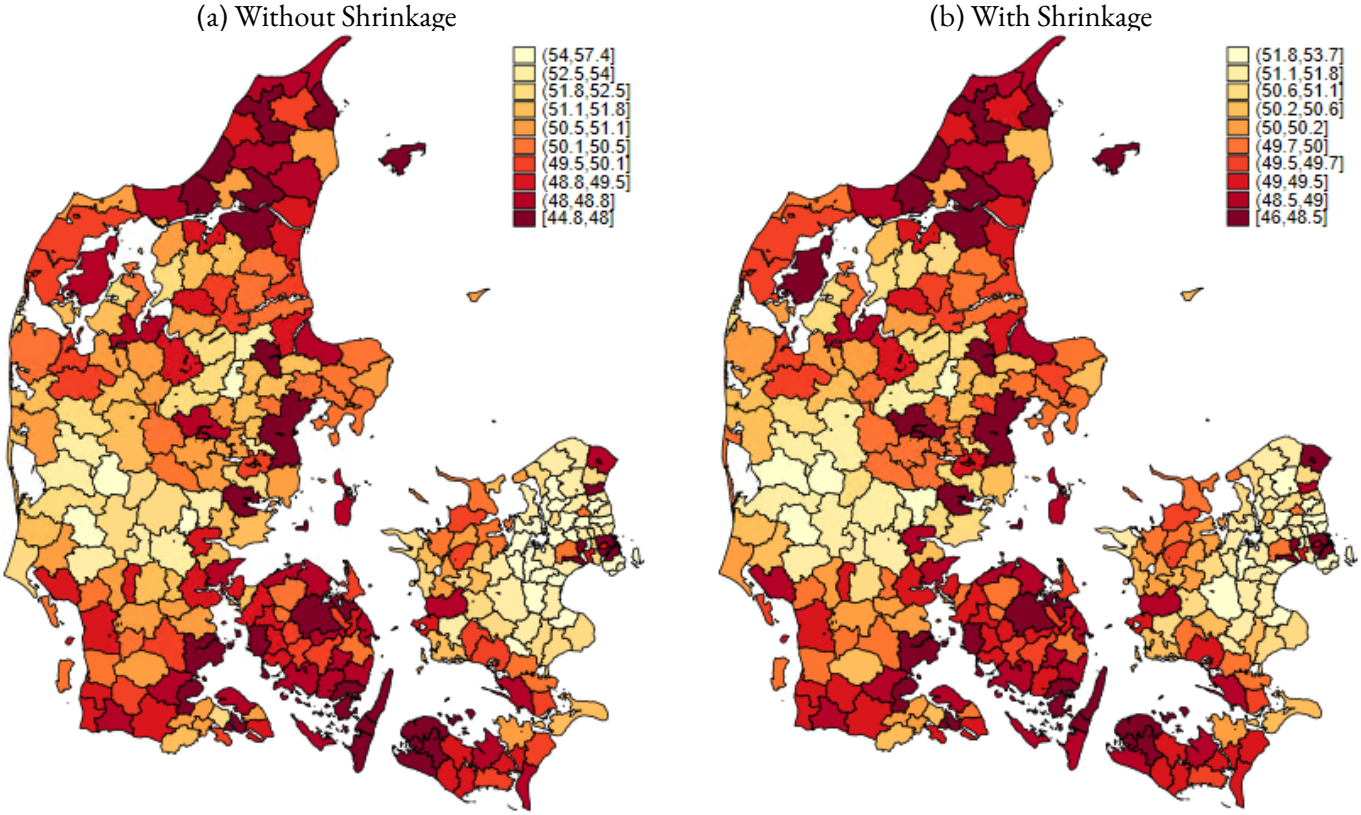
Notes: This figure shows developments in Realized and Ongoing Opportunity across arrival cohorts of refugee children aged 0–17 at arrival whose parents received asylum in Denmark between 1986 and 1998 and who were subject to the dispersal policy. Each dot shows the average value of the Opportunity measure for refugee children in the corresponding arrival cohort. Both measures in this figure are estimated without empirical Bayes shrinkage.

Appendix Figure A8: Association Between Realized and Ongoing Opportunity



Notes: This figure displays a binned scatterplot of the relationship between Realized Opportunity and Ongoing Opportunity, along with a fitted linear regression line, estimated in our sample of refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. The reported standard error for the slope coefficient is clustered at the municipality level. The opportunity measures are standardized to have mean zero and standard deviation 1, and both are estimated with empirical Bayes shrinkage.

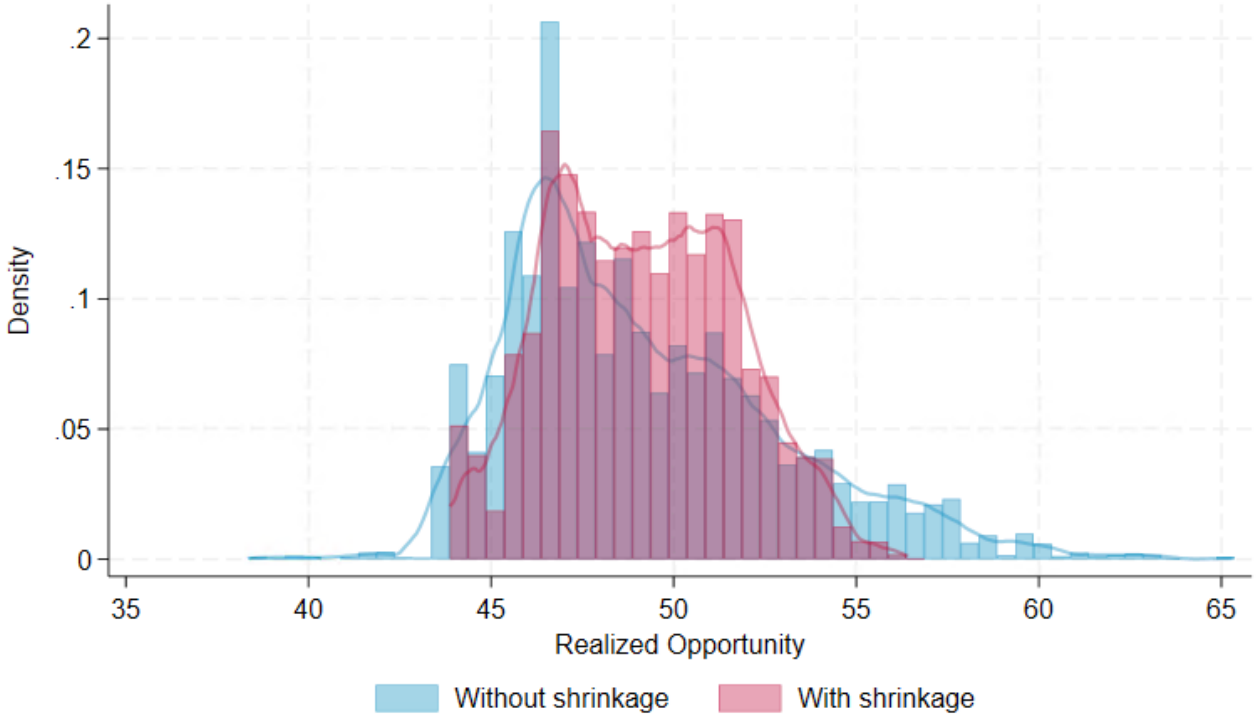
Appendix Figure A9: Maps of Realized Opportunity Without and With Bayesian Shrinkage



Notes: This figure displays two maps of Denmark's municipalities under the pre-2007 municipal boundaries. Panel (a) shows the predicted national income rank at ages 26–30 for children whose parents were at the 25th percentile of the national income distribution, averaged over 1986–1998 (Realized Opportunity). Panel (b) shows the same measure as Panel (a), but with empirical Bayes shrinkage applied to adjust for estimation noise.

Appendix Figure A10: Opportunity Measures Without and With Bayesian Shrinkage

(a) Realized Opportunity



(b) Ongoing Opportunity



Notes: These figures present histograms with kernel density overlays of Realized and Ongoing Opportunity in panels (a) and (b), respectively, without and with empirical Bayes shrinkage, for the sample of refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. The unit of observation is the individual refugee child. With shrinkage, the mean (standard deviation) of Realized Opportunity is 49.12 (2.55); for Ongoing Opportunity, the mean (standard deviation) is 48.81 (3.13). Without shrinkage, the corresponding mean (standard deviation) values for Realized and Ongoing Opportunity are 49.19 (3.75) and 48.53 (4.03), respectively.

Appendix Table A1: Intergenerational Income Rank-Rank Intercept and Slope Estimates Across Countries

Country / Study	Earnings Years	Q–Q Intercept	Q–Q Slope
Denmark	1980-2022	45-46	0.13-0.16
USA (Chetty et al., 2014)	1996-2012	34	0.32-0.35
Netherlands (Beekers, 2024)	1995-2018	34	0.20-0.25
Australia (Deutscher and Mazumder, 2023)	1991-2015	40	0.215
Sweden (Bratberg et al., 2017)	1978-2007	40	0.215
Norway (Bratberg et al., 2017)	1978-2006	40	0.223
Germany (Bratberg et al., 2017)	1984-2012	38	0.245
Canada (Corak, 2020)	1978-2008	38	0.242
Italy (Acciari et al., 2022)	1998-2018	39	0.220
Brazil (Britto et al., Forthcoming)	1991-2019	23	0.539

Notes: This table reports estimates of intercepts and intergenerational rank-rank slopes (Q-Q slopes) across countries. All estimates outside Denmark are drawn from other published studies, with the source for each estimate listed in the table. The estimates for Denmark correspond to the intercept and slope estimates shown in Figure 1 and Appendix Figure A3 in this paper.

Appendix B

Mover-Exposure Design Among Non-Refugees

Appendix Figure B1: Histograms of the Change in Opportunity Measures for Non-Refugee Movers

(a) Realized Opportunity



(b) Ongoing Opportunity



Notes: These figures present histograms with kernel density overlays of Realized and Ongoing Opportunity, shown in panels (a) and (b), respectively, with and without empirical Bayes shrinkage, for the sample of non-refugees aged 0–22 whose parents moved once from one municipality to another before the child turned 23 during 1986–2014 and for whom we observe income rank at ages 26–30. The unit of observation is the individual child. With shrinkage, the mean (standard deviation) change in Realized Opportunity is 0.26 (3.53), and the mean (standard deviation) change in Ongoing Opportunity is 0.43 (4.12). Without shrinkage, the corresponding mean (standard deviation) values for changes in Realized and Ongoing Opportunity are 0.31 (5.47) and 0.43 (5.94), respectively.

Appendix Table B1: Descriptive Statistics for Non-Refugee Movers

	(1) N	(2) Mean	(3) S.D.	(4) p5	(5) p95	(6) Min	(7) Max
Child characteristics							
Age, child	248,214	12.808	6.986	2.000	22.000	1.000	22.000
Male, child	248,214	0.507	0.500	0.000	1.000	0.000	1.000
Parental characteristics							
Age, parent	248,214	42.060	8.611	28.000	56.000	18.000	80.000
Male, parent	248,214	0.933	0.250	0.000	1.000	0.000	1.000
Married, parent	248,214	0.639	0.480	0.000	1.000	0.000	1.000
No. of kids, ages 0-2	248,214	0.211	0.455	0.000	1.000	0.000	4.000
No. of kids, ages 3-17	248,214	1.273	1.095	0.000	3.000	0.000	11.000
Unknown educ., parent	248,214	0.037	0.188	0.000	0.000	0.000	1.000
Basic educ., parent	248,214	0.319	0.466	0.000	1.000	0.000	1.000
Vocational educ., parent	248,214	0.388	0.487	0.000	1.000	0.000	1.000
Academic educ., parent	248,214	0.256	0.437	0.000	1.000	0.000	1.000
Municipality characteristics							
Change in Realized Opportunity	247,801	0.258	3.531	-5.481	6.301	-13.048	13.048
Change in Ongoing Opportunity	227,057	0.431	4.119	-6.591	7.353	-14.470	13.701
Outcomes							
Years of educ., child	240,205	13.298	2.572	9.000	17.000	0.000	20.000
Pr(Compulsory school, child)	245,861	0.199	0.399	0.000	1.000	0.000	1.000
Pr(High school., child)	245,861	0.090	0.287	0.000	1.000	0.000	1.000
Pr(Vocational educ., child)	245,861	0.504	0.500	0.000	1.000	0.000	1.000
Pr(Academic educ., child)	245,861	0.184	0.387	0.000	1.000	0.000	1.000
Employment at 26	238,504	0.854	0.353	0.000	1.000	0.000	1.000
Earnings at 26-30	248,214	28.652	19.260	0.004	58.458	0.000	200.000
Child income rank	248,214	52.260	27.957	6.204	95.325	2.783	99.501
Parent income rank	236,910	51.207	30.212	0.000	96.201	0.000	99.491
Pr(Crime conviction by 30)	248,214	0.186	0.389	0.000	1.000	0.000	1.000
Pr(Property offense by 30)	248,214	0.119	0.324	0.000	1.000	0.000	1.000
Pr(Violent assault by 30)	248,214	0.039	0.194	0.000	0.000	0.000	1.000
Pr(Other crime by 30)	248,214	0.051	0.220	0.000	1.000	0.000	1.000
No. of convictions by 30	248,214	0.531	1.978	0.000	3.000	0.000	41.000
No. of property offenses by 30	248,214	0.259	1.145	0.000	1.000	0.000	27.000
No. of violent assaults by 30	248,214	0.062	0.388	0.000	0.000	0.000	11.000
No. of other crimes by 30	248,214	0.209	0.906	0.000	1.000	0.000	22.000

Notes: This table reports descriptive statistics for the sample of non-refugees aged 0–22 whose parents moved once from one municipality to another before the child turned 23 during 1986–2014, and for whom we observe income rank at ages 26–30. The table presents the mean, standard deviation, 5th and 95th percentiles, and the minimum and maximum values for child and parental characteristics, as well as changes in Realized and Ongoing Opportunity between origin and destination municipalities (with shrinkage). It also reports child income rank measured at ages 26–30 and parental income rank measured when the child was aged 24–28. In this table, to comply with Statistics Denmark's disclosure requirements, parental ages are bottom- and top-coded at 18 and 80, respectively; earnings at ages 26–30 are top-coded at 200 thousand USD; and the numbers of criminal convictions, property crimes, violent crimes, and other crimes by age 30 are top-coded at 41, 27, 11, and 22, respectively. These adjustments are applied only to the table and not to the data used in the subsequent analysis.

Appendix C

Additional Refugee Analyses

Appendix C1

Additional Descriptive Statistics

Appendix Table C1: Refugees' Country of Origin

	(1) N	(2) Mean	(3) S.D.
Lebanon	12,415	0.276	0.447
Iraq	12,415	0.225	0.418
Iran	12,415	0.124	0.329
Somalia	12,415	0.096	0.295
Sri Lanka	12,415	0.089	0.284
Vietnam	12,415	0.085	0.279
Afghanistan	12,415	0.054	0.226
Kuwait	12,415	0.010	0.099
Armenia	12,415	0.007	0.083
Ethiopia	12,415	0.006	0.074
Other	12,415	0.028	0.166

Notes: This table shows the ten most common countries of origin, with the remaining countries grouped into "Other," in our sample of refugee children who arrived in Denmark between 1986 and 1998 at ages 0–17, immigrated within one year of their parents' arrival, and were subject to the dispersal policy. The final analysis sample excludes 10 refugee children who would otherwise have been the only individuals in the sample from their respective countries of origin.

Appendix Table C2: Summary Statistics for the Analysis Sample

	(1) N	(2) Mean	(3) S.D.	(4) p5	(5) p95	(6) Min	(7) Max
Child characteristics							
Age, child	12,415	7.416	4.654	1.000	16.000	0.000	17.000
Male, child	12,415	0.543	0.498	0.000	1.000	0.000	1.000
Parental characteristics							
Age, parent	12,415	36.831	8.613	25.000	53.000	18.000	75.000
Male, parent	12,415	0.699	0.459	0.000	1.000	0.000	1.000
Married, parent	12,415	0.907	0.290	0.000	1.000	0.000	1.000
No. of kids, ages 0-2	12,415	0.459	0.627	0.000	2.000	0.000	3.000
No. of kids, ages 3-17	12,415	2.986	1.805	0.000	6.000	0.000	9.000
Unknown educ., parent	12,415	0.427	0.495	0.000	1.000	0.000	1.000
Basic Educ., parent	12,415	0.255	0.436	0.000	1.000	0.000	1.000
Vocational Educ., parent	12,415	0.131	0.338	0.000	1.000	0.000	1.000
Academic educ., parent	12,415	0.187	0.390	0.000	1.000	0.000	1.000
Municipality characteristics							
log(population)	12,415	10.902	1.224	8.994	13.052	8.376	13.102
% non-western immig.	12,415	2.506	1.797	0.495	6.027	0.059	11.813
% co-nationals	12,415	0.289	0.235	0.014	0.724	0.000	5.069
% with high-school	12,415	59.097	6.872	47.134	70.506	38.174	79.420
Employment rate	12,415	77.204	4.728	69.811	85.540	65.125	91.763
Mean earnings	12,415	28.806	3.638	25.011	36.097	22.462	48.674
Poverty rate	12,415	20.934	5.163	11.813	26.786	5.668	32.933
Gini coefficient	12,415	33.726	2.683	29.943	38.447	26.197	39.772
Crime rate	12,415	2.522	0.744	1.389	4.004	0.380	5.387
% teen births	12,415	1.826	0.995	0.113	3.460	0.000	10.658
Realized Opportunity	12,415	49.126	2.545	45.173	53.479	43.870	56.423
Ongoing Opportunity	11,460	48.814	3.134	43.815	53.537	42.273	57.870
Outcomes							
Years of educ., child	10,455	12.316	2.810	9.000	17.000	0.000	20.000
Pr(Compulsory school, child)	10,566	0.393	0.488	0.000	1.000	0.000	1.000
Pr(High school, child)	10,566	0.104	0.305	0.000	1.000	0.000	1.000
Pr(Vocational educ., child)	10,566	0.353	0.478	0.000	1.000	0.000	1.000
Pr(Academic educ., child)	10,566	0.139	0.346	0.000	1.000	0.000	1.000
Pr(Employment at 26)	10,468	0.691	0.462	0.000	1.000	0.000	1.000
Earnings at 26-30	10,475	19.649	18.935	0.000	53.000	0.000	120.000
Child income rank	10,475	38.797	28.180	4.463	92.243	2.981	99.501
Parental income rank	9,737	13.452	17.903	2.958	54.663	2.855	99.461
Pr(Crime conviction by 30)	12,415	0.365	0.481	0.000	1.000	0.000	1.000
Pr(Property offense by 30)	12,415	0.260	0.439	0.000	1.000	0.000	1.000
Pr(Violent assault by 30)	12,415	0.131	0.338	0.000	1.000	0.000	1.000
Pr(Other crime by 30)	12,415	0.064	0.245	0.000	1.000	0.000	1.000
No. of convictions by 30	12,415	1.637	4.041	0.000	10.000	0.000	40.000
No. of property offenses by 30	12,415	0.683	1.916	0.000	4.000	0.000	21.000
No. of violent assaults by 30	12,415	0.251	0.833	0.000	2.000	0.000	9.000
No. of other crimes by 30	12,415	0.703	2.093	0.000	4.000	0.000	22.000
Pr(Teen parenthood by 19)	12,415	0.032	0.177	0.000	0.000	0.000	1.000
Pr(Teen marriage by 19)	12,166	0.057	0.233	0.000	1.000	0.000	1.000
Pr(Parent at 30)	12,042	0.465	0.499	0.000	1.000	0.000	1.000
Pr(Partnered at 30)	11,010	0.515	0.500	0.000	1.000	0.000	1.000
Pr(Divorced at 30)	11,010	0.054	0.226	0.000	1.000	0.000	1.000
Pr(Partnered with Dane at 30)	11,010	0.112	0.316	0.000	1.000	0.000	1.000

Notes: This table reports descriptive statistics for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and whose parents were subject to the dispersal policy. It presents the mean, standard deviation, 5th and 95th percentiles, and minimum and maximum values for child, parental, and municipality characteristics measured at arrival, along with later-life outcomes. Child outcomes are measured at ages 26–30, except teen parenthood, which is measured by age 19; parental income rank is measured when the child was aged 24–28. Crime outcomes are measured up to and including age 30. Parental characteristics are measured for the first-arriving parent. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2). Incomes are deflated to 2015 DKK using the Danish consumer price index and reported in USD at a fixed exchange rate of 7.76 DKK per USD. In this table, but not in the data used in subsequent analyses, percentiles for earnings at ages 26–30 are rounded to the nearest thousand dollars and top-coded at 120 to comply with Statistics Denmark's confidentiality requirements. Likewise, parental age at arrival is bottom- and top-coded at 18 and 75, respectively. The number of children aged 3–17 is top-coded at 9; counts of convictions, property crimes, violent assaults, and other crimes are top-coded at 40, 21, 9, and 22, respectively.

Appendix C2

Additional Balance Checks

Appendix Table C3: Evidence on the Quasi-Random Placement of Refugee Children

Dependent var.:	(1) log(Pop.)	(2) % non-western immig.	(3) % co-nationals	(4) % with high-school	(5) Employment rate	(6) Mean earnings	(7) Poverty rate	(8) Gini index	(9) Crime rate	(10) % teen births	(11) Realized Opportunity	(12) Realized Opportunity
Unobserved at Time of Allocation												
Unknown educ., parent	0.024	0.012	-0.014	-0.035	-0.232	-0.075	0.209	0.065	-0.004	0.060	-0.144	-0.119
	(0.059)	(0.072)	(0.010)	(0.286)	(0.203)	(0.170)	(0.219)	(0.129)	(0.035)	(0.041)	(0.123)	(0.129)
Basic educ., parent	-0.006	0.014	0.004	-0.027	-0.115	-0.038	-0.092	0.102	-0.019	0.016	-0.173	-0.130
	(0.062)	(0.077)	(0.011)	(0.307)	(0.214)	(0.180)	(0.236)	(0.137)	(0.037)	(0.046)	(0.130)	(0.136)
Academic educ., parent	-0.006	0.057	0.004	0.156	-0.031	0.002	0.009	0.025	-0.036	0.037	-0.171	-0.146
	(0.067)	(0.087)	(0.012)	(0.312)	(0.231)	(0.191)	(0.258)	(0.149)	(0.039)	(0.046)	(0.138)	(0.144)
Observed at Time of Allocation												
Age, parent	0.005**	0.004	0.001*	0.017	-0.014*	-0.005	0.010	0.006	0.004**	0.001	-0.004	-0.003
	(0.003)	(0.003)	(0.000)	(0.012)	(0.009)	(0.007)	(0.010)	(0.006)	(0.002)	(0.002)	(0.005)	(0.005)
Male, parent	-0.174***	-0.250***	-0.027***	-0.650***	0.317*	-0.050	-0.377***	-0.161*	-0.093***	-0.022	0.054	0.031
	(0.044)	(0.054)	(0.008)	(0.215)	(0.162)	(0.134)	(0.185)	(0.098)	(0.026)	(0.035)	(0.093)	(0.097)
Married, parent	0.050	0.053	0.025**	-0.387	-0.504**	-0.324*	0.182	0.237*	0.021	0.028	-0.345**	-0.279**
	(0.061)	(0.067)	(0.010)	(0.317)	(0.231)	(0.196)	(0.255)	(0.126)	(0.036)	(0.052)	(0.137)	(0.141)
No. of children, ages 0-2	-0.004	-0.105**	-0.001	-0.145	-0.037	-0.168*	0.083	0.036	-0.030	-0.017	-0.053	-0.047
	(0.038)	(0.043)	(0.005)	(0.183)	(0.127)	(0.102)	(0.140)	(0.082)	(0.021)	(0.028)	(0.072)	(0.076)
No. of children, ages 3-17	-0.037**	-0.051***	-0.005***	-0.053	0.078*	0.016	-0.114***	-0.020	-0.016***	-0.016	0.017	0.004
	(0.015)	(0.016)	(0.002)	(0.066)	(0.047)	(0.039)	(0.054)	(0.031)	(0.007)	(0.010)	(0.027)	(0.028)
Age, child	0.003	0.001	0.000	0.018	-0.009	-0.003	0.014	0.012*	-0.002	0.001	-0.008	-0.007
	(0.003)	(0.004)	(0.001)	(0.015)	(0.011)	(0.009)	(0.012)	(0.007)	(0.002)	(0.002)	(0.006)	(0.007)
Male, child	-0.032	-0.039	-0.007*	-0.101	0.088	0.037	-0.066	-0.092**	0.003	-0.004	0.090**	0.082*
	(0.021)	(0.028)	(0.004)	(0.102)	(0.074)	(0.060)	(0.086)	(0.046)	(0.013)	(0.017)	(0.043)	(0.046)
Observations	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	10,474
R-squared	0.083	0.308	0.302	0.327	0.253	0.122	0.176	0.119	0.126	0.141	0.132	0.139
Households	5555	5555	5555	5555	5555	5555	5555	5555	5555	5555	5555	4950
Unobserved chars.: $Pr > F$	0.916	0.915	0.0710	0.881	0.598	0.955	0.425	0.879	0.710	0.467	0.560	0.747
Observed chars.: $Pr > F$	0.00	0.00	0.00	0.003	0.0550	0.307	0.108	0.035	0.002	0.846	0.067	0.287
Mean of dep. var	10.90	2.506	0.289	59.10	77.20	28.81	20.93	33.73	2.522	1.826	49.13	49.12

*Notes: This table presents regression estimates of municipality-level characteristics on child and parental attributes for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. All variables are measured at the time of arrival. Some were unobserved by the Danish Refugee Council when making allocation decisions about how refugees were dispersed across municipalities, while others were observable. $Pr > F$ is the p-value from the F-test of the joint insignificance of the indicated variable set. The educational-attainment dummies are basic education, academic education, and unknown education, with vocational education as the omitted reference category. Column (12) restricts the sample to children whose income rank at age 30 is observable. All regressions control for year-of-arrival and country-of-origin fixed effects. Standard errors are clustered at the household level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C4: Tests for Selective Attrition

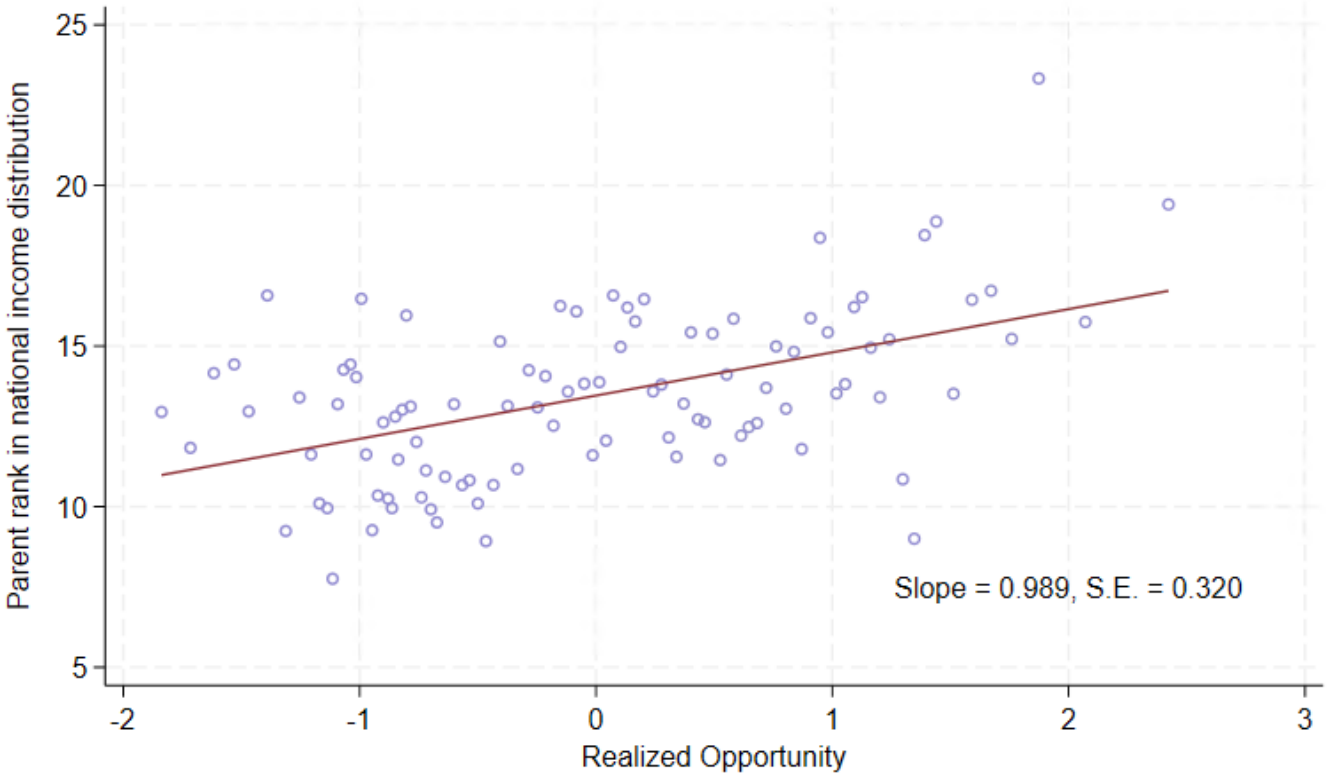
Dependent var.:	(1) No outmigration within 15 years	(2) Observed child rank	(3) Observed parent rank
Realized Opportunity	0.002	-0.005	-0.006
	(0.006)	(0.005)	(0.006)
Child characteristics			
Age, child	-0.007***	0.027***	0.026***
	(0.001)	(0.001)	(0.002)
Male, child	0.052***	0.043***	0.039***
	(0.006)	(0.005)	(0.007)
Observable parental characteristics			
Age, parent	0.001	-0.002***	-0.003***
	(0.001)	(0.000)	(0.001)
Male, parent	-0.024**	0.003	0.021*
	(0.010)	(0.008)	(0.011)
No. of children, ages 0-2	0.006	-0.032***	-0.025***
	(0.006)	(0.008)	(0.009)
No. of children, ages 3-17	0.013***	0.012***	0.013***
	(0.003)	(0.002)	(0.002)
Married	-0.011	-0.017	0.014
	(0.011)	(0.013)	(0.017)
Unobservable parental characteristics			
Unknown educ.	-0.020	-0.015*	-0.015
	(0.016)	(0.009)	(0.011)
Basic educ.	0.005	-0.008	0.005
	(0.017)	(0.009)	(0.011)
Academic educ.	0.002	0.002	0.022
	(0.017)	(0.010)	(0.013)
Observations	15,316	12,415	12,415
R-squared	0.102	0.214	0.149
Clusters	222	215	215
Mean dep. var	0.811	0.844	0.784

*Notes: This table reports regression estimates of indicators for no outmigration within 15 years of arrival, observing child income rank at ages 26–30, and observing parental income rank at ages 24–28 on Realized Opportunity for refugee children who arrived in Denmark between 1986 and 1998 at ages 0–17, immigrated within one year of their parents' arrival, and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and is standardized to have mean zero and standard deviation one. The sample used to estimate effects on the indicators for observing child and parental income ranks is restricted to individuals who did not outmigrate within the first 15 years after arrival, i.e., the main estimation sample. Child and parental characteristics are measured at arrival; parental characteristics refer to the first-arriving parent. All the regressions include year-of-arrival and country-of-origin fixed effects. Robust standard errors, clustered at the municipality level, are shown in parentheses. Significance levels: ***p<0.01, **p<0.05, *p<0.10.*

Appendix C3

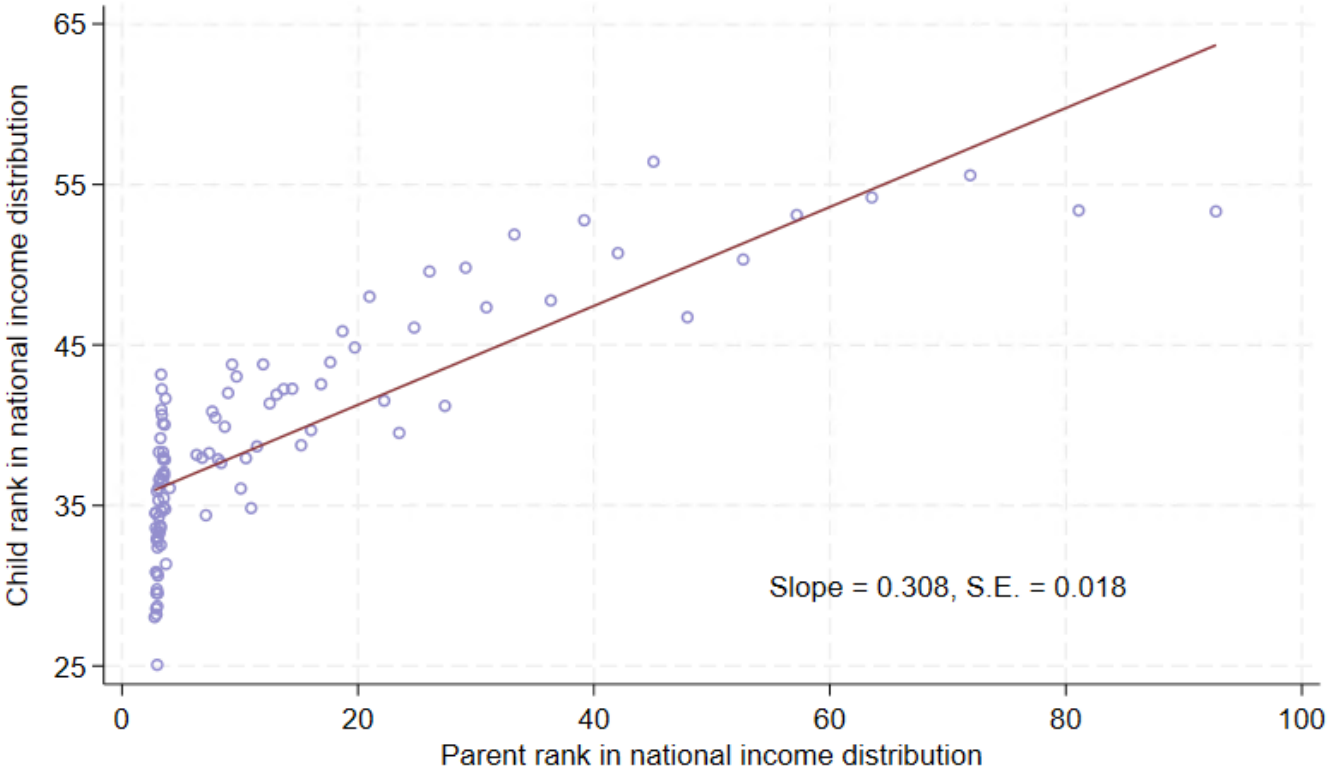
Additional Results on Potential Pathways

Appendix Figure C1: Parent Income Rank versus Realized Opportunity



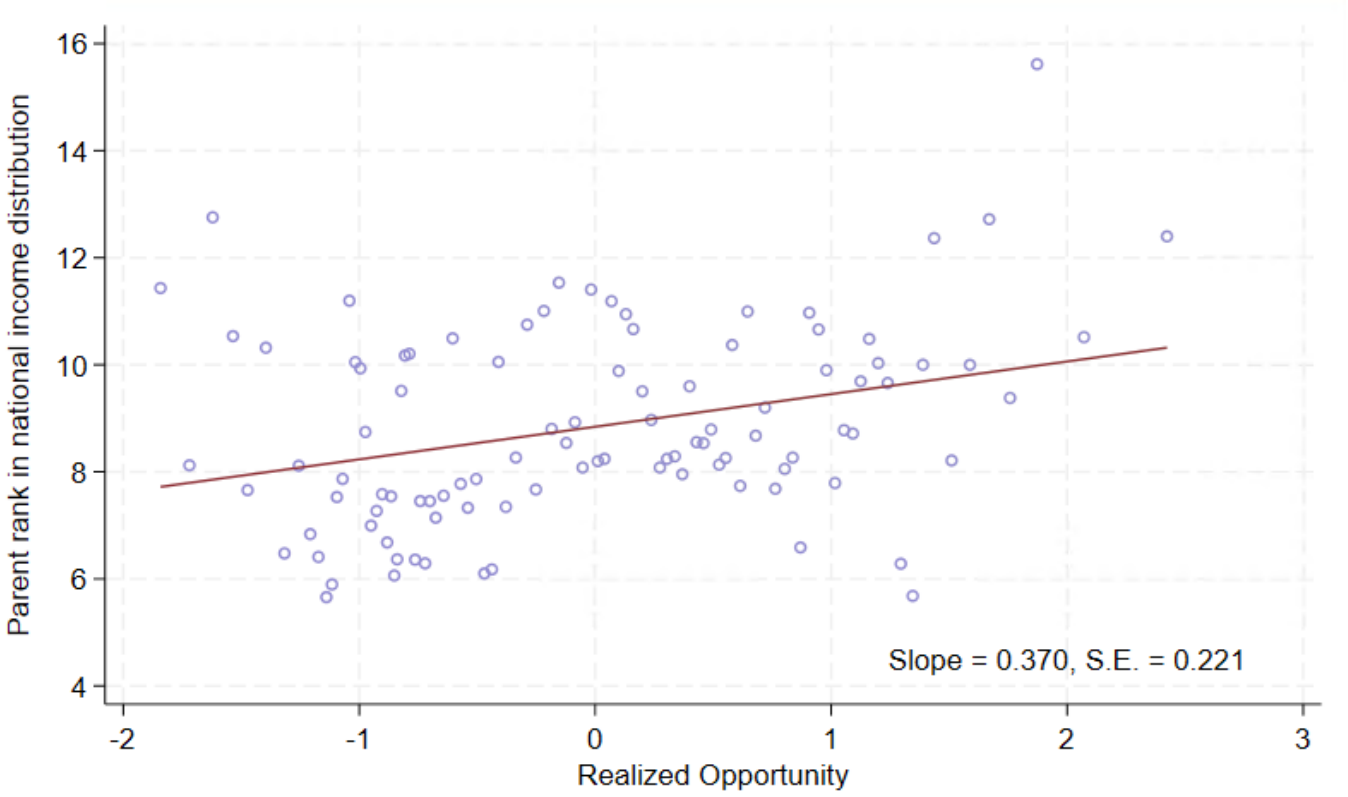
Notes: This figure presents a binned scatterplot of the relationship between Realized Opportunity and parental income rank for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 24–28. The figure plots 100 binned means along with a fitted linear regression line. The fitted line is estimated controlling for year of arrival, country of origin, child characteristics (age at arrival and gender), and parental characteristics (age at arrival, gender of the first-arriving parent, marital status at arrival, number of children at arrival, and education at arrival). Realized Opportunity is standardized and estimated with empirical Bayes shrinkage. The reported standard error for the slope coefficient is clustered at the municipality level.

Appendix Figure C2: Child Income Rank versus Parental Income Rank, Refugee Families



Notes: This figure presents a binned scatterplot of the relationship between parental rank and child rank in the national income distribution for refugee children who arrived in Denmark within one year of their parents' arrival between 1986 and 1998, were aged 0–17 at arrival, and were subject to the dispersal policy. Child income is measured as labor income averaged over ages 26–30. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 24–28. The figure plots 100 binned means along with a fitted linear regression line, estimated controlling for year of arrival. The reported standard error for the slope coefficient is clustered at the household level.

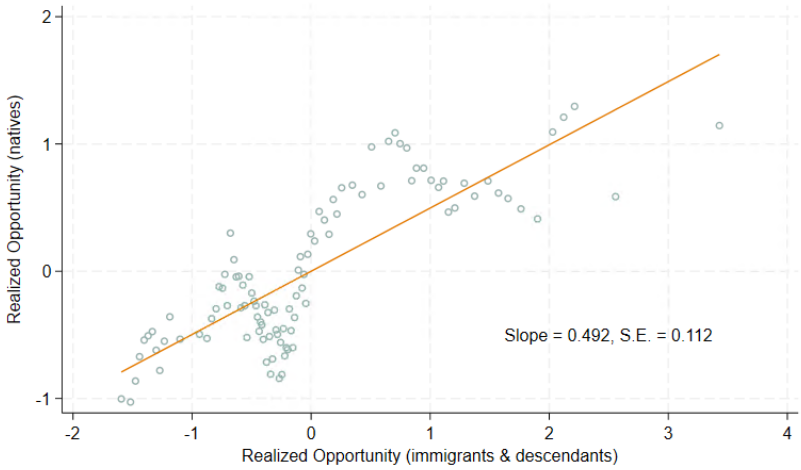
Appendix Figure C3: Parent Income Rank Measured at ages 15-19 versus Realized Opportunity



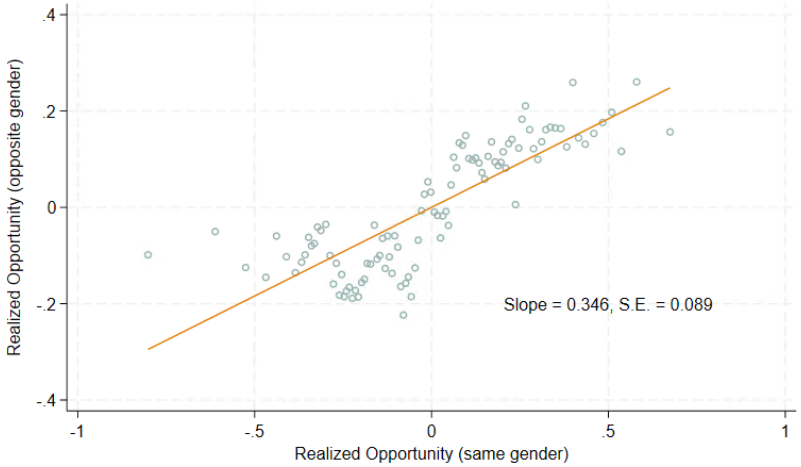
Notes: This figure presents a binned scatterplot of the relationship between Realized Opportunity and parental income rank for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 15–19. The figure plots 100 binned means along with a fitted linear regression line. The fitted line is estimated controlling for year of arrival, country of origin, child characteristics (age at arrival and gender), and parental characteristics (age at arrival, gender of the first-arriving parent, marital status at arrival, number of children at arrival, and education at arrival). Realized Opportunity is standardized and estimated with empirical Bayes shrinkage. The reported standard error for the slope coefficient is clustered at the municipality level.

Appendix Figure C4: Associations Between Opportunity Measures

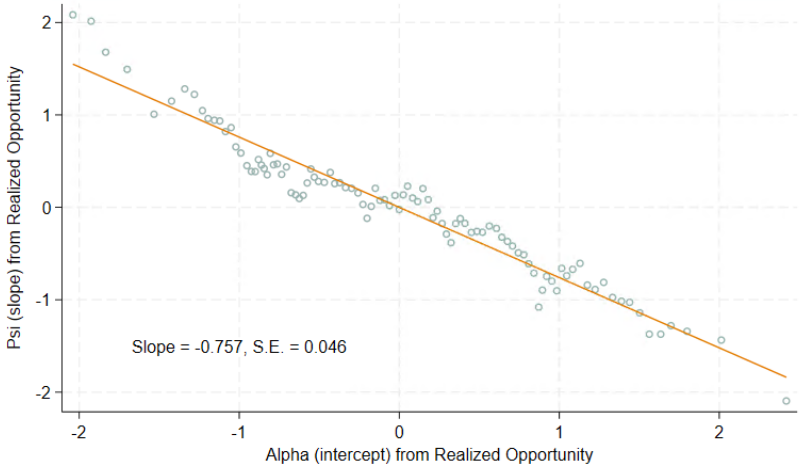
(a) Immigrants and Descendants vs. Natives



(b) Same vs. Opposite Gender



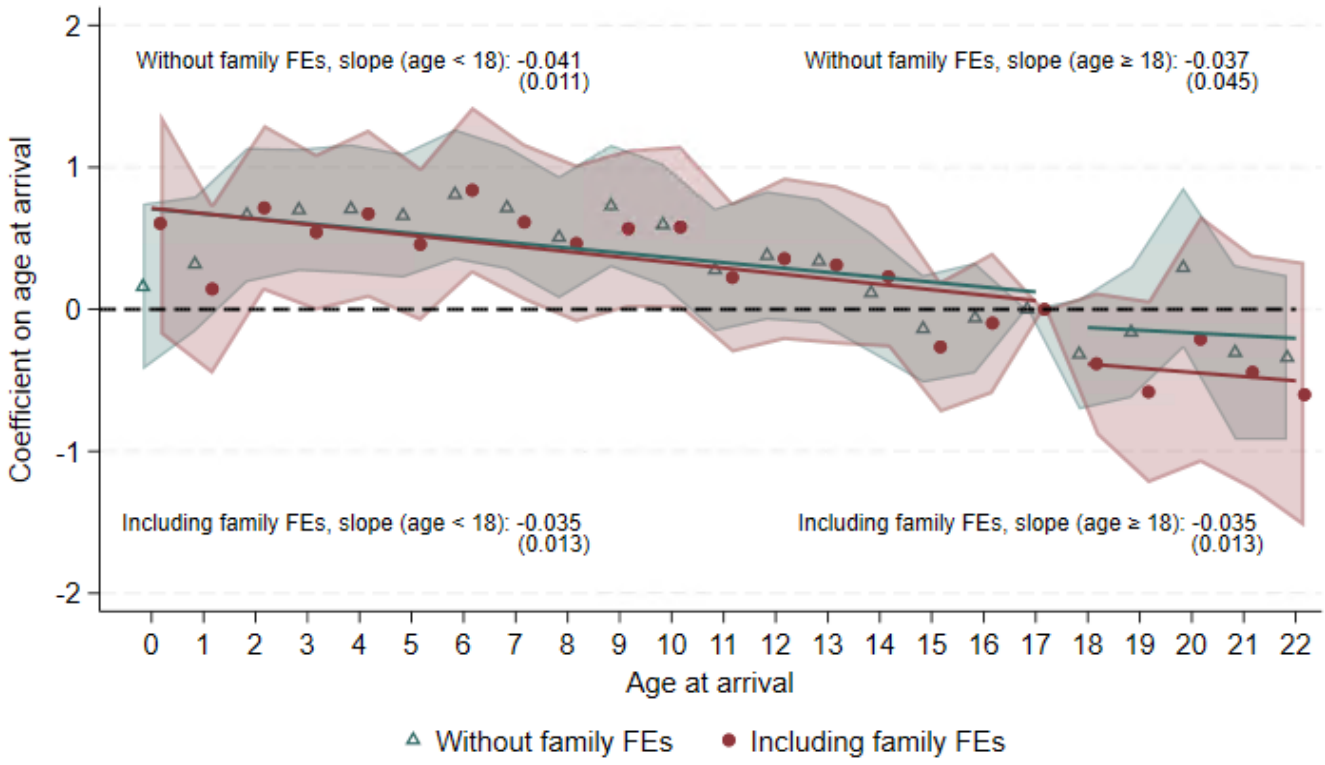
(c) Intercept vs. Slope



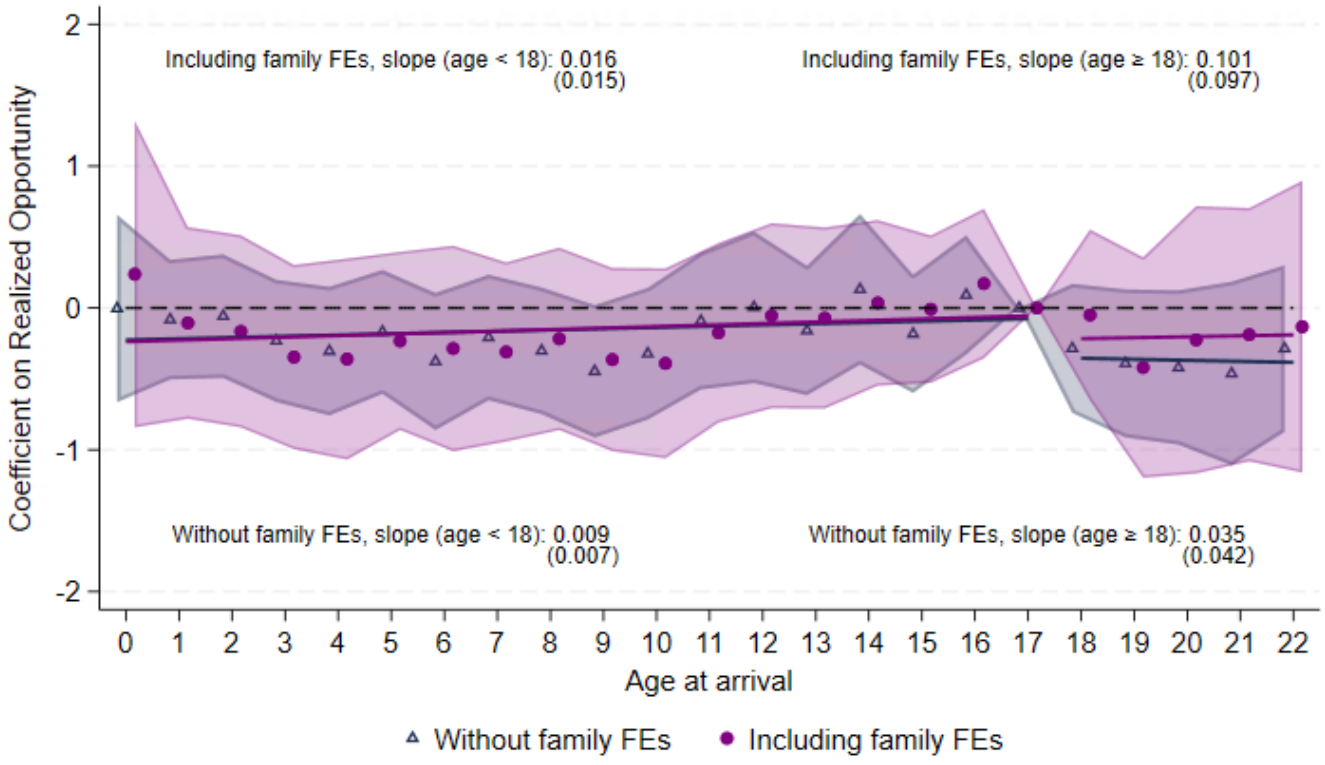
Notes: This figure displays three binned scatterplots with fitted linear regression lines, estimated in the sample of refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Panel (a) shows the relationship between Realized Opportunity estimated for immigrants and descendants and Realized Opportunity for natives. Panel (b) shows the relationship between gender-specific Realized Opportunity estimated for each individual's own gender and Realized Opportunity estimated for the opposite gender. Panel (c) shows the relationship between the slope and intercept from the Realized Opportunity measures. The sample in each figure is restricted to refugees dispersed to municipalities in years for which the given Opportunity measures could be estimated using at least 10 observations. The Opportunity measures are standardized and estimated with empirical Bayes shrinkage. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. The reported standard error for the slope coefficient is clustered at the municipality level.

Appendix Figure C5: Age-at-Arrival and Crime

(a) Number of criminal convictions and Age-at-Arrival



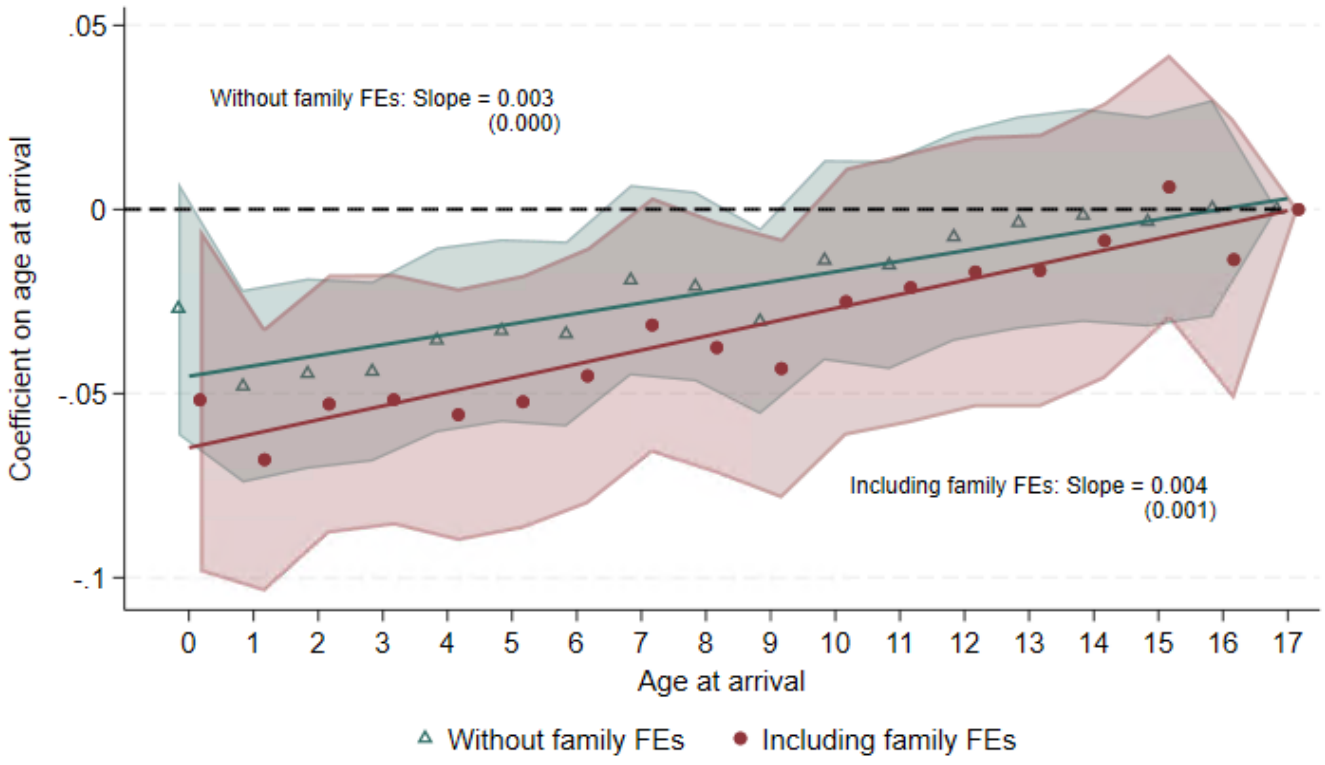
(b) Effects of Realized Opportunity by Age-at-Arrival



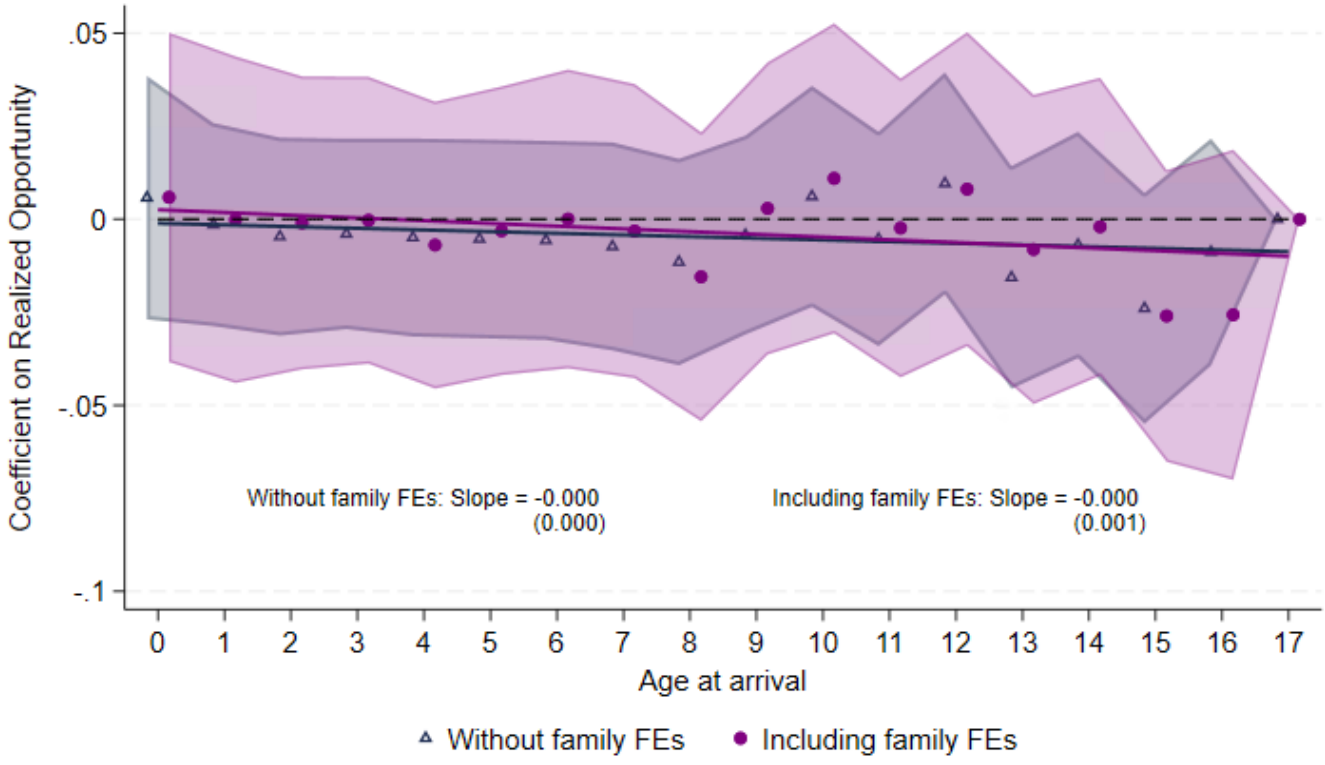
Notes: The estimates are from regressions of the number of criminal convictions measured by age 30 on age at arrival, Realized Opportunity, and other child and parental characteristics. The sample consists of refugee children who arrived in Denmark at ages 0–22 within one year of their parents' arrival during 1986–1998 and were subject to the dispersal policy. Child and parental characteristics are measured at arrival. The coefficient for age 17 is normalized to zero. Panel A reports the coefficients on the age-at-arrival indicators. Panel B reports the coefficients on the interactions between the age-at-arrival indicators and Realized Opportunity. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. The reported slope coefficients and standard errors are estimated using microdata, with age entered as a continuous variable in the regressions. The 95 percent confidence intervals are reported using standard errors clustered at the household level.

Appendix Figure C6: Age-at-Arrival and Teen Parenthood

(a) Probability of teen parenthood and Age-at-Arrival



(b) Effects of Realized Opportunity by Age-at-Arrival



Notes: The estimates are from regressions of the probability of parenthood by age 19 on age at arrival, Realized Opportunity, and other child and parental characteristics. The sample consists of refugee children who arrived in Denmark at ages 0–22 within one year of their parents' arrival during 1986–1998 and were subject to the dispersal policy. Child and parental characteristics are measured at arrival. The coefficient for age 17 is normalized to zero. Panel A reports the coefficients on the age-at-arrival indicators. Panel B reports the coefficients on the interactions between the age-at-arrival indicators and Realized Opportunity. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. The reported slope coefficients and standard errors are estimated using microdata, with age entered as a continuous variable in the regressions. The 95 percent confidence intervals are reported using standard errors clustered at the household level.

Appendix Table C5: Correlations of child outcomes and parental income rank

Dependent var.:	(1) Income rank at 26-30	(2) No. criminal convictions	(3) Pr(Teen parenthood)
Parental income rank	0.308***	-0.020***	-0.001***
	(0.018)	(0.003)	(0.000)
Observations	9,744	9,681	9,681
Mean dep. var	39.26	1.710	0.036

This table presents estimates from regressions of child income rank, criminal convictions, and teenage parenthood on parental income rank for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Child income rank is averaged over ages 26–30, criminal convictions are measured up to and including age 30, and teenage parenthood is measured by age 19. Parental income is defined as the labor income of the primary parental earner, averaged over the years when the child was aged 24–28 in column (1) and over ages 15–19 in the remaining columns. The regressions include controls for year-of-arrival fixed effects. Standard errors are clustered at the household level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

Appendix Table C6: Effects by Age at Arrival

Dependent var.:	(1) Income rank at 26-30	(2) No. criminal convictions	(3) Pr(Teen parenthood)
Panel A: Age 0-5 at arrival			
Realized Opportunity	1.029**	-0.112*	-0.001
	(0.461)	(0.061)	(0.002)
Observations	3,447	5,081	5,081
Mean dep. var	43.47	1.745	0.022
Panel B: Age 6-11 at arrival			
Realized Opportunity	1.515***	-0.229***	-0.003
	(0.483)	(0.066)	(0.002)
Observations	4,284	4,539	4,539
Mean dep. var	39.31	1.763	0.033
Panel C: Age 12-17 at arrival			
Realized Opportunity	1.844***	0.055	-0.009***
	(0.547)	(0.066)	(0.003)
Observations	2,726	2,781	2,781
Mean dep. var	32.10	1.242	0.049
Panel D: Age 18-22 at arrival			
Realized Opportunity	2.824***	-0.204***	
	(1.023)	(0.068)	
Observations	560	568	
Mean dep. var	23.08	0.512	

*Notes: This table shows results from a set of regressions for refugees who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998, and whose parents were subject to the dispersal policy. Panel A shows the effects for refugee children aged 0–5 at arrival, Panel B for those aged 6–11 at arrival, Panel C for those aged 12–17 at arrival, and Panel D for those aged 18–22 at arrival (who arrived with at least one parent). In Panel D, the results for teenage parenthood in column (3) are omitted because of insufficient variation in the outcomes, as these outcomes are primarily measured at ages before arrival. Child income rank is measured at ages 26–30, criminal convictions are measured up to and including age 30, and teenage parenthood is measured by age 19. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C7: Different Dimensions of Realized Opportunity

Sample:	All	All	All	Boys	Girls	All
	(1)	(2)	(3)	(4)	(5)	(6)
Dependent var.:	Income rank at 26-30	Income rank at 26-30	Income rank at 26-30	Income rank at 26-30	Income rank at 26-30	Income rank at 26-30
Realized Opportunity, immig. & desc.	1.475***		0.920**			
	(0.412)		(0.357)			
Realized Opportunity, natives		1.519***	1.127***			
		(0.419)	(0.349)			
Realized Opportunity, Boys				1.000**	0.523	
				(0.485)	(0.384)	
Realized Opportunity, Girls				0.414	1.571***	
				(0.493)	(0.404)	
α from Realized Opportunity						2.735***
						(0.470)
ψ from Realized Opportunity						1.999***
						(0.447)
Observations	3,505	3,505	3,505	5,820	4,640	10,474
R-squared	0.130	0.130	0.131	0.070	0.166	0.104
Clusters	31	31	31	208	200	213
Mean dep. var	37.23	37.23	37.23	41.84	34.93	38.79

*Notes: This table reports regression estimates of child income rank (averaged over ages 26–30) on municipality-level measures of Opportunity for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and is standardized to have mean zero and standard deviation one. All explanatory variables are measured at arrival and estimated with empirical Bayes shrinkage. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. The sample in columns (1)–(3) is restricted to refugees dispersed to municipalities for which it was possible to estimate Realized Opportunity based on immigrants/descendants who were permanent residents in the municipality, and for which the measure could be estimated using at least 10 observations. In columns (4) and (5), the samples are boys and girls, respectively. Column (6) reports results for the main refugee sample, and decomposes Realized Opportunity into its intercept component, α, and slope component, ψ. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C8: Child income rank regressed on Realized Opportunity and other municipality-level characteristics

Dependent var.:	(1) Income rank at 26-30	(2) Income rank at 26-30	(3) Income rank at 26-30	(4) Income rank at 26-30	(5) Income rank at 26-30	(6) Income rank at 26-30	(7) Income rank at 26-30	(8) Income rank at 26-30	(9) Income rank at 26-30	(10) Income rank at 26-30	(11) Income rank at 26-30
Realized Opportunity	1.447***										
	(0.327)										
log(Pop.)		-1.316***									
		(0.387)									
% non-western immig.			0.166								
			(0.502)								
% co-nationals				-0.482							
				(0.376)							
% with high-school					-0.462						
					(0.594)						
Employment rate						1.262***					
						(0.381)					
Mean earnings							1.366***				
							(0.408)				
Poverty rate								-1.659***			
								(0.490)			
Gini index									-1.710***		
									(0.269)		
Crime rate										0.616	
										(0.404)	
% teen births											0.438
											(0.422)
Observations	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474	10,474
R-squared	0.103	0.103	0.101	0.101	0.101	0.102	0.103	0.104	0.104	0.101	0.101
Clusters	213	213	213	213	213	213	213	213	213	213	213
Mean dep. var	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79	38.79

*Notes: This table reports regression estimates of child income rank (averaged over ages 26–30) on municipality-level characteristics—including Realized Opportunity—entered individually, for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and is standardized to have mean zero and standard deviation one. All explanatory variables are standardized and measured at arrival. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C9: Additional horse race regressions

Dependent var.:	(1) Income rank at 26-30	(2) Income rank at 26-30	(3) Income rank at 26-30	(4) Income rank at 26-30	(5) Income rank at 26-30	(6) Income rank at 26-30	(7) Income rank at 26-30
Realized Opportunity	1.447***			0.663*	0.521		0.330
	(0.327)			(0.391)	(0.443)		(0.444)
Poverty rate		-1.659***		-1.206**		-0.763	-0.635
		(0.498)		(0.569)		(0.590)	(0.587)
Gini index			-1.710***		-1.365***	-1.166**	-1.038*
			(0.269)		(0.440)	(0.482)	(0.545)
Observations	10,474	10,474	10,474	10,474	10,474	10,474	10,474
R-squared	0.103	0.104	0.104	0.104	0.104	0.105	0.105
Clusters	213	213	213	213	213	213	213
Mean dep. var.	38.79	38.79	38.79	38.79	38.79	38.79	38.79

*Notes: This table presents regression estimates of child income rank (averaged over ages 26–30) on Realized Opportunity and other municipality-level characteristics for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and is standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. All explanatory variables are measured at the time of arrival. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.*

Appendix Table C10: Horse Race Regressions for Crime with Realized Opportunity

Dependent var.:	(1) No. criminal convictions	(2) No. criminal convictions	(3) No. criminal convictions	(4) No. criminal convictions	(5) No. criminal convictions	(6) No. criminal convictions	(7) No. criminal convictions	(8) No. criminal convictions	(9) No. criminal convictions	(10) No. criminal convictions	(11) No. criminal convictions
Realized Opportunity	-0.113***	-0.086	-0.107**	-0.110**	-0.100	-0.104**	-0.164***	-0.110*	-0.124*	-0.104***	-0.113***
	(0.042)	(0.061)	(0.045)	(0.046)	(0.067)	(0.042)	(0.054)	(0.063)	(0.066)	(0.040)	(0.042)
log(Pop.)		0.041									
		(0.072)									
% non-western immig.			0.016								
			(0.054)								
% co-nationals				0.014							
				(0.049)							
% with high-school					0.066						
					(0.066)						
Employment rate						-0.020					
						(0.083)					
Mean earnings							0.080				
							(0.049)				
Poverty rate								0.005			
								(0.074)			
Gini index									-0.016		
									(0.071)		
Crime rate										0.083*	
										(0.044)	
% teen births											0.006
											(0.044)
Observations	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415
R-squared	0.135	0.135	0.135	0.135	0.135	0.135	0.136	0.135	0.135	0.136	0.135
Clusters	215	215	215	215	215	215	215	215	215	215	215
Mean dep. var	1.637	1.637	1.637	1.637	1.637	1.637	1.637	1.637	1.637	1.637	1.637

*Notes: This table reports regression estimates of the number of criminal convictions (measured up to and including age 30) on Realized Opportunity and other municipality-level characteristics for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and is standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. All explanatory variables are standardized and measured at arrival. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C11: Horse Race Regressions for Teenage Parenthood with Realized Opportunity

Dependent var.:	(1) Pr(Teen parenthood)	(2) Pr(Teen parenthood)	(3) Pr(Teen parenthood)	(4) Pr(Teen parenthood)	(5) Pr(Teen parenthood)	(6) Pr(Teen parenthood)	(7) Pr(Teen parenthood)	(8) Pr(Teen parenthood)	(9) Pr(Teen parenthood)	(10) Pr(Teen parenthood)	(11) Pr(Teen parenthood)
Realized Opportunity	-0.003*	-0.004**	-0.004**	-0.003**	-0.003*	-0.003*	-0.002	-0.002	-0.002	-0.003**	-0.003**
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
log(Pop.)		-0.001									
		(0.002)									
% non-western immig.			-0.002								
			(0.002)								
% co-nationals				-0.001							
				(0.002)							
% with high-school					0.000						
					(0.002)						
Employment rate						0.000					
						(0.003)					
Mean earnings							-0.001				
							(0.002)				
Poverty rate								0.002			
								(0.003)			
Gini index									0.001		
									(0.002)		
Crime rate										-0.002	
										(0.002)	
% teen births											-0.003
											(0.002)
Observations	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415	12,415
R-squared	0.056	0.056	0.056	0.056	0.056	0.056	0.056	0.056	0.056	0.056	0.056
Clusters	215	215	215	215	215	215	215	215	215	215	215
Mean dep. var	0.032	0.032	0.032	0.032	0.032	0.032	0.032	0.032	0.032	0.032	0.032

*Notes: This table reports regression estimates of the probability of teenage parenthood (measured up to and including age 19) on Realized Opportunity and other municipality-level characteristics for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Realized Opportunity is the predicted child income rank at ages 26–30 for parents at the 25th percentile, estimated by municipality × birth cohort using permanent-resident families (see Section 3.2) and is standardized to have mean zero and standard deviation one. Realized Opportunity is measured with empirical Bayes shrinkage. All explanatory variables are standardized and measured at arrival. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix C4

Heterogeneous Effects

Appendix Table C12: Heterogeneous Effects

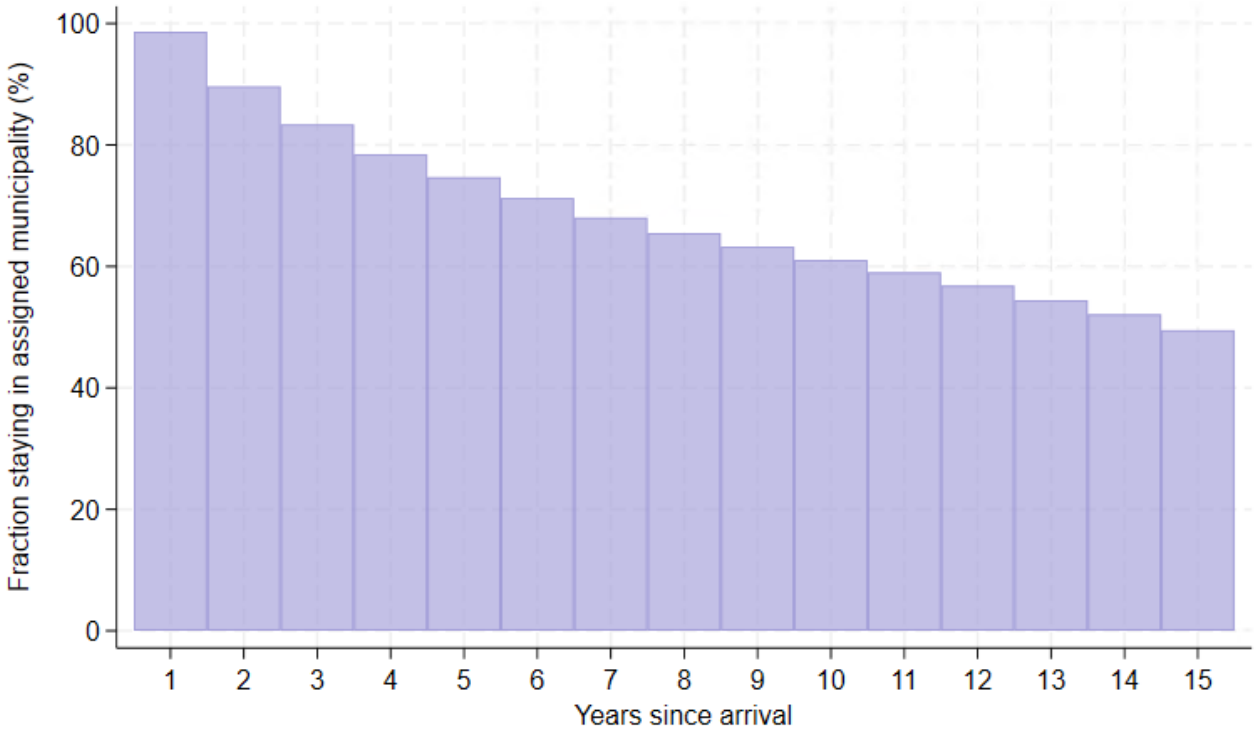
Dependent var.:	(1) Income rank at 26-30	(2) No. criminal convictions	(3) Pr(Teen parenthood)
Panel A: Boys			
Realized Opportunity	1.258***	-0.195***	-0.001
	(0.449)	(0.074)	(0.001)
Observations	5,820	6,731	6,731
Mean dep. var	41.84	2.766	0.005
Panel B: Girls			
Realized Opportunity	1.711***	-0.019	-0.005*
	(0.363)	(0.016)	(0.003)
Observations	4,640	5,671	5,671
Mean dep. var	34.93	0.299	0.065
Panel C: High parental level of education			
Realized Opportunity	2.225**	-0.211***	-0.005
	(0.905)	(0.072)	(0.005)
Observations	1,946	2,319	2,319
Mean dep. var	41.39	1.571	0.031
Panel D: Low parental level of education			
Realized Opportunity	1.280***	-0.090**	-0.003
	(0.289)	(0.044)	(0.002)
Observations	8,522	10,091	10,091
Mean dep. var	38.19	1.653	0.033
Panel E: Origin from Africa			
Realized Opportunity	1.264	-0.464***	-0.003
	(0.992)	(0.133)	(0.003)
Observations	1,054	1,422	1,422
Mean dep. var	32.48	2.525	0.018
Panel F: Origin from the Middle East			
Realized Opportunity	1.558***	-0.118**	-0.005**
	(0.355)	(0.053)	(0.002)
Observations	6,711	7,945	7,945
Mean dep. var	37.10	1.865	0.042
Panel G: Origin from Asia, excluding the Middle East			
Realized Opportunity	1.160	0.054	0.004
	(0.798)	(0.044)	(0.002)
Observations	2,635	2,950	2,950
Mean dep. var	45.60	0.618	0.013

*Notes: This table shows results from a set of regressions for refugees who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998, and whose parents were subject to the dispersal policy. Panel A shows the effects for boys, and Panel B shows the effects for girls. Panel C shows the effects for refugees whose first-arriving parent had a bachelor's degree or more from the home country (high parental level of education), and Panel D shows the effects for those whose first-arriving parent did not have a bachelor's degree or more from the home country (low parental level of education). Panel E shows the effects for those originating from Africa, Panel F for those from the Middle East, and Panel G for those originating from Asia, excluding the Middle East. Child income rank is measured at ages 26–30, criminal convictions are measured up to and including age 30, and teenage parenthood is measured by age 19. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix C5

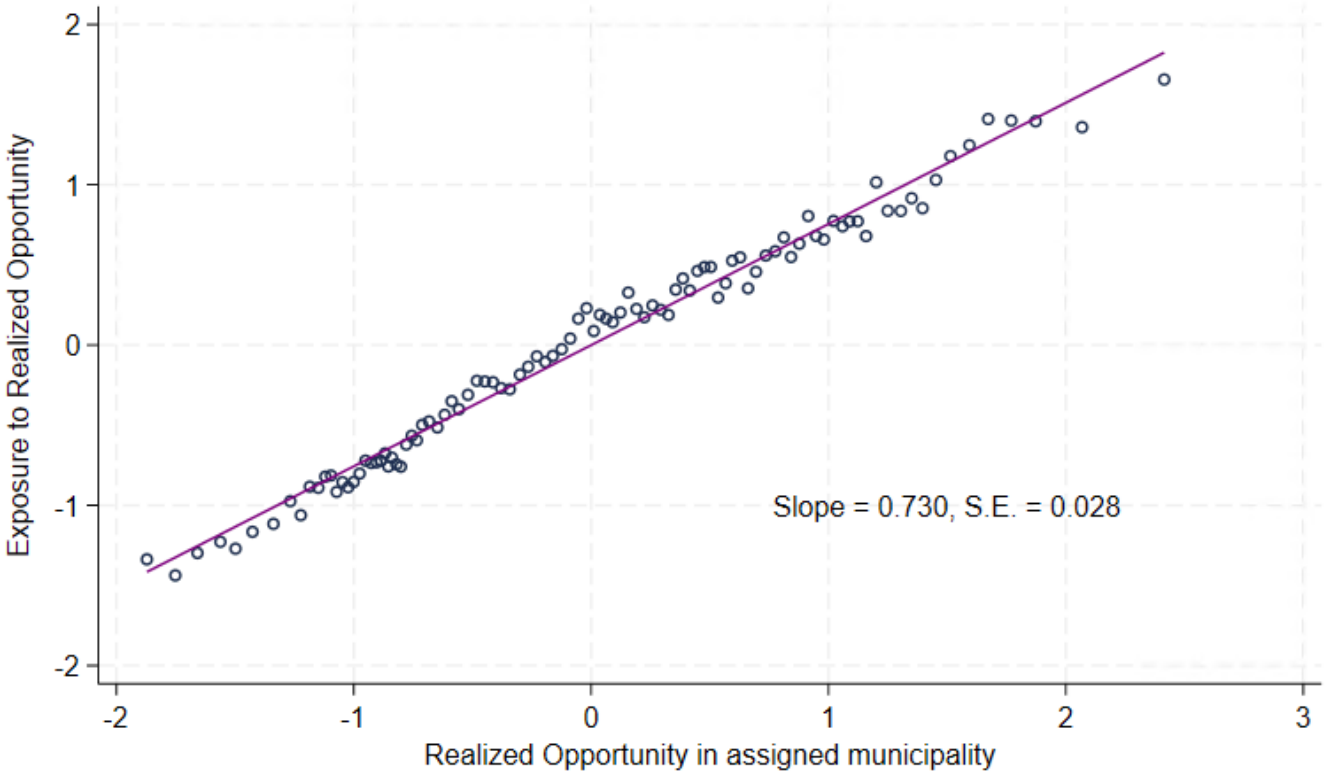
Robustness Checks

Appendix Figure C7: Fraction of refugees living in assigned municipality



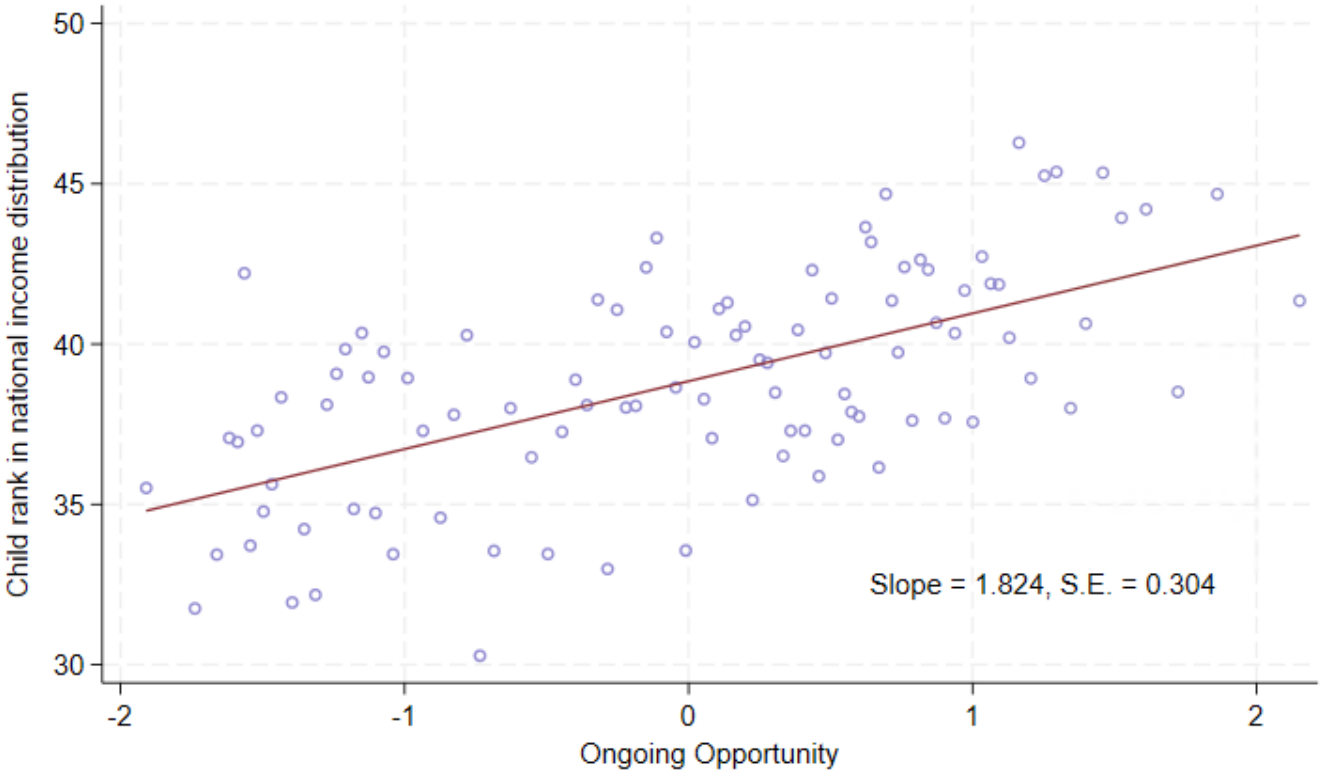
Notes: This figure shows the share of refugee children (aged 0–17 at arrival) who reside in their initial municipality as a function of years since their parents' arrival. In this sample, the average length of exposure to the municipality of assignment was 10.23 years.

Appendix Figure C8: Actual Exposure to Realized Opportunity and Assigned Realized Opportunity



Notes: This figure presents a binned scatterplot of the relationship between exposure to Realized Opportunity in the municipality of residence up to age 25 and Realized Opportunity in the municipality of assignment. The sample consists of refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. The figure plots 100 binned means along with a fitted linear regression line. The fitted line is estimated controlling for year of arrival, country of origin, child characteristics (age at arrival and gender), and parental characteristics (age at arrival, gender of the first-arriving parent, marital status at arrival, number of children at arrival, and education at arrival). The measures of Realized Opportunity are standardized and estimated with empirical Bayes shrinkage. The reported standard error for the slope coefficient is clustered at the municipality level.

Appendix Figure C9: Child Income Rank versus Ongoing Opportunity



Notes: This figure presents a binned scatterplot of the relationship between Ongoing Opportunity and child income rank for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and were subject to the dispersal policy. Child income is measured as labor income averaged over ages 26–30. The figure plots 100 binned means along with a fitted linear regression line. The fitted line is estimated controlling for year of arrival, country of origin, child characteristics (age at arrival and gender), and parental characteristics (age at arrival, gender of the first-arriving parent, marital status at arrival, number of children at arrival, and education at arrival). Ongoing Opportunity is standardized and estimated with empirical Bayes shrinkage. The reported standard error for the slope coefficient is clustered at the municipality level.

Appendix Table C13: Robustness to Measurement of Opportunity

Dependent var.:	(1) Income rank at 26-30	(2) No. criminal convictions	(3) Pr(Teen parenthood)
Panel A: Baseline Model			
Realized Opportunity (T)	1.482***	-0.106**	-0.004**
	(0.333)	(0.043)	(0.002)
Observations	9,162	10,892	10,892
Mean dep. var	38.91	1.650	0.030
Panel B: Realized Opportunity Measured at T+1			
Realized Opportunity (T+1)	1.216***	-0.077*	-0.003*
	(0.375)	(0.040)	(0.002)
Observations	9,162	10,892	10,892
Mean dep. var	38.91	1.650	0.030
Panel C: Realized Opportunity Measured at T-1			
Realized Opportunity (T-1)	1.288***	-0.087**	-0.004**
	(0.371)	(0.039)	(0.002)
Observations	9,162	10,892	10,892
Mean dep. var	38.91	1.650	0.030
Panel D: A single regression			
Realized Opportunity (T)	1.253**	-0.113	-0.002
	(0.537)	(0.077)	(0.003)
Realized Opportunity (T+1)	0.019	0.026	0.000
	(0.650)	(0.078)	(0.003)
Realized Opportunity (T-1)	0.286	-0.019	-0.003
	(0.529)	(0.076)	(0.003)
Observations	9,162	10,892	10,892
Mean dep. var	38.91	1.650	0.030
Panel E: Without shrinkage			
Realized Opportunity w/o shrinkage	1.414***	-0.101**	-0.004**
	(0.331)	(0.044)	(0.002)
Observations	10,474	12,415	12,415
Mean dep. var	38.79	1.637	0.032

*Notes: This table shows results from a set of regressions for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998, and whose parents were subject to the dispersal policy. Panel A shows the coefficients on Realized Opportunity measured in the year of arrival (T), Panel B the year after arrival ($T+1$), and Panel C the year before arrival ($T-1$). The results in Panel A for Realized Opportunity measured in the year of arrival (T) may differ slightly from the main results because the estimation samples differ. Specifically, Realized Opportunity is not defined for years before 1986, and the estimation sample is restricted to observations for which Realized Opportunity is defined in the year of arrival (T), the year after arrival ($T+1$), and the year before arrival ($T-1$). Panel D shows results from a regression in which all three measures of Realized Opportunity, measured in different years around arrival, are included in the same regression. Panel E shows the coefficients on Realized Opportunity measured without empirical Bayes shrinkage. Child income rank is measured at ages 26–30, criminal convictions are measured up to and including age 30, and teenage parenthood is measured by age 19. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C14: Robustness to Price Deflator

Income deflator:	House Price Index	Consumer Price Index
	(1)	(2)
Dependent var.:	Income rank ages 26-30	Income rank ages 26-30
Realized Opportunity	1.535***	1.877***
	(0.574)	(0.424)
Child characteristics		
Age, child	-1.161***	-1.300***
	(0.112)	(0.115)
Male, child	9.457***	9.528***
	(0.763)	(0.658)
Observable parental characteristics		
Age, parent	-0.005	-0.008
	(0.045)	(0.046)
Male, parent	1.249	0.928
	(0.829)	(0.907)
No. of children, ages 0-2	-2.133***	-2.643***
	(0.590)	(0.598)
No. of children, ages 3-17	-0.820***	-0.875***
	(0.224)	(0.211)
Married	0.111	-0.207
	(1.038)	(1.054)
Unobservable parental characteristics		
Unknown educ.	0.579	0.758
	(1.020)	(0.978)
Basic educ.	0.915	1.332
	(1.262)	(1.207)
Academic educ.	1.632	2.227
	(1.616)	(1.559)
Observations	7,563	7,563
R-squared	0.143	0.128
Clusters	207	207
Mean dep. var	36.95	37.53
SD dep. var	27.26	27.77

*Notes: This table presents results from a set of regressions for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998 and whose parents were subject to the dispersal policy. Both columns show results from regressions of child income rank, measured at ages 26–30, on Realized Opportunity in the municipality of assignment. In column (1), child income rank is deflated using a house price index based on the child's municipality of residence at ages 26–30. The house price index is measured as the average house price per square meter in a municipality-year relative to that in Copenhagen in 2015. Because the sample size is smaller than in the main results in Table 2, column (2) presents, for comparison, results in which child income rank is deflated using the consumer price index (CPI), as in the rest of the paper. The sample sizes are smaller than in the main results because house price data are available only for the years 1987–2016. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*

Appendix Table C15: Robustness to Sample Composition and Inference

Dependent var.:	(1) Income rank at 26-30	(2) No. criminal convictions	(3) Pr(Teen parenthood)
Panel A: Drop 5 biggest muns.			
Realized Opportunity	1.427***	-0.156**	-0.002
	(0.401)	(0.065)	(0.002)
Observations	6,753	8,017	8,017
Mean dep. var	40.28	1.545	0.029
Panel B: Bootstrapped S.E.'s			
Realized Opportunity	1.463***	-0.113***	-0.003**
	(0.247)	(0.033)	(0.001)
Observations	10,474	12,415	12,415
Mean dep. var	38.79	1.637	0.032

*Notes: This table shows results from a set of regressions for refugee children aged 0–17 at arrival who immigrated to Denmark within one year of their parents' arrival between 1986 and 1998, and whose parents were subject to the dispersal policy. Panel A shows results from regressions that drop refugees dispersed to one of the five largest municipalities (Copenhagen, Aarhus, Odense, Aalborg, and Esbjerg) from the sample. Panel B shows results from regressions of outcomes on Realized Opportunity with cluster-bootstrapped standard errors. Child income rank is measured at ages 26–30, criminal convictions are measured up to and including age 30, and teenage parenthood is measured by age 19. The regressions control for year-of-arrival and country-of-origin fixed effects, in addition to the parental and child characteristics included in Table 1. Standard errors are clustered at the municipality level. Significance levels: *** p<0.01, ** p<0.05, * p<0.10.*