The Long-Run Effects of Refugee Inflows on Native Children's Economic Mobility

Short title: Refugees and Intergenerational Mobility

Jesper Eriksen[1,*]

Abstract: I examine how refugee inflows affect the intergenerational economic mobility of native children. The analysis exploits the quasi-random allocation of refugees across Danish municipalities between 1986 and 1998 and comprehensive Danish administrative data that link the full population of parents and children. I find that children from lower-income families who grew up in municipalities with above-median refugee inflows have, on average, 1.8 percentage-point lower expected income rank in adulthood, equivalent to about USD 2,100 (2015 values), than comparable children in municipalities with below-median inflows. Children from higher-income families are unaffected. The effects do not differ by gender but are stronger for low-income children from less-affluent municipalities than for those from more-affluent municipalities.

Keywords: refugees, natives, intergenerational mobility, allocation policy, long-run effects

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[*]**Correspondence address:** jesper.eriksen@econ.ku.dk.

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1 Introduction

In 2020, more than 23 million people were classified as refugees, having fled democratic and violent crises in their home countries, according to the United Nations Refugee Agency (UNCHR, 2020). Between 2015 and 2020, over two million refugees arrived in Europe from the Middle East and North Africa.[1] Large streams of refugees have recently been used as political arguments for large policy changes, such as closing borders and reducing welfare benefits. But despite the political claims about the effects of refugees on local societies, it remains an open question exactly how inflows of refugees may affect their host societies.

One set of studies has examined the effects of refugee and immigrant inflows on native adults' wages, labour-market participation, and political preferences, finding effects that range from negative to positive depending on the degree of complementarity or substitutability between natives, immigrants, and refugees in local labour markets (Dustmann *et al.*, 2016; Foged and Peri, 2016). In contrast, a smaller set of studies has investigated the effects of immigrants and refugees on native children's short-run outcomes, finding negative or no effects on educational achievement and anti-social behaviour, such as bullying (Gould *et al.*, 2009; Jensen and Rasmussen, 2011; Cascio and Lewis, 2012; Schneeweis, 2015). Both strands of the literature primarily focus on short-run effects.

Short-run effects, however, may misrepresent the long-run benefit-cost structure of immigration policies. For example, the Moving To Opportunity (MTO) experiment provided randomly selected families from sevearl major United States cities with housing vouchers to move out of impoverished neighbourhoods. Early evaluations of the MTO experiment

[1]Northern Europe received 766,000 refugees, corresponding to more than 2.3 percent of the local population (UNHCR, 2019; UNCHR, 2020). The Northern European countries listed by the UNHCR include Denmark, Finland, Norway, Sweden, and Iceland, as well as Estonia, Latvia, and Lithuania.

found no effects on children's educational achievement, and a slightly higher likelihood of sons committing crimes (Ludwig *et al.*, 2008, 2013). In a recent re-evaluation of the long-run income effects of the experiment, Chetty *et al.* (2016) show that adult earnings improved for children from the treated families compared with those from control families. Applied to the context of refugee inflows, these findings highlight the importance of examining the long-run economic impact on native children when evaluating refugee policies.

This paper investigates the long-run effects of refugee inflows on native children's income in adulthood. A common challenge in studies of the effects of immigrant or refugee inflows is that the refugees and immigrants may self-select into specific areas. As a result, comparing differences in outcomes among natives in affected and non-affected areas may conflate area-specific effects with immigrant and immigrant- and refugee-specific effects. I address this identification problem by studying a Danish refugee allocation policy in place from 1986 to 1998 which caused plausibly exogenous variation in local refugee shares at a municipal level. Over this period, Denmark received the equivalent of 0.8 percent of its native population in refugees, the same share as Denmark received from 2010 to 2018 during the refugee crisis in the Middle East and North Africa.[2] The allocation policy caused quasi-randomly selected municipalities to have large shares of refugees early on, while others received their required share later in the allocation period.

Studying the long-run effects of refugee inflows on children's adulthood incomes by parental income requires combined data on refugee allocations, the locations of children and parents during childhood, and the adult incomes of both generations. I obtain these data from Danish administrative registers, which provide micro-level information on the full

[2]Appendix A shows recent refugee inflow to Denmark using data collected from Statistics Denmark.

population from 1980 to 2015. My analysis focuses on all children born between 1973 and 1980 in Denmark, who grew up while refugees were being allocated across municipalities, and for whom the national income registers contains at least one observation of income during their late thirties and early forties—an age range at which earnings are a reasonable proxy for permanent income (Landersø and Heckman, 2017). This effectively covers nearly the full population of Danish born children from these birth cohorts. The resulting sample includes 537,000 children linked to their parents, for whom I observe parents' and children's adulthood income. I convert parental and child incomes into cohort-specific income ranks in the main analysis. Finally, I identify refugees subject to the allocation policy using data on country of origin and date of first arrival in Denmark. The comprehensive nature of the Danish administrative data makes it possible to investigate long-run effects of refugee inflows despite the substantive data requirements raised by the research question.

My identification strategy relies on the quasi-random allocation of refugees to municipalities early or late in the allocation period. This assumption could be violated if refugees were allocated, for example, to municipalities that had few immigrants or higher average income levels prior to 1986 as these characteristics may correlate with local children's adulthood income and their parents' income. I show that refugee shares do not predict relevant municipal characteristics, such as prior immigrant shares and average family income in the municipalities. The only exception is municipality size, which reflects the fact that Copenhagen, the largest municipality, was subject to specific allocation rules. I discuss the allocation rules in Section 3. Excluding children raised in the Copenhagen municipality from the analysis does not change my main findings.

The main finding of the paper is that native children from lower-income households may experience negative long-run income effects of refugee inflows. I estimate these effect by regressing child adult income ranks onto their parents' income ranks together with an indicator for the child growing up in a municipality that had a below-median share of refugees allocated by 1994, measured relative to the 1986 pre-reform municipal population, and an interaction between the parental income rank and the indicator. Refugee shares are calculated relative to the pre-reform population to reflect the allocation rule that assigned refugees in proportion to municipal population size. Under the assumption that the refugee share is quasi-randomly assigned, the coefficients on the treatment indicator and the interaction term can be interpreted as the causal effect of the refugee allocation on children's adulthood income conditional on their parental income rank. Children who grew up in areas with above-median cumulative shares of refugees have lower annual incomes ranks in adulthood on average. To illustrate the magnitude of the estimated effects, consider children born to parents at the 25th income percentile. For children in municipalities with below-median refugee shares by 1994, the expected income rank is $38.6 + 25 * 0.27 = 45.4$. For children in municipalities with above-median refugee shares, the expected rank is $(38.6 - 2.5) + 25 * (0.27 + 0.033) = 43.6$. The difference is 1.8 percentage points, corresponding to about USD 2,100 per year in 2015 values. For children from families at the bottom 10th percentile, the estimated effect 2.2 percentage points, equivalent to about USD 2,700.

In supplementary analysis, I show that the effects do not differ by gender, but the effects do differ by municipal average family income. First, I estimate the model separately for males and females, as prior studies on immigrant effects suggest that males are more likely to experience short-run negative earnings and academic achievement impacts. In contrast,

I find that the estimated effect does not differ by gender. Second, I investigate whether the effects vary by municipalities' average family income, estimating the model separately for municipalities in the bottom and top halves of the income distribution. The negative effect of refugee inflows is stronger in the lower-income municipalities. As refugee shares are uncorrelated with pre-allocation average municipal family income, this pattern suggests that conditions in less-affluent municipalities, such as limited school resources, may be an important mitigator for the estimated effects. This interpretation is consistent with earlier short-run studies on immigrant exposure and with recent evidence (e.g., Jackson *et al.* 2016; Biasi *et al.* 2025) showing that local resources matter for student outcomes.

My findings contribute to a large literature investigating refugees' and immigrants' effects on child and adult natives. Card (2001, 2009), Dustmann *et al.* (2016), Dustmann *et al.* (2025), Edo (2019) and Foged and Peri (2016) review the literature focusing on how immigrant and refugee inflows affect adult natives' labour market attachment and incomes. A takeaway from this literature is that immigrants and refugees will have negative effects on native workers when they are close substitutes and labour and product markets are competitive (Borjas, 2003; Dustmann *et al.*, 2017; Prantl and Spitz-Oener, 2020), and null or positive when they complement the native workers (Foged and Peri, 2016; Peri and Sparber, 2009; Tabellini, 2020). An important characteristic of the majority of these studies is that they focus on short-run outcomes (Borjas and Monras, 2017).[3] The second strand of immigrant and refugee effect studies investigates effects on native children. These studies can be divided into two groups. The first set of studies estimate the effects of immigrant and refugee peers within schools on native children and have found both positive and negative

[3]Cohen-Goldner and Paserman (2011) is an example of one of the few studies considering the longer-run effects of refugee inflows to Israel, finding positive short-run but positive long-run earnings effects.

effects on native children's achievement, school environment, and likelihood of obtaining high school equivalent degrees (e.g., Figlio and Özek 2019; Gould *et al.* 2009; Hassan *et al.* 2023; Hunt 2017; Jensen and Rasmussen 2011; Schneeweis 2015; Ohinata and van Ours 2013. The second strand of literature focuses on school flight, showing that more affluent families are more likely to move their children, either by moving themselves or entering their children in private school when (internal or external) migrant shares increase (Derenoncourt, 2022; Rangvid, 2010). I discuss these two strands of literature in more detail in Section 2, where I discuss the possible mechanisms that may create long-run effects of refugee inflows on native children's adulthood incomes. I contribute to the immigrant and refugee effects literature by studying the long-run effects of local refugee allocations on native children's adulthood labor market outcomes, providing plausibly well-identified effects of refugee shares on children's long-run adulthood outcomes beyond the setting of schools, which has been the primary focus of refugee studies. While the prior literature on the effects on short-run effects on children is mixed, my paper suggests that long-run effects may be substantive.

My paper is closely related to a small set of studies that also use the Danish refugee allocation policy to estimate the effects of allocation on refugees themeselves and on local's political preferences. The studies on effects on the refugees have shown that the characteristics of their local municipality can affect both reallocation patterns, labor market attachment and earnings, and criminal and health outcomes of the refugees (Damm, 2009a, 2014; Damm and Dustmann, 2014; Eckert *et al.*, 2019; Hasager and Jørgensen, 2024; Dustmann *et al.*, 2023; Foverskov *et al.*, 2024). The study by Dustmann *et al.* (2019) on refugee inflow effects on natives' political preferences is most closely related to this paper by focusing on effects

on others than the refugees. They find evidence of increased right-wing support in municipalities that early on received more refugees. I draw on the same quasi-random refugee allocation as these previous papers, but focus instead on effects on native children's long-run outcomes.

My findings also contribute to the intergenerational mobility literature. Like the immigration and refugee effects literature, the intergenerational mobility literature has its root in early theoretical and empirical studies from the 1970s and 1980s (e.g. Becker and Tomes 1979, 1986, and Atkinson 1980). The main emphasis in the early literature was to estimate the extent to which children's outcomes were determined by parental background, often at a national level (see Solon 2002, Black and Devereux 2011, Mogstad and Torsvik 2023 and Torche 2015 for surveys of this literature). A set of papers in this tradition focuses particularly on the intergenerational mobility of second-generation immigrants (Abramitzky *et al.*, 2021; Borjas, 1993; Collins and Zimran, 2019; Aydemir *et al.*, 2009; Bolotnyy and Bratu, 2018; Hammarstedt and Palme, 2012).[4] More recently, the intergenerational mobility literature has made use of the growing availability of large-scale administrative data to estimate differences in intergenerational mobility across locations within countries (e.g., Acciari *et al.* 2022; Chetty *et al.* 2014, 2020, 2025; Connolly *et al.* 2019; Corak 2020; Güell *et al.* 2018; Heidrich 2017). While some authors have argued that the effects of locations may be smaller than contended in some papers (e.g., Mogstad and Torsvik 2023), the findings from these papers raise the question of what drives the local differences. Some previous papers have studied how shocks to local environments affect the observed mobility. For example, Sharkey

[4]In a recent study, Boustan *et al.* (2025) estimates immigrants' intergenerational economic mobility across 15 different countries, and OECD (2018) compiles seven different cross-country comparisons of native and immigrant intergenerational mobility. My emphasis in this paper differs from this strand of literature by focusing on the effects of arriving refugees on natives.

and Torrats-Espinosa (2017) study the effects of local police funding reforms on local mobility, and Derenoncourt (2022) estimate the effects on local intergenerational mobility of internal migration from the South to Northen cities in the United States during the Great Migration.[5] I build on prior work estimating municipal variation in intergenerational mobility in Denmark showing that within-country variation in intergenerational mobility in Denmark corersponds to that found in Canada, and is slightly less varied than in the United States (Eriksen and Munk, 2020). Focusing on the quasi-random allocation of refugees, I show that policies affecting the composition of locals can have intergenerational income effects.

Despite the benefits of the Danish comprehensive data and quasi-random allocation of refugees due to the Danish refugee allocations between 1986–1998, my findings are subject to a set of limitations. First, the Danish refugees were assigned to municipalities and not school catchment areas which would enroll refugee children in certain schools, as was the case for Haitian refugees studied by Figlio and Özek (2019). Therefore, my analysis is limited to the higher municipal level of aggregation than the school level, where I would expect the mechanisms to play out. A second important limitation is that while I observe long-run earnings outcomes for the children, I do not observe short-run academic outcomes typically studied in the immigrant effect literature, such as exam or test scores. Danish students take final exams at the end of mandatory schooling in 9th grade but the exam scores have only been recorded from the 2002/2003 school year. As a result, I am not able to verify short-run

[5]In related papers, Pekkarinen *et al.* (2009) and Havnes and Mogstad (2011, 2015) investigate staggered reform implementations across Norwegian and Finlandish municipalities to investigate effects of school tracking and on child care reforms. Chetty and Hendren (2018a,b) focus on movers across municipalities to estimate the effects of local areas on children's outcomes, and Deutscher (2020) extend the same analysis to Australia.

fundings from the previous literature. Estimating short- and long-run outcomes using the same data sources would be an important task for future research.

The paper is structured as follows. In the next section, I discuss potential drivers of refugee effects based on the previous literature. Section 3 describes the refugee allocation policy, which I use as exogenous variation in refugee shares in the analysis. Section 4 describes the data and contains descriptive statistics on the main sample of children and parents. Section 5 describes the estimation procedures. Section 6 contains the main results from the paper, as well as various robustness checks. The last section concludes the paper.

2 Potential drivers of refugee effects

The prior literature on child effects from refugee and immigrant inflows provide some guidance to understanding how native children's long-run economic labor market outcomes could be affected by the allocation of refugee allocations during their childhood. In this section, I discuss the prior findings on each of the two channels that go through educational environments, peer effects and school flight, that has been studied in the previous literature. Both focus on the educational attainment and achievements of children, which can be translated into monetary returns in the labour market.

The prior evidence on immigrant and refugee peer effects in education is focused on what happens to native children's learning and social environments as shares of refugee or immigrant children change within schools or classrooms. The existing evidence on immigrant and refugee peer effects range from negative to null effects on test scores and negative effects

on social environments.[6] Jensen and Rasmussen (2011) use PISA test score data paired with Danish administrative data to show that children in schools with more immigrants have performed worse on the PISA test. In OLS and IV regressions they find a negative effect for both native and immigrant children. Gould *et al.* (2009) also find negative effects on subsequent educational attainment from having relatively more refugees in 5th grades within Israeli schools. In contrast, studying grade retention in Linz, Austria and PISA test score results in Dutch schools, Schneeweis (2015) and Ohinata and van Ours (2013) find null effects. Ohinata and van Ours (2013) also test for effects on anti-social behaviour in classes, finding more bullying in classes with relatively more immigrants. In more recent work, Figlio and Özek (2019) study the effects of arrival of Haitian natural disaster refugees to Florida primary schools using administrative data from Florida, and Hassan *et al.* (2023) study the effects of arrival of refugees to Danish school classes between 2008-2015 on test scores in school-by-year fixed effects design. In both studies, the authors find no evidence of test score effects from the inflow of refugees, but Figlio and Özek (2019) find increased prevalence of anti-social behaviour. While these previous studies show mixed findings of the possible effects of refugee and immigrants shares in schools and classrooms, they indicate that there may be both academic and social learning effects from the arrival of refugees.

The school flight channel is less well documented, but suggest that as immigrant shares increase in public schools, more affluent families may choose to move their children to private schools. Betts and Fairlie (2003) document this notion for public and private schools in the United States, coining the term school flight. Rangvid (2010) similarly provides descriptive

[6] A recent study by Hunt (2017) also find positive effects on educational attainment in the United States. However, the estimates are derived from derived from state-level regressions of native children's educational attainment on state immigrants shares instrumented by 1940s state-level immigrant shares. Early immigrants may have sorted towards local characteristics that correlate with presentday quality of primary schools, in which case the estimates may reflect both quality and immigrant effects.

evidence from cross-sectional data on school attendance and parental background in Copenhagen, Denmark, showing that as immigrant shares rise in public schools, affluent children become more likely to enroll in private schools. This type of school shifting by affluent children may lead to two effects on learning and subsequent labour market outcomes. First, if high-income children are more likely to move to private schools when refugees arrive at their local school, they may not be affected by the refugee inflow. Second, when the high-income children switch this may have negative impacts on the remaining students' educational attainment and earnings, for example through the removal of role model parents. In a recent paper from Norway, Cattan *et al.* (2025) show that children become more likely to attend elite educations when they have more peers with elite-educated parents. High income children also typically perform better in school (Blanden *et al.*, 2023). If there are learning externalities in the classroom (e.g., Sacerdote 2014), then moving the affluent children can lead to further reductions of the quality of learning in the primary schools of remaining children.

Taken together, the previous literature suggests that refugee arrivals may affect both academic and social learning, as well as education choices through shifts in role models. The link between cognitive and social skills and future earnings has been extensively documented (e.g., Deming 2017; Edin *et al.* 2022; Heckman *et al.* 2013), and detrimental effects on either cognitive or social skills could potentially change earnings substantially. Similarly, the relation between education field choices and economic outcomes has also been extensively documented, indicating that certain (elite) majors carry substantial earnings returns (e.g. Bleemer and Mehta 2022; Dahl *et al.* 2023; Kirkeboen *et al.* 2016). It is therefore likely that inflow of refugees have long-term effects on economic outcomes, and additionally that

these effects may be stronger for children from lower-income families than higher-income families.

3 Refugee Allocation Scheme, 1986–1998

The Danish refugee allocation policy in place between 1986–1998 was originally developed as a response to increasing refugee flows to Denmark in the early 1980s.[7] These early inflows raised alarm that some Danish municipalities would receive large numbers of refugees, while others would receive few, creating a disproportionate cost burden for some municipalities. To account for the unequal burdens, the new allocation scheme would allocate some refugees proportionate to population size across municipalities in upcoming years. If some municipalities received their share early on, they would not receive more refugees later on until all municipalities had done so. To implement the policy, the government assigned the Danish Refugee Council (DRC), an organisation established in the 1950s to aid refugees in integrating into Danish society, with the task of assigning refugees to municipalities when granted asylum. The DRC was and remains a non-governmental organisation (NGO) and acts outside the scope of political interests of municipalities and other government actors, which would help ensure that no local political interests would be more likely to be served than others when assigning refugees to municipalities.

The allocation took place in two consecutive steps, which together ensured that no region of Denmark would receive its share of refugees earlier than others. When arriving in Denmark, asylum seekers were initally assigned to one of several Red Cross reception centres for

[7] This overview draws on Damm and Dustmann (2014), Dustmann *et al.* (2019), and Damm (2009a,b, 2014) who have done extensive interviews with programme participants to ensure the validity of the information on the policy features. In the overview, I emphasise the features which allow me to use the reform in the analysis as a quasi-random refugee shock to each municipality.

asylum seekers. If the individual had their asylum application acknowledged, they would be considered acknowledged refugees in the country, and become subject to the two-tier asylum allocation scheme. First, a refugee who was granted asylum in Denmark got allocated to one of the 15 Danish counties. After initial allocation to a county, the local arm of the DRC was given the task of finding housing for the refugee in the municipality. These local arms were located within municipalities, and the government-mandated that offices be moved to new locations after about 3–5 years, at which point the government expected the municipality to have received its share of refugees. As a result of the allocation mechanism and the changing locations of offices, some municipalities received their share of refugees early and others later on. This allocation rotation mechanism is the source of the plausibly quasi-random allocation of refugees across municipalities, which I use in the main analysis to obtain causal estimates.

The allocation of refugees to municipalities was double-blind from the perspective of both refugees and municipalities, which ensured that only a limited set of information was used by the DRC to determine where refugees would be allocated to. First, refugees subject to the allocation mechanism were not given the option to inform the DRC about their preferences over municipalities. They were simply assigned to the municipalities in which the DRC found housing for them. Similarly, municipalities were not able to determine when and which refugees they would receive. They were only informed that they would receive refugees *after* the DRC had found local housing for the refugee in the municipality. The DRC only used limited information in deciding where a refugee should be allocated. First, it relied on available rental housing to be able to assign refugees to municipalities. This lead to special accommodation for the capital city of Denmark, Copenhagen, and closely located

Frederiksberg, where rental housing was sparse, according to interviews with former DRC employees described by Dustmann *et al.* (2019). Secondly, the DRC was mandated to group refugees by nationality, ensuring that incoming refugees could have a support structure of similar refugees. This second policy also strengthened the tendency for some municipalities to receive their share of refugees early on, as refugees tended to arrive in waves as conflicts in their home country escalated. As a result, I am not able to analyse how country-of-origins may matter for the estimated findings, despite the fact that differences in local reasons for being granted refugee status may be important for the effects of refugees on others, for example through traumatic experience imprints. I discuss implications for the empirical analysis in Section 5.

A total of 76,673 refugees were granted asylum in Denmark over the period 1986–1998, but not all were subject to the allocation policy (Damm, 2014). The DRC was instructed to ensure that whenever an individual arrived in the country, they would be reunited with any family members who had already been granted asylum.[8] As a result, only the first arriving members of a family were subject to the policy. Families arriving together would be assigned to the same municipality. To account for this in the analysis, I focus on the initially arriving individuals above the age of 18, who do not have close family members who already live in Denmark, resulting in a sample of refugees who were plausibly randomly assigned to the Danish municipalities.

After the initial assignment, the refugees were allowed to re-allocate within and across municipalities, although they were given additional resources, such as language classes, within the municipality they were assigned to during the first 18 months after assignment. (Damm,

[8] Additionally, as the Yugoslavian wars started in the early 1990s, a large amount of former Yugoslav refugees fled to Denmark within a short period. These were all subject to special emergency asylum and allocation processes. As a result, I do not include these in the analysis.

2014, Table A6) shows for a sample of male refugees identified from the Danish administrative data similarly as in this paper, that while some refugees did move, the majority stayed in their assigned municipality. In the first year of settlement, all refugees remained in the municipality, and even neighbourhood they were assigned to. After three years, 60 percent of refugees remained in the same municipality. Of these, about 24 percent of refugees moved neighbourhoods within municipalities. After five years, more than 50 percent of the male refugees remained in their assignment municipalities. The general tendency for refugees to remain in their assigned municipalities underscore that the initial allocation was likely to lead to higher shares of refugees. However, as some refugees did move after initial settlement, the results in the analysis should be viewed not as effects of having refugees permanently assigned to any given municipality, but rather as Intent-To-Treat effects (Ludwig *et al.*, 2008), capturing part of the total effects that would arise had all refugees stayed in their assigned municipality.

While the setup of the refugee allocation scheme suggests that municipalities received their share of refugees early or late quasi-randomly, it is still possible that the DRC may have favoured sending refugees to some types of municipalities earlier than others. In the next section, I implement a simple test for non-random assignment, regressing various municipality characteristics on the cumulative refugee shares in 1994. The tests show that refugee shares can predict municipality size, but can no predict local education and income levels. Importantly, the refugee shares do not predict prior shares of refugees in the municipalities, which would have been a concern for estimating the effects of increased shares of refugees. This provides evidence that the municipal allocation of refugees early versus late was indeed quasi-random.

4 Data

Studying the long-run effects of refugee on children's adulthood income by parental background requires both data on refugees allocated to local areas, as well as historical information on native's locations, and adulthood income information for children and their parents. I use data from Danish administrative datasets and publicly available sources, allowing me to collect this information.[9] The following subsections describes the construction of datasets used in the analysis, starting with the refugee shares.

4.1 Refugees

I identify refugees and the municipalities they are assigned to during the dispersal policy of 1986–1998 using information from the Danish demographic registers available from 1980 onwards and policy characteristics. The demographic register contains individual level information on the individual's age, home adress, country of origin and citizenship, parent-child linkages, and spousal information. While it is possible to discern natives and non-natives by country of origin in the Danish administrative data, the data only contains indicators for being a refugee from 1997. Following Dustmann *et al.* (2019), I therefore construct the refugee sample from the administrative dataset using demographic information, country of origin and citizenship, time of first arrival to Denmark, and information on whether the country of origin and citizenship were eligible for refugee status during the allocation period. Specifically, I identify refugees subject to the allocation policy as those individuals satisfying the following requirements:

[9] Access to the administrative data is provided on a limited basis. For a description of the criteria for access, see the following website from Statistics Denmark: `https://www.dst.dk/en/TilSalg/Forskningsservice`.

- The individual must originate from a country that Denmark received refugees from in the period 1986 to 1998.[10]
- The individual must enter Denmark for the first time in the period 1986 to 1998.
- The individual is not a child of a non-refugee.
- The individual arrived less than a year after any registered spouse or parents.
- The individual is not younger than 18 years and arriving without parents.

The final dataset of refugees match the data constructed in the related prior (Dustmann *et al.*, 2019). Table 1 shows the average percentage of summed refugees received by municipality relative to each municipality's population over the years 1986–1998. The increasing average and median percentage of refugees reflect the growing number of refugees received by the Danish municipalities. The increasing standard deviation reflects the differences across municipalities in the shares of refugees, which I use to identify the effects of refugee shares on the native children's adulthood earnings.

Table 1. *Cumulative refugee percentages of 1985 municipality inhabitants*

Year	Mean	Median	SD
1986	0.07	0.03	0.12
1990	0.15	0.09	0.16
1994	0.21	0.14	0.21
1998	0.46	0.41	0.32

Note: The table includes all refugees counted in the main sample who were subject to allocation policy.

[10]The countries from which Denmark received refugees in this period were Iraq, Iran, Vietnam, Sri Lanka, refugees from Lebanon with no citizenship, Ethiopia, Afghanistan, Somalia, Serbia-Montenegro, Croatia, Macedonia, and Slovenia.

4.2 The income of children and parents

In this section, I describe how I construct measures of permanent adulthood income for children and parents from demographic registers linking parents and children, and income registers containing yearly income from 1980-2015. Before describing the data construction, I first go through some of the typical concerns raised in intergenerational mobility studies about income measurement.

4.2.1 Measuring income and intergenerational mobility

The first concern in estimating refugee inflow effects on children's adulthood income conditional on parents' adulthood income is attenuation bias from transitory income shocks (Mazumder, 2005; Solon, 1992). In any given year, an individual's income may be subject to short-term fluctuation, for example due to temporary unemployment or receiving an unexpected bonus. This type of measurement noise introduces a simple errors-in-variables bias in linear regression coefficient estimates. The typical approach used to minimise this type of transitory bias is to average observed income across a set of years. I average parents' income over the full period of 1980 (the first year of observed income) to the year the child turns 24.[11] For children, I average income over six years whenever possible, using income information from 2010–2015, the latest years available in the data.

The second concern is life cycle bias, which can arise because some groups of children are on different initial income trajectories than others (Grawe, 2006; Haider and Solon, 2006;

[11] Chetty *et al.* (2014) suggest that measuring income over 4–5 years is sufficient to minimise attenuation bias when working with United States administrative income data from the IRS. Landersø and Heckman (2017) empirically find that Danish administrative data is somewhat more sensitive with respect to measuring parental income. Measuring only a few years of income when the child is young can lead to up to 30 percent bias in mobility estimates. They find that removing such bias is possible when aggregating over more than five years and including late teen years. I also investigate the effects of choosing age 20 and 18 as cut-offs, finding similar results in the main analysis.

Nybom and Stuhler, 2017). This can, for example, happen when some students get their highest degree at the end of academic or vocational high school, and others finish years later with university degrees. While the children at university have no or low incomes, the children working have relatively high incomes. However, as the students with university degrees finish up, they face steeper income profiles as highly educated workers, in general, have higher initial and lifetime wages than workers with less than university degrees (e.g., Goldin and Katz 2008). Landersø and Heckman (2017) find that measuring children's income in the early twenties in Denmark leads to a sufficiently strong bias that mobility estimates become negative. To handle the potential for life cycle bias, Nybom and Stuhler (2016) suggest measuring income at midlife whenever possible, based on their work with lifetime income profiles obtained from Swedish administrative data.[12] I measure the income of children as close to midlife as possible, starting in 2010 and ending in the latest year possible, 2015. On average, parents in the sample have their children in their late 20s. As a result of the measurement period from 1980 to the year the child turns 24, I therefore also capture parents' income by mid-life, as well as slightly before and after.

The third concern for measuring intergenerational mobility is the linearity of the relation between parents and children's income. Early papers on intergenerational mobility emphasised that the relation between parents and children's income might neither theoretically be linear (e.g., Becker and Tomes 1986), nor was it most often empirically linear (e.g., Couch and Lillard 2004; Grawe 2004). There are several ways in which one can capture such non-linearities, such as adding higher-order polynomials of right-hand-side variables in linear regressions to approximate curvature, spline regressions, or using quantile regressions

[12]Several others point to a similar approach to handling life-cycle bias, including Haider and Solon (2006). Landersø and Heckman (2017) shows similar results for Denmark as Nybom and Stuhler (2016) for Sweden, however with more limited income samples.

(e.g., Grawe (2004)). These approaches, however, become cumbersome due to many involved parameters when attempting to compare predicted child outcome from parental outcome for different groups.[13] A simpler approach taken in more recent studies, such as Chetty *et al.* (2014, 2020), Chetty and Hendren (2018a,b), and Landersø and Heckman (2017) is to transform actual income into income ranks. This is done by ranking all children or parents' income within a cohort. Chetty *et al.* (2014) show that the relation between children's family income rank and parents' income rank is nearly linear in the United States when measured with the IRS administrative income data, and Landersø and Heckman (2017) find the same result for Denmark using similar administrative data. As the relation between ranks is nearly linear, this means that the simple prediction of income ranks can be done in simple two-parameter linear models such as the one seen in equation 1, where R_i^c is the rank of children, R_i^p parental rank, and β_0 and β_1 the two parameters describing the relation.

$$R_i^c = \beta_0 + \beta_1 R_i^p + \varepsilon_i \tag{1}$$

A particularly useful feature of using this estimation procedure is that differences in outcomes between children who grow up in areas with relatively many refugees can be summarised by the shift in intercept, and the shift in slope from interacting these with a binary

[13] Assume that I attempt to predict children's outcome y_i^c from parents outcome y_i^p, and that the relationship is non-linear. I do this using a second order polynomial expression: $y_i^c = \beta_0 + \beta_1 y_i^p + \beta_2 (y_i^p)^2 + \epsilon$. This expression contains three parameters that I want estimate, which can then be used to predict child outcomes from parental outcomes: $\{\beta_0, \beta_1, \beta_2\}$. This can be done in a simple OLS regression. Assume instead that I want to estimate a model that distinguishes between two groups of children, as done, for example, by Chetty *et al.* (2020) when comparing intergenerational mobility of African American and White children in the United States. We can do this by interacting each parental variable with a dummy for belonging to the compared group, g_1; $y_i^c = \beta_0 + \beta_1 y_i^p + \beta_2 (y_i^p)^2 + \delta_0 g_{1,i} + \delta_1 y_i^p \cdot g_{1,i} + \delta_2 (y_i^p)^2 \cdot g_{1,i} + \epsilon_i$. The number of parameters to estimate and use for predictions have now risen to $3 * 2 = 6$, $\{\beta_0, \beta_1, \beta_2, \delta_0, \delta_1, \delta_2\}$. Furthermore, as the relation between outcomes is non-linear, simply looking at δ parameters is not sufficient to tell the predicted differences in outcomes of children from the two groups. Instead, one must use all parameters to estimate the child outcome from any given parental income.

indicator for growing up in these municipalities. In addition, it is possible to convert the income ranks to actual income by mapping the ranks back to actual income. This can be done, for example, by using local linear regressions to estimate the expected family income at any given child's family income rank. To simplify the analysis, I estimate models using children's family income ranks and parental income ranks measured within each child's cohort.

4.2.2 Income data construction

I first identify the children and parents, starting by finding all individuals in the income registers observed in the period 2010–2015 when I measure children's income, who were born between 1973 and 1980. I then match children to parents using the unique identifiers from demographic registers for each year from 1980, the first year in the registers, until the year the child turns 24.[14] For the 1973 cohort, this is 1997, and for the 1980 cohort, it is 2004. To enter the main sample, the child-parents pair must be observed at least one year from 1980 until the year the child turns 24, and for at least one year in this period, at least one parent must have observable income in the administrative register. Similarly, the child must have observable income for at least one year from 2010 to 2015. Table 2 shows the total number of observations for each cohort by conditions described in the column header. The sample size falls from 75,413 observations for the 1973 cohort to 61,396 in the 1980 cohort, reflecting a general decline in fertility in the Danish population over the same period. In Columns 3 and 4 I show the number of children for each cohort for whom a mother and or father is observed. I observe slightly fewer fathers than mothers in the dataset, but most children have both an observed father and mother, and (less than one percent do not).

[14]In less than 0.2 percent of child-year observations, a child changes parents in the register. This can happen, for example, if the child is adopted. For these children, I assign the parent whom the child is registered with for longest among the observed years, and resolve ties by picking the first observed parent.

Table 2. *Observations by cohort*

Cohort	Child and Parents Rank	Mother	Father	Municipality
1973	75470	75456	74888	75461
1974	71402	71406	70835	71409
1975	71620	71527	70975	71596
1976	72195	72130	71455	72203
1977	66389	66324	65703	66400
1978	63055	62990	62366	63074
1979	63760	63728	63023	63803
1980	61376	61335	60614	61424

Note: Child and parents imply that both the child and both parents are observed with income in the dataset. Mother implies that the mother is observed with income, and Father similarly for fathers. Municipality refers to the number of children for whom a municipality in childhood could be observed.

As the primary interest is long-run income for all children regardless of their labour market relation, I measure income as the sum of all income from salaries, income from own business, capital income, and all taxable social transfers, such as unemployment benefits.[15] For children, I measure income as the time-average of children's family income. This includes both the child and any registered partner's income. I focus on the child's family income, and not individual income, as this is the main approach taken in recent intergenerational mobility literature, making comparisons to prior research possible.[16] For parents, I measure their income as the time-averaged sum of mothers' and fathers' incomes. Before taking time-averages, I deflate all income to 2015 values using the Danish CPI.

Table 3 shows summary statistics on the income of parents and children, and the child's family income, and figure B.1 in the appendix shows histograms of children's individual and family income, and parents' total and individual incomes. The table and figures indicate

[15]For a full description of these variables in Danish, see `https://www.dst.dk/da/Statistik/dokumentation/Times/personindkomst/perindkialt` and `https://www.dst.dk/da/Statistik/dokumentation/Times/personindkomst/perindkialt-13`.

[16]I also include results where I substitute children's family income with the child's personal income. These models show similar results as in the main analysis with children's family income.

that general economic well-being has increased over time, but also that there is substantial variation in income across both the parental and child income distributions.

The summary table also shows statistics on the demographic characteristics of the sample. The table first shows that nearly all children are observed across all six years in the sample (the mean number of child observations is 5.9), suggesting that the data has good coverage. Females make up 50 percent of the sample, and nearly 40 percent receive some sort of tertiary degree before first observed between 2010 and 2015. In 52 percent of child observations, the child is married, but 71 percent live together with some type of partner. This is important as it is possible that children's individual and family income rank may differ as a result. I account for this in the robustness check where I use children's individual outcome as a main test of statistical significance.

4.2.3 Children's home municipality

To estimate the effects of refugee allocation on children requires observing for each child a municipality in which they grew up and where they would have been affected by the refugee allocation. I construct the housing information by assigning each child to the municipality I observe them living in for most years from 1980 to the age of 20. Unlike the measurement of parents' income, which I continue to age 24, I choose the earlier age 20 here as many children move away from their parent's homes and home municipalities in their late teens and early twenties to study, work, or start their own families. For children who live in more than one municipality, which many children do as they move away from their parents to start tertiary education, I assign them to the municipality they spent most of their time in. When a child

Table 3. *Summary Statistics*

	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
Income						
Child's family income	546,040	688.287	695.753	432.656	658.881	845.361
Child's income	546,019	392.523	514.514	277.568	358.998	450.854
Partner's income	450,719	406.798	527.016	283.341	364.166	463.566
Parents' income	545,311	627.123	443.387	465.960	580.777	715.568
Mom's income	542,749	242.988	122.991	184.070	236.370	292.430
Dad's income	534,069	412.097	419.397	285.354	355.148	458.672
Child's housing						
Years in main municipality	545,370	14.966	3.913	13.000	15.000	18.000
Municipalities lived in	545,370	1.747	1.027	1.000	1.000	2.000
Years observed in muni. sample	545,370	16.917	2.926	15.000	17.000	19.000
Cohabitating with parents	545,370	0.683	0.373	0.375	0.875	1.000
Cohabitating with mother	545,370	0.151	0.280	0.000	0.000	0.188
Cohabitating with father	545,370	0.024	0.111	0.000	0.000	0.000
Not cohabitating with parents	545,370	0.141	0.213	0.000	0.062	0.176
Child's characteristics						
Observations (2010–2015)	546,085	5.903	0.563	6.000	6.000	6.000
Female	546,085	0.493	0.500	0.000	0.000	1.000
Married	546,085	0.519	0.462	0.000	0.667	1.000
Cohabiting w. partner	546,085	0.711	0.397	0.333	1.000	1.000
Number of partner's	546,085	0.877	0.465	1.000	1.000	1.000
Upper sec. or tert. degree	546,683	0.766	0.424	1	1	1
Tertiary degree	546,683	0.381	0.486	0	0	1
Employed	543,950	0.837	0.302	0.857	1.000	1.000
Outside labor force	543,950	0.127	0.278	0.000	0.000	0.000
Mother's characteristics						
Observations (1980–child age 24)	545,493	20.781	3.181	19.000	21.000	23.000
Age at birth	542,678	26.491	4.787	23.000	26.000	29.000
Married	542,678	0.796	0.327	0.667	1.000	1.000
Immigrant	542,678	0.049	0.216	0.000	0.000	0.000
Upper sec. or tert. degree	546,683	0.532	0.499	0	1	1
Tertiary degree	546,683	0.202	0.402	0	0	0
Employed	542,388	0.774	0.297	0.650	0.909	1.000
Outside labor force	542,388	0.158	0.267	0.000	0.000	0.200
Father's characteristics						
Observations (1980–child age 24)	540,435	20.025	3.969	18.000	20.000	23.000
Age at birth	533,550	29.317	5.631	26.000	29.000	32.000
Married	533,550	0.823	0.312	0.773	1.000	1.000
Immigrant	533,550	0.049	0.215	0.000	0.000	0.000
Upper sec. or tert. degree	546,683	0.619	0.486	0	1	1
Tertiary degree	546,683	0.199	0.399	0	0	0
Employed	533,102	0.862	0.247	0.840	1.000	1.000
Outside labor force	533,102	0.086	0.204	0.000	0.000	0.048

Note: The table shows summary statistics for the main sample based on administrative data. This consists of children from the 1973-1980 cohorts observed with income information between 2010 and 2015, and at least one observation of parental income from 1980 until the year the child turns 18. Income is the pre-tax sum of salaries, own business and capital income, and public transfers, deflated to 2015 values using the Danish CPI. Partners are the cohabiting inidividuals with whom either (1) the child is married, (2) the child is not married but has shared children, (3) cohabitates with in housing with no other adults, and is less than 15 years younger or older than. The main municipality is the one the child is observed in for most years from 1980 to the year the child turns 20, and municipalities lived in are total municipalities in the same period. Highest attained education is the first observed education level for parents in 1980 to the year of child age 18, and 2010–2015 for children.

has spent an equal amount of time in two municipalities, I assign the child to the one the child is first observed in.

The final column in Table 2 shows that I obtain housing information for nearly all children that I observe in adulthood. The descriptive statistics on children's outcomes in Table 3 in addition indicate the while children on average are observed in 1.7 municipalities, they spend a substantial amount of time within one municipality, with an average of 15 years spent in their primary assigned municipality.

5 Empirical Strategy

This section describes the main empirical strategy I use in the paper to estimate the effects of refugees on children's long-run outcomes. I start by describing the self-selection challenge often faced in studies of immigrant or refugee effects, and how the use of the refugee allocation policy allow me to overcome this challenge. I then briefly summarise the estimation procedure used in the intergenerational mobility literature to obtain estimates of expected child income (rank) by parental income (rank), and how I use the same framework to estimate the conditional effect of refugee shocks.

A common problem in studies of the effects of immigrants and refugees on natives is self-selection or assignment into specific areas (e.g. Borjas 1987, and Abramitzky and Boustan 2017). The fact that immigrants tend to self-select into living in areas which already have relatively high levels of similar immigrants is the basis for the commonly used shift-share instrument, where prior levels of settled immigrants is used to instrument present levels (e.g., Jaeger *et al.* 2018; Card 2001; Dustmann *et al.* 2017; Derenoncourt 2022; Tabellini

2020). I solve the self-selection problem using the quasi-random assignments of refugees to municipalities in Denmark in the period 1986 to 1998. In this period, children observed in the sample I investigate in the paper were between the age of six and 25 (1980 and 1973 cohorts). As described in Section 3, the policy lead to higher shares of refugees within some municipalities, and in Section 4 I show that these refugees shares, in fact, were plausibly exogenous as they could not predict local municipality characteristics. Because refugees were only initially assigned but could move around, the estimates using the initial allocation should be interpreted as Intent-To-Treat effects from having a higher refugee share initially assigned to the municipality.

In the discussion of potential mechanisms, I showed that it is likely that effects running through either the peer effects of school flight channels are likely to lead to treatment effects varying by parental income, as more affluent families, for example, may be more likely to move their children to private schools. To estimate this effect, I draw on the estimation procedure from the studies of intergenerational mobility. The goal in most intergenerational mobility studies has been to describe the relation between parents' and children's outcomes. In the simplest approach, the authors regress a child's outcome on the parents' outcome, preferably measured in some period when the child is growing up, in which case the regression coefficient for parents characteristic summarises the rate of transmission from parents to children. This model can be seen in equation 2, where Y_i^c is child i's outcome, and Y_i^p the parents' outcome, and β_1 describe the transmission rate from parents to children.

$$Y_i^c = \beta_0 + \beta_1 Y_i^p + e_i \tag{2}$$

As a measure of parental and child adulthood outcomes, I focus on children's and parents' income rank (Chetty *et al.*, 2014). In the income rank model, the parameters β_0 and β_1 produces an affine function from parents' income rank to the children's income rank. In the terminology of Chetty *et al.* (2014), β_0 is the absolute mobility at parental income rank 0, and β_1 as relative income rank mobility, the expected increase in a child's income rank when parental income rank increases by one percentage point. The model assumes a linear relation between parents' and children's income. When this is satisfied, estimates of the parameters can be used to predict children's income rank from parents'.

To estimate the effect of refugee inflows on children's income conditional on their parents' income, I focus on the reduced form model in Equation 3, where I interact R_i^p, parents' income rank, with an indicator for the child growing up in a municipality with more than the median percentage of refugees arriving in the municipality by 1994 relative to the 1986 population in the municipality, I_m,

$$R_i^c = \lambda_0 + \lambda_1 R_i^p + \delta_0 I_m + \delta_1 R_i^p I_m + \epsilon_i. \tag{3}$$

The indicator I_m and interaction between I_m and parental income rank allows me to estimate how child income ranks vary with exposure across parental income ranks. The parameter δ_0 measures the difference in expected income rank of children born to parents at income rank 0, and δ_1 measures the difference in relative rank mobility for children in municipalities above and below the median share of refugees.

One main concern with the identification of the refugee effects is whether the allocation of refugees is plausibly ecogenous to other municipal characteristics that may affect the local

native children's long-run outcomes. I am able to show that the allocation of refugees is plausibly exogenous to characteristics of the municipalities the refugees arrive in. Table 5 shows the coefficients from regressing a set of municipal characteristics on the cumulative share of refugees to the 1986 population (the reference point for the allocation policy). The municipal characteristics include the log of 1980 population in the municipality, the percentage of non-EU immigrants, the log average family income within the municipality, and the percentage of vocationally- and tertiary-educated individuals within the 16 to 69-year-old population. The findings show that the refugee shares cannot predict the municipal characteristics likely to impact schooling quality of natives, such as the average family income, and the share of vocationally- and tertiary-educated individuals in the municipality. This indicates that whether a municipality received it's share of refugees early on is not related to these characteristics, and thus supports the identifying assumption. It is particularly reassuring that the policy is not able to predict pre-reform non-EU immigrant shares, which could be driving results through the channels described in Section 2. The only exception is a correlation with the municipal size. This may be explained by the fact that the largest municipality, Copenhagen, was subject to special allocation rules, as the allocation depended on the ease with which the DRC could find housing in the municipalities or establish their offices. In robustness checks, I include first include pre-policy sizes of municipalities and secondly exclude children growing up in the city of Copenhagen, showing that the findings are robust to accounting for size-specific characteristics.

The findings may also depend on the definition of the refugee share, the rank-rank linearity assumption underlying Equation 3, and how incomes are measured ahead of constructing income ranks. In supplementary analyses, I show that substituting the main child's family

Table 5. *Predicting municipal characteristics with refugee allocation to test for randomized allocation*

	Dependent variable:				
	Log of Population 1980	Pct. non-EU Immigrants	Log mean family income	Pct. vocationally educated	Pct. tertiary educated
	(1)	(2)	(3)	(4)	(5)
1994 cum. pct. refugees	1.246***	0.241	−0.018	0.010	0.014
	(0.208)	(0.146)	(0.034)	(0.010)	(0.014)
Constant	9.140***	0.360***	12.949***	0.327***	0.149***
	(0.062)	(0.044)	(0.010)	(0.003)	(0.004)
Observations	253	253	253	253	253
R^2	0.126	0.011	0.001	0.005	0.004

Note: The table shows coefficients from regressing municipal characteristics on the 1994 cumulative percentage of refugees arrived since 1986 to 1986 population. Parentheses show heteroskedasticity robust standard errors. OLS standard errors show similar results. Log population is the logarithm of all inhabitants in the municipality in 1980. Regressions using the number of inhabitants show qualitatively similar results. The number is collected from Statistics Denmark's (2019) BEF1 table. The pct. of non-EU immigrants is calculated for 1980 from the administrative datasets, and include all registered non-EU immigrants as percent of 1980 population. The *log mean family income* is the 1987 to 2000 average full pre-tax income of families in Danish municipalities (converted from post-reform to pre-reform municipalities) including public transfers and capital income, collected from Statistics Denmark's table INDPF122. I rely on the publicly available Statistics Denmark table as the administrative datasets used in the analysis have been limited in early years. *Pct. vocationally educated* is the share of average from 1991 to 2000 pct of inhabitants age 16-69 who have a vocational education as highest attained education. The data is collected from Statistics Denmark's table HFU1. *Pct. tertiary educated* is similarly calculated as the pct. of the population with a short or professional bachelor or a University degree. Stars indicate significance levels, * is 10 pct, ** 5 pct, and *** 1 pct.

income rank with the child's own income rank, using a continuous rather than binary measure of immigrant exposure, and using a non-parametric local linear regression estimation technique do not change the main findings.

6 Estimates of the refugee effect

This section contains the estimates of the refugee effects on children's adulthood income ranks conditional on parental income income. I first estimate an income rank model and

show that the estimated coefficients match those found in previous Danish intergenerational mobility studies. I then estimate the main model from Equation 3, finding substantial negative effects of 1.8 percentage points for children born to parents at the 25th income percentile. The effect is stronger for children with parents at lower income ranks. I then do several robustness checks to check the results, starting by including cohort fixed effects, including controls for population size in 1980 (six years before the policy was initiated) and for 1985 share of refugees in the municipality, and finally excluding the largest metropolis municipality, Copenhagen, from the analysis as Copenhagen was partly absented from the policy allocation. These robustness checks do not change the main results.

6.1 The effect of refugees on income rank

I first show that the baseline model, regressing children's family income rank on parents' without interacting with refugee shares, is in line with prior research from Denmark by Landersø and Heckman (2016). The coefficient estimates can be seen in Table 6, Column 1. The estimate of β_0 is 0.368, and the estimate of relative mobility, β_1, is 0.293. These estimates are in line with prior evidence on intergenerational income rank mobility in Denmark (I discuss this in appendix C), suggesting that my findings are not driven by an unreasonable sample selection.

The estimates of the effects of the child's municipality getting an above-median share of reguees prior to 1994 is shown in Table 6, Column 2, where the estimate of δ_0 and δ_1 are -.025 and .033. Both estimates are statistically signficantly different from zero.The parameter estimates indicate, first, that the poorest children who grow up in areas with above-median

Table 6. *The relation between parents and children's family income, conditional on parental income, for municipalities with above and below-median shares of cumulative refugees in 1994.*

	Child's family income rank				
	(1)	(2)	(3)	(4)	(5)
Parents' rank	0.294***	0.272***	0.272***	0.272***	0.277***
	(0.001)	(0.003)	(0.003)	(0.003)	(0.003)
Above median refugee cum. pct (1994)		−0.024***	−0.018***	−0.014***	−0.010***
		(0.002)	(0.002)	(0.002)	(0.002)
Parents' rank × Above med. ref.		0.031***	0.026***	0.026***	0.023***
pct (1994)		(0.003)	(0.003)	(0.003)	(0.003)
Municipal inhabitants (1985)				−0.00000***	−0.00000***
				(0.000)	(0.000)
Municipal immigrant share (1985)					−0.006***
					(0.0003)
Cohort FE	Y	Y	Y	Y	Y
Copenhagen omitted			Y	Y	Y
N	545,267	525,307	496,672	496,672	496,672
R^2	0.081	0.081	0.078	0.079	0.080

The table shows intergenerational mobility estimates interacted with an indicator for the municipality having above-median share of refugees to 1986 population in 1994 in the municipality the child grew up. The sample consist of all children observed with income in 2010–2015, with at least one parental income observation from 1980 to the year the child turns 24, and where the child can be assigned to a municipality from 1980 to the year the child turns 20. Child's family income is the sum of the child's and partner's income, average between 2010 and 2015 pre-tax salary, personal business and capital income, and public transfers. Parents income is the sum of mother's and father's income averaged from 1980 to the year the child turns 24. Ranks are calculated within cohort and group (child's family, child's, parents'). Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

shares of refugees have 2.5 percentage points lower family incomes than their peers in municipalities with fewer refugees. Secondly, the positive effect on the slope coefficient means that as parental income ranks increases, the negative effect on children diminishes. This implies that children born to relatively well off parents are not affected by the refugee inflow.

To illustrate the effect sizes, consider a children born to parents at the 25th income percentile. For children in the areas with below-median levels of refugees by 1994, the expected family income rank is $38.6 + 25 * 0.27 = 45.4$. For children who grow up in municipalities with above-median shares of refugees, the expected income rank is $(38.6 - 2.5) + 25 * (0.27 + 0.033) = 43.6$. The difference is 1.8 percentage points, or the equivalent

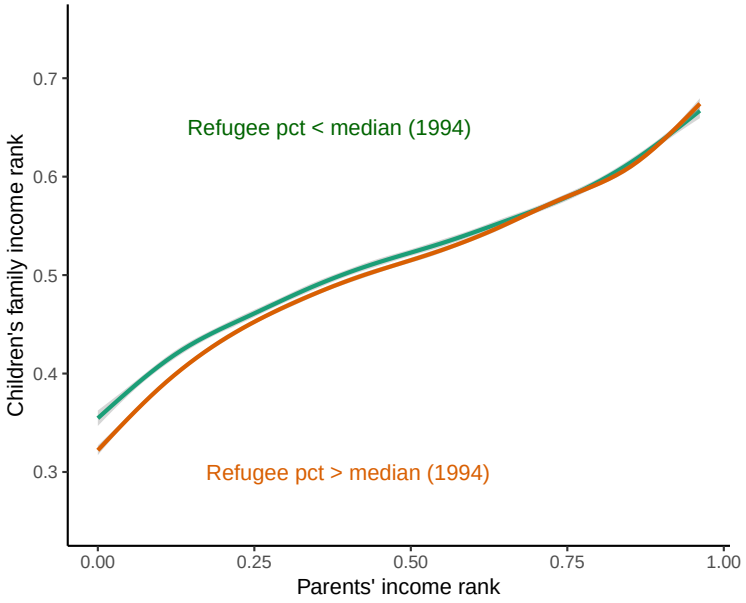
of USD 2,100 in 2015 values. I calculate the income equivalence of children's family income rank from local linear approximation of children's family income by rank. Figure B.2 shows the relation between children's income. First, I regress children's family income on family income ranks using a simple linear relation within the interval of 40 to 50th family income ranks. I then use the estimated linear relation to predict expected family income from income ranks. For children born to parents at the bottom 10 percent, the effect estimate is 2.17 percentage points and USD 2,700. About 75 percent of all children born to parents below the 25th income percentile grow up in areas with above-median shares of refugees. In the sample of 537,000 children, this corresponds to about 98,000 children, out of a population of 5.7 million, who in their adulthood earn (at least) an average of USD 2,110 less than children from areas with less than median shares of refugees allocated. The effect of refugees on expected family income percentiles fades out above the median parental income rank, at the 75th income percentile.[17]

I graphically depict the estimated effects in a linearly parameterised model in appendix figure D.1, which shows the the expected income of children in municipalities with above and below-median shares of refugees. However, it is also possible to test whether the effects are indeed linear by estimating the model nonparametrically. I do this in figure 1, estimating local linear regressions separately for the above-median and below-median refugee share municipalities. Grey bands show confidence interval based on bootstrapped standard errors. The non-parametric plot supports the main findings: the lower the parental income, the

[17]The expected income percentile at which the two populations become equal can be calculated from the linear expected income rank relations. This happens at $R_i^{p*} = \frac{\delta_0}{\delta_1} = \frac{.025}{.033} = .7576$. Multiplying by 100, the parental rank at which expected family income is similar is the 75th income percentile.

stronger the effects of the refugee shares. It also shows reasonable support for the choice of a linear baseline specification.

Figure 1. *Local linear regressions of child's family income rank by parents' income rank, comparing areas with above and below 1994 median share of refugees.*



6.2 Robustness Tests

A potential concern with respect to the causal interpretation of the main result is that the allocation of refugees is not entirely exogenous concerning the relation between parents and children's income ranks. The allocation mechanism considered Copenhagen, the capital of Denmark, differently from the rest of the country. A second potential concern is that the DRC workers considered municipalities existing levels of immigrants when assigning new refugees. Finally, a third concern might be that the Refugee share variable in main model in Table 6, Column 2, is picking up cohort variation in expected outcomes of children.

To mitigate these concerns, I re-estimate the main model, controlling for these factors. First, in Column 3, I estimate the model with cohort fixed effects, which control for trends in intergenerational mobility over cohorts that the refugee variable might be picking up. The results are similar. Secondly, I control for the municipal population size in 1980 and the share of immigrants to the full population in each municipality in 1985 in Column 4. Doing so slightly lowers the point estimates of effects to -.01 and .025, but does not change the main result. Finally, I consider the effect of leaving out Copenhagen, the Capital municipality, in Column 5. Doing so does not change the results compared to the prior robustness checks.

An additional concern with the specification is the binary variable for refugee shares. As an alternative, I interact parental income ranks with a continuous measure of refugee shares in Table 7, using instead the cumulative percentage of refugees to 1986 population in municipalities as a measure of the refugee inflow. The results of this regression are qualitatively similar to the main specifications, showing that the results are robust to reasonable alternate specifications.

6.3 Heterogeneity by Gender and Municipal Average Income

In this section, I investigate if the effects of the reform vary by gender and municipal income. I do so by splitting the full sample of children first by gender and then by average family income in the municipality and estimating the main specification for each subsample.

The effects of the reform may vary by gender if, for example, boys are more likely to react to anti-social behaviour in classes. Evaluations of the MTO experiment has found that boys become more likely to have anti-social behaviors after being allocated to new areas (Ludwig *et al.*, 2008). Table 8 shows the results of regressions with and without controls by gender.

Table 7. *The relation between parents and children's family income, conditional on continuous cumulative refugee shares by 1994 as a percentage of 1986 population.*

	Child's family income rank				
	(1)	(2)	(3)	(4)	(5)
Parents' rank	0.294***	0.275***	0.273***	0.273***	0.277***
	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Cumsum refugee pct (1994)		−0.046***	−0.039***	−0.026***	−0.027***
		(0.003)	(0.003)	(0.004)	(0.004)
Parents' rank × cumsum refugee		0.063***	0.058***	0.058***	0.052***
pct (1994)		(0.006)	(0.006)	(0.006)	(0.006)
Municipal inhabitants (1985)				−0.00000***	−0.00000***
				(0.000)	(0.000)
Municipal immigrant share (1985)					−0.006***
					(0.0003)
Cohort FE	Y	Y	Y	Y	Y
Copenhagen omitted			Y	Y	Y
N	545,267	525,307	496,672	496,672	496,672
R^2	0.081	0.081	0.078	0.079	0.080

The table shows intergenerational mobility estimates interacted with the cumulative share of refugees to 1986 population in the municipality the child grew up. The sample consist of all children observed with income in 2010–2015, with at least one parental income observation from 1980 to the year the child turns 24, and where the child can be assigned to a municipality from 1980 to the year the child turns 20. Child's family income is the sum of the child's and partner's income, average between 2010 and 2015 pre-tax salary, personal business and capital income, and public transfers. Parents income is the sum of mother's and father's income averaged from 1980 to the year the child turns 24. Ranks are calculated within cohort and group (child's family, child's, parents'). Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

I find no evidence of differential effects along these lines, with point estimates being nearly similar.

The effects may vary by municipality income levels if, for example, local resources affect how well the schools integrate refugees. Previous literature on school spending suggests that available resources can have meaningful impacts on students' academic outcomes (Jackson *et al.*, 2021). Table 9 shows the estimated effects from splitting the sample by the average family income of the municipality residents (averaged over 1987 to 2000), with and without adding the identification controls. Figure D.2 in the appendix shows this result nonparametrically, estimating children's income rank by parental income rank, splitting samples by

Table 8. *The relation between parents and children's family income, conditional on parental income, interacted with cumulative refugee share by 1994 of 1986 population.*

	Child's family income rank			
	Female sample		Male sample	
	(1)	(2)	(3)	(4)
Parents' rank	0.253***	0.258***	0.289***	0.295***
	(0.004)	(0.004)	(0.004)	(0.004)
Above median refugee cum. pct (1994)	−0.025***	−0.012***	−0.023***	−0.008***
	(0.002)	(0.002)	(0.002)	(0.002)
Parents' rank × Above med. ref.	0.031***	0.024***	0.030***	0.023***
pct (1994)	(0.004)	(0.004)	(0.004)	(0.004)
Municipal inhabitants (1980)		−0.00000***		−0.00000***
		(0.000)		(0.000)
Municipal immigrant share (1985)		−0.006***		−0.006***
		(0.0004)		(0.0004)
Cohort FE	Y	Y	Y	Y
Copenhagen omitted			Y	Y
N	258,793	244,707	266,514	251,965
R^2	0.074	0.072	0.090	0.088

The table shows intergenerational mobility estimates interacted with an indicator for the municipality having above-median share of refugees to 1986 population in 1994 in the municipality the child grew up. The sample consist of either female or male children observed with income in 2010–2015, with at least one parental income observation from 1980 to the year the child turns 24, and where the child can be assigned to a municipality from 1980 to the year the child turns 20. Child's family income is the sum of the child's and partner's income, average between 2010 and 2015 pre-tax salary, personal business and capital income, and public transfers. Parents income is the sum of mother's and father's income averaged from 1980 to the year the child turns 24. Ranks are calculated within cohort and group (child's family, child's, parents'). Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

refugee shares in 1994 and by average family income in the municipality. I find substantial variation in effects by municipal income. With full robustness controls, children growing up in municipalities with below-median average family income have a refugee-share-by-parental-income-rank slope of 0.038, whereas children from above-median income municipalities have an interaction slope of 0.016. Thus, children in low-income municipalities appear to be more affected by the refugee arrival.

Table 9. *The relation between parents and children's family income, conditional on parental income, interacted with cumulative refugee share by 1994 of 1986 population.*

	Child's family income rank			
	Below median income municipality		Above median income municipality	
	(1)	(2)	(3)	(4)
Parents' rank	0.272***	0.273***	0.272***	0.276***
	(0.004)	(0.004)	(0.004)	(0.004)
Above median refugee cum. pct (1994)	−0.034***	−0.014***	−0.012***	−0.008***
	(0.002)	(0.002)	(0.002)	(0.002)
Parents' rank × Above med. ref.	0.040***	0.038***	0.018***	0.016***
pct (1994)	(0.005)	(0.005)	(0.004)	(0.004)
Municipal inhabitants (1980)		−0.000		−0.00000
		(0.000)		(0.00000)
Municipal immigrant share (1985)		−0.008***		−0.006***
		(0.001)		(0.0003)
Cohort FE	Y	Y	Y	Y
Copenhagen omitted			Y	Y
N	247,903	219,268	273,868	273,868
R^2	0.086	0.084	0.074	0.075

The table shows intergenerational mobility estimates interacted with an indicator for the municipality having above-median share of refugees to 1986 population in 1994 in the municipality the child grew up. The average municipal family income is measured using Statistics Denmark Table INDPF122 data, averaged by municipality over 1987–2000. The estimation sample consist of children observed with income in 2010–2015, with at least one parental income observation from 1980 to the year the child turns 24, and where the child can be assigned to a municipality from 1980 to the year the child turns 20. Child's family income is the sum of the child's and partner's income, average between 2010 and 2015 pre-tax salary, personal business and capital income, and public transfers. Parents income is the sum of mother's and father's income averaged from 1980 to the year the child turns 24. Ranks are calculated within cohort and group (child's family, child's, parents'). Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

7 Conclusion

In this paper, I investigate the causal effect of refugee inflows on native children's adulthood family income. The identification of causal effects comes from a Danish refugee allocation mechanism in place from 1986 to 1998, which randomly allocated refugees to Danish municipalities over this time-period. As a result, Danish municipalities had varying levels of refugees to local pre-mechanism population over time and place.

I focus on children's expected family income in adulthood conditional on the income in the family they grow up in. Estimating the effect of the share of refugees to 1986 local population in the municipality the child grows up in, I find a negative income effect for children from low-income families, and no effect for children from high-income families. For children born to parents at the 25th income percentile, the effect is a difference of 1.8 percentile ranks between children growing up in municipalities with above and below-median shares of refugees. This corresponds to a difference of approximately USD 2,100 in 2015 value. The effect is larger for children born to parents at the 10th parental income percentile at 2.1 percentile ranks and USD 2,700. The result is robust to a range of tests of the empirical specification, including the use of a continuous measure of refugees, exclusion of the capital municipality in Denmark, and controlling for pre-mechanism population size and refugee shares of the municipalities. In supplementary analyses find that the effects are larger present in municipalities with less affluent families. This is persistent with an explanation, where high-income municipalities have more resources available to integrate refugees effectively and can provide better learning environments for both refugee and non-refugee children.

The results of the analysis are relevant for understanding the implications of refugee allocation policies as well as for the literature on intergenerational mobility. In particular, the results emphasise the need for considering impacts on poorer native children in addition to those on the incoming refugees. A potential way of doing so, and that would be suggested also by prior research on refugee outcomes (e.g., Damm 2014, and Damm and Dustmann 2014), is to allocate refugees to areas with better functioning labour markets, less crime, and larger degrees of social mobility. The paper contributes to the literature on spatial variation in intergenerational mobility (e.g., Derenoncourt (2022); Chetty *et al.* (2020)) by

showing that policies that are not directly targeted towards the local population may still have spillover effects on the economic mobility of the local population.

The paper gives rise to three important future research avenues. First, the findings rely on initial assignment of refugees to municipalities. As a result, it is not possible to investigate whether the effects, in fact, are driven by interactions of non-refugee children and the refugee's children, as I posit in Section 2. An important avenue for future research would be to estimate the causal effect on children's long-run income from having more refugees in local school classes. Secondly, due to data restrictions, I cannot investigate if the effects found in the study to some extent vary by the characteristics of the refugees coming into the local area. Over the period 1986 to 1998, Denmark received refugees from 14 different countries. As noted by Damm (2014), these refugees did vary somewhat in their educational background. Because of varying circumstances leading to the migration from home countries, they will also likely have different experiences from war and/or democratic crises in their home countries. These distinctions in refugee characteristics are additionally important for the external validity of the findings as applied to refugees from other countries. Clarifying how these differences affect the long-run income effects is important work for future research. A final avenue for future research is to combine the long-run effects findings from this paper with additional information about the refugee policy in place between 1986 and 1998 to help estimate total benefit-costs of the refugee policy. This is particularly important for highlighting the extent to which alterations, such as assigning refugees to more affluent municipalities, is likely to produce more cost-effective policies. One way of approaching such a future analysis is to incorporate the findings from prior studies on refugee and native adult

effects (e.g., Damm 2014, and Damm and Dustmann 2014) in Marginal Value of Public Funds approach described by Hendren and Sprung-Keyser (2020).

Affiliations

[1]Department of Economics, University of Copenhagen, Øster Farimagsgade 5, DK-1353 Copenhagen K, Denmark.

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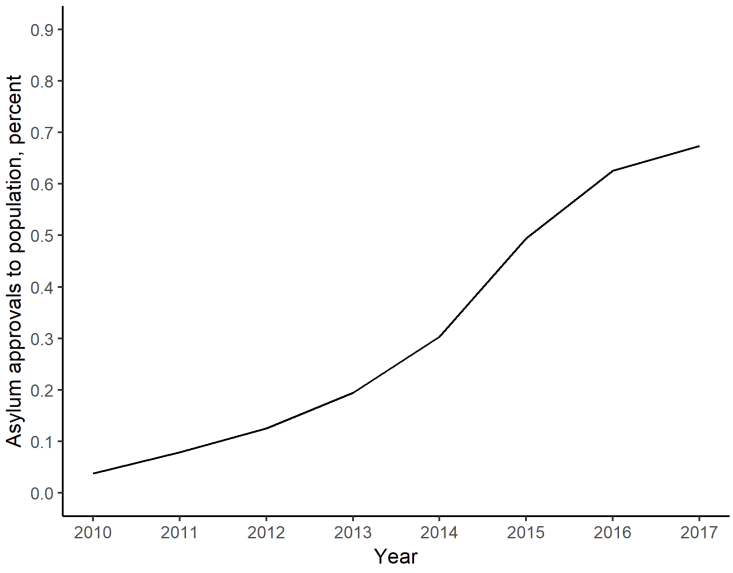
A Refugee inflows 2010 to 2017

Figure A.1 shows the the cumulative number of approved asylum requests to the total population in Denmark from 2010 to 2017 using data from Statistics Denmark, tables VAN66 and FOLK4.

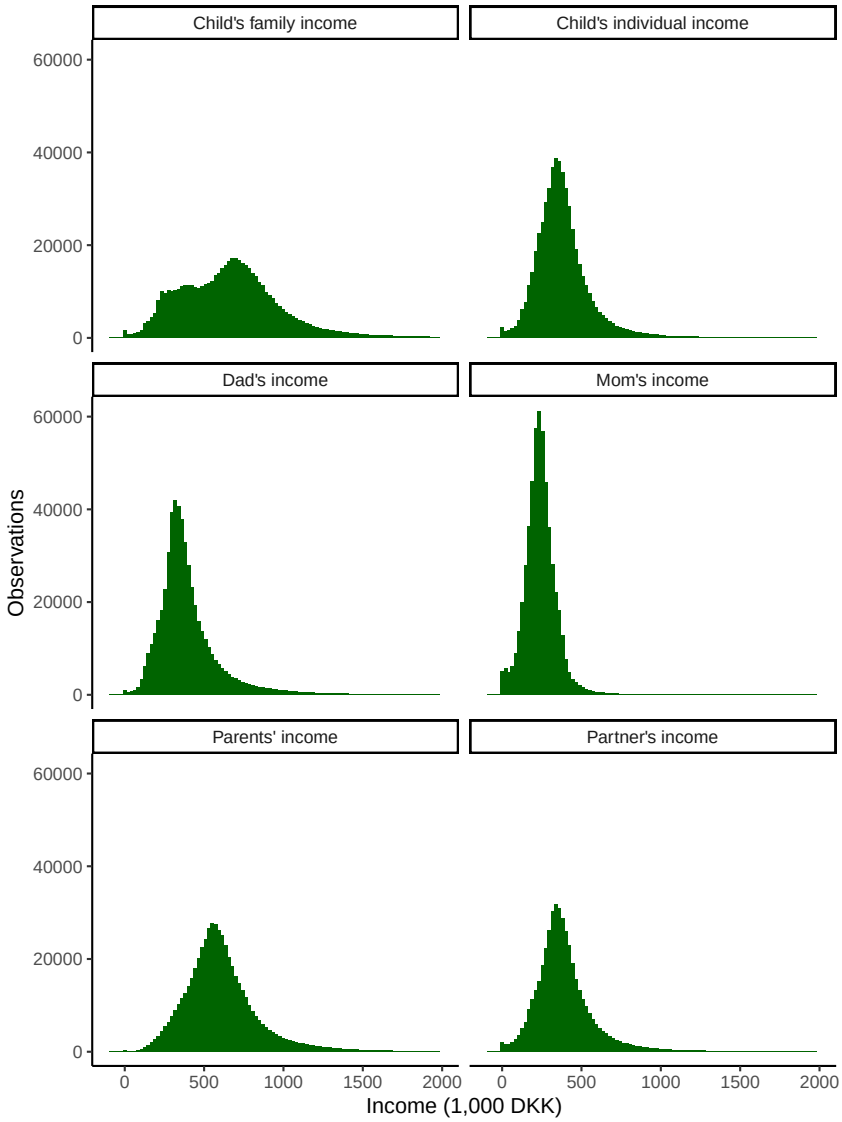
Figure A.1. *Danish cumulative approved asylum applications to total population, 2010–2017*



Note: The data is collected from Statistics Denmark, Tables VAN66 and FOLK3.

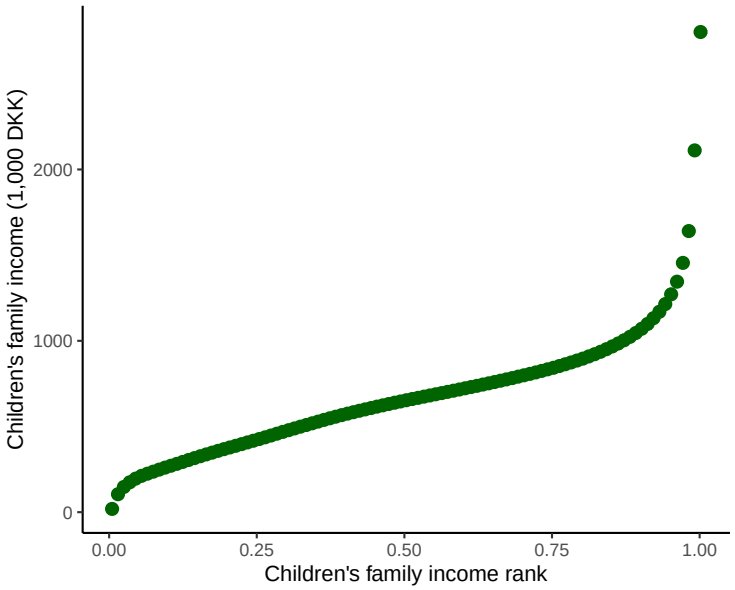
B Descriptive Statistics

Figure B.1. *Children's time-averaged family income (1,000 DKK), 2010–2015.*



Note: The figure shows a histogram of children's time-averaged family income, their individual income, and parents total and invididual incomes. For children, the income is average from 2010 to 2015, and for parents the income is averaged over 1980 to the year the child turns 24. Family income is the sum of the child's and any spouses' total income before taxes. Total income is the sum of income from salaries, own business income, capital income, and all transfers subject to taxation. All income is deflated to 2015 values prior to time-averaging.

Figure B.2. *Binned mean of Children's family income (1,000 DKK) by children's family income rank.*



Note: The figure shows binned means of children's family time-averaged family income over the period 2010–2015 by their family income rank, comparing them to other children in their cohort. The income is measured as the sum of the child's and any spouses' total income and deflated using the Danish CPI to 2015 DKK values. Total income is the sum of income from salaries, own business income, capital income, and all transfers subject to taxation.

C Baseline Intergenerational Mobility Estimates

To assert that the dataset used produces results in line with prior research on intergenerational mobility from Denmark, I estimate and show standard intergenerational income rank mobility estimates in Table C.1.[18] The estimate comes from regressing a measure of children's family or individual income rank on some measure of parental income rank. Two studies provide recent points of comparison. Landersø and Heckman (2017) estimate total pre-tax income rank mobility to between 0.256 when including all observations, and 0.332 when excluding the bottom percentile (Landersø and Heckman, 2016, Table A17, Column 1). The first Column in table C.1 shows a point estimate of 0.293, showing that the present analysis aligns with prior results. The estimates are largely similar, and differences in estimates can be attributed to a smaller sample of children and parents in Landersø and Heckman (2016), consisting of children born between 1973 and 1975, and a shorter time-period for the measurement of parents income, which exacerbates temporary large losses from personal businesses and likely drive the bottom one percent effects on the mobility estimate in their estimation, and not in mine.

Boserup *et al.* (2014) also estimate intergenerational persistence. They find a slope estimate of 0.14, substantially below the estimate of 0.293, which I and Landersø and Heckman (2017) find. However, transitory income and life cycle bias can explain this difference. Boserup, Kopczuk and Kreiner construct a sample that includes all individuals observed in 2009–2011 in the Danish income administrative data, and who has parents who are alive and exist in the income registers in 1997–1999. The sample of children and parents, as a result, is substantially different from the one I use here. For example, the average age of children in

[18] Table C.2 contains similar estimates of the intergenerational income elasticity obtained from regressing children's log income on parents' log income. The results are again similar to the prior literature.

the sample is 33.9, with a standard deviation of 8.2 years, substantially below the preferred age considering life cycle bias (around age 40, see Landersø and Heckman 2016). This can result in substantial bias, sufficient to explain the difference in parameter estimates.

The remaining Columns show variations in the estimation procedure. As is also common, the relation with mother's income is weaker than the total parental family income, and father's income is substantially closer to parental income (e.g., Black and Devereux 2011). Interestingly, when using children's individual income rank, the rank mobility estimate is nearly similar to when I use children's family income, which suggests that either measure is usable in the main analysis.

Table C.1. *Full sample Intergenerational Income Rank Mobility Estimates*

	Dependent variable:					
	Children's Family Income Rank			Children's Individual Income Rank		
	(1)	(2)	(3)	(4)	(5)	(6)
Parents' income rank	0.293***			0.300***		
	(0.001)			(0.001)		
Mother's income rank		0.166***			0.183***	
		(0.001)			(0.001)	
Father's income rank			0.268***			0.272***
			(0.001)			(0.001)
Constant	0.368***	0.430***	0.386***	0.368***	0.425***	0.386***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Observations	545,146	542,569	533,902	545,125	542,550	533,881
R^2	0.080	0.025	0.064	0.085	0.031	0.067

Note: The table shows coefficients from regressing children's family or individual income rank on parents', mother's, or father's income rank. Parentheses show heteroskedasticity robust standard errors. Income is measured as total income pre-tax, including wage, capital, and own-business-income and public transfers. Children's individual and family income is measured from 2010 to 2015, deflated and averaged. Family income is the sum of any spouse's and the child's income whenever a spouse exist. Parent's income is the sum of mother's and father's income measured from 1980 to the year the child turns 18, deflated and time-averaged. Mother's and Father's individual incomes are calculated similarly. Ranks are constructed by ranking against all other observations of income within cohorts to account for different ages at measurement. Stars indicate significance levels, * is 10 pct, ** 5 pct, and *** 1 pct.

Table C.2. *Full sample Intergenerational Income Elasticity Estimates*

	Dependent variable:					
	Children's Family Log Income			Children's Individual Log Income		
	(1)	(2)	(3)	(4)	(5)	(6)
Parents' log income	0.339***			0.290***		
	(0.002)			(0.002)		
Mother's log income		0.095***			0.086***	
		(0.001)			(0.001)	
Father's log income			0.252***			0.215***
			(0.002)			(0.002)
Constant	8.789***	12.112***	10.067***	8.905***	11.700***	9.999***
	(0.026)	(0.018)	(0.022)	(0.023)	(0.016)	(0.019)
Observations	544,161	541,263	532,409	543,729	540,834	531,985
R^2	0.052	0.008	0.040	0.048	0.008	0.036

Note: The table shows intergenerational income elasticity estimates obtained from regressing children's family or individual log income on parents', mother's, or father's log incom. Parentheses show heteroskedasticity robust standard errors. Income is measured as total income pre-tax, including wage, capital, and own-business-income and public transfers. Children's individual and family income is measured from 2010 to 2015, deflated and averaged. Family income is the sum of any spouse's and the child's income whenever a spouse exist. Parent's income is the sum of mother's and father's income measured from 1980 to the year the child turns 18, deflated and time-averaged. Mother's and Father's individual incomes are calculated similarly. For observations with zero or negative income (less than 1 percent of all observations) the observation is assigned a positive value of 1,000 DKK in 2015 value. Stars indicate significance levels, * is 10 pct, ** 5 pct, and *** 1 pct.

D Additional results

Figure D.1. *Expected child's family income rank by parents' income rank, comparing areas with above and below 1994 median share of refugees.*

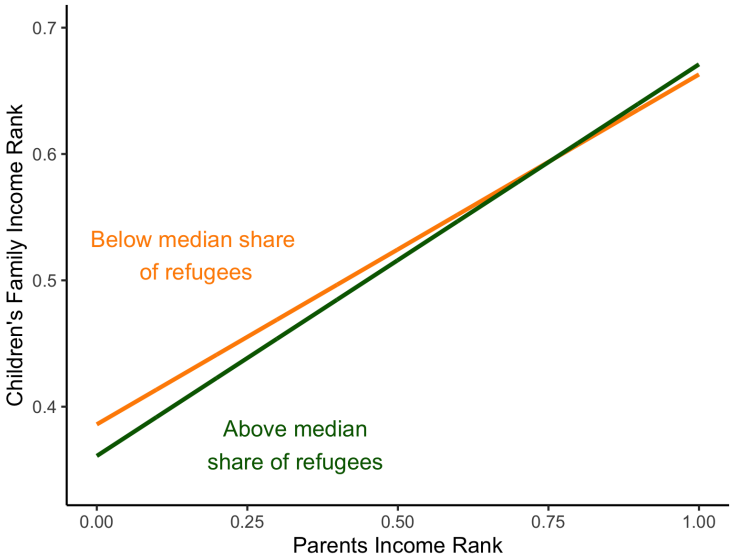


Figure D.2. *Local linear regressions of child's family income rank by parents' income rank, comparing areas with above and below 1994 median share of refugees. The sample is split by median average family income across municipality.*