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Unintended Consequences of Welfare Cuts on Children and Adolescents

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Abstract: This paper studies the effects of a large welfare benefit reduction on the children in the affected families. The welfare cut targeted adult refugees who received residency in Denmark, and it reduced their disposable income by 30 percent on average over the first five years. We show that children exposed to the welfare cut during preschool and school-age obtained lower GPAs, experienced reduced well-being and overall education levels, and suffered lower employment and earnings as adults. Children in their teens at exposure faced large increases in conviction probabilities for violent and property crimes.

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Keywords: Social assistance, welfare state, crime, education, inequality.

JEL: I24, I30, J10, K14

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Introduction

Transfer reductions are often intended to provide work incentives. However, they may have serious adverse consequences for target individuals and their families, with potentially harmful effects on children being a particular concern. The impact of social safety net reforms on children's long-run outcomes is studied in a number of papers (see e.g., Almond et al., 2018; East et al., 2023; Hendren and Sprung-Keyser, 2020; Hoynes et al., 2016, for recent reviews).[1] Yet, conclusive evidence on how cash transfer levels shape the lives of children among at-risk populations such as refugees across both the immediate, intermediate, and long run is sparse.

This paper fills this void by studying the unintended consequences of the Danish "Start Aid" reform enacted in 2002, which lowered cash transfers to newly arrived refugees.[2] Our analysis of the effects of the reform on children's lives across almost two decades provides critical insights into unwanted potential consequences of welfare policies with a similar focus on immigrants that have been implemented or proposed throughout Europe and North America over the past 20 years. For example, between 2000 and 2019, the EU-27 countries passed 176 bills on refugee and migrant welfare eligibility, program requirements, or welfare levels (OECD International Migration Outlook 2006-2019; OECD Trends in International Migration 1997-2004).

"Start Aid" reduced means-tested cash transfer levels to refugees who received residency in Denmark after the day of enactment by 40 percent on average, with reductions by up to 50 percent for couples and families with children. The reform effectively randomized families

[1] One strand of literature investigates the effects of parents' increase in working hours (and thereby also family income) on children's skills and education (e.g., Agostinelli and Sorrenti, 2021; Løken et al., 2018) and the effects of cash transfers on parental time-use and behavior in families with infants (Gennetian et al., 2022), while other studies document positive effects for children following increased family income due to, for example, EITC expansions (e.g., Dahl and Lochner, 2012; Hoynes et al., 2015, McInnis et al., 2023), the timing of tax credits during infancy (Barr et al., 2022), or program roll-out during the early 20th century (Aizer et al., 2016).

[2] An extensive literature has also studied labor supply responses to reforms focusing on vulnerable families such as single mothers (e.g., Eissa and Liebman, 1996; Hoynes, 1996; Meyer and Rosenbaum, 2001, Mogstad and Pronzato, 2012, Godøy et al., forthcoming) and refugees (e.g., Fasani et al., 2021).

already in Denmark to two different welfare regimes that differ only by transfer eligibility due to the short time between the reform's proposal and its enactment (March and July 2002, respectively) and the long average processing time for asylum cases of around 15 months.[3] The reform provides a clean design for studying welfare transfer reductions' immediate and longer-term effects This allows us to analyze the effects on children's lives on numerous dimensions such as test scores, educational attainment, employment and earnings in adulthood, and crime. We also present evidence of both short and longer-run cost-benefits of the transfer reduction.

Based on administrative register data tracking individuals up to two decades after the reform's implementation, we illustrate a large drop in disposable income of around 50 percent in the immediate aftermath of the reform, with an average 30 percent reduction in disposable income over the first five years after residency. For preschool-aged children, upon reform exposure, the reform reduced the likelihood of attending preschool by 24 percent. Moreover, there are dramatic longer-term consequences, with 9th-grade GPAs dropping by 0.18 of a standard deviation, thereby increasing the pre-existing achievement gap between refugees and native Danes by 40 percent.[4] We show that the reform resulted in GPA reductions across the entire GPA distribution. The adverse reform effects on academic achievement also occur at lower grade levels, particularly in vocabulary and language comprehension. At the same time, the reform led to lower self-assessed well-being and self-esteem during school hours. Therefore, the reform inhibited children's skill formation and academic development at an early age.

The reduction in disposable income also led to a sharp deterioration of children's educational attainment. The total years of completed schooling for refugee children of school

[3] To receive residency in Denmark, refugees must first request asylum. While processing their asylum case, the refugees live in accommodation centers.

[4] In line with previous research (Brell et al., 2020; Evans and Fitzgerald, 2017), children perform worse in school the older they are at exposure to the reform, with each additional year being associated with a 10 percent of a standard deviation lower 9th grade GPA.

age at exposure to the reform decreased by almost 0.6 years, corresponding to 20 percent of post-compulsory education in the pre-reform group. By following the group of children into adulthood, we also find that the reform led to lower employment rates and earnings, thereby increasing the earnings gap between native Danes and immigrants. For teenagers who accompany their parents, the reduction in transfer levels manifests in a doubling in crime rates, driven by both property and violent crime.

Our analysis indicates that the detrimental effects on children's lives work through two channels. First, the reform impeded integration efforts and connection to Danish society on several levels; fewer children attended public preschools, more children experienced learning difficulties and felt inferior in school, and adolescents became more violent. Second, the lower disposable income induced unintended behavior to supplement family income; a substantial number of adolescents engaged in property crime, and our results suggest that children dropped out of school earlier than they otherwise would have to take up work.

In the last section of this paper, we combine the effects the reform had on parents and their children, as identified in this paper.[5] The reform resulted in a short-run positive return of around $2,500 per family. This short-run effect is driven by adults' positive labor supply response to the reform. Yet, the large detrimental effects on children outweigh any positive short-term effects for adults. As the unintended longer-run consequences on children kick in, the reform effect turns negative at approximately -$12,000 per family. Reductions in transfer payments inevitably involves a trade-off between lowering public expenses and increasing employment incentives on the one hand, and potential adverse responses of the target group,

[5] In Dustmann et al. (2023), we study the reform's impact on adults' outcomes. Dustmann et al. (2023) show that employment doubles from a pre-reform mean of 10% in the first year after implementation but still leaves around 80% of families reliant on transfers. However, the employment effects quickly fade and are nearly zero five years after implementation. In addition, the reform also resulted in increasing adult crime rates, including women who become more likely to commit subsistence crimes.

which may materialize in the longer run, on the other. Our findings emphasize the importance of considering both aspects when evaluating policies.

Our conclusion that cash transfer reductions have severe consequences for the target group's children along such dimensions as skill acquisition, education, and earnings adds to the literature relating family income and welfare reforms to children's later outcomes (e.g., Aizer et al., 2016; Bailey et al. 2020, Dahl and Lochner, 2012; East et al. 2023; Hoynes et al., 2016; Løken et al., 2012; Løken et al. 2018).[6] We contribute to this literature by studying the overall effects on GPA, test scores, and crime and identifying effects for different age ranges and skill domains. By focusing on children in refugee families, we also contribute to the literature studying the integration of immigrants (see, e.g., Brell et al., 2020, for a recent review) and provide policy-relevant insights on how welfare policies can affect the lives of refugees and their families stressing how a policy that appears to yield a positive return in the short run may result in substantial negative returns in the longer run. Moreover, our results are in line with the previously estimated effects of changes in disposable income among vulnerable groups (e.g., Akee et al., 2010; 2018; Deshpande, 2016; Deshpande and Mueller-Smith, 2022, studying Native Americans and low-income youths, respectively) and in stark contrast to the absence of effects of changes in disposable income for children among more affluent groups (e.g., Cesarini et al., 2016, studying lottery winners in Sweden).

Our analysis of the adverse impact on children's earnings when they reach adulthood provides contemporaneous evidence to previous studies presenting long-run evaluations of 19th and early 20th-century welfare policies on both the target group and their children (e.g., Bleakley and Ferrie, 2016; Aizer et al., 2016; Melander and Miotto, 2023).

[6] The welfare policies studied earlier range from Medicaid (e.g., Brown et al., 2020; Goodman-Bacon, 2021), in-kind transfers such as Food Stamps / SNAP (e.g., Bailey et al., 2020; Hoynes et al., 2016), and the effects of transfer income through changes to welfare eligibility and requirements (e.g., Grogger and Karoly, 2005) but without any clear identification of the effects of cash transfer levels in isolation, which this paper focusses on.

Our results for adolescents' crime contribute to work that associates crime with either the timing of welfare payment (e.g., Foley, 2011; Carr and Packham, 2017), welfare levels (Dustmann et al., 2023), welfare eligibility (Deshpande and Mueller-Smith, 2022; Khanna et al., 2023; Yang, 2017), temporary cash-assistance (Palmer et al., 2019), and state variation in welfare reform implementation in the U.S. (Corman et al., 2014).[7] We complement this literature by showing that cash transfer cuts affect various domains of criminal behavior – not only among the target group but also among their children – and how the consequences of exposure to lower family income for criminal behavior may accumulate over time for adolescents in high-risk populations.

2. Background, Estimation, and Data

2.1 The Start Aid Reform

The generous social assistance (SoA) transfers, liberal refugee legislation, and low labor market attachment of non-Western immigrants resulted in rapidly increasing welfare expenses for refugees in Denmark during the 1990s. To promote refugees' integration into the labor market and Danish society more broadly while reducing costs, a newly elected government proposed a bill on March 1st, 2002, approved on June 6th and dubbed "Start Aid." The bill reduced transfer levels for refugees who received residency in Denmark after the reform's enactment on July 1st, 2002 (Danish Prime Minister's Office, 2002).[8] Since the date of residency determines whether families are exposed to the Start Aid reform, we use age at *residency* and *reform exposure* interchangeably.

[7] Several recent studies have also documented the effects of in-kind transfers such as food stamps (Tuttle, 2019) and Medicaid coverage (Arenberg et al., 2020; He and Barkowski, 2020; Jacome, 2022) on crime.

[8] The bill states that "..to ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly, the incentives for finding employment must be strengthened" (author translation of official text: http://webarkiv.ft.dk/Samling/20012/lovforslag_som_fremsat/L126.htm, accessed 21-09-2022). Appendix B.1 describes the reform in detail and also provides detailed SoA and Start Aid transfer levels by household type.

Before receiving residency, refugees must request asylum and live in accommodation centers (refugee camps). During that time, their application is processed and decided upon (the specific timing of the decision depends on the individual caseworker's caseload). As the average processing time in question was 15 months (Hvidtfeldt and Schultz-Nielsen, 2018), refugees already awaiting a decision on their asylum request were effectively randomized to Start Aid or SoA based on when they received residency around the reform's implementation.

Fig. 1A illustrates the reform's impact on average transfer rates for families with children by their timing of residency relative to the reform. The figure shows that refugees were, on average, eligible for transfers of $1,300 per month if they received residency before the reform. However, transfer levels dropped to around $750 for refugees with post-reform residency. Thus, transfer reductions amounted to almost 50 percent for families with children relative to the pre-reform level.[9]

Fig. 2 presents the reform's effects on parents' employment, earnings and transfer income along with the corresponding results for all adults from Dustmann et al. (2023). The reform led to substantial short-run effects on employment and labor earnings, both increasing by approximately 100% in the first year following residency to Denmark (an effect of around 9 percentage points and $1,000 in annual labor earnings, respectively). However, the labor market attachment of refugees remained low, and the reform effects were short-lived and fading within a few years.[10]

Thus, while the reform did lead some adults to find employment faster than they otherwise would have, average disposable income (transfer income and earnings minus tax

[9] Both SoA and Start Aid are means-tested at the household level with implied marginal tax rates close to 100 percent until labor income is sufficiently high to exhaust all welfare eligibility, thereby providing a clear example of welfare dependency with the potential of a poverty trap at zero labor supply.

[10] Figs. 2 (employment, labor earnings, and transfer income), A.1 (labor market outcomes by gender), and A.2 (crime by gender) also show that parents' reform responses do not vary significantly by children's age at residency. The only exception is the employment response for fathers of children aged 14-18 at residency, which is lower than for the other groups, leading to a lower labor force drop out for mothers in this group (due to the household-level means-testing where wives lose their transfer eligibility when husbands take up employment).

payments) dropped from around $1,400 to $800 (see Fig. 1B), with the fraction with an average monthly disposable income below $750 (approximately equal to the U.S. poverty threshold, U.S. Census Bureau, 2020) increasing from a pre-reform level close to zero to a post-reform level of around 50 percent (Fig. 1C). As illustrated by Fig. A.2, this sharp reduction in disposable income resulted in higher crime rates among parents. The crime increases were related to subsistence such as shoplifting in supermarkets (see Dustmann et al. 2023). Moreover, the disposable income reductions were persistent, with Fig. 1B illustrating a decrease in average disposable income for the first five years after residency of around 30 percent.[11]

2.2 Econometric Framework

We estimate the effect of the reform using a regression discontinuity design that compares refugee children whose parents received residency just before and after the reform:

$$y_i = \alpha + \beta * reform_i + g(Z_i)'\pi + X_i'\gamma + \epsilon_i, \qquad (1)$$

where y_i is an outcome of individual i, $reform_i$ is a dummy indicating whether residency was granted pre- or post-reform (i.e., whether parents of individual *i* were eligible for SoA or Start Aid transfers), and Z_i is the running variable counting months between the residency decision and the reform date (we allow for different trends on each side of the reform).[12] Moreover, X_i collects observable characteristics,[13] and ϵ_i is an idiosyncratic error term. The parameter β measures the causal effect of the transfer reduction for refugees if there is no discontinuity in

[11] Fig. A.3, illustrating how the reform affected the distribution of household disposable income, shows significant reductions throughout the entire distribution.

[12] We assign children their parents' residency dates because that is used to determine transfer eligibility. We will treat Z_i as linear functions pre- and post-reform in the main specification. However, we show that all results are robust to alternative strategies, such as using a local polynomial regression discontinuity design (see Calonico et al. 2018).

[13] Our main specification only controls for the running variable $g(Z_i)$ and not covariates X_i to avoid imprecision in the specifications with few observations (e.g., crime in by gender Table A.8). Table A.5 shows that the paper's main results remain unaffected when we control for X_i, which is to be expected given the balancing of refugees' characteristics around the reform implementation (see, e.g., Table 1).

the sample density and refugees' characteristics around the reform. In Section 2.3, we provide evidence supporting these identifying assumptions.

While Eq. (1) is our main specification, we also use three extensions. First, to facilitate a more direct comparison between the estimated effects of the reform and the literature studying welfare reform effects and labor supply elasticities, we report estimated elasticities of educational outcomes, earnings, and crime concerning transfer levels. We evaluate these as $\frac{0.5*\beta}{\alpha+0.5*\beta} / \frac{Start\ Aid-SoA}{SoA+Start\ Aid}$. Here α, β are the estimated parameters for each outcome in Eq. (1), and SoA and Start Aid indicate the transfer levels that pre- and post-reform refugees are eligible for, respectively.[14]

Second, we estimate how the reform has affected refugees' full GPA distribution by creating a series of dummies $1[y_i \leq x]$ for whether a child's GPA y_i was x or below, varying x from the minimum to the top of the GPA distribution. By estimating Eq. (1) with these dummy variables as outcomes, we capture the changes in the cumulative GPA distribution with estimates of α and $\alpha + \beta$ corresponding to the pre-and post-reform cumulative densities, respectively.

Third, to pinpoint with greater precision the ages when refugee children are most sensitive to the transfer reduction, we also estimate the reform effect as in Eq. (1) while weighting observations according to the distance between their age at residency A_i and A_0 varying the kernel center A_0 from 2-10:

$$\min_{\alpha[A_0], \beta[A_0], \pi[A_0]} \sum_{i=1}^{N} K_{h_\lambda}(A_0, A_i)$$

[14] We estimate the elasticities by computing the percentage change in each outcome relative to the percentage change in the transfers for which each household is eligible. To ensure that the estimated elasticities are invariant to base specifications, we calculate percentage changes relative to the midpoints between pre- and post-reform levels: $\frac{0.5*\beta}{\alpha+0.5*\beta}$ for outcomes and $\frac{Start\ Aid-SoA}{SoA+Start\ Aid}$ for transfers. To see how these two specifications are equivalent, note that $Start\ Aid - SoA = \mu$ constitutes the change in welfare benefits due the reform such that $\frac{Start\ Aid-SoA}{SoA+Star\ \ Aid} = \frac{\mu}{SoA+SoA+\mu} = \frac{0.5*\mu}{SoA+0.5*\mu}$.

$$\times \{y_i - (\alpha[A_0] + \beta[A_0] * reform_i + g(Z_i)[A_0]'\pi)\}^2 \qquad (2)$$

such that the estimates of $\alpha[A_0]$ in Eq. (2) will capture the pre-reform level and estimates of $\beta[A_0]$ will capture the effect of the reform at local age ranges.[15]

Throughout the analysis, we cluster standard errors by the running variable except when we estimate elasticities (see Tables 2 and 3), where we report bootstrapped standard errors.

2.3 Data

Our data are drawn from administrative registers of all refugees who receive residency in Denmark.[16] We describe all outcomes and data sources in detail in Appendix B.2.

We limit the sample to families with children receiving residency in a window of 18 months around the reform (January 2001 to December 2003), and we focus on refugees from countries that do not change status during the period in question.[17] This results in a sample of 6,888 individuals; 3,406 adults aged 18-45 with children and 3,482 children aged 0-18 (children's residency date correspond to at least one of their parents' residency date ensuring that the running variable is tied to the transfer eligibility of the parent). Using unique individual identifiers in the Danish registers, we merge the sample of refugees with information on income from tax authorities, labor market outcomes, crime from police and court records, and education from the Ministry of Education.

When we study educational outcomes, we use register data for a wide range of attainments reported by educational institutions to the Ministry of Education. We consider measures for "completed education" (both years of schooling and specific degrees such as lower

[15] $K_{h_\lambda}(A_0, A_i)$ is an Epanechnikov kernel weighting observations around each age A_0.

[16] Our sample includes only refugees and individuals who are family reunified to refugees, because labor migrants, their families, and other nonrefugee migrants are ineligible for SoA or Start Aid and thus unaffected by the reform.

[17] This relates to individuals from Afghanistan and the former Yugoslavia. The changes were administrative decisions that took place in the months preceding the reform as a result of contemporaneous conflicts in these countries unrelated to the Start Aid reform. We also limit the sample to refugees who do not emigrate. Table A.1 present balancing tests of remigration for all adults, parents, and children. There are no changes in remigration around the reform.

secondary or vocational degrees) and 9th-grade GPA at the end of compulsory education. We supplement this with register data on whether the children attended preschool, language (Danish) test scores in 6th grade (along with scores for the three specific language and reading components), and questionnaire responses from the children about their perception of their well-being and self-esteem in school.[18]

We measure earnings (including self-employment) and disposable income (earnings and transfers minus tax payments) based on tax authority records. An individual is employed if they have positive earnings. The last year of income data is 2019, which allows us to measure reform effects until 16 years after residency for all individuals in the window of 18 months around the reform.

The crime data are obtained from police and court records for all crimes in Denmark. In addition to unique individual identifiers, the data includes unique case identifiers, specific offense and conviction dates for our entire sample, and detailed offense codes that enable us to identify the exact crime type committed. We focus on crimes that lead to a conviction, and we count crime by the date of the offense (such that, for example, "crime in year 1" is a crime committed during the first year after residency that leads to a conviction at some later point in time).

[18] As the questionnaire was introduced in 2015, we only have information for 8th and 9th grade responses from children younger than six at residency. The questionnaire asks for several dimensions ranging from the relationship with teachers and peers to the assessment of schools' sanitation quality. We limit the focus to children's views of their school life and consider responses to the following questions: i) How often can you solve problems if you try hard enough?; ii) how often do you succeed in what you set out to do?; iii) how often does your stomach hurt?. The questionnaire responses are measured on a Likert-scale: "Very often", "Often", "Sometimes", "Rarely", and "Never". We construct the average score across the responses to three questions (assigning responses values 1,..,5). In addition, students can also choose a "do not wish to respond" category. There is no selection in missing questionnaire information across the reform: Estimating Eq. (1) with a dummy indicating no response / no questionnaire information as outcome results in a coefficient of 0.018 (0.039).

2.4 Age of first exposure to the reform

The importance of circumstances during early childhood is well illustrated by Cunha and Heckman (2007), among others. However, several studies have shown multiple stages of development sensitivity (e.g., Carneiro et al., 2021). For example, the preschool age range is a sensitive period for pre- and early-literacy skill development (Mol and Bus, 2011) and for second language acquisition (e.g., Böhlmark, 2008; Newport, 2002). It is also an age range where the substantial skill gaps between Danish and non-Western immigrant children manifest (e.g., Bleses et al., 2016), and where large changes in behavior and skills that are related to self-regulation unfold (e.g., Akee et al., 2018; Belsky et al., 2020; Steinberg 2014). Thus, the effects of the reform-induced drop in household income will likely differ according to the age of exposure.

The results presented in the main text consider the critical ages for each of our different outcome domains – GPA and language test scores, educational attainment, and crime – as identified in Eshaghnia et al. (2023), who find that critical ages for investments differ strongly across outcomes.

When considering impacts on GPA and test scores, we focus on children exposed to the reform before school starts (age 0-7).[19] When investigating the effect on educational attainment and earnings, we focus on children exposed to the reform in school-age (age 7-14). When estimating the effects on crime, we focus on children exposed to the reform in adolescence (age 14-18).[20] We report results for all age groups in the Table A.2.

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[19] Jakobsen et al. (2017) have examined the impact of the 2002 Start Aid reform on children's 9th grade test scores and enrollment in upper-secondary education but only for children of school age at the time of residency. We confirm their null-finding for the outcomes and children in question but revise their conclusion of no effect of the reform. By studying a broader group of children with longer follow-up period, we show that children receiving residency in preschool age experience significantly lower test scores and children receiving residency in school age experience substantially lower educational *attainment*, earlier school drop-out, and lower long run employment and earnings.

[20] In addition, the three age-groups also minimize the risk of rejecting reform effects due to flooring: i) GPA decreases strongly in age-of-residency leaving little room for negative effects for children aged 8 and above at residency; ii) educational attainment is not a meaningful outcome for those exposed to the reform as adolescents, because only a few refugees older than 14 at residency enter the Danish education system, and we cannot measure

Results

Table 1 presents summary characteristics of the sample (columns 1 and 4). The average age of adults (parents) and children at the time of residency was 32 and eight, respectively. Around 60 percent of adults in the sample are female, and 14 percent are single. Each parent has, on average, 2.6 children.

We also present a variety of balancing tests around the reform implementation. There is no sign of discontinuities in refugees' observable characteristics or sample density – neither overall nor in specific age groups.[21] Columns 2 and 5 show the results of conditional balancing tests for observable characteristics for adults and children separately, where we have regressed the reform indicator on all the covariates (using "Region of origin: Africa" as reference category) and the running variable. We report p-values from F-tests for joint significance of the estimated coefficients for the covariates in Panel B. Columns 3 and 6 shows the corresponding unconditional balancing tests for children, where we have regressed each covariate on the reform indicator and the running variable (Table A.3 reports the balancing tests separately for the three age-groups). Of the 69 reported tests, only two are significant at a 5% level (just as we would expect if the reform was as good as random). Panel C of Table 1 presents the results from tests of changes in sample density around the reform (McCrary, 2008), showing no sign of a discontinuity.

While the results in Tables 1 and A.2 consider broad regions of origin, Fig. A.4 tests the composition of all 53 individual origin countries in our sample across the reform – i.e.,

the total effect on schooling for those exposed to the reform before school starts, as some in this group will still need to complete their final education; iii) the legal criminal age in Denmark was 15 during the period in question, which implies that immediate reform effects on crime can only be measured for individuals older than 14 at residency.

[21] The absence of structural breaks around the reform in refugee characteristics and the running variable density is the fundamental identifying assumption irrespective of any longer-term changes in migration flow to Denmark that may have followed the reform, as suggested in Agersnap et al. (2020). Dustmann et al. (2023) present further balancing tests for the sample of refugees considered in this paper. All tests confirm the validity of the identifying assumptions and the empirical strategy outlined above.

whether there is a change in the fraction from Iraq, Somalia, and so forth. We only reject balancing tests for specific origin countries around the reform to the degree we would expect if the reform was as-good-as-random for the sample in question (e.g., 5% of tests at a 5% significance level).

Fig. 3 plots outcomes (GPA, years of schooling, earnings, and crime) across the timing of the reform, displaying strong discontinuities. Figs. A.5 and A.6 contrast the actual discontinuities to the corresponding outcomes predicted from OLS regressions on the covariates in Table 1. The pre-and post-reform trends for predicted outcomes connect across the timing of the reform with no discrete changes.

3.1 Reform Effects on Skill Formation

We first investigate the effects for children aged 0-7 at exposure to the reform (Panel A in Table 2). Column 1 shows a substantial reduction in 9th grade GPA of 18 percent of a standard deviation, increasing the already significant test score gap between refugee children and their native counterparts by around 40 percent. Similarly, the fraction of refugees in the lowest GPA quartile increases from a pre-reform level of 41 percent by 8.8 percentage points to a post-reform level of 50 percent (column 2; see Table A.4 for results for all test score quartiles). Moreover, column 3 indicates that the reform reduced children's responses to well-being and self-esteem in school by around 0.23 of a standard deviation suggesting that the reform inhibited refugee children's skill formation and led to an increased alienation in Danish schools and society. The withdrawal from educational institutions appears to begin even before school starts: Column 4 shows that the reform led to a considerable reduction in daycare and preschool enrollment (for children aged 0-5 at the time of residency). This 24 percent reduction (-0.174/0.733, column 4) is remarkable given the intense focus in the public debate on the importance of immigrant children's preschool use as a lever to improve their Danish language skills at an early age (Ministry of Children and Social Affairs, 2018).

Table A.5 shows that the paper's main estimation results are stable across different specifications, including covariates and year of residency fixed effects, donut specifications around the reform, and using a local polynomial regression discontinuity design instead of a linear specification.

Fig. 4 expands the previous results on GPA in two dimensions. First, Fig. 4A presents the pre- and post-reform cumulative 9th-grade GPA distributions for children aged 0-7 at residency (see Section 2.2 for a description of the estimation procedure), along with the total population cumulative distribution. The figure shows that the entire pre-reform distribution shifts substantially leftwards compared to the full population (natives and immigrants) distribution. For example, the GPA at the 20th percentile for pre-reform refugees corresponds to the 12th percentile in the total population. In contrast, the GPA at the 80th percentile for pre-reform refugees corresponds to the 65th percentile in the total population. However, as the figure shows, the cumulative distribution for post-reform refugees is shifted leftward even further. The 20th and 80th percentiles of the post-reform distribution correspond roughly to the 8th and 57th percentiles in the total population.

Second, to investigate whether there are specific age ranges where children are susceptible to the transfer cuts induced by the reform, we consider children younger than ten years at reform exposure. Figs. 4B and 4C show the effects of the reform on children's 9th grade GPA across ages at reform exposure based on Eq. (2). Fig. 4B illustrates that the significant reduction in GPA is driven by children exposed to reform close to school start.

Fig. 4C visualizes the impact across ages at reform exposure more clearly. The figure compares the pre- and post-reform 9th grade GPA averages ($\alpha[A_0]$ for pre-reform and $\alpha[A_0] + \beta[A_0]$ for post-reform levels, see Eq. (2)) across age at residency, where the vertical axis carries age at residency (A_0 in Eq. (2)). The figure shows a clear negative association between school performance and age at residency, with being one year older at reform exposure leading to a drop of 12 percent of a standard deviation on average and with the steepest decline for school-aged children aged 8 to 10. Comparison of the GPA across different ages for pre- and post-reform refugee children shows that the "learning loss" due to the reform corresponds to being several years older when receiving residency in Denmark. For example, a child receiving residency post-reform at age 5 has an average GPA corresponding to a child receiving residency pre-reform at age 8. Thus, the reform has delayed refugee children's skill development and integration, corresponding to arriving in Denmark several years later than they did.[22]

Figs. 4D-F extends the results by considering 6th-grade test scores on Danish language proficiency. As the test scores were collected in 2010, we can only estimate the relationship between the reform, age at residency, and test scores for children aged seven or younger at residency. The graphs in Figs. 4D-E mimic Figs. 4B-C closely, showing a largely negative and significant reform effect on language test scores for those receiving residency between ages 4 and 7. Fig. 4F splits test scores into various components and shows that the lower grade 6 test scores are mainly driven by "Vocabulary and language comprehension" deficiencies. All this suggests that the onset of the learning losses was initiated shortly after residency to Denmark when the children were close to school start, thereby hindering their academic development more broadly as time progressed.

In sum, the results presented in this section suggest a strong withdrawal from Danish society. For example, as public preschool fees are means tested, the reduction in preschool enrolment likely does not reflect percuniary considerations but instead an reform induced unwillingness to conform with Danish childcare institutions and norms.

[22] The negative relationship between age at residency and test scores may also explain the null-finding for children in school age at residency, which Table A.2 and Jacobsen et al. (2017) show. Average test scores for children aged 7-14 at residency were 1.2 standard deviations below the national mean (see Table A.2, column 1, panel C) leaving very little room for any further reduction.

3.2 Reform Effects on Educational Attainment and Earnings

We next consider the reform's impact on children's educational attainment and earnings, focusing on children aged 7-14 at reform exposure.

Panel B in Table 2 shows that reform exposure during school age led to substantially lower educational attainment. Years of schooling drop by around 0.55 from a pre-reform mean of 12, corresponding to a reduction of 20 percent of educational attainment beyond the nine years of compulsory education (column 5), while the fraction with no further educational attainment beyond lower secondary schooling increases by almost 30 percent (0.101/0.373, column 6). This results in an even more significant gap in educational attainment between native Danes and refugees, widening from 1.3 to 1.9 years due to the reform (13.5 years relative to 12.2 and 11.6 years, see column 5). This substantial decrease in post-lower secondary education is mainly driven by a reduction in those who obtain vocational degrees (column 7).[23] Table A.6 suggests that the reduction in completed schooling and early school drop-out is linked to an earlier labor market entrance. The table shows that the fraction who are not working but enrolled in education at age 17-18 decreases by eight percentage points while earnings of 17–18-year-olds increase; fewer had earnings between \$0-1,499 per year, but more had earnings between \$1,500-2,999 per year (corresponding to work around 10-15 hours per week on average). There are no changes in the probability of earning more than \$3,000 annually.

These results likely reflect unintended behavior to supplement family income induced by the reform. While this may appear as a optimal response in the very short run, early school drop out affect labor market attachment and earnings potential negatively in the longer run as

[23] We group education into four main categories: i) lower secondary schooling; ii) high school; iii) vocational degrees; iv) college degrees and higher. Table A.6 shows results for all four educational categories. A vocational degree requires 2-3.5 years combined enrollment in vocational schools and apprenticeship training (for technical professions such as carpenters) or 1.5-3 years of enrollment in vocational colleges (for welfare professions such as health care workers).

shown in Columns 8 and 9 in Table 2. Here, we investigate the longer-run consequences of lower educational attainment on employment and earnings. The reform reduced the probability of working 15-16 years after reform exposure (i.e., between ages 23-30) by seven percentage points (9 percent relative to the pre-reform mean, column 8). Moreover, it led to an annual earnings reduction of around 25 percent (-3.428/13.574, column 9), increasing the native-refugee gap in earnings from about 35 percent to 50 percent.

3.3 Reform Effects on Adolescents' Crime

We turn next to the consequences of the reform on criminal behavior. Estimates in Panel A of Table 3 show that adolescents' crime response to the reform builds up over the years after residency. During the first five years, the fraction aged 14-18 at residency with a criminal conviction increased from a pre-reform level of 16 percent to a post-reform level of 28 percent (0.160+0.121), and the number of criminal convictions more than doubled from a pre-reform level of 0.27 to a post-reform level of 0.64 (0.265+0.370).

The table also shows that the effects on the number of crime convictions (intensive margin) continue to increase beyond year five, while the impact on the probability of receiving a crime conviction (extensive margin) remains relatively stable. Thus, the crime effects during the first five years after reform exposure are due to offenders who would not otherwise have committed any crime, while the effects beyond year five after exposure are driven by those who were offenders by year five and continue to commit more crime than they otherwise would have.[24]

Distinguishing by crime type shows that the extensive margin effects on adolescents' crime during the first five years after reform exposure are driven by property crime, such as

[24] The increase in youth employment (i.e., absence of idleness) when dropping out of school as suggested from Table A.7 may explain why there are no effects on crime (see Table A.2, Panel C, column 5) even though education decreases substantially for those aged 7-14 at residency.

larceny and theft, and violent crime, with the probabilities of receiving a crime conviction of the respective type increasing by around 11 and 7 percentage points.

While the more immediate effects of the reform (over the first five years) may have led to criminal behavior to compensate for the lower disposable income (property crime), in the longer run, the intensive margin effects on crime are mainly driven by violence. The estimated reform effects on the number of violent crime convictions doubled from years 5-10 (from 0.10 to 0.19), while the impact on the number of property crimes remained unchanged. Moreover, the rise in crime is concentrated among males, whose crime conviction probability increases by almost 20 percentage points during the first ten years after residency (see Table A.8).

The finding of increased violent crime stands in contrast to both the reform effects on adults' crime (see Dustmann et al., 2023), which are presented in Fig. A.2, and findings by Deshpande and Mueller-Smith (2022), who show that lower transfer levels induce economically motivated crimes.[25] Our results suggest that the reform led adolescents to engage in crime and conflict-oriented behavior, perhaps due to feeling "left out" and deprived of lawful ways to achieve aspirations (e.g., Blau and Blau, 1982; Merton, 1938).

Panel B of Table 3 presents the estimated elasticities of adolescents' crime with respect to transfers, showing that a one percent decrease in welfare benefits results in approximately a one percent increase in the probability that adolescents receive a criminal conviction over the following ten years. The estimated elasticities for crime measured as the number of convictions are even larger, with a one percent decrease in welfare benefits leading to 1.4 percent more crimes on average over ten years. Hence, the unintended responses to the reform for adolescent children are – just as we found for earnings in Section 3.2 – substantially larger (in relative terms) than adults' intended labor supply responses.

[25] Related, Khanna et al. (2023) show that discouraging formal employment via benefits eligibility criteria induces organized crime but not crimes characterized by impulsivity.

The literature has documented an intergenerational link in crime (see Wildeman, 2020, for a review). For example, several studies have found that parental incarceration increases crime among the offenders' children (e.g., Wildeman and Andersen, 2017; Dobbie et al., 2018, Finlay et al., 2023), suggesting that family disadvantage may push children into a criminal trajectory. To investigate possible links between the reform and criminal behavior of parents and their adolescent children, we analyze whether children's crimes were influenced by an indicator of whether their parents received a criminal conviction after residency in Denmark. Panel C of Table 3 reports the results. The reform increased the probability that both a parent and child in a family received a criminal conviction. While less than 2 percent of pre-reform families experience a criminal conviction for both parent and child during the first ten years after residency, this is the case for around 8 percent of post-reform families (0.017+0.062, column 3). Comparing the magnitudes of the estimated effects in Panels A and C (e.g., the 0.135 reform effect in Panel A vs. the 0.062 reform effect in Panel C) indicates that a substantial part of the reform effect on adolescents' crime is concentrated in families where the parents also engage in crime (this is not a crime committed together; parents' crime is mainly theft while children's crime consists of a broader array of offenses).[26]

4. Overall Effects of the Reform

We next provide some simple calculations on the combined immediate and longer-term effects of the reform on adults' earnings (the increase of which through incentivizing labor market participation was the intended objective of the reform), the effects on crime of parents, and the detrimental effects on children that we discuss in this paper.

[26] As parents' crime in the first year after residency also increases following the reform (see Fig. A.2), we cannot definitively conclude that the increase in crime within a family as shown in Panel C reflect only adolescents' reform response. However, as parents' crime response is concentrated in year one after the reform, an alternative specification obviating this issue only counts the overlap between children's and parents' crime convictions from year two onward (i.e., where the parents no longer have increased crime rates as a response to the reform). In that case, we get similar, albeit less precisely estimated, results, which suggest a direct link between adolescents' crime response and parents' criminal behavior.

We estimate the reform effect (as in Eq. 1) for each generation g (children and adults), outcome y (earnings and crime), and time t since residency (two-year bins from years 1-2 to years 15-16 years):

$$y_{i,t} = \alpha_{g,t}^{y} + \beta_{g,t}^{y} * reform_i + g_{g,t}^{y}(Z_i)'\pi + \epsilon_{i,t} \quad (3)$$

The estimates are presented in Table A.9. We then calculate the net returns of the reform as the sum of effects on children and adults for each outcome and year-bin ($\beta_{child,t}^{y} + \beta_{adult,t}^{y}$). To monetize the crime effects, we use the estimates reported in Cohen and Piquero (2009), where we weigh their reported costs across crime types by the pattern observed in our sample such that the net return for crime in, for example, years 3-4 after residency is estimated as the reform effect on adults' crime in years 3-4 after residency multiplied with the average costs of their crime in those years plus the reform effects on adolescents' crime multiplied with the average costs of their crime in those years.

Fig. 5A presents the estimated net returns. The dotted line in the figure shows initially positive returns on earnings due to the employment response by adults, which turns negative in the long run as the effects on children's earnings begin to dominate. In contrast, the effects on crime translated into monetary units (dashed line) are negative in the first eight years following residency. During the first two years, these negative effects are driven by adults, while the reform effect on adolescents' crime causes the impact in years 3-8. This simple exercise illustrates that conclusions of whether the reform leads to positive overall returns depend on the outcomes and the time horizon considered.

To illustrate that better, we next estimate the cumulative return of the reform (using an annual 2% discount rate) evaluated at different time horizons (from a short-run perspective in years 1-2 until a longer-run perspective in years 15-16 after residency) for a family of two adults and two children, aged 7-14 and 14-18 at time of residency.[27] This composition corresponds to the average family in our sample.

$$Cumulative\ return_{y,t} = \sum_{t=1-2}^{T} 0.98^{t} \left(\beta_{child,t}^{crime} + 2 * \beta_{adult,t}^{crime} + \beta_{child,t}^{earnings} + 2 * \beta_{adult,t}^{earnings}\right),$$

$$for\ T = 1-2; \ldots ; 15-16 \qquad (4)$$

Fig. 5B shows that a short-run evaluation would conclude that the reform had a positive cumulative return. However, as soon as the unintended consequences for children dominate, the cumulative return becomes negative. Over a 16-year time horizon, the reform generated a negative return of around -$12,000 for the average family in the sample. This is a conservative estimate as we cannot yet measure the consequences of younger children's poorer language skills when they reach adulthood.

5. Conclusions

In this paper, we study the effects of a substantial reduction in cash transfers for refugee immigrants on their children's educational attainment, earnings, test scores, language proficiency, well-being, and crime. We find substantial adverse effects on the educational attainment of children whose parents were affected by the reform through lower participation rates in preschool programs, lower GPAs, poorer well-being and lower self-esteem, fewer years of completed schooling, and lower earnings as adults. Moreover, we also show that exposure to the reform led to a considerable rise in adolescent crime. The reform affected children in numerous dimensions, with stark negative and long-term consequences for their future lives.

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[27] For example, the estimated cumulative return in year 5-6 is calculated as the discounted sum of net returns for earnings and crime in years 1-2, 3-4, and 5-6:
$0.98^{2}\left(\beta_{child,1-2}^{crime} + 2 * \beta_{adult,1-2}^{crime} + \beta_{child,1-2}^{earnings} + 2 * \beta_{adult,1-2}^{earnings}\right) + 0.98^{4}\left(\beta_{child,3-4}^{crime} + 2 * \beta_{adult,3-4}^{crime} + \beta_{child,3-4}^{earnings} + 2 * \beta_{adult,3-4}^{earnings}\right) + 0.98^{6}\left(\beta_{child,5-6}^{crime} + 2 * \beta_{adult,5-6}^{crime} + \beta_{child,5-6}^{earnings} + 2 * \beta_{adult,5-6}^{earnings}\right).$

This paper's central message is that welfare reforms targeted at increasing individuals' labor supply by reducing transfer payments can have unintended and very detrimental consequences for their children. While the reform we study here initially yields a positive return due to the increase in adults' labor supply, these are outweighed by the adverse, longer-term effects that run through their children's poorer educational and labor market performance and higher crime rates. Our findings emphasize the importance of considering not just the intended but also the unintended consequences of reforms when evaluating their overall benefits. Moreover, they also stress the relevance of assessing not just immediate but also long-term effects. Although our study relates to a specific reform in Denmark, our conclusions are relevant for current discussions of welfare reforms aimed at groups like the one studied in this paper.

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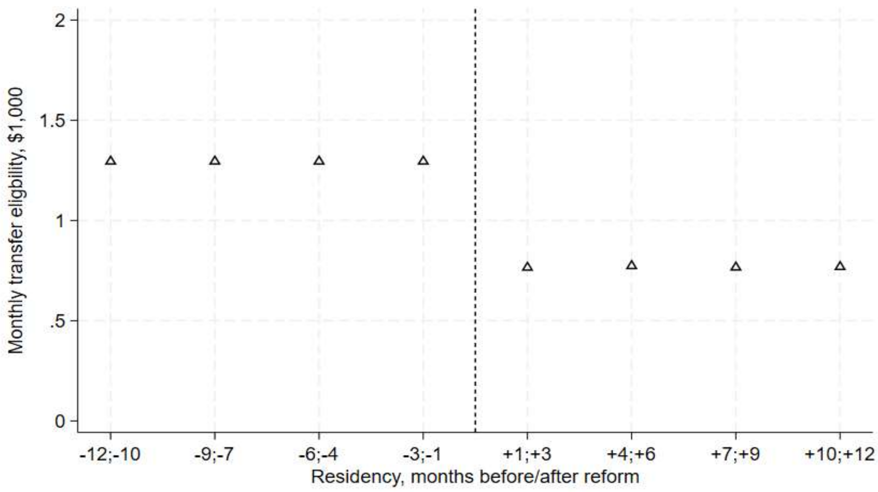
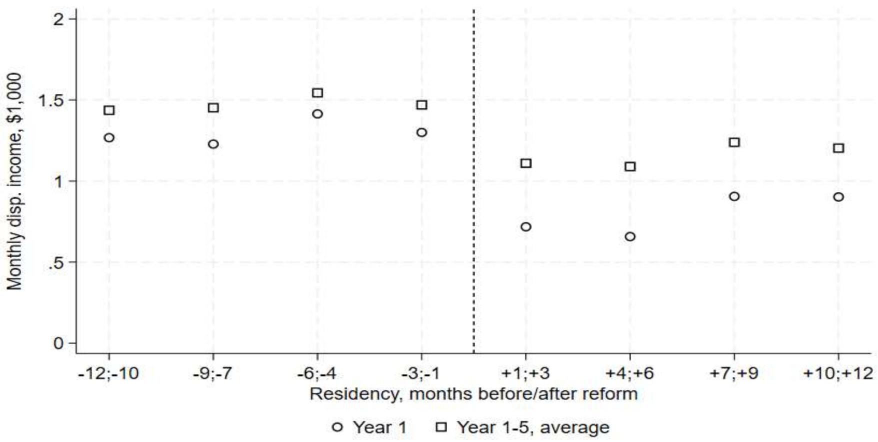
Figure 1. Refugees' transfers and disposable income by timing of residency before/after reform.

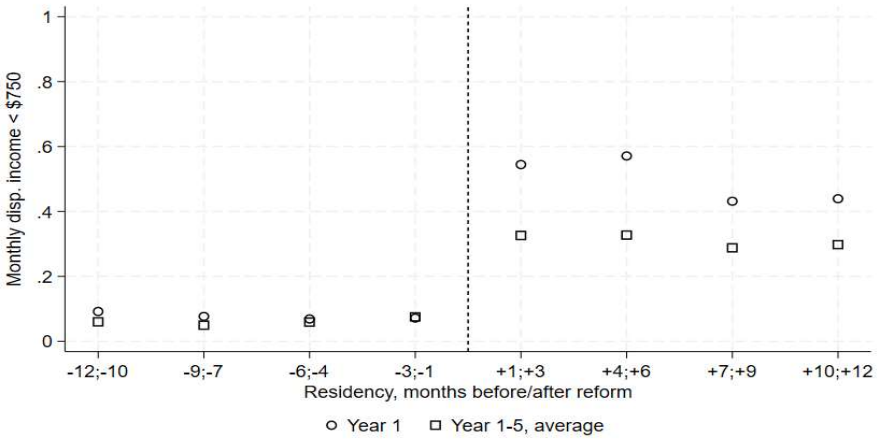
A) Monthly transfers that individuals are eligible for, $1,000



B) Monthly disposable income, year 1 and average of year 1-5 after residency, $1,000



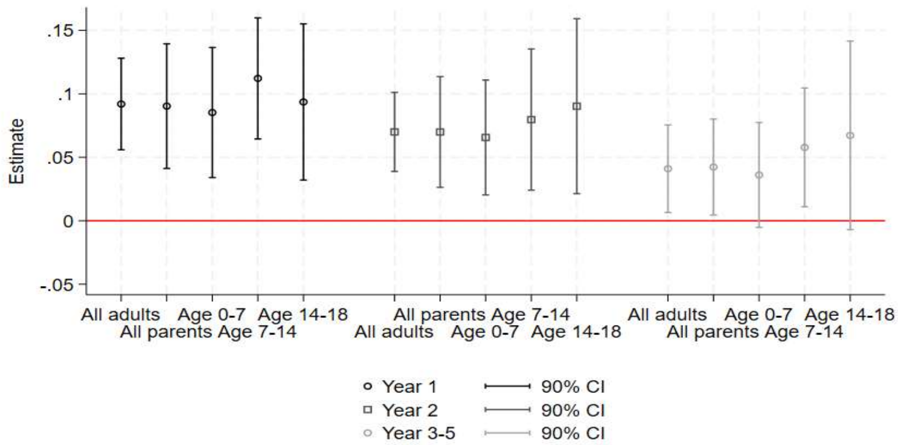
C) Fraction with average monthly disposable income < $750, year 1 and average of year 1-5 after residency



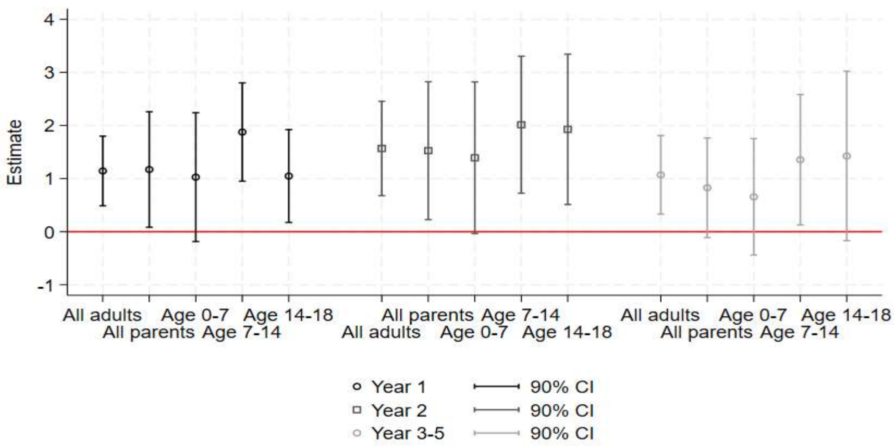
Note: The figure plots the transfer levels refugees are eligible for and their realized disposable (post tax and transfers) income by timing of residency relative to the reform in bins of three months. Fig. A) shows transfers (SoA or Start Aid) that refugees aged 18-45 with children are eligible for on average. Fig. B) shows means of actual disposable (post tax and transfers) income for refugees aged 18-45 with children at residency in their first year after residency (circles) and the first five years after residency on average (squares). Fig. C) shows the fraction with an average monthly disposable income below $750 in the first year after residency (circles) and the first five years after residency on average (squares).

Figure 2. Reform effect on adults employment, labor earnigns, and transfer income, by time after residency and age of children.

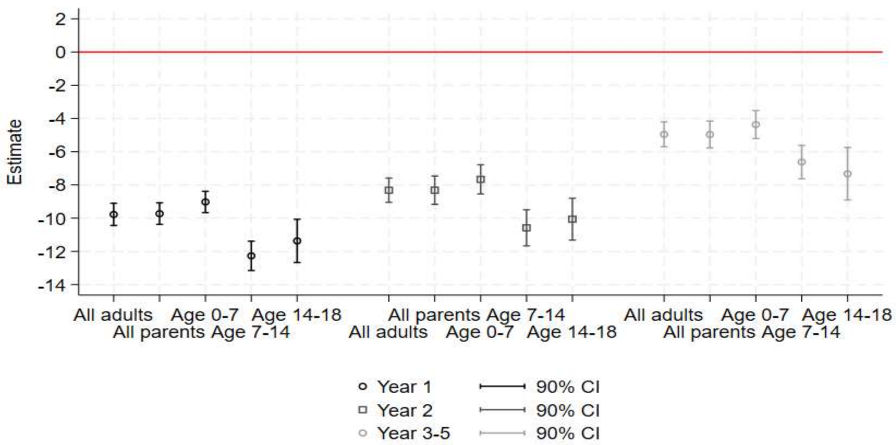
A) Employment



B) Labor earnings, $1,000

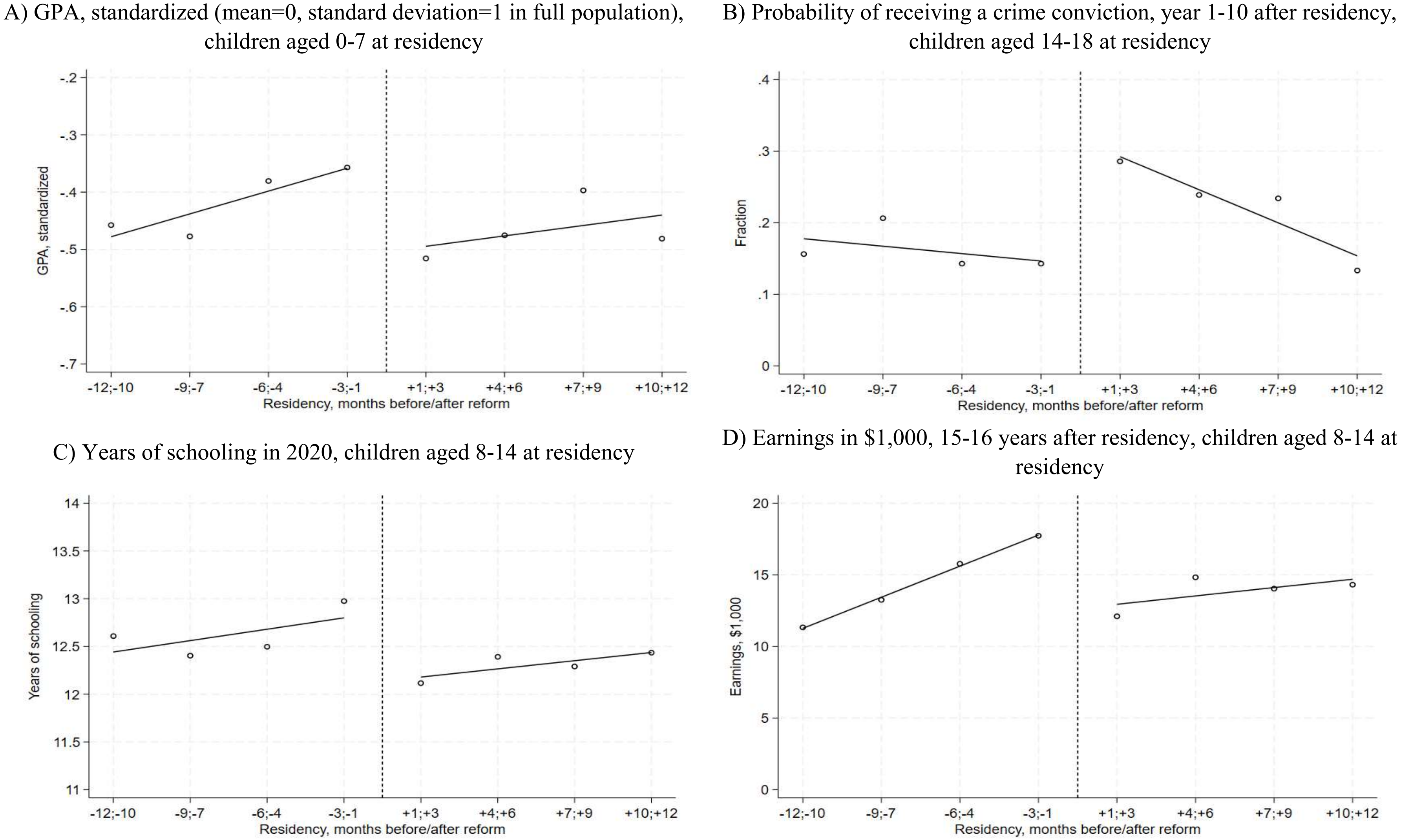


C) Transfer income, $1,000



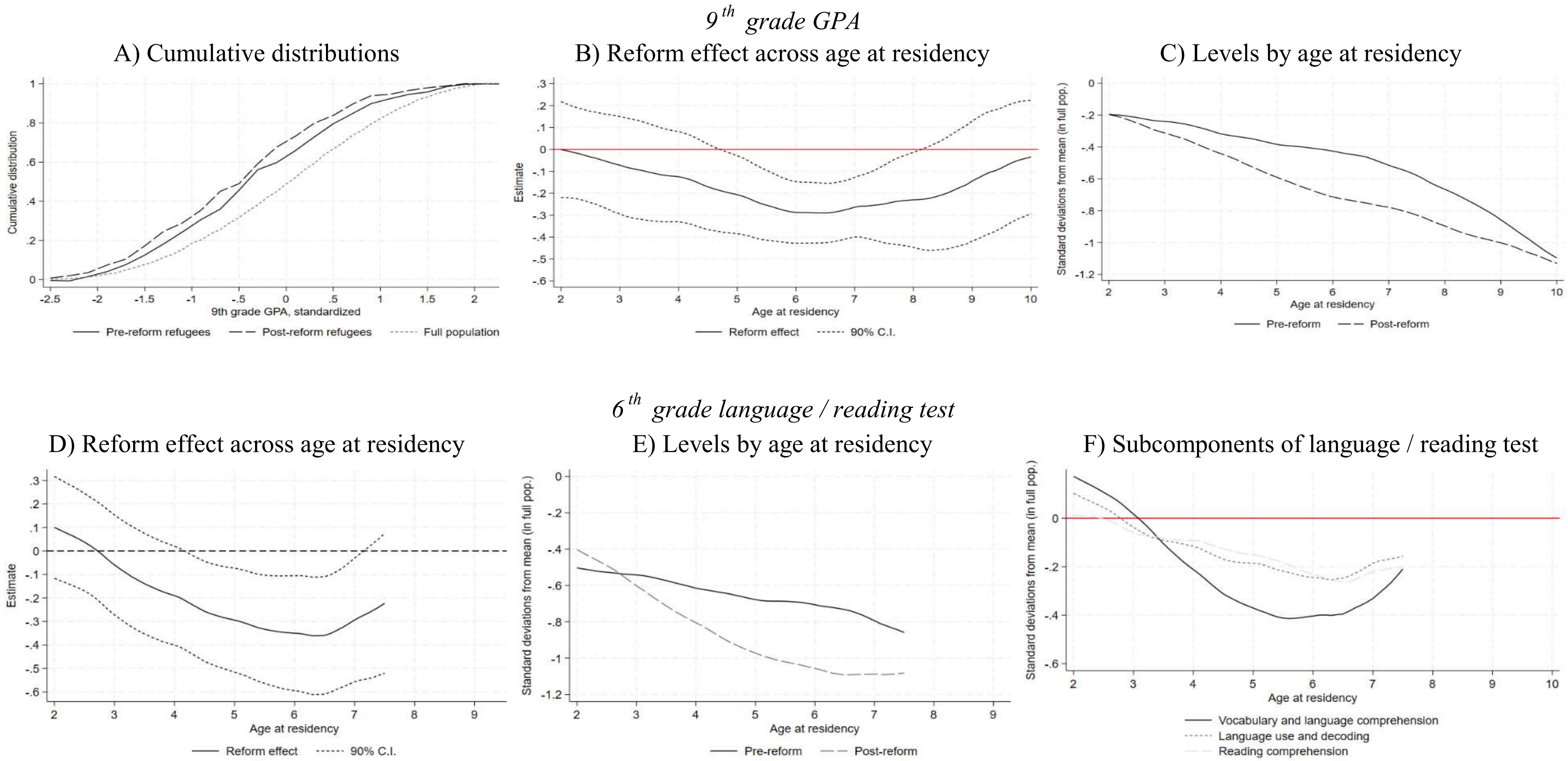
Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform along with 90% confidence intervals on adults employment (Fig. A), labor earnings (Fig. B), and transfer income (Fig. C) for all adults (estimates from Table 3 in Dustmann, Landersø, and Andersen, forthcoming), all parents, parents with children aged 0-7 at residency, parents with children aged 7-14 at residency, and parents with children aged 14-18 at residency. Standard errors are clustered by residency month

Figure 3. Refugees' GPA, crime, education, and earnings by timing of residency before/after reform.



Note: The figure shows raw plots, by timing of residency in bins, of GPA (standardized) for refugees age 0-7 at residency in Fig A), the fraction of refugees aged 14-18 at residency with a crime conviction during their first 10 years after residency in Fig. B), average years of schooling measured in 2020 for refugees aged 8-14 at residency in Fig. C), and average earnings measured 15-16 years after residency for refugees aged 8-14 at residency in Fig. D). All figures contain linear slopes of the predictions before and after the reform based on Eq. (1).

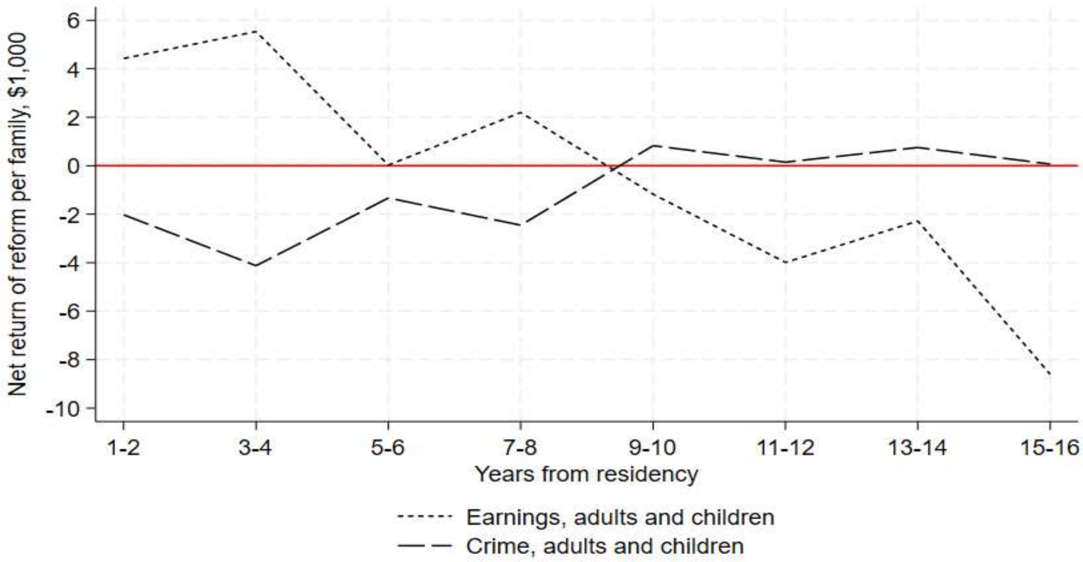
Figure 4. Heterogeneity in reform effect on 9th grade GPA and 6th grade language / reading test scores.



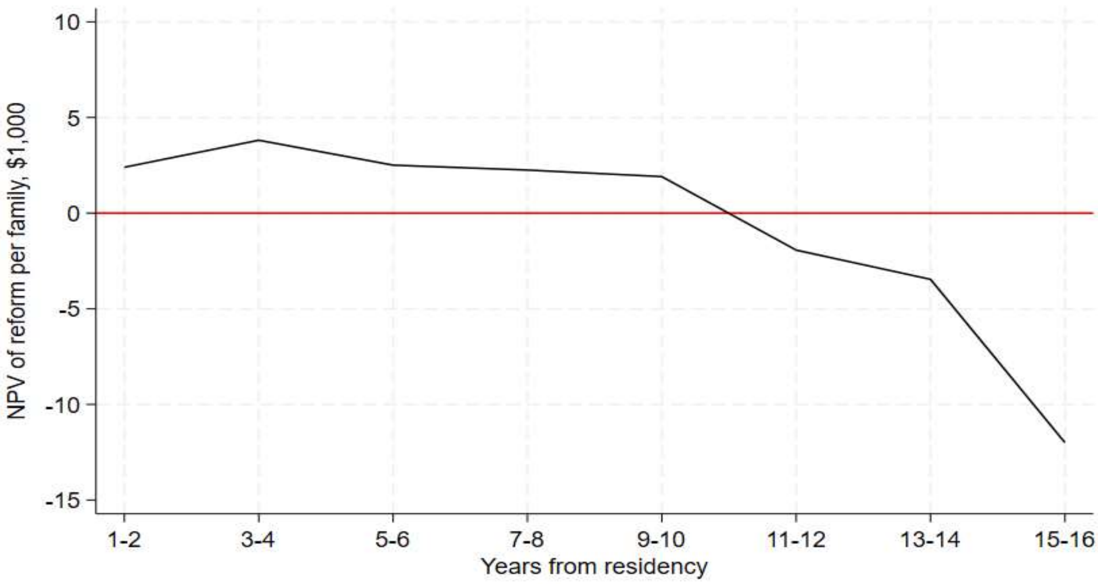
Note: Figs. A-C) show the estimated effects of being granted residency after the reform relative to before the reform on 9th grade GPA. Fig. A) shows cumulative distributions pre- and post-reform for refugees aged 0-7 at residency. Fig. B) shows the effect of the reform across age at residency estimated using Eq. (2). Fig. C) shows pre- and post-reform levels estimated as the regression constant and the regression constant plus the reform effect, respectively. Figs. D-F) shows the estimated effects of the reform across age at residency on 6th grade language / reading test scores. Fig. D) shows the effect of the reform across age at residency estimated using Eq. (2). Fig. E) shows pre- and post-reform levels estimated as the regression constant and the regression constant plus the reform effect, respectively. Fig. F) shows shows the effect of the reform across age at residency estimated using Eq. (2) for the three subcomponents of the 6th grade language test scores.

Figure 5. Cost-benefit analysis of the reform, by years since residency.

A) Net-returns of reform, adults' and children's earnings and crime, $1,000



B) Cumulative returns of reform ($1,000) based on reform effects on earnings and crime



Note: The figure shows a cost-benefit analysis of the reform based on the reform effects on adults' and children's earnings and crime in two year bins from year 1-2 to 15-16 after residency. The cost-benefit analysis takes the viewpoint of an utalitarian social planner, and we do not place weight on distributional issues between e.g., low vs. high income families or natives vs. immigrants. Fig. A) shows the sum of reform effects on adults' and children's annual earnings and the costs associated with the reform effects on adults' and children's crime. Crime costs are based on the reported "willingness to pay to reduce crime" from Cohen and Piquero (2009) weighted with the prevalence of each crime type in our sample. Reform effects are estimated based on Eq. (3) using as outcomes i) earnings of adults aged 18-45 at residency with children, ii) earnings of children aged 7-14 at residency, iii) crime convictions of adults aged 18-45 at residency with children, and iv) crime convictions of children aged 14-18 at residency. Figure B) shows the net-present-value (NPV) of the reform for the average family in the sample (two adults, one child aged 7-14, and one child aged 14-18). Cumulative returns are defined as the sum of the net-returns for earnings and crime from year 1-2 until the year in question (such that e.g., the estimate in year 5-6 equals the discounted sum of the net-return for earnings and crime in year 1-2, 3-4, and 5-6). We use a discount rate of 2% per year.

Table 1. Sample summary and balancing tests.

	(1)	(2)	(3)	(4)	(5)	(6)
	Parents			Children		
	Sample mean	Conditional test	Unconditional test	Sample mean	Conditional test	Unconditional test
A) Sample characteristics						
Treatment (residency post reform)	0.366	-	-	0.439	-	-
	(0.482)			(0.496)		
Age at residency	31.819	-0.000	-0.640*	8.271	-0.000	0.077
	(6.539)	(0.000)	(0.357)	(4.439)	(0.001)	(0.402)
Gender (female=1)	0.596	0.017	0.062	0.456	-0.011	-0.050
	(0.491)	(0.012)	(0.037)	(0.498)	(0.008)	(0.031)
Region of origin: Asia	0.719	0.024	0.087	0.741	0.041	0.131
	(0.450)	(0.032)	(0.087)	(0.438)	(0.036)	(0.087)
Region of origin: Africa	0.218	-	-0.064	0.208	-	-0.087
	(0.413)		(0.083)	(0.406)		(0.082)
Region of origin: Eastern Europe	0.053	-0.012	-0.020	0.050	-0.033	-0.041**
	(0.224)	(0.032)	(0.020)	(0.217)	(0.029)	(0.019)
Region of origin: South America	0.010	-0.006	-0.003	0.002	-0.088	-0.003
	(0.099)	(0.054)	(0.008)	(0.041)	(0.111)	(0.004)
Refugee permit status	0.551	-0.002	-0.051	0.266	0.010	-0.013
	(0.497)	(0.018)	(0.057)	(0.442)	(0.023)	(0.062)
Single	0.143	0.000	0.002	-	-	-
	(0.350)	(0.016)	(0.027)			
Number of children	2.584	-0.007	-0.303*	-	-	-
	(1.086)	(0.005)	(0.176)			
B) Balancing test, covariates						
P-value, F-test		0.420			0.471	
C) Balancing test, sample density						
P-value McCrary test	0.184			0.404		
Observations	3,406	3,406	3,406	3,482	3,482	3,482

Note: The table shows average sample characteristics for parents in column 1 and children (at the time of residency) in column 4 receiving residency +/- 18 months around the reform. Columns 2 and 5 present conditional balancing tests (with 'Region of origin: Africa' as reference category) of covariates across the reform for the sample of parents and children, respectively, where we regress a dummy indicating whether residency was granted pre- or post-reform on all covariates and the running variable (allowing for different slopes in the running variable on each side of the cutoff). The table also presents P-values from F-test of joint significance of the covariates in the conditional balancing tests (see Panel B). Columns 3 and 6 present unconditional balancing tests of covariates from regressing each observable characteristic on a dummy indicating whether residency is granted pre- or post-reform conditional on the running variable (allowing for different slopes in the running variable on each side of the cutoff). 'Region of origin: South America' also includes few (<5) stateless individuals. 'Refugee permit status' is an indicator of grounds for asylum (refugee permit status=1; being family reunified spouse / child of an individual with refugee permit status=0). 'Single' refers to marital status upon residency. 'Number of children' refers to the number of children upon residency. Standard deviations are shown in parentheses below the means (columns 1 and 3) and standard errors below the estimates (columns 2, 4, and 5). Standard errors are clustered by month of residency. Panel C shows P-values from McCrary tests (McCrary, 2008) for discontinuity in the sample density for both parents and children.

* p<0.1; ** p<0.05; *** p<0.01

Table 2. Reform effect on refugees' 9th grade GPA, education, and earnings (1,000 USD) by age at time of exposure to the reform.

	A) Refugees aged 0-7 at exposure				*B) Refugees aged 7-14 at exposure*				
	(1) GPA, standardized, 9th grade	*(2) P(9th grade GPA in 1st quartile)*	*(3) Well-being/ self-esteem in school*	*(4) P(daycare/ preschool)*	*(5) Years of schooling*	*(6) P(Not beyond lower secondary school)*	*(7) P(Vocational degree)*	*(8) Employment 15-16 years post residency*	*(9) Earnings 15-16 years post residency*
Reform effect	-0.177*	0.088*	-0.226**	-0.174**	-0.570**	0.101**	-0.089**	-0.070*	-3.428**
	(0.091)	(0.043)	(0.107)	(0.082)	(0.277)	(0.047)	(0.043)	(0.041)	(1.574)
Pre-reform mean	-0.443	0.410	0.258	0.733	12.166	0.373	0.363	0.786	13.574
Full population mean	0.000	0.250	0.000	0.867	13.457	0.163	0.451	0.853	20.094
Elasticity: % Effect / 1% increase in benefits	-	-0.763*	-	1.347***	0.096**	-0.590**	-0.062	0.173*	0.449**
		(0.427)		(0.490)	(0.046)	(0.280)	(0.458)	(0.103)	(0.225)
Observations	1,535	1,535	485	933	1,343	1,343	1,343	1,343	1,343

Note: The table shows reform effects for refugees in preschool age at exposure (age 0-7) in columns 1-4 and refugees in school age at exposure (ages 8-14) in columns 5-9. The table shows reform effects on 9th grade GPA (standardized to mean=0 and sd=1 in the full population, col. 1), the probability that refugees' GPA are in the lowest test score quartile (in the full population, col. 2), average of item responses relating to well-being / self-esteem in school (How often can you solve problems if you only try hard enough; how often can you succeed in what you set out to do; how often does your stomach hurt, col. 3), the probability that refugees attend daycare or preschool in the first year after residency (only those aged 0-5 at residency, col. 4), years of completed schooling (col. 5), education-levels for refugees aged 0-14 at the time of residency (cols. 6-7), employment 15-16 years after residency (defined as wage earnings>0) (col. 8), earnings in 1,000 2020 USD (col. 9) . Education levels: lower secondary schooling (maximum 10 years of schooling) in col. 6 and vocational degrees (vocational high school and short degrees) in col. 7. The table also shows percentage change of the outcomes relative to the percentage change in welfare transfers parents' are eligible for (elasticities). Education is measured in 2020. Standard errors are clustered by residency month in Panel A, and calculated based on 500 bootstraps in Panel B.
* p<0.1; ** p<0.05; *** p<0.01

Table 3. Reform effect on adolescents' crime in year 1, year 1-5, and year 1-10 after residency.

		(1)	(2)	(3)	(4)	(5)	(6)
			P(crime)			*Number of crimes*	
		Year 1	Accumulated year 1-5	Accumulated year 1-10	Year 1	Accumulated year 1-5	Accumulated year 1-10
A) Reform effects on adolescents' crime							
All crime	Reform effect	0.014	0.121**	0.135***	0.014	0.370***	0.525*
		(0.021)	(0.046)	(0.041)	(0.021)	(0.126)	(0.260)
	Pre-reform mean	0.029	0.160	0.206	0.029	0.265	0.466
Property	Reform effect	-0.007	0.108*	0.030	-0.007	0.227*	0.229
		(0.018)	(0.059)	(0.059)	(0.018)	(0.113)	(0.225)
	Pre-reform mean	0.025	0.088	0.143	0.025	0.134	0.244
Violence	Reform effect	0.018	0.065**	0.125***	0.018	0.100***	0.186***
		(0.013)	(0.028)	(0.029)	(0.013)	(0.032)	(0.066)
	Pre-reform mean	0.004	0.076	0.097	0.004	0.088	0.151
B) Elasticies of crime wrt welfare benefits							
All crime	Estimate	-	-1.070***	-0.961**	-	-1.614***	-1.336**
			(0.440)	(0.413)		(0.430)	(0.551)
C) Adolescents' crime interacted with parents' crime.							
All crime	Reform effect	0.007	0.052	0.062*	0.007	0.131	0.124
		(0.007)	(0.035)	(0.037)	(0.007)	(0.093)	(0.126)
	Pre-reform mean	0.001	0.003	0.017	0.001	0.004	0.034
Property	Reform effect	0.007	0.052	0.043	0.007	0.089	0.052
		(0.007)	(0.035)	(0.038)	(0.007)	(0.075)	(0.107)
	Pre-reform mean	0.001	0.003	0.015	0.001	0.004	0.025
Violence	Reform effect	-	-	0.036***	-	-	0.052**
				(0.011)			(0.020)
	Pre-reform mean	0.000	0.000	0.003	0.000	0.000	0.005
Observations		467	467	467	467	467	467

Note: The table shows reform effects on crime (all, property, violent crime) in year 1, and accumulated from year 1-5 and year 1-10 after residency for refugees aged 14-18 at exposure in Panel A. Panel B shows elasticities of crime with respect to welfare benefits: % change in p(crime) or the number of crimes relative to % change in the benefits parents are eligible for. Panel C shows reform effect on the 14-18 year olds' crime interacted with a dummy indicating whether parents' have received a crime conviction during the same period. Standard errors are clustered by residency month in Panels A and C, and calculated based on 500 boostraps in Panel B.
* p<0.1; ** p<0.05; *** p<0.01

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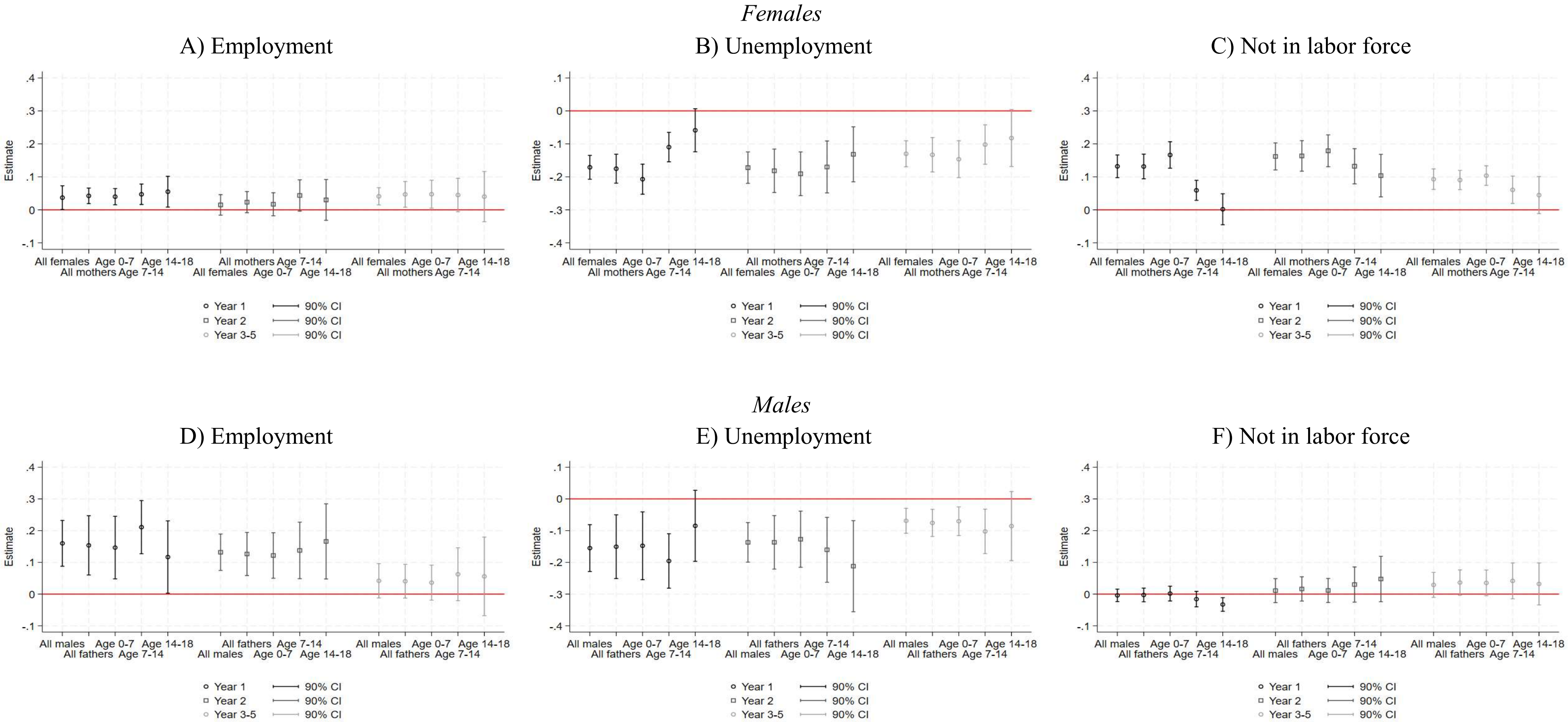
Appendix A

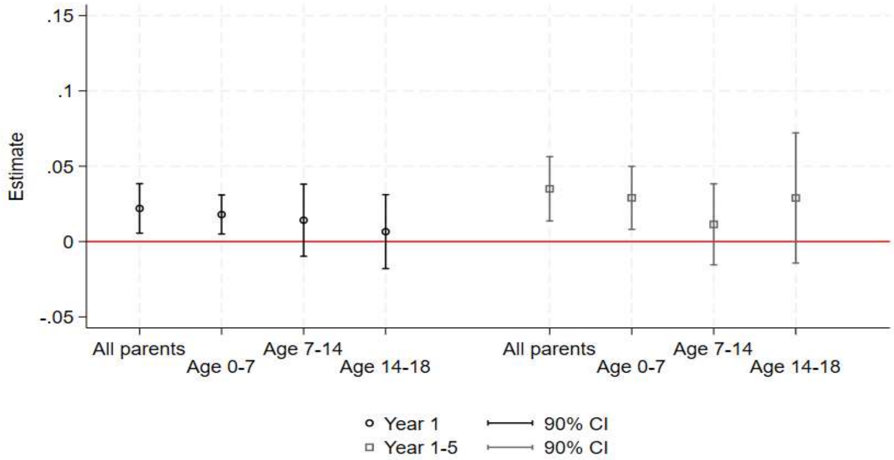
Figure A.1. Reform effect on adults labor market outcomes, by time after residency, age of children, and parents' gender.



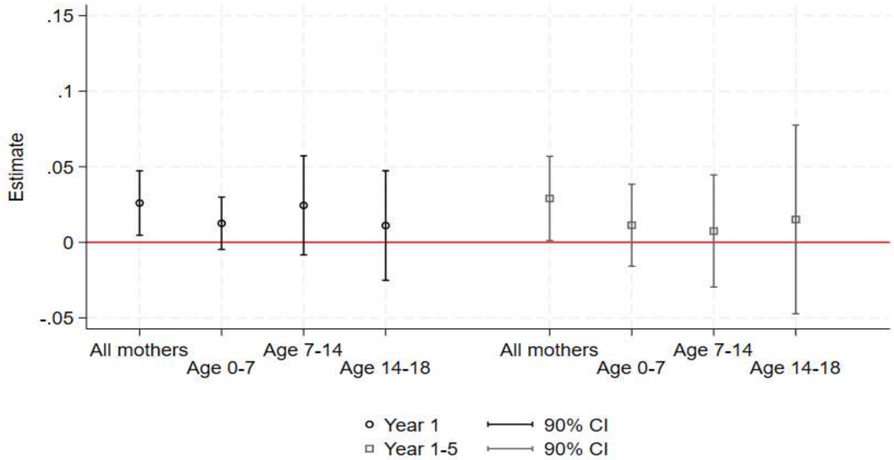
Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform along with 90% confidence intervals on adults employment (Figs. A, D), unemployment (Figs. B, E), and not in the labor force (Figs. C, F) for all adults (estimates from Table 4 in Dustmann, Landersø, and Andersen, forthcoming), all parents, parents with children aged 0-7 at residency, parents with children aged 7-14 at residency, and parents with children aged 14-18 at residency. Standard errors are clustered by residency month

Figure A.2. Effects of reform on crime for adults, year 1 and 5 after residency, by parents' gender and age of children.

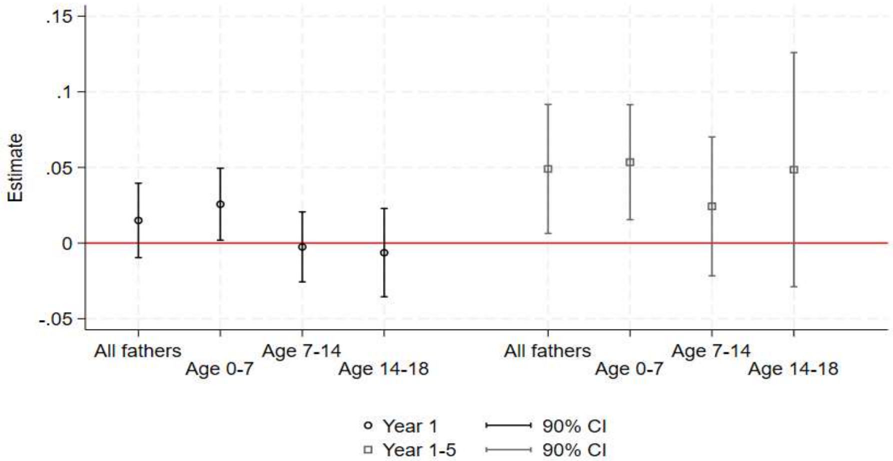
A) Probability of crime, all parents



B) Probability of crime, mothers



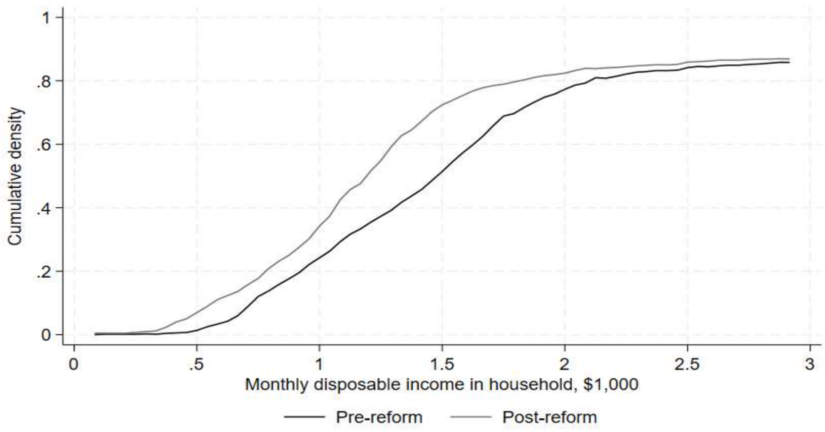
C) Probability of crime, all fathers



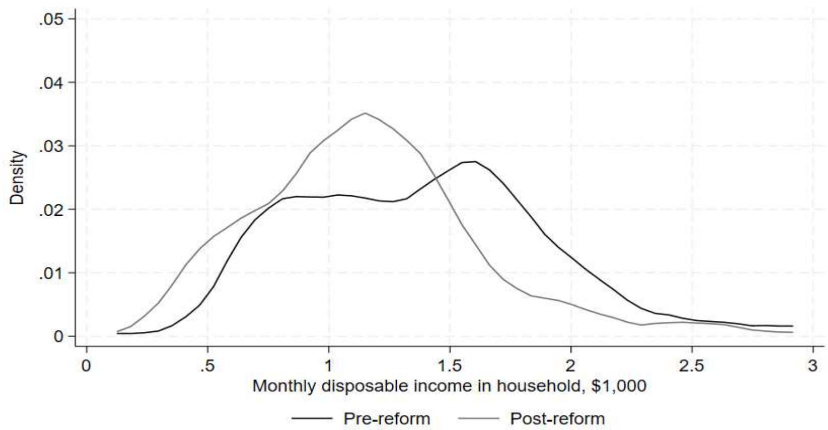
Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform along with 90% confidence intervals on the probability of having received a crime conviction in year 1 and accumulated from year 1-5 for all parents/mothers/fathers (corresponding to Table 8 in Dustmann et al., 2023) and separately for parents/mothers/fathers with children aged 0-7, parents/mothers/fathers with children aged 7-14, and parents/mothers/fathers with children aged 14-18. Standard errors are clustered by residency month.

Figure A.3. Reform effect on household disposable income per month, year 1 after residency.

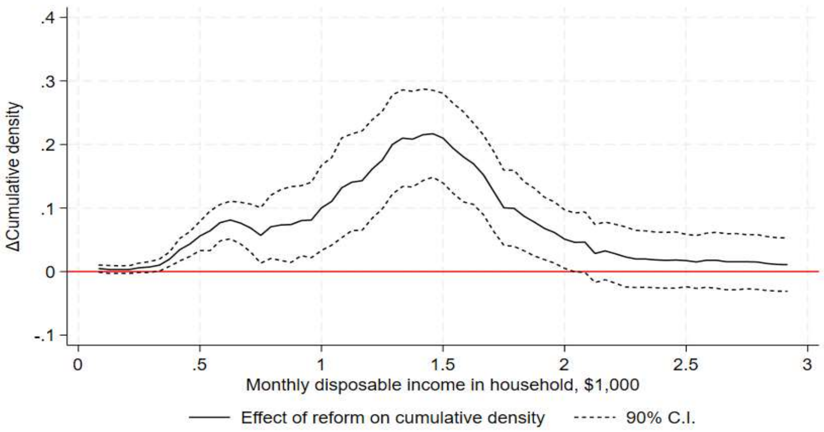
A) Cumulative density



B) Probability density



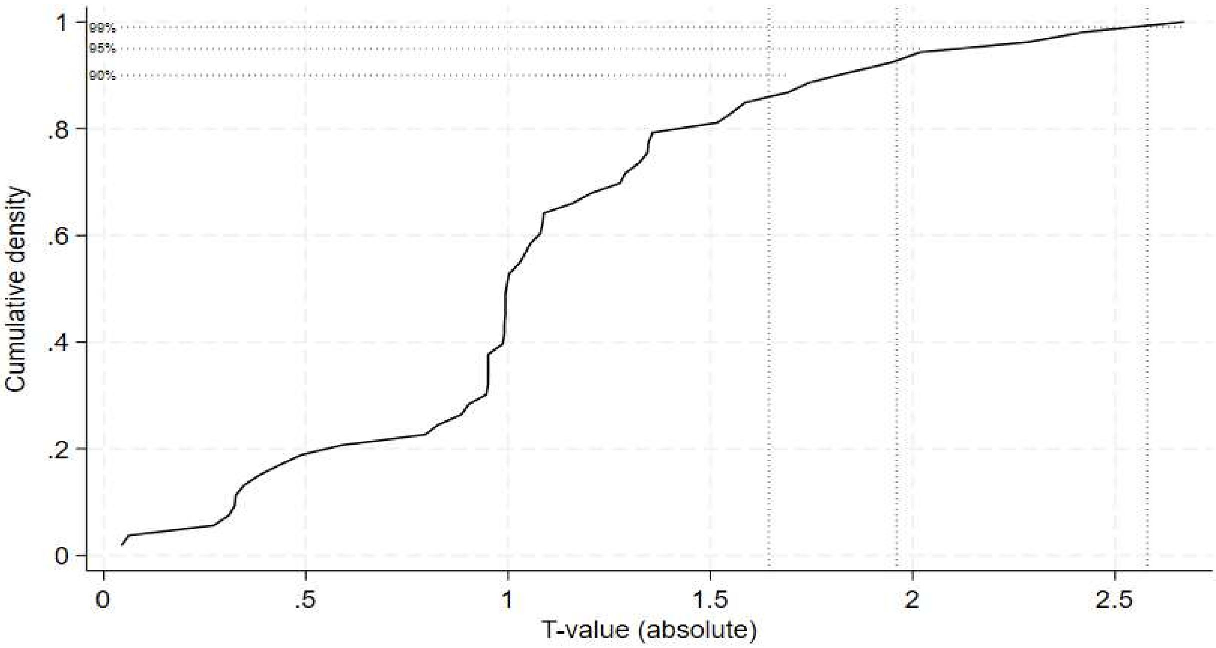
C) Pre-post reform difference cumulative density



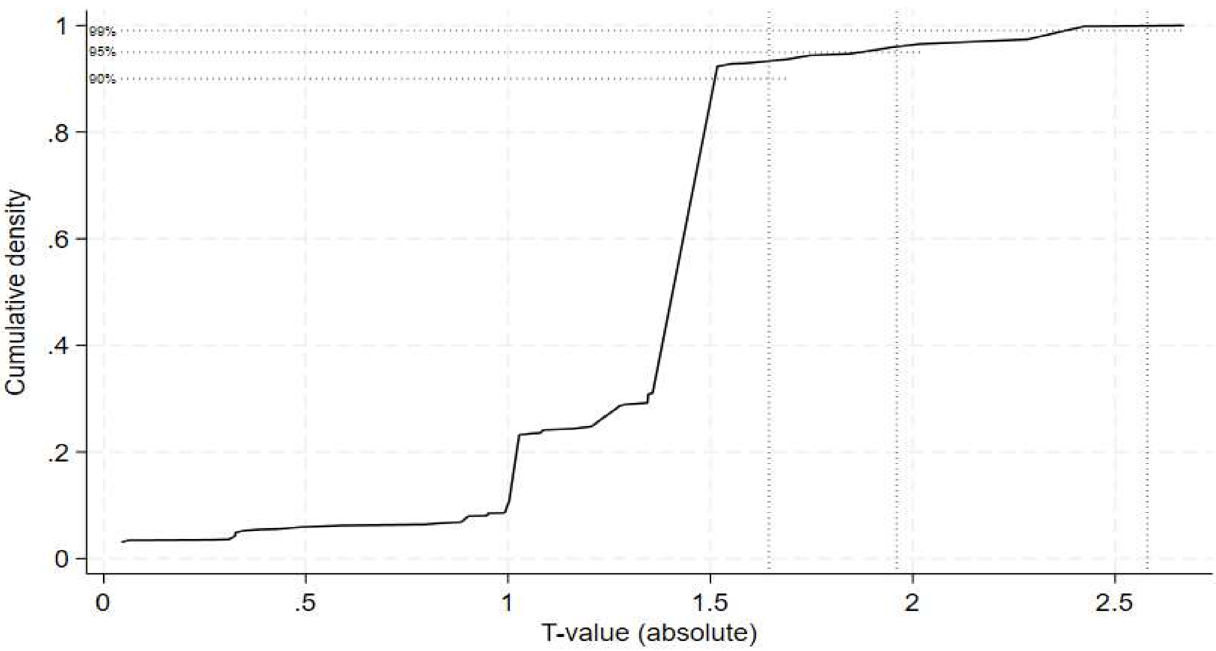
Note: The figure shows household level disposable income (per month) in the first year after residency for refugees granted residency just before the reform and just after the reform. To construct the figures, we create a series of dummies (1[y≤x]) for whether disposable income is x or below, varying x from zero to the top of the income distribution (from $0 to $3,000). Fig. A) is constructed by estimating Eq. (1) with these dummy variables as outcomes with the regression intercept being the pre-reform cumulative distribution and the regression intercept plus the reform effect being the post-reform cumulative distribution at each level of disposable income. Fig. B) shows the increments of the cumulative distribution at each level of disposable income. Fig C) shows the difference between the pre- and post-reform cumulative distributions at each level of disposable income along with 90% confidence intervals. Standard errors for confidence intervals are clustered by residency month.

Figure A.4. Balancing of country of origin.

A) Cumulative distribution with equal weight to each country

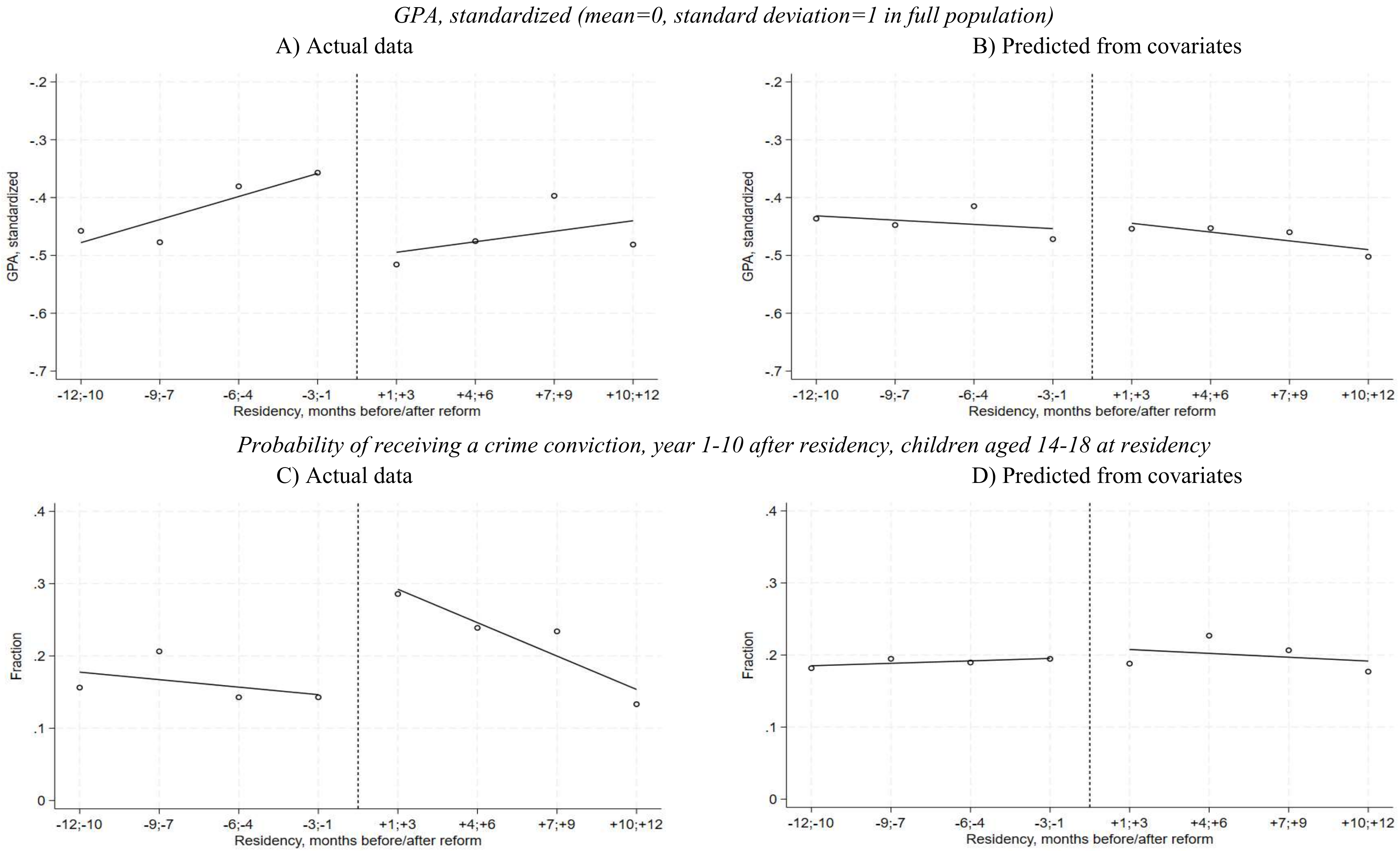


B) Cumulative distribution with weight to each country according to sample composition



Note: The figure summarizes balancing tests of origin for each country observed in the full sample of children aged 0-18 at residency. For a given country of origin, we construct a dummy indicating whether individuals come from this particular country and regress it on a dummy indicating whether residency is granted pre- or post-reform conditional on the running variable (allowing for different slopes in the running variable on each side of the cutoff). The figure plots the cumulated density of the T-values for all 53 countries of origin in the sample. Fig. A) shows the distribution where each country receives equal weight. Fig. B) shows the cumulate distribution where each each country receives weight according to the sample composition.

Figure A.5. Comparing refugees' GPA and crime with predictions based on covariates.

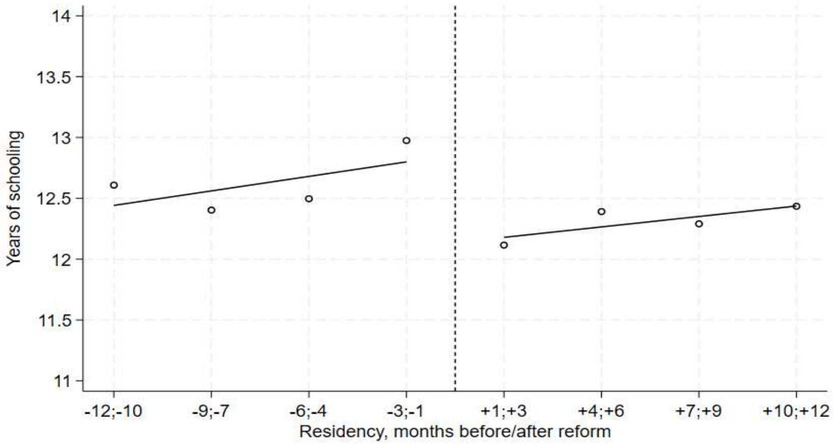


Note: The figure shows raw plots of actual data (as presented in Fig. 2) and contrast these to predicted outcomes from covariates (see Table 1) plotted by timing of residency relative to the reform. Figs. A) and B) show GPA (standardized) for refugees age 0-7 at residecy; Figs. C) and D) show the fraction of refugees aged 14-18 at residency with a crime conviction during their first 10 years after residency. All figures contain linear slopes of the predictions before and after the reform based on Eq. (1).

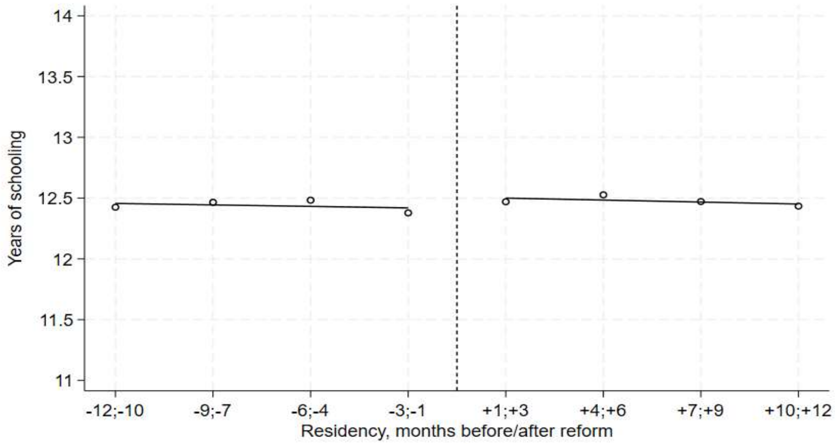
Figure A.6. Comparing refugees' years of schooling and earnings with predictions based on covariates.

Years of schooling in 2020, children aged 8-14 at residency

A) Actual data

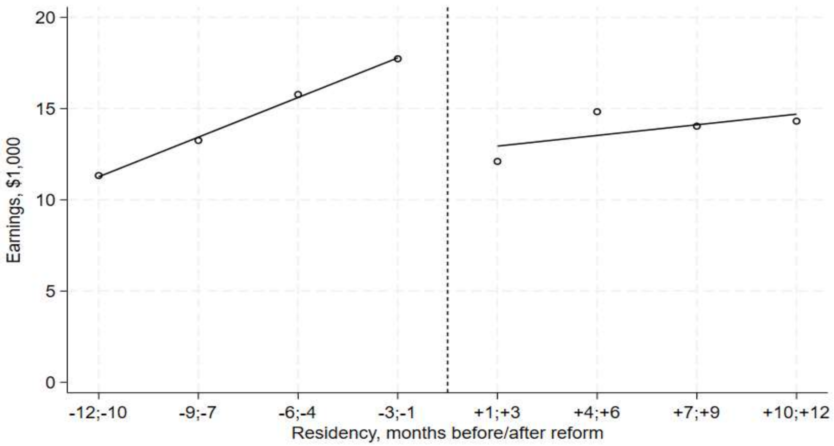


B) Predicted from covariates

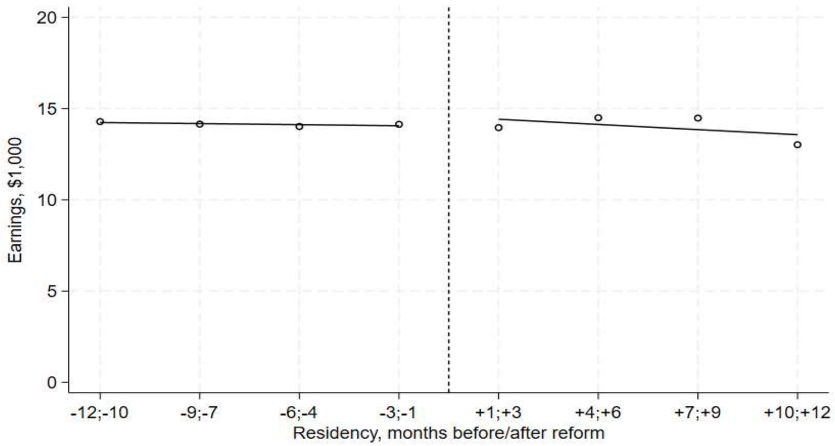


Earnings in $1,000, 15-16 years after residency, children aged 8-14 at residency

C) Actual data



D) Predicted from covariates



Note: The figure shows raw plots of actual data (as presented in Fig. 2) and contrast these to predicted outcomes from covariates (see Table 1) plotted by timing of residency relative to the reform. Figs A) and B) show average years of schooling measured in 2020 for refugees aged 8-14 at residency; Figs. C) and D) show average earnings measured 15-16 years after residency for refugees aged 8-14 at residency. All figures contain linear slopes of the predictions before and after the reform based on Eq. (1).

Table A.1. Balancing test, fraction that remigrate for children, all adults, and parents.

	(1)	(2)	(3)
	Children	All adults	Parents
Reform	-0.009	-0.030	-0.008
	(0.015)	(0.032)	(0.019)

Note: The table shows the estimated change in the fraction that remigrate during the first 10 years after residency for those granted residency after the reform relative to before the reform separately for children, all adults, and parents in columns 1, 2, and 3, respectively. The results are estimated by regressing an dummy of remigration on a dummy of whether residency was granted pre- or post-reform and the running variable (allowing for different slopes in the running variable on each side of the cutoff). The results in column 2 are reprints from Table A.3 in Dustmann, Landersø, and Andersen (2023). Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.2. Reform effects on all children in the sample.

	(1)	(2)	(3)	(4)	(5)
	9^{th} grade GPA	Years of schooling	Employment 15-16 years after residency	Earnings 15-16 years after residency	P(crime), year 1-10 after residency
A) All children					
Reform effect	-0.105	-0.127	-0.034	-0.887	0.008
	(0.072)	(0.198)	(0.030)	(1.150)	(0.019)
Pre-reform mean	-0.753	11.696	0.779	9.635	0.139
Observations	2,615	3,470	3,470	3,470	3,482
B) Children aged 0-7					
Reform effect	-0.177*	0.050	-0.000	0.320	-0.021
	(0.091)	(0.161)	(0.034)	(0.708)	(0.013)
Pre-reform mean	-0.443	11.361	0.777	4.698	0.034
Observations	1,535	1,660	1,660	1,660	1,660
C) Children aged 7-14					
Reform effect	-0.006	-0.570**	-0.070*	-3.428**	0.000
	(0.134)	(0.277)	(0.041)	(1.574)	(0.036)
Pre-reform mean	-1.193	12.166	0.786	13.574	0.245
Observations	1,010	1,343	1,343	1,343	1,355
D) Children aged 14-18					
Reform effect	-	0.237	-0.063	0.525	0.135***
		(0.451)	(0.076)	(4.069)	(0.041)
Pre-reform mean	-	11.466	0.770	18.232	0.206

Note: The table shows reform effects for the full sample of refugee children in Panel A, children aged 0-7 at residency in Panel B, children aged 8-14 at residency in Panel C, and children aged 14-18 in Panel D, on 9th grade GPA in column 1, years of completed schooling in column 2, employment 15-16 years after residency in column 3, earnings 15-16 years after residency, and the probability of receiving a crime conviction during year 1-10 after residency. The number of observations vary as some outcomes are not measured for specific groups; for example, 9th grade GPA requires that the refugee child in was not too old to enter the Danish regular school system following residency. Standard errors are clustered by residency month. The results reported in the main text are: Column 1, Panel B (Table 2), Columns 2-4, Panel C (Table 2), and Column 5, Panel D (Table 3).

* p<0.1; ** p<0.05; *** p<0.01

Table A.4. Reform effect on 9th grade GPA for refugees aged 0-14 at residency.

	(1) Age 0-7 at residency	(2) Age 7-14 at residency
GPA, standardized, 9th grade		
Reform effect	-0.160*	-0.026
	(0.094)	(0.123)
Pre-reform mean	-0.443	-1.193
GPA, rank, 9th grade		
Reform effect	-0.042	0.001
	(0.027)	(0.032)
Pre-reform mean	0.373	0.190
P(GPA in 1st quartile)		
Reform effect	0.088*	-0.042
	(0.043)	(0.048)
Pre-reform mean	0.410	0.737
P(GPA in 2nd quartile)		
Reform effect	0.006	0.017
	(0.032)	(0.035)
Pre-reform mean	0.260	0.146
P(GPA in 3rd quartile)		
Reform effect	-0.053	0.018
	(0.034)	(0.040)
Pre-reform mean	0.213	0.084
GPA in 4th quartile		
Reform effect	-0.041	0.007
	(0.031)	(0.025)
Pre-reform mean	0.116	0.033
Observations	1,535	1,010

Note: The table shows reform effects on and pre-reform means of 9th grade GPA (standardized to mean=0 and sd=1) and rank of 9th grade GPA (where rank is measured in the full population GPA distribution) for refugees age 0-14 at the time of residency. GPA is measured for refugees attending 9th grade exams from 2002-2019. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.3. Balancing tests for child sample for each age-group of children.

	(1)	(2)	(3)	(4)	(5)	(6)
	Conditional balancing test			Unconditional balancing test		
Child group age range	0-7	7-14	14-18	0-7	7-14	14-18
Age at residency	0.001	-0.001	-0.003	0.129	-0.023	-0.057
	(0.003)	(0.003)	(0.008)	(0.171)	(0.153)	(0.270)
Gender (female=1)	-0.011	-0.004	-0.028	-0.053	-0.022	-0.098
	(0.011)	(0.016)	(0.022)	(0.046)	(0.053)	(0.076)
Region of origin: Asia	0.035	0.049	0.051	0.141	0.119	0.123
	(0.035)	(0.041)	(0.049)	(0.090)	(0.091)	(0.117)
Region of origin: Africa	-	-	-	-0.077	-0.099	-0.108
				(0.086)	(0.085)	(0.101)
Region of origin: Eastern Europe	-0.049	-0.002	-0.004	-0.059**	-0.018	-0.015
	(0.035)	(0.038)	(0.081)	(0.027)	(0.022)	(0.044)
Region of origin: South America	-0.123	-0.030	-	-0.005	-0.001	-
	(0.097)	(0.121)		(0.006)	(0.003)	
Refugee permit status	0.002	0.019	0.017	-0.042	0.021	0.022
	(0.021)	(0.032)	(0.026)	(0.065)	(0.078)	(0.055)
P-value, F-test	0.179	0.816	0.691	-	-	-
Observations	1,660	1,355	467	1,660	1,355	467

Note: The table shows results from balancing tests for children sepeareted into the age-groups we consider in the main results. Columns 1-3 present conditional balancing of covariates (with 'Region of origin: Africa' as reference category) across the reform from regressing a dummy indicating whether residency was granted pre- or post-reform on all covariates and the running variable (allowing for different slopes in the running variable on each side of the cutoff). The table also presents P-values from F-test of joint significance of the covariates in the conditional balance test for both parents and children. Columns 4-6 present unconditional balancing of covariates from regressing each observable characteristic on a dummy indicating whether residency is granted pre- or post-reform conditional on the running variable (allowing for different slopes in the running variable on each side of the cutoff). 'Region of origin: South America' also includes few (<5) stateless individuals. Results are not presented for this variable for children aged 14-18 because there no observations with this origin in this group. 'Refugee permit status' is an indicator of grounds for asylum (refugee permit status=1; being family reunified spouse / child of an individual with refugee permit status=0). Standard errors are clustered by month of residency.

* p<0.1; ** p<0.05; *** p<0.01

Table A.5. Reform effect on refugees' outcomes by different specifications.

	(1)	(2)	(3)	(4)	(5)	(6)	Observations
A) Years of schooling, children aged 7-14							
Reform effect	-0.570**	-0.555*	-0.547*	-0.481	-0.547*	-0.595**	1343
	(0.277)	(0.280)	(0.277)	(0.288)	(0.295)	(0.301)	
B) Employment 15-16 years after residency, children aged 7-14							
Reform effect	-0.070*	-0.073*	-0.077*	-0.073*	-0.065	-0.075*	1343
	(0.041)	(0.041)	(0.042)	(0.040)	(0.042)	(0.043)	
C) Earnings 15-16 years after residency, children aged 7-14							
Reform effect	-3.428**	-3.657**	-3.777**	-3.209*	-3.474**	-3.814**	1343
	(1.574)	(1.558)	(1.554)	(1.771)	(1.610)	(1.657)	
D) 9th grade GPA, children aged 0-7							
Reform effect	-0.177*	-0.188**	-0.160*	-0.163	-0.165*	-0.189*	1535
	(0.090)	(0.091)	(0.094)	(0.100)	(0.095)	(0.098)	
E) P(crime), year 1-10 after residency, children aged 14-18							
Reform effect	0.135***	0.120***	0.090**	0.106**	0.125**	0.148***	467
	(0.041)	(0.039)	(0.038)	(0.042)	(0.053)	(0.044)	
Year of residency fixed effects		X	X	X	X		
Observable characteristics			X				
Donut around reform				X			
Reduced bandwidth					X		
Local Polynomial RDD						X	

Note: The table shows reform effects on years of schooling, employment 15-16 years after residency (defined as wage earnings>0), wage earnings 15-16 years after residency (in 1,000 2020 USD), 9th grade GPA, and the probability of having received a crime conviction (as presented in Tables 2 and 3) for different estimation specifications. Column 1 shows results as presented in Tables 2 and 3. Column 2 shows estimated reform effects conditional on year-of-residency fixed effects. Column 3 shows estimated reform effects conditional on year-of-residency fixed effects and covariates (see Table 1). Column 4 shows estimated reform effects in a donut specification where we exclude observations receiving residency in the last month before and the first month after reform. Column 5 shows estimated reform effects where the bandwidth has been reduced to one year pre- and post-reform. Standard errors are clustered by residency month. Columns 1-5 are estimated based on Eq. (1) with running variables entering linearly with uniform weights. Column 6 show results based on local polynomial regression discontinuity design (Calonico et al., 2018) using a triangular kernel and a bandwidth of 18 months on each side of the reform. The column 'Observations' refer to the number of observations in the bandwidth of 18 months around the reform.

* $p<0.1$; ** $p<0.05$; *** $p<0.01$

Table A.6. Reform effect on completed education for refugees aged 0-14 at residency.

	(1) Age 0-7 at residency	(2) Age 7-14 at residency
Years of schooling		
Reform effect	0.050	-0.547**
	(0.161)	(0.277)
Pre-reform mean	11.361	12.166
Full population mean	11.047	13.457
P(lower secondary schooling)		
Reform effect	-0.014	0.101**
	(0.050)	(0.047)
Pre-reform mean	0.369	0.373
Full population mean	0.450	0.163
P(High school)		
Reform effect	0.006	0.008
	(0.054)	(0.043)
Pre-reform mean	0.495	0.138
Full population mean	0.450	0.158
P(Vocational degree)		
Reform effect	0.009	-0.089*
	(0.020)	(0.047)
Pre-reform mean	0.091	0.363
Full population mean	0.082	0.451
P(College degree or higher)		
Reform effect	-0.001	-0.022
	(0.018)	(0.034)
Pre-reform mean	0.044	0.125
Full population mean	0.018	0.227
Observations	1,660	1,343

Note: The table shows reform effects on and pre-reform means of years of completed schooling and specific education-levels for refugees aged 0-14 at the time of residency. Education levels are collapsed to i) lower secondary schooling (maximum 10 years of schooling), ii) high school (academic track), iii) vocational degrees (vocational high school and short degrees in vocational colleges), iv) all college and university degrees. Education is measured in 2020. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.7. Reform effect on earnings and work/study at age 17-18 for refugees in school age at reform exposure.

	A) Earnings				*B) Work and/or studying*			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	P(Earnings: $0-1,499)	P(Earnings: $1,500-2,999)	P(Earnings: $3,000-4,499)	P(Earnings: $4,500-)	P(No work, not studying)	P(Work, not studying)	P(Work, studying)	P(No work, studying)
Reform effect	-0.108*	0.067**	0.015	0.016	0.003	0.068	0.014	-0.084**
	(0.056)	(0.031)	(0.029)	(0.044)	(0.036)	(0.060)	(0.055)	(0.037)
Pre-reform mean	0.527	0.141	0.090	0.248	0.163	0.374	0.344	0.119
Observations	1,343	1,343	1,343	1,343	1,343	1,343	1,343	1,343

Note: The table shows reform effects for refugees in school age at exposure (aged 7-14). Panel A) shows reform effects on the probability of having average annual earnings at age 17-18 in the range $0-1,499 (col. 1), $1,500-2,999 (col. 2), $3,000-4,499 (col. 3), $4,500 or higher (col. 4). Panel B) shows reform effects on the probability of not working and not studying (col. 5), working and not studying (col. 6), working and studying (col. 7), and working and not studying (col. 8) at age 17-18. Working in coloumns 5-8 is defined as having earnings>0. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.8. Reform effect on adolescents' crime in year 1, year 1-5, and year 1-10 after residency, by gender.

		P(crime)			*Number of crimes*		
		Year 1	Year 1-5	Year 1-10	Year 1	Year 1-5	Year 1-10
A) All crime							
All	Reform effect	0.014	0.121**	0.135***	0.014	0.370***	0.525*
		(0.021)	(0.046)	(0.041)	(0.021)	(0.126)	(0.260)
	Pre-reform mean	0.029	0.160	0.206	0.029	0.265	0.466
Males	Reform effect	0.023	0.123**	0.190***	0.023	0.398***	0.716*
		(0.025)	(0.052)	(0.065)	(0.025)	(0.145)	(0.421)
	Pre-reform mean	0.034	0.223	0.291	0.034	0.392	0.703
Females	Reform effect	-0.004	0.074	-0.023	-0.004	0.238*	0.004
		(0.021)	(0.049)	(0.067)	(0.021)	(0.139)	(0.082)
	Pre-reform mean	0.022	0.056	0.067	0.022	0.056	0.078
B) Property crime							
All	Reform effect	-0.007	0.108*	0.030	-0.007	0.227*	0.229
		(0.018)	(0.059)	(0.059)	(0.018)	(0.113)	(0.225)
	Pre-reform mean	0.025	0.088	0.143	0.025	0.134	0.244
Males	Reform effect	-0.001	0.131*	0.039	-0.001	0.276**	0.311
		(0.021)	(0.065)	(0.083)	(0.021)	(0.125)	(0.358)
	Pre-reform mean	0.027	0.108	0.190	0.027	0.182	0.345
Females	Reform effect	-0.019	0.057	-0.023	-0.019	0.111	0.004
		(0.019)	(0.058)	(0.067)	(0.019)	(0.124)	(0.082)
	Pre-reform mean	0.022	0.056	0.067	0.022	0.056	0.078
C) Violent crime							
All	Reform effect	0.018	0.065**	0.125***	0.018	0.100***	0.186***
		(0.013)	(0.028)	(0.029)	(0.013)	(0.032)	(0.066)
	Pre-reform mean	0.004	0.076	0.097	0.004	0.088	0.151
Males	Reform effect	0.027	0.081*	0.171***	0.027	0.133***	0.250**
		(0.021)	(0.041)	(0.044)	(0.021)	(0.046)	(0.104)
	Pre-reform mean	0.007	0.122	0.155	0.007	0.142	0.243
Females	Reform effect	-	-	-	-	-	-
	Pre-reform mean	0.000	0.000	0.000	0.000	0.000	0.000
Observations all:		467	467	467	467	467	467
Observations males:		285	285	285	285	285	285
Observations females:		182	182	182	182	182	182

Note: The table shows reform effects on and pre-reform means of crime convictions (all crime in Panel A, property crimes in Panel B, and violence in Panel C) in year 1, year 1-5, and year 1-10 after residency for refugees between age 14 and 18 at residency. The table reports estimates for all adolescents as in Table 3 and separately by gender. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.9. Reform effect on parents' and children's earnings and crime, by time since residency.

	(1)	(2)	(3)	(4)
	Parents		*Children*	
	Earnings, $1,000	Number of crimes	Earnings, $1,000	Number of crimes
Year 1-2	1.106*	0.031**	0.000	0.092*
	(0.562)	(0.012)	(0.000)	(0.047)
Year 3-4	1.329*	0.024*	0.110	0.231***
	(0.765)	(0.014)	(0.110)	(0.069)
Year 5-6	0.040	0.002	-0.066	0.091
	(0.730)	(0.011)	(0.415)	(0.089)
Year 7-8	0.781	0.021**	-0.462	0.129
	(1.001)	(0.010)	(0.596)	(0.084)
Year 9-10	0.250	-0.021**	-1.086	-0.018
	(0.986)	(0.010)	(0.748)	(0.051)
Year 11-12	-0.309	0.016	-1.377	-0.040
	(1.161)	(0.010)	(0.894)	(0.053)
Year 13-14	0.159	0.000	-1.459	-0.059*
	(1.221)	(0.011)	(1.515)	(0.032)
Year 15-16	-0.436	0.003	-3.428*	-0.012
	(1.219)	(0.005)	(1.574)	(0.033)
Observations	3,406	3,406	1,343	1,343

Note: The table shows the estimated effects of the reform on parents' earnings and number of crime convictions in columns 1 and 2, respectively, and on children's earnings and number of crime convictions in columns 3 (children aged 7-14 at residency) and 4 (children aged 14-18 at residency), respectively. The outcomes are measured in two-year bins and not accumulated from residency (as in e.g., Table 3). Standard errors are clustered by residency month.
* p<0.1; ** p<0.05; *** p<0.01

B. Appendix for Online Publication

B.1 The Start Aid Reform, Background, and Details

B.1.1 The Asylum Process

Most individuals who request asylum in Denmark under the 1951 Geneva Convention for Refugees do so after entering the country as undocumented migrants. After making the request, applicants are transferred to the Sandholmlejren reception center, registered as asylum seekers, issued an ID card confirming their status, put through a full medical check, and interviewed about current and past health issues. While the Danish Immigration Service processes their applications, it covers their living expenses and provides health care. Noncompliance with obligations during the asylum process, such as failure to attend interviews or providing inaccurate information, results in application rejection.

The first step in the formal asylum process is determining whether Denmark is responsible for the application according to the Dublin Convention.[1] If it is, the applicant is transferred from Sandholmlejren to one of the accommodation centers (refugee camps) located around the country, which the Danish Red Cross administers. Here, applicants receive a cash allowance and engage in introductory language courses and training programs. They remain in the refugee camp while the authorities decide the asylum case based on information provided by the applicants about why they are seeking asylum and on information about conditions in the asylum seeker's country of origin.

The process from the asylum application to the final decision consists of two main steps (see Hvidtfeldt et al., 2018, for a further description). First, the Danish Immigration Service assesses the conditions in the country of origin to determine whether refugee status is warranted. This may take several months and sometimes involve "fact-finding missions" to specific countries and regions. Once

[1] The Dublin convention (in effect from 1997) ensures that asylum seekers do not file applications in several EU member states simultaneously and prevents them from orbiting between member states in search of asylum.

this first step has been completed, a caseworker from the Danish Immigration Service interviews the applicant in the second step. The timing of this interview depends on the current caseload and availability of interpreters. The caseworker may also decide that additional interviews are required to assess the applicant's case. If the application is rejected, it is automatically referred to the Danish Refugee Appeals Board for review and a final decision.

Married applicants are each assigned a separate asylum case ID and processed individually, even if they apply together on the same day. In some instances, if the authorities grant residency to one spouse, they may give residency to the other simultaneously if residency would have been granted at a later time. The vast majority of married couples, however, are processed and assigned residence separately. During our study period, the entire application process for those granted residency was about 15 months on average. However, as described above, there was considerable variation in processing times according to individual circumstances and immigration agency workload. Those seeking asylum in Denmark at the time of the Start Aid reform came from various countries, but mainly from Middle Eastern and North African nations.

B.1.2 The Start Aid Bill

Start Aid was implemented in response to what many considered an overly generous welfare scheme with too few employment incentives. Its main objective was to promote refugee integration into the labor market and the broader society by increasing work incentives (Danish Prime Minister's Office, 2002). One challenge legislators faced when formulating the reform bill, however, was how to reduce benefits only for a particular subpopulation (refugees) without violating the UN's Universal Declaration of Human Rights and the 1951 Refugee Convention.[2] Start Aid was therefore defined so

[2] Article 23 of the Convention makes the following stipulation: "The Contracting States shall accord to refugees lawfully staying in their territory the same treatment with respect to public relief and assistance as is accorded to their nationals."

as not to discriminate by origin or residency status formally. Individuals remained eligible for the preexisting SoA benefit program if they were already living in Denmark on the reform enactment date or had lived in EU/EFTA countries for 7 of the past eight years. This criterion eliminated all native Danes from the new Start Aid program, leaving (newly arrived) immigrants as the only affected group. It did not affect labor migrants or families reunified with nonrefugee citizens of Denmark because these were ineligible for either SoA or Start Aid.[3]

Table B.1 shows the pre- and post-reform transfer levels across household types and the percentage reduction in transfer levels.

Table B.1. Transfer rates (SoA and Start Aid) by residency before / after the reform.

Status	Age	Children	Before Reform (SoA) in $	After reform (Start Aid) in $	Pct. Transfer Reduction
Couple	>= 25	0	1,131	604	47
Couple	>= 25	1	1,503	755	50
Couple	>= 25	>= 2	1,503	906	40
Couple	< 25	1	1,503	755	50
Couple	< 25	>= 2	1,503	906	40
Single or couple	< 25	0	729	604	17
Single	>= 25	0	1,131	729	36
Single	>= 25	1	1,503	911	39
Single	>= 25	>= 2	1,503	1,093	27
Single	< 25	1	1,503	786	48
Single	< 25	>= 2	1,503	969	36
Live with parents	< 25	0	352	300	15

Note: The table shows transfer levels (for refugees eligible for full SoA or Start Aid). All amounts are reported in 2020 PPP-adjusted USD with transfer levels as defined in 2002. The table shows how transfer levels for individuals in different household types are affected by the reform. Young refugees without children are affected the least as they were already entitled to comparatively low levels of SoA before the reform. All other groups are entitled to at least 25% lower transfers after the reform. Couples are affected the most with 40-50% lower transfer levels.

[3] Labor migrants can only stay in Denmark for up to 3 months after their employment ends or until their work permit expires, and the costs for family reunification of nonrefugees are borne by the spouse residing in Denmark.

B.2 Data Construction and Definitions

Our analysis is based on a compilation of register data sets. Our starting point is the Danish Immigration Service's records on all residence permits from January 1, 1997. We select residencies granted from 1997-2006, resulting in 252,795 residency permits. From these records, we extract all permits given to refugees (a total of 67,375), consisting of 44,232 new migrant refugees and 23,143 to refugees granted residency through family reunification. We merge these data with the Historical Migrations database, which contains exact information on when refugees were granted residency, their country of origin, and whether and when they left Denmark again. From this database, we obtain our base sample of refugees who were granted residency within 18 months of the reform. Based on this sample, we keep families with children at the time of residence.

During the months preceding the reform, two temporary changes to case processing procedures took place because of contemporaneous conflicts. First, following the fall of the Taliban regime, the Danish Immigration Service suspended processing of new applications by Afghans in late January 2002 (Refugee Appeals Board, 2002, p. 142) until the situation in Afghanistan had been investigated further. This led to a large drop in residency permits issued to asylum seekers from Afghanistan around the reform. Second, following the NATO bombings in 1999 and the subsequent installment of NATO forces (KFOR), Kosovo was reclassified as a "safe zone" by Danish courts in the spring of 2002 (Refugee Appeals Board, 2002, p. 114). Both these administrative changes were unrelated to the Start Aid reform. Nonetheless, they resulted in a sudden change in the number of residencies granted to refugees from these countries that largely coincided with the introduction of the reform. We therefore exclude refugees from Afghanistan and the former Yugoslavia from our final sample.

We explain the key variables we match to that data set in the subsections below.

B.2.1 Income

The income register, compiled from tax authority records, contains annual information on income items such as labor earnings, self-employment income, transfer income, and tax payments (the data also includes information on capital income, profits from businesses, assets, and liabilities). Because Denmark has complete third-party information (i.e., all income is reported directly by its issuers), the income data encompass all legal income. For our analysis, we consider two main types of income measured from the first year after residency onward: Earnings (including wage earnings, self-employment income, and profits from businesses – all measured pre-tax) and post-tax disposable income (which equals pre-tax earnings plus public transfers minus tax payments). We define employment as having positive earnings. All income is reported in 2020 (PPP adjusted) USD.

B.3.2 Educational Attainment

We use the 2020 Register on Completed Education (the last year of available data) to measure completed schooling years. Based on official enrollment and graduation information from the Danish Ministry of Education and updated annually to reflect each citizen's educational status on October 1, the register includes information on all grade levels and the associated years of completed schooling. We also consider the completion of specific education. Education levels are collapsed into i) lower secondary schooling (maximum ten years of schooling), ii) high school (academic track), iii) vocational degrees (vocational high school and short degrees in vocational colleges), and iv) all college and university degrees.

B.3.3 Additional educational outcomes

- We measure preschool enrollment based on annual data from the daycare register.

- We measure 9th grade GPA as the average grades from all exams children attend in 9th grade. We standardize GPA to a mean of zero and a standard deviation of one in the total population. We also calculate the ranks of GPA based on the total population.

- We consider compulsory (Danish) language test scores for grade 6 (available from their 2009 implementation onward) to measure language attainment. The total test score is a composite of three underlying measures: language comprehension, decoding, and reading comprehension.

Language comprehension is the ability to construct the meaning of spoken language, which in turn rests on deeper constructs such as linguistic knowledge (how the language works) and context knowledge about the background of a given statement.

Decoding is the ability to relate text (letters and words) to the sounds and meaning of spoken language.

Reading comprehension is a broader skill based on the former two abilities.

The various constructs are tested across different complexity levels using such tools as multiple choice (dichotomous and polytomous), word insertion and splitting, and coloring. We use the standardization approach outlined in Beuchert and Nandrup (2018).

- We measure well-being/self-esteem in school using survey responses from Danish public schools. Questions are scored on a Likert scale with categories “Very often”, “Often”, “Sometimes”, “Rarely”, and “Never”. We measure this average of item responses from the questions:

How often can you solve problems if you only try hard enough?

How often can you succeed in what you set out to do?

How often does your stomach hurt?

We standardize this to mean zero and standard deviation 1 in the full population. It should be noted that the scores are not constructed for between-population comparisons (cf. the pre-reform mean for refugee children of 0.258 in column 3, Table 2). For example, based on the same survey data from

Danish public schools, Loft and Waldfogel (2021) find that immigrant children's satisfaction with school and social well-being surpasses their native counterparts by up to 0.5 standard deviations even though immigrant children perform worse on all objective measures such as test scores, educational attainment, health, and crime.

B.3.4 Crime

The crime data include exact information on offense dates, charges, incarcerations, and convictions. Each entry contains unique case-specific and individual-specific identifiers that allow us to match each crime to individuals in our sample. We thus measure individual criminal activity based on convictions for offenses against the criminal code, which the Central Police register categorizes under specific labels (e.g., "theft from supermarket").[4] A criminal conviction is a court ruling of the suspect's guilt that results in a sentence (either a fine, suspended sentence or imprisonment). We observe the exact crime dates to define a "crime in year 1," for example, as a crime committed within the first 365 days after residency is granted.

[4] Arrests, although a common measure of criminal activity in the U.S., are infrequent in Denmark, and the Danish equivalent to arrests as an outcome would be charges.

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