

NBER WORKING PAPER SERIES

INTERGENERATIONAL SPILLOVER EFFECTS OF LANGUAGE TRAINING FOR
REFUGEES

Mette Foged
Linea Hasager
Giovanni Peri
Jacob N. Arendt
Iben Bolvig

Working Paper 30341
<http://www.nber.org/papers/w30341>

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
August 2022

This project is conducted as part of the Economic Assimilation Research Network (EARN), generously financed by the Innovation Fund Denmark (grant no. 6149-00024B). We thank the Ministry of Immigration and Integration for assistance with data and reports on early language training in Denmark. The project benefited from feedback and discussions with numerous seminar participants. The project has been approved by the relevant authorities in Denmark, including Statistics Denmark and the GDPR office at University of Copenhagen (journal nr. 514-0101/19-2000) The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

NBER working papers are circulated for discussion and comment purposes. They have not been peer-reviewed or been subject to the review by the NBER Board of Directors that accompanies official NBER publications.

© 2022 by Mette Foged, Linea Hasager, Giovanni Peri, Jacob N. Arendt, and Iben Bolvig. All rights reserved. Short sections of text, not to exceed two paragraphs, may be quoted without explicit permission provided that full credit, including © notice, is given to the source.

Intergenerational Spillover Effects of Language Training for Refugees
Mette Foged, Linea Hasager, Giovanni Peri, Jacob N. Arendt, and Iben Bolvig
NBER Working Paper No. 30341
August 2022
JEL No. I21,J24,J30,J6

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 show that a reform in Denmark in 1999, that expanded language training for adult refugees and improved their economic integration, had significant intergenerational spillover effects in terms of higher completion rates from lower secondary school and lower juvenile crime rates. The effects on crime are driven by boys who were below school-starting age when their parents were treated.

Mette Foged
Department of Economics
University of Copenhagen
Øster Farimagsgade 5, building 26
1353 Copenhagen K
Denmark
Mette.Foged@econ.ku.dk

Linea Hasager
University College London
Drayton House, 30 Gordon Street
London WC1H 0AX
United Kingdom
l.hasager@ucl.ac.uk

Giovanni Peri
Department of Economics
University of California, Davis
One Shields Avenue
Davis, CA 95616
and NBER
gperi@ucdavis.edu

Jacob N. Arendt
The Rockwool Foundation's Research Unit
Ny Kongensgade 6, DK 1472
Copenhagen K
Denmark
jar@rff.dk

Iben Bolvig
The Danish Center for
Social Science Research (VIVE)
Olof Palmes Alle 22, DK 8200
Aarhus N
Denmark
IbBo@vive.dk

I 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).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, Frattini, and Lanzara, 2012), and are more likely to commit juvenile crime and come in contact with criminal justice early in life as dropping out of school is strongly correlated with criminal behavior (Bell, Costa, and Machin, 2022).²

This paper analyzes whether integration policies for newly arrived adult refugees, focused on teaching them the host country language and proven to be effective in improving their economic opportunities (Foged et al., 2022), improve the 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 did not affect refugees admitted before this date. The sharp threshold date of implementation makes individuals admitted right before or right after January 1, 1999 amenable to a Regression Discontinuity (RD) analysis. The innovation of this article is that we link the policy treatment to the refugees' children, born between 1990 and 2001, to analyze education and crime outcomes, when the children are 15 to 18 years old.

Refugees in Denmark are comparable to refugees in many other European countries in terms of their economic performance (see Brell, Dustmann, and Preston, 2020) and their crime rates (e.g., Bell, Fasani, and Machin, 2013). Between 1999 and 2016, 2 percent of adult refugees were convicted of a crime compared to 1 percent of the native born population. Children of refugees also had a larger probability of committing a crime than the children of native born. This is typical for refugees in EU countries; and hence, the results in this paper are likely to apply to the much broader issue of schooling and integration of second generation refugees in Europe.

The reform in 1999 introduced one main policy change and three smaller additional ones. The first and most significant change was a substantial increase in the duration (430 extra hours) and the quality (clearer learning goals, national tests and funding) of adult host-country language training. The second was a temporary 30% reduction in welfare benefits for refugees older than 25 or with children, which

¹Such 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).

²Bell, Fasani, and Machin (2013) show that first generation refugees are also more likely to commit crime.

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

only lasted 13 months. The other two changes, concerning geographic distribution of refugees and the mandatory nature of the language class were not so crucial to understand the impact of the reform (see Section II).

Foged et al. (2022) find that annual earnings of refugees treated by the reform increased by \$2,500 over an 18 year follow up period. The refugees were 3-4 percentage points more likely to work and worked 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 type of refugees most affected by the reform further suggest that expanded language training was the reform component most likely to produce the long-run effects.

In this paper, by linking refugees to their spouses and children, we analyze the effects on family structure, fertility and on second generation outcomes. While we do not find effects on the marriage probability and fertility, we find that children of refugees treated by the reform were more likely to complete lower secondary school and to take the final exam. Those effects are similar across birth cohorts (born between 1990 and 2001) but larger and only statistically significant for boys. Additionally, boys, who were younger than school age, when their parents were treated, were significantly less likely to be convicted of property or violent crime when they were between 15 and 18 of age.⁴ We do not observe any significant impact on girls, who are generally less likely to commit crime and more likely to complete lower secondary school.

Very few papers establish in a causal way the effects of integration policies on labor market outcomes of refugees (as summarized in Foged, Hasager, and Peri, 2022; Arendt, Dustmann, and Ku, 2022) and even fewer connect the impact of these policies to outcomes of the next generation (Damm and Dustmann, 2014; Andersen, Dustmann, and Landersø, 2019; Pesola and Sarvimäki, 2022). Our paper, therefore, constitutes an early contribution in this area.

Our paper is also connected to two larger and broader 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, e.g.), and its positive effects on children’s schooling and integration from moving economically disadvantaged families to better neighborhoods. Other studies such as Damm and Dustmann (2014) and Piopiunik and Ruhose (2017) have focused on the initial random placement of refugees across neighborhoods and found that their children had higher

⁴This is consistent with a growing literature (due to Cunha and Heckman, 2008; Cunha, Heckman, and Schennach, 2010) suggesting that policies have a stronger impact if it affected the parents while the children were very young.

probability of criminal activity if the parents were placed in a high-crime neighborhood. They find stronger effects on boys, suggesting that young men are the most vulnerable to the effect of delinquent neighborhood influences in their early teens. Another relevant paper is Couttenier et al. (2019). They find that policies fostering better labor market integration reduce the likelihood that adult refugees commit crime.

The second relevant area of the literature is the one evaluating the impact of human capital investment policies on schooling and early criminal outcomes of disadvantaged children. A few studies have analyzed the impact of educational policies directed to immigrant children's access to preschool (Currie and Thomas, 1999; Felfe and Huber, 2017; Cornelissen et al., 2018) or offering immigrant students a tutoring program (Carlana, La Ferrara, and Pinotti, 2022). The closer paper to ours is Pesola and Sarvimäki (2022). They analyze the intergenerational spillover effects of a reform in Finland restructuring active labor market policy for unemployed immigrants towards more language training and less search and employment support. The reform was implemented in 1999 for all immigrants who registered in Finland on or after May 1, 1997, generating a discontinuity and quasi-random assignment to treatment around this date. By linking children's education outcomes to the mean earnings of individuals with that education, they find that the reform improved the educational earnings potential and probability to be either employed, studying or in some training program for children whose parents were treated by the reform.

II The 1999 Reform

The reform we study was implemented starting in 1999 and applied to individuals who were granted refugee status on or after January 1, 1999. Those who were granted refugee status before this date were not treated by the reform. Once granted refugee status (the date of admission), 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.⁵ The potential instruction time was increased by 31 percent (from 1,370 to 1,800 hours) and the course period was increased from 18 to 36 months. Centralized learning goals, national tests and funding to improve teachers' qualifications were launched at the same date threshold.

⁵The reform was based on legislative changes 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 sprogcetre", <https://www.retsinformation.dk/Forms/R0710.aspx?id=87625>).

Three smaller additional policy changes were incorporated in the 1999 reform, and affected the same cohorts of refugees. First, a temporary reduction in welfare benefits for refugees, limited to those over age 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 temporary increase in property crime, the fact that the earnings and employment of treated refugees were not affected during the initial years, but grew gradually in the following years, suggest that language training rather than the initial welfare cut generated the long-run effects found by (Foged et al., 2022). The second change was the intention of dispersing refugees more across municipalities and in a more mechanical way.⁶ This intention did not affect allocation of refugees across municipalities within the first year after the reform (Foged et al., 2022), which is the period we use to define treated individuals, likely due to inertia and capacity constraints in finding suitable housing. 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, Damm, and Schultz-Nielsen, 2020; Foged et al., 2022).⁷ Since mobility is mainly delayed from rural areas, and employment opportunities are often poorer in these areas, it means that the positive labor market effects on the treated adult refugees could be downward biased.

III Data

Our sample includes children born in the first three years after the reform or before arrival but still in pre-school age or primary school age at the time of arrival. We observe 6,467 children who are born between 1990 to 2001 and have at least one parent who was granted asylum in Denmark within a four year window around January 1, 1999.⁸ The restrictions mean that all considered children attended lower secondary (compulsory) school in Denmark and we observe all of them up to age 18 (included). Hence, we follow the children up to 2019 when the youngest among them are 18 years old.

The outcomes we consider are completing and taking the exams at the end of lower secondary

⁶The 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>.

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

⁸Hence, the parents are a sub-sample of the individual studied in Foged et al. (2022). All parents are born between 1948 and 1982.

education⁹ at age 18 or younger as well as criminal convictions between 15 and 18 years of age. Natives typically complete lower secondary school at age 16, while refugee children, whose schooling career is often interrupted and delayed, typically take a few more years. We split criminal convictions by the three main types of crime committed in our sample; namely property, violent and narcotics crime.

To replicate the main findings of Foged et al. (2022) for the parents in our sample we consider: The annual gross earnings, measured in year-2000 prices and converted to USD using an exchange rate of 6.6 DKK/USD.¹⁰ An indicator for working in a complex job, defined as jobs in occupations from managers (*ISCO 1*) to services and sales workers (*ISCO 5*), which require some level of schooling and technical knowledge or literacy and good communication skills. Finally, we report the impact of the reform on disposable income (deflated to USD in year-2000) and criminal convictions of parents. We add information on marriage and fertility to evaluate the impact of the reform on the probability (a dummy) for marrying a Danish citizen and the total number of children born after admission.

III.A Summary Statistics

Table 1 shows the summary statistics for the refugee sample (column 1) and for native born (column 2). The comparison group of native born parents is chosen within the same birth cohorts as adult refugees (shown in Panel a). Similarly, native children are from the same birth cohorts as the children of the refugees (shown in Panel b). The average annual earnings of refugees over 18 years from 1999 to 2016 is only 8 thousand USD compared to 33 thousand USD for natives. The employment rate measured in full-time equivalents is 0.19 for refugees and 0.67 for native born. These averages imply a substantial fraction of non-employed among the refugees, and a large gap relative to natives in terms of both employment and earnings. Most natives (85 percent) have some employment during a year (the sum of the fraction in complex and non-complex jobs in the table), while this is only the case for 30 percent of the refugees. The distribution across jobs is also different between refugees and natives, with the first group concentrated in non-complex jobs and the majority of natives working in complex jobs.

Disposable income includes public income transfers and subtracts taxes and shows a narrower income gap between refugees and natives. Still, refugees have substantially lower disposable income than natives. Refugees are more likely to be convicted for a crime. The average crime rate from 1999 to 2016 is two percent for refugees and one percent for natives. In terms of demographic variables

⁹It 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 high school, completion without examination likely worsens the possibilities of continuing into further education.

¹⁰The descriptive statistics include employment measured in full-time equivalents (Table 1).

refugee parents have 3.7 children on average while native born parents have only 1.8 children on average (conditional on having children). Very few refugee parents are married to a Danish citizen, which is unsurprising given that the overwhelming majority of families were formed prior to admission (86 percent are married at arrival, Table 2). Almost half of Danish citizens are married to another Dane, which is close to the marriage rate in Denmark.

Panel b shows the summary statistics for the variables that we consider as outcomes for the children, comparing children of refugees and children of natives. Panels c and d show those statistics separately by gender. 92 percent of the children of refugees completed lower secondary school, and 86 percent took the final exam for lower secondary school before or at 18 years of age. The corresponding numbers for natives are 96 and 92 percent. The last set of outcomes we analyze for children are criminal convictions. First an indicator for being convicted of a crime between age 15 and 18, and then similar indicators for each of the three main types of crime. Property crime is the most common conviction, with 8 percent of the children of refugees and 3 percent of the children of natives convicted of such a crime between age 15 and 18. 4 percent of the refugee children are convicted of violent crimes and 2 percent of narcotics crime before turning 19. Among children of native born, these numbers are both 1 percent. Notice, that boys (Panel c) are 3 to 4 times more likely than girls (Panel d) 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 of age. Boys are also more likely than girls to not attend the exams at the end of lower secondary education. Hence, refugee children, especially the boys, are at high risk of entering a criminal career and of not completing compulsory (lower secondary) schooling with the exams that are crucial for continuing education.

IV Empirical Design and Identification

IV.A Empirical Specification

The admission date of the adult refugee 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 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 ($D_i = 1\{x_i \geq c\}$). The estimation equation is as follows:

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

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.¹¹ Therefore, in this specification, the intercept α captures the pre-reform mean at the boundary point, which we use when re-scaling the estimated coefficients into percentage effects.

We estimate equation 1 with Weighted Least Squares using the mean-squared error optimal bandwidth from Calonico et al. (2019) and a triangular kernel to give more weight to the observations closer to the cutoff. 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 with treated parent(s) with children of households with no treated parent(s). The remaining part of the sample includes in the control group households where one parent (the later arrival) could potentially be treated.¹² A specification using the admission of the father as running variable produces similar results (shown in the Online Appendix Table A.2), because the mother rarely (3 percent) arrives before the father.¹³

IV.B Balancing Tests Around the Threshold

The summary statistics in Table 2 provide a picture of the main characteristics of the adult refugees (Panel a) and their children (Panel b). The average age of parents is 32. Most of them are married and arrive with children. The majority originated from either Iraq (47 percent) or Afghanistan (18 percent) and spoke Arabic or Dari (a dialect of Farsi spoken in Afghanistan).¹⁴ We show assignment to language

¹¹The recent consensus, exemplified in Gelman and Imbens (2019), is in favor of the local linear specification. We analyze alternative specifications estimating second order and zero order local polynomials $((x_i - c)^2)$ in the running variable) on either side of the cutoff as well as a specification with controls in the robustness checks (Table 3).

¹²For instance, 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.

¹³We 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. More importantly, fathers could be important role models; especially, for the boys, who are less likely to participate in exams at the end of primary school and 3-4 times more likely to commit juvenile crime than girls, see Table 1. Hence, we think that most reasonable division between treatment and control group is one that use the first parent as the running variable.

¹⁴Data on the mother tongue as well as the assigned language training track comes from the language training centers.

training track as this is an indicator of literacy, skills and aspirations at arrival. 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 (17 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 no evidence of any significant differences in characteristics of the sample on the two sides of the admission threshold of January 1, 1999. Column 3 shows the point estimate of the discontinuity in a specific characteristic at the threshold and column 4 shows the 95% confidence interval. For both adults and children, the discontinuity estimate is very small relative to the mean and the confidence interval always contain zero. This is consistent with the validity of our strategy in assessing the impact of the reform, as all measurable characteristics are distributed between treatment and control (pre-post January 1 admission) in a way that is indistinguishable from random.¹⁵

V Results

V.A Parents

Figure 1 show the dynamics of the causal effects of the reform from year 1 to year 18 after admission. The values of the estimates for year 18 can be found in Online Appendix Table A.1. Panels a and b reproduce the main results from Foged et al. (2022) and show that the reform improved treated refugees' long-run labor market outcomes. Average annual earnings (Panel a) increased by USD 2,100. This is an economically large effect equal to 33 percent of the baseline (USD 6,317). The point estimates are not statistically significant but they are otherwise similar to Foged et al. (2022), who achieve higher precision as they use a larger sample (8,558 adults compared to the 5,197 adult parents analyzed here). The share of parents working in a complex job (Panel b) grew by 8 percentage points, significant at the five percent level (compared to a highly significant ($p < 0.01$) point estimate of 0.092 in Foged et al., 2022). Online Appendix Table A.1 shows that the positive effect of the reform on earnings is entirely driven by parents whose native language does not use the Latin alphabet. The effect is large and statistically significant for this group (consistent with Foged et al., 2022), suggesting that they benefit

¹⁵Online Appendix Figure 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. (2022).

more from having more hours of class to learn to read and write in Danish, because they often need to first learn the Latin alphabet.

The parents were also hit by a 13 months cut in welfare benefits. Foged et al. (2022) show that this aspect of the reform led to a one year reduction in disposable income and a one year increase in criminal charges. This was driven by shoplifting (a crime associated with material needs) and the effect on charges disappeared in year 2, when the pre-reform transfers were resumed. Panels c and d of Figure 1 show the entire dynamics of disposable income and total criminal convictions as these outcomes are important indicators of the environment in which the children grew up. Disposable income dropped by 3,200 USD in the first year for the treated group, but it was almost back to the level of the control group in year 2 when the welfare cut was reversed. The impacts on criminal convictions are the mirror images of the impacts on disposable income, suggesting that the shoplifting crime might be occasional and related to a very low level of income making it hard for refugees to pay for their living expenses (see also Andersen, Dustmann, and Landersø, 2019). Notice also that the profile of labor market outcomes are not consistent with economic incentives pushing refugees into employment in year 1 (due to lower income), as we do not see significantly higher employment of the treated group starting in year 1, but rather we see a gradual effect consistent with the acquisition of language skills.

Greater assimilation into the Danish labor market may encourage assimilation to social and cultural norms of the host society. This could modify preferences towards marrying outside of the ethnic group or having fewer children, as average fertility of native Danish women was significantly lower than that in the countries of origin of refugees.

We find, however, no effect on probability of marrying a Danish native (Panel e). We also checked the probability of marrying outside one's ethnic group (not shown) and found no indication that marriage outcomes have been affected by the reform. We find no effect on the total number of children born after admission (Panel f) nor on the time to first child birth after admission (not shown). Hence, marriage outcomes and the size and composition of families of the refugees did not change in a significant way because of the reform (Table 2). One potential explanation is that most families in our sample were formed before admission to Denmark and preferences towards marriage and fertility is less amendable to changes in adulthood.¹⁶

¹⁶86 percent were able to document their marriage and they had more than one child on average at arrival (Table 1).

V.B Children

Table 3 shows the impact of the reform on the outcomes of refugees' children when they are 15 to 18 years old. We first show our main specification in Panel a and then investigate the robustness to alternative specifications in Panels b to d.¹⁷

Treated children perform better in lower secondary education. We measure this in two 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 school (column 2). Passing such exams is the typical path for continuing an academic track. However, it is possible to be granted exemption. Continuing it is still possible but subject to enrollment tests at upper secondary school. 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 exam is larger in magnitude and statistically more significant. The latter estimates suggest that roughly 12 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 c) and inclusion of controls (Panel d). The estimates are smaller in Panel b, which shows simple comparisons of children in families admitted very close to the cutoff. The narrow bandwidth increases the share of households in the control group were the second parent is treated. This could lead to downward bias compared to the ideal scenario were fully treated households would be compared only to fully untreated households.

The second part of Table 3 (Columns 3 to 6) shows the effects on criminal convictions of refugees' children (between 15 and 18 years of age). We see that some effects, especially on property and violent crime are large in magnitude (relative to the mean) albeit only marginally significant. These average effects hide substantial gender and cohort heterogeneity that we explore in Table 4.

It is usually the case, and certainly in our sample of refugees' children (see Table 1) that males have lower schooling attainments (lower probability of taking the final exam in lower secondary school) and higher tendency to commit juvenile crime than females. Research shows that disadvantaged family backgrounds, especially the lack of a male model, may be more detrimental for boys (Bertrand and Pan, 2013; Chetty et al., 2016; Autor et al., 2019; Andersen, Dustmann, and Landersø, 2019). In this context it is particularly significant and very interesting, as shown in Panel a of Table 4, that boys benefit more than girls from having the first parent (likely two parents) treated by the reform, compared to no

¹⁷Online Appendix Figures A.2 to A.4 show plots of the main outcomes by one-month bins of the running variable, robustness to alternative bandwidth choices, and RD estimates at placebo cutoffs.

treatment in the control group or maybe the last parent (typically the mother) treated. The impact on exam attendance for boys is rather uniform across cohorts and very significant. 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 final exams for compulsory schooling if their parents had access to expanded language training. The increase is large in magnitude, between 17 and 20 percentage points. Moreover, this is accompanied by a very large reduction in crime for the boys whose parents were treated when they were very young and mostly grew up in Denmark. The negative relationship between schooling and crime outcomes seems consistent with the very strong incapacitation effect that schooling in adolescence has on crime (see Bell, Costa, and Machin, 2022). The positive effects in lower secondary school and the negative effects on crime could be due to both better language skills and better social integration of the treated households and also due to improved economic conditions for the families when the children are teenagers. The economic effects on the parents (Figure 1) have not fully materialized when the early cohorts are teenagers. This could be part of the explanation for why we found more pronounced improvements for the younger boys across schooling and crime outcomes.¹⁸

We also find some evidence of larger school attainment and crime reducing effects for children in households that likely speak a language not using the Latin alphabet, 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 higher integration and better labor market potentials (Online Appendix Table A.1). The differences across Latin/non-Latin Alphabet of parents' mother tongue is not large, but the improvements in schooling and crime is larger and more precisely estimated for the non-Latin group, showing a more clear tendency towards a better track for these kids.

VI Conclusion

This paper is one of the very few and early studies analyzing 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., 2022). In this paper, by linking refugees admitted around the threshold

¹⁸Foged et al. (2022) confirm that the impact of expanded language training on earnings is gradual and does not fully materialize before 8-10 years after admission, where the effect starts to be statistically significant.

date to their children, we investigate if the reform produced spillover effects on their 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 exams at the end of it. By exploring the heterogeneity of the effects, we see stronger positive educational effects on boys, and a clear crime reduction effects on boys, who were very young at the time of the reform. These are the children who were still in lower secondary school when the economic conditions of the adult refugees were improved by the reform. We also see indications of stronger effects for refugee children from families speaking languages very different from Danish. This suggests that the positive spillover effects were stronger for children, whose parents originated from different cultures and whose native language is further from Danish. While the improved schooling effect is also present on children who were already of school-age when the parents were treated, the negative effect on criminal convictions is only present for children who were in pre-schooling ages at the time of reform. 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 of lowering early criminal behavior.¹⁹ 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.

¹⁹The temporary (one-year) welfare reduction, associated with temporarily higher property crime of refugees at the time of reform may have attenuated the positive effect of human capital in the long-run on crime reduction of older children. They may have experienced the property crimes in the family when already in school age. In this sense the mostly language-training-driven crime-reduction effects could be under-estimated.

References

- Abramitzky, Ran, Leah Boustan, Elisa Jacome, and Santiago Perez. 2021. “Intergenerational Mobility of Immigrants in the United States over Two Centuries.” *American Economic Review* 111 (2):580–608. URL <https://www.aeaweb.org/articles?id=10.1257/aer.20191586>.
- Andersen, Lars Højsgaard, Christian Dustmann, and Rasmus Landersø. 2019. “Lowering Welfare Benefits: Intended and Unintended Consequences for Migrants and their Families.” *CReAM Discussion Paper* 05/19.
- Arendt, Jacob Nielsen, Christian Dustmann, and Hyejin Ku. 2022. “Refugee Migration and the Labor Market: Lessons from 40 Years of Post-arrival Policies in Denmark.” *CReAM Discussion Paper* 09/22.
- Autor, David, David Figlio, Krzysztof Karbownik, Jeffrey Roth, and Melanie Wasserman. 2019. “Family Disadvantage and the Gender Gap in Behavioral and Educational Outcomes.” *American Economic Journal: Applied Economics* 11 (3):338–381.
- Azlor, Luz, Anna Piil Damm, and Marie Louise Schultz-Nielsen. 2020. “Local Labor Demand and Immigrant Employment.” *Labour Economics* 63 (101808).
- Bell, Brian, Rui Costa, and Stephen Machin. 2022. “Why Does Education Reduce Crime?” *Journal of Political Economy* 130 (3):732–765.
- Bell, Brian, Francesco Fasani, and Stephen Machin. 2013. “Crime and Immigration: Evidence from Large Immigrant Waves.” *The Review of Economics and Statistics* 95 (4):1278–1290. URL https://doi.org/10.1162/REST_a_00337.
- Bertrand, Marianne and Jessica Pan. 2013. “The Trouble with Boys: Social Influences and the Gender Gap in Disruptive Behavior.” *American Economic Journal: Applied Economics* 5 (1):32–64.
- Björklund, Anders and Kjell G Salvanes. 2011. *Education and family background: Mechanisms and policies*, vol. 3. Elsevier, 201–247.
- Black, Sandra E and Paul J Devereux. 2011. *Recent developments in intergenerational mobility, Handbook of Labor Economics*, vol. 4B, chap. 16. Elsevier.
- Black, Sandra E, Paul J Devereux, and Kjell G Salvanes. 2005. “Why the apple doesn’t fall far: Understanding intergenerational transmission of human capital.” *American economic review* 95 (1):437–449.
- Borjas, George J. 1992. “Ethnic Capital and Intergenerational Mobility.” *The Quarterly Journal of Economics* 107 (1):123–150. URL <http://www.jstor.org/stable/2118325>.
- Brell, Courtney, Christian Dustmann, and Ian Preston. 2020. “The Labor Market Integration of Refugee Migrants in High-Income Countries.” *Journal of Economic Perspectives* 34 (1):94–121.
- Calonico, Sebastian, Matias D. Cattaneo, Max H. Farrell, and Roco Titiunik. 2019. “Regression Discontinuity Designs Using Covariates.” *The Review of Economics and Statistics* 101 (3):442–451.
- Carlana, Michela, Eliana La Ferrara, and Paolo Pinotti. 2022. “Goals and Gaps: Educational Careers of Immigrant Children.” *Econometrica* 90 (1):1–29.
- Chetty, Raj, John N Friedman, Emmanuel Saez, Nicholas Turner, and Danny Yagan. 2020. “Income Segregation and Intergenerational Mobility Across Colleges in the United States*.” *The Quarterly Journal of Economics* 135 (3):1567–1633. URL <https://doi.org/10.1093/qje/qjaa005>.

- Chetty, Raj, Nathaniel Hendren, Frina Lin, Jeremy Maherovitz, and Benjamin Scuderi. 2016. “Childhood Environment and Gender Gaps in Adulthood.” *American Economic Review: Papers & Proceedings* 106 (5):282–288.
- Cornelissen, Thomas, Christian Dustmann, Anna Raute, and Uta Schönberg. 2018. “Who Benefits from Universal Child Care? Estimating Marginal Returns to Early Child Care Attendance.” *Journal of Political Economy* 126 (6):2356–2409.
- Couttenier, Mathieu, Veronica Petrencu, Dominic Rohner, and Mathias Thoenig. 2019. “The Violent Legacy of Conflict: Evidence on Asylum Seekers, Crime, and Public Policy in Switzerland.” *American Economic Review* 109 (12):4378–4425.
- Cunha, Flavio and James J Heckman. 2008. “Formulating, Identifying and Estimating the Technology of Cognitive and Noncognitive Skill Formation.” *Journal of Human Resources* 43 (4):738–782.
- Cunha, Flavio, James J Heckman, and Susanne M Schennach. 2010. “Estimating the technology of cognitive and noncognitive skill formation.” *Econometrica* 78 (3):883–931.
- Currie, Janet and Duncan Thomas. 1999. “Does Head Start Help Hispanic Children?” *Journal of Public Economics* 74 (2):235–262.
- Damm, Anna Piil and Christian Dustmann. 2014. “Does Growing Up in a High Crime Neighborhood Affect Youth Criminal Behavior?” *American Economic Review* 104 (6):1806–32.
- Dustmann, Christian, Tommaso Frattini, and Gianandrea Lanzara. 2012. “Educational Achievement of Second-Generation Immigrants: An International Comparison.” *Economic Policy* 27 (69):143–185.
- Felfe, Christina and Martin Huber. 2017. “Does Preschool Boost the Development of Minority Children?: The Case of Roma Children.” *Journal of the Royal Statistical Society: Series A (Statistics in Society)* 180 (2):475–502.
- Foged, Mette, Linea Hasager, and Giovanni Peri. 2022. “Comparing the Effects of Policies for the Labor Market Integration of Refugees.” *Prepared for Special Issue of Journal of Labor Economics* .
- Foged, Mette, Linea Hasager, Giovanni Peri, Jacob Nielsen Arendt, and Iben Bolvig. 2022. “Language Training and Refugees’ Integration.” *The Review of Economics and Statistics (accepted)* .
- Gelman, Andrew and Guido Imbens. 2019. “Why High-Order Polynomials Should Not Be Used in Regression Discontinuity Designs.” *Journal of Business & Economic Statistics* 37 (3):447–456.
- Lee, David S. and Thomas Lemieux. 2010. “Regression Discontinuity Designs in Economics.” *Journal of Economic Literature* 48 (2):281–355.
- Nielsen, Chantal Pohl and Kræn Blume Jensen. 2006. “Integrationslovens betydning for flygtninges bosætning.” *AKF report* .
- Pesola, Hanna and Matti Sarvimäki. 2022. “Intergenerational Spillovers of Integration Policies: Evidence from Finland’s Integration Plans.” *CReAM Discussion Paper 12/22* .
- Piopiunik, Marc and Jens Ruhose. 2017. “Immigration, Regional Conditions, and Crime: Evidence from an Allocation Policy in Germany.” *European Economic Review* 92:258–282.

Tables and Figures

Table 1: Means of Outcomes and Comparison with Natives

	Refugees (1)	Natives (2)
<i>Panel a. Adults</i>		
Earnings	7.79	33.24
Employment	0.19	0.67
Complex Job	0.12	0.54
Non-Complex Job	0.18	0.31
Disposable Income	17.87	27.47
Criminal Convictions	0.02	0.01
Married to a Dane	0.01	0.48
Number of Children	3.70	1.76
<i>Panel b. All Children</i>		
Completed Lower Secondary School	0.92	0.96
Took Final Exam in Lower Secondary School	0.86	0.92
Convicted of Crime	0.12	0.05
Convicted of Property Crime	0.08	0.03
Convicted of Violent Crime	0.04	0.01
Convicted of Narcotics Crime	0.02	0.01
<i>Panel c. Boys</i>		
Completed Lower Secondary School	0.92	0.96
Took Final Exam in Lower Secondary School	0.84	0.91
Convicted of Crime	0.18	0.06
Convicted of Property Crime	0.12	0.04
Convicted of Violent Crime	0.06	0.01
Convicted of Narcotics Crime	0.03	0.02
<i>Panel d. Girls</i>		
Completed Lower Secondary School	0.92	0.96
Took Final Exam in Lower Secondary School	0.88	0.93
Convicted of Crime	0.05	0.02
Convicted of Property Crime	0.04	0.02
Convicted of Violent Crime	0.01	0.00
Convicted of Narcotics Crime	0.00	0.00

Notes: Panel a shows the outcomes for the adult refugees and native born of the same age (birth cohorts 1948-1982). Labor market outcomes and married to a Dane are sample means from 1999 to 2016 while number of children is measured in 2016. Disposal income includes public transfers and is after tax. Panels b, c and d show the means of outcomes for their children (birth cohorts 1990-2001) measured when they are 15 to 18 years old.

Table 2: Summary Statistics and Balancing Tests

	Mean (1)	S.D. (2)	RD Estimate (3)	Confidence Interval (4)
<i>Panel a. Adults</i>				
Age	32.25	6.87	-0.19	[-1.63 ; 1.24]
Married	0.86	0.35	0.10	[0.02 ; 0.19]
Female	0.49	0.50	0.03	[-0.07 ; 0.13]
No. Children < 3y	0.30	0.52	0.04	[-0.07 ; 0.14]
No. Children 3-17y	1.42	1.68	0.09	[-0.35 ; 0.54]
Iraq	0.47	0.50	0.07	[-0.07 ; 0.20]
Afghanistan	0.18	0.38	-0.01	[-0.10 ; 0.07]
Other Country	0.35	0.48	-0.06	[-0.21 ; 0.09]
Speaks Arabic	0.38	0.49	0.01	[-0.10 ; 0.11]
Speaks Dari	0.10	0.30	-0.01	[-0.06 ; 0.04]
Danish 1	0.26	0.44	-0.07	[-0.21 ; 0.06]
Danish 2	0.37	0.48	-0.00	[-0.12 ; 0.12]
Danish 3	0.29	0.45	0.04	[-0.08 ; 0.17]
Quota Refugee	0.10	0.31	-0.01	[-0.06 ; 0.03]
Convention Refugee	0.17	0.38	-0.12	[-0.24 ; -0.00]
Family-Reunified	0.26	0.44	0.04	[-0.09 ; 0.18]
Other Refugee	0.46	0.50	0.03	[-0.11 ; 0.18]
<i>Panel b. Children</i>				
Female	0.47	0.50	0.02	[-0.09 ; 0.14]
Born Before 1999	0.75	0.43	-0.03	[-0.13 ; 0.08]
Iraq	0.44	0.50	-0.07	[-0.20 ; 0.06]
Afghanistan	0.23	0.42	-0.08	[-0.18 ; 0.03]
Other Country	0.33	0.47	-0.01	[-0.14 ; 0.11]

Notes: Summary statistics (columns 1-2) and balancing tests (columns 3-4) of the impact of the reform on predetermined characteristics of adult refugees obtaining refugee status in Denmark between January 1997 and December 2000 (Panel a) and characteristics of their children (Panel b). The 95-percent confidence intervals are constructed based on the robust standard errors (column 4). The RD estimates are from local linear regressions of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Age, marital status and the number of children are measured at date of immigration. Danish 1 to 3 refer to the language track the individual 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 RD estimate in Panel b is based on admission of the first parent. The number of observations in Panel a is 5,197 and 6,467 in Panel b.

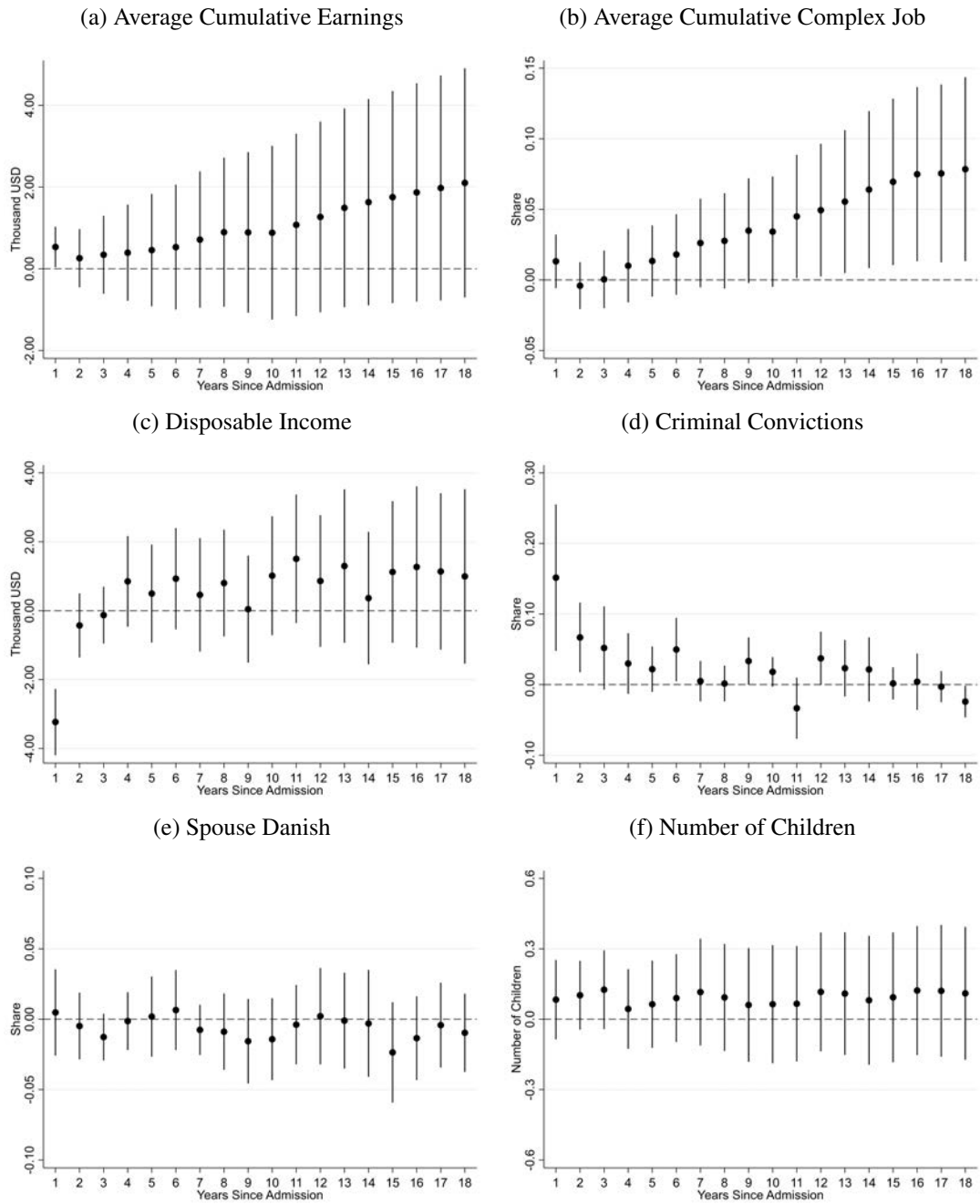


Figure 1: The Impact of the Reform on the Parents

Notes: The graphs shows RD estimates and 95-percent confidence intervals based on robust standard errors from a local linear estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019).

Table 3: The Impact of the Reform on the Children

	Lower Secondary School		Criminal Convictions			
	Completed (1)	Took Final Exam (2)	Total (3)	Property Crime (4)	Violent Crime (5)	Narcotics Crime (6)
<i>Panel a. Main (Linear)</i>						
RD Estimate	0.082** (0.038)	0.120*** (0.046)	-0.074 (0.047)	-0.072* (0.040)	-0.038* (0.020)	-0.025 (0.018)
Mean of Untreated at Cutoff	0.878	0.816	0.156	0.129	0.054	0.041
Bandwidth	142	144	147	156	202	182
<i>Panel b. Treatment-Control Difference in Means</i>						
Estimate	0.049* (0.029)	0.055* (0.033)	-0.017 (0.024)	-0.048* (0.027)	-0.013 (0.015)	-0.009 (0.013)
Mean of Untreated	0.894	0.845	0.135	0.104	0.045	0.032
Bandwidth	51	61	105	57	94	74
<i>Panel c. 2nd Order</i>						
RD Estimate	0.073* (0.042)	0.114** (0.049)	-0.072 (0.049)	-0.070 (0.043)	-0.077** (0.030)	-0.054* (0.031)
Mean of Untreated at Cutoff	0.869	0.812	0.170	0.136	0.069	0.059
Bandwidth	257	280	290	297	242	198
<i>Panel d. Controls</i>						
RD Estimate	0.085** (0.036)	0.119*** (0.043)	-0.053 (0.038)	-0.056 (0.034)	-0.024 (0.019)	-0.017 (0.014)
Mean of Untreated at Cutoff	0.877	0.817	0.154	0.122	0.050	0.031
Bandwidth	151	157	195	188	226	265
N	6,467	6,467	6,467	6,467	6,467	6,467

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). We use the admission date of the first parent as the running variable. The outcomes measuring taking the final exam in lower secondary school or completing are dummies for taking the exam or completing before age 18. The crime outcomes are dummies for being convicted of a crime at age 15-18.

Table 4: Heterogeneous Effects on the Children

	Lower Secondary School		Criminal Convictions			
	Completed (1)	Took Final Exam (2)	Total (3)	Property Crime (4)	Violent Crime (5)	Narcotics Crime (6)
<i>Panel a. Gender and Birth Cohort</i>						
Boys Born 1990-1995	0.116 (0.079)	0.199** (0.089)	-0.002 (0.117)	-0.082 (0.102)	-0.008 (0.043)	-0.050 (0.037)
Mean of Untreated at Cutoff	0.831	0.716	0.196	0.202	0.046	0.064
Boys Born 1996-2001	0.072 (0.069)	0.173** (0.088)	-0.206** (0.084)	-0.126* (0.066)	-0.107** (0.050)	-0.031 (0.048)
Mean of Untreated at Cutoff	0.879	0.762	0.240	0.148	0.130	0.070
Girls Born 1990-1995	0.037 (0.090)	-0.006 (0.089)	-0.066 (0.060)	-0.057 (0.058)	-0.008 (0.010)	0.000 (.)
Mean of Untreated at Cutoff	0.913	0.956	0.069	0.068	0.003	0.000
Girls Born 1996-2001	0.102 (0.082)	0.072 (0.096)	-0.009 (0.069)	-0.013 (0.065)	-0.008 (0.009)	-0.007 (0.007)
Mean of Untreated at Cutoff	0.899	0.888	0.062	0.065	0.008	0.007
<i>Panel b. Parent's Alphabet of Mother Tongue</i>						
Latin Alphabet	0.089 (0.063)	0.100 (0.072)	-0.066 (0.075)	-0.053 (0.067)	-0.029 (0.032)	-0.020 (0.021)
Mean of Untreated at Cutoff	0.888	0.860	0.163	0.149	0.051	0.027
Other Alphabet Than Latin	0.086 (0.053)	0.131** (0.065)	-0.090 (0.062)	-0.115** (0.050)	-0.038 (0.026)	-0.020 (0.027)
Mean of Untreated at Cutoff	0.854	0.780	0.158	0.131	0.050	0.043

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). 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 samples sizes are 1,534 (Latin alphabet), 4,933 (other alphabet), 1,890 (urban municipality), 4,577 (rural municipality), 1,692 (born 1990-1992), 4,775 (born 1993-2001), 3,955 (girls), 3,412 (boys). We use the admission date of the first parent as the running variable. The outcomes measuring taking the final exam in lower secondary school or completing are dummies for taking the exam or completing before age 18. The crime outcomes are dummies for being convicted of a crime at age 15-18.

A Online Appendix

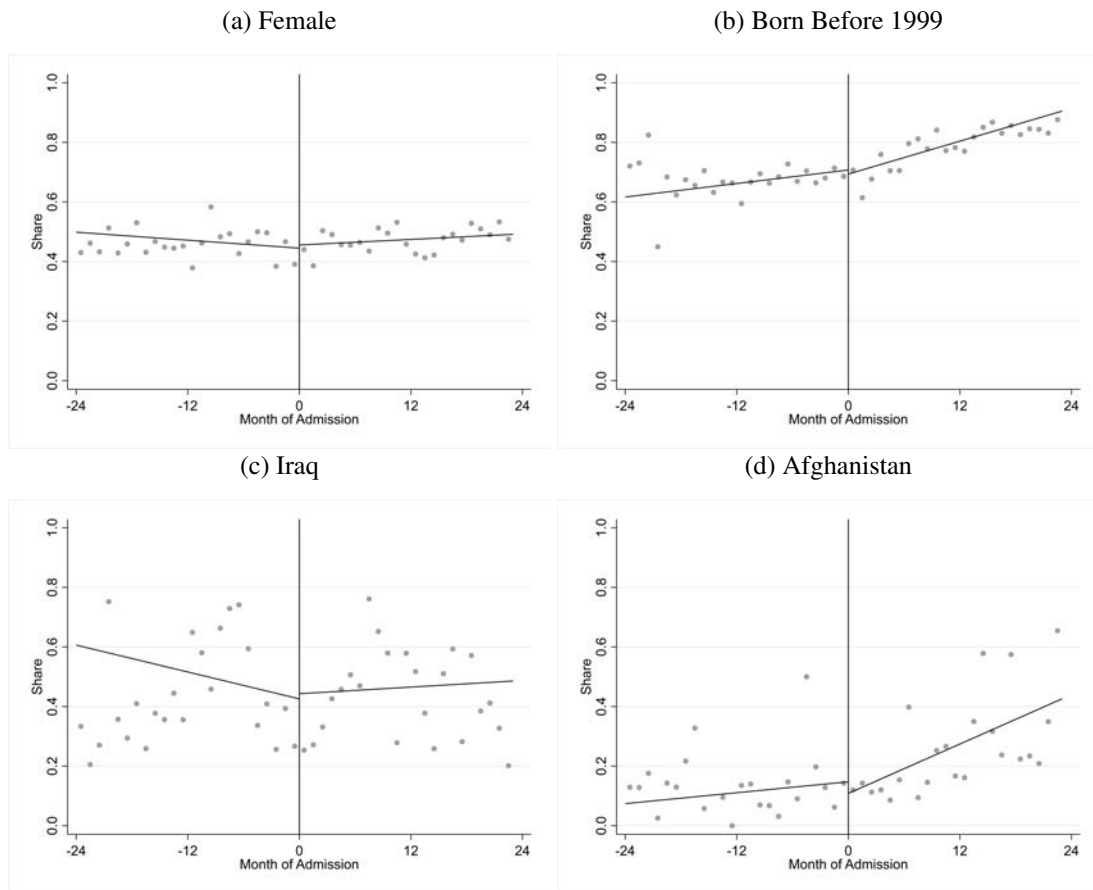


Figure A.1: Predetermined Covariates by Month of Admission of the First Parent

Notes: Each panel shows a predetermined covariate from Table 2. The dots correspond to sample means by one-month bins. Bins with less than 5 observations are excluded.

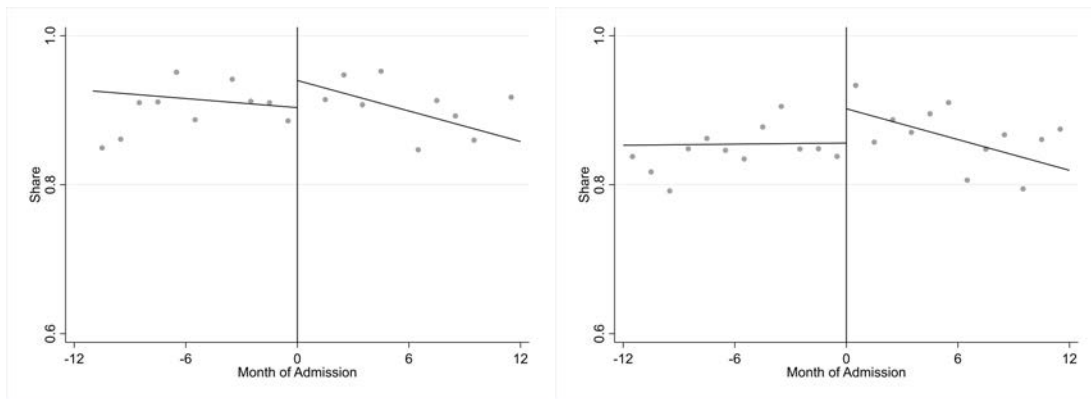
Table A.1: The Impact of the Reform on Parents

	Earnings (1)	Complex Job (2)	Disposable Income (3)	Spouse Danish (4)	Number of Children (5)	Convicted (6)
<i>Panel a. All Parents</i>						
RD Estimate	2.103 (1.431)	0.078** (0.033)	0.996 (1.293)	-0.010 (0.014)	0.110 (0.144)	-0.024** (0.011)
Mean of Untreated at Cutoff	6.317	0.097	17.714	0.022	1.423	0.022
Bandwidth	202	163	188	181	228	176
N	5,197	5,197	5,197	5,197	5,197	5,197
<i>Panel b. Alphabet of Mother Tongue</i>						
Latin Alphabet	-1.546 (1.844)	0.106** (0.050)	0.693 (1.492)	-0.012 (0.017)	-0.099 (0.214)	-0.064* (0.035)
Mean of Untreated at Cutoff	8.272	0.075	17.443	0.017	1.484	0.059
Other Alphabet Than Latin	4.866** (2.004)	0.069 (0.046)	1.657 (1.953)	-0.002 (0.021)	0.223 (0.195)	-0.003 (0.003)
Mean of Untreated at Cutoff	4.820	0.102	17.540	0.022	1.418	0.002

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Table entries are the RD estimates ($\hat{\tau}$), robust standard errors in parentheses, the mean of the outcome for the untreated group measured at the cutoff ($\hat{\alpha}$), and the bandwidth from estimation of model (1) using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panel a, b and c show the main specification (linear), the simple difference in means between treated and control (zero polynomials and no kernel weighting), and second order polynomials. Panel d includes control variables. 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). The outcomes (shown in the columns) are the average of yearly outcomes over 18 years in columns 1-5. Column 6 provides the impact on the number of children born after admission until year 18, and column 7 shows the year of the first-born child post admission (measured in years after admission).

(a) Completed Lower Secondary School

(b) Took Final Exam in Lower Secondary School



(c) Convicted of Crime

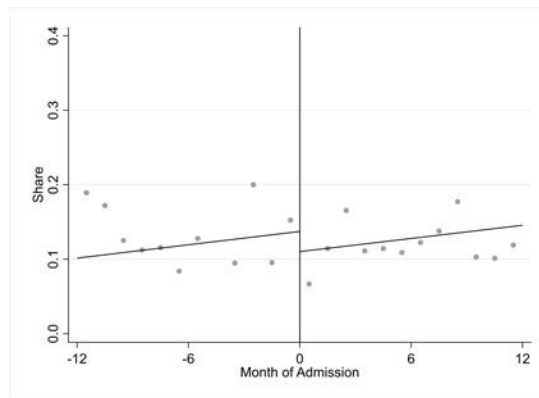
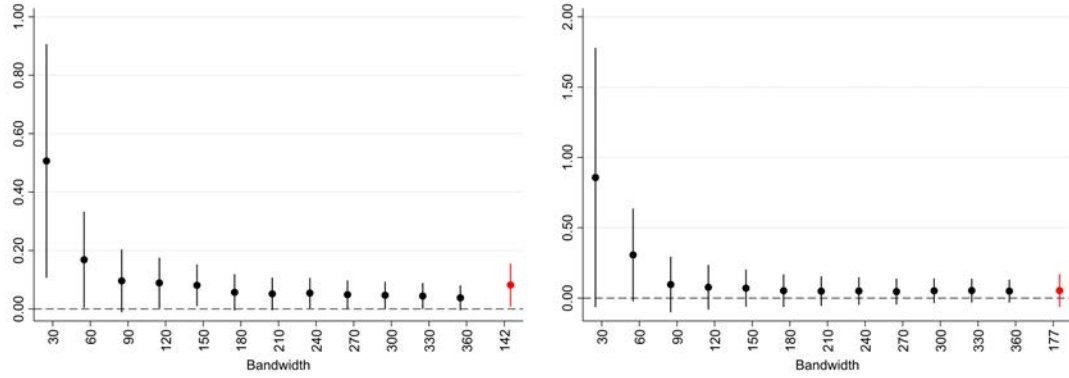


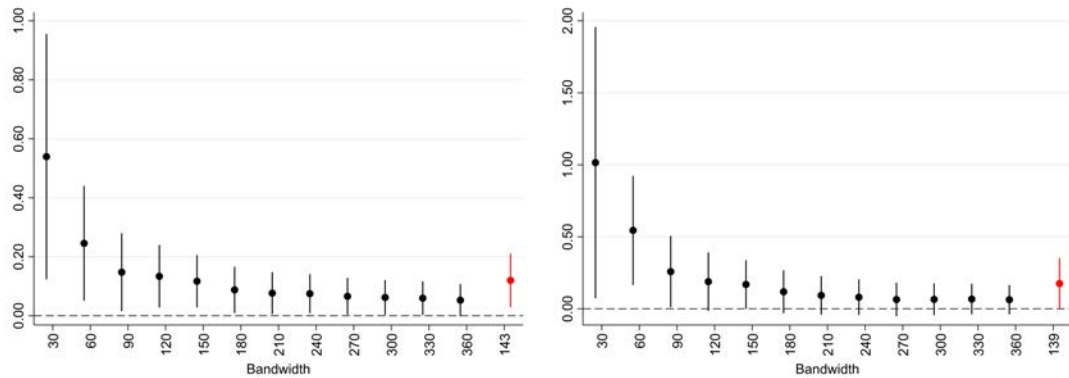
Figure A.2: Long-Run Integration Measures by Month of Admission

Notes: The graphs show sample means of education and crime outcomes.

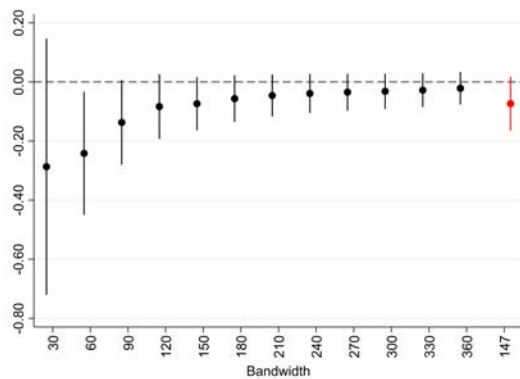
(a) Completed Lower Secondary School, All Children (b) Completed Lower Secondary School, Boys Born 1996-2001



(c) Took Final Exam in Lower Secondary School, All Children (d) Took Final Exam in Lower Secondary School, Boys Born 1996-2001



(e) Convicted of Crime, All Children



(f) Convicted of Crime, Boys Born 1996-2001

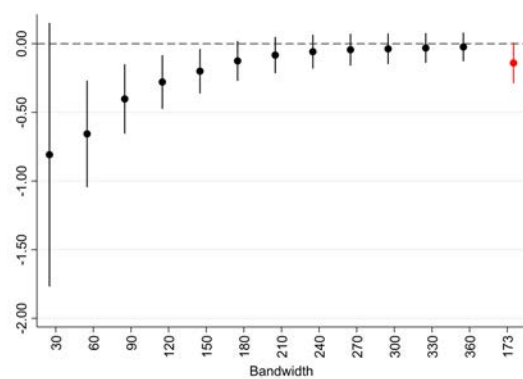
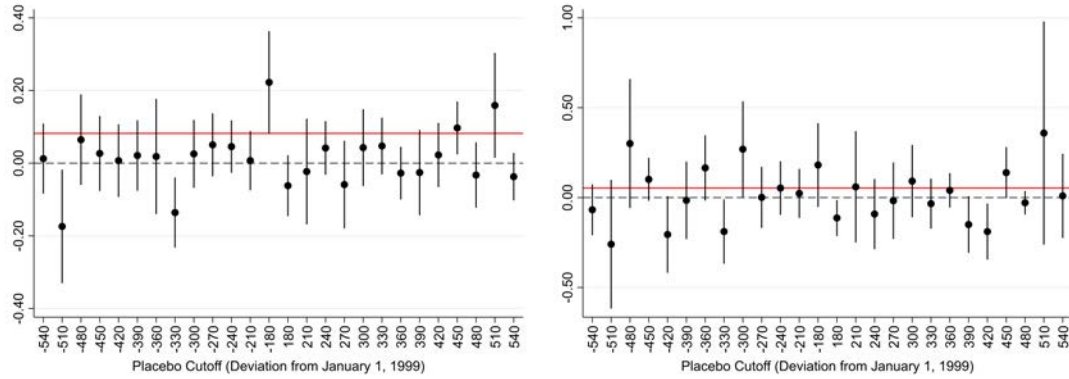


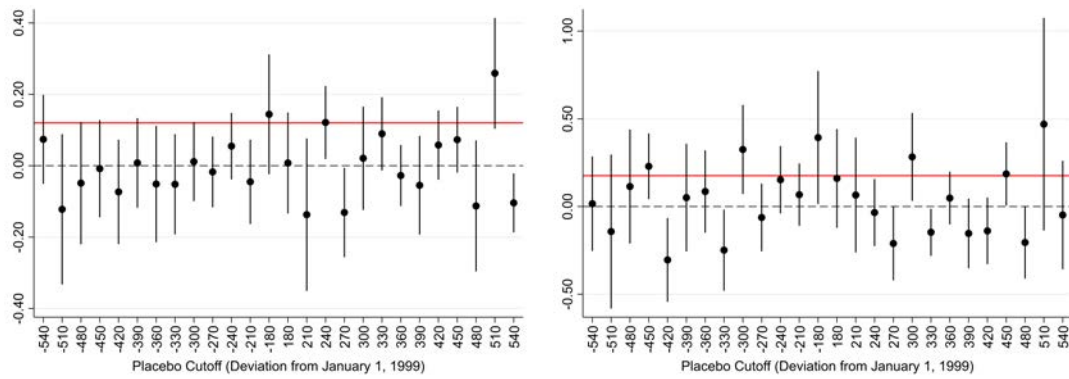
Figure A.3: Sensitivity to Choice of Bandwidth

Notes: The red dots and bars are the RD estimates and 95-percent confidence interval of the RD estimates reported in Table 3.

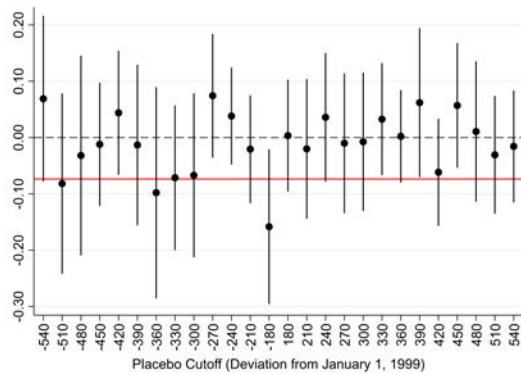
(a) Completed Lower Secondary School, All Children (b) Completed Lower Secondary School, Boys Born 1996-2001



(c) Took Final Exam in Lower Secondary School, All Children (d) Took Final Exam in Lower Secondary School, Boys Born 1996-2001



(e) Convicted of Crime, All Children



(f) Convicted of Crime, Boys Born 1996-2001

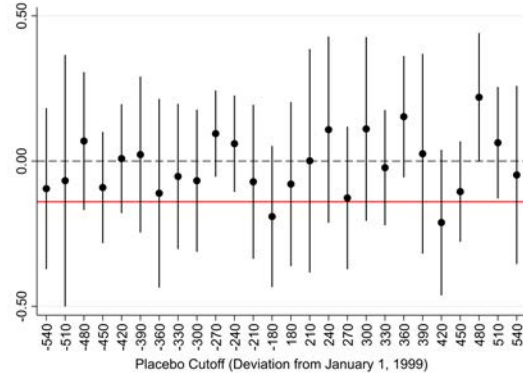


Figure A.4: RD Estimates at Placebo Cutoffs

Notes: The red horizontal lines are the RD estimates reported in Table 3. For placebo cutoffs before January 1st, 1999 we only use data for admissions in 1998-1999. For placebo cutoffs after January 1st, 1999 we only use data for admissions from 1999-2000.

Table A.2: The Impact of the Reform on the Children (Father's Admission Date)

	Lower Secondary School		Criminal Convictions			
	Completed (1)	Took Final Exam (2)	Total (3)	Property Crime (4)	Violent Crime (5)	Narcotics Crime (6)
<i>Panel a. Main (Linear)</i>						
RD Estimate	0.003 (0.034)	0.048 (0.043)	-0.043 (0.036)	-0.059* (0.034)	-0.037* (0.021)	-0.028 (0.019)
Mean of Untreated at Cutoff	0.935	0.874	0.135	0.107	0.052	0.044
Bandwidth	163	168	218	186	214	206
<i>Panel b. Treatment-Control Difference in Means</i>						
Estimate	0.008 (0.022)	0.045 (0.028)	-0.013 (0.029)	-0.011 (0.025)	-0.018 (0.013)	-0.020 (0.014)
Mean of Untreated	0.921	0.868	0.133	0.095	0.043	0.039
Bandwidth	90	73	80	78	112	85
<i>Panel c. 2nd Order</i>						
RD Estimate	-0.010 (0.046)	0.047 (0.051)	-0.064 (0.047)	-0.079* (0.043)	-0.068** (0.029)	-0.075* (0.043)
Mean of Untreated at Cutoff	0.960	0.873	0.141	0.116	0.063	0.075
Bandwidth	214	270	302	286	267	186
<i>Panel d. Controls</i>						
RD Estimate	0.003 (0.033)	0.036 (0.036)	-0.066* (0.040)	-0.068* (0.035)	-0.048** (0.022)	-0.024 (0.017)
Mean of Untreated at Cutoff	0.935	0.877	0.135	0.109	0.056	0.038
Bandwidth	162	216	175	172	188	244
N	5,495	5,495	5,495	5,495	5,495	5,495

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). We use the admission date of the father as the running variable. The outcomes measuring taking the final exam in lower secondary school or completing are dummies for taking the exam or completing before age 18. The crime outcomes are dummies for being convicted of a crime at age 15-18.