

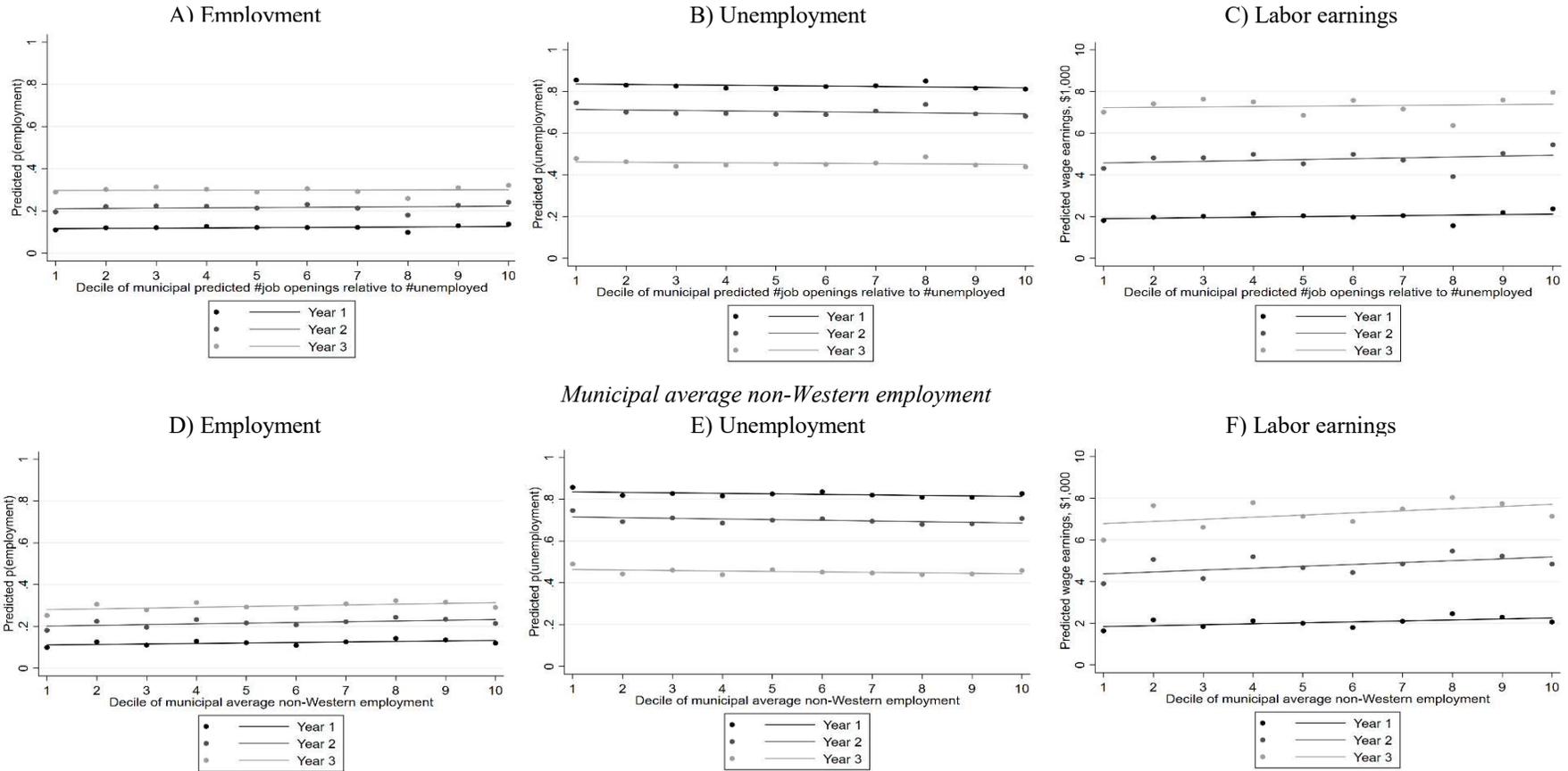
Refugee Benefit Cuts: Online Appendix

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A. Appendix Figures and Tables

Figure A.1. Outcomes predicted from observable characteristics plotted across the local labor demand indicators.

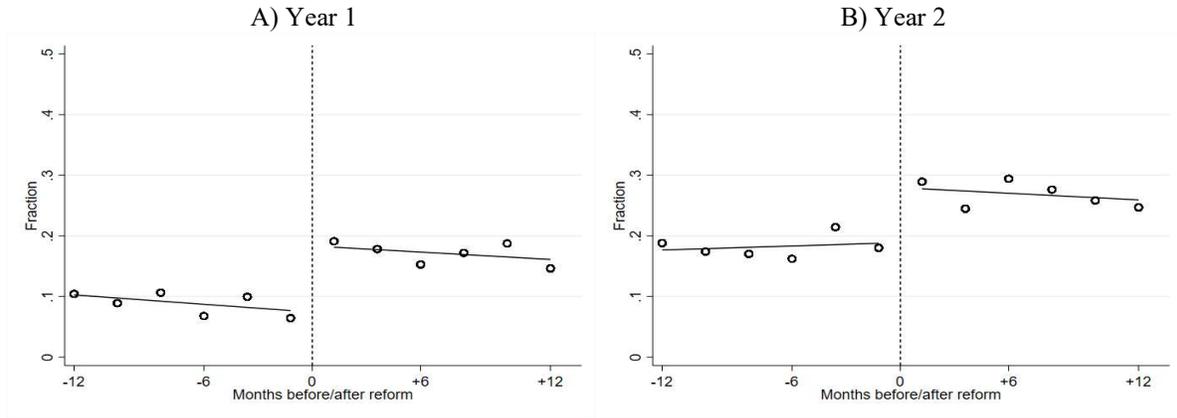
Municipal predicted job openings relative to number of unemployed



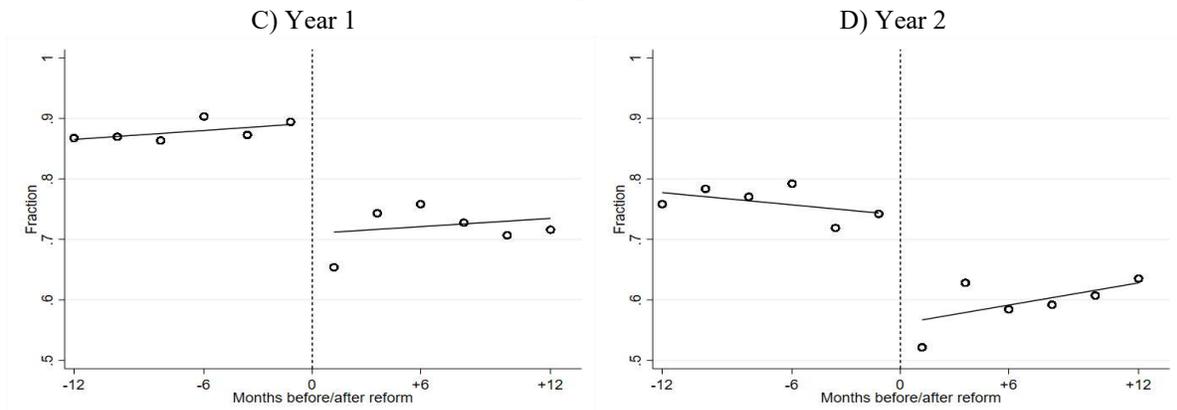
Note: The figure shows employment rates, unemployment rates, and labor earnings during the first year post residency for deciles of the two local labor demand indicators as predicted from an OLS regression using the covariates described in Table 1. Panels A-C show the equivalent across deciles of municipal predicted number of job openings relative to number of unemployed. Panels D-F present predicted outcomes plotted across deciles of municipal average non-Western employment. None of the slopes are significantly different from zero at the 10% level.

Figure A.2. Labor market outcomes first two years after residency.

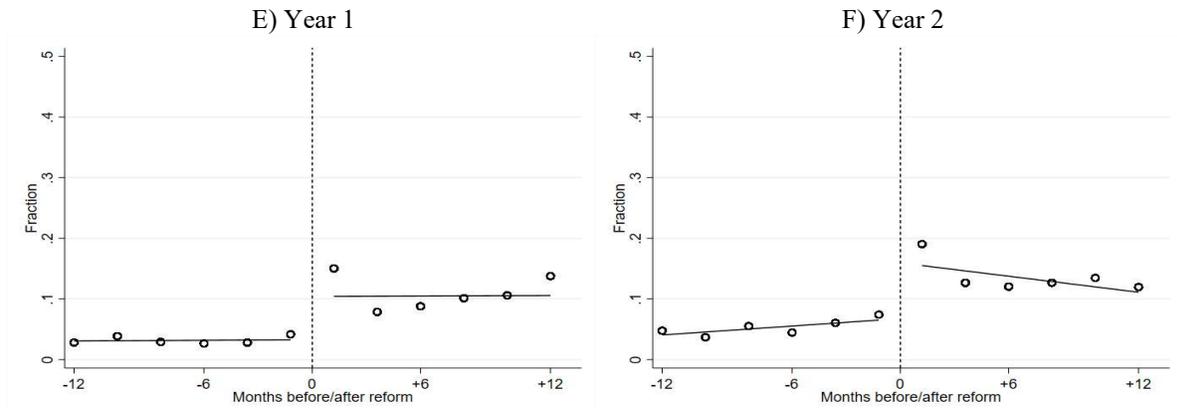
Employment



Unemployment

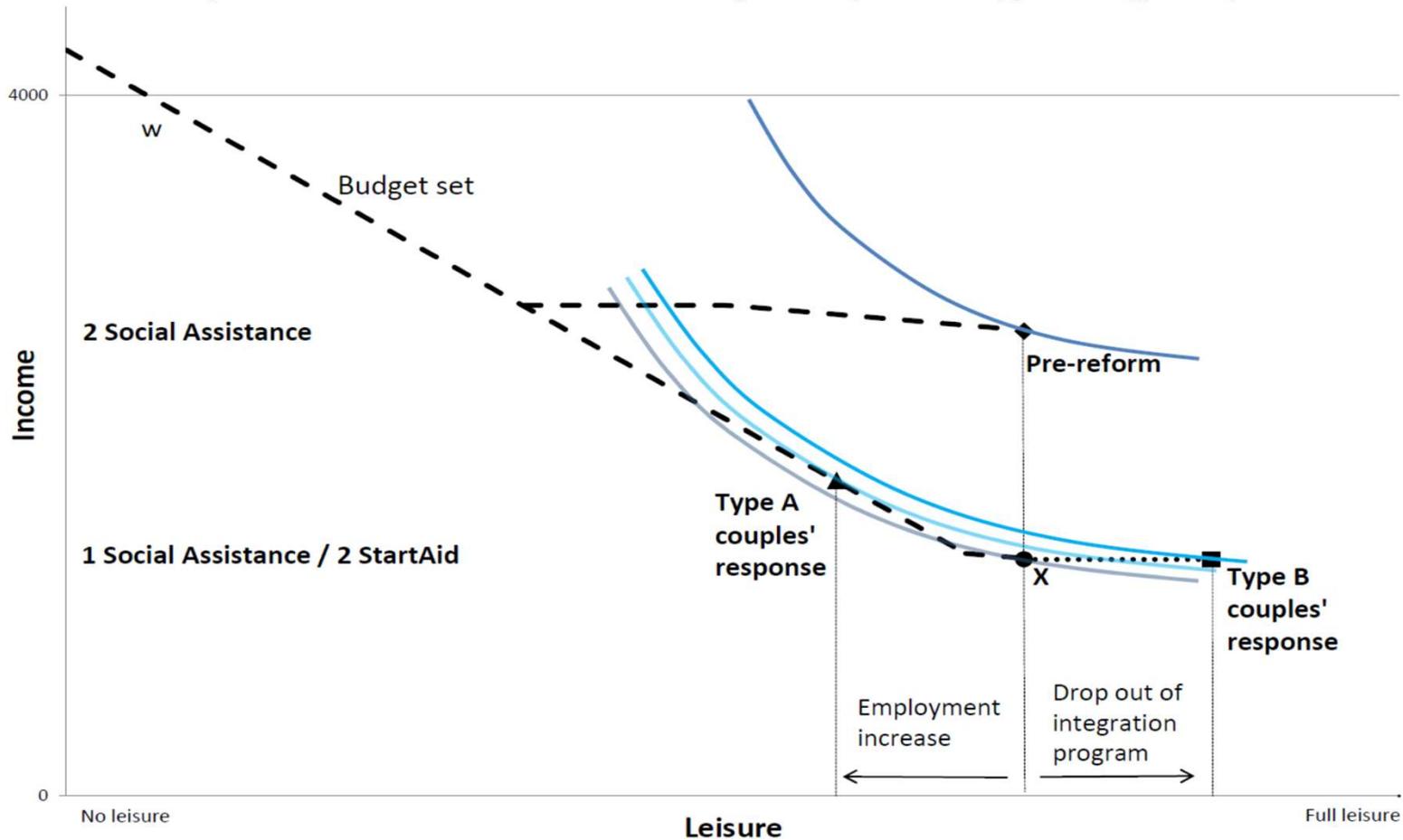


Not in the labor force



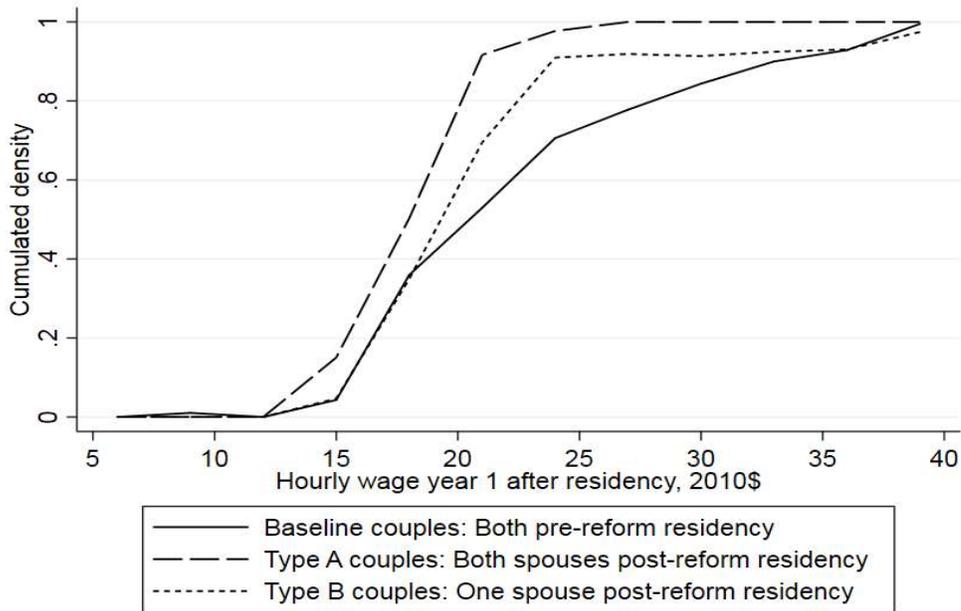
Note: The figure shows employment (A-B), unemployment (C-D), and not in the labor force (E-F) rates in year 1 and 2 by timing of residency relative to the reform. The dashed vertical line indicates the timing of the reform in July 2002. The figure contains linear slopes of the outcomes before and after the reform, to mimic our estimation strategy.

Figure A.3: Illustration of intuition behind the heterogeneous responses from Type A and Type B couples



Note: The figure presents a static labor supply framework where the horizontal axis designates leisure or work, and the vertical axis represents income, which can come from either work or transfers (if they participate in integration courses). The dashed lines represent the pre- and post-policy budget sets, with the almost horizontal parts of the budget sets corresponding to the range of labor supply affected by means testing, and the slope of the steeper segment (after the break-even points where SoA/Start Aid is exhausted) corresponding to the wage w . The reform lowers transfers from the pre-reform level at the diamond-mark to the circle. Type A couples can reach a higher indifference curve by increasing labor supply to the triangle. Type B couples can reach an even higher indifference curve by dropping out of integration programs thereby increasing leisure.

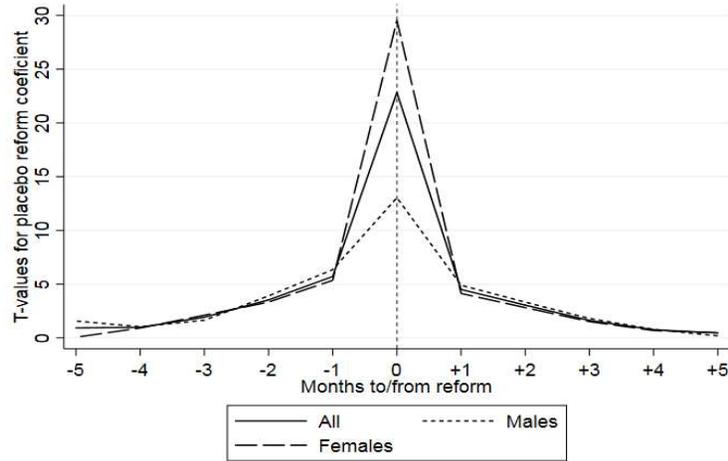
Figure A.4. Change in distributions of hourly wage rates as result of the reform, by household type, males in couples, year 1.



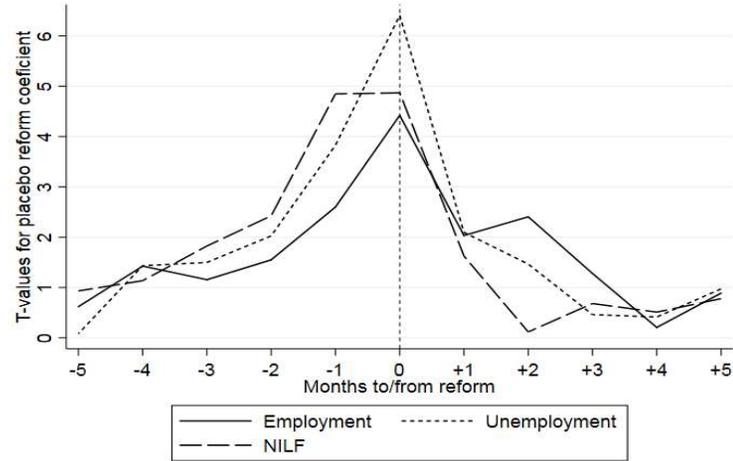
Note: The figure shows the distributions of hourly wage rates for males (from the couples sample) by the three family types. The distributions are constructed by creating a series of dummies ($1[y \leq x]$) for whether hourly wages are x or below, varying x from zero to the top of the earnings distribution (from \$0 to \$40). By estimating Eq. (2) with these dummy variables as outcomes, we capture the changes in the cumulative hourly wage distribution.

Figure A.5. Placebo reform estimates before and after actual timing of reform (time 0).

A) Placebo reform estimates, transfers year 1



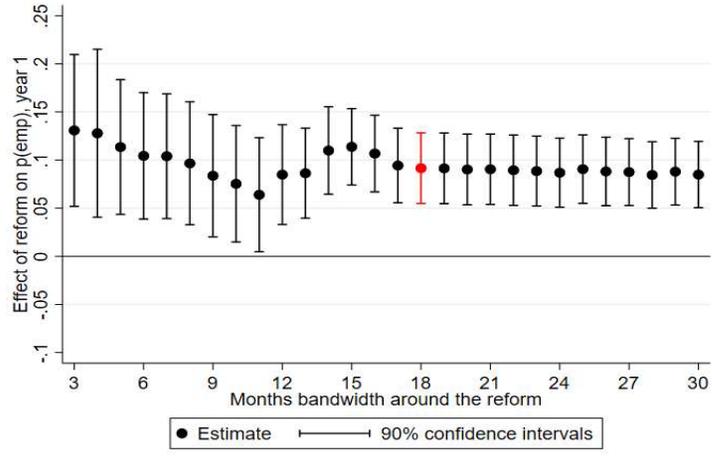
B) Placebo reform estimates, labor market outcomes year 1



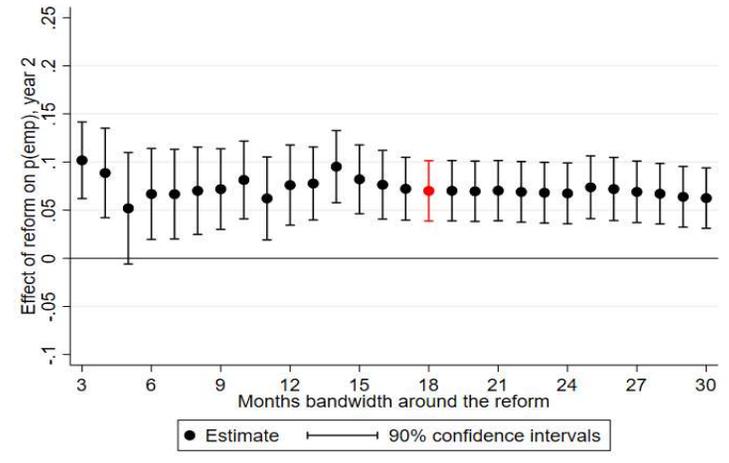
Note: The figure shows the t-values of placebo reform estimates from 5 months before to 5 months after the actual reform (at time 0) on transfers at individual level (Panel A) and employment, unemployment and not in the labor force rates, (NILF) (Panel B). Each estimate is constructed from a sample of ± 18 months from the placebo reform date in question (e.g., for placebo reform at time -4 the data is sampled from month -22 to time 14). We generate for each period between -5 to +5 a placebo reform dummy $P_reform = 0$ if residency is granted before that time and $= 1$ if it is granted after (we construct new running variables $Z_placebo$ in a similar way). We then estimate Eq. (1) with each of the new placebo datasets and placebo reform dummies: $y = \alpha + \beta * P_reform + g(Z_placebo)' \pi + \varepsilon$ such that estimates at time 0 are the actual reform estimates (shown in Table 2).

Figure A.6. The effects of the reform on employment using different sampling bandwidths around the reform.

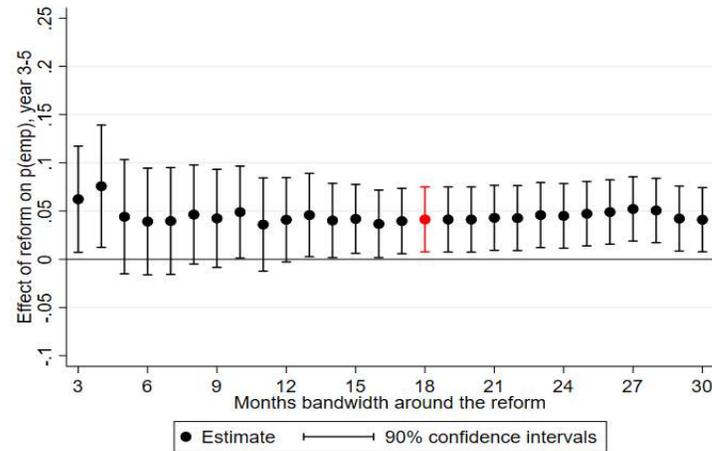
A) Employment in year 1 after residency



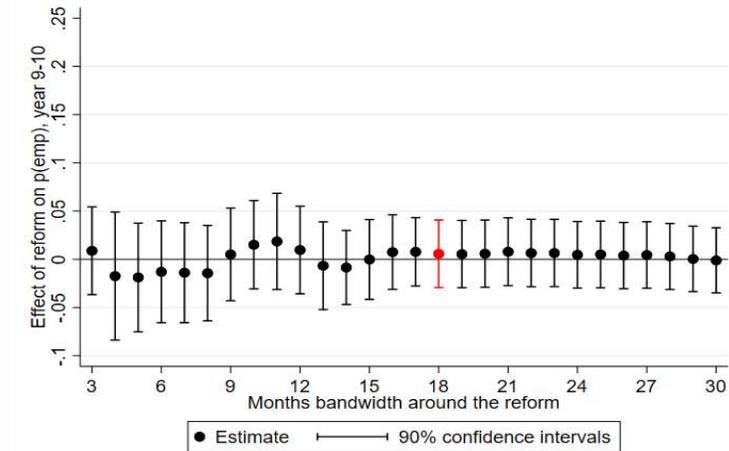
B) Employment in year 2 after residency



C) Employment in years 3-5 after residency

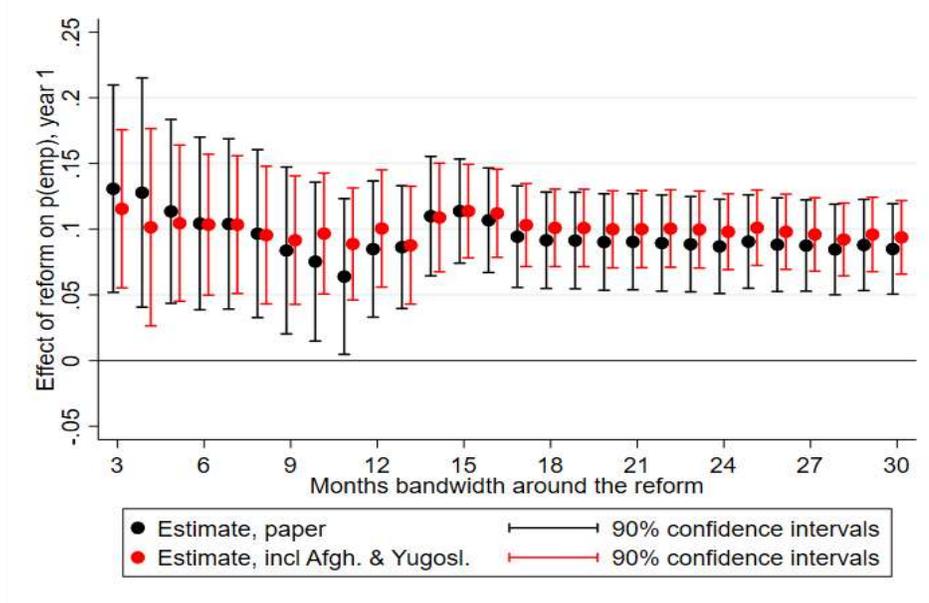


D) Employment in years 9-10 after residency



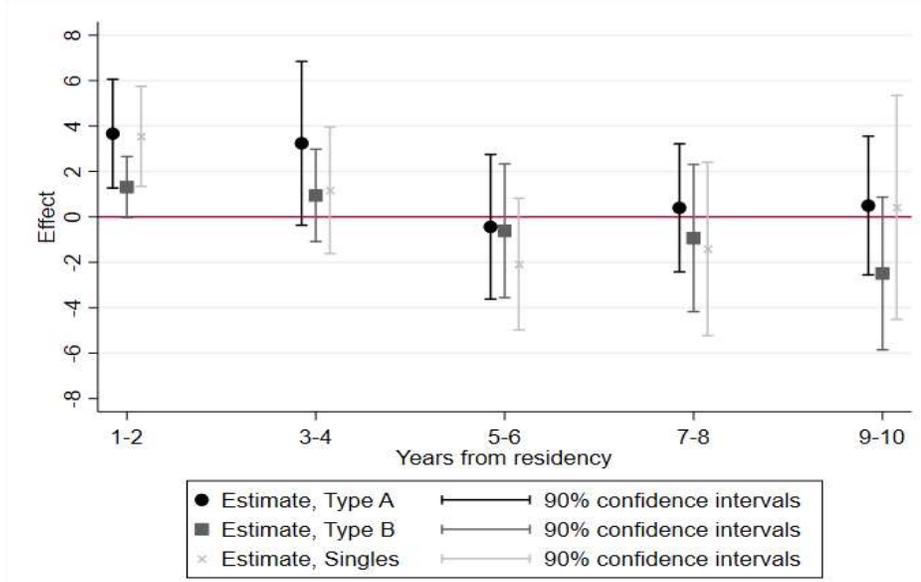
Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform on the subsequent employment probability in years 1, 2, 3-5 and 9-10 after residency. The figure shows estimates for different sampling bandwidths from +3 months around the reform to +30 months around the reform. The estimates marked with red (at 18) are those reported in the main text.

Figure A.7. Effects of the reform on employment in the first year after residency, with and without Afghans and Yugoslavs, using different sampling bandwidths around the reform.



Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform on the subsequent probability of being employed in year 1 after residency for adults aged 18-55 at the time of residency. The figure shows estimates for the base sample without Afghans and Yugoslavs (black), and the sample including Afghans and Yugoslavs (red), for different sampling bandwidths from +-3 months around the reform to +-30 months around the reform.

Figure A.8. Effect of reform on males' labor earnings, 1-10 years after residency.

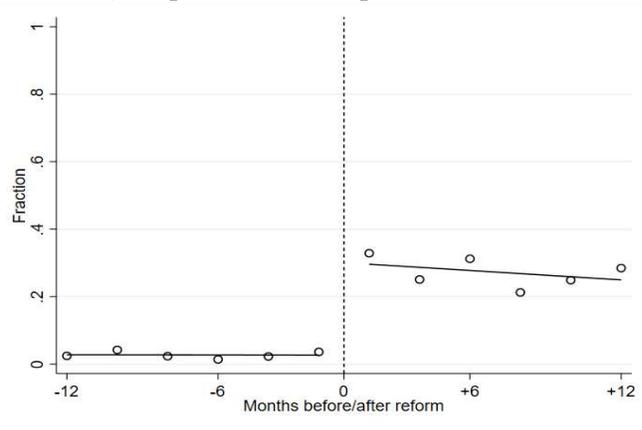
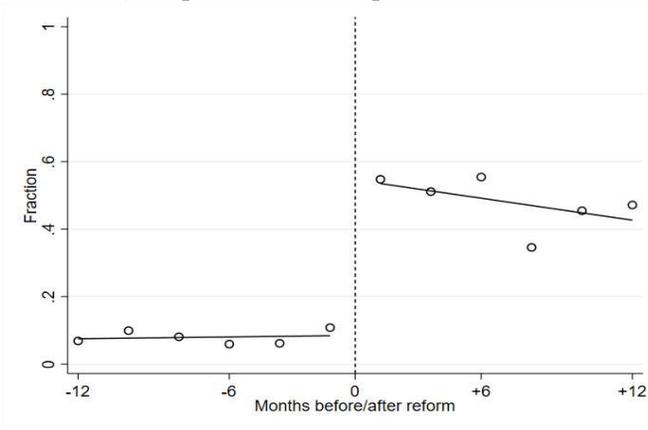


Note: The figure shows estimated effect of the reform and 90% confidence intervals on males' labor earnings in year 1-10 from residency. Standard errors are clustered on twoway level by residency month and household for couples and by residency month for singles.

Figure A.9. Fraction with low disposable income and crime convictions by crime type.

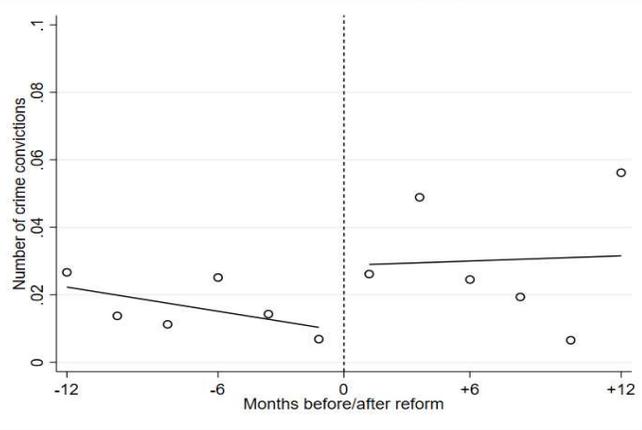
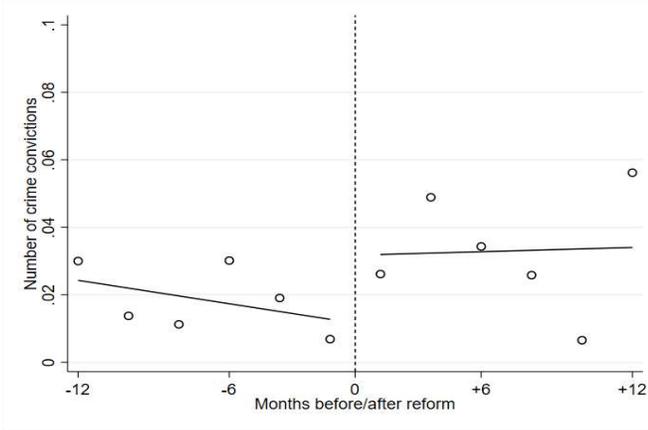
A) Disposable income per month < \$750

B) Disposable income per month < \$500



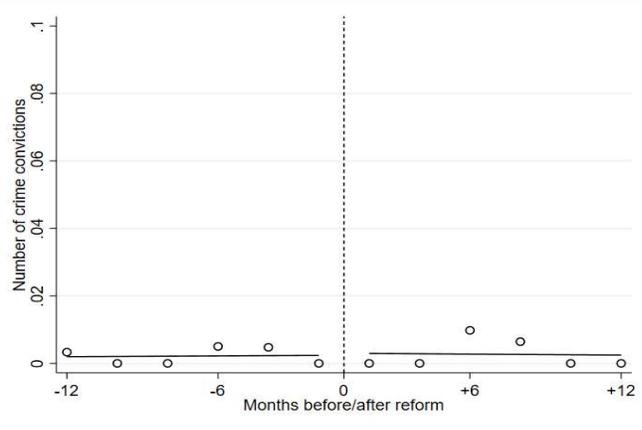
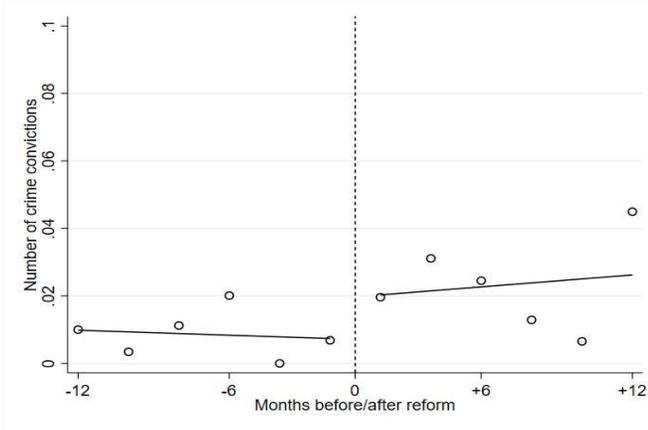
C) Crime convictions, year 1, all crime

D) Crime convictions, year 1, property crime



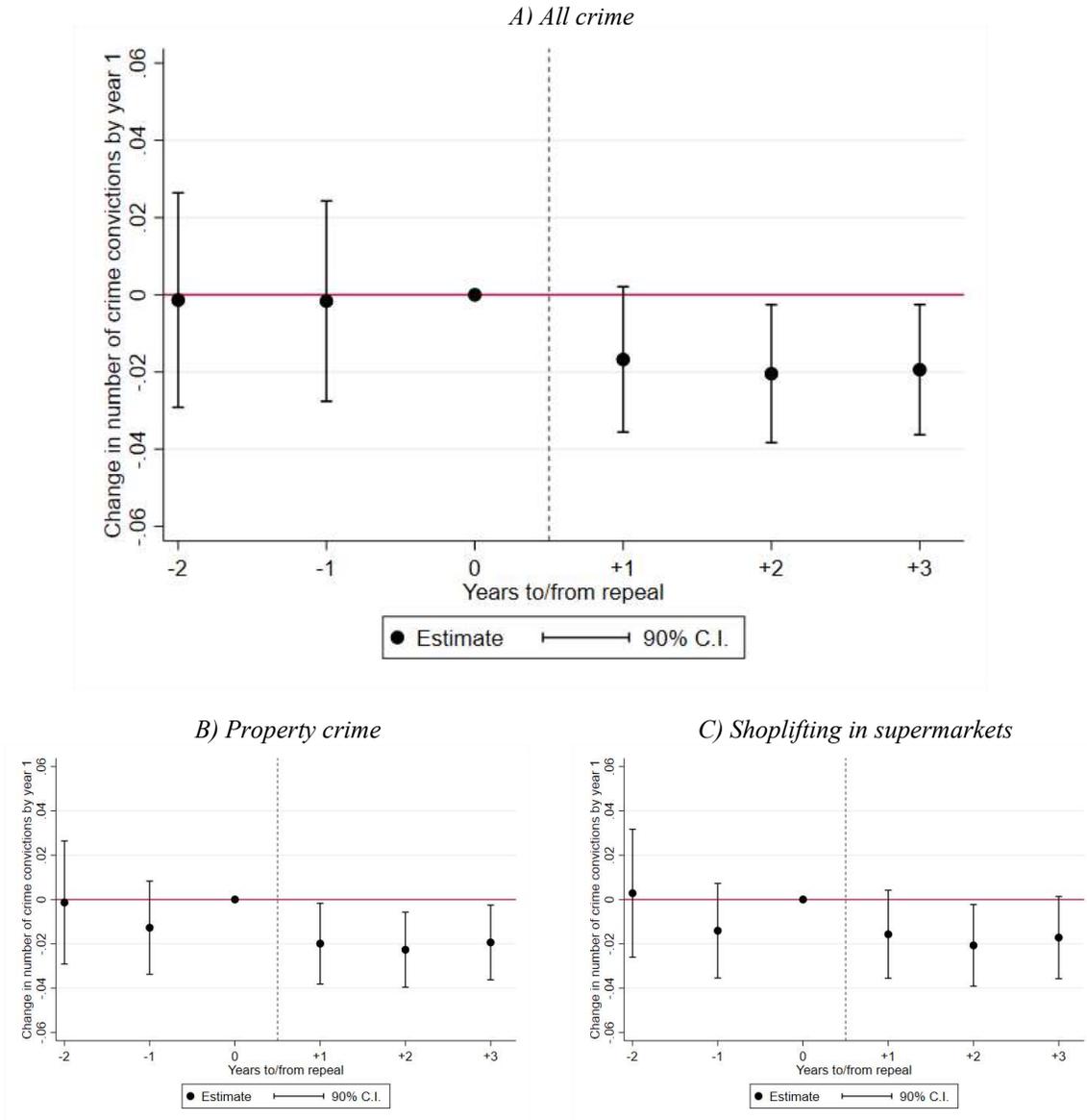
E) Crime convictions, year 1, theft in supermarket

F) Crime convictions, year 1, violence



Note: The figure shows average outcomes from Tables 7 and 8 plotted by timing of residency relative to the reform. Panels A-B show the the fraction with post-tax disposable income below \$500 and \$750 per month. Panels C-F show, by crime type, the average number of crime convictions for all adults aged 18-45 at the time of residency with children. The dashed vertical line indicates the timing of the reform in July 2002. The figure contains linear slopes of the outcomes before and after the reform, to mimic our estimation strategy.

Figure A.10. Crime conviction differences in year 1 after residency around the repeal for females.



Note: The figure shows, for female refugees aged 18-45 at residency with children, the estimated differences in the number of crime convictions for crimes committed in the 1st year after residency according to whether the refugees were exposed to the repeal of the Start Aid (increasing transfers in 2012) marked by the vertical dashed line. The figure shows (as in Table 8) crime differences for all crime in A), property crime in B), and shoplifting in supermarkets in C). The pre-repeal (control) years include those who received residency in 2009-11 (-2 to 0 on the x-axis in the figure) and the post-repeal (treatment) years include those who were granted residency in 2012-14 (1 to 3 on the x-axis in the figure). The vertical lines indicate 90% confidence intervals.

Table A.1. Implied marginal tax rate at the participation margin and break-even point by residency before / after the reform.

Status	Age	Children	A) Transfer levels			B) Implied marginal tax rates: Single / both spouses before reform	C) Implied marginal tax rates: Single after reform / Type A couples	D) Implied marginal tax rates: Type B couples			
			Before Reform (SoA)	After reform (Start Aid)	Pct. Transfer Reduction	Break-even point	Implied marginal tax rate	Break-even point	Implied marginal tax rate	Break-even point	Implied marginal tax rate
Couple	>= 25	0	1,020	545	47	2,286	0.935	1,596	0.821	1,233	0.935
Couple	>= 25	1	1,356	682	50	2,959	0.935	1,994	0.821	1,542	0.935
Couple	>= 25	>= 2	1,356	818	40	2,959	0.935	2,393	0.821	1,850	0.935
Couple	< 25	1	1,356	682	50	2,959	0.935	1,994	0.821	1,542	0.935
Couple	< 25	>= 2	1,356	818	40	2,959	0.935	2,393	0.821	1,850	0.935
Single or couple	< 25	0	658	545	17	744	0.935	798	0.821	617	0.935
Single	>= 25	0	1,020	658	36	1,152	0.935	961	0.821	-	-
Single	>= 25	1	1,356	822	39	1,533	0.935	1,201	0.821	-	-
Single	>= 25	>= 2	1,356	986	27	1,533	0.935	1,441	0.821	-	-
Single	< 25	1	1,356	710	48	1,533	0.935	1,038	0.821	-	-
Single	< 25	>= 2	1,356	874	36	1,533	0.935	1,278	0.821	-	-
Live with parents	< 25	0	317	271	15	359	0.935	393	0.821	-	-
Average in sample			1,256	748	40						

Note: The table shows transfer levels (for refugees eligible for full SoA or Start Aid) and implied marginal tax rates (once labor earnings are above zero) due to means testing of transfers by household type. All amounts are reported in 2010 PPP-adjusted USD with transfer levels as defined in 2002. Panel A shows how transfer levels for individuals in different household types are affected by the reform. Young refugees without children are affected the least as they were already entitled to comparatively low levels of SoA before the reform. All other groups are entitled to at least 25% lower transfers after the reform. Couples are affected the most with 40-50% lower transfer levels. The row "Average in sample" presents the average pre- and post-reform rates based on the sample composition of the different household types. Panels B-D show the implied marginal tax rate on labor earnings at the participation margin (i.e. implied tax on first dollar earned) and the break-even point (where one dollar earned returns one dollar in gross income) for different household types and by treatment status. Calculations are based on the average minimum hourly wage for unskilled workers across several sectors. "Type A couples" are couples where both receive residency after the reform. "Type B couples" are couples where one receives residency before the reform and one after the reform.

Table A.2. Conditional balancing test of covariates across reform.

	<i>A) Base sample</i>	<i>B) Full sample</i>	<i>C) Couples sample</i>
Age at residency	-0.001 (0.001)	-0.000 (0.001)	-0.002** (0.001)
Female	0.014 (0.010)	0.007 (0.015)	0.015 (0.024)
# of children	-0.005 (0.001)	-0.004 (0.004)	-0.010 (0.006)
Single	0.001 (0.013)	-0.007 (0.012)	-
Eastern Europe/former USSR	-0.012 (0.025)	-0.034 (0.027)	-0.043 (0.038)
Rest of the world	-0.033 (0.027)	-0.015 (0.023)	0.011 (0.041)
Refugee permit status	-0.006 (0.017)	0.000 (0.015)	0.023 (0.037)
First residency in couple	-	-	-0.008 (0.030)
P(F)	0.517	0.310	0.382
Observations	4,843	8,506	4,072
Running variable	X	X	X

Note: The table extends Table 1, column 4. The table shows full regression results and F-tests of conditional balancing of covariates across the reform. Panels A and B show results from regressing a dummy indicating whether residency was granted pre- or post-reform on all covariates and the running variable for the main sample (age 18-55) and the full sample (including children). Panel C shows the equivalent results for the couples sample including a dummy indicating whether the spouse in question is the first or last to receive residency. The table hence reports the individual γ 's and an F-test for joint significance of the γ 's (allowing for different slopes in the running variable on each side of the cutoff) from the regression:

$$reform = a + X' \gamma + g(Z)' \pi + \varepsilon$$

with standard errors in parentheses. The results from Panel A are also presented in Table 1, column 4. Covariates include age at residency, gender, number of children, marital status (except for Panel C as all couples are married), country of origin (Eastern Europe/former USSR and rest of world, with predominantly Muslim countries as reference category), and refugee permit status (is residency given on grounds of being a refugee, or from being the spouse / child of an individual with refugee status). For couples, 'First residency in couple' is a dummy for whether the spouse in question is the first or last to receive residency.

* p<0.1; ** p<0.05; *** p<0.01

Table A.3. Unconditional balancing test across reform.

	A) Base sample	B) Incl. re-migrants	C) Full sample	D) Couples sample
Age at residency	-0.873 (0.561)	-0.934 (0.557)	-0.841 (0.767)	-1.904*** (0.580)
Female	0.056 (0.040)	0.042 (0.036)	0.022 (0.027)	0.062 (0.047)
# of children	-0.137 (0.129)	-0.104 (0.104)	-0.073 (0.046)	-0.078 (0.087)
Single	0.008 (0.038)	0.004 (0.037)	0.015 (0.022)	-
Muslim countries	0.037 (0.042)	0.028 (0.036)	0.040 (0.035)	0.015 (0.049)
Eastern Europe/former USSR	-0.025 (0.020)	-0.020 (0.017)	-0.023 (0.018)	-0.031 (0.027)
Rest of the world	-0.012 (0.036)	-0.008 (0.031)	-0.017 (0.030)	0.016 (0.041)
Refugee permit status	-0.049 (0.056)	-0.033 (0.058)	-0.026 (0.057)	0.013 (0.073)
Waiting time in asylum center	0.994 (2.048)	0.553 (1.808)	-	-
Remigrated	-	-0.030 (0.032)	-	-
First residency in couple	-	-	-	-0.044 (0.045)
Observations	4,843	5,747	8,506	4,072
Running variable	X	X	X	X

Note: The table extends Table 1, column 5. The table shows estimation results of regressing each observable characteristic on a dummy indicating whether residency is granted pre- or post-reform conditional on the running variable (allowing for different slopes in the running variable on each side of the cutoff). The table thus reports the individual γ 's from:

$$x = a + \gamma * reform + g(Z)' \pi + \varepsilon$$

with standard errors in parentheses. Each cell represents one regression and shows the change in the observable characteristic around the reform, by sample. The results from Panel A are also presented in Table 1, column 5. Covariates: age at residency, gender, number of children, marital status, country of origin and refugee permit status (residency given to a refugee=1; being the spouse / child of an individual with refugee status=0). Re-migrants are those who left Denmark during the follow up period. For couples, 'First residency in couple' is a dummy for whether the spouse in question is the first or last to receive residency. Waiting time in asylum center is months spent from asylum application date until the residency is granted. The average waiting time in the base sample is 15.25 months. We have estimated these statistics using data from Hvidtfeldt et al. (2018) (who have access to confined data via the Danish Ministry of Integration and the Red Cross), where we have replicated our main sample selection. However, the confined data does not enable us to include waiting time in the full conditional balancing test in Table A.2.

* p<0.1; ** p<0.05; *** p<0.01

Table A.4. Formal McCrary tests of discontinuity in running variable across different bandwidth choices.

	10%	20%	30%	40%	50%	60%	70%	80%	90%	Optimal BW	110%	120%	130%	140%	150%
A) Adults	0.031 (0.347)	0.031 (0.245)	0.031 (0.020)	0.301 (0.173)	0.114 (0.159)	0.097 (0.148)	0.056 (0.141)	-0.024 (0.133)	-0.065 (0.125)	-0.028 (0.115)	-0.023 (0.107)	-0.028 (0.098)	0.041 (0.095)	0.096 (0.092)	0.148 (0.088)
B) All refugees	-0.101 (0.259)	-0.101 (0.183)	-0.190 (0.148)	-0.147 (0.133)	-0.097 (0.123)	-0.076 (0.116)	-0.094 (0.109)	-0.155 (0.102)	-0.135 (0.093)	-0.116 (0.085)	-0.108 (0.078)	-0.041 (0.074)	0.020 (0.071)	0.067 (0.068)	0.100 (0.065)

Note: The table presents McCrary tests of discontinuity in the running variable. The table shows the log difference in density of the running variable around the reform and the corresponding standard errors (in parentheses) for bandwidths from 10% of the optimal bandwidth to 150% of the optimal bandwidth. Bandwidths are chosen as in McCrary (2008) resulting in an optimal bandwidth of approximately 6.6 months. Observations: 4,843.

* p<0.1; ** p<0.05; *** p<0.01

Table A.5. Conditional balancing test of covariates across assignment municipality's labor demand indicators.

	<i>A) Municipal average non-Western employment</i>				<i>B) Job openings relative to number of unemployed</i>				<i>C) Predicted job openings relative to number of unemployed</i>			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
F-value	1.319	1.223	0.775	1.099	0.846	1.257	1.067	1.090	1.106	1.105	0.966	1.168
P(F)	0.167	0.225	0.761	0.346	0.657	0.205	0.384	0.359	0.343	0.324	0.527	0.249
Variables included in the test:												
Observable characteristics	X	X	X	X	X	X	X	X	X	X	X	X
Start Aid reform		X		X		X		X		X		X
Calendar month of residency			X	X			X	X			X	X

Note: The table shows estimates from regressing labor market indicators (municipal average non-Western employment in Panel A; municipal number of job openings relative to number of unemployed in Panel B (which we include to show that the actual job openings are also unrelated to refugee characteristics); and the predicted municipal number of job openings relative to number of unemployed in Panel C) on observable characteristics (see Table 1) and timing of residency. "Start Aid reform" refers to the running variables pre- and post-reform and a dummy indicating whether residency was granted after the reform. "Calendar month of residency" refers to dummies indicating whether residency was granted in February, ... , December with January as reference. Standard errors are clustered by allocation municipality. Observations: 4.843.

* p<0.1; ** p<0.05; *** p<0.01

Table A.6. Mobility away from allocation municipality and municipal activation requirements.

	<i>Geographic mobility</i>		<i>Municipal activation requirements</i>	
	(1)	(2)	(3)	(4)
	P(Move)	P(Move and find employment)	Fraction of time where job-related activation is part of integration program	Fraction of time spent in other components of integration programs
A) Using job openings in low / unskilled jobs				
<i>High demand</i>				
Reform effect	0.013 (0.017)	0.005 (0.009)	0.158*** (0.026)	-0.330*** (0.046)
Pre reform mean	0.033	0.013	0.074	0.822
<i>Low demand</i>				
Reform effect	0.013 (0.020)	0.007 (0.013)	0.159*** (0.027)	-0.329*** (0.048)
Pre reform mean	0.072	0.021	0.059	0.783
<i>High-low difference</i>				
Reform effect	0.026 (0.026)	-0.002 (0.016)	-0.001 (0.036)	-0.001 (0.058)
B) Using average employment of non-Western immigrants				
<i>High demand</i>				
Reform effect	0.014 (0.014)	0.014 (0.009)	0.162*** (0.031)	-0.313*** (0.048)
Pre reform mean	0.039	0.010	0.072	0.810
<i>Low demand</i>				
Reform effect	-0.009 (0.022)	-0.001 (0.014)	0.156*** (0.024)	-0.345*** (0.040)
Pre reform mean	0.061	0.023	0.062	0.800
<i>High-low difference</i>				
Reform effect	0.022 (0.027)	0.014 (0.018)	0.005 (0.038)	0.032 (0.048)
Observations	4,843	4,843	4,843	4,843

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform separately for refugees assigned to municipalities with high / low local labor demand on the subsequent (1) probability of moving to another municipality within the first two years after residency, (2) probability of moving to another municipality and finding employment within the first two years, (3) fraction of time during the first two years after residency spent receiving transfers where job-related activation is part of the program, and (4) fraction of time during the first two years after residency spent receiving transfers where job-related activation is not a part of the program. The table also shows pre-reform means of the outcome variables. High / low labor demand is defined in Panel A by being assigned to a municipality with above/below median of the predicted ratio of the number of job openings in low / unskilled work relative to the number of unemployed individuals, and in Panel B by being assigned to a municipality with above/below median employment rate of non-Western immigrants in 1999-2001. Standard errors are clustered on twoway level by residency month and allocation municipality.

* p<0.1; ** p<0.05; *** p<0.01

Table A.7. Effect of reform on subsequent type of occupation.

Years since residency	Highly skilled	Medium skilled / office	Sales	Medium skilled vocational	Basic skilled work	Unspecified self- employment	Unskilled manual work
A) All							
1	0.004 (0.004)	0.002 (0.003)	0.008 (0.006)	0.002 (0.005)	0.008 (0.006)	0.005 (0.003)	0.063*** (0.021)
2	0.003 (0.006)	-0.002 (0.006)	0.008 (0.009)	0.002 (0.008)	0.003 (0.008)	0.001 (0.003)	0.054** (0.025)
3-5	0.003 (0.005)	-0.002 (0.004)	-0.005 (0.011)	-0.001 (0.007)	-0.009 (0.011)	-0.004 (0.003)	0.059*** (0.019)
B) <12 years education							
1	-0.000 (0.002)	-0.000 (0.003)	0.007 (0.007)	-0.002 (0.007)	0.008 (0.006)	0.011 (0.006)	0.072*** (0.025)
2	0.004 (0.003)	0.003 (0.007)	0.011 (0.011)	0.007 (0.008)	-0.004 (0.010)	0.002 (0.003)	0.028 (0.039)
3-5	0.001 (0.004)	-0.005 (0.005)	-0.003 (0.014)	-0.006 (0.006)	-0.012 (0.014)	-0.004 (0.003)	0.056** (0.022)
C) >=12 years education							
1	0.010 (0.007)	0.004 (0.004)	0.007 (0.008)	0.005 (0.005)	0.007 (0.009)	-0.000 (0.003)	0.052* (0.030)
2	0.004 (0.010)	-0.006 (0.008)	0.005 (0.012)	-0.003 (0.015)	0.012 (0.011)	0.000 (0.006)	0.079** (0.037)
3-5	0.009 (0.010)	0.004 (0.007)	-0.007 (0.014)	0.006 (0.013)	-0.004 (0.016)	-0.003 (0.005)	0.060** (0.023)
Observations	4,843	4,843	4,843	4,843	4,843	4,843	4,843

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on the subsequent probability of being employed in a given type of occupation for the main sample of adults (aged 18-55 at the time of residency) in year 1, 2, and the average of years 3-5 since residency. Panel A shows results for all individuals in the sample. Panels B and C show results by level of education upon residency (self-reported). All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.8. Effect of reform on subsequent labor market outcomes, by gender.

Years since residency	(1)	(2)	(3)	(4)	(5)	(6)
	Employment	Unemployment	Not in the labor force	Employment	Unemployment	Not in the labor force
A) Full sample						
1	0.160*** (0.044)	-0.155*** (0.045)	-0.004 (0.012)	0.037 (0.022)	-0.171*** (0.022)	0.132*** (0.021)
2	0.132*** (0.035)	-0.137*** (0.038)	0.011 (0.023)	0.015 (0.019)	-0.172*** (0.029)	0.162*** (0.025)
3-5	0.042 (0.033)	-0.069*** (0.024)	0.029 (0.024)	0.041** (0.016)	-0.130*** (0.024)	0.093*** (0.019)
B) <12 years of education						
1	0.193*** (0.060)	-0.192*** (0.058)	0.001 (0.019)	0.034 (0.024)	-0.176*** (0.028)	0.139*** (0.026)
2	0.127** (0.062)	-0.155** (0.066)	0.036 (0.024)	-0.003 (0.025)	-0.145*** (0.037)	0.151*** (0.028)
3-5	0.034 (0.039)	-0.087*** (0.032)	0.055* (0.029)	0.020 (0.033)	-0.118*** (0.039)	0.102*** (0.022)
C) >=12 years of education						
1	0.133** (0.051)	-0.126** (0.051)	-0.007 (0.017)	0.035 (0.035)	-0.159*** (0.034)	0.124*** (0.045)
2	0.141*** (0.048)	-0.128** (0.047)	-0.010 (0.036)	0.038 (0.043)	-0.208*** (0.046)	0.177*** (0.048)
3-5	0.057 (0.047)	-0.061* (0.036)	0.006 (0.027)	0.077** (0.029)	-0.157*** (0.027)	0.085*** (0.026)
Observations	2,390	2,390	2,390	2,453	2,453	2,453

Note: The table shows the estimated effects, by gender (males columns 1-3; females columns 4-6) and education upon residency, of being granted residency after the reform relative to before the reform on subsequent probability of being employed, unemployed, and not in the labor force for the base sample of adults (aged 18-55 at the time of residency) in year 1 and 2, and the average of years 3-5 after residency. Panel A reproduces the full sample results (cf. Table 3). Panels B and C show estimates by education level. All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.9. Elasticities of labor earnings and the number of crime convictions with respect to benefit levels, year 1 and accumulated from year 1-5 following residency.

	Year 1	Accumulated Year 1-5
A) Labor earnings elasticity by household type		
Type A couples	-1.362*** (0.283)	-0.793*** (0.260)
Type B couples	-0.375 (0.489)	-0.155 (0.284)
Singles	-1.049*** (0.477)	0.104 (0.382)
B) Comparing labor earnings elasticities with crime elasticities, adults aged 18-45 with children		
Elasticity of labor earnings with respect to benefit levels	-0.701* (0.398)	-0.323* (0.190)
Elasticity of crime with respect to benefit levels	-1.480*** (0.526)	-0.883** (0.363)

Note: The table shows the implied labor earnings and crime elasticities with respect to benefit levels. The elasticities are calculated as the percentage change in labor earnings and number of crime convictions, respectively, relative to the percentage change in potential benefit levels induced by the reform. Panel A) shows labor earnings elasticities for Type A and Type B couples, and singles (corresponding to the results on labor market outcomes presented in Table 4). Panel B) shows results for adults aged 18-45 with children (the same sample as the crime results used in Table 8). Standard errors are calculated based on 500 bootstraps.

* p<0.1; ** p<0.05; *** p<0.01

Table A.10. Effect of a placebo reform in 2000 and the actual reform in 2002 on subsequent annual individual transfers, labor earnings (both measured in USD 1,000), employment, unemployment, and fraction not in the labor force.

Years since residency	<i>A) Transfers</i>		<i>B) Labor earnings</i>		<i>C) Employment rate</i>		<i>D) Unemployment rate</i>		<i>E) Not in labor force</i>	
	Placebo reform	Actual reform	Placebo reform	Actual reform	Placebo reform	Actual reform	Placebo reform	Actual reform	Placebo reform	Actual reform
1	-0.070 (0.249)	-9.775*** (0.407)	0.308 (0.241)	1.144*** (0.400)	-0.000 (0.014)	0.092*** (0.022)	0.002 (0.021)	-0.164*** (0.027)	0.003 (0.014)	0.072*** (0.014)
2	-0.294 (0.278)	-8.320*** (0.446)	0.649 (0.511)	1.567*** (0.541)	0.002 (0.018)	0.070*** (0.019)	-0.012 (0.025)	-0.158*** (0.028)	0.012 (0.014)	0.093*** (0.020)
3-5	-0.326 (0.331)	-4.956*** (0.457)	0.262 (0.697)	1.070** (0.451)	0.002 (0.018)	0.041* (0.021)	-0.003 (0.018)	-0.104*** (0.015)	-0.002 (0.020)	0.066*** (0.012)
Observations	5,903	4,843	5,903	4,843	5,903	4,843	5,903	4,843	5,903	4,843

Note: The table shows the estimated effects of the reform as presented in Table 2 and for a placebo reform 2 years earlier in July 2000 on subsequent income from transfers and labor earnings (on individual level), and the probability of being employed, unemployed, or not in the labor force measured for adults (aged 18-55 at residency) in year 1, 2, and the average of years 3-5 since residency. Estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.11. Robustness check of the impact of model specification.

Years since residency	(1) Main estimate	(2) Quad. running variable	(3) Donut sample	(4) Reduced bandwidth	(5) Incl. Afg/ Yugoslav
A) Employment					
1	0.092*** (0.022)	0.098*** (0.035)	0.054*** (0.016)	0.085*** (0.030)	0.100*** (0.018)
2	0.070*** (0.019)	0.070** (0.026)	0.046** (0.020)	0.075*** (0.023)	0.094*** (0.015)
3-5	0.041* (0.021)	0.039 (0.031)	0.045** (0.021)	0.085* (0.030)	0.076*** (0.016)
Observations	4,843	4,843	4,439	3,362	7,456
B) Unemployment					
1	-0.164*** (0.027)	-0.202*** (0.042)	-0.109*** (0.015)	-0.173*** (0.035)	-0.183*** (0.021)
2	-0.158*** (0.028)	-0.188*** (0.045)	-0.098*** (0.020)	-0.188*** (0.035)	-0.190*** (0.023)
3-5	-0.104*** (0.015)	-0.090* (0.023)	-0.097*** (0.020)	-0.083*** (0.020)	-0.141*** (0.011)
Observations	4,843	4,843	4,439	3,362	7,456
C) Not in labor force					
1	0.072*** (0.014)	0.098*** (0.019)	0.056*** (0.006)	0.085*** (0.019)	0.082*** (0.012)
2	0.093*** (0.020)	0.123*** (0.030)	0.056*** (0.009)	0.116*** (0.029)	0.100*** (0.018)
3-5	0.066*** (0.012)	0.056*** (0.015)	0.056*** (0.015)	0.045** (0.017)	0.068*** (0.011)
Observations	4,843	4,843	4,439	3,362	7,456

Note: The table shows robustness tests of the main results for labor market outcomes using alternative specifications. The table shows the estimated effects of being granted residency after the reform relative to before the reform. Column 1 shows the main results as reported in Table 2; Column 2 shows estimates when including a quadratic running variable on each side of the reform along with the linear running variable; Column 3 shows estimates from a donut sample where we exclude two months on each side of the reform; Column 4 shows estimates from using reduced bandwidth of +/- 12 months around the reform; and Column 5 shows estimates from the sample including Afghan and ex-Yugoslavian refugees. All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.12. Estimated reform effect by different conditioning sets

Years since residency	(1)	(2)	(3)
A) Transfers			
1	-10.285*** (0.538)	-9.735*** (0.425)	-9.775*** (0.407)
2	-8.853*** (0.577)	-8.380*** (0.500)	-8.320*** (0.446)
3-5	-5.183*** (0.449)	-5.048*** (0.464)	-4.956*** (0.457)
B) Labor earnings			
1	1.094* (0.614)	1.643* (0.657)	1.144*** (0.400)
2	1.436* (0.750)	1.643** (0.742)	1.567*** (0.541)
3-5	0.576 (0.657)	1.118** (0.496)	1.070** (0.451)
C) Employment			
1	0.090*** (0.029)	0.094*** (0.030)	0.092*** (0.022)
2	0.069** (0.029)	0.076*** (0.030)	0.070*** (0.022)
3-5	0.031 (0.025)	0.045** (0.022)	0.041* (0.021)
D) Unemployment			
1	-0.102*** (0.029)	-0.110*** (0.030)	-0.164*** (0.027)
2	0.168*** (0.043)	-0.165*** (0.041)	-0.158*** (0.028)
3-5	-0.102*** (0.025)	-0.110*** (0.022)	-0.104*** (0.015)
Observations	4,843	4,843	4,843
Running variable	X	X	X
Covariates		X	X
Year fixed effects			X

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on subsequent income from transfers and labor earnings (at the individual level), and the probability of being employed and unemployed for the base sample of adults (aged 18-55 at the time of residency) in year 1, 2, and the average of years 3-5 since residency. The table shows the estimates without any additional controls than the running variable (column 1), controlling for covariates (column 2), and controlling for year fixed effects (column 3) corresponding to the estimates reported in Table 2. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.13. Effect of the reform on employment of labor migrants and refugee migrants.

Years since residency	Labor migrants	Refugees
1	0.016 (0.032)	0.092*** (0.022)
2	-0.004 (0.027)	0.070*** (0.019)
3-5	0.001 (0.020)	0.041* (0.021)
Observations	8,169	4,843

Note: The table shows the estimated effects of the reform for refugees as presented in Table 2 and for labor migrants who are not affected by the reform as they are ineligible for Social Assistance and Start Aid. The sample of labor migrants are defined as non-EU/EEA citizens with work-visa (requiring a pre-existing job-contract in Denmark before migration) and EU/EEA citizens (excluding students). We only include labor migrants' first migration to Denmark in the sample, and define employment as a dummy indicating any employment in Denmark in a given year. Standard errors are clustered by residency month.

* $p < 0.1$; ** $p < 0.05$; *** $p < 0.01$

Table A.14. Comparing main results using linear slopes and triangular weights

	Year 1		Year 2	
	Linear	Triangular (LLR)	Linear	Triangular (LLR)
<i>A) Full sample</i>				
Employment	0.092*** (0.022)	0.091*** (0.030)	0.070*** (0.019)	0.069*** (0.030)
Unemployment	-0.164*** (0.027)	-0.168*** (0.041)	-0.158*** (0.028)	-0.168*** (0.044)
Not in labor force	0.072*** (0.014)	0.076*** (0.020)	0.093*** (0.020)	0.104*** (0.027)
<i>B) Males</i>				
Employment	0.160*** (0.044)	0.169*** (0.059)	0.132*** (0.035)	0.147*** (0.043)
Unemployment	-0.155*** (0.045)	-0.167*** (0.063)	-0.137*** (0.038)	-0.160*** (0.054)
Not in labor force	-0.004 (0.012)	-0.001 (0.014)	0.011 (0.023)	0.019 (0.026)
<i>C) Females</i>				
Employment	0.037 (0.022)	0.043*** (0.015)	0.015 (0.019)	0.025 (0.019)
Unemployment	-0.171*** (0.022)	-0.178*** (0.031)	-0.172*** (0.029)	-0.188*** (0.042)
Not in labor force	0.132*** (0.021)	0.136*** (0.026)	0.162*** (0.025)	0.168*** (0.032)
<i>D) By job openings in low / unskilled jobs</i>				
High demand	0.198*** (0.051)	0.201*** (0.052)	0.214*** (0.046)	0.219*** (0.049)
Low demand	0.127 (0.085)	0.126 (0.091)	0.052 (0.068)	0.047 (0.071)
<i>E) By average employment of non-Western immigrants</i>				
High demand	0.180*** (0.052)	0.181*** (0.052)	0.224*** (0.048)	0.229*** (0.041)
Low demand	0.157** (0.077)	0.157* (0.080)	0.079 (0.059)	0.074 (0.065)
<i>F) Crime (Year 1 and accumulated from year 1-5)</i>				
Males	0.015 (0.015)	0.015 (0.016)	0.049* (0.026)	0.052* (0.028)
Females	0.026** (0.013)	0.026* (0.014)	0.027 (0.017)	0.028 (0.018)

Note: The table compares the main result of the effect of the reform estimated using a linear specification (labelled "linear") with corresponding estimated effects of the reform using triangular weights in the regression discontinuity design (labelled "Triangular LLR"). Standard errors in parentheses. Linear specification results are from Panel A); Table 2; Panels B) and C): Table 3; Panels D) and E): Table 5; Panels F): Table 7.

* p<0.1; ** p<0.05; *** p<0.01

Table A.15. Effect of the reform on employment by assignment municipality, full sample.

	Year 1	Year 2	Years 3-5
A) Using job openings in low / unskilled jobs			
<i>High demand</i>			
Reform effect	0.104*** (0.031)	0.116*** (0.044)	0.055 (0.035)
Pre-reform mean	0.105	0.200	0.350
Post-reform mean	0.209	0.316	0.405
<i>Low demand</i>			
Reform effect	0.074* (0.042)	0.021 (0.040)	0.009 (0.026)
Pre-reform mean	0.102	0.173	0.295
Post-reform mean	0.176	0.194	0.304
B) Using average employment of non-Western immigrants			
<i>High demand</i>			
Reform effect	0.100*** (0.031)	0.113*** (0.034)	0.059* (0.032)
Pre-reform mean	0.113	0.207	0.353
Post-reform mean	0.213	0.320	0.412
<i>Low demand</i>			
Reform effect	0.082* (0.041)	0.035 (0.043)	0.010 (0.028)
Pre-reform mean	0.095	0.170	0.295
Post-reform mean	0.177	0.205	0.305
Observations	4,843	4,843	4,843

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform separately for refugees assigned to municipalities with high / low local labor demand on the subsequent probability of being employed, measured for adults aged 18-55 at the time of residency in year 1, 2, and the average of years 3-5 since residency. The table also shows pre-reform means and post-reform means (pre-reform mean + reform effect) of the outcome variables. High / low labor demand is defined in Panel A as being assigned to a municipality with above/below median of the predicted ratio of the number of job openings in low / unskilled work relative to the number of unemployed individuals, and in Panel B as being assigned to a municipality with above/below median employment rate of non-Western immigrants in 1999-2001. Standard errors are clustered on twoway level by residency month and allocation municipality.

* p<0.1; ** p<0.05; *** p<0.01

Table A.16. Effect of the reform on employment by assignment municipality without controls and with controls for individual and municipality characteristics (as in Table 5).

	Year 1	Year 2	Years 3-5
A) Using job openings in low / unskilled jobs			
<i>High demand</i>			
Reform effect without controls	0.198***	0.214***	0.113***
	-0.051	-0.046	-0.043
Reform effect mun. and individual characteristics	0.184***	0.207***	0.097**
	(0.048)	(0.045)	(0.042)
<i>Low demand</i>			
Reform effect without controls	0.127	0.052	-0.023
	(0.085)	(0.068)	(0.032)
Reform effect mun. and individual characteristics	0.125	0.042	-0.032
	(0.078)	(0.055)	(0.026)
B) Using average employment of non-Western immigrants			
<i>High demand</i>			
Reform effect without controls	0.180***	0.224***	0.119***
	(0.052)	(0.048)	(0.042)
Reform effect mun. and individual characteristics	0.163***	0.206***	0.096**
	(0.048)	(0.048)	(0.044)
<i>Low demand</i>			
Reform effect without controls	0.157**	0.079	-0.007
	(0.077)	(0.059)	(0.039)
Reform effect mun. and individual characteristics	0.157**	0.068	-0.013
	(0.071)	(0.051)	(0.030)
Observations	4,843	4,843	4,843

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform separately for refugees assigned to municipalities with high / low local labor demand on the subsequent probability of being employed (for adults aged 18-55 at the time of residency) in year 1, 2, and the average of years 3-5 since residency while controlling for individual characteristics (see Table 1) and municipality population size, the share of non-Western immigrants, voting share on anti-immigrant parties, voting share on right-wing government, population density, and including region fixed effects (regions are: 1 municipalities close to Copenhagen; 2 the remaining Greater Copenhagen area; 3 the remainder of Zealand; 4 Funen; 5 South Jutland; 6 West Jutland; 7 East Jutland; 8 North Jutland). The table also reproduces estimates from Table 5 for comparison. Standard errors are clustered on twoway level by residency month and allocation municipality.

* p<0.1; ** p<0.05; *** p<0.01

Table A.17. Effect of reform on males' labor earnings in the average of years 3-5 by assignment municipality.

	(1)	(2)	(3)	(4)
	<i>Reform effect full sample</i>	<i>Reform effect low demand</i>	<i>Reform effect high demand</i>	<i>Difference high- low demand</i>
A) Labor earnings, \$1,000	1.753** (0.842)	-2.199 (1.500)	5.035*** (1.516)	7.234*** (2.506)
B) 1[Labor earnings \$0-1,499]	-0.033 (0.035)	0.116*** (0.049)	-0.155*** (0.060)	-0.271*** (0.086)
C) 1[Labor earnings \$1,500-2,999]	0.027 (0.038)	-0.048 (0.037)	0.090 (0.057)	0.138** (0.066)
D) 1[Labor earnings \$3,000-4,499]	0.008 (0.025)	-0.011 (0.047)	0.021 (0.029)	0.032 (0.057)
E) 1[Labor earnings \$4,500 and above]	-0.002 (0.013)	-0.056*** (0.019)	0.044** (0.021)	0.101*** (0.031)

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on males' average labor earnings in years 3-5 in Panel A, and the probability of having average labor earnings in years 3-5 from \$0-1,499 in Panel B, \$1,500-2,999 in Panel C, \$3,000-4,499 in Panel D, and \$4,500 or above in Panel E. Column 1 show the reform effects for all males, and columns 2 and 3 show the reform effects for males according to whether they were assigned to a municipality with above/below median of the predicted ratio of the number of job openings in low/unskilled work relative to the number of unemployed individuals. Column 4 shows the difference between reform effects in high and low demand municipalities (column 3 minus column 2). Standard errors are clustered on twoway level by residency month and allocation municipality, except in Column 1 where standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.18. Reform effect on refugees' outcomes by different specifications.

	(1)	(2)	(3)	(4)	(5)
A) P(crime), year 1-5, adults					
Reform effect	0.035** (0.013)	0.032** (0.013)	0.033** (0.013)	0.036** (0.014)	0.031** (0.013)
B) Number of crimes, year 1-5, adults					
Reform effect	0.054*** (0.019)	0.050*** (0.018)	0.053** (0.020)	0.054** (0.020)	0.050** (0.019)
Year of residency fixed effects		X	X	X	X
Observable characteristics			X		
Donut around reform				X	
Reduced bandwidth					X

Note: The table shows robustness tests of the main results for crime using alternative specifications. The table shows the estimated effects of being granted residency after the reform relative to before the reform. Column 1 shows the results estimated without any controls except the running variables; Column 2 includes year of residency fixed effects; Column 3 controls for observable characteristics (see Table 1); Column 4 shows estimates from a donut sample where we exclude two months on each side of the reform; Column 5 shows estimates from using reduced bandwidth of +/- 12 months around the reform. Standard errors are clustered by residency month.

* p<0.1; ** p<0.05; *** p<0.01

Table A.19. Effects of reform on crime and the probability of having monthly disposable income below \$500, \$750, and \$1,000, respectively, by local labor demand.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
	P(crime)				Low disposable income								
	<i>All crime</i>	<i>Property</i>	<i>Theft from superm.</i>	<i>Violence</i>	<i>Disposable income < \$500</i>			<i>Disposable income < \$750</i>			<i>Disposable income < \$1,000</i>		
	Year 5	Year 5	Year 5	Year 5	Year 1	Year 2	Year 3-5	Year 1	Year 2	Year 3-5	Year 1	Year 2	Year 3-5
A) Low demand													
Reform effect	0.041*	0.037**	0.027*	-0.002	0.336***	0.263***	0.140***	0.459***	0.292***	0.206***	0.532***	0.393***	0.275***
	(0.021)	(0.018)	(0.015)	(0.011)	(0.040)	(0.035)	(0.031)	(0.042)	(0.036)	(0.044)	(0.054)	(0.042)	(0.051)
Pre-reform mean	0.102	0.058	0.036	0.010	0.032	0.028	0.013	0.081	0.055	0.036	0.191	0.114	0.074
B) High demand													
Reform effect	0.030*	0.028	0.021	0.007	0.327***	0.238***	0.084***	0.513***	0.362***	0.143***	0.624***	0.437***	0.246***
	(0.017)	(0.020)	(0.013)	(0.009)	(0.046)	(0.041)	(0.030)	(0.038)	(0.039)	(0.028)	(0.041)	(0.056)	(0.033)
Pre-reform mean	0.089	0.058	0.039	0.014	0.014	0.018	0.014	0.034	0.030	0.024	0.134	0.091	0.064
C) High-low difference in reform effect													
	-0.010	-0.009	-0.006	0.009	-0.009	-0.025	-0.057*	0.055	0.070	-0.064	0.092**	0.045	-0.029
	(0.025)	(0.026)	(0.017)	(0.989)	(0.053)	(0.051)	(0.030)	(0.060)	(0.056)	(0.040)	(0.045)	(0.078)	(0.054)
Observations	3,406	3,406	3,406	3,406	3,406	3,406	3,406	3,406	3,406	3,406	3,406	3,406	3,406

Note: The table shows reform effects on and pre-reform means of the probability of having received a crime conviction accumulated from residency until year 5 (columns 1-4), the probability of having post-tax disposable income below \$500 per month (columns 5-7), the probability of having post-tax disposable income below \$750 per month (columns 8-10), and the probability of having post-tax disposable income below \$1,000 per month (columns 11-13). The outcomes in columns 5-13 are defined by dividing annual disposable income (in year 1, year 2, and year 3-5, respectively) by 12 thereby expressing the average income in each month in that year. Panel A shows results for low demand municipalities, Panel B shows results for high demand municipalities, and Panel C shows the differences between estimates in Panels A and B. High/low labor demand is defined as being assigned to a municipality with above/below median of the predicted ratio of the number of job openings in low/unskilled work relative to the number of unemployed individuals. The table shows results for all adults aged 18-45 years at the time of residency with children (as in Table 8). Standard errors are clustered on twoway level by residency month and allocation municipality.

* p<0.1; ** p<0.05; *** p<0.01

B. Appendix for Online Publication, Additional documentation:

B.1 The Asylum Process

Most individuals who request asylum in Denmark do so after having entered the country as undocumented migrants. After making the request, applicants are transferred to the Sandholmlejren reception center. While the Danish Immigration Service (DIS) processes their applications, it covers their living expenses and provides health care. If Denmark is responsible for the application according to the Dublin Convention, the applicant is transferred to an accommodation center located around the country. The process from the asylum application to the final decision consists of two main steps (see Hvidtfeldt et al., 2018). First, the DIS assesses the conditions in the country of origin to determine whether refugee status is warranted. This may take several months, and in some cases also involve “fact-finding missions” to specific countries and regions. Once this step has been completed, a caseworker from the DIS interviews the applicant in the second step. The timing of this interview depends on the current caseload and availability of interpreters. The caseworker may also decide that additional interviews are required to assess the applicant’s case. If the application is rejected, it is automatically referred to the Danish Refugee Appeals Board for review and a final decision.

Married applicants are each assigned a separate asylum case ID and processed individually even if they apply together on the same day. During our study period, the full application process for those granted residency was about 15 months on average, but, as described above, there was considerable variation in processing times according to individual circumstances and immigration agency workload. Those seeking asylum in Denmark at the time of the Start Aid reform came from a variety of countries, but mainly from Middle Eastern and North African nations.

Upon receipt of residency, refugees are allocated to a municipality. The municipality is then responsible for finding suitable accommodation and enrolling the refugees into an integration program. These integration programs, which begin immediately after residency is granted, are meant to assist refugees to find employment. They consist of two compulsory components: (i) lessons in the Danish language and cultural education courses throughout the week, and (ii) active labor market programs. Within the first week after residency, each refugee receives specific individual guidelines for course participation and activation requirements, which are revised and adapted every three months. The two main program components take up at least 30 hours per week, although there is variation in the weekly workload. During employment spells, the obligation to comply with the program is discontinued but resumes in case of new unemployment spells.

B.2 Data Construction and Definitions

Our analysis is based on several register data sets. We start with the Danish Immigration Service's records (Ministry of Immigration and Integration, 2011), extract all permits given to refugees, and merge these data with exact information on when refugees were granted residency, their country of origin, and whether and when they left Denmark again (Statistics Denmark, 2022a, b, d, k, n). From this, we obtain our study sample of adults, spouses, and adolescents.

The income register (Statistics Denmark, 2022f) contains annual information on labor earnings, transfer income, and tax payments. Because Denmark has full third-party information (i.e., all income is reported directly by its issuers), the income data encompass all legal income. For our analysis, we consider four main types of income measured from the first year post residency onward: labor earnings (measured pre-tax), transfers (measured pre-tax), pre-tax gross income (which for our sample equals labor earnings plus transfers), and post-tax disposable income (which equals pre-tax gross income minus tax payments). We also use these data to obtain public expenditures for refugees, which we define as transfer payments minus tax payments. We supplement the income variables with register data on hourly wage rates estimated using annual labor earnings divided by annual hours worked.

Labor market status (Statistics Denmark, 2022c, e, l) consists of three mutually exclusive states: *employment*, *unemployment*, and *not in the labor force* (NILF), as defined by the International Labour Organisation (ILO). We categorize occupations into seven categories based on type and skill intensity (using ILO's International Standard Classification of Occupations): i) high level of skills / manager, ii) medium level of skills / office related, iii) sales / services, iv) vocational work requiring medium / basic skills, v) construction / primary sector work requiring basic skills, vi) unspecified self-employment, and vii) unskilled manual labor requiring few / no skills. When we consider jobs requiring some skills vs. unskilled manual labor, the former consists of categories i)-vi) and the latter of vii).

The crime data (Statistics Denmark, 2022g, h, i, j) is based on information from the criminal courts and the police collected by the Ministry of Justice and Statistics Denmark. The data include exact information on offense dates, as well as charges, arrests, incarcerations, and convictions. Each entry contains unique case-specific and individual-specific identifiers that allow us to match each crime to individuals in our sample. We thus measure individual criminal activity based on convictions for offenses against the criminal code, which the Central Police register categorizes under specific labels (e.g., "theft from supermarket"). Our preferred measure of criminality, crime conviction, which we

always relate to the date on which the crime was committed, refers to court rulings (or pre-court settlements) of the suspect's guilt that result in a sentence (either a fine, suspended sentence, or imprisonment). We measure crime by the exact date of the crime, so that "crime in year 1," for example, is crime committed within the first 365 days after residency is granted.

We construct two municipality-level indicators of local labor demand. I) *The number of job openings in low- and unskilled work relative to the number of unemployed individuals in the municipality* is computed by dividing the number of job openings posted in each municipality for low- and unskilled work by the number of unemployed individuals in that municipality in 2002 and 2003 and then taking the average for those two years. We obtain the number of job openings at the postal code level from Denmark's first job-portal (www.jobindex.dk), which starts in 2002 (Jobindex.dk, 2018). We count job openings in low- and unskilled work and aggregate the individual postal codes to the municipal level. We then divide this number by each municipality's stock of unemployed individuals, which we calculate by combining information on municipality of residence from the full population register with individual level unemployment information from the labor market register. This gives us the ratio of job openings for low- and unskilled jobs relative to job-searchers. As the job portal data only allows us to measure the average numbers of job openings in each municipality *after* the reform (2002 and 2003), we use local employment conditions in the same municipality before our sample window (1999-2001) to predict the number of job openings in low- and unskilled work relative to the number of unemployed individuals in the municipality over that period.¹ Specifically, we regress the average ratios of job openings to unemployed within each municipality for years 2002 and 2003 on employment conditions in the same municipality measured in years 1999-2001 and compute the predictions (but estimates are very similar when we instead use the actual job openings in 2002-2003).² Based on these, we then rank municipalities from 0-1, with 0 being the municipality with the lowest predicted number of job openings and 1 the municipality with the highest predicted number of job openings per unemployed individual. In our analysis, we distinguish municipalities by being above or below the (unweighted) median (Tables 5, 8, A.6, A.15, A.16, A.17, and A.19, and Fig. 6) and across the full range of percentiles (Table 6).

II) *The average employment rates of non-Western immigrants*, which we study to confirm the robustness of our results. We construct this as the fraction of 20–60-year-old non-Western immigrants

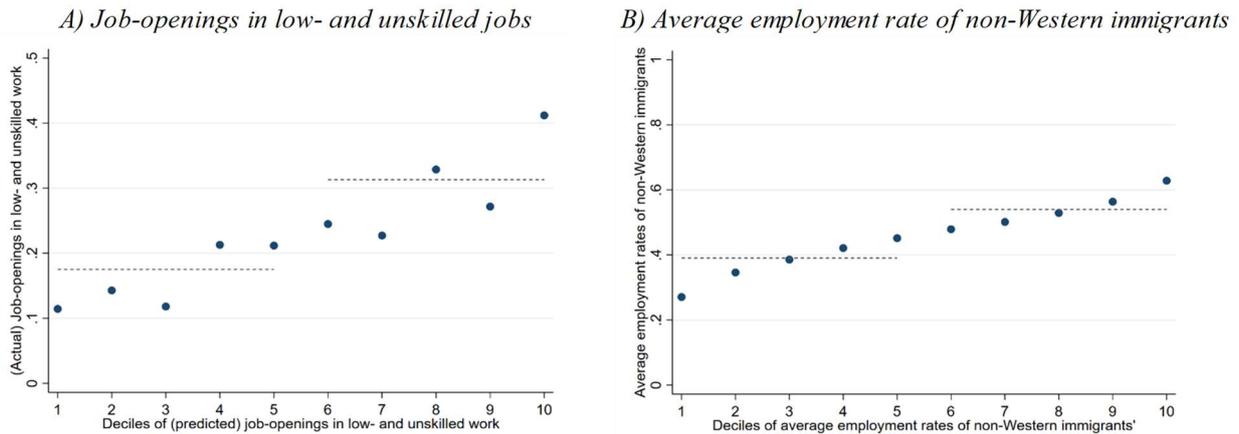
¹ There is strong persistence over time in municipalities' actual number of job openings relative to the number of unemployed, with correlation coefficients ranging from 0.95 one year apart to 0.62 ten years apart.

² We regress job openings on municipal average employment rate, non-Western immigrants' employment rate, unskilled individuals' employment rate, and immigrants' average labor earnings, and these variables interacted with each other.

in a municipality who are in employment during the years 1999-2001. As most non-Western immigrants are former refugees and similarly skilled to the refugees we consider here, this variable is likely to capture the availability of the type of jobs refugees are qualified to fill. Moreover, as refugee dispersal and subsequent settlement patterns were in previous years unrelated to employment prospects (see e.g., Nielsen and Jensen, 2006), non-Western immigrants' employment rates across municipalities can be expected to largely reflect variation in labor demand. We construct this variable from the full population registers, which provide for each individual information on country of origin, age, and municipality of residence for years 1999-2001, where the data is recorded on January 1st of each year. We select non-Western immigrants and merge this data with individual level employment information from the labor market registers to construct non-Western immigrants' average employment rates. We then rank municipalities from 0 to 1 according to this average employment rate. In our analysis, we separate municipalities by the median (Tables 5, A.6, A.15, A.16, and A.17).

We refer to municipalities with local labor demand indicators above median as *high-demand municipalities*, and municipalities with local labor demand indicators below median as *low-demand municipalities*. Fig. B.1A plots actual job openings in low- and unskilled work against the deciles of the predicted job openings in low- and unskilled work. The figure shows substantial variation in job openings in low- and unskilled work across municipalities, with around 0.1 (0.4) job openings per unemployed in the lowest (highest) deciles. Moreover, when we separate the sample by the median, the horizontal dashed lines in Fig. B.1A show that low-demand municipalities have around 0.17 job openings in low- and unskilled work per unemployed, compared to high-demand municipalities with around 0.32 job openings in low- and unskilled work per unemployed. Fig. B.1B shows similar associations between deciles and average employment rates of non-Western immigrants for 1999-2001. The average employment rate is below 0.3 for the lowest decile, but around 0.6 for the highest decile. Furthermore, as again illustrated by the horizontal dashed lines in the figure, when we separate municipalities by the median, we compare low-demand municipalities with average employment rates of non-Western immigrants around 0.39 to high-demand municipalities with average employment rates of non-Western immigrants around 0.55.

Figure B.1. Variation in the two local labor demand indicators across municipalities.



Note: The figure shows variation in the two local labor demand indicators across municipalities. Panel A shows the average actual job-openings per unemployed in low- and unskilled jobs across ranks of predicted job-openings in low- and unskilled jobs. Panel B shows the average employment rate of non-Western immigrants across ranks of the same measure. The horizontal dashed lines indicated the average of the two measures when separated by the median.

For analysis of the effects of the repeal of the Start Aid reform that affected all refugees from January 1st, 2012, we focus on adults who received residency between 2008 and 2014 and were aged between 18 and 55 at the time of receiving residency (as in our main sample). For these individuals, we obtain information on characteristics such as date of residency and demographic background information from the same data sources as for the main sample (see description earlier in this section).

Because the repeal was implemented for *all* refugees at the same point in time, everyone is affected by it, but different entry cohorts are exposed after different durations of residency in Denmark. When estimating the effects of the repeal on employment responses, we aim to measure outcomes as closely to the residency date as possible, as time spent under different schemes would dilute the effects. We consider employment outcomes only for two years after residency. We define employment as having non-zero labor earnings within a given calendar year. We define crime as described above.

We study the effects of the repeal in an event study analysis comparing refugees' employment (crime) rates in the years before the repeal with refugees' employment (crime) in the years after the repeal. For example, the cohort that received residency in 2011 would receive Start Aid transfers during the first year in Denmark, but full SoA transfers in the second year. Therefore, when we measure employment (crime) in the first year after residency we define as the treatment group those granted residency in 2012-2014 (who all had their first year following their residency decision after the repeal). Likewise, when we consider the effects on employment (crime) in the second year after residency, we define the treatment group as those granted residency in 2011-2013.

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