Refugees and Intergenerational Mobility

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Abstract

This paper investigates the causal effect of refugee inflows on native children's intergenerational mobility. Identification is based on the random allocation of refugees to Danish municipalities from 1986 to 1998. For children born to parents at the 25th income percentile, growing up in a municipality with an above-median share of refugees is associated with a 1.8 percentage point lower expected family income rank in adulthood, approximately $2,100, compared to children from municipalities with below-median shares. Children from high-income families are not affected, and children from high income municipalities are less affected.

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1 Introduction

Across the world, more than 23 million people have been classified as refugees in 2020 (UNCHR, 2020). A large share of these refugees come from Middle Eastern and North African countries, from which more than 2 million refugees arrived in Europe between 2015 and 2020. 766,000 refugees arrived in Northern European from 2012 to 2018, this is 2.3 percent of the local population (UNHCR, 2019; UNCHR, 2020).[1] The large inflows of refugees to new host countries underscore the importance of developing policies that minimize excess costs and burdens, while supporting the best possible outcomes for refugees and the population. The economic benefits and costs may include transfers and integration spending on refugees, as well as taxes earned from their labor, but also the impact of certain allocations of refugees to programs and local areas on the native population.

A developing literature investigates the effects of refugee and immigrant inflows on non-refugees' short-run outcomes (see Edo (2019) and Dustmann et al. (2016) for recent reviews). The findings on effects on adult non-refugees' labor market outcomes vary from negative to positive, depending on the extend of complementarity versus substitutability between natives and immigrants and refugees (Dustmann et al., 2016; Foged and Peri, 2016). A smaller literature investigates effects on native *children's* short-run outcomes, and in contrast typically find negative or no effect on educational achievement such as test scores and increased anti-social behavior, such as bullying, in schools with more refugees (Gould et al. 2009; Jensen and Rasmussen 2011; Cascio and Lewis 2012; Schneeweis 2015).

The short-run effects, however, may misrepresent the long-run effects and thereby subsequent evaluation of refugee policies. Hendren and Sprung-Keyser (2020) compare a measure of benefit-cost structure, the marginal value of public funds, for more than 100 public policies from the US with and without accounting for the long-run policy impact on children's income. They find that

[1] The Northern European countries listed by the UNHCR include Denmark, Finland, Norway, Sweden, and Iceland, as well as Estonia, Latvia, and Lithuania.

when taking into account long-run effects, policies that at short-run evaluation seems relatively uneffective may become the most effective. The effect is driven by children's increased income, for example resulting from increased transfers to the family during childhood, which can increase levels of education investment and lead to higher wages. These higher wages can then be taxed later on and in net-present value be counted against the cost of the policy. Translated to the setting of refugee inflows, the findings of (Hendren and Sprung-Keyser, 2020) suggests the importance of investigating the long-run economic impact on non-refugee children to inform policy choice.

This paper contributes to the refugee and immigration literature by empirically investigating the long-run effects of refugee inflow on children's adult income. I study this in a Danish setting, using a Danish refugee allocation policy in place from 1986 to 1998 as reasonably exogenous variation in local refugee shares at a municipal level. Over this period, Denmark received the equivalent of 0.8 percent of its non-refugee population in refugees, the same share as was received from 2010 to 2018 during the present refugee crisis in the Middle East and North Africa.[2] The policy led quasi-randomly selected municipalities to have large shares of refugees early on, while others received their required share later in the period.[3] Accounting for heterogeneity in effects by parental background, I then estimate econometric models that specify children's economic outcome as a function of parental background interacted with refugee shares in the municipality the child grows up in. This choice is informed by the short-run studies on child-effects, which suggest that chil-

[2]Appendix A contains an extended discussion of the recent refugee inflow to Denmark using data collected from Statistics Denmark.

[3]The allocation scheme has also been used by Dustmann et al. (2019) to estimate refugee inflow effects on political outcomes, and by Damm and Dustmann (2014) and Eckert et al. (2019) to estimate the effects on *refugees* likelihood of committing crime and labor market outcomes from being assigned to municipalities with certain characteristics. Other papers that have used the same allocation scheme include Damm (2009b) , Damm (2009a) and Damm (2014) who study the effects on *refugees*' likelihood of relocating and finding jobs from being assigned to certain municipalities. Foged and Peri (2016) use initial refugee allocations as instruments for later immigrant settlement to study labor market outcomes of natives. Importantly, none of these prior studies have focused on the effects of refugee allocation on native children's outcomes.

dren from poorer families are more likely to interact with children of refugees and immigrants (e.g. Rangvid 2010). I discuss these studies and potential mechanisms in section 3. The approach draws directly on the intergenerational mobility literature, where for example Derenoncourt (2019) use related models to estimate effects of the great migration on the expected income of children conditional on parental background in Northern US cities, and Chetty et al. (2020) study differences in expected child outcome by parental background and race in the US. As a result, the paper also relates to the intergenerational mobility literature.

The data used to measure children's and parents' income, determine in which municipalities children grew up, and refugee shares at the municipal level come from Danish administrative registers. This administrative data is available from 1980 to 2015 and covers the full Danish population with mandatory social security identifiers. I focus on a full population sample of children born between 1973 and 1980, who grow up while refugees were being allocated to the municipalities, and for whom I can observe income in the late thirties and early forties when the income of individuals become a reasonable proxy for permanent income (Landersø and Heckman, 2017). The full sample consists of 537,000 children who can be linked to parents. As a measure of economic outcome, I focus on family level total pre-tax income, including salaries, own-business, and capital income, and social transfers, and convert these to ranked incomes within cohorts. This measure of economic behavior has become standard in the intergenerational mobility literature,and converts often non-linear relation between parents and children's income to near-linear ones, which can be captured in simple linear models (e.g. Chetty et al. 2014; Landersø and Heckman 2017). Finally, I construct a sample of refugees subject to the allocation policy using data on country of origin and first arrival in Denmark.

The main finding of the paper is that non-refugee children from lower-income households experience negative effects of refugee inflows. Children who grow up in areas with above-median cumulative numbers of refugees to 1986 pre-reform municipal population on average have lower expected annual incomes. I calculate refugees relative to the pre-reform population to account for the allocation rule of ensuring refugees proportional to population shares. As an example of the findings, consider the effect for children born to parents at the 25th income percentile. For children in the areas with below-median levels of refugees by 1994, the expected income rank is $38.6 + 25 * 0.27 = 45.4$. For children who grow up in municipalities with above-median shares, the expected income rank is $(38.6 - 2.5) + 25 * (0.27 + 0.033) = 43.6$. The difference is 1.8 percentage points, equivalent to \$2,100 in 2015 values. For children at the bottom 10th percentile, the effect is 2.17 percentage points and USD 2,700. I include several controls in the main regression, including cohort fixed effects, local municipality population size, as well as the pre-reform share of refugees out of the total population, which do not control away the effects.

The paper contains several robustness checks. First, I show that refugee shares do not predict relevant municipal characteristics, such as prior immigrant shares and average family income in the municipalities. One exception is the size of the municipality, which can be accounted for by the fact that Copenhagen, the largest municipality, was subject to specific allocation rules, which I discuss in section 4. I also consider alternative specifications of the refugee share, including substituting the binary above/below-median shares of refugees with the cumulative percentage and estimating effects using the child's income instead of the child's family income, finding again similar results.

As additional tests of effect heterogeneity, I develop two extensions. First, I estimate the model separately for males and females as prior studies on immigrant effects have found that males are more likely to experience short-run negative effects. However, I find that males and females experience effects equally. Secondly, I investigate whether the effects may vary by the average family income in municipalities, estimating the model separately within the bottom and top half of municipalities by average family income. I find that the effect of refugees is stronger in the bottom half municipalities, conditioning again in parental income, meaning the result is not driven purely by composition effects. As the refugee shares do not themselves predict average family income in the municipalities, this finding suggests that refugee policies would want to allocate refugees to municipalities that are, on average, more affluent. This can be explained by a variety of factors, but one explanation consistent with the prior short-run findings on immigrant effects is that schools in these municipalities have more resources to aid the integration of children of the refugees in the schools.

The paper is structured as follows. In the next section, I outline the existing literature which the paper contributes to, and section 3 contains a discussion of potential drivers of refugee effects. Section 4 describes the refugee allocation policy, which I use as exogenous variation in refugee shares in the analysis. Section 5 describes the data and estimation procedures, contains descriptive statistics on the main sample of children and parents. Section 7 contains the main results and various robustness checks. The last section concludes the paper.

2 Existing literature

The paper contributes to a large literature investigating various aspects of refugees' and immigrants' effects on natives, including both children and adults. Card (2001, 2009), Dustmann et al. (2016), Edo (2019) and Foged and Peri (2016) review the largest strand of these pieces of literature on natives' labor market attachment and incomes. The takeaway from this literature is that immigrants and refugees will have negative effects on native workers when they are close substitutes and labor and product markets are competitive (Borjas, 2003; Dustmann et al., 2017; Prantl and Spitz-Oener, 2020), and null or slightly positive when they complement the native workers (Foged and Peri, 2016; Peri and Sparber, 2009; Tabellini, 2020).[4] Borjas and Monras (2017) show that the same effects likely exist for refugees, showing empirical evidence from across 4 different refugee inflows to Miami from Florida, the Soviets to Israel, Algeria to France, and Yugoslavia to Austria and Switzerland. An important characteristic of the majority of these studies is that they focus on short-run outcomes, and as is noted in the panel discussion for the paper by Borjas and Monras

[4]Dustmann et al. (2013) is an example of a paper that shows both effects in the UK. The authors estimate the effects of immigration on native workers wages at various points in the income distribution, showing that low-income workers are more likely to be close substitutes with immigrant workers, and higher-income workers less so, possibly with positive wage effects due to complementarity.

(2017, pp. 412) more research is needed on long-run effects.[5]

A second, and more closely related, set of papers investigate effects on native children. I review these in detail in section 3 as I discuss likely mechanisms that can drive long-run 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. These studies have found both positive (Hunt, 2017) and negative effects (Gould et al., 2009) on native students' likelihood of achieving high school equivalent degrees. Focusing on achievement, Schneeweis (2015) and Ohinata and van Ours (2013) find null effects for Austrian and Dutch children from having more immigrants in classes when controlling for school fixed effects, whereas Jensen and Rasmussen (2011) show negative effects on Danish students PISA test scores when instrumenting the shares with the average number of inhabitants within schools. A notable example of well-identified causal evidence comes from the study of inflows of Haitian earthquake refugees to Florida schools by Figlio and Özek (2019) who show clear null effects on native students' education outcomes. These findings, in general, all focus on relatively short-run outcomes of children while they are still in school, and in many cases, do not provide clear evidence of effects. In the school flight literature, findings are in general negative, showing that more affluent families are more likely to move their children, either by moving themselves or entering their children in private school when migrant shares increase (Derenoncourt, 2019; Rangvid, 2010).

This paper adds to the immigrant and refugee effects literature in two ways. First, the results show that long-run effects exist for children, in contrast to the medium-run null effects found for adult workers in Israel by Cohen-Goldner and Paserman (2011). This suggests that further research is also warranted for long-run effects on adults, as also argued for in the discussion of Borjas and Monras (2017). Secondly, this paper is the first to provide well-identified effects of refugee shares on children's outcomes beyond the setting of schools, which has been the primary focus of refugee studies. While the prior literature on the effects on children is mixed, these papers suggest that

[5]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 effects.

although short-run effects are limited, long-run effects can be substantial.[6] The paper is, however, limited in the extent to which in can speak to mechanisms, as unlike in the case of Figlio and Özek (2019), refugees are not assigned to school areas, but rather the broader municipal level.

In addition to the larger literature on immigrant and refugee effects, this paper is related to a small set of papers that also use the Danish refugee allocation policy to estimate the effects of allocation either on refugees or political preferences. Damm (2009a, 2014), Damm and Dustmann (2014), and Eckert et al. (2019) study the assigned refugees' re-allocation patterns and the effects on the refugees' labor market and criminal behavior of being assigned to municipalities and neighborhoods with certain labor market and crime characteristics. The paper by Dustmann et al. (2019) is more closely related to the paper by studying effects on natives' political preferences, finding evidence of increased right-wing support in municipalities that received more refugees early on. This paper differs from these prior studies by focusing on native children's long-run outcomes but share the reliance on the quasi-random allocation of refugees to obtain causal estimates.

The paper also relates and contributes to the intergenerational mobility literature by estimating the effects of refugees on children's income conditional on parents' income, the primary relationship of interest in this 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).[7] The main emphasis in

[6]This notion that long-run effects can become enhanced versions of short-run effects is also mirrored in the setting of the large scale Moving To Opportunity experiment, which provided randomly selected families with housing vouchers to help them out of impoverished neighborhoods. Early evaluations found minuscule effects on children's educational achievements, and slightly higher likelihoods of sons committing crimes. In a recent re-evaluation of long-run effects, Chetty et al. (2016) show that adult yearly earnings had improved substantially for the treated families.

[7]An earlier sociological literature has emphasized intergenerational persistence in vocation starting already in the 1960s (Blau and Duncan, 1967). Torche (2015) gives an overview of the often overlapping Sociological and Economic studies of intergenerational mobility. I focus here on the more recent Economic approach as the main outcome is income.

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, and Black and Devereux 2011).[8] A smaller set of papers focus particularly on the intergenerational mobility of second-generation immigrants, including Abramitzky et al. (2020), Borjas (1993), and Collins and Zimran (2019) on nineteenth and twentieth-century migrants to the United States, Aydemir et al. (2009) from Canada, and Bolotnyy and Bratu (2018) and Hammarstedt and Palme (2012) from Sweden.[9] By estimation procedure, this paper relates to this main literature, and in particular the studies on immigrants intergenerational mobility relative to natives. The paper adds to the literature by providing estimates of effects from refugees on natives, shifting the emphasis from descriptive to causal analysis in the non-native studies of intergenerational mobility.

The spatial emphasis in this paper is mirrored in a more recent intergenerational mobility literature, which estimates differences in intergenerational mobility within countries such as the United States, Canada, Sweden, and Italy, often using large scale administrative datasets (Güell et al., 2018; Chetty et al., 2014, 2020; Corak, 2019; Connolly et al., 2019; Heidrich, 2017). This within-country variation can be used to estimate the effect of place-based policies or shocks on intergenerational mobility. Sharkey and Torrats-Espinosa (2017) and Derenoncourt (2019), for example, estimate the effects of increased policing of mass internal migration to Northen US cities during the Great Migration on local intergenerational mobility.[10] The results from this paper build on

[8]The set of countries for which estimates were available was limited to a small set of countries including the US (e.g. Solon 1992, and Bjorklund and Jantti (1997)), the Scandinavian countries (Björklund and Chadwick, 2003; Björklund and Jäntti, 2012; Björklund et al., 2006, 2009; Bonke et al., 2005; Landersø and Heckman, 2017), Britain (Blanden, 2007; Blanden et al., 2004), and Canada (Corak, 2004, 2006).

[9]OECD (2018) in addition compiles 7 different cross-country comparisons of natives' and immigrants' intergenerational mobility.

[10]Relatedly, 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

the prior Danish study of municipal variation in intergenerational mobility by Eriksen and Munk (2020), who show that within-country variation in intergenerational mobility in Denmark is at about the same level as in Canada, and slightly less than the United States. This study shows that one potential driver of these differences can be refugee and immigration policies. The within-country literature also helps put the findings in perspective. Derenoncourt (2019) finds that a one standard deviation increase in the local black population in Northern cities during the Great Migration led to a three percentiles drop in expected income ranks for low-income families. The direction and size of my main finding, about two percentage point drop in expected income ranks as a result of a larger inflow of refugees, is largely similar, although the magnitude of the increase of refugees in Denmark between 1986 and 1998 was substantially lower than the movements taking place in the United States during the Great Migration.

The results from the paper can help guide future research and policy subject to limitations. The Danish setting of the paper suggests that conclusions on effects may be more relevant for relatively high-income countries with relatively large welfare states, such as the Northern European countries. This opens up future research opportunities for extending the analysis to countries that are also affected by large refugee inflows, such as the Southern European countries where many refugees arrived during the recent refugee crisis, as well as Middle and North American countries, where refugees seek asylum from certain South American countries. The paper nevertheless provides first estimates of long-run effects from refugee dispersals, which can be used to guide future considerations of, particularly allocations of refugees. Additionally, future research can consider how to incorporate these estimates into more elaborate benefit-cost models, such as the Marginal Value of Public Funds framework (Hendren and Sprung-Keyser, 2020). Still, the results of the policy provide initial policy guidance, suggesting that at a minimum, it may be preferred to initially allocate refugees to municipalities with higher average income or to provide additional resources to those municipalities that receive refugees to minimize negative effects on less affluent children in

children's outcomes, and Deutscher (2020) extend the analysis to Australia.

the receiving areas. This policy suggestion is subject to the constraint that the paper cannot directly investigate within school effects of refugee shares, as refugees were allocated at a municipal level, and draws instead on prior short-run findings from the immigrant and refugee effect literature.

3 Potential drivers of refugee effects

The prior literature on child effects from refugee and immigrant inflows provide some guidance to understanding how children's long-run economic outcome may become affected. In this section, I discuss the prior findings on each of the two channels, 'peer effects' and 'school flight', that can be identified from the prior studies. These both focus on the educational attainment and achievements of children, which can be translated into monetary returns in the labor market.

The prior evidence on 'peer effects' in education is focused on what happens to native children as the shares of refugee or immigrant children rise within schools or classes. The typical emphasis is on test score or educational achievement, and the extend of anti-social behavior within classes. When students face worse learning environments, they may become less likely to obtain higher education, or to receive lower returns to the education they do receive (e.g. Kirkeboen et al. (2016)). Similarly, Deming (2017) has shown that prosocial skills can increase labor market returns across a variety of sectors,[11] and (Kosse et al., 2020) show compelling experimental evidence that prosocial behavior can be affected by the social environment children face.

The existing evidence on 'peer effects' range from negative to null effects on test scores and negative effects on social environments.[12] Jensen and Rasmussen (2011) study immigrant shares

[11]The effects of non-cognitive skills, which incorporate certain aspects of social skills, has also been documented to affect wages across different settings, including the Perry Pre-school program from the United States (Heckman et al., 2006, 2013).

[12]A recent study by Hunt (2017) also find positive effects on educational attainment in the United States. However, the estimates derived from state-level regressions of native children's educational attainment on state immigrants shares. While Hunt does instrument immigrant shares at the state level with 1940s levels, it remains possible that the

in Danish schools. Using PISA test score data and administrative data, they test whether children in schools with more refugees have performed worse on the PISA test. In OLS and IV regressions they find a negative effect for both native and immigrant children.[13] Gould et al. (2009) also find negative effects on subsequent education attainment from having relatively more refugees in 5th grades within Israeli schools. In contrast, Schneeweis (2015), and in Ohinata and van Ours' (2013) study effects on grade repetition in Linz, Austria, and PISA test scores in Dutch schools, and both find null effects. Grade repetition may be too strong a test, not showing smaller learning effects, but the Dutch findings indicate that learning effects may be limited. Ohinata and van Ours (2013) also test for effects on anti-social behavior in classes, finding more bullying in classes with relatively more immigrants. In more recent work, Figlio and Özek (2019) study the effects of Haitian natural disaster refugees to Florida primary schools using administrative data from Florida. In this study, the authors find no evidence of test score effects from the inflow of refugees but find increased prevalence of anti-social behavior.

The varied findings from these studies suggest that higher refugee shares in municipalities can lead to adverse effects on students' learning or environments with more anti-social behavior. In the setting of this paper, if native children attend schools also attended by relatively more children of the refugees, we may expect worse labor market outcomes for these children compared to their peers in schools with fewer 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 US, coining the term 'school flight'. Rangvid (2010) similarly provides descriptive evidence from

positive result can be driven by state differences, such as in quality of primary schools, and immigrant self-selection towards better areas.

[13] The results from the instrumental variable analysis may still reflect correlations rather than causal effects. The immigrant share in the larger geographic area can also be a result of general negative selection, meaning that a selection biased variable would be instrumented with another selection biased variable.

cross-sectional data on school attendance and parental background in Copenhagen, Denmark. Rangvid also finds that more affluent children are enrolled in private schools in areas with more immigrant children.[14] If high-income students move to schools less affected by refugee incomes, this may lead to two results on learning and subsequent labor market outcomes. First, high-income students are not likely to be affected by refugee inflows if their elasticity of substitution between schools is high with respect to the quality of institutions, and they move at small changes in quality of peers. This creates differential effects along with the native children's parental income distribution of refugee inflows. Secondly, if the high-income peers in general also provide positive peer effects to less affluent peers due for example, to externalities to parental investment in the affluent children (e.g., Sacerdote 2014, moving the affluent children can lead to further reductions of the quality of learning in the primary schools of remaining children.

The two channels described here suggest that refugee inflows to local areas can lead to decreased long-run earnings of children in the schools that refugee children attend. Unfortunately, the assignment of refugees happened to municipalities during the 1986-1998 period. As a result, it is not possible to obtain estimates of effects within schools, but only effects from having refugees assigned to the local municipalities. As a result, the findings of the upcoming sections do not aid in determining which channel may be more important but provide suggestive evidence on the existence of them.

4 Refugee Allocation Scheme, 1986-1998

The Danish refugee allocation policy of 1986-1998 was originally developed as a response to increasing refugee flows in the early 1980s, which led some Danish municipalities to receive a higher

[14] As another example of flights, The Great Migration by African Americans from the South to the North of the US also led to flights from inner cities to suburban areas by white families (Boustan, 2020; Derenoncourt, 2019).

share of refugees, and thereby to share a disproportionate share of the cost burden of integration.[15] 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 organization 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-government entity 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 one area of Denmark would receive its share of refugees early on. When arriving in Denmark, asylum seekers were iniitally assigned to one of several Red Cross reception centers for 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 utilize in the main

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

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. I discuss the caveat this raises in section 5.

Across the period 1986 to 1998, 76,673 refugees were granted asylum in Denmark (Damm, 2014), but not all were subject to the allocation policy. 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.[16] 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

[16] 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.

refugees who were plausibly randomly assigned to the Danish municipalities.

After the initial assignment, the refugees assigned to any given municipality 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 (2014), in table A6, shows for a sample of male refugees constructed similarly to that in this paper (which, however, includes both genders) that the assigned refugees did move some, but the majority stayed in their assigned municipality. In the first year of settlement, all refugees remained in the municipality, and even neighborhood they were assigned to. After three years, 60 percent of refugees remained in the same municipality. In addition to the forty percent who moved across municipalities, about 24 percent of refugees moved neighborhoods within municipalities. After five years, more than 50 percent of the male refugees remained in the municipalities they were assigned to. 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, which capture some fraction of total effects of the refugee assignment in the municipality had residents continuously been subject to the full refugee shares (Ludwig et al., 2008).

Despite the evidence that the refugee allocation policy distributed refugees randomly, the DRC may have preferred sending refugees to some areas. I implement a simple test for non-random assignment in the next section, 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.

5 Data

I collect data for the analysis from Danish administrative datasets and publicly available sources.[17] Each of the following subsections describes the construction of datasets used in the analysis, starting with the refugee shares.

5.1 Refugees

First, I create a sample of refugees subject to the dispersal policy of 1986-1998. There does not exist publicly available data that discern refugees subject to and not subject to the allocation policy. I instead construct the refugee sample from the administrative dataset.[18] The administrative data does not contain specific identifiers for refugees before 1997. I identify the refugees among regular immigrants from their time of arrival, origin country, and information about family already living in Denmark:

- The individual must originate from a country that Denmark received refugees from in the period 1986 to 1998.[19]
- 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.

[17]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`. I refer to sources of publicly available data.

[18]Dustmann et al. (2019) follow a similar approach to constructing the refugee sample used in the analysis.

[19]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.

- The individual is not younger than 18 years and arriving without parents.

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.

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 to each municipality's population. The table only contains the years used in the analysis. The increasing average and median percentage of refugees reflect the growing number of refugees received by the Danish municipalities. The increasing standard deviation similarly reflects the variation I use to identify the effects of refugee shares.

However, I am able to show that the allocation of refugees is plausibly exogenous to characteristics of the municipalities the refugees arrive in. Table 2 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. Importantly, 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 better-educated individuals in the municipality. It is also reassuring that the policy is not able to predict pre-reform non-EU immigrant shares, which could

Table 2: 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.

be driving results through the channels described in section 3.[20] The finding that larger municipalities were more likely to receive their refugee share early on may be explained by the ease with which the DRC could find housing in the municipalities or establish their offices. In robustness checks in the analysis, I include pre-policy sizes of municipalities to account for this, finding that the results are robust to this change.

5.2 Income

Next, I describe how I construct the income samples for children and parents, and then how this construction of the data solves the main issues faced in intergenerational mobility studies. The second main dataset contains income information for native parents and children. I use the income data to predict children's long-run average income from parents' long-run average income, conditional on the share of refugees allocated to the municipality. I focus on long-run average income, the income, which the individual earns on average over a lifetime. Focusing on the expected long-run income, one can estimate gained or lost tax revenue from alternate refugee policies using these income measures.[21] Before describing the data construction, however, I first go through some of the typical concerns about measuring long-run income in intergenerational mobility studies, which this paper relies on.

Measuring income and intergenerational mobility

The first main concern in predicting children's income from parents' is attenuation bias from transitory income shocks (Mazumder, 2005; Solon, 1992). Measuring income in a single year can pick up short term fluctuation in income, which is not informative about permanent income (a worker

[20]The result holds when including all immigrants.

[21]Many papers in the recent intergenerational mobility literature similarly attempt to accurately predict the long-run income of children from parents' long-run income (e.g. Chetty et al. 2014; Landersø and Heckman 2017; Nybom and Stuhler 2016 and Nybom and Stuhler 2017).

may, for example, receive a bonus one year, or realize a high capital return on a rising stock). This type of measurement noise introduces a simple errors-in-variables bias in coefficient estimates from linear regressions, which is the main tool I use for the following empirical analysis.[22] I minimize this type of bias by measuring parents' income over the full period of 1980 (the first year of observed income) to the year the child turns 24.[23] For children, I average income over 6 years whenever possible, using income information from 2010-2015, where 2015 is the latest year available in my 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; 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.[24] I measure the income

[22]Chetty et al. (2014) suggest that measuring income over 4-5 years is sufficient to minimize attenuation bias when working with US 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.

[23]I also investigate the effects of choosing age 20 and 18 as cut-offs, finding similar results in the main analysis

[24]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

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 emphasized 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 (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.[25] 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

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with more limited income samples.

[25] 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: $y_i^c = \beta_0 + \beta_1 y_i^p + \beta_2 (y_i^p)^2 + \varepsilon$. 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; $y_i^c = \beta_0 + \beta_1 y_i^p + \beta_2 (y_i^p)^2 + \delta_0 g_{1,i} + \delta_1 y_i^p \cdot g_{1,i} + \delta_2 (y_i^p)^2 \cdot g_{1,i} + \varepsilon_i$. 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.

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 US when measured with the IRS administrative income data, and Landersø and Heckman (2017) find the same result for Denmark using similar administrative data to what I use here. 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 slope from interacting these with a binary 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 focus on children's family income ranks and parental income ranks.

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.[26] For the 1973 cohort, this is 1997, and for the 1980 cohort, it is 2004. To enter the main sample,

[26] 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.

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 3 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 1 percent do not).

Table 3: 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 labor 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.[27] For children, I measure

[27]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`.

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.[28] 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 4 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 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 summary 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.

Table 4: 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

continued on next page

[28]In the appendix 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.

Table 4: Summary Statistics

	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
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 assigned 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 secondary or tertiary 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 to 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 secondary or tertiary 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						

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Table 4: Summary Statistics

	N	Mean	St. Dev.	Pctl(25)	Median	Pctl(75)
Observations (1980 to 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 secondary or tertiary 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. Obs. numbers are conditional on cohort fixed effects to account for the shorter observation window (1980 to year turning 18) for older cohorts. 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. Assigned municipality is the one the child is observed in for most years from 1980 to the year the child turns 20, and municipalities are the total number of municipalities the child has lived 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. Information on employment and labor force participation is based on employment information for November in each year observed.

5.3 Housing

To estimate the effects of refugee inflows on children, each child must be assigned to a municipality. The main purpose is to get an accurate description of where children grow up and as a result may interact with or in other ways be affected by the presence of the refugees. I, therefore, 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 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 3 shows that I obtain housing information for nearly all children that I observe in adulthood. The descriptive statistics on children's outcomes in table 4 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. This is consistent with many children moving away from their home municipalities by their late twenties. Most often, the children also cohabitate either with both parents (68 percent) or one of the parents (17.5 percent).

6 Empirical Strategy

A common problem in studies of the effects of immigrants and refugees on natives is self-selection or assignment into specific areas (see 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 has become the foundation for a common approach to estimating immigrant effects using shift-share instruments, which uses prior levels of settled immigrants as instrument for present levels.[29]

I solve the self-selection problem in this paper 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 6 and 25 (1980 and 1973 cohorts). As described in section 4, the policy lead to higher shares of refugees within some municipalities, and in section 5 I show that these refugees shares, in fact, were plausibly exogenous as they could not

[29]See Jaeger et al. (2018) for an overview of the use of Shift-share instruments, and e.g. Card (2001), Dustmann et al. (2017), Derenoncourt (2019), and Tabellini (2020) for examples of applications.

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 language of experiments, take-up of the policy will vary by children because some children are exposed, and others are not (Ludwig et al., 2008).

The discussion of potential channels of refugee effects in section 3 showed that parental income may be an important mediator of refugee effects on children's long-run outcomes. To estimate this effect, I draw on the modelling 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 summarizes the rate of transmission from parents to children.[30] This model can be seen 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.

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

[30]Another traditional understanding of the parameters is to predict how many generations it takes before a certain income disadvantage for a family has been evened out. Let the two outcome variables be the logarithm of a relevant measure of permanent income. The model then shows the best linear prediction of the percentage change in children's outcomes when parents' outcomes increase by 1 percent. In early studies of US intergenerational mobility, this parameter was often estimated to be around 0.4 for the United States population (e.g. Solon 1999). In that case, the parameter β_1 shows the predicted percentage change in child income from a percentage change in children's income. β_0 simply standardizes income across generation, taking into consideration that economies on average have tended to get richer over time. This implies that we can calculate the number of generations it takes for a family's income to regress to the mean income in a generation. For example, assume that a family's income is 30 percent below the median income, and β_1 = 0.4. Then the child's income will be 12 percent below the next generations average, and grandchildren's income 4.8 percent below their generations average. If instead β_0 = 0.2, as has been found for the Scandinavian countries, the children's expected income is only 6 percent below the average, and the grandchildren will have caught up with the parental generation.

In this paper, I focus on the prediction of children's income rank from parents' income rank, which became popularized by Chetty et al. (2014). In this 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 $beta_1$ 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'. An example of the use of this interpretation comes from the comparison of expected child income ranks for African American and White children in the United States for children born to parents at the 25th income percentile by Chetty et al. (2020). For White americans, they finds that $\beta_0 = 36.8$, and $\beta_1 = 0.32$, and so the expected income rank of a white child is $\hat{R}^c_{W,25} = 36.8 + 0.32 * 25 = 44.8$. For African American children they find $\beta_0 = 26.35$ and $\beta_1 = 0.28$, and so $\hat{R}^c_{B,25} = 26.35 + 0.32 * 25 = 32.6$. The substantial difference in expected income of African American and White children at the same parental income rank indicates the substantial differences in opportunities documented by the authors for different ethnic groups in the United States.

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^p_i, 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 to the 1986 population in the municipality, I_m.[31] I_m is an indicator for the child growing up in a municipality with more than the median

[31] This is a reduced form model as I do not explicitly model the pathways through which the refugee inflows affect children. Low and Meghir (2017) and DellaVigna (2018) provide recent discussions of the benefits and limitations to structural and reduced form modelling in economics. The reduced form approach is typical in studies of immigrants and refugees' effects on natives, in which there are clear treatment (relative inflow of refugees or immigrants) and outcomes defined (e.g., Card 2001, Dustmann et al. 2017, and Prantl and Spitz-Oener 2020).

percentage of refugees arriving in the municipality to the 1986 population in the municipality.

$$R_i^c = \lambda_0 + \lambda_1 R_i^p + \delta_0 I_m + \delta_1 R_i^p I_m + \varepsilon_i \quad (3)$$

Including I_m in the model, I model the difference in expected income ranks using the spatial variation in refugee shares across municipalities. 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.

The indicator and interaction terms included in the estimation allow me to estimate the effects of refugees conditional on parents' income rank. Using these differences in parameters I can determine the effect of refugee inflows on children's income across the parental income rank distribution. In particular, this allows me to show if particular groups of children, for example, born to parents at lower income ranks, are more or less affected by the inflow.

I also consider various robustness checks in the analysis. First, I include pre-policy population and immigrant shares, and exclude the city of Copenhagen. Secondly, I substitute the main child's family income rank with the child's own income rank. Thirdly, I estimate effects using a continuous rather than binary measure of immigrant exposure. These variations all support the main findings in the paper. Because the main models from the study are based on assumptions of linearity between children's and parents income rank, I also show non-parametric estimations based on local linear regressions which underscore both the relevance of the linearity assumption and the existence of effects for less affluent children and no effects for more affluent children. Finally, to try to dig into the potential drivers of effects, I estimate the models separately by gender, and by the average municipal family income to investigate first if the effects are gender-specific, and secondly if it may be possible to minimize the negative effects by assigning refugees to more affluent families.

7 Results

In this section I show the empirical results of the analysis, starting by estimating a national level model of children's expected income rank conditional on parents'. 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 (some 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.

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 5, column 1. The estimate of β_0 is 0.368, and the estimate of relative mobility, β_1, is 0.293. This estimate is in line with prior evidence on intergenerational income rank mobility in Denmark (Landersø and Heckman, 2017).[32] I therefore proceed with the estimation of the main model.

The estimation of the the effects of refugee shares is shown in table 5, column 2, where both the estimate of δ_0 and δ_1 are statistically significant at -.025 and .033.[33] The parameter estimates indicate, first, that the poorest children who grow up in areas with above-median 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 only children born to relatively well off parents are not affected by the refugee inflow.

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[32] I discuss the comparison of this finding with respect to the prior literature in more depth in appendix C.

[33] Standard errors in table 5 are clustered at the children's home municipality level to account for spatial correlation in errors Colin Cameron and Miller (2015).

Table 5: The effect of refugee share on non-refugees' intergenerational mobility

	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.		0.031***	0.026***	0.026***	0.023***
ref. 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

Note: 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, and 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'). Population and refugee numbers are from Statistics Denmark. Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

As an example of the effect, take 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 $2,100 in 2015 values.[34] For children born to the bottom 10 percent, the effect is 2.17 percentage points and $2,700 USD. 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 mio. Danes, who in their adulthood earn (at least) an average of $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.[35]

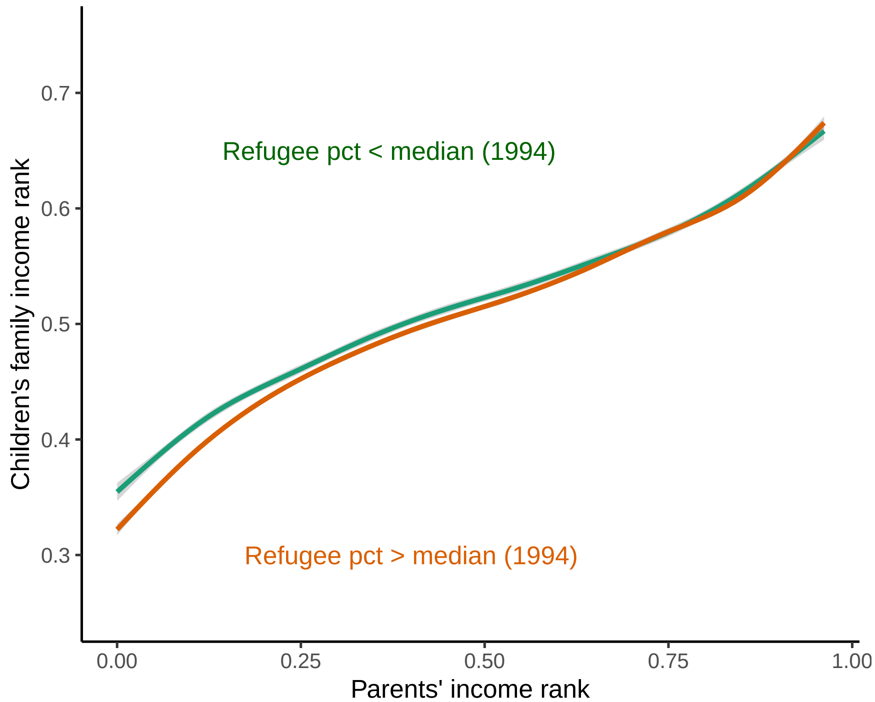
I graphically depict the estimated effects in a linearly parameterized model in appendix figure D.1, which shows the the expected income of children in municipalities with above and below-median shares of refugees. However, it is also possible to test whether the effects are indeed linear by estimating 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

[34]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.

[35]The expected income percentile at which the two populations become equal can simply be calculated as from the linear expected income rank relations. The income of children from below-median share of incomes is $E[R_i^c|\text{below-median share}] = \lambda_0 + \lambda_1 * R_i^p$, and that of children in above-median shares is $E[R_i^c|\text{above-median share}] = (\lambda_0 + \delta_0) + (\lambda_1 + \delta_1) * R_i^p$. Setting the two equations equal and solving for the parental income rank gives $R_i^{p*} = \frac{\delta_0}{\delta_1} = \frac{.025}{.033} = .7576$. Multiplying by 100, the parental rank at which expected family income is similar is the 75th income percentile.

main findings: the lower the parental income, the 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.



7.1 Controlling for Initial Refugees Shares and Capital City Effects

A potential concern with respect to the results is that the allocation of refugees is not entirely exogenous concerning the relation between parents and children's income ranks. The allocation mechanism, for example, considered the 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 5, 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. 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 I use in this paper is the binary variable for refugee shares. This could be masking a near non-existing effect for most of the observations. As an alternative, I interact with parental income ranks with a continuous measure of refugee shares in table 6, using instead the cumulative percentage of refugees to 1986 population in municipalities as a measure of the refugee inflow. However, the results of this regression are qualitatively similar to the main specifications, showing that the results are robust to reasonable alternate specifications.

7.2 Effect heterogeneity

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 across these municipalities.

The effects of the reform may vary by gender if, for example, boys are more likely to react to anti-social behavior on classes, or respond more strongly to variations in teacher attention. Table 7 shows the results of regressions with and without controls by gender. I find no evidence of differential effects along these lines, with point estimates being nearly overlapping.

However, splitting the sample by the average family income of the municipality (average from 1987 to 2000), I find substantial variation in effects. Prior to controlling for all factors, the interaction term between parents' income rank and the binary refugee share indicator, the slope relative to income is 0.02 percentiles steeper per parental percentile change, and the effect remains after removing Copenhagen from the sample and controlling for inhabitants and immigrant shares in 1985. This finding suggests that it may, in fact, be possible to alleviate some of the effects of

Table 6: Effect of cumulative percentage of refugees by 1994 on non-refugee intergenerational mobility.

	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		0.063^{***}	0.058^{***}	0.058^{***}	0.052^{***}
refugee 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, and 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'). Population and refugee numbers are from Statistics Denmark. Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

Table 7: Refugee effect on intergenerational mobility conditional on child gender

	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. pct (1994)	0.031***	0.024***	0.030***	0.023***
	(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, and 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'). Population and refugee numbers are from Statistics Denmark. Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

refugee shares by assigning refugees to more affluent municipalities. Figure D.2 in the appendix shows this result nonparametrically, estimating children's income rank by parental income rank, splitting samples by refugee shares in 1994 and by average family income in the municipality.

Table 8: Refugee effect on intergenerational mobility conditional on average income in child's municipality

	Child's family income rank			
	Below median ncome 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. pct (1994)	0.040***	0.038***	0.018***	0.016***
	(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 family income is measured using Statistics Denmark Table INDPF122 data, averaged by municipality over 1987 to 2000. No earlier periods are available in the Statistics Denmark tables. The estimation sample consist of either female or male children observed with income in 2010-2015, and 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'). Population and refugee numbers are from Statistics Denmark. Parentheses show heteroskedasticity robust standard errors. Stars indicate levels of statistical significance: * 0.1, ** 0.05, *** 0.01.

8 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 $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 $2,700. The result is largely robust to several variations of estimation, 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 additional analyses, I find no variation in effects by municipal income levels. I do however find that the effects are larger present in municipalities with less affluent families. This is consistent 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 investigating the policy implications of refugee policies. In particular, the results emphasize the need for considering impacts on poorer native children in addition to those on the incoming refugees from countries in crisis. I leave for future research to combine the findings from this paper with prior research on effects on refugees and short-run effects on children and adults (e.g. Damm 2014, and Damm and Dustmann 2014)) to develop full models of the economic effects of refugee policy schemes that can adequately weigh the benefit-cost structure of varying policy features.

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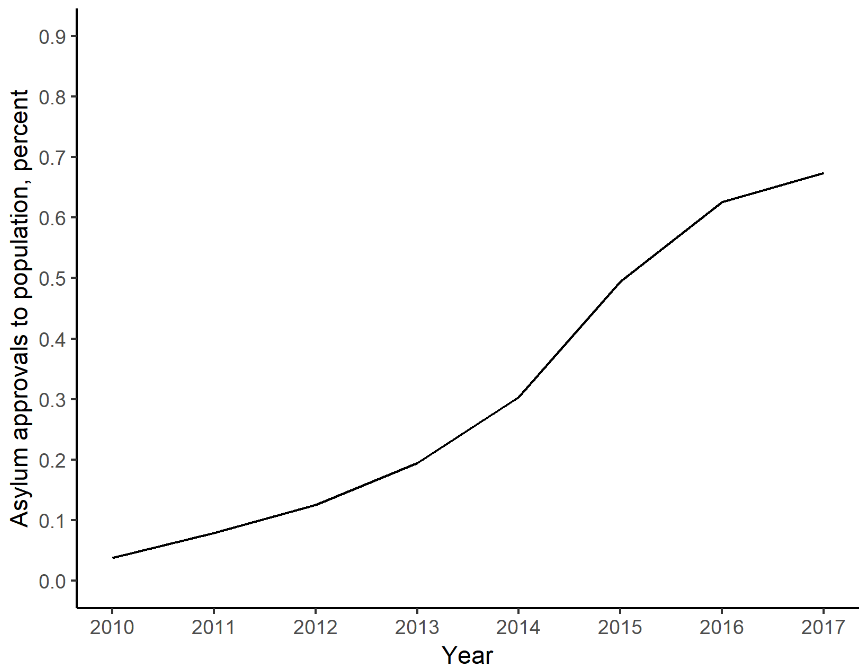
Appendix

A Danish refugee inflows

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.

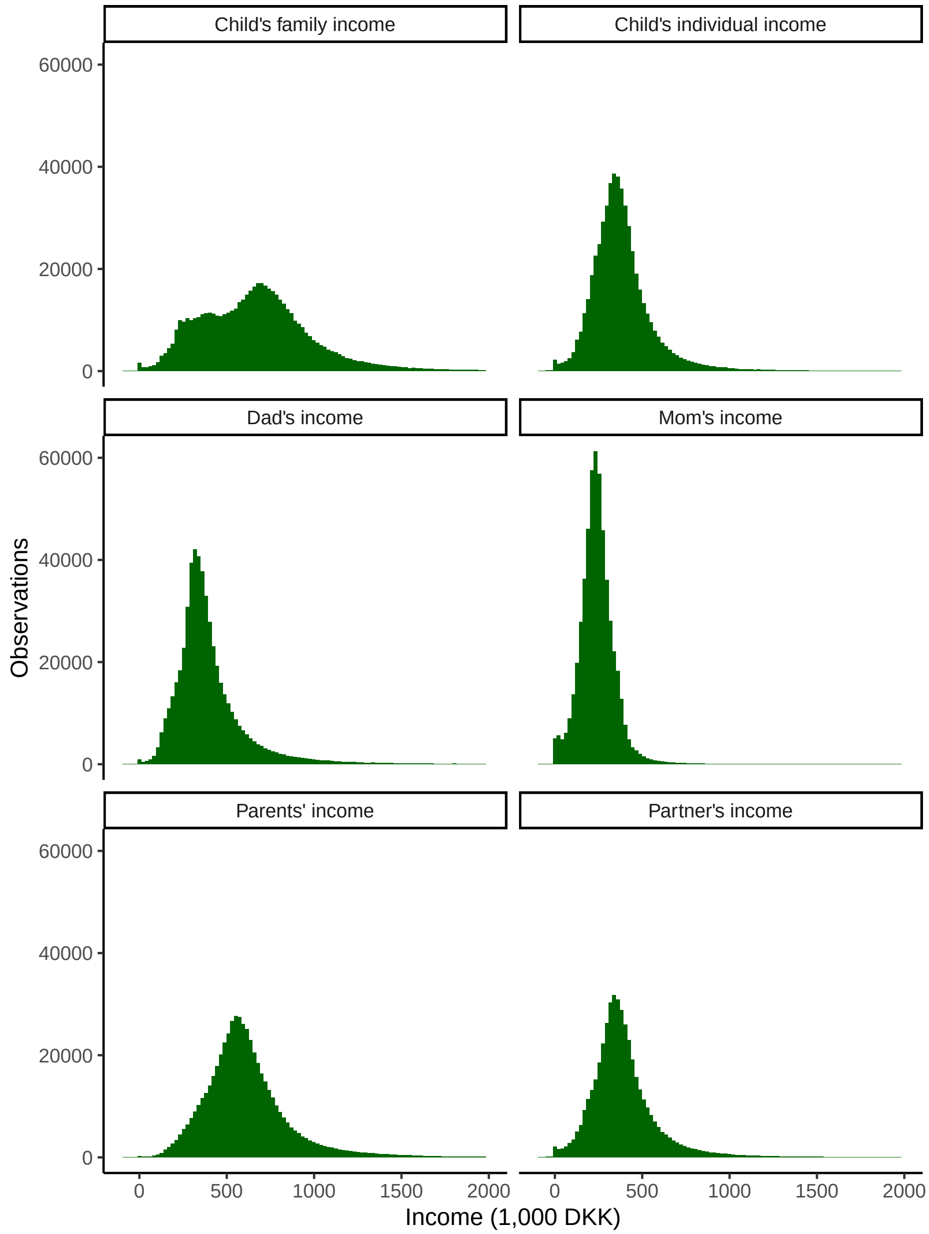
Figure A.1: Danish cumulative approved asylum applications to total population, 2010-2017



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

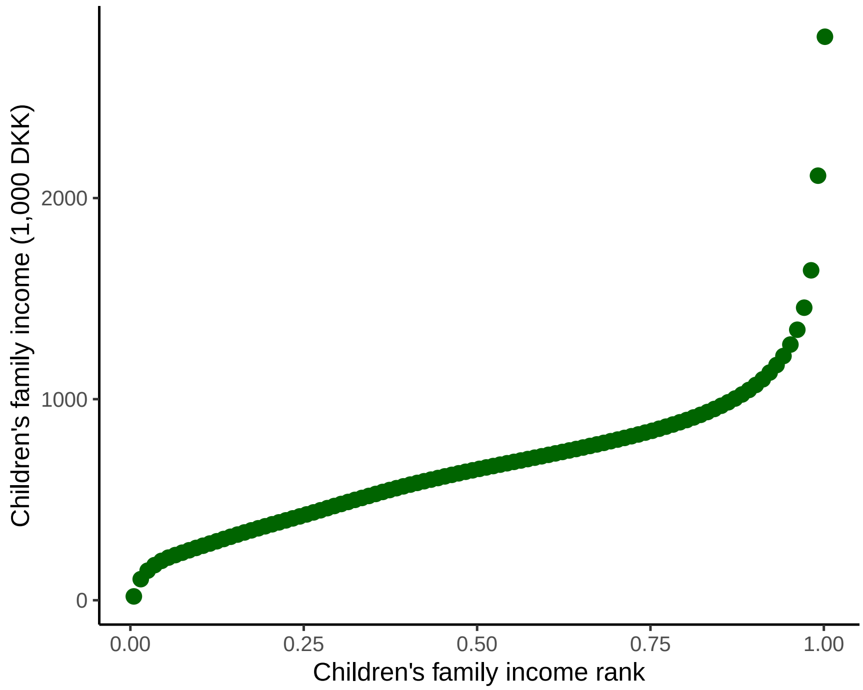
B Descriptive statistics

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.[36] 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, which is preferable concerning concerns of transitory income shocks and life cycle bias (Nybom and Stuhler, 2016, 2017). For example, the average age of children in the sample is 33.9, with a standard error 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

[36]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.

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.

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 (see 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.

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.

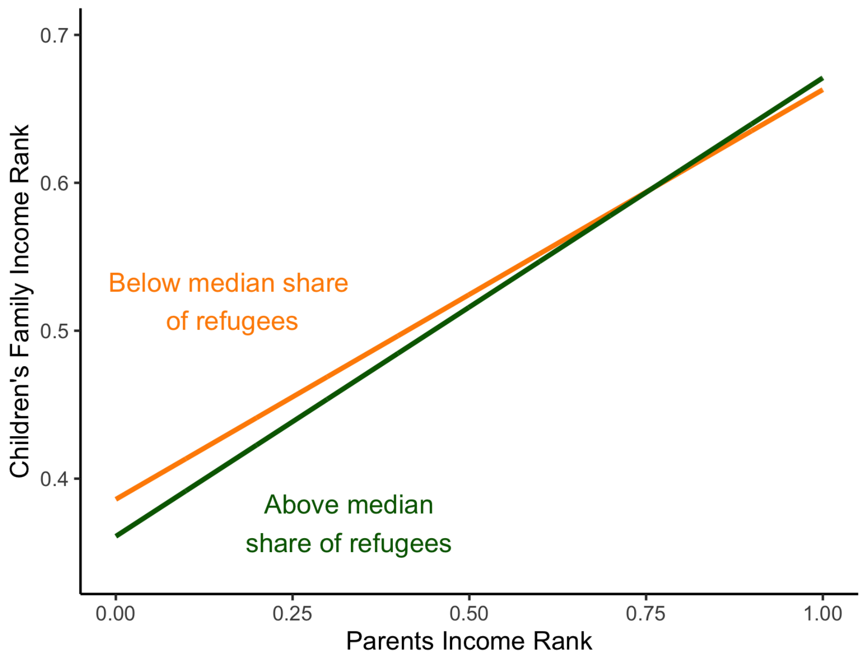


Figure D.2: Local linear regressions of child's family income rank by parents' income rank, comparing areas with above and below 1994 median share of refugees. The sample is split by median average family income across municipality.