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Intergenerational spillover effects of language training for refugees

Mette Foged [a,*], Linea Hasager [a,b], Giovanni Peri [c], Jacob Nielsen Arendt [d], Iben Bolvig [e]

[a] University of Copenhagen, Øster Farimagsgade 5 Building 26, DK 1353 Copenhagen, Denmark
[b] CReAM, University College London, Drayton House, 30 Gordon Street, London WC1H 0AX, UK
[c] University of California, Davis, One Shields Avenue, Davis CA 95616, United States
[d] The Rockwool Foundation's Research Unit, Ny Kongensgade 6, DK 1472 Copenhagen K, Denmark
[e] The Danish Center for Social Science Research (VIVE), Olof Palmes Alle 22, DK 8200 Aarhus N, Denmark

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ABSTRACT

Children of refugees are among the most economically disadvantaged youth in several European countries. They are more likely to drop out of school and to commit crime. We find that a reform in Denmark in 1999 that expanded language training for adult refugees and was shown to improve their earnings and job market outcomes permanently, also increased lower secondary school completion rates and decreased juvenile crime rates for their children. The crime effect is entirely due to boys who were below school age when their parents received language training. The older cohorts who were in elementary school when their parents received language training performed better in lower secondary school. Boys were more likely to finish lower secondary school and to sit the final exams, and girls achieved higher grade point averages in the exams.

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1. Introduction

The economic success of first generation immigrants is associated with better schooling outcomes and better social integration of their children (see the classic paper by Borjas, 1992, and more recently Abramitzky et al., 2021, and Jensen and Manning, 2022).[1] We also know that children from disadvantaged economic conditions and culturally distant countries fare poorly in terms of early schooling performance (e.g., Dustmann et al., 2012), and they are more likely to commit juvenile crime and experience the criminal justice system early in life, as dropping out of school is strongly correlated with criminal behavior (Bell et al., 2022).[2]

This paper analyzes whether an integration policy for newly arrived adult refugees, which focused on teaching them the host country language and which was shown to be effective in improving their economic outcomes (Foged et al., 2022b), had any impact on schooling and crime outcomes of their children.[3] To do this, we consider a reform in Denmark that expanded language training for refugees admitted on or after January 1, 1999, but that did not affect refugees admitted before this date. The sharp threshold date of implementation makes data on individuals admitted right before or right after January 1, 1999 amenable to a Regression Discontinuity (RD) analysis. The innovation of this article, relative to Foged et al. (2022b), which studies the effects on the adult refugees themselves, is that we link the policy treatment to the refugees' children and we analyze their educational and criminal outcomes when they were 15 to 18 years old.

* Corresponding author.
E-mail addresses: Mette.Foged@econ.ku.dk (M. Foged), l.hasager@ucl.ac.uk (L. Hasager), gperi@ucdavis.edu (G. Peri), jar@rff.dk (J.N. Arendt), IbBo@vive.dk (I. Bolvig).

[1] Such correlation between the economic performance of parents and the education and early outcomes of children is significant and robust for natives as well (e.g., Chetty et al., 2020; Heckman and Landersø, 2022).

[2] Bell et al., 2013 show that first generation refugees are also more likely to commit crime.

[3] Theories of intergenerational transmission of human capital (e.g., Black et al., 2005; Björklund and Salvanes, 2011; Black and Devereux, 2011) suggest that investment in human capital, especially in fundamental skills, such as language comprehension and speaking, might have a positive spillover effect on the children.

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The reform in 1999 introduced one main policy change and three smaller additional ones. The main change was a substantial increase in the duration (430 extra hours) and the quality (clearer learning goals, national tests and funding) of adult language training. A smaller change was a 30 percent reduction in welfare benefits for refugees older than 25 or with children, which only lasted 13 months. The other two smaller changes concerned geographic distribution of refugees and the mandatory nature of the language classes and were not so crucial to the impact of the reform (further details in Section 2).

Foged et al. (2022b), precursor to this paper, analyzed the impact on adult refugees only (no analysis of children). In that paper, we found that annual earnings of refugees treated by the reform increased by $2,500 annually over the 18 years following the reform. The refugees were 3–4 percentage points more likely to work and were 9–10 percentage points more likely to work in jobs requiring more schooling and better interpersonal communication skills. They were also more likely to obtain additional education (in Danish) compared to the control group. The timing of the effect and types of refugees most affected by the reform further suggest that expanded language training was the reform component most likely to produce such long-run effects (further details in Section 2).

In this paper, we extend this work to the outcomes of children. Children of refugees treated by the reform performed better in lower secondary school and were less likely to commit crime. The effects differ by gender. The reduction in juvenile crime is fully driven by boys who were below school age when their parents started language training. The educational responses are stronger and more significant for children who were already in elementary school when their parents attended language training. Boys were more likely to finish lower secondary school, take the final exams and continue their post-compulsory education. Girls improved their grade point average (GPA) in the lower secondary school final exams. Girls are generally much less likely to commit crime and to fail in lower secondary school, and we do not find any effect on these relatively unlikely outcomes for girls.

Our findings are relevant to other countries, because refugees in Denmark are comparable to refugees in many other European countries in terms of their characteristics, economic performance and crime rates (see Bell et al., 2013; Brell et al., 2020; Jensen and Manning, 2022). Between 1999 and 2016, 2 percent of adult refugees in Denmark were convicted of a crime compared to 1 percent of the native-born population. Children of refugees had a higher probability of committing a crime and dropping out of school than the children of native-born parents. This is typical not just of Denmark but of most EU countries. Therefore, the results in this paper are likely to apply to the issue of schooling and integration of second-generation refugees in Europe.

A small and recent literature establishes in a causal way the effects of integration policies on labor market outcomes of refugees (as summarized in Foged et al., 2022a; Arendt et al., 2022). A few papers connect the impact of refugees' integration policies to the outcomes of their children. Those papers are Damm and Dustmann (2014), Andersen et al. (2019) and Pesola and Sarvimäki (2022). Our paper constitutes an early contribution in this area and is the very first study to identify the intergenerational effect of a reform of language training for refugees on the educational and criminal outcomes of their children with a clean identification strategy.

More broadly, this paper is connected to two larger areas of literature. The first is constituted by those papers evaluating the impact of neighborhoods and socioeconomic deprivation on the schooling and early social outcomes of children. A famous example is the analyses of the "Moving to Opportunity" program (Chetty et al., 2016), and its positive effects on children's schooling and integration from moving economically disadvantaged families to better neighborhoods. Damm and Dustmann (2014), Piopiunik and Ruhose (2017) and Danzer et al. (2022) focus on the initial placement of refugees across neighborhoods to identify such effects. They find that refugees' children had a higher probability of criminal activity if the parents were placed in a high-crime neighborhood. Additionally, they find that high exposure to a higher similar ethnic concentration reduces immigrant children's language proficiency and increases their school dropout probability. Interestingly, for the crime outcomes, Damm and Dustmann (2014) find stronger effects on boys, as we do, suggesting that young men, who are much more likely to commit crime than young women, are the most vulnerable to the effect of negative early influences. None of these papers focuses on the impact of language proficiency of immigrant (refugee) parents on their children's outcomes.

The second related area of the literature is the one evaluating the impact of early schooling policies directed to disadvantaged immigrant children themselves on their later schooling outcomes and criminal outcomes. Currie and Thomas (1999), Felfe and Huber (2017) and Cornelissen et al. (2018) analyze the impact of educational policies directed to immigrant or minority children's access to preschool. Carlana et al. (2022) analyze the effects of offering immigrant students a tutoring program. All those studies evaluate policies directed to the children of immigrants rather than their parents. Pesola and Sarvimäki's (2022) study is closest to our study. They analyze the intergenerational spillover effects of a reform in Finland that changed the active labor market policy for all unemployed immigrants towards more language training and less search and employment support. They find that the reform improved the educational earnings potential and probability of being either employed, studying or in some training program for children whose parents were treated by the reform. Our analysis differs from theirs as the reform we consider directly and more substantially affected language training. Additionally, we only focus on refugees (rather than all immigrants) and we look at criminal outcomes.

2. The 1999 reform and its impact on parents

The reform we study was proposed in April and passed in June 1998 and applied to individuals who were granted refugee status on or after January 1, 1999.[4] Those who were granted refugee status before this date were not treated by the reform. The mean processing time for asylum cases in this period was just over 400 days (Foged et al., 2022b) and the vast majority of those who were granted asylum around the threshold date had applied for asylum before the law was passed. Therefore, they could not manipulate treatment assignment.

Once granted refugee status, refugees are allowed to work and are offered an integration program, consisting of employment support and language training. The 1999 reform introduced longer and better funded language training classes. The potential instruction time was increased by 31 percent (from 1,370 to 1,800 h) and the period of training was increased from 18 to 36 months. Centralized learning goals, national tests and funding to improve teachers' qualifications were launched at the same threshold date.

Three smaller additional policy changes were incorporated in the reform package and affected the same cohorts of refugees. First,

[4] The legislative changes are described in the first Act on Integration of Immigrants (Act 474 from July 1, 1998 ("Lov om integration af udlændinge i Danmark", https://www.retsinformation.dk/eli/lta/1998/474) and the new Act on Language Education for Adult Immigrants (Act 487 from July 1, 1998 ("Lov om undervisning i dansk som andetsprog for voksne udlændinge m.fl. og sprogcentre", https://www.retsinformation.dk/Forms/R0710.aspx?id=87625.

a temporary reduction in welfare benefits for refugees, limited to those over the age of 25 or with children, was introduced. This change was rolled back after 13 months (February 1, 2000). While it represented a significant one-year cut in disposable income and led to a one-year increase in property crime, the fact that the earnings and employment of treated refugees were not affected during the initial years, but rather grew progressively in the following years, suggests that language training rather than the initial welfare cut generated the long-run effects (see Foged et al., 2022b).[5] In the long-run, treated parents have higher income and were less (not more) likely to have been convicted of a crime (Appendix Table A.1).

The second change was the intention to disperse refugees across municipalities in a more systematic way.[6] However, likely due to inertia and capacity constraints in housing availability, allocation of refugees across municipalities was not affected within the first year following the reform (Foged et al., 2022b), which is the period we use to define treated individuals.

Finally, the reform reinforced the incentives to stay in the municipality of placement for the duration of the language classes (about three years), as welfare benefits became conditional on participation in the 3-year integration program. Hence, treated refugees delayed post-settlement residential mobility, as they were attending the language course, and this delay may have affected the outcomes of the treated (Nielsen and Jensen, 2006; Azlor et al., 2020; Foged et al., 2022b).[7] Since mobility was mainly delayed from rural areas, and employment opportunities are usually worse in these areas, this feature may attenuate the positive labor market effects on the treated adult refugees.

We begin by replicating key results in Foged et al. (2022b) and extending their analysis with a few additional outcomes to better understand the impact of the reform on parents before we turn to children's outcomes. The results are presented in Appendix Table A.1. The average annual earnings of parents increased by USD 1,900 or 27 percent of the baseline.[8] The share of parents working in a complex job grew by 8 percentage points. The positive effect of the reform on earnings is entirely driven by parents whose native language does not use the Latin alphabet, suggesting that they benefit from having more hours of class time to learn reading and writing in Danish, as they need to learn the Latin alphabet first. These findings are consistent with Foged et al. (2022b). Language training and greater assimilation into the Danish labor market could modify preferences and encourage marrying outside of the ethnic group or having fewer children, as average fertility of Danish women is lower than that in the countries of origin of refugees. We find no effect on the probability of marrying a Danish native or on completed fertility. One plausible explanation is that most families in our sample were formed before admission to Denmark and preferences towards marriage and fertility may be less amenable to changes in adulthood.[9] This is important as compositional changes in family structure around the cutoff could otherwise interfere with our findings.

3. Data

3.1. Sample

The children in our sample were born within the first three years following the reform or before the reform but still within the primary school age when they started their lives in Denmark. We observe 5,902 children who were born between 1990 to 2001, were still in Denmark when they turned 18, and had at least one parent who was granted asylum in Denmark within a four-year window around January 1, 1999.[10] The restrictions mean that all considered children attended lower secondary (compulsory) school in Denmark and we observe all of them up to and including age 18.

3.2. Outcomes

Table 1 shows summary statistics for our sample of refugee children (column 1) and for native-born children (column 2) from the same birth cohorts. Panel a shows the summary statistics for the main outcomes that we consider and compares children of refugees and children of natives. Panels b and c show those statistics separately by gender. 96 percent of all children had completed lower secondary school by the year they turned 18. Natives typically complete lower secondary school at age 16, while refugee children, whose schooling career is often interrupted and delayed, typically take one or two more years. 92 percent of children of native-born parents took the lower secondary school final exams before or at 18 years of age, while the corresponding number for children of refugees is 90 percent. It is possible to be exempted from the final exams, e.g., due to low language proficiency, and still complete lower secondary school. But because exam grades are used to assess eligibility for upper secondary school, completion without examination likely worsens the possibilities of continuing into further education. Children of refugees, who take the final exams at the end of lower secondary education, have a GPA substantially below that of natives.[11] They are also less likely to enroll in post lower secondary education at age 18 or younger and more than twice as likely to commit juvenile crime.

We observe several related measures of criminal activity. We show an indicator for being convicted of a crime and an indicator for being charged with a crime between ages 15 and 18. We refer to this as juvenile crime. We also show indicators for convictions and charges of the two most common types of crimes for refugees; namely property crime and violent crime.[12] Panels b and c reveal important differences in schooling and crime outcomes across gender. Boys (Panel b) are 3 to 4 times more likely than girls (Panel c) to commit juvenile crime. Almost one out of five male children of refugees have been convicted of one or more crimes between 15 and 18 years of age. Boys are also less likely than girls to sit the exams at the end of lower secondary education. Hence, refugee children, especially the boys, are at risk of not completing compulsory (lower secondary) schooling by taking the final exams that are crucial for continuing education.

[5] The increase in crime rates were driven by shoplifting (a crime associated with material needs) and lasted for the exact duration of the lower benefits Foged et al. (2022b). The authors, therefore, conclude that shoplifting crime among the adults was occasional and related to a very low level of income making it hard for refugees to pay for their living expenses (see also Andersen et al., 2019).

[6] The new act on the dispersal of refugees. In Danish: Bekendtgørelse nr. 630 af 25/08/1998; Bekendtgørelse om boligplacering af flygtninge, https://www.retsinformation.dk/eli/lta/1998/630.

[7] The share who has left their municipality of placement is not statistically different between the treatment and the control groups from years 8 to 18 after admission of the parents.

[8] The point estimate is not statistically significant but it is similar to Foged et al. (2022b), who achieve higher precision as they use a larger sample (8,558 adults compared to the 4,771 parents analyzed here).

[9] 86 percent of the parents were able to document their marriage and parents had more than one child on average at arrival (Table 2).

[10] The parents are a sub-sample of the individuals studied in Foged et al. (2022b).

[11] The scale ranges from −3 to 12. Five marks designate passing (12, 10, 7, 4 and 02) and two marks corresponds to not passing (00 and −3).

[12] In the Online Appendix we show results using count variables for the number of criminal convictions and charges. These results are qualitatively similar. The coefficients are larger in magnitude because those that enter a criminal career typically have more than one criminal conviction between 15 and 18 years of age.

Table 1
Means of outcomes and comparison with natives.

	Refugee (1)	Native born (2)
Panel a. All Children		
Completed Lower Secondary School	0.96	0.96
Took Lower Secondary School Final Exams	0.90	0.92
GPA in Lower Secondary School	5.27	6.71
Post Compulsory School	0.89	0.94
Convicted of Crime	0.13	0.05
Charged with Crime	0.17	0.06
Panel b. Boys		
Completed Lower Secondary School	0.96	0.96
Took Lower Secondary School Final Exams	0.88	0.91
GPA in Lower Secondary School	5.01	6.40
Post Compulsory School	0.88	0.93
Convicted of Crime	0.19	0.06
Charged with Crime	0.26	0.08
Panel c. Girls		
Completed Lower Secondary School	0.97	0.96
Took Lower Secondary School Final Exams	0.93	0.93
GPA in Lower Secondary School	5.54	7.02
Post Compulsory School	0.90	0.94
Convicted of Crime	0.05	0.02
Charged with Crime	0.06	0.03

Notes: Panels a, b, and c show the means of outcomes for children of refugees and children of native born parents from the same birth cohorts (1990–2001). Educational outcomes are measured at age 18 and crime outcomes are indicators for any criminal charges or convictions between 15 and 18 years of age.

4. Empirical design and identification

4.1. Empirical specification

The admission date of the parent determines treatment status, and we use this in a Regression Discontinuity (RD) design to analyze the intergenerational spillover effects of the 1999 reform. We consider child i as treated, $D_i = 1$, if the parent was granted refugee status on or after the cutoff date of January 1, 1999. The time in days between the cutoff date, c, and the date of admission of the parent, x_i, is the "running variable" and the baseline specification assumes locally linear trends on either side of the threshold for treatment ($D_i = 1\{x_i \geqslant c\}$). The estimation equation is as follows:

$$Y_{it} = \alpha + \tau D_i + \beta_1(x_i - c) + \beta_2 D_i(x_i - c) + \varepsilon_{it}. \quad (1)$$

The coefficient τ is the regression discontinuity (RD) estimate and captures the causal average effect of the reform on the outcome of the child, Y_{it}. The two linear terms $\beta_1(x_i - c)$ and $\beta_2 D_i(x_i - c)$ capture the linear dependence of outcomes on admission dates, relative to the cutoff date.[13] Therefore, in this specification, the intercept α captures the pre-reform mean at the boundary point, which we report in the tables with estimated reform effects and use to re-scale the estimated coefficients into percentage effects.

We estimate Eq. 1 with Weighted Least Squares using the mean-squared error (MSE) optimal bandwidth from Calonico et al. (2019) and a triangular kernel to give more weight to the observations closer to the cutoff.[14] Standard errors are heteroskedasticity-robust following Lee and Lemieux (2010).

The main specification uses the admission of the first parent as the running variable. This means that both parents are treated if the child has two (refugee) parents in Denmark. 37 percent have only one (refugee) parent, and 25 percent have parents who were granted refugee status simultaneously. Therefore, for the majority of the sample (62 percent) we are comparing children of households where the parents, who are present, are treated with children of households with no treated parents. The remaining part of the sample includes, in the control group, households where one parent (the later arrival) could potentially be treated.[15] A specification using the admission of the father as running variable produces similar results (shown in the Appendix Table A.3), because the mother rarely (3 percent) arrives before the father.[16]

4.2. Balancing tests around the threshold

The summary statistics in Table 2 provide a picture of the main characteristics of the children of refugees (Panel a) and their parents (Panel b). The average age of parents is 32.5, and most of them are married and arrive with children. Most of the children and their parents originate from either Iraq (43 percent of children) or Afghanistan (24 percent of children) and the parents spoke Arabic or Dari (a dialect of Farsi spoken in Afghanistan) as their primary language at arrival. We use the language training track the refugee is assigned to as an indicator of literacy, skills and aspirations at arrival.[17] Those with primary schooling or less and those who are unfamiliar with the Latin Alphabet are typically assigned to Danish 1, which sets a slow pace for learning the basics of the Danish language. Tertiary educated refugees are most often assigned to Danish 3, which is a fast learner track, and Danish 2 is an intermediate group. Finally, parents are admitted to Denmark as UNHCR quota refugees (10 percent), under the Geneva convention (18 percent), as family to existing refugees in Denmark (26 percent) or under subsidiary Danish legislation (46 percent).

Columns 3 and 4 of Table 2 show RD estimates and 95 percent confidence intervals for differences in the characteristics of the children and their parents around the threshold. The estimated discontinuities are generally very small and statistically insignificant across the threshold date of January 1, 1999. Only one (married) out of 27 covariates tested is weakly significant, which could arise by pure chance.[18] To provide further evidence that lingering imbalances in the predetermined covariates listed in Table 2 do not affect our results, we perform a robustness check where we include the covariates in the regression (Panel e of Table 3).

5. Results

5.1. Average effects and robustness checks

Table 3 shows the impact of the reform on the outcomes of the children when they are 15 to 18 years old. We first show our main

[13] The recent consensus, exemplified in Gelman and Imbens (2019), is in favor of the local linear specification. We analyze alternative specifications in the robustness checks (Table 3) estimating a second order polynomial ($(x_i - c)^2$) in the running variable) on either side of the cutoff as well as a specification that adds control variables.

[14] We check whether our results are sensitive to the choice of kernel and potential outliers close to the cutoff by using a uniform kernel instead of the triangular one and by excluding observations in a two-weeks window around January 1st in a "donut" specification (Table 3).

[15] If the first admitted parent is granted refugee status shortly before the cutoff date, then it is likely that the second parent (when there are two parents present and they are not admitted simultaneously) arrives after and is therefore treated.

[16] We lose 15 percent with no father present if we use the father instead of the first admitted parent. Similarly, 22 percent do not have their mother present and they would be lost if we use the admission date of the mother. We think that the most reasonable division between treatment and control group is one that uses the first parent as the running variable.

[17] Data on the mother tongue as well as the assigned language training track comes from the language training centers.

[18] Appendix Fig. A.1 shows the characteristics of the child by one-month bins of the admission date of the first parent. Similar plots for the adults and further checks of the identification strategy can be found in Foged et al. (2022b).

Table 2
Summary statistics and balancing tests.

	Mean (1)	S.D. (2)	RD Estimate (3)	Confidence Interval (4)
		Panel a. Children		
Female	0.47	0.50	0.04	[-0.07; 0.14]
Born Before 1999	0.76	0.43	-0.01	[-0.14; 0.11]
Iraq	0.43	0.49	-0.05	[-0.19; 0.08]
Afghanistan	0.24	0.43	-0.09	[-0.21; 0.02]
Other Country	0.33	0.47	-0.00	[-0.14; 0.13]
Parents Admitted on the Same Date	0.25	0.43	-0.01	[-0.15; 0.13]
Father Admitted First	0.35	0.48	0.04	[-0.10; 0.18]
Mother Admitted First	0.03	0.18	0.02	[-0.02; 0.06]
Only Father in Denmark	0.22	0.42	-0.05	[-0.14; 0.04]
Only Mother in Denmark	0.15	0.35	-0.06	[-0.16; 0.04]
		Panel b. Parents		
Age	32.50	6.86	0.04	[-1.40; 1.48]
Married	0.86	0.35	0.10	[0.03; 0.18]
Female	0.49	0.50	0.02	[-0.09; 0.12]
No. Children $<$ 3y	0.30	0.52	0.02	[-0.09; 0.13]
No. Children 3–17y	1.46	1.70	0.07	[-0.38; 0.52]
Iraq	0.46	0.50	0.06	[-0.08; 0.19]
Afghanistan	0.19	0.39	-0.02	[-0.11; 0.07]
Other Country	0.35	0.48	-0.04	[-0.19; 0.11]
Speaks Arabic	0.37	0.48	0.01	[-0.09; 0.12]
Speaks Dari	0.11	0.31	-0.01	[-0.06; 0.04]
Danish 1	0.27	0.44	-0.06	[-0.19; 0.06]
Danish 2	0.37	0.48	-0.01	[-0.13; 0.11]
Danish 3	0.29	0.45	0.08	[-0.06; 0.21]
Quota Refugee	0.10	0.30	-0.02	[-0.07; 0.03]
Convention Refugee	0.18	0.38	-0.12	[-0.24; 0.01]
Family-Reunified	0.26	0.44	0.03	[-0.10; 0.17]
Other Refugee	0.46	0.50	0.05	[-0.11; 0.20]

Notes: Summary statistics (columns 1–2) and balancing tests (columns 3–4) of the impact of the reform on predetermined characteristics of the children of the refugees (Panel a) and of their parents (Panel b), who obtained refugee status in Denmark between January 1997 and December 2000. The RD estimates (column 3) are from local linear regressions of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). The 95-percent confidence intervals (column 4) are constructed based on the robust standard errors. Danish 1 to 3 refer to the language track the parent was initially placed in. Quota refugee refers to those granted refugee status under the UNCHR quota, and Convention refugee refers to the Geneva Convention. The number of observations in Panel a is 5,902 and 4,771 in Panel b.

specification in Panel a and then investigate the robustness to alternative specifications in Panels b to e.

Treated children perform better in lower secondary education. We measure this in four different ways. First, we observe whether the child completed lower secondary education (column 1). Second, we observe whether the child took the final exams that mark the end of lower secondary education (column 2). Finally, we observe GPA in the final exams (column 3) and enrolment in post lower secondary education (column 4). Passing the final exams at the end of lower secondary education is the typical path for continuing an academic track education. However, it is possible to be granted an exemption. Continuing is still possible but subject to enrollment tests for upper secondary school entry.

We find a robust and statistically significant effect on the probability of completing lower secondary education before turning 19 (normal completion age would be 16). The effect on whether the child takes the final exams is larger in magnitude and suggests that roughly 10 percent of the refugee children are on a better educational track if their parents have been treated by the reform. The results are robust to alternative specifications, such as a second order polynomial on either side of the cutoff (Panel d), inclusion of controls (Panel e) and alternative weighting of observations (triangular kernel combined with zero weight close to the cutoff in Panel b and a simple uniform kernel in Panel c). The effects of the reform on GPA and post lower secondary education are always positive but not statistically significant for the average child. The second part of Table 3 (columns 5 to 10) shows the effects on criminal convictions and charges when the children are between 15 and 18 years of age. The estimated RD coefficients for the crime outcomes are all negative but rarely significant for the average child. The average effects hide substantial gender and cohort heterogeneity that we explore in Section 5.2.[19]

5.2. Heterogeneous effects

It is usually the case, and certainly in our sample of refugee children, that males have lower schooling attainments and a higher tendency to commit juvenile crime than females (see Table 1). Table 4 and Table 5 unfold the impact of the reform on children's education and crime outcomes looking into the heterogeneity.

Research shows that disadvantaged family backgrounds may be more detrimental for boys than girls (Bertrand and Pan, 2013; Chetty et al., 2016; Autor et al., 2019; Andersen et al., 2019). In this context it is particularly significant and interesting that boys benefit more than girls from having parents treated by the reform. Panel a of Table 4 shows that boys who were young when their parents were treated (born 1996–2001) as well as those who were older (born 1990–1995) and might have had their schooling interrupted while fleeing, are significantly more likely to take the lower secondary school final exams if their parents had access to

[19] Appendix Figs. A.2 and A.4 show robustness checks for nine out of ten outcomes (GPA is shown in Appendix Figs. A.5 to A.7). Completing lower secondary school and taking the final exams (Panels a and b in Appendix Figs. A.2 and A.4) were the only outcomes for which we found robust and significant effect sizes in Table 3 and it is relevant to discuss their robustness: The discontinuities in these two outcomes are visible from a simple plot of the sample means by one-month bins of the running variable (Appendix Fig. A.2). The RD estimates are precisely estimated for bandwidths larger than 2 or 3 months (Appendix Fig. A.3). Placebo estimates generally bounce around zero and are rarely statistically significant. Very few placebo estimates are larger than the estimated reform effect (Appendix Fig. A.4).

Table 3
The impact of the reform on the children.

	Lower Secondary School			Post Lower	Convicted			Charged		
	Completed	Took Final Exams	GPA	Secondary Education	Any Crime	Property Crime	Violent Crime	Any Crime	Property Crime	Violent Crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Panel a. Main (Linear)										
RD Estimate	0.062**	0.095**	0.426	0.058	−0.067	−0.055	−0.051**	−0.115*	−0.159***	−0.062
	(0.030)	(0.042)	(0.354)	(0.049)	(0.047)	(0.039)	(0.023)	(0.063)	(0.062)	(0.038)
Mean of Untreated at Cutoff	0.923	0.865	4.905	0.846	0.154	0.118	0.062	0.274	0.255	0.116
Bandwidth	146	144	168	144	153	168	182	139	129	164
Panel b. Donut Specification										
RD Estimate	0.060*	0.090**	0.437	0.045	−0.057	−0.049	−0.049**	−0.101	−0.142**	−0.057
	(0.031)	(0.043)	(0.363)	(0.051)	(0.048)	(0.040)	(0.023)	(0.065)	(0.062)	(0.040)
Mean of Untreated at Cutoff	0.923	0.866	4.905	0.848	0.155	0.118	0.062	0.273	0.249	0.115
Bandwidth	146	146	168	148	156	169	185	145	132	166
Panel c. Uniform Kernel										
RD Estimate	0.059**	0.061*	0.533	0.037	−0.046	−0.048	−0.064***	−0.091	−0.121**	−0.078**
	(0.027)	(0.034)	(0.347)	(0.046)	(0.050)	(0.043)	(0.024)	(0.062)	(0.055)	(0.035)
Mean of Untreated at Cutoff	0.929	0.881	4.903	0.863	0.143	0.107	0.064	0.268	0.227	0.124
Bandwidth	135	164	149	128	120	121	154	116	114	156
Panel d. 2nd Order										
RD Estimate	0.064**	0.099**	0.356	0.047	−0.067	−0.063	−0.084**	−0.131**	−0.154**	−0.083*
	(0.032)	(0.045)	(0.422)	(0.053)	(0.051)	(0.049)	(0.033)	(0.065)	(0.060)	(0.047)
Mean of Untreated at Cutoff	0.917	0.861	4.846	0.845	0.165	0.126	0.077	0.292	0.250	0.128
Bandwidth	281	283	277	275	290	261	240	282	273	264
Panel e. Controls										
RD Estimate	0.059**	0.082**	0.519*	0.049	−0.049	−0.049	−0.027	−0.124**	−0.148***	−0.054*
	(0.028)	(0.034)	(0.273)	(0.045)	(0.039)	(0.035)	(0.020)	(0.055)	(0.054)	(0.032)
Mean of Untreated at Cutoff	0.924	0.875	5.039	0.853	0.151	0.115	0.052	0.269	0.236	0.110
Bandwidth	157	190	229	160	199	192	229	166	148	209
N	5,902	5,902	5,334	5,902	5,902	5,902	5,902	5,902	5,902	5,902

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table entries are the RD estimates ($\hat{\tau}$), robust standard errors in parentheses, and the mean of the outcome for the untreated group measured at the cutoff ($\hat{\alpha}$) from local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). The outcomes measuring completed, took final exams and post lower secondary education are dummies for completing lower secondary school, taking the final exams or continuing education beyond lower secondary education before age 18. GPA is the grade point average conditional on taking the final exams at the end of lower secondary school. The crime outcomes are dummies for being convicted of or charged with a crime between 15 and 18 years of age. In Panel e we control for the characteristics of the first parent (the father if both parents are admitted on the same date). The control variables are age, age squared, unmarried, female, number of children between 0–2 years old and 3–17 years old, Iraq, Afghanistan, speaks Arabic, speaks Dari, Danish 1, 2 or 3 (unknown level is the reference), quota refugee, family-reunified or other refugee (convention refugee is the reference).

expanded language training. The increase is large in magnitude, between 15 and 22 percentage points. The older boys are also significantly more likely to complete lower secondary school (compulsory schooling) and to continue on to post-compulsory education if the parents are treated. Girls are generally more likely to take the final exams (Table 1) and they are not affected on this margin. Older girls (born between 1990 and 1995) perform better in the final exams (higher GPA) if their parents are treated.[20]

Panel a of Table 5 shows that the better performance in school is accompanied by a very large reduction in crime rates for the younger boys (born 1996–2001).[21,22] They are less likely to be charged with property crimes and violent crimes. The two most common types of crime in our data. These findings are consistent with Andersen et al. (2019) who find that reduced welfare benefits for adult refugees increases the propensity to commit violent crime for their male children. The impact they estimate for property crime is also large but less precisely estimated. Additionally, Damm and Dustmann (2014) find that exposure to criminal neighbors at arrival raises violent crime for young male refugees (and not for females), but does not impact other types of crime. Finally, our results on crime and on dropping out of secondary school – limited to boys – is consistent with Bell et al. (2022) who emphasize the incapacitation effect of attending school for teenagers as the main channel through which schooling reduces violent crime for young males.

The younger boys (born 1996–2001) were very young when their parents were treated and had their entire schooling in Denmark. They were more likely to be teenagers in a household with better language skills and more economic resources, while the economic effects of the reform on the parents had not fully materialized when the early cohorts (born 1990–1995) became teenagers. Different economic conditions could be part of the explanation for why we found significant improvements in crime outcomes for the younger boys.[23]

[20] Panels a to d of Appendix Figs. A.5 to A.7 show the robustness of these results for older boys and girls. The discontinuities in outcomes are clear from the simple plot of outcome means by one-month bins, the point estimates are generally stable and significant from bandwidths larger than two or three months (well below the MSE-optimal bandwidth), and the RD estimates are rarely large and significant at placebo cutoff dates.

[21] Table A.2 in the Online Appendix shows very similar results using the number of criminal charges/convictions. Refugees, who commit crime, generally commit more than one crime (1.7 convictions conditional on being convicted and 4.5 charges conditional on being charged) and the coefficients are larger.

[22] Panels e and f of Appendix Figs. A.5 to A.7 show robustness checks of convictions and charges of violent crimes, the typical crime outcome in related papers (Damm and Dustmann, 2014; Andersen et al., 2019; Bell et al., 2022).

[23] Foged et al. (2022b) show that the impact of expanded language training on earnings is gradual and does not fully materialize until 8–10 years after asylum, where the effect starts to be statistically significant. The effects for the subset of parents are similar but not statistically significant (see Table A.1 of the Online Appendix).

Table 4
Heterogeneous effects on schooling.

	Lower Secondary School			Post Lower
	Completed (1)	Took Final Exams (2)	GPA (3)	Secondary Education (4)
		Panel a. Gender and Birth Cohort		
Boys Born 1990–1995	0.157**	0.218***	0.303	0.204**
	(0.071)	(0.081)	(0.661)	(0.088)
Mean of Untreated at Cutoff	0.823	0.730	4.244	0.734
Boys Born 1996–2001	0.049	0.145*	0.318	0.024
	(0.041)	(0.074)	(0.649)	(0.077)
Mean of Untreated at Cutoff	0.959	0.845	4.977	0.926
Girls Born 1990–1995	−0.004	−0.046	1.411**	0.059
	(0.085)	(0.086)	(0.648)	(0.121)
Mean of Untreated at Cutoff	0.937	0.975	4.085	0.764
Girls Born 1996–2001	0.023	0.005	−0.032	−0.083
	(0.017)	(0.067)	(0.843)	(0.094)
Mean of Untreated at Cutoff	0.988	0.965	6.175	0.957
		Panel b. Parent's Alphabet of Mother Tongue		
Latin Alphabet	0.085	0.097	0.472	0.118
	(0.060)	(0.072)	(0.564)	(0.081)
Mean of Untreated at Cutoff	0.891	0.861	4.721	0.786
Other Alphabet Than Latin	0.065**	0.098*	0.301	0.031
	(0.032)	(0.053)	(0.464)	(0.066)
Mean of Untreated at Cutoff	0.929	0.866	5.175	0.872

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table entries are the RD estimates ($\widehat{\tau}$), robust standard errors in parentheses, and the mean of the outcome for the untreated group measured at the cutoff ($\widehat{\alpha}$) from local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Each estimation is based on a sub-sample (described in the row) and the optimal bandwidth from the full sample (Panel a in Table 3). The sample sizes in columns (1), (2) and (4) are 1,437 (Latin alphabet), 4,465 (other alphabet), 1,684 (boys born 1990–1995), 1,438 (boys born 1996–2001), 1,435 (girls born 1990–1995), 1,345 (girls born 1996–2001). The sample sizes in column (3) are 1,280 (Latin alphabet), 4,054 (other alphabet), 1,420 (boys born 1990–1995), 1,340 (boys born 1996–2001), 1,295 (girls born 1990–1995), 1,279 (girls born 1996–2001). The outcomes in columns 1, 2, and 4 are dummies for completing lower secondary school, taking the lower secondary school final exams, and continuing education beyond lower secondary education at age 18 or earlier. GPA in column 3 is the grade point average conditional on taking the final exams at the end of lower secondary school.

Table 5
Heterogeneous effects on juvenile crime.

	Convicted			Charged		
	Any Crime (1)	Property Crime (2)	Violent Crime (3)	Any Crime (4)	Property Crime (5)	Violent Crime (6)
			Panel a. Gender and Birth Cohort			
Boys Born 1990–1995	−0.010	−0.078	−0.022	−0.089	−0.126	−0.009
	(0.115)	(0.096)	(0.047)	(0.143)	(0.146)	(0.094)
Mean of Untreated at Cutoff	0.203	0.197	0.049	0.405	0.365	0.150
Boys Born 1996–2001	−0.160**	−0.061	−0.146**	−0.315***	−0.395***	−0.228***
	(0.080)	(0.066)	(0.057)	(0.106)	(0.089)	(0.058)
Mean of Untreated at Cutoff	0.220	0.108	0.162	0.390	0.376	0.229
Girls Born 1990–1995	−0.068	−0.052	−0.006	−0.078	−0.076	−0.003
	(0.062)	(0.059)	(0.012)	(0.064)	(0.066)	(0.012)
Mean of Untreated at Cutoff	0.071	0.071	−0.001	0.069	0.067	−0.005
Girls Born 1996–2001	−0.020	−0.021	−0.008	0.067	0.016	0.023
	(0.075)	(0.068)	(0.008)	(0.124)	(0.118)	(0.056)
Mean of Untreated at Cutoff	0.074	0.073	0.008	0.120	0.108	0.031
			Panel b. Parent's Alphabet of Mother Tongue			
Latin Alphabet	−0.074	−0.051	−0.033	−0.063	−0.115	0.004
	(0.074)	(0.064)	(0.037)	(0.098)	(0.099)	(0.063)
Mean of Untreated at Cutoff	0.165	0.146	0.048	0.254	0.277	0.083
Other Alphabet Than Latin	−0.070	−0.086*	−0.056*	−0.172**	−0.243***	−0.101**
	(0.065)	(0.049)	(0.033)	(0.085)	(0.075)	(0.048)
Mean of Untreated at Cutoff	0.151	0.117	0.061	0.291	0.257	0.119

Notes: * $p<0.10$, ** $p<0.05$, *** $p<0.01$. Table entries are the RD estimates ($\widehat{\tau}$), robust standard errors in parentheses, and the mean of the outcome for the untreated group measured at the cutoff ($\widehat{\alpha}$) from local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Each estimation is based on a sub-sample (described in the row) and the optimal bandwidth from the full sample (Panel a in Table 3). The sample sizes are 1,437 (Latin alphabet), 4,465 (other alphabet), 1,684 (boys born 1990–1995), 1,438 (boys born 1996–2001), 1,435 (girls born 1990–1995), 1,345 (girls born 1996–2001). The outcomes are dummies for being convicted of or charged with a crime (any crime and the two most common crimes in the sample) between 15 and 18 years of age.

We also find evidence of increased school attainment and crime reducing effects for children in households that likely speak a language that does not use the Latin alphabet (Panel b of Tables 4 and 5), representing mainly the Afghani and Iraqi refugees. These families are likely further away in culture and language from Danish nationals and therefore benefited more from the longer language training program in terms of better labor market potentials and higher earnings.[24] The differences across Latin/non-Latin alphabet of parents' mother tongue in children's outcomes are not statistically significant, but the reduction in juvenile crime is larger and more precisely estimated for the non-Latin group, showing a clearer tendency towards a better track for those children who grew up in households with more financial resources if their parents were treated.

6. Conclusion

This paper is one of the very few and early studies to analyze the intergenerational spillover effects of integration policies. We start from an important and promising reform implemented in Denmark in 1999, which expanded the instruction time and allocated more resources to language training, and which earlier research has shown to be effective in improving long-term labor market opportunities for the affected adult refugees (see Foged et al., 2022b). In this paper, by linking refugees admitted around the threshold date to their children, we investigate whether the reform produced spillover effects on the children's performance in lower secondary education and criminal activity.

We find that children of refugees who were treated by the reform were more likely to complete lower secondary school and more likely to take the final exams at the end of it. By exploring the heterogeneity of the effects, we see stronger positive educational effects on children who were in elementary school when their parents attended language training. The boys became less likely to drop out of lower secondary school and more likely to continue their education, while girls achieved higher GPA at the end of lower secondary school. The younger boys, who were below school starting age when their parents were treated and still in lower secondary school when the economic conditions of the adult refugees were improved by the reform, also show improved schooling outcomes (less significant than the older cohorts) and show a significant reduction in juvenile crime. A longer and earlier exposure to a better family environment in terms of human capital and better income may, therefore, be needed to achieve the effect on criminal activity.

We also see indications of stronger effects for refugee children from families who speak languages that are very different from Danish. This suggests that the positive spillover effects were stronger for children, whose parents originated from more different cultures and whose native language is further from Danish. These parents also had the strongest, significant earnings gain.

While our identification strategy does not allow us to disentangle the role of higher income and that of better human capital of parents in generating these effects, it clearly adds to the strong evidence of the important effects of language training of refugees for their own and their children's success.

Data availability

The data that has been used is confidential.

[24] Appendix Table A.1 confirms that the earnings effect is very large and statistically significant for this group (consistent with Foged et al., 2022b).

Declaration of Competing Interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

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Appendix A. Supplementary material

Supplementary data associated with this article can be found, in the online version, at https://doi.org/10.1016/j.jpubeco.2023.104840.

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