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# Refugee Benefit Cuts†

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*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* I32, I38, J15, J22, J64, K42)

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 (SoA) (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 of 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

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[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 SoA and public benefits in Canada (Bryden 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 to 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).

the type of work that 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, Frattini, and Minale 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 percent compared to the previous 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 ten 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 the 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 prereform 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. Moreover, this disincentive for second earners was enhanced by a specific feature of the reform's implementation that implied that in some households, transfers

[3] See Brell, Dustmann, and Preston (2020) for evidence.

[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, Schultz-Nielsen, and Tranæs (2007) and Rosholm and Vejlin (2010).

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 that the household-level means testing did. This doubled the labor force exits of females, a sizable 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, Slemrod, and Giertz 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 that 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 percent and reduced public expenditures by 60 percent 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 percent. 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, McKinnish, and Sanders 2003) and to 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, Frattini, and Minale 2021).[6]

Overall, the Start Aid welfare reform lowered benefits to refugee immigrants by 40 percent, 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 prereform to almost 50 percent postreform. 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

[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, Gelbach, and Hoynes 2006 and Lemieux and Milligan 2008).

[6] Azlor, Damm, and Schultz-Nielsen (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 focuses 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.

to the few studies that associate crime with welfare payment timing (e.g., Foley 2011; Carr and Packham 2017), the welfare eligibility of youths (Deshpande and Mueller-Smith 2022) and criminal offenders (Yang 2017), and/or state variation in welfare reform implementation in the United States (Corman, Dave, and Reichman 2014).

## I. Background and Data

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

Denmark's 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, Schultz-Nielsen, and Tranæs 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 percent of the GDP and 3.4 percent 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 percent lower than SoA payments (rates are based on age and family type; the reform lowered transfer rates by 40 percent on average when we weight the pre/postreform changes by our sample composition; see online Appendix Table A.1).[8] The Start Aid program was in effect until January 1, 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, 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

[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 March 20, 2017). Online Appendix B.1 provides a more detailed description of the background and the Start Aid reform.

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

date.[9] The timing of residency around the reform thus provides a clean identification of the reform's effects (as detailed in Section III).

### 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 predetermined 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 IV 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.

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

[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).

[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).

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 IVC, we classify couples into three groups.[12] If both spouses received residency prereform, both are entitled to SoA. These couples constitute our reference category. If both received residency postreform, both are entitled to Start Aid. We refer to these as "Type A" couples. If one spouse received residency prereform and the other postreform, their combined benefits are capped at two times Start Aid, with the first-arriving spouse keeping the full SoA and the last-arriving spouse 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. online Appendix 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.

Figure 1 illustrates how labor earnings translate into pretax gross income when transfers are reduced due to the means test for prereform, 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 percent and 100 percent 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

[12] Married applicants are each assigned their own asylum case ID and are processed individually even if they apply together on the same date. In our sample, 18 percent of the married couples have the same application and approval dates, around 1 percent have the same application date but a different approval date, 15 percent have different application dates but the same approval date, and 67 percent 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 percent and largest for couples, with reductions ranging between 40 and 50 percent (see online Appendix Table A.1). Thus, when transfers were capped at two times Start Aid at the 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).

[14] The vertical difference between the solid (prereform couples) and dotted/dashed (Type A and B couples) lines in the intersections with the $y$-axis at zero labor earnings in online Appendix Figure 1A shows the monthly benefit reduction induced by the reform, with the slopes representing the means-testing rates. Because 91 percent of couples in our sample have children, we use a one-child family as a benchmark for couples' transfers. Online Appendix 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 online Appendix 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 percent and 82.1 percent for Type A and B couples, respectively. All income values reported in the paper are in 2010 purchasing power parity (PPP)–adjusted US dollars (\$1 = 7.76 kr.).

[15] For prereform 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 postreform 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 prereform SoA discount, resulting in a monthly break-even point that is around \$500 lower than it is for Type A couples. The low-bracket marginal tax rate of 44 percent applies to those with labor earnings above the break-even point.

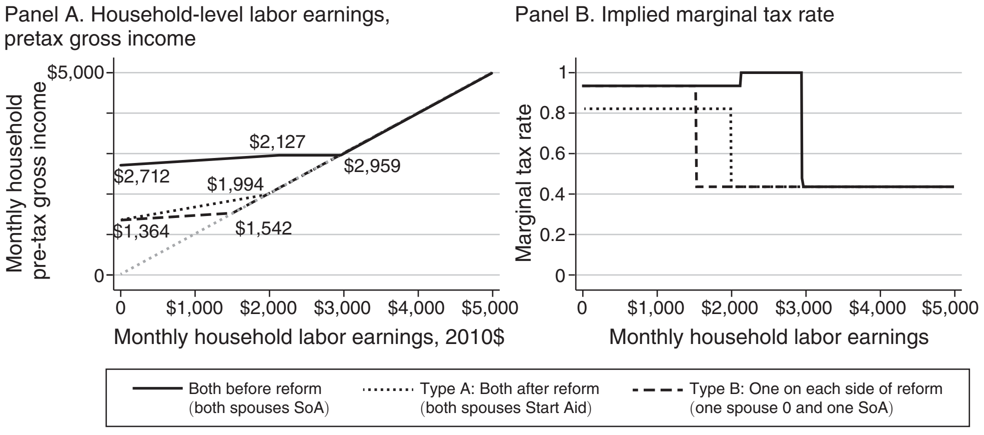


Figure 1. Labor Earnings, Pretax Gross Income, and Implied Marginal Tax Rates from Means Testing

*Notes:* Panel A shows the relationship between labor earnings (measured pretax) and pretax gross income due to means testing, by household types. The solid line shows prereform benefit schedules and the dashed lines postreform 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 pretax gross income at no labor income (intercept), and amounts noted away from the *y*-axis refer to monthly household labor earnings (on the *x*-axis). Panel B shows the corresponding marginal tax rates, calculated as $(1 - slope) + t \times slope$, where $t$ is the 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.

is obliged, in both 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]

## D. *Data and Samples*

Our sample consists of refugees whose treatment status (pre- or postreform) 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

[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.

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, 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, Kosovo was reclassified as a "safe zone" by Danish courts in the spring of 2002 (Refugee Appeals Board 2002, 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 remigrate within nine 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 IVA).[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 percent with 2 prereform residencies, 13 percent with 2 postreform residencies, and 30 percent 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 onward (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 prereform

[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, Larsen, and Olme 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 percent 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.

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 that refugees can perform.[21] Online Appendix B.2 details the construction of the two measures and provides descriptives. As we show in Section IV, 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 is 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 pretax, where those who have no earnings are set to zero), transfer income (measured pretax), pretax gross income (which equals labor earnings plus transfer income), and posttax disposable income (which equals pretax 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 IVG also reports effects on employment until ten 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 include 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 online Appendix B.2.

[21] Municipal average employment rates of non-Western immigrants are also used by Azlor, Damm, and Schultz-Nielsen (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.

TABLE 1—SAMPLE MEANS OF COVARIATES AND BALANCING TESTS, BASE SAMPLE AGED 18–55 AT RESIDENCY

| | Panel A. Sample means | | | Panel B. Balancing tests | |
|---|---|---|---|---|---|
| | All | Prereform residency | Postreform residency | Conditional test | Unconditional test |
| | (1) | (2) | (3) | (4) | (5) |
| 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) |
| Number 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 |

*Notes:* 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 postreform 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 postreform on all covariates and the running variable (allowing for different slopes in the running variable on each side of the cutoff). Online Appendix 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 postreform conditional on the running variable (allowing for different slopes in the running variable on each side of the cutoff). Online Appendix Table A.3 extends these results for alternative sample definitions. "Number of children" refers to the number of children upon residency. "Muslim countries" refer to refugeees from majority-Muslim countries, not to 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).

## 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 postreform 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 postreform is smaller. Of the refugees in the base sample, 84 percent are immigrants from predominantly Muslim countries (around half of Iraqi origin). Residency based on refugee status is granted to 62 percent 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 postreform 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

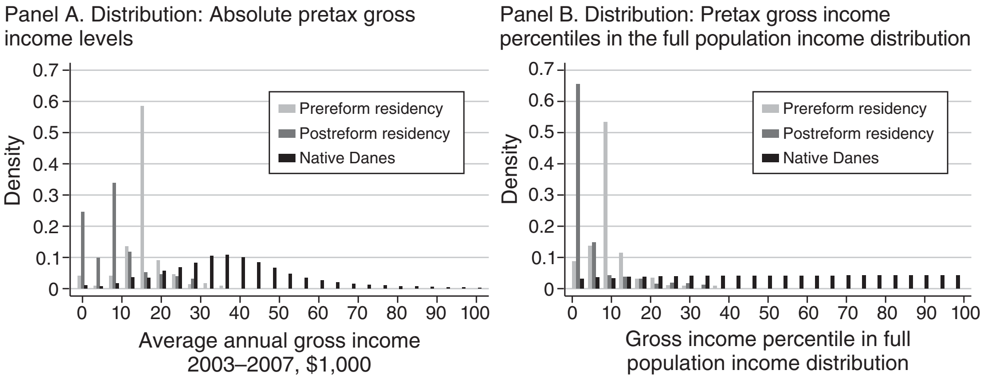


Figure 2. Pretax Gross Income Distributions for Refugees, Pre- and Postreform Residency, and Native Danes

*Notes:* The figure shows the pretax 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 of adult native Danes (age 30 or above) for comparison. The pretax gross income distributions are measured from 2003 to 2007. Panel A presents the distribution of pretax gross income levels, and panel B presents, for each of the three groups, the distribution of pretax gross income percentiles (in the full population income distribution).

balanced (Table 1, panel B), with no discontinuities in covariates around the reform timing. We will return to this point in Section IVA.

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

## 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:

$$y_i = \alpha + \beta \times reform_i + g(Z_i)'\pi + \mathbf{X}_i'\gamma + \varepsilon_i, \tag{1}$$

where $y_i$ is an outcome for individual $i$ measured $\tau$ 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 $\mathbf{X}_i$ collects observable characteristics, and $\varepsilon_i$ is an idiosyncratic error term. The parameter of interest is $\beta$. 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 postreform treatment categories (see Section IIC), 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 themself and their spouse. We define three states: (i) both spouses receive prereform residency and qualify for full SoA (baseline), (ii) both spouses receive postreform residency and qualify for Start Aid (Type A), and (iii) the two spouses receive residency on either side of the reform, with the prereform resident keeping full SoA while benefits are capped at two times Start Aid, which effectively makes the postreform 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

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

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 postreform for each spouse. The parameters $\beta_1$ and $\beta_2$ 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 $\beta_1$ and $\beta_2$ (and $\alpha$, $\pi_1$, and $\pi_2$) 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 prereform local labor demand indicators and estimate

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

The parameter $\alpha_g$ captures the prereform levels in group $g$, $\beta_g$ measures the reform effect for group $g$, and $f(Z_i)'\pi_g$ allows for different pre- and postreform slopes in the running variable across municipality groups.[24] In most cases, we group

[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 postreform, 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 postreform trends around the reform in the balancing tests. Moreover, we use "month" as the running variable as, for administrative reasons, refugees typically receive their residency decision on the first of a given month and are allocated to a municipality at the same time.

[23] See Card, Chetty, and Weber (2007a) for a further discussion of identification with double discontinuity.

[24] When estimating equation (1), we cluster standard errors by the running variable. When estimating equations (2) and (3), we use the two-way clustering method proposed in Cameron, Gelbach, and Miller (2011). For equation

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 postresidency for each value of the running variable, as predicted from an OLS regression using the covariates from Table 1 (cf. Card, Chetty, and Weber 2007a). The pre- and postreform 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 postreform 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 online Appendix 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 online Appendix 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 nine 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 2 of the 44 individual balancing tests performed is the estimated parameter significant at the 10 percent level. While the aforementioned results—particularly the ones for waiting times in refugee camps—illustrate that caseworkers have not responded to the reform by granting more residencies just prior to the implementation of Start Aid, we perform McCrary (2008) tests of differences in the running variable density (residencies per month) around the reform date, varying the bandwidth selection from 10 percent to 150 percent of the optimal bandwidth to confirm robustness. None of the specifications reveal structural breaks (online Appendix Table A.4).[25]

(2), we cluster by the running variable and household, and for equation (3), we cluster by the running variable and allocation municipality.

[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, Jensen, and Kleven (2020). Also, the absence of any changes

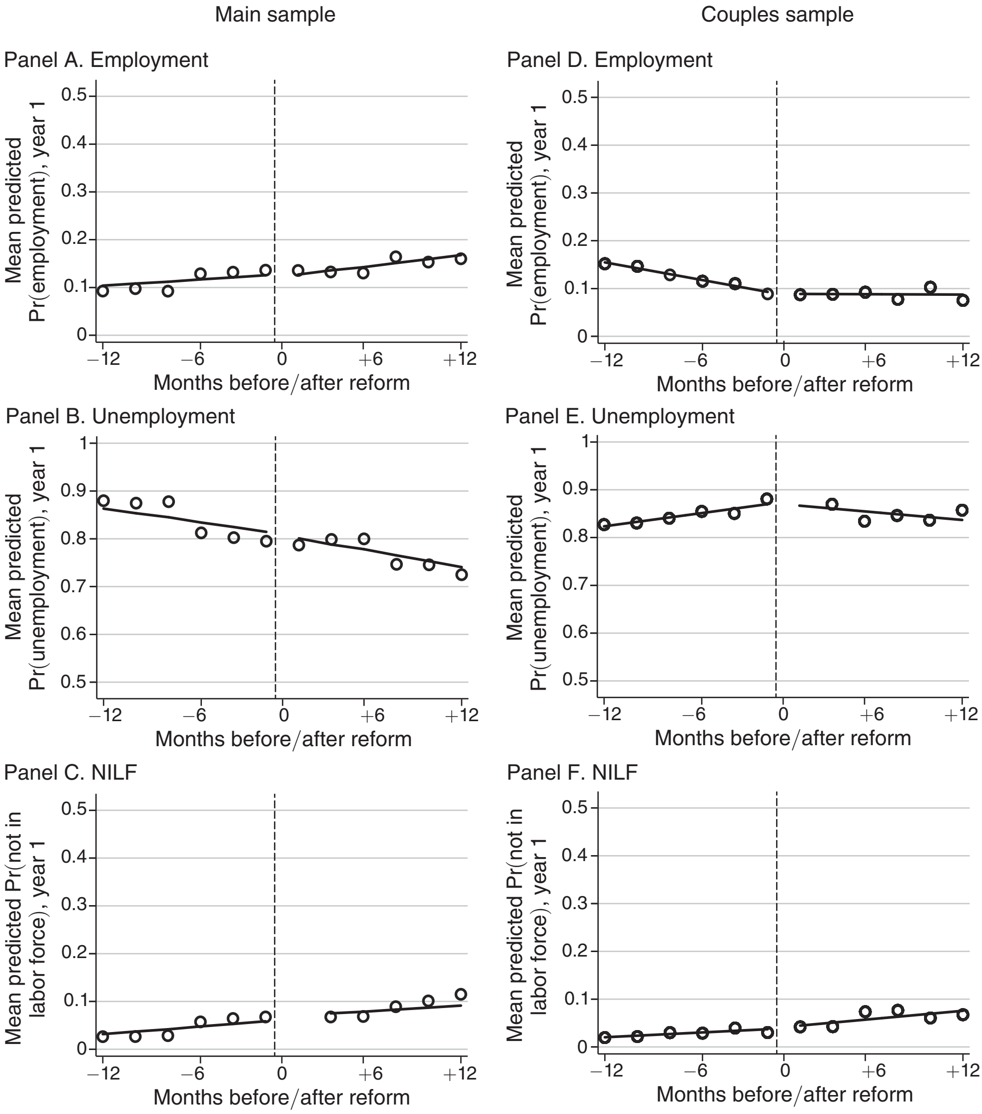


Figure 3. Labor Market Outcomes One Year after Residency, Predicted from Background Characteristics Alone

*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.

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

in sample characteristics and density around the reform verifies that caseworkers did not manipulate cases to place certain families pre- or postreform.

to local labor demand. To test this, we regress the average non-Western employment rates and the observed and predicted job openings in low-/unskilled work (described in Section IID) on the characteristics of the refugees in Table 1. There is no sign of selective allocation for any of the indicators (online Appendix 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 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 the 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 (online Appendix 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, pretax gross income, and posttax 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 prereform, only 10 percent of pretax gross income in the first year after residency comes from labor earnings (about \$1,900), with the remaining 90 percent coming from transfers (about \$20,500). The figure also reveals that although labor earnings increase in response to the reform, pretax gross income drops to almost half the prereform level, and average posttax disposable income falls by around 40 percent to (or below) Denmark's estimated subsistence minimum (which is around \$8,800; see Hansen 2002).

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

[26] As shown in online Appendix 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.

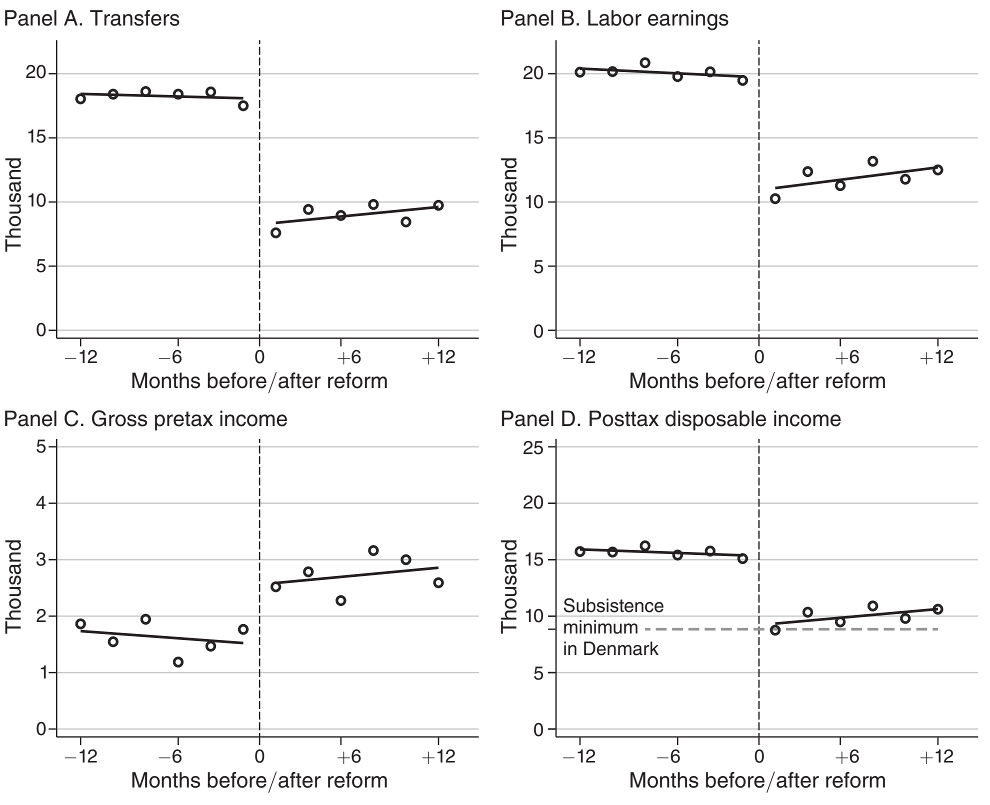


Figure 4. Individual Income, the First Full Year after Residency

*Notes:* The figure shows individual level transfer income (panel A), labor earnings (panel B), pretax gross income (panel C), and posttax disposable income (panel D) by bimonthly bins of residency timing. The dashed vertical line indicates the timing of the reform in July 2002. The horizontal line in panel 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 US poverty threshold (cf. US Census Bureau).

form of equation (1) and using levels instead of logs for income because of zeros in annual individual income measures). We also report the prereform 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 percent, 45 percent, and 30 percent 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.

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

Table 2—Effect of Reform on Subsequent Annual Individual Transfers, Labor Earnings (Both Measured in US$1,000s), Employment, Unemployment, and Fraction Not in the Labor Force

| | Transfer income (1) | Labor earnings (2) | Employment rate (3) | Unemployment rate (4) | Not in labor force (5) |
|---|---|---|---|---|---|
| Years since residency | Prereform mean | Prereform mean | Prereform mean | Prereform mean | Prereform 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 − years 3–5 | 7.875 | 0.123 | 1.677 | −1.943 | 0.246 |
| Observations | 4,843 | 4,843 | 4,843 | 4,843 | 4,843 |

*Notes:* 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, year 2, and the average of years 3–5 since residency. The table also shows prereform 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.

The reform effect on employment is nearly exclusively due to unskilled manual work (online Appendix Table A.7), and the effects are homogeneous over different education groups (online Appendix Table A.8), which is a first piece of evidence that either education accumulated in the home country is 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 V, 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 United States that shows that lower benefit levels may reduce the quality of jobs that refugees take. See also Rosholm, Scott, and Husted (2006) and Fasani, Frattini, and Minale (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 prereform refugees have higher unemployment rates (and, thus, higher participation rates in integration courses to be eligible for SoA, which we also show in column 4 of online Appendix Table A.6) in the first years after residency, they could potentially acquire more language skills than postreform refugees. This in turn may contribute to the longer-run fade out in employment effects and lower job quality in low-demand areas.

Table 3—Effect of Reform on Subsequent Labor Market Outcomes, by Gender

| | Males | | | Females | | |
|---|---|---|---|---|---|---|
| Years since residency | Employment (1) | Unemployment (2) | Not in the labor force (3) | Employment (4) | Unemployment (5) | Not in the labor force (6) |
| 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–years 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 |

*Notes:* 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, year 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.

Table 2 shows further that the reform lowered unemployment by around 16, 16, and 10 percentage points in years 1, 2, and 3–5, respectively (column 4), a decrease far larger than the increase in employment. The difference is explained by a dramatic increase in 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 underscore the importance (amply stressed by Bratberg and Vaage 2000; Card, Chetty, and Weber 2007b; and Kyyrä and Ollikainen 2008) of distinguishing between welfare benefits' effects on unemployment and those on employment and total nonemployment.

### 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 allo-

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

cated 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 II). 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 prerequisite 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 percent of our adult sample) relative to baseline couples in panels A and B, respectively, and postreform singles relative to prereform 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 prereform 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 postreform than prereform, 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, 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

[29] We illustrate the intuition underlying the different incentives for Type A and B couples in a simple static labor supply framework in online Appendix 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 spouse, 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 the 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 versus Type B, and Type B versus 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).

Table 4—Effect of Reform on Subsequent Labor Market Outcomes, by Gender and Household Type

| Year | Employment (1) | Unemployment (2) | Not in the labor force (3) | Employment (4) | Unemployment (5) | Not in the labor force (6) |
|---|---|---|---|---|---|---|
| | Males | | | Females | | |
| *Panel A. Type A couples, both granted residency after reform* | | | | | | |
| 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) |
| | Males | | | Females | | |
| *Panel B. Type B couples, one granted residency after reform* | | | | | | |
| 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) |
| | Males | | | Females | | |
| *Panel C. Singles* | | | | | | |
| 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) |

| | Both spouses in employment (1) | Only one spouse in employment (2) | Female dropout when male employment (3) |
|---|---|---|---|
| *Panel D. Separating employment effects and females labor force dropouts by dual versus single earners* | | | |
| 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 prereform couples) and singles (relative to prereform 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 first outcome is a dummy equal to 1 if both spouses were employed, the second a dummy equal to 1 if only one spouse was employed, and the third a dummy equal to 1 if the female was not in the labor force and her husband was employed. Panels A, B, D are estimated by equation (2), and panel C is estimated by equation (1). Standard errors (in parentheses) are clustered on two-way level by residency month and household for couples and by residency month for singles.

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 6 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 versus 0.38 (and insignificant) for Type B couples in year 1; see online Appendix 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 1, 2000, (i.e., two years before the actual reform) around an 18-month bandwidth, which are all very close to zero and insignificant (see online Appendix 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 (online Appendix Table A.11). All these estimates are similar to those reported above. Third, we present the estimated effects of the reform on employment from years 1–10 after residency across different bandwidth choices for the main estimation sample (online Appendix Figure A.6) and for year 1 including refugees from both Afghanistan and Yugoslavia (online Appendix Figure A.7). Point estimates are remarkably stable across specifications. Fourth, online Appendix 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, online Appendix Table A.13 presents the estimated differences in employment rates of labor migrants—who are ineligible for SoA 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

[31] The asymmetric allocation of transfers also has implications for reservation wages, with the highest wage rates needed to induce labor supply in the prereform 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 (online Appendix Figure A.4).

[32] This finding also supports 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- versus last-arriving spouse or whether social norms and labor-market-related gender roles also play a role. Females receive residency last in 86 percent 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 zero for those who received residency in the months leading up to the reform and 10–15 percent 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 five months before to five months after the actual reform. Regardless of whether we use transfer income (online Appendix Figure A.5A: full sample and by gender) or employment, unemployment, and NILF as outcomes (online Appendix Figure A.5B), the $t$-values from the estimated $\beta$'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 four 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).

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 online Appendix 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). Online Appendix Table A.14 compares all the main findings in the paper with estimates where the pre- and postreform 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 21, 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. Online 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, respectively) employment in years 1 and 2 after receiving residency. Overall, prerepeal estimates are all insignificant and close to zero, with the exception of females, where borderline significant results in year $-2$ 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 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 IVC) 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 ten years postresidency. Although overall labor supply effects are initially considerable in magnitude—close to 15 percentage points on average (Figure 6, panel A)—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 couples, and Type B couples, Figure 6, panel B shows that employment responses for Type B couples

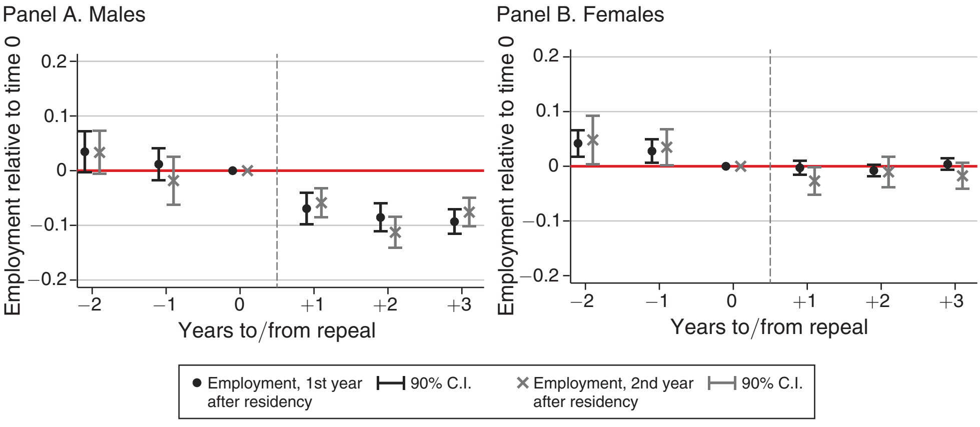


Figure 5. Employment Differences in Years 1 and 2 after Residency around the Repeal

*Notes:* The figure shows estimated differences in employment and 90 percent confidence intervals in the first and second 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 first year after residency, the prerepeal (control) years include those receiving residency in 2009–2011 (−2 to 0 on the *x*-axis in the figure), and the postrepeal (treatment) years include those receiving residency in 2012–2014 (1 to 3 on the *x*-axis in the figure). When measuring employment in the second year after the repeal, the prerepeal (control) years include those receiving residency in 2008–2010 (−2 to 0 on *x*-axis in the figure), and the postrepeal (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.

disappear after the first two years, while those for Type A couples and singles are more persistent but also disappear about five to six years after the reform (while year 1–2 estimates are significantly different from estimates in years 5–10 for the full sample in Figure 6, panel A, 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 6, panel B).

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

[34] Online Appendix 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.

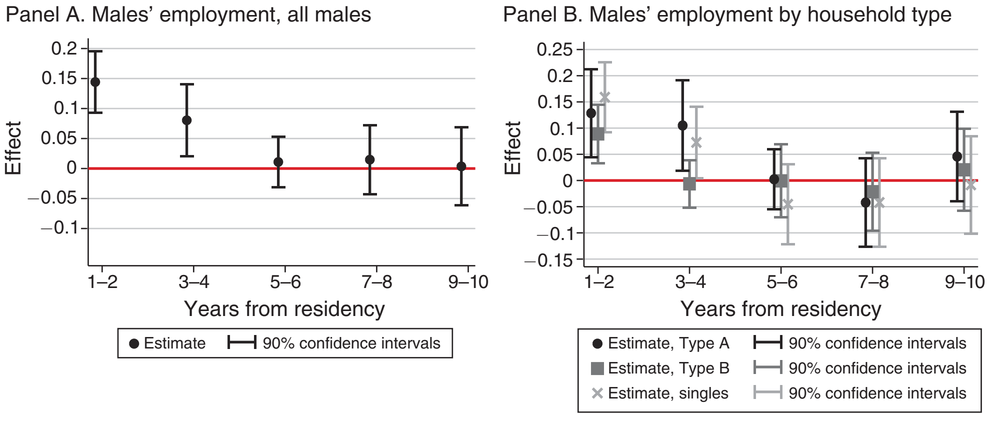


Figure 6. Effect of Reform on Males' Employment Rates, One to Ten Years after Residency

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

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 online Appendix Table A.7 (discussed in Section IVB), 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 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 exogenous 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 II, 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.

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

Table 5—Effect of the Reform on Males' Employment and Job Type by Assignment Municipality

| | Year 1 | Year 2 | Years 3–5 |
|---|---|---|---|
| *Panel 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) |
| Prereform mean | *0.164* | *0.298* | *0.465* |
| Low demand, reform effect | 0.125 | 0.042 | −0.032 |
| | (0.078) | (0.055) | (0.026) |
| Prereform 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) |
| *Panel 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) |
| Prereform mean | *0.178* | *0.306* | *0.475* |
| Low demand, reform effect | 0.157 | 0.068 | −0.013 |
| | (0.071) | (0.051) | (0.030) |
| Prereform 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) |
| *Panel C. Decomposing high–low difference by inflow and stay in employment* | | | |
| Inflow from nonemployment | 0.059 | 0.132 | −0.004 |
| | (0.072) | (0.063) | (0.031) |
| Stay in employment | – | 0.033 | 0.133 |
| | | (0.073) | (0.054) |
| *Panel D. Decomposing high–low difference by job type* | | | |
| *Unskilled manual work* | | | |
| Inflow from nonemployment | 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 nonemployment | 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 |

*Notes:* 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 above/below the median of the predicted ratio of the number of job openings in low-/unskilled work relative to the number of unemployed individuals, and defined in panel B as being assigned to a municipality with an 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.

### 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 prereform 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 0, and point estimates decrease from around 13 percentage points in year 1 to 4 percentage points in year 2 and to around 0 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 postreform residency has on average spent 0.61 $(= 0.059 + 0.165 + 0.129 \times 3)$ years more in employment (which amounts to 33 percent of the average prereform 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 types of jobs that 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

[36] Online Appendix Table A.15 shows the corresponding results for the full sample. Online Appendix 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.

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

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 percent are accounted for by job changes from an unskilled job to a job requiring some skills, and 75 percent by continuation in jobs requiring some skills.

Overall, therefore, the reform not only had substantially higher and far more persistent employment effects in high-demand municipalities compared to low-demand municipalities, 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. Online Appendix Table A.17 shows the estimated effect of the reform on labor earnings levels and the labor earnings distribution in years 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 7, panel A displays the reform effects the first ten years after residency for males separately for high- and low-demand municipalities as well as the differences between the two. After three to four 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 percent level.

Figure 7, panel B shows that the reform affects the probability that refugees continue in employment from one year to the next positively throughout the first ten 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 fifth, fiftieth, and ninetieth percentiles of local labor demand over the first five years after residency on employment, labor earnings, and public expenditures.[39]

[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 (in a job requiring either some skills or unskilled manual work) in year $t$ from nonemployment in year $t-1$ plus the fraction that continues in employment from year $t-1$ to year $t$ (with employment in year $t$ in a job requiring either some skills or unskilled manual work).

[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 fifth, fiftieth, and ninetieth percentiles as center of the kernels, respectively. Prereform rows show the estimated

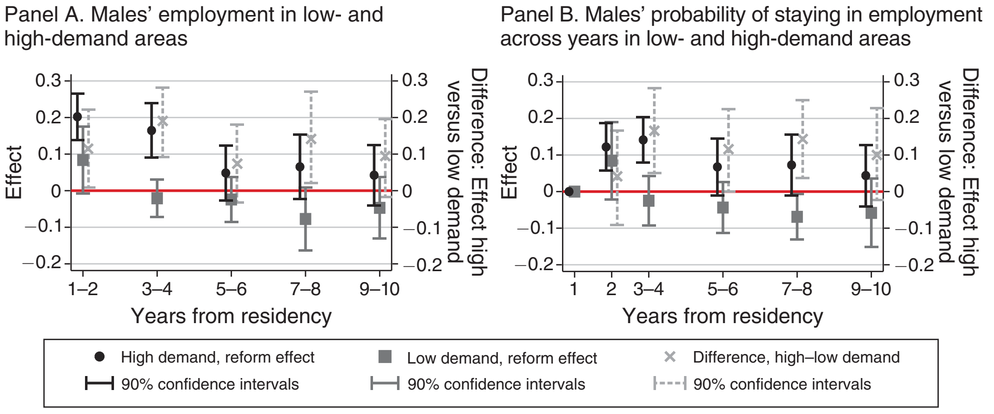


Figure 7. Effect of Reform on Males' Employment Rates, One to Ten Years after Residency, by Local Labor Demand

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

Following the reform, employment and labor earnings increased by 15–20 percent on average over the first 5 years after residency (panels A and B). While there were no significant reform effects at the fifth percentile, the reform led to 30–35 percent higher employment and labor earnings at the ninetieth percentile of local labor demand. The *overall* difference in postreform refugees' labor earnings between municipalities at low and high levels of local demand conditions (columns 2 and 4) amounts to 70 percent (\$5,416 versus \$9,291), compared to a difference of only 15 percent (\$5,875 versus \$6,824) for prereform refugees.

On average, the reform resulted in a reduction of public expenditures per refugee by almost 50 percent, 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 percent relative to the prereform mean in municipalities with the lowest labor demand, but of around 60 percent 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 suboptimal allocation policies.[41] Moreover, while

constant term ($\alpha$, cf. equation (1)). Postreform rows show the estimated constant plus the reform effect ($\alpha + \beta$, cf. equation (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 V.

[41] One concern is that unequal allocation may lead to unwanted political responses of majority populations. However, Dustmann, Vasiljeva, and Damm (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 2021).

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 | Full sample (1) | 5th (2) | 50th (3) | 95th (4) | Difference 95th − 5th (5) |
|---|---|---|---|---|---|
| *Panel A. Employment* | | | | | |
| Prereform | 0.252 | 0.234 | 0.244 | 0.275 | 0.041 |
| Postreform | 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) |
| *Panel B. Labor earnings, $1,000* | | | | | |
| Prereform | 6.258 | 5.875 | 6.065 | 6.824 | 0.949 |
| Postreform | 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) |
| *Panel C. Public expenditures, $1,000* | | | | | |
| Prereform | 11.190 | 11.440 | 11.341 | 10.841 | −0.599 |
| Postreform | 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) |
| *Panel D. Disposable income, $1,000* | | | | | |
| Prereform | 17.819 | 17.633 | 17.756 | 18.077 | 0.444 |
| Postreform | 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) |

*Notes:* 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) in years 1–5. Panel D: effects on average posttax disposable income (pretax 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 equation (1). Columns 2–4 present results from three regressions for each outcome where we estimate equation (1) weighting observations by kernels with centers at the fifth, fiftieth, and ninety-fifth percentiles of local labor demand, respectively, using an Epanechnikov weighting kernel. Hence, the estimates reflect the reform effects in municipalities close to the fifth, fiftieth, and ninety-fifth percentiles. Column 5 presents the difference between rows 4 and 2. Standard errors are clustered on two-way level by residency month and allocation municipality, except in column 1 where standard errors are clustered by residency month. Observations: 4,843.

average prereform disposable income was more than 35 percent higher than average postreform disposable income in low-demand areas, the reform only resulted in a disposable income reduction of 25 percent 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 posttax disposable income corresponding to less than $500 per month (slightly above the US census's deep-poverty threshold, panel A), less than $750 per month (slightly below the US census's 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

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 | Years 3–5 | $t$-value: year 1 − years 3–5 |
|---|---|---|---|---|
| *Panel A. Disposable income < \$500* | | | | |
| Prereform | 0.027 | 0.024 | 0.021 | |
| Postreform | 0.311 | 0.237 | 0.115 | |
| Reform effect | 0.284 | 0.213 | 0.094 | 5.554 |
| | (0.027) | (0.024) | (0.021) | |
| *Panel B. Disposable income < \$750* | | | | |
| Prereform | 0.087 | 0.071 | 0.047 | |
| Postreform | 0.557 | 0.356 | 0.200 | |
| Reform effect | 0.470 | 0.285 | 0.153 | 9.307 |
| | (0.026) | (0.025) | (0.022) | |
| *Panel C. Disposable income < \$1,000* | | | | |
| Prereform | 0.254 | 0.188 | 0.121 | |
| Postreform | 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 | |

*Notes:* 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 posttax 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 years 3–5 after residency. The column "$t$-value: year 1 − years 3–5" shows the $t$-value from a test of difference between the year 1 and years 3–5 estimates. Standard errors are clustered by residency month.

that the reform led to an increase of between 30–50 percentage points (depending on cutoff) in the probability of experiencing very low posttax disposable incomes. For example, the probability of having less than \$750 per month in the first year after residency increases from 9 percent to more than 50 percent (online Appendix Figures A.9A and A9.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 (2022) 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

[42] We focus on adults aged 18–45 at residency with children (70 percent of the main sample) because crime rates for older individuals are close to 0 and the largest benefit cuts were experienced for families with children. Online Appendix Table A.18 shows that estimates are robust to in/exclusion of control variables and alternative specifications such as a donut specification or narrower bandwidth. Online Appendix 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, Years 1 and 5 after Residency, by Gender

| | All adults | | Males | | Females | |
|---|---|---|---|---|---|---|
| | Pr(crime) (1) | # crimes (2) | Pr(crime) (3) | # crimes (4) | Pr(crime) (5) | # crimes (6) |
| | Year 1 | | | | | |
| *Panel 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) |
| Prereform mean | 0.018 | 0.021 | 0.024 | 0.027 | 0.013 | 0.016 |
| *Panel 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) |
| Prereform mean | 0.016 | 0.019 | 0.020 | 0.023 | 0.013 | 0.016 |
| *Panel 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) |
| Prereform mean | 0.010 | 0.011 | 0.009 | 0.009 | 0.011 | 0.013 |
| *Panel D. Violence* | | | | | | |
| Reform effect | −0.000 | −0.000 | 0.004 | 0.004 | – | – |
| | (0.003) | (0.003) | (0.009) | (0.009) | | |
| Prereform mean | 0.002 | 0.002 | 0.004 | 0.004 | 0.000 | 0.000 |
| | Years 1–5 | | | | | |
| *Panel 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) |
| Prereform mean | 0.072 | 0.094 | 0.089 | 0.116 | 0.058 | 0.078 |
| *Panel 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) |
| Prereform mean | 0.058 | 0.077 | 0.062 | 0.083 | 0.056 | 0.072 |
| *Panel 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) |
| Prereform mean | 0.037 | 0.049 | 0.032 | 0.042 | 0.042 | 0.054 |
| *Panel 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) |
| Prereform 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 |

*Notes:* The table shows reform effects on and prereform 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 crimes) 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 are 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.

by around 125 percent (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 3–4 report effects on males' 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 IVE in online Appendix 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 it does in Section IVE, 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 prereform 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 of whether we observe differences in crime effects across local labor demand in a similar way as we did for employment. Columns 1–4 of online Appendix 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 online Appendix Table A.19, which show that the immediate increase in the probability of living with 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 online Appendix 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 IVC). Results show that a 1 percent increase in benefit levels lowers crime by almost 150 percent in year 1 and 90 percent in years 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 policymakers.

## 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 four to five 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 the penalization of females for not participating in integration programs. The magnitude of the response of females forgoing 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 the labor force participation of refugees needs 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 the allocation of refugees to areas with poor labor market conditions not only impedes future opportunities but dramatically counteracts 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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[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 percent (Moghadam 2013) and gender norms are very different from those in Denmark.

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