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THE EFFECT OF WELFARE BENEFIT REDUCTIONS ON THE INTEGRATION OF REFUGEES

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The effect of welfare benefit reductions on the integration of refugees

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Abstract

This study utilizes two policy experiments that reduced welfare benefits, first for one group of newly arrived refugees, and second for another group of refugees who had been in the country for at least ten months. The results show that refugees respond quickly to both benefit reductions, but that men and women respond on different margins. Males enter employment faster when they experience a benefit reduction from the time of arrival and the group who experience the benefit reduction after ten months catches up. The benefit reduction has unintended effects on crime that mirrors this picture, but it is clearer for women than for men. Women do not respond to the benefit reduction on the labor market, but utilize primary health care more. There are no significant differences after two years in the country between those treated from arrival and those treated after ten months.

Keywords: Immigrant, Unemployment, Welfare Benefits, Crime, Health Care

JEL code: J15; J61, J64, J68

Introduction

It has been well documented in recent years that refugees have low employment rates in many Western countries, even after having lived for a substantial number of years in the countries (Bratsberg et al 2017; Dustmann et al. 2017; Fasani et al. 2017; Schultz-Nielsen 2017; Åslund et al. 2017). One potential reason for the lack of labor market integration is the combination of relatively generous welfare benefits and high labor income taxes that prevails in some Western countries, which may leave the refugees with limited economic incentives to work. This study tests this hypothesis by examining the response to a substantial reduction in the welfare benefits refugees are entitled to when they are unemployed. A special focus on refugees is warranted because there is a public concern and dispute about how to handle humanitarian migration; consequently, a number of countries such as the US, Denmark, the Netherlands, Hungary, and Austria have implemented policies that limit the access to social support or welfare benefits for immigrants (OECD 2018; 2019).

The study examines a double policy experiment to test the responses to welfare benefit reductions. In 2015 the Danish Government introduced a new type of welfare benefits called "*integrationsydelse*" (integration benefits), for all unemployed immigrants who were granted residence in Denmark from September 2015. The new type of benefits was 10-60% lower than previous benefit levels. The second experiment occurred ten months later, and entailed that unemployed immigrants who had arrived in Denmark before September 2015 were also subjected to the benefit reduction. The two policies produce a discontinuity in the level of welfare benefits that I use to identify the response to the generosity of welfare benefits by means of the regression discontinuity (RD) design.

A main contribution of the study is that the double policy experiment provides a rare opportunity to double check the response to the benefit reduction. Another contribution is that I describe the monthly short-term dynamics of the effects from the time of arrival and relate them to the timing of the two policies. This is possible due to the access to detailed administrative data. The data also enabled me to identify humanitarian immigrants, who are known to have very different labor market trajectories than most other immigrants[1].

The study contributes to the larger literature on how public policy can impact labor market participation by adjusting the economic incentive to work. A large part of this literature is based on studies of the means-tested earnings tax credit programs in the US and UK. This literature generally supports that employment-contingent benefits have a positive impact on employment for low-income families, particularly single parents (Blundell 2000; 2013; Chetty et al. 2013; Eissa and Liebman 1996; Francesconi and van der Klaauw 2007; Gregg and Harkness 2009; Meyer and Rosenbaum 2001; Nichols and Rothstein 2016; van den Linden 2016). In a similar vein, four studies from continental Europe have examined the effect of welfare-benefit generosity for young unemployed people (Bargain and Doorley 2011, 2017; Jonassen 2013; Lemieux and Milligan 2008). All four studies find that a reduction in welfare-benefit levels implies a positive but modest increase in employment or a modest reduction in welfare-benefit dependence. These findings cannot, however, necessarily be extended to groups of disadvantaged or long-term unemployed people. A few studies have examined specific employment-contingent premiums for the long-term unemployed but with mixed evidence (Card and Robins 2005; Van der Klaauw and Van Ours 2013; Arendt and Koldziejczyk 2019). Work that is closer in spirit to the current study are the studies that examine the consequences of the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 (PRWORA) for immigrants in the US[2]. The PWRORA replaced previous welfare programs

[1] The type of immigrant is defined in terms of residence permits and the types are further described below.
[2] It is stressed that there is a large literature that addresses the effects of the US welfare reforms in general (e.g. Schoeni and Blank 2000; Blank 2002; Grogger and Karoly 2005; Blank 2009), but only few of these studies identify the effects of the generosity of the welfare benefits per se and few studies consider the most disadvantaged unemployed, such as refugees.

by the Temporary Assistance for Needy Families (TANF), but limited the access to this program for immigrants. Some US states created programs that ensured access to welfare benefits for immigrants, and some of the states ensured a benefit level beyond minimum standards. Kaestner and Kaushal (2005) utilize such state variation and find that foreign-born women increased their labor supply when the access to welfare benefits was restricted, with larger effects for the recently arrived. Similar results have been found by Borjas (2016), who shows that the labor supply response is sufficient to alleviate an increase in poverty rates that could have been the consequence of limited access to welfare benefits. Refugees were, however, exempted from these welfare cutbacks, and LoPalo (2019) finds that refugees who arrived in states with more generous welfare benefits during the TANF program experienced higher wages in the longer run, without experiencing a reduction in their employment rates.

Looking outside the US, four studies have estimated the effects of a large welfare-benefit reduction that occurred in Denmark in 2002 for newly arrived refugees. All four studies find a positive employment effect (Huynh et al. 2007; Rosholm and Vejlin 2010; Andersen et al. 2012; Andersen et al. 2019). In contrast to the US experience, the labor supply response is far from sufficient to compensate the drop in income that arises from the benefit reduction. Andersen et al. (2019) argued that a means-testing procedure at the household level in Denmark contributes to the overall negative effects. Given that at least one study in the US has also questioned whether benefit reductions are effective for refugees in the longer run (LoPalo 2019), and that the Danish studies are based on the consequences from a single policy reform for a small and narrow population, there seems to be a need for more research on the issue.

A welfare benefit reduction can have adverse effects on both benefit recipients and their off-spring, particularly when the labor supply response to the reduction is not sufficient to alleviate an income reduction. It is too soon to evaluate the consequences on the off-spring of the groups affected by the policy change considered in the current paper[3], so I focus on adverse effects for the benefit recipients in terms of adult crime.

The adverse effects of benefit reductions are even less well documented than the labor market effects. A negative relationship between economic outcomes and crime has been documented in many studies, but the empirical literature has mainly been based on aggregate data, where it is notoriously hard to produce causal evidence (Bushway 2011). It has been found that crime rates correlate negatively over a monthly cycle with the timing of welfare benefit payments in the US (Dobkin and Puller 2007; Foley 2011; Wright et al. 2011). Yang (2017) has found that access to the TANF program for convicted drug felons reduces recidivism into prison. In concurrence with this finding, Andersen et al. (2019) has found that the welfare benefit reduction in 2002 led to an increase in adult property crime rates for refugees in Denmark. In contrast, such adverse effects do not seem to arise when welfare reforms have a positive impact on income. Examples are provided again by the PRWORA reform, which reduced property crime rates for the target group of mainly lone mothers in the US (Schoeni et al. 2000; Corman et al. 2014).

In addition to studying crime as an adverse outcome, I also look at whether the welfare benefit reduction has an impact on health-care utilization. Reduced welfare benefits from the time of arrival may add to an already stressful arrival in the host country, as documented for instance by a higher prevalence of mental diseases among refugees than among other immigrants (Hynie 2018). A recent evaluation has documented that mental problems are also prevalent among asylum seekers in

[3] The effect of a welfare benefit reduction on the off-spring has only been studied in a few papers. The Danish welfare benefit reduction in 2002 worsened outcomes for the off-spring in terms of language test scores, years of education and teenage crime rates (Andersen et al. 2019). Several studies have examined the consequences of US welfare reforms that aimed at reducing welfare dependency in general. Some of the welfare reforms in the 1990s have, for instance, been shown to reduce drop-out rates in high school for the off-spring of the affected adults (Kaestner, Korenman and O'Neill, 2003; Koball, 2007; Dave et al., 2012), but also that they had a number of negative effects on adolescent behavior, particularly for boys, such as skipping school, property damage, smoking and drug use (Dave et al. 2019).

Danish refugee camps (Rigsrevisionen 2018). Once admitted, all refugees have free access to health care and an interpreter is paid for in Denmark. Increased use of specialized health care utilization could therefore be a rough proxy for health problems. However, the reduction in welfare benefits may also increase the use of primary health care, for instance in an attempt to access disability pension. In that sense, use of primary care can be viewed as a robustness check for a behavioral response. I know of no previous studies which have considered the consequences of welfare-benefit generosity on health or health-care utilization[4]. A related study found that the overall decrease in welfare-benefit caseloads that occurred during the 1990s in the US (of which the welfare reforms only explained a part) had no effect on the health status of low-educated women but improved their health behaviors (Kaestner and Tarlov 2006).

A final strand of literature of relevance to the current study has examined whether the generosity of welfare benefits affects the number of immigrants seeking to the country (Ortega and Peri, 2009; Schultz-Nielsen, 2016; Brekke et al., 2017; Kleven et al. 2019). This is not the focus in the current study, and it is important to stress that such behavior does not affect our results since the population who was affected by the benefit reduction had applied for residence before the policy was announced.

The paper is organized as follows: In the next section, I describe the institutional settings and the policy reform used for identification. Then the administrative register data and the empirical design are explained. The results are presented in section five and they are discussed in the last section.

[4] Several studies have considered the utilization of health care in the US after the PWRORA reform. Because the reform itself reduced access to health care the results in these studies can not be interpreted as a consequence of reduced welfare benefits.

Institutional settings

In this section, I describe the process by which refugees and their family members obtain legal residence, and I describe the activities they must participate in, in order to qualify for welfare benefits.

Asylum and subsidiary protection can be obtained by application once in the country (illegally) or from abroad through agreements with the United Nations. When applying for asylum in Denmark, the applicant waits for the decision in an asylum camp, and in 2015-16 the waiting time was around 6 to 12 months (Hvidfeldt and Schultz-Nielsen 2017). Once granted residence, settlement across municipalities is determined by a public dispersal policy. This dispersal policy allocates refugees based on quotas determined by the number of immigrants from non-Western countries already in the municipality. When they have obtained legal residence, the refugees can apply for family reunification. These procedures not only entail that immigrants do not settle in places determined by labor market options, but also that the decision to immigrate to Denmark was taken long before the welfare benefits were reduced. The procedures therefore effectively turn off two selection mechanisms often present in migration studies.

All refugees and their family members are eligible for welfare benefits from the time of settlement. The welfare benefits are not time limited and, prior to the benefit reduction, are generous when seen in an international context (Hansen and Schultz-Nielsen 2015). The refugees can be sanctioned financially by withholding welfare benefits if they do not participate in a three-year long introduction program that is offered from the first month of arrival. The aim of the program is to help immigrants become self-supported, so the program consists of employment support for the unemployed and a Danish language course. The language course is offered at three levels, and the participants are divided into levels based on their education level from abroad: Level 1 is for the illiterate and those who have not completed primary schooling, level 2 is for those who have completed primary education, and level 3 is for those who have an education beyond primary schooling.

As I also examine the consequences of the welfare-benefit reduction on health-care utilization, I briefly describe the Danish health-care system. Immigrants with legal residence have access to health care from the date they are granted residence. There are no user fees for use of the public health-care system, but co-payments for medical prescriptions are required. In terms of planned specialized care, a referral by a general practitioner is required. An assessment from a general practitioner is also needed to qualify for disability benefits. Since 2013, municipalities have been obligated to offer a health examination to refugees and their family members within three months of their arrival.[5] The public health-care system also finances the use of an interpreter for newly arrived immigrants for the first three years they are in the country when such services are needed. Because refugees often suffer from the experience of traumatic events and may also have lacked medical attention prior to arrival, I expect that the use of primary care is higher for newly arrived refugees in the years after arrival than for the general population.

The welfare benefit reduction

To counter the disincentives from a generous welfare-benefit system for newly arrived refugees, welfare benefits have been reduced for immigrants on several occasions in Denmark. This study focuses on a reduction of the welfare benefits that took place in 2015. At this time, immigrants and natives had been eligible for the same amount of welfare benefits since 2012. The high level of welfare benefits is called "*kontanthjælp*" (social assistance). The reduced level of welfare benefits was called "*integrationsydelse*" (integration benefits)[6]. The bill behind the reduction was proposed

[5] From July 2016, a health examination has been offered based on an assessment of the individual's needs. This does not affect our study population.
[6] It is similar, but not identical, to the reduced benefits called ´start aid´ examined in the papers mentioned in the introduction.

in June 2015 and passed by Parliament in August that year. It entered into force on 1 September 2015 for immigrants who had obtained legal residence and for whom the municipality took over the integration responsibility from this date and onwards (Act No. 1000, 2015, section 16(5) and 6).

Table 1 illustrates the level of social assistance and integration benefits across different population groups. The table shows that the integration benefits are 10-60% below the social assistance level. The largest difference occurs for couples and persons older than 25 with no children. The system is means-tested, so any other income is subtracted from the welfare benefits, except for DKK 25 per hour of work. The means-testing system works on a household level, so it affects couples twice: Income beyond the welfare benefit level that the individual working would be entitled to is also subtracted from the welfare benefits of the individual who is not working.

Table 1. The level of welfare benefits across sub-population groups.

Dependent child:	Yes, single		Yes, couple		No		
Age:	<30	30+	<30	30+	-25	25-30	30+
Integration benefits (USD)	1829	1829	1280	1280	915	915	915
Social assistance (USD)	2022	2241	2022	2241	1036	1611	2326
Reduction	10%	18%	37%	43%	12%	43%	61%

Note: The amounts are shown in USD, converted from 2015 Danish kroner at an exchange rate of 6.5 DKK/USD. The level for immigrants excludes a bonus for passing the Danish course level 2, which usually takes two to three years, and the level with children is shown for parents with custody. The level for natives includes a bonus for participation in active labor market activities. Source: Act No. 468 (2016).

Since different integration policies are often implemented at the same time, it is important to stress that, in the current case, there were no other changes that occurred simultaneously with the benefit reduction. However, an amendment to the act entailed that all immigrants who have resided in Denmark for less than seven years within the past eight years were finally subjected to the benefit reduction (Act No. 300, 2016). The amendment was the result of the negotiations for the annual Finance Act in November 2015 (Ministry of Finance 2015). The amendment was proposed in January 2016, it was passed by Parliament on March 22, and entered into force on July 1 2016. I describe below how I use these two policies to identify the effect of the benefit reduction after I have described the data.

Data

I use administrative data with information on all immigrants with residence in Denmark. The population is restricted to refugees granted asylum in accordance with the Geneva convention, and immigrants who are granted subsidiary protection or other humanitarian protection, as well as their adult family members. I refer to this selection of immigrants as refugees in short. Among the refugees I focus on the group aged between 18 and 64 in the year of arrival who were registered as living in a municipality for the first time one year before and one year after September 2015 and who received welfare benefits within one month upon their arrival. The sample consists of 7,400 men and 4,781 women. 46% of the sample arrived from September 2015 and therefore experienced a benefit reduction from the time of their arrival, while the remaining part experienced the benefit reduction ten months after their arrival or later. Even though both groups experienced the benefit reduction, I refer to the first group as the treatment group and the second as the control group.

Table 2. Mean characteristics for refugees arriving one year before and after reduction of benefits

	Men			Women		
	Control	Treated	Total	Control	Treated	Total
Any child	0.18	0.23	0.20	0.66	0.69	0.67
Child aged 0-2	0.09	0.14	0.11	0.29	0.35	0.32
Child aged 3-6	0.13	0.14	0.13	0.49	0.49	0.49
Age	31.3	29.5	30.6	30.8	30.6	30.7
Couple	0.20	0.26	0.22	0.70	0.74	0.72
Married	0.50	0.42	0.47	0.75	0.69	0.72
Syria	0.74	0.72	0.73	0.73	0.82	0.78
Eritrea	0.19	0.20	0.19	0.13	0.09	0.10
Other country	0.07	0.09	0.08	0.14	0.09	0.11
Danish course level 1	0.35	0.37	0.36	0.39	0.40	0.40
Danish course level 2	0.55	0.53	0.54	0.51	0.50	0.50
Danish course level 3	0.09	0.08	0.09	0.08	0.07	0.08
N	4436	2964	7400	2078	2703	4781

Notes: Refugees arriving 12 months before (control) and after (treated) 1 September 2015.

Table 2 presents mean characteristics for the treated and control groups in this sample. It can be seen that three out of four men who arrive during the period are from Syria, only one in five are part of a couple, and few have children. In contrast, a much larger share of the women are part of a couple and have a child when they arrive. Close to 100% participate in a Danish language course; around half are enrolled in the Danish course at level 2 for those who have a primary education.

The outcomes considered in the study are labor market outcomes, criminal charges, and health-care utilization. I use employment, pre-tax labor income and hours of work reported to tax authorities as the main labor market outcomes. Employment is measured as having positive labor income in the given month[7]. I supplement these labor market outcomes with annual receipt of welfare benefits, all types of transfer incomes, and disposable income. Annual income measures are available until 2017 only, i.e. up to two years after arrival. Crime is measured from the administrative data obtained from Statistics Denmark and contains information on dates and type of criminal charges. Because of the short time horizon, I focus on criminal charges, and not convictions. I include charges based on criminal law, where the main types of crime are violence and property crime. Finally, data on public health-care utilization is obtained from Statistics Denmark. I focus on the number of annual health care contacts (face-to-face meeting, telephone meeting or email instructions), and I distinguish between health care obtained from a general practitioner, contacts related to mental health problems at a psychologist or a psychiatrist, contacts related to physical problems at a physiotherapist or chiropractor, and finally total contacts including dentists and other specialists.

[7] I focus on regular hiring and therefore do not include employment, hours worked or labor income in months with subsidized employment.

Empirical design

The two policies described above, the law on the benefit reduction and its amendment, produce two experiments that reduce the level of welfare benefits spaced ten months apart: The first applies to refugees who arrive after 1 September 2015, and the second to refugees who arrived before September 2015.

I use the policy experiments to identify the intent-to-treat effect of the benefit reductions by means of a regression discontinuity (RD) design (Thistlewaite and Campbell 1960; Lee and Lemeiux 2010). The running variable is the date at which the municipality took over the integration responsibility, which I measure as the first week receiving welfare benefits, and the threshold is September 1 2015.[8] The main RD estimates are obtained by the ordinary least squares estimator of the following equation:

$$y_{it} = \alpha + \beta_t 1(r_i \geq 0) + g_1(r_i)1(r_i < 0) + g_2(r_i)1(r_i \geq 0) + \pi X_i + \epsilon_{it},$$

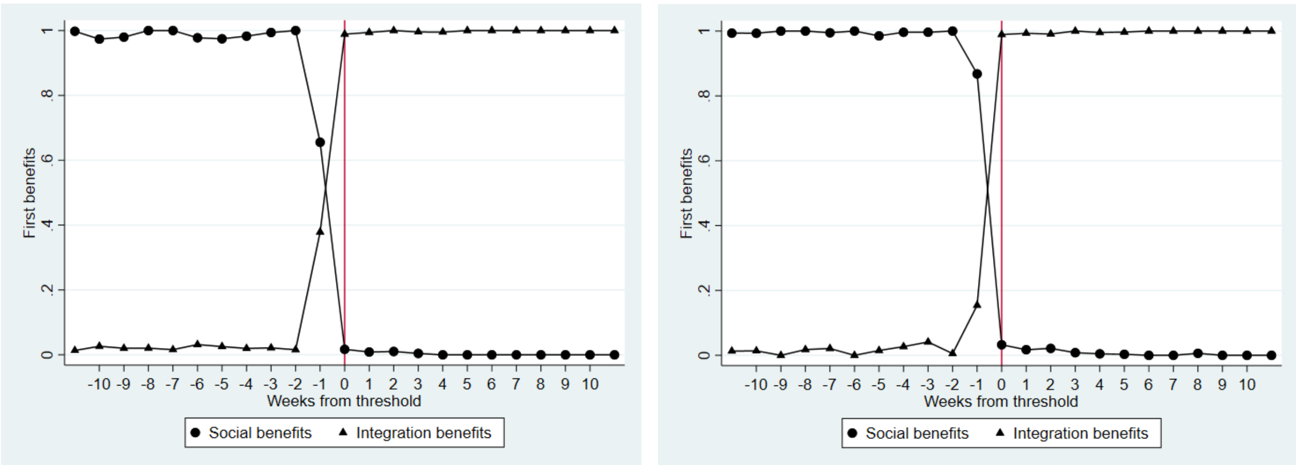
Where y_{it} is the outcome for person *i*, in period *t*, where period 0 is the time where the refugee starts receiving welfare benefits. r_i is the running variable (week of first benefit receipt), g_1 and g_2 are polynomials in the running variable that are allowed to differ on each side of the cut-off date (September 1 2015), and X_i is observed characteristics determined prior to period 0. I use first- and second-order polynomials of g_1 and g_2 to avoid problems of higher-order polynomials (Gelman and Imbens 2018). I have also estimated the effects non-parametrically using the rdrobust estimator (Calonico et al. 2017). The sign of the non-parametric estimates is mostly the same as the parametric estimates, but almost all are insignificant, so they are not shown.

[8] Immigrants who obtained residence in August were not exempted from the reduction if the municipalities took over the integration responsibility for them in September 2015. I therefore use the week of first benefit receipt as the running variable. On average, there is a two-week gap from time of residence to time of first welfare benefits.

Since the two benefit reductions occur ten months apart, an effect at or before ten months is the effect of the benefit reduction at the time of arrival versus no benefit reduction. Effects after ten months compare the early treated with the later treated, after both groups have been treated. It is important to stress that an absence of effects after ten months does not imply that a benefit reduction has no effect beyond ten months after arrival, but merely that the impact of early and later reductions does not differ.

It is possible that the control group anticipates the reduction. Such an anticipation provides an incentive for the control group to find employment before the benefit reduction after ten months. It could therefore limit the effect of the immediate reduction, which would then be a downward-biased estimate of the effect of the benefit reduction versus no benefit reduction. Note that the anticipation effects imply that refugees will react to the expectation of such a drop in income, before the income reduction. This could for instance be the case for health-care utilization, if the anticipation of a benefit reduction produces a stressful environment, or if it induces immigrants to seek health care to become eligible for disability pension.

Figure 1. Type of welfare benefits received in the first week, for men (left) and women (right)

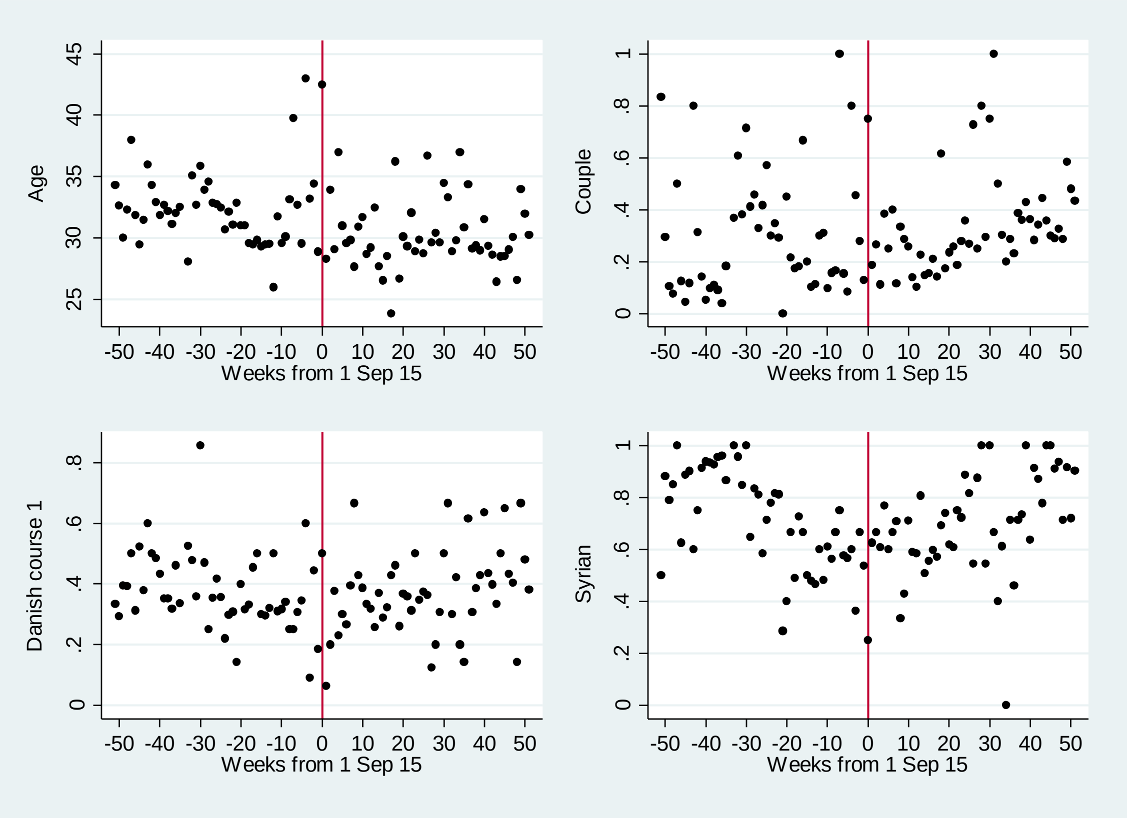


Notes: The first axis is centered around the first week of September 2015.

Figure 1 shows that the benefit reform had a clean impact on the type of benefits received in the first week of benefit receipt: Before September 2015, close to 100% receive social assistance, whereas after a rapid transition in the week before September, close to 100% receive the lower integration benefits from September 2015. To avoid that the take-up in the week before September attenuates the true effect, this week is excluded from the main sample. I later show that the results are not affected by this exclusion. The majority applied for asylum several months before the benefit reduction was proposed. There is therefore limited scope for manipulation around the threshold by the refugees. Figure A.1 in the appendix illustrates the number of weekly arrivals across the running variable and shows no signs of manipulation around the threshold, beyond within-monthly variation.

Figure 2 shows the means of four main characteristics for male arrival cohorts around the threshold. They show no signs of discontinuous shifts in mean characteristics. A similar figure for women is found in Appendix Figure A.2. When I test for discontinuities in the observable characteristics at the threshold, I mostly find insignificant effects, but there are significant effects for, for example, country of origin (see Appendix Figure A.2). I provide a robustness test below which shows that these discontinuities are not likely to be able to explain the presence of effects.

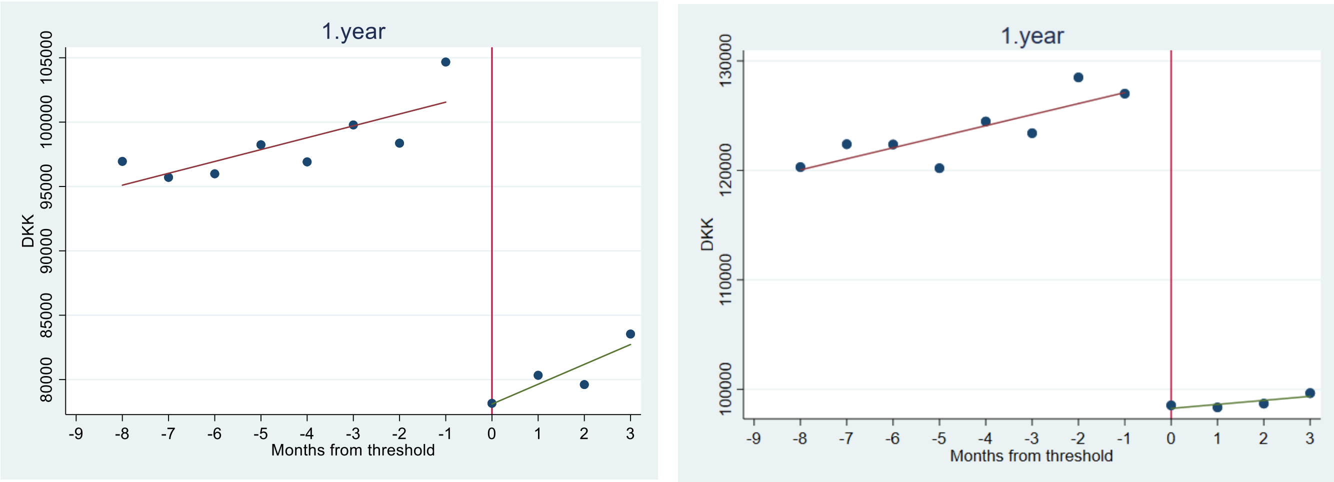
Figure 2. Variation in average characteristics by time of first welfare benefit, men



Notes: Mean characteristics for male refugees by number of weeks from the first week of September 2015.

The amount of welfare benefits received by the treatment and the control group in the first year after arrival is presented in Figure 3. The data on welfare benefits is collected on an annual basis, so it is shown for 2016 for the monthly cohorts arriving in 2015. The amount of welfare benefits drops from around DKK 100,000 (USD 15,000) for men arriving before September 2015 to around DKK 80,000 (USD 13,000) for men arriving in the last four months of 2015, and from DKK 125,000 (USD 19,000) to DKK 100,000 (USD 15,000) for women, i.e. relative changes of 20% for both genders. The higher levels for women may reflect that more women are living with children and that fewer women are working one year after their arrival. The upward trend likely reflects that cohorts to the left in the figure have stayed longer and therefore have had more time to leave welfare benefits.

Figure 3. Welfare benefits in 2016, by arrival month in 2015, for men (left) and women (right)



Notes: The first axis is the first month with welfare benefits and is centered around September 2015 (=0).

Results

Effects on labor market outcomes

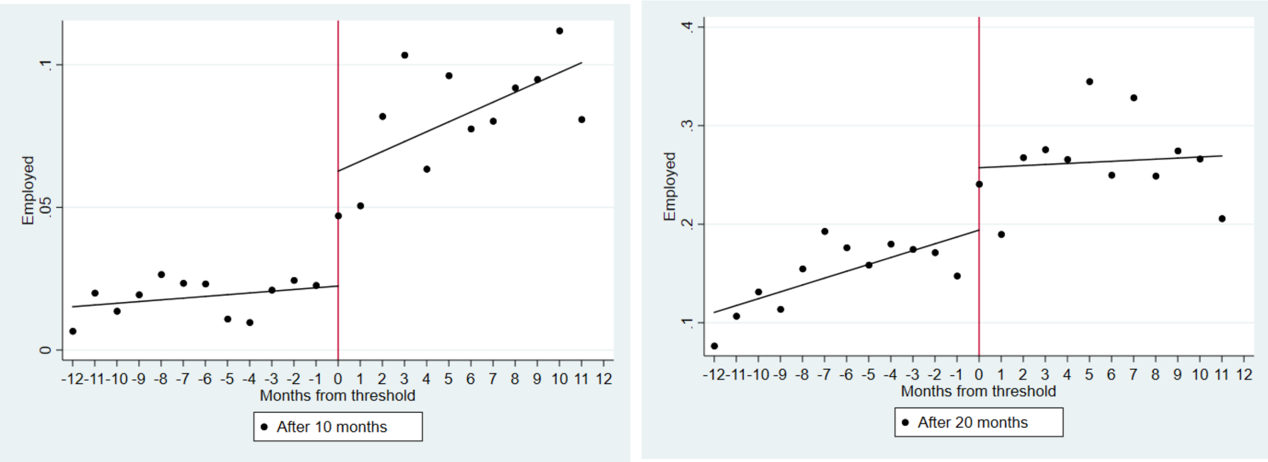
Before presenting the RD estimates, it is informative to inspect the outcomes graphically to visualize an effect. This is done for monthly employment indicators in Figures 4 and 5 for men and women, respectively. They show the mean employment rates 10 and 20 months after the first receipt of welfare benefits by the calendar months of first benefit receipt, where zero corresponds to September 2015. Note the different scales of the second axis on the figures.

Figure 4 shows an increasing time trend for men, indicating that cohorts that arrive later are finding jobs to a higher extent[9]. This might reflect an improving economy as well as an impact of other employment initiatives towards refugees that were implemented in 2016. The effect of the benefit reduction is the difference in the linear trends on each side of the threshold value at zero. The difference is around 5 percentage points, lifting employment rates from a very low level just before

[9] Note that Figures 4 and 5 have fixed the length of time after benefit receipt, in contrast to Figure 3. Therefore, whereas the trend in Figure 3 is picking up months since arrival, the trend in Figure 4 and 5 is picking up calendar time. This may partly explain the opposite direction of the trends in the two figures (they are opposite because the reverse sign is expected when comparing employment and welfare-benefit receipt).

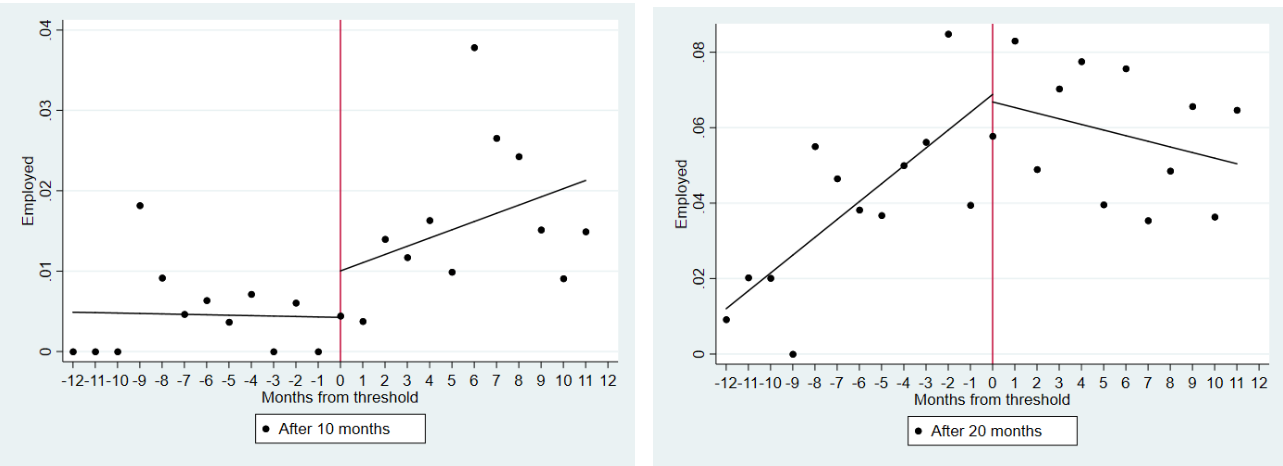
the threshold at 2-3% after 10 months and at 15-20% after 20 months. Figure 5 shows no sign of any effects for women at either 10 or 20 months.

Figure 4. Employment rates after 10 and 20 months since t = 0, men



Notes: The first axis is the first month with welfare benefits and is centered around September 2015 (=0).

Figure 5. Employment rates after 10 and 20 months since t=0, women

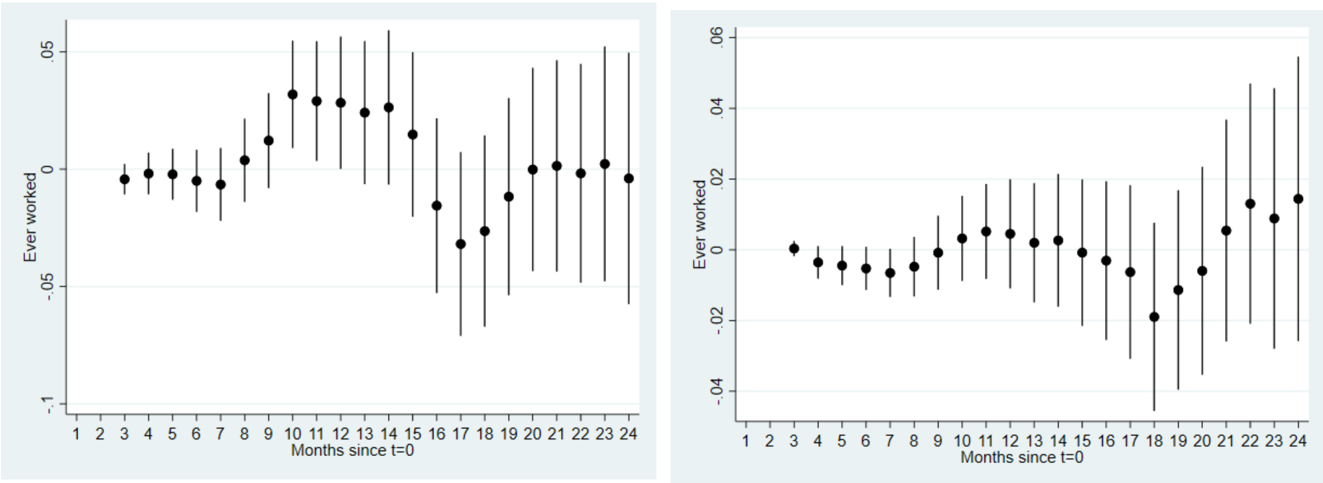


Notes: The first axis is the first month with welfare benefits and is centered around September 2015 (=0).

I collect the accumulated response over time on the extensive margin in Figure 6. It shows the RD estimates at a given point after the first benefit receipt measured by the share of refugees who have worked at some point up until then. The estimates are from a linear specification in the running variable and include controls for observed covariates at arrival.

It is important to distinguish the results up to month 10 from the results at later months, because the early results are based on comparisons where the control group has not yet had its benefits reduced. Figure 6 shows that the effect of the benefit reduction on the share who has worked starts to rise 8 months after t = 0 and that the effect peaks after 10 months (at 3.2 percentage points). This is exactly at the time when the control group also experiences the benefit reduction. Thereafter, the effect fades out and becomes significantly negative after 16 months. This is consistent with an interpretation that once the control group experiences the benefit reduction, they catch up relatively quickly. The right part of the figure confirms the absence of effects for women.

Figure 6. RD estimates of the effects of reduced benefits on the share who has worked, men (left) and women (right)



Notes: Each dot represents an RD estimate from a linear specification. All estimates control for observed characteristics at arrival: age, any children, any children aged 0-2, any children aged 3-6, married, couple, from Syria, from Eritrea, municipality at arrival, and Danish course level. The vertical lines represent 95%-confidence intervals.

I show additional results on other labor market outcomes for men to examine the mechanisms at work in Table 3. There are no significant effects on these outcomes for women, so the results are placed in Appendix Table B.1. The row in Table 3 labelled '1. Hours' shows the effect on the monthly hours of work (not accumulated). The hours of work for the early treated group has increased by 4.9 hours in the 10th month after the reduction of the benefits. The effect becomes significantly negative after 16 months. It therefore mirrors the results found when using the share who has worked as an outcome, indicating that the effects are driven by changes at the extensive margin, as expected. The set of estimates in the row labelled ‘2. Labor income’ shows that the sign of the effects on monthly labor income also mirror those on the employment rate: The group who experienced the benefit reduction from the time of arrival earn DKK 420 (USD 65) ten months after, but the advantage vanishes after 16 months. The last row labelled ‘3. Hourly wage’ shows the effect on hourly wages among those who are employed. It shows a tendency towards a reduction, but it is only the reduction after 10 months which is significant at a 10% level. The baseline hourly wage is close to DKK 150 (USD 23) which is just above the minimum wages determined in the collective agreements. Because employment is affected on the extensive margin, this result can either reflect that the reservation wage drops for those affected by the benefit reduction, or that the benefit reduction induces more persons with the lowest earnings potential to find employment.

Table 3. RD estimates of the labor market effects of reduced benefits, men

	Months since arrival						
	8	10	12	16	20	22	Cumulated
1. Hours	2.365*	4.876***	3.515*	-4.435*	2.932	7.852**	-9.120
	(1.226)	(1.552)	(1.841)	(2.378)	(2.819)	(3.063)	(9.330)
Baseline	0.99	1.14	6.34	0.149	0.170	0.220	
2. Labor income	9.571	419.8***	-77.29	-704.3**	699.6**	517.6	-3155.7
	(106.5)	(142.7)	(194.9)	(273.5)	(349.1)	(390.4)	(2752.8)
Baseline	158.7	189.5	1006.1	2173.4	2407.1	3343.1	
3. Hourly wages	-156.0	-21.32*	-17.77	-41.31	-2.865	3.618	
	(117.7)	(12.71)	(11.30)	(25.27)	(6.333)	(12.83)	
Baseline	149.2	161.0	150.4	147.3	149.8	151.1	

Note: OLS estimates with the weekly running variable entering linearly on each side of the threshold. Standard errors in parentheses. All estimates control for observed characteristics at arrival: age, any children, any children aged 0-2, any children aged 3-6, married, couple, from Syria, from Eritrea, municipality at arrival, and Danish course level. The effects on wages are conditional on employment. The baseline levels are calculated for the cohorts arriving in the three months leading up to September 2015. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The results for employment and labor income up to and including month 16 are robust when using a quadratic RD specification (see Appendix Table B.2). The effect on hours is still positive but insignificant. Table 3 shows a significant positive effect after 20 months, but this is not robust when using a quadratic RD specification. The variations in the effect seen after 16 months are therefore likely to reflect that later effects are estimated with greater uncertainty. The cumulated effects are shown in the final column and they are insignificant.

To be able to examine the joint effect of benefit reduction and labor market responses, I complement these results with effects on the amount of welfare benefits received, total transfer income, and disposable income, which are all measured annually. The results are presented in Table 4 and confirm that both men and women experience a welfare benefit reduction of more than DKK 26,000 (USD 4000) annually in the first calendar year after their arrival.

Table 4. RD estimates of the effects of reduced benefits on annual income (DKK)

	Welfare Benefits	Transfer Income	Disposable Income
Men			
YSA=1	-26,172.9***	-27,560.1***	-20,397.3***
	(2499.5)	(2798.6)	(2646.7)
YSA=2	5174.9*	5731.8*	62.54
	(3119.8)	(3418.6)	(4291.7)
Women			
YSA=1	-30,860.5***	-31,508.7***	-20,676.5***
	(2316.0)	(3069.2)	(2458.7)
YSA=2	2876.5	735.8	151.7
	(3417.6)	(3863.1)	(3262.3)

Note: RD estimates with a linear specification, see Table 3.
Annual outcomes. YSA is years since arrival. Includes arrivals in 2015.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

The larger decrease in welfare benefits for women is likely to reflect a composition effect: More women living with a partner and children, who were affected more by the reduction, and more men below 25 without children, who were affected less. It could also reflect that men respond to the welfare benefit reduction and women do not, so that men are able to counter part of the income reduction. The results in the second column contain the effect on total income transfers. In addition to welfare benefits, total income transfers include, for example, disability pension and sickness benefits. A possible consequence of the benefit reduction could be that refugees would seek to alleviate the benefit reduction by applying for disability pension. The results show that if this is the case, they do not succeed: The effects are almost identical to the effect on welfare benefits. The similarity in the results for welfare benefits and total transfer income also rules out that the refugees drop out entirely from the welfare system. Finally, the results in the third column show that the refugees experience a substantial drop in disposable income in their first year as a result of the welfare benefit reduction: Disposable incomed drops by more than DKK 20,000 (or more than USD 3000), i.e. more than two-thirds the size of the drop in welfare benefits. This is the RD estimate corresponding to Figure 3. Thus the small increase in labor income that we saw in Table 3 far from compensates for the reduction in welfare benefits. Similar results are obtained with a quadratic RD specification (Appendix Table B.3).

The effect on welfare benefits is positive for men after two years in the country (*YSA=2*) and there is no effect on disposable income for either men or women. The results confirm that the difference between those who are treated from their arrival and those who are treated ten months later levels out in the second year after arrival when both groups are treated. The positive effect is consistent with a scenario where the control group responds more quickly to the delayed benefit reduction, once it occurs.

Robustness

I examine the robustness of the results for labor market outcomes in several different ways. The results are shown in Appendix B. I show that the peak effect on employment after ten months is also obtained when the sample is narrowed month by month to refugees arriving six months before and after the threshold (Appendix Table B.4). I also show that the estimates are not sensitive to the exclusion of the benefit receipt in the week before September (Appendix Table B.5). I conduct several placebo tests in which I estimate RD effects at artificial thresholds within the control or treatment groups (Appendix Figure B.1 and B.2). They show that 80 of the 85 placebo estimates are lower than the actual estimate for men, but are all insignificant. A larger number of placebo estimates are higher than the actual estimate for women, and some are significant, but the effects are still small. Finally, I examine whether the results are sensitive to the inclusion of control variables. The estimates are of the same sign and significance without controls and are extremely stable across specifications with different sets of covariates, except when it comes to country of origin: The effect is reduced by 17% when country of origin is included as a control variable (Appendix Table B.6). I examine the results by different countries of origin in the next section. Overall, these robustness tests show that the early labor market impact of the immediate benefit reduction seems robust.

Heterogeneity in employment effects

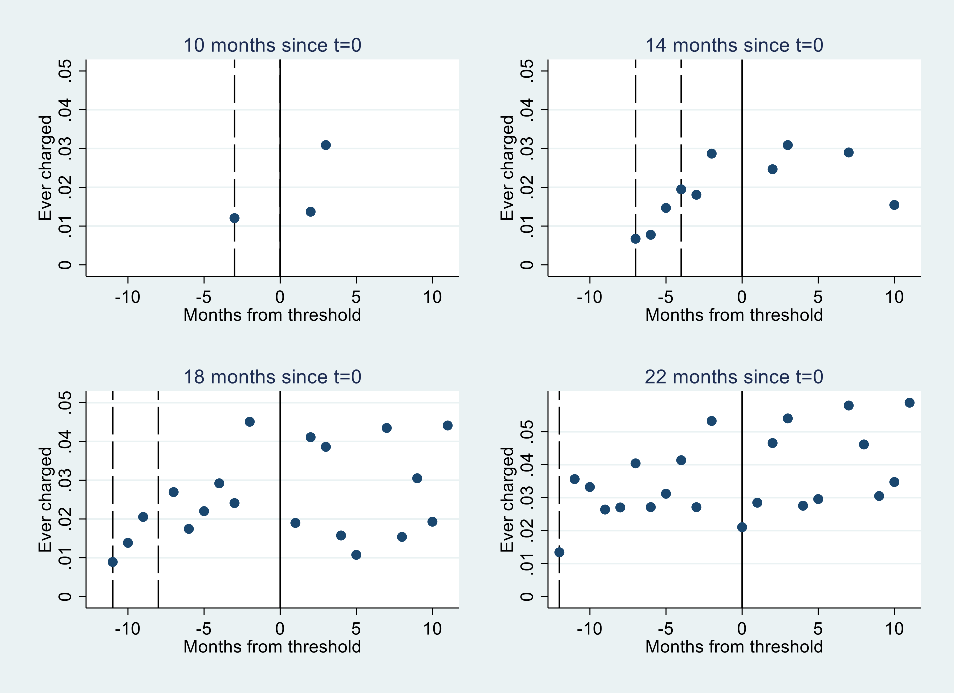
It was documented in Table 1 that the reduction of social assistance differs across different population groups. I therefore examine whether the effect of the reduced benefits vary across subgroups with given characteristics. These characteristics are age, cohabitation status, having children younger than 6, country of origin, and the level of the language course in Danish. As mentioned above, all refugees are assigned to a language course based on their education level. These heterogeneity tests also serve the purpose of testing whether effects differ systematically across groups with different characteristics that showed signs of discontinuities at the threshold. The results are shown in Appendix Table B.7.

I focus on the results after 10 and 20 months. The effects after 10 months are not significantly different from each other in any of the groups on a 5% significance level. The differences are, however, more similar across age groups and country of origin, and are larger across cohabitation status (where it is significant on a 10% level), by having children or not and by educational background (proxied by the Danish course level). The robust results across country of origin once again indicate that discontinuities in characteristics do not seem to drive the results. The larger effect for men living in couples than for single men and for men with children could be the result of a larger welfare-benefit reduction for these groups (cf. Table 1). It could also reflect that they are more resourceful, which is also likely to be the explanation for the larger effect for men with a higher level of education. For women, there are no significant differences either, and all the effects are small. When we look at the effects after twenty months, i.e. when the control group has also experienced a benefit reduction, the effects diverge more, but are still not significantly different within groups.

Effects on crime

Having confirmed that the benefit reductions have the intended labor market effects, I examine whether they have unintended effects on crime. Criminal charges and particularly convictions are relatively rare: 4% of the men in the control group had been charged with a crime after two years in the country and less than 1% were convicted, partly reflecting that it takes time to reach a conviction for some types of crime charges. For women in the control group only 0.8% had been charged with a crime and only 0.5% were convicted after two years. The greater similarity between charges and convictions for women than for men probably reflects that theft constitutes a much large share of the charges for women, and the time needed to reach a conviction, once charged, is much lower for this type of crime than for most other types of crime. Given that the share of crimes is so low, I only present the effects on whether the individual has ever been charged with a crime. Figure 7 presents the share of men who had been charged with a crime after 10, 14, 18 and 22 months since t=0 (the first week with benefits).

Figure 7. Criminal charges at 10, 14, 18 and 22 months since t = 0, men

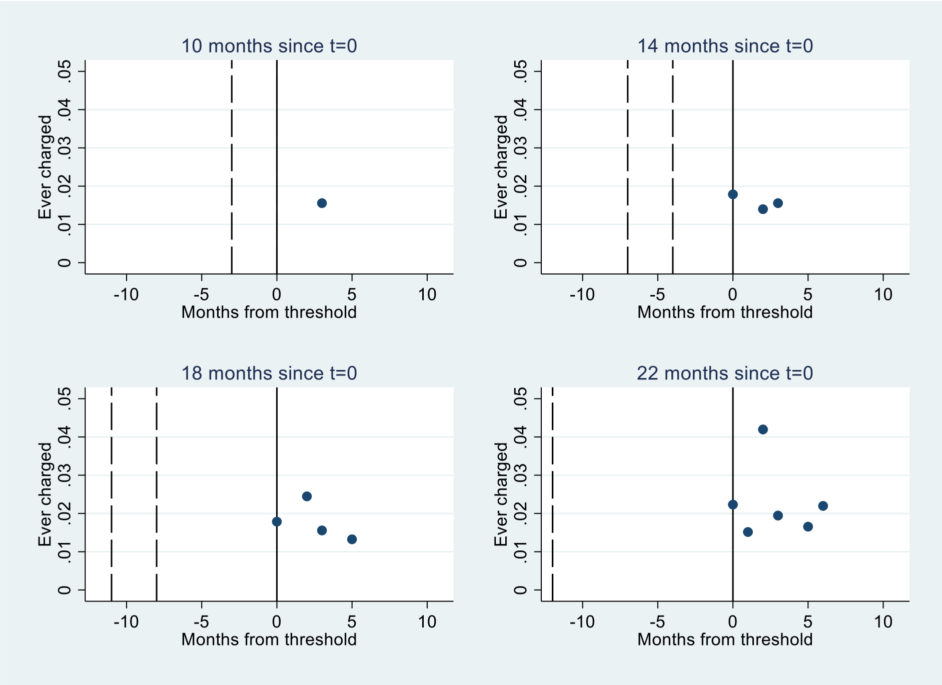


Notes: The first axis defines the arrival cohorts by first month with welfare benefits. It is centered around September 2015 (t=0). The first dashed line is the time when the amendment to the law that reduced benefits in the control group was passed and second dashed line is the time when the amendment is effective. Only means based on more than 3 persons are shown.

I only show shares based on more than three persons[10]. The upper left part of Figure 7 shows that this is only the case for three monthly cohorts within 10 months. The remaining parts of the figure show that criminal charges increase when looking at outcomes after longer periods of time in the country (i.e. at 14, 18 and 22 months since arrival). None of the four parts of Figure 7 show any clear signs of a discontinuity around the threshold.

[10] I am required to do so by Statistics Denmark for confidentiality reasons when it comes to sensitive data such as crimes. But by doing so, I also avoid that months with few persons disturb the picture.

Figure 8. Criminal charges at 10, 14, 18 and 22 months since t = 0, women



Notes: The first axis is the first month with welfare benefits and is centered around September 2015 (t=0). The first dashed line is the time when the amendment to the law that reduced benefits in the control group was passed and second dashed line is the time when the amendment is effective. Only means based on more than 3 persons are shown.

Figure 7 reveals another interesting feature, however, namely that the passing and enactment of the amendment to the law may have had an effect on the control group. This is illustrated by the presence of the vertical dashed lines: In the figure denoted t = 14, four monthly cohorts in the control group have experienced the benefit reduction in July 2016 (those arriving in the four months before September 2015). This is shown by the second dashed line at -4 on the first axis. Likewise, the first dashed line at -7 indicates that seven cohorts in the control group have experienced that the law amendment was passed by Parliament in March 2016. It is seen that the criminal charge rates increase in the control group, once these points in time are passed.

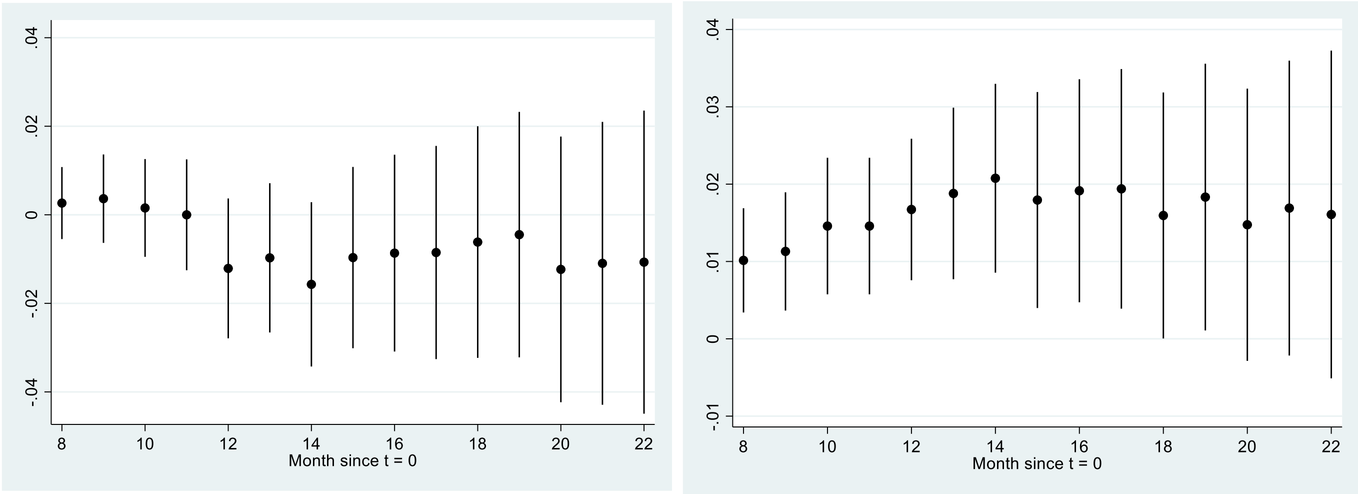
Figure 8 shows similar results for women. These results show that there is a much lower number of months with more than three persons charged, and they are all in the group that is treated from time of arrival (i.e. to the right of the threshold at 0).

Figure 9 presents the RD estimates on total criminal charges for men and women. I have used a quadratic specification to capture the curvature to the left of the threshold in Figure 7. There is a very small positive effect at month 9 for men, which is significant at a 10% level. The estimates become negative after 12 months but are insignificant at a 5% level. For women, the estimates are positive and significant, but they drop slightly after 15 months and become insignificant after 20 months. The results are robust across specifications for women, but not for men (Appendix Tables B.8 and B.9): The negative effects vanish when using a linear specification for men. Overall, the results for men therefore mirror those on labor market outcomes (with inverted sign), but I am hesitant to emphasize them too much, due to the sensitivity to specification and lack of clear effects in the graphical presentations. Similar results are obtained when I look at charges for theft or violence for men (Appendix Table B.8). For women, the results are entirely driven by thefts (Appendix Table B.9), as also found in Andersen et al. (2019)[11].

To further examine the crime response for men, I attempt to take into account that the control group seems to react to the passing and enactment of the law amendment, as visualized in Figure 7. To do this, I use the RD estimator with a rolling threshold in the following way: When outcomes are measured, for instance at 14 months, there are four monthly cohorts in the control group who have been treated. The threshold is therefore set to -4, instead of 0, so outcomes between those who are treated are compared with the rest of the control group, who have not yet been treated. In another set of estimations I included the part of the control group who know they will be treated, when the law amendment has been passed. Both sets of estimates are larger than the effects shown in Figure 9, and look more similar to those found for women, but most are still not significant at conventional levels (Appendix Figure B.3).

[11] Similar results are also found when looking at crime convictions, but the actual numbers behind are so small, that I do not want to emphasize them and they are therefore not shown.

Figure 9. RD estimates of the effects of reduced benefits on criminal charges, men (left) and women (right)

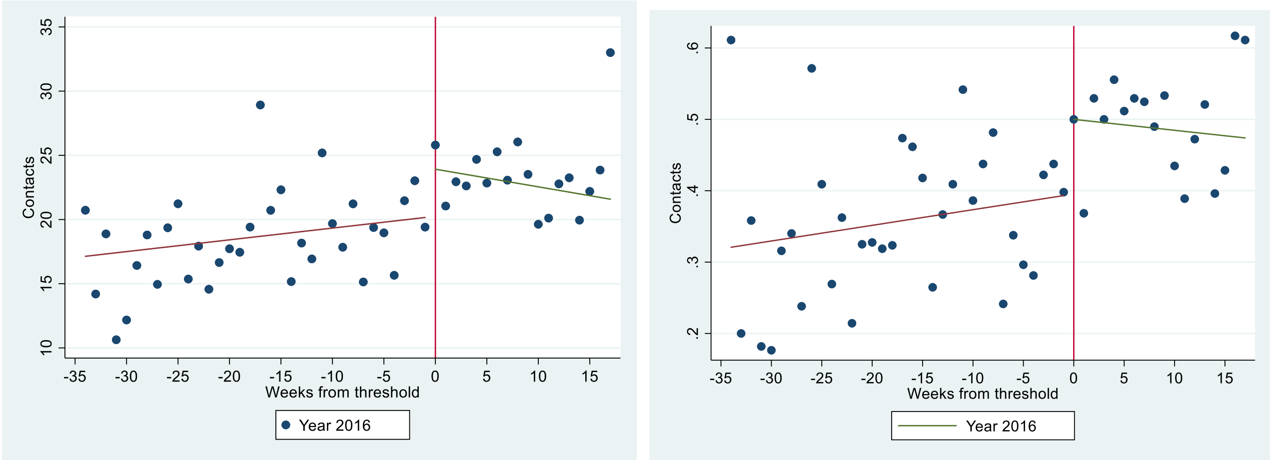


Notes: RD estimates from a quadratic specification, controlling for covariates. The vertical lines represent 95%-confidence intervals.

Effects on primary health care utilization

This final empirical section describes the results with regard to use of primary health care. Provided that health care is free, an increase in the use of specialized health care can be a proxy for health deteriorations. As described above, publicly financed specialized care requires a referral from a general practitioner. The use of general practice is therefore likely also to reflect a demand for referrals, for instance with the aim to prove qualification for disability pension.

Figure 10. Annual number of general practice contacts and share with more than 20 visits in 2016 by time of first welfare benefit, women



Notes: The first axis is the first week with welfare benefits and is centered around the first week of September 2015 (=0). Only shown for refugees arrived in 2015.

The number of health care contacts is measured annually, so I consider health-care utilization in 2016 and 2017 for immigrants who arrived in 2015. The left part of Figure 10 presents the mean number of contacts to a general practitioner by week of first benefit receipt for women. On average, women have 18-25 annual contacts (including e.g. email consultations and prescription renewals) with their general practitioner in the first year. The right part of the figure shows more than a third have more than 20 annual contacts. The two figures show a tendency toward an increase in contacts driven by an increase in the share with many contacts. A similar figure for men is found in Appendix Figure B.4, which shows no sign of any effect. It is noted that the number of health contacts is around 50% higher than the national average in Denmark for both men and women[12].

The RD estimates on the effect on annual contacts with different health-care providers are presented for women in Table 5. The number of annual contacts to mental or physical specialist caregivers are less than one visit, and the total number of contacts varies from 20-35 in the first year. The table shows that the total number of contacts increases in the first year, and more than 80% of the increase is driven by an increase in contacts to a general practitioner. There is no significant change in the use of mental or physical health-care providers, nor in the use of other types of health-care providers. The estimates for men are presented in Appendix Table B.10 and confirm the absence of an effect for men.

[12] The average number of contacts for persons aged 30-59 in Denmark was 7.3 for men and 12.4 for women (www.statistikbanken.dk, table SYGFAM). Of these contacts, men had 4.7 contacts with a general practitioner and women had 8.2.

Table 5. RD effects on primary health-care utilization (annual number of contacts), women

Year of use	General practice	Mental health[a]	Physical health[b]	Other care	Total contacts	General practice > 20
2016	2.560**	-0.0127	-0.0552	0.540	3.101*	0.0849**
	(1.171)	(0.122)	(0.262)	(0.915)	(1.586)	(0.0386)
2017	1.179	0.149	0.287	0.143	1.322	0.00585
	(1.246)	(0.151)	(0.415)	(1.012)	(1.737)	(0.0391)

Notes: OLS estimates with quadratic specification, see also Table 3. The sample includes those who receive welfare benefits for the first time in 2015. [a] Covers psychological or psychiatric treatment. [b] Covers physiotherapy or chiropractic care.
** $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Discussion

The current study has shown that marginalized groups of unemployed, newly arrived refugees respond quickly to a large reduction in the welfare benefits they are entitled to. I used two policy experiments to double-check the short-term responses. However, whereas male refugees respond in terms of their labor market behavior, female refugees respond on other margins. The benefit reduction more than doubles the share of males who are employed 10 months after their arrival; however, it must be considered that the starting point was a very low level. When cohorts who had arrived earlier also have their benefits reduced 10 months after their arrival, they quickly catch up with those who were treated from the time of their arrival. There are no significant differences in the share who are employed or in the number of accumulated work hours and labor income after two years in the country between the group who experience the immediate reduction and those who experience it after 10 months.

The men who find employment faster because of the reduction in welfare benefits, do so at a lower hourly wage, so the total labor income response is small and far from compensates the income reduction. This increases the risk of unintended effects, and I do find a tendency towards a response in terms of criminal charges that mirrors the male labor market effects: A small increase in criminal charges for those who experience the benefit reduction from arrival, followed by a fade-out once the control group is treated. This pattern is, however, insignificant at conventional levels. There are no significant differences in health-care utilization between the group of men who experience the benefit reduction from the time of arrival and the group who experience it after 10 months. Since the outcomes are mainly measured after both groups have experienced the benefit reduction, I cannot rule out that the benefit reduction affects these outcomes when compared to a longer period of higher welfare benefits.

The results for women are almost the opposite of those for men: There is no labor market response to the benefit reduction in the first year after arrival, nor once the control group experiences the benefit reduction after ten months. The women respond on other margins: Women who experience a benefit reduction from the time of their arrival increase their health-care utilization and commit more crimes in the first year after arrival. The effect on crime rates is driven by increased theft, and just as expected, it levels out and becomes insignificant after 1½ years when the control group is also treated. The increase in health-care utilization is driven by an increase in the share with many contacts with a general practitioner. In particular, I see no changes in the use of psychologists or psychiatrists or with other types of health-care providers. At first sight, these results therefore show no signs of worsened mental health problems. The increased use of general practice for women may, however, have different explanations. It may indicate an increase in mild conditions that can be treated by the general practitioner and use of medication. It could also indicate the presence of more severe undiagnosed conditions. Finally, it may be that the benefit reduction induces more women to seek health care, for example, to qualify for disability benefits[13].

[13] There are special rules for disability benefits for refugees, and even though it is the municipality that decides on disability benefits, the refugee can apply for a case being initiated and the municipality is obliged to collect medical certificates from among others the refugee's general practitioner. Source: https://www.sundhed.dk/sundhedsfaglig/laegehaandbogen/socialmedicin/sociale-ydelser/foertidspension/

Another take-away from the current study is that while there is a clear response on different margins of refugees who experience a benefit reduction, I see no clear advantage or disadvantage after two years in the country between those who experience the benefit reduction from arrival and those who experience it after 10 months. There is a tendency towards a faster response to the economic incentive from the latter group, which is to be expected because they have had more time in the country that enables them to respond to the economic incentive.

The labor market and crime effects are overall in concordance with studies that examine an earlier benefit reduction in Denmark (Huynh et al. 2007; Rosholm and Vejlin 2010; Andersen et al. 2012; Andersen et al. 2019). The effects are smaller in absolute size than the results in previous studies, but this is to be expected for several reasons. The benefit reduction was smaller in 2015 than it was in 2002. The refugees in 2015 experienced a drop in the level of welfare benefits by 20% in the first year after arrival compared to nearly 50% in 2002 (Andersen et al. 2019). The smaller effects on crime, particularly for men, could also arise because the level of crime has fallen in general. Thus, looking at the share of immigrant males from non-Western countries aged 15-29 who were convicted in a given year, this number has fallen from 9-12% in 2002-04 to 5-6% in 2015-6[14]. And finally, it is also obvious that both the announcement of the later reduction and the reduction itself mitigates the effects in 2016 and onwards.

The labor market effects for men are at odds with findings for refugees in the US, where a higher benefit level did not impact employment but raised hourly wages (LoPalo 2019). This is likely because the generosity and type of support at the outset is very different in the US compared with Denmark. Moreover, the labor market response in the Danish case far from compensates the benefit reduction, which increase the risk for unintended effects. This stands in stark contrast to the broader

[14] www.statistikbanken.dk, TABLE STRAFNA9, FOLK2.

literature on welfare reform in the US, where welfare reform cutbacks increased labor supply without increasing poverty (Blank 2002; Grogger and Karoly 2005; Borjas 2016).

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Appendix A: Validation of the RD design

Table A.1. RD estimates for discontinuities at the threshold in observed characteristics, men

	Linear	s.e.	Quadratic	s.e.
Any child	-0.0108	(0.0110)	0.00114	(0.0187)
Child aged 0-2	0.000151	(0.0143)	-0.0406***	(0.0242)
Child aged 3-6	-0.0160	(0.0166)	0.0349	(0.0281)
Age	-0.161	(0.391)	-0.226	(0.663)
Couple	-0.0351*	(0.0112)	0.00621	(0.0190)
Married	-0.00443	(0.0201)	0.0462	(0.0339)
Syria	0.0941*	(0.0199)	0.242*	(0.0337)
Eritrea	-0.181*	(0.0171)	-0.215*	(0.0291)
Other country	0.0870*	(0.0131)	-0.0272	(0.0222)
Danish course level 1	0.0454***	(0.0232)	0.0151	(0.0395)
Danish course level 2	-0.0566**	(0.0243)	-0.0136	(0.0414)
Danish course level 3	0.00340	(0.0138)	-0.00428	(0.0235)
N	7400		7400	

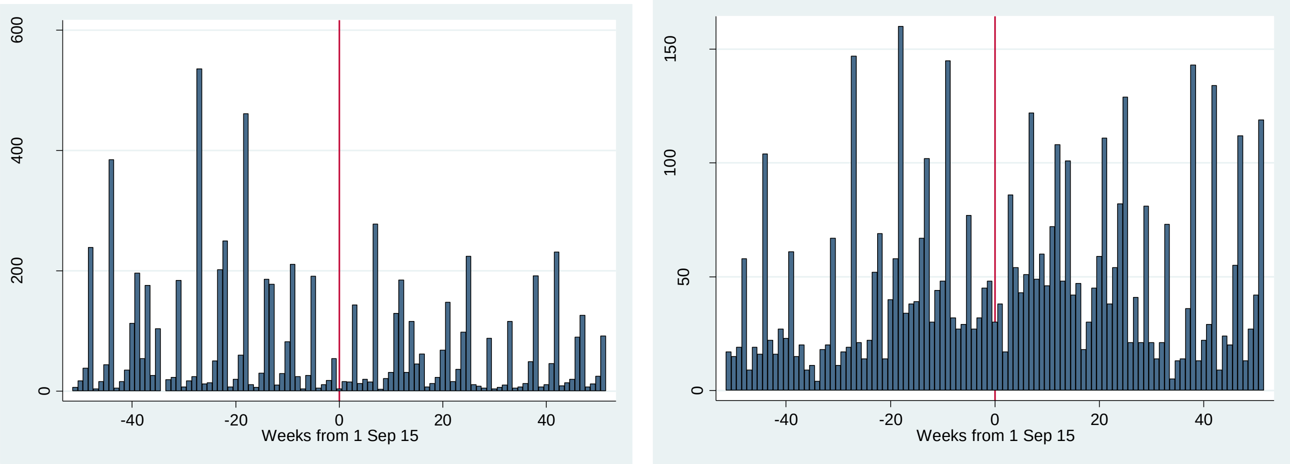
Note: See Table 3 for explanation of the linear and quadratic model.
Other covariates except the outcome are included.
* *p* < 0.1, ** *p* < 0.05, *** *p* < 0.01.

Table A.2. RD estimates for discontinuities at the threshold in observed characteristics, women

	Linear	s.e.	Quadratic	s.e.
Any child	0.00843	(0.0220)	0.00329	(0.0347)
Child aged 0-2	0.0333	(0.0270)	0.000984	(0.0427)
Child aged 3-6	0.0238	(0.0342)	0.147*	(0.0541)
Age	-0.303	(0.442)	0.0739	(0.700)
Couple	-0.000385	(0.0185)	-0.00675	(0.0294)
Married	-0.0134	(0.0207)	-0.0410	(0.0329)
Syria	0.00222	(0.0205)	0.0360	(0.0326)
Eritrea	-0.0900*	(0.0146)	-0.0419***	(0.0233)
Other country	0.0877*	(0.0172)	0.00590	(0.0274)
Danish course level 1	0.00317	(0.0266)	-0.0285	(0.0423)
Danish course level 2	0.0200	(0.0274)	0.0336	(0.0436)
Danish course level 3	-0.0190	(0.0146)	0.00112	(0.0232)
N	4781		4781	

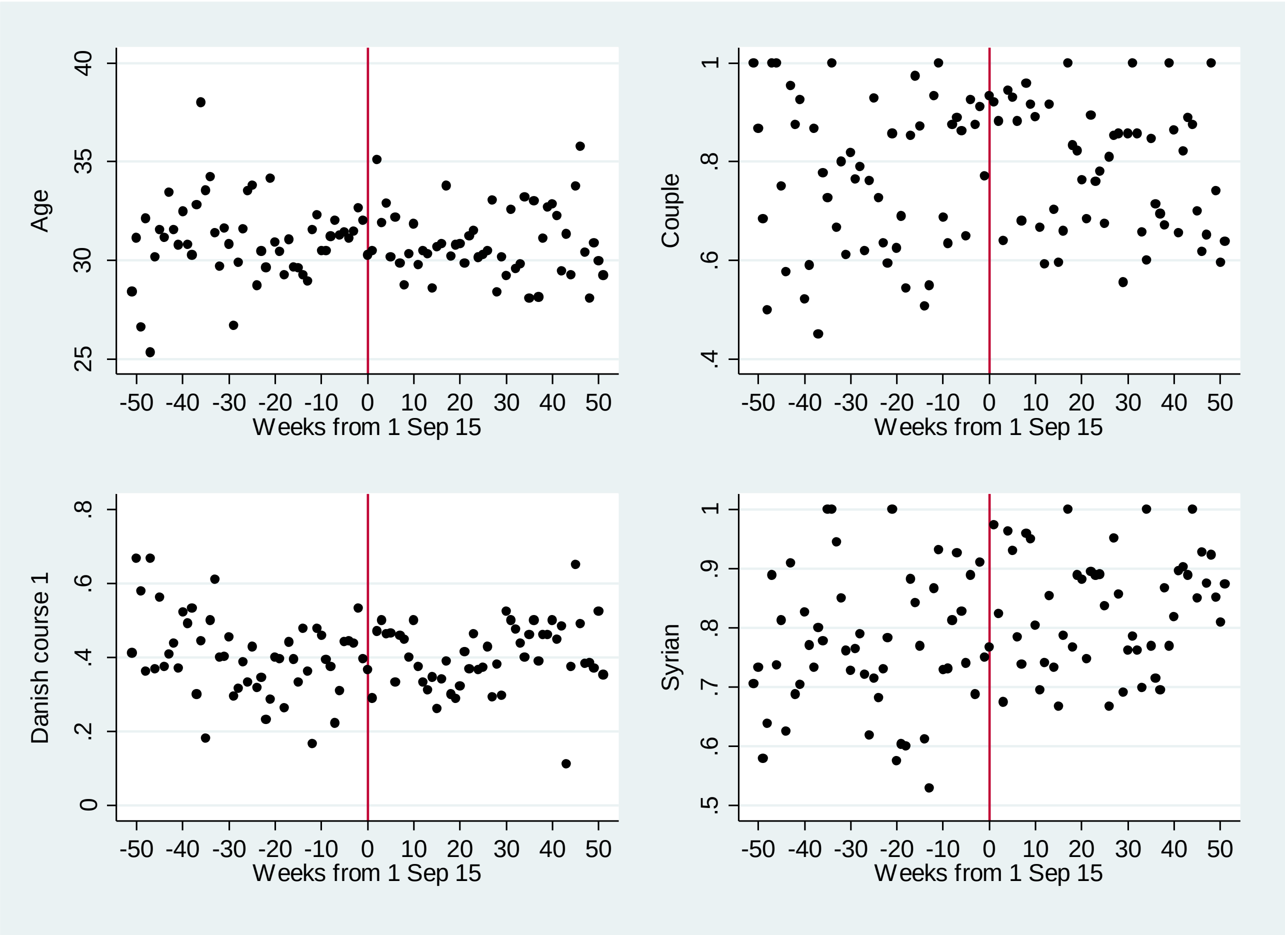
Note: See Table 3 for explanation of the linear and quadratic model.
Other covariates except the outcome are included.
* *$p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.*

Figure A.1. Number of weekly arrivals, for men (left) and women (right)



Notes: The running variable is week of arrival, where 0 is the first week of September 2015.

Figure A.2. Variation in average characteristics by the time of first welfare benefit, women



Notes: Mean characteristics for refugees with the same first week of benefit receipt, by number of weeks from the first week of September 2015.

Appendix B: Robustness tests and additional results

Table B.1. RD estimates on labor market outcomes, women.

	Months since t = 0				
	10	12	16	20	22
1. Employment	0.00754	-0.000382	-0.00727	0.00652	0.0184
	(0.00558)	(0.00675)	(0.00966)	(0.0124)	(0.0142)
Baseline	0.002	0.02	0.04	0.06	0.065069
2. Labor income	55.00	-96.36	-143.9	161.6	102.1
	(69.38)	(81.65)	(113.5)	(152.8)	(189.7)
Baseline	19.6	165.4	461.1	601.6	710.1
3. Hourly wages	NA	NA	-12.85	-37.17**	-35.26
			(25.64)	(14.50)	(27.47)
Baseline			144.5	139.4	134.3261

Note: See Table 3. * *p < 0.1*, ** *p < 0.05*, *** *p < 0.01*.

Table B.2 Employment effects with a quadratic RD specification, men

	Months since arrival						
	8	10	12	16	20	22	Cumulated
1. Employment	0.0164	0.0301*	0.025	-0.0594**	0.019	0.0198	0.0019
	(0.0129)	(0.0171)	(0.0209)	(0.0277)	(0.0330)	(0.0365)	(0.0413)
2. Hours	2.586	3.707	4.199	-9.695**	-4.138	4.918	-1.444
	(2.075)	(2.627)	(3.115)	(4.025)	(4.758)	(5.224)	(14.48)
3. Labor income	-31.29	416.4*	-209.8	-1345.6***	-308.3	32.70	-8192.3*
	(180.2)	(241.5)	(329.8)	(462.9)	(589.6)	(666.0)	(4561.1)
4. Hourly wages	-72.89	-48.16**	-23.92	-19.42	-11.21	-23.00	
	(177.4)	(20.27)	(17.98)	(41.77)	(10.66)	(21.37)	

Note: See Table 3. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.3. RD estimates of effects of reduced benefits on annual income (DKK), quadratic specification

	Welfare Benefits	Transfer Income	Disposable Income
Men			
YSA=1	-26742.7***	-25122.3***	-13001.9***
	(4382.9)	(4906.1)	(4639.6)
YSA=2	1684.5	5232.2	13403.4*
	(5431.9)	(5951.6)	(7466.8)
Women			
YSA=1	-31486.2***	-29680.5***	-19588.2***
	(3602.5)	(4775.1)	(3825.5)
YSA=2	383.3	301.7	-1160.6
	(5311.9)	(6005.0)	(5071.2)

Note: See Table 3 for an explanation of the quadratic model.
Transfer income includes welfare benefits and all other public income transfers.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.4. Estimates of employment effects after 10 months, with different sized windows

	Months on each side of the threshold						
	6	7	8	9	10	11	12
Men	0.0300*	0.0377***	0.0380***	0.0351***	0.0307***	0.0333***	0.0324***
	(0.0153)	(0.0137)	(0.0129)	(0.0119)	(0.0112)	(0.0105)	(0.0101)
N	4016	4749	5236	5891	6515	7100	7400
Women							
	0.00183	0.00255	0.00492	0.00803	0.00851	0.00891	0.00806
	(0.00753)	(0.00706)	(0.00687)	(0.00668)	(0.00608)	(0.00573)	(0.00555)
N	3058	3386	3701	3954	4323	4672	4781

Note: See Table 3 for an explanation of the linear model. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.5. Employment effects, without donut sample

	Months since t = 0					
	8	10	12	16	20	24
Men	0.0102	0.0318***	0.0154	-0.0353**	0.0463**	-0.00906
	(0.00750)	(0.00988)	(0.0121)	(0.0161)	(0.0191)	(0.0245)
N	7545	7545	7545	7545	7545	6596
Women	-0.000167	0.00592	-0.00107	-0.00712	0.00463	0.00671
	(0.00374)	(0.00544)	(0.00655)	(0.00934)	(0.0121)	(0.0161)
N	4915	4915	4915	4915	4915	4083

Note: Linear specification, see Table 3 for an explanation. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.6. Sensitivity to inclusion of covariates, employment indicator.

Months since t = 0	Model (1)	Model (2)	Model (3)	Model (4)	Model (5)	Model (6)	Model (7)	Model (8)
Men								
8	0.0209***	0.0208***	0.0218***	0.0202***	0.0200***	0.0128*	0.0133*	0.0138*
	(0.00758)	(0.00761)	(0.00758)	(0.00759)	(0.00760)	(0.00764)	(0.00764)	(0.00765)
10	0.0418***	0.0426***	0.0437***	0.0418***	0.0414***	0.0347***	0.0354***	0.0362***
	(0.0100)	(0.0100)	(0.0100)	(0.0100)	(0.0100)	(0.0101)	(0.0101)	(0.0101)
12	0.0327***	0.0331***	0.0323***	0.0302**	0.0296**	0.0191	0.0200	0.0204*
	(0.0122)	(0.0122)	(0.0122)	(0.0123)	(0.0123)	(0.0123)	(0.0123)	(0.0124)
14	0.0304**	0.0312**	0.0297**	0.0274*	0.0266*	0.0157	0.0168	0.0176
	(0.0141)	(0.0141)	(0.0141)	(0.0141)	(0.0142)	(0.0142)	(0.0142)	(0.0142)
16	-0.0207	-0.0200	-0.0195	-0.0208	-0.0213	-0.0315*	-0.0302*	-0.0286*
	(0.0162)	(0.0162)	(0.0162)	(0.0163)	(0.0163)	(0.0164)	(0.0164)	(0.0164)
18	-0.00388	-0.00229	-0.00193	-0.00305	-0.00233	-0.0116	-0.00985	-0.00811
	(0.0180)	(0.0179)	(0.0179)	(0.0179)	(0.0180)	(0.0181)	(0.0181)	(0.0181)
20	0.0469**	0.0475**	0.0468**	0.0468**	0.0488**	0.0401**	0.0426**	0.0449**
	(0.0195)	(0.0194)	(0.0193)	(0.0194)	(0.0194)	(0.0196)	(0.0195)	(0.0195)
22	0.0334	0.0347	0.0301	0.0311	0.0329	0.0212	0.0238	0.0266
	(0.0214)	(0.0213)	(0.0212)	(0.0212)	(0.0213)	(0.0215)	(0.0214)	(0.0214)
Women								
8	0.000717	0.000754	0.00147	0.00180	0.00181	0.00170	0.00215	0.00200
	(0.00366)	(0.00369)	(0.00372)	(0.00372)	(0.00373)	(0.00375)	(0.00374)	(0.00374)
10	0.00469	0.00499	0.00641	0.00709	0.00706	0.00706	0.00754	0.00751
	(0.00545)	(0.00549)	(0.00556)	(0.00555)	(0.00555)	(0.00558)	(0.00558)	(0.00558)
12	-0.00279	-0.00285	-0.00198	-0.00126	-0.00125	-0.000993	-0.000382	-0.000367
	(0.00659)	(0.00664)	(0.00673)	(0.00671)	(0.00672)	(0.00675)	(0.00675)	(0.00676)
14	-0.00150	-0.00216	-0.000285	0.000558	0.000685	0.000238	0.000559	0.000198
	(0.00775)	(0.00780)	(0.00791)	(0.00789)	(0.00790)	(0.00794)	(0.00794)	(0.00794)
16	-0.00966	-0.0112	-0.00982	-0.00864	-0.00895	-0.00828	-0.00727	-0.00756
	(0.00948)	(0.00953)	(0.00964)	(0.00961)	(0.00962)	(0.00967)	(0.00966)	(0.00966)
18	-0.0238**	-0.0251**	-0.0238**	-0.0225**	-0.0217*	-0.0214*	-0.0200*	-0.0203*
	(0.0112)	(0.0112)	(0.0113)	(0.0113)	(0.0113)	(0.0114)	(0.0114)	(0.0114)
20	0.00309	0.00226	0.00402	0.00520	0.00487	0.00498	0.00652	0.00617
	(0.0123)	(0.0124)	(0.0124)	(0.0124)	(0.0124)	(0.0124)	(0.0124)	(0.0124)
22	0.0163	0.0135	0.0156	0.0161	0.0157	0.0167	0.0184	0.0181
	(0.0140)	(0.0141)	(0.0141)	(0.0141)	(0.0141)	(0.0142)	(0.0142)	(0.0142)

Note: Linear specification with different sets of covariates, see Table 3. The following covariates are included in model (1): No covariates, (2): Age, (3): adds municipality, (4): adds children, couple and marital status, (5): adds refugee status, (6): adds country of origin: Syria, Eritrea or other country, (7) adds Danish course level, (8): Replace dummies for Syria and Eritrea with top ten list of country of origin. (7) is the results shown in Table 3. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.7. Heterogeneity in employment effects

MSA	age<25	25<age<30	age>30	Small children	Older children	No children
Men						
8	0.0135	-0.00157	0.0226**	0.0184	0.00959	0.0138
	(0.0163)	(0.0170)	(0.00949)	(0.0192)	(0.0134)	(0.00886)
10	0.0470**	0.0277	0.0343**	0.0596*	0.0761***	0.0313***
	(0.0210)	(0.0219)	(0.0134)	(0.0309)	(0.0252)	(0.0113)
16	-0.0113	-0.0556	-0.0175	0.0140	0.0504	-0.0417**
	(0.0327)	(0.0363)	(0.0224)	(0.0552)	(0.0485)	(0.0181)
20	0.0485	0.0639	0.0319	-0.0153	-0.0199	0.0489**
	(0.0384)	(0.0430)	(0.0270)	(0.0663)	(0.0575)	(0.0215)
24	0.0237	-0.0247	-0.00700	0.117*	-0.0400	-0.00418
	(0.0477)	(0.0540)	(0.0362)	(0.0703)	(0.0752)	(0.0276)
N	1827	1559	3063	998	932	5268
Women						
8	0.00519	-0.000331	0.000134	0.00274	-0.000273	0.00369
	(0.00766)	(0.00908)	(0.00451)	(0.00355)	(0.00297)	(0.00994)
10	0.00728	0.0175	0.00195	0.00448	0.00252	0.0192
	(0.0106)	(0.0148)	(0.00683)	(0.00481)	(0.00598)	(0.0141)
16	0.00257	-0.00631	-0.0100	0.00683	0.00521	-0.0225
	(0.0207)	(0.0196)	(0.0138)	(0.0111)	(0.0119)	(0.0217)
20	-0.00545	0.0158	0.00283	0.0199	0.0310*	-0.0166
	(0.0239)	(0.0255)	(0.0183)	(0.0155)	(0.0159)	(0.0255)
24	0.0275	0.0168	0.0184	0.0303	0.0285	0.00771
	(0.0321)	(0.0344)	(0.0245)	(0.0184)	(0.0216)	(0.0333)
N	1273	1212	2296	2473	2616	1554

(Continues)

Table B.7 (Continued). Heterogeneity in employment effects.

MSA	single	couple	Not Syrian	Syrian	Danish 1	Danish 2	Danish 3
Men							
8	0.0106	0.0263*	0.0201	0.0143	0.0247**	0.00777	-0.0100
	(0.00900)	(0.0138)	(0.0129)	(0.00983)	(0.0118)	(0.0103)	(0.0399)
10	0.0284**	0.0641***	0.0405**	0.0311**	0.0315**	0.0328**	0.0553
	(0.0115)	(0.0219)	(0.0181)	(0.0127)	(0.0160)	(0.0135)	(0.0495)
16	-0.0428**	0.0159	-0.0940***	0.000849	0.0175	-0.0619***	-0.0688
	(0.0183)	(0.0396)	(0.0306)	(0.0203)	(0.0270)	(0.0222)	(0.0732)
20	0.0543**	-0.0326	0.00113	0.0582**	0.0706**	0.0323	-0.0408
	(0.0218)	(0.0474)	(0.0364)	(0.0242)	(0.0316)	(0.0271)	(0.0820)
24	0.000516	-0.0863	-0.0653	0.0263	-0.0182	-0.0101	-0.0758
	(0.0279)	(0.0626)	(0.0442)	(0.0318)	(0.0408)	(0.0353)	(0.101)
N	5134	1315	1864	4585	2258	3546	571
Women							
8	0.00495	0.000715	-0.0100	0.00340	-0.00116	0.00336	0.0485
	(0.0115)	(0.00334)	(0.0119)	(0.00379)	(0.00471)	(0.00475)	(0.0314)
10	0.0273	0.000627	0.0224	0.000725	-0.00264	0.0155*	0.0234
	(0.0167)	(0.00514)	(0.0185)	(0.00546)	(0.00647)	(0.00862)	(0.0384)
16	-0.0108	-0.00507	-0.0382	-0.00223	-0.00357	-0.00347	-0.00411
	(0.0244)	(0.0101)	(0.0276)	(0.0102)	(0.0131)	(0.0147)	(0.0548)
20	0.00726	0.00500	-0.000399	0.00458	0.0121	0.00722	0.0228
	(0.0289)	(0.0136)	(0.0340)	(0.0133)	(0.0178)	(0.0186)	(0.0665)
24	0.0581	0.00785	0.0269	0.00885	0.00924	0.00956	0.0847
	(0.0374)	(0.0186)	(0.0422)	(0.0181)	(0.0239)	(0.0251)	(0.0879)
N	1327	3454	1045	3736	1891	2400	367

Note: Linear specification, see Table 3. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.8. RD effects on criminal charges, men

Months after t=0:	All	Theft	Property
Linear			
10	0.00421***	0.000679**	0.00231***
	(0.000755)	(0.000304)	(0.000560)
16	-0.0129*	-0.00385*	-0.0116**
	(0.00656)	(0.00223)	(0.00491)
22	-0.0102	-0.00365	-0.00688
	(0.0105)	(0.00350)	(0.00793)
Quadratic			
10	0.00421***	0.000679**	0.00231***
	(0.000755)	(0.000304)	(0.000560)
16	-0.00311	-0.00279	-0.00870
	(0.0111)	(0.00377)	(0.00831)
22	-0.0130	-0.00791	-0.0154
	(0.0180)	(0.00596)	(0.0135)

Note: Linear and quadratic specifications, see Table 3. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.9. RD effects on criminal charges, women

Months after t=0:	All crime	Theft	Property
Linear			
10	0.00252*	0.00231*	0.00252*
	(0.000727)	(0.000696)	(0.000727)
16	0.0182*	0.0160*	0.0154*
	(0.00466)	(0.00394)	(0.00419)
22	0.0249*	0.0194*	0.0223*
	(0.00681)	(0.00564)	(0.00618)
Quadratic			
10	0.00252*	0.00231*	0.00252*
	(0.000727)	(0.000696)	(0.000727)
16	0.0191*	0.0153**	0.0162**
	(0.00736)	(0.00623)	(0.00662)
22	0.0153	0.00824	0.0108
	(0.0107)	(0.00888)	(0.00974)

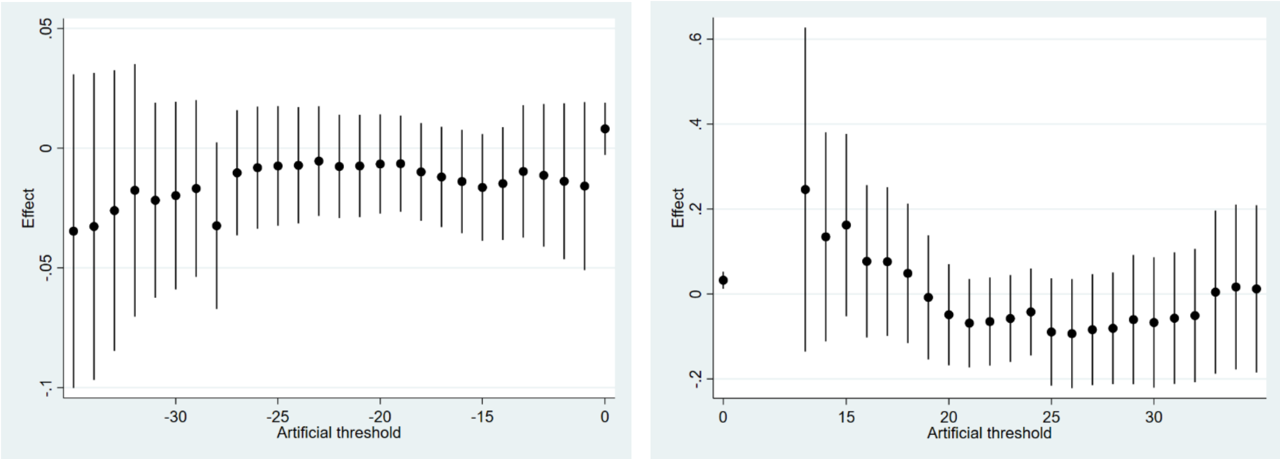
Note: Linear and quadratic specifications, see Table 3. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table B.10. RD effects on primary health-care utilization, men

Year of use:	General practice	Mental health	Physical health	Other care	Total contacts	General practice > 20
2016	-0.518	0.0168	0.103	0.913	0.395	-0.00228
	(0.728)	(0.106)	(0.222)	(0.887)	(1.239)	(0.0222)
2017	0.849	-0.0531	0.0275	0.365	1.214	0.0241
	(0.824)	(0.120)	(0.217)	(0.836)	(1.288)	(0.0226)

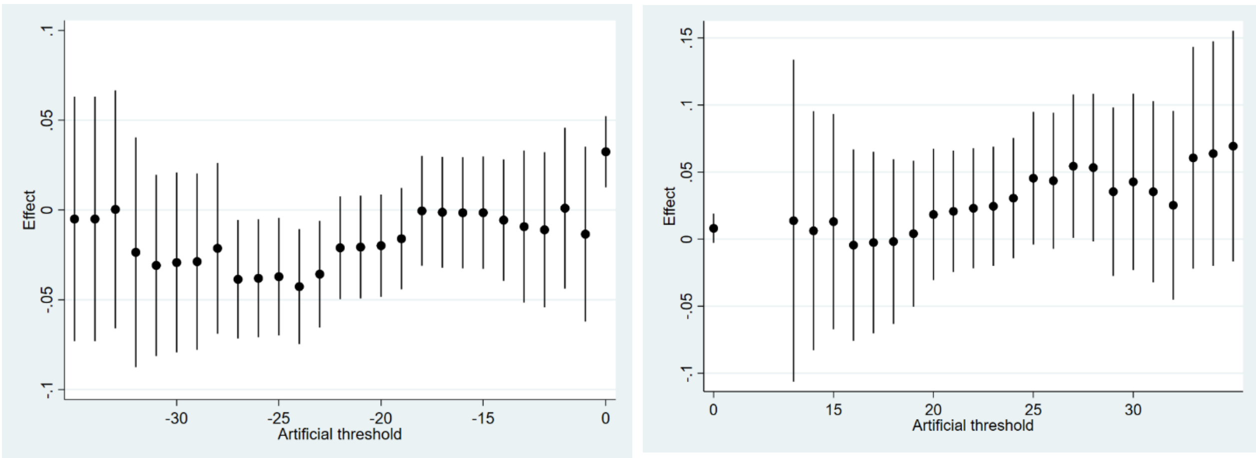
Note: See Table 3 for an explanation of the linear model. The sample includes those who receive welfare benefits for the first time in 2015. [a] Covers psychological or psychiatric treatment. [b] Covers physiotherapy or chiropractic care.
* *p < 0.1*, ** *p < 0.05*, *** *p < 0.01*.

Figure B.1. Placebo estimates for employment after 10 months, men



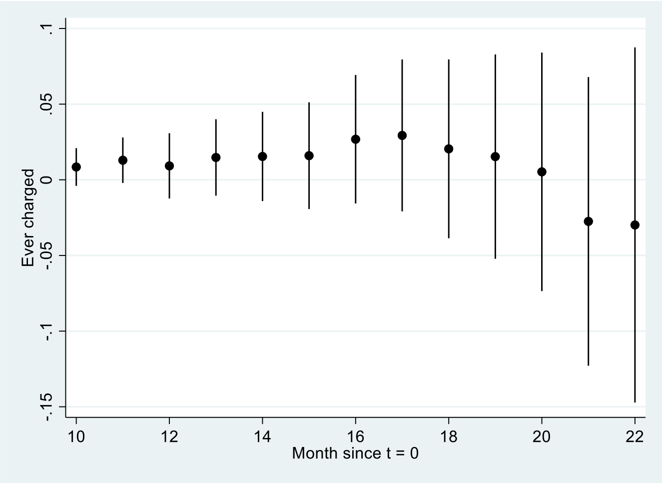
Notes: Each dot is an RD estimate based on the first-order OLS specification based on the control group (left) and the treatment group (right), where the threshold is artificially set at a given week. The estimate at zero is the true estimate. The vertical lines represent 95%-confidence intervals. No estimate could be produced for artificial threshold at week 1-10 in the treatment group.

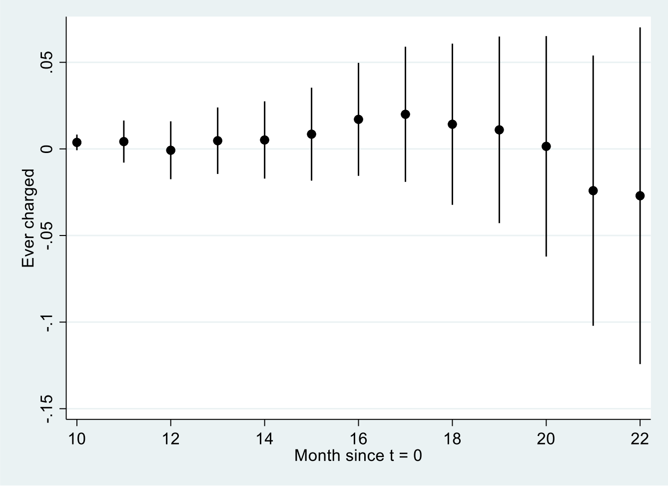
Figure B.2. Placebo estimates for employment after 10 months, women



Notes: Each dot is an RD estimate based on the first-order OLS specification based on the control group (left) and the treatment group (right), where the threshold is artificially set at a given week. The estimate at zero is the true estimate. The vertical lines represent 95%-confidence intervals. No estimate could be produced for artificial threshold at week 1-10 in the treatment group.

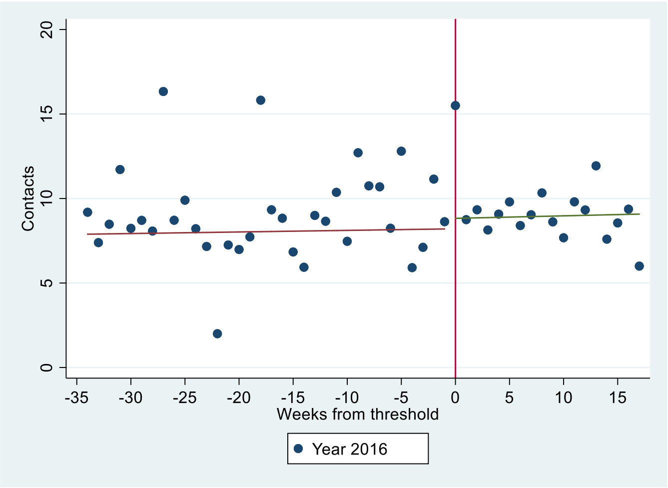
Figure B.3. RD estimates on criminal charges, when the part of the control group who are treated (left) or know they will be treated (right) are included in the treatment group, men

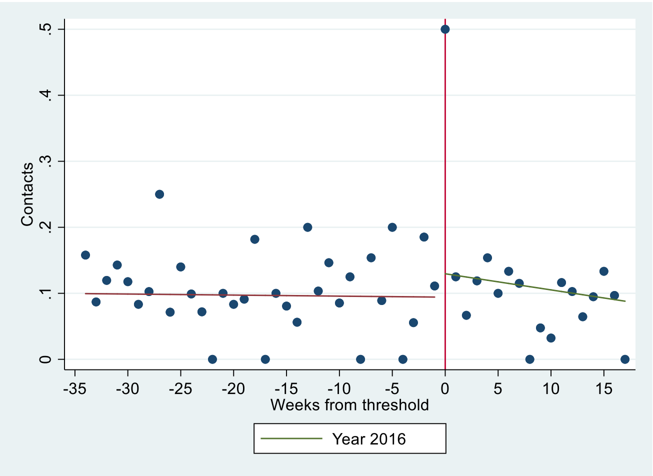




Notes: RD estimates from a quadratic specification, controlling for covariates. The control group is treated at July 2016. They know this at the announcement in April 2016. The vertical lines represent 95%-confidence intervals.

Figure B.4. Annual number of general practice contacts and share with more than 20 visits in 2016 by time of first welfare benefit, men





Notes: The first axis is the week with welfare benefits and is centered around the first week of September 2015 (=0). Only shown for refugees arrived in 2015.