

Unintended Consequences of Welfare Cuts on Children and Adolescents[†]

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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. (JEL I26, I31, I38, J13, J15, J31, K42)

Transfer reductions are often intended to provide work incentives. However, they may have serious adverse consequences for target individuals and their families. A particular concern is the potentially harmful effects on children at sensitive ages for skill development, educational attainment, and risky behavior. Although the impact of social safety net reforms on children's long-run outcomes is studied in a number of papers (see, e.g., Almond, Currie, and Duque 2018; East et al. 2023; Hendren and Sprung-Keyser 2020; Hoynes, Schanzenbach, and Almond 2016, for recent reviews), 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.¹

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

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¹One 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, Lommerud, and Reinso 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, Miller, and Simon 2015; McInnis, Michelsmore, and Pilkauskas 2023), the timing of tax credits during infancy (Barr, Eggleston, and Smith 2022), or program rollout during the early twentieth century (Aizer et al. 2016).

refugees.² Our analysis of the effects of the reform on children's lives across almost two decades provides critical insights into 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 up to 50 percent for couples and families with children. The reform effectively randomized families 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 for 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 ninth-grade GPAs dropping by 0.18 of a standard deviation, thereby increasing the preexisting 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.⁵ 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 age at exposure to the reform decreased by almost 0.6 years, corresponding

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. 2024) and refugees (e.g., Fasani, Frattini, and Minale 2021).

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, Dustmann, and Preston 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 ninth grade GPA.

⁵ The lower preschool enrollment potentially contributes to the negative reform effects on test scores and GPA. However, lower preschool attendance cannot account for the full effects on test scores and GPA as we also identify test score and GPA reductions for children aged 6–7 at residency (i.e., children who were too old to attend preschool).

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 works 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 done to take up work instead.

In the last section of this paper, we combine the effects the reform had on parents and their children, as identified in this paper.⁶ The reform resulted in a short-run, positive return of around \$2,500 for the average family in our sample. 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 aggregate return becomes negative at approximately $-\$12,000$ for the average family in our sample. Reductions in transfer payments inevitably involve a trade-off between lowering public expenses and increasing employment incentives on the one hand, and potential adverse responses of the target group, 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 dimensions such 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, Schanzenbach, and Almond 2016; Løken, Mogstad, and Wiswall 2012; Løken, Lommerud, and Reinso 2018).⁷ We contribute to this literature by studying the overall effects on GPA, test scores, and crime, and by 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, Dustmann, and Preston 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

⁶In Dustmann, Landersø, and Andersen (2024a), we study the reform's impact on adults' outcomes. Dustmann, Landersø, and Andersen (2024) show that employment doubles from a pre-reform mean of 10 percent in the first year after implementation but still leaves around 80 percent 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.

⁷The welfare policies studied earlier range from Medicaid (e.g., Brown, Kowalski, and Lurie 2020; Goodman-Bacon 2021), in-kind transfers such as Food Stamps/SNAP (e.g., Bailey et al. 2020; Hoynes, Schanzenbach, and Almond 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.

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, studying Native Americans, and Deshpande 2016, and Deshpande and Mueller-Smith 2022, studying low-income youths) 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 support previous studies presenting long-run evaluations of nineteenth- and early twentieth-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).

Our results for adolescents' crimes contribute to work that associates crime with either the timing of welfare payment (e.g., Foley 2011; Carr and Packham 2017), welfare levels (Dustmann, Landersø, and Andersen 2024a), welfare eligibility (Deshpande and Mueller-Smith 2022; Khanna et al. 2023; Yang 2017), temporary cash assistance (Palmer, Phillips, and Sullivan 2019), or state variation in welfare reform implementation in the United States (Corman, Dave, and Reichman 2014).⁸ 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.

I. Background, Estimation, and Data

A. *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 1, 2002, approved on June 6 and dubbed "Start Aid." The bill reduced transfer levels for refugees who received residency in Denmark after the reform's enactment on July 1, 2002 (Danish Prime Minister's Office 2002).⁹ Since the date of residency determines whether families are exposed to the Start Aid reform, we use age at residency and reform exposure interchangeably.

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

⁸Several recent studies have also documented the effects of in-kind transfers such as food stamps (Tuttle 2019) and Medicaid coverage (Arenberg, Neller, and Stripling 2020; He and Barkowski 2020; Jácóme 2022) on crime.

⁹The 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.

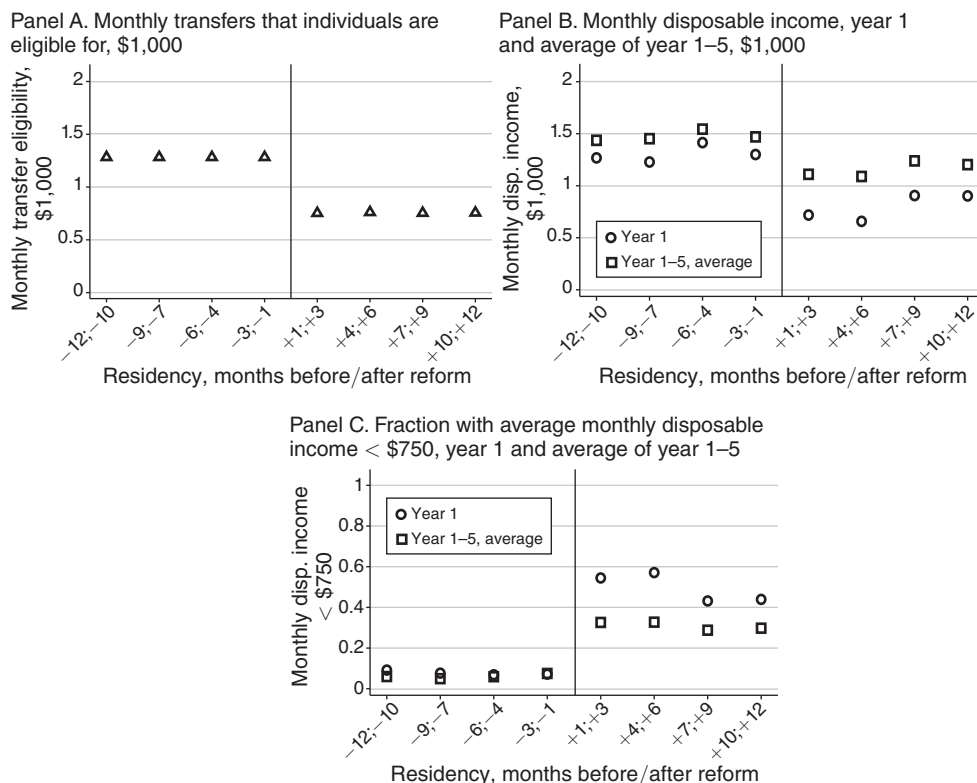


FIGURE 1. REFUGEES' TRANSFERS AND DISPOSABLE INCOME BY TIMING OF RESIDENCY BEFORE/AFTER REFORM

Notes: 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. Panel A shows transfers (SoA or Start Aid) that refugees aged 18–45 with children are eligible for on average. Panel 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). Panel 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).

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.

Figure 1, panel A 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.¹⁰

¹⁰ 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.

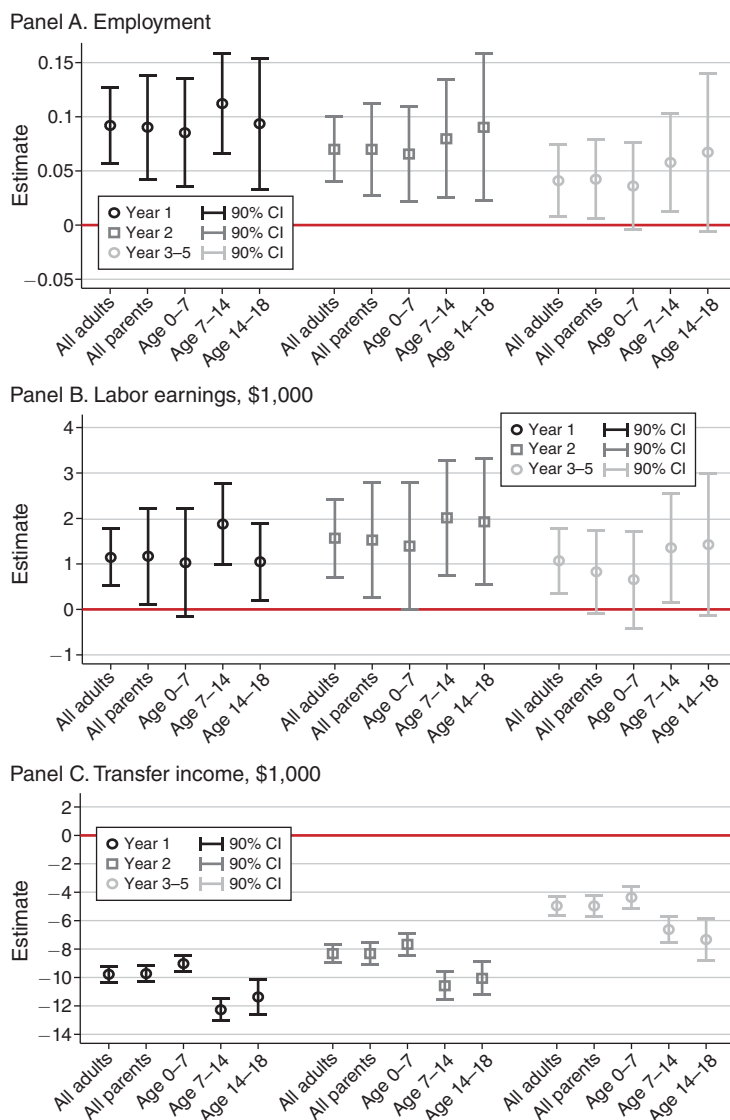


FIGURE 2. REFORM EFFECT ON ADULTS' EMPLOYMENT, LABOR EARNINGS, AND TRANSFER INCOME, BY TIME AFTER RESIDENCY AND AGE OF CHILDREN

Notes: The figure shows the estimated effects of being granted residency after the reform relative to before the reform along with 90 percent confidence intervals on adults' employment (panel A), labor earnings (panel B), and transfer income (panel C) for all adults (estimates from Table 3 in Dustmann, Landersø, and Andersen 2024a), 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 2 presents the reform's effects on parents' employment, earnings, and transfer income along with the corresponding results for all adults from Dustmann, Landersø, and Andersen (2024a). The reform led to substantial short-run effects on employment and labor earnings, both increasing by approximately 100 percent 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.¹¹

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 payments) dropped from around \$1,400 to \$800 (see Figure 1, panel B), with the fraction with an average monthly disposable income below \$750 (approximately equal to the US poverty threshold (US Census Bureau 2020)) increasing from a pre-reform level close to zero to a post-reform level of around 50 percent (Figure 1, panel C). As illustrated by online Appendix Figure A.2, this sharp reduction in disposable income resulted in higher crime rates among parents. The increasing crime rates were related to subsistence, such as shoplifting in supermarkets (see Dustmann, Landersø, and Andersen 2024a). Moreover, the disposable income reductions were persistent, with Figure 1, panel B illustrating a decrease in average disposable income for the first five years after residency of around 30 percent.¹²

B. 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:

$$(1) \quad y_i = \alpha + \beta \times \text{reform}_i + g(Z_i)' \pi + X_i' \gamma + \varepsilon_i,$$

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).¹³ Moreover, X_i collects observable characteristics,¹⁴ 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 the sample density and refugees' characteristics around the reform. In Section IC, we provide evidence supporting these identifying assumptions.

While equation (1) is our main specification, we also use three extensions. First, to facilitate a more direct comparison between the estimated effects of the reform

¹¹ Figs. 2 (employment, labor earnings, and transfer income), Online Appendix Figure A.1 (labor market outcomes by gender), and Online Appendix Figure 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).

¹² Online Appendix Figure A.3, illustrating how the reform affected the distribution of household disposable income, shows significant reductions throughout the entire distribution.

¹³ 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. 2019).

¹⁴ 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 online Appendix Table A.8). Online Appendix 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).

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

$$\frac{0.5 \times \beta}{\alpha + 0.5 \times \beta} \bigg/ \frac{StartAid - SoA}{SoA + StartAid}$$

Here α and β are the estimated parameters for each outcome in equation (1), and SoA and Start Aid indicate the transfer levels that pre- and post-reform refugees are eligible for, respectively.¹⁵

Second, we estimate how the reform has affected refugees' full GPA distribution by creating a series of dummies $\mathbf{1}\{y_i \leq x\}$ 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 equation (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 equation (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:

$$(2) \quad \min_{\alpha[A_0], \beta[A_0], \pi[A_0]} \sum_{i=1}^N K_{h_\lambda}(A_0, A_i) \times \left\{ y_i - \left(\alpha[A_0] + \beta[A_0] \times reform_i + g(Z_i)' \pi[A_0] \right) \right\}^2$$

such that the estimates of $\alpha[A_0]$ in equation (2) will capture the pre-reform level and estimates of $\beta[A_0]$ will capture the effect of the reform at local age ranges.¹⁶

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.

C. Data

Our data is drawn from administrative registers of all refugees who receive residency in Denmark.¹⁷ We describe all outcomes and data sources in detail in online Appendix B.2.

¹⁵We 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: $(0.5 \times \beta)/(\alpha + 0.5 \times \beta)$ for outcomes and $(StartAid - SoA)/(SoA + StartAid)$ for transfers. To see how these two specifications are equivalent, note that $StartAid - SoA = \mu$ constitutes the change in welfare benefits due the reform such that $\frac{StartAid - SoA}{SoA + StartAid} = \frac{\mu}{SoA + SoA + \mu} = \frac{0.5 \times \mu}{SoA + 0.5 \times \mu}$.

¹⁶ $K_{h_\lambda}(A_0, A_i)$ is an Epanechnikov kernel weighting observations around each age A_0 .

¹⁷Our 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.

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.¹⁸ 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 corresponds 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 secondary or vocational degrees) and ninth-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 sixth 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.¹⁹

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 is 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).

¹⁸This 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. Online Appendix Table A.1 present balancing tests of remigration for all adults, parents, and children. There are no changes in remigration around the reform.

¹⁹As the questionnaire was introduced in 2015, we only have information for eighth and ninth 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 equation (1) with a dummy indicating no response/no questionnaire information as outcome results in a coefficient of 0.018 (0.039).

D. 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 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, Heckman, and Landersø (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).²⁰ When investigating the effect on educational attainment and earnings, we focus on children exposed to the reform during school-age (age 7–14). When estimating the effects on crime, we focus on children exposed to the reform in adolescence (age 14–18).²¹ We report results for all age groups in the online Appendix Table A2.

II. 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.²² Columns 2 and 5 show

²⁰ Jakobsen, Kaarsen, and Vasiljeva (2017) have examined the impact of the 2002 Start Aid reform on children's ninth 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 dropout, and lower long run employment and earnings.

²¹ 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 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.

²² 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

TABLE 1—SAMPLE SUMMARY AND BALANCING TESTS

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

Notes: 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 cut-off). 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.

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

that may have followed the reform, as suggested in Agersnap, Jensen, and Kleven (2020). Dustmann, Landersø, and Andersen (2024a) 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.

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 (online Appendix 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 percent 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.

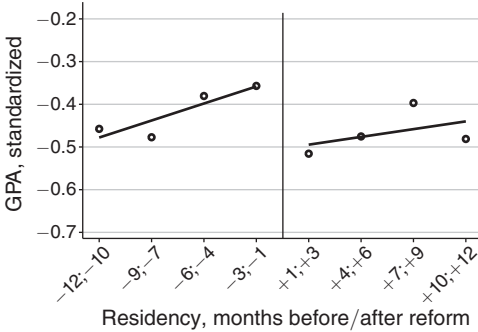
While the results in Tables 1 and Online Appendix Table A.2 consider broad regions of origin, online Appendix Figure A.4 tests the composition of all 53 individual origin countries in our sample across the reform—i.e., whether there is a change in the fraction from Iraq, Somalia, and so forth. We only 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 percent of tests at a 5 percent significance level).

Figure 3 plots outcomes (GPA, years of schooling, earnings, and crime) across the timing of the reform, displaying strong discontinuities. Online Appendix Figures 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.

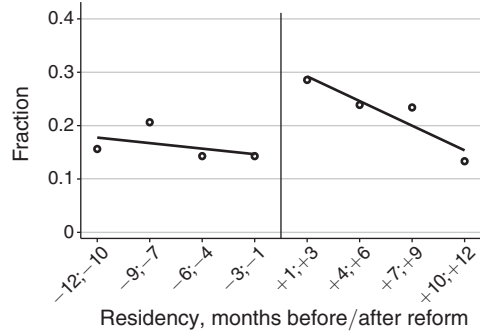
A. Reform Effects on Skill Formation

We first investigate the effects for children aged 0–7 at exposure to the reform (Table 2). Column 1 shows a substantial reduction in ninth 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 online Appendix 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 of Table 2 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 (Danish Ministry of Children and Social Affairs 2018). Online Appendix 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.

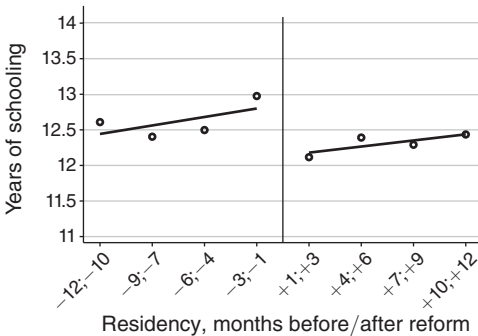
Panel A. GPA, standardized (mean = 0, std. dev. = 1 in full population), children aged 0–7 at residency



Panel B. Probability of receiving a crime conviction, year 1–10 after residency, children aged 14–18 at residency



Panel C. Years of schooling in 2020, children aged 7–14 at residency



Panel D. Earnings in \$1,000, 15–16 years after residency, children aged 7–14 at residency

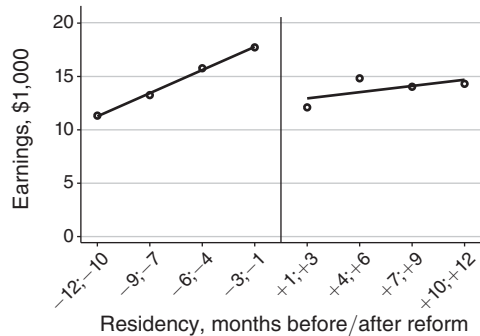


FIGURE 3. REFUGEES' GPA, CRIME, EDUCATION, AND EARNINGS BY TIMING OF RESIDENCY BEFORE/AFTER REFORM

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

Figure 4 expands the previous results on GPA in two dimensions. First, Figure 4, panel A presents the pre- and post-reform cumulative ninth-grade GPA distributions for children aged 0–7 at residency (see Section IB 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 leftward compared to the full population (natives and immigrants) distribution. For example, the GPA at the twentieth percentile for pre-reform refugees corresponds to the twelfth percentile in the total population. In contrast, the GPA at the eightieth percentile for pre-reform refugees corresponds to the sixty-fifth percentile in the total population. However, as the figure shows, the cumulative distribution for post-reform refugees is shifted leftward even further. The twentieth and eightieth percentiles of the post-reform distribution correspond roughly to the eighth and fifty-seventh percentiles in the total population.

TABLE 2—REFORM EFFECT ON REFUGEES' NINTH GRADE GPA, WELL-BEING, AND DAYCARE ENROLLMENT, REFUGEES AGED 0–7 AT TIME OF EXPOSURE TO THE REFORM

	GPA standardized 9th grade (1)	P(9th grade GPA in first quartile) (2)	Well-being/ self-esteem in school (3)	P(daycare/ preschool) (4)
Reform effect	−0.177 (0.091)	0.088 (0.043)	−0.226 (0.107)	−0.174 (0.082)
Pre-reform mean	−0.443	0.410	0.258	0.733
Full population mean	0.000	0.250	0.000	0.867
Elasticity: % Effect / 1% increase in benefits	—	−0.763 (0.427)	—	1.347 (0.490)
Observations	1,535	1,535	485	933

Notes: The table shows reform effects for refugees in preschool age at exposure (age 0–7). The table shows reform effects on ninth grade GPA (standardized to mean = 0 and SD = 1 in the full population, column 1), the probability that refugees' GPA are in the lowest test score quartile (in the full population, column 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, column 3), the probability that refugees attend daycare or preschool in the first year after residency (only those aged 0–5 at residency, column 4). The table also shows percentage change of the outcomes relative to the percentage change in welfare transfers parents are eligible for (elasticities). Standard errors are clustered by residency month for reform effects and calculated based on 500 bootstraps for elasticities.

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. Figures 4, panels B and C show the effects of the reform on children's ninth grade GPA across ages at reform exposure based on equation (2). Figure 4, panel B illustrates that the significant reduction in GPA is driven by children exposed to reform close to school start.

Figure 4, panel C visualizes the impact across ages at reform exposure more clearly. The figure compares the pre- and post-reform ninth grade GPA averages ($\alpha[A_0]$ for pre-reform and $\alpha[A_0] + \beta[A_0]$ for post-reform levels, see equation (2)) across age at residency, where the vertical axis carries age at residency (A_0 in equation (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.²³

²³ The negative relationship between age at residency and test scores may also explain the null finding for children in school age at residency, which online Appendix Table A.2 and Jakobsen, Kaarsen, and Vasiljeva (2017) show. Average test scores for children aged 7–14 at residency were 1.2 standard deviations below the national mean (see online Appendix Table A.2, column 1, panel C), leaving very little room for any further reduction.

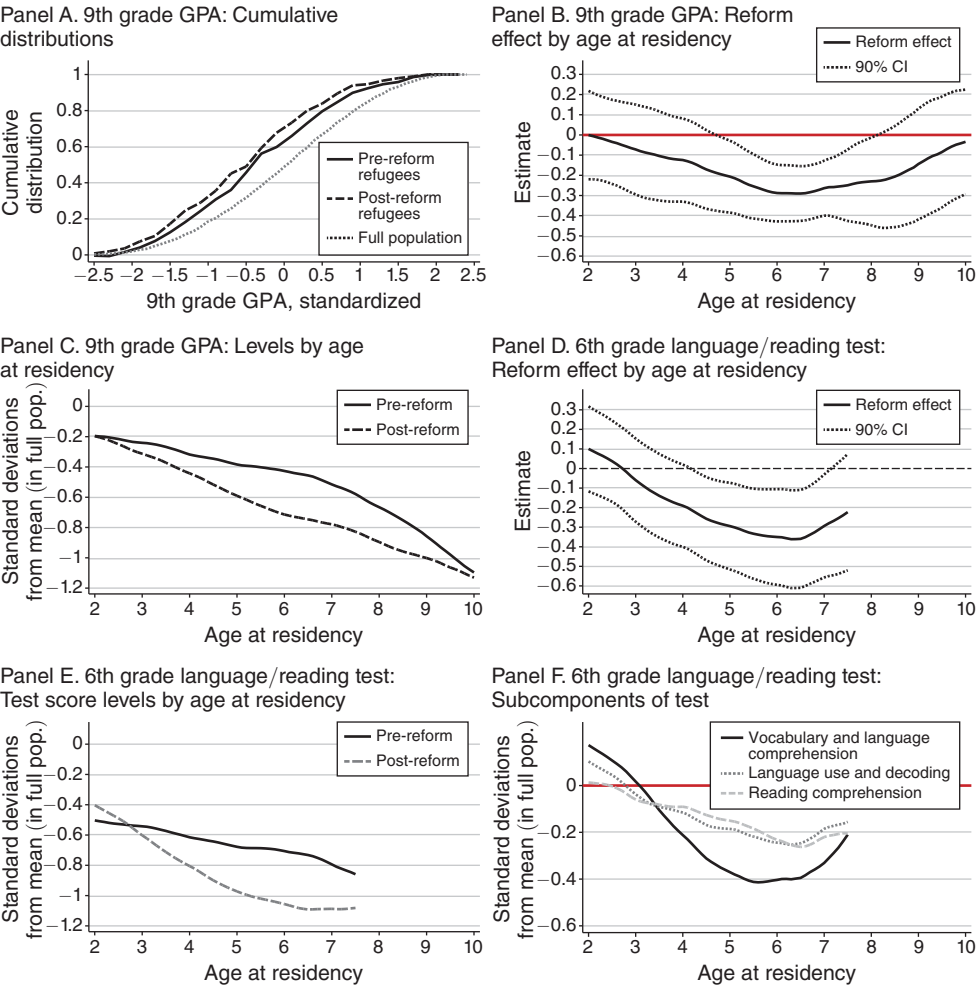


FIGURE 4. HETEROGENEITY IN REFORM EFFECT ON NINTH GRADE GPA AND SIXTH GRADE LANGUAGE/READING TEST SCORES

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

Figure 4, panels D–F extend the results by considering sixth-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 Figure 4, panels D–E mimic Figure 4, panels B–C closely, showing a largely negative and significant

TABLE 3—REFORM EFFECT ON REFUGEES’ EDUCATION AND EARNINGS (1,000 USD),
REFUGEES AGED 7–14 AT TIME OF EXPOSURE TO THE REFORM

	Years of schooling (1)	P(not beyond lower secondary schooling) (2)	P(vocational degree) (3)	Employment 15–16 years post residency (4)	Earnings 15–16 years post residency (5)
Reform effect	−0.570 (0.277)	0.101 (0.047)	−0.089 (0.043)	−0.070 (0.041)	−3.428 (1.574)
Pre-reform mean	12.166	0.373	0.363	0.786	13.574
Full population mean	13.457	0.163	0.451	0.853	20.094
Elasticity: % Effect / 1% increase in benefits	0.096 (0.046)	−0.590 (0.280)	−0.062 (0.458)	0.173 (0.103)	0.449 (0.225)
Observations	1,343	1,343	1,343	1,343	1,343

Notes: The table shows reform effects refugees in school age at exposure (ages 7–14). The table shows reform effects on years of completed schooling (column 1), education-levels for refugees aged 0–14 at the time of residency (columns 2–3), employment 15–16 years after residency (defined as wage earnings > 0) (column 4), earnings in 1,000 2020 USD (column 5). Education levels: lower secondary schooling (maximum 10 years of schooling) in column 2 and vocational degrees (vocational high school and short degrees) in column 3. 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 for reform effects and calculated based on 500 bootstraps elasticities.

reform effect on language test scores for those receiving residency between ages 4 and 7. Figure 4, panel F 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 enrollment likely does not reflect pecuniary considerations but instead a reform-induced unwillingness to conform with Danish childcare institutions and norms.

B. 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.

Table 3 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 9 years of compulsory education (column 1), while the fraction with no further educational attainment beyond lower secondary schooling increases by almost 30 percent (0.101/0.373, column 2). 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 1). This substantial decrease in post-lower secondary education is mainly driven by a reduction in those who obtain vocational degrees (column 3).²⁴

Online Appendix Table A.6 suggests that the reduction in completed schooling and early school dropout 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 8 percentage points while earnings of 17–18-year-olds increase; fewer had earnings between \$0 and \$1,499 per year, but more had earnings between \$1,500 and \$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 an optimal response in the very short run, early school dropout affects labor market attachment and earnings potential negatively in the longer run as shown in columns 4 and 5 in Table 3. 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.

C. Reform Effects on Adolescents Crimes

We turn next to the consequences of the reform on criminal behavior. Estimates in Panel A of Table 4 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 crimes than they otherwise would have.²⁵

Distinguishing by crime type shows that the extensive margin effects on adolescents' crimes during the first five years after reform exposure are driven by property crime, such as larceny and theft, and violent crime, with the probabilities of

²⁴ We group education into four main categories: (i) lower secondary schooling; (ii) high school; (iii) vocational degrees; (iv) college degrees and higher. Online Appendix 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).

²⁵ The increase in youth employment (i.e., absence of idleness) when dropping out of school as suggested from online Appendix Table A.7 may explain why there are no effects on crime (see Online Appendix Table A.2, panel C, column 5) even though education decreases substantially for those aged 7–14 at residency.

TABLE 4—REFORM EFFECT ON ADOLESCENTS’ CRIME IN YEAR 1, YEAR 1–5, AND YEAR 1–10 AFTER RESIDENCY

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

Notes: 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: Percent change in $p(\text{crime})$ or the number of crimes relative to percent 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 bootstraps in panel B.

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 online Appendix Table A.8).

The finding of increased violent crime stands in contrast to both the reform effects on adults' crimes (see Dustmann, Landersø, and Andersen 2024a), which are presented in online Appendix Figure A.2, and findings by Deshpande and Mueller-Smith (2022), who show that lower transfer levels induce economically motivated crimes.²⁶ 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).

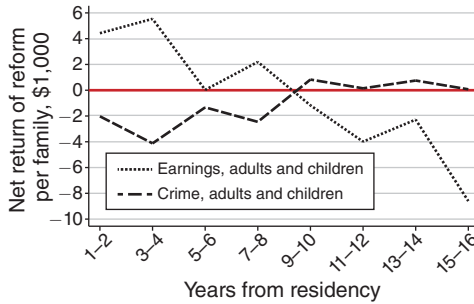
Panel B of Table 4 presents the estimated elasticities of adolescents' crimes with respect to transfers, showing that a 1 percent decrease in welfare benefits results in approximately a 1 percent increase in the probability that adolescents receive a criminal conviction over the following ten years. The estimated elasticities for crime measured as the number of convictions are even larger, with a 1 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 IIB—substantially larger (in relative terms) than adults' intended labor supply responses.

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, Mueller-Smith, and Street 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 4 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 versus the 0.062 reform effect in panel C) indicates that a substantial part of the reform effect on adolescents' crimes is concentrated in families where the parents also engage in crimes (this is not a crime committed together; parents' crimes are mainly theft while children's crimes consists of a broader array of offenses).²⁷

²⁶Related, Khanna et al. (2023) show that discouraging formal employment via benefits eligibility criteria induces organized crime but not crimes characterized by impulsivity.

²⁷As parents' crimes in the first year after residency also increases following the reform (see online Appendix Figure 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.

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



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

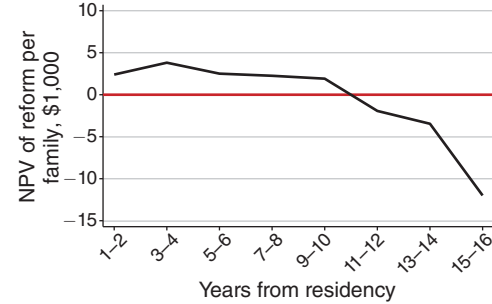


FIGURE 5. COST-BENEFIT ANALYSIS OF THE REFORM, BY YEARS SINCE RESIDENCY

Notes: 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 utilitarian social planner, and we do not place weight on distributional issues between e.g., low versus high income families or natives versus immigrants. Panel 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 equation (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. Panel 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 percent per year.

III. 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 crimes of parents, and the detrimental effects on children that we discuss in this paper.

We estimate the reform effect (as in equation (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):

$$(3) \quad y_{i,t} = \alpha_{g,t}^y + \beta_{g,t}^y \times \text{reform}_i + g_{g,t}^y(Z_i)' \pi + \varepsilon_{i,t}$$

The estimates are presented in online Appendix Table A9. 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' crimes in years 3–4 after residency multiplied with the average costs of their crime in those years plus the reform effects on adolescents' crimes multiplied with the average costs of their crimes in those years.

Figure 5, panel A 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' crimes 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 percent discount rate) evaluated at different time horizons (from a short-run perspective in years 1–2 to 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.²⁸ This composition corresponds to the average family in our sample.

$$(4) \quad \text{Cumulative return}_{y,t} = \sum_{t=1-2}^T 0.98^t (\beta_{child,t}^{crime} + 2 \times \beta_{adult,t}^{crime} + \beta_{child,t}^{earnings} + 2 \times \beta_{adult,t}^{earnings}), \text{ for } T = 1-2, \dots, 15-16$$

Figure 5, panel B 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.

IV. 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.

The central message of this paper is that welfare reforms targeted at increasing individuals' labor supply by reducing transfer payments can have unintended and

²⁸ For example, the estimated cumulative return in year 5–6 is calculated as the discounted sum of net returns for earnings and crimes in years 1–2, 3–4, and 5–6:

$$0.98^2 (\beta_{child,1-2}^{crime} + 2 \times \beta_{adult,1-2}^{crime} + \beta_{child,1-2}^{earnings} + 2 \times \beta_{adult,1-2}^{earnings}) + 0.98^4 (\beta_{child,3-4}^{crime} + 2 \times \beta_{adult,3-4}^{crime} + \beta_{child,3-4}^{earnings} + 2 \times \beta_{adult,3-4}^{earnings}) + 0.98^6 (\beta_{child,5-6}^{crime} + 2 \times \beta_{adult,5-6}^{crime} + \beta_{child,5-6}^{earnings} + 2 \times \beta_{adult,5-6}^{earnings}).$$

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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