INTEGRATING REFUGEES:
LANGUAGE TRAINING OR WORK-FIRST INCENTIVES?

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ABSTRACT

Social and economic integration of refugees are key to their personal fulfillment and to producing positive effects in the host country. We evaluate the impact of a reform that expanded and improved early language classes to refugees in Denmark while also temporarily lowering welfare benefits for a subgroup of them. The policy change applied to those who obtained refugee status in Denmark on or after January 1, 1999. Using a regression discontinuity design around the cutoff date we find that employment and earnings gradually diverged for the treated group after completion of the language program. The effect was significant and resulted in four percentage points permanently higher employment and almost USD 2,510 in extra yearly earnings over eighteen years. We do not find temporary or permanent labor market effects of cutting welfare benefits, but we find evidence of temporarily higher property crime when refugees received lower benefits. We also find that children of refugees who received enhanced language classes were more likely to complete lower secondary school and less likely to commit crime.

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1 Introduction

International refugees, who usually escape from extreme conditions of persecution or war, begin their lives in the host country with significant disadvantages. Research shows that even in the medium and long run, they fare worse than economic immigrants in their labor market and social outcomes. Even after several years the employment and wage gap between them and natives and other immigrants is significant (Fasani, Frattini, and Minale, 2018; Dustmann et al., 2017; Brell, Dustmann, and Preston, 2020). Considering data from the 2000-2014 period, Schultz-Nielsen (2017); Bratsberg, Raaum, and Røed (2017) show that the employment probability of refugee men in Denmark and Norway was, respectively, 31 and 22 percentage points lower than their native counterparts ten years after immigration, even after controlling for a wide range of individual background characteristics and local labor market conditions[1]. Policies aimed at improving the labor market integration of refugees and humanitarian migrants, therefore, have the potential to bring large returns to the migrants and to their host society. Refugees that are productively employed earn more, contribute more to the local economy and increase tax revenues in the host country. These economic benefits make other forms of social integration more likely.

While the goal of enhancing the labor market outcomes of refugees is a common goal of host countries, the way to effectively achieve such outcomes is less clear. Integration efforts, therefore, have taken many forms. Several countries, such as the US, leave this effort to local interventions (Bloemraad and De Graauw, 2012; Williamson, 2018), often coordinated by private charities and churches. Several Northern European governments, instead, in line with their larger involvement in the economy and society, support and fund early interventions for refugees. These interventions are usually a combination of several approaches, including active labor market support, provision of incentives, and teaching of relevant skills. Which one of these approaches is preferred varies across countries and periods. How effective each of these approaches are in improving labor market and social outcomes for refugees has been subject to strong debate and to very limited rigorous analysis.

In an approach that one can characterize as "skill first" and specifically "language skill first", Germany passed an integration act in 2005 that mandated a six-month language class and a similar length course in German history, law and culture for newly arrived refugees (Hübschmann, 2015; Martin et al., 2016). France introduced *Contract d'accueil et d'intgration* in 2007, which also focused on "country-

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[1]The corresponding gaps are even larger for refugee women at 45 and 30 percent for Denmark and Norway, respectively. Other studies that do not distinguish refugees from other immigrants, due to data limitations, still find large and persistent gaps for immigrants coming from poor, underdeveloped, war-torn countries, likely reflecting a combination of differences in skill and pre-migration experiences (since Chiswick, 1978; Borjas, 1985).

specific skills" requiring immigrants to participate in language training and civic training (Lochmann, Rapoport, and Speciale, 2019). Since the 1990s Northern European countries have been using integration programs that combine language-skill training with employment support. These programs typically last between two and five years (Arendt, 2018; Joyce, 2017) with an equal emphasis on "language skill first" and "job first". A country, like the United States, that has limited availability of skill- and language-training options for refugees, but that is relatively rich in low-wage jobs available to refugees, can be considered as following a model of "job first" in the integration of refugees. The evaluation of the effectiveness of these approaches has been very limited. Which policies, between teaching language skills, helping job finding or providing incentives to work has been more conducive to refugees getting and keeping a job in the long run? And which one ensures better earnings and better social outcomes? Which of these policies helps the second generation refugees? We know very little about these questions. The present paper sheds light on the causal effects of two alternative policies, language training and job incentives, on short and long-run outcomes of refugees.

This paper uses a policy change to analyze the effect of improved language training on the economic assimilation of refugees. In general, we have very little causal evidence about the broader question: How does language learning improve the labor market options and earnings of immigrants? The literature established a strong and positive association between language skills/proficiency and the earnings of immigrants.[2] Part of the correlation likely arises from omitted variable bias, as more able and more motivated immigrants acquire better language skills and earn more. Part of the correlation likely reflects a causal link between language proficiency and earnings in the host country (Borjas, 1994). Without a clear identification strategy it is hard to separate these two components or even to say whether language instruction is effective at all in enhancing job outcomes. This is especially true when we talk about specific language classes offered to refugees (or other immigrants) that may crowd out time for other potentially productivity enhancing activities, such as learning job-specific skills, or looking for a job.

Our identification strategy involves a reform of the Danish refugee integration program that allows us to isolate the effect of language training in the early years after arrival in Denmark. The reform involved an expansion and improvement of language learning provision in Denmark. The program was mandatory for refugees and family-reunified with refugees. However, only those who obtained refugee status in Denmark on or after January 1, 1999 were entitled to such extensive language classes. Hence, we use a regression discontinuity (RD) design based on the date when refugee status was granted to identify treated and non-treated refugees. While the language training component was the more relevant

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[2]See for instance Chiswick (1991); Chiswick and Miller (1995) and Dustmann (1994).

part of the reform, another change was also introduced with the same discontinuity date, namely a significant reduction of welfare benefits for refugees over the age of 25 or with children. This second change was only temporary, as it was rolled back after 13 months (February 1, 2000), and it only targeted refugees aged 25 or older or refugees who had children. This policy was justified as a way of creating strong incentives for newly arrived refugees to find a job. The fact that not all refugees in 1999 were subject to it, and the fact that it was only in place for a year allows us to analyze temporary effects for the targeted group as being specifically produced by such a policy, and it is distinguishable from the impact of language training, which is a permanent policy accessible to all refugees 18 years and older.[3]

Several features make this reform interesting and the content of this paper novel. First, the introduction of this program with a sharp time discontinuity allows us to implement a credible estimation strategy for causal inference, as assignment of refugees to the treatment around the threshold is as good as random. Second, as we follow refugees over time and we can observe their education and training, we can see whether language classes were the catalyst for further education. Third, we can look at the long-run effects occurring up to two decades after the reform and refugees' settlement in Denmark. Fourth, we also observe the children of the immigrants, and we can therefore analyze any effect on fertility and on the second generation.

Several clear and important findings emerge from the analysis of the Danish reform. First, we find that being exposed to the reform permanently increases the employment probability and earnings over an eighteen year period. The positive and significant effects emerge four-five years after the reform was implemented and are significant six years after the reform. Consistent with gradual language acquisition and with the fact that the language classes were mostly attended in the first three years after settlement in Denmark, we find precisely estimated zero effects on employment and earnings during the first three years, followed by a gradual improvement and then consistently higher earnings and employment in the treatment group from around year six after arrival. The effect is driven by increases in the probability of employment and higher earnings. Second, we also find evidence of substantial formal skill upgrading in the form of more years of schooling, presumably because the better language skills facilitated

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[3]There is some doubt about whether the policy of welfare reduction to refugees was fully enforced, even in 1999. A report released in December 1999 showed that affected individuals had been compensated by increases in supplementary transfers (Ministry of Interior Affairs, 1999). The welfare portion of the reform was heavily debated already before it came into effect, and UNHCR stated in the summer of 1998 that the welfare benefit reduction "violates Article 23 of the UN Refugee Convention". The UNHCR, however, agreed to the Danish Ministry of Interior Affairs' requirement to await an evaluation. In November 1999, the Danish Prime Minister announced that the reduced benefit did not work as intended, based on results from an evaluation published in December 1999 (Ministry of Interior Affairs, 1999). The evaluation showed that very few immigrants could supplement the welfare benefit with income from work, which was part of the motivation for the reduced welfare benefits, and that immigrants, to a large extent, received supplemental transfer payments on top of their basic welfare benefit.

access to education in Denmark. We also see evidence of occupational upgrading to jobs with a stronger communication requirement. Third we find, for the treated group 25 and older, an increase in the probability of committing property crime in the first year after the reform. This period and group of refugees corresponds to those who lost most of their welfare payments (temporarily) due to the reform. This is consistent with the idea that lower income for this group pushed some of them, temporarily, to shoplifting and other forms of property crime. These effects disappear in the following years when the welfare payments returned to the original level. Finally, we do not find any impact of the reform on fertility, and when we look at the next generation, we see that children of treated refugees are more likely to complete lower secondary school and less likely to commit a crime when 16 years or older.

Analyzing the costs and benefits of the policy change, we find that the benefit-cost ratio from language classes varies between two and fifteen, measured eighteen years after the reform. The time before the aggregate net present value of the additional investment in the language skills of the refugees is positive is five-six years. Similarly, it takes six years before the investment breaks even in the government's fiscal budget.

A closely related paper to this one is Sarvimäki and Hämäläinen (2016) who study a similar reform in Finland in 1999. The Finnish reform, however, mandated individual "integration plans" consisting of a sequence of active labor market programs (ALMPs) tailored to the individual and put together at meetings between the refugee and the caseworker. The reform did not allocate additional economic resources. The authors show that "integration plans" changed the composition of services provided to refugees towards more language training and less regular ALMP, and the reform had very significant effects. Earnings increased by 47 percent and cumulative social benefits decreased by 13 percent for those obtaining "integration plans" ten years later. Our paper is new and different relative to Sarvimäki and Hämäläinen (2016) in several respects. First, we separate the impact of additional language training as an individual policy and contrast this with the impact of a reduction in welfare benefits. Second, we analyze outcomes besides employment and earnings, such as schooling, mobility and crime, which are quite relevant in designing policies for refugees' integration. Third, we also establish whether the early intervention for refugees has effects on the next generation.

Another recent paper related to ours, specifically in the evaluation of the impact of language training on employment of immigrants, is Lochmann, Rapoport, and Speciale (2019). The authors use the introduction of a program in France that assigns non-EU immigrants to language training based on a threshold in their score in an initial test.[4] Such a discontinuity in the score produces random assignment

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[4]France, due to its history, is different from settings such as the Danish one where the immigrants do not know the language

to the language treatment around the threshold. The authors find that 100 hours of language training increases labor force participation by 15 to 27 percentage points. This is a sizable effect; measured two years after completion of classes and equivalent to roughly a quarter of the sample mean (labor force participation is 81 percent). The authors cannot analyze longer-term effects, nor other outcomes due to data limitations.

By looking at outcomes of refugees' children, our paper also contributes to a growing literature on family disadvantage and child outcomes. The schooling outcomes of children of disadvantaged families, especially boys, seem harmed disproportionately by challenging childhood environments (e.g. Bertrand and Pan, 2013; Chetty et al., 2016; Autor et al., 2019). In a paper focused on the consequences of reducing welfare benefits for refugees in Denmark, Andersen, Dustmann, and Landersøe (2016) show that such measures increased crime rates and lowered educational achievements among teenage household members.

Our paper has implications for the debate about "job first" or "skill first", which continues to affect policies towards refugees in many countries. For instance, Northern European countries have been moving away from the policy of "skill first" in recent years. Language training played a pivotal role in the early integration support in Denmark for decades. However, this changed in 2016 when all refugees were required to actively search for jobs and participate in job-training immediately upon arrival. The reform clearly shifted the focus of early interventions towards "job-first". Arendt (2019) finds large positive employment effects of these new policies one year after settlement in Denmark. The effect, however, seems to fade out already half a year later. Joona and Nekby (2012) evaluate a trial introduction program in Sweden where new immigrants were randomly selected to receive more intensive counselling than the regular employment support and also find significant employment effects one year after assignment. There is, however, no available evidence of longer-run effects. Other researchers have analyzed the impact of regular ALMPs for immigrants. The evidence, while still preliminary, points towards larger program effects than those found in the broader ALMP evaluation literature, which is usually rather negative in its assessment of the effectiveness of those programs.[5] The impact of these ALMP policies, however, seems short-lived, while the returns to language training documented by Sarvimäki and Hämäläinen and this paper are long lasting. The combination of strong effects of language skills on long-run employment and no effect of short-term incentives (reducing welfare transfers) on employment

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prior to arrival.

[5] Åslund and Johansson (2011) conclude that more intensive counselling is associated with better employment for immigrants. Clausen et al. (2009) and Heinesen, Husted, and Rosholm (2013) suggest that ALMPs reduce time to find the first regular job, using timing-of-events duration models.

found in this paper, seems to suggest the need for a reconsideration of the recent direction taken by the refugee policies in Denmark and other European countries.[6]

The rest of the paper is structured as follows. Section 3 discusses the reform and its implementation. Section 4 describes the identification strategy. Section 5 describes the samples and the key outcomes we look at. We analyze the direct impact on the working-age refugees who were targeted by the policy (section 6.1) and the effects on their children (section 7). Section 8 concludes the paper.

2   Refugees in Denmark: Recent History

Following a period of low immigration and small numbers of refugee arrivals in the 1970s and early 1980s, a significant number of refugees have resettled in Denmark. The largest inflows of refugees to date are the Bosnians in the early 1990s and recently people fleeing the war in Syria. In both cases more than 30,000 were resettled in Denmark within a few years. In between these major influxes, annual refugee admissions to Denmark have averaged 2,500. Denmark grants refugee status to persons entitled to protection under the UNHCR Refugee Convention as well as supplementary national rules.

Displaced individuals who apply for asylum in Denmark are registered by the police and referred to accommodation centers where they live until their case has been determined. The centers are usually in sparsely populated areas and the asylum seekers were not allowed to work (this changed in 2013). Most asylum seekers, therefore, have very little contact with Danes and the Danish society prior to obtaining refugee status. The refugees waited more than one year before a decision on asylum was made around the time of the reform. Once/if the asylum claim is recognized, the refugee is resettled in a municipality, that is chosen by the government with the aim to distribute the refugees across municipalities and encourage assimilation.[7] The municipality is responsible for finding suitable public housing for the refugees they receive and generally provides social support and unemployment services for its citizens. Language training was offered up to 1.5 years after settlement at schools run by the Danish Refugee Council. This was changed by the 1999 reform (section 3).

Similar to refugees in other European countries, refugees in Denmark have shown a significant initial disadvantage and slow convergence to native earnings and employment probabilities. Refugees arrive without a job and are dependent on welfare early on. Employment and earnings grow during the first five to ten years after settlement but then seem to stabilize with a considerable gap between them and

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[6]Consistent with our results but extended to a more general analysis, Card, Kluve, and Weber (2010, 2018) conclude that the returns to employment support, such as job search assistance, are small or zero, whereas returns to programs with a clear skill-training component are larger but only after a couple of years.

[7]UNCHR Quota refugees are resettled directly from refugee camps abroad. They constitute 10 percent of our sample.

natives and other immigrants.[8] The employment gap between refugee men and (low skilled) native-born men is 31 (15-16) percentage points ten years after immigration (Schultz-Nielsen, 2017, Table 2). While the countries of origin have changed, the initial conditions and the difficulty in integrating economically and socially have been persistent features of the Danish experience as well as of the experience of other European countries (Schultz-Nielsen, 2017; Bratsberg, Raaum, and Røed, 2017; Brell, Dustmann, and Preston, 2020).

3   The Reform

The first Act on Integration and the new Act on Language Education went into effect in Denmark on January 1, 1999. Refugees granted refugee status before this date were subject to the old rules whereas individuals obtaining it after this date were subject to the new rules. The main purpose of the reform was to promote integration into civil society and improve labor market outcomes for newly arrived refugees. The central measure to achieve these goals was higher-quality and more extensive language training. The reform simultaneously modified somewhat the work-incentives for a subgroup of older workers, as it temporarily lowered their welfare benefits. This section describes details of this reform and it documents the changes for newly arrived refugees and for the family members reunified with them. Participation in the new program was mandatory for this group and non-participants could be financially sanctioned and, more importantly, acquiring permanent residence status required proof of completion of the program.

The reduction in welfare benefits for newly arrived immigrants was temporary, and it was implemented with a strict age threshold. Those aged 18 to 24 without children received the additional language training with no other changes. Instead those aged 25 or older or with children received both the expanded language training and a decrease in welfare benefits. Welfare benefits to this group were cut to 71 percent (75 percent if children present) of the regular welfare benefits of unemployed individuals in Denmark. The cuts were not trivial, amounting to a 25-29 percent cut in benefits, but they were smaller than the cuts to welfare benefits directed at immigrants in 2002 in Denmark and studied by Andersen, Dustmann, and Landersøe (2016), Rosholm and Vejlin (2010) and Agersnap, Jensen, and Kleven (2019). The cut in the level of welfare benefits for the 25 or older age group was withdrawn after 13 months on February 1, 2000. Hence the group of 25 years and older shared with the younger group of refugees arrived on or after January 1, 1999, the higher level of language training, but for one year was subject to

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[8]Appendix Figures 1.1 to 1.3 in Schultz-Nielsen (2017) show employment rates, earnings and welfare dependency of a refugee cohort very similar to ours and two subsequent cohorts.

lower welfare transfers. A comparison between the two groups (below 25 years of age without children and the rest) allows us to analyze, in addition to the effect of language training, whether any additional work-incentive effect was generated by lowering welfare benefits for the older group. Work-incentive effects should only be in place, if at all, within the 13 months the reduction was in place, and only for the older group. After 13 months all age groups continued to share the intensified language treatment and had no differences in welfare treatment.

What was the exact nature of the change introduced by the reform? The new policy increased the language learning time from 1,370 to 1,800 hours and allowed such training to occur over an extended period of 36 months; a doubling of the 18-month limit existing before.[9] The whole structure and the quality of the language training was also changed; centralized goals and national tests were introduced and resources to increase the qualifications of the teachers were provided. The Act on Integration also introduced a course in civic education. While important, this was more limited in scope when compared to the great boost of language education. Only 20 hours were to be dedicated to civic education, while 430 hours of additional language training were provided. We refer to the augmented language and civic education provided by the reform as *"integration services"*. The regular active labor market policies (ALMPs), including on-the-job training, which were not targeted or changed by the reform are referred to as *"employment services"*.

Another minor administrative change produced by the reform was that, after January 1, 1999, the municipalities (rather than the Danish Refugee Council as it was before the reform) became responsible for managing language training, while they also remained responsible for managing the ALMPs for all residents. Therefore, the reform combined the responsibility for integration and employment services in one place, namely the municipalities. As it took around two months between admission into Denmark and resettlement in a Danish municipality, refugees arriving shortly before and after the cutoff both received employment and language support from the municipalities. This implies that this small administrative change was not perfectly aligned with the discontinuity in the change in language training and lower transfers. Municipalities were only fully reimbursed for the additional language training for people obtaining refugee status from January 1, 1999 on, and only a subgroup of those were subject to the reduction in welfare benefits.

While the responsibility for language training of all refugees moved to municipalities as of January 1, 1999, reimbursement per refugee was larger for those arriving after January 1, 1999 relative to those who

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[9]The reform resulted in more extensive language training than offered to immigrants in most other countries (e.g. the German introductory language course is 600-900 hours and Sweden offers on average 525 hours of language training).

arrived before. The reimbursement was constituted by a fixed monthly payment per refugee and activity-based payments for months where the refugee was receiving employment or integration services. Both the fixed and the activity-based payments were higher for refugees arriving after the reform and they lasted twice as long (36 compared to 18 months).[10] The activity-based payments were USD 212 and 288 (DKK 1,400 and 1,900) for employment services and USD 500 (DKK 3,300) for integration services to refugees arriving after January 1, 1999, whereas no earmarked payment was made for integration services to refugees arriving before the reform, (as per Act on Integration 1999, section 45(5), 45(6) and 45(7) and section 59(3) and 59(4)). Hence, municipalities had to provide more services to those arriving after the cutoff and they had no incentive to increase service provision above the entitlement for those arriving earlier, since there was no reimbursement of this.

Figure 1 Panels a and b show the share of individuals receiving integration and employment services within the first three years for the refugees who obtained refugee status in Denmark in a four year window around January 1, 1999 (day 0 in the graphs) as recorded by municipal staff. As expected from the description of the policy change, we observe a large discontinuity in the share receiving integration services (Panel a) and no discontinuity in the share receiving employment support (Panel b) from the municipalities.[11] Additionally Panels c and d of Figure 1 show the number of hours of service provided, conditional on receiving the service. Even in this case the difference in language learning around the cutoff is large and significant and equals 1,506 hours (RD estimate).[12] Instead, we see no discontinuity in the hours of employment support (RD estimate is insignificant -41 hours) received during the first three years following settlement in the municipality. When looking at the time profile of these treatments (Appendix Table A.1), we see that more intensive language training after the reform crowded out time spent in employment support in the early months. This difference was significant within the first year after arrival. After one year of settlement in the municipality there was no significant difference in employment services, either in participation or cumulative hours. However, treated refugees continued to be more likely to be enrolled in language training and to accumulate additional hours of integration services, steadily growing towards the 1,800 hours they were entitled to after the reform.

Using additional data from all language training facilities (rather than only from the municipalities), we find that the treated refugees participated in more hours of language classes and were enrolled for

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[10]The fixed payments were USD 340 and 515, respectively, per month for refugees arriving before and after the reform.

[11]The RD estimates are 54 percentage points and insignificant -8 percentage points (Appendix Table A.1).

[12]Note, this difference is far beyond the difference in entitlement to language training (1,370 and 1,800). This may partly reflect that participation was not mandatory before the reform and partly that it was provided by other authorities and not necessarily recorded in the municipal service provision statistics. Evidence from language training facilities (Appendix Figure A.1 described below suggest the latter interpretation is correct.)

longer than the untreated ones but both groups had the same probability of ever attending the course (Appendix Figure A.1). Furthermore, these data from the training facilities reveal that treated and control refugees were rarely in the same classes, which suggests that language support was different; not just in the number of hours but also in the quality and structure provided.

The Act on Integration also brought a new procedure for the settlement of refugees. The settlement process was aimed at producing a uniform distribution of refugees across municipalities both before and after 1999. Prior to 1999, however, the Danish Refugee Council was responsible for the settlement of refugees by making bilateral arrangements directly with the municipalities. From 1999, the settlement was decided in a more mechanical way, following a model that first distributed the refugees according to county quotas, while taking the number of immigrants already living in the municipalities into account. The municipalities within a county were then supposed to agree upon the within-county settlement, but if they failed to agree, the central government would determine the number by municipal quotas. Hence, after the reform the distribution was somewhat more mechanical and the central government was only involved in cases of disagreement.

This somewhat different procedure does not need to imply any specific sorting or selection in the distribution of refugees across municipalities. To rule out that it generated different sorting, potentially confounding the impact of increased language training, we will include a set of municipality characteristics in the balancing tests between treated and control refugees that we conduct in section 4, Table 1. We also investigate the post-assignment mobility out of the municipality (section 6.2, Figure 7). We find no differences in municipality characteristics nor in the probability of moving by the end of the integration program between treated and control refugees, implying that there was no different incentive to relocate for the post-January 1, 1999 admissions.

4   Identification and Empirical Design

The impact evaluation we perform uses the pre-determined and arbitrary cutoff for the date of obtaining refugee status in Denmark. Admission after January 1, 1999 determines eligibility for the longer and improved language training and for temporarily lower welfare benefits if one is 25 or older. We apply a Regression Discontinuity Design (RDD) focusing on refugees admitted around the discontinuity date. The Danish government was in charge of granting residence permits, so that the exact admission date was outside the control of refugees as well as of municipalities, which provided the integration services. Individual caseworkers could potentially affect the speed of processing of some applications. This sec-

tion describes the estimating equation, discusses potential threats to identification and presents some tests of validity of our identification strategy, namely a large set of tests of balancing of pre-treatment characteristics.

We consider individual refugees, and their treated/untreated status is determined by the date when they were granted refugee status in Denmark. Specifically, the treatment variable for individual i, D_i, is an indicator equal to one for being admitted at or after January 1, 1999 and equal to 0 otherwise. Our running variable, x_i, is the distance between this cutoff date and the date of admission, and we assume a linear trend in the admitted refugee cohorts at both sides of the cutoff in the following way:

$$Y_{it} = \alpha + \tau D_i + \beta_1 x_i + \beta_2 D_i x_i + \varepsilon_{it} \tag{1}$$

where τ captures the causal local average treatment effect of the reform, and Y_{it} is the outcome of interest measured at $t = 1, 2 \ldots 18$ years after the reform.[13] We estimate a separate regression for outcomes in each year from 1 to 18 after the reform using Weighted Least Squares (WLS) with a triangular kernel to give more weight to the observations closer to the cutoff. We restrict the observations used in the estimations to a narrow window around the cutoff, determined by the optimal bandwidth selection algorithm from Calonico et al. (2019). The selected bandwidth is much smaller than the four year window included in our full sample and ranges between 216 and 277 days for the main outcomes we analyze.

Asylum seekers obtaining refugee status in Denmark around the cutoff would have applied for asylum long before the new legislation came into effect. Most refugees, at the time of applying, had no knowledge of the changes that were about to happen. The Act on Integration was proposed on April 16, 1998, and passed on June 26, 1998. In comparison, the average waiting time for decisions on applications for asylum was 13 months at the time (Table 1), and the median was slightly more than ten months. Hence, a majority of the refugees who were granted asylum near January 1, 1999 had applied before the law was even proposed the first time.[14]

Most applications were turned in well before the reform was approved and there is no recorded or anecdotal evidence that authorities had changed the processing time of asylum applications in response to the reform or around the cutoff. A check that "strategic timing" in the approval of applications did not occur is that the number of approved applications should not show any unusual bunching (or drop)

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[13]Note that τ is free from confounding factors even when $cov(x, \epsilon) \neq 0$ as long as the linear function correctly specifies potential outcomes. We analyze second and third order polynomials in our robustness checks and all our results are insensitive to the choice of functional form of the running variable.

[14]See Hvidtfeldt et al. (2018) for more information on the process and waiting times in the Danish asylum system.

on either side of the cutoff. Figure 2 shows the raw distribution of admissions by month from January 1, 1997 to February 28, 2001 in Panel a. Then, in order to account for seasonality and time trends in refugee admissions, we show, for each month, the share of annual admissions in deviation from the mean share for that month over the four years in Panel b.[15] A value of zero in the observations shown in Panel b implies that the admissions that month equal the average for that month over the time period shown. More applications were approved in December 1998 compared to January 1999 (Panel a), but January is right at the mean for this month and December is slightly below (-0.03 percentage points) its typical share of annual admissions (Panel b). The most unusual months (out of the 50 months we show) are April 2000 (-0.05), April 1997 (0.04) and May 1998 (0.04). We conclude that the density of applications is smooth around the cut-off date and nothing indicates that the approval process was slowed or accelerated around the cutoff.[16]

A second possibility is that case workers continued to process a uniform number of cases around the cutoff but were being selective about which cases to allow into the new policy regime. Moreover, there was also a change in the resettlement process, starting on January 1, 1999, which could have generated differential sorting of refugees in specific locations after the cutoff date. Table 1 shows the summary statistics of predetermined individual characteristics available in our data (Panel a) and the characteristics of their first municipality of residence (Panel b), and each column shows a different statistic. We first report the sample mean and standard deviation of these variables. Then the difference around the RD threshold for the individual variables and initial municipality characteristics, and the confidence interval for testing the hypothesis that the discontinuity is equal to 0. These are checks of our identifying assumption that the distribution of individual characteristics and of characteristics of the first assigned municipalities are random around the discontinuity.[17] The average age of refugees at admission in the selected sample was 31.5 years (we have excluded individuals older than 49 as we follow individuals for 18 years). The typical refugee is married, male and the average number of children is slightly above one at the time of admission. The single most important country of origin around the reform was Iraq, representing 44 percent of our sample (all Iraqi refugees in the sample arrived to Denmark before the beginning of the Iraq war). 16 percent of refugees were from Afghanistan, and

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[15]The admission data are only available from January 1997. Our full sample includes refugees arriving within a four year window around the reform as explained in section 5. Figure 2 includes two additional months to illustrate better the seasonality around New Year.

[16]Besides evidence of no manipulation, even if refugees could manipulate the date, it was not clear ex-ante that they would want to qualify for the new policy. While it increased language training, it was combined with sanctions for non-participation and a reduction in the level of welfare benefits.

[17]We use local linear regressions with the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019) in order to test the discontinuity. Appendix Figure A.2 illustrates the balancing tests for each of the variables in the Table 2.

the rest come from a range of smaller origin categories (see section 5 for a thorough description of our sample and data.).

We find no evidence of differences in composition and characteristics of refugees between those who arrived before and those who arrived after January 1, 1999. They are of similar age, equally likely to be married and have the same number of children. The refugees could not be employed upon arrival and we do not have information on education received in their country of origin. Instead we use the assignment to language training as a rough indicator of skills. The language training facilities place refugees into Danish 1, 2 or 3 based on their skills at arrival. Those with primary schooling or less are typically assigned to the lowest level, Danish 1. Tertiary educated refugees are most often assigned to Danish 3, and Danish 2 is an intermediate group. Using this measure of educational attainment the tests in Table 1 show no differences in the skills around the cutoff. Finally, we see from the summary statistics that 35 percent of the admitted refugees spoke Arabic as their mother tongue, while 9 percent reported Dari as their first language. These are the most widespread languages in Iraq and Afghanistan.

33 percent of refugees were placed in urban municipalities (Table 1 Panel b). The RD estimate is 0.11. Although not significant, this is a relatively large difference. However, it turns out that it is driven by municipalities on the margin of being classified as urban. Despite the fact that 17 percent of our sample is placed in one of the five largest cities in Denmark, there is no difference across January 1, 1999 in this variable (RD estimate 0.01 and standard error 0.05).[18] So there is no evidence of selective placement in the largest cities. Furthermore, the treated and the control groups are distributed in municipalities with similar employment and unemployment rates, similar income and similar numbers of co-nationals.

5 Data

Our analysis is based on Danish administrative register data, which are hosted by and made available to us through Statistics Denmark. The Integrated Database for Labor Market Research ("IDA"), the Income Register as well as databases on enrollment in and completion of schooling and a database on convictions for criminal activities are merged and used to construct the main outcomes. We link these longitudinal data to the Population Register and to the information on admission class and date of receipt of residency from the Admission Register, which includes all refugees admitted to Denmark. The combined data provide key demographic variables, such as age, gender, address, and allow identification

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[18]The five largest cities are Copenhagen (incl. Frederiksberg), Aarhus, Aalborg, Odense and Esbjerg.

of refugees, their spouses and children as well as their characteristics and outcomes. The main summary statistics of the demographic and geographical variables are shown in Table 1. The Migration Register includes the country of origin and date of settlement in a Danish municipality for each refugee. Finally, we link each individual to the Register for Labor Market Policy Measures ("AMFORA"), which includes the services received in the municipality of settlement, and to additional information on the refugee from the language training facilities, such as his/her mother tongue and their initial assignment to language training track 1, 2 or 3, which is a proxy of their schooling level at arrival. Specifically, those with primary schooling or less and those who do not know the Latin alphabet are typically assigned to Danish 1. Tertiary educated refugees are typically assigned to Danish 3, which is regarded as a fast-learner track and the exams that mark the completion of Danish 3 are at the highest level. Danish 2 is for the intermediate group and the Danish level upon completion lies in between the two other tracks. Survey evidence on education level of refugees, covering roughly half of our sample, shows that the refugees are less educated than natives. This implies that our group appears typical of most cases of resettled refugees (Brell, Dustmann, and Preston, 2020; Capps et al., 2015). Specifically, 41 percent of refugees have only basic schooling upon arrival in Denmark while this is the case for 30 percent of natives in 1999 in the same age range. We also report the percentage of refugees admitted under different channels, such as the Geneva convention, UN quota refugees and family-reunification with existing refugees. The majority, 53 percent, are granted refugee status under supplementary Danish laws.

In order to take advantage of the discontinuity in policy treatment around January 1, 1999, the sample we focus on includes all refugees and family-members reunified with existing refugees who were admitted in a four year window around that date.[19] We restrict our attention to individuals who were between 18 and 49 years old at the time of being granted asylum (or being reunfied to an existing refugee) in Denmark and we follow them until 2016, spanning a full 18 years after the reform. The age range is chosen to include individuals who were old enough to be employed and to be included in the program at the time of arrival, still young enough to be of working age (usually below 65 at this time) during the period analyzed so that we could follow their labor market outcomes for the whole period. As in some specifications we control (and check) for the geographic distribution of refugees and municipality characteristics, we drop a few individuals for whom no address could be found within the first two years of arrival. These steps leave us with a sample of 8,558 individuals who were admitted to Denmark between January 1, 1997 and December 31, 2000. We refer to them as the *"refugee sample"*.

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[19]Somali refugees are excluded from our analysis due to some irregularities in the processing of asylum applications for this particular group in the last months of 1998. The family-reunified in our sample are members of the immediate family (spouse and children) of a refugee in Denmark.

These 8,558 refugees can be linked to 16,598 children, of whom, 45 percent (7,436) were born before arrival in Denmark and 14 percent (2,294) were born after arrival but before 2003. In order to observe schooling and social outcomes for these children, given that our sample ends in 2018, we restrict the "*children sample*" to those 9,730 born in 1986 to 2002. This allows us to observe schooling outcomes into secondary school and outcomes in teenage years for most of these children. In particular, we observe graduation from lower primary school for everyone who is in the correct grade for age, and we also observe early measures of juvenile crime for the 16+ year olds.

In our empirical analysis, we consider several labor market outcomes for the adult refugees as well as other outcomes.[20] The summary statistics of the main outcome variables are shown in Table 2. We measure employment as the fraction of a full-time working year. This measure takes the value one if the worker was a full time-employee during the whole year. The fraction is less than one and measures the share of a full-time equivalent if the individual was either a part-time employee or not employed in some periods throughout the year. Notice from the summary statistics of Table 2 that the average annual employment in the sample of refugees is only 0.2, implying substantial non-employment or under-employment for this group throughout the sample period. The group of refugees begins its stay in Denmark without a job, as refugees were not allowed to work while waiting for their status decision. In the year 1999 only 3 percent of them were employed. Then in the following 18 years their average employment rate steadily rises to about 30 percent in 2009 and then it decreases a bit (with the recession of 2010) and stabilizes at around 20 percent. This profile implies that refugees are very far from participation rates similar to natives (equal to 81 percent in 1999 and 87 percent in 2016). This low average employment rate is also useful for putting the impact estimated in this analysis into perspective.

A second outcome we analyze is the annual gross earnings measured in US Dollars and deflated using the consumer price index from Statistics Denmark using the year 2000 as the base year. Employment and earnings can be calculated for the entire sample and these are the main economic outcomes we analyze. If no information on earnings or employment was provided in the registers, we assigned no employment and zero income to the observation. A considerable share of refugees have no earned income or employment and so the average annual income is a very modest USD 8,600. In our empirical analysis we report the impact of the reform on outcomes in levels and translate them into percentage effects (relative to the pre-reform mean at the cutoff point) using α from equation (1).

Additionally, we analyze a number of other outcomes to understand potential mechanisms behind

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[20]The Integrated Database for Labor Market Research (IDA) that we rely on to construct outcomes for the parent sample is available from Statistics Denmark with a few year lag. This is the reason why we only follow the refugees until 2016 while we can follow the educational and crime outcomes of their children until 2018.

our main results and also to have a more complete picture of the intended and potentially unintended consequences of the policy change. These outcomes include residential mobility, namely changes in municipality of residence relative to the one of settlement, human capital upgrading measured by completion of education programs in Denmark, occupational changes and criminal convictions. The summary statistics in Table 2 show that 57 percent of refugees were not living in the same municipality of settlement by 2016, and 55 percent of them were living in an urban municipality. An important part of the economic effects of the policy may occur through occupational change, as better language training enabled refugees to access more complex and language intensive jobs. In order to characterize occupations according to their "task content" we define "task upgrading" as the use of communication relative to manual tasks in a job.[21] We measure the manual and communication task content of occupations for those who were employed at the end of November each year.

For criminal outcomes, we use data on the number and type of criminal charges and convictions. Our main measure of criminal behavior is whether the individual has been convicted of a crime, a property crime and, more specifically, of shoplifting in a supermarket, which is the most common criminal activity in our sample. The probability of being convicted of a crime for a refugee in the first year in Denmark is about 5 percent, and the probability of ever being convicted of a crime in the period we analyze is 24 percent. Among those, property crimes are the more common, with 17 percent of the sample ever convicted of it and, in particular, shoplifting is the most common single crime with eight percent of refugees ever being convicted of it. As comparison, 13.7 percent of natives aged 18-49 in 1999, were ever convicted of a crime. If we focus only on natives with primary education or less, the percentage is 25.8 percent. Overall there is a larger probability that refugees committed a crime relative to natives.[22]

Finally, for the children of refugees born before 2003 we analyze two types of outcomes. The first is schooling outcomes; specifically, exams at the end of lower secondary school, graduation from lower secondary school, and enrollment in upper secondary education. The second is criminal outcomes, namely being charged with some crime as a juvenile including the type and severity of the crime.

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[21]The task outcomes use information from the O*NET database (US Bureau of Labor Statistics) merged to Danish register data via the International Standard Classification of Occupation.

[22]Notice, however, that the conviction rates for this group of refugees decreased steadily over time, so that the early years were those with highest conviction rates for them (around 5 percent), while after 10 years in the country, the yearly conviction rate was less than two percent. Furthermore, education levels need to be compared with caution across native born and refugees since the source is different and potential downgrading of their skills reduces their earning ability.

6 Estimated Effects on Refugees

6.1 Main Labor Market Effects

Figures 3 and 4 show the main results for labor market outcomes. They represent the RD point estimates and the 95-percent confidence intervals of the impact of the reform on the employment rate and gross earnings of refugees for each year from 1999 (year 1) to 2016 (year 18). The two top panels (a and b) show the average estimates for the whole group of refugees, while the two bottom panels (c and d) show separately the effects every fourth year for refugees aged 18-24 with no dependents (children), who were only subject to the increased language treatment, and those above 25 who also had a reduction in benefits in year one (as explained in section 3).[23] Panel a plots the estimated impact in each specific year, while Panel b plots the average cumulative impact on the employment rate (in Figure 3) or on earnings (in Figure 4).

Looking first at the average effects over time, in Panels a and b, two features emerge clearly both for employment (Figure 3) and labor market earnings (Figure 4). First, while we see small to no labor market effects in years 1 to 4, when the refugees were attending the extra training in language classes, we see positive effects on the treated group from year five on, when the classes were completed. The magnitude of the employment effect is in the order of 7-8 percentage points of a full working year in years 6 to 8 after the reform, then there is a decline in the effect, corresponding to the recession in 2009-10. The second fact emerging from the graphs is that the long-term effect stabilizes around 4 percentage points with a dip during the recession.[24]

The profile and time pattern of the treatment effects on earnings (Figure 4) are very similar to those on employment, with annual earnings approximately USD 3,100 higher for the treated group 6 to 8 years after admission to Denmark. As labor income is the main source of earnings of refugees, this seems logical. These are large effects due to the low baseline of employment among refugees. Fromr 6 to 7 years after the reform, annual employment was about 38-56 percent higher for the treated group and annual earnings about 42-50 percent higher. Over 18 years this accumulated into a total of 0.76 years (9 months) of extra employment, corresponding to a 4 percentage point higher employment rate in each

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[23]We exclude from the 18-24 year old group 518 individuals (30.6 percent) with children present because all individuals with a child present experienced a reduction in benefits to 75 percent of the previous level. To keep the group of 18-24 as untreated by the reduction in welfare, we include the 18-24 with children in others. Effects by finer age groups and excluding children at both sides of the cutoff are shown in Appendix Figure A.3.

[24]Employment is measured as a fraction of a full working year. Hence, annual outcomes can be interpreted as a fraction of one year full-time equivalents and cumulative outcomes may be interpreted as a fraction of years of employment. Effects are more noisy if we split employment into intensive and extensive margins, but we similarly find relatively small coefficients in the first couple of years (more on this below, in Figure A.5).

year for the treated. In terms of earnings, the treated group accumulated USD 45,267 higher income from employment (measured in 2000-prices) over 18 years which corresponded to an average of USD 2,510 higher earnings per year. Relative to the baseline these are large effects equal to 23.0 and 33.7 percent rise in employment and earnings, respectively.[25]

Notice that in the first 2 to 3 years after the policy is enacted, there is a precisely estimated zero difference in employment and earnings between the treated and the non-treated. This is very consistent with a genuine language skill acquisition effect that accrues over time. During the first 3 to 4 years refugees are learning and this produces an impact on labor market outcomes in the following years. Panels c and d of Figures 3 and 4 separate individuals who are aged 18-24 without children from the rest as the first group had no change in welfare in the first year while the others experienced a drop in welfare for one year. Panels c and d show the dynamic effects every four years for the younger group (in blue) and the older group (in red) separately. While the effect on the young is estimated with a significant error due to the small sample, there are no significant differences in the effects between the two groups. The average accumulated effects, in particular, show very similar estimates in the two groups, both for employment and earnings. In the yearly effects we actually see a larger (positive) impact for the young group, while the other group shows a very precise zero effect estimated even for the first four years. This suggests that the early one-year reduction of benefits did not have any work-incentive effects for newcomers, and did not generate any early earnings. Probably this is because very few refugees were able to supplement the welfare benefits with income from work in the very early years, which is consistent with the finding in the report by the Ministry of Interior Affairs (1999) that led to the abandonment of the benefit reduction. This is in line with the idea that the early hurdle to employment for newly arrived refugees was a lack of skills, and language skills among them, rather than a lack of willingness and incentives to be employed. Overall the effects by age are quite similar and we can never rule out homogeneous effects on earnings and employment across the two groups or even across a finer partition of workers into six-year age groups (Appendix Figure A.3). While it is possible that the zero effect of the drop in welfare benefits is due to the fact that this was a small drop, or to the fact that our sample size is not large enough to precisely identify small effects, the evidence from these results is also consistent with the idea that there is no quick fix to increase employment of refugees in early years. However, improving their language skills through increased training pays off after 4-5 years when they can use those skills in the labor market.

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[25]The baseline is 0.15-0.19 average years worked (annual) and 6,159-7,371 USD average yearly salary (annual) 6-7 years after the reform, and 3.3 worked years (cumulative) and USD 134,378 (cumulative) by 18 years after the reform for employment and earnings, respectively.

Figure 5 presents a series of robustness checks for the main results captured by the coefficient on the average cumulative employment and earnings 18 years after the reform. This coefficient summarizes whether there is a persistent long-run effect and its magnitude in terms of gains in employment rate and earnings per year. The first two panels (a and b) show that the estimated magnitude and significance of the effects are very robust to the choice of bandwidth. Only with an extremely small bandwidth of 30 days around the cutoff is the estimate imprecise and not significant, but from bandwidths of 60 days to 360 (including the optimal bandwidth which is 216 for employment and 277 for earnings) the obtained estimate is significant and stable. In Panels c and d we show the estimates obtained at arbitrary cutoff dates before and after the actual cutoff date. The red line shows the estimate of the impact at the actual discontinuity. In most cases at arbitrary cutoff points we estimate very small and non-significant effects. Occasionally some significant effects are estimated. Since the reform was implemented on January 1, 1999, it is also reassuring that there is no effect of January 1998 (-365) or January 2000 (+365), ruling out that some structural reoccurring seasonal events related to the new year are driving our results (three out of four point estimates are zero and the last one has the opposite sign to our results). In Panels e and f we see that the estimates are not sensitive to a series of additional checks. First we include control variables, then we use second or third order polynomials instead of local linear regressions. In the "donut" specification we exclude refugees admitted in the two year window around New Year 1999. Finally, we also show the estimates from a simple OLS regression where the time to cutoff is a linear variable and obtain a similar estimate of the discontinuity effect. One can also read the stability of the estimated long-run effect with respect to bandwidth as well as the "donut" estimates as showing that the impact on those arriving right after January 1999, who were exposed to a full year of welfare benefits reduction, is not different from the impact on those arriving after one year and who experienced almost no welfare reduction at all (as the benefit reduction was discontinued in February 2000). For later admissions, the time with lower income is shorter and for those towards the right end of the range shown in Figure 5, Panels a and b, the marginal admissions have the exact same benefit level as the control group. This is another piece of evidence that is consistent with no employment effect from the welfare cuts.[26] So, overall, the results are rather insensitive to the inclusion of controls and to the change in bandwidth, suggesting that the cutoff was indeed random. Using other functional forms and a simple OLS estimator one would also get a similar, although slightly larger, estimate (Panels e and f).

Figure A.5 in the Appendix investigates the margins of the employment and of the earnings re-

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[26]Appendix Figure A.4 shows the sample means of the outcomes 18 years after the reform by one-month bins around the cutoff and a linearly fitted regression line. These graphs confirm that there is a discontinuity in the outcomes around the reform and clear level difference before and after.

sponses. We find no evidence that employment time varies with treatment for employed individuals, which is the intensive margin of employment responses. Panel a shows that conditional on working (employment larger than zero) there is no effect on employment measured as a fraction of a full working year. Panel b then shows that there is no effect on the share working full time, also conditional on working. Turning instead to extensive margin responses, we do see a tendency for the treated group to have higher probability of working, starting 5 to 6 years after the beginning of the treatment period (Panel c). The annual effects are rarely significant. If we instead measure the effect on having ever been employed, which is a measure of the extreme extensive margin, treated refugees are 7.8 percentage points (not statistically different from zero) more likely to have had a job by the end of the analysis window. Panel d shows that earnings of the employed rise, revealing that earnings effects are not driven solely by higher employment but also by higher earnings among the employed.

6.2 Effects on Schooling, Occupations and Mobility

As we showed in the previous section, overall, the reform increased employment gradually starting after 4 to 5 years from the beginning of the treatment, which corresponds to the early post-program period. Then higher earnings, conditional on employment, seemed to occur later in the analysis period, also suggesting some form of upgrading/improvement in job type. Figure 6 analyzes some outcomes that can be potential mechanisms that help to explain the observed labor market dynamics. Panel a of the figure shows that treated refugees were also more likely to pursue additional education in Denmark. The average effect emerges 8 to 9 years after the beginning of treatment, and 4 to 5 years after the end of the language training. When separating the young (under 25) from the older in Panel c, we see that the effect is driven exclusively by the young. This group is more likely to obtain lower secondary education (significant) and a subgroup of them may also obtain academic education in Denmark (insignificant) early on (Appendix Figure A.6, Panels b and f). By far the largest skill upgrading effect is vocational education programs, which lasts for 2 to 4 years and can be attended while working and earning (Appendix Figure A.6, Panel d).[27] Interestingly, this effect arises much later and is only significant from around 9 to 10 years after 1999. This is consistent with the increased enrollment after the recession hit and the employment effect on the treated declined for a while. Plausibly, the refugees who spoke better Danish, and were employed may have reduced their employment during the recession and used this time to further qualify themselves with formal education so as to consolidate their permanent strengths in the Danish labor market (Appendix Figure A.6, Panels a, c and e show enrollment). This transition to more

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[27]Paid work-based learning is part of the vocational training programs.

education is what likely allowed them to obtain better paid jobs after The Great Recession.

Panels b and d of Figure 6 show that treated refugees tended to upgrade their occupations towards jobs with higher communication (and less manual) content. The transition towards more communication intensive jobs begins already three years after 1999, when refugees finish their language training and is significant in most periods after 8 years. Panel d shows that both young and old refugees experienced this occupational upgrading in the early years after completion of the training, with a potentially stronger long-run effect for older refugees. While the younger group may have mainly benefited from better language training by adding some formal vocational schooling, the older group simply transitioned to less manual-task intensive occupations.

Finally, in Figure 7 we analyze whether the language training treatment affected refugees' geographical mobility. Panels a and c show the impact on the probability of having moved from (not living in) the municipality of initial resettlement each year after treatment. As usual Panel a shows the average yearly effects and Panel c shows the effect by age group every four years. The pattern is very interesting, and it reveals a lower probability that treated refugees move out of the municipality in the early years, but then no substantial difference in the probability of moving in the long-run. Essentially, such a pattern reveals a delay in the probability of moving from the settlement municipality, lasting until 5 to 6 years after 1999. This seems consistent with a significant rate of overall mobility of refugees from their initial municipality of resettlement, and with the incentives of the treated group to stay for the years of language training, but then leave, potentially to pursue jobs, further education and upgrading of their economic status. The lower mobility effects appear a bit stronger for the young group who may have stayed longer in the original municipality to find a first job. Panels b and d reveal no differential tendency of treated refugees to be in or move to urban areas, neither soon after the treatment nor in the long run.

6.3 Effects on Adult Crime

Labor market success and economic integration are very desirable and important goals for refugees. Achieving them would improve their material and psychological well-being, would increase local benefits from hosting them and would improve the opinion of natives about them (Bansak, Hainmueller, and Hangartner, 2016). In terms of affecting the opinion of citizens, however, there is an even more important dimension related to potential criminal activities of refugees. Either as a consequence of their marginalization, trauma or disenfranchisement, there is a diffused perception that refugees are more likely to commit crimes and several statistics show higher crime rates for refugees than for natives in

several European countries.[28] For Denmark, Andersen, Dustmann, and Landersøe (2016) report that the refugee crime rate is about double that of similar natives, and most of this difference is due to property crime, mainly shoplifting. Our summary statistics confirm these orders of magnitudes for the group of refugees considered in this paper.

Therefore it is interesting to analyze whether the language training and/or the reduced welfare treatment analyzed in this paper had any impact on the probability that refugees commit crime. Figure 8 shows the impact on the average probability of being convicted of any crime for the whole group of refugees every year (Panel a) and then divided by being subject to a welfare cut or not (splitting under 25s without dependents and over 25s or with children) every four years (Panel c). Then, Panels b and d show the same figures, but for the probability of being convicted of a shoplifting crime, which is by far the largest property crime committed. Panel a shows that the probability of being convicted of a crime is 11 percentage points higher for the treated group in the first year after the reform only (Panels a and d). Already in year two and in all following years, the difference from the non-treated group is very small and insignificant (or of negative sign). Panel b shows the same pattern specifically for the shoplifting crime, with an increase by 7 percentage points in the first year and no effect subsequently. As the first year after 1999 is also the period in which the subgroup over 25 years experienced reduced welfare benefits, it is interesting to analyze whether this is also the group experiencing an increase in criminal convictions. This is exactly what is shown by Panels c and d. The older group of treated refugees shows a higher propensity to be convicted in the first year, but in all the subsequent periods there is no significant effect of language treatment. It would appear, therefore, that language training has no long or short run impact on the crime rates of refugees. However, the reduction in welfare payment early on seems to be associated with higher shoplifting by the group subject to that measure. While the effect is precisely estimated, we do not want to overemphasize it due to the very short period (one year only) in which this policy was in place. Nevertheless, this finding is consistent with what Andersen, Dustmann, and Landersøe (2016) find in response to the re-introduction of lower welfare benefits to immigrants in 2002. The treated group under 25 without children never shows any difference in criminal convictions, suggesting that there is no significant effect of language training on this dimension of the refugees' social integration.

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[28]Several German politicians and newspapers linked an increase in crime in 2015 to the arrival of an exceptionally high number of refugees and asylum seekers (see BBC News, 2018).

7 Effects on Fertility and on the Second Generation

The statistically significant and economically important effects of language training on employment and earnings of refugees may have generated consequences that propagate to family life, social integration and, possibly, to the next generation. First, as treated refugees are more likely to be employed and to have obtained education in Denmark, they have a more direct connection with the Danish culture, and this may have an impact on marriage, family choices and fertility. Second, their better economic situation and higher degree of integration in the Danish society may have affected investment in their children and the outcomes of the second generation. We analyze these two aspects in turn.

7.1 Marriage and Fertility

In this section we analyze whether the better language training (and possibly the ensuing economic success) affected marriage outcomes and the probability of having children, their number and their timing (i.e. the age of the mother at birth). If better language skills (and/or working and earning higher income) imply better assimilation into the Danish culture, this may result in the modification of preferences towards marrying (outside of their ethnic group) or having fewer children (as average fertility of native Danish women is significantly lower than that in the countries of origin of refugees).

We first look at inter-ethnic marriage, namely the probability of marrying a Danish or a person of nationality different from theirs. Intermarriage is often a strong sign of social integration. The RD estimates (available upon request) do not show any significant effect of treatment on these two outcomes. We then analyze several outcomes related to fertility. Figure 9 shows four graphs illustrating the effect of treatment on four different indicators. Panel a shows the average number of children at completed fertility for refugees grouped by monthly bins and spanning two years before and after the cutoff date of arrival. The panel shows that number of children born after admission is the same on the two sides of the cutoff and no discontinuity is visible in a simple local linear approximation at the cutoff. The control as well as the treatment group both have a little more than one child after arrival. We also find no systematic differences in the timing of child births after admission. The treated and non-treated group both have a similar probability of having a child in most years, from one to eighteen, after admission (Panel b), be it the first or a later child (Panels c and d).[29] These insignificant effects on inter-marriage and on the fertility rates of the treated group imply that the language training did not change in any significant way the structure, size and composition of the families of the refugees. This implies that

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[29]The graphs look similar if we study fertility from 9 months after admission such that we only capture fertility decisions after admission status is known.

effects on their children, if present, are likely arising from the better skills, economic conditions and education of their parents rather than from changed size-type of family. We analyze children's outcomes in the next section.

7.2 Children of Refugees

In order to study the effect of parents' treatment on children we need to choose whether the treatment affects at least one parent, or specifically the father or the mother, or if both parents are treated. This choice will also determine who constitutes the control sample. As the father was admitted before the mother in 57.3 percent of the children sample, and in 19 percent of the cases the two were admitted together, choosing the treatment of the father implies, in two thirds of the cases, that both parents were treated. We consider the admission of the first parent (which implies that both parents were treated) as our main specification in column 1 of Table 3. In this case the control group includes children with zero or one treated parent. We also consider, as an alternative, having the father treated and we show the results in column 2. Alternatively, we consider admission of the last parent as the assignment to treatment (column 3) or the treatment of the mother (column 4). Both of these cases imply that it is more likely that these children have only the mother treated while the father, who arrived earlier, is untreated. Moreover, in measuring children's outcomes, in Table 3 we distinguish between boys (columns 1 to 4) and girls (columns 5 to 8).

We select all children born before 2003 as our sample, and we analyze five outcomes for them up to 2018. These outcomes are: taking the tests at the end of the lower secondary school, completing lower secondary education, enrollment in upper secondary education and criminal charges and convictions. The younger of the children considered in this sample were turning 16 in 2018, the last year of our sample, the oldest were thirty-two years old in 2018.

Children born after admission and before 2003 were typically born while their parents were attending the three-year language program and they would be of school age after their parents completed the program. Children born before admission, on the other hand, were on average 6 to 7 years old (based on the admission date of the first parent) when their parents were attending the three-year integration program.[30] The estimates of column 1 of Table 3 show that boys of treated parents were 16 percentage points more likely to reach the final exams of lower secondary education (9th grade graduation exams), 11 percentage points more likely to graduate and 16 percentage points less likely to be charged with a

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[30]Table A.2 shows descriptive statistics for children born before 2003, split into children born after admission of the last parent in Panel a and children born before admission of the last parent in Panel b.

crime by 2018, relative to similar children of non-treated refugees. No significant effects on enrolling in upper secondary education are found. The estimates on crime are weaker and smaller when we consider as treated those children with only the second parent (or the mother) treated (see columns 3 and 4). The treatment of both parents, and specifically of the father, seems to be associated with stronger and more significant crime reduction for boys. Looking at columns 5 to 8 in the table, which show the coefficient on girls' outcomes, we find very small point estimates and no significant coefficient on any of the variables. A large body of literature emphasizes how boys' basic school outcomes may be more sensitive to the family conditions and especially to the presence and role played by the father (see for instance DiPrete and Buchmann, 2013). In our case, if the father of the boy had acquired better Danish language skills, was possibly more integrated in Danish society and more likely to value the education of his children, boys may have been strongly affected by that. Figure A.8 shows a series of robustness checks for the main outcomes for which we find a significant effect on boys, namely attending exams in lower secondary school and being charged with a crime.[31] The point estimates are mostly statistically significant but vary in size across the robustness checks we perform. In general, Panels e and f of Figure A.8 show that methods that place lower weight on observations close to the cutoff and use a wider range of the data to estimate the impact produce smaller and more credible results. There are a few outlier observations near the cutoff that affect the estimates. Higher order polynomials, allowing a close fit at the cutoff, produce even more extreme observations, while OLS and a "donut" approach to RDD (leaving out two weeks on either side of the cutoff) produce smaller estimates. In most cases the estimates are negative and significant for criminal charges for boys.

8   Discussion, Cost-Benefit Analysis and Concluding Remarks

In this paper we analyze the impact that an expansion of language training for refugees in Denmark had on their employment, earnings, education and other important social outcomes. The sudden introduction of this provision that was only applicable to refugees who obtained refugee status on or after January 1, 1999 and the arbitrariness of refugees admitted on either side of the cutoff date allows a causal estimation of its effect. After eighteen years, refugees who got more and better language training had accumulated three-quarters of a year additional employment and were (permanently) about four percentage points more likely to be employed (a 23 percent rise relative to the baseline) and earned USD 2,500 more (a

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[31]Using being convicted or being charged with a crime as the outcome variable in the analysis show similar patterns. Effects on charges are higher and more often significant and we only report charges here. Similarly, we leave out girls in the robustness checks for brevity, as no significant effects are found for them.

34 percent rise relative to the baseline) than the control group. As these effects appear to be permanent, and present after 18 years, this implies a great success. It is interesting, however, to make a cost-benefit analysis of the government's investment in these language courses.

We conduct a simple cost-benefit analysis calculated as the discounted stream of benefits minus the discounted stream of costs. We include three types of costs and benefits in the base scenario. The main benefit is the increase in annual earnings. This was estimated above. The main cost is the operating costs of the extension of the language course. This is approximated by the monthly activity-based payment for integration services during the extended time spent in the language course. The language course lasted for about 18 months prior to the reform and the reform raised the resources for the language course by 30 percent, so we assume that the time in the language course increased by 6 months, taking place in the second and the third year of the program. The third component in the cost-benefit analysis is the changes to the deadweight cost of taxation. This is calculated as the impact of the reform on the fiscal budget multiplied by a tax distortion rate. The impact on the fiscal budget is the sum of the operating costs of the extended program and the effects on tax revenue and savings on welfare benefits. The savings on welfare benefits are estimated (available upon request), and the effects on tax revenue are estimated by an average tax rate times the effect on earnings estimated in section 6.1. We use a societal discount rate of 3 percent and a tax distortion rate of 50 percent in the base scenario (see e.g. Heckman et al., 2010, for a similar approach). The time horizon is the 18 years over which outcomes are measured.

The results of this cost-benefit analysis are shown in Table 4. In the base scenario, shown in the first row, the extended language course has a net present value of USD 40,000 per participant and the benefit-cost ratio is 15 with break-even after 5 years. The next four rows of Table 4 show the results when we vary central assumptions in the analysis. First, "Alternative price" is based on data from 2008 on prices per module instead of prices stated in the law we analyze (Act on Integration, section 45(5), 45(6) and 45(7) and section 59(3) and 59(4)). Second, we use a discount rate of 7 percent instead of 3 percent in the baseline. Third, we set the tax distortion rate to 0 instead of 50 percent. Finally, we include the cost of additional education obtained among the young.[32] The net present value is above 22,000 USD, the benefit-cost ratio ranges from 10 to 15 without the cost of formal education but drops to 2 if we include the cost of the additional formal education obtained among the treated. The time to break-even does not exceed 6 years. The reform has similarly large gains for the government due to income tax savings on social welfare payments and added tax payments from the increase in earnings. This also holds when we include the added costs of further education (Appendix Table B.1).

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[32]Section B in the Appendix describes and discusses the assumptions in more detail.

A second finding of the paper is that the temporary (small) reduction in welfare payments to refugees older than 25 in 1999 did not increase their propensity to work or their labor earnings, but instead may have increased their propensity to commit crimes, especially shoplifting. In this case, while there were some savings from welfare, those should, in principle, be compared with the costs of crime. However, since the major part of the increase in crime comes from increases in shoplifting, the costs are sufficiently small to be negligible.[33]

The data is more noisy and the sample is smaller when we consider the effects on the next generation. Being able, however, to establish an effect that passes onto the next generation is very powerful as it would mean a very long-lasting impact. There are two main reasons why estimated impacts on children are more noisy and uncertain. First, it seems to matter whether one or both parents were treated and, in particular, whether the father received the Danish training too, or only the mother. Second, effects vary by the gender of the child. Boys seem much more sensitive to parents' treatment and possibly especially to treatment of the father in their criminal outcomes. Other researchers focusing on economic disadvantages have found that unfavorable family backgrounds are especially harmful for boys (Bertrand and Pan, 2013; Chetty et al., 2016; Autor et al., 2019; Andersen, Dustmann, and Landersøe, 2016). Our results broadly confirm this, but we acknowledge that the estimates are noisy in this small sample and somewhat sensitive to outliers close to the cutoff. Hence, we should be more cautious when interpreting the magnitude of the impact on the second generation. The sign and significance is clear, however. Across estimates, we consistently find that better language skills and labor market integration of the parents improve boys' progress in school and reduce their crime rates. Girls, instead, perhaps because they are at lower risk of performing badly on these margins, do not seem to show any effect.

Our results suggest that investing in the language skills of refugees upon settlement in the host country puts the refugees on a better trajectory with higher human capital (a large share use the acquired language proficiency to further upgrade their qualifications), higher earnings and more employment. Furthermore, the positive results extend beyond the first generation. Children of treated parents do better in school and are less likely to enter into a criminal career. From a societal and public finance perspective, this is expensive during the first years but pays off in less than a decade.
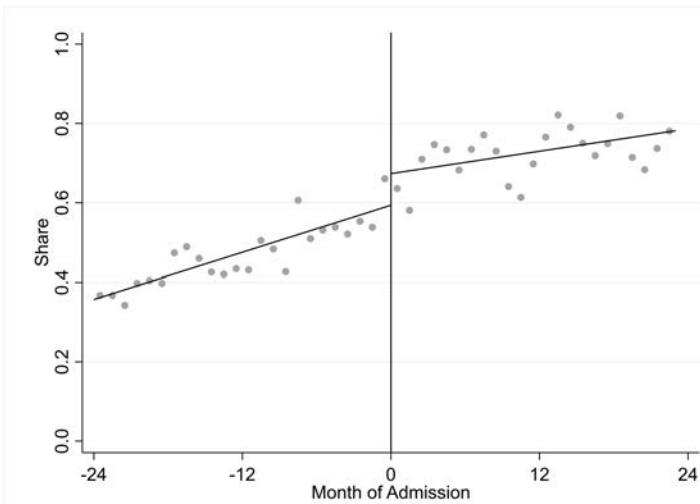
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[33]Further details on the cost of crime are found in Section B in the Appendix.

Figure 1: Cumulative Service Provision Over the First Three Years After Settlement
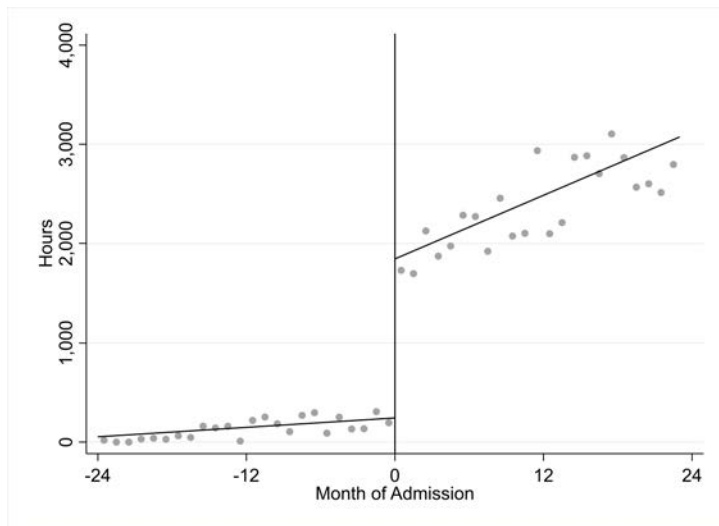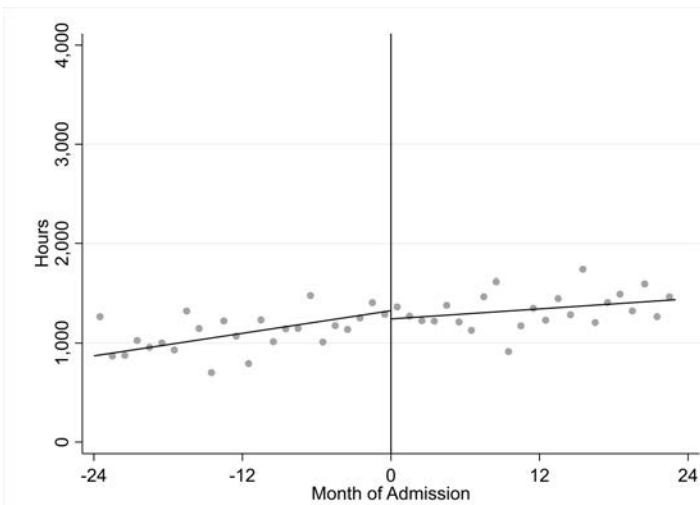
(a) Integration Services
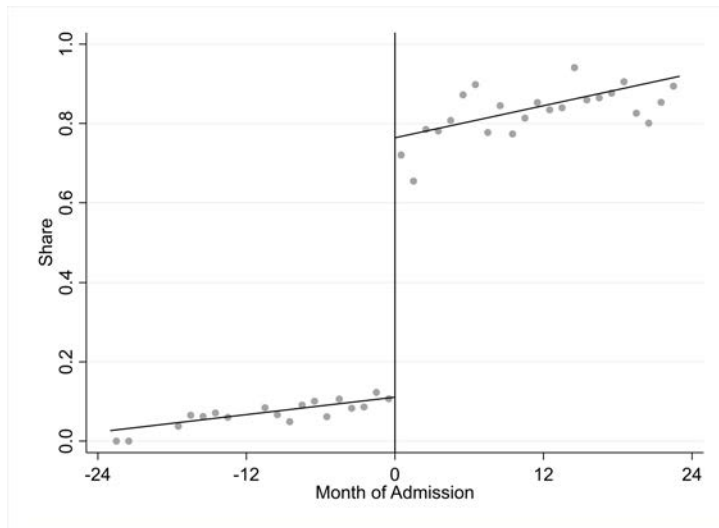
(b) Employment Services

(c) Integration Services

(d) Employment Services

Notes: The dots correspond to sample means by one-month bins. Bins with fewer than 5 observations are excluded. Panels a and b show the share ever participating, while Panels c and d show the cumulative hours. The lines are fitted by estimating linear regressions using the triangular kernel and a bandwidth of 24 months.

Figure 2: Refugees by Month of Receiving Residency

(a) Distribution of Admissions
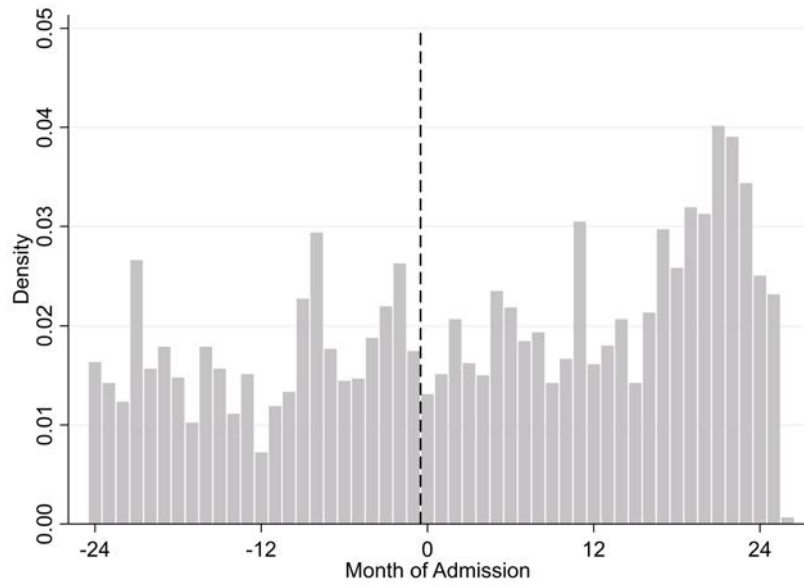
(b) Demeaned Monthly Share of Annual Admissions
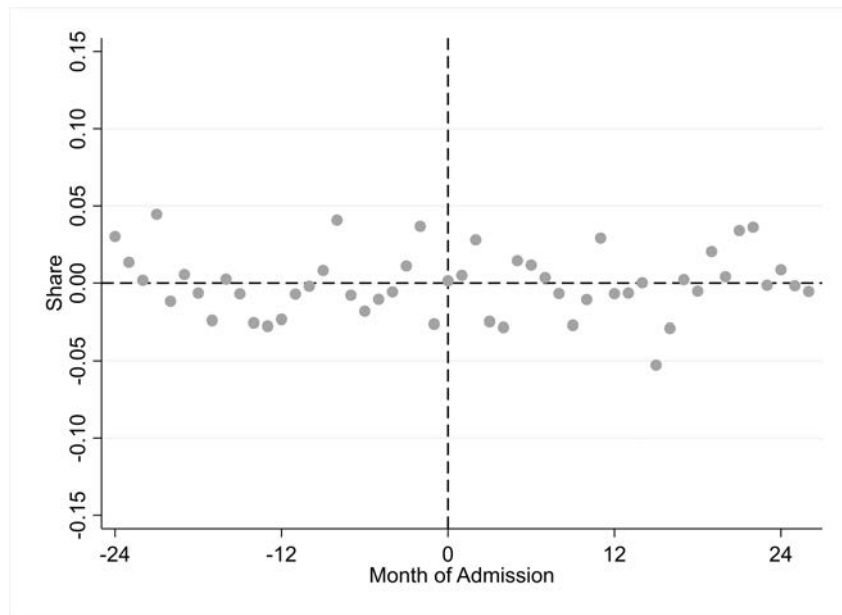
Notes: Admissions from January 1997 to February 2001. Panel a shows the density and Panel b shows the share of refugees in each month demeaned by the average monthly share of annual admissions, excluding the 6 months before and after the reform.

Table 1: Summary Statistics and Balancing Tests

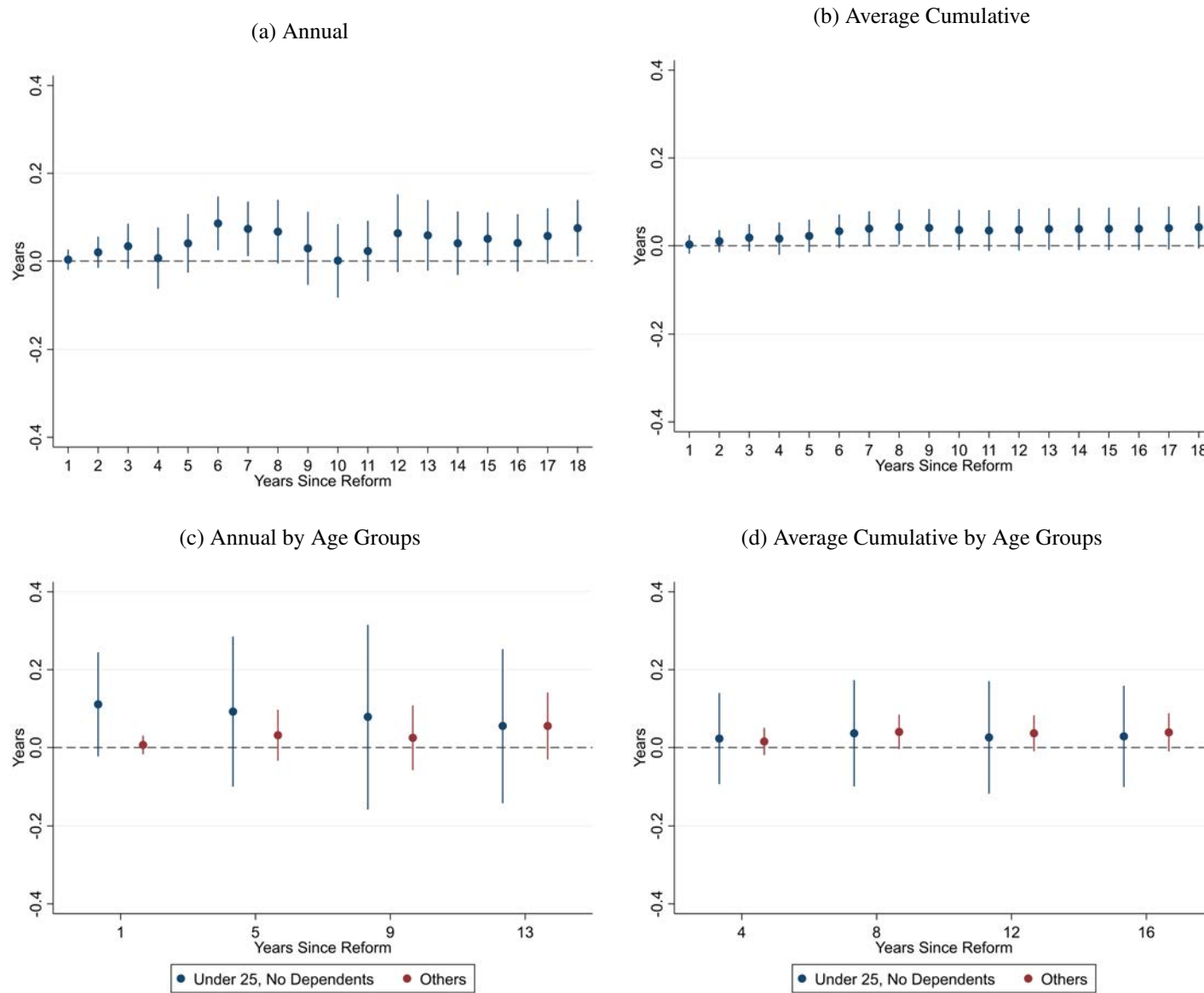|	Mean	S.D.	RD Estimate	Confidence Interval
	(1)	(2)	(3)	(4)
Panel a. Individual Characteristics				
Age	31.50	7.68	-0.39	[-1.71 ; 0.92]
Married	0.67	0.47	0.05	[-0.04 ; 0.13]
Female	0.42	0.49	0.04	[-0.04 ; 0.13]
No. Children < 3y	0.19	0.43	0.03	[-0.05 ; 0.10]
No. Children 3-17y	1.00	1.51	-0.04	[-0.42 ; 0.34]
Iraq	0.44	0.50	-0.03	[-0.14 ; 0.08]
Afghanistan	0.16	0.36	-0.02	[-0.11 ; 0.07]
Other Country	0.40	0.49	0.03	[-0.10 ; 0.15]
Speaks Arabic	0.35	0.48	-0.04	[-0.13 ; 0.05]
Speaks Dari	0.09	0.29	-0.01	[-0.05 ; 0.03]
Danish 1	0.22	0.42	-0.07	[-0.19 ; 0.05]
Danish 2	0.35	0.48	-0.00	[-0.09 ; 0.08]
Danish 3	0.32	0.46	0.05	[-0.04 ; 0.15]
Quota Refugee	0.10	0.30	-0.03	[-0.08 ; 0.02]
Convention Refugee	0.18	0.38	-0.08	[-0.19 ; 0.02]
Family-Reunified	0.19	0.40	0.03	[-0.07 ; 0.13]
Other Refugee	0.53	0.50	0.01	[-0.13 ; 0.14]
Wait Time Asylum (Days)	404.33	348.11	16.58	[-72.41 ; 105.56]
Panel b. Municipality Characteristics				
Urban Municipality	0.33	0.47	0.11	[-0.01 ; 0.24]
Five Largest Cities	0.17	0.37	0.01	[-0.08 ; 0.10]
Employment Rate 1996	0.74	0.04	-0.00	[-0.01 ; 0.01]
Unemployment Rate 1996	0.09	0.02	0.00	[-0.00 ; 0.01]
Avg. Labor Income 1996	28.52	3.69	0.43	[-0.37 ; 1.23]
Number of Co-Nationals	222.09	679.63	-23.55	[-180.01 ; 132.91]
Share of Co-Nationals	0.00	0.00	0.00	[-0.00 ; 0.00]

Notes: Summary statistics (columns 1-2) and placebo tests (columns 3-4) of the impact of the reform on predetermined variables for refugees obtaining refugee status in Denmark between January 1997 and December 2000. 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 on. Wait time for asylum is the number of days between application and admission, and it is calculated for refugees (excluding quota refugees). Municipality characteristics refer to the municipality the refugee is placed in after admission. Average income in municipality is measured in 1,000 USD (2000 level). The number of observations is 8,558, except for waiting time which is calculated for 5,956 refugees (not applicable for family-reunification or UN quota refugees).

Table 2: Summary Statistics of Outcomes

|	Mean	S.D.
	(1)	(2)
Annual Employment	0.20	0.26
Annual Earnings	8.60	12.41
Cumulative Employment Year 9	1.49	2.08
Cumulative Employment Year 18	3.61	4.64
Cumulative Earnings Year 9	57.71	86.90
Cumulative Earnings Year 18	152.41	220.28
Not Living in Initial Municipality Year 18	0.57	0.49
Living in Urban Municipality Year 18	0.55	0.50
Obtained Education in Denmark	0.13	0.33
Convicted of a Crime Year 1	0.05	0.21
Convicted of a Property Crime Year 1	0.04	0.20
Convicted of Shoplifting (Supermarket) Year 1	0.02	0.15
Ever Convicted of a Crime	0.24	0.43
Ever Convicted of a Property Crime	0.17	0.37
Ever Convicted of Shoplifting (Supermarket)	0.08	0.28

Notes: Earnings are measured in 1,000 USD (2000 level). Annual employment and earnings are the averages of annual employment and earnings in 1999-2016. The number of observations of 8,558, except for criminal convictions in year 1 where the number is 5,656.

Figure 3: Employment Effects

(a) Annual

(b) Average Cumulative

(c) Annual by Age Groups

(d) Average Cumulative by Age Groups

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panels b and d show cumulative employment divided by the number of years since the reform.
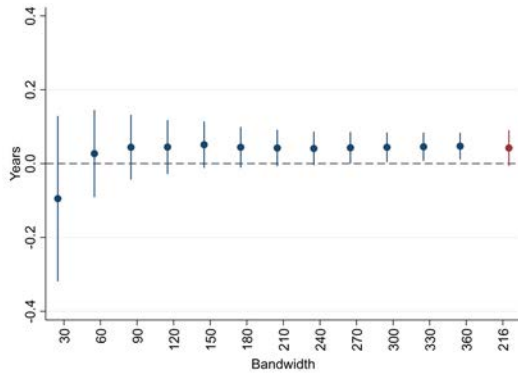
Figure 4: Earnings Effects
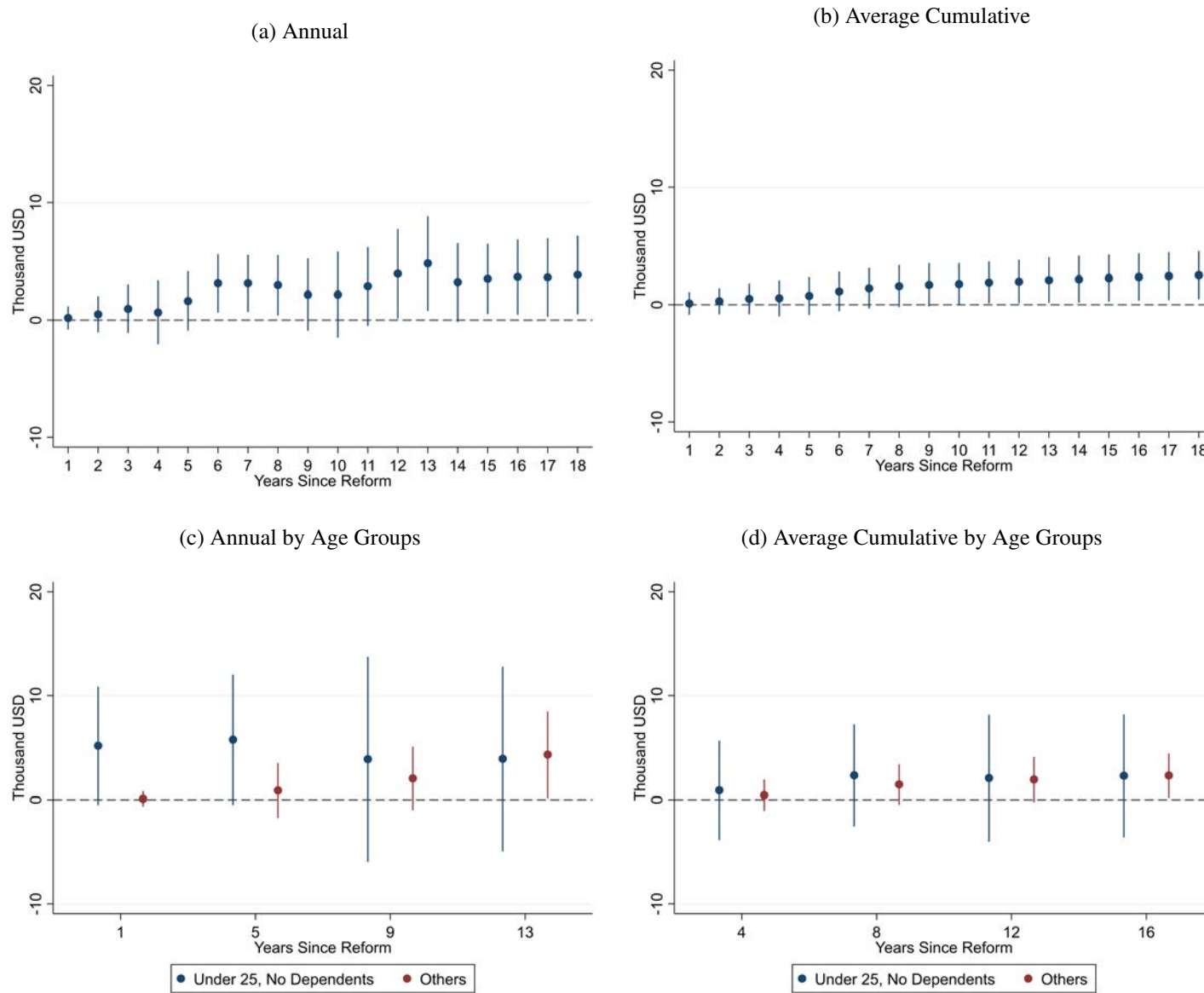
(a) Annual

(b) Average Cumulative

(c) Annual by Age Groups

(d) Average Cumulative by Age Groups

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panels b and d show cumulative earnings divided by the number of years since the reform.

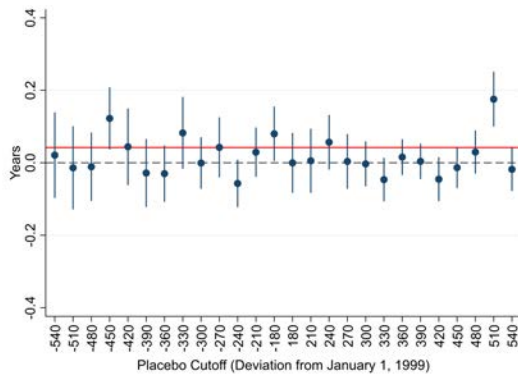Figure 5: Robustness Checks, Average Cumulative Employment and Earnings

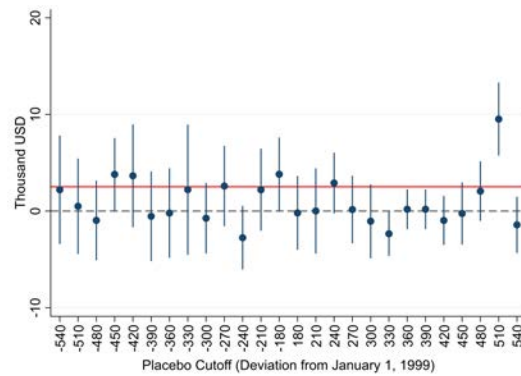(a) Bandwidth, Avg. Cumulative Employment   (b) Bandwidth, Avg. Cumulative Earnings
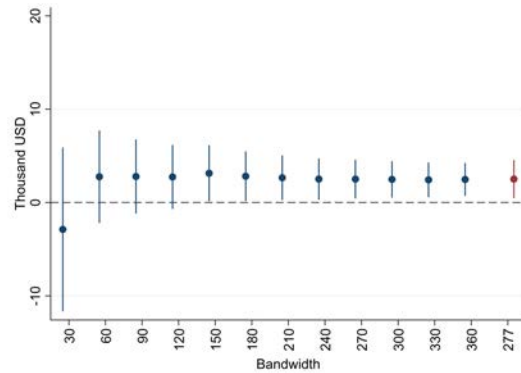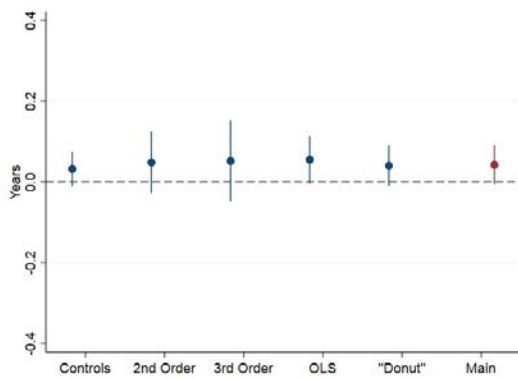
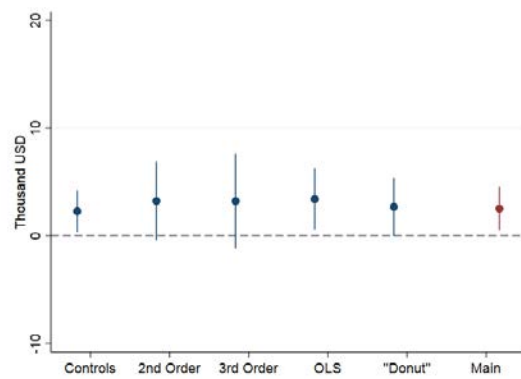(c) Placebo Cutoffs, Avg. Cumulative Employment   (d) Placebo Cutoffs, Avg. Cumulative Earnings

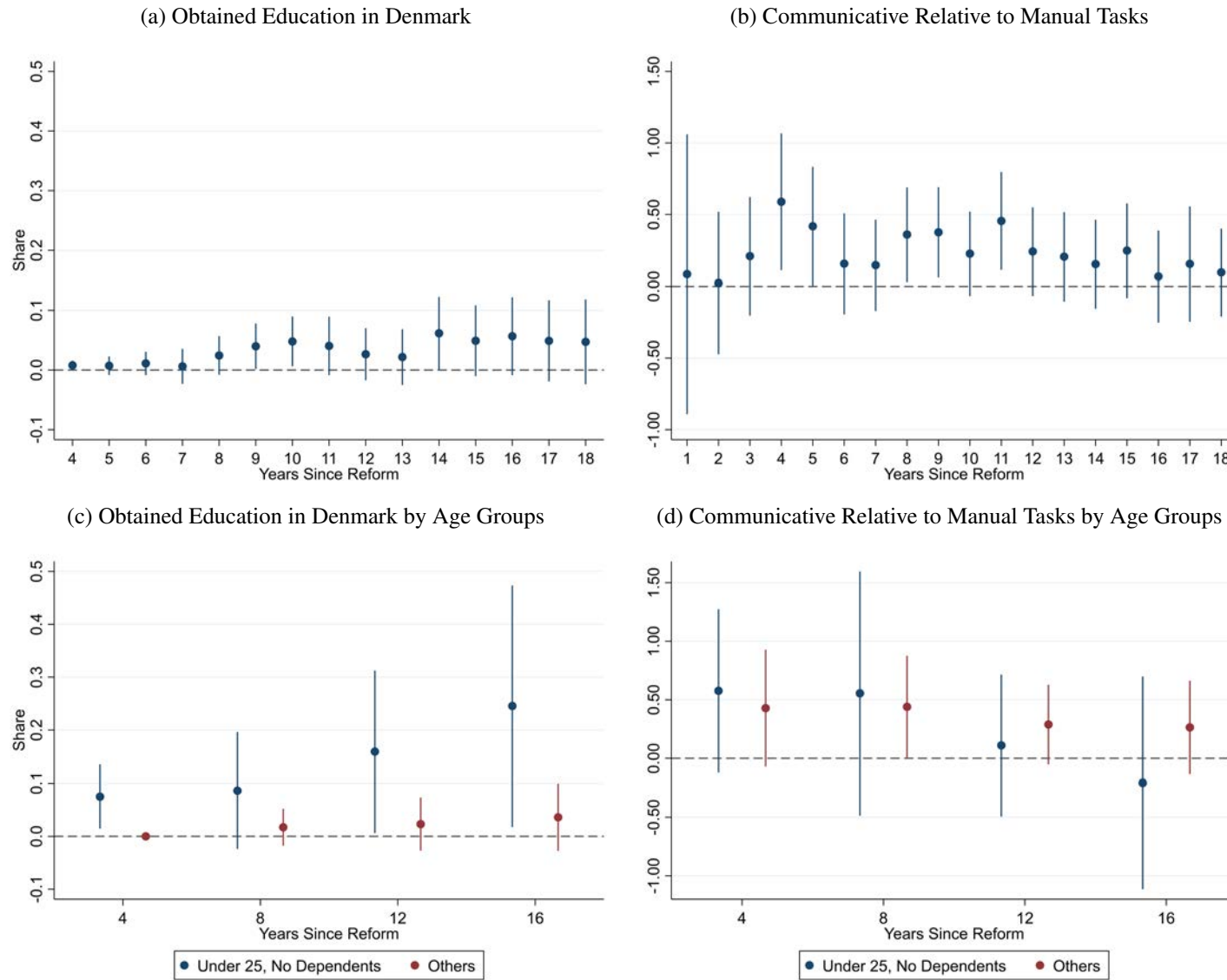(e) Avg. Cumulative Employment   (f) Avg. Cumulative Earnings

Notes: The red dot and bar are the estimate and 95-percent confidence interval from year 18 in Panel b of Figures 3 and 4 using the optimal bandwidth from Calonico et al. (2019). Panels a and b show the sensitivity to the choice of bandwidth. Panel c and d examine estimates at made-up cutoff points (red horizontal line is the estimate from Panel b of Figures 3 and 4). Panels e and f compare the main specification to estimates including control variables, using 2nd or 3rd order polynomials or OLS, and excluding refugees admitted in the four weeks immediately around the cutoff ("donut"). 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).

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Figure 6: Mechanisms Behind Employment and Earnings Effects

(a) Obtained Education in Denmark

(b) Communicative Relative to Manual Tasks

(c) Obtained Education in Denmark by Age Groups

(d) Communicative Relative to Manual Tasks by Age Groups

● Under 25, No Dependents   ● Others

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panels a and b show, respectively, the impact on obtaining education in Denmark and on the log(communicative tasks)-log(manual tasks). Panels c and d show the same outcomes by age groups.

Figure 7: Mobility Effects
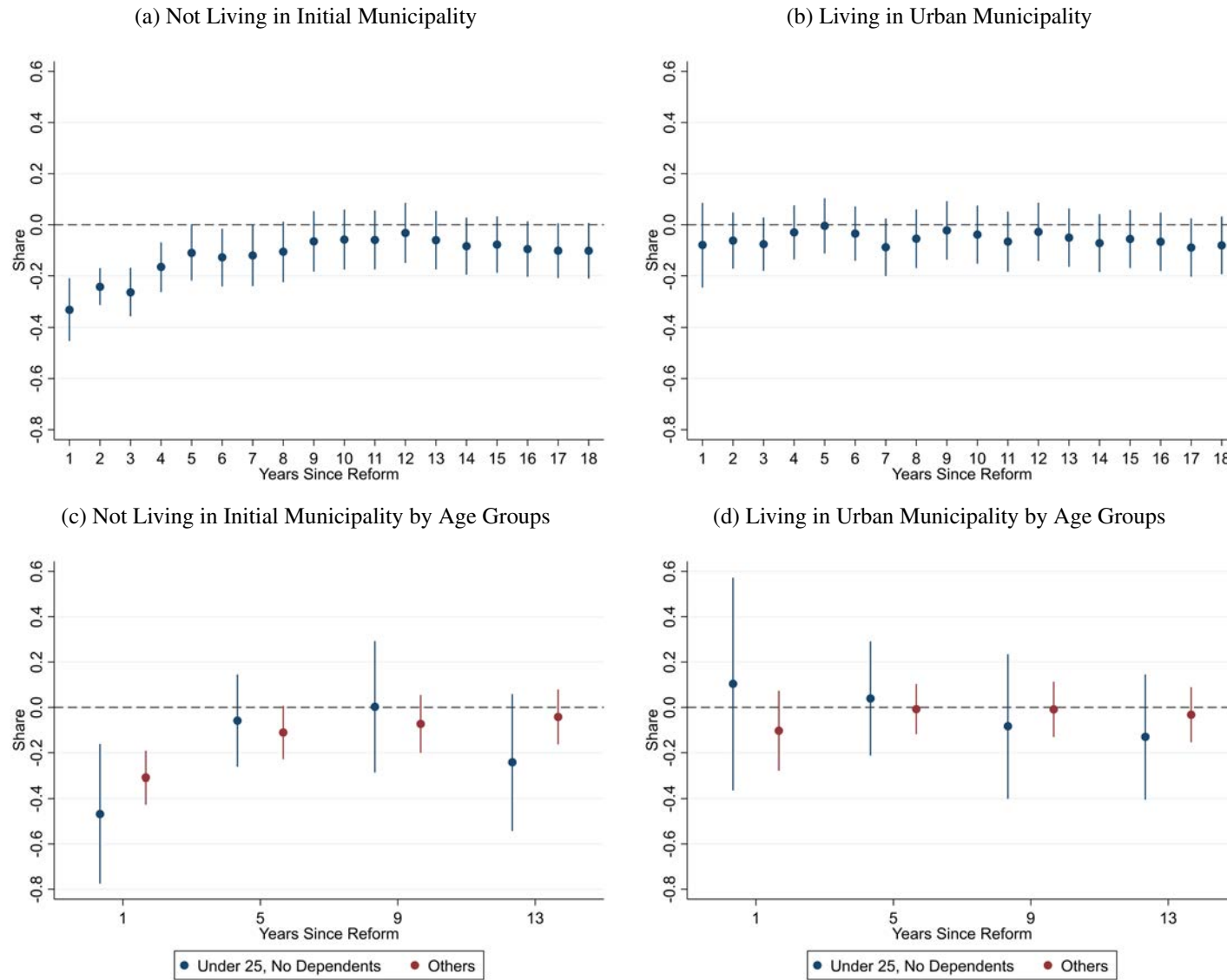
Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panels a and b show the share not living in their initial municipality and the share living in an urban municipality. Panels c and d show the same outcomes by age groups.

Figure 8: Impact on Annual Criminal Convictions

(a) All Crimes

(b) Shoplifting in Supermarkets

(c) All Crimes, by Age Groups

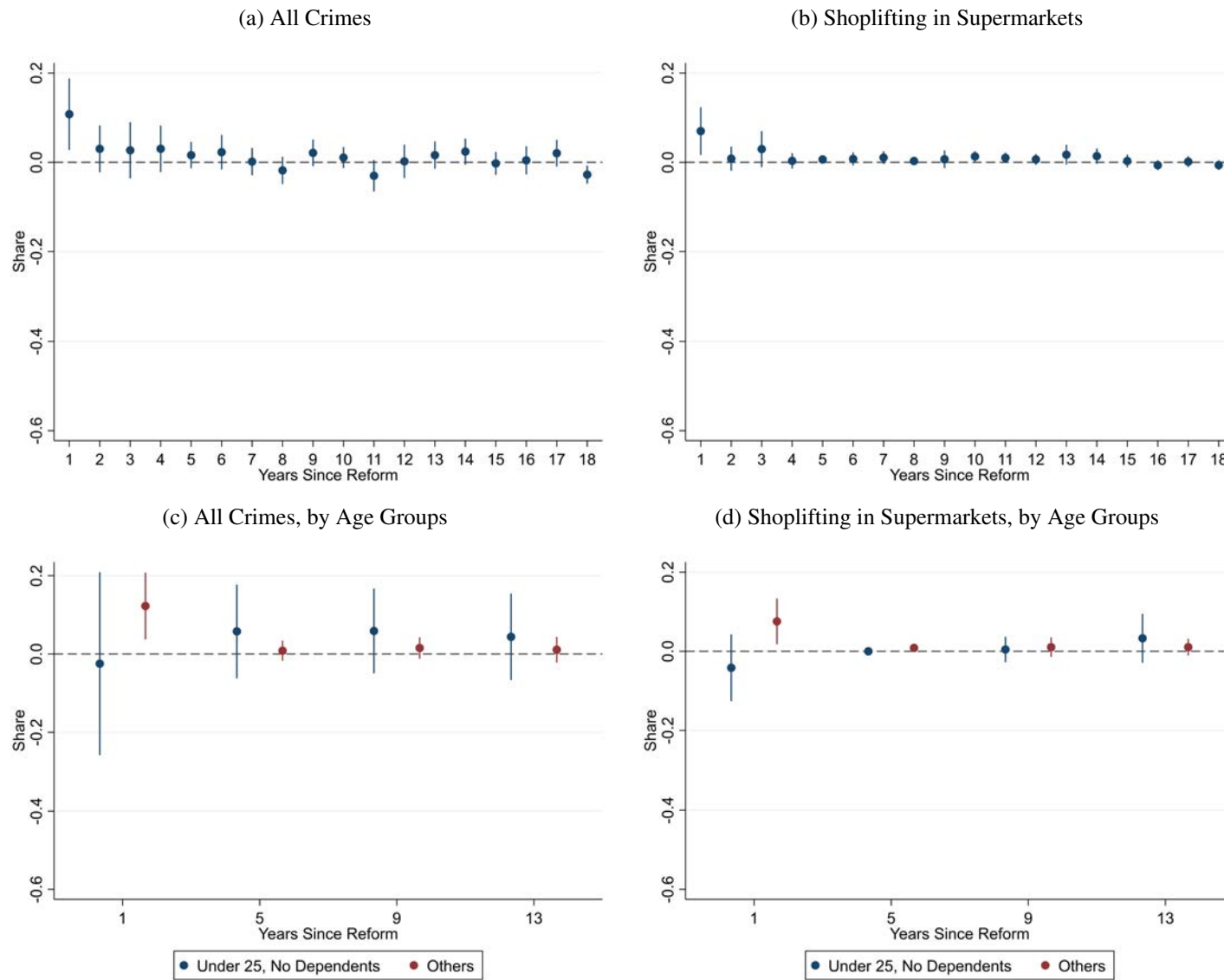(d) Shoplifting in Supermarkets, by Age Groups

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019).

Figure 9: Fertility

(a) Number of Children Born after Admission

(b) Child Born

(c) First Child

(d) Later Children

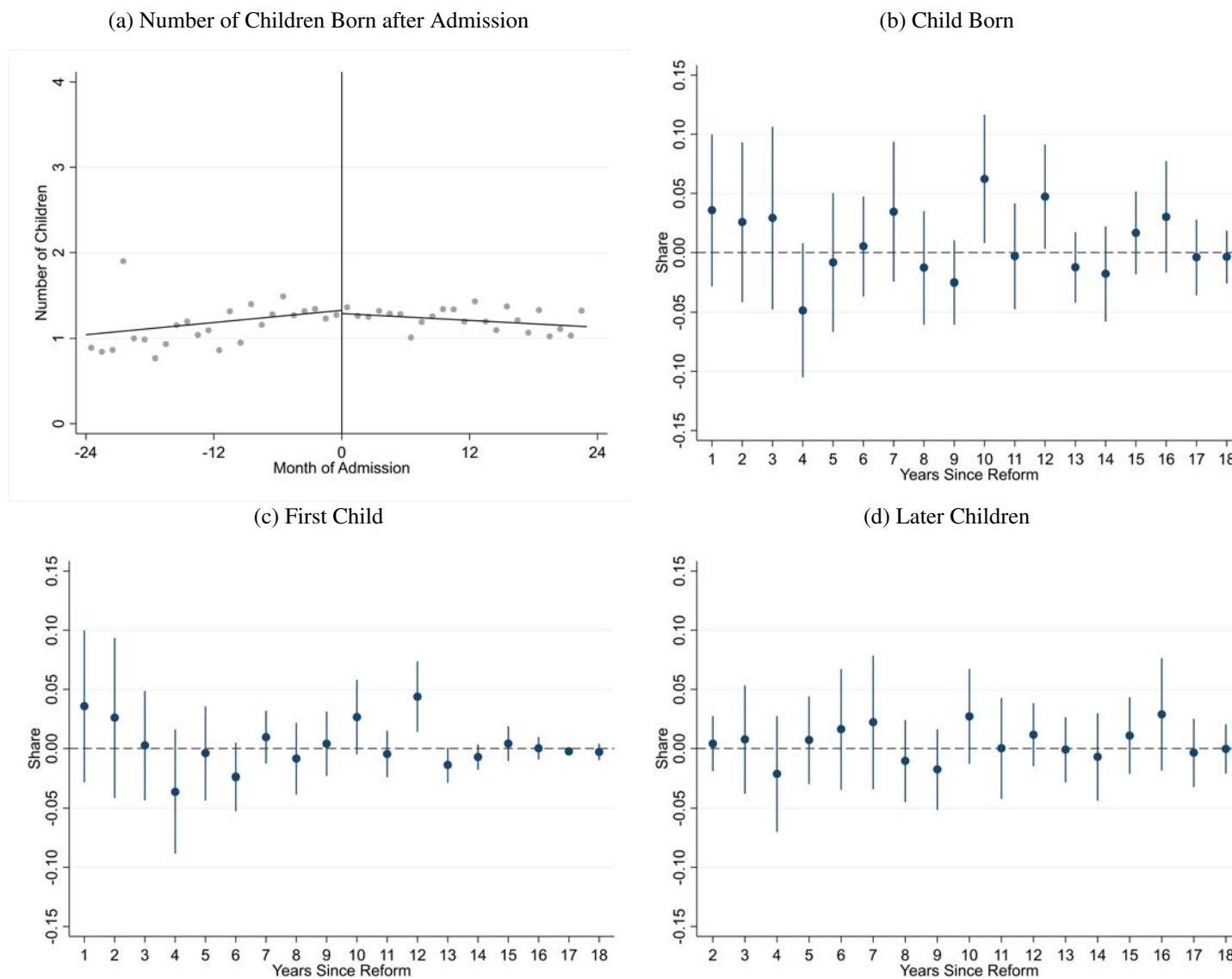*Notes:* Panel a shows the total number of children born after admission. Panels b to d show post admission fertility RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019).

Table 3: Education and Criminal Outcomes of Children

	Boys				Girls			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Took Any Exam in Lower Secondary School	0.163**	0.137*	0.238***	0.162***	0.073	0.024	-0.020	-0.006
	(0.071)	(0.082)	(0.059)	(0.056)	(0.045)	(0.049)	(0.055)	(0.050)
Graduated Lower Secondary School	0.113**	0.094**	0.103*	0.178***	0.008	-0.037	0.010	-0.009
	(0.055)	(0.047)	(0.059)	(0.066)	(0.075)	(0.071)	(0.061)	(0.063)
Enrolled in Upper Secondary Education	0.066	0.015	0.068	0.089*	-0.027	-0.015	-0.062	-0.081
	(0.053)	(0.054)	(0.051)	(0.053)	(0.073)	(0.072)	(0.060)	(0.064)
Charged with a Crime	-0.159*	-0.236**	-0.061	-0.025	0.001	-0.046	-0.013	0.023
	(0.096)	(0.101)	(0.079)	(0.074)	(0.050)	(0.039)	(0.039)	(0.044)
Convicted of a Crime	-0.111	-0.163	-0.049	-0.003	-0.028	-0.054	-0.025	-0.016
	(0.093)	(0.102)	(0.071)	(0.067)	(0.042)	(0.037)	(0.034)	(0.036)
N	5,542	4,834	5,542	5,140	4,188	3,698	4,188	3,865

Notes: $*p = 0.10, **p = 0.05, ***p = 0.01$. RD estimates from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Children of refugees born before 2003. (1) uses the admission date of the first parent. (2) uses the admission date of the father. (3) uses the admission date of the last parent. (4) uses the admission date of the mother.

Table 4: Societal Cost-Benefit Analysis

	B (1)	C (2)	DWL (3)	NPV (4)	$(B + DWL)/C$ (5)	Years Before $NPV > 0$ (6)
Base scenario	35.2	-2.8	7.7	40.1	15.4	5
Alternative price	35.2	-3.9	7.2	38.4	10.8	6
Discount rate 7%	23.0	-2.8	5.0	25.3	10.1	5
Tax distortion rate	35.2	-2.8	0.0	32.4	12.6	5
With cost of education	35.2	-16.0	3.1	22.3	2.4	6

Notes: Columns 1 to 5 are measured in 1,000 USD and column 6 is years.

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A Appendix: Additional Tables and Figures

Table A.1: Service Provision Over the First Three Years After Arrival, Treatment

	0.5 Year	1 Year	1.5 Years	2 Years	2.5 Years	3 Years
			(a) Share Ever Participating			
Total	0.316***	0.309***	0.265***	0.253***	0.201***	0.142***
	(0.052)	(0.060)	(0.060)	(0.038)	(0.040)	(0.051)
Integration Services	0.331***	0.419***	0.463***	0.509***	0.526***	0.540***
	(0.042)	(0.047)	(0.050)	(0.050)	(0.049)	(0.050)
Employment Services	0.102**	0.080**	0.029	-0.038	-0.066	-0.081
	(0.048)	(0.039)	(0.067)	(0.065)	(0.063)	(0.064)
N	8,558	8,558	8,558	8,558	8,558	8,558
			(b) Cumulative Hours			
Total	-166.354	322.636***	557.090***	949.190***	1,156.244***	1,480.112***
	(102.465)	(83.882)	(118.520)	(131.735)	(175.006)	(191.919)
Integration Services	186.886***	466.413***	735.749***	1,024.134***	1,254.709***	1,505.645***
	(30.188)	(54.807)	(69.222)	(91.399)	(115.661)	(134.303)
Employment Services	-340.585***	-128.438*	-136.104	-77.769	-63.613	-41.269
	(100.664)	(73.026)	(97.409)	(100.435)	(135.303)	(151.996)
N	3,940	4,629	5,169	5,618	6,029	6,353

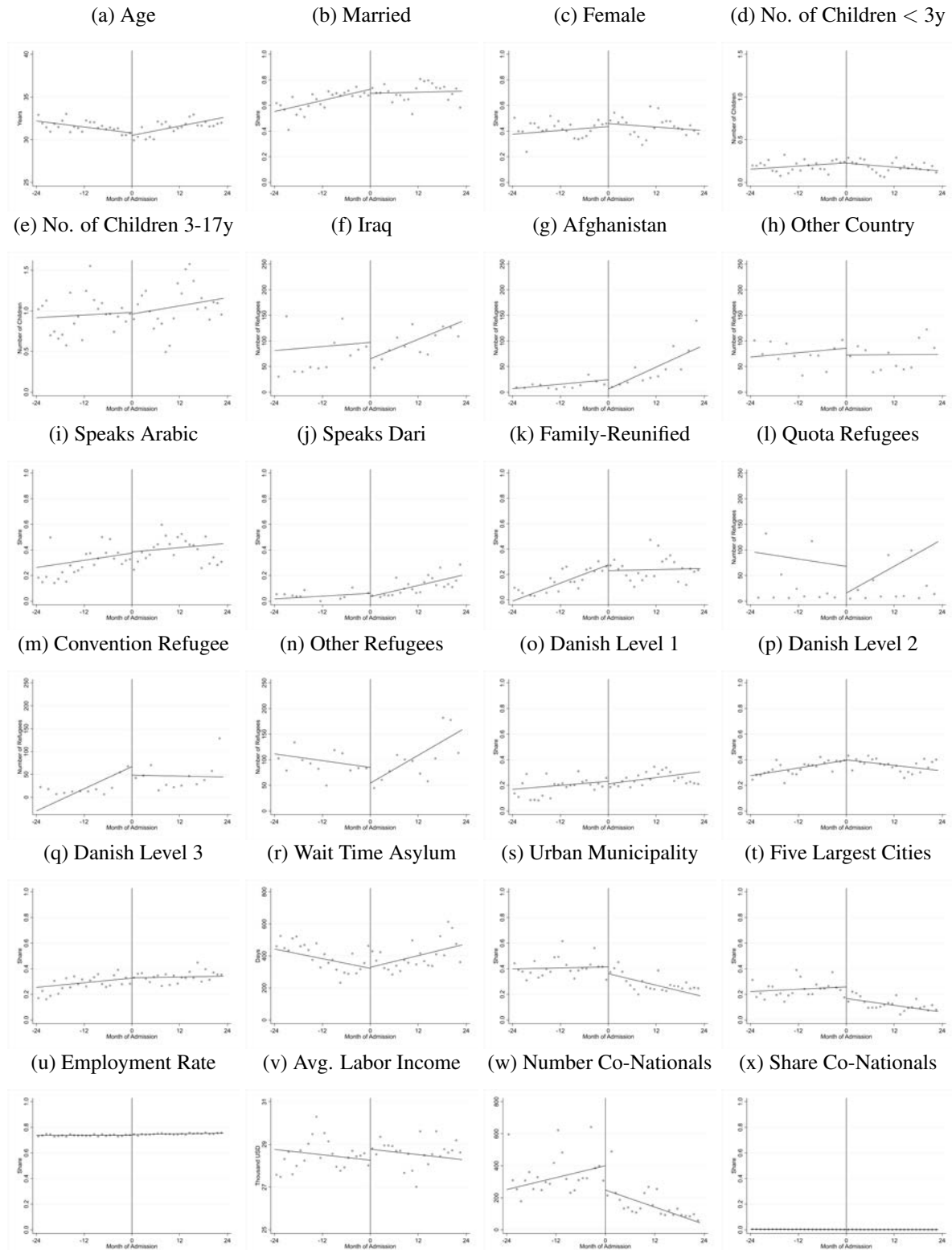*Notes:* $*p = 0.10, **p = 0.05, ***p = 0.01$. Panel a shows RD estimates for the share ever participating after 0.5-3 years. Panel b shows RD estimates for the cumulative hours conditional on participation.

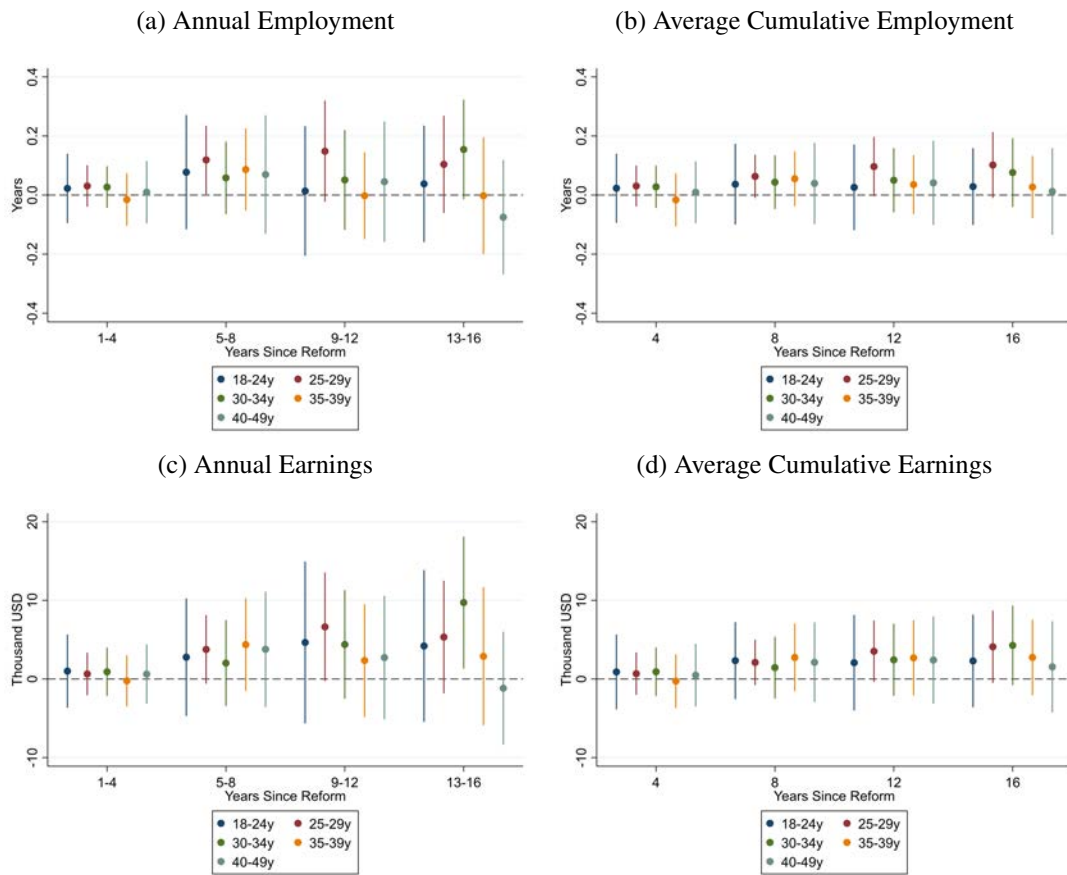Figure A.1: Language Learning Provision and Attendance, Treatment
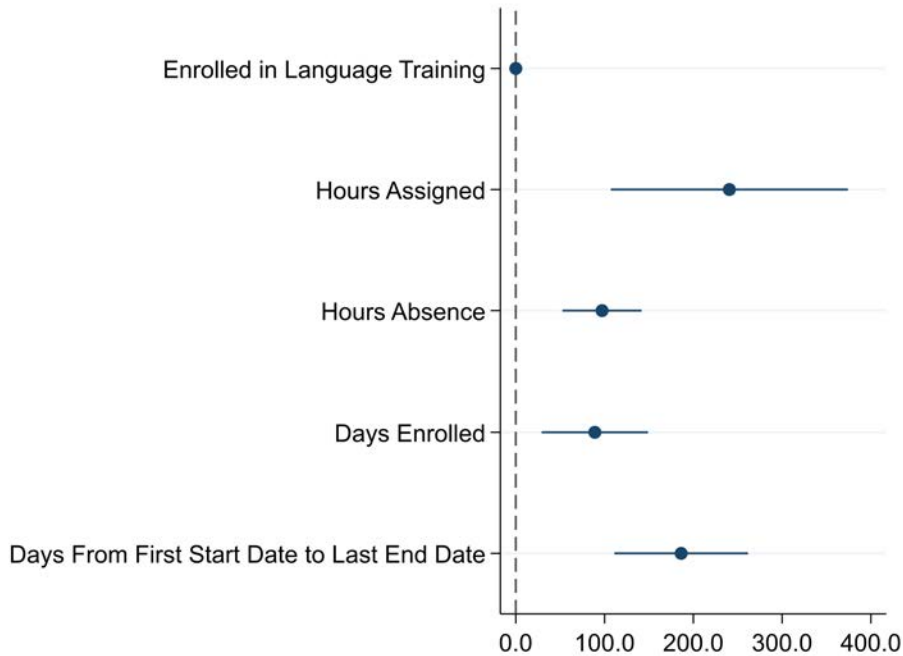
Notes: RD estimates and 95-percent confidence limits from linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019).

Figure A.2: Predetermined Covariates by Month of Admission

(a) Age   (b) Married   (c) Female   (d) No. of Children < 3y

(e) No. of Children 3-17y   (f) Iraq   (g) Afghanistan   (h) Other Country

(i) Speaks Arabic   (j) Speaks Dari   (k) Family-Reunified   (l) Quota Refugees

(m) Convention Refugee   (n) Other Refugees   (o) Danish Level 1   (p) Danish Level 2

(q) Danish Level 3   (r) Wait Time Asylum   (s) Urban Municipality   (t) Five Largest Cities

(u) Employment Rate   (v) Avg. Labor Income   (w) Number Co-Nationals   (x) Share Co-Nationals

Notes: The graphs show predetermined covariates for refugees. The dots correspond to sample means by one-month bins. Bins with less than 5 observations are excluded. Panels o to q show the Danish language training track in which a refugee was initially placed, this is a rough measure of skills from abroad (see main text).

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Figure A.3: Impacts by Age Groups

(a) Annual Employment

(b) Average Cumulative Employment

(c) Annual Earnings

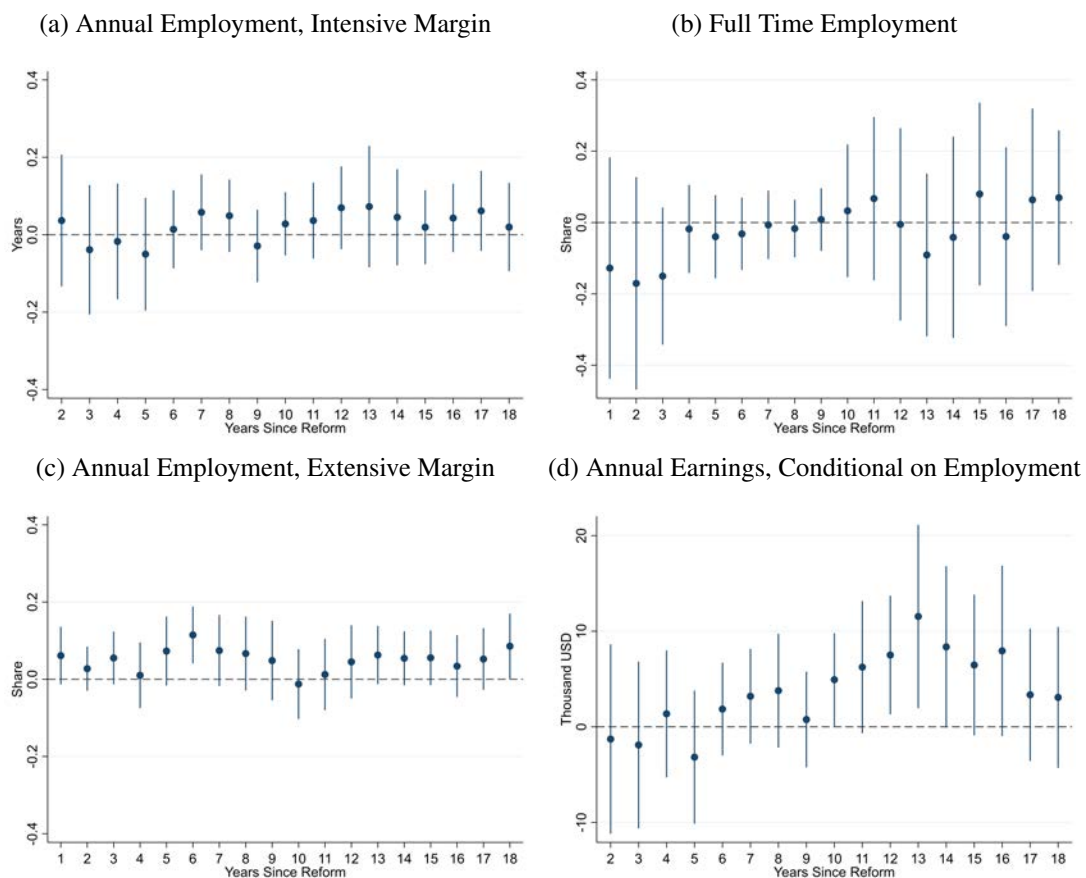### (d) Average Cumulative Earnings

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Estimates are shown for refugees without dependents for the 18-24 year age group.

Figure A.4: Sample Means of Average Annual Employment and Earnings

(a) Average Annual Employment
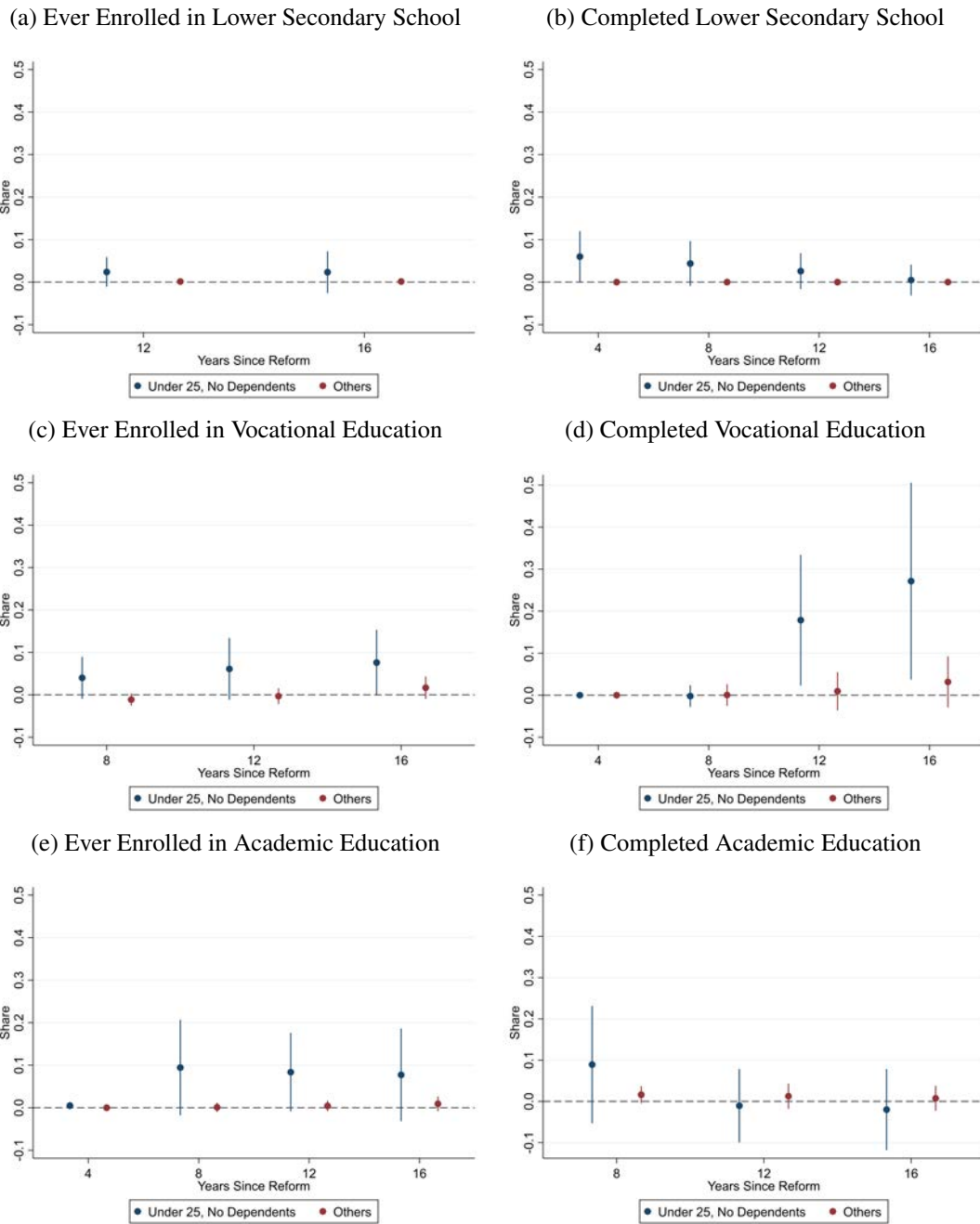

(b) Average Annual Earnings
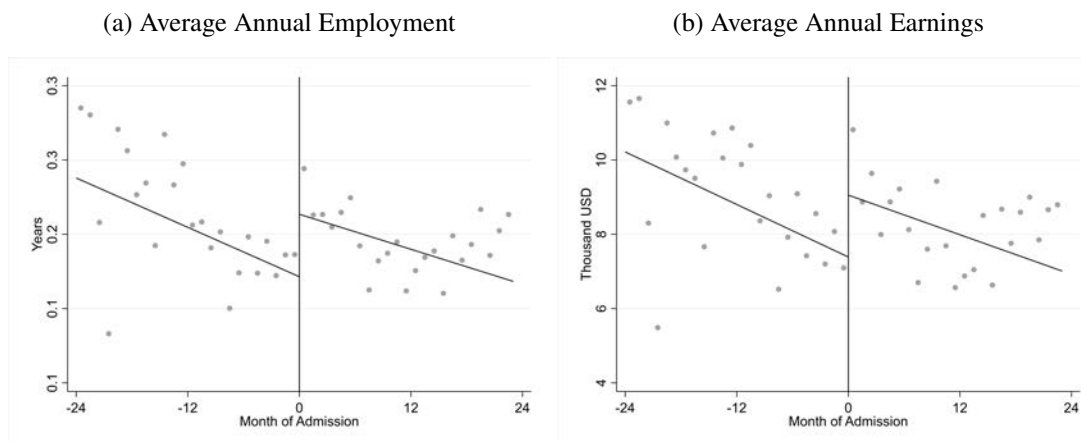
Notes: The graphs show sample means of average annual employment and average annual earnings over an 18-year-period by month of admission. Bins with less than 5 observations are excluded.

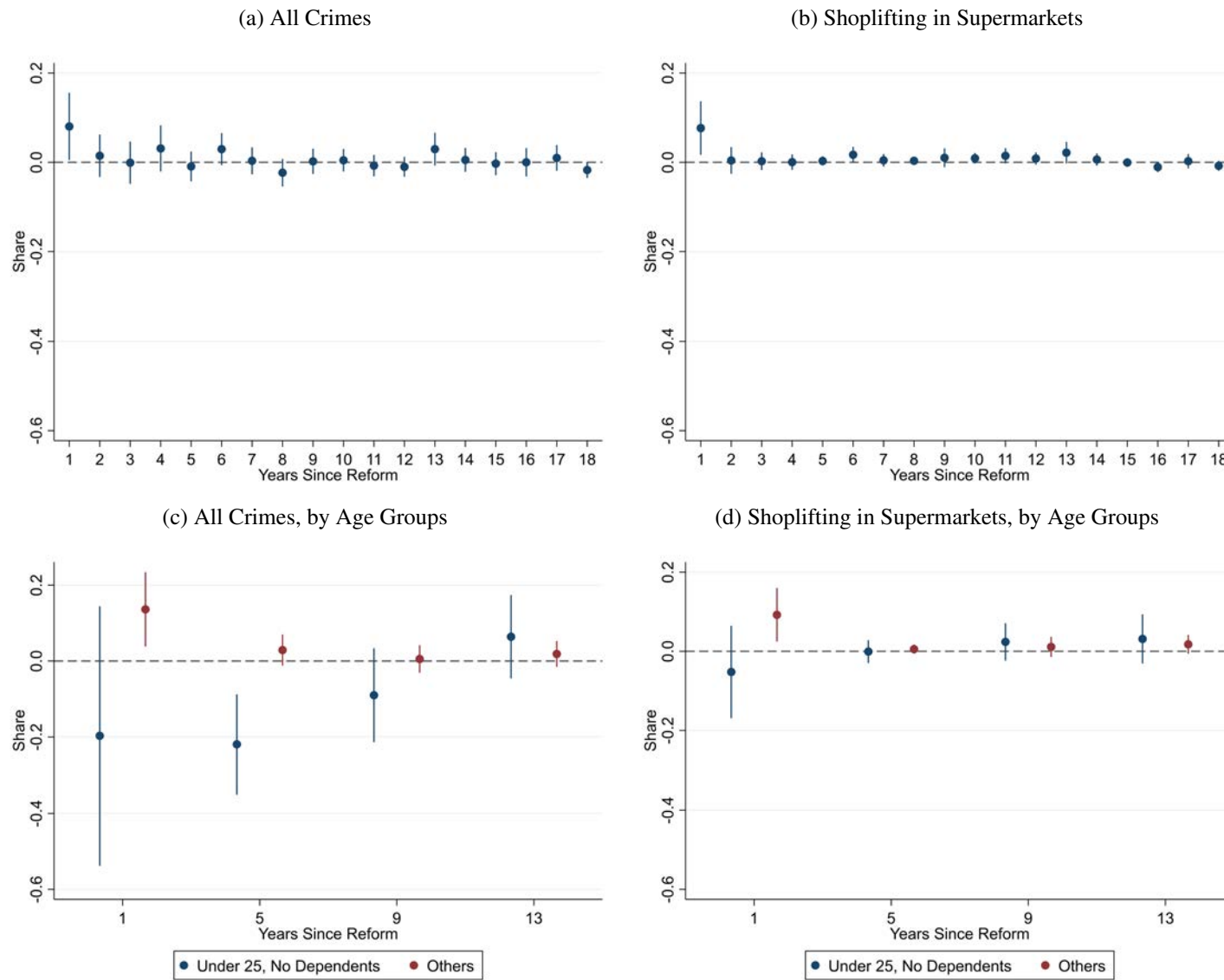Figure A.5: Margins of Employment on Earnings Responses

(a) Annual Employment, Intensive Margin

(b) Full Time Employment

(c) Annual Employment, Extensive Margin

(d) Annual Earnings, Conditional on Employment

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). Panel a shows the intensive margin response (fraction of a full time year worked conditional on working). Panel b shows the impact on full time employment relative to less than full time employment, conditional on working. Panel c shows the extensive margin employment response and Panel d the impacts on annual earnings for those who work.

Figure A.6: Education Impacts by Age Groups

(a) Ever Enrolled in Lower Secondary School

(b) Completed Lower Secondary School

(c) Ever Enrolled in Vocational Education

(d) Completed Vocational Education

(e) Ever Enrolled in Academic Education

(f) Completed Academic Education

Notes: RD estimates and 95-percent confidence limits from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019). The results are shown for refugees younger than 25 years without dependents at immigration and others.

Figure A.7: Impact on Annual Criminal Charges

(a) All Crimes

(b) Shoplifting in Supermarkets

(c) All Crimes, by Age Groups

(d) Shoplifting in Supermarkets, by Age Groups

Notes: RD estimates and 95-percent confidence intervals from local linear regressions using the triangular kernel and the optimal bandwidth selector from Calonico et al. (2019).

Table A.2: Summary Statistics for Children of Refugees

|	Panel a. Born After Admission			Panel b. Born Before Admission		
	Mean	S.D.	N	Mean	S.D.	N
Male	0.52	0.50	2,294	0.59	0.49	7,436
Year of Birth	2000	1.33	2,294	1992	3.69	7,436
Age Mother at Childbirth	28.45	5.30	2,013	25.48	5.37	6,992
Age Father at Childbirth	33.12	5.55	1,991	29.93	5.72	6,535
All Parents Refugees or Family-Reunified	0.85	0.36	2,294	0.96	0.19	7,436
Parents Admitted on Same Day	0.20	0.40	2,294	0.25	0.43	7,436
Parents Admitted Before Reform	0.39	0.49	2,294	0.64	0.48	7,436
One Parent Admitted Before Reform	0.17	0.38	2,294	0.07	0.25	7,436
Parents Admitted After Reform	0.44	0.50	2,294	0.29	0.45	7,436

Notes: Panel a shows summary statistics for children born between 1997-2002 after their last refugee parent was admitted. Panel b shows the same statistics for children of refugees born before their parents were admitted.

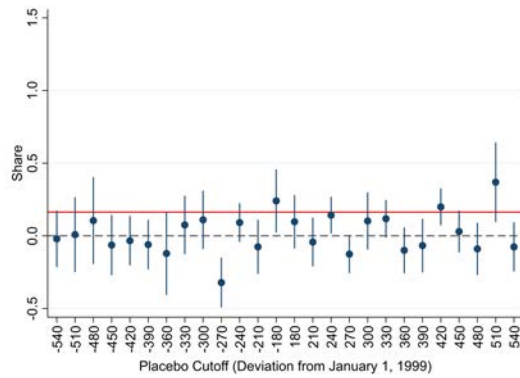## Figure A.8: Robustness Checks, Boys

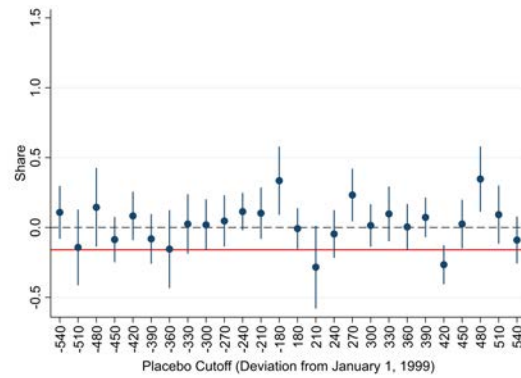(a) Bandwidth, Took Any Exam in Lower Secondary School
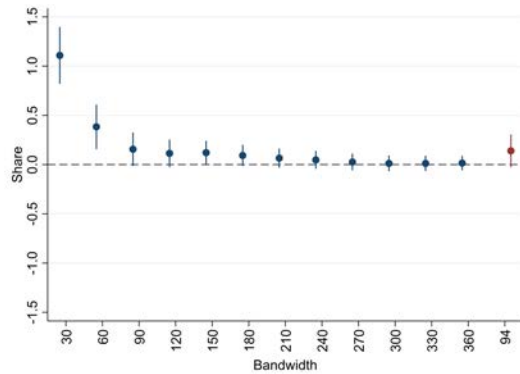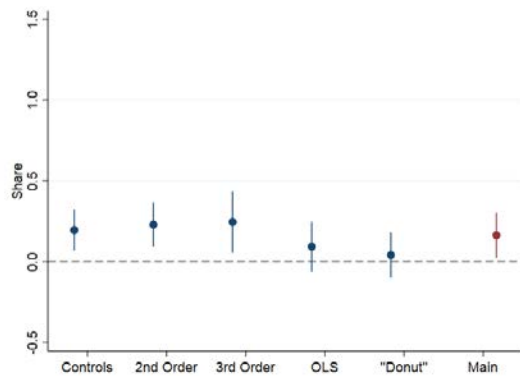
(b) Bandwidth, Charged with a Crime
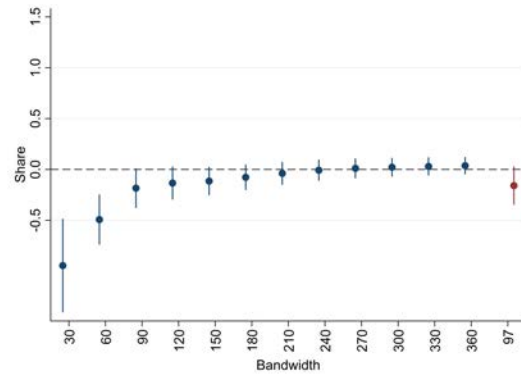
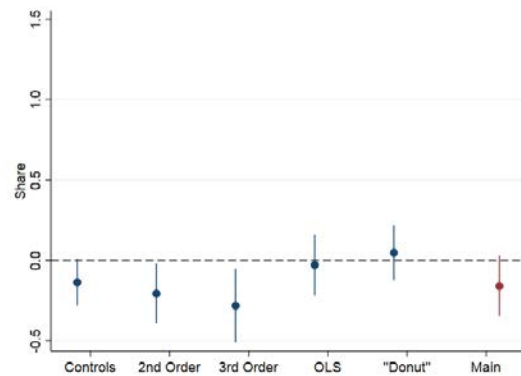(c) Placebo Cutoffs, Took Any Exam in Lower Secondary School

(d) Placebo Cutoffs, Charged with a Crime

(e) Took Any Exam in Lower Secondary School

(f) Charged with a Crime

Notes: The red dots and bars show the estimate and 95-percent confidence interval from column 1 of Table 3 using the optimal bandwidth from Calonico et al. (2019). The sample is boys born before 2003 using the first parental admission date as assignment to treatment. Panels a and b show the sensitivity to the choice of bandwidth. Panels c and d examine estimates at made-up cutoff points. The red solid line shows the estimates at the true cutoff. Panels e and f compare the main specification (red) to estimates including parental control variables, using 2nd or 3rd order polynomials or OLS, and excluding refugees admitted in the four weeks immediately around the cutoff ("donut"). 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, Danish 1, 2 or 3 (unknown level is the reference), quota refugee, family-reunification or other refugee (convention refugee is the reference).

B Appendix: Cost-Benefit Analysis of the Language Training Expansion

We calculate the net present value (NPV) of the expansion of language training as:

$$NPV = B - DWL - C = \sum_{i=1}^{18} \frac{b_i - dwl_i - c_i}{(1+r)^i}$$

Where B are the benefits, C are the costs, and r is the societal discount rate. DWL is the deadweight loss (or gain) from changes in the distortion of taxation as a result of the impact on the fiscal budget. We also calculate the benefit-cost ratio, and the time needed before the NPV is positive.

We include gross earnings as the only benefit. They are estimated directly in section 6.1. It implies that any positive impacts on the offspring are neglected. The costs of the additional hours provided in the language course is measured by the operating costs described in section 3 and taken directly from the law we analyze (Act on Integration, section 45(5), 45(6) and 45(7) and section 59(3) and 59(4)). In addition to these costs and benefits, we include the deadweight loss to society resulting from the impact on the fiscal budget. In the main analysis we assume a tax distortion rate of 50 percent (see e.g. Heckman et al., 2010, for a similar approach).

We calculate the net present value of the change in the fiscal budget as the sum of the changes in tax revenue, savings on welfare benefits and the added costs of the program. Here we assume an average income tax rate of 32 percent for workers and of 26 percent for the unemployed.[34] The savings on welfare benefits are not counted as a benefit in the cost-benefit analysis for society, as it is a transfer (redistribution). All Danish prices are converted to Danish price levels in 2000 using the consumer price index and to USD using an exchange rate of 6.6 DKK/USD.[35]

Hence, in the base scenario (in the first row of Table 4 in the main text) we make the following assumptions:

1. The reform extends the previous language learning provision by 30 percent (430 hours) and increases the maximum duration of the program from 18 months to 3 years (see section 3 in the main text). We assume the additional language instruction is equal to 6 months (0.3×18 months = 6 months), and assume that the operating costs per month equal the activity-based payment of DKK 3,300 to the municipality. We split the costs equally between the second and third year of

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[34]This is based on mean average tax payments for the employed and unemployed in a report by Rambøll and the Danish Center for Social Science Research (2013), the best estimates of the relevant average tax rates we are aware of.

[35]These conversions are applied to all nominal variables throughout the paper.

the program.

2. The discount rate is 3 percent.

3. The tax distortion rate is 50 percent.

We conduct simple sensitivity analyses for each of the items above and add an additional cost through the impact on education (in row two to four of Table 4 in the main text):

1. Use the earliest operating costs with direct information on prices per module available (`www.integrationsbarometer.dk`). This is from 2008 and the price is deflated to 2000 prices. The average pay per module in 2008 was DKK 33,735, 20,064 and 17,394 for a participant in Danish course 1, 2 and 3, respectively. We use a weighted average with the share of participants at each course level in 1999 (0.32, 0.35, 0.33; Table 2). We assume one module is one sixth of the total hours ($1,830/6 = 340$ hours), so the extra hours amount to 1.4 modules.[36]

2. Discount rate is 7 percent.

3. The tax distortion rate is 0 percent (this is the worst case since it enters as savings in the net costs).

4. Include the costs of additional education for the population below 25 (Appendix Figure A.6).[37] They constitute 13.7 percent of the sample. The price is approximated by the public expenditure per pupil for basic, vocational and academic education. We use the simple average across types of education, and hence not weighted by uptake of immigrants. It is assumed that basic schooling completion takes two years, vocational education takes four years and academic education takes five years. The earliest year for which we have information on prices is 2014 for basic and vocational education and from 2007 for academic education. They are deflated to 2000 prices using the consumer price index. The added cost of education is not included in the base scenario because educational attainment takes time, and we are therefore not seeing all the benefits that comes from it.

We neglect the cost of crime because it is assessed to be very small. This is based on the following: The welfare reduction raised criminal convictions in the first year by 10 percent, arising mainly from an increase in shoplifting. The sentence for shoplifting is a small fine in Denmark, so the costs consist

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[36]The total hours is 1,830 after the reform and there are six modules after the reform. This structure did not exist before. This approach gives as a higher estimate of the operating costs of the program compared to our baseline.

[37]For basic schooling and vocational training: `https://www.uvm.dk/institutioner-og-drift/oekonomi-og-drift/regulerede-institutioner/takstkatalog-og-finanslov/takstkatalog` and for academic education: `https://ufm.dk/uddannelse/videregaende-uddannelse/universiteter/okonomi/uddannelsestilskud` (accessed December 12, 2019).

of resources spent by the police, the court, the defence and the prosecution. Using previous estimates of these costs, an estimate of the cost of such a crime is USD 200. Results of the societal cost-benefit analysis are shown in Table 4 and discussed in section 8.

The impact on the fiscal budget is shown in Table B.1 below. Income tax and savings on social welfare payments are positive for the public coffers and the operating costs are the costs for the public coffers.[38]

Although we do not provide estimates of the uncertainty of the numbers in Table B.1, they are all sufficiently large to support the conclusion that the additional investment in language training was beneficial to the society and the government. The net monetary benefit for the refugee is simply the sum of the impact on the difference in post-tax earnings and benefits because the program is free of charge for the participants. This difference is also large and positive, effectively ruling out that the net benefit for the recipient, which could include mental costs of attending the course, is negative.

Table B.1: Fiscal Cost-Benefit Analysis

Government	Income Tax and Welfare Savings (1)	Operating Costs (2)	NPV (3)	B/C (4)	Years Before $NPV > 0$ (5)
Base scenario	18.0	-2.6	15.4	7.0	6
Alternative price	18.0	-3.6	14.4	5.0	6
Discount rate 7%	12.4	-2.4	10.0	5.3	6
With education	18.0	-12.0	6.3	1.5	6

Notes: The rows are the different scenarios described in the text. Columns 1 to 4 are measured in 1,000 USD and column 5 is years.

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[38]Note a change in the deadweight loss from distortionary taxation is only a cost (or benefit in our case) from the perspective of society and is therefore not part of the fiscal cost-benefit analysis.