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Refugee Benefit Cuts

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Abstract: This paper analyzes the effects of Denmark's Start Aid welfare reform that targets refugees. Implemented in 2002, it enables us to study not only the reform's immediate effects, but also its longer-term consequences, and its repeal a decade later. The reform-induced large transfer cuts led to an increase in employment rates, but only in the short run. Overall, the reform increased poverty rates and led to a rise in subsistence crime. Moreover, local demand conditions generate substantial heterogeneity in the reform's effects on immediate and longer-term employment. [87 words]

Keywords: Social assistance, welfare state, labor market outcomes, labor demand, migration.

JEL: E64, I30, J60

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1. Introduction

In response to recent large immigration flows and a sharp rise in anti-immigration sentiment, many governments are restricting access to welfare benefits for refugee immigrants.[1] For instance, in 2014, Canada took measures to limit immigrant access to social assistance (following a first round of cuts in 2012), and in 2016 and 2019, Germany limited access to social benefits and reduced levels for groups of refugees.[2] These reforms are often justified as a means to incentivize labor force participation, but not much research exists that investigates their effects, partly because lack of data due to their recent implementation. Moreover, immediate effects of such reforms on employment, earnings, and labor market participation may differ from long term consequences, about which we know even less, while reform design may induce unanticipated disincentives amplified by traditional role behavior in refugee households. Finally, unfavorable labor demand conditions for the type of work low-skilled individuals supply may counteract reform incentives.[3] This is particularly relevant for refugees who are often unprepared for the labor market of the country that provides protection (Fasani et al., 2021).

This paper provides critically needed evidence on these issues, by analyzing the effects of Denmark's Start Aid welfare reform that intended to *"ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly*" (Danish Parliament L126, 2002). The reform reduced welfare benefits for refugees with asylum claims approved after July 1st, 2002, by around 40% compared to the previous social assistance (SoA) level. While sharing many of the features of other more recent reforms and reform proposals targeting refugee immigrants, Start Aid was implemented in 2002, which allows us to study not only its immediate effects, but also its repeal 10

[1] A refugee is an asylum seeker whose asylum application has been approved and who has thus been granted residency and entitlement to welfare benefits (Hatton, 2020).
[2] Other policies implemented include restriction of immigrants'/refugees' access to social assistance and public benefits in Canada (CBC, 2014), Finland, France, Latvia, Lithuania, the Netherlands (OECD International Migration Outlook 2017, 2018), and Switzerland (Swissinfo, 2017); transfer cuts in Austria, Sweden, and Germany (OECD International Migration Outlook 2019, 2020); and further adjustments of transfer levels and eligibility in Denmark in 2015 and 2018. More generally, from 2000-2019, EU27 countries passed 176 bills on refugee and migrant welfare eligibility, program requirements, or welfare levels (OECD International Migration Outlook 2006-2020; OECD Trends in International Migration 1997-2004).
[3] See Brell et al. (2020) for evidence.

years later, and its longer-term consequences for refugees and their families. In addition, in its implementation phase the reform quasi randomly allocated households across two different support allocation schemes that were equivalent in overall benefit payments but created different incentives, mostly for females, for participation in integration programs and the labor market. This offers opportunity to study how small design differences impact on outcomes for these populations. Moreover, the reform was implemented during a period when refugees were quasi-randomly allocated across municipalities. This provides us with a second research design to study the causal effect of local labor demand conditions on the effects of the reform, which is otherwise typically impossible due to sorting of target populations across local labor markets.

We show that the reform doubled average labor earnings and increased employment rates in its immediate aftermath, while its repeal a decade later (which *increased* transfers to the pre-reform level in 2012) had the exact opposite effect, underscoring the robustness of the short run result.[4] However, the short run effects did not carry over to the longer run, with both average labor earnings and employment effects fading out quickly and being close to zero five years after reform implementation. Conclusions about a policy's effects drawn from average short-term labor market outcomes are thus not indicative for the overall and longer-term impact – a finding that complements the long strand of literature studying the labor supply effects of welfare reforms and means-tested transfers (e.g., Eissa and Liebman, 1996; Hoynes, 1996; Meyer and Rosenbaum, 2001; Moffitt 2002, 2015).

We identify two channels that attenuate the reform's effects on refugees' employment and impede their labor market integration. First, the combination between the reform and the household-level means test led more females to drop out of the labor force because they became ineligible for transfers when their husband took up employment; a finding that underscores the importance of considering within-household incentives (e.g., Eissa and Hoynes, 2004) when designing transfer policies.

[4] Our estimates on the immediate impact of the reform are similar to those of earlier short-term evaluations of the Start Aid reform, see Huynh et al, (2007) and Rosholm and Vejlin (2010).

Moreover, this disincentive for second earners was enhanced by a specific feature of the reform's implementation that implied that in some households, transfers to both partners were paid to one spouse only (typically the male), which removed labor force participation incentives for the other spouse in the same way as the household-level means testing did. This doubled labor force exits of females, a sizeable response that may be partly due to views about female labor force participation in traditional refugee communities, illustrating that responses in minority populations may differ from those expected in majority populations, as also found in Dahl et al. (2020). More generally, these findings demonstrate the sensitivity of reform effects and estimated labor supply elasticities to small variations in reform designs (see e.g., Chetty et al., 2011; Kleven and Schultz, 2014; Saez et al., 2012).[5]

Second, using the quasi-random allocation of refugees across Denmark's 270 municipalities as a second design, we show that local labor demand for the type of work refugees can supply is indeed essential for the reform's outcomes. While employment effects disappear after one year for refugees allocated to municipalities with low demand, they remain significant until year 5 after residency for those allocated to municipalities with high demand. Moreover, the reform induced take-up of employment in lower quality jobs with lower job stability in low demand municipalities but led to more persistent and higher quality employment relationships in high demand municipalities. Overall, the reform increased refugees' average income from labor earnings during the first five years by almost 40% and reduced public expenditures by 60% in municipalities with the highest labor demand, whereas there were no significant changes to income from labor earnings in low demand municipalities, and public expenses only declined by 35%. These estimates constitute a first causal assessment of the sensitivity of reform effects to local demand conditions. Our findings not only call into question the common policy of equally distributing refugees across regions, but also speak directly to previous studies that have linked local labor demand to welfare use (see e.g. Hoynes, 2000; Black et al. 2003),

[5] We illustrate that the heterogeneous household-level responses on employment and labor force participation follow exactly what would be predicted in a simple static labor supply framework (as in e.g., Bitler et al., 2006 and Lemieux and Milligan, 2008).

and an active literature that discusses whether effects of welfare reforms and employment regulations are confounded by business cycles (e.g., Ziliak et al., 2000, Lemieux and Milligan, 2008, Ganong and Liebman, 2018, Kleven, 2019, Fasani et al., 2021).[6]

Overall, the Start Aid welfare reform lowered benefits to refugee immigrants by 40%, a shortfall that could only partly be compensated by higher labor supply, so that the majority witnessed a dramatic reduction in disposable income, with the share of individuals falling below the poverty line increasing from close to zero pre-reform to almost 50% post-reform. We show that this severe reduction to disposable income is accompanied by a sharp rise in crime, in particular subsistence crime (e.g., grocery store shoplifting). The crime increase is particularly notable for females, a group with otherwise low crime rates. These findings contribute to the few studies that associate crime with either welfare payment timing (e.g., Foley, 2011; Carr and Packham, 2017), welfare eligibility of youths (Deshpande and Mueller-Smith, forthcoming) and criminal offenders (Yang, 2017), and/or state variation in welfare reform implementation in the U.S. (Corman et al., 2014).

2 Background and Data

2.1 Social Assistance, the Start Aid Reform, and Benefit Eligibility

Denmark's social assistance (SoA) benefits are among the most generous in the world and the country once had some of the most liberal refugee immigration laws (Andersen et al., 2012; Huynh et al., 2007; Pedersen, 2013). By 2001, because of large inflows of individuals with high levels of welfare uptake, net welfare transfers to non-Western immigrants reached 0.83% of the GDP and 3.4% of total public spending (Matthiessen, 2009). On March 1, 2002, a newly elected Danish government proposed a bill that replaced SoA for refugees with a new Start Aid benefit scheme intended to promote their labor

[6] Azlor et al. (2020), Damm and Rosholm (2010), and Åslund and Rooth (2007) find that the economic conditions at initial allocation affect immigrants' subsequent labor market outcomes. While our analysis focusses on the interaction between local labor market conditions and the welfare reform, we also confirm these earlier studies' findings and complement them further by showing how local labor demand affects labor earnings and job types.

market participation (Danish Parliament, 2002).[7] Approved on June 6 and implemented on July 1, the reform assigned all refugees granted residency after the reform date to the Start Aid program, whose transfers were approximately 40% lower than SoA payments (rates are based on age and family type; the reform lowered transfer rates by 40% on average when we weight the pre-post reform changes by our sample composition, see Table A.1.[8] The Start Aid program was in effect until January 1st, 2012, when it was repealed following a change in government.

To receive residency, refugees must first request asylum, which most do after entering the country as undocumented migrants. Once asylum is requested, the applicant is transferred to a central reception center. After the formal application process begins, the Danish Red Cross assigns the refugee to an accommodation center (refugee camp) while the application is processed by the Danish Immigration Service. Refugees are not allowed to work before their residency is approved (implying that *all* refugees become welfare recipients once they receive residency), and the centers provide both food (either directly or via food stamps) and health care. There is no cap on the number of residencies granted within a specific period, and the application process takes on average 15 months during the period we study (Hvidtfeldt and Schultz-Nielsen, 2018, Fig. 6.1), which, due to the reform's very short implementation period, effectively randomizes individuals already in Denmark to Start Aid or SoA based on when they are granted residency around the reform implementation date.[9] The timing of residency around the reform thus provides a clean identification of the reform's effects (as detailed in Section 3).

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[7] "To ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly, the incentives for finding employment must be strengthened" (author translation of official remarks: http://webarkiv.ft.dk/Samling/20012/lovforslag_som_fremsat/L126.htm, accessed 03-20-2017). Appendix B.1 provides a more detailed description of the background and the Start Aid reform.

[8] Start Aid levels (pre-tax) are on par with median Temporary Assistance for Needy Families and Supplemental Nutrition Assistance Program levels in the U.S. (TANF: Falk 2014 who reports levels by state and year; SNAP: http://kff.org/other/state-indicator/avg-monthly-food-stamp-benefits).

[9] The specific waiting time for an individual refugee depends mainly on the caseload of asylum applications at that given point in time and the information available to Danish authorities relating to the conditions in the countries of origin (that is, if the Danish Immigration Service needs to search for additional documentation before the case can be processed), cf. Hvidtfeldt et al. (2018).

2.2 Refugee Allocation across Local Labor Markets

To study how local labor market conditions interact with the reform, we use a quasi-random allocation scheme for refugee placement that was in effect during the implementation of the reform. Upon being granted residency to Denmark, the Danish Immigration Service allocated refugees to each of Denmark's 13 counties. Each refugee was then assigned to a municipality, following a pre-determined quota system.[10] Counties and municipalities had no information about the refugees' characteristics when their quotas are set for the year to come, and county and municipality officials were only informed about the country of origin and whether refugees have family members who already live in a specific municipality. This effectively made it impossible to cream-skim based on, for example, refugees' employment prospects, and from the refugees' perspective, the assignment was as-good-as-random. We show in Section 4 that refugees' characteristics are not associated with local labor market indicators. Following assignment, refugees were required to remain in their assigned municipality for a minimum of three years to receive transfers. The vast majority stayed in the municipality of assignment even in the longer run, irrespective of local employment prospects (we validate this for our sample below; see also Nielsen and Jensen, 2006).

2.3 Eligibility, Household Entitlements, and Reform Implementation

Eligibility for both SoA and Start Aid is conditional on participation in an integration program, which comprises courses in the Danish language and Danish society and acculturation, as well as active labor market programs.[11] Failure to comply with these obligations results in immediate transfer ineligibility.

[10] The allocation of refugees across municipalities is in proportion to population size. In 2016, vice chairman of the Danish Municipalities' Association, Jacob Bundsgaard, commented on the allocation: "Today, it is basically completely random where refugees are allocated. But as a prerequisite for integration is that one joins the workforce, we suggest that the match between refugee characteristics and municipal labor markets is considered" (author's own translation).

[11] The integration program consists of two main components. The first is mandatory Danish and cultural lessons several times per week, and the second is active labor market programs. The two components together take up about 30 hours per week. The obligation to comply with the integration program discontinues during employment spells but resumes in case of new unemployment spells (see the Law of Integration of Immigrants in Denmark: https://www.retsinformation.dk/Forms/R0710.aspx?id=28907#K4.

Being the lowest tier of the Danish welfare system, SoA and Start Aid receipt has no time limit as long as recipients satisfy the rules for integration program participation.

SoA and Start Aid are means tested, and for couples, the means test is at the household level. Hence, not only do refugees lose their own SoA or Start Aid because of labor earnings, any labor earnings from the first earned dollar onward reduces the benefits of both partners. Means testing thus works as a household-level "negative income tax" that provides strong extensive margin disincentives, which will be central for understanding females' responses to the reform. In addition, the reform was implemented in two distinct ways, according to the residency dates of each spouse. As such, in our analysis of couples' labor supply responses in Section 4.3, we classify couples into three groups.[12] If both spouses received residency pre-reform, both are entitled to SoA. These couples constitute our reference category. If both received residency post-reform, both are entitled to Start Aid. We refer to these as "Type A" couples. If one spouse received residency pre-reform and the other post-reform, their combined benefits are capped at two times Start Aid with the first arriving spouse keeping the full SoA and the last arriving whatever may be left. We refer to these as "Type B" couples. Because SoA is almost twice as high as Start Aid (although with variation across household types, cf. Table A.1), the last arriving spouse in Type B couples is effectively ineligible for any benefits.[13] One important implication of this allocation scheme is that last arriving spouses in Type B couples cannot be (heavily) penalized for nonparticipation in integration programs as the individual has no (or only very few) benefits to cut.

[12] Married applicants are each assigned their own asylum case ID and processed individually even if they apply together on the same date. In our sample, 18% of the married couples have the same application and approval dates, around 1% have the same application date but different approval date, 15% have different application dates but the same approval date, and 67% share neither application nor approval dates. Unmarried couples are processed as two single individuals having independent case processing times.

[13] The average transfer reduction was 40%, and largest for couples, with reductions ranging between 40 and 50% (see Table A.1). Thus, when transfers were capped at two times Start Aid at household level, the last arriving spouse in Type B couples was either ineligible for any transfers or only eligible for $30-150 per month (and only if they followed the integration courses which take up around 30 hours per week).

Fig. 1 illustrates how labor earnings translate into pre-tax gross income when transfers are reduced due to the means test for pre-reform, Type A, and Type B couples, respectively.[14] The means test on SoA and Start Aid implies an effective marginal tax rate of between 83% and 100% on any labor earnings below a break-even point (the point at which there is no SoA or Start Aid left to means test).[15]

To respond to the incentives, refugees need to be aware not only of means testing and household-dependent variations but also of benefit caps and the effects of integration program noncompliance on benefit eligibility. The municipality of residence is obliged, both in physical meetings (with an interpreter when required) and written communication (sample letters to welfare recipients are available upon request), to explain to potential welfare beneficiaries such issues as (i) compulsory participation in an integration program, (ii) the withholding of transfers for noncompliance, and (iii) the limiting of transfers for Type B couples to the spouse granted residency first.[16]

2.4 Data and Samples

Our sample consists of refugees whose treatment status (pre- or post-reform) is determined by the exact date on which residency was awarded.[17] To derive information on this sample's labor market outcomes (including employment status, income, and occupation) and demographic characteristics (including age, gender, education level, and date of birth), we use register data recorded by public agencies and

[14] The vertical difference between the solid (pre-reform couples) and dotted/dashed (Type A and B couples) lines in the intersections with the y-axis at zero labor earnings in Fig. 1A shows the monthly benefit reduction induced by the reform with the slopes representing the means testing rates. Because 91% of couples in our sample have children, we use a one-child family as a benchmark for couples' transfers. Table A.1 shows the extensive margin implied marginal tax rates and the break-even points by family type. Transfers are subject to the same income taxes as labor earnings, which we factor in when calculating marginal tax rates in Fig. 1B. The means testing rates for singles correspond to those of Type A couples at half of their transfer level and break-even point. The implied marginal tax rates are 93.5% and 82.1% for Type A and B couples, respectively. All income values reported in the paper are in 2010 PPP adjusted USD (1$=7.76DKK).

[15] For pre-reform couples, the break-even point is at around $3,000 per month, while for Type A and B couples, it is about $2,000 and $1,500, respectively. The break-even point for Type B couples is lower because it combines pre- and post-reform features. Although total household transfers when the spouses are not working is the same as if both were on Start Aid (Type A), all household transfers are paid as SoA to one spouse in type B couples. Hence, the discount from means testing equals the pre-reform SoA discount, resulting in a monthly break-even point that is around $500 lower than for Type A couples. The low bracket marginal tax rate of 44% applies to those with labor earnings above the break-even point.

[16] Danish authorities are required by Administrative Law, section 7, no. 1 to ensure that citizens and refugees have understood the rules and regulations that pertain to their benefit reception, as well as any changes to their entitlements.

[17] Our sample includes only refugees and individuals who are family reunified with refugees, because labor migrants, their families, and other nonrefugee migrants are ineligible for SoA or Start Aid and thus unaffected by the reform.

then compiled and organized by Statistics Denmark. Because this database assigns unique personal identification numbers to individuals, their spouses, and their parents, we can merge the information for an individual with that of the rest of their family to construct records for each household.

Our initial sample comprises 8,512 individuals granted residency (via a refugee status or family reunification) between January 1, 2001, and December 31, 2003, at ages 18 to 55. Two temporary changes to case processing procedures happened in the months preceding the reform as a result of contemporaneous conflicts. First, following the fall of the Taliban regime in late 2001, the Danish Immigration Service suspended processing of new applications by Afghans in late January 2002 (Refugee Appeals Board, 2002, p. 142) until the situation in Afghanistan had been investigated. This led to a large drop in residency permits issued to Afghans around the reform. Second, following the NATO bombings in 1999 and the subsequent installment of NATO forces (KFOR), Kosovo was reclassified as a "safe zone" by Danish courts in the spring of 2002 (Refugee Appeals Board, 2002, p. 114). While unrelated to the Start Aid reform, these administrative alterations nonetheless resulted in a sudden change in the number of residencies granted to refugees from these countries that largely coincided with the introduction of the reform. We therefore exclude refugees from Afghanistan and the former Yugoslavia from our final sample, but we provide robustness tests including the two groups, which show that in practice our estimates are unaffected by this exclusion.

We also exclude those who re-migrate within 9 years after being granted residency and later test for selectivity over the reform period to ensure that remigration patterns are not related to the reform (see Section 4.1).[18] Our *base sample* thus consists of 4,843 individuals who received residency within our observation window and were aged 18 to 55 on the date residency was granted. Collectively, these individuals had 3,299 children aged 0 to 17 at the time that residency was granted. In our analysis of

[18] As noted earlier, the lengthy asylum process (on average 15 months) precludes the possibility that announcement effects compromise our identification because those receiving residency around the reform's enactment date had already submitted their applications before the reform was proposed. Similarly, a contemporaneous bill that changed the rules governing when (but not whether) individuals could apply for permanent asylum (see Kilström et al., 2018) does not affect our identification as it took effect only for asylum applications lodged from March 2002 onward.

couples' joint responses, we add in the spouses of all individuals in the base sample, which results in a balanced *couples sample* of 4,072 individuals (2,036 couples, 57% with two pre-reform residencies, 13% with two post-reform residencies, and 30% with residencies on either side of the reform).[19]

We use two indicators to measure local labor demand in the assignment municipalities. First, we take the number of job openings in low skilled / unskilled positions (e.g., construction, cleaning, and warehouse work) relative to the number of unemployed individuals in each municipality.[20] As this local job-opening information is only available from 2002 onwards (www.jobindex.dk, which includes all openings posted on the internet), we address simultaneity concerns by regressing the number of job openings per unemployed individual in 2002 and 2003 on pre-reform municipality characteristics and use the resulting predictions in our analysis. For brevity we refer to this measure as *job openings in low- and unskilled work*. Our second measure is the municipal average employment rate of non-Western immigrants from 1999 to 2001, which captures a strong element of demand for the type of work refugees can perform.[21] Appendix B.2 details the construction of the two measures and provides descriptives. As we show in Section 4, both measures of local labor market conditions are unrelated to the characteristics of assigned refugees in our sample.

2.5 Outcomes

We determine labor market status from the first full year after residency onward, distinguishing between three mutually exclusive states: (in) *employment*, (in) *unemployment*, and *not in the labor force* (NILF). The unemployed are individuals available to the labor market who are participating in integration programs but are not currently working. Employed and unemployed individuals constitute

[19] Results for couples are robust to limiting the sample to the 90% of couples in which both spouses received residency within the ± 18 month window around the reform.
[20] There is strong persistence in the local labor demand indicators over time. For example, the correlation between ranks of municipalities according to the number of job openings per unemployed in year t and year t+5 is around 0.8. The correlation between rank in year t and t+10 is around 0.7.
[21] Municipal average employment rates of non-Western immigrants are also used by Azlor et al. (2020) as measures of local demand for immigrant labor, while Åslund and Rooth (2007) consider municipal average unemployment rates. Similarly, Hoynes (2000) uses average local labor market outcomes to proxy labor demand conditions, while Notowidigdo (2020) presents an alternative estimation strategy by using a Bartik instrument to identify local labor demand shocks.

the labor force, and the residual group is, by construction, not in the labor force. Most of this group are ineligible for transfers, due to neither working nor participating in integration programs. A remaining (small) group are eligible for disability benefits. This group is exempt from both integration programs and transfer reduction.

We consider four measures of income, all based on tax authority records: labor earnings (measured pre-tax, where those who have no earnings are set to zero), transfer income (measured pre-tax), pre-tax gross income (which equals labor earnings plus transfer income), and post-tax disposable income (which equals pre-tax gross income minus tax payments). Based on the income data, we construct a measure of public expenditures as transfer income minus tax payments. We supplement the income data with hourly wage rate data and occupational classifications. Most of our analysis focuses on the first five years after residency, but Section 4.7 also reports effects on employment until 10 years after residency.

Our measure of crime is based on police and court records for all criminal convictions in Denmark. In addition to the unique individual identifiers allowing us to link the crime data to the sample of refugees, the data also includes unique case identifiers along with specific offense and conviction dates for our entire sample, and detailed offense codes that enable us to identify the exact crime type committed. We focus here on crimes that lead to a conviction and we count crime by the date of the offense (such that, for example, “crime in year 1” is crime committed during the first year after residency that leads to a conviction at some later point in time). We describe all outcomes and data sources in greater detail in Appendix B.2.

2.6 Descriptive Statistics

Table 1, Panel A lists the covariate means for the base sample of adults aged 18 to 55, again distinguishing between pre- and post-reform residency. As the inflow of refugees to Denmark slows over our sample period (as in most other European countries, cf. Hatton, 2009), the number of residencies granted post reform is smaller. Of the refugees in the base sample, 84% are immigrants from predominantly Muslim countries (around half of Iraqi origin). Residency based on refugee status is granted to 62% of the sample, while the remainder receive residency as a result of family reunification. Upon residency, each adult has on average two children. Although the table reveals some differences between the pre- and post-reform groups (e.g., share of females), tests of our key assumption of comparability in the limit around the reform cutoff date confirm the observable characteristics to be balanced (Panel B, Table 1), with no discontinuities in covariates around the reform timing. We will return to this point in Section 4.1.

Fig. 2 plots the distribution of average pre-tax gross income (labor earnings and transfers) from 2003-2007 for adult refugees granted either pre- or post-reform residency in 2002, together with the pre-tax gross income distribution for native Danes. Whereas refugees with pre-reform residency are clustered in the lowest 15 percentiles of the Danish pre-tax gross income distribution, with annual pre-tax gross incomes of $15,000 or below, almost all refugees granted residency after July 1, 2002, fall into the lowest 8% of the pre-tax gross income distribution with pre-tax gross incomes below $10,000.

3 Estimation and Identification Strategy

Because the benefit reform studied here induced a large drop in transfers for refugees who received residency following its implementation, we first estimate the reform's effect on individuals using a regression discontinuity design that compares those granted residency just before and just after the reform cutoff date:

$$y_i = \alpha + \beta * reform_i + g(Z_i)'\pi + X_i'\gamma + \epsilon_i, \qquad (1)$$

where y_i is an outcome for individual i measured τ years after residency, $reform_i$ is a dummy variable indicating whether individual i received residency after the reform date, and Z_i is a running variable counting months between the residency decision and the reform date.[22] The vector X_i collects

[22] To allow for separate trends on each side of the reform, we define $g(\cdot)$ to be linear by different linear functions pre- and post-reform, but we show that the estimated effects of the reform are robust to other definitions of $g(\cdot)$. We also allow for separate pre- and post-reform trends around the reform in the balancing tests. Moreover, we use "month" as the running

observable characteristics, and ϵ_i is an idiosyncratic error term. The parameter of interest is β. It measures the effect of being eligible for Start Aid instead of SoA among individuals granted residency just around the reform.

To better understand how the reform affects households' joint decisions, part of our analysis focuses on couples. Here, we have two post-reform treatment categories (see Section 2.3), which we capture by extending Eq. (1) to allow the outcome of individual i in household f to be affected by the residency timings of both themselves and their spouse. We define three states: (i) both spouses receive pre-reform residency and qualify for full SoA (baseline), (ii) both spouses receive post-reform residency and qualify for Start Aid (Type A), and (iii) the two spouses receive residency on either side of the reform, with the pre-reform resident keeping full SoA while benefits are capped at two times Start Aid, which effectively makes the post-reform resident spouse ineligible for any benefits (Type B). We define Type A and Type B couples by two disjoint treatment dummies, A_i and B_i, with baseline couples as the reference category.[23] We estimate the reform's effects on outcome y_{if} of individual i from family f as

$$y_{if} = \alpha + \beta_1 * A_{if} + g(Z_{1f})'\pi_1 + \beta_2 * B_{if} + g(Z_{2f})'\pi_2 + \epsilon_{if}, \qquad (2)$$

where $g(Z_{1f})$ and $g(Z_{2f})$ control for the running variables that count the months to and from the reform for each spouse while allowing for different trends pre- and post-reform for each spouse. The parameters β_1 and β_2 measure the effects for Type A and Type B couples, respectively, with baseline couples as the reference category. We also interact Eq. (2) with gender, thereby estimating β_1 and β_2 (and α, π_1, π_2) separately for males and females.

A unique feature of our data is that individuals in our sample were also quasi-randomly allocated across Denmark's municipalities, which allows us to obtain causal estimates of how local labor demand affects the reform's impact on employment. We group municipalities according to their pre-reform local labor demand indicators into groups labelled $g = 1, \dots, G$ and estimate:

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variable as refugees typically receive for administrative reasons their residency decision at the 1st of a given month and are allocated to a municipality at the same time.

[23] See Card et al. (2007a) for a further discussion of identification with double discontinuity.

$$y_{ig} = \alpha_g + \beta_g * reform_i + f(Z_i)'\pi_g + \varepsilon_i. \qquad (3)$$

The parameter α_g captures the pre-reform levels in group g, β_g measures the reform effect for group g, and $f(Z_i)'\pi_g$ allows for different pre- and post-reform slopes in the running variable across municipality groups.[24] In practice we divide municipalities into two groups, by whether they are above or below median in a given demand indicator.

4 Average Effects of the Reform

4.1 Balancing Tests

Our key identifying assumption is that with respect to those individuals whose residency is granted just before or just after the reform, the cutoff date is as good as random. This assumption is helped by the fact that the time span between reform announcement and implementation (3 months) was short, and that – given the lengthy asylum process which lasts on average 15 months (Hvidtfeldt and Schultz-Nielsen, 2018) – refugees affected by the reform were already in Denmark at the announcement date. As a first visual balancing test around the reform date, Fig. A.1 shows the employment, unemployment, and NILF rates during the first-year post residency for each value of the running variable, as predicted from an OLS regression using the covariates from Table 1 (cf. Card et al., 2007a). The pre- and post-reform trends are connected with no discontinuities in the predicted outcomes at the reform date, indicating no compositional changes to the sample around the cutoff.

To further assess the validity of our design, we also perform a barrage of formal tests. We regress a dummy for pre- versus post-reform residency on the running variable and the covariates to assess whether the observable characteristics change around the reform date. Column 4 in Table 1 shows results for our main sample and Table A.2 presents results for alternative sample definitions. We next regress each covariate separately on the reform dummy (conditional on the running variable, see

[24] When estimating Eq. (1), we cluster standard errors by the running variable. When estimating Eqs. (2) and (3) we use the two-way clustering method proposed in Cameron et al. (2011). For Eq. (2), we cluster by the running variable and household, and for Eq. (3), we cluster by the running variable and allocation municipality.

column 5 of Table 1 and Table A.3), including waiting times individuals spent in refugee camps before being granted residency (to test whether waiting times change across the reform, based on data from Hvidtfeldt et al., 2018), a dummy variable indicating whether an individual leaves Denmark over the 9 years after residency was granted (to investigate a possible increase in remigration after the reform) and a dummy variable indicating whether the spouse arrives first or last. In only two of the 44 individual balancing tests performed is the estimated parameter significant at the 10% level. While the aforementioned results – particularly the ones for waiting times in refugee camps – illustrate that case workers have *not* responded to the reform by granting more residencies just prior to the implementation of Start Aid, we perform McCrary tests of differences in the running variable density (residencies per month) around the reform date, varying the bandwidth selection from 10% to 150% of the optimal bandwidth to confirm robustness. None of the specifications reveal structural breaks (Table A.4).[25]

As we explain above, to estimate the causal effect of local labor market conditions on the reform's effect on employment, we rely on the quasi-random allocation of refugees across municipalities, which we group according to their local labor demand. To test this quasi-random allocation, we regress the average non-Western employment rates, and observed and predicted job openings in low / unskilled work (described in Section 2.4) on the characteristics of the refugees in Table 1. There is no sign of selective allocation for any of the indicators (Table A.5, columns 1, 5, and 9). To test whether any differential allocation is observed across the reform, we next include a reform dummy (indicating whether the refugee received residency before or after the reform), and the running variables on each side of the reform in columns 2, 6, and 10 of the table. Again, we do not observe any sign of selection into specific municipalities. To address the concern that refugees who are granted residency earlier and later in the calendar (i.e., administrative) year were assigned to different types of municipalities (defined as above or below the median in a given local labor demand indicator), we also run regressions

[25] The absence of structural breaks around the reform in refugee characteristics and the running variable density is the key identifying assumption irrespective of any longer-term changes in migration flow to Denmark that may have followed the reform as suggested in Agersnap et al. (2020). Also, the absence of any changes in sample characteristics and density around the reform verifies that caseworkers did not manipulate cases to place certain families pre- or post-reform.

where we include calendar month of residency in the tests. In sum, there are no significant associations between the local labor demand indicators and refugee characteristics, the timing of residency relative to the reform, or calendar month of residency, with p-values for joint significance in the balancing tests ranging from 0.167 to 0.761.

We also predict employment rates, unemployment rates, and labor earnings in years 1, 2, and 3 after the residency decision, based on observed characteristics (analogous to Fig. A.1, but with the addition of variables on timing of residency), and plot the predictions against deciles of the two labor market indicators (Fig. A.2). There are no changes in predicted outcomes across the two indicators. Finally, we test for differences in municipalities' job policies by studying whether municipalities' use of activation and training requirements differ across the local labor demand indicators, and we test for selective moving patterns across local labor demand. We find no evidence of differences.[26]

4.2 Short and Medium Run Reform Effects

As a first illustration of the reform's immediate impact, Fig. 3 shows transfer income, labor earnings, pre-tax gross income and post-tax disposable income in the first year after residency plotted by timing of residency relative to the reform. The figure documents the large drop in transfers following the reform. Moreover, it shows that pre reform, only 10% of pre-tax gross income in the first year after residency comes from labor earnings (about $1,900) with the remaining 90% coming from transfers (about $20,500). The figure also reveals that although labor earnings increase in response to the reform, pre-tax gross income drops to almost half the pre-reform level and average post-tax disposable income falls by around 40% to (or below) Denmark's estimated subsistence minimum (which is around $8,800, see Hansen, 2002).

[26] As shown in Table A.6 (columns 1 and 2), there are no differences in geographical mobility to or away from low and high demand municipalities across the reform. Table A.6, columns 3 and 4, compares high and low demand municipalities' use of activation and training requirements. There are no significant differences.

In Panels A and B of Table 2, we separately estimate the effects of the reform on transfers and labor earnings by time since residency (running regressions of the form of Eq. (1) and using levels instead of logs for income because of zeros in annual individual income measures). We report the pre-reform means as benchmarks in the first column of each panel. In years 1, 2, and 3–5 after residency, annual transfer income drops by approximately $10,000, $8,000, and $5,000, which corresponds to 55%, 45%, and 30% reductions, respectively. At the same time, labor earnings rise by $1,100 – $1,600. However, while large in relative size, earnings remain low in absolute levels and the reform's effects on earnings far from compensate for the lower benefit levels.

Similarly, average first-year employment rates post-reform almost double in the first year after the reform, from 10.3% to 19.5% (Panel C), which is in line with Huynh et al. (2007) and Rosholm and Vejlin (2010). Yet, the reform effect reduces to 7 percentage points (or 37%) in year 2, and to 4 percentage points in years 3-5 – an estimate that is significantly lower (at a 10% level) than the year 1 effect.

The reform effect on employment is nearly exclusively due to unskilled manual work (Table A.7), and the effects are homogeneous over different education groups (Table A.8), which is a first piece of evidence that education accumulated in the home country is either of little value in the Danish labor market, or refugees lack complementary skills (such as language) to make these skills productive.[27] Thus, the overall labor supply effect of the lower transfers appears to be that refugees were incentivized to take up employment faster than they otherwise would have, and that this was mainly in unskilled manual work. We will return to this point in Section 5 where we investigate how the reform's effects on employment, job stability, and job types are mediated by local labor demand.

[27] This resembles LoPalo's (2019) finding for the US that shows that lower benefit levels may reduce the quality of jobs that refugees take. See also Rosholm et al. (2006) and Fasani et al. (2018) on mismatch between occupations in Europe and refugees' employment. Foged et al. (forthcoming) find that the introduction of integration courses in 1999 led to higher labor earnings for refugees. Hence, as pre-reform refugees have higher unemployment rates (and thus higher participation rates in integration courses to be eligible for social assistance, which we also show in column 4 of Table A.6) in the first years after residency, they could potentially acquire more language skills than post-reform refugees This in turn may contribute to the longer run fade out in employment effects and lower job-quality in low demand areas.

Table 2 shows further that the reform lowered unemployment by around 16, 16, and 10 percentage points in years 1, 2, and 3–5 (Panel D); a decrease far larger than the increase in employment. The difference is explained by a dramatic increase of individuals leaving the labor force: in year 1, the share of those out of the labor force increases by 7 percentage points.[28] Table 3, which provides estimates by gender, shows that the reason for this increase is due to females, who show only a small insignificant employment response in these initial years, but experience a large reduction in unemployment rates. In contrast, for males, the drop in unemployment is accompanied by a corresponding increase in employment. The decrease in female unemployment and increase in labor force exits as a response to the reform underscores the importance (amply stressed by Bratberg and Vaage, 2000; Card et al., 2007b; and Kyyra and Ollikainen, 2008) of distinguishing between welfare benefits' effects on unemployment versus those on employment and total nonemployment.

4.3 Employment, Disincentive Effects, and Nonparticipation of Females

There are two reasons for the strong increase in female labor force exits in response to the reform. First, household level means testing reduces female transfers if the male takes up employment. Second, within-household incentives were affected when the same overall transfers for the household were differently allocated within couples according to whether both spouses arrived after the reform (Type A couples where both partners received Start Aid), or the first spouse arrived before and the second after the reform (Type B couples where the first received SoA while the second received virtually no transfers at all, see Section 2). In Type B couples, the last arriving spouse's incentive to remain transfer-eligible by staying in the workforce and attending integration courses (around 30 hours per week and a pre-requisite for transfer receipt) was essentially removed.[29]

[28] The sharp discontinuity around the reform date is further illustrated by Fig. A.3, which shows labor market outcomes during year 1 and 2 after residency for individuals granted residency around the reform date. Here, employment rates increase from a pre-reform mean of 10% to around 20%, while unemployment rates decrease from 90% to around 70-75%, with the difference attributable to an increase in the NILF rate.

[29] We illustrate the intuition underlying the different incentives for Type A and B couples in a simple static labor supply framework in Fig. A.4. (see Lemieux and Milligan, 2008, for a similar illustration). By reducing the SoA for each partner

Our empirical assessment confirms these predictions. We separately estimate the effects on labor market outcomes by gender and household type, distinguishing between effects for Type A and B couples (75% of our adult sample) relative to baseline couples in Panels A and B, respectively, and post-reform singles relative to pre reform singles in Panel C.[30] The estimates in Panels A-C of Table 4 show that male employment in year 1 increases by 15 and 8 percentage points for Type A and Type B couples, respectively, and 17 percentage points for singles (compared to pre-reform households). Female employment in Type A couples increases in year 1 by 8 percentage points, the unemployment rate decreases by 17 percentage points, and the fraction of women not in the labor force increases by 9 percentage points. Type B females and singles have a more muted (and insignificant) employment response of about 3 percentage points although point estimates are not significantly different across Type A, Type B, and single females. However, for Type B females unemployment is 20 percentage points lower post reform than pre reform, which is accounted for by a 17-percentage point increase in the fraction not in the labor force, a reform effect that is significantly larger than the corresponding estimates for single females in both years and for Type A females in year 2 after residency.

Comparing the reform effect on the probability that both spouses are in employment (6.5 percentage points in Panel D, row 1) with females' employment response for Type A couples (7.9 percentage points in Panel A, row 1) shows that almost all female employment responses can be explained by an increase in dual earner households. The table further suggests a link between the increased employment uptake of husbands (6.4 percentage points in Panel D, row 3) and the increased fraction of females not in the labor force (9.0 percentage points in Panel A, row 3) in Type A couples, which is likely due to

to Start Aid, the reform decreases Type A couples' nonlabor income and the couple improves utility by supplying some labor. For Type B couples, household level transfers drop by the same amount, but transfers are unchanged at SoA for the first arriving spouse and reduced to zero for the last arriving, who thus cannot be penalized for dropping out of an integration program. Type B couples can thereby increase household leisure (with an implicit price of leisure equal to zero) without reducing transfer income by dropping out of integration courses and the labor force.

[30] Because differences in residency dates may now cause "time since residency" to capture different periods for each spouse, we align spouses' outcomes by defining this variable as time since residency of the last arriving spouse. This way we also center the outcomes by the residency that defines a household's treatment status (Baseline vs. Type B and Type B vs. Type A), which is determined by the timing of residency for the last arriving spouse. To ensure that our results are not driven by this definition, we replicate our findings using time since residency for the first arrived spouse (results available on request).

means testing. Our results for Type A couples thus illustrate the importance of household-level responses and the potential adverse consequences of disincentives inherent to transfer systems with household-level means testing.

Obviously, Type B couples where partners arrive on both sides of the reform occur only during the implementation period and are therefore less relevant for assessment of the reform's longer-term impact. Nevertheless, the findings show that ignoring the difference in responses may lead to inaccurate conclusions about the reform's immediate effects. From Panel D, we also see that employment effects in Type B couples are driven solely by *single earner* responses where only the first arriving spouse finds employment, inducing means testing of the other spouse's transfers. Thus, the 10-percentage point difference between total female labor force exits in Type B couples (0.173, cf. column 3 in Panel B) and those who drop out when their spouses find employment (0.070, cf. column 3 in Panel D) constitutes a lower bound for the disincentives induced by the reform's asymmetric benefit allocation.[31]

The analysis of Type A and B couples also illustrates how subtle differences in incentives can generate large differences in labor supply responses and in key policy parameters such as household level elasticities of labor earnings with respect to benefit levels (which we estimate to be 1.36 for Type A couples vs. 0.38 (and insignificant) for Type B couples in year 1, see Table A.9, Panel A).[32]

4.4 Robustness Tests

We have performed an array of robustness tests. First, we construct estimates defining a placebo reform dummy for individuals who received residency before or after July 1st 2000 (i.e. two years before the

[31] The asymmetric allocation of transfers has also implications for reservation wages, with the highest wage rates needed to induce labor supply in the pre-reform group, while the wage rate for Type B couples required to supply labor should be higher than that required for Type A couples, which is precisely in line with the estimated hourly wage distributions for male spouses (Fig. A.5).

[32] This finding supports also Ashenfelter's (1983) evidence that elasticities depend on the implied tax rates from means testing and nonpecuniary costs of welfare receipt. A related question is whether the gender differences in reform-effects relate to first vs. last arriving spouse or whether social norms and labor-market related gender roles also play a role. Females receive residency last in 86% of couples, which leaves us with too small a sample size for analysis. A simple plot (available on request) of the fraction of last-arriving males who are not in the labor force increase is close to 0 for those who received residency in the months leading up to the reform and 10-15% for those who received residency in the months after the reform. Thus, the labor force withdrawal also appears to be present for last-arriving males in Type B couples.

actual reform) around an 18-month bandwidth, which are all very close to zero and insignificant (see Table A.10).[33] Second, we estimate models of the effect of the reform on labor market outcomes with more flexible running variables, a donut-sampling (excluding the months around the reform), a reduced bandwidth, and including Afghans and Yugoslavs (Table A.11). All these estimates are similar to those reported above. Third, we present the estimated effects of the reform on employment from year 1-10 after residency across different bandwidth choices for the main estimation sample, for year one both including and excluding refugees from Afghanistan and Yugoslavia (Fig. A.7 and Fig. A.8). Point estimates are remarkably stable across specifications. Fourth, Table A.12 shows that point estimates reported in Table 2 are unaffected by the choice of conditioning variables (which only serve to increase precision). Fifth, Table A.13 presents the estimated differences in employment rates of labor migrants – who are ineligible for SA and Start Aid – according to whether they receive residency before or after the Start Aid reform (mimicking the design for refugee migrants). All estimates for labor migrants' employment in years 1-5 after residency are close to zero and insignificant. Hence, our findings are not results of general changes in the Danish labor market. Finally, our main empirical specification relies on a linear running variable. While Table A.11 replicates our findings for labor market outcomes using a quadratic running variable, another approach is to estimate effects using local linear regression (LLR). Table A.14 compares all the main findings in the paper with estimates where the pre- and post-reform slopes are estimated using LLR. All our conclusions remain unchanged.

4.5 The Repeal of the Reform

The Start Aid reform was repealed ten years after its introduction, on January 1st, 2012 (proposed on November 21st, 2011), when transfers to all refugees were increased to pre-Start Aid levels. While the

[33] We have also estimated Eq. (1) using placebo reforms from 5 months before to 5 months after the actual reform. Regardless of whether we use transfer income (Fig. A.6A: full sample and by gender) or employment, unemployment, and NILF as outcomes (Fig. A.6B), the *t*-values from the estimated β's are between 0 and 1 (except for males' transfers in placebo month -5, where the t value is 1.5) for placebo reforms more than 4 months on either side of the actual reform. As the timing of the placebo reform converges toward the true reform date, the *t*-values increase, jumping dramatically to reach their maximum level at this date (the figure's center).

repeal of Start Aid affected all refugees and thus does not provide an obvious control group, it is nevertheless insightful and a further robustness test to investigate whether similar responses can be observed as at its introduction. Appendix B.2 describes the data used for the repeal analysis.

Fig. 4 presents event study estimates of the effect of the repeal on males' and females' (Panels A and B) employment in years 1 and 2 after receiving residency. Overall, pre-repeal estimates are all insignificant and close to zero, with the exception of females where borderline significant results in year -3 suggest a slight violation of the parallel trends assumption. Employment rates in the first two years after receiving residency drop by around 8-10 percentage points for males who are affected by the repeal. This is smaller than the estimated opposite effect of the 2002 introduction of Start Aid, where males' employment increased by 16 and 13 percentage points in years 1 and 2 after residency, respectively. For females, the employment effects are close to zero, similar to what we find when Start Aid is introduced. The fraction of females who receive no transfers and instead exit the labor force also decreases after the repeal (not shown here) with a similar magnitude to the reduction in males' employment (cf. Fig. 4). This is likely a result of a reduction in means testing of females' transfers (see section 4.3) when fewer males find employment. Overall, these estimates follow the same patterns (with opposite signs) as was found for the introduction of the Start Aid reform in 2002.

4.6 Long Run Reform Effects

Fig. 5 extends the time horizon and summarizes the reform's effects on the probability of adult males being in employment up to 10 years post residency. Although overall labor supply effects are initially considerable in magnitude – close to 15 percentage points on average (Fig. 5A) – they decrease significantly relative to the initial effects and remain statistically insignificant after about 5-6 years (i.e., the reform effects have faded several years before the repeal increased transfer levels in 2012). Distinguishing between singles, Type A and Type B couples, Fig. 5B shows that employment responses for Type B couples disappear after the first two years, while those for Type A couples and singles are more persistent but also disappear about 5-6 years after the reform (while year 1-2 estimates are significantly different from estimates in years 5-10 for the full sample in Fig. 5A, we cannot reject that year 1-2 estimates are equal to estimates for later years once the sample is split by household type in Fig. 5B). Thus, while the reform induces substantial labor supply responses in the first two years after its implementation, the reform's average effects appear to dissipate in the longer run.[34] However, as we will show in the next section, this results masks substantial and significant heterogeneity in the effects of the reform driven by local labor demand differences.

5 Reform Effects and Local Labor Demand

While the previous section illustrates how labor supply incentives affect the responses to the reform, employment uptake also depends on the demand side of the labor market. The way welfare policies interact with labor demand is indeed a central question in understanding their impacts. For example, Ziliak et al. (2000), Ganong and Liebman (2018), and Kleven (2019) point out that estimated employment effects of welfare reforms such as expansions of the Earned Income Tax Credit (EITC) may partly be driven by business cycles. Moreover, when assessing reform effects for refugees, this issue is particularly relevant, as the skills refugees bring with them may be of little value in the Danish labor market, partly because of lack of complementary skills such as language proficiency. This is supported by Table A.7 (discussed in Section 4.2) which shows that refugees who enter employment in the first year after their arrival almost always take low skilled manual jobs, no matter what their level of education is.

How local labor demand conditions mediate the effects of a welfare reform is not easily analyzed for two reasons. First, most studies of welfare reforms similar to ours use temporal or spatial variation in reform implementation for identification.[35] In contrast, our use of a discontinuity design to study a reform that has been implemented uniformly throughout the entire country allows distinction of reform

[34] Fig. A.9 displays the reform effects on labor earnings by household type. The results are similar to our findings on employment, albeit more imprecisely estimated.
[35] See, for example, the vast literature studying the effects of welfare reforms in the U.S. (surveyed in e.g., Hendren and Sprung-Keyser, 2020, who review the study of U.S. public policies over 50 years) and Borjas (2002) who focusses on immigrants.

effects across local labor markets. Secondly, spatial selection of individuals will distort any estimates that seek to understand the effects of local economic conditions on a reform's effect, an issue that is particularly severe when investigating the effect of a welfare reform that targets immigrants. To address the sorting problem, we utilize that the implementation of our reform overlapped with a period where refugees, upon obtaining residency, were quasi-randomly allocated across municipalities. This provides exogeneous variation in local conditions that allows us to study how the reform's immediate and longer-term effects interact with local labor demand. As explained in Section 2, we use two indicators for local labor demand, based on job-openings in low- and unskilled work, and on municipal average employment rates of non-Western immigrants.

5.1 Employment Effects, Jobs Quality, and Local Labor Demand

Table 5 presents the effects of the Start Aid reform on employment in years 1-5 after residency, distinguishing between municipalities where local labor demand is above and below the median, and focusing on males (who are driving the employment response of the reform).[36] All estimates condition on a range of other municipality level characteristics, such as population density (population size divided by area size in each municipality), size of immigrant population, voting share for anti-immigrant parties, and regional fixed effects. Panel A, which displays estimates for overall employment effects alongside pre-reform employment levels and the difference between effects in high- and low demand municipalities, illustrates large differences in the effect on employment between high- and low demand municipalities. The employment effects of the reform in high demand municipalities are around 20 percentage points in years 1 and 2 and decrease to 10 percentage points in years 3-5. In low demand municipalities, none of the estimates are significantly different from zero, and point estimates decrease from around 13 percentage points in year 1, to 5 percentage points in year

[36] Table A.15 shows the corresponding results for the full sample. Table A.16 shows that conclusions are unaffected by the inclusion of municipality characteristics as controls, stressing that the quasi-random allocation (to high/low demand areas) indeed allows us to capture the effects of local labor demand.

2, and to around zero in years 3-5. Thus, employment effects of the reform are strikingly different across municipalities with different demand conditions. Aggregating the differences in reform effects over the first five years after residency shows that each male refugee with post-reform residency has on average spent 0.61 (0.059+0.165+0.129∗3) years more in employment (which amounts to 33% of the average pre reform level for males) because of the reform if he is assigned to a high demand municipality relative to a low demand one. Estimates using the alternative labor demand indicator in Panel B (non-Western immigrants' employment rates) are very similar.

In Panel C of Table 5, we decompose the total difference in employment effects between high- and low demand municipalities for years 2 and 3-5 (0.165 and 0.129) into differences in inflows and continuation in employment from one year to the next.[37] The estimates show that higher inflows into employment in high demand municipalities explain most of the difference in year 2, while a higher probability of staying in employment drives the difference in years 3-5. Thus, higher employment effects of the reform in high demand municipalities in the first two years after the reform are explained by more individuals entering employment, while in later years they are mainly driven by those remaining employed who found work early on.

To investigate further whether local demand conditions affect the type of jobs individuals take in response to the reform, we next decompose the difference in the reform's effects between high- and low demand municipalities into effects on employment in unskilled manual work and work that requires some skills (Panel D of Table 5).[38] The results show that the differences in inflows are driven by take up of unskilled manual work, while the differences in the probability of staying in employment are due to jobs that require some skills. Decomposing the difference of 12.5 percentage points in the

[37] The fraction in employment in year t equals the fraction entering employment in year t from non-employment in year t-1 plus the fraction that continues in employment from year t-1 to year t.

[38] The former category is the lowest category of unskilled work such as cleaning or scaffolding work, and the latter consists of, for example, installation or transport of basic equipment, or miscellaneous sales work. We decompose employment by occupation type in year t as the fraction entering employment (either in a job requiring some skills or unskilled manual work) in year t from non-employment in year t-1 plus the fraction that continues in employment from year t-1 to year t (with employment in year t either in a job requiring some skills or unskilled manual work).

probability of staying in employment for work requiring some skills between high and low demand municipalities in years 3-5 further shows that 25% are accounted for by job changes from an unskilled job to a job requiring some skills and 75% by continuation in jobs requiring some skills.

Overall, therefore, the reform had not only substantially higher and far more persistent employment effects in high demand municipalities compared to low demand municipalities, but it also led to more stable employment in higher quality jobs. The different longer-run impact of the reform across local labor demand is also evident from the changes to the labor earnings distribution. Table A.17 shows the estimated effect of the reform on labor earnings levels and the labor earnings distribution in year 3-5 in low and high demand municipalities, respectively. The reform resulted in a downward shift in the labor earnings distribution in low demand areas but an upward shift in high demand areas.

To describe differences across local labor demand further, Fig. 6A displays the reform effects the first 10 years after residency for males separately for high and low demand municipalities, and the differences between the two. After 3-4 years the effects disappear in low demand municipalities, but they remain positive in high demand municipalities (although imprecisely estimated), with the difference between low and high demand municipalities being positive and close to being statistically significant at the 10% level. Fig. 6B shows that the reform affects the probability that refugees continue in employment from one year to the next positively throughout the first 10 years in high demand municipalities, while reform effects turn negative after years 3-4 years in municipalities with low demand.

5.3 Public Expenditure and Local Labor Demand

The differences in employment outcomes and labor earnings across local labor demand will also influence the effect of the reform on public spending. Table 6 displays (for males and females jointly) the yearly average reform effects and reform effects at the 5th, 50th, and 95th percentiles of local labor demand over the first 5 years after residency on employment, labor earnings, and public expenditures.[39]

Following the reform, employment and labor earnings increased by 15-20% on average over the first five years after residency (Panels A and B). While there were no significant reform effects at the 5th percentile, the reform led to 30-35% higher employment and labor earnings at the 95th percentile of local labor demand. The *overall* difference in post-reform refugees' labor earnings between municipalities at low and high levels of local demand conditions (columns 2 and 4) amounts to 70% ($5,416 vs. $9,291), compared to a difference of only 15% ($5.875 vs. $6,824) for pre-reform refugees.

On average, the reform resulted in a reduction of public expenditures per refugee by almost 50%, through the combination of lower transfers and increased tax payments from labor earnings (Panel C).[40] However, the substantial differences in reform effects on employment and earnings across local labor markets with different labor demand conditions result in a reduction of just 35% relative to the pre-reform mean in municipalities with the lowest labor demand, but of around 60% in municipalities with the highest labor demand. These findings have important implications for refugee allocation policies, which often quasi-randomly assign refugees to local labor markets without taking account of local labor demand conditions. Our findings suggest that the success of reforms aimed at increasing labor supply incentives may be impeded by sub-optimal allocation policies.[41] Moreover, while average pre-reform disposable income was more than 35% higher than average post-reform disposable income in low demand areas, the reform only resulted in a disposable income reduction of 25% in high demand areas (Panel D). Thus, through increased employment rates, labor earnings, and self-sufficiency, the

[39] The estimates reported in column 2-4 of Table 6 are based on Eq. (1) where we weight observations by an Epanechnikov kernel according to the ranking of municipalities' local labor demand (the running variable in the reform estimates are still linear as in the remainder of the analyses). Columns 2-4 present the estimates with the 5th, 50th, and 95th percentiles as center of the kernels, respectively. Pre reform rows show the estimated constant term (α, cf. Eq. 1). Post reform rows show the estimated constant plus the reform effect ($\alpha + \beta$, cf. Eq. 1).

[40] It should be noted that this does not consider adverse effects on refugees through benefit cuts and other channels, to which we return in Section 5.

[41] One concern is that unequal allocation may lead to unwanted political responses of majority populations. However, Dustmann et al. (2019) show that vote shares of anti-immigrant parties are not positively affected by refugee allocations in more urban municipalities (see also related work by Steinmayr 2020).

overall reduction in disposable income that refugees experienced due to the reform was substantially lower in high demand areas.

6. Reform Effects on Poverty and Crime

Table 7 shows the effects of the reform on the probability of living with an annual post-tax disposable income corresponding to less than $500 per month (slightly above the U.S. Census' deep-poverty threshold, Panel A), less than $750 per month (slightly below the U.S. Census' poverty threshold, Panel B), and less than $1,000 per month (Panel C) in years 1, 2, and 3-5 after residency. The estimates show that the reform led to an increase of between 30-50 percentage points (depending on cut-off) in the probability of experiencing very low post-tax disposable incomes. For example, the probability of having less than $750 per month in the first year after residency increases from 9% to almost 50% (Fig. A.10A and B present plots of the fraction with low disposable income by timing of residency relative to the reform). Moreover, the effects are most pronounced in the first year following residency where most refugees rely on public benefits.

A natural question is whether the large decrease in disposable income led to increases in crime, as for example, recent work by Deshpande and Mueller-Smith (forthcoming) would suggest. To investigate this, Panel A of Table 8 displays the estimated effect of the reform on adults' crime for year 1, and accumulated for years 1-5 after residency, where columns 1 and 2 show results for probabilities of receiving a crime conviction, and columns 3 and 4 for the number of crime convictions. [42] The table shows that the reform caused the probability of committing a crime and the number of crimes committed by refugee adults to increase by around 125 percent (0.023/0.018 and 0.026/0.021) in the first year after residency. This increase is entirely driven by property crimes, particularly shoplifting in supermarkets. There are no effects of the reform on violent crime. The similarity of estimates in

[42] We focus on adults aged 18-45 at residency with children (70% of the main sample) because crime rates for older individuals are close to zero and the largest benefit cuts were experienced for families with children. Table A.18 show that estimates are robust to in/exclusion of control variables, and alternative specifications such as a donut specification or narrower bandwidth. Figs. A.10C-F present (by crime type) the average number of crime convictions in year 1 by timing of residency relative to the reform.

columns 1 and 2 suggests that the crime increase is driven by an extensive margin response where individuals who otherwise would not have committed a crime, now do so (as opposed to an intensive margin response where offenders commit more crime). The effects are strongest for females whose number of criminal convictions almost triples, an effect almost exclusively driven by supermarket shoplifting (columns 4-8 report effects on males' crime and columns 9-12 report effects on females' crime).

Thus, our analysis implies that cutting benefits to or below a subsistence minimum leads to more property crime, even for population groups with low baseline crime levels, such as adult females. To investigate further whether the repeal of the reform resulted in analogous reductions in crime committed in the first years after residency, we replicate the analysis from Section 4.5 in Fig. A.11 for females, the most responsive group to the transfer cut in 2002. While the same caveat in terms of identification applies here as in Section 4.5, it is striking how females' response to the repeal closely mirrors the effects seen after the introduction of the Start Aid reform: increasing transfers to pre-reform levels lowers overall crime, an effect that is mainly driven by a reduction of property crime, namely shoplifting in supermarkets.

The strong increase in crime following the transfer reduction begs the question whether we observe differences in crime effects across local labor demand in a similar way as we did for employment. Columns 1-4 of Table A.19 show that, while point estimates for the reform effects on crime are indeed larger in low than in high demand areas, the differences are not significant. One reason may be that the immediate income reduction is almost the same irrespective of local labor demand: Almost all refugees rely heavily on income from transfers upon obtaining residency. This is supported by columns 5-13 in Table A.19 which show that the immediate increase in the probability of living with a low disposable income did not differ initially across areas with different local labor demand. It is only in the longer run that being allocated to a high demand municipality may serve as a stepping-stone away from poverty.

To compare the magnitude of the reform's effects on crime with its effects on labor supply, Panel B of Table A.9 presents the estimated elasticities of crime with respect to benefit levels in years 1 and 5 since residency and contrasts them to the elasticities of labor earnings with respect to benefit levels (see also Section 4.3). Results show that a 1% increase in benefit levels lowers crime by almost 150% in year 1 and 90% in year 1-5 on average (the estimated elasticities are -1.480 and 0.883, respectively). Comparing these estimates to those for labor earnings (the estimated elasticities are -0.701 and -0.323, respectively) illustrates that the reform produced a percentage change in crime that is at least as high (in absolute terms) as the percentage change in labor earnings. Thus, our results suggest that the adverse (unintended) responses following large welfare cuts can – in relative terms – be at least as large as the labor supply responses that were intended by policy makers.

7 Discussion and Conclusion

The objective of the 2002 Start Aid reform was to *"ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly*" – an intention that it shares with reforms recently implemented or currently considered by other countries. Despite a large initial employment response driven by males, effects of the reform on labor supply disappeared after about 4-5 years. Moreover, the reform led to large and persistent female labor force exits, in part due to the allocation of a household's overall transfer payment to one partner only (in most cases the husband) for couples arriving on both sides of the reform, and in part because of means testing. Both essentially remove penalization of females for not participating in integration programs. The magnitude of the response of females foregoing future labor market opportunities for the sake of leisure or home production may be partly explained by refugee communities holding traditional views about role behavior and exhibiting strong preference for females conducting home activities rather than integration programs and job search.[43] This stresses that incentivizing labor force participation of refugees need to carefully address behavioral norms in target populations.

Our analysis reveals a striking impact of local labor demand conditions on the reform's effect, which we can assess due to a random assignment policy for refugees concurrent with the reform implementation. We find that the short-lived reform effect on employment is mainly due to poorer job matches and less persistent employment relationships in low demand municipalities. In other words, the policy led many refugees to take up temporary and low-quality employment relationships in areas where demand conditions were unfavorable. Overall, these findings emphasize that allocation of refugees to areas with poor labor market conditions do not only impede future opportunities, but dramatically counteract intended reform incentives for employment and integration. Perhaps most concerning is the impact the reform had on the probability of living in poverty and the increase in subsistence type crime such as shoplifting in supermarkets. Our findings therefore have important implications for current discussions of welfare reforms aimed at groups similar to the one studied in this paper and are relevant not least for the political response to present and future refugee inflows.

[43] Most refugees in Denmark migrate from Middle Eastern and North African countries where the female share of the formal labor force is between 10 and 20% (Moghadam, 2013) and gender norms are very different from those in Denmark.

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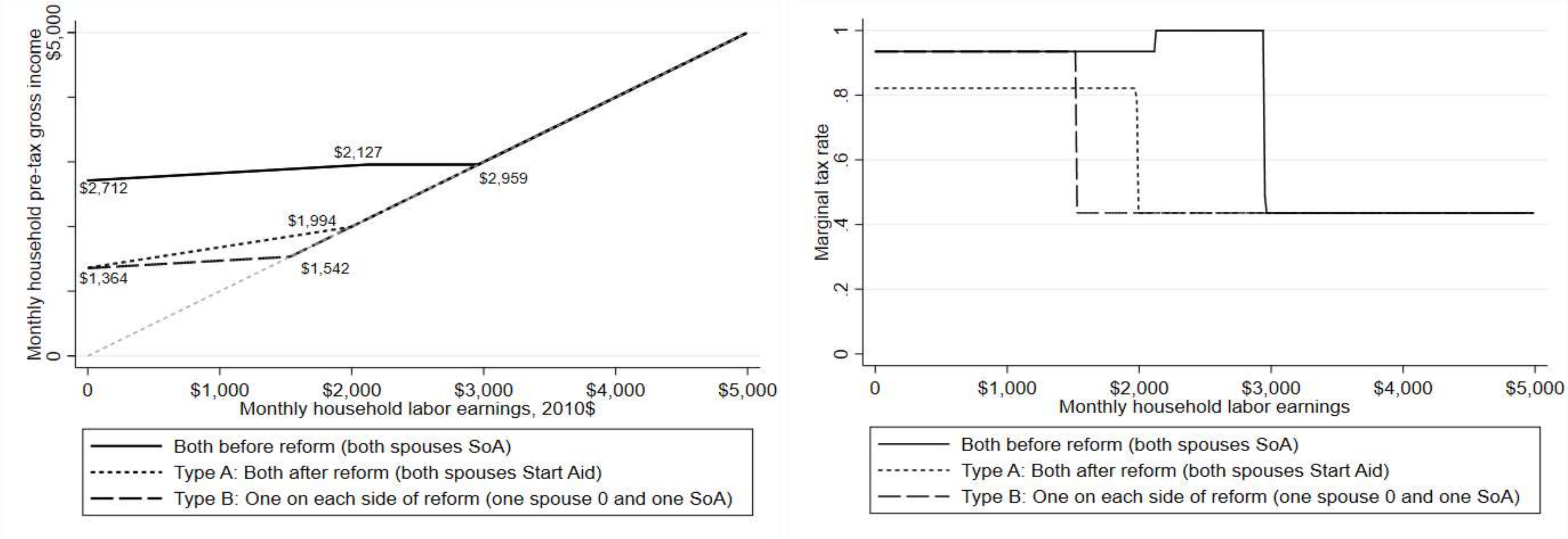
Figure 1. Labor earnings, pre-tax gross income, and implied marginal tax rates from means testing.

A) Household level labor earnings, pre-tax gross income

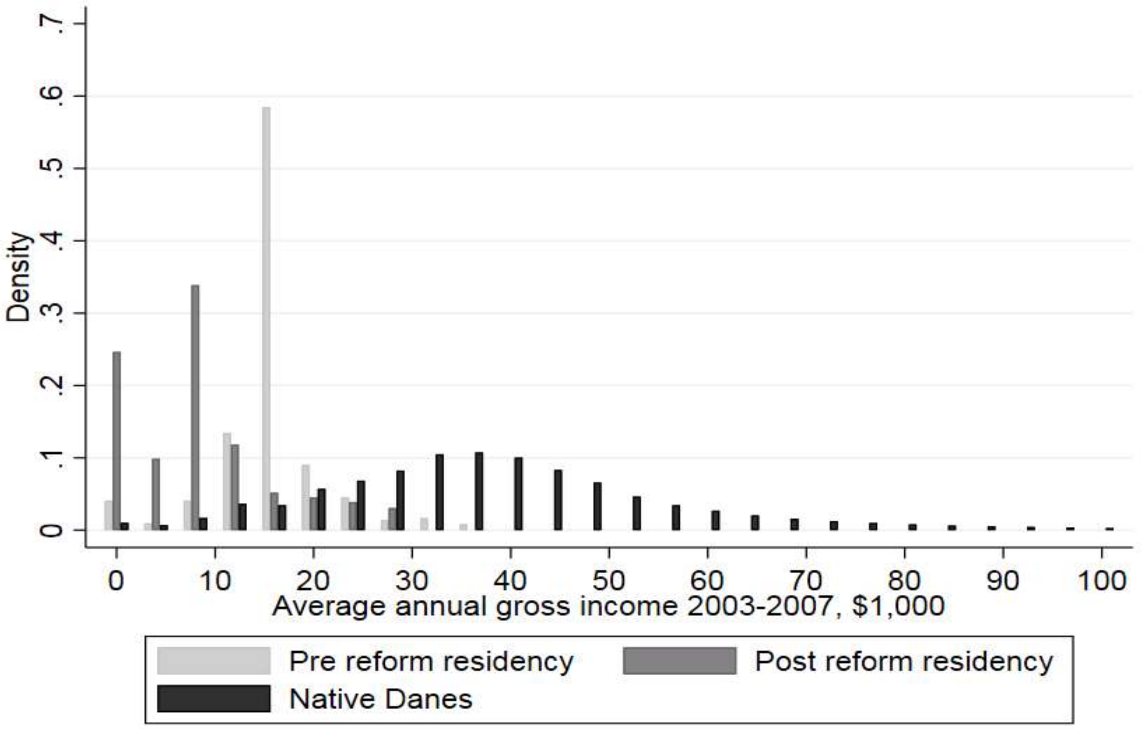
B) Implied marginal tax rate



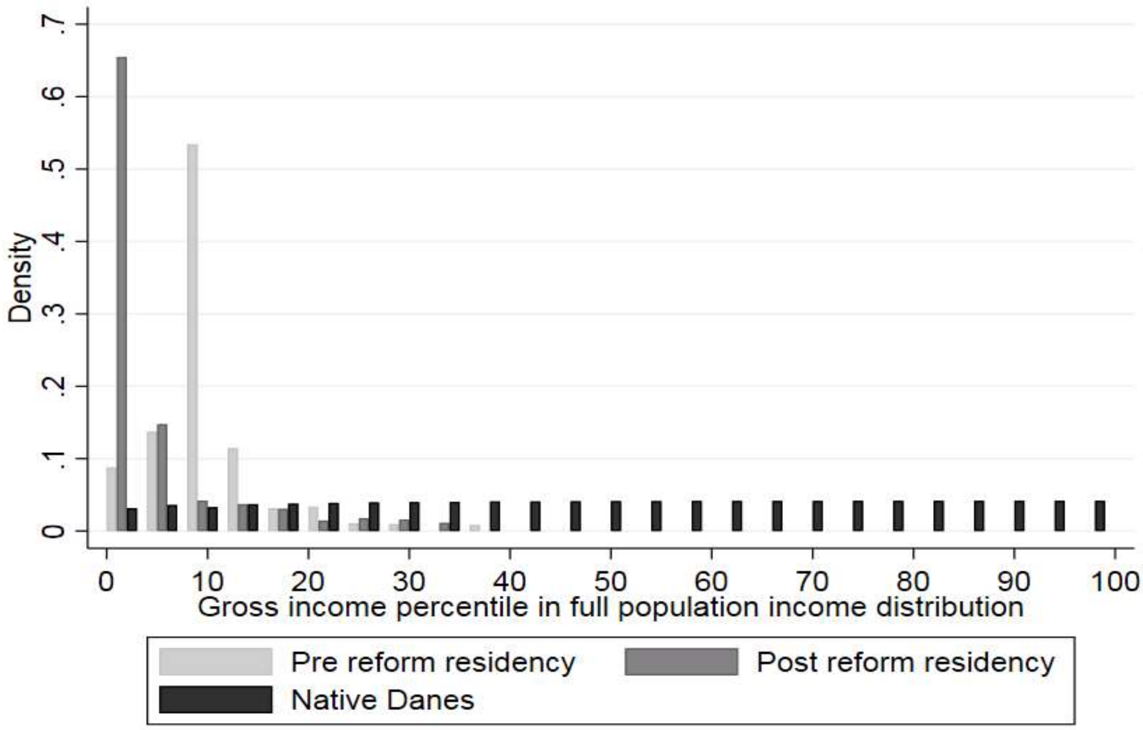
Note: Panel A shows the relationship between labor earnings (measured pre-tax) and pre-tax gross income due to means testing, by household types. The solid line shows pre-reform benefit schedule and the dashed lines post-reform schedules. Beside the lower benefits, the slopes differ due to varying means testing rates (between 0.8 and 1). Amounts noted on the y-axis (2,712 / 1,364) refer to monthly household pre-tax gross income at no labor income (intercept), and amounts noted away from y-axis refer to monthly household labor earnings (on x-axis). Panel B shows the corresponding marginal tax rates calculated as (1-slope)+t*slope, where t is marginal tax rate of 0.44 in the at lowest tax bracket. The means testing rates for singles correspond to those for Type A couples. But as the figure considers household level transfers, transfer levels and break-even points for singles are half of Type A couples'.

Figure 2. Pre-tax gross income distributions for refugees by pre- and post-reform residency, and native Danes.

A) Distribution of absolute pre-tax gross income levels

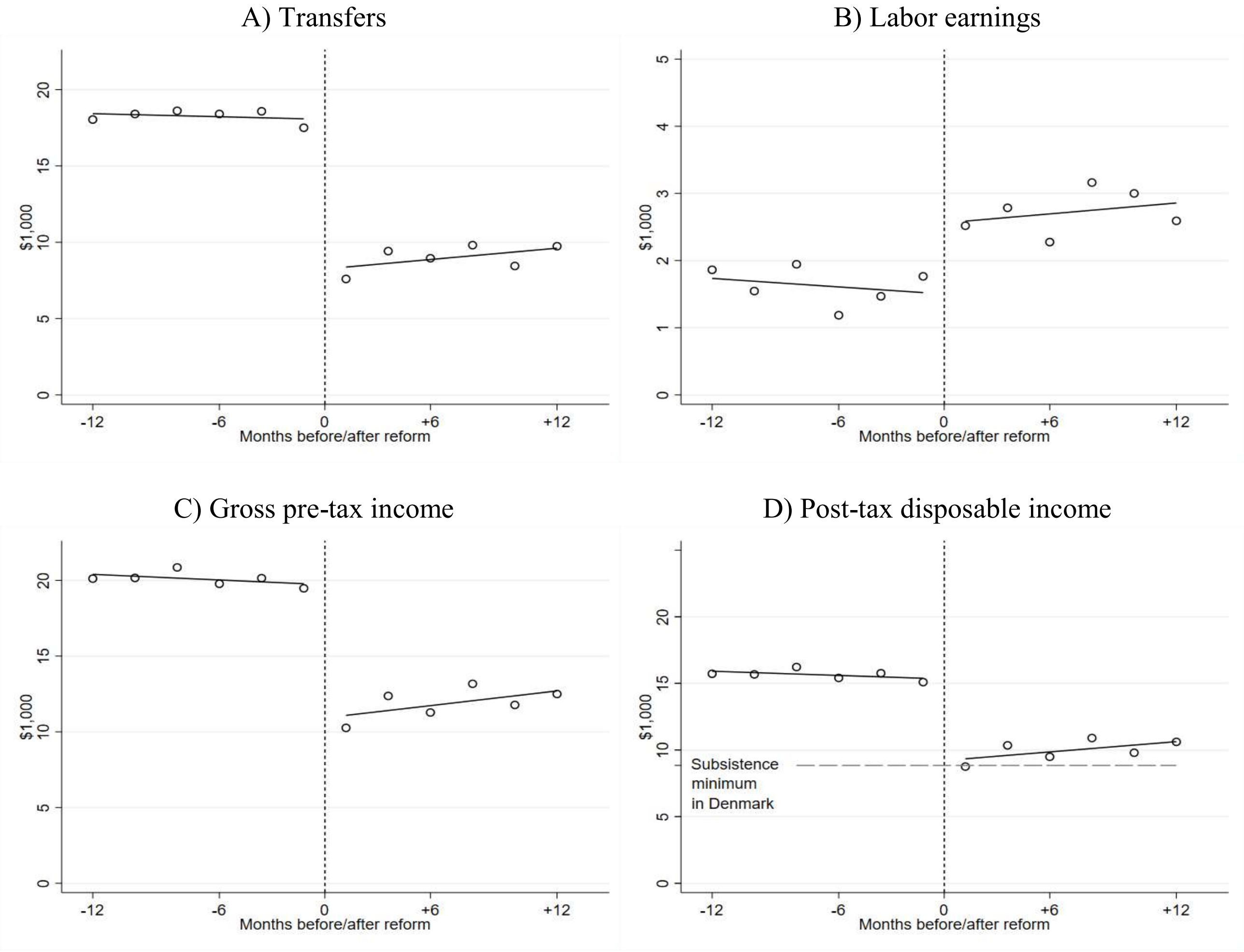


B) Distribution of pre-tax gross income percentiles in the full population income distribution



Note: The figure shows the pre-tax gross income distributions of adult refugees (age 30 or above) receiving residency in 2002 by whether they received residency before the reform (eligible for SoA) or after the reform (eligible for Start Aid) and adult native Danes (age 30 or above) for comparison. The pre-tax gross income distributions are measured from 2003-2007. Panel A presents the distribution of pre-tax gross income levels and Panel B presents, for each of the three groups, the distribution of pre-tax gross income percentiles (in the full population income distribution).

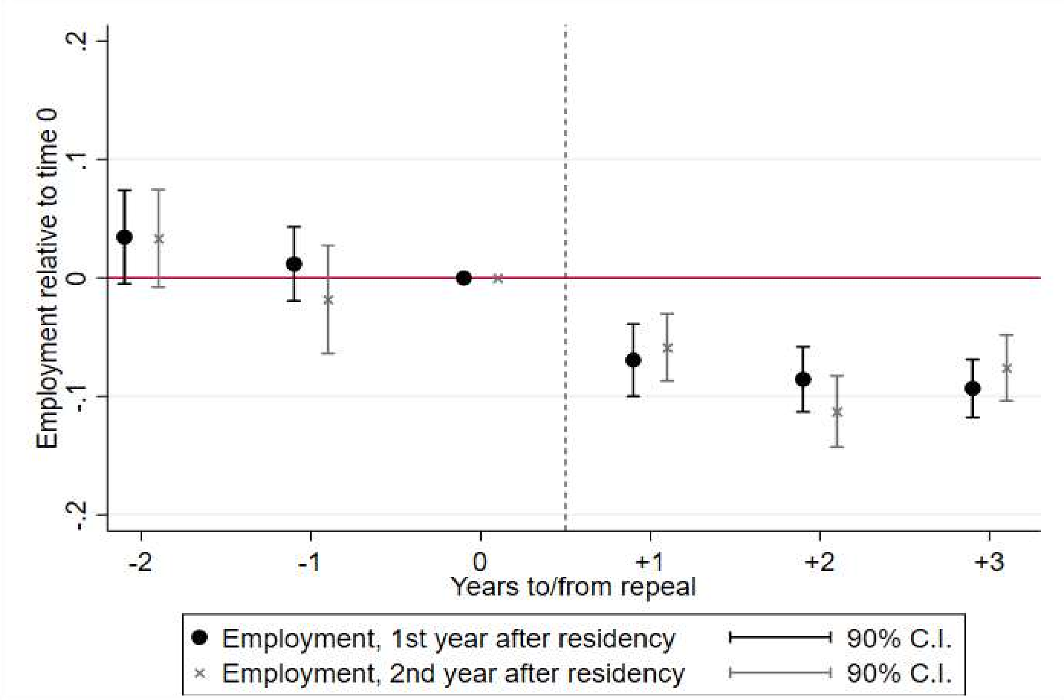
Figure 3. Individual income, the first full year after residency.

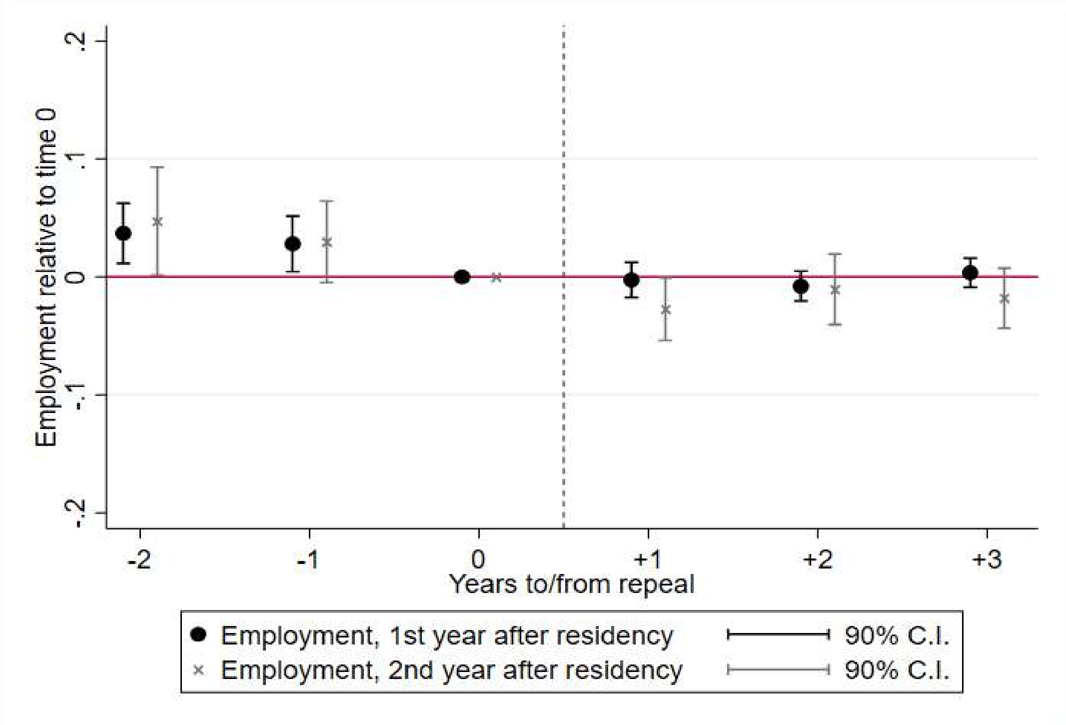


Note: The figure shows individual level transfer income (A), labor earnings (B), pre-tax gross income (C), and post-tax disposable income (D) by bi-monthly bins of residency timing. The dashed vertical line indicates the timing of the reform in July 2002. The horizontal line in D) is the estimated subsistence minimum in Denmark (Hansen, 2002) weighted across the different household types in our sample. The subsistence minimum budget includes the cheapest food, housing, and clothes available, no transportation, no replacement of durable goods, and no activities for children. The thresshold is approximately equal to the PPP adjusted U.S. poverty threshold (cf. U.S. Census Bureau).

Figure 4. Employment differences in year 1 and 2 after residency around the repeal.

A) Males

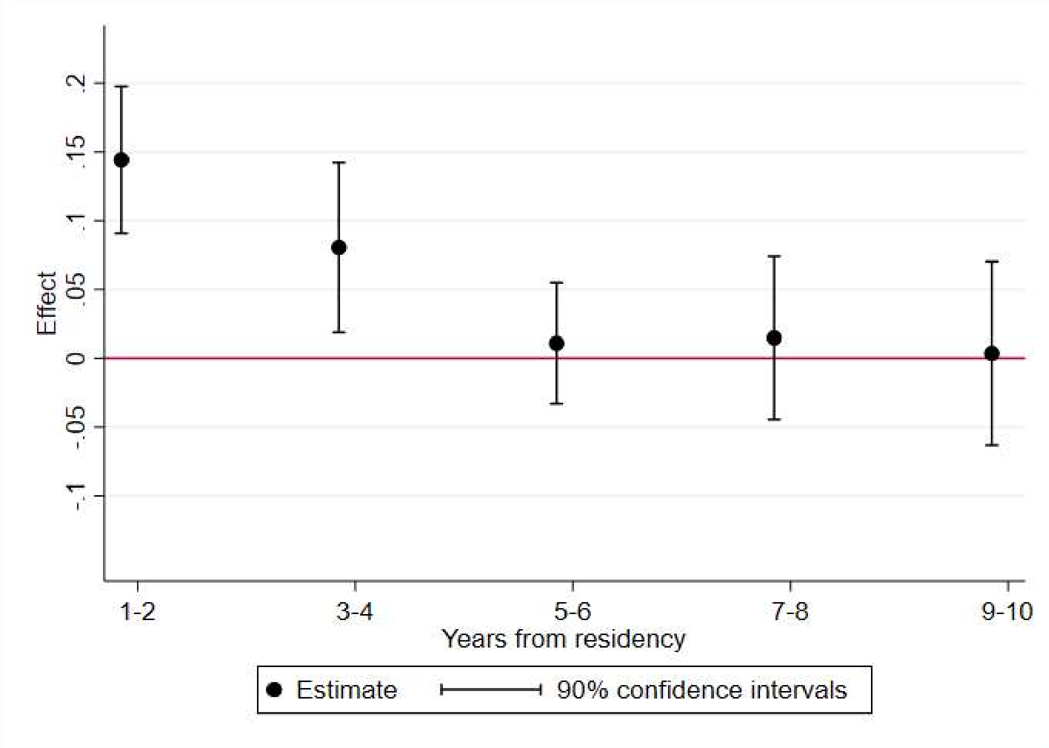




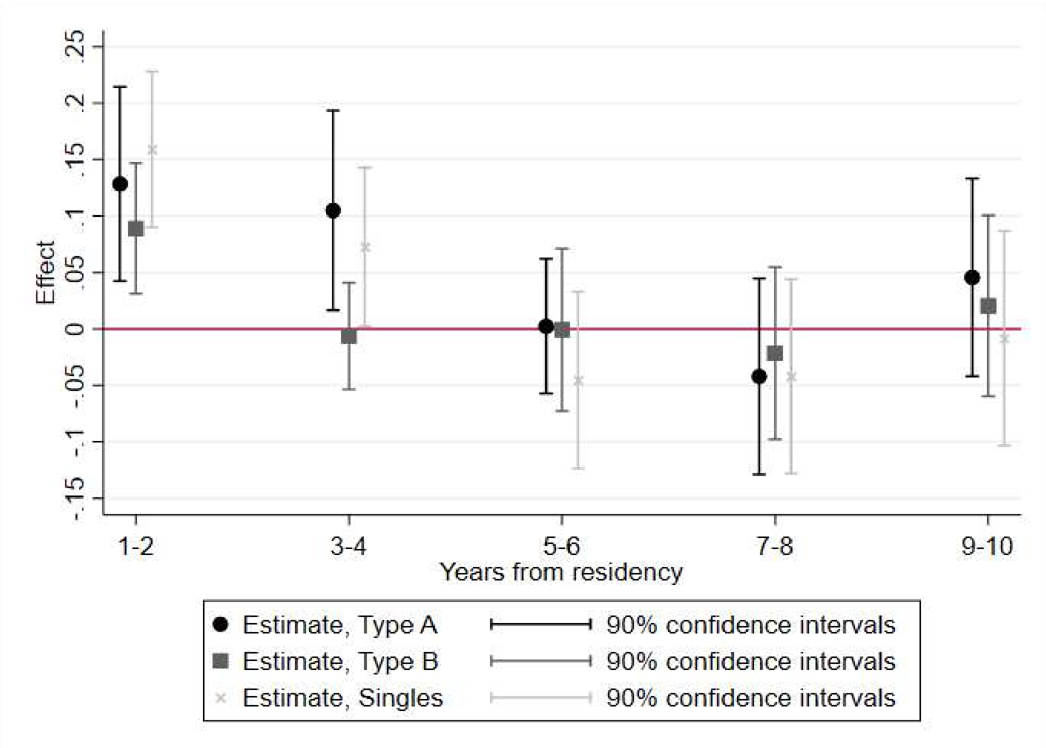
Note: The figure shows estimated differences in employment and 90% confidence intervals in the 1st and 2nd year after residency according to whether refugees were exposed to the repeal of the Start Aid (increasing transfers in 2012) marked by the vertical dashed line. When measuring employment in the 1st year after residency, the pre-repeal (control) years include those receiving residency in 2009-2011 (-2 to 0 on the x-axis in the figure) and the post-repeal (treatment) years include those receiving residency in 2012-2014 (1 to 3 on the x-axis in the figure). When measuring employment in the 2nd year after the repeal, the pre-repeal (control) years include those receiving residency in 2008-2010 (-2 to 0 on x-axis in the figure) and the post-repeal (treatment) years include those receiving residency in 2011-2013 (1 to 3 on the x-axis in the figure). Year 0 is the reference group in each estimation.

Figure 5. Effect of reform on males' employment rates, 1-10 years after residency.

A) Males' employment, all males



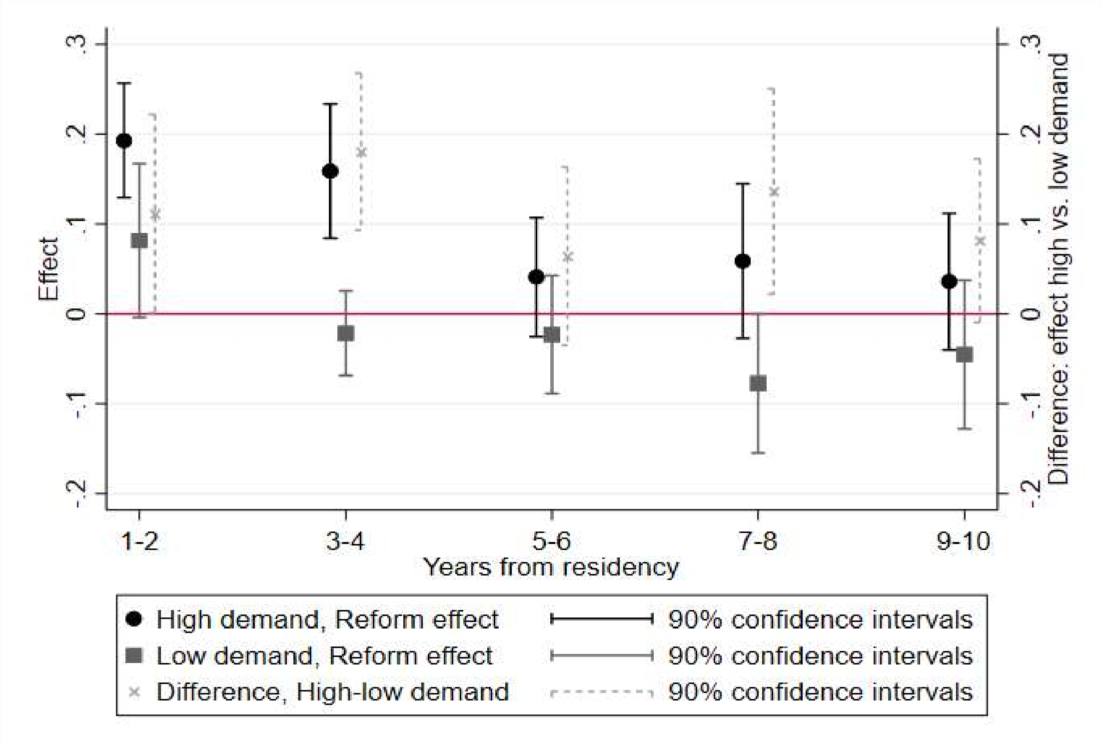
B) Males' employment by household type



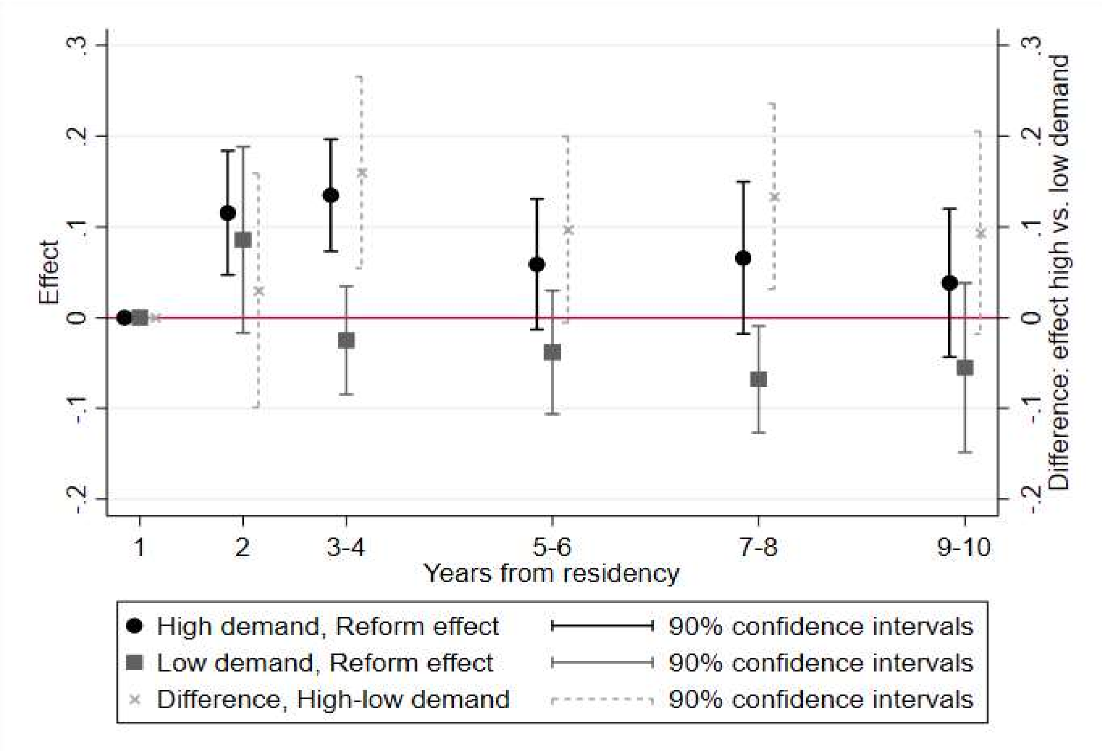
Note: The figure shows estimated effect of the reform and 90% confidence intervals on A) males' probability of being employed estimated by Eq. (1), and B) males' probability of being employed by household type estimated by Eq. (2). Standard errors are in A) clustered by residency month and in B) clustered on twoway level by residency month and household.

Figure 6. Effect of reform on males' employment rates, 1-10 years after residency, by local labor demand.

A) Males' employment in low and high demand areas.



B) Males' probability of staying in employment from year to year in low and high demand areas.



Note: The figure shows estimated effect of the reform and 90% confidence intervals on A) males' probability of being employed by local labor demand and B) males' probability of staying in employment from one year to the next. Both Fig. A) and B) are estimated by Eq. (3). Standard errors are clustered on twoway level by residency month and allocation municipality.

Table 1. Sample means of covariates and balancing tests, base sample aged 18-55 at residency.

	A) Sample means			*B) Balancing tests*	
	(1)	(2)	(3)	(4)	(5)
	All	Pre reform residency	Post reform residency	Conditional test	Unconditional test
Reform=1	0.371	0.000	1.000	-	-
	(0.483)	-	-		
Age at residency	32.625	32.700	32.498	-0.001	-0.873
	(8.270)	(8.311)	(8.202)	(0.001)	(0.561)
Female	0.507	0.475	0.560	0.014	0.056
	(0.500)	(0.499)	(0.496)	(0.010)	(0.040)
# of children	2.257	2.346	2.106	-0.005	-0.137
	(1.903)	(1.931)	(1.847)	(0.001)	(0.129)
Single	0.246	0.226	0.279	0.001	0.008
	(0.431)	(0.418)	(0.449)	(0.013)	(0.038)
Muslim countries	0.838	0.878	0.769	-	0.037
	(0.369)	(0.327)	(0.422)		(0.042)
Eastern Europe/former USSR	0.055	0.050	0.062	-0.012	-0.025
	(0.227)	(0.219)	(0.241)	(0.025)	(0.020)
Rest of the world	0.108	0.071	0.170	-0.033	-0.012
	(0.310)	(0.257)	(0.375)	(0.027)	(0.036)
Refugee permit status	0.618	0.635	0.588	-0.006	-0.049
	(0.486)	(0.482)	(0.492)	(0.017)	(0.056)
Observations	4,843	3,044	1,799	4,843	4,843

Note: The table shows sample means and balancing tests for the base sample of adults receiving residency +/- 18 months around the reform. Panel A presents sample means for all and by pre- and post-reform residency separately (with standard deviations in parentheses). Panel B presents estimation results from balancing tests (with standard errors in parentheses). Column 4 presents conditional balancing of covariates across the reform from regressing a dummy indicating whether residency was granted pre- or post-reform on all covariates and the running variable (allowing for different slopes in the running variable on each side of the cutoff). Table A.2 extends these results for alternative sample definitions. Column 5 presents unconditional balancing of covariates from 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). Table A.3 extends these results for alternative sample definitions. '# of children' refers to the number of children upon residency. 'Muslim countries' refers to refugeees from majority-Muslim countries and not the individual's religion. 'Refugee' refers to the individual receving residency based on refugee status (is residency given on grounds of being a refugee 1, or from being the spouse of an individual with refugee status, 0).

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

Table 2. Effect of reform on subsequent annual individual transfers, labor earnings (both measured in USD 1,000), employment, unemployment, and fraction not in the labor force.

	A) Transfer income		*B) Labor earnings*		*C) Employment rate*		*D) Unemployment rate*		*E) Not in labor force*	
Years since residency	Pre reform mean	Estimate	Pre reform mean	Estimate	Pre reform mean	Estimate	Pre reform mean	Estimate	Pre reform mean	Estimate
1	18.431	-9.775***	1.852	1.144***	0.103	0.092***	0.868	-0.164***	0.028	0.072***
	(7.663)	(0.407)	(6.353)	(0.400)	(0.305)	(0.022)	(0.339)	(0.027)	(0.164)	(0.014)
2	17.979	-8.320***	4.182	1.567***	0.188	0.070***	0.755	-0.158***	0.053	0.093***
	(8.655)	(0.446)	(10.401)	(0.541)	(0.391)	(0.019)	(0.430)	(0.028)	(0.224)	(0.020)
3-5	15.849	-4.956***	8.424	1.070**	0.323	0.041*	0.581	-0.104***	0.093	0.066***
	(8.760)	(0.457)	(13.273)	(0.451)	(0.390)	(0.021)	(0.401)	(0.015)	(0.230)	(0.012)
T-value: year 1 - year 3-5		7.875		0.123		1.677		-1.943		0.246
Observations	3,044	4,843	3,044	4,843	3,044	4,843	3,044	4,843	3,044	4,843

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, unemployed, and not in the labor force measured 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 also shows pre reform means of the outcome variables and tests of the differences between estimates for year 1 and years 3-5. All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors in parentheses. Standard errors are clustered by residency month.

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

Table 3. Effect of reform on subsequent labor market outcomes, by gender.

Years since residency	(1)	(2) Males	(3)	(4)	(5) Females	(6)
	Employment	Unemployment	Not in the labor force	Employment	Unemployment	Not in the labor force
1	0.160***	-0.155***	-0.004	0.037	-0.171***	0.132***
	(0.044)	(0.045)	(0.012)	(0.022)	(0.022)	(0.021)
2	0.132***	-0.137***	0.011	0.015	-0.172***	0.162***
	(0.035)	(0.038)	(0.023)	(0.019)	(0.029)	(0.025)
3-5	0.042	-0.069***	0.029	0.041**	-0.130***	0.093***
	(0.033)	(0.024)	(0.024)	(0.016)	(0.024)	(0.019)
T-value: year 1 - year 3-5	2.145	-1.686	0.932	-0.147	-1.259	1.377
Observations	2,390	2,390	2,390	2,453	2,453	2,453

Note: The table shows the estimated effects, by gender, of being granted residency after the reform relative to before the reform on the 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 since residency. The table also shows tests of the differences between estimates for year 1 and years 3-5. Columns 1-3 present results for males, and columns 4-6 present results for females. All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors in parentheses. Standard errors are clustered by residency month.

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

Table 4. Effect of reform on subsequent labor market outcomes, by gender and household type.

Treatment	Year	(1) Employment	(2) Unemployment	(3) Not in the labor force
A) Type A Couples, both granted residency after reform				
Males	1	0.153***	-0.202***	0.049
		(0.054)	(0.046)	(0.032)
	2	0.095*	-0.134**	0.039
		(0.059)	(0.064)	(0.036)
Females	1	0.079***	-0.169***	0.090***
		(0.033)	(0.051)	(0.031)
	2	0.081*	-0.110**	0.031
		(0.044)	(0.053)	(0.044)
B) Type B Couples, one granted residency after reform				
Males	1	0.075*	-0.101***	0.025
		(0.043)	(0.043)	(0.024)
	2	0.107**	-0.137***	0.029
		(0.046)	(0.055)	(0.032)
Females	1	0.031	-0.204***	0.173***
		(0.018)	(0.043)	(0.038)
	2	0.036	-0.209***	0.176***
		(0.024)	(0.046)	(0.042)
C) Singles				
Males	1	0.168**	-0.172***	0.004
		(0.065)	(0.075)	(0.0267)
	2	0.149*	-0.138*	0.009
		(0.074)	(0.082)	(0.043)
Females	1	0.028	-0.030	-0.018
		(0.044)	(0.114)	(0.058)
	2	0.009	-0.002	0.026
		(0.097)	(0.107)	(0.072)
D) Seperating employment effects and females labor force dropouts by dual versus single earners				
		Both spouses in employment	Only one spouse in employment	Female dropout when male employment
Type A couples	1	0.065**	0.103***	0.064***
		(0.030)	(0.044)	(0.019)
Type B couples	1	-0.001	0.109***	0.070***
		(0.015)	(0.038)	(0.022)

Note: The table shows the estimated effects of being granted residency after the reform on labor market outcomes by household type. Panels A-C) show effects for Type A and B couples (relative to pre reform couples), and singles (relative to pre reform singles) on the probability of being employed, unemployed, or not in the labor force. Panel D) further seperates couples' employment effects and labor force dropouts in year 1. The 1st outcome is a dummy=1 if both spouses were employed, the 2nd a dummy=1 if only one spouse was employed, and the 3rd a dummy=1 if the female was not in the labor force and her husband was employed. Panels A, B, D) are estimated by Eq. (2), and C) is estimated by Eq. (1). Standard errors (in parentheses) are clustered on twoway level by residency month and household for couples, and by residency month for singles.

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

Table 5. Effect of the reform on males' employment and job-type by assignment municipality.

	Year 1	Year 2	Years 3-5
A) Employment, using job openings in low / unskilled jobs			
High demand, Reform effect	0.184***	0.207***	0.097**
	(0.048)	(0.045)	(0.042)
Pre-reform mean	*0.164*	*0.298*	*0.465*
Low demand, Reform effect	0.125	0.042	-0.032
	(0.078)	(0.055)	(0.026)
Pre-reform mean	*0.174*	*0.274*	*0.443*
High-low difference in reform effect	0.059	0.165***	0.129***
	(0.072)	(0.068)	(0.045)
B) Employment, using average employment of non-Western immigrants			
High demand, Reform effect	0.163***	0.206***	0.096**
	(0.048)	(0.048)	(0.044)
Pre-reform mean	*0.178*	*0.306*	*0.475*
Low demand, Reform effect	0.157**	0.068	-0.013
	(0.071)	(0.051)	(0.030)
Pre-reform mean	*0.158*	*0.270*	*0.435*
High-low difference in reform effect	0.006	0.138**	0.109**
	(0.061)	(0.063)	(0.050)
C) Decomposing high-low difference by inflow and stay in employment			
Inflow from non-employment	0.059	0.132**	-0.004
	(0.072)	(0.063)	(0.031)
Stay in employment	-	0.033	0.133***
		(0.073)	(0.054)
D) Decomposing high-low difference by job-type			
Unskilled manual work			
Inflow from non-employment	0.033	0.144***	-0.014
	(0.055)	(0.053)	(0.020)
Stay in employment	-	0.023	0.008
		(0.049)	(0.028)
Work requiring some skills			
Inflow from non-employment	0.038	-0.012	0.009
	(0.045)	(0.037)	(0.018)
Stay in employment	-	0.010	0.125**
		(0.035)	(0.058)
Observations	2,390	2,390	2,390

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform for male refugees (aged 18-55 at the time of residency) assigned to municipalities with high/low local labor demand. High/low labor demand is defined in Panels A, C and D 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. Panels A and B show employment effects in years 1, 2, and 3-5 since residency. Panel C uses that *employment = inflow + stay in employment* to decompose the difference in employment effects. Panel D decomposes employment effects into unskilled manual work and work requiring some skills by inflow and stay in employment. Standard errors are clustered on twoway level by residency month and allocation municipality.

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

Table 6. Effects of the reform on average employment, labor earnings, disposable income, and public expenditures for years 1-5, by assignment municipality's labor demand.

	(1)	(2)	(3)	(4)	(5)
Percentiles	*Full sample*	*5th*	*50th*	*95th*	*Difference 95th-5th*
A) Employment					
Pre reform	0.252	0.234	0.244	0.275	0.028
Post reform	0.302	0.261	0.294	0.352	0.091
Reform effect	0.050**	0.027	0.050*	0.077**	0.050
	(0.024)	(0.030)	(0.027)	(0.035)	(0.046)
B) Labor earnings, $1,000					
Pre reform	6.258	5.875	6.065	6.824	0.949
Post reform	7.103	5.416	6.626	9.291	3.875
Reform effect	0.845	-0.459	0.561	2.467***	2.926**
	(0.940)	(0.708)	(1.038)	(1.400)	(1.400)
C) Public expenditures, $1,000					
Pre reform	11.190	11.440	11.341	10.841	0.338
Post reform	5.967	7.403	6.084	4.534	-2.869
Reform effect	-5.223***	-4.037***	-5.257***	-6.307***	-2.270**
	(0.415)	(0.698)	(0.580)	(0.698)	(0.987)
D) Disposable income, $1,000					
Pre reform	17.819	17.633	17.756	18.077	0.444
Post reform	13.301	12.980	12.944	14.110	1.130
Reform effect	-4.518***	-4.653***	-4.812***	-3.966***	0.687
	(0.523)	(0.538)	(0.572)	(0.705)	(0.887)

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform for refugees (aged 18-55 at the time of residency) assigned to municipalities with different local labor demand. Panel A: effects on average employment in years 1-5. Panel B: effects on average labor earnings in years 1-5. Panel C: effects on average public expenditures (transfer income minus tax payments) years 1-5. Panel D: effects on average post-tax disposable income (pre-tax gross income minus tax payments) in years 1-5. Rank of local labor demand is defined from the municipal predicted ratio of the number of job openings in low / unskilled work relative to the number of unemployed individuals. Column 1 presents estimates of Eq. (1). Columns 2-4 present results from three regressions for each outcome where we estimate Eq. (1) weighting observations by kernels with center at the 5th, 50th, and 95th percentiles of local labor demand, respectively, using an Epanechnikov weighting kernel. Hence the estimates reflect the reform effects in municipalities close the 5th, 50th, and 95th percentiles. Column 5 presents the difference between rows 4 and 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. Observations: 4,843.

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

Table 7. Effects of the reform on the probability of having monthly disposable income below $500, $750, and $1,000, respectively

Income per month	Year 1	Year 2	Year 3-5	T-value: year 1 - year 3-5
A) Disposable income<$500				
Pre reform	0.027	0.024	0.021	
Post reform	0.311	0.237	0.115	
Reform effect	0.284***	0.213***	0.094***	5.554
	(0.027)	(0.024)	(0.021)	
B) Disposable income<$750				
Pre reform	0.087	0.071	0.047	
Post reform	0.557	0.356	0.200	
Reform effect	0.470***	0.285***	0.153***	9.307
	(0.026)	(0.025)	(0.022)	
C) Disposable income<$1,000				
Pre reform	0.254	0.188	0.121	
Post reform	0.755	0.532	0.644	
Reform effect	0.501***	0.344***	0.223***	5.012
	(0.049)	(0.028)	(0.026)	
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 for refugees (aged 18-55 at the time of residency) on the probability of having post-tax disposable income below $500, $750, and $1,000 per month. The outcome is defined by dividing annual disposable income by 12 thereby expressing the average income in each month in that year. The table shows results for disposable income in year 1, year 2, and the average of year 3-5 after residency. The column "T-value: year 1 - year 3-5" shows the t-value from a test of difference between the year 1 and year 3-5 estimates. Standard errors are clustered by residency month.

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

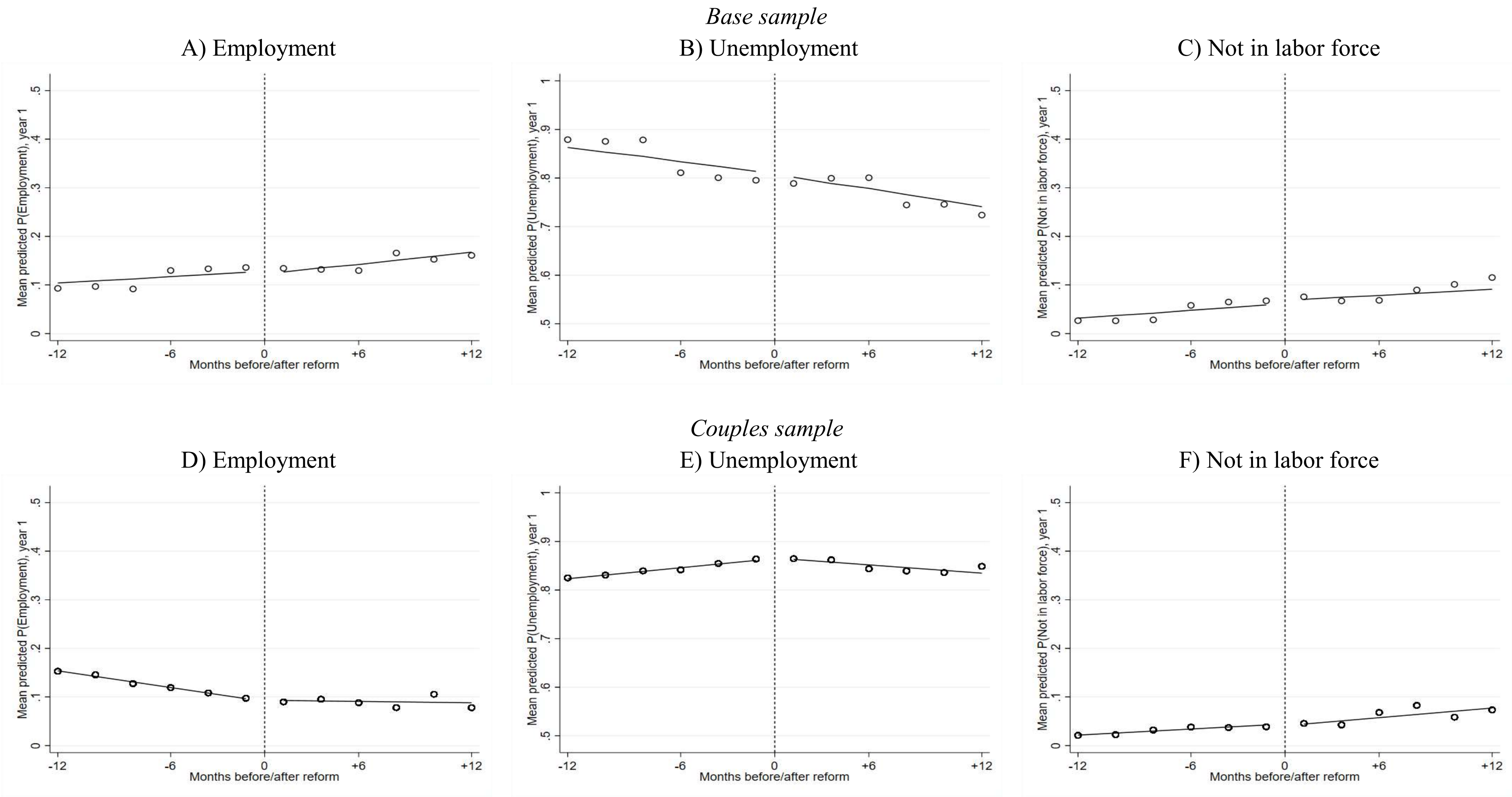
Table 8. Effects of reform on crime for adults, year 1 and 5 after residency, by gender and local labor demand.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	All adults				**Males**				**Females**			
	P(crime)		*Number of crimes*		*P(crime)*		*Number of crimes*		*P(crime)*		*Number of crimes*	
	Year 1	Year 5	Year 1	Year 5	Year 1	Year 5	Year 1	Year 5	Year 1	Year 5	Year 1	Year 5
A) All crime												
Reform effect	0.022**	0.035**	0.026**	0.054**	0.015	0.049*	0.023	0.092**	0.026**	0.029*	0.029**	0.035
	(0.010)	(0.013)	(0.011)	(0.019)	(0.015)	(0.026)	(0.017)	(0.040)	(0.013)	(0.017)	(0.014)	(0.027)
Pre-reform mean	0.018	0.072	0.021	0.094	0.024	0.089	0.027	0.116	0.013	0.058	0.016	0.078
B) Property												
Reform effect	0.022**	0.033**	0.027**	0.052**	0.012	0.040	0.019	0.071	0.026**	0.026	0.030**	0.042
	(0.011)	(0.014)	(0.012)	(0.020)	(0.016)	(0.030)	(0.019)	(0.047)	(0.013)	(0.018)	(0.014)	(0.026)
Pre-reform mean	0.016	0.058	0.019	0.077	0.020	0.062	0.023	0.083	0.013	0.056	0.016	0.072
C) Theft from supermarket												
Reform effect	0.020**	0.024*	0.023**	0.038**	0.011	0.019	0.011	0.034	0.023**	0.025	0.028**	0.040*
	(0.008)	(0.013)	(0.009)	(0.016)	(0.009)	(0.019)	(0.009)	(0.028)	(0.011)	(0.016)	(0.011)	(0.021)
Pre-reform mean	0.010	0.037	0.011	0.049	0.009	0.032	0.009	0.042	0.011	0.042	0.013	0.054
D) Violence												
Reform effect	-0.000	0.002	-0.000	0.001	0.004	0.018	0.004	0.024	-	-0.010	-	-0.004
	(0.003)	(0.009)	(0.003)	(0.010)	(0.009)	(0.021)	(0.009)	(0.022)		(0.009)		(0.004)
Pre-reform mean	0.002	0.012	0.002	0.013	0.004	0.024	0.004	0.024	0.000	0.003	0.000	0.004
Observations	3,406	3,406	3,406	3,406	1,376	1,376	1,376	1,376	2,030	2,030	2,030	2,030

Note: The table shows reform effects on and pre-reform means of the probability of having received a crime conviction and the accumlated number of crime convictions for all adults and seperately for males and females between 18-45 at the time of residency (as very few above age 45 commit crime) with children. The table shows results for all crimes, property crimes, shoplifting from supermarkets, and violence. All crimes consist of "property", "violent", and a residual "other crime" (the two former categories drive the main results - results for other crime is available upon request). "Theft from supermarket" is a subset of "property" crime. Standard errors are clustered by residency month. Observations (all adults below age 45 at residency with children): 3,406.
* $p<0.1$; ** $p<0.05$; *** $p<0.01$

A. Appendix Figures and Tables

Figure A.1. Labor market outcomes 1 year after residency, predicted from background characteristics alone.

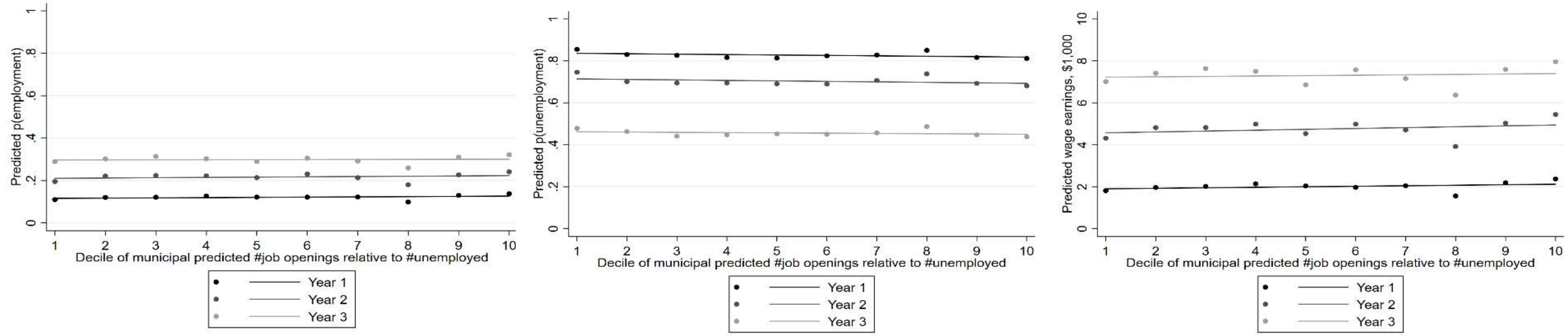


Note: The figure shows employment, unemployment, and not in the labor force rates in the first year after residency predicted from OLS estimations using the full set of covariates (see Table 1) for the base sample (Panels A-C) and couples sample (Panels D-F). The figure shows the predicted outcomes plotted by timing of residency relative to the reform, and the figure contains linear slopes of the predictions before and after the reform, to mimic our estimation strategy. The dashed vertical line indicates the timing of the reform in July 2002.

Figure A.2. Outcomes predicted from observable characteristics plotted across the local labor demand indicators.

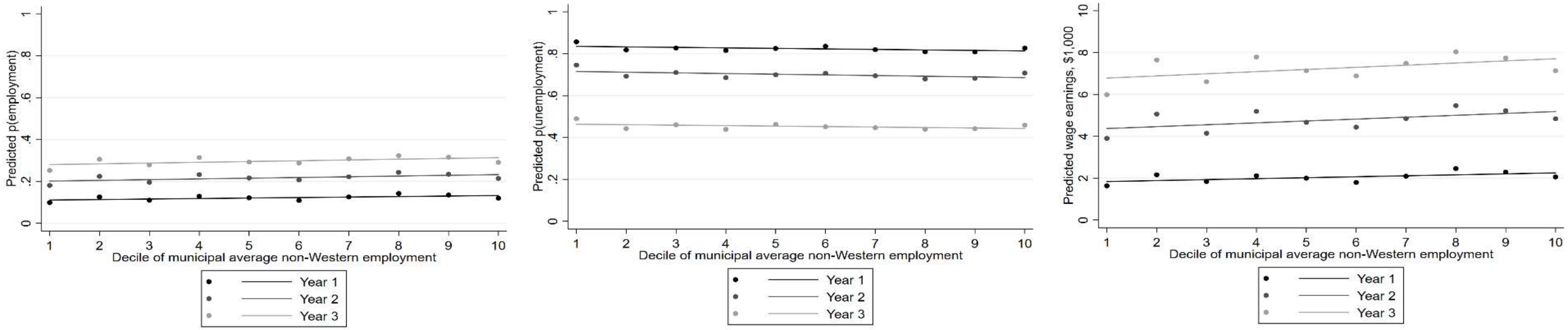
Municipal predicted job openings relative to number of unemployed

A) Employment B) Unemployment C) Labor earnings



Municipal average non-Western employment

D) Employment E) Unemployment F) Labor earnings

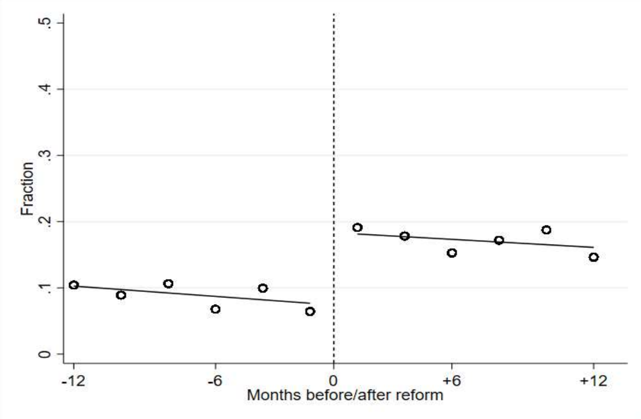


Note: The figure shows employment rates, unemployment rates, and labor earnings during the first year post residency for deciles of the two local labor demand indicators as predicted from an OLS regression using the covariates described in Table 1. Panels A-C show the equivalent across deciles of municipal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

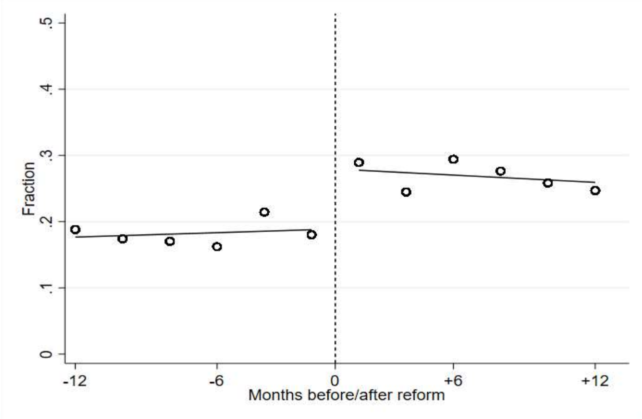
Figure A.3. Labor market outcomes first two years after residency.

Employment

A) Year 1

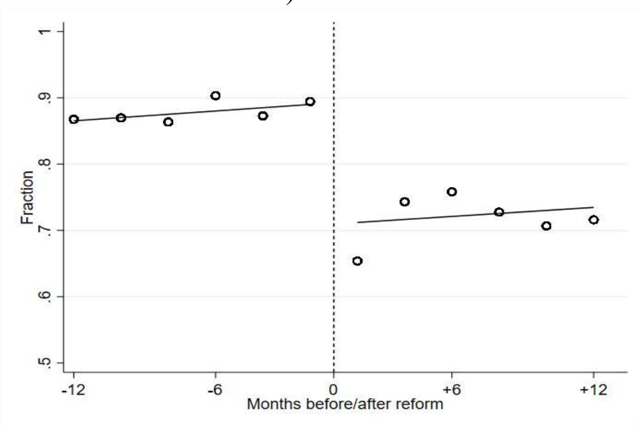


B) Year 2

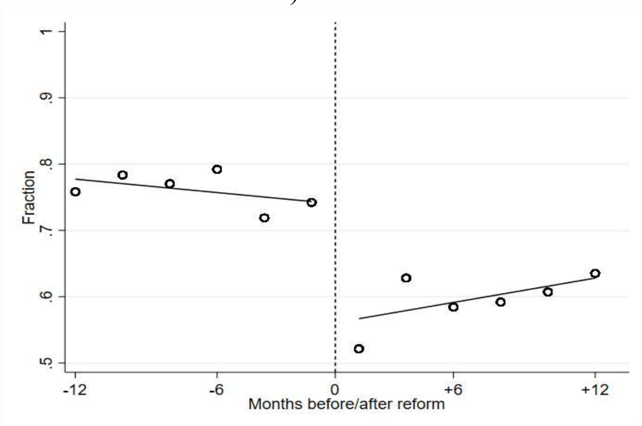


Unemployment

C) Year 1

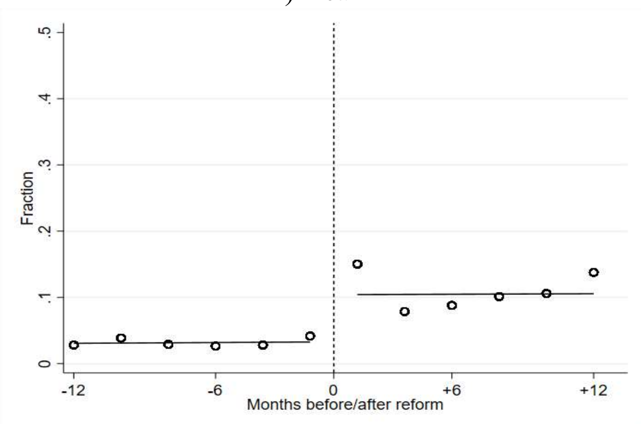


D) Year 2

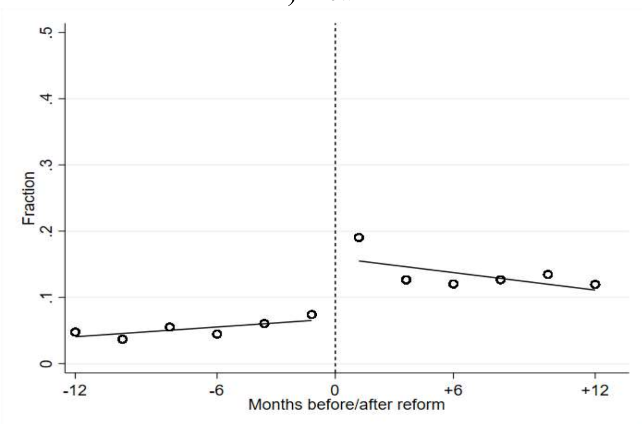


Not in the labor force

E) Year 1

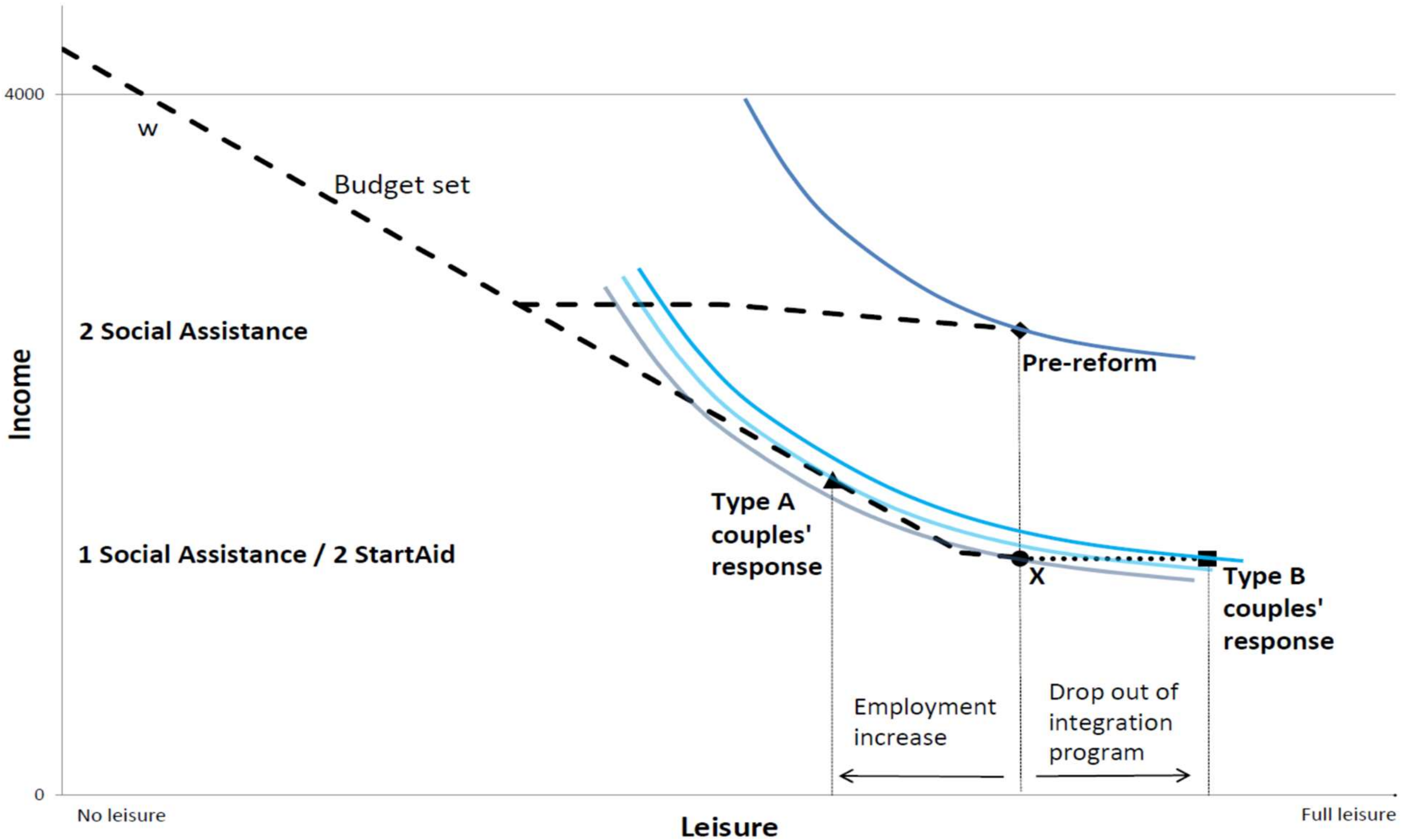


F) Year 2



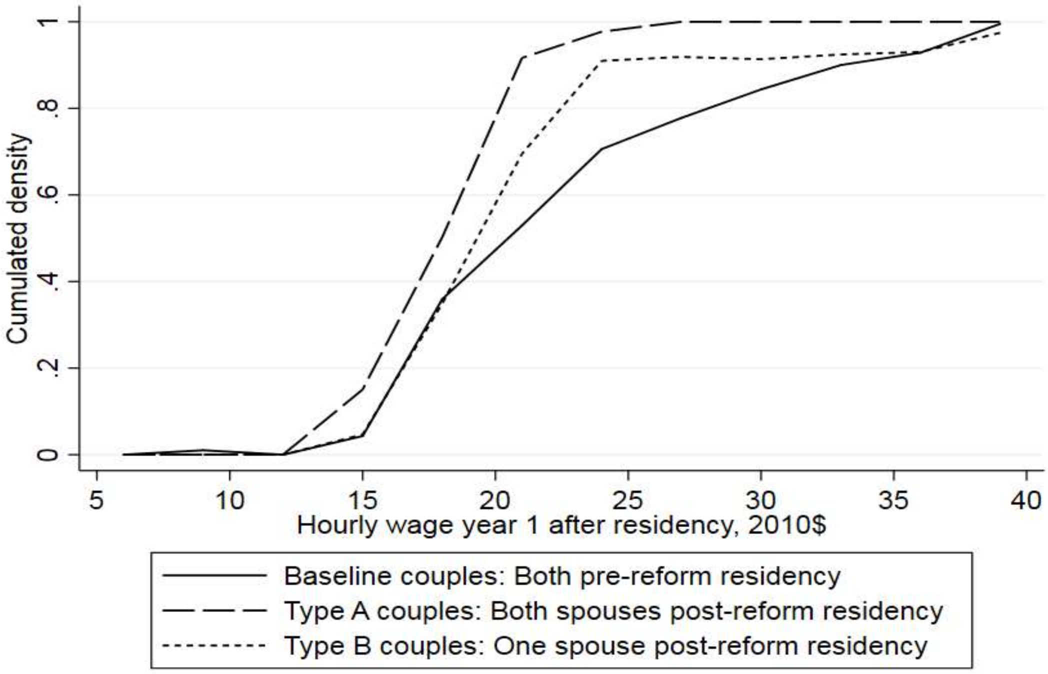
Note: The figure shows employment (A-B), unemployment (C-D), and not in the labor force (E-F) rates in year 1 and 2 by timing of residency relative to the reform. The dashed vertical line indicates the timing of the reform in July 2002. The figure contains linear slopes of the outcomes before and after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

Figure A.4: 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.

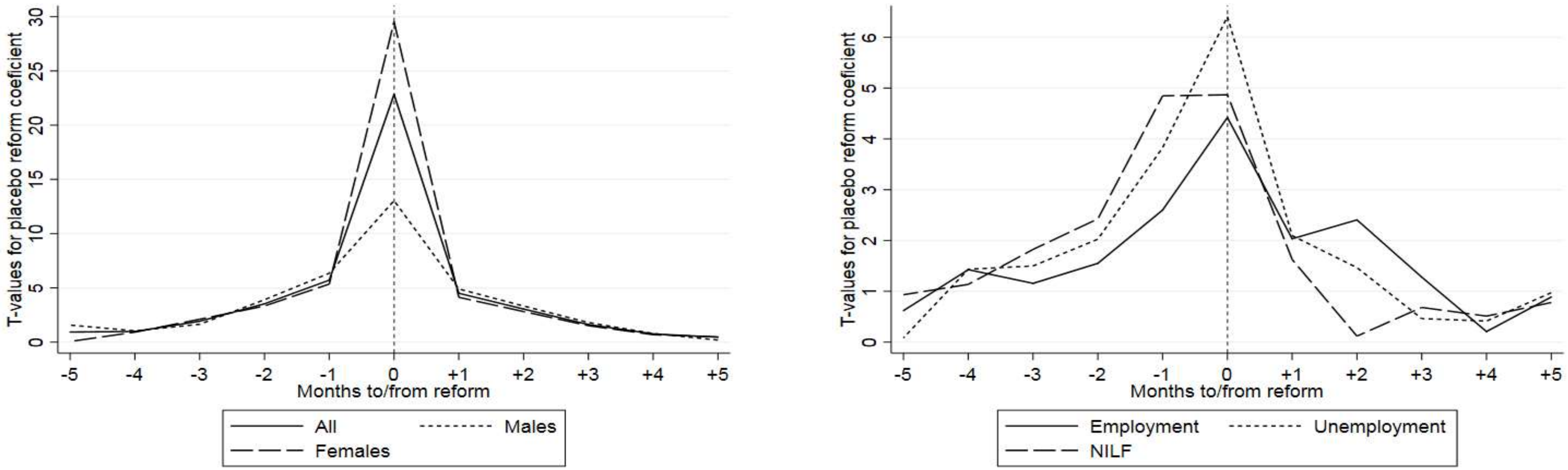
Figure A.5. Change in distributions of hourly wage rates as result of the reform, by household type, males in couples, year 1.



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≤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.6. Placebo reform estimates before and after actual timing of reform (time 0).

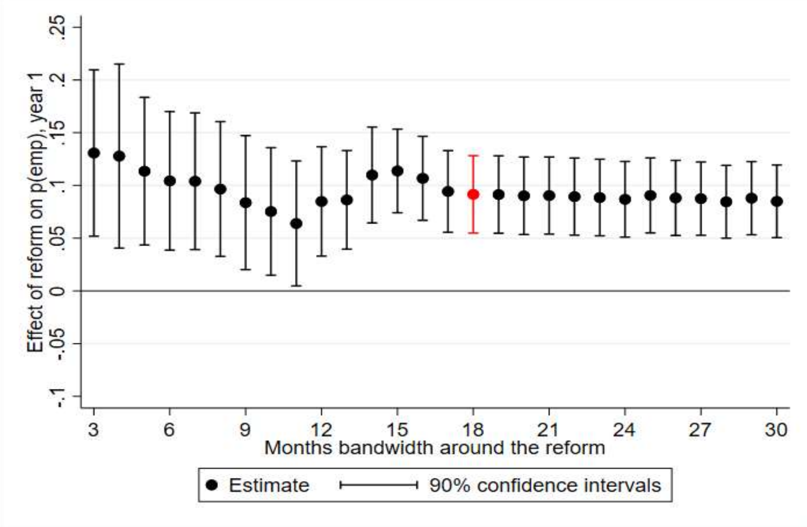
A) Placebo reform estimates, transfers year 1 | *B) Placebo reform estimates, labor market outcomes year 1*



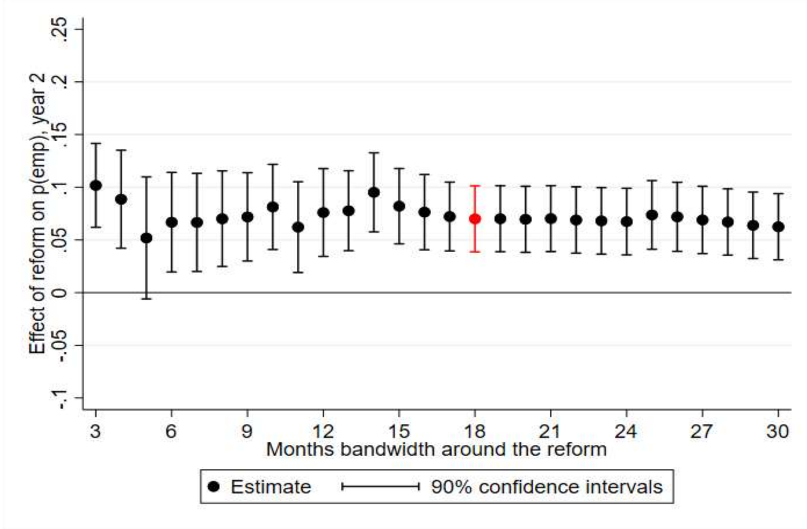
Note: The figure shows the t-values of placebo reform estimates from 5 months before to 5 months after the actual reform (at time 0) on transfers at individual level (Panel A) and employment, unemployment and not in the labor force rates, (NILF) (Panel B). Each estimate is constructed from a sample of +-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.7. The effects of the reform on employment using different sampling bandwidths around the reform.

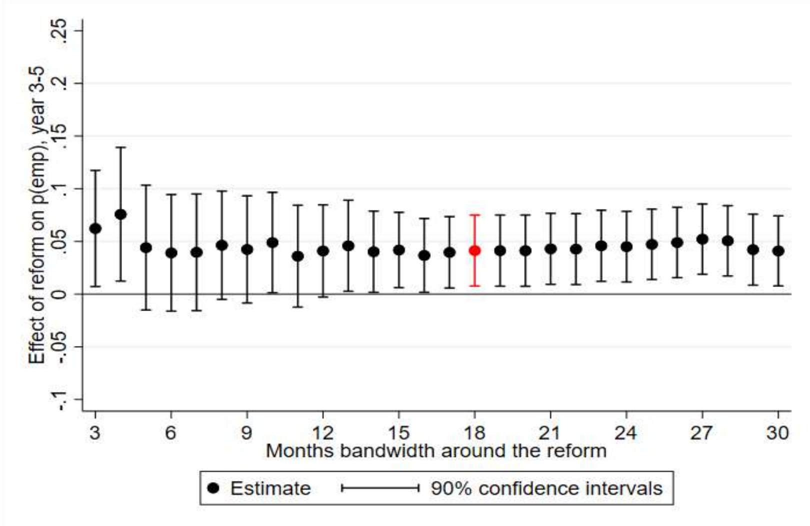
A) Employment in year 1 after residency



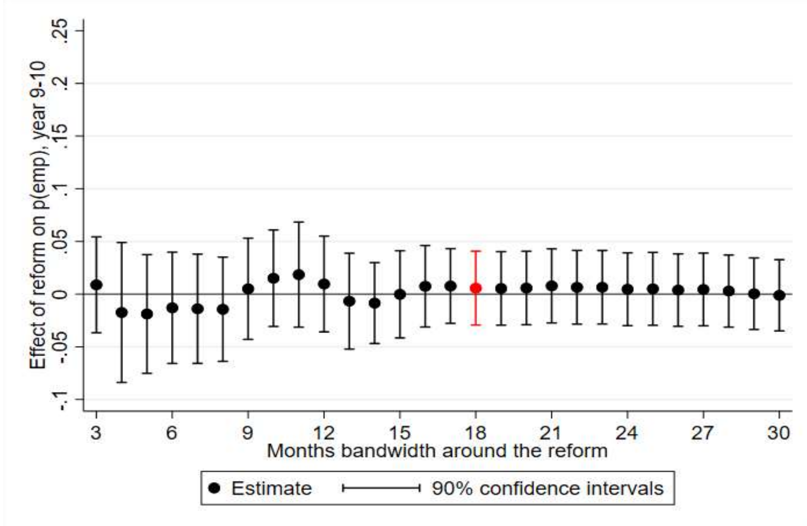
B) Employment in year 2 after residency



C) Employment in years 3-5 after residency

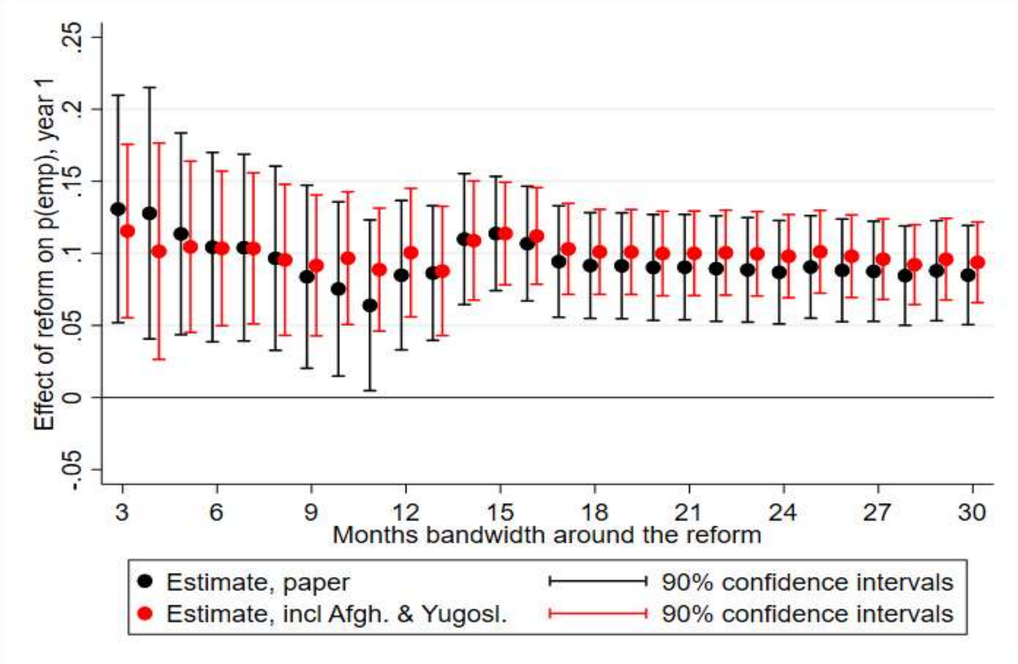


D) Employment in years 9-10 after residency



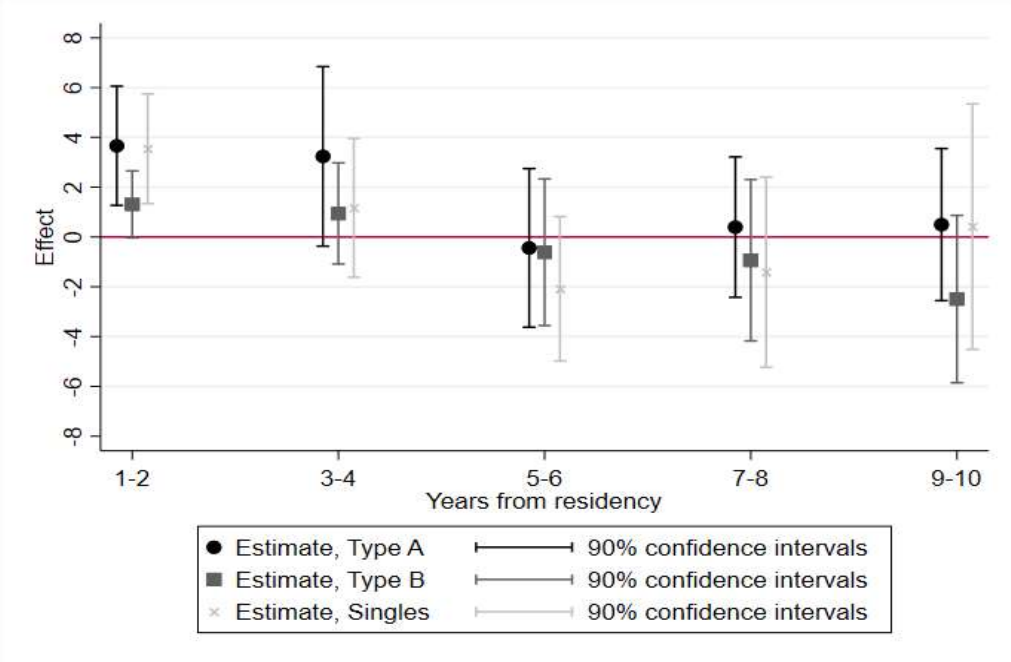
Note: The figure shows the estimated effects of being granted residency after the reform relative to before the reform on the subsequent employment probability in years 1, 2, 3-5 and 9-10 after residency. The figure shows estimates for different sampling bandwiths from +-3 months around the reform to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

Figure A.8. 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.

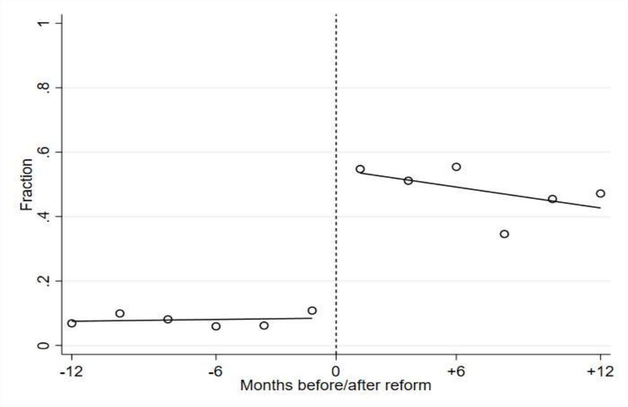
Figure A.9. Effect of reform on males' labor earnings, 1-10 years after residency.



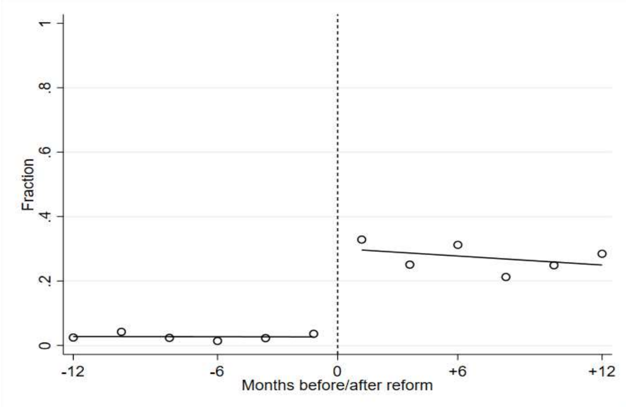
Note: The figure shows estimated effect of the reform and 90% confidence intervals on males' labor earnings in year 1-10 from residency. Standard errors are clustered on twoway level by residency month and household for couples and by residency month for singles.

Figure A.10. Fraction with low disposable income and crime convictions by crime type.

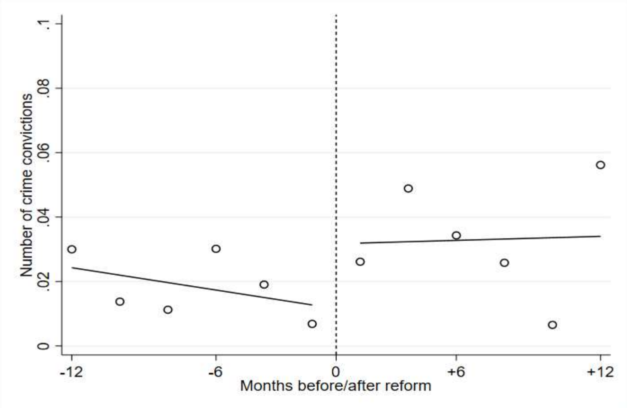
A) Disposable income per month<$750



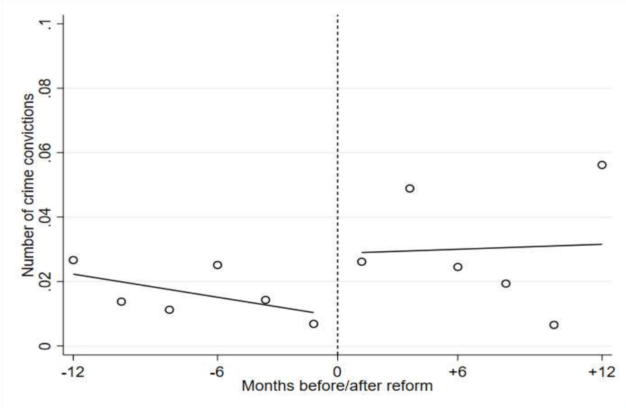
B) Disposable income per month<$500



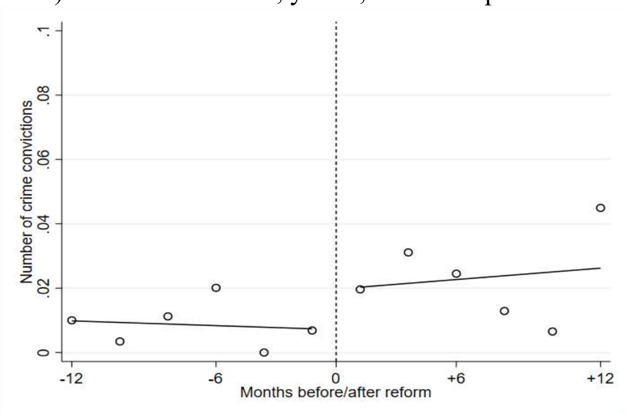
C) Crime convictions, year 1, all crime



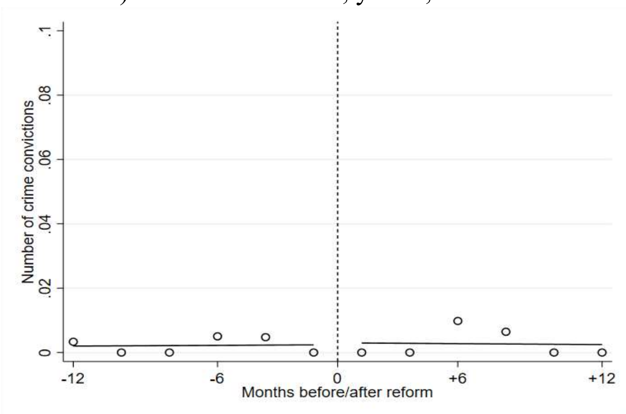
D) Crime convictions, year 1, property crime



E) Crime convictions, year 1, theft in supermarket



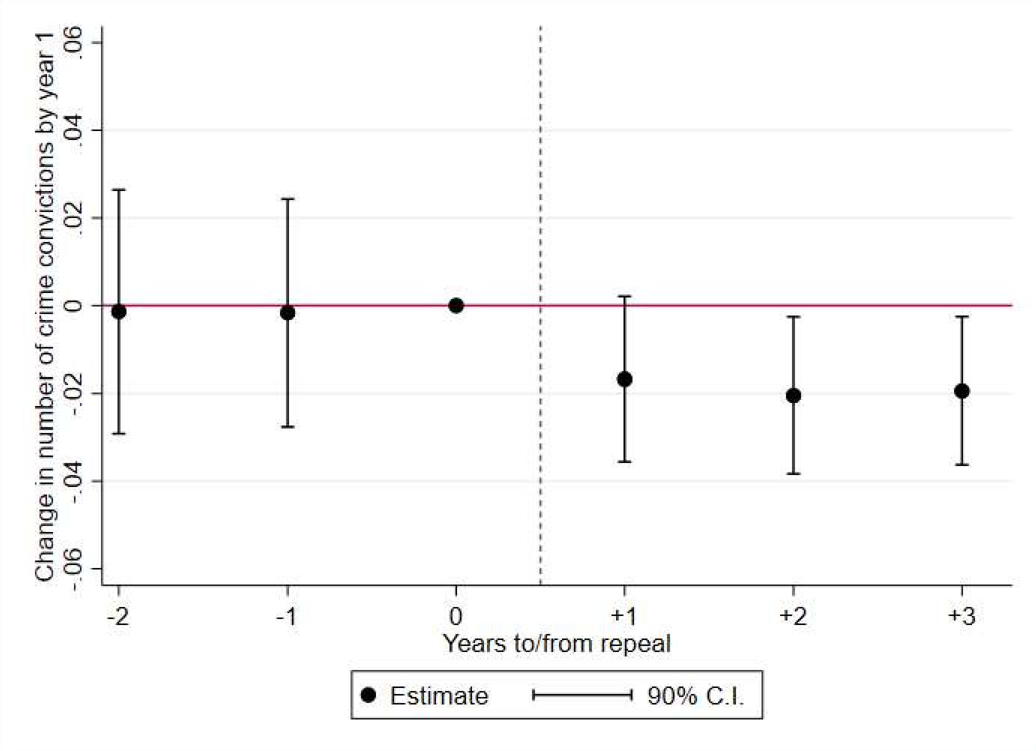
F) Crime convictions, year 1, violence



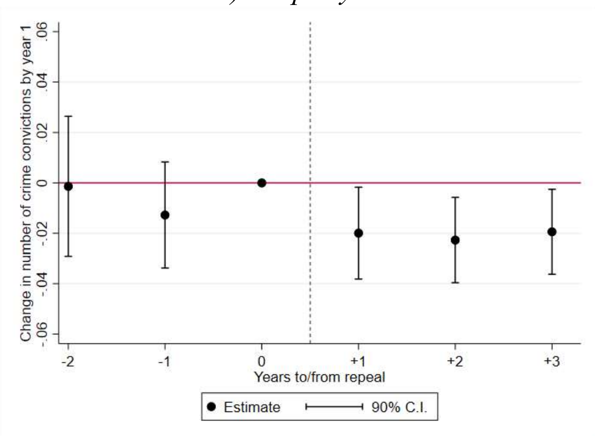
Note: The figure shows average outcomes from Tables 7 and 8 plotted by timing of residency relative to the reform. Panels A-B show the the fraction with post-tax disposable income below $500 and $750 per month. Panels C-F show, by crime type, the average number of crime convictions for all adults aged 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.11. Crime conviction differences in year 1 after residency around the repeal for females.

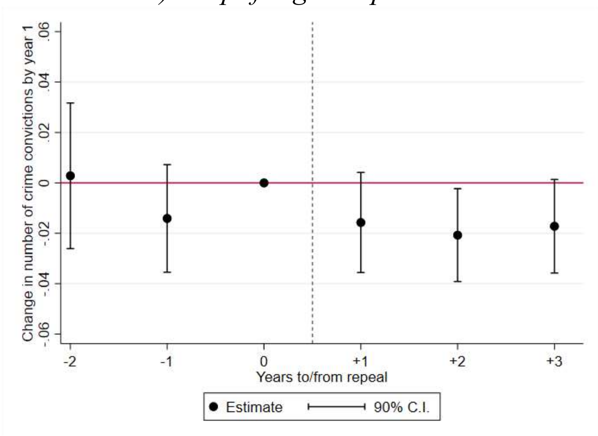
A) All crime



B) Property crime



C) Shoplifting in supermarkets



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.

			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*	
Status	Age	Children	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.

	A) Base sample	*B) Full sample*	*C) Couples sample*
Age at residency	-0.001	-0.000	-0.002**
	(0.001)	(0.001)	(0.001)
Female	0.014	0.007	0.015
	(0.010)	(0.015)	(0.024)
# of children	-0.005	-0.004	-0.010
	(0.001)	(0.004)	(0.006)
Single	0.001	-0.007	-
	(0.013)	(0.012)	
Eastern Europe/former USSR	-0.012	-0.034	-0.043
	(0.025)	(0.027)	(0.038)
Rest of the world	-0.033	-0.015	0.011
	(0.027)	(0.023)	(0.041)
Refugee permit status	-0.006	0.000	0.023
	(0.017)	(0.015)	(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	-0.934	-0.841	-1.904***
	(0.561)	(0.557)	(0.767)	(0.580)
Female	0.056	0.042	0.022	0.062
	(0.040)	(0.036)	(0.027)	(0.047)
# of children	-0.137	-0.104	-0.073	-0.078
	(0.129)	(0.104)	(0.046)	(0.087)
Single	0.008	0.004	0.015	-
	(0.038)	(0.037)	(0.022)	
Muslim countries	0.037	0.028	0.040	0.015
	(0.042)	(0.036)	(0.035)	(0.049)
Eastern Europe/former USSR	-0.025	-0.020	-0.023	-0.031
	(0.020)	(0.017)	(0.018)	(0.027)
Rest of the world	-0.012	-0.008	-0.017	0.016
	(0.036)	(0.031)	(0.030)	(0.041)
Refugee permit status	-0.049	-0.033	-0.026	0.013
	(0.056)	(0.058)	(0.057)	(0.073)
Waiting time in asylum center	0.994	0.553	-	-
	(2.048)	(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	0.031	0.031	0.301	0.114	0.097	0.056	-0.024	-0.065	-0.028	-0.023	-0.028	0.041	0.096	0.148
	(0.347)	(0.245)	(0.020)	(0.173)	(0.159)	(0.148)	(0.141)	(0.133)	(0.125)	(0.115)	(0.107)	(0.098)	(0.095)	(0.092)	(0.088)
B) All refugees	-0.101	-0.101	-0.190	-0.147	-0.097	-0.076	-0.094	-0.155	-0.135	-0.116	-0.108	-0.041	0.020	0.067	0.100
	(0.259)	(0.183)	(0.148)	(0.133)	(0.123)	(0.116)	(0.109)	(0.102)	(0.093)	(0.085)	(0.078)	(0.074)	(0.071)	(0.068)	(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 variale 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.

	A) Municipal average non-Western employment				*B) Job openings relative to number of unemployed*				*C) Predicted job openings relative to number of unemployed*			
	(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.

	Geographic mobility		*Municipal activation requirements*	
	(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				
High demand				
Reform effect	0.013	0.005	0.158***	-0.330***
	(0.017)	(0.009)	(0.026)	(0.046)
Pre reform mean	0.033	0.013	0.074	0.822
Low demand				
Reform effect	0.013	0.007	0.159***	-0.329***
	(0.020)	(0.013)	(0.027)	(0.048)
Pre reform mean	0.072	0.021	0.059	0.783
High-low difference				
Reform effect	0.026	-0.002	-0.001	-0.001
	(0.026)	(0.016)	(0.036)	(0.058)
B) Using average employment of non-Western immigrants				
High demand				
Reform effect	0.014	0.014	0.162***	-0.313***
	(0.014)	(0.009)	(0.031)	(0.048)
Pre reform mean	0.039	0.010	0.072	0.810
Low demand				
Reform effect	-0.009	-0.001	0.156***	-0.345***
	(0.022)	(0.014)	(0.024)	(0.040)
Pre reform mean	0.061	0.023	0.062	0.800
High-low difference				
Reform effect	0.022	0.014	0.005	0.032
	(0.027)	(0.018)	(0.038)	(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	0.002	0.008	0.002	0.008	0.005	0.063***
	(0.004)	(0.003)	(0.006)	(0.005)	(0.006)	(0.003)	(0.021)
2	0.003	-0.002	0.008	0.002	0.003	0.001	0.054**
	(0.006)	(0.006)	(0.009)	(0.008)	(0.008)	(0.003)	(0.025)
3-5	0.003	-0.002	-0.005	-0.001	-0.009	-0.004	0.059***
	(0.005)	(0.004)	(0.011)	(0.007)	(0.011)	(0.003)	(0.019)
B) <12 years education							
1	-0.000	-0.000	0.007	-0.002	0.008	0.011	0.072***
	(0.002)	(0.003)	(0.007)	(0.007)	(0.006)	(0.006)	(0.025)
2	0.004	0.003	0.011	0.007	-0.004	0.002	0.028
	(0.003)	(0.007)	(0.011)	(0.008)	(0.010)	(0.003)	(0.039)
3-5	0.001	-0.005	-0.003	-0.006	-0.012	-0.004	0.056**
	(0.004)	(0.005)	(0.014)	(0.006)	(0.014)	(0.003)	(0.022)
C) >=12 years education							
1	0.010	0.004	0.007	0.005	0.007	-0.000	0.052*
	(0.007)	(0.004)	(0.008)	(0.005)	(0.009)	(0.003)	(0.030)
2	0.004	-0.006	0.005	-0.003	0.012	0.000	0.079**
	(0.010)	(0.008)	(0.012)	(0.015)	(0.011)	(0.006)	(0.037)
3-5	0.009	0.004	-0.007	0.006	-0.004	-0.003	0.060**
	(0.010)	(0.007)	(0.014)	(0.013)	(0.016)	(0.005)	(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)
		Males			Females	
	Employment	Unemployment	Not in the labor force	Employment	Unemployment	Not in the labor force
A) Full sample						
1	0.160***	-0.155***	-0.004	0.037	-0.171***	0.132***
	(0.044)	(0.045)	(0.012)	(0.022)	(0.022)	(0.021)
2	0.132***	-0.137***	0.011	0.015	-0.172***	0.162***
	(0.035)	(0.038)	(0.023)	(0.019)	(0.029)	(0.025)
3-5	0.042	-0.069***	0.029	0.041**	-0.130***	0.093***
	(0.033)	(0.024)	(0.024)	(0.016)	(0.024)	(0.019)
B) <12 years of education						
1	0.193***	-0.192***	0.001	0.034	-0.176***	0.139***
	(0.060)	(0.058)	(0.019)	(0.024)	(0.028)	(0.026)
2	0.127**	-0.155**	0.036	-0.003	-0.145***	0.151***
	(0.062)	(0.066)	(0.024)	(0.025)	(0.037)	(0.028)
3-5	0.034	-0.087***	0.055*	0.020	-0.118***	0.102***
	(0.039)	(0.032)	(0.029)	(0.033)	(0.039)	(0.022)
C) >=12 years of education						
1	0.133**	-0.126**	-0.007	0.035	-0.159***	0.124***
	(0.051)	(0.051)	(0.017)	(0.035)	(0.034)	(0.045)
2	0.141***	-0.128**	-0.010	0.038	-0.208***	0.177***
	(0.048)	(0.047)	(0.036)	(0.043)	(0.046)	(0.048)
3-5	0.057	-0.061*	0.006	0.077**	-0.157***	0.085***
	(0.047)	(0.036)	(0.027)	(0.029)	(0.027)	(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***	-0.793***
	(0.283)	(0.260)
Type B couples	-0.375	-0.155
	(0.489)	(0.284)
Singles	-1.049***	0.104
	(0.477)	(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*	-0.323*
	(0.398)	(0.190)
Elasticity of crime with respect to benefit levels	-1.480***	-0.883**
	(0.526)	(0.363)

Note: The table shows the implied labor earnings and crime elastiticies 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	*A) Transfers*		*B) Labor earnings*		*C) Employment rate*		*D) Unemployment rate*		*E) Not in labor force*	
	Placebo reform	Actual reform	Placebo reform	Actual reform	Placebo reform	Actual reform	Placebo reform	Actual reform	Placebo reform	Actual reform
1	-0.070	-9.775***	0.308	1.144***	-0.000	0.092***	0.002	-0.164***	0.003	0.072***
	(0.249)	(0.407)	(0.241)	(0.400)	(0.014)	(0.022)	(0.021)	(0.027)	(0.014)	(0.014)
2	-0.294	-8.320***	0.649	1.567***	0.002	0.070***	-0.012	-0.158***	0.012	0.093***
	(0.278)	(0.446)	(0.511)	(0.541)	(0.018)	(0.019)	(0.025)	(0.028)	(0.014)	(0.020)
3-5	-0.326	-4.956***	0.262	1.070**	0.002	0.041*	-0.003	-0.104***	-0.002	0.066***
	(0.331)	(0.457)	(0.697)	(0.451)	(0.018)	(0.021)	(0.018)	(0.015)	(0.020)	(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***	0.098***	0.054***	0.085***	0.100***
	(0.022)	(0.035)	(0.016)	(0.030)	(0.018)
2	0.070***	0.070**	0.046**	0.075***	0.094***
	(0.019)	(0.026)	(0.020)	(0.023)	(0.015)
3-5	0.041*	0.039	0.045**	0.085*	0.076***
	(0.021)	(0.031)	(0.021)	(0.030)	(0.016)
Observations	4,843	4,843	4,439	3,362	7,456
B) Unemployment					
1	-0.164***	-0.202***	-0.109***	-0.173***	-0.183***
	(0.027)	(0.042)	(0.015)	(0.035)	(0.021)
2	-0.158***	-0.188***	-0.098***	-0.188***	-0.190***
	(0.028)	(0.045)	(0.020)	(0.035)	(0.023)
3-5	-0.104***	-0.090*	-0.097***	-0.083***	-0.141***
	(0.015)	(0.023)	(0.020)	(0.020)	(0.011)
Observations	4,843	4,843	4,439	3,362	7,456
C) Not in labor force					
1	0.072***	0.098***	0.056***	0.085***	0.082***
	(0.014)	(0.019)	(0.006)	(0.019)	(0.012)
2	0.093***	0.123***	0.056***	0.116***	0.100***
	(0.020)	(0.030)	(0.009)	(0.029)	(0.018)
3-5	0.066***	0.056***	0.056***	0.045**	0.068***
	(0.012)	(0.015)	(0.015)	(0.017)	(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***	-9.735***	-9.775***
	(0.538)	(0.425)	(0.407)
2	-8.853***	-8.380***	-8.320***
	(0.577)	(0.500)	(0.446)
3-5	-5.183***	-5.048***	-4.956***
	(0.449)	(0.464)	(0.457)
B) Labor earnings			
1	1.094*	1.643*	1.144***
	(0.614)	(0.657)	(0.400)
2	1.436*	1.643**	1.567***
	(0.750)	(0.742)	(0.541)
3-5	0.576	1.118**	1.070**
	(0.657)	(0.496)	(0.451)
C) Employment			
1	0.090***	0.094***	0.092***
	(0.029)	(0.030)	(0.022)
2	0.069**	0.076***	0.070***
	(0.029)	(0.030)	(0.022)
3-5	0.031	0.045**	0.041*
	(0.025)	(0.022)	(0.021)
D) Unemployment			
1	-0.102***	-0.110***	-0.164***
	(0.029)	(0.030)	(0.027)
2	0.168***	-0.165***	-0.158***
	(0.043)	(0.041)	(0.028)
3-5	-0.102***	-0.110***	-0.104***
	(0.025)	(0.022)	(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	0.092***
	(0.032)	(0.022)
2	-0.004	0.070***
	(0.027)	(0.019)
3-5	0.001	0.041*
	(0.020)	(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)
A) Full sample				
Employment	0.092***	0.091***	0.070***	0.069***
	(0.022)	(0.030)	(0.019)	(0.030)
Unemployment	-0.164***	-0.168***	-0.158***	-0.168***
	(0.027)	(0.041)	(0.028)	(0.044)
Not in labor force	0.072***	0.076***	0.093***	0.104***
	(0.014)	(0.020)	(0.020)	(0.027)
B) Males				
Employment	0.160***	0.169***	0.132***	0.147***
	(0.044)	(0.059)	(0.035)	(0.043)
Unemployment	-0.155***	-0.167***	-0.137***	-0.160***
	(0.045)	(0.063)	(0.038)	(0.054)
Not in labor force	-0.004	-0.001	0.011	0.019
	(0.012)	(0.014)	(0.023)	(0.026)
C) Females				
Employment	0.037	0.043***	0.015	0.025
	(0.022)	(0.015)	(0.019)	(0.019)
Unemployment	-0.171***	-0.178***	-0.172***	-0.188***
	(0.022)	(0.031)	(0.029)	(0.042)
Not in labor force	0.132***	0.136***	0.162***	0.168***
	(0.021)	(0.026)	(0.025)	(0.032)
D) By job openings in low / unskilled jobs				
High demand	0.198***	0.201***	0.214***	0.219***
	(0.051)	(0.052)	(0.046)	(0.049)
Low demand	0.127	0.126	0.052	0.047
	(0.085)	(0.091)	(0.068)	(0.071)
E) By average employment of non-Western immigrants				
High demand	0.180***	0.181***	0.224***	0.229***
	(0.052)	(0.052)	(0.048)	(0.041)
Low demand	0.157**	0.157*	0.079	0.074
	(0.077)	(0.080)	(0.059)	(0.065)
F) Crime (Year 1 and accumulated from year 1-5)				
Males	0.015	0.015	0.049*	0.052*
	(0.015)	(0.016)	(0.026)	(0.028)
Females	0.026**	0.026*	0.027	0.028
	(0.013)	(0.014)	(0.017)	(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			
High demand			
Reform effect	0.104***	0.116***	0.055
	(0.031)	(0.044)	(0.035)
Pre-reform mean	0.105	0.200	0.350
Post-reform mean	0.209	0.316	0.405
Low demand			
Reform effect	0.074*	0.021	0.009
	(0.042)	(0.040)	(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			
High demand			
Reform effect	0.100***	0.113***	0.059*
	(0.031)	(0.034)	(0.032)
Pre-reform mean	0.113	0.207	0.353
Post-reform mean	0.213	0.320	0.412
Low demand			
Reform effect	0.082*	0.035	0.010
	(0.041)	(0.043)	(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			
High demand			
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)
Low demand			
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			
High demand			
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)
Low demand			
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 controling 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) *Reform effect full sample*	*(2)* *Reform effect low demand*	*(3)* *Reform effect high demand*	*(4)* *Difference high-low demand*
A) Labor earnings, $1,000	1.753**	-2.199	5.035***	7.234***
	(0.842)	(1.500)	(1.516)	(2.506)
B) 1[Labor earnings $0-1,499]	-0.033	0.116***	-0.155***	-0.271***
	(0.035)	(0.049)	(0.060)	(0.086)
C) 1[Labor earnings $1,500-2,999]	0.027	-0.048	0.090	0.138**
	(0.038)	(0.037)	(0.057)	(0.066)
D) 1[Labor earnings $3,000-4,499]	0.008	-0.011	0.021	0.032
	(0.025)	(0.047)	(0.029)	(0.057)
E) 1[Labor earnings $4,500 and above]	-0.002	-0.056***	0.044**	0.101***
	(0.013)	(0.019)	(0.021)	(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**	0.032**	0.033**	0.036**	0.031**
		(0.013)	(0.013)	(0.013)	(0.014)	(0.013)
B) Number of crimes, year 1-5, adults						
	Reform effect	0.054***	0.050***	0.053**	0.054**	0.050**
		(0.019)	(0.018)	(0.020)	(0.020)	(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**								
	All crime	*Property*	*Theft from superm.*	*Violence*	*Disposable income<$500*			*Disposable income<$750*			*Disposable income<$1,000*		
	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, 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. From this, we obtain our study sample of adults, spouses, and adolescents.

The income register 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 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 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. 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 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

[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.

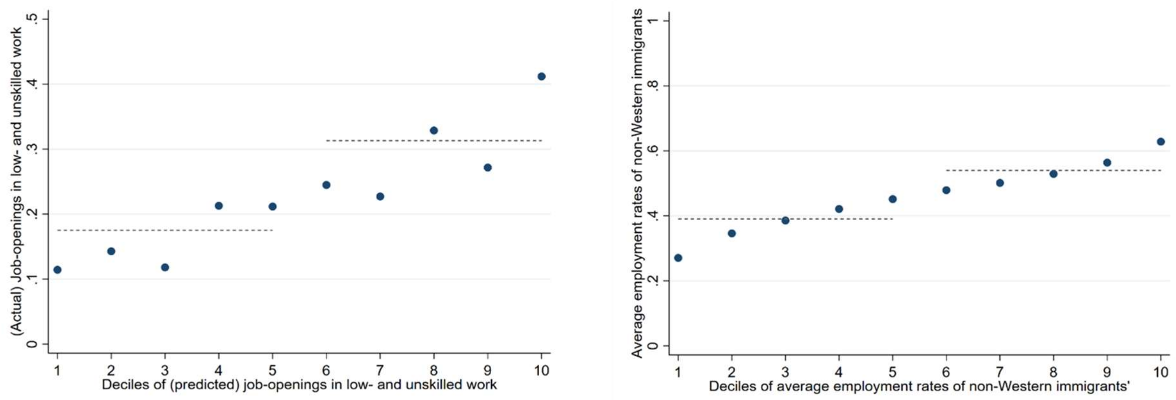
[2] We regress job openings on municipal average employment rate, non-Western immigrants' employment rate, unskilled individuals' employment rate, and immigrants' average labor earnings, and these variables interacted with each other.

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.

A) Job-openings in low- and unskilled jobs *B) Average employment rate of non-Western immigrants*



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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Refugee Benefit Cuts

By **CHRISTIAN DUSTMANN, RASMUS LANDERSØ, AND LARS HØJSGAARD ANDERSEN***

This paper analyzes the effects of Denmark's Start Aid welfare reform that targets refugees. Implemented in 2002, it enables us to study not only the reform's immediate effects, but also its longer-term consequences, and its repeal a decade later. The reform-induced large transfer cuts led to an increase in employment rates, but only in the short run. Overall, the reform increased poverty rates and led to a rise in subsistence crime. Moreover, local demand conditions generate substantial heterogeneity in the reform's effects on immediate and longer-term employment. (JEL: E64, I30, J60*)*

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In response to recent large immigration flows and a sharp rise in anti-immigration sentiment, many governments are restricting access to welfare benefits for refugee immigrants.[1] For instance, in 2014, Canada took measures to limit immigrant access to social assistance (following a first round of cuts in 2012), and in 2016 and 2019, Germany limited access to social benefits and reduced levels for groups of refugees.[2] These reforms are often justified as a means to incentivize labor force participation, but not much research exists that investigates their effects, partly because lack of data due to their recent implementation. Moreover, immediate effects of such reforms on employment, earnings, and labor market participation may differ from long term consequences, about which we know even less, while reform design may induce unanticipated disincentives amplified by traditional gender roles in refugee households. Finally, unfavorable labor demand conditions for the type of work low-skilled individuals supply may counteract reform incentives.[3] This is particularly relevant for refugees who are often unprepared for the labor market of the country that provides protection (Fasani et al., 2021).

This paper provides critically needed evidence on these issues by analyzing the effects of Denmark's Start Aid welfare reform that intended to *"ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly*" (Danish Parliament L126, 2002). The reform reduced welfare benefits for refugees with asylum claims approved after July 1, 2002, by around 40% compared to the previous social assistance (SoA) level. While

[1] A refugee is an asylum seeker whose asylum application has been approved and who has thus been granted residency and entitlement to welfare benefits (Hatton, 2020).

[2] Other policies implemented include restriction of immigrants'/refugees' access to social assistance and public benefits in Canada (CBC, 2014), Finland, France, Latvia, Lithuania, the Netherlands (OECD International Migration Outlook 2017, 2018), and Switzerland (Swissinfo, 2017); transfer cuts in Austria, Sweden, and Germany (OECD International Migration Outlook 2019, 2020); and further adjustments of transfer levels and eligibility in Denmark in 2015 and 2018. More generally, from 2000-2019, EU27 countries passed 176 bills on refugee and migrant welfare eligibility, program requirements, or welfare levels (OECD International Migration Outlook 2006-2020; OECD Trends in International Migration 1997-2004).

[3] See Brell et al. (2020) for evidence.

sharing many of the features of other more recent reforms and reform proposals targeting refugee immigrants, Start Aid was implemented in 2002, which allows us to study not only its immediate effects, but also its repeal 10 years later, and its longer-term consequences for refugees and their families. In addition, in its implementation phase the reform quasi randomly allocated households across two different support allocation schemes that were equivalent in overall benefit payments but created different incentives, mostly for females, for participation in integration programs and the labor market. This offers an opportunity to study how small design differences affect outcomes for these populations. Moreover, the reform was implemented during a period when refugees were quasi-randomly allocated across municipalities. This provides us with a second research design to study how local labor demand conditions mediate the effects of the reform, which is otherwise typically impossible due to sorting of target populations across local labor markets.

We show that the reform doubled average labor earnings and increased employment rates in its immediate aftermath, while its repeal a decade later (which *increased* transfers to the pre-reform level in 2012) had the exact opposite effect, underscoring the robustness of the short run result.[4] However, the short run effects did not carry over to the longer run, with both average labor earnings and employment effects fading out quickly and being close to zero five years after reform implementation. Conclusions about a policy's effects drawn from average short-term labor market outcomes are thus not indicative for the overall and longer-term impact – a finding that complements the long strand of literature studying the labor supply effects of welfare reforms and means-tested transfers (e.g., Eissa and Liebman, 1996; Hoynes, 1996; Meyer and Rosenbaum, 2001; Moffitt 2002, 2015).

[4] Our estimates on the immediate impact of the reform are similar to those of earlier short-term evaluations of the Start Aid reform, see Huynh et al, (2007) and Rosholm and Vejlin (2010).

We identify two channels that attenuate the reform's effects on refugees' employment and impede their labor market integration. First, the combination between the reform and the household-level means test led more females to drop out of the labor force because they became ineligible for transfers when their husband took up employment; a finding that underscores the importance of considering within-household incentives (e.g., Eissa and Hoynes, 2004) when designing transfer policies. Moreover, this disincentive for second earners was enhanced by a specific feature of the reform's implementation that implied that in some households, transfers to both partners were paid to one spouse only (typically the male), which removed labor force participation incentives for the other spouse in the same way as the household-level means testing did. This doubled labor force exits of females, a sizeable response that may be partly due to views about female labor force participation in traditional refugee communities, illustrating that responses in minority populations may differ from those expected in majority populations, as also found in Dahl et al. (2020). More generally, these findings demonstrate the sensitivity of reform effects and estimated labor supply elasticities to small variations in reform designs (see e.g., Chetty et al., 2011; Kleven and Schultz, 2014; Saez et al., 2012).[5]

Second, using the quasi-random allocation of refugees across Denmark's 270 municipalities as a second design, we show that local labor demand for the type of work refugees can supply is indeed essential for the reform's outcomes. While employment effects disappear after one year for refugees allocated to municipalities with low demand, they remain significant until year 5 after residency for those allocated to municipalities with high demand. Moreover, the reform induced take-up of employment in lower quality jobs with lower job

[5] We illustrate that the heterogeneous household-level responses on employment and labor force participation follow exactly what would be predicted in a simple static labor supply framework (as in e.g., Bitler et al., 2006 and Lemieux and Milligan, 2008).

stability in low demand municipalities but led to more persistent and higher quality employment relationships in high demand municipalities. Overall, the reform increased refugees' average income from labor earnings during the first five years by almost 40% and reduced public expenditures by 60% in municipalities with the highest labor demand, whereas there were no significant changes to income from labor earnings in low demand municipalities, and public expenses only declined by 35%. These estimates constitute a first direct assessment of the sensitivity of reform effects to local demand conditions. Our findings not only call into question the common policy of equally distributing refugees across regions, but also speak directly to previous studies that have linked local labor demand to welfare use (see e.g. Hoynes, 2000; Black et al. 2003), and an active literature that discusses whether effects of welfare reforms and employment regulations are confounded by business cycles (e.g., Ziliak et al., 2000, Lemieux and Milligan, 2008, Ganong and Liebman, 2018, Kleven, 2019, Fasani et al., 2021).[6]

Overall, the Start Aid welfare reform lowered benefits to refugee immigrants by 40%, a shortfall that could only partly be compensated by higher labor supply, so that the majority witnessed a dramatic reduction in disposable income, with the share of individuals falling below the poverty line increasing from close to zero pre-reform to almost 50% post-reform. We show that this severe reduction to disposable income is accompanied by a sharp rise in crime, in particular subsistence crime (e.g., grocery store shoplifting). The crime increase is particularly notable for females, a group with otherwise low crime rates. These findings contribute to the few studies that associate crime with either welfare payment timing (e.g., Foley, 2011; Carr and Packham, 2017), welfare eligibility

[6] Azlor et al. (2020), Damm and Rosholm (2010), and Åslund and Rooth (2007) find that the economic conditions at initial allocation affect immigrants' subsequent labor market outcomes. While our analysis focusses on the interaction between local labor market conditions and the welfare reform, we also confirm these earlier studies' findings and complement them further by showing how local labor demand affects labor earnings and job types.

of youths (Deshpande and Mueller-Smith, forthcoming) and criminal offenders (Yang, 2017), and/or state variation in welfare reform implementation in the U.S. (Corman et al., 2014).

I. Background and Data

A. Social Assistance, the Start Aid Reform, and Benefit Eligibility

Denmark's social assistance (SoA) benefits are among the most generous in the world and the country once had some of the most liberal refugee immigration laws (Andersen et al., 2012; Huynh et al., 2007; Pedersen, 2013). By 2001, because of large inflows of individuals with high levels of welfare uptake, net welfare transfers to non-Western immigrants reached 0.83% of the GDP and 3.4% of total public spending (Matthiessen, 2009). On March 1, 2002, a newly elected Danish government proposed a bill that replaced SoA for refugees with a new Start Aid benefit scheme intended to promote their labor market participation (Danish Parliament, 2002).[7] Approved on June 6 and implemented on July 1, the reform assigned all refugees granted residency after the reform date to the Start Aid program, whose transfers were approximately 40% lower than SoA payments (rates are based on age and family type; the reform lowered transfer rates by 40% on average when we weight the pre-post reform changes by our sample composition, see Table A.1).[8] The Start Aid program was in effect until January 1, 2012, when it was repealed following a change in government.

[7] "To ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly, the incentives for finding employment must be strengthened" (author translation of official remarks: http://webarkiv.ft.dk/Samling/20012/lovforslag_som_fremsat/L126.htm, accessed 03-20-2017). Appendix B.1 provides a more detailed description of the background and the Start Aid reform.

[8] Start Aid levels (pre-tax) are on par with median Temporary Assistance for Needy Families and Supplemental Nutrition Assistance Program levels in the U.S. (TANF: Falk 2014 who reports levels by state and year; SNAP: http://kff.org/other/state-indicator/avg-monthly-food-stamp-benefits).

To receive residency, refugees must first request asylum, which most do after entering the country as undocumented migrants. Once asylum is requested, the applicant is transferred to a central reception center. After the formal application process begins, the Danish Red Cross assigns the refugee to an accommodation center (refugee camp) while the application is processed by the Danish Immigration Service. Refugees are not allowed to work before their residency is approved (implying that *all* refugees become welfare recipients once they receive residency), and the centers provide both food (either directly or via food stamps) and health care. There is no cap on the number of residencies granted within a specific period, and the application process takes on average 15 months during the period we study (Hvidtfeldt and Schultz-Nielsen, 2018, Figure 6.1), which, due to the reform's very short implementation period, effectively randomizes individuals already in Denmark to Start Aid or SoA based on when they are granted residency around the reform implementation date.[9] The timing of residency around the reform thus provides a clean identification of the reform's effects (as detailed in Section 3).

B. Refugee Allocation across Local Labor Markets

To study how local labor market conditions interact with the reform, we use a quasi-random allocation scheme for refugee placement that was in effect during the implementation of the reform. Upon being granted residency to Denmark, the Danish Immigration Service allocated refugees to each of Denmark's 13 counties. Each refugee was then assigned to a municipality, following a pre-determined

[9] The specific waiting time for an individual refugee depends mainly on the caseload of asylum applications at that given point in time and the information available to Danish authorities relating to the conditions in the countries of origin (that is, if the Danish Immigration Service needs to search for additional documentation before the case can be processed), cf. Hvidtfeldt et al. (2018).

quota system.[10] Counties and municipalities had no information about the refugees' characteristics when their quotas were set for the year to come, and county and municipality officials were only informed about the country of origin and whether refugees had family members who already lived in a specific municipality. This effectively made it impossible to cream-skim based on, for example, refugees' employment prospects, and from the refugees' perspective, the assignment was as-good-as-random. We show in Section 4 that refugees' characteristics are not associated with local labor market indicators. Following assignment, refugees were required to remain in their assigned municipality for a minimum of three years to receive transfers. The vast majority stayed in the municipality of assignment even in the longer run, irrespective of local employment prospects (we validate this for our sample below; see also Nielsen and Jensen, 2006).

C. Eligibility, Household Entitlements, and Reform Implementation

Eligibility for both SoA and Start Aid is conditional on participation in an integration program, which comprises courses in the Danish language and Danish society and acculturation, as well as active labor market programs.[11] Failure to comply with these obligations results in immediate transfer ineligibility. Being the lowest tier of the Danish welfare system, SoA and Start Aid receipts have no time limit as long as recipients satisfy the rules for integration program participation.

[10] The allocation of refugees across municipalities is in proportion to population size. In 2016, vice chairman of the Danish Municipalities' Association, Jacob Bundsgaard, commented on the allocation: "Today, it is basically completely random where refugees are allocated. But as a prerequisite for integration is that one joins the workforce, we suggest that the match between refugee characteristics and municipal labor markets is considered" (author's own translation).

[11] The integration program consists of two main components. The first is mandatory Danish and cultural lessons several times per week, and the second is active labor market programs. The two components together take up about 30 hours per week. The obligation to comply with the integration program discontinues during employment spells but resumes in case of new unemployment spells (see the Law of Integration of Immigrants in Denmark: https://www.retsinformation.dk/Forms/R0710.aspx?id=28907#K4.

SoA and Start Aid are means tested, and for couples, the means test is at the household level. Hence, not only do refugees lose their own SoA or Start Aid because of labor earnings, any labor earnings from the first earned dollar onward reduces the benefits of both partners. Means testing thus works as a household-level "negative income tax" that provides strong extensive margin disincentives, which will be central for understanding females' responses to the reform. In addition, the reform was implemented in two distinct ways, according to the residency dates of each spouse. As such, in our analysis of couples' labor supply responses in Section 4.3, we classify couples into three groups.[12] If both spouses received residency pre-reform, both are entitled to SoA. These couples constitute our reference category. If both received residency post-reform, both are entitled to Start Aid. We refer to these as "Type A" couples. If one spouse received residency pre-reform and the other post-reform, their combined benefits are capped at two times Start Aid with the first arriving spouse keeping the full SoA and the last arriving receiving whatever may be left. We refer to these as "Type B" couples. Because SoA is almost twice as high as Start Aid (although with variation across household types, cf. Table A.1), the last arriving spouse in Type B couples is effectively ineligible for any benefits.[13] One important implication of this allocation scheme is that last arriving spouses in Type B couples cannot be (heavily) penalized for nonparticipation in integration programs as the individual has no (or only very few) benefits to cut.

[12] Married applicants are each assigned their own asylum case ID and processed individually even if they apply together on the same date. In our sample, 18% of the married couples have the same application and approval dates, around 1% have the same application date but different approval date, 15% have different application dates but the same approval date, and 67% share neither application nor approval dates. Unmarried couples are processed as two single individuals having independent case processing times.

[13] The average transfer reduction was 40%, and largest for couples, with reductions ranging between 40 and 50% (see Table A.1). Thus, when transfers were capped at two times Start Aid at household level, the last arriving spouse in Type B couples was either ineligible for any transfers or only eligible for $30-150 per month (and only if they followed the integration courses which take up around 30 hours per week).

Figure 1 illustrates how labor earnings translate into pre-tax gross income when transfers are reduced due to the means test for pre-reform, Type A, and Type B couples, respectively.[14] The means test on SoA and Start Aid implies an effective marginal tax rate of between 83% and 100% on any labor earnings below a break-even point (the point at which there is no SoA or Start Aid left to means test).[15]

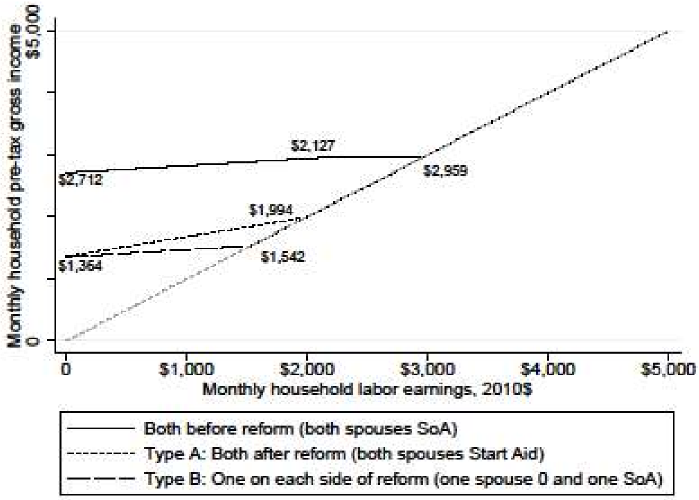
To respond to the incentives, refugees need to be aware not only of means testing and household-dependent variations but also of benefit caps and the effects of integration program noncompliance on benefit eligibility. The municipality of residence is obliged, both in physical meetings (with an interpreter when required) and written communication (sample letters to welfare recipients are available upon request), to explain to potential welfare beneficiaries such issues as (i) compulsory participation in an integration program, (ii) the withholding of transfers for noncompliance, and (iii) the limiting of transfers for Type B couples to the spouse granted residency first.[16]

14 The vertical difference between the solid (pre-reform couples) and dotted/dashed (Type A and B couples) lines in the intersections with the y-axis at zero labor earnings in Figure 1A shows the monthly benefit reduction induced by the reform with the slopes representing the means testing rates. Because 91% of couples in our sample have children, we use a one-child family as a benchmark for couples' transfers. Table A.1 shows the extensive margin implied marginal tax rates and the break-even points by family type. Transfers are subject to the same income taxes as labor earnings, which we factor in when calculating marginal tax rates in Figure 1B. The means testing rates for singles correspond to those of Type A couples at half of their transfer level and break-even point. The implied marginal tax rates are 93.5% and 82.1% for Type A and B couples, respectively. All income values reported in the paper are in 2010 PPP adjusted USD (1$=7.76DKK).

15 For pre-reform couples, the break-even point is at around $3,000 per month, while for Type A and B couples, it is about $2,000 and $1,500, respectively. The break-even point for Type B couples is lower because it combines pre- and post-reform features. Although total household transfers when the spouses are not working is the same as if both were on Start Aid (Type A), all household transfers are paid as SoA to one spouse in type B couples. Hence, the discount from means testing equals the pre-reform SoA discount, resulting in a monthly break-even point that is around $500 lower than for Type A couples. The low bracket marginal tax rate of 44% applies to those with labor earnings above the break-even point.

16 Danish authorities are required by Administrative Law, section 7, no. 1 to ensure that citizens and refugees have understood the rules and regulations that pertain to their benefit reception, as well as any changes to their entitlements.

Panel A. Household level labor earnings, pre-tax gross income



Panel B. Implied marginal tax rate.

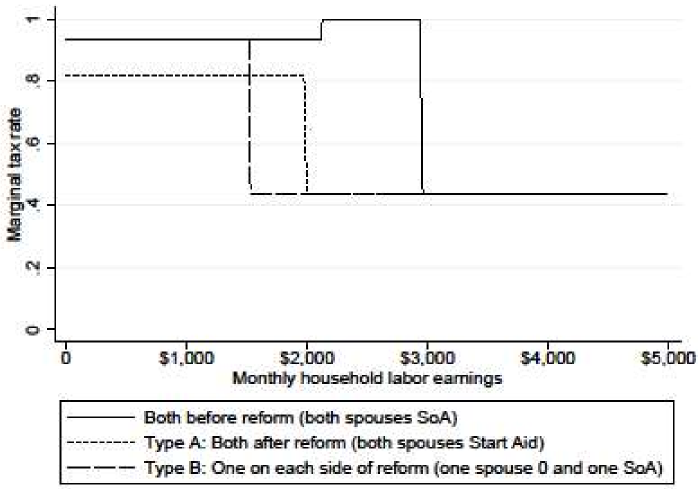


FIGURE 1. LABOR EARNINGS, PRE-TAX GROSS INCOME, AND IMPLIED MARGINAL TAX RATES FROM MEANS TESTING

Notes: Panel A shows the relationship between labor earnings (measured pre-tax) and pre-tax gross income due to means testing, by household types. The solid line shows pre-reform benefit schedule and the dashed lines post-reform schedules. Beside the lower benefits, the slopes differ due to varying means testing rates (between 0.8 and 1). Amounts noted on the y-axis (2,712 / 1,364) refer to monthly household pre-tax gross income at no labor income (intercept), and amounts noted away from y-axis refer to monthly household labor earnings (on x-axis). Panel B shows the corresponding marginal tax rates calculated as (1-slope)+t*slope, where t is marginal tax rate of 0.44 in the lowest tax bracket. The means testing rates for singles correspond to those for Type A couples. But as the figure considers household level transfers, transfer levels and break-even points for singles are half of Type A couples.

D. Data and Samples

Our sample consists of refugees whose treatment status (pre- or post-reform) is determined by the exact date on which residency was awarded.[17] To derive information on this sample's labor market outcomes (including employment status, income, and occupation) and demographic characteristics (including age, gender, education level, and date of birth), we use register data recorded by public agencies and then compiled and organized by Statistics Denmark. Because this database assigns unique personal identification numbers to individuals, their spouses, and their parents, we can merge the information for an individual with that of the rest of their family to construct records for each household.

Our initial sample comprises 8,512 individuals granted residency (via a refugee status or family reunification) between January 1, 2001, and December 31, 2003, at ages 18 to 55. Two temporary changes to case processing procedures happened in the months preceding the reform as a result of contemporaneous conflicts. First, following the fall of the Taliban regime in late 2001, the Danish Immigration Service suspended processing of new applications by Afghans in late January 2002 (Refugee Appeals Board, 2002, p. 142) until the situation in Afghanistan had been investigated. This led to a large drop in residency permits issued to Afghans around the reform. Second, following the NATO bombings in 1999 and the subsequent installment of NATO forces (KFOR), Kosovo was reclassified as a "safe zone" by Danish courts in the spring of 2002 (Refugee Appeals Board, 2002, p. 114). While unrelated to the Start Aid reform, these administrative alterations nonetheless resulted in a sudden change in the number of residencies granted to refugees from these countries that largely coincided with the introduction of the reform. We therefore exclude refugees from Afghanistan and

[17] Our sample includes only refugees and individuals who are family reunified with refugees, because labor migrants, their families, and other nonrefugee migrants are ineligible for SoA or Start Aid and thus unaffected by the reform.

the former Yugoslavia from our final sample, but we provide robustness tests including the two groups, which show that in practice our estimates are unaffected by this exclusion.

We also exclude those who re-migrate within 9 years after being granted residency and later test for selectivity over the reform period to ensure that remigration patterns are not related to the reform (see Section 4.1).[18] Our *base sample* thus consists of 4,843 individuals who received residency within our observation window and were aged 18 to 55 on the date residency was granted. Collectively, these individuals had 3,299 children aged 0 to 17 at the time that residency was granted. In our analysis of couples' joint responses, we add in the spouses of all individuals in the base sample, which results in a balanced *couples sample* of 4,072 individuals (2,036 couples, 57% with two pre-reform residencies, 13% with two post-reform residencies, and 30% with residencies on either side of the reform).[19]

We use two indicators to measure local labor demand in the assignment municipalities. First, we take the number of job openings in low skilled / unskilled positions (e.g., construction, cleaning, and warehouse work) relative to the number of unemployed individuals in each municipality.[20] As this local job-opening information is only available from 2002 onwards (www.jobindex.dk, which includes all openings posted on the internet), we address simultaneity concerns by regressing the number of job openings per unemployed individual in 2002 and 2003 on pre-reform municipality characteristics and use the resulting

[18] As noted earlier, the lengthy asylum process (on average 15 months) precludes the possibility that announcement effects compromise our identification because those receiving residency around the reform's enactment date had already submitted their applications before the reform was proposed. Similarly, a contemporaneous bill that changed the rules governing when (but not whether) individuals could apply for permanent asylum (see Kilström et al., 2018) does not affect our identification as it took effect only for asylum applications lodged from March 2002 onward.

[19] Results for couples are robust to limiting the sample to the 90% of couples in which both spouses received residency within the +-18 month window around the reform.

[20] There is strong persistence in the local labor demand indicators over time. For example, the correlation between ranks of municipalities according to the number of job openings per unemployed in year t and year t+5 is around 0.8. The correlation between rank in year t and t+10 is around 0.7.

predictions in our analysis. For brevity we refer to this measure as *job openings in low- and unskilled work*. Our second measure is the municipal average employment rate of non-Western immigrants from 1999 to 2001, which captures a strong element of demand for the type of work refugees can perform.[21] Appendix B.2 details the construction of the two measures and provides descriptives. As we show in Section 4, both measures of local labor market conditions are unrelated to the characteristics of assigned refugees in our sample.

E. Outcomes

We determine labor market status from the first full year after residency onward, distinguishing between three mutually exclusive states: (in) *employment*, (in) *unemployment*, and *not in the labor force* (NILF). The unemployed are individuals available to the labor market who are participating in integration programs but are not currently working. Employed and unemployed individuals constitute the labor force, and the residual group is, by construction, not in the labor force. Most of this group are ineligible for transfers, due to neither working nor participating in integration programs. A remaining (small) group are eligible for disability benefits. This group is exempt from both integration programs and transfer reduction.

We consider four measures of income, all based on tax authority records: labor earnings (measured pre-tax, where those who have no earnings are set to zero), transfer income (measured pre-tax), pre-tax gross income (which equals labor earnings plus transfer income), and post-tax disposable income (which equals pre-tax gross income minus tax payments). Based on the income data, we construct a

[21] Municipal average employment rates of non-Western immigrants are also used by Azlor et al. (2020) as measures of local demand for immigrant labor, while Åslund and Rooth (2007) consider municipal average unemployment rates. Similarly, Hoynes (2000) uses average local labor market outcomes to proxy labor demand conditions, while Notowidigdo (2020) presents an alternative estimation strategy by using a Bartik instrument to identify local labor demand shocks.

measure of public expenditures as transfer income minus tax payments. We supplement the income data with hourly wage rate data and occupational classifications. Most of our analysis focuses on the first five years after residency, but Section 4.7 also reports effects on employment until 10 years after residency.

Our measure of crime is based on police and court records for all criminal convictions in Denmark. In addition to the unique individual identifiers allowing us to link the crime data to the sample of refugees, the data also includes unique case identifiers along with specific offense and conviction dates for our entire sample, and detailed offense codes that enable us to identify the exact crime type committed. We focus here on crimes that lead to a conviction and we count crime by the date of the offense (such that, for example, "crime in year 1" is crime committed during the first year after residency that leads to a conviction at some later point in time). We describe all outcomes and data sources in greater detail in Appendix B.2.

F. Descriptive Statistics

Table 1, Panel A lists the covariate means for the base sample of adults aged 18 to 55, again distinguishing between pre- and post-reform residency. As the inflow of refugees to Denmark slows over our sample period (as in most other European countries, cf. Hatton, 2009), the number of residencies granted post reform is smaller. Of the refugees in the base sample, 84% are immigrants from predominantly Muslim countries (around half of Iraqi origin). Residency based on refugee status is granted to 62% of the sample, while the remainder receive residency as a result of family reunification. Upon residency, each adult has on average two children. Although the table reveals some differences between the pre- and post-reform groups (e.g., share of females), tests of our key assumption of comparability in the limit around the reform cutoff date confirm the observable characteristics to be balanced (Panel B, Table 1), with no discontinuities in covariates around the reform timing. We will return to this point in Section 4.1.

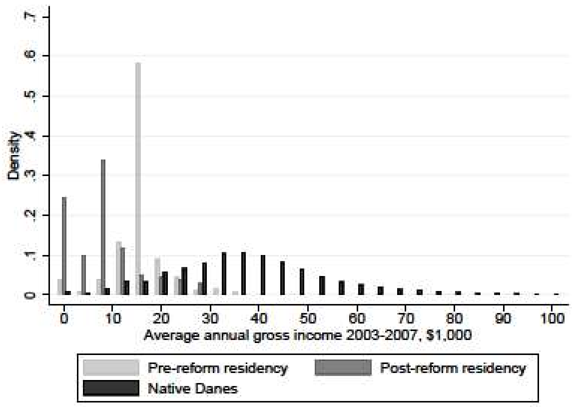
Table 1. Sample means of covariates and balancing tests, base sample aged 18-55 at residency.

	A) Sample means			*B) Balancing tests*	
	(1)	(2)	(3)	(4)	(5)
	All	Pre-reform residency	Post-reform residency	Conditional test	Unconditional test
Reform=1	0.371	0.000	1.000	-	-
	(0.483)	-	-		
Age at residency	32.625	32.700	32.498	-0.001	-0.873
	(8.270)	(8.311)	(8.202)	(0.001)	(0.561)
Female	0.507	0.475	0.560	0.014	0.056
	(0.500)	(0.499)	(0.496)	(0.010)	(0.040)
# of children	2.257	2.346	2.106	-0.005	-0.137
	(1.903)	(1.931)	(1.847)	(0.001)	(0.129)
Single	0.246	0.226	0.279	0.001	0.008
	(0.431)	(0.418)	(0.449)	(0.013)	(0.038)
Muslim countries	0.838	0.878	0.769	-	0.037
	(0.369)	(0.327)	(0.422)		(0.042)
Eastern Europe/former USSR	0.055	0.050	0.062	-0.012	-0.025
	(0.227)	(0.219)	(0.241)	(0.025)	(0.020)
Rest of the world	0.108	0.071	0.170	-0.033	-0.012
	(0.310)	(0.257)	(0.375)	(0.027)	(0.036)
Refugee permit status	0.618	0.635	0.588	-0.006	-0.049
	(0.486)	(0.482)	(0.492)	(0.017)	(0.056)
Observations	4,843	3,044	1,799	4,843	4,843

Note: The table shows sample means and balancing tests for the base sample of adults receiving residency +/- 18 months around the reform. Panel A presents sample means for all and by pre- and post-reform residency separately (with standard deviations in parentheses). Panel B presents estimation results from balancing tests (with standard errors in parentheses). Column 4 presents conditional balancing of covariates across the reform from regressing a dummy indicating whether residency was granted pre- or post-reform on all covariates and the running variable (allowing for different slopes in the running variable on each side of the cutoff). Table A.2 extends these results for alternative sample definitions. Column 5 presents unconditional balancing of covariates from 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). Table A.3 extends these results for alternative sample definitions. '# of children' refers to the number of children upon residency. 'Muslim countries' refer to refugeees from majority-Muslim countries and not the individual's religion. 'Refugee' refers to the individual receving residency based on refugee status (is residency given on grounds of being a refugee 1, or from being the spouse of an individual with refugee status, 0).

Figure 2 plots the distribution of average pre-tax gross income (labor earnings and transfers) from 2003-2007 for adult refugees granted either pre- or post-reform residency in 2002, together with the pre-tax gross income distribution for native Danes. Whereas refugees with pre-reform residency are clustered in the lowest 15 percentiles of the Danish pre-tax gross income distribution, with annual pre-tax gross incomes of $15,000 or below, almost all refugees granted residency after July 1, 2002, fall into the lowest 8% of the pre-tax gross income distribution with pre-tax gross incomes below $10,000.

Panel A. Distribution: absolute pre-tax gross income levels.



Panel B. Distribution: pre-tax gross income percentiles in the full population income distribution.

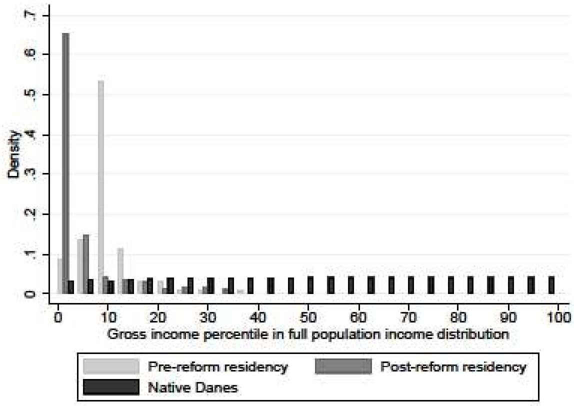


FIGURE 2. PRE-TAX GROSS INCOME DISTRIBUTIONS FOR REFUGEES, PRE- AND POST-REFORM RESIDENCY, AND NATIVE DANES

Notes: The figure shows the pre-tax gross income distributions of adult refugees (age 30 or above) receiving residency in 2002 by whether they received residency before the reform (eligible for SoA) or after the reform (eligible for Start Aid) and adult native Danes (age 30 or above) for comparison. The pre-tax gross income distributions are measured from 2003-2007. Panel A presents the distribution of pre-tax gross income levels and Panel B presents, for each of the three groups, the distribution of pre-tax gross income percentiles (in the full population income distribution).

II. Estimation and Identification Strategy

Because the benefit reform studied here induced a large drop in transfers for refugees who received residency following its implementation, we first estimate the reform's effect on individuals using a regression discontinuity design that compares those granted residency just before and just after the reform cutoff date:

$$(1) \qquad y_i \;= \alpha + \beta * reform_i + g(Z_i)'\pi + X_i'\gamma + \varepsilon_i.$$

Where y_i is an outcome for individual i measured τ years after residency, $reform_i$ is a dummy variable indicating whether individual i received residency after the reform date, and Z_i is a running variable counting months between the residency decision and the reform date.[22] The vector X_i collects observable characteristics, and ε_i is an idiosyncratic error term. The parameter of interest is β. It measures the effect of being eligible for Start Aid instead of SoA among individuals granted residency just around the reform.

To better understand how the reform affects households' joint decisions, part of our analysis focuses on couples. Here, we have two post-reform treatment categories (see Section 2.3), which we capture by extending Equation (1) to allow the outcome of individual i in household f to be affected by the residency timings of both themselves and their spouse. We define three states: (i) both spouses receive pre-reform residency and qualify for full SoA (baseline), (ii) both spouses receive post-reform residency and qualify for Start Aid (Type A), and (iii) the two spouses receive residency on either side of the reform, with the pre-reform

[22] To allow for separate trends on each side of the reform, we define g(.) to be linear by different linear functions pre- and post-reform, but we show that the estimated effects of the reform are robust to other definitions of g(.). We also allow for separate pre- and post-reform trends around the reform in the balancing tests. Moreover, we use "month" as the running variable as refugees typically receive for administrative reasons their residency decision at the 1st of a given month and are allocated to a municipality at the same time.

resident keeping full SoA while benefits are capped at two times Start Aid, which effectively makes the post-reform resident spouse ineligible for any benefits (Type B). We define Type A and Type B couples by two disjoint treatment dummies, A_i and B_i, with baseline couples as the reference category.[23] We estimate the reform's effects on outcome y_{if} of individual i from family f as

$$(2) \qquad y_{if} = \alpha + \beta_1 * A_{if} + g(Z_{1f})'\pi_1 + \beta_2 * B_{if} + g(Z_{2f})'\pi_2 + \varepsilon_{if}$$

where $g(Z_{1f})$ and $g(Z_{2f})$ control for the running variables that count the months to and from the reform for each spouse while allowing for different trends pre- and post-reform for each spouse. The parameters β_1 and β_2 measure the effects for Type A and Type B couples, respectively, with baseline couples as the reference category. We also interact Equation (2) with gender, thereby estimating β_1 and β_2 (and α, π_1, π_2) separately for males and females.

A unique feature of our data is that individuals in our sample were also quasi-randomly allocated across Denmark's municipalities, which allows us to estimate how local labor demand affects the reform's impact on employment. We assign municipalities into groups g according to their pre-reform local labor demand indicators and estimate:

$$(3) \qquad y_{ig} = \alpha_g + \beta_g * reform_i + f(Z_i)'\pi_g + \varepsilon_i.$$

The parameter α_g captures the pre-reform levels in group g, β_g measures the reform effect for group g, and $f(Z_i)'\pi_g$ allows for different pre- and post-reform

[23] See Card et al. (2007a) for a further discussion of identification with double discontinuity.

slopes in the running variable across municipality groups.[24] In most cases we group municipalities by whether they are above or below the median in a given demand indicator, but we also consider more granular defined groups.

III. Average Effects of the Reform

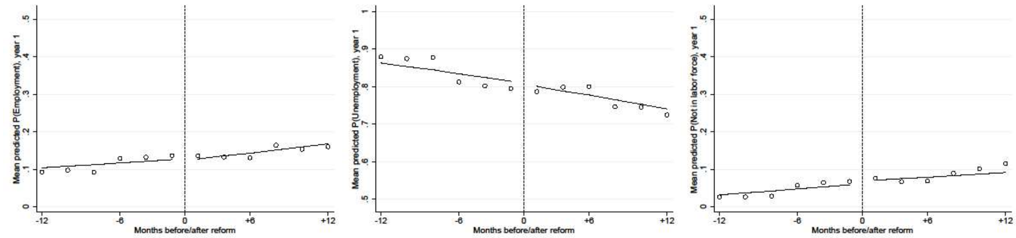
A. Balancing Tests

Our key identifying assumption is that with respect to those individuals whose residency is granted just before or just after the reform, the cutoff date is as good as random. This assumption is helped by the fact that the time span between reform announcement and implementation (3 months) was short, and that – given the lengthy asylum process which lasts on average 15 months (Hvidtfeldt and Schultz-Nielsen, 2018) – refugees affected by the reform were already in Denmark at the announcement date. As a first visual balancing test around the reform date, Figure 3 shows the employment, unemployment, and NILF rates during the first-year post residency for each value of the running variable, as predicted from an OLS regression using the covariates from Table 1 (cf. Card et al., 2007a). The pre- and post-reform trends are connected with no discontinuities in the predicted outcomes at the reform date, indicating no compositional changes to the sample around the cutoff.

[24] When estimating Equation (1), we cluster standard errors by the running variable. When estimating Eqs. (2) and (3) we use the two-way clustering method proposed in Cameron et al. (2011). For Equation (2), we cluster by the running variable and household, and for Equation (3), we cluster by the running variable and allocation municipality.

Main Sample

Panel A. Employment Panel B. Unemployment Panel C. NILF



Couples Sample

Panel D. Employment Panel E. Unemployment Panel F. NILF

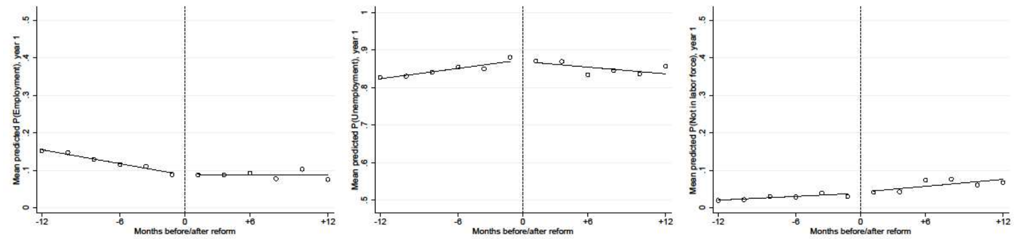


FIGURE 3. LABOR MARKET OUTCOMES 1 YEAR AFTER RESIDENCY, PREDICTED FROM BACKGROUND CHARACTERISTICS ALONE

Notes: The figure shows employment, unemployment, and not in the labor force rates in the first year after residency predicted from OLS estimations using the full set of covariates (see Table 1) for the base sample (Panels A-C) and couples sample (Panels D-F). The figure shows the predicted outcomes plotted by timing of residency relative to the reform, and the figure contains linear slopes of the predictions before and after the reform, to mimic our estimation strategy. The dashed vertical line indicates the timing of the reform in July 2002.

To further assess the validity of our design, we also perform a barrage of formal tests. We regress a dummy for pre- versus post-reform residency on the running variable and the covariates to assess whether the observable characteristics change around the reform date. Column 4 in Table 1 shows results for our main sample and Table A.2 presents results for alternative sample definitions. We next regress each covariate separately on the reform dummy (conditional on the running variable, see column 5 of Table 1 and Table A.3), including waiting times individuals spent in refugee camps before being granted residency (to test whether waiting times change across the reform, based on data from Hvidtfeldt et al., 2018), a dummy variable indicating whether an individual leaves Denmark over the 9 years after residency was granted (to investigate a possible increase in remigration after the reform) and a dummy variable indicating whether the spouse arrives first or last. In only two of the 44 individual balancing tests performed is the estimated parameter significant at the 10% level. While the aforementioned results – particularly the ones for waiting times in refugee camps – illustrate that case workers have not responded to the reform by granting more residencies just prior to the implementation of Start Aid, we perform McCrary tests of differences in the running variable density (residencies per month) around the reform date, varying the bandwidth selection from 10% to 150% of the optimal bandwidth to confirm robustness. None of the specifications reveal structural breaks (Table A.4).[25]

A causal interpretation of local labor demand's role for reform effects relies on the allocation of refugees across municipalities being as good as random and unrelated to local labor demand. To test this, we regress the average non-Western employment rates, and observed and predicted job openings in low / unskilled work (described in Section 2.4) on the characteristics of the refugees in Table 1. There is no sign of selective allocation for any of the indicators (Table A.5, columns 1, 5, and 9). To test whether any differential allocation is observed across the reform, we next include a reform dummy (indicating whether the refugee received residency before or after the reform), and the running variables on each side of the reform in columns 2, 6, and 10 of the table. Again, we do not observe any sign of selection into specific municipalities. To address the concern that refugees who were granted residency earlier and later in the calendar (i.e., administrative) year were assigned to different types of municipalities (defined as above or below the median in a given local labor demand indicator), we also run

[25] The absence of structural breaks around the reform in refugee characteristics and the running variable density is the key identifying assumption irrespective of any longer-term changes in migration flow to Denmark that may have followed the reform as suggested in Agersnap et al. (2020). Also, the absence of any changes in sample characteristics and density around the reform verifies that caseworkers did not manipulate cases to place certain families pre- or post-reform.

regressions where we include calendar month of residency in the tests. In sum, there are no significant associations between the local labor demand indicators and refugee characteristics, the timing of residency relative to the reform, or calendar month of residency, with p-values for joint significance in the balancing tests ranging from 0.167 to 0.761.

We also predict employment rates, unemployment rates, and labor earnings in years 1, 2, and 3 after the residency decision, based on observed characteristics (analogous to Figure 3, but with the addition of variables on timing of residency), and plot the predictions against deciles of the two labor market indicators (Figure A.1). There are no changes in predicted outcomes across the two indicators. Finally, we test for differences in municipalities' job policies by studying whether municipalities' use of activation and training requirements differ across the local labor demand indicators, and we test for selective moving patterns across local labor demand. We find no evidence of differences.[26]

B. Short and Medium Run Reform Effects

As a first illustration of the reform's immediate impact, Figure 4 shows transfer income, labor earnings, pre-tax gross income, and post-tax disposable income in the first year after residency plotted by timing of residency relative to the reform. The figure documents the large drop in transfers following the reform. Moreover, it shows that pre reform, only 10% of pre-tax gross income in the first year after residency comes from labor earnings (about $1,900) with the remaining 90% coming from transfers (about $20,500). The figure also reveals that although labor earnings increase in response to the reform, pre-tax gross income drops to almost half the pre-reform level and average post-tax disposable income falls by around

[26] As shown in Table A.6 (columns 1 and 2), there are no differences in geographical mobility to or away from low and high demand municipalities across the reform. Table A.6, columns 3 and 4, compares high and low demand municipalities' use of activation and training requirements. There are no significant differences.

40% to (or below) Denmark's estimated subsistence minimum (which is around \$8,800, see Hansen, 2002).

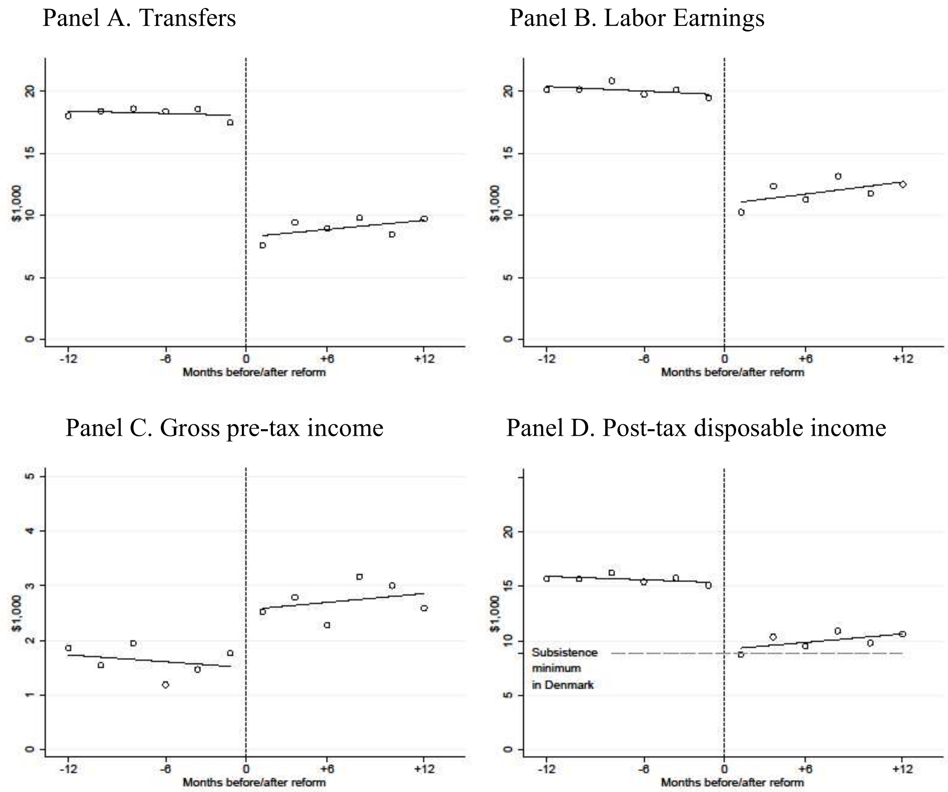


FIGURE 4. INDIVIDUAL INCOME, THE FIRST FULL YEAR AFTER RESIDENCY

Notes: The figure shows individual level transfer income (A), labor earnings (B), pre-tax gross income (C), and post-tax disposable income (D) by bi-monthly bins of residency timing. The dashed vertical line indicates the timing of the reform in July 2002. The horizontal line in D) is the estimated subsistence minimum in Denmark (Hansen, 2002) weighted across the different household types in our sample. The subsistence minimum budget includes the cheapest food, housing, and clothes available, no transportation, no replacement of durable goods, and no activities for children. The threshold is approximately equal to the PPP adjusted U.S. poverty threshold (cf. U.S. Census Bureau)..

In columns 1 and 2 of Table 2, we separately estimate the effects of the reform on transfers and labor earnings by time since residency (running regressions of the form of Equation (1) and using levels instead of logs for income because of zeros in annual individual income measures). We also report the pre-reform means as benchmarks. In years 1, 2, and 3–5 after residency, annual transfer income drops by approximately \$10,000, \$8,000, and \$5,000, which corresponds to 55%, 45%, and 30% reductions, respectively. At the same time, labor earnings rise by \$1,100 – \$1,600. However, while large in relative size, earnings remain low in absolute levels and the reform's effects on earnings far from compensate the lower benefit levels.

Table 2. Effect of reform on subsequent annual individual transfers, labor earnings (both measured in USD 1,000), employment, unemployment, and fraction not in the labor force.

	(1) *Transfer income*	**(2)** *Labor earnings*	**(3)** *Employment rate*	**(4)** *Unemployment rate*	**(5)** *Not in labor force*
Years since residency	Pre-reform mean	Pre-reform mean	Pre-reform mean	Pre-reform mean	Pre-reform mean
1	18.431	1.852	0.103	0.868	0.028
	(7.663)	(6.353)	(0.305)	(0.339)	(0.164)
2	17.979	4.182	0.188	0.755	0.053
	(8.655)	(10.401)	(0.391)	(0.430)	(0.224)
3-5	15.849	8.424	0.323	0.581	0.093
	(8.760)	(13.273)	(0.390)	(0.401)	(0.230)
Observations	3,044	3,044	3,044	3,044	3,044
Years since residency	Estimate	Estimate	Estimate	Estimate	Estimate
1	-9.775	1.144	0.092	-0.164	0.072
	(0.407)	(0.400)	(0.022)	(0.027)	(0.014)
2	-8.320	1.567	0.070	-0.158	0.093
	(0.446)	(0.541)	(0.019)	(0.028)	(0.020)
3-5	-4.956	1.070	0.041	-0.104	0.066
	(0.457)	(0.451)	(0.021)	(0.015)	(0.012)
T-value: year 1 - year 3-5	7.875	0.123	1.677	-1.943	0.246
Observations	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 subsequent income from transfers and labor earnings (at the individual level), and the probability of being employed, unemployed, and not in the labor force measured 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 also shows pre-reform means of the outcome variables and tests of the differences between estimates for year 1 and years 3-5. All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors in parentheses. Standard errors are clustered by residency month.

Similarly, average first-year employment rates post-reform almost double in the first year after the reform, from 10.3% to 19.5% (column 3), which is in line with Huynh et al. (2007) and Rosholm and Vejlin (2010). Yet, the reform effect reduces to 7 percentage points (or 37%) in year 2, and to 4 percentage points in years 3-5 – an estimate that is significantly lower (at a 10% level) than the year 1 effect.

The reform effect on employment is nearly exclusively due to unskilled manual work (Table A.7), and the effects are homogeneous over different education groups (Table A.8), which is a first piece of evidence that education accumulated in the home country is either of little value in the Danish labor market, or refugees lack complementary skills (such as language) to make these skills productive.[27] Thus, the overall labor supply effect of the lower transfers appears to be that refugees were incentivized to take up employment faster than they otherwise would have, and that this was mainly in unskilled manual work. We will return to this point in Section 5 where we investigate how the reform's effects on employment, job stability, and job types are mediated by local labor demand.

Table 2 shows further that the reform lowered unemployment by around 16, 16, and 10 percentage points in years 1, 2, and 3–5 (column 4); a decrease far larger than the increase in employment. The difference is explained by a dramatic increase of individuals leaving the labor force: in year 1, the share of those out of the labor force increases by 7 percentage points.[28] Table 3, which provides estimates by gender, shows that the reason for this increase is due to females, who show only a small insignificant employment response in these initial years, but experience a large reduction in unemployment rates. In contrast, for males, the drop in unemployment is accompanied by a corresponding increase in

[27] This resembles LoPalo's (2019) finding for the US that shows that lower benefit levels may reduce the quality of jobs that refugees take. See also Rosholm et al. (2006) and Fasani et al. (2018) on mismatch between occupations in Europe and refugees' employment. Foged et al. (forthcoming) find that the introduction of integration courses in 1999 led to higher labor earnings for refugees. Hence, as pre-reform refugees have higher unemployment rates (and thus higher participation rates in integration courses to be eligible for social assistance, which we also show in column 4 of Table A.6) in the first years after residency, they could potentially acquire more language skills than post-reform refugees This in turn may contribute to the longer run fade out in employment effects and lower job-quality in low demand areas.

[28] The sharp discontinuity around the reform date is further illustrated by Figure A.2, which shows labor market outcomes during year 1 and 2 after residency for individuals granted residency around the reform date. Here, employment rates increase from a pre-reform mean of 10% to around 20%, while unemployment rates decrease from 90% to around 70-75%, with the difference attributable to an increase in the NILF rate.

employment. The decrease in female unemployment and increase in labor force exits as a response to the reform underscores the importance (amply stressed by Bratberg and Vaage, 2000; Card et al., 2007b; and Kyyra and Ollikainen, 2008) of distinguishing between welfare benefits' effects on unemployment versus those on employment and total nonemployment.

Table 3. Effect of reform on subsequent labor market outcomes, by gender.

Years since residency	(1) Males Employment	(2) Males Unemployment	(3) Males Not in the labor force	(4) Females Employment	(5) Females Unemployment	(6) Females Not in the labor force
1	0.160	-0.155	-0.004	0.037	-0.171	0.132
	(0.044)	(0.045)	(0.012)	(0.022)	(0.022)	(0.021)
2	0.132	-0.137	0.011	0.015	-0.172	0.162
	(0.035)	(0.038)	(0.023)	(0.019)	(0.029)	(0.025)
3-5	0.042	-0.069	0.029	0.041	-0.130	0.093
	(0.033)	(0.024)	(0.024)	(0.016)	(0.024)	(0.019)
T-value: year 1 - year 3-5	2.145	-1.686	0.932	-0.147	-1.259	1.377
Observations	2,390	2,390	2,390	2,453	2,453	2,453

Note: The table shows the estimated effects, by gender, of being granted residency after the reform relative to before the reform on the 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 since residency. The table also shows tests of the differences between estimates for year 1 and years 3-5. Columns 1-3 present results for males, and columns 4-6 present results for females. All estimates are conditional on the running variable, covariates (see Table 1), and year fixed effects. Standard errors in parentheses. Standard errors are clustered by residency month.

C. Employment, Disincentive Effects, and Nonparticipation of Females

There are two reasons for the strong increase in female labor force exits in response to the reform. First, household level means testing reduces female transfers if the male takes up employment. Second, within-household incentives were affected when the same overall transfers for the household were differently allocated within couples according to whether both spouses arrived after the reform (Type A couples where both partners received Start Aid), or the first spouse arrived before and the second after the reform (Type B couples where the first received SoA while the second received virtually no transfers at all, see Section 2). In Type B couples, the last arriving spouse's incentive to remain transfer-eligible by staying in the workforce and attending integration courses (around 30 hours per week and a pre-requisite for transfer receipt) was essentially removed.[29]

Our empirical assessment confirms these predictions. We separately estimate the effects on labor market outcomes by gender and household type, distinguishing between effects for Type A and B couples (75% of our adult sample) relative to baseline couples in Panels A and B, respectively, and post-reform singles relative to pre-reform singles in Panel C.[30] The estimates in Panels A-C of Table 4 show that male employment in year 1 increases by 15 and 8 percentage points for Type A and Type B couples, respectively, and 17 percentage points for singles (compared to pre-reform households). Female employment in Type A couples increases in year 1 by 8 percentage points, the unemployment rate decreases by 17 percentage points, and the fraction of women not in the labor force increases by 9 percentage points. Type B females and singles have a more muted (and insignificant) employment response of about 3 percentage points although point estimates are not significantly different across Type A, Type B, and single females. However, for Type B females unemployment is 20 percentage points lower post reform than pre reform, which is accounted for by a 17 percentage points increase in the fraction not in the labor

[29] We illustrate the intuition underlying the different incentives for Type A and B couples in a simple static labor supply framework in Figure A.3. (see Lemieux and Milligan, 2008, for a similar illustration). By reducing the SoA for each partner to Start Aid, the reform decreases Type A couples' nonlabor income and the couple improves utility by supplying some labor. For Type B couples, household level transfers drop by the same amount, but transfers are unchanged at SoA for the first arriving spouse and reduced to zero for the last arriving, who thus cannot be penalized for dropping out of an integration program. Type B couples can thereby increase household leisure (with an implicit price of leisure equal to zero) without reducing transfer income by dropping out of integration courses and the labor force.

[30] Because differences in residency dates may now cause "time since residency" to capture different periods for each spouse, we align spouses' outcomes by defining this variable as time since residency of the last arriving spouse. This way we also center the outcomes by the residency that defines a household's treatment status (Baseline vs. Type B and Type B vs. Type A), which is determined by the timing of residency for the last arriving spouse. To ensure that our results are not driven by this definition, we replicate our findings using time since residency for the first arrived spouse (results available on request).

force, a reform effect that is significantly larger than the corresponding estimates for single females in both years and for Type A females in year 2 after residency.

Comparing the reform effect on the probability that both spouses are in employment (6.5 percentage points in Panel D, column 1) with females' employment response for Type A couples (7.9 percentage points in Panel A, column 4) shows that almost all female employment responses can be explained by an increase in dual earner households. The table further suggests a link between the increased employment uptake of husbands (6.4 percentage points in Panel D, column 3) and the increased fraction of females not in the labor force (9.0 percentage points in Panel A, column 6) in Type A couples, which is likely due to means testing. Our results for Type A couples thus illustrate the importance of household-level responses and the potential adverse consequences of disincentives inherent to transfer systems with household-level means testing.

Obviously, Type B couples where partners arrive on both sides of the reform occur only during the implementation period and are therefore less relevant for assessment of the reform's longer-term impact. Nevertheless, the findings show that ignoring the difference in responses may lead to inaccurate conclusions about the reform's immediate effects. From Panel D, we also see that employment effects in Type B couples are driven solely by *single earner* responses where only the first arriving spouse finds employment, inducing means testing of the other spouse's transfers. Thus, the 10-percentage point difference between total female labor force exits in Type B couples (0.173, cf. column 3 in Panel B) and those who drop out when their spouses find employment (0.070, cf. column 3 in Panel D) constitutes a lower bound for the disincentives induced by the reform's asymmetric benefit allocation.[31]

[31] The asymmetric allocation of transfers has also implications for reservation wages, with the highest wage rates needed to induce labor supply in the pre-reform group, while the wage rate for Type B couples required to supply labor

Table 4. Effect of reform on subsequent labor market outcomes, by gender and household type

Year	(1) Employment	(2) Unemployment	(3) Not in the labor force	(4) Employment	(5) Unemployment	(6) Not in the labor force
A) Type A Couples, both granted residency after reform						
		Males			*Females*	
Year 1	0.153	-0.202	0.049	0.079	-0.169	0.090
	(0.054)	(0.046)	(0.032)	(0.033)	(0.051)	(0.031)
Year 2	0.095	-0.134	0.039	0.081	-0.110	0.031
	(0.059)	(0.064)	(0.036)	(0.044)	(0.053)	(0.044)
B) Type B Couples, one granted residency after reform						
		Males			*Females*	
Year 1	0.075	-0.101	0.025	0.031	-0.204	0.173
	(0.043)	(0.043)	(0.024)	(0.018)	(0.043)	(0.038)
Year 2	0.107	-0.137	0.029	0.036	-0.209	0.176
	(0.046)	(0.055)	(0.032)	(0.024)	(0.046)	(0.042)
C) Singles						
		Males			*Females*	
Year 1	0.168	-0.172	0.004	0.028	-0.030	-0.018
	(0.065)	(0.075)	(0.0267)	(0.044)	(0.114)	(0.058)
Year 2	0.149	-0.138	0.009	0.009	-0.002	0.026
	(0.074)	(0.082)	(0.043)	(0.097)	(0.107)	(0.072)

D) Separating employment effects and females labor force dropouts by dual versus single earners

	(1) Both spouses in employment	(2) Only one spouse in employment	(3) Female dropout when male employment
Type A couples, Year 1	0.065	0.103	0.064
	(0.030)	(0.044)	(0.019)
Type B couples, Year 1	-0.001	0.109	0.070
	(0.015)	(0.038)	(0.022)

Notes: The table shows the estimated effects of being granted residency after the reform on labor market outcomes by household type. Panels A-C) show effects for Type A and B couples (relative to pre-reform couples), and singles (relative to pre-reform singles) on the probability of being employed, unemployed, or not in the labor force. Panel D) further separates couples' employment effects and labor force dropouts in year 1. The 1st outcome is a dummy=1 if both spouses were employed, the 2nd a dummy=1 if only one spouse was employed, and the 3rd a dummy=1 if the female was not in the labor force and her husband was employed. Panels A, B, D) are estimated by Eq. (2), and C) is estimated by Eq. (1). Standard errors (in parentheses) are clustered on two-way level by residency month and household for couples, and by residency month for singles.

The analysis of Type A and B couples also illustrates how subtle differences in incentives can generate large differences in labor supply responses and in key policy parameters such as household level elasticities of labor earnings with

should be higher than that required for Type A couples, which is precisely in line with the estimated hourly wage distributions for male spouses (Figure A.4).

respect to benefit levels (which we estimate to be 1.36 for Type A couples vs. 0.38 (and insignificant) for Type B couples in year 1, see Table A.9, Panel A).[32]

D. Robustness Tests

We have performed an array of robustness tests. First, we construct estimates defining a placebo reform dummy for individuals who received residency before or after July 1st 2000 (i.e. two years before the actual reform) around an 18-month bandwidth, which are all very close to zero and insignificant (see Table A.10).[33] Second, we estimate models of the effect of the reform on labor market outcomes with more flexible running variables, a donut-sampling (excluding the months around the reform), a reduced bandwidth, and including Afghans and Yugoslavs (Table A.11). All these estimates are similar to those reported above. Third, we present the estimated effects of the reform on employment from year 1-10 after residency across different bandwidth choices for the main estimation sample (Figure A.6), and for year one both including refugees from Afghanistan and Yugoslavia (Figure A.7). Point estimates are remarkably stable across specifications. Fourth, Table A.12 shows that point estimates reported in Table 2 are unaffected by the choice of conditioning variables (which only serve to increase precision). Fifth, Table A.13 presents the estimated differences in employment rates of labor migrants – who are ineligible for SoA and Start Aid –

[32] This finding supports also Ashenfelter's (1983) evidence that elasticities depend on the implied tax rates from means testing and nonpecuniary costs of welfare receipt. A related question is whether the gender differences in reform-effects relate to first vs. last arriving spouse or whether social norms and labor-market related gender roles also play a role. Females receive residency last in 86% of couples, which leaves us with too small a sample size for analysis. A simple plot (available on request) of the fraction of last-arriving males who are not in the labor force increase is close to 0 for those who received residency in the months leading up to the reform and 10-15% for those who received residency in the months after the reform. Thus, the labor force withdrawal also appears to be present for last-arriving males in Type B couples.

[33] We have also estimated Equation (1) using placebo reforms from 5 months before to 5 months after the actual reform. Regardless of whether we use transfer income (Figure A.5A: full sample and by gender) or employment, unemployment, and NILF as outcomes (Figure A.5B), the *t*-values from the estimated β's are between 0 and 1 (except for males' transfers in placebo month -5, where the *t*-value is 1.5) for placebo reforms more than 4 months on either side of the actual reform. As the timing of the placebo reform converges toward the true reform date, the *t*-values increase, jumping dramatically to reach their maximum level at this date (the figure's center).

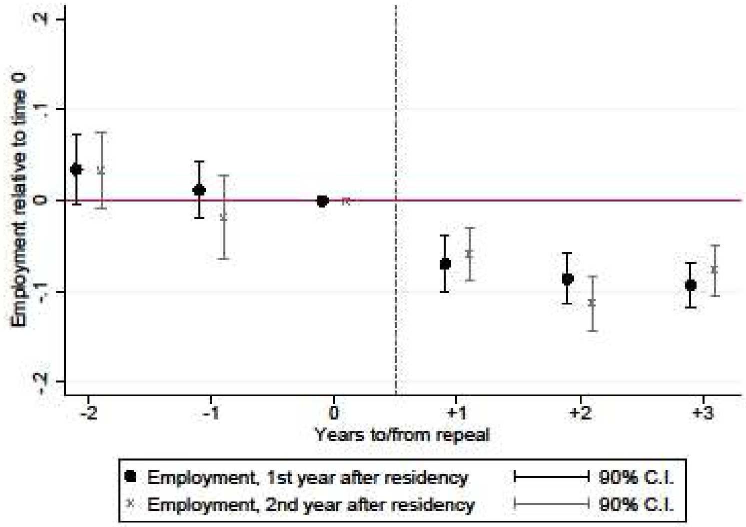
according to whether they receive residency before or after the Start Aid reform (mimicking the design for refugee migrants). All estimates for labor migrants' employment in years 1-5 after residency are close to zero and insignificant. Hence, our findings are not results of general changes in the Danish labor market. Finally, our main empirical specification relies on a linear running variable. While Table A.11 replicates our findings for labor market outcomes using a quadratic running variable, another approach is to estimate effects using local linear regression (LLR). Table A.14 compares all the main findings in the paper with estimates where the pre- and post-reform slopes are estimated using LLR. All our conclusions remain unchanged.

E. The Repeal of the Reform

The Start Aid reform was repealed ten years after its introduction, on January 1, 2012 (proposed on November 21st, 2011), when transfers to all refugees were increased to pre-Start Aid levels. While the repeal of Start Aid affected all refugees and thus does not provide an obvious control group, it is nevertheless insightful and a further robustness test to investigate whether similar responses can be observed as at its introduction. Appendix B.2 describes the data used for the repeal analysis.

Figure 5 presents event study estimates of the effect of the repeal on males' and females' (Panels A and B) employment in years 1 and 2 after receiving residency. Overall, pre-repeal estimates are all insignificant and close to zero, with the exception of females, where borderline significant results in year -3 suggest a slight violation of the parallel trends assumption.

Panel A. Males



Panel B. Females

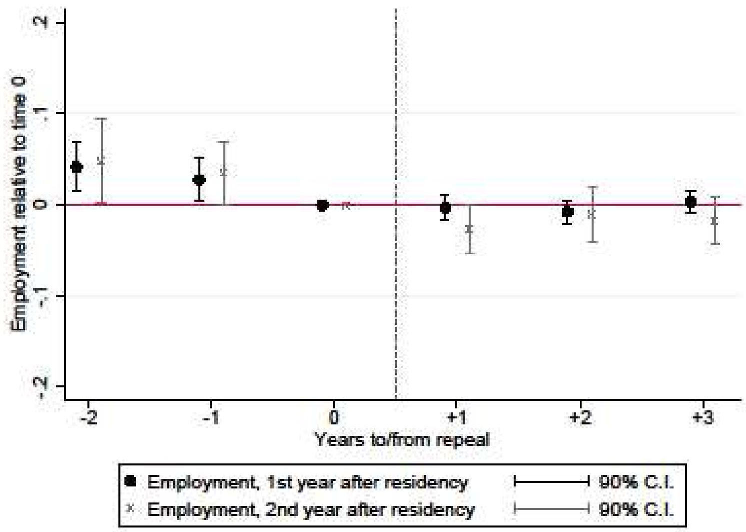


FIGURE 5. EMPLOYMENT DIFFERENCES IN YEAR 1 AND 2 AFTER RESIDENCY AROUND THE REPEAL

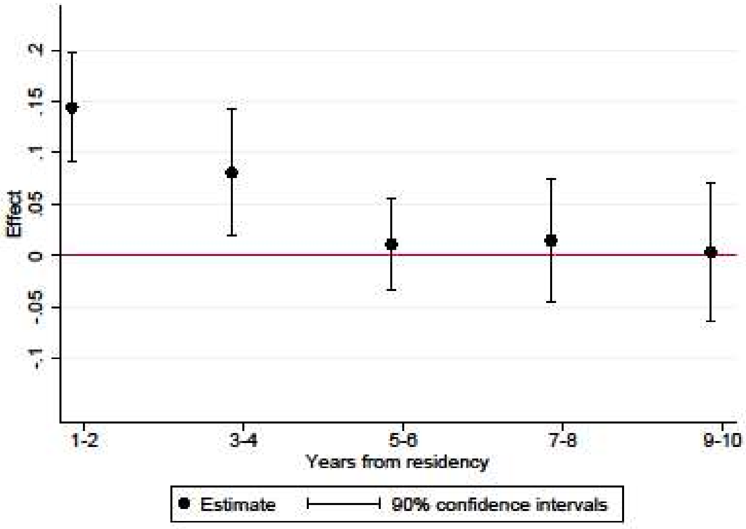
Notes: The figure shows estimated differences in employment and 90% confidence intervals in the 1st and 2nd year after residency according to whether refugees were exposed to the repeal of the Start Aid (increasing transfers in 2012) marked by the vertical dashed line. When measuring employment in the 1st year after residency, the pre-repeal (control) years include those receiving residency in 2009-2011 (-2 to 0 on the x-axis in the figure) and the post-repeal (treatment) years include those receiving residency in 2012-2014 (1 to 3 on the x-axis in the figure). When measuring employment in the 2nd year after the repeal, the pre-repeal (control) years include those receiving residency in 2008-2010 (-2 to 0 on x-axis in the figure) and the post-repeal (treatment) years include those receiving residency in 2011-2013 (1 to 3 on the x-axis in the figure). Year 0 is the reference group in each estimation.

Employment rates in the first two years after receiving residency drop by around 8-10 percentage points for males who are affected by the repeal. This is smaller than the estimated opposite effect of the 2002 introduction of Start Aid, where males' employment increased by 16 and 13 percentage points in years 1 and 2 after residency, respectively. For females, the employment effects are close to zero, similar to what we find when Start Aid was introduced. The fraction of females who receive no transfers and instead exit the labor force also decreases after the repeal (not shown here) with a similar magnitude to the reduction in males' employment (cf. Figure 5). This is likely a result of a reduction in means testing of females' transfers (see section 4.3) when fewer males find employment. Overall, these estimates follow the same patterns (with opposite signs) as was found for the introduction of the Start Aid reform in 2002.

F. Long Run Reform Effects

Figure 6 extends the time horizon and summarizes the reform's effects on the probability of adult males being in employment up to 10 years post residency. Although overall labor supply effects are initially considerable in magnitude – close to 15 percentage points on average (Figure 6A) – they decrease significantly relative to the initial effects and remain statistically insignificant after about 5-6 years (i.e., the reform effects have faded several years before the repeal increased transfer levels in 2012). Distinguishing between singles, Type A and Type B couples, Figure 6B shows that employment responses for Type B couples disappear after the first two years, while those for Type A couples and singles are more persistent but also disappear about 5-6 years after the reform (while year 1-2 estimates are significantly different from estimates in years 5-10 for the full sample in Figure 6A, we cannot reject that year 1-2 estimates are equal to estimates for later years once the sample is split by household type in Figure 6B).

Panel A. Males' employment, all males



Panel B. Males' employment by household type

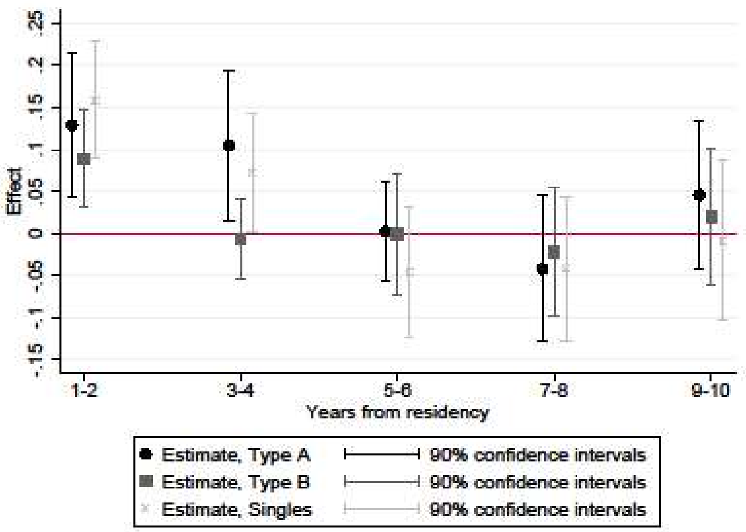


FIGURE 6. EFFECT OF REFORM ON MALES' EMPLOYMENT RATES, 1-10 YEARS AFTER RESIDENCY

Notes: The figure shows estimated effect of the reform and 90% confidence intervals on A) males' probability of being employed estimated by Eq. (1), and B) males' probability of being employed by household type estimated by Eq. (2). Standard errors are in A) clustered by residency month and in B) clustered on two-way level by residency month and household.

Thus, while the reform induces substantial labor supply responses in the first two years after its implementation, the reform's average effects appear to dissipate in the longer run.[34] However, as we will show in the next section, this result masks substantial and significant heterogeneity in the effects of the reform driven by local labor demand differences.

IV. Reform Effects and Local Labor Demand

While the previous section illustrates how labor supply incentives affect the responses to the reform, employment uptake also depends on the demand side of the labor market. The way welfare policies interact with labor demand is indeed a central question in understanding their impacts. For example, Ziliak et al. (2000), Ganong and Liebman (2018), and Kleven (2019) point out that estimated employment effects of welfare reforms such as expansions of the Earned Income Tax Credit (EITC) may partly be driven by business cycles. Moreover, when assessing reform effects for refugees, this issue is particularly relevant, as the skills refugees bring with them may be of little value in the Danish labor market, partly because of lack of complementary skills such as language proficiency. This is supported by Table A.7 (discussed in Section 4.2) which shows that refugees who enter employment in the first year after their arrival almost always take low skilled manual jobs, no matter what their level of education is.

How local labor demand conditions mediate the effects of a welfare reform is not easily analyzed for two reasons. First, most studies of welfare reforms similar to ours use temporal or spatial variation in reform implementation for identification.[35] In contrast, our use of a discontinuity design to study a reform

[34] Figure A.8 displays the reform effects on labor earnings by household type. The results are similar to our findings on employment, albeit more imprecisely estimated.

[35] See, for example, the vast literature studying the effects of welfare reforms in the U.S. (e.g., Hendren and Sprung-Keyser, 2020, review studies of U.S. public policies over 50 years) and Borjas (2002) who focusses on immigrants.

that has been implemented uniformly throughout the entire country allows distinction of reform effects across local labor markets. Secondly, spatial selection of individuals will distort any estimates that seek to understand the effects of local economic conditions on a reform's effect, an issue that is particularly severe when investigating the effect of a welfare reform that targets immigrants. To address the sorting problem, we utilize that the implementation of our reform overlapped with a period where refugees, upon obtaining residency, were quasi-randomly allocated across municipalities. This provides exogeneous variation in local conditions that allows us to study how the reform's immediate and longer-term effects interact with local labor demand. As explained in Section 2, we use two indicators for local labor demand, based on job-openings in low- and unskilled work, and on municipal average employment rates of non-Western immigrants.

A. *Employment Effects, Jobs Quality, and Local Labor Demand*

Table 5 presents the effects of the Start Aid reform on employment in years 1-5 after residency, distinguishing between municipalities where local labor demand is above and below the median, and focusing on males (who are driving the employment response of the reform).[36] All estimates condition on a range of other municipality level characteristics, such as population density (population size divided by area size in each municipality), size of immigrant population, voting share for anti-immigrant parties, and regional fixed effects. Panel A, which displays estimates for overall employment effects alongside pre-reform employment levels and the difference between effects in high- and low demand municipalities, illustrates large differences in the effect on employment between high- and low demand municipalities.

36 Table A.15 shows the corresponding results for the full sample. Table A.16 shows that conclusions are unaffected by the inclusion of municipality characteristics as controls, stressing that the quasi-random allocation (to high/low demand areas) indeed allows us to capture the effects of local labor demand.

Table 5. Effect of the reform on males' employment and job-type by assignment municipality.

	Year 1	Year 2	Years 3-5
A) Employment, using job openings in low / unskilled jobs			
High demand, Reform effect	0.184	0.207	0.097
	(0.048)	(0.045)	(0.042)
Pre-reform mean	*0.164*	*0.298*	*0.465*
Low demand, Reform effect	0.125	0.042	-0.032
	(0.078)	(0.055)	(0.026)
Pre-reform mean	*0.174*	*0.274*	*0.443*
High-low difference in reform effect	0.059	0.165	0.129
	(0.072)	(0.068)	(0.045)
B) Employment, using average employment of non-Western immigrants			
High demand, Reform effect	0.163	0.206	0.096
	(0.048)	(0.048)	(0.044)
Pre-reform mean	*0.178*	*0.306*	*0.475*
Low demand, Reform effect	0.157	0.068	-0.013
	(0.071)	(0.051)	(0.030)
Pre-reform mean	*0.158*	*0.270*	*0.435*
High-low difference in reform effect	0.006	0.138	0.109
	(0.061)	(0.063)	(0.050)
C) Decomposing high-low difference by inflow and stay in employment			
Inflow from non-employment	0.059	0.132	-0.004
	(0.072)	(0.063)	(0.031)
Stay in employment	-	0.033	0.133
		(0.073)	(0.054)
D) Decomposing high-low difference by job-type			
Unskilled manual work			
Inflow from non-employment	0.033	0.144	-0.014
	(0.055)	(0.053)	(0.020)
Stay in employment	-	0.023	0.008
		(0.049)	(0.028)
Work requiring some skills			
Inflow from non-employment	0.038	-0.012	0.009
	(0.045)	(0.037)	(0.018)
Stay in employment	-	0.010	0.125
		(0.035)	(0.058)
Observations	2,390	2,390	2,390

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform for male refugees (aged 18-55 at the time of residency) assigned to municipalities with high/low local labor demand. High/low labor demand is defined in Panels A, C and D 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. Panels A and B show employment effects in years 1, 2, and 3-5 since residency. Panel C uses that *employment = inflow + stay in employment* to decompose the difference in employment effects. Panel D decomposes employment effects into unskilled manual work and work requiring some skills by inflow and stay in employment. Standard errors are clustered on two-way level by residency month and allocation municipality.

The employment effects of the reform in high demand municipalities are around 20 percentage points in years 1 and 2 and decrease to 10 percentage points in years 3-5. In low demand municipalities, none of the estimates are significantly different from zero, and point estimates decrease from around 13 percentage points in year 1, to 5 percentage points in year 2, and to around zero in years 3-5. Thus, employment effects of the reform are strikingly different across municipalities with different demand conditions. Aggregating the differences in reform effects over the first five years after residency shows that each male refugee with post-reform residency has on average spent 0.61 (0.059+0.165+0.129*3) years more in employment (which amounts to 33% of the average pre-reform level for males) because of the reform if he is assigned to a high demand municipality relative to a low demand one. Estimates using the alternative labor demand indicator in Panel B (non-Western immigrants' employment rates) are very similar.

In Panel C of Table 5, we decompose the total difference in employment effects between high- and low demand municipalities for years 2 and 3-5 (0.165 and 0.129) into differences in inflows and continuation in employment from one year to the next.[37] The estimates show that higher inflows into employment in high demand municipalities explain most of the difference in year 2, while a higher probability of staying in employment drives the difference in years 3-5. Thus, higher employment effects of the reform in high demand municipalities in the first two years after the reform are explained by more individuals entering employment, while in later years they are mainly driven by those remaining employed who found work early on.

[37] The fraction in employment in year t equals the fraction entering employment in year t from non-employment in year t-1 plus the fraction that continues in employment from year t-1 to year t.

To investigate further whether local demand conditions affect the type of jobs individuals take in response to the reform, we next decompose the difference in the reform's effects between high- and low demand municipalities into effects on employment in unskilled manual work and work that requires some skills (Panel D of Table 5).[38] The results show that the differences in inflows are driven by take up of unskilled manual work, while the differences in the probability of staying in employment are due to jobs that require some skills. Decomposing the difference of 12.5 percentage points in the probability of staying in employment for work requiring some skills between high and low demand municipalities in years 3-5 further shows that 25% are accounted for by job changes from an unskilled job to a job requiring some skills and 75% by continuation in jobs requiring some skills.

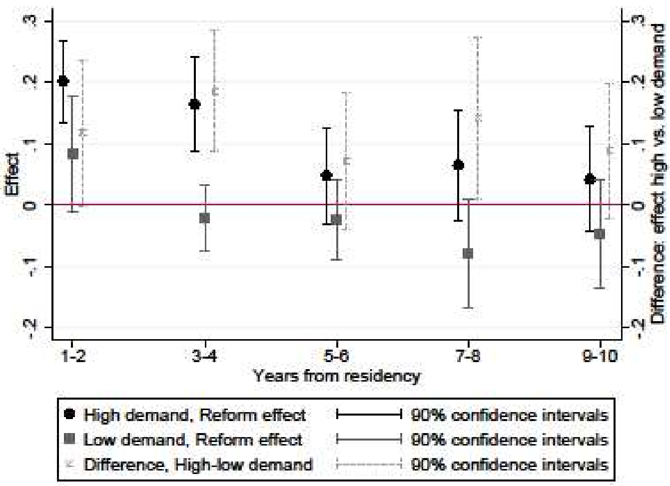
Overall, therefore, the reform had not only substantially higher and far more persistent employment effects in high demand municipalities compared to low demand municipalities, but it also led to more stable employment in higher quality jobs. The different longer-run impact of the reform across local labor demand is also evident from the changes to the labor earnings distribution. Table A.17 shows the estimated effect of the reform on labor earnings levels and the labor earnings distribution in year 3-5 in low and high demand municipalities, respectively. The reform resulted in a downward shift in the labor earnings distribution in low demand areas but an upward shift in high demand areas.

To describe differences across local labor demand further, Figure 7A displays the reform effects the first 10 years after residency for males separately for high and low demand municipalities, and the differences between the two. After 3-4 years the effects disappear in low demand municipalities, but they remain positive

[38] The former category is the lowest category of unskilled work such as cleaning or scaffolding work, and the latter consists of, for example, installation or transport of basic equipment, or miscellaneous sales work. We decompose employment by occupation type in year t as the fraction entering employment (either in a job requiring some skills or unskilled manual work) in year t from non-employment in year t-1 plus the fraction that continues in employment from year t-1 to year t (with employment in year t either in a job requiring some skills or unskilled manual work).

in high demand municipalities (although imprecisely estimated), with the difference between low and high demand municipalities being positive and close to being statistically significant at the 10% level.

Panel A. Males' employment in low and high demand areas



Panel B. Males' probability of staying in employment across years in low and high demand areas

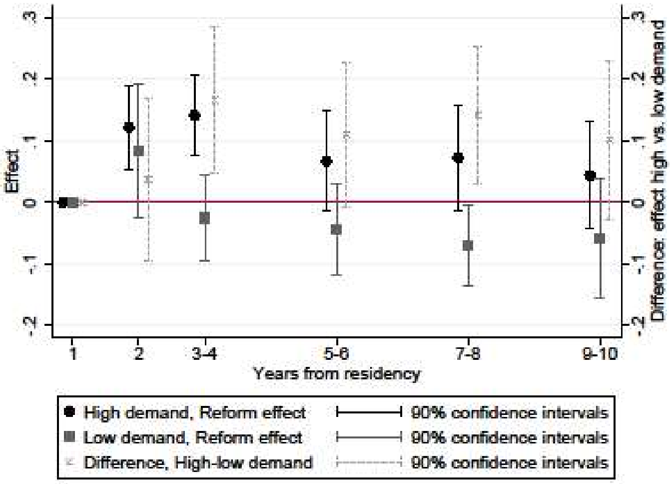


FIGURE 7. EFFECT OF REFORM ON MALES' EMPLOYMENT RATES, 1-10 YEARS AFTER RESIDENCY, BY LOCAL LABOR DEMAND

Notes: The figure shows estimated effect of the reform and 90% confidence intervals on A) males' probability of being employed by local labor demand and B) males' probability of staying in employment from one year to the next. Both Fig. A) and B) are estimated by Eq. (3). Standard errors are clustered on two-way level by residency month and allocation municipality.

Figure 7B shows that the reform affects the probability that refugees continue in employment from one year to the next positively throughout the first 10 years in high demand municipalities, while reform effects turn negative after years 3-4 years in municipalities with low demand.

B. Public Expenditure and Local Labor Demand

The differences in employment outcomes and labor earnings across local labor demand will also influence the effect of the reform on public spending. Table 6 displays (for males and females jointly) the yearly average reform effects and reform effects at the 5th, 50th, and 95th percentiles of local labor demand over the first 5 years after residency on employment, labor earnings, and public expenditures.[39]

Following the reform, employment and labor earnings increased by 15-20% on average over the first five years after residency (Panels A and B). While there were no significant reform effects at the 5th percentile, the reform led to 30-35% higher employment and labor earnings at the 95th percentile of local labor demand. The *overall* difference in post-reform refugees' labor earnings between municipalities at low and high levels of local demand conditions (columns 2 and 4) amounts to 70% ($5,416 vs. $9,291), compared to a difference of only 15% ($5,875 vs. $6,824) for pre-reform refugees.

[39] The estimates reported in column 2-4 of Table 6 are based on Equation (1) where we weight observations by an Epanechnikov kernel according to the ranking of municipalities' local labor demand (the running variable in the reform estimates are still linear as in the remainder of the analyses). Columns 2-4 present the estimates with the 5th, 50th, and 95th percentiles as center of the kernels, respectively. Pre-reform rows show the estimated constant term (α, cf. Equation 1). Post-reform rows show the estimated constant plus the reform effect ($\alpha + \beta$, cf. Equation 1).

Table 6. Effects of the reform on average employment, labor earnings, disposable income, and public expenditures for years 1-5, by assignment municipality's labor demand.

Percentiles	**(1)** *Full sample*	(2) *5th*	**(3)** *50th*	**(4)** *95th*	**(5***)* *Difference 95th-5th*
A) Employment					
Pre-reform	0.252	0.234	0.244	0.275	0.028
Post-reform	0.302	0.261	0.294	0.352	0.091
Reform effect	0.050	0.027	0.050	0.077	0.050
	(0.024)	(0.030)	(0.027)	(0.035)	(0.046)
B) Labor earnings, $1,000					
Pre-reform	6.258	5.875	6.065	6.824	0.949
Post-reform	7.103	5.416	6.626	9.291	3.875
Reform effect	0.845	-0.459	0.561	2.467	2.926
	(0.940)	(0.708)	(1.038)	(1.400)	(1.400)
C) Public expenditures, $1,000					
Pre-reform	11.190	11.440	11.341	10.841	0.338
Post-reform	5.967	7.403	6.084	4.534	-2.869
Reform effect	-5.223	-4.037	-5.257	-6.307	-2.270
	(0.415)	(0.698)	(0.580)	(0.698)	(0.987)
D) Disposable income, $1,000					
Pre-reform	17.819	17.633	17.756	18.077	0.444
Post-reform	13.301	12.980	12.944	14.110	1.130
Reform effect	-4.518	-4.653	-4.812	-3.966	0.687
	(0.523)	(0.538)	(0.572)	(0.705)	(0.887)

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform for refugees (aged 18-55 at the time of residency) assigned to municipalities with different local labor demand. Panel A: effects on average employment in years 1-5. Panel B: effects on average labor earnings in years 1-5. Panel C: effects on average public expenditures (transfer income minus tax payments) years 1-5. Panel D: effects on average post-tax disposable income (pre-tax gross income minus tax payments) in years 1-5. Rank of local labor demand is defined from the municipal predicted ratio of the number of job openings in low / unskilled work relative to the number of unemployed individuals. Column 1 presents estimates of Eq. (1). Columns 2-4 present results from three regressions for each outcome where we estimate Eq. (1) weighting observations by kernels with center at the 5th, 50th, and 95th percentiles of local labor demand, respectively, using an Epanechnikov weighting kernel. Hence the estimates reflect the reform effects in municipalities close the 5th, 50th, and 95th percentiles. Column 5 presents the difference between rows 4 and 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. Observations: 4,843.

On average, the reform resulted in a reduction of public expenditures per refugee by almost 50%, through the combination of lower transfers and increased tax payments from labor earnings (Panel C).[40] However, the substantial differences in reform effects on employment and earnings across local labor markets with different labor demand conditions result in a reduction of just 35% relative to the pre-reform mean in municipalities with the lowest labor demand, but of around 60% in municipalities with the highest labor demand. These findings have important implications for refugee allocation policies, which often quasi-randomly assign refugees to local labor markets without taking account of local labor demand conditions. Our findings suggest that the success of reforms aimed at increasing labor supply incentives may be impeded by sub-optimal allocation policies.[41] Moreover, while average pre-reform disposable income was more than 35% higher than average post-reform disposable income in low demand areas, the reform only resulted in a disposable income reduction of 25% in high demand areas (Panel D). Thus, through increased employment rates, labor earnings, and self-sufficiency, the overall reduction in disposable income that refugees experienced due to the reform was substantially lower in high demand areas.

V. Reform Effects on Poverty and Crime

Table 7 shows the effects of the reform on the probability of living with an annual post-tax disposable income corresponding to less than \$500 per month (slightly above the U.S. Census' deep-poverty threshold, Panel A), less than \$750 per month (slightly below the U.S. Census' poverty threshold, Panel B), and less than \$1,000 per month (Panel C) in years 1, 2, and 3-5 after residency. The estimates show that the reform led to an increase of between 30-50 percentage

[40] It should be noted that this does not consider adverse effects on refugees through benefit cuts and other channels, to which we return in Section 5.

[41] One concern is that unequal allocation may lead to unwanted political responses of majority populations. However, Dustmann et al. (2019) show that vote shares of anti-immigrant parties are not positively affected by refugee allocations in more urban municipalities (see also related work by Steinmayr 2020).

points (depending on cut-off) in the probability of experiencing very low post-tax disposable incomes. For example, the probability of having less than $750 per month in the first year after residency increases from 9% to almost 50% (Figure A.9A and B present plots of the fraction with low disposable income by timing of residency relative to the reform). Moreover, the effects are most pronounced in the first year following residency where most refugees rely on public benefits.

Table 7. Effects of the reform on the probability of having monthly disposable income below $500, $750, and $1,000, respectively

Income per month	Year 1	Year 2	Year 3-5	T-value: year 1 - year 3-5
A) Disposable income<$500				
Pre-reform	0.027	0.024	0.021	
Post-reform	0.311	0.237	0.115	
Reform effect	0.284	0.213	0.094	5.554
	(0.027)	(0.024)	(0.021)	
B) Disposable income<$750				
Pre-reform	0.087	0.071	0.047	
Post-reform	0.557	0.356	0.200	
Reform effect	0.470	0.285	0.153	9.307
	(0.026)	(0.025)	(0.022)	
C) Disposable income<$1,000				
Pre-reform	0.254	0.188	0.121	
Post-reform	0.755	0.532	0.644	
Reform effect	0.501	0.344	0.223	5.012
	(0.049)	(0.028)	(0.026)	
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 for refugees (aged 18-55 at the time of residency) on the probability of having post-tax disposable income below $500, $750, and $1,000 per month. The outcome is defined by dividing annual disposable income by 12 thereby expressing the average income in each month in that year. The table shows results for disposable income in year 1, year 2, and the average of year 3-5 after residency. The column "T-value: year 1 - year 3-5" shows the *t*-value from a test of difference between the year 1 and year 3-5 estimates. Standard errors are clustered by residency month.

A natural question is whether the large decrease in disposable income led to increases in crime, as for example, recent work by Deshpande and Mueller-Smith (forthcoming) would suggest. To investigate this, Panel A of Table 8 displays the estimated effect of the reform on adults' crime for year 1, and Panel B shows results accumulated for years 1-5 after residency. Columns 1 and 2 show results for probabilities of receiving a crime conviction and the number of crime convictions, respectively.[42] The table shows that the reform caused the probability of committing a crime and the number of crimes committed by refugee adults to increase by around 125% (0.022/0.018 and 0.026/0.021) in the first year after residency. This increase is entirely driven by property crimes, particularly shoplifting in supermarkets. There are no significant effects on violent crime. The similarity of estimates in columns 1 and 2 suggests that the crime increase is driven by an extensive margin response where individuals who otherwise would not have committed a crime, now do so (as opposed to an intensive margin response where offenders commit more crime). The effects are strongest for females (columns 5-6) whose number of criminal convictions almost triples, an effect almost exclusively driven by supermarket shoplifting (columns 4-8 report effects on males' crime and columns 9-12 report effects on females' crime).

Thus, our analysis implies that cutting benefits to or below a subsistence-minimum lead to more property crime, even for population groups with low baseline crime levels, such as adult females. To investigate further whether the repeal of the reform resulted in analogous reductions in crime committed in the first years after residency, we replicate the analysis from Section 4.5 in Figure A.10 for females, the most responsive group to the transfer cut in 2002. While the same caveat in terms of identification applies here as in Section 4.5, it is striking how females' response to the repeal closely mirrors the effects seen after the introduction of the Start Aid reform: increasing transfers to pre-reform levels lowers overall crime, an effect that is mainly driven by a reduction of property crime, namely shoplifting in supermarkets.

[42] We focus on adults aged 18-45 at residency with children (70% of the main sample) because crime rates for older individuals are close to zero and the largest benefit cuts were experienced for families with children. Table A.18 show that estimates are robust to in/exclusion of control variables, and alternative specifications such as a donut specification or narrower bandwidth. Figures A.10C-F present (by crime type) the average number of crime convictions in year 1 by timing of residency relative to the reform.

Table 8. Effects of reform on crime for adults, year 1 and 5 after residency, by gender.

	All adults		**Males**		**Females**	
	(1)	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
	P(crime)	*# crimes*	*P(crime)*	*# crimes*	*P(crime)*	*# crimes*
			Year 1			
A) All crime						
Reform effect	0.022	0.026	0.015	0.023	0.026	0.029
	(0.010)	(0.011)	(0.015)	(0.017)	(0.013)	(0.014)
Pre-reform mean	0.018	0.021	0.024	0.027	0.013	0.016
B) Property						
Reform effect	0.022	0.027	0.012	0.019	0.026	0.030
	(0.011)	(0.012)	(0.016)	(0.019)	(0.013)	(0.014)
Pre-reform mean	0.016	0.019	0.020	0.023	0.013	0.016
C) Theft from supermarket						
Reform effect	0.020	0.023	0.011	0.011	0.023	0.028
	(0.008)	(0.009)	(0.009)	(0.009)	(0.011)	(0.011)
Pre-reform mean	0.010	0.011	0.009	0.009	0.011	0.013
D) Violence						
Reform effect	-0.000	-0.000	0.004	0.004	-	-
	(0.003)	(0.003)	(0.009)	(0.009)		
Pre-reform mean	0.002	0.002	0.004	0.004	0.000	0.000
			Year 1-5			
E) All crime						
Reform effect	0.035	0.054	0.049	0.092	0.029	0.035
	(0.013)	(0.019)	(0.026)	(0.040)	(0.017)	(0.027)
Pre-reform mean	0.072	0.094	0.089	0.116	0.058	0.078
F) Property						
Reform effect	0.033	0.052	0.040	0.071	0.026	0.042
	(0.014)	(0.020)	(0.030)	(0.047)	(0.018)	(0.026)
Pre-reform mean	0.058	0.077	0.062	0.083	0.056	0.072
G) Theft from supermarket						
Reform effect	0.024	0.038	0.019	0.034	0.025	0.040
	(0.013)	(0.016)	(0.019)	(0.028)	(0.016)	(0.021)
Pre-reform mean	0.037	0.049	0.032	0.042	0.042	0.054
H) Violence						
Reform effect	0.002	0.001	0.018	0.024	-0.010	-0.004
	(0.009)	(0.010)	(0.021)	(0.022)	(0.009)	(0.004)
Pre-reform mean	0.012	0.013	0.024	0.024	0.003	0.004
Observations	3,406	3,406	1,376	1,376	2,030	2,030

Note: The table shows reform effects on and pre-reform means of the probability of having received a crime conviction and the accumulated number of crime convictions for all adults and separately for males and females between 18-45 at the time of residency (as very few above age 45 commit crime) with children. The table shows results for all crimes, property crimes, shoplifting from supermarkets, and violence. All crimes consist of "property", "violent", and a residual "other crime" (the two former categories drive the main results - results for other crime is available upon request). "Theft from supermarket" is a subset of "property" crime. Standard errors are clustered by residency month. Observations (all adults below age 45 at residency with children): 3,406.

The strong increase in crime following the transfer reduction begs the question whether we observe differences in crime effects across local labor demand in a similar way as we did for employment. Columns 1-4 of Table A.19 show that, while point estimates for the reform effects on crime are indeed larger in low than in high demand areas, the differences are not significant. One reason may be that the immediate income reduction is almost the same irrespective of local labor demand: Almost all refugees rely heavily on income from transfers upon obtaining residency. This is supported by columns 5-13 in Table A.19 which show that the immediate increase in the probability of living with a low disposable income did not differ initially across areas with different local labor demand. It is only in the longer run that being allocated to a high demand municipality may serve as a stepping-stone away from poverty.

To compare the magnitude of the reform's effects on crime with its effects on labor supply, Panel B of Table A.9 presents the estimated elasticities of crime with respect to benefit levels in years 1 and 5 since residency and contrasts them to the elasticities of labor earnings with respect to benefit levels (see also Section 4.3). Results show that a 1% increase in benefit levels lowers crime by almost 150% in year 1 and 90% in year 1-5 on average (the estimated elasticities are -1.480 and 0.883, respectively). Comparing these estimates to those for labor earnings (the estimated elasticities are -0.701 and -0.323, respectively) illustrates that the reform produced a percentage change in crime that is at least as high (in absolute terms) as the percentage change in labor earnings. Thus, our results suggest that the adverse (unintended) responses following large welfare cuts can – in relative terms – be at least as large as the labor supply responses that were intended by policy makers.

VI. Discussion and Conclusion

The objective of the 2002 Start Aid reform was to *"ensure that refugees and immigrants living in Denmark are better integrated and find employment more quickly*" – an intention that it shares with reforms recently implemented or currently considered by other countries. Despite a large initial employment response driven by males, effects of the reform on labor supply disappeared after about 4-5 years. Moreover, the reform led to large and persistent female labor force exits, in part due to the allocation of a household's overall transfer payment to one partner only (in most cases the husband) for couples arriving on both sides of the reform, and in part because of means testing. Both essentially remove penalization of females for not participating in integration programs. The magnitude of the response of females foregoing future labor market opportunities for the sake of leisure or home production may be partly explained by refugee communities holding traditional views about gender roles and exhibiting strong preference for females conducting home activities rather than integration programs and job search.[43] This stresses that incentivizing labor force participation of refugees need to carefully address behavioral norms in target populations.

Our analysis reveals a striking impact of local labor demand conditions on the reform's effect, which we can assess due to a random assignment policy for refugees concurrent with the reform implementation. We find that the short-lived reform effect on employment is mainly due to poorer job matches and less persistent employment relationships in low demand municipalities. In other words, the policy led many refugees to take up temporary and low-quality employment relationships in areas where demand conditions were unfavorable.

[43] Most refugees in Denmark migrate from Middle Eastern and North African countries where the female share of the formal labor force is between 10 and 20% (Moghadam, 2013) and gender norms are very different from those in Denmark.

Overall, these findings emphasize that allocation of refugees to areas with poor labor market conditions do not only impede future opportunities, but dramatically counteract intended reform incentives for employment and integration. Perhaps most concerning is the impact the reform had on the probability of living in poverty and the increase in subsistence type crime such as shoplifting in supermarkets. Our findings therefore have important implications for current discussions of welfare reforms aimed at groups similar to the one studied in this paper and are relevant not least for the political response to present and future refugee inflows.

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