

Kilström, Matilda; Larsen, Birthe; Olme, Elisabet

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Should I Stay or Must I Go? Temporary Protection and Refugee Outcomes

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**Copenhagen
Business School**
HANDELSHØJSKOLEN

Department of Economics

Copenhagen Business School

Working paper 5-2018

Should I Stay or Must I Go?

Temporary Protection and Refugee
Outcomes

Birthe Larsen
Matilda Kilström
Elisabet Olme

Should I Stay or Must I Go?

Temporary Protection and Refugee Outcomes*

Matilda Kilström[†]

Birthe Larsen[‡]

Elisabet Olme[§]

May 9, 2018

Abstract

We study a Danish reform in 2002 that lowered the ex ante probability of refugees receiving permanent residency by prolonging the time period before they were eligible to apply for permanent residency. Assignment to the new rules was completely determined by the date of the asylum application and the reform was implemented retroactively. We formulate a simple search and matching model to derive predictions that can be tested using our data. Then we study the effects on educational and labor market outcomes and find that the reform significantly increased enrollment in formal education, especially for females and low skilled individuals. In terms of employment and earnings, coefficients are in general negative but non-significant. Other outcomes of interest are also studied. The reform had a negative impact on criminal activity driven by a reduction among males. There are no effects on health outcomes and significant but relatively small negative effects on childbearing for females. The results do not seem to be driven by selection, since the reform had no significant effect on the share that stayed in Denmark in the long run.

JEL Codes: J15, J24, J61, K37

Keywords: refugees, human capital, immigration law

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[†]IIES Stockholm University, matilda.kilstrom@iies.su.se

[‡]Copenhagen Business School, bl.eco@cbs.dk

[§]Stockholm University, elisabet.olme@ne.su.se

1 Introduction

Recent developments around the world have led to a large inflow of asylum seekers to Europe. In response to the increased numbers of asylum seekers, many European countries have implemented stricter immigration policies. The motivation has been to reduce immigration and/or improve integration of immigrants granted residency. One such policy is the shift from permanent to temporary residence permits for refugees.¹ Although several countries have, or are about to, implement such reforms, we do not have any empirical evidence on their effects on refugees' integration in society in general and on the labor market in particular. A priori, it is possible to think that a shift to temporary permits could have both positive and negative effects on integration in society and on the labor market. On the one hand, the expected return to investment in country specific human capital falls if the probability of receiving permanent residency falls. On the other hand, actions that lead to labor market attachment during the time with temporary residency are incentivised when they increase the probability for permanent residency.

In the public debate, stronger incentives for labor market investments in the host country have been put forward as an argument in favor of temporary permits. Proponents argue that this policy will strengthen incentives to integrate. At the same time, opponents have highlighted the cost of increased stress from a lower probability of being granted permanent residency, and a decrease in expected returns to investments in country specific human capital. Because we still know very little about the impact of this type of policy it is important to try to understand its effects. Specifically, we want to address the following question: what are the effects of changes to the probability of being granted permanent residency? Because of the potentially counteracting effects discussed above, this is ultimately an empirical question which remains to be answered. Furthermore, while immigrants' entrance to the labor market is relatively well studied, less attention has been given to the specific challenges of those given refuge in a new country. In fact, we know very little about the integration process of refugees and their labor market prospects. In a recent paper, *Fasani et al.* (2018) show that refugees have worse labor market outcomes compared to other immigrants across European countries. This calls for more research focusing on refugee immigration to Europe, with a focus on immigrants' outcomes.

In this paper we study the effects of a Danish reform, implemented in 2002 as part of a reform package, that changed the eligibility requirements for permanent residency, thereby lowering the ex ante probability of being granted a permanent residence permit. This was done by increasing the length of the time period a refugee would have had to be a legal resident (on temporary residence permits) in Denmark before being eligible to apply for permanent residency. During the time with temporary status a residence permit could be withdrawn if the grounds for protection were no longer valid, and if the individual did not have the right to stay based on other grounds, such as for

¹For example, in July 2016 Sweden introduced a temporary law shifting from permanent to temporary residence permits and limiting access to family reunification. Among several other changes to refugee policy, in December 2014 Australia reintroduced temporary protection visas - that cannot be changed into permanent status - for those who arrive without a valid visa.

example through labor market attachment. The way that this reform component was implemented allows us to distinguish the effect of a longer temporary period from other reform components, as the change we are studying was the only one implemented retroactively. The change applied to individuals who lodged their first asylum application on or after February 28, 2002. This meant that refugees who applied for asylum from February 28, 2002 and onward faced a longer time period with temporary status, during which they risked losing the grounds for protection, before they could apply for permanent residency. All else equal, the ex ante probability of receiving permanent residency in Denmark on the grounds of asylum was thus higher prior to the reform.² We study the reform in a regression discontinuity in time setting. Register data allows us to track individuals granted asylum and observe a large set of outcomes over time. Thus, we are able to empirically dig deeper into the mechanisms at work and to consider the impact of the reform on different subgroups of refugees. We are interested in the behavioral responses to this reform component, and our focus is on outcomes that are relevant for integration and/or the assessment of grounds for prolonged residency, and that the individual could affect herself. Our main outcomes are therefore in terms of educational investments and status on the labor market. Labor market attachment can in itself be viewed as a measure of integration, whereas education can be considered as an investment in integration. We study the full sample as well as sample splits based on gender and skill level (high skill denotes individuals with university education while low skill is below university education).

Our results suggest that lowering the ex ante probability of receiving permanent residency increased enrollment in education by 17 percentage points at the cutoff point. Enrollment is measured as the share that is ever enrolled in education (not including Danish courses) during the first twelve years of residency in Denmark. We also show higher enrollment rates for the treatment group throughout the twelve years by plotting enrollment rates over time. The increase in enrollment is mainly driven by females and low skilled individuals. The effect for females is an increase of around 21–27 percentage points at the cutoff. We interpret the positive effect on enrollment in education as an increased investment in human capital and integration. To understand the impact of this increase we consider several other educational outcomes. We find no significant effects on enrolling in labor market training or adult education, on the propensity to complete an education or the number of years in education. In terms of labor market outcomes we focus on the share of individuals that are ever employed (or self-employed) during the first twelve years in Denmark, and on their earnings measured three and seven years after arrival. We do not estimate any significant effects on labor market outcomes but the coefficients are negative and, if anything, suggest a negative impact of the reform in the long run. This is also true when we look at earnings conditional on employment. There is no change in the number of times individuals change jobs,

²This, however, does not mean that the reform necessarily changed whether an individual got to stay in Denmark or not. In fact, we show that around 90 percent of individuals (in both the control and treatment group) are still in Denmark twelve years after their first arrival. The individual could (1) have had asylum reasons throughout the time period with temporary permits, or (2) established a labor market attachment which could be used as grounds for prolonged temporary residency. Although a refugee had no control over the development in their home country, or the Danish authorities' assessment of whether grounds for asylum were still valid, they could affect their attachment to the labor market and thus affect the probability of staying in Denmark.

but for high skilled individuals we do find a marginally significant negative effect on the highest skill level ever achieved on a job. One potential explanation for this could be if high skilled accept jobs they are over-qualified for, or if employers are reluctant to invest in high skilled workers with a more uncertain future in Denmark.

There are other outcomes of interest in this context. We study whether the reform had any effect on criminal activity, health or fertility behavior. Engaging in criminal activity could be seen as an alternative to entering the regular labor market, but criminal activities also make it harder to get permanent residency after the reform. Increased uncertainty from a lower probability of permanent residency may also have a direct effect on an individual's health, which in turn could affect future labor market outcomes. In terms of fertility, the reform and the implied increase in uncertainty may have affected the willingness to bring children into the world. Finally, we are interested in asylum holders' duration in Denmark, for two reasons. First, the reform could have affected the willingness and ability to stay in Denmark as the prerequisites to stay changed, which in itself is an interesting outcome. Second, if the fraction staying in Denmark changed, results on other outcomes may be driven by this selection rather than by behavioral responses among those staying in Denmark. We find a negative effect on conviction rates for property crimes during the first twelve years in Denmark of around 10 percentage points. This decrease is concentrated among males. We find no significant differences between the two groups in terms of health, fertility or the share that is still in Denmark twelve years after their first asylum application. The latter finding suggests that any effects we pick up are unlikely to be due to an indirect effect - operating through selection - that would occur if some group was more likely than another to stay in Denmark.

To further understand the mechanisms at work we set up a theoretical search and matching model, to derive predictions that can be compared to our empirical findings. This model focuses on labor market outcomes and includes heterogeneity in terms of productivity - with high and low skilled agents - as well as a human capital investment decision. We use the model to study education and labor market outcomes under different assumptions about the impact of the reform. In particular, the empirical findings on education are in line with the implications from our theoretical setting where low skilled individuals are, *ex ante*, more negatively affected by the reform. Empirically we think that this group consists of individuals further away from the labor market. In the model this is true regardless of whether they are employed or unemployed.

The remainder of this paper is organized as follows. Section 2 gives a brief overview of the related literature and Section 3 describes the institutional settings in Denmark and the reform that we are studying. Section 4 presents a theoretical framework to analyze the potential effects, while Section 5 describes the empirical strategy, as well as the data and the identification strategy. In Section 6 we present our main results and robustness checks are performed in Section 7. Section 8 presents data and results on other outcomes, while Section 9 concludes.

2 Related literature

There is to our knowledge no other paper that specifically studies the long-run effects on refugees of a prolonged temporary status. Changes to immigration policy are particularly difficult to evaluate due to difficulties in finding a valid comparison group. Previously, some studies have compared different types of immigrants to assess the importance of, for example, the time horizon in the host country. As different types of immigrant may differ in important ways we would preferably like to study the effects within one specific group of immigrants. At the same time, even when looking at one type of immigrants (for example refugees) there may be substantial heterogeneity. It is well known that the characteristics of refugees from a given country can change over time. All of this implies that estimating the effects of changes to the probability of being granted permanent residency is challenging.

There are related studies that consider the difference between temporary and permanent migration spells in other contexts. For example, [Chen *et al.* \(2016\)](#) study the selection into temporary or permanent migration. Temporary, short-term, migration is typically a response to fluctuations in the local labor market, while long-term migration is more stable. They show that long-term migrants are more strongly positively selected and relate this to higher returns to matching. [Cortes \(2004\)](#) analyzes heterogeneity between refugees and economic immigrants in terms of their time horizons. Assuming that refugees cannot return to their country of origin, and thus face a longer time horizon, they have stronger incentives to invest in country-specific human capital. Her study is related to, and theoretically builds on, [Duleep & Regets \(1999\)](#) and their model on human capital accumulation. Furthermore, [Orrenius & Zavodny \(2015\)](#) study the effects on labor market outcomes of granting specific groups of immigrants temporary protected status (TPS) in the United States, and show that, in general, it appears that even having a temporary permit - compared to illegal status - improves labor market opportunities for immigrants. In [Cortes \(2004\)](#) and [Orrenius & Zavodny \(2015\)](#), a distinction is made between immigrants with different time horizons (refugees vs. economic immigrants) and between immigrants with different legal status. One benefit of our setup is that we can look at the importance of the time horizon and status in the host country within one group of immigrants, refugees. Arguably, refugees are likely to be different in many aspects compared to for example economic immigrants, and, since they constitute a more marginalized group in relation to the labor market, it is important to understand the effects of changing conditions for them specifically.

Several papers study immigration and crime and how policy matters in this context. In a recent paper, [Pinotti \(2017\)](#) uses a regression discontinuity design to show that immigrant legalization reduces crime rates among immigrants in Italy. The proposed mechanism is that legalization increases the opportunity cost of crime by improving access to the regular labor market. [Mastrobuoni & Pinotti \(2015\)](#) find a reduction in crime following the European Union enlargement. [Baker \(2015\)](#) also finds a negative effect on crime from legalization of undocumented immigrants in the United States. In [Lozano & Sørensen \(2011\)](#), the authors study the effect of legalization on earnings among Mexican immigrants in the United States, and find an increase in occupational

wages. They interpret their findings as support of immigrants finding better jobs following legalization. Fasani (2016) finds small and non-persistent reductions in crime following a wave of amnesty programs in Italy. Furthermore, Fasani (2015) highlights the importance of policy design in shaping effects of legalization on crime. Considering a different type of outcome, Dustmann *et al.* (2017) study consumption effects of legalization. They show that undocumented immigrants consume less than documented immigrants, and argue that this is because of lower income. More closely related to the outcomes studied in this paper, Devillanova *et al.* (2014) study employment effects of legalization following an amnesty program in Italy and find positive effects of prospective legal status on employment probability. Legalization policies are clearly important to study, but they are conceptually different to policies of permanent or temporary residence permits. It is not necessarily the case that findings from the legalization literature translate to other types of policy changes, such as the one we study.

Another closely related paper in terms of the type of policy studied is Blomqvist *et al.* (2018). They study the short-run effects, over a one-year horizon, of restricting access to permanent residency in Sweden and find some evidence of a higher probability of enrolling in basic Swedish education. Finally, Mansouri *et al.* (2010) provide a comparative study of temporary permit regimes in Denmark, Germany and Australia. Through interviews with NGOs they conclude that introducing temporary residence permits, or prolonging the temporary status, increased uncertainty for refugees and suggest that integration has been made more difficult as a result. A key advantage of our study is that we are able to quantify the response to prolonged temporary status and that we can study the mechanisms through which refugees were affected.

There are several relevant papers using Danish data to study immigrants' outcomes. Clausen *et al.* (2009) analyze the effects on labor market integration for immigrants from Danish active labor market programs (ALMPs). They find mixed effects depending on the type of program, but in general positive effects from language training and participation in wage subsidy programs.³ Other aspects of the 2002 reform package in Denmark have also been studied. There were several aspects to the general reform package; notably limiting access to the welfare state and to family reunification. Huynh *et al.* (2007) study the employment effects of limiting access to the welfare state, finding positive employment effects from reduced benefits. The authors exploit the discontinuity that arises from the fact that only those granted asylum after July 1, 2002 were subject to the new benefit rules. Similarly, Rosholm & Vejlin (2010) analyze the effects of lowering benefits on both job finding and job separation rates. Rather than using a regression discontinuity approach they implement a mixed proportional hazard model. In line with the evidence from Huynh *et al.* (2007) the authors find small positive effects on the job finding rate. In this paper we instead study another part of the reform package to shed light on the effects of lower ex ante probability of receiving permanent residence.

³See Sarvimäki & Hämäläinen (2016) for a paper on ALMP in Finland. They find positive effects on earnings following compliant participation.

3 Institutional settings

Denmark has seen the number of asylum applicants vary a lot over the years. 2001 denoted a peak in the number of asylum seekers, and between 2001 and 2002 the number of asylum seekers was cut in half to 6,068 from 12,512, with most of the asylum seekers arriving from Afghanistan, Iraq and Former Republic of Yugoslavia. This is the time period of immediate interest for us, and as we will see in Section 3.1 it is a time period of substantial change in terms of asylum policies.

The process of applying for asylum in Denmark is governed by the Aliens Act and the decisions are made by the Danish Immigration Service (DIS), while appeals are handled by the Refugee Appeals Board.⁴ The process of applying for asylum in Denmark and the different types of permits are described in more detail in Appendix A.

3.1 The 2002 reform package

On November 27, 2001, a new minority centre-right-wing coalition government was appointed in Denmark. This shift of government reflected a shift in public opinion on immigration (see for example Mansouri *et al.* (2010)). The new government introduced a number of legislative changes regarding asylum and immigration policies that were passed by the Danish parliament as amendments to the Aliens Act and the Integration Act. We will study the effects of a reform component that changed the criteria for eligibility for permanent residency in Denmark (henceforth referred to as the reform). This change was part of a suggestion for a new Bill to amend the Danish Integration Act, presented by the new government in February 2002 (Ersbøll & Gravesen, 2010). The Bill was passed by the Danish parliament (Folketinget) on June 6 2002.⁵ The explicit aim of this reform package was to limit the number of asylum seekers to Denmark, while still respecting international obligations, and speed up the integration process (The Danish Immigration Service, 2003).

Both prior to and post the reform, individuals given asylum were initially granted a temporary residence permit if protection was deemed necessary. During the temporary status, the residence permit could be discontinued if the grounds for residency were no longer valid. Generally, temporary protection would remain valid if the need for protection remained and there were no legal reasons to withdraw it.⁶ Refugees could also be allowed to remain on a temporary residence permit based on labor market attachment, even if there was no longer any need for protection. After a certain time period as a resident in Denmark, a refugee (above 18 years old) would be eligible to apply for permanent residency.

The main change to the eligibility to apply for permanent residency was the change in how

⁴Individuals granted asylum for humanitarian reasons are an exception, and in these cases the Ministry for Foreigners, Integration and Housing (in 2002, the Ministry for Integration) makes the decision. If an asylum seeker's application is rejected, he/she can still be given asylum for humanitarian reasons.

⁵Discussions began in January when a new aliens policy was introduced, and this gave rise to the suggested Bill to amend the Integration Act in February. The Bill that was eventually adopted implied changes to the Aliens Act as well. Bill no. L 152 entered into force as Act no. 365 of June 6 2002.

⁶Paragraph 11 in the Aliens Act.

long one would have had to be a legal resident in Denmark on a temporary residence permit. Prior to the reform, three years was sufficient, whereas after the reform a refugee would have to wait for seven years until able to apply for a permanent residence permit.⁷ This change implied that individuals subject to the new rules would have to live with temporary protection for a longer time period, facing the risk of having their permit discontinued. Once eligible to apply for permanent residency, refugees would be granted permanent residence if the need for protection remained or if they had a labor market attachment (given fulfillment of some supplementary conditions), unless there were legal reasons to withdraw the residence permit. Prior to the reform, these conditions included completing an integration program and having limited public debt. Under the new rules, in addition to completing the integration course, asylum seekers would now have to pass a language test and have no overdue public debt. In addition, while a criminal record would previously lead to longer waiting time, a serious criminal record would after the reform prevent permanent residency (Ersbøll & Gravesen, 2010). In total this means that getting permanent residency was made more difficult by the reform.

In addition to the changes in requirements for being eligible to apply for permanent residency, the 2002 reform package also lowered benefit levels, made family reunification more difficult, abolished the *de facto* status and the possibility to apply for asylum at Danish embassies abroad. We are able to isolate the effect of changes to the eligibility for permanent residency from other parts of the reform package. This was the only component introduced retroactively and it applied to all individuals who *lodged* their asylum application on or after February 28, 2002 (the date on which the new Bill was proposed). The other components of the reform package took effect after the Bill was passed, on July 1, 2002. For more details about the other components of the reform, see Appendix B.

Another potentially important reform came in 2003, allowing immigrants that had lodged their applications on or after February 28, 2002 to apply for permanent residency already after five years if they were "well integrated", i.e. if they had strong labor market attachment and had not relied on social welfare.⁸ Furthermore, in case of exceptional reasons in terms of successful integration, it was possible to receive a permanent permit also after three years of legal residency (Ersbøll & Gravesen, 2010). What this implies for our analysis is that the integration motive was made stronger.⁹

The key take away from the policy change introduced by Act no. 365 of June 6 2002, for our

⁷Formally, the reform implied that if the refugee had held a legal permit on the basis of paragraph 7–9 of the Aliens Act for at least seven years, counting from the date of approval of the temporary permit, he/she was eligible to apply for permanent residency. Paragraphs 7–9 included permits for all the categories of refugees that we consider, and, in particular, paragraph 9 included specific permits based on labor market attachment.

⁸This was an addition to paragraph 11 in the Aliens Act, entered into force as Act no. 425 of June 10 2003, and the formal requirement implies that the applicant should have lived legally in Denmark for at least five years and have been self-supporting with a solid labor market attachment for the last three years.

⁹During 2002 there were some other important changes to decision practices for specific refugee groups. These are unrelated to the policy changes studied in this paper, but they are relevant to highlight since they affected the approval rates for specific nationalities. In particular, changes applied to asylum seekers from Afghanistan, Iraq and Kosovo, for whom, following a reassessment of the security situations, requirements for asylum were made stricter.

analysis, is that it implied a lower ex ante probability of being granted permanent residency based on asylum reasons. At the same time, following this reform there was still the possibility of getting permanent residency through labor market attachment, and one potential effect is that this option became more important. In terms of this incentive we think that different groups of refugees may have been differentially affected. In particular, we do not think that all groups face the same labor market prospects, and therefore the option of securing residency through labor market attachment will be more realistic for some groups than others. The types of differences we will focus on in the empirical analysis are in terms of gender and skill levels.

4 Theoretical framework

Changes to the probability of being granted permanent residency in Denmark could affect labor market and education investments in the host country in two opposite directions. On the one hand, an ex ante lower probability of permanent residency based on asylum reasons could increase incentives to qualify for permanent residency based on labor market attachment, and thus increase investments in country specific human capital. Such investments could for example be getting an education. If this was the case, we would expect to see positive effects on educational and/or labor market outcomes. On the other hand, with a lower probability of staying in the long run, the expected payoff to country specific investments is lower. The lower probability could in this case deter asylum seekers and we would expect to see negative effects on educational and/or labor market outcomes. A key argument in favor of temporary protection is the idea of its positive effects on integration. This claim is, however, clearly subject to verification because of the potentially counteracting effects. To shed light on this question we set up a search and matching model, that includes a choice of whether or not to invest in human capital, to derive predictions that can be tested in our empirical setting.¹⁰

The theoretical model aims to provide a framework that can facilitate the interpretation of our empirical results. Specifically, it can help to understand potential transmission mechanisms of a policy change such as the one studied here. The model is intentionally kept as simple as possible to focus on the key questions of interest. We solve the model to derive endogenous expressions for wages, labor market conditions and the decision of whether or not to invest in education. Then we consider comparative statics for these expressions to study the response to a policy change.

4.1 A search and matching model with human capital investments

In our model, individuals are either educated (high skill h) or uneducated (low skill l). We make the simplifying assumption that there are separate markets for high and low skilled workers. The different skill levels, denoted $m \in \{h, l\}$, correspond to productivity levels $y^h > y^l$.¹¹ We further assume that refugees may be in a temporary or a permanent state, $s \in \{T, P\}$. The type of

¹⁰For studies on investments in host country specific human capital, see for example Chiswick (1978), Cortes (2004) and Duleep & Regets (1999).

¹¹See Bennett *et al.* (2017) for a model where firms supply jobs for both immigrants and natives.

productivity we have in mind is host country specific and we assume that the value of being in the host country is larger than the value of being in the home country, as this is consistent with the asylum seeker fleeing the home country. We can therefore disregard the home country in the model. As refugee seekers may lose temporary residency – something which became, ex ante, even more likely with the 2002 law change – we include an exogenous probability of being deported from the host country. Increasing this parameter value and examining the impact on employment and education is then one of the scenarios we consider below. If the individual is deported he or she gets nothing. Firms supply v_s^m vacancies and unemployment rates are given by u_s^m . The transition rate for an unemployed refugee worker of skill level m into employment in state s is given by $f_s^m(\theta_s^m) = (\theta_s^m)^\alpha$, where $\theta_s^m = v_s^m/u_s^m$ is the labor market tightness. Firm's transition rates are given by $q_s^m(\theta_s^m) = (\theta_s^m)^{\alpha-1}$. $\alpha \in (0, 1)$ is the match efficiency. We turn to the value functions of workers and firms. Let U_T^m and E_T^m denote the expected present values of unemployment and employment in the temporary state. The value functions are then given by:

$$rU_T^m = f_T^m(E_T^m - U_T^m) + \rho_u^m(U_P^m - U_T^m) - \mathbf{\Gamma}(m)c(e) - (a + d_u)U_T^m, \quad (1)$$

$$rE_T^m = w_T^m + \sigma(U_T^m - E_T^m) + \rho_e^m(E_P^m - E_T^m) - \mathbf{\Gamma}(m)c(e) - (a + d_e)E_T^m, \quad (2)$$

where r is the exogenous discount rate, ρ_u^m and ρ_e^m are the probabilities of moving from the temporary to the permanent state as unemployed and employed respectively, $\mathbf{\Gamma}(m)$ is an indicator function which takes the value one if the worker acquires education and zero otherwise, $c(e)$ is the cost of acquiring education, a is the exogenous transition rate out of the labor force, d_u and d_e are the probabilities of being deported while unemployed or employed, and w_T^m is the sectoral wage. We assume that individual workers i have different abilities, e_i , and therefore different costs of obtaining education, $c(e_i)$. The variable e_i is assumed to be uniformly distributed, $e_i \in [0, 1]$, and the costs are decreasing in ability at a decreasing rate, $c'(e_i) < 0$ and $c''(e_i) > 0$. Furthermore, in order to guarantee a non-trivial solution where some, but not all, individuals choose to acquire education, we assume that the individual with the highest ability faces a very low cost of education, $c(1) = 0$, and the individual with the lowest ability faces very high costs of education, i.e. $\lim_{e_i \rightarrow 0} c(e_i) = \infty$. Hence, $\mathbf{\Gamma}(h) = 1$ and $\mathbf{\Gamma}(l) = 0$.¹²

For the permanent state, the values of unemployment and employment are instead determined by:

$$rU_P^m = f_P^m(E_P^m - U_P^m) - \mathbf{\Gamma}(m)c(e) - aU_P^m, \quad (3)$$

$$rE_P^m = w_P^m + \sigma(U_P^m - E_P^m) - \mathbf{\Gamma}(m)c(e) - aE_P^m, \quad (4)$$

¹²We assume that the educational cost is a cost to acquire and maintain education or skills. This is a simplifying assumption and is not important for the results.

where w_P^m is the sectoral wage in the permanent state. From the firm's perspective, we let J_T^m and V_T^m represent the expected present value of an occupied job and a vacant job in the transitory state. The value functions for a job paying the wage w_T^m and a vacant job are then:

$$rJ_T^m = y^m - w_T^m + \sigma(V_T^m - J_T^m) + \rho_e^m(J_P^m - J_T^m) - (a + d_e)J_T^m, \quad (5)$$

$$rV_T^m = q_T^m(J_T^m - V_T^m) - k - aV_T^m, \quad (6)$$

where k are hiring costs. For firms in the permanent state we instead have:

$$rJ_P^m = y^m - w_P^m + \sigma(V_P^m - J_P^m) - aJ_P^m, \quad (7)$$

$$rV_P^m = q_P^m(J_P^m - V_P^m) - k - aV_P^m. \quad (8)$$

Free entry gives $V_s^m = 0$ and therefore we can also write $\frac{k}{q_s^m} = J_s^m$.

4.1.1 Wages

Wages are determined by Nash Bargaining with equal bargaining power, giving the first order condition $E_s^m - U_s^m = J_s^m - V_s^m$. For the permanent state we use equations (1)-(6) and assume free entry. Furthermore, in the baseline case, we assume that the risk of being deported is the same for individuals who are employed and unemployed, i.e. $d_e = d_u = d$. Then we can look at the impact of a change in the deportation rate in Section 4.1.4. We make the simplifying assumption that $\rho_e^h = \rho_u^h$. The rationale behind this is that despite the general increase in the number of years before an immigrant could apply for permanent residency increased, some special conditions were in place for workers. Hence, uneducated workers, as they empirically face a higher unemployment rate, were worse off than educated workers and, in particular, if they happened to be unemployed. We arrive at the following expression for wages in the permanent state:

$$w_P^m = 0.5(y^m + k\theta_P^m), \quad (9)$$

From this expression we see that wages are increasing in labor market tightness and in the productivity. For the transitory state we get a slightly more complicated expression for wages:

$$w_T^m = 0.5 \left(y^m + \frac{r + a + d + \rho_e^m}{r + a + d + \rho_u^m} k\theta_T^m - \frac{(\rho_e^m - \rho_u^m)}{r + a} \frac{r + a + d}{r + a + d + \rho_u^m} k\theta_P^m \right), \quad (10)$$

which is still increasing in productivity.¹³

¹³For educated workers, as we assume $\rho_e^h = \rho_u^h$, we can simplify to get:

$$w_T^h = 0.5(y^h + k\theta_T^h). \quad (11)$$

4.1.2 Labor market tightness

Next we turn to labor market conditions. Labor market tightness, θ_s^m , is defined as vacancies relative to the unemployment rate. Here we derive expressions for the transitory and permanent state only in terms of exogenous parameters and the endogenous labor market tightness. For the transitory state we use equations (5)–(6) and assume free entry to arrive at the following expression for uneducated workers:

$$(r + a + \sigma + \rho_e^l + d)2k(\theta_T^l)^{1-\alpha} = \frac{(r + a + \sigma + \rho_e^l)y^l}{r + a + \sigma} - \frac{r + a + d + \rho_e^l}{r + a + d + \rho_u^l}k\theta_T^l + \left(\frac{\rho_e^l - \rho_u^l}{r + a} \frac{r + a + d}{r + a + d + \rho_u^l} - \frac{\rho_e^l}{r + a + \sigma} \right) \theta_P^l, \quad (12)$$

and for educated workers:

$$(r + a + \sigma + \rho_e^h + d)2k(\theta_T^h)^{1-\alpha} = \frac{(r + a + \sigma + \rho_e^h)y^h}{r + a + \sigma} - k\theta_T^h - \frac{\rho_e^h}{r + a + \sigma}k\theta_P^h. \quad (13)$$

For the permanent state we instead get:

$$(r + a + \sigma)2k(\theta_P^m)^{1-\alpha} = y^m - k\theta_P^m. \quad (14)$$

We can show that labor market tightness facing temporary workers is lower than labor market tightness facing permanent workers, $\theta_T^m < \theta_P^m$.¹⁴ This is then consistent with a higher employment rate for permanent workers than temporary permit workers. The reason is that the firm supplying vacancies to temporary permit workers faces a lower duration of a potential match and therefore supplies fewer vacancies for a given pool of unemployed job seekers. In Section C we show that labor market tightness, θ_T^m , increases in ρ_e^m and decreases in ρ_u^m . Furthermore, wages, w_T^m , increase in ρ_u^m whereas the effect of a change in ρ_e^m on wages is indeterminate.¹⁵

4.1.3 Education

We now turn to the human capital investment decision: whether or not to invest in education. For simplicity, we consider workers in the transitory state, which is the main state of interest in the empirical part of the paper. When a worker makes this decision, she compares the value of unemployment as an educated worker, bearing the associated costs of education, to the value of unemployment as an uneducated worker. The marginal worker has ability level, \hat{e} , which makes her indifferent between acquiring higher education and remaining as an uneducated worker. We write the condition determining the ability level of the marginal worker as:

$$rU_T^h(\hat{e}) = rU_T^l. \quad (15)$$

¹⁴Details available upon request.

¹⁵We also note that labor market tightness decreases when the deportation rate, d , increases. The same is true for wages.

Workers proceed to higher education if the expected income gains from education exceed their cost of education. We rewrite equation (1) and subtract this expression from equation (3), and use the free entry condition to arrive at the following rewritten expression for (15):

$$\left\{ (r+a)\theta_T^h + \rho_u^h \theta_P^h - \frac{r+a+d+\rho_u^h}{r+a+d+\rho_u^l} \left((r+a)\theta_T^l + \rho_u^l \theta_P^l \right) \right\} \frac{k}{r+a+\rho_u^h} = c(\hat{e}). \quad (16)$$

Here we have a condition determining \hat{e} as a function of exogenous parameters and endogenous variables, θ_T^m and θ_P^m for $m \in \{h, l\}$. Workers with ability level e_i below the threshold level, $e_i \leq \hat{e}$, choose not to invest in education, whereas workers with $e_i > \hat{e}$ choose to do so. Hence, \hat{e} and $(1-\hat{e})$ constitute the uneducated and educated labor forces respectively. The right hand side of equation (16) is equal to the expected income gain of investing in education. This gain needs to be positive in order for at least some workers to proceed to higher education. The fact that productivity is higher for educated workers means that there is an educational wage premium, which in turn provides incentives for higher education as well as a higher probability of getting a job.

4.1.4 Impact of a policy change

Finally, we turn to the impact of a policy change on employment and education. For this purpose we will consider three different cases, which are consistent with the law change and which give us hypotheses to test in the data. As mentioned above, despite the general increase in the number of years before an immigrant could apply for permanent residency increased, some special conditions were in place for workers. Hence, uneducated workers were worse off than educated workers as they have a lower probability of obtaining employment than educated workers. In the first case, we therefore assume that the likelihood of obtaining a permanent permit is reduced only for uneducated and unemployed individuals, $d\rho_u^l < 0$. In the second case we consider the impact of a decrease in the likelihood of obtaining a permanent permit for employed uneducated workers, $d\rho_e^l < 0$. Finally, in order to capture a final related implication of the law change, namely that the probability of losing a temporary permit increased, we consider an increase in the deportation risk, $dd_e = dd_u = dd > 0$.

Case 1 *If $d\rho_u^l < 0$, this means that the likelihood of obtaining permanent residency is decreased only for unemployed uneducated individuals. In this case education increases. There are two counteracting forces at play. First, the relative value of being educated (and unemployed) increases as the value of being uneducated unemployed decreases. This increases the value of obtaining an education. Second, there is an increase in labor market tightness for the uneducated workers because their wages fall and this increases employment (they will be more eager to have a job when the value of being unemployed falls). This effect tends to reduce the number of individuals that acquire education. The former effect dominates and more individuals invest in education.*

Case 2 *If $d\rho_e^l < 0$, the likelihood of obtaining a permanent residency for employed uneducated workers diminishes. A lower probability of permanent residency reduces labor market tightness, and thus employment, for this group of workers. This is because the match between a worker and a*

firm will last shorter. The negative impact on labor market tightness dominates (the effect on wages is indeterminate) and incentives to acquire education increases. There is an increase in education investments.

Case 3 *Finally, if $\rho_e^m = \rho_u^m$ we can look at the impact of $dd > 0$, i.e. an increase in the deportation rate. The value of unemployment falls when the probability of losing a temporary permit increases equally for both educated and uneducated workers. Fewer vacancies are supplied and labour market tightness falls for both educational groups which reduces employment. The impact will be stronger for educated workers because of their higher productivity and thus fewer individuals acquire education.*

It should be noted that our analysis, thus far, abstracts from the fact that the intensity of the effects may differ across time.¹⁶ In our setting we are interested in changes to behavior that occur over time. Specifically, we look at outcomes over time after approval and compare individuals with different lengths of temporary protection. Dynamic effects will be discussed further in Section 6.

5 Empirical strategy

In this section we describe our data sources and the empirical strategy used for analysis.

5.1 Data

Our main data set is register data collected by Statistics Denmark. For the purpose of this study we combine two sources of Danish micro data. First, from Statistics Denmark we have register data on a broad set of outcomes for all immigrants in Denmark between the years 1997–2015. This data set includes all immigrants who were registered as living in Denmark on January 1 in any of the years 1997 to 2015, which means that we can follow our sample up until twelve years after their initial application for asylum was approved. Second, using unique register data from the Danish Immigration Service we observe, for each individual, the type of residence permit as well as dates of application and approval. Using individual identifiers, this data can be linked to our main data set and enables us to define relevant treatment and control groups, as discussed in more detail in Section 5.2. Our main variables of interest include educational investments and labor market outcomes. We study enrollment defined as the share of individuals that, at some point during the twelve years of data that we observe, enroll in general education or in education at the university level specifically.¹⁷ In terms of labor market outcomes, we focus on employment status and labor earnings (including self-employment). Employment status is the share of individuals that are registered as employed (or self-employed) at some point during the twelve years that we observe,

¹⁶As shown in for example the literature on effects of unemployment insurance (UI) on unemployment duration, where duration dependence can be expected to matter. See for example Nekoei & Weber (2017) where extended UI benefits are found to lengthen unemployment, but also provide a better matching (measured in terms of wages). Rosholm & Toomet (2005) is an example allowing for discouragement.

¹⁷Data comes from the educational registers UDDA and VEUV.

whereas earnings are primarily measured after three and seven years of residency in Denmark (in our analysis we also consider earnings at each specific year in time).¹⁸

From register data we also collect information on demographic characteristics (age, gender, nationality, marital status and the number of children in the household) to be used as control variables in the analysis.¹⁹ In addition, from the educational registers, we impute two different measures of skill level at arrival. First, we use the highest level of education completed before arrival in Denmark (primary/secondary or higher).²⁰ Second, we use the entry level of Danish language courses (1,2 or 3), because entry level is determined by the individual’s skill level.²¹ These measures of initial skill level are used both as control variables and to split the sample in order to study heterogeneous effects.²²

Sample restrictions We remove individuals with no information on application date, and those who applied for asylum before November 1, 2001 or after June 30, 2002. Without information on application date we are not able to classify our relevant control and treatment groups. Figure 1 shows a time line of the period of interest and the way we split our sample into a control and a treatment group. Our control group is defined as individuals applying for asylum between November 1, 2001 and February 27, 2002, while the treatment group includes individuals applying between February 28 and June 30, 2002. The sample split is chosen to ensure that nothing else is happening that would affect those applying prior to and post the cutoff differentially. As discussed in Section 3.1 there were several components to the 2002 reform, apart from the prolonged waiting time for permanent residency. To avoid confounding effects from these other components, that mainly relied on the date of approval, we also restrict our sample to individuals whose applications were approved after July 1, 2002. The reason for this restriction is that we want to compare asylum holders who only differ in terms of which rules regarding permanent residency they are subject to, and not in any other dimensions. As described in Section 3.1 the reform included changes to, for example, the benefit structure. Because of long processing times this restriction on the approval date does not reduce our sample by much. Figure 2 shows the fraction of individuals in 2001 and 2002 whose applications were approved post July 1, 2002, by month of application. We also exclude individuals

¹⁸Data comes from the INK and RAS registers.

¹⁹These variables come from the population register (BEF). To determine marital status at arrival, we assume that if the date for the first change in marital status is missing, the change must have happened before arriving to Denmark (or it would have been recorded). Children at arrival is defined by considering all children born before the application year and associated with the first family identifier available in the registers after the first asylum application.

²⁰Primary/secondary education includes early childhood education and primary education as well as lower and upper secondary education. Higher education includes university studies (short cycle tertiary, bachelor, master and doctoral).

²¹Level 1 is for students with no or limited educational background, or those who are considered to have limited learning abilities because of trauma, level 2 is for students with some (normal) educational background and level 3 is for students with higher education (who often speak several languages).

²²All information on education comes from the two registers UDDA and VEUV.

lodging their application from abroad.²³ The unit of analysis is throughout the individual. Finally, because we are interested in educational and labor market outcomes, we focus on individuals who are between the age of 16–60 at the time of application.

5.2 Identification

The implementation of the reform implies that refugees who applied for asylum prior to February 28, 2002 (henceforth referred to as the cutoff) were able to apply for permanent residency three years after approval, whereas those who applied after the cutoff had to wait for seven years. The fact that this reform was implemented retroactively gives rise to a regression discontinuity in time setting with no possibility of manipulation around the cutoff. The reform was decided on June 6, 2002 and took effect on July 1, 2002 - but the part of the reform that we are studying applied retroactively to everyone who applied from February 28, 2002 and onward. This means that neither immigrants, nor the decision makers at the DIS, could have perfectly manipulated the date of application in order to achieve a certain treatment status. Intuitively, individuals who applied just before the cutoff should therefore be comparable to individuals who applied just after the cutoff.

Looking at aggregate statistics from [The Danish Immigration Service \(2003\)](#) in Figure 3, we conclude that there is no major change in the number of lodged asylum applications in Denmark from February to March 2002. In our data, we observe only individuals whose asylum application was subsequently approved. Figure 4 shows the number of approved applications by month of application and type of asylum during 2002. Again, we see no notable change in the number of approvals around the cutoff. As we observe the actual date of application, we also present a histogram of the number of granted asylum applications using the week of application in Figure 5.²⁴ The absence of a spike in the density of applications made just before the cutoff is in line with our intuition as the reform was implemented retroactively, leaving no room for manipulation.²⁵

In the regression discontinuity framework, treatment effects are identified by estimating the magnitude of the discontinuity at the cutoff. While the sharp cutoff implied by the reform intuitively lends itself to the regression discontinuity approach, ideally one would want to compare individuals on each side close to the cutoff. As Denmark approves a relatively small number of asylum seekers, we have to use a relatively broad bandwidth of four months on each side of the cutoff (119 days between November 2001 to June 2002). This leads attention to the inherent trade off between precision and bias in the regression discontinuity framework. Extending the bandwidth around the cutoff increases precision, but also the risk of introducing bias. As our running variable is the date of application, we have to estimate treatment effects parametrically in order to avoid confounding time-varying effects. The regression equation is specified as:

²³This means dropping three individuals that would otherwise have been included in the control group.

²⁴We aggregate to the weekly level to comply with the micro data policy of Statistics Denmark.

²⁵In Table 6 in Appendix H.4.1 we present the results from a formal test of manipulation at the cutoff using the Stata package `RDDENSITY`. We implement this test for a linear and a quadratic specification. For the linear specification we get a p-value of 0.121 while the quadratic specification gives us a p-value of 0.935.

$$Y_i = \alpha + \beta T_i + h(\tilde{x}_i) + T_i h(\tilde{x}_i) + \mathbf{Z}_i + \epsilon_i, \quad (17)$$

where Y_i is the outcome of individual i , \tilde{x}_i is the normalized date of application such that February 28, 2002, is set to zero and $h(\cdot)$ is a continuous function of the date of application, T_i is an indicator for treatment status with $T_i = 0$ if $\tilde{x}_i < 0$ and $T_i = 1$ otherwise, and ϵ_i is the error term. We include an interaction between $h(\tilde{x}_i)$ and the treatment indicator T_i , to allow for different trends over time on each side of the cutoff. β is the coefficient of interest measuring the effect of being subject to the new rules on permanent residency. In our main specification, $h(\cdot)$ is specified as a linear function. In Section 7 we vary the order of the polynomial to test the robustness of our results. Furthermore, all specifications are estimated with and without a vector of predetermined individual characteristics, \mathbf{Z}_i , to increase efficiency and confirm that covariates do not affect the point estimates.²⁶

For the graphical representation (in Section F), we plot the mean of each outcome for evenly spaced bins of the running variable. For each plot, we fit a global linear polynomial to approximate the population CEF, using a uniform kernel and evenly spaced bins.²⁷ For all plots, we use the full bandwidth of 119 days and we do not include covariates. We also present similar graphs of predetermined characteristics, to substantiate the continuity assumption underlying the regression discontinuity framework, see Figures 7–9 in Section E. In terms of predetermined characteristics we look at demographic characteristics and educational background. For demographic characteristics we look at the fraction of males, household characteristics and average age, as well as nationality. For educational background we look at both a self-reported measure of the highest level of education achieved, and the level of Danish studies that the individual is assigned to. The reason to consider these characteristics is because they are predetermined variables that we believe may impact how individuals are affected by the reform. We find significant discontinuities in terms of nationalities. There is a positive jump for other nationalities (of 0.175), and a negative jump for Afghans (-0.128).²⁸ There is a marginally significant discontinuity in the share of males (0.136). We also estimate labor earnings one year after approval by regressing labor earnings on predetermined characteristics. We then estimate the regression discontinuity using the predicted values for earnings and do not find any discontinuities in this variable.

Table 1 compare means of predetermined characteristics for the control and treatment group

²⁶Although it has been standard practice in regression discontinuity designs to cluster on the running variable, we choose to follow Kolesár & Rothe (2017) and abstain from clustering using only robust standard errors. We have repeated all estimations for the full sample with clustering on the running variable and find that not clustering is the more conservative approach for all outcomes. Results from estimations using clustering are available on request.

²⁷These plots are produced using the Stata package RDPLLOT, for more details see Sebastian Calonico & Titiunik (2014). Graphs using a quadratic polynomial for our main outcomes are available upon request.

²⁸Other nationalities is defined as a dummy equal to one if the individual is not from one of the most common countries of origin: Afghanistan, Former Yugoslavia, Iraq or Somalia.

as well as their normalized difference.²⁹ The normalized difference gives us a scale-invariant measure of the magnitude of the difference between the groups. We consider differences above 0.25 to be signs of sizable differences. We note that the groups are generally well balanced over the whole 8 month period that defines our sample of interest. Again, the biggest differences arise in terms of nationalities: there are more Iraqis in the control group, and more individuals from other nationalities in the treatment group. In addition, there are fewer males in the treatment group. Apart from these variables the two groups appear balanced. However, as a small sample size is challenging in a regression discontinuity framework, we complement our analysis by looking at the difference in average outcomes over time by treatment status.

Heterogeneous effects Finally, to capture potential heterogeneity in response to the reform we split the sample by (i) level of education at arrival (below/above university level, henceforth referred to as low/high skilled) and (ii) gender (males/females).³⁰ The reform may have had different impact on different groups of refugees depending on their access to the labor market as this could determine how much they were able to affect their probability of being granted residency based on labor market attachment. We believe that these sample splits can capture important differences in terms of labor market access. Related to our theoretical model, we believe that these groups may also differ in their cost of acquiring education in the host country. Other potentially interesting sample splits would be to look at age groups and country of origin, but splitting the sample along these dimensions is not feasible due to the small sample size and the distribution of these variables. For the subgroup analysis we split the sample by subgroup and estimate equation 17.

6 Results

This section presents and discusses our empirical findings. We present both estimates from the regression analysis and graphical evidence. The graphical evidence is based on the estimated discontinuity at the cutoff date. For these figures we always use a bandwidth of 119 days on each side of the cutoff and we do not include any covariates. For the regression analysis we present results with and without covariates for the 119 days bandwidth.

²⁹See Imbens & Woolridge (2009) for a motivation for using the normalized difference. The measure is defined as:

$$nd = \frac{\bar{x}_t - \bar{x}_c}{\sqrt{(sd_t^2 + sd_c^2)/2}}$$

³⁰We check the overlap between these groups and note the following. Around 47 percent of females are classified as low skilled, and around 22 percent as high skilled. Among males the division is similar with 43 percent of men classified as low skilled and 26 percent classified as high skilled. For around 30 percent of both males and females we do not observe skill level at arrival. The correlation between gender and skill level is 0.07.

6.1 Educational outcomes

Human capital investments can be viewed as part of the integration process. Table 2 shows our estimated results for the full sample as well as for the subgroups based on gender and skill level. First we look at enrollment in formal education. This variable measures the share of individuals that, at some point during the twelve years we observe in our data, enroll in any type of formal education (primary, secondary or university). Columns (1) and (2) show the estimated coefficients with and without covariates for the full sample. We estimate a positive effect of around 17 percentage points at the cutoff. The effect is significant at the 5 percent level without controls and at the 1 percent level for the specifications with controls. Panel (a) in Figure 10 confirms this picture and we observe a clear jump at the threshold. Turning to the subgroups analyzed in columns (3–10) we see that the effect is driven by females and, to a lesser degree, low skilled individuals. For females the estimated effect is an increase of 22 percentage points at the cutoff, significant at the 5 percent level for the specification with covariates (the effect is slightly stronger without covariates). Next, we estimate the effect on enrollment at the university level. This variable measures the share of individuals that enroll in university education at some point in time during the twelve years that we observe. The estimated coefficient is positive at around 6 percentage points but not significant (for low skilled the estimated coefficient is around 11 percentage points and significant at the 10 percent level).³¹ Figure 12 shows the evolution of enrollment rates over time (panel (a)). Here we look at the share of individuals that are enrolled in education in a specific year (through a fitted quadratic polynomial with 95 percent confidence intervals). This shows that the treatment group has overall higher enrollment rates over time for general education. Finally, in Appendix H.3.1 (panel (a) in Figure 21) we look at the estimated effect at the cutoff for enrollment in education at each year after arrival separately. We note that the effect, if anything, appears to be stronger over time.

We interpret the positive effect on enrollment in education as an increased investment in human capital and integration. However, in order for integration to be successful it is relevant to consider what this enrollment results in. Therefore, we also consider different types of education that might be particularly relevant for access to the labor market (adult education and labor market training), the propensity to complete an education and the number of years in education (throughout we exclude Danish courses which are mandatory for both groups). These results are available upon request. We do not find significant effects on any of these variables. For females, who show the largest increase in the propensity to be enrolled in education, we find support for a higher propensity to be classified as a student in the long run (measured seven years after approval). This implies that they are more likely to be students compared to working or being unemployed, and could indicate that females substitute work for education in order to increase their human capital. Alternatively, females could be having a harder time getting access to the labor market and enroll

³¹We also test for difference in means between the control and treatment group. There is a marginally significant difference between the two groups for enrollment in general education, with higher enrollment rates for the treatment group.

in education by necessity rather than choice.

Our findings for enrollment are in line with the hypotheses in Case 1 and Case 2 from our theoretical model, i.e. that the change in the ex ante probability of obtaining a permanent residence permit was especially pronounced for low skill individuals or those further away from the labor market. We believe that females are, if anything, likely to be less attached to the labor market compared to males. Thus, our findings, with strong effects among females and low skilled, are in line with the theoretical model. If the only change was an increased deportation rate, as in Case 3, we would instead have expected a decrease in enrollment. We now turn to look directly at labor market outcomes.

6.2 Labor market outcomes

Labor market outcomes are a direct measure of attachment to the labor market which makes them directly relevant to study. Furthermore, in the previous section we did not find any significant effects on human capital investments for males. This could be because they instead find jobs to a greater extent, which makes it important to look at labor market outcomes also for subgroups. We consider whether an individual was ever employed (including self-employment) in Denmark and look at labor earnings after three and seven years of residency. Employment is defined as the share of individuals (in the treatment and control group) that are ever employed or self-employed during the twelve years following their initial approval of asylum. Table 3 shows the estimated regression results for the full sample and our four subgroups. In addition, panels (c)–(e) in Figure 10 show the graphical presentation of our results. For the full sample we do not find any significant effects on employment or earnings. When we look at earnings conditional on employment we have a marginally significant negative effect when including controls (these results are available upon request). All coefficients are negative but imprecisely estimated. The graphical evidence also show a small negative jump, but there are no indications of a sizable and significant negative effect. Turning to the subgroups the picture is very similar to the results for the full sample. However, females (and to some extent high skilled) show significant negative effects on earnings. This is consistent with more females being enrolled in education. For males, who were not more likely to be enrolled in education, there is no significant effect on any of the labor related outcomes. Figure 13 confirms this picture looking graphically at employment and earnings for each year, and in Appendix H.3.1 (Figure 21) we estimate the coefficients for each of the different years after arrival. The conclusions remain the same. If anything, there are signs of a more negative impact on earnings in later years. To try to understand potential mechanisms, Figure 23 in Appendix H.3.2 looks at the fitted quadratic polynomial over time for earnings conditional on employment in the different subgroups (because the subgroups are small already before conditioning on employment we do not estimate regressions for this outcome). We note a difference in that earnings for women who are employed appear to be very similar in both groups, whereas there is a divergence for the high skilled individuals with the control group experiencing stronger earnings over time. It is noteworthy that the divergence between the control and the treatment group appears 3–5 years after approval for

this subgroup. This is around the time when individuals in the control group are eligible to apply for permanent residency status, and is in line with high skilled individuals in the treatment group accepting jobs with lower earnings compared to the control group. This could be a sign of weaker bargaining power of the individuals in the treatment group, or that employers are more reluctant to invest in individuals whose future in the country (and thus also in the firm) is more uncertain. This is consistent with Case 3 in our model, where employment falls for both educational groups.

We also look at the highest skill level ever achieved on the labor market during the years we observe (more details on skill level in Appendix H.5) and the number of times an individual changes workplace. We find no difference in the number of times individuals change workplace during the twelve years we observe them in Denmark, but they generally appear to do so at a decreasing rate over time (which may be in line with a more stable labor market attachment). For high skilled we estimate a weakly significant negative effect on the highest skill level achieved on a job. This could imply that high skilled accept jobs that they are potentially over-qualified for. We conclude that the increased enrollment in education does not seem to have translated into improved labor market outcomes at any time horizon.³²

In terms of labor market outcomes, it can be related to our theoretical model in the following way. Case 1 and 2 had the same implications for investments in education, but differed in the impact on labor market tightness and therefore employment. The empirical evidence is consistent with the relative reduction in the probability of obtaining permanent residency for employed versus unemployed workers being such that the counteracting impacts cancel out. Females, however, do show negative effects on earnings which is more in line with Case 1, i.e. that unemployed and uneducated individuals were most negatively affected, as we think of females to be further away from the labor market. It should, however, be noted that we do not see a divergence in earnings conditional on employment when we look at earnings dynamics graphically. This is, in turn, consistent with Case 3 in our theoretical model and in line with the reform having a negative impact on refugees in general, because of the perceived risk of losing a match, and on individuals further away from the labor market in particular.

7 Sensitivity analysis

In this section we perform sensitivity analysis to assess the robustness of our results. In particular, we consider standard tests for the validity of our regression discontinuity approach. Furthermore, we investigate the importance of calendar effects.

7.1 Placebo tests

A standard test in this type of design is to test for placebo effects by estimating the same model, but varying the location of the cutoff. Discontinuities at other cutoff points (where nothing happened) may suggest that discontinuities at the real cutoff are not due to the reform. We split the main

³²Results are available upon request.

sample into the control and treatment group separately. Then, following Imbens & Lemieux (2008), we test for discontinuities in our outcome variables at the median date of application in each of the two groups. The advantage of splitting the sample into the control and treatment group is that we avoid fitting a regression function over a point where we expect a discontinuity to occur. We could test for discontinuities at other points within each of these sub-samples, but using the median gives us more power to detect potential discontinuities. Tables 7–9 in Appendix H.4.2 present the results from this placebo analysis. For most variables, we do not find any significant discontinuities at the placebo cutoff. For employment we estimate a significant (at the 10 percent level) discontinuity with inclusion of covariates. In addition, for a few other variables, when we include the other outcomes discussed in more detail in Section 8, we detect a significant jump at the placebo cutoff. This is the case for the share registered in Denmark, number of births and hospital visits for the treatment group. Given the narrow bandwidth we have to implement for this test and the lower number of observations we end up with, it is not surprising that we detect a few discontinuities as we are not able to estimate the time trend and control function as well.

7.2 Choice of bandwidth

Given that the placebo test detected a few discontinuities at other values of the running variable than the true cutoff point, we want to assess the robustness of our results in greater detail. More specifically, we do a thorough investigation into the sensitivity of our results to changes in the bandwidth. Our main results, presented in Tables 2-3, are estimated using a bandwidth of 119 days around the cutoff point. We cannot extend the bandwidth further without including individuals in the treatment group that were also subject to for example the change in benefit levels. For this reason, our sensitivity analysis is restricted to analyzing the effects when decreasing the bandwidth.

For both predetermined characteristics and outcome variables, we present coefficients and confidence intervals from estimating the regression discontinuity equation using bandwidths starting at 21 days and then increasing the bandwidth by two days at the time until reaching 119 days (our preferred bandwidth). Figures 15–17 in Appendix H.1 present the results from this analysis for predetermined characteristics. Although confidence intervals suggest that even for smaller bandwidths, the coefficients are in general not significantly different from zero, the coefficients become much more stable at broader bandwidths. This analysis motivates the choice to use a bandwidth of 119 days. Figures 18–20 in Appendix H.2 present the same type of analysis for our outcome variables, and confirms our interpretation of the results. At broader bandwidths, the coefficients are not sensitive to bandwidth changes.

Many papers that use the regression discontinuity approach choose to use optimal bandwidth selection, a data driven approach to select how many observations on each side of the cutoff should be used in the estimation. We have chosen to instead use the broadest bandwidth possible to isolate the effect of this reform, i.e. use as many observations as possible without including individuals that were also subject to other components of the 2002 reform. This gives us the bandwidth of 119 days. We also estimate regressions for our main outcomes using the optimal bandwidth. Using optimal

bandwidth selection, about 100 observations are used in estimations compared to the sample size of 635 when using the 119 days bandwidth. In general, coefficients estimated with optimal bandwidth are in line with or larger in magnitude compared to our preferred specification. The exception is enrollment, where the magnitude is smaller and non-significant without controls (although with controls the magnitude is larger and the estimate is significant at the 10 percent level) when using optimal bandwidth. In light of the low number of observations used in these estimations, the 119 days bandwidth remains our preferred specification.³³

7.3 Assumptions on the regression specification

Section 6 presented results from regression analysis using a linear function. We also replicate all our main results using a quadratic polynomial. The main reason to include higher order polynomials is to capture non-linearities in the underlying data. In our case, however, using a higher order polynomial often appears to lead to overfitting and, thus, overestimation of the effect. Using the linear specification is therefore the more conservative choice for most outcomes. The results for enrollment are, however, sensitive to the inclusion of a second order polynomial. The estimated effect is smaller in magnitude and imprecisely estimated. Looking at the graphical evidence in Figure 10 (a), the linear fit seems more appropriate. The linear case remains therefore our preferred specification.

The main specification uses a uniform kernel, but we have estimated all the results for the full sample using a triangular kernel as well. The motivation for using a triangular kernel is that it gives more weight to observations close to the cutoff, but given the low number of observations in our sample, the uniform kernel remains our preferred specification. In general, the coefficients using a triangular kernel are in line with or even larger in magnitude compared to our preferred specification. For enrollment the effect is slightly weaker at 13–15 percentage points. In addition, a few other outcomes are now significant.³⁴

7.4 Calendar effects

Our treatment group arrives, by definition, later to Denmark compared to our control group. One worry related to this is that any observed effect depends on this difference rather than the reform itself. For example, calendar effects could potentially confound our results if the state of the labor market differs between the points in time when the control and the treatment group get their asylum approvals.³⁵ In 2002, asylum seekers were not allowed to work until their applications were approved, which means that we need to consider the difference in approval rates between treatment and control. We note that the distribution of approval dates for the treatment and control groups

³³Results using optimal bandwidth selection are available upon request.

³⁴Results using a quadratic polynomial and using a triangular kernel are available upon request.

³⁵Another concern could be if we think that asylum seekers arriving between November and February are inherently different compared to asylum seekers arriving in the spring. We can control for potential differences in observed characteristics but are not able to control for differences in unobserved characteristics.

are quite similar, suggesting that there are no substantial differences in the time when our control and treatment group are allowed to enter the labor market.³⁶

Because approval dates of the two groups look rather similar (see Figure 6), it is possible that processing times instead differ. We note that the control group had somewhat longer processing time, implying that they spent more time in the asylum center awaiting their decision. If we believe that the time in processing matters, for example because of discouragement from a lack of meaningful activities or because a longer time spent in Denmark gave the control group an advantage before entering the labor market, this could be of relevance. The differences are, however, not so large that we believe they are likely to impact the results.³⁷ Furthermore, Figure 7 shows that there is no discontinuity in the processing time at the cutoff.

The appointment of a new government, on November 27, 2001, was, however, clearly associated with stricter immigration policies to come. Discussions of these policies started formally in January–February 2002 and there was media coverage on the intentions to implement measures aimed at reducing immigration. This means that immigrants could have been aware of the intention to reform Danish asylum policies. Still they would not have been able to foresee the exact timing of the reform.

8 Other outcomes

We have focused on outcomes related to the labor market and human capital investments. These variables are most directly related to the reform we are studying. However, there are several other ways in which the lower ex ante probability of permanent residency (through a longer time period with temporary residency) may have affected individual behavior. In addition to educational investments and labor market outcomes, we therefore also explore if the reform affected crime rates, health, family composition and the duration in Denmark. These outcomes are of interest to understand the full impact of the reform.

Access to criminal registers allows us to observe whether individuals have been convicted for any crime, as well as property crimes separately, during their time in Denmark. Our measure of criminal activity captures the share of individuals that have ever been convicted during the twelve years that we observe.³⁸ This variable is included to consider potential outside options to the regular labor market, as well as potential deterring effects of the prolonged temporary status. We also assess the impact of the reform on health outcomes. In particular, we consider the number of hospital/doctor visits over the twelve year period that we observe.³⁹ This adds a dimension to the analysis as an individual’s health status may affect both her current and future labor market prospects. Furthermore, there is anecdotal evidence that the reform imposed stress on refugees. This makes health itself a relevant outcome if we want to consider the direct impact on refugees’

³⁶Results are available upon request.

³⁷Results are available upon request.

³⁸From the KRAF register we access information on charges and convictions.

³⁹This variable is created using data from the LPRPOP register on health care utilization and diagnoses.

welfare, and more generally potential costs to society. Finally, we consider the effects on fertility behavior by studying the number of births during the first twelve years in Denmark.⁴⁰ The intuition is that increased uncertainty about the future in Denmark could have discouraged individuals from having children.

8.1 Duration in Denmark

First, however, we study if the reform had an impact on whether asylum holders actually stayed in Denmark during the twelve years that we can observe them in the data. Individuals in the treatment group faced the risk of losing their residence status for a longer time before they were eligible to apply for permanent residency. This could lead to more individuals leaving Denmark, because their asylum claim was no longer valid and they did not qualify for residency based on labor market attachment. Further, asylum holders may have left Denmark by choice, due to the change in regulations. This highlights the importance of looking at how long these individuals stay in Denmark, since any effects on other outcome variables could potentially be driven by selection effects of individuals having to, or choosing to, leave Denmark. Estimation results in Table 4 show that there is no significant difference between the control and treatment group in the share that is still registered in Denmark in 2015. This is true for the full sample as well as the different subgroups, confirmed graphically in panel (e) of Figure 11, and it facilitates the interpretation of our other results. It is unlikely that any effects are driven by selection in either the control or treatment group.⁴¹

8.2 Crime

A change in the permit structure could have affected crime rates as the opportunity cost of criminal activity may have changed. The reform we study could also have had a more direct effect on crime rates, since a criminal record reflected negatively on applications for prolonged residency, implying that a longer time period with temporary residency in combination with these stricter rules may have deterred individuals from committing crimes. In addition, the reform may have impacted the immigrant's view of the host country and affected her willingness to comply with its norms and laws.

We study the impact on convictions in general as well as on property crime specifically. This variable measures the share of individuals that are ever convicted of any crime (for the general case), or a property crime. Table 4 presents regression estimates for the full sample and our subgroups. For the full sample we note a negative but non-significant coefficient of between 6 and 12 percentage points. If we instead look at property crime rates we note a significant (at the

⁴⁰We use an indicator equal to one if the number of children in a family increases from one year to the other as a measure of a birth.

⁴¹In Figure 22 in Appendix H.3.1, we present regression discontinuity estimates of the share still in Denmark for each individual year up until twelve years after application. These results confirm that there is no significant difference in the share staying in Denmark. In Table 5 in Appendix H we also confirm that the groups are still relatively well balanced in 2015.

5 percent level including controls) decrease of around 12 percentage points. This effect is mainly driven by a reduction of around 25 percentage points for males (significant at the 1 percent level).⁴²

The negative jump is confirmed in panel (a) and (b) in Figure 11 as well as in panels (a) and (b) in Figure 14, showing the crime rates in the control and treatment group for the different years over time. Panels (a) and (b) in Figure 22 in Appendix H.3.1 show that there is no clear trend in the estimated coefficients over time.

8.3 Fertility behavior and Health

Finally, we study the impact on fertility behavior and health. The lower ex ante probability of getting permanent residency in Denmark may have induced asylum holders to delay having children. A reason for this could have been to achieve a more stable situation before starting a family.⁴³ Our variable of interest is the total number of times that individuals have children during the years we observe.

In Table 4, the coefficients on the number of children born in the full sample are negative, but the effect is non-significant. For females we estimate a significant (at the 5 percent level) decrease in the number of new births, with a coefficient of around -0.7. Panel (c) in Figure 11 confirms this picture for the discontinuity estimation, and panel (c) in Figure 14 shows no clear graphical differences over time. Figure 22 in Appendix H.3.1 shows that there is not a very clear pattern if we look at the estimated effect at different years after arrival. If anything, there is a small upward trend in the coefficient in the later years.

Finally, anecdotal evidence suggests that the reform created a more stressful situation for asylum holders due to increased uncertainty about their future in Denmark. We explore this by studying the effect on the total number of visits to health care centers and hospitals (during the twelve years we observe). The results suggest no significant impact of the reform. This is true also looking at the long run and at the estimated impact at different time horizons.

9 Conclusion

This paper studies the effects of lowering the ex ante probability of receiving permanent residency on refugees' outcomes. We exploit a Danish reform in 2002 that prolonged the time period during which a refugee was required to have been a legal resident before eligible to apply for a permanent

⁴²There is also a statistically significant difference in means between the two groups, with lower rates for the treatment group.

⁴³This is in line with the model outlined by [Ranjan \(1999\)](#). He suggests that the irreversible aspect associated with childbearing, together with the ability to postpone, lead people to postpone childbearing when there is uncertainty about future income. The analogue to our context is straightforward, uncertainty about the future in Denmark may have lead asylum holders to postpone childbearing. [Gustafsson \(2005\)](#) emphasizes that changes in fertility behavior do not need to be driven by a change in expected family sizes, but due to changed timing of family formation. Postponing having children pays off, because women who have children later earn more, all else equal. This could play a role in our context, where the prolonged temporary period of the treatment group may have increased the value of labor market attachment as an alternative way of receiving residency.

resident permit. In light of recent asylum policy changes in Europe and elsewhere, this is an important question with very little evidence to guide policymakers on how such reforms affect the integration of refugees and their labor market prospects. While proponents of temporary protection regimes often argue that stronger incentives to qualify for residency based on labor market attachment will speed up the process of entering the labor market, we find no evidence of positive effects on labor market outcomes. There is no difference in the employment rate between individuals in the control and treatment group. Similarly there is no evidence of increased earnings, the estimated coefficient is negative (but not significantly different from zero). On the other hand, opponents argue that lowering the prospects of staying in the new host country may deter country specific human capital investments since the expected payoff of doing so is discounted at a higher rate. In contrast to this, we find large and significant effects on the enrollment rate, that are driven by females and low skilled. The findings on investments in education are in line with the cases from our theoretical model when the ex ante probability of getting permanent residency is more negatively affected for low skilled individuals, which in the empirical analysis corresponds to individuals further from the labor market.

We want to emphasize some limitations of our study. While the reform studied in this paper in many ways is ideal for studying the effect of prolonged temporary status, the setting also implies some challenges. Our sample size is limited for two reasons: (1) we only study outcomes of individuals that are actually approved asylum, and (2) we need to restrict the time interval to four months before and after the reform to avoid confounding policy changes. This means that we need to be careful about the interpretability of our results. Furthermore, the external validity of our results depends on the institutional setting. First, the composition of refugees is clearly time dependent and depends on many things outside the control of the policy maker. Second, temporary protection regimes may be designed in many different ways, making it more or less easy to compare other settings to the one we are studying in this paper. It is therefore relevant to compare our results to future studies of temporary regimes studied in other settings. Finally, this paper describes a variety of outcomes in terms of human capital investments and other variables that may be affected by the reform. We have abstracted from many other outcomes in this first study of the long run impact of lower ex ante probability of permanent residency. For example, the role of intra-household relationships may be important in order to understand heterogeneity in responses between females and males. Exploring this and other areas of interest, as well as more detailed mechanisms, remain interesting tasks for the future.

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A The Danish asylum process

The process of applying for asylum in Denmark is governed by the Aliens Act from 1983, to which several changes have been made over the years. This section will briefly describe the Danish asylum system in effect around 2002 and builds upon information in [The Danish Immigration Service \(2003\)](#). An asylum seeker arriving to Denmark under these conditions should report to the police once at the boarder. The application is filed either at a local police station or at the center in Sandholm. First, the Danish Immigration Service (DIS) will confirm that Denmark is responsible for processing the asylum application. Asylum seekers that are not rejected at the boarder will be sent to a registration center and, once identity and travel routes to Denmark have been established, to one of several accommodation centers in the country. During the time when the application for asylum is processed, accommodation and financial support of the asylum seeker is provided by the DIS.⁴⁴ Most asylum seekers will be accommodated at a residence center until the final decision is made, but after six months from the application date the asylum seeker is allowed to find own housing until the claim has been processed (but they are not allowed to buy property). During the time when the application is processed, the asylum seeker is not allowed to accept any paid work. Voluntary activities are provided, and there are also some compulsory activities.

The asylum application is handled by the DIS, which is the first instance of decision and they will determine if the asylum application falls under the provisions of the Geneva Convention or the Danish Aliens Act. The assessment is made using information provided by the asylum seeker as well as information collected by the DIS on the asylum seekers country of origin. Convention refugee status is regulated by the UN 1951 refugee convention. Asylum seekers who do not directly qualify as refugees according to the definition of the Refugee Convention but who risk the death penalty or being subjected to torture or inhuman or degrading treatment or punishment in case of return to his or her country of origin get protection status. This category extends the refugee status beyond the UN refugee convention, to individuals with "asylum reasons similar to those in the convention". Prior to the reform in 2002 this would fall under the de facto refugee status in the Danish Aliens Act. As part of the reform, a new Status B was introduced with much stricter criteria to get protection status. There are two different procedures, the normal procedure and the manifestly unfounded procedure.⁴⁵ If an application is rejected by the DIS under the normal procedure, it is automatically appealed to the Refugee Appeals Board⁴⁶ (whose decision is final), in order to speed up the process. If the application is rejected, the individual can still obtain a residence permit for humanitarian reasons or for other exceptional reasons (in which case decisions are made by the Ministry of Integration, later the Ministry for Foreigners, Integration and Housing), although very

⁴⁴In cooperation with the Danish Red Cross, the Danish Emergency Management unit and the municipalities of Hanstholm and Brevst.

⁴⁵The manifestly unfounded procedure is applied when it is clear that the application cannot be approved. In this case, there is no possibility to appeal, and the applicant has to leave Denmark immediately. It is, however, required that the Danish Refugee Council (a NGO) agrees with the DIS's assessment. If the Danish Refugee Council uses their veto, the case will instead be processed under the normal procedure.

⁴⁶An independent body with representatives from the government and the Danish Bar and Law Society.

few individuals are considered for these types of residence permits. These decisions are final, and cannot be appealed. If granted asylum based on humanitarian reasons, one can only stay in the country for as long as those reasons are still present. What constitutes humanitarian reason has varied over time and used to include for example families with small children from countries in war and individuals suffering the effects of torture. Nowadays it is only granted to individuals with life-threatening illness who cannot get treatment in their own country. Once recognized as a refugee, social benefits are given on the same condition as for Danish citizens. The DIS required, according to the 1999 integration plan, that the refugee resided in a specific municipality during a three year integration program. Refugees would be assigned a municipality based on a quota system designed to achieve even distribution, with considerations to circumstances related to the municipality and the refugee. The integration program consisted of Danish culture courses, language classes, and vocational training. After three years (prior to the reform), permanent residence permits were conditioned on the performance in the integration program.

B Other reform components

We briefly describe the other components of the reform package below:

1. Access to the Danish welfare state was limited. Following the reform, individuals were required to have been a resident in Denmark for seven out of the eight most recent years to get the standard level of benefits. For others, benefits were lowered by 35 percent. This part of the reform also applied to native Danes who had lived abroad. The change applied to all asylum seekers who got their applications *granted* after July 1, 2002.
2. Family reunification of refugees was discouraged in several ways. First, by disallowing reunification for spouses under the age of 24 (both spouses had to be 24 years of age or older). In addition, if a Danish citizen wanted to sponsor a spouse, the couple had to prove that their “ties” were stronger to Denmark than to the country of the non-Danish spouse. Further, a Danish citizen could no longer sponsor a parent aged 60 years or older. This change applied to applications for family reunion *lodged* after July 1, 2002.
3. The *de facto* refugee status was abolished. This status previously implied that an individual could get asylum even if the criteria of the UN Geneva Convention from 1951 were not satisfied. This was no longer possible. Instead, a new status B was introduced with a more narrow scope. This change applied to all refugees who *lodged* their applications after July 1, 2002.
4. Prior to the reform, it was possible to apply for asylum in Denmark at a Danish embassy or consulate abroad. This possibility was abolished by the reform.⁴⁷ The possibility to lodge an

⁴⁷During the first six months of 2002, 354 individuals lodged their asylum applications from abroad. In 2001 that number was 1,933 with a vast majority of the applications (1,669) coming from the embassy in Afghanistan (The Danish Immigration Service, 2003).

application abroad was removed as of July 1, 2002.

We define our control and treatment groups to make sure these other changes do not interfere with the component of interest for this study.

C Model details

C.1 Derivatives

To study the impact on θ_T^l of changing ρ_e^l and ρ_u^l we evaluate the expression in equation (12) around $\rho_e = \rho_u$:

$$\begin{aligned}\frac{d\theta_T^l}{d\rho_e^l} &= \frac{\left(\frac{r+a+\sigma+d}{r+a+\sigma+\rho_e^l+d} \left(\frac{y^l-k\theta_P^l}{r+a+\sigma} - \frac{y^l-k\theta_T^l}{r+a+\sigma+d}\right) + \frac{r+a+d}{r+a} k\theta_P^l - k\theta_T^l\right)}{((r+a+\sigma+\rho_e^l+d)(1-\alpha)2(\theta_T^l)^{-\alpha} + 1)k} > 0, \\ \frac{d\theta_T^l}{d\rho_u^l} &= -\frac{\left(\frac{1}{r+a+\rho_e^l+d}\right) \left(\frac{1}{r+a}(r+a+d)k\theta_P^l - k\theta_T^l\right)}{((r+a+\sigma+\rho_e^l+d)(1-\alpha)2k(\theta_T^l)^{-\alpha} + k)} < 0.\end{aligned}$$

The impact on labor market tightness from a change in ρ_e^l is positive, whereas the impact from a change in ρ_u^l is negative. Next we look at the impact of changes to ρ_e^l and ρ_u^l on wages in the transitory state, w_T^l . We get:

$$\begin{aligned}\frac{dw_T^l}{d\rho_e^l} &= -0.5 \left[\frac{\left(\frac{r+a+d}{r+a}\right)\theta_P^l k - \theta_T^l k}{r+a+d+\rho_u^l} - \frac{\frac{r+a+\sigma+d}{r+a+\sigma+\rho_e^l+d} \left(\frac{y^l-\theta_P^l k}{r+\sigma+a} - \frac{y^l-\theta_T^l k}{r+\sigma+a+d}\right) + \frac{\theta_P^l k \left(\frac{r+a+d}{r+a}\right) - k\theta_T^l}{r+a+d+\rho_u^l}}{(r+a+\sigma+\rho_e^l+d)2(1-\alpha)(\theta_T^l)^{-\alpha} + 1} \right] < / > 0, \\ \frac{dw_T^l}{d\rho_u^l} &= 0.5k \left[\frac{\theta_P^l (r+a+d)}{(r+a)(r+a+d+\rho_e^l)} - \frac{\theta_T^l}{r+a+d+\rho_e^l} \right] \left[1 - \frac{1}{(r+a+\sigma+\rho_e^l+d)2(1-\alpha)(\theta_T^l)^{-\alpha} + 1} \right] > 0.\end{aligned}$$

This means that the impact on wages from a change in ρ_e^l is indeterminate, whereas the effect of a change in ρ_u^l is positive. Finally, we look at the impact of an increase in $d_e = d_u = d$ (for skill group m):

$$\frac{d\theta_T^m}{dd} = \frac{-2k(\theta_T^m)^{1-\alpha}}{(r+a+\sigma+d+\rho_e^m)(1-\alpha)2k(\theta_T^m)^{-\alpha} + k} < 0.$$

We see that labor market tightness decreases as the deportation risk increases, implying that labor market conditions deteriorate.

C.2 Impact of policy

Case 1: In the first case we consider a reduction in the likelihood of obtaining permanent residency only for unemployed low skill workers, $d\rho_u^l < 0$ (around $\rho_e^h = \rho_e^l = \rho_u^h = \rho_u^l$). We then find that:

$$\frac{d\hat{e}}{d\rho_u^l} = \left\{ \frac{1}{r+a+d+\rho_u^l} \left[\theta_T^l - \frac{(r+a+d)\theta_P^l}{r+a} \right] - \frac{d\theta_T^l}{d\rho_u^l} \right\} \frac{k(r+a)}{r+a+\rho_u^h} \frac{1}{c'(\hat{e})} > 0.$$

Noting that $c'(e) < 0$ we find that there is a positive relationship between \hat{e} and ρ_u^l . Because the policy change implied a decrease in ρ_u^l we conclude that the impact of the policy is a decrease in

\hat{e} and thus an increase in the share of individuals that acquire education (remember that \hat{e} is the share of uneducated individuals).

Case 2: In the second case we consider a reduction in the likelihood of obtaining permanent residency only for employed low skill workers, $d\rho_e^l < 0$ (around $\rho_u^h = \rho_u^l$). We then find that:

$$\frac{d\hat{e}}{d\rho_e^l} = -\frac{(r+a)k}{r+a+\rho_u^h} \frac{d\theta_T^l}{d\rho_e^l} \frac{1}{c'(\hat{e})} > 0.$$

There is a positive relationship between the share of uneducated individuals and the probability of permanent residency. This means that a decrease in ρ_e^l will increase investments in education.

Case 3: Finally, in the third case, we consider an increase in the deportation risk, $d_e = d_u = d$ (when $\rho_u^m = \rho_e^m$). We find that:

$$\frac{d\hat{e}}{dd} = \left(\frac{d\theta_T^h}{dd} - \frac{d\theta_T^l}{dd} \right) \frac{(r+a)k}{r+a+\rho_u^h} \frac{1}{c'(\hat{e})} > 0.$$

This implies that when the deportation risk increases, fewer individuals invest in education.

D Descriptive analysis

Figure 1: Overview of the time period of interest



Figure 2: Fraction granted asylum post July 1, 2002, by month of application 2001-2002

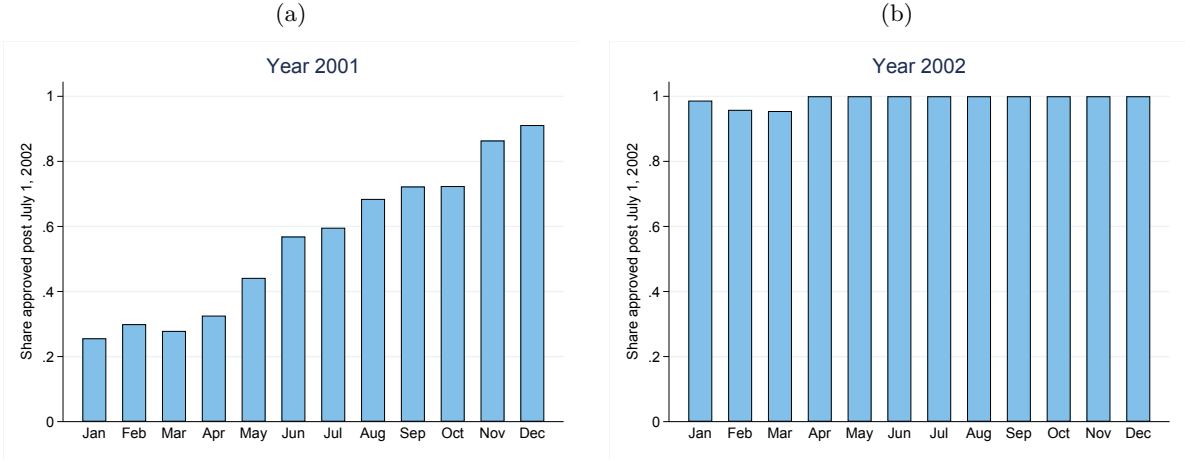
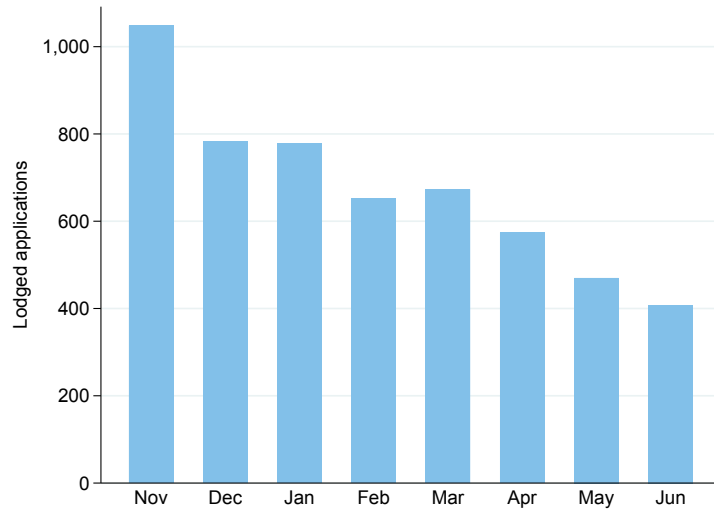
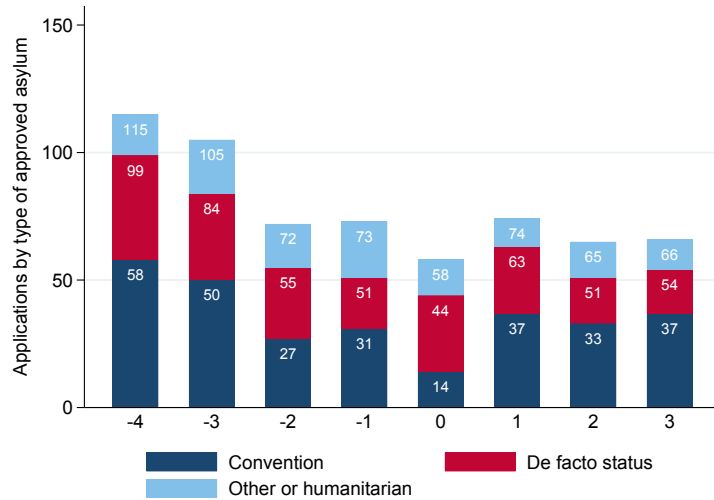


Figure 3: Asylum applications lodged in Denmark, by normalized month of application 2001–2002



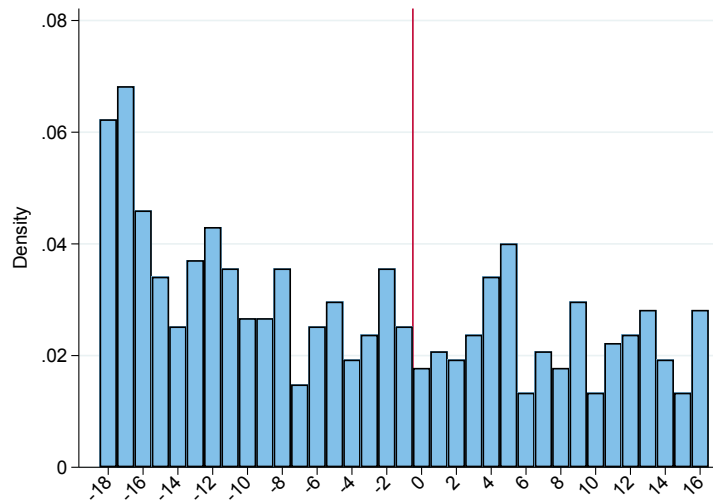
Data source: The Danish Immigration Service (2003).

Figure 4: Number of approved applications, by normalized month of application and type of asylum 2001–2002



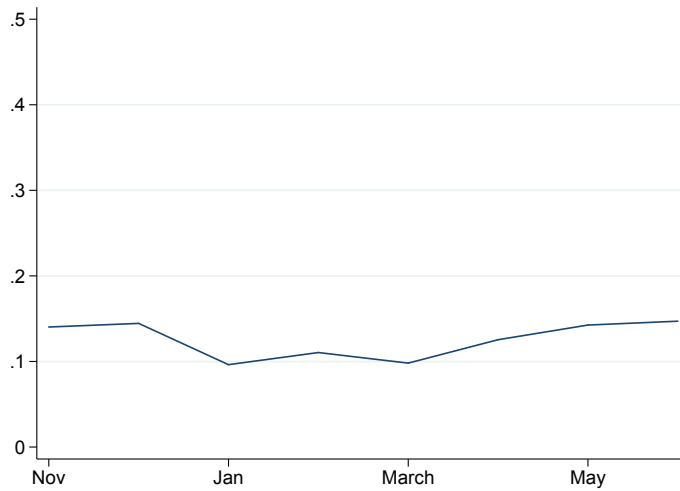
Data source: UDLST.

Figure 5: Density of running variable
 Approved applications lodged November 2001 to June 2002



Notes: Week 17 excluded, due to too few observations ($n < 5$),
 to comply with the rules of Statistics Denmark.

Figure 6: Approval rates for lodged applications, November 2001 – June 2002



Data source: Statistics Denmark.

E Balance

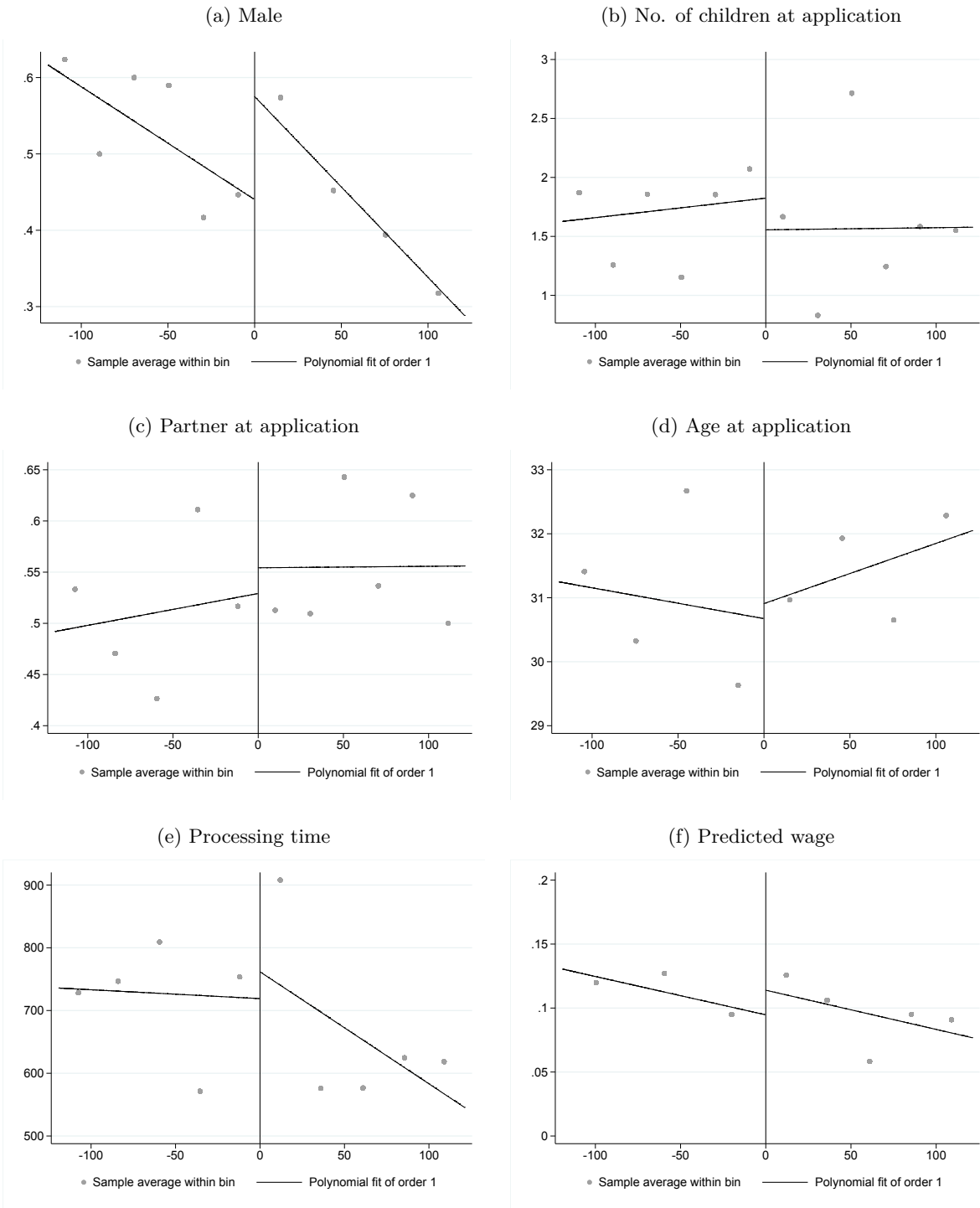
Table 1: Comparison of means (bw 119 days)

	(1)	(2)	(2)–(1)
	Control	Treatment	Normalized difference
<i>Demographic characteristics</i>			
Male	0.54 (0.50)	0.43 (0.50)	-0.22
No. of children	1.71 (1.90)	1.57 (2.03)	-0.07
Partner	0.51 (0.50)	0.56 (0.50)	0.10
Age at application	31.00 (9.10)	31.47 (9.72)	0.05
<i>Education</i>			
Danish 1	0.21 (0.41)	0.24 (0.43)	0.07
Danish 2	0.40 (0.49)	0.36 (0.48)	-0.08
Danish 3	0.27 (0.45)	0.27 (0.45)	0.00
Primary or secondary	0.45 (0.50)	0.47 (0.50)	0.04
Higher	0.24 (0.43)	0.22 (0.42)	-0.05
<i>Country of origin</i>			
Afghanistan	0.34 (0.48)	0.30 (0.46)	-0.09
Iraq	0.19 (0.39)	0.10 (0.30)	-0.26
Former Yugoslavia	0.10 (0.31)	0.15 (0.36)	0.15
Somalia	0.17 (0.37)	0.13 (0.33)	-0.11
Other	0.19 (0.40)	0.32 (0.47)	0.30
<i>N</i>	372	263	

Notes: Values in parenthesis are (s.d.). Demographic characteristics are measured at application. Danish 1 - Danish 3 indicates the level of Danish courses assigned at approval, whereas primary or secondary and higher education indicates the level of education acquired prior to applying for asylum in Denmark. The normalized difference is defined as $\frac{\bar{x}_t - \bar{x}_c}{\sqrt{(sd_t^2 + sd_c^2)/2}}$.

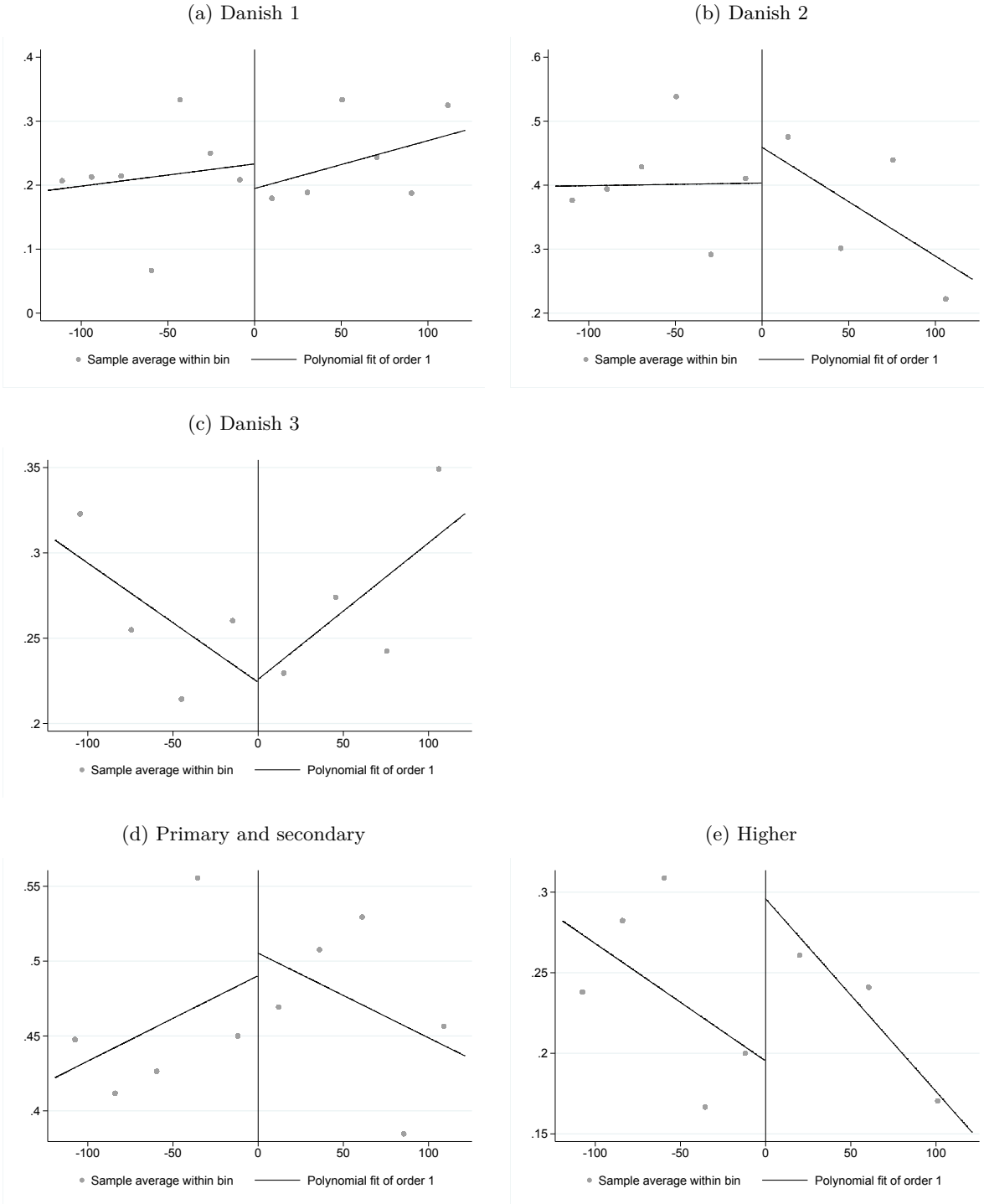
E.1 Regression discontinuity graphs of covariates

Figure 7: General



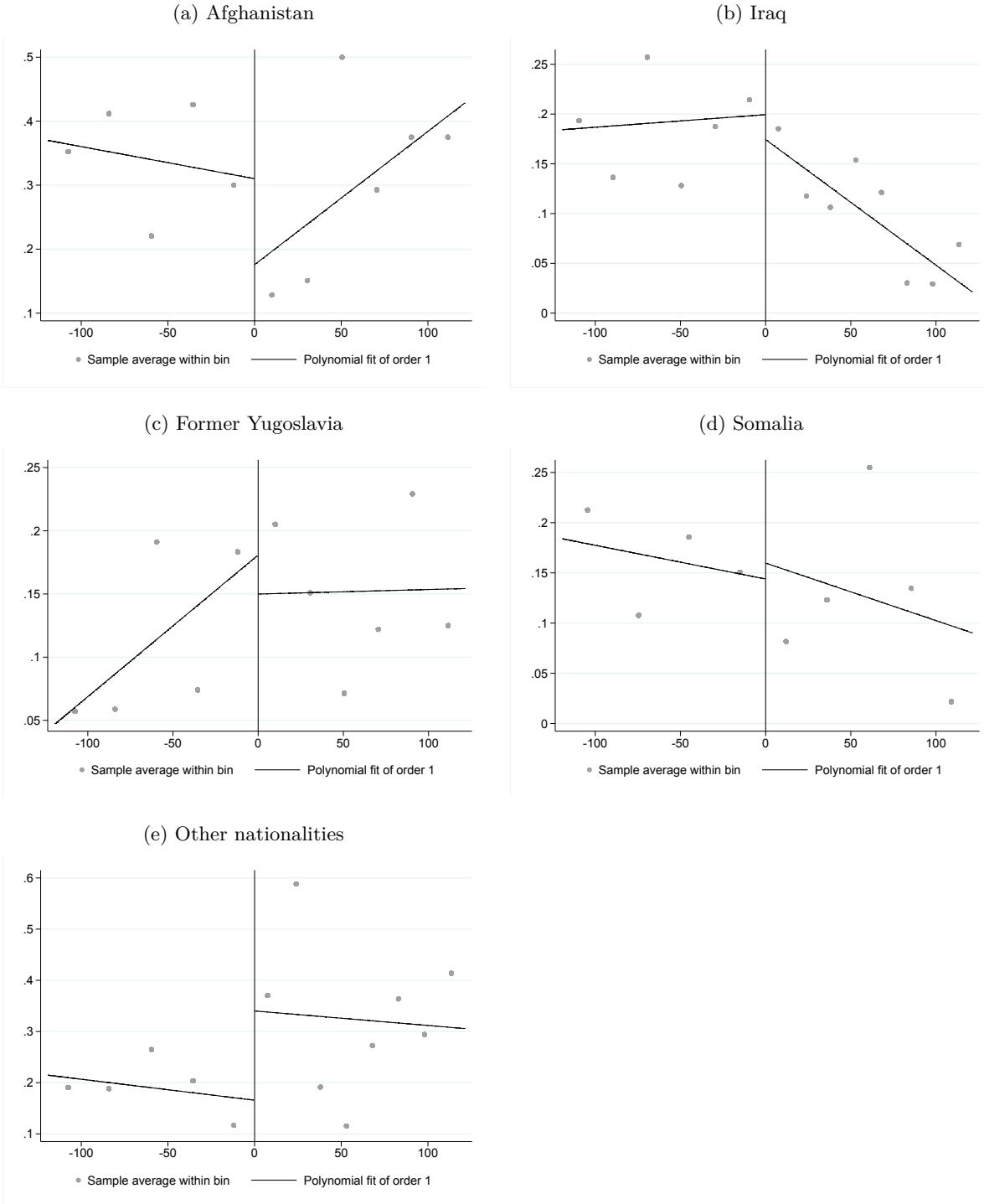
Notes: The graphs are generated using evenly spaced bins, a linear polynomial and a uniform kernel.

Figure 8: Danish language courses and education level



Notes: The graphs are generated using evenly spaced bins, a linear polynomial and a uniform kernel.

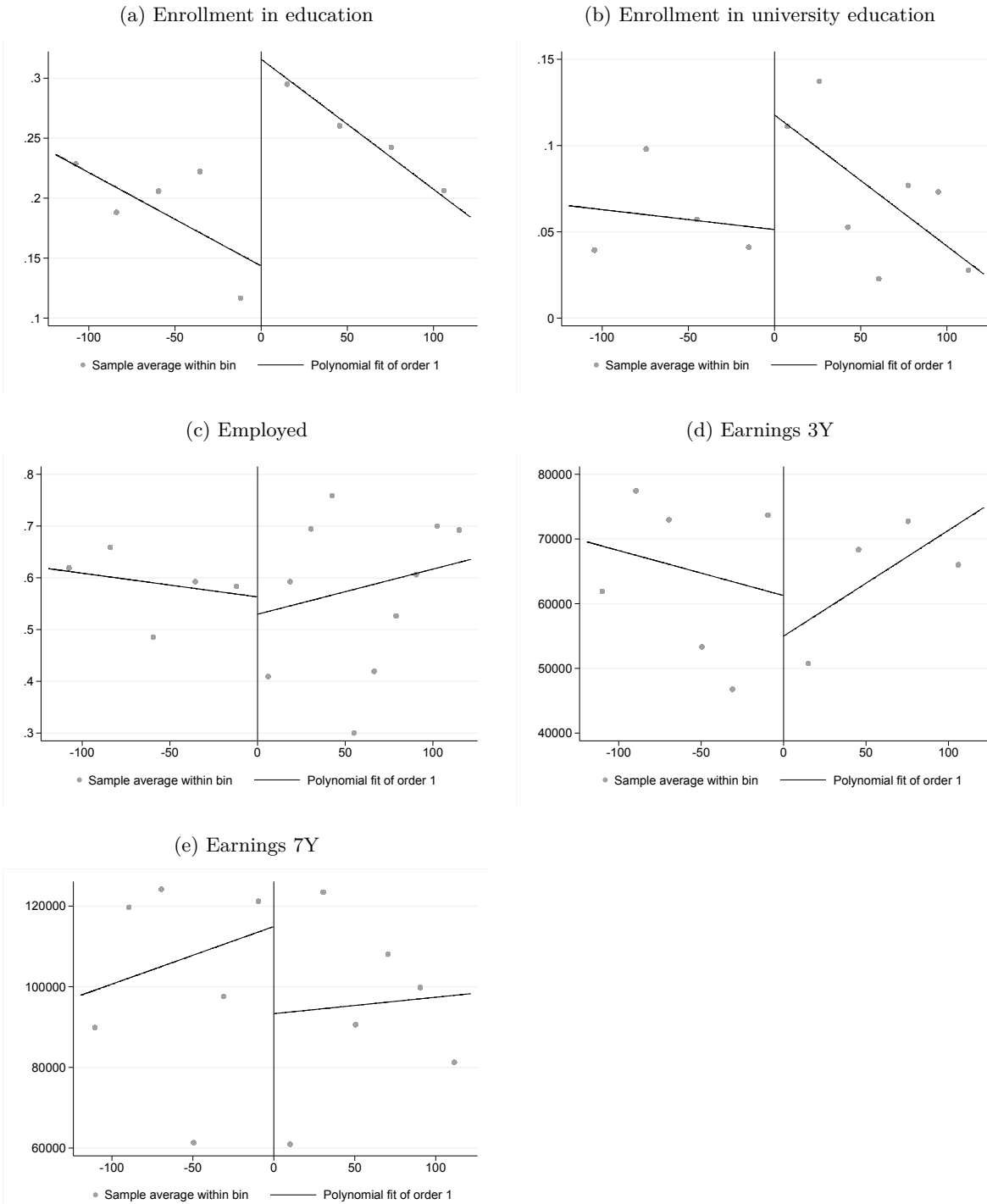
Figure 9: Country of origin



Notes: The graphs are generated using evenly spaced bins, a linear polynomial and a uniform kernel.

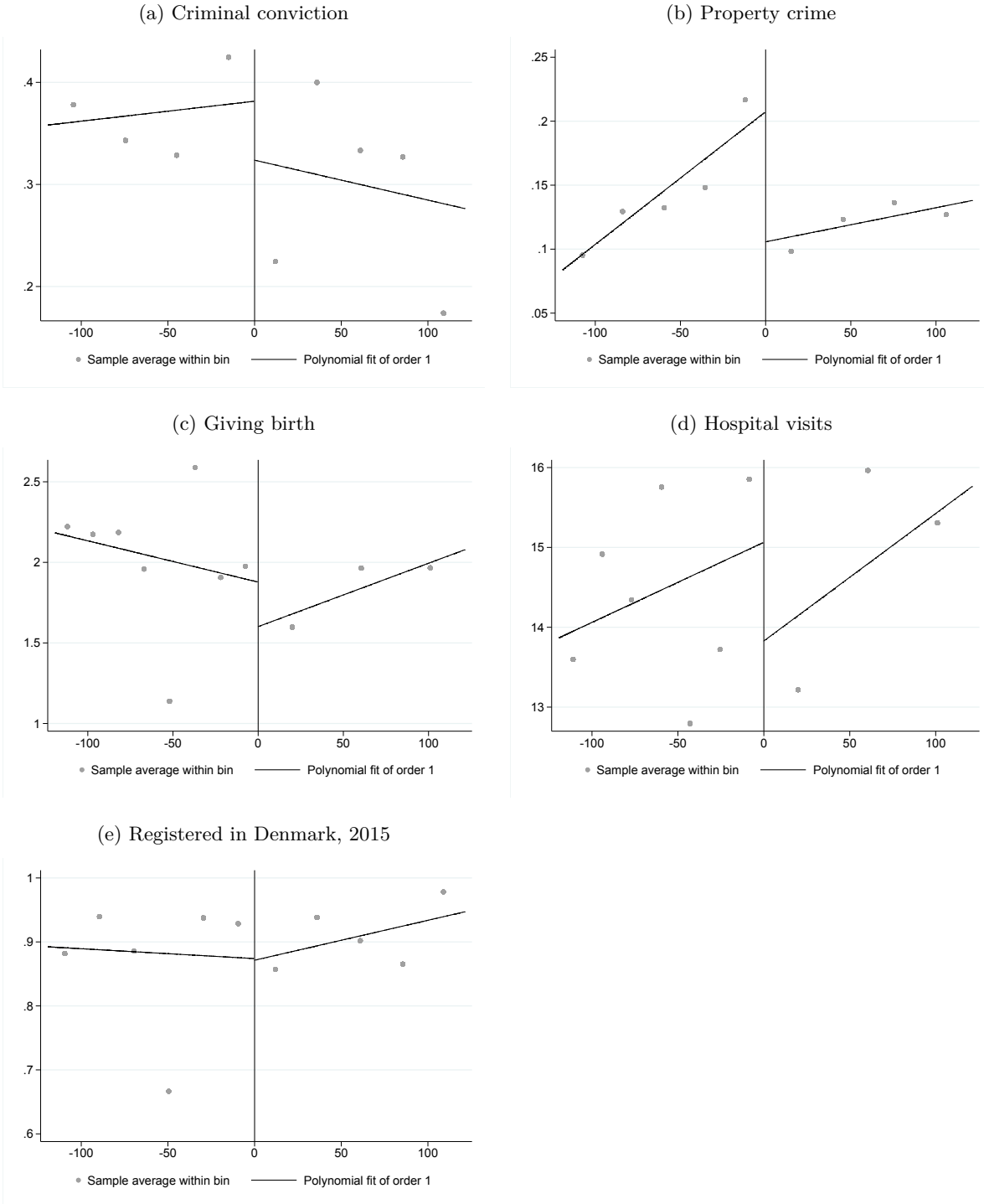
F Regression discontinuity graphs and regressions

Figure 10: Education and labor market outcomes



Notes: The graphs are generated using evenly spaced bins, a linear polynomial and a uniform kernel. Enrollment is a dummy variable equal to one if the individual at some point is enrolled in general education. Enrollment university is the corresponding variable for university education. Employed is a dummy equal to one if the individual was ever employed in Denmark. Earnings is total labor earnings from employment and/or self-employment after three and seven years.

Figure 11: Crime, Fertility behavior and Health



Notes: The graphs are generated using evenly spaced bins, a linear polynomial and a uniform kernel. Registered in Denmark 2015 is a dummy equal to one if the individual is registered in Denmark year 2015. Criminal conviction is a dummy equal to one if ever convicted of any crime. Property crime is equal to one if ever convicted of a property crime. Giving birth is the number of times a family has more children. Hospital visits is the number of doctor/hospital visits.

Table 2: Education outcomes

Outcome	Full sample		Male		Female		High skill		Low skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Enrollment	0.176** (0.070)	0.172*** (0.066)	0.094 (0.096)	0.114 (0.092)	0.278*** (0.103)	0.218** (0.093)	0.002 (0.138)	-0.046 (0.133)	0.180* (0.104)	0.157* (0.095)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
Enrollment university	0.066 (0.043)	0.059 (0.043)	0.066 (0.063)	0.070 (0.063)	0.071 (0.060)	0.038 (0.056)	-0.099 (0.107)	-0.138 (0.107)	0.119* (0.063)	0.107* (0.059)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
Covariates	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Regressions are estimated for the different groups using a polynomial of order 1 and a uniform kernel. High skilled is defined as having a university education, while low skilled have primary or secondary education upon arrival in Denmark. Covariates include age at application, gender, partner, number of children, education level and dummies for the most common nationalities (Afghanistan, Former Yugoslavia, Iraq and Somalia). Enrollment is a dummy variable equal to one if the individual at some point is enrolled in general education. Enrollment university is the corresponding variable for university education. *, ** and *** denotes significance levels at the 10 percent, 5 percent and 1 percent levels respectively.

Table 3: Labor market outcomes

Outcome	Full sample		Male		Female		High skill		Low skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Employed	-0.041 (0.080)	-0.096 (0.073)	-0.002 (0.100)	0.008 (0.095)	-0.088 (0.121)	-0.137 (0.111)	-0.037 (0.170)	-0.134 (0.147)	-0.053 (0.111)	-0.135 (0.101)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
Earnings 3Y	-7,051 (19,971)	-22,536 (18,054)	-9,772 (34,664)	-12,544 (33,728)	-21,887 (14,046)	-27,372** (13,682)	7,385 (39,737)	-4,854 (34,465)	11,043 (26,473)	-10,544 (22,529)
<i>N</i>	601	601	294	294	307	307	143	143	285	285
Earnings 7Y	-22,722 (27,907)	-41,671 (25,969)	-13,564 (50,659)	-16,631 (49,954)	-46,607* (23,948)	-53,064** (23,539)	-90,460 (58,893)	-125,518** (53,692)	2,481 (30,982)	-17,994 (29,911)
<i>N</i>	563	563	266	266	297	297	133	133	278	278
Covariates	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Regressions are estimated for the different groups using a polynomial of order 1 and a uniform kernel. High skilled is defined as having a university education, while low skilled have primary or secondary education upon arrival in Denmark. Covariates include age at application, gender, partner, number of children, education level and dummies for the most common nationalities (Afghanistan, Former Yugoslavia, Iraq and Somalia). Employed is a dummy equal to one if the individual was ever employed in Denmark. Earnings is total labor earnings from employment and/or self-employment after three and seven years. *, ** and *** denotes significance levels at the 10 percent, 5 percent and 1 percent levels respectively.

Table 4: Crime, Fertility behavior and Health

Outcome	Full sample		Male		Female		High skill		Low skill	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
In Denmark 2015	-0.009 (0.049)	0.003 (0.048)	0.047 (0.077)	0.061 (0.075)	-0.052 (0.055)	-0.041 (0.055)	-0.020 (0.100)	-0.005 (0.100)	-0.044 (0.069)	-0.024 (0.065)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
<i>Crime</i>										
Criminal conviction	-0.065 (0.076)	-0.119 (0.072)	-0.313*** (0.110)	-0.269** (0.107)	0.103 (0.010)	0.054 (0.099)	0.001 (0.166)	-0.065 (0.143)	-0.044 (0.111)	-0.049 (0.103)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
Property crime	-0.105* (0.056)	-0.119** (0.056)	-0.282*** (0.073)	-0.251*** (0.072)	0.057 (0.087)	0.031 (0.087)	-0.197 (0.127)	-0.222* (0.119)	-0.075 (0.087)	-0.071 (0.085)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
<i>Fertility behavior</i>										
Giving birth	-0.311 (0.257)	-0.188 (0.236)	0.393 (0.368)	0.313 (0.341)	-0.729** (0.364)	-0.661** (0.305)	-0.498 (0.590)	0.030 (0.563)	-0.362 (0.395)	-0.292 (0.343)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
<i>Health status</i>										
Hospital visits	-1.312 (2.098)	-0.000 (1.983)	0.755 (2.235)	-0.040 (2.217)	-1.130 (3.412)	0.281 (3.431)	1.091 (6.819)	3.627 (6.328)	-1.077 (2.681)	0.118 (2.324)
<i>N</i>	635	635	315	315	320	320	150	150	292	292
Covariates	NO	YES	NO	YES	NO	YES	NO	YES	NO	YES

Notes: Regressions are estimated for the different groups using a polynomial of order 1 and a uniform kernel. High skilled is defined as having a university education, while low skilled have primary or secondary education upon arrival in Denmark. Covariates include age at application, gender, partner, number of children, education level and dummies for the most common nationalities (Afghanistan, Former Yugoslavia, Iraq and Somalia). In Denmark 2015 is a dummy equal to one if the individual is registered in Denmark year 2015. Criminal conviction is a dummy equal to one if ever convicted of any crime. Property crime is equal to one if ever convicted of a property crime. Giving birth is the number of times a family has more children. Hospital visits is the number of doctor/hospital visits. *, ** and *** denotes significance levels at the 10 percent, 5 percent and 1 percent levels respectively.

G Long term outcomes

G.1 Quadratic polynomial fitted over time

Figure 12: Education outcomes

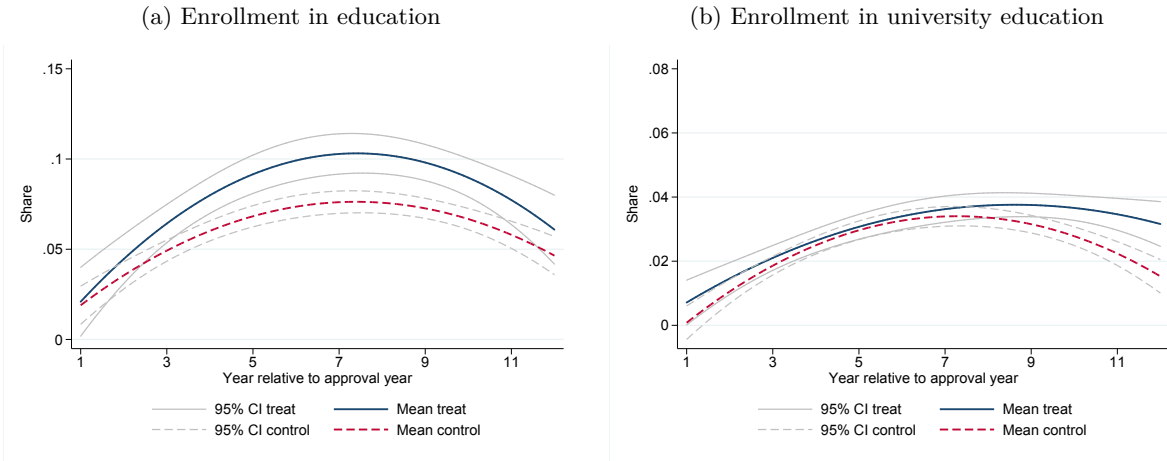


Figure 13: Labor market outcomes

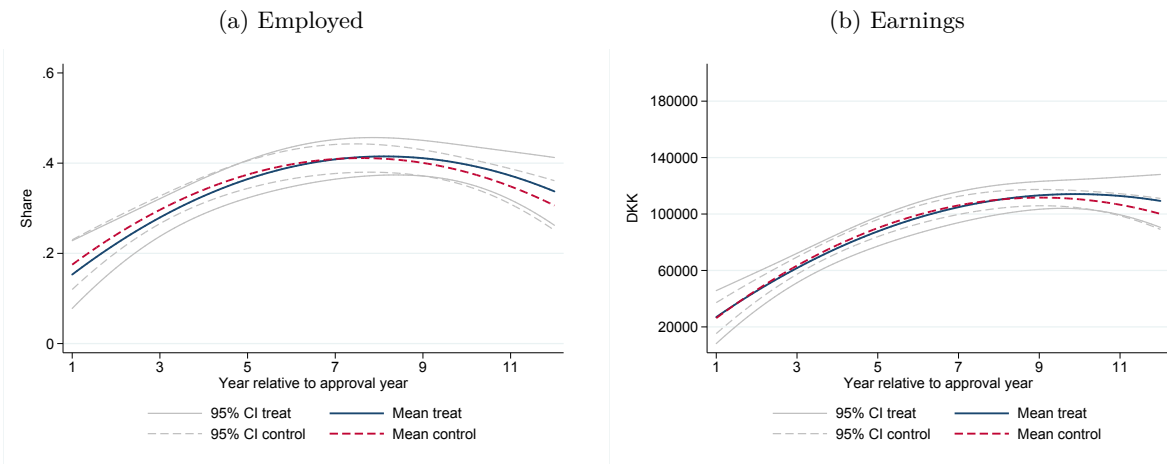
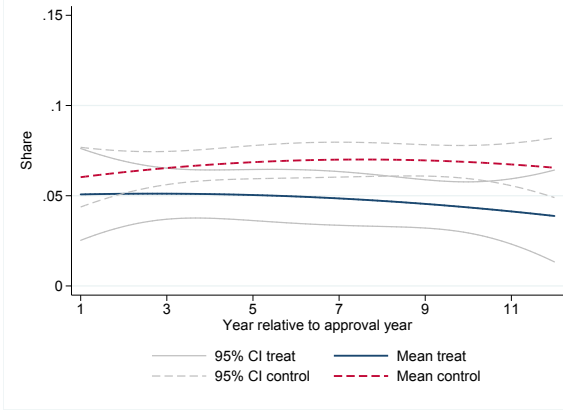
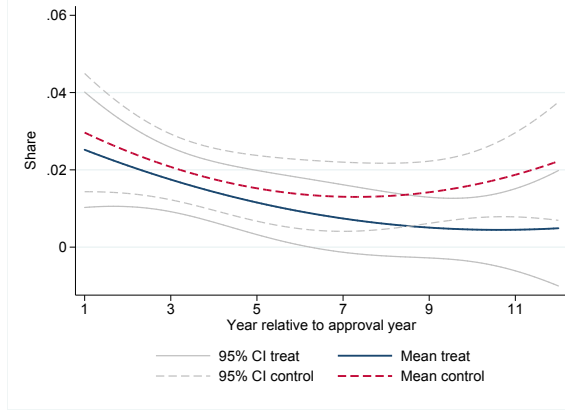


Figure 14: Crime, Fertility behavior and Health

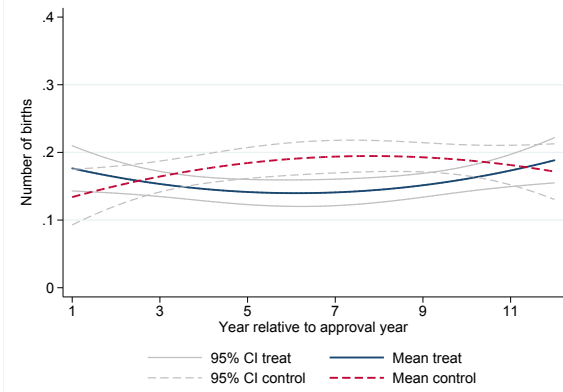
(a) Criminal conviction



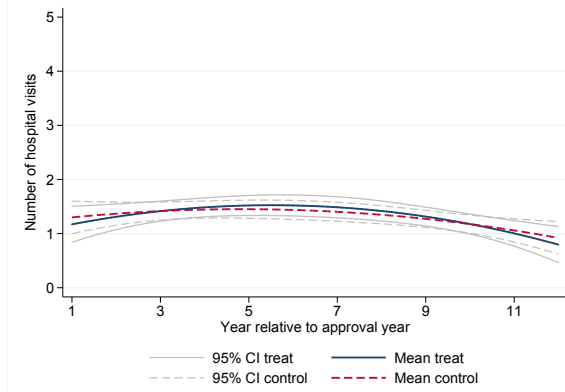
(b) Property crime



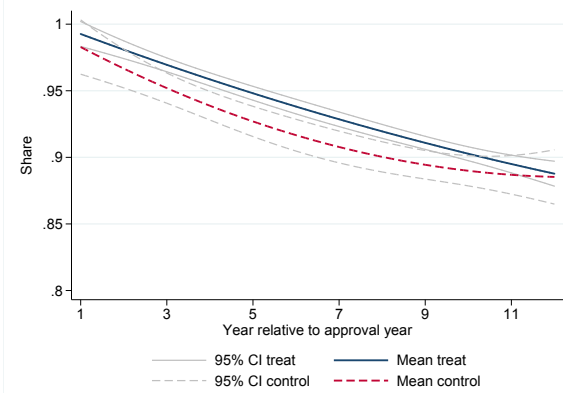
(c) Giving birth



(d) Hospital visits



(e) Fraction still living in Denmark



H Balance of covariates

Table 5: Comparison of means for individuals residing in Denmark 2015 (bw 119 days)

	(1)	(2)	(2)–(1)
	Control	Treatment	Normalized difference
<i>Demographic characteristics</i>			
Male	0.51 (0.50)	0.43 (0.50)	-0.16
No. of children	1.78 (1.91)	1.66 (2.05)	-0.06
Partner	0.53 (0.50)	0.57 (0.50)	0.08
Age	30.92 (9.15)	31.65 (9.73)	0.08
<i>Education</i>			
Danish 1	0.23 (0.42)	0.25 (0.43)	0.05
Danish 2	0.40 (0.49)	0.36 (0.48)	-0.08
Danish 3	0.26 (0.44)	0.28 (0.45)	0.04
Primary or secondary	0.48 (0.50)	0.47 (0.50)	-0.02
Higher	0.23 (0.42)	0.23 (0.42)	0.00
<i>Country of origin</i>			
Afghanistan	0.37 (0.48)	0.33 (0.47)	-0.08
Iraq	0.19 (0.39)	0.10 (0.30)	-0.26
Former Yugoslavia	0.10 (0.30)	0.16 (0.37)	0.18
Somalia	0.13 (0.34)	0.10 (0.30)	-0.09
Other	0.21 (0.41)	0.31 (0.47)	0.23
<i>N</i>	329	239	

Notes: Values in parenthesis are (s.d.). This table show the means and normalized difference for individuals in our sample that were still residing in Denmark in 2015. Demographic characteristics are measured at application. Danish 1 - Danish 3 indicates the level of Danish courses assigned at approval, whereas primary or secondary and higher education indicates the level of education acquired prior to applying for asylum in Denmark. The normalized difference is defined as $\frac{\bar{x}_t - \bar{x}_c}{\sqrt{(sd_t^2 + sd_c^2)/2}}$.

H.1 Regression discontinuity coefficients on covariates for different bandwidths

Figure 15: General

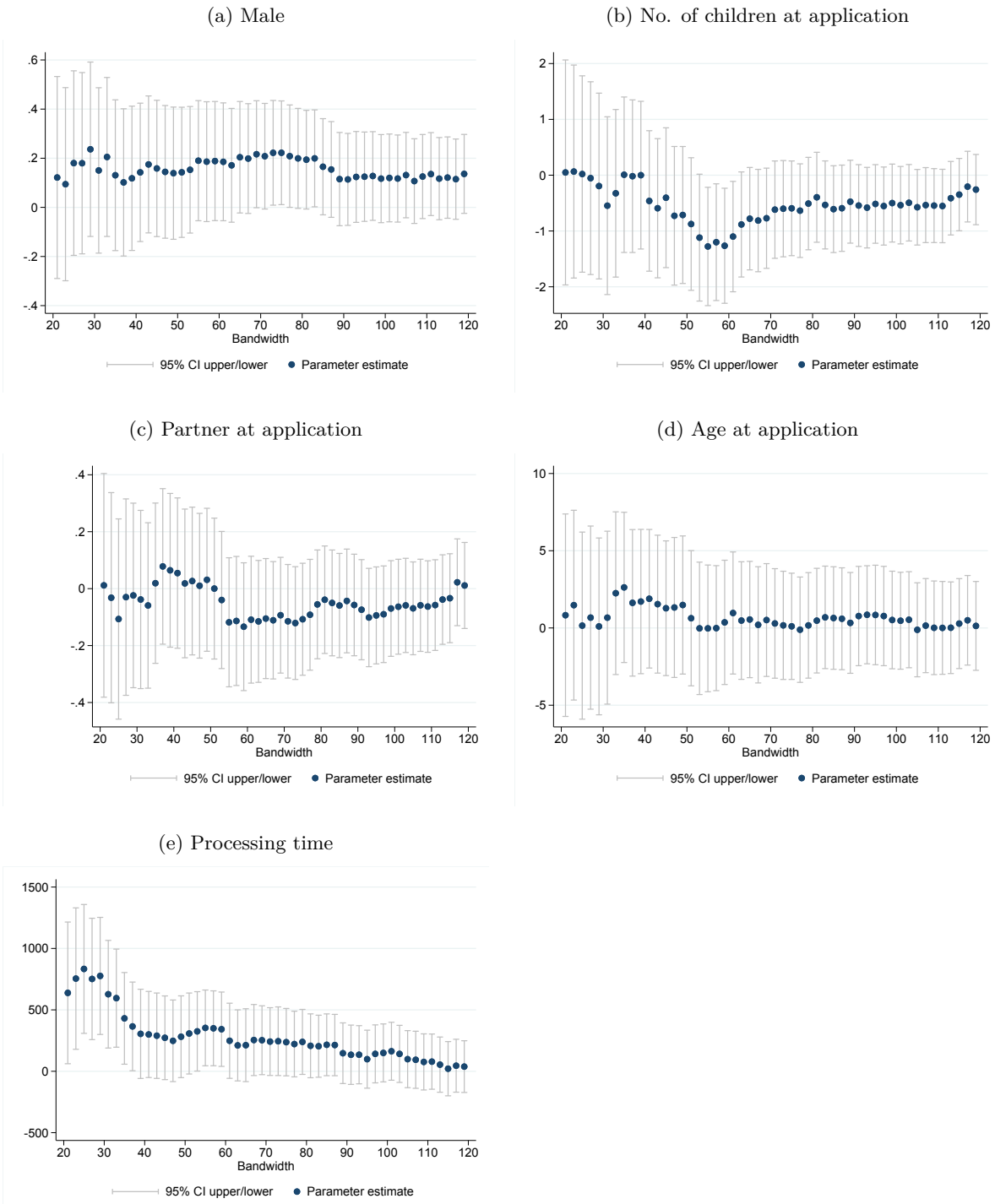
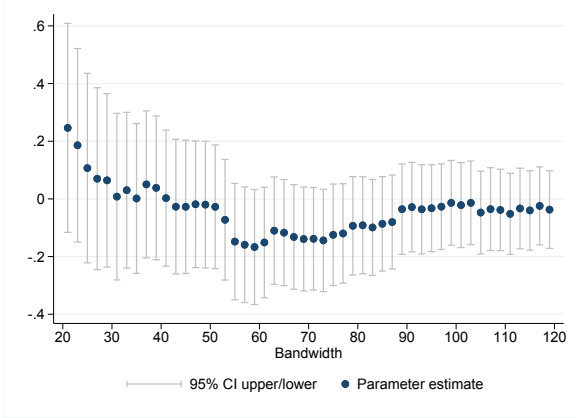
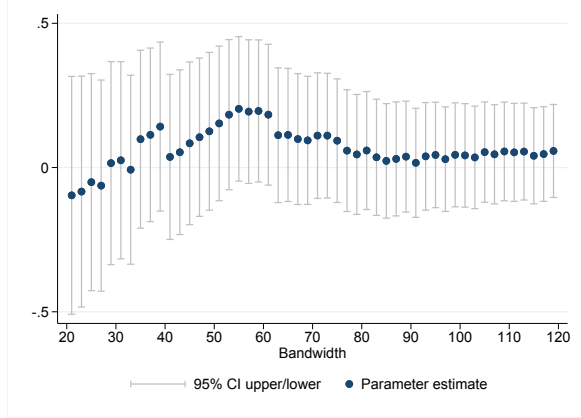


Figure 16: Danish language courses and education level

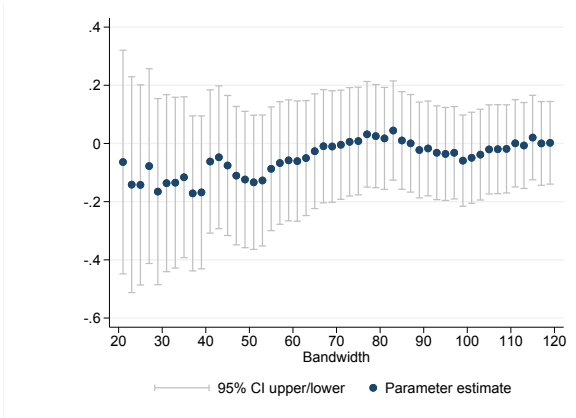
(a) Danish 1



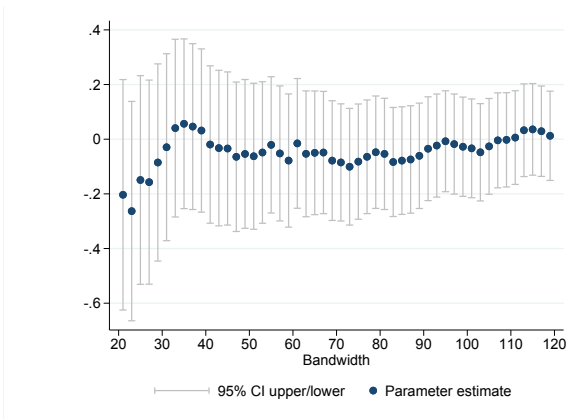
(b) Danish 2



(c) Danish 3



(d) Primary and secondary



(e) Higher

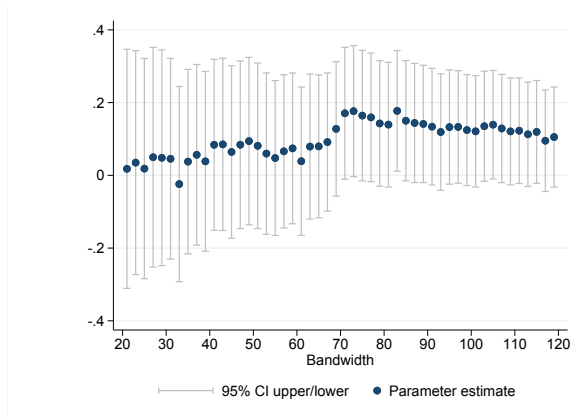
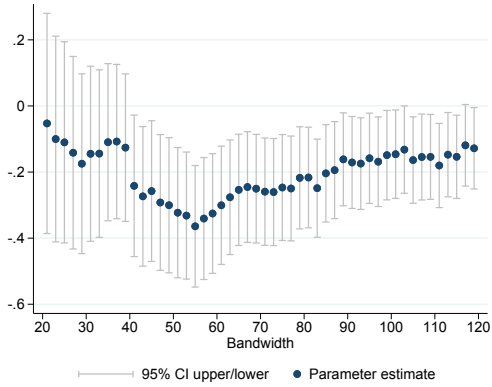
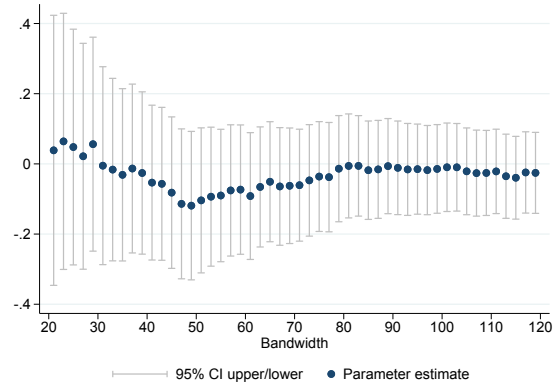


Figure 17: Country of origin

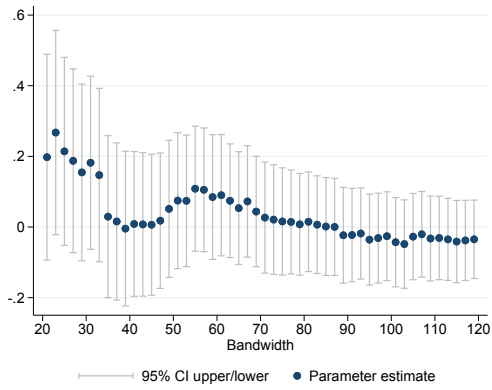
(a) Afghanistan



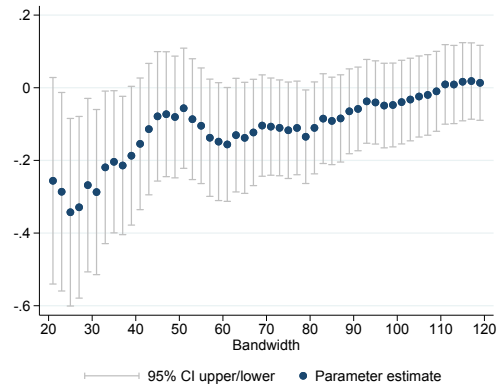
(b) Iraq



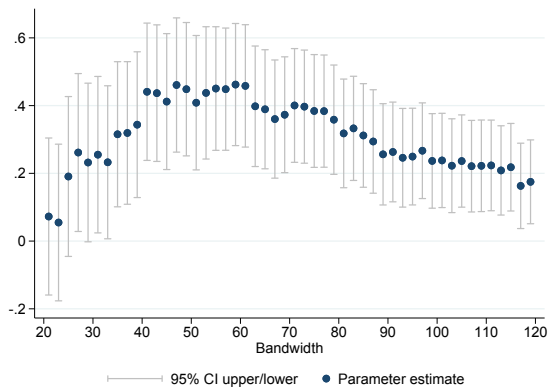
(c) Former Yugoslavia



(d) Somalia



(e) Other nationalities



H.2 Outcomes at the cutoff for different bandwidths (incl. controls)

Figure 18: Education outcomes

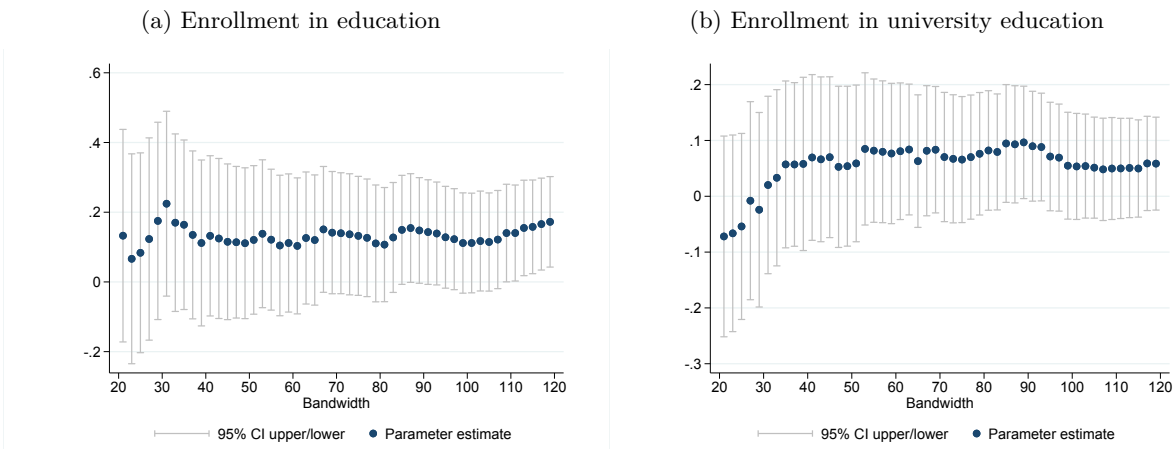
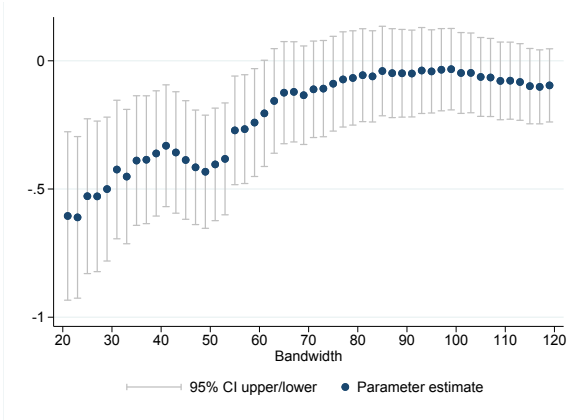
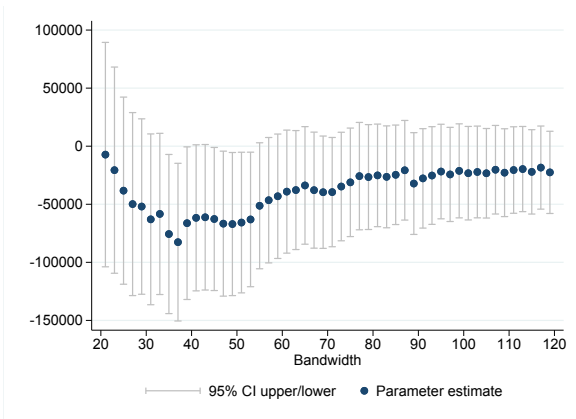


Figure 19: Labor market outcomes

(a) Employed



(b) Earnings 3Y



(c) Earnings 7Y

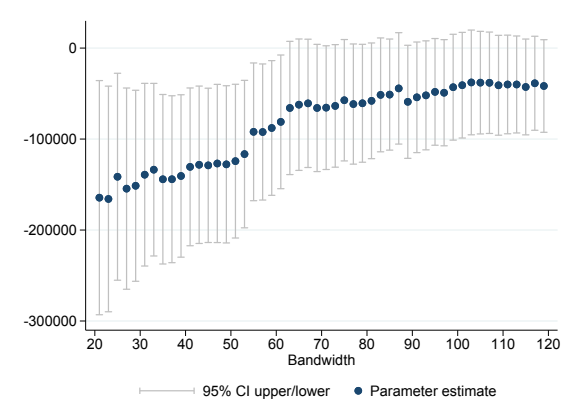
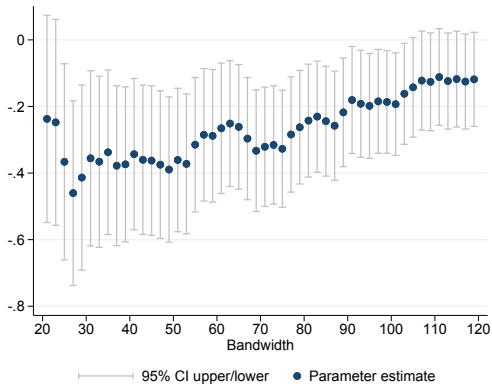
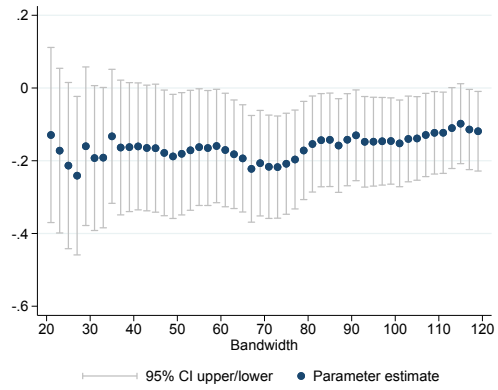


Figure 20: Crime, Fertility behavior and Health

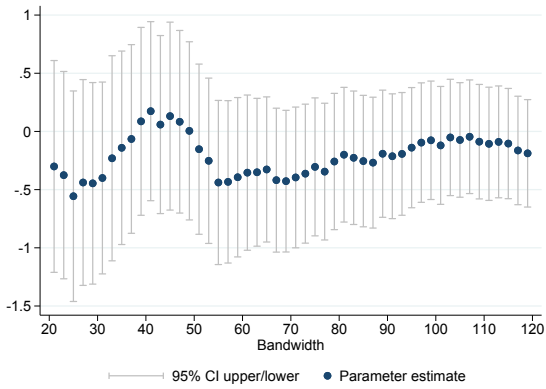
(a) Criminal conviction



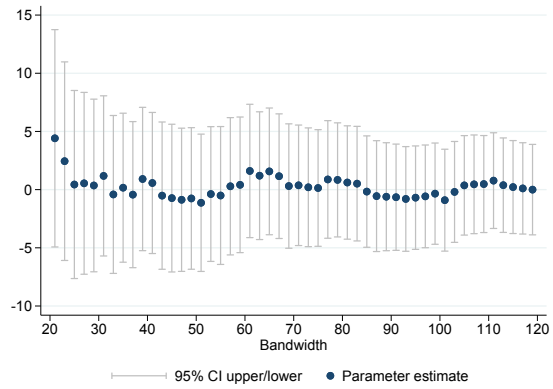
(b) Property crime



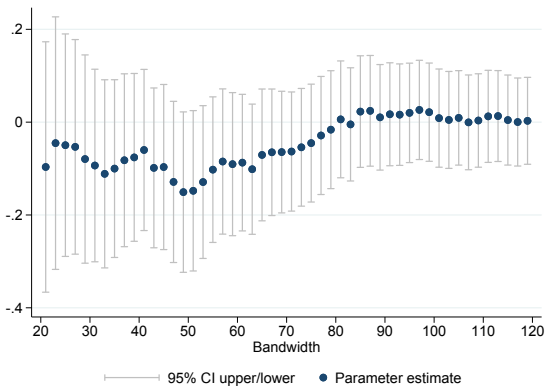
(c) Giving birth



(d) Hospital visits



(e) Registered in Denmark, 2015



H.3 Outcomes over time

H.3.1 Regression discontinuity coefficients over time

Figure 21: Education and labor market outcomes

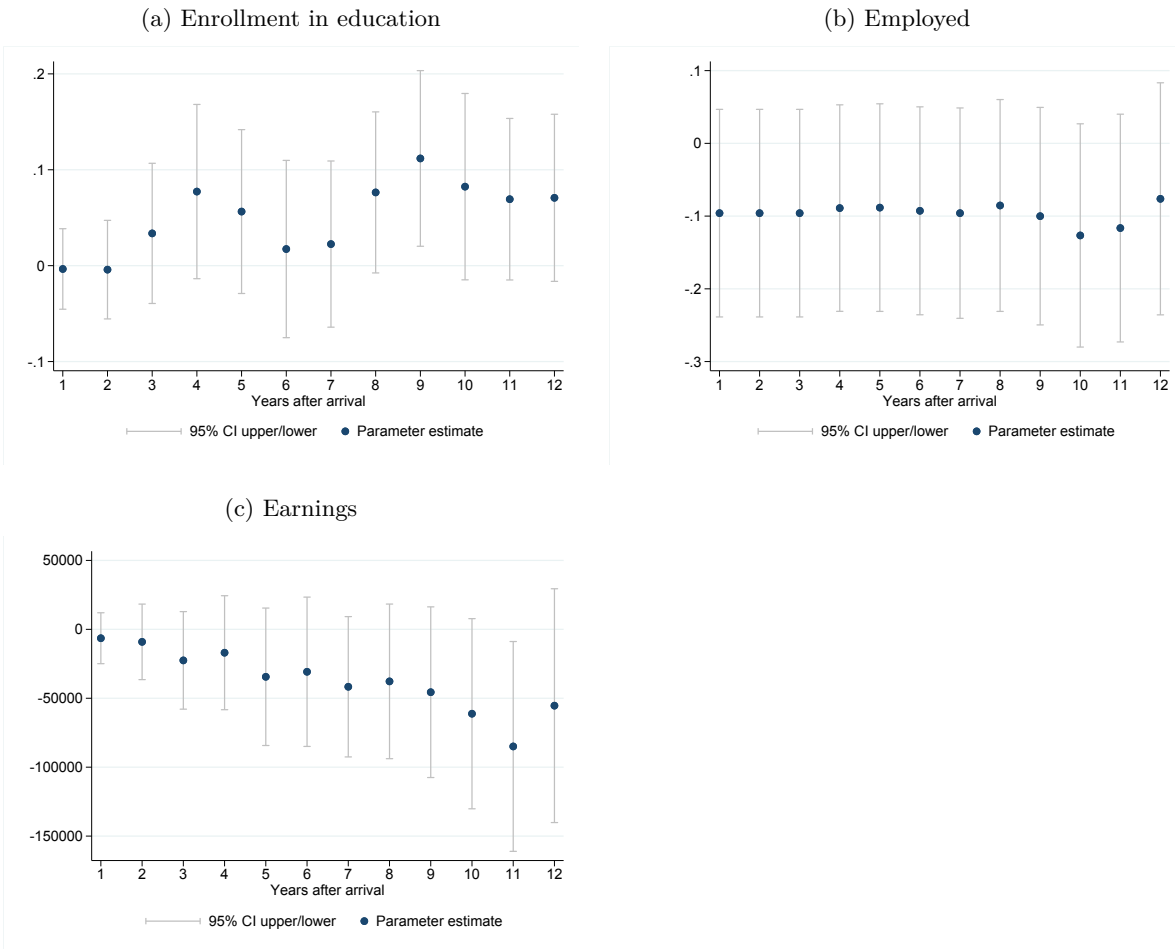
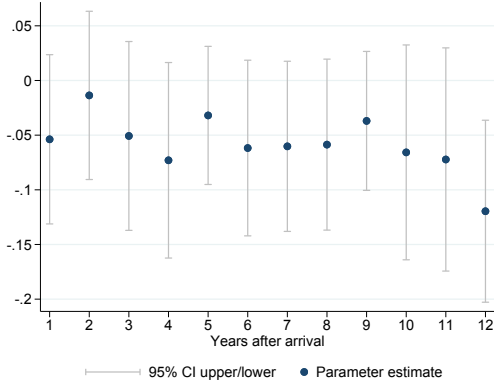
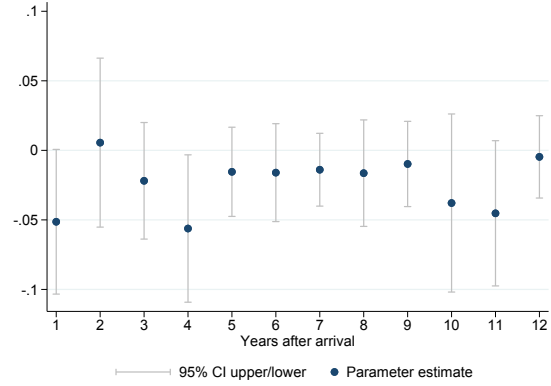


Figure 22: Crime, Fertility behavior and Health

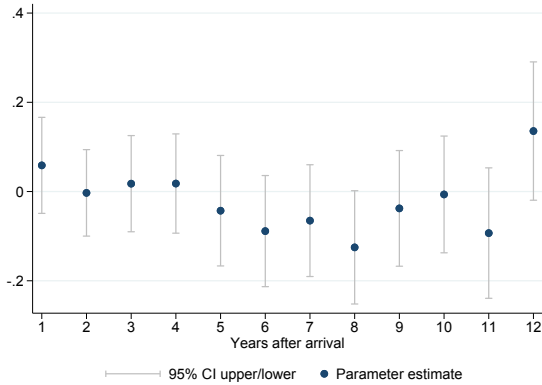
(a) Criminal conviction



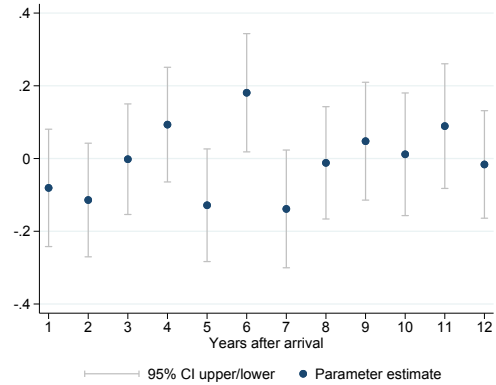
(b) Property crime



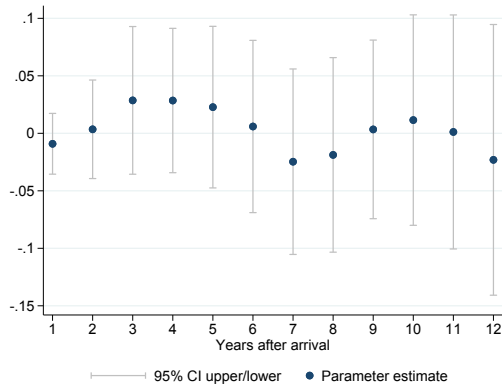
(c) Giving birth



(d) Hospital visits

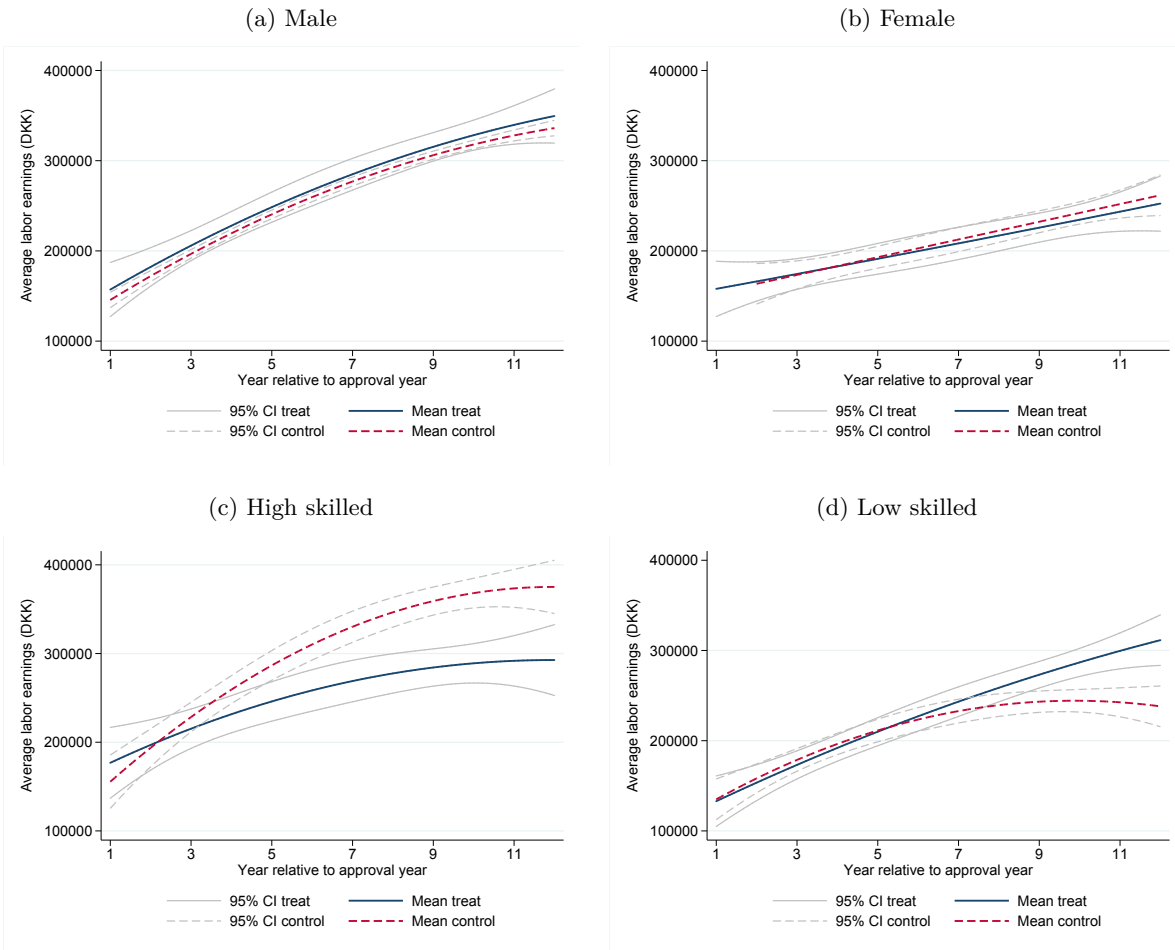


(e) Registered in Denmark, 2015



H.3.2 Outcomes over time: heterogeneous effects

Figure 23: Earnings conditional on employment



H.4 Test for manipulation of the running variable

H.4.1 Density test

Table 6: Regression Discontinuity Density test

	bw 30 days		bw 60 days		bw 90 days		bw 119 days	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
p-value	0.017	0.623	0.363	0.009	0.200	0.077	0.121	0.935
Degree of polynomial	1	2	1	2	1	2	1	2

Notes: Test is implemented using the RDDENSITY command in Stata, using the robust bias-corrected estimates. Reported values are p-values from this test.

H.4.2 Placebo tests

Table 7: Placebo test: Education outcomes

Outcome	Left of the cutoff		Right of the cutoff	
	(1)	(2)	(3)	(4)
Enrollment	0.298 (0.187)	0.279 (0.181)	0.042 (0.208)	-0.093 (0.202)
Enrollment university	0.048 (0.114)	0.090 (0.112)	0.048 (0.048)	-0.066 (0.064)
Covariates	NO	YES	NO	YES

Notes: Regressions are estimated for the full sample using a polynomial of order 1 and a uniform kernel. Covariates include age, gender, partner, number of children, education level (all measured at application) and dummies for the most common nationalities (Afghanistan, Former Yugoslavia, Iraq and Somalia). We split the sample in two halves at the cutoff. Then we run the regression on each sample using the median as cutoff.

Table 8: Placebo test: Labor market outcomes

Outcome	Left of the cutoff		Right of the cutoff	
	(1)	(2)	(3)	(4)
Employed	-0.019 (0.221)	-0.090 (0.201)	0.211 (0.366)	0.874* (0.451)
Earnings 3Y	-9,993 (78,314)	-27,485 (59,605)	53,063 (41,749)	43,639 (60,357)
Earnings 7Y	35,296 (85,831)	4,172 (73,671)	72,119 (65,266)	54,080 (76,644)
Covariates	NO	YES	NO	YES

Notes: Regressions are estimated for the full sample using a polynomial of order 1 and a uniform kernel. Covariates include age, gender, partner, number of children, education level (all measured at application) and dummies for the most common nationalities (Afghanistan, Former Yugoslavia, Iraq and Somalia). We split the sample in two halves at the cutoff. Then we run the regression on each sample using the median as cutoff.

Table 9: Placebo test: Crime, Fertility behavior and Health

Outcome	Left of the cutoff		Right of the cutoff	
	(1)	(2)	(3)	(4)
In Denmark 2015	0.104 (0.101)	0.116 (0.089)	0.226 (0.146)	0.222* (0.124)
<i>Crime</i>				
Criminal conviction	-0.214 (0.200)	-0.209 (0.194)	0.271 (0.203)	-0.002 (0.217)
Property crime	-0.144 (0.168)	-0.064 (0.147)	0.163 (0.194)	0.147 (0.168)
<i>Fertility behavior</i>				
Giving birth	-0.575 (0.687)	-0.295 (0.669)	-1.803** (0.891)	1.881*** (0.664)
<i>Health status</i>				
Hospital visits	-2.654 (7.311)	-5.605 (5.629)	-1.059 (5.799)	-16.570** (7.757)
Covariates	NO	YES	NO	YES

Notes: Regressions are estimated for the full sample using a polynomial of order 1 and a uniform kernel. Covariates include age, gender, partner, number of children, education level (all measured at application) and dummies for the most common nationalities (Afghanistan, Former Yugoslavia, Iraq and Somalia). We split the sample in two halves at the cutoff. Then we run the regression on each sample using the median as cutoff.

H.5 Mapping from DISCO codes to skill levels

We use the Danish version of the International Standard Classifications of Occupations (DISCO) to map working functions, that we observe in the data, into different skill levels. We define skill level by picking one occupation per year and person. If a person has more than one job, we take the job with the highest skill level. If there is more than one job with the same skill level, we pick the most common job within that skill level. Finally, if a person has two jobs from different occupations that have the same skill level in a given year, we pick the occupation with the highest ranking according to the DISCO code. If a person has equally many jobs in two occupations with the same skill level and one of the occupations is in the armed forces, we pick the civilian occupation as the main occupation. This variable is used to see if there is a change in average skill level over time.

Code	Description	Skill level
1	Managers	3 + 4 ^a
2	Professionals	4
3	Technicians and Associate Professionals	3
4	Clerical Support Workers	2
5	Services and Sales Workers	2
6	Skilled Agricultural, Forestry and Fishery Workers	2
7	Craft and Related Trades Workers	2
8	Plant and Machine Operators and Assemblers	2
9	Elementary Occupations	1
0	Armed Forces Occupations	1, 2 + 4 ^b

^aLevel 3 for managers in Hospitality, Retail and Service. Other managers have skill level 4.

^bMilitary officers are level 4, other occupations are at level 1. Non-commissioned Officers count as skill level 2.