

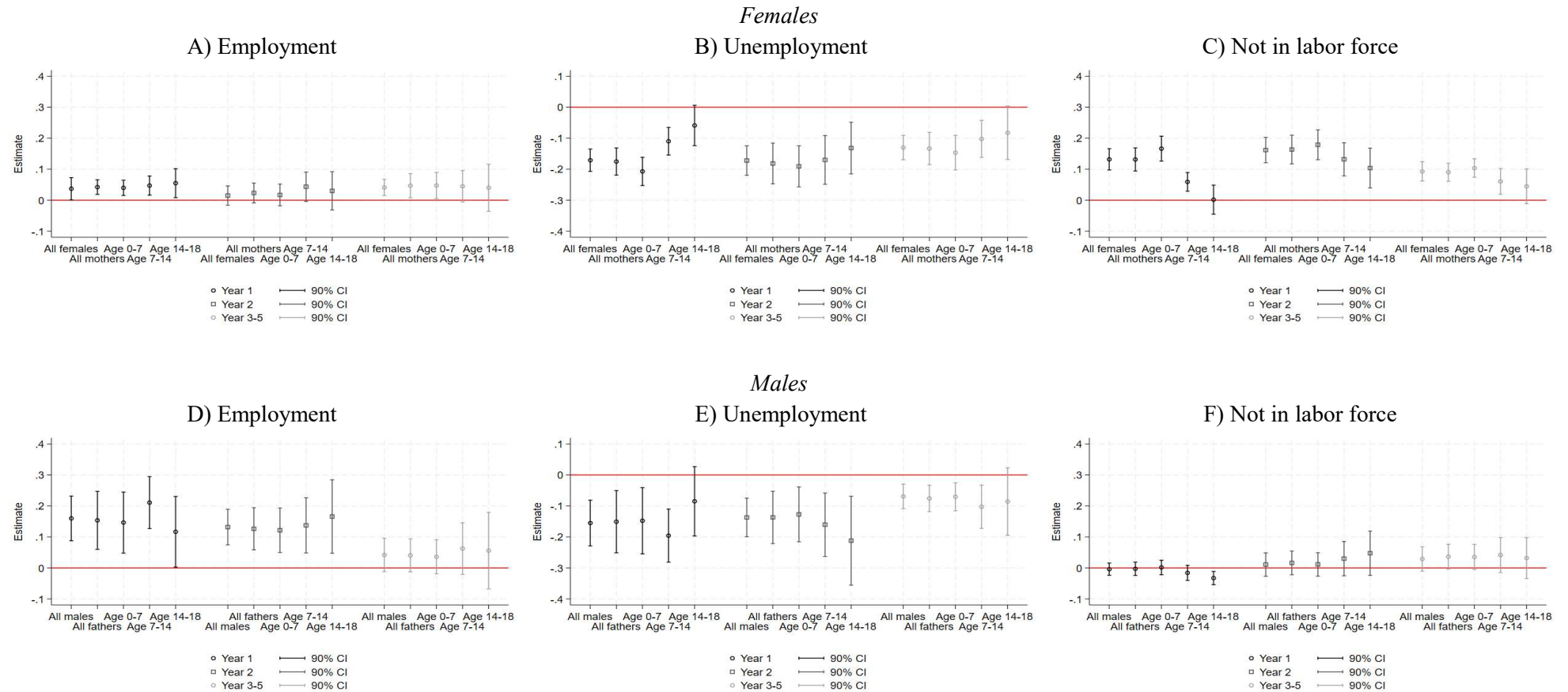
Unintended Consequences of Welfare Cuts on Children and Adolescents

By **Christian Dustmann, Rasmus Landersø, and Lars Højsgaard Andersen**

Online Appendix

A. Appendix Figures and Tables

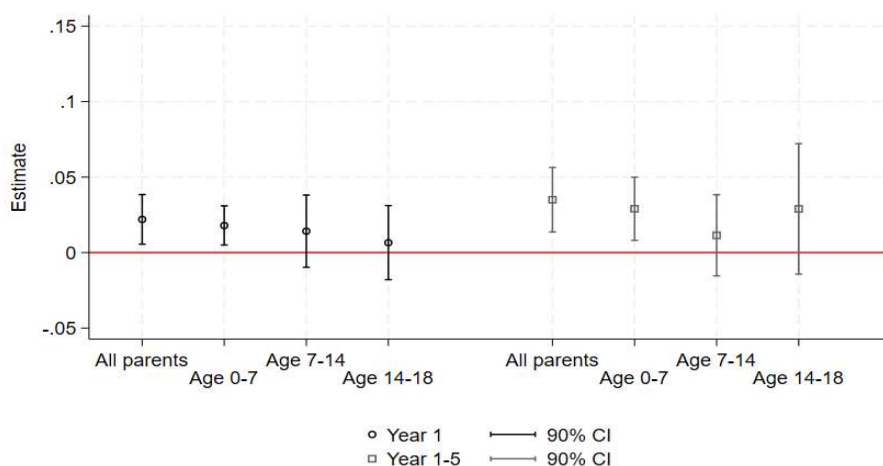
Figure A.1. Reform effect on adults labor market outcomes, by time after residency, age of children, and parents' gender.



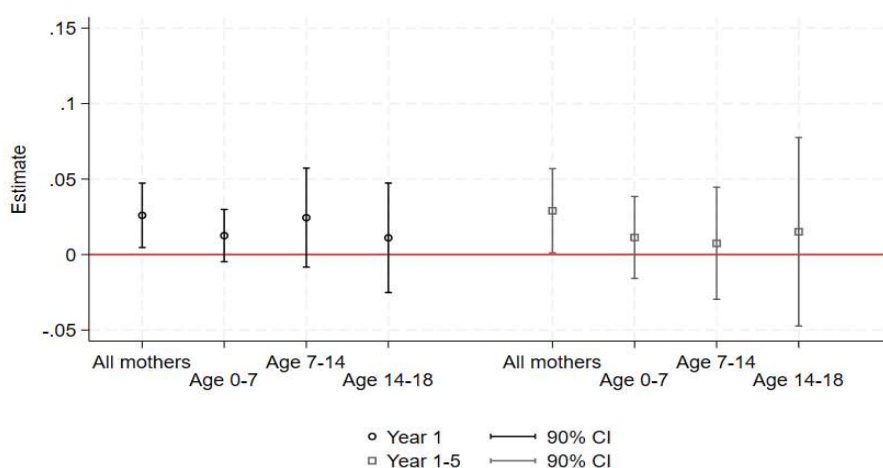
Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform along with 90% confidence intervals on adults employment (Figs. A, D), unemployment (Figs. B, E), and not in the labor force (Figs. C, F) for all adults (estimates from Table 4 in Dustmann, 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.

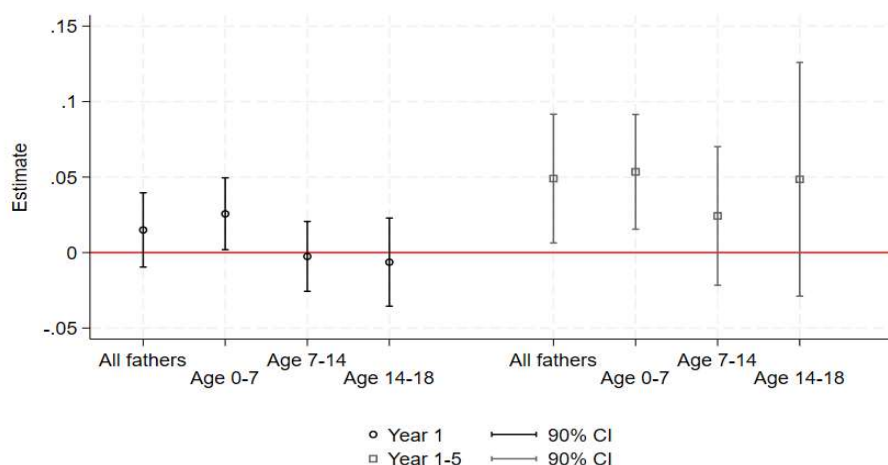
A) Probability of crime, all parents



B) Probability of crime, mothers

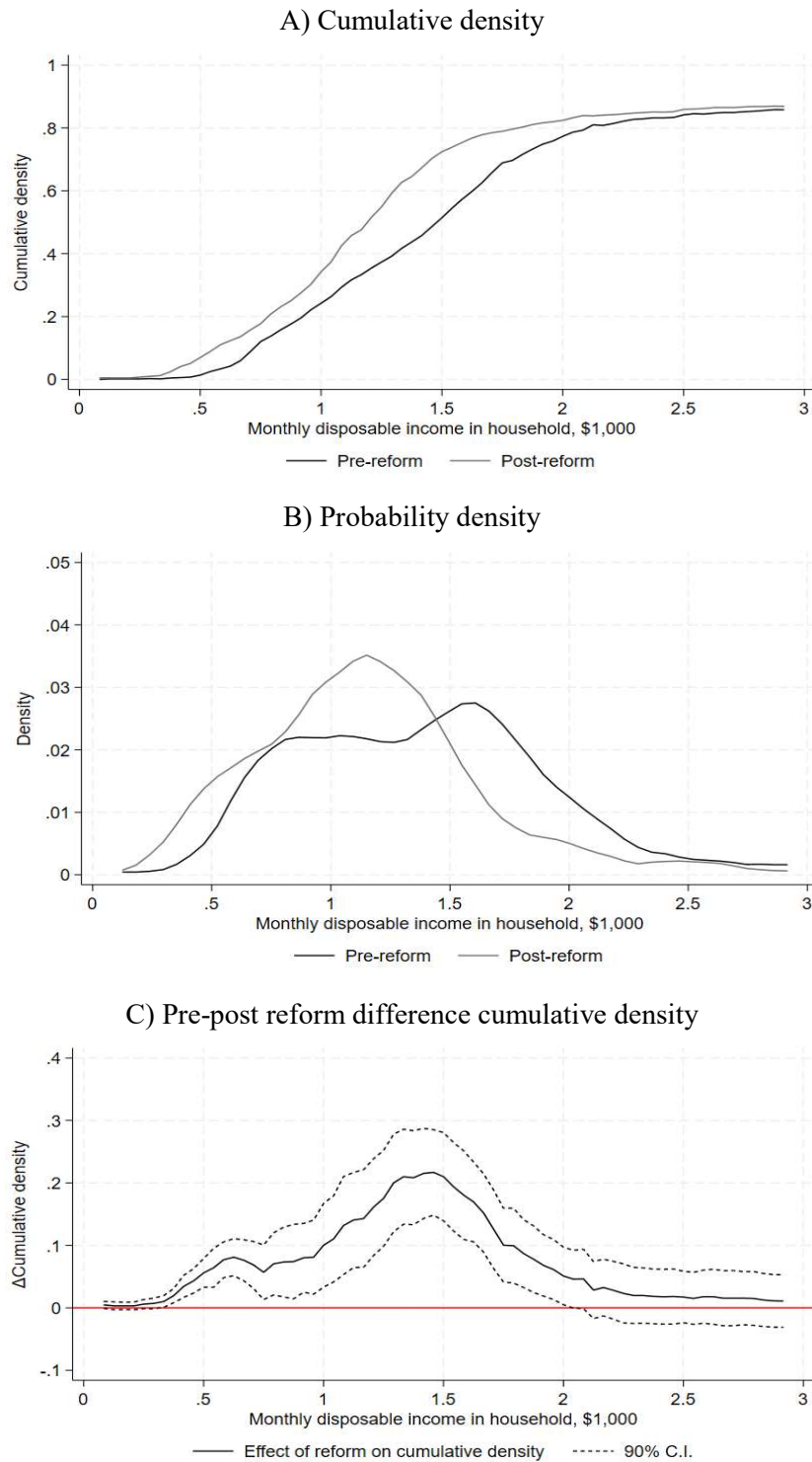


C) Probability of crime, all fathers



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.

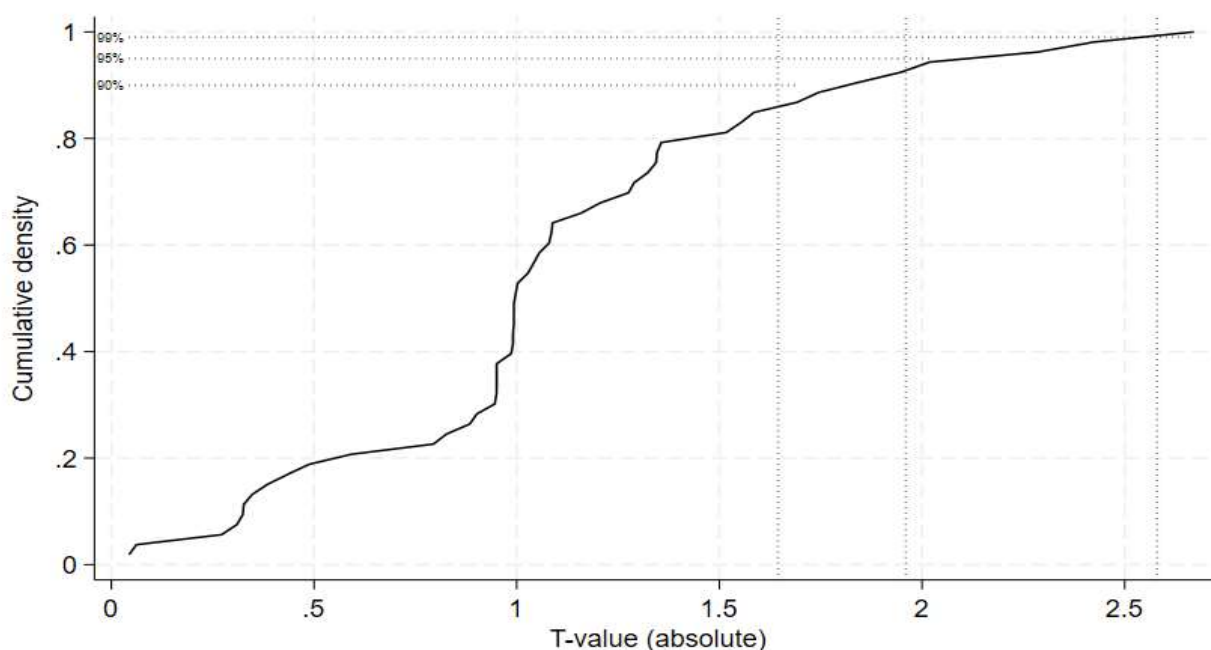
Figure A.3. Reform effect on household disposable income per month, year 1 after residency.



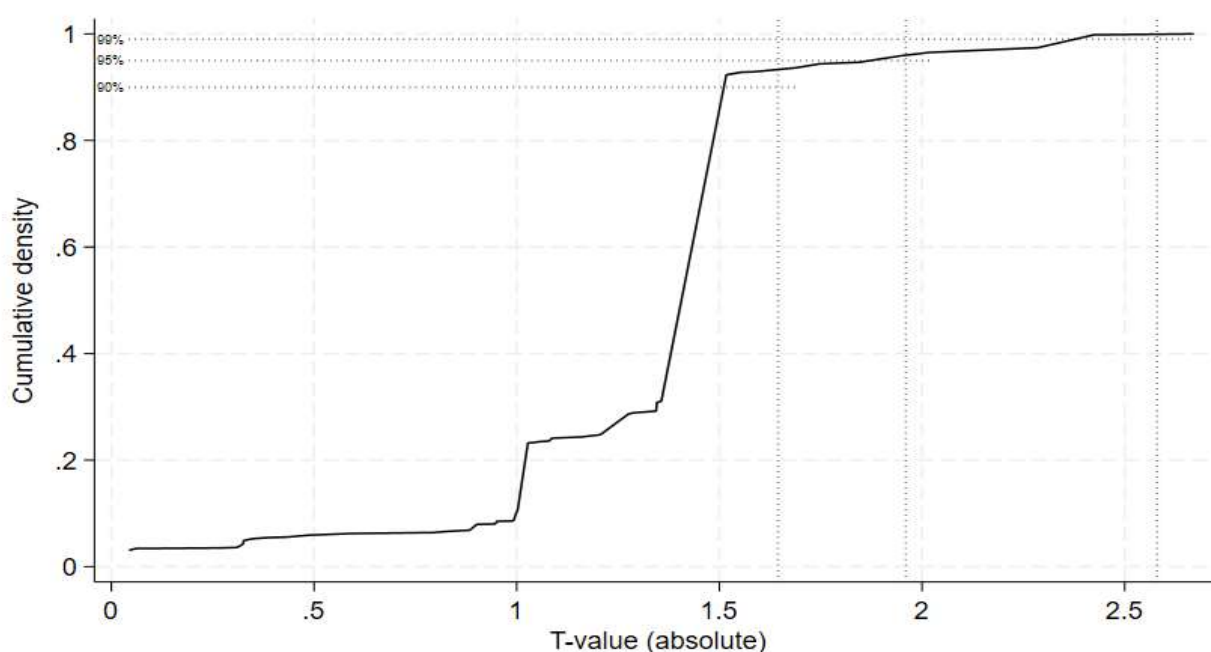
Note: The figure shows household level disposable income (per month) in the first year after residency for refugees granted residency just before the reform and just after the reform. To construct the figures, we create a series of dummies ($1[y \leq x]$) for whether disposable income is x or below, 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

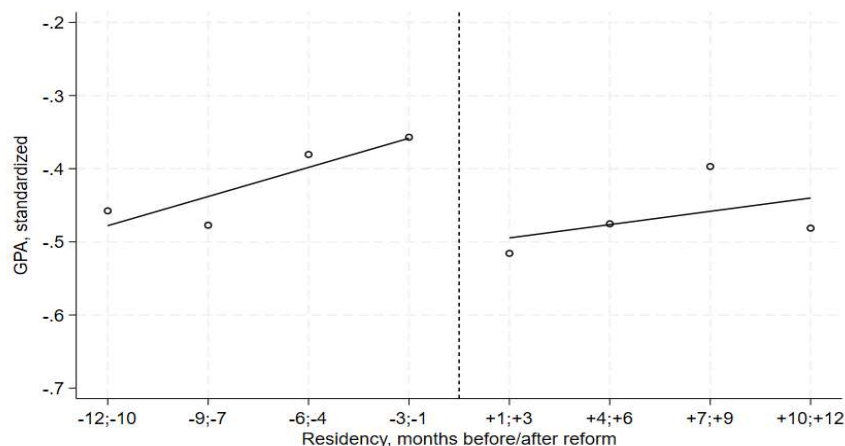


Note: The figure summarizes balancing tests of origin for each country observed in the full sample of children aged 0-18 at residency. For a given country of origin, we construct a dummy indicating whether individuals come from this particular country and regress it on a dummy indicating whether residency 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 country receives weight according to the sample composition.

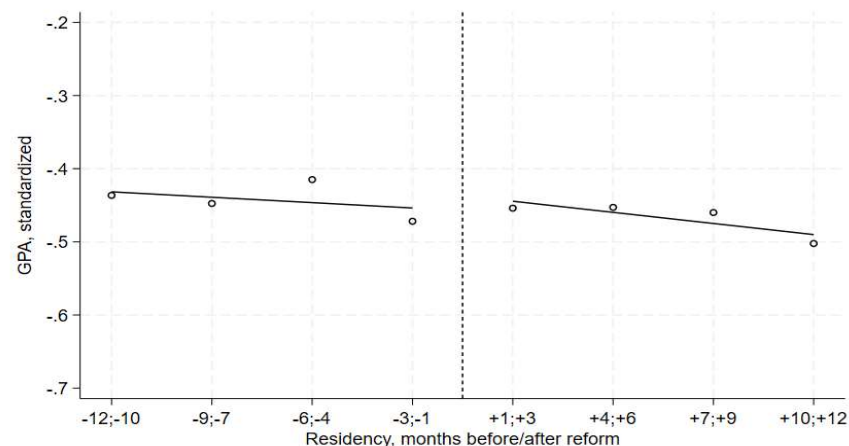
Figure A.5. Comparing refugees' GPA and crime with predictions based on covariates.

GPA, standardized (mean=0, standard deviation=1 in full population)

A) Actual data

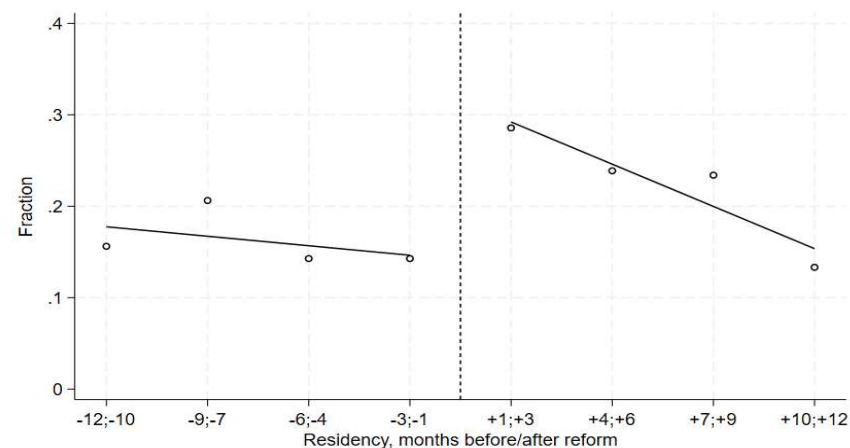


B) Predicted from covariates

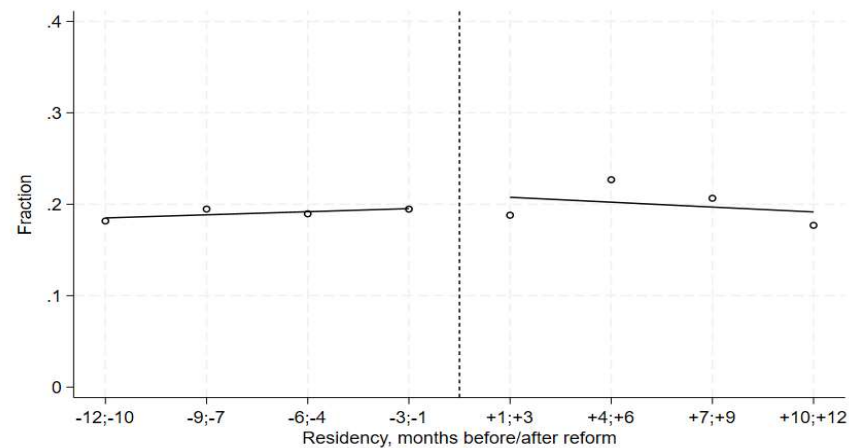


Probability of receiving a crime conviction, year 1-10 after residency, children aged 14-18 at residency

C) Actual data



D) Predicted from covariates

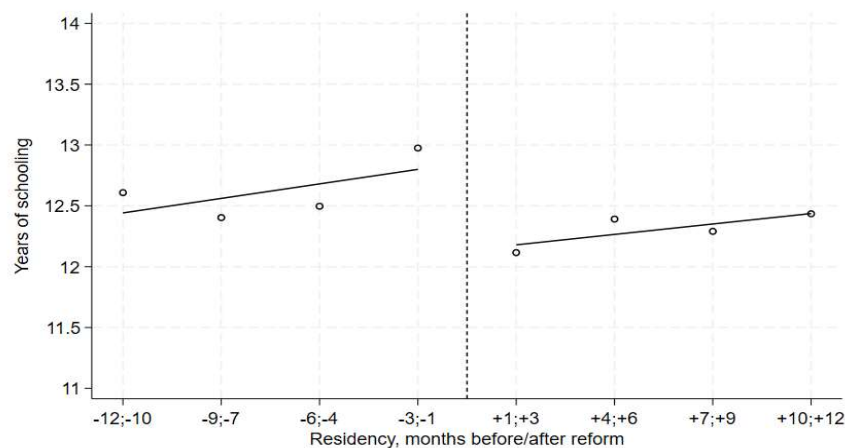


Note: The figure shows raw plots of actual data (as presented in Fig. 2) and contrast these to predicted outcomes from covariates (see Table 1) plotted by timing of residency relative to the reform. Figs. A) and B) show GPA (standardized) for refugees age 0-7 at residency; Figs. C) and D) show the fraction 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).

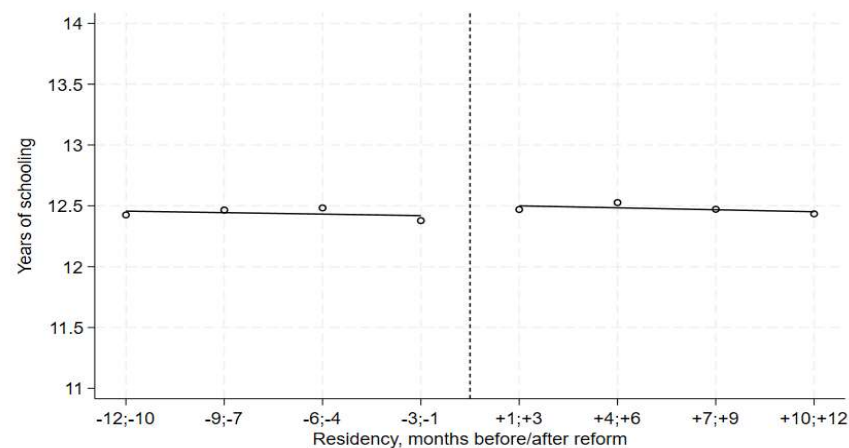
Figure A.6. Comparing refugees' years of schooling and earnings with predictions based on covariates.

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

A) Actual data

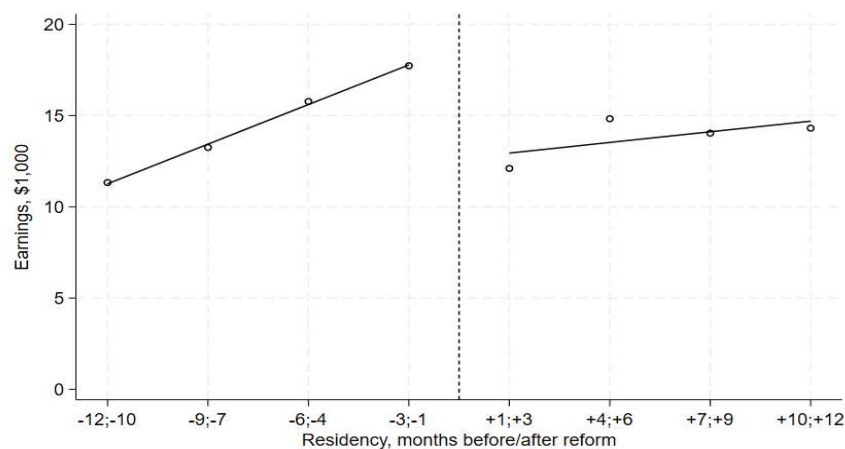


B) Predicted from covariates

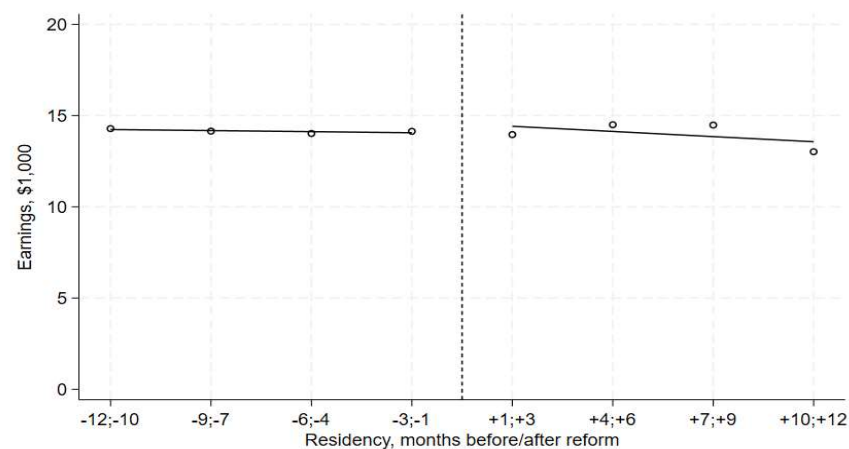


Earnings in \$1,000, 15-16 years after residency, children aged 8-14 at residency

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) Children	(2) All adults	(3) Parents
Reform	-0.009 (0.015)	-0.030 (0.032)	-0.008 (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 (0.072)	-0.127 (0.198)	-0.034 (0.030)	-0.887 (1.150)	0.008 (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* (0.091)	0.050 (0.161)	-0.000 (0.034)	0.320 (0.708)	-0.021 (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 (0.134)	-0.570** (0.277)	-0.070* (0.041)	-3.428** (1.574)	0.000 (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 (0.451)	-0.063 (0.076)	0.525 (4.069)	0.135*** (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 3), and Column 5, Panel D (Table 4).

* 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 (0.003)	-0.001 (0.003)	-0.003 (0.008)	0.129 (0.171)	-0.023 (0.153)	-0.057 (0.270)
Gender (female=1)	-0.011 (0.011)	-0.004 (0.016)	-0.028 (0.022)	-0.053 (0.046)	-0.022 (0.053)	-0.098 (0.076)
Region of origin: Asia	0.035 (0.035)	0.049 (0.041)	0.051 (0.049)	0.141 (0.090)	0.119 (0.091)	0.123 (0.117)
Region of origin: Africa	-	-	-	-0.077 (0.086)	-0.099 (0.085)	-0.108 (0.101)
Region of origin: Eastern Europe	-0.049 (0.035)	-0.002 (0.038)	-0.004 (0.081)	-0.059** (0.027)	-0.018 (0.022)	-0.015 (0.044)
Region of origin: South America	-0.123 (0.097)	-0.030 (0.121)	-	-0.005 (0.006)	-0.001 (0.003)	-
Refugee permit status	0.002 (0.021)	0.019 (0.032)	0.017 (0.026)	-0.042 (0.065)	0.021 (0.078)	0.022 (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 separeted 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.4. Reform effect on 9th grade GPA for refugees aged 0-14 at residency.

	(1)	(2)
	Age 0-7 at residency	Age 7-14 at residency
<i>GPA, standardized, 9th grade</i>		
Reform effect	-0.160*	-0.026
	(0.094)	(0.123)
Pre-reform mean	-0.443	-1.193
<i>GPA, rank, 9th grade</i>		
Reform effect	-0.042	0.001
	(0.027)	(0.032)
Pre-reform mean	0.373	0.190
<i>P(GPA in 1st quartile)</i>		
Reform effect	0.088*	-0.042
	(0.043)	(0.048)
Pre-reform mean	0.410	0.737
<i>P(GPA in 2nd quartile)</i>		
Reform effect	0.006	0.017
	(0.032)	(0.035)
Pre-reform mean	0.260	0.146
<i>P(GPA in 3rd quartile)</i>		
Reform effect	-0.053	0.018
	(0.034)	(0.040)
Pre-reform mean	0.213	0.084
<i>GPA in 4th quartile</i>		
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.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** (0.277)	-0.555* (0.280)	-0.547* (0.277)	-0.481 (0.288)	-0.547* (0.295)	-0.595** (0.301)	1343
B) Employment 15-16 years after residency, children aged 7-14							
Reform effect	-0.070* (0.041)	-0.073* (0.041)	-0.077* (0.042)	-0.073* (0.040)	-0.065 (0.042)	-0.075* (0.043)	1343
C) Earnings 15-16 years after residency, children aged 7-14							
Reform effect	-3.428** (1.574)	-3.657** (1.558)	-3.777** (1.554)	-3.209* (1.771)	-3.474** (1.610)	-3.814** (1.657)	1343
D) 9 th grade GPA, children aged 0-7							
Reform effect	-0.177* (0.090)	-0.188** (0.091)	-0.160* (0.094)	-0.163 (0.100)	-0.165* (0.095)	-0.189* (0.098)	1535
E) P(crime), year 1-10 after residency, children aged 14-18							
Reform effect	0.135*** (0.041)	0.120*** (0.039)	0.090** (0.038)	0.106** (0.042)	0.125** (0.053)	0.148*** (0.044)	467
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, 3 and 4. 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
<i>Years of schooling</i>		
Reform effect	0.050 (0.161)	-0.547** (0.277)
Pre-reform mean	11.361	12.166
Full population mean	11.047	13.457
<i>P(lower secondary schooling)</i>		
Reform effect	-0.014 (0.050)	0.101** (0.047)
Pre-reform mean	0.369	0.373
Full population mean	0.450	0.163
<i>P(High school)</i>		
Reform effect	0.006 (0.054)	0.008 (0.043)
Pre-reform mean	0.495	0.138
Full population mean	0.450	0.158
<i>P(Vocational degree)</i>		
Reform effect	0.009 (0.020)	-0.089* (0.047)
Pre-reform mean	0.091	0.363
Full population mean	0.082	0.451
<i>P(College degree or higher)</i>		
Reform effect	-0.001 (0.018)	-0.022 (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.

	<i>A) Earnings</i>				<i>B) Work and/or studying</i>			
	(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 columns 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.

		<i>P(crime)</i>			<i>Number of crimes</i>		
		Year 1	Year 1-5	Year 1-10	Year 1	Year 1-5	Year 1-10
A) All crime							
All	Reform effect	0.014 (0.021)	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
Males	Reform effect	0.023 (0.025)	0.123** (0.052)	0.190*** (0.065)	0.023 (0.025)	0.398*** (0.145)	0.716* (0.421)
	Pre-reform mean	0.034	0.223	0.291	0.034	0.392	0.703
Females	Reform effect	-0.004 (0.021)	0.074 (0.049)	-0.023 (0.067)	-0.004 (0.021)	0.238* (0.139)	0.004 (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 (0.018)	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
Males	Reform effect	-0.001 (0.021)	0.131* (0.065)	0.039 (0.083)	-0.001 (0.021)	0.276** (0.125)	0.311 (0.358)
	Pre-reform mean	0.027	0.108	0.190	0.027	0.182	0.345
Females	Reform effect	-0.019 (0.019)	0.057 (0.058)	-0.023 (0.067)	-0.019 (0.019)	0.111 (0.124)	0.004 (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 (0.013)	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
Males	Reform effect	0.027 (0.021)	0.081* (0.041)	0.171*** (0.044)	0.027 (0.021)	0.133*** (0.046)	0.250** (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 4 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)
	<i>Parents</i>		<i>Children</i>	
	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 4). Standard errors are clustered by residency month.

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

B. Appendix for Online Publication: Additional Documentation

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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