Lowering Welfare Benefits: Intended and Unintended Consequences for Migrants and their Families

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Abstract: In this paper we study intended and unintended effects of a large welfare reform in Denmark that targeted refugee immigrants. The reform, referred to as "Start Aid", was proposed in March, approved by June, and enacted on July 1st of 2002. Individuals who were granted residency up until July 1st were eligible for the same Social Assistance levels as native Danes, while those who were granted residency after July 1st received up to 50 percent lower *Start Aid* levels. We first investigate the primary effect of the reform. We find that for males the reform resulted in an approximately 100% increase in labor earnings and employment, driven solely by an extensive margin response with employment in unskilled jobs. We also show that the reform led to a dramatic drop in disposable income and induced disincentives partly through means-testing, and partly through a particular design feature of the reform, leading married females to withdraw from the labor force and welfare system while also attenuating males' employment response to the lower benefit level. Studying the secondary consequence of the reform, we find that the reduction in welfare benefits led to a 7-fold increase of women being caught stealing groceries in supermarkets, whereas men are more likely to commit violent crimes. We also show that children who received residency during the preschool age were 25% less likely to be enrolled in Danish daycares and preschools and 5%-points more likely to end up in the poorest performing group in language test scores, and that boys completed less education. Youths were also more likely to commit both property and violent crime because of the reform.

Keywords: Social assistance, labor market outcomes, migration.

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1 Introduction

As direct consequences of the great recession and large increases in international migration, countries are increasingly worried about abuse of their welfare- and transfer systems. Many have implemented reforms that reduce social transfer levels generally or for particular populations.[1] The primary aim with these policies is to incentivize individuals to get off welfare, find employment, and be self-sufficient. Yet, reforms of public transfers are often double-edged. One the one hand, the potential employment effects are large as means-tested public transfers effectively increase the marginal tax on labor, provide strong participation margin disincentives, and produce a possible 'poverty trap'. On the other hand, there may be other barriers to employment than welfare-dependency per se, causing reforms to reduce household income of already low-income families. This trade-off between balancing efficiency and equality is one of welfare states' main challenges. Understanding the mechanisms that induce or impede labor supply and whether the policies have derived effects in other dimensions is therefore central.

This paper investigates these questions by studying both primary and secondary effects of a large welfare reform in Denmark targeting refugee immigrants. This reform was motivated by the high welfare uptake and economic inactivity of this group (Bratsberg et al., 2010; Pedersen, 2010). Denmark has one of Europe's highest levels of Social Assistance (SA), with means-testing producing an implied marginal tax rate on labor earnings of almost 100%, thereby providing a textbook example of a Negative Income Tax (NIT) with large extensive margin disincentives. The reform, labeled "Start Aid", reduced SA levels of refugees, whose residency in Denmark was granted after the reform date, by up to 50%, making it one of Europe's lowest levels of welfare-transfers (Schultz-Nielsen and Hansen, 2015). It was proposed in March, approved by June, and enacted on July 1st of 2002. The very short implementation period and the lengthy process of deciding refugees' asylum applications means that asylum seekers who were granted asylum around the cut-off date July 1st were most likely already in Denmark before the reform was proposed and approved. As individuals who were granted residency just before that date were eligible for the same SA levels as native

[1] Since 2002, EU27 countries have passed 126 bills tightening welfare levels/eligibility of immigrants and/or refugees.

Danes, while those who were granted residency after July 1st received up to 50 percent lower *Start Aid* levels, this provides a large discontinuity that we exploit for our analysis.

The reform led to dramatic drops in transfers for a population with very high non-employment rates in an attempt to boost labor supply incentives. We indeed find that average labor earnings and employment rates almost doubled the first year after residency to Denmark as a response to the lower benefits. We further find that increased labor earnings and labor supply was solely extensive margin responses. Effects were driven by males who increased employment by 16%-points; an almost 100% increase relative to the pre-reform employment rate of 17%. Thus, at first glance we largely confirm the intended effects of the reform. However, there were substantial impediments lowering this response through three channels: means-testing, demand side constraints, and the reform distorting incentives within specific households.

First, because of means-testing from married males increasing employment by 10%-points, the reform effectively reduced females' labor force participation (and participation in integration programs as language courses) by 10%-points thus potentially hindering their integration. Also, by reducing transfer levels the break-even point where $1 earned increases income by $1 dropped considerably. The entire labor earnings response of couples and most of singles' response increase from a level of zero to a level within the range where implied marginal tax rates changed, illustrating responsiveness to incentives. Yet, while the reform lowered benefits substantially, the 'poverty-traps' of means-testing remained; post-reform implied marginal tax rates were 82-93%, thus failing to improve participation margin incentives except via income effects.

Second, pre-reform SA levels, which still applied to the rest of the labor force, serve in unison with union minimum wages as wage floor in Denmark. While the reform made refugees accept lower wages compared to those with residency before the reform, the lower bound of wages received were unchanged even though incentives to accept wages at lower levels increased. This suggests a demand-side constraint to the skills supplied by refugee migrants, which is also illustrated by the positive employment effect being solely driven by employment in unskilled manual jobs irrespective of previous education and/or training.

Third, a feature of the reform determined that households where one partner received residency pre-reform and the other post-reform could at maximum receive benefits equivalent to two times Start Aid,

which was paid to the first arrived spouse (most often the husband). Therefore, only one had an obligation of job-search and training / language courses while the partner who received residency after the reform received no welfare payments at all and could drop out of these courses without any repercussion. This design more than doubled females' labor force exits relative to the simple means-testing effect thereby hindering integration further. It also made males less likely to take up work than they would otherwise have been. Allowing one spouse more leisure / household production without loss of income compared to the case where both spouses received Start Aid conditional on program participation reduced labor supply effects.

As the reform led to a dramatic drop in disposable income for the residual 60-70% that still relied on welfare benefits, we also study secondary consequences. For instance, monthly benefits to a single parent with one child dropped from around $1,400 to $700.[2] We find that the reform led to a 7-fold increase of females being caught stealing groceries in supermarkets (from a low base prevalence), while males committed more violent crime. We also investigate the consequences of the reform on children's lives. Children who received residency at age 0-5 were 25% less likely to be enrolled in daycares and preschools, and 5%-points more likely to end up among the poorest performing in language tests. Also, children in their teens at time of residency completed 8 months less education, an effect driven by boys, and youths of both genders were more likely to commit crime after the reform. Thus, the reform had harmful secondary effects on refugees, and in particular their children, which may more than outweigh the surplus and benefits achieved through lower transfer levels and labor supply responses.

This paper touches on several general questions investigated within labor economics. First, it speaks to the literature that analyzes labor supply and employment responses to means-tested transfers (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2002; Saez, 2002),[3] the nexus between extensive and intensive margin responses (Heckman, 1993; Blundell and MaCurdy, 1999) and welfare transfers to immigrants (see e.g. Borjas, 2002; Bratsberg et al., 2010; Rosholm and Vejlin, 2010). We contribute to this literature by showing that increasing incentives for a heavily marginalized group resulted in a large labor supply response

[2] 2010 PPP-adjusted USD, using OECD's individual consumption PPP-adjustment.
[3] See Moffitt (2002; 2015) for reviews of the literature on means-tested transfers. See Krueger and Meyer (2002) for an extensive review of the literature on social transfers as UI benefits more generally and Røed and Zang (2003), Mastrobuoni (2005); Lalive et al. (2006); Card, et al. (2007b); Lemieux and Milligan (2008) for more recent examples.

at the extensive margin. We also illustrate that there likely exist barriers relating to demand-side constraints possibly because refugees' productivity levels are below the general wage floor, and that employment responses were driven exclusively by uptake in unskilled manual work.

Second, our paper contributes to the literature that investigates how couples are affected by differences in the incentives they face. The specific design of the reform, where couples were treated differently depending on whether one or both received residency pre/post reform, provides us with a unique opportunity to investigate to what extent reductions in females' labor supply and transitions into non-employment, were due to effects induced by simple means-testing from males finding employment or disincentives to integrate by an opportunity to avoid program-participation without this affecting total household benefits. We add therefore to earlier studies of spillovers from Unemployment Insurance benefits as in Cullen and Gruber (2000), Earned Income Tax Credit as in Eissa and Hoynes (2004), and Disability Insurance as in Autor et al. (2017) in two dimensions. First by showing that the opportunity to avoid integration and labor market programs reduced females' labor force participation more than twice as much as means-testing alone while also dampening males' employment response. And second, by illustrating that spillovers within couples not only arise from incentives relating to household income, but also from preferences attached to for example the importance of household production vs. labor supply and integration. Such preferences may differ across subgroups and should be taken into account to avoid unintended consequences when designing policies. This finding is particularly important in view of the recent large refugee migrations into European countries.

Third, we add to the literature linking family income to children's outcomes (e.g., Dahl and Lochner, 2012; Duncan et al., 2011; Løken et al., 2012). While low cost universal childcare has been found to improve long run outcomes of children from low-resource families (e.g., Havnes and Mogstad, 2011), we find that lowering income may keep parents from making use of this, with serious consequences for e.g. children's language acquisition as children with post-reform residency were more than twice as likely to fall in the low tail of language test scores. We further find that lower family income sparked youth crime and resulted in fewer years of completed schooling for boys. We thereby provide insights into the link between family income and children's outcomes, in particular for marginalized families. Finally, we find that all

effects on children arise from the income reductions per se. We thus also contribute to the literature by separating effects of changed family income from parents' responses to changed incentives.

The paper unfolds as follows: Section 2 describes Denmark's refugee policy, Social Assistance and the Start Aid reform, and the data and sample. Section 3 details the identification strategy. Section 4 presents the results; first primary effects, then effects on the income distribution and disincentive effects. Section 5 presents secondary effects on crime and children's outcomes. Finally, Section 6 concludes.

2 Background and Data

2.1 Denmark's Refugee Policy

Most individuals who request asylum in Denmark do so after having entered Denmark as undocumented migrants. Individuals who request asylum in Denmark under the 1951 Geneva Convention for Refugees typically reside at an accommodation center (a refugee camp) while the application is being processed by the Danish Immigration Service. During the application process, living expenses, which include healthcare and child education, are covered by the Immigration Service.[4] The first step of the asylum application process is determining whether Denmark is responsible for processing the application. If this is the case the authorities next decide whether or not the applicant should be granted asylum. If the application is rejected, an appeal to the Danish Refugee Appeals Board is possible. The application process takes on average about 15 months with considerable variation in case processing times, and the processing of each case depends on applicants' individual circumstances and the workload of the immigration agency (some individual cases may take up to 4 years). Married couples who apply for asylum in Denmark each get their own asylum case ID and their applications are processed individually even if the spouses apply for asylum together on the same date. In some instances, if authorities grant residency to one spouse, they may grant residency to the other spouse simultaneously if they are certain that residency would have been granted at a later point. However, the majority of married couples are treated separately. In our sample, spouses have the same application date and

[4] In some cases, and following separate application processes, applicants may instead live with a family member (typically their spouse) who resides legally in Denmark or in a self-financed residence (e.g., a sublet). If the applicant is married to a person who resides legally in Denmark, the spouse is expected to cover the living expenses.

approval date in 26% of all cases for married couples, while 9% have different application dates but the same approval date, and 65% of couples neither share application date nor approval date.[5]

Once a refugee receives residency he or she is assigned to a municipality, which is then responsible for finding suitable accommodation and for offering an 'integration program' which adult refugees must attend, in order to be eligible for welfare benefits (Social Assistance or Start Aid). The integration program consists of language courses, a course in Danish society and culture, and a requirement to accept suitable employment if offered (i.e., activation). Over the period we consider, asylum applicants to Denmark came from a variety of countries, but mainly from Middle Eastern and North African countries.

2.2 Social Assistance and the Start Aid Program

In Denmark, Social Assistance (SA) constitutes the welfare benefit targeted at uninsured unemployed individuals and is among the most generous in the world (Nielsen and Pedersen, 2015). At the same time, Denmark has historically had one of the world's most liberal refugee immigration laws. Regulations were tightened from the late 1990's onwards, restricting for instance family reunification. Despite these restrictions, however, during the early 2000s Denmark granted asylum to more individuals per capita than, for example, Sweden, the U.K., and Germany. New requirements also aimed at a speeding up integration, such as mandatory language courses and courses in Danish society and culture, and a requirement to accept employment if offered. Yet, once being granted residency in Denmark, refugees had very low labor market attachment: The employment rate was approximately 5% after 5 months of residency, increasing to 15% after 15 months and 40% after 4.5 years (Andersen et al., 2012). The combination of low labor market attachment and high levels of welfare benefits led to net welfare transfers to non-Western immigrants to reach 0.83 % of GDP and 3.4 % of total public spending in 2001 (Matthiessen, 2009).

On March 1st 2002, the Danish government proposed a bill which reduced the SA levels that individuals who received residency in Denmark were entitled to.[6] The aim of the bill was to promote labor market integration of refugees, thereby paving the way for integration into the Danish society more broadly,

[5] Unmarried couples are processed as two single individuals and their case processing times are independent.
[6] This refers to unemployed individuals that were ineligible for unemployment insurance benefits, which all newly arrived immigrants are.

by increasing incentives substantially (Danish Prime Minister's Office, 2002).[7] In order not to violate the United Nation's Universal Declaration of Human Rights, the program (called "Start Aid") was formulated such that in principle it was applicable to all Danish residents. Start Aid was therefore introduced to *all* individuals who had not lived in Denmark in 7 of the past 8 years, but *effectively* only targeting immigrants. Approved on June 6th, the policy was set into effect on July 1st (Law no. 361). As a result, refugees who were granted residency before July 1st 2002 were entitled to the same level of SA payments as unemployed (and uninsured) Danish citizens, while refugees who were granted residency after the reform were only entitled to the new benefits scheme, Start Aid. Because the asylum application process takes on average 15 months (Hvidtfeldt et al., 2017), and implementation of the welfare reform took place just 4 weeks after ratification, the reform effectively led to a randomization of refugee migrants around July 1st, which provides us with a clean identification design. We explain this in more detail in Section 3.

Rates of SA or Start Aid are determined by the individual's age and family type, as shown in Table 1. Eligibility is evaluated monthly and payments are adjusted when individuals' eligibility criteria change. Start Aid transfers are substantially lower than SA transfers, with reductions ranging from 15 to 50 percent, depending on individual characteristics. Figure 1 shows Social Assistance levels for Germany, Great Britain, the Netherlands, Sweden, and Denmark, and contrasts them with the new Start Aid levels introduced after July 1st 2002. Levels are reported in 2010 PPP-adjusted USD (using OECD's individual consumption PPP-adjustment). The figure illustrates that, among the countries depicted, the introduction of Start Aid moved Denmark from being the country with the most generous benefit scheme to the country with the least generous scheme for those affected by the reform.[8] Comparing Start Aid to U.S. benefits levels, the reform brought Danish levels for refugees almost on par with the median US levels of TANF and SNAP.[9]

[7] "Refugees and immigrants who live in Denmark must be better integrated and find employment faster. The incentives for finding employment should therefore be strengthened." (own translation of official remarks to the bill proposal, available in Danish: http://webarkiv.ft.dk/Samling/20012/lovforslag_som_fremsat/L126.htm, accessed 03-20-2017).
[8] Here for a family of one or two adults and two children - the family type we depict in Figure 1. Andersen et al. (2012) show that refugees receiving Start Aid had deficits on almost all household budget types even when excluding expenses to transportation, communication, leisure activities, and savings for e.g., the replacement of durable consumer goods.
[9] Source: SNAP (Supplemental Nutrition Assistance Program) levels: http://kff.org/other/state-indicator/avg-monthly-food-stamp-benefits, accessed 12-22-2016. See Falk (2014) for Temporary Assistance for Needy Families (TANF) levels by states and years. Also, Start Aid levels fall below cash benefit levels in the states with the highest TANF rates.

Because refugees who were granted residency before July 1st of 2002 were entitled to SA but refugees who were granted residency after were entitled to Start Aid, couples were placed in one of three different states depending on the timing of their residencies. This is illustrated in Figure 2. If both spouses received residency before the reform, both were entitled to SA (Panel A). If both spouses received residency after the reform, both were entitled to Start Aid (Panel C). However, as the government wanted the reform to target as large a group of refugees as possible, it faced a challenge in ensuring that benefits to couples where one spouse received residency pre-reform and the other post-reform (Panel B) did not exceed that of couples where both received residency post-reform. The solution was to cap the last arrived spouse's benefits such that total household benefits did not exceed what two individuals on Start Aid would receive. As this equals (or almost equals) pre-reform SA, the last arriving spouse was effectively not entitled to any benefits.

2.2.2 Social Assistance, Start Aid, and Tax Rates

Both SA and Start Aid are means-tested such that any earnings from work or asset values reduce benefits. The means-testing is on household level so one spouse's earnings exceeding SA or Start Aid levels lead to reductions in the spouse's benefits too. Thus, both SA and Start Aid work as a household level Negative Income Tax (NIT). As noted by Saez (2002) and Immervoll et al. (2007), among others, NITs provide strong extensive margin disincentives and may result in a 'poverty-trap'. This is evident in our case from Figure 3 which shows how labor earnings translate into gross income and the implied marginal tax rates from means-testing. The intersection with the vertical axis shows the benefits reduction from the reform; the slopes represent the means-testing rates, which are close to 1:1 implying marginal tax rates of almost 100% from any positive earnings above zero up to a break-even point where $1 earned increases income by $1.

The solid line in Figure 3A shows how labor earnings map into gross income for singles prior to the reform. The dashed line in 3A shows the corresponding for singles after the reform. When an individual had labor earnings the SA or Start Aid was reduced accordingly. There was a discount to the reduction with a slightly lower means-testing rate post-reform. Pre-reform individuals had all SA means-tested by labor earnings except $1.54 per hour worked. Post-reform, the means-testing discount changed to $4.21 per hour. The discounts (for employment at the minimum wage) correspond to 11.6% (SA) and 31.6% (Start Aid).

Figure 3B shows the corresponding implied marginal tax rates including the low bracket income tax applying to non-means-tested income. As the means-testing rates were not 100% (albeit close to and marginally lower post-reform), the break-even points - $1,152 for individuals on SA and $961 for individuals on Start Aid - were somewhat above the SA and Start Aid levels presented in Table 1. After the break-even points, benefit eligibility was fully exhausted and individuals paid the usual low bracket income tax.

Figures 3C and D shows the equivalent for couples[10] distinguishing by whether both receive residency before the reform, both after the reform, or on each side of the reform. The same discounts to the means-testing which were just mentioned applied to couples as well. For couples on SA and where one spouse worked full time at the minimum wage (producing $2,127 in earnings from work), the other spouse still received $832 in SA, resulting in a household income of $2,959. For earnings above this threshold, the SA was reduced 1:1 by earnings with an implied marginal tax rates of 100%. Thereby SA was fully exhausted for couples earning $2,959. Couples on Start Aid could only earn $1,994 before the Start Aid was reduced to 0. As Figure 3D shows, the earnings range where implied marginal tax rates for couples were between 80-100% thus dropped from around $3,000 to $2,000 as a result of the reform.

Couples where one spouse received SA and the other spouse did not receive any benefits experienced a combination of the pre- and post-reform scenarios. Here, total household income when the spouses did not work was at the same level as what the couple would receive if both were on Start Aid as seen from the intersection with the vertical axis in Figure 3C. But because only one spouse received SA, the discount to means-testing was lower than if both spouses received Start Aid, producing an implied marginal tax rate of roughly 95%. When earnings exceeded SA plus the discount at $1,542 (around $500 lower than for couples where both received Start Aid), the implied marginal tax rate dropped to the low income bracket tax rate.

Finally, comparing Figure 3A to Figure 3C also shows that the distance between pre- and post-reform break-even points was wider for couples than for singles. The reason is that benefits dropped more for couples while discounts to means-testing were only given once per household: A single receiving residency

[10] As 91% of couples have children, we consider a one-child family. This is our benchmark in illustration of couples' transfers in the remainder of the paper. For singles, we consider an individual without children; the modal 'single' in our sample. Table A.1 shows extensive margin implied marginal tax rates at and break-even points for all family types.

after the reform and earning $500 would have around $400 of the Start Aid deducted. A similar amount would be deducted when earning $500 for an individual in a couple where both received residency post-reform. With a larger drop in the intersection with the y-axis (transfers levels) for couples, but similar slopes for couples and singles, the distance between pre- and post-reform break-even points was widest for couples.

In conclusion, there were intermediate income ranges between the pre- and post-reform break-even points, varying by household type, where implied marginal tax rates differed substantially (40-60%-points). Yet, while the reform was designed to boost employment incentives among refugees by lowering benefits levels substantially, Figure 3 shows that all groups still experienced high implied marginal tax rates for low incomes producing poor labor supply incentives at the participation margin both before and after the reform.

2.2.3 Information

In order to respond to the benefit reduction and changed incentive structure that is illustrated in Figure 3, refugees must have information about this. Were individuals aware of means-testing and the inter-household dependence? Were couples aware of the possible asymmetry in the incentive structure and how non-compliance to integration-courses affected benefit eligibility? And were the rates at which benefits were capped clear? Appendix B presents examples of letters from municipalities to Social Assistance recipients relating to these three pivotal questions. In all three instances are recipients explicitly made aware of the incentives and consequences of their choices. Moreover, for spouses receiving residency on each side of the reform, this further implies that they were made aware of the particular incentive structure they faced: A) program participation (e.g. language courses, activation programs) is compulsory, B) non-compliance implies that one cannot receive transfers, but C) only the first arriving spouse will in practice receive transfers making B) an empty threat from the perspective of the last arriving spouse.

2.3 Data and Samples

Our analysis is based on various Danish register data sets, recorded by public and governmental agencies and collected and organized by Statistics Denmark. The different data sources can be linked across registers and

across years using a unique personal identification number. Children's, spouses', and parents' unique identifiers are linked, which allows us to merge characteristics of one individual to his/her entire family.

We first use data from the Danish Immigration Service, which records all residence permits issued between January 1st, 1997 and March 31st, 2006 to a total of 252,795 persons. We extract permits given to refugees (67,375) including first-migrant refugees (44,232) and refugees who are granted residency through family reunification (23,143). We thereby exclude labor migrants and their families, and other non-refugees.[11] We merge the data to the "Database of Historical Migrations", which contains exact information on when refugees were granted residency, their country of origin, and whether and when they left Denmark again. The exact date of receiving residency defines each refugee's treatment status (before/after July 1st 2002), as well as the running variable (time before/after the reform that residency was granted). The individual identifiers allow us to add information from the Danish full population demographic register, labor market register, occupational register, income register, educational register, and criminal register. From the demographic register we add information on marital status, as well as age, gender, and date of birth.

We define the main sample as individuals in the age range between 18 and 55 when they received residency in Denmark, and we also collect data on the children of this group whom we will also investigate below. We restrict our observation window to individuals who were granted residency between January 1st 2001 and December 31st 2003, which results in a sample of 8,512 individuals. From this sample we exclude refugees from Afghanistan (1,833) and Ex-Yugoslavia (932) because of changes in the inflow from these two countries at the time of the reform, due to ongoing or ending conflicts in those regions, which could compromise the balancing of the final sample around the reform. We further exclude the 904 refugees who re-migrate within the following nine years (in section 5.1 we test for selectivity over the reform period to ensure that re-migration patterns are not affected by the Start Aid reform). Our final sample includes 4,843 individuals aged between 18-55 when granted residency and who arrived between January 1st 2001 and December 31st 2003. These individuals had 3,299 children aged between 0-17 when residency was granted.

[11] These groups are not eligible for SA / Start Aid and are thus not affected by the reform in 2002. Labor migrants may only stay in the country for up to three months after their employment ended, or until their work permit expires, and the spouse residing in Denmark is required to cover all costs associated with family reunification to non-refugees.

For the analysis of couples' responses, we refine the main sample to include the spouses of all individuals who received residency within the main sample window of ± 18 months around the reform. Hence, if one spouse receives residency one month before the reform, and is thus in our main sample, and the other spouse 20 months before the reform, and therefore excluded from our main sample, both will be included in the couples' sample. This produces a balanced sample of 4,072 individuals in 2,036 couples.[12]

2.4 Outcomes

Income: The income register contains annual information on labor earnings, capital income, profits from businesses, taxes, transfers income, assets, and liabilities. The income register stem from tax authorities' records. As Denmark has full third party information, which means that all income is reported directly from employers and welfare offices etc. to tax authorities, the income data include therefore all legal income.[13] In the paper, we consider four main income measures: Labor earnings, transfer income, total gross income, and net-of-tax (disposable) income. We use transfer income to investigate the direct effect of the reform through the reduction of welfare benefits.[14] We use labor earnings to investigate the employment related response of the reform. We use gross income to measure the effect of the reform on income and we use net-of-tax income to measure the effect on actual disposable income. We also add data from the IDA register, which provide information on hourly wage rates for all wage earners in work-places with 10 or more employees.[15]

Labor market outcomes: Labor market status consists of three mutually exclusive states: *employment*, *unemployment*, and *not in the labor force* (*NILF*). NILF includes individuals who are permanently unable / exempted from work as for example disability insurance and pension recipients. But NILF also consists of those who have dropped completely out of the welfare system. We label this latter status *Dropped out*.

[12] Results are robust to limiting the sample to couples where both receive residency in the ± 18 months window
[13] Studying tax evasion in Denmark, Kleven et al. (2011) find that it is low due to the universal third party reporting, and that evasion is mostly present for self-employed or individuals closely connected to their employer. None of these characteristics match our sample of newly arrived refugees we consider in the present study.
[14] We have also investigated whether lower welfare benefits affects reception of other means-tested transfers such as housing support, or one-time funding of expenses to a train ticket to Copenhagen to see immigration authorities or a heating bill (as Start Aid lowered benefits to subsistence level). The average monthly increase per person in other means-tested benefits following the reform was $43, thus only a minor fraction of the reduction induced by the reform.
[15] Hourly wages in the IDA register are estimated using annual labor earnings divided by annual hours worked approximated using compulsory pension payments (ATP) which are tied to working hours. Thus, the estimates contain measurement error and Statistics Denmark provide a score rating the likely error. We only use reported wage rates that are rated as 'Reliable and applicable' (75% of wage rates in couples; 72% for singles).

Combining this with information from the occupational registers allows us to categorize the type of occupation that each employed individual holds. We define seven categories based on type and skill intensity required: 1) high level of skills or manager , 2) medium level of skills or office related, 3) sales or services, 4) vocational work requiring medium or basic skills, 5) construction or primary sector work requiring basic skills, 6) unspecified self-employment, and 7) unskilled manual labor requiring few/no skills. We measure labor market outcomes from the year after the residency has been granted and onwards.[16]

Crime: Crime data with exact information on offense dates include measures of charges, arrests, incarcerations, and convictions. Importantly, each entry contains a unique case specific identifier and a unique individual identifier which allow us to uniquely match each case and crime to individuals in our sample. We measure individual criminal activity based on charges and convictions for offenses against the criminal code.[17] Crime convictions, which is our preferred measure of criminality, measure court rulings of the suspect's guilt that result in a sentence (either a fine, a suspended sentence or imprisonment). We always relate convictions to the date of the crime. The Central Police register categorizes crimes by detailed codes (e.g., 'theft from supermarket') which we use to identify the exact type of crime affected by the reform.

Children's outcomes: In addition we will also investigate children's daycare / preschool use and educational outcomes. We use the daycare register containing information on enrollment in daycares and preschools to identify daycare / preschool use. We also use data on Danish test-scores during grades 2, 4, 6, and 8, which is available from 2008 and onwards. Finally, we use the educational register, with annual information on enrollment and completion of educations to measure total years of completed schooling.

2.5 Sample descriptives

Table 2 presents descriptive statistics of the samples. Panel A shows means of covariates for the full sample of adults aged between 18-55 when they were granted residency between January 1st 2001 and December 31st

[16] In a supplementary analysis presented in the Appendix we also use weekly measures of welfare benefits recorded in the DREAM register, which records each individual's primary public benefit reception (if any) during each week. The register does not contain information on employment, and we can, therefore, not use the DREAM register to separate the residual components to welfare dependency (employment from self-sufficiency / out of the welfare system).
[17] Arrests, a common measure of criminal activity in the U.S., are not frequent in Denmark, and the Danish equivalent to arrests is charges. Throughout the analysis we omit traffic offenses.

2003 (the main sample), distinguishing between those who received residency before and after the reform. Over our sample period there was a downward trend in the inflow of refugees to Denmark. Therefore, there were fewer residencies granted to the post-reform group than to the pre-reform group. In Section 4.1 we show that there was no discontinuity in the number of residencies around the timing of the reform. 80% of refugees in the main sample fled/migrated from predominantly Muslim countries, and around half of the sample had Iraqi origin. 62% of the sample received residency based on refugee status and the remainder were granted residency as result of family reunification. Adults had on average two children, while almost all adults arrived without siblings. Although the table shows some differences in characteristics between the pre- and post-reform groups, one should note that the key assumption in our setup is not comparability of average characteristics for the two groups, but comparability in the limit around the reform. In Section 4.1, we test this pivotal assumption and show that observable characteristics are balanced around this limit.

Panels B and C in Table 2 show means of covariates for couples where at least one of the spouses received residency within our sample window, distinguishing between those who had their residency granted before and after the reform, and gender in Panel B, and by whether both received residency before the reform, one of each side, or both after in Panel C. On demographic characteristics, the pre- and post-reform groups were very similar, although the share of females was higher in the post-reform group. This gender difference in Panel B is caused by the downward trend in immigrant inflow coupled with the pattern that males are typically the first arriving refugee from a family followed by the spouse and their children, which is also evidenced as males were the first of the spouses to receive residency in 86% of the couples (therefore also are more likely to have refugee permit status while the females are more likely to appear as family reunified to refugees). Regarding the other background characteristics, the distribution is similar to the overall sample. Similar is seen for Panel C, except for country of origin and likelihood of having refugee permit status. Again, these differences are caused by overall trends in the data.

Table 3 summarizes the main outcome variables after residency has been granted. Panel A shows that transfer income constituted the main income source for all years considered, albeit the level and fraction of income from transfers decreases with time since residency. Roughly 85% of income came from public

transfers in year 1, which dropped to an average of 57% in years 3-5. Further, levels of total gross income and net-of-tax (disposable) income increased with the time of residency during years 1 to 5. Panel B presents average labor market statistics. The panel shows that only 12% of the sample were employed in year 1 after residency, with the majority being unemployed. As time progresses, unemployment rates decreased and employment rates increased. A sizeable fraction exits the labor force. Of this group, the table shows that in the first two years following residency the majority drops completely out of the welfare system without SA / Start Aid eligibility. The residual fraction of adults, who were not in the labor force but still in the welfare system, were either on disability pension, on early retirement pension, or enrolled in an education.

As a first visual illustration of the impact the reform had on incomes, Figure 4 shows average total income including transfers during the first full year following residency by month of residency. The vertical dashed line marks the date of the reform. The figure illustrates that the reform had a dramatic impact on average income, with the total gross income among refugees granted residency after the reform dropping almost to half of the pre-reform level. The figure also shows that labor earnings increased somewhat after the reform – from about $2,000 per year to $3,000 per year, though not offsetting the reduction in benefits.

In similar vein, Figure 5 shows labor market outcomes of individuals around the introduction of the reform. Panel A reveals that employment rates are consistently higher post-reform than pre-reform, albeit still at a low level of around 20%. While pre-reform unemployment rates were around 90%, they dropped after the reform by about 15-20%-points. Thus, unemployment decreased more than employment increased. This is explained by an increase in the fraction of individuals not in the labor force (NILF), which rises from around 3% percent to around 10%. This seems puzzling, and suggests that the reform let to an increase in employment take-up, but also to an increase in inactivity. In Section 4.3 we will investigate this further.

3 Econometric Framework and Identification Strategy

As we explain above, the vast majority of refugees that claim asylum do so in Denmark. That means that they spend several months in Denmark, typically in refugee camps, before they receive residency. It is therefore likely that individuals who received residency around the reform date have arrived in Denmark before the reform has been announced. Given this setting, we estimate the average effect of the reform as a

reduced form equation using a regression discontinuity design that compares refugees who received residency just before with refugees who received residency just after the reform date:

$$y_{i\tau} = \alpha + \beta * reform_i + f(Z_i)'\pi + X'_i\gamma + \varepsilon_{i\tau}, \qquad (1)$$

Where $y_{i\tau}$ is the outcome of interest measured τ years after receiving residency, $reform_i$ is a dummy variable indicating whether individual *i* received residency before or after the reform (July 1st 2002), $f(Z_i)$ is a flexible running variable counting the months between individual *i* being granted residency and the reform date,[18] X_i measures observable characteristics, and ε_i is an idiosyncratic error term. The parameter of interest is β, which measures the average treatment effect of being eligible for Start Aid instead of Social Assistance for those who received residency just after the reform relative to those who received residency just before if the timing of residency around the reform date is as good as random. We conduct a wide range of balancing tests to verify this assumption (see Section 4.1). In our analysis below we will also allow for heterogeneous responses by interacting $reform_i$ and $f(Z_i)$ with observable characteristics.

While the estimation of Equation (1) will inform us of the average effect of the reduction in benefits levels, the reform implies a separate treatment for spouses who were granted residency on each side of the reform as we describe in detail in Section 2.1. As shown in Figure 2A, couples where both spouses received residency before July 1st 2002, were eligible for full Social Assistance, whereas couples where both spouses received residency on July 1st 2002 or later (Figure 2C) were eligible for the reduced Start Aid (which was approximately half the size of SA). However, in couples where spouses received residency on each side of the reform (Figure 2B) benefits were capped at two times Start Aid and paid to the spouse who arrived before the reform, while the spouse who arrived post-reform was effectively ineligible for any benefits.

To capture the heterogeneous treatments that depend on both ones' own and spouse's timing of residency, we extend Equation (1) to allow the outcome of individual i in household f to be affected by the timing of residency of both oneself and one's spouse. Therefore, there are now three possible treatment states instead of two (pre- and post-reform residency): First, both spouses receive residency before the reform.

[18] In practice we define f() as a linear function before the reform and a linear function after the reform. In Table A.2 in the Appendix we replicate our main results using alternative specifications. Standard errors are clustered at the running variable (month of residency).

Second, both spouses receive residency after the reform. And third, spouses receive residency on each side of the reform. This is captured by two non-overlapping treatment dummies A_i and B_i, where A_i indicates that spouses received residency on both sides of the reform, and B_i indicates that both spouses received residency after the reform.[19] Consequently, the reference category is couples where both spouses received residency before the reform. We estimate the effects of the reform on individual *i*'s outcome $y_{i\tau f}$ as:

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

where $f(Z_{1f})$ and $f(Z_{2f})$ flexibly control for the running variables counting the months to / from the timing of the reform for each spouse. The parameter β_1 measures the effect of one spouse receiving residency after the reform and one before the reform, while β_2 measures the effects of both spouses being granted residency after the reform, where the reference category are couples where both receive residency before the reform. We also interact Equation (2) with gender, thus estimating β_1 and β_2 separately for males and females.[20]

4 Results

4.1 Balancing tests

For a first visual test of the balancing of refugees' observable characteristics around the timing of the reform, we present in Figure 6 predictions of employment, unemployment, and not in the labor force rates in the first year following residency, for each value of the running variable (months) as also done in e.g., Card et al. (2007b). We predict these outcomes using a full set of covariates: age at residency, whether the refugee has any siblings, gender, number of children, marital status, country of origin, and resident permit status along with a dummy indicating order of residency for couples. We do this for both the main sample and the couples' sample. The figures illustrate that there are no sign of discontinuities in the predicted outcomes around the reform for either sample, with very similar slopes before and after the reform date.

[19] See Card, Chetty, and Weber (2007b) for a further discussion of *Identification with double discontinuity*.
[20] We do, however, not separately test for differences in effects when husbands receive residency first relative to the cases where wives receive residency first. As husbands receive residency first in the vast majority of cases (86%), we do not have the power to separate effects formally along this margin without relying heavily on functional form of the running variables. However, we do provide an informal analysis in Section 4.3.2.

Table 4 presents the individual estimates from regressions of whether the individual was granted residency before or after the reform on aforementioned covariates and the running variable along with F-statistics and corresponding p-values testing for joint significance of the covariates. Panel A shows results for the main sample of adults, Panel B for the full sample including children, and Panel C for the couples' sample, where we test for differences in the observable characteristics of individuals in couples. The table show that there is no jointly significant difference in observable characteristics across the timing of the reform, with p-values between 0.359 and 0.794. Furthermore, as we only observe individuals once they have been granted residency to Denmark and not while in asylum camps before, a confounding circumstance could be if the reform changed the asylum process of those granted residency. In Table A.3 we show that this is not the case using supplementary data on the exact time spent in asylum camps and case processing from Hvidtfeldt et al. (2017) made available to us by the authors. The table presents tests for each covariate separately in the three samples, including time spend in asylum camps. The table also presents tests for selectiveness in remigration by including re-migrants in our sample and testing for differences in a dummy indicating remigration around the reform. We do not find any significant differences in the 37 individual balancing tests around the timing of the reform except in one instance. Figure A.1 presents McCrary tests of differences in the density of the running variable (residencies per month) around the introduction of Start Aid using both optimal bandwidth selection, and smaller and larger bandwidths to confirm robustness. We find no structural breaks or sign of manipulation of the running variable around the reform in any specification.

4.2 Primary Effects of the Reform

4.2.1 Effects on Income

We first investigate the effect on income in more detail, estimating models as in Equation (1). Table 5 shows the estimated effects of the reform on the log total gross income (Panel A) and post-tax income (Panel B) (on individual level) for the first five years after residency.[21] The different columns report specifications that condition on different sets of observables. Coefficients are very similar across specifications, which supports

[21] We consider the total income during the first five years in this log-income specification, as some individuals have 0 income in individual years.

our findings in the previous section that the sample is balanced around the reform date.[22] In line with Figure 1, the table illustrates that individual pre-tax income is radically reduced by the reform over the next five years, by around 0.35 log points, or 41%. Tax progression only slightly reduces this disadvantage, with post-tax income being still approximately 35% lower than pre- reform. Hence, the net effect of Start Aid was a large and durable reduction in disposable income for the average individual in our sample.

In Table 6 we separate the effects on total income for the first five years by income type and timing. As some individuals have zero income in some of the years, we now use levels instead of logs. The table shows the estimated effects of the reform on reception of public transfers (Panel A), labor earnings (Panel B), total gross pre-tax income (Panel C), and total post-tax income (Panel D). Panel A illustrates that annual transfer income is reduced by approximately $10,000, $8,000 and $5,000 in years 1, 2, and 3-5 following residency, corresponding to a 55%, 50%, and 30% reduction, respectively. Panel B shows that annual labor earnings increased by between $1,100 and $1,560 for those with residency after the reform, amounting to a 60%, 35%, and 15% increase in years 1, 2, and 3-5 from residency. While these effects were substantial, in particular when contrasted with the pre-reform mean of $1,852 in year 1, they far from compensated the lower benefit levels, as evident in Panels C and D. Total pre- and post-tax incomes dropped relative to the pre-reform means by approximately 40% in year 1, 30% in year 2, and on average 17% in years 3-5.

4.2.2 Effects on labor market outcomes

Table 7 presents the average effect of the reform on labor market outcomes for the first five years following residency, distinguishing between employment, unemployment, and not being in the labor force (Panels A-C, respectively). The first column in each panel reports the pre-reform mean as a benchmark. Figure A.2 in the Appendix presents the estimated mean effects visually. From a benchmark situation in the pre-reform period of around 10%, the reform almost doubled average employment rates by approximately 9%-points. As Start Aid levels are approximately half of pre-reform SA levels, the implied elasticity is almost 1. We thereby reaffirm and expand the results from Eissa and Liebman (1996) and Meyer and Rosenbaum (2001). While the effect of the reform fades over the following years, it remains sizeable relative to the pre-reform means.

[22] As this is the case for all results, we will only report results using the full specification in the remainder of the paper.

In year 2, the effect is around 7%-points, reducing to about 4%-points in years 3-5, which correspond to a 40% and 12% increase relative to the pre-reform mean, respectively. Turning to Panel B, the reform lowered unemployment rates (i.e. the fraction of individuals being eligible for SA / Start Aid) by almost 17%-points in year 1, 16%-points in year 2, and on average 7%-points in years 3-5. Hence, unemployment rates decrease by more than employment rates increase. As Panel C reveals, this is due to an increased likelihood of not being in the labor force. NILF-rates increase by 7%-points in year 1 and by more than 9%-points in year 2. Hence, relative to pre-reform rates of 3% and 5%, respectively, the reform triples inactivity in the first two years. The effects on NILF-rates remain significant in years 3-5, at a magnitude of about 3%-points.

Table 8 shows the effects of the reform on employment rates, unemployment rates, and NILF-rates, where we now distinguish between males and females (Panels A-C). Furthermore, we also consider a sub-category for individuals who are not in the labor force, and who have dropped out of the welfare system altogether and are thus not entitled to any public transfers ("Dropped Out", Panel D). Panel A in Table 8 shows that employment effects in the first two years are entirely driven by males. In year 1 and 2 males experience effect sizes of 16%-points and 13%-points, respectively. There is no employment response by females. Unemployment effects in Panel B show that the entire employment response by males is accompanied by a corresponding drop in unemployment. For females, we observe an even larger reduction in unemployment rates, 17%-points in the first two years and 8%-points in years 3-5. Panel C shows that it is driven by a large reduction in labor force participation. In the first two years following residency, females are 13-16%-points more likely to drop out of the labor force, whereas males' labor force participation is unaffected. Panel D further illustrates that all exits from the labor force are complete exits from the welfare system. Again, effects are largest in the first two years (13-16%-points) but are also sizeable and highly significant in year 3-5. Because of the reform, approximately 10%-points of females (4 fold the size of the pre-reform mean) permanently withdrew from the Danish labor market and welfare system.[23] The effects are in similar proportions irrespective of level of qualification, as shown in Table A.5 in the Appendix.

[23] The difference between effects on 'NILF' in Panel C and 'Dropped out' in Panel D arise from insignificant variation in disability pension, early retirement pension, and education enrollment.

Several important implications follow from these results. First, the reform led to a substantial take-up in employment and by that measure, the reform produced the intended effect. The average employment response relative to the pre-policy employment level is indeed noteworthy in comparison with earlier studies, but perhaps less so once the large benefit reduction is taken into consideration.[24] Second, Tables 7 and 8 also point at unintended consequences of the reform: The stronger reduction in unemployment is explained by an increase in the share of individuals (females) who dropped out of the labor force. These findings correspond to Bratberg and Vaage (2000), Card et al. (2007a), and Kyyra and Ollikainen (2008) who illustrate the importance of distinguishing effects of benefit eligibility on unemployment, total non-employment, and employment. Card et al. (2007a) for instance stress that unemployment reductions may mask labor force exits rather than being caused by higher employment rates. But moreover, females' substantial labor force exits indicate that the reform induced more complex responses than the average primary effects this first subsection shows. We will therefore focus on investigating the underlying causes in the paper's next section.

4.3 Margins of Response, Incentives and Disincentives

In this section we focus on how the responses to the changed incentives shaped different household types' income levels and labor market outcomes. We do so in two steps. In the first subsection, we investigate the effect on income levels, the income distribution, and wage rates across household type. In the subsequent subsection, we investigate how the reform affected employment, unemployment, and labor force participation with particular focus on the (dis)incentives introduced via asymmetry within households, and the nexus between means-testing and actively exiting the labor force.

4.3.1 Effects of the Reform on Income Levels and Wage Rates

As we now turn our attention to effects separately by household type, we estimate effects for couples and singles separately. For singles we use Equation (1) as previously, but for couples we estimate Equation (2)

[24] There is an extensive literature studying welfare-reforms during the 1990's in the U.S. (for a review see Blank, 2002). Employment incentives by tax cuts through the Earned Income Tax Credit to low-skilled individuals or individuals with weak labor market attachments have been shown to increase employment (see Eissa and Liebman (1996), Meyer and Rosenbaum (2001)). More recent studies are Card, Chetty, and Weber (2007a, b), Lalive, van Ours, and Zweimüller (2006), and Røed and Zang (2003) who all investigate changes in UI benefit entitlements on the duration of UI claims (see Card, Kluve, and Weber (2010) for an extensive review).

in order to capture the heterogeneous responses within households. We also re-define the sample of couples to include the spouses of all individuals who received residency within the main sample's window of ± 18 months around the reform, as detailed in Section 2.3.[25] Hence, our sample now consists of a balanced sample of two observations for all couples where at least one spouse is granted residency ± 18 months around the reform. It should be noted that, when spouses receive residency at different points in time, an outcome variable relating to e.g. the first year after residency could capture different points in time for the two partners. To align outcomes of both spouses so that effects can be compared directly within the couple, we redefine 'time since residency' by counting the time that passed since the last arriving spouses' residency. For example, income in year 1 will refer to income the first year after the last arriving spouse's residency.

Table 9 shows estimation results for the effect of the reform on income, by income type, gender, and household type. Panel A thus reports estimates of β_2 in equation (2). Panel B reports results for couples where only one of the spouses received residency after the reform, and again, the reference category is couples where both received residency before the reform. Panel B thus reports estimates of β_1 from equation (2). The difference between β_1 and β_2 therefore measures the net effect of providing couples with equal potential transfers on household level, but in some couples with symmetric benefit schemes between husbands and wives and others with asymmetric benefit schemes. Panels C and D corresponds to Panels A and B, respectively, using household income instead of individual level income. Finally, Panel E shows estimated effects for singles, reporting estimates of β from Equation (1) with singles who received residency before the reform as reference group.

Panel A shows that males had higher labor earnings, whereas females' labor earnings did not change significantly following the reform. The reduction to welfare transfers was approximately similar for males and females in this household type: Around $7,000 in year 1, and $6,000 in year 2 following the residency of the last arriving spouse. Panel B shows results if only one spouse received residency after the reform, and results in this panel illustrate the heterogeneous response within households. Males' reception of welfare transfers as well as their labor earnings was not affected significantly by the reform, as they all remained on

[25] Results are robust to limiting the sample to couples where both receive residency within the original sample window of +- 18 months of the reform.

the pre-reform Social Assistance levels. But females experienced decreases in transfers, gross income, and net-of-tax income without experiencing any change to labor earnings. Panels C and D show results for households, and comparing results in these panels to results in Panels A and B shows that females in households affected asymmetrically by the reform experienced the full household reduction in transfers of approximately \$11-13,000 per year, thereby effectively reducing their welfare claims to 0.[26] Finally, Panel E shows that the average transfers received by singles' dropped by \$6,700 and \$5,500 in year 1 and 2, respectively, illustrating that the reduction induced by the reform for this group was, albeit still substantial, not as large as for couples. Labor earnings increased significantly by \$2,700 while yearly total gross income and net-of-tax income was reduced by \$2,600-3,600 and \$2,000-2,800 respectively.

We next investigate how the reform changed the distribution of annual labor earnings. We therefore create a series of dummy variables indicating whether an individual or household earned a level of \$*x* or below in the first year following the last arrived spouse's residency $1[y \leq x]$, and vary *x* from 0 and to the top of the labor earnings distribution. Using these dummy variables as outcomes in estimations of either Equations (1) or (2) will thereby provide us with the estimated change in the cumulative distribution. The results are presented in Figure 7. Figure 7A and B show the results for singles. Figures 7A and 7B show the effect of the reform on the cumulative distribution, and the differences between the pre- and post-reform groups. Comparing the differences between the two groups at an income level of 0, we see an extensive margin effect on income of approximately 20%-points relative to the pre-reform mean of 40%. Furthermore, assuming that individuals who increase labor at the extensive margin do not place themselves in the earnings distribution exactly where individuals with intensive margin responses were placed, intensive margin responses would appear as income ranges in Figure 7B where the difference between the two groups grows because intensive margin changes would produce less mass at a given point in the post-reform distribution

[26] In Table A.6 in the Appendix we break the effects down by individual income source: labor earnings, residual earnings, transfers (pensions, temporary assistance (UI, SA, Start Aid), housing support, child benefits, educational support). The table shows that effects of the reform on net-of-tax income across households who were affected symmetrically and asymmetrically differ by approximately \$1,500 in year 1 as a result of small difference in housing support, which increases more for the group that is affected asymmetrically, and a cap on one-time benefits to families with children that only applied when both spouses received residency after the reform (in total a \$1,000 difference in year 1). While also constituting a minor fraction of the net impact of the reform, these differences are, however, not something couples have discretion of. Therefore it does not induce different incentives between households affected asymmetrically by the reform relative to households affected symmetrically.

relative to the pre-reform distribution. This is not the case as the difference is largest at the extensive margin (at 0) and the groups converge thereafter, thus showing that the entire response to the reform was an extensive margin effect. Roughly half of this extensive margin effect resulted in incomes below the pre-reform annual equivalent to the break-even point (the point from Figure 3A multiplied by 12) marked by the dashed line in Figure 7B, while the remainder of the response results in higher levels of labor earnings.

Turning to couples, Figure 7C shows the estimated cumulative distributions for labor earnings on household level, while Figure 7D shows the differences between couples where both spouses received residency before the reform and couples where only one spouse received residency after it. Figure 7E shows the differences between couples where both spouses received residency before the reform and couples where both spouses received residency after the reform. The dashed lines in Figures D and E mark the pre-reform break-even point shown in Figure 3C. The figures show large extensive margin effects, no sign of intensive margin effects where the differences between the lines grow, and that the entire response to labor earnings of couples is located below the break-even point where implied marginal tax rates differ. Furthermore, we see a remarkable difference in effects by comparing couples by whether one or both spouses were granted residency after the reform. Even though the two household types were eligible for the same amount of transfers, the reform induced an extensive margin response of roughly 20%-points if both spouses received residency after the reform, but only half of that if the spouses received residency on each side of the reform.

These results corroborate the arguments made by Heckman (1993); Blundell and MaCurdy (1999) and findings from Eissa and Liebman (1996); Meyer and Rosenbaum (2001). Extensive margin labor supply elasticites for specific subgroups are substantial and likely larger than intensive margin elasticities. The probability that households, where both spouses receive residency after the reform, supply any labor is tripled relative to the pre-reform probability; a much larger effect than the intensive margin elasticities found for Denmark (see Chetty et al., 2011; Kleven and Schultz, 2014).[27]

[27] Chetty et al. find (micro) elasticities that are not significantly different from 0 using income bunching around tax kinks, while Kleven and Schultz estimate labor income elasiticies of 0.05 for wage earners using several different tax reforms as identification.

Figure 8 describes the intuition behind the disconnect between the response in household where one received residency after the reform and households where both received residency after the reform. Figure 8A illustrates the consequences of the reform and the resulting responses on household level and why some respond by taking up employment while others drop out of the labor force completely.[28] Before the reform, households were eligible for benefits equal to point A if they provided a minimum of 'work' in e.g., integration courses. The reform reduced benefits levels to half of point A. If both household members comply with the eligibility criteria this puts them in point B. This induces some to take up employment at wage rate w_{min} and arrive in point D, thereby jumping to a higher indifference curve. However, some couples (those receiving residency on each side of the reform) could increase leisure without reducing income by having the last arrived spouse drop out of the labor force – such couples arrive in point C. We have already confirmed this prediction in Table 9 and Figure 7 – labor earnings / employment responses were largest when both spouses received residency after the reform.

Yet, Figure 8A also suggest an underlying consequence of the reform relating to wage rates. Whether point C is preferred to point D depends on reservation wages. Figure 8B elucidates this further. Points A1, B1, and C1 in Figure 8B are identical to points A, B, and C in Figure 8A. Points A2, B2, and C2 illustrate the tangents of wage rates w_{max}, w_{min}, and w_{med} where individuals in the three household types are indifferent between supplying labor and being on benefits. The figure shows that the highest wage rates are needed for the pre-reform group who has the highest welfare benefit level. For couples where only one spouse received residency after the reform, an intermediate wage rate (w_{med}) is needed, while for couples both receiving residency after the reform a lower wage rate (w_{min}) is sufficient. We next turn to these predictions and analyze the effects of the reform on wage rates conditional on employment.

Figure 9 shows the estimated effects on the cumulative hourly wage distribution. As we did for annual income, we now construct a series of dummy variables $1[w \leq x]$ and vary x from 0 to the maximum wage observed in the data. Again, we present results for singles and couples separately.[29] Figure 9A shows the

[28] Thus, we are treating the household as unitary.
[29] Results for couples are estimated by Equation (2). Results for singles plot the pre- and post reform cumulative distributions to improve precision. The estimated effects using Equation (1) for singles is similar but noisy.

wage distributions for males in couples. The vertical dashed lines mark the hourly equivalent of the break-even points from Figure 3C (with monthly earnings divided by 160, the monthly hours of work in a standard full-time job). The figure shows that the reform reduced modal wages as predicted above. If one spouse received residency after the reform, wages are concentrated at lower levels than pre-reform wages, while the lowest wage rates are received by couples where both received residency after the reform. It is evident from the figure that there is no mass below the left vertical dashed line which marks the post-reform break-even point of two Start Aids. This point is approximately equal to the pre-reform break-even point of one SA and also the collective bargain minimum wage. As discussed in Rosholm et al. (2006); Tranæs (2006); Landersø and Heckman (2017), the pre-reform SA levels and collective bargain minimum wages serve as a binding wage floor in the Danish labor market. This is illustrated by Figure 9A and is even more pronounced when we consider singles' hourly wage distributions in Figure 9B. Here, the dashed lines mark the break-even points from Figure 3A. As we now consider individual and not household level the points are, as described earlier, at a lower level and more closely spaced. Yet, even though singles' break-even points fall considerably below couples' levels, there is no mass below the lowest wage rates observed previously for couples. Furthermore, Figure 8B shows that singles who received residency after the reform simply enter the existing wage distribution, as implied by the pre- and postreform distributions being very similar, thus not creating additional mass in the cumulative wage distribution conditional on employment at any level.

These results emphasize a further limitation of the reform to increasing employment. Not only do Negative Income Taxes / means-testing provide few extensive margin incentives, but our findings suggest that the demand for the type of labor that the least productive individuals supply in a highly unionized / skilled labor market as the Danish one is non-existent and constrained by collective bargaining. As discussed by Acemoglu (2001), welfare benefits and minimum wages may have formed a long-run equilibrium in Denmark with no demand for labor at productivity levels below this wage floor.[30,31] Furthermore, Kreiner et al. (2017) find that a discontinuity in the minimum wage of 40% at age 18 (from youth to adult rates) in

[30] Resulting in demand for what Acemoglu labels 'good' (high paying) but not for 'bad' (low paying) jobs.
[31] While our wage data contain measurement error (discussed in Section 2), the distributions shown here and the wage floor / minimum observed wage correspond closely to the distribution for non-western immigrants in Tranæs (2006) who use survey data.

Denmark results in a 33% reduction in employment thus illustrating the employment potential of locally reducing minimum wages for unskilled individuals. To elucidate this further, we analyze the type of jobs individuals take up. In Table A.4 we separate the employment effects from Table 7 by type of occupation. Panel A shows that only employment in 'unskilled manual work' increased as a response to Start Aid, while the remaining job-types were unaffected, and Panels B and C show that employment effects do not differ across educational levels. Irrespective of education from origin country individuals took up unskilled manual labor jobs implying that refugees' skills upon arrival to Denmark did not translate into better jobs. The move from SA to Start Aid may well have lowered reservation wages of refugee migrants, therefore increasing their willingness to become economically active. Yet, wage floors may have led to structural demand-side constraints such that available jobs pay a wage rate above the productivity level of most recently arrived refugees, possibly reinforced by a lack of language and non-transferable skills (see discussions of refugees' skills in relation to the Danish labor market in Rosholm et al., 2006, Schultz-Nielsen, 2008). [32]

4.3.2 Employment, Disincentive Effects, and Non-Participation

We now turn to elucidating the earlier result on females' labor force exits and thereby also the last prediction from Figure 8 on how incentives induced by the reform affect 'leisure' vs. 'work' for the different household types. Here, labor force exits may have had two potential causes with very different implications. First, as SA and Start Aid are means-tested, the employment effects may implicitly induce labor force exits of spouses. Second, the effects could arise from the particular feature of the reform that introduced an asymmetric incentive structure in couples who received residency on each side of the reform. Table 9 shows the estimated effects of the reform on labor market outcomes by household type and gender. Panel A reports results for couples where both received residency after the reform. The reference category is couples where both received residency before the reform. Panel A shows that males' employment rates increased by 15%-points in year 1 and almost 10%-points in year 2 after the last spouse received residency, with unemployment rates decreasing slightly more, though these differences are not significant. Turning to

[32] This is in line with Schultz-Nielsen and Skaksen (2017), who show that for immigrants from these origin countries, that returns to education from the country of origin is close to zero, with almost all employment being in jobs which only require compulsory schooling irrespective of previous education and training.

females, their employment rates increased by 8%-points in year 1 after the second spouse received residency if both spouses received residency after the reform. Estimates for year 2 are less precise but still sizeable. The table also shows that females' unemployment rates decreased by 17%-points in year 1 and 11%-points in year 2, and NILF-rates increased by 9%-points in year 1 but not significantly in year 2.

Turning to the estimated effects of the reform if only the last arriving spouse received residency after the reform in Panel B, we see that males' employment rates increase by 7.5%-points and 11%-points in the first two years following residency of the last arriving spouse while unemployment rates dropped slightly (and not significantly) more. For females, however, there is no detectable employment response while unemployment rates decrease by more than 20%-points. Hence, not only were males less likely to take up employment when they were the sole benefit-recipient, thus obstructing the intended effect of the reform, but females were 15-20%-points more likely to leave the welfare system in the first two years following residency compared to the situation where both spouses received residency after the reform. As males arrived first in 86% of couples we do not have enough data for couples where females arrive first to separately estimate effects by gender of first arrived. But because males are typically the first of refugee spouses to arrive to a country, it is relevant to assess whether labor force exits for females in couples arriving on each side of the reform has roots in gender roles associated with the labor market or whether they simply arise from females being the last of spouses to arrive and therefore being the ones who get residency after the reform – and are thus the ones who would refrain from receiving Start Aid. Therefore, Figure A.3 shows labor market outcomes for couples where the husband arrived first and the wife second, and vice versa by month of last arrived spouse's residency. Unemployment rates drop in both family types around the reform, for females who receive residency last exclusively due to higher NILF rates while last-arriving males also show a weak employment response which we do not find for females (compare Figures A.3.A and A.3.C). Thus, gender differences in responses of couple where residency was granted on each side of the reform were likely driven by gender roles attached to both the labor market and migration patterns in unison.

Finally, Panel C in Table 9 shows effects for singles (both genders due to the sample size). The reform increased employment by 14-15%-points. In year 1, unemployment was reduced by a similar magnitude. In

year 2, however, unemployment rates continued to decline as labor force exits increased. The employment effects were driven by males and the decline in labor force participation were driven by females who find a partner in the years following residency in Denmark and thereby lose their benefits due to means-testing.[33]

For a more detailed presentation, we also estimate the effects of the reform on the probability of receiving SA or Start Aid on a weekly basis and report results in Appendix Figure A.4.[34] The figure confirms the earlier findings. The reform resulted in males being 15%-points less likely to claim benefits during the first year and a half following the residency of the last arriving spouse, whereas in couples where one spouse received residency after the reform the effects are less pronounced. Here, the probability of claiming benefits only decreased by 5-10%-points. In households where both spouses received residency after the reform, females' probability of claiming SA / Start Aid dropped by 10%-points after the reform, while the corresponding response among females in households where only one spouse received residency after the reform was almost a 40%-points drop initially, stabilizing in subsequent years around 20-30%-points.

4.3.3 Bounding the Effect of Means-Testing

Because SA and Start Aid are means-tested on household level, it is unclear how much of the lower benefit reception and increased labor force exits can be attributed the direct effect of the reform and individuals actively exiting the labor force, and how much result from means-testing if some spouses find employment.

In order to address this, and as a further robustness test of the estimated effects of the asymmetric benefit structure, we simulate the case where all labor force exits for individuals with employed spouses are driven by means-testing. We do so by setting the dummy indicating that one is completely out the welfare system (*Dropped out*) to 0 if the other spouse holds a job. This imputation will then nullify the response to means-testing in the most extreme case in which the employment of one's spouse resulted in one's exit from the

[33] Though we lack precision, we find no significant employment effects when we exclude single males, and no significant effects on NILF when we exclude females who find a partner in the years following residency to Denmark.
[34] We use weekly measures of welfare benefits from in the DREAM register, recording the primary public benefit reception (if any). This allows us to measure precisely when individuals receive public benefits. The data do not, however, contain employment information. Hence, we cannot separate the residual components to welfare dependency, 1) employment and 2) self-sufficiency / out of the welfare system, on a weekly basis. The figure is constructed by estimating Equation (2) using as dependent variable a dummy indicating either SA or Start Aid reception for each week from residency of the last arriving spouse and the following three years (156 estimations on weekly level).

labor force. Table 11's Panel A shows the results for households where both spouses received residency post-reform, and Panel B shows the estimation results for households where only one spouse received residency post-reform. In columns 1-2 we present the estimates using the actual dropout rates from Table 9 while columns 3-4 present results using the imputed data that nullify the estimated effects of means-testing.

By comparing columns 2 and 4 in Panel A it is evident that for females in couples where both received residency after the reform we see that estimates are insignificant and close to zero when corrected for the impact of means-testing (column 4). But for females in couples where only one received residency after the reform (Panel B) we still find an increased probability of being fully out of the welfare system. The bound of the effects are approximately 9%-points. Means-testing thus explains the entire response in exits from the labor market when both spouses received residency after the reform, but not when only one received residency post-reform. If anything, the discrepancies between the responses from females where one or two household member receive residency after the reform increase.

5 Derived Effects of the Reform

While the reform studied in this paper attempted to address low employment among refugees by lowering benefits levels substantially, our results show that the intended consequences of the reform were likely obstructed by disincentivizing labor force participation due to high implied marginal tax rates for low incomes, the possibility for some to increase leisure (exit the labor force) without affecting potential benefits, and a lack of demand for the type of labor that refugees supply. In sum, depending on household type, the likelihood that at least one within the average couple takes up employment increased by 10-20%-points from a baseline of 10%, leaving the residual 70-80% with the full income reduction that the reform induced. As described in Section 2, the reform effectively lowered benefit levels to (or below) a subsistence minimum in Denmark. Such strong income reductions may have had consequences along other margins, such as criminal behavior and children's lives. In the next subsection we first investigate the reform's effects on adult crime and subsequently the effects of the reform on children's outcomes. We then investigate the channels of the effects on children's outcomes, showing that the effects on children stem from the reduced family income following the reform, and not derived as for example parents increasing their labor supply.

5.1 Effects on Adults' Crime

Figure A.5A-C shows the reduced form effects of the reform on crime for couples during the first year after residency and Table 12 presents the corresponding estimation results of the reform on the probability of receiving a crime conviction and the number of crime convictions. Also, Figure A.6 presents the estimated mean effects on crime for the full sample from year 1-5 showing that the effects persist.

Panel A of the table shows that males' crime convictions increase in couples where both spouses receive residency after the reform (i.e., where each spouses' incomes are reduced by almost 50%). As a result of the introduction of Start Aid, males were 4%-points more likely to receive a crime conviction within the first year in Denmark. This effect is driven by shoplifting and violence (Table A.7 in Appendix shows results by crime types). Panel B of the table shows that there is no response for males in couples where only one spouse received residency after the reform (i.e., the 20-30% of couples where females absorbed the full income reduction experienced by the household as a result of the reform). Females' crime rates also increased significantly in couples where both spouses received residency after the reform (Panel A). The effects on females' crimes conviction probabilities and number of convictions are very large in magnitude (relative to a pre-reform mean close to zero), and solely driven by shoplifting, in particular shoplifting in supermarkets (Table A.7 in Appendix). For females in couples where one partner arrived after the reform, estimated effects are slightly smaller and less precise (Panel B). The point estimates suggest, however, a tripling of conviction probabilities compared to the pre-reform period. As was already mentioned, Start Aid reduced income to or below a subsistence minimum in Denmark, and our findings suggest that the income reductions following the introduction of Start Aid induced criminal behavior for both partners when their transfers were cut by about 50%. Given their low base crime-propensities, the effect on married women is most dramatic: we consider this an important result showing the detrimental effects the reform had on this vulnerable group through disincentivizing integration and labor market participation while reducing income substantially. Our findings thereby add causal evidence to the results in Carr and Packham (2017), Corman et al. (2014), and Foley (2011) who show that scarcity is associated with criminal responses such as crimes related to obtaining food (which is what we find for females). Furthermore, when one partner received

residency before the reform, males' crime rates was unaffected, suggesting that when males' transfer status remains unchanged, it keeps them from engaging in more crime, despite the reduction in total family income.

5.2 Effects on Children

A large fraction of refugees arrived in Denmark with children, as we show in Section 2. As several papers document a link between parental income and children's outcomes, the stark reduction in average family income following the Start Aid reform had potentially effects on children which we now investigate.[35] We therefore limit our sample to families with children (Table A.8 and A.9 replicates the main findings by for this sample). We analyze the effects for children in groups divided by age upon residency to Denmark.

5.2.1 Residency During Early Childhood

We first analyze the effects the reform had on the share of children in the age range between 0-5 years who enroll into daycare or preschool. Panel A in Table 13 provides the estimates and Figure A.5D presents the corresponding reduced form figure. The effect indicates on average 15%-points lower likelihood of attending daycare or preschool as a result of the reform during the first two years after residency. This is a large effect that amounts to 28% and 21% reduction in years 1 and 2, respectively. Low enrollment rates of immigrants in daycares and preschools are strongly debated in Denmark,[36] and are viewed as one of the key causes behind the substantial language skill gaps between children with Danish and non-Western background (see e.g. Bleses et al., 2016; Eriksen and Hvidtfeldt, 2016). Cornelissen et al. (2016) show also that immigrant children, while having the lowest propensities to enroll in preschool and daycare, have at the same time the highest benefits from attendance, in terms of cognitive development. Panel B shows the estimated effects of the reform on Danish language test scores taken in 2008 for the same group of children and Figure A.5E shows the corresponding reduced form figure. The tests were introduced in 2008[37] and are compulsory for all children in grades 2, 4, 6, and 8. The test measures children's skills within three cognitive areas of Danish

[35] Several papers have documented a link between family income and children's outcomes. Recent examples include Aizer et al. (2016), Dahl and Lochner (2012), Duncan et al. (2011), Hoynes et al. (2016), and Løken et al. (2012). See Currie and Almond (2011) and Heckman and Mosso (2014) for an extensive review of this literature.
[36] See Berlingske Tidende, December 26th, 2016 and Radio 24syv, February 23rd, 2017 for examples of the debate.
[37] 75 children (7.9%) have missing test scores. We impute their test scores using test scores from subsequent years. The share with missing test scores does not change as a consequence of the Start Aid reform.

language, namely language comprehension, decoding, and reading comprehension.[38] Scores were standardized as advised by the practical guide in Beuchert and Nandrup (2015), who also provide further details on the tests. As children's time of residency and age differ, they attend different grades in a given year and we therefore measure test scores at different grade levels with grades 2 and 4 being modal grades. We see from the table that the pre-reform mean was 0.676 standard deviations below the national average. Thus, we reaffirm what is also found by the earlier studies comparing academic test scores of refugees and natives. While there is no detectable mean effect of Start Aid on children's test scores, we see that the reform did indeed make children 5%-points more likely to be placed among the lowest test scores from a baseline prevalence of 3%. These findings are in line with studies on among others the association between poverty and children's brain development (Hair et al., 2015), the effects of the EITC / child benefits on test scores (Chetty et al., 2011; Dahl and Lochner, 2012, Milligan and Stabile, 2011), the effects of income on long-run health, education and IQ (Aizer et al., 2016; Løken et al., 2012), the effects of food stamps on children's health (Hoynes et al., 2016,) and underline that very low family income may obstruct early-life skill acquisition in particular for already low-achieving and/or vulnerable children.

5.2.2 Residency During Middle Childhood

To assess the overall long run impact of the reform on the completed education of children, we now turn to the final years of completed education as measured in 2016 (i.e., 13-15 years after being granted residency to Denmark) for children who were between age 2 and 13 upon receiving their residency. [39] Figure A.5F shows a strong discontinuity in years of completed education for males who were 10-13 years old at residency. The estimation results in Table 14 confirm this pattern. Column A shows the estimated mean effects of the reform on the education of all children age 2-13 at residency. Columns B and C show the effects by gender. All columns show the average effects and effects by whether or not children were younger than 10 years upon residency. The table shows that there are no significant effects of the Start Aid reform on children's

[38] A variety of item tools are applied to measure skills on different levels of complexity, and the test uses, for example, multiple-choice (dichotomous as well as polytomous), word insertion, word splitting, and coloring tasks.
[39] Denmark has no minimum school leaving age but a minimum number of compulsory grades. Danish law stipulates that education is compulsory until completion of 9th grade. Children younger than age 2 upon residency had not reached the end of compulsory education by the last year of available data, 2016, and are therefore excluded in this analysis.

average educational attainment. However, once we separate effects by age at residency, we see that children aged 10-13 had 0.6 fewer years of completed schooling. This reduction is driven by males, who complete almost 1 year less schooling as a result of the reform. One explanation of why we find negative consequences of the reform on completed education for children in their teens upon residency to Denmark, but not for younger children, is that the latter may not yet have reached their final education, thus with effects to materialize later on.[40] Another likely mechanism is the overall effects of age at migration on assimilation (see Bleakley and Chin, 2004, Åslund et al., 2015). The effects of the reform may be mitigated for children who have been exposed to Danish institutions from an early age.[41]

5.2.3 Residency During Adolescence

As the reform resulted in increased crime rates for parents, a natural next question relates to whether the reform also affected adolescents' crime rates? Table 15 shows the estimated effects of the reform on the probability of having a crime conviction for an offense committed at or before age 20.[42] Panel A shows the estimated effects of the reform on all children age 11-18 at residency. Panel B shows estimated effects by gender while Panel C shows the estimated effects by age at residency (younger than 14 / 14 or older). From Panel A we see that the reform led to significantly more crime convictions overall and a significantly higher rate of violent crime. Panel B further shows that the reform increased shoplifting for females and violent crime for males (which was the same results as we found for adults; we do not, however, find any evidence of substantial correlation between the criminality of parents and their children). From Panel C we see that the effects are driven by adolescents who were in their peak crime ages upon residency: the reform resulted in significantly more crime convictions in general, more shoplifting, and more violence for adolescents age 14-18 upon residency. The corresponding reduced form figure is shown in Figure A.5G. In conclusion, we find no evidence of any positive effects on children's lives but several examples of negative effects of the reform.

[40] Duncan and Chase-Landsdale (2001) also find that the impact of family income is largest for older children.
[41] Jakobsen et al. (2016) investigate the effects of the reform on children's' 9th grade test scores and earnings during late teens. They find no significant mean effects but that fewer attend exams (likely biasing estimates).
[42] The age of criminal responsibility in Denmark is 15, after which youths are considered fully responsible for their criminal acts and subject to imprisonment

5.2.4 A Consequence of Lower Income or Parental Labor Supply?

As discussed by for example Heckman and Mosso (2014), the mechanisms linking household income and children's outcomes are often unclear because changes to welfare benefits also induce behavioral changes in other margins which may in turn also affect children via e.g. lower parental time investments or social control (see e.g., Aizer, 2004). The results presented for children's outcomes in this section naturally beg similar questions. To investigate this, we replicate the analysis from section 4.3.3 where we imputed outcomes of individuals whose spouses held a job to nullify the response to means-testing when estimating effects on labor force exits. Now we rerun the entire analysis for children and replace the outcomes by the pre-reform mean if parents' total labor earnings the first three years after the reform exceeded $25,000. Thus, all differences between receiving residency pre-reform and post-reform are nullified for children whose parents obtain employment during some periods after their residency in Denmark, leaving any remaining effects of the reform to those whose parents remain in non-employment. The results are shown in Table A.10 along with the estimates presented in Tables 13-15 for comparison. The table shows that most estimates are virtually unchanged while the few diverging results become stronger after negating responses from children with working parents. We therefore conclude that the reform's effects on children are driven by the income loss and not by elements related to parental labor supply. Some effects on children's outcomes, for example lower daycare enrollment rates, may arise directly to reduce households' expenses, while other effects, for example on adolescent crime or education, may arise as implicit results of lower consumption possibilities and increased strain associated with near-poverty income. Finally, one aspect of the lower household income that most children experienced which we have not been able to investigate is the utility-loss derived from lower consumption and increased strain. Reducing income by 30% on average in families with children, who depend highly on welfare benefits, is likely to have large costs in this aspect as well.

6 Conclusion

This paper studies intended and unintended consequences of a large welfare reform in Denmark, proposed in March, approved by June, and enacted on July 1st of 2002, targeting refugee immigrants. Individuals who

were granted residency just before July 1st were eligible for the same SA levels as native Danes, while those who were granted residency after that date received up to 50 percent lower *Start Aid* levels.

The reform led to dramatic drops in transfers for a population with high non-employment rates, thus potentially creating large incentives to take up work. We show that employment and labor earnings indeed almost doubled for males in the first year. This extensive margin response led to additional employment solely in unskilled jobs, irrespective of individuals' educational background. Together with evidence that no jobs were taken below a certain threshold, this suggests demand-side constraints to refugee migrants' employment due to the high Danish wage floor combined with the low productivity of refugee migrants.

Further, the reform created disincentives induced partly through means-testing. Moreover, a policy design feature that paid benefits only to the first arrived spouse in households where one partner received residency pre-reform and the other post-reform left the last arrived partner with no benefit payments. This induced a disincentive to remain in the labor force and to oblige to the requirement to attend integration- and language programs, and, as a consequence, led to a withdrawal from the labor force for an already vulnerable and hard to integrate population – married females who arrived as refugees from mainly Middle Eastern countries. In addition, while the reform lowered modal wage rates, having males being sole transfer recipients also attenuated both their employment response to the reform and the effect the benefit reduction had on wages conditional on employment. This was certainly unintended by the policy maker, who may have wrongly assessed the preferences and family role allocations in families with different cultural heritage.

Therefore, while the reform may have had the intended effects on male unemployment, it also led to a dramatic drop in disposable income for the residual 60-70% of this population that still relied on income from welfare benefits. Studying the secondary consequences of the reform, we find that it led to a 7-fold increase of women being convicted for property crimes (from low baseline prevalence); the main reason being caught stealing groceries in supermarkets. Men are more likely to commit violent crimes. Interestingly, males seem to respond to the reform in terms of an increase in their criminal activity only if they and their partner received residency after the reform, so that both their transfers have been reduced by one half; there

is no response in male crime rates if their own transfer payments remain the same, although transfers on household level are reduced by half.

The reform also affected the children of the target population. About 91% of couples in our sample arrived with children. We show that children who received residency during the preschool age are 25% less likely to be enrolled in Danish daycares and preschools, and 5%-points more likely to end up among the poorest performing students in Danish language test scores. Finally, we find that reductions to family income spark youth crime such as violence or shoplifting, and reduce years of completed schooling. Overall, our results suggest that reforms as the one in question may induce serious consequences for children and their families, and that the costs from these secondary effects may outweigh the surplus and benefits achieved from lower transfer levels and increased labor supply.

Overall, we draw two main conclusions from our analysis. First, the evaluation of reforms of transfer policies such as the one investigated here should not only focus on the primary and intended objectives (i.e. employment take-up and reduction in unemployment), but also ought to consider how incentives change more broadly, whether supply is the only obstacle to employment, and what are the possible secondary effects on for example crime and children's lives. Although the primary objective may be achieved, the effects may be attenuated while the secondary effects may lead to serious consequences that far outweigh the fiscal benefits of reform. Second, when designing policies that target populations that differ from the majority population (in our case refugees from mainly Middle Eastern countries), policy makers need to be aware of different responses due to different preferences and cultural heritage, and avoid design features that may lead to unintended disincentives within households. Inducing one spouse to withdraw from the labor force and any integration measures may seem a shortsighted choice from a Danish perspective, but it could be the preferred option for individuals with a different cultural background and different family roles of females. These insights are particularly relevant today, where many countries (not just in Europe) struggle with the integration of refugee populations, many of the same origin countries than those studied here.

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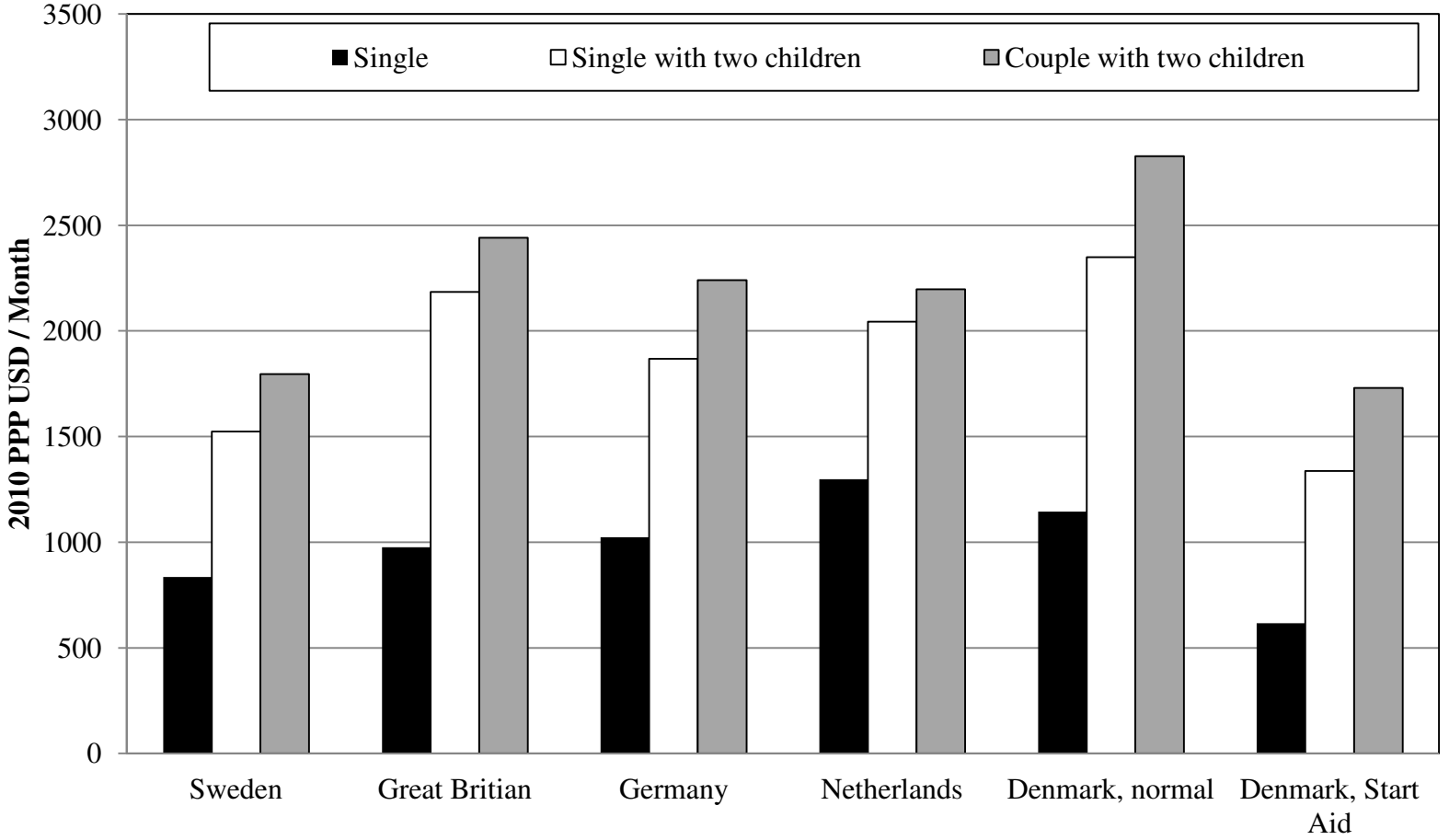
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Figures and Tables from the main text

Figure 1. Social assistance including housing benefits, by country

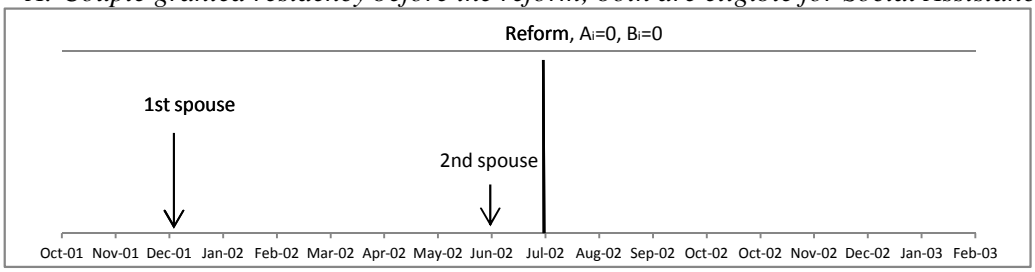


Source: Hansen and Schultz-Nielsen (2015)

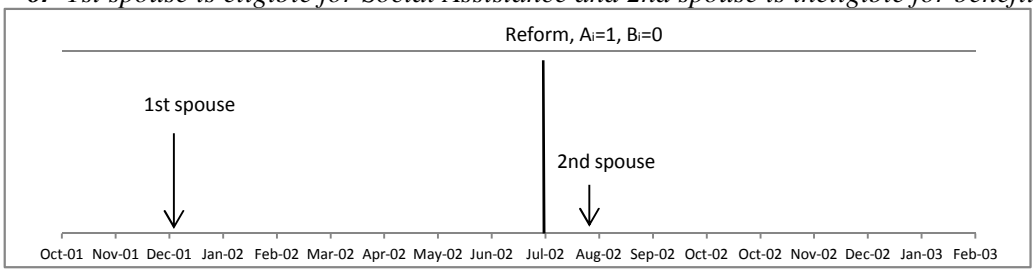
Note: Figure shows Social Assistance levels in different countries for different household types along with the pre-reform Social Assistance and post-reform Start Aid levels in Denmark.

Figure 2: Treament status of couples depending on timing of residency

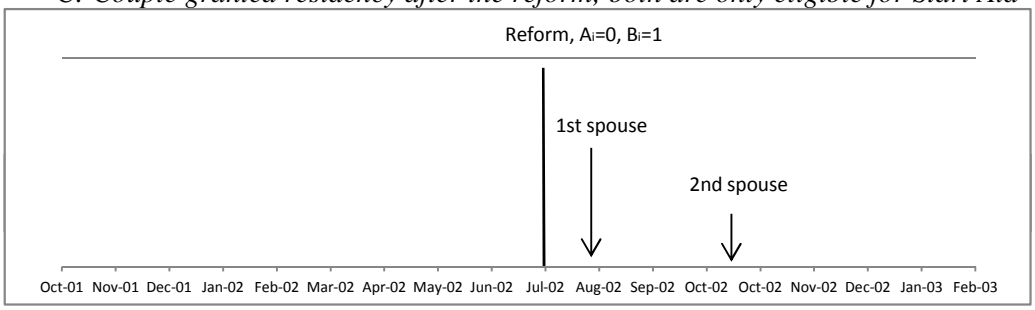
A: Couple granted residency before the reform; both are eligible for Social Assistance



B: Couple granted residency on both sides of the reform; both are either eligible for Start Aid ***or*** *1st spouse is eligible for Social Assistance and 2nd spouse is ineligible for benefits*

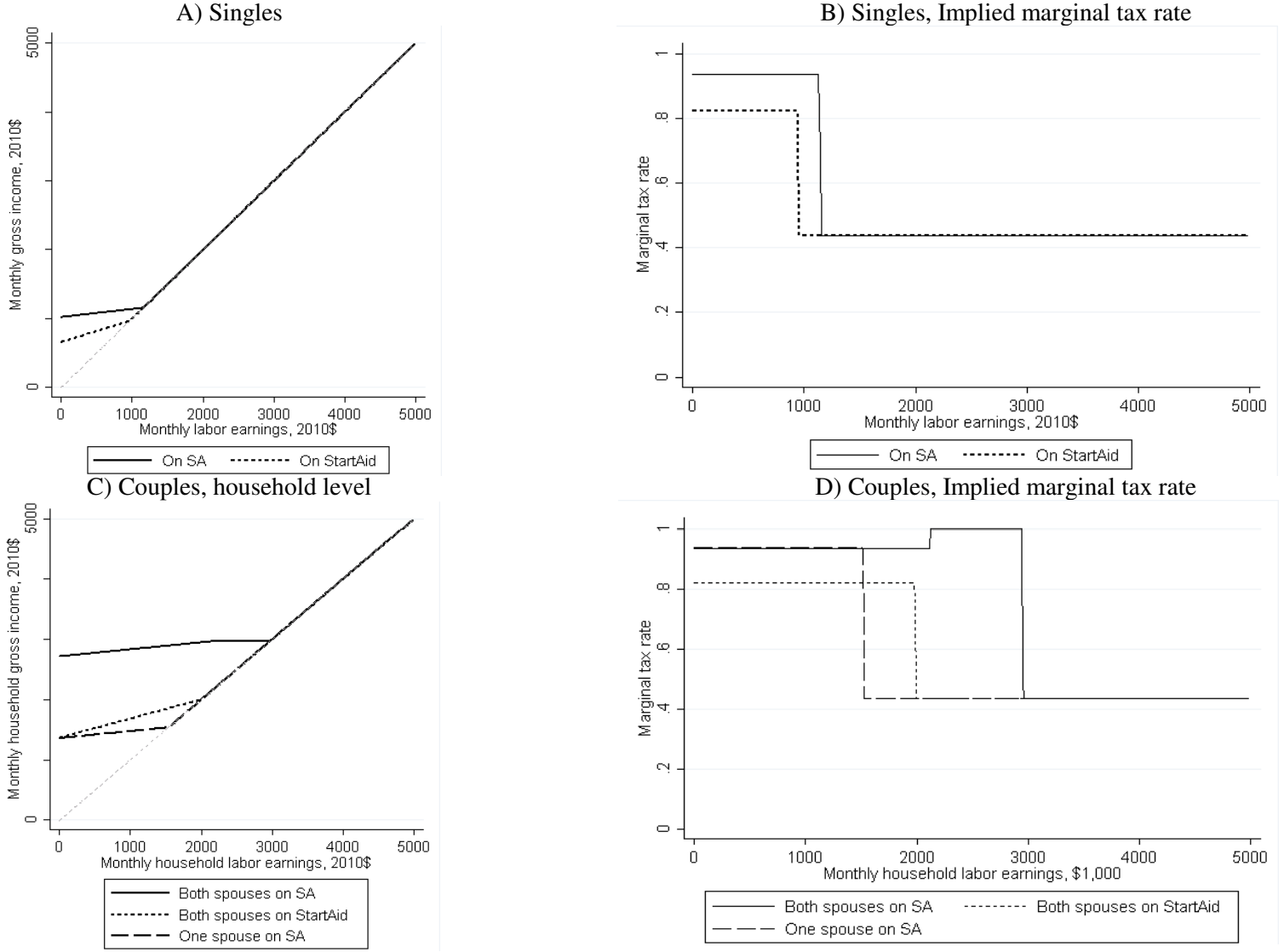


C: Couple granted residency after the reform; both are only eligible for Start Aid



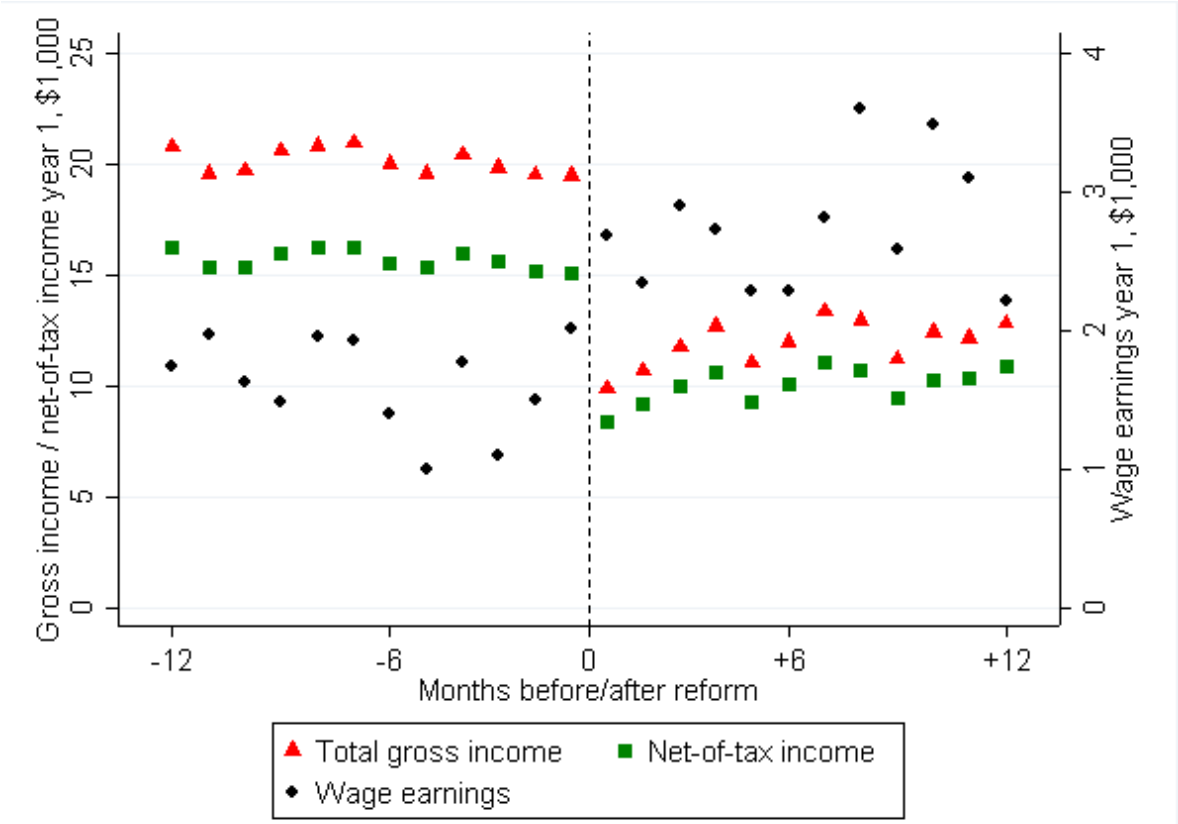
Note: The figure illustrates the three different treatment states depending on the timing of each spouse's residency. In Figure A, both spouses receive residency before the reform and are thus eligible for full pre-reform Social Assistance. Here both of the treatment dummy variables A_i and $B_i = 0$. In Figure B (with the treatment dummy $A_i = 1$) one spouse received residency before the reform and one after. Thus couples could choose between letting the first arrived spouse receive full pre-reform SA or both receive Start Aid. Finally, in Figure C (with the treatment dummy $B_i = 1$) both spouses received residency after the reform and are thus only eligible for Start Aid. This identification is detailed further in Section 3.

Figure 3: Labor earnings, gross income, and implied marginal tax rates from means testing



Note: Figures A and C show the relationship between labor earnings and gross income resulting from means testing, by household types. The solid lines show the pre-reform states and the dashed lines the post-reform states. Beside the change in benefits levels, the slopes differ as a result of varying means testing rates (between 0.8 and 1). B and D show the corresponding marginal tax rates calculated as (1-slope) + τ*slope, where τ is marginal tax at lowest tax bracket of 0.44.

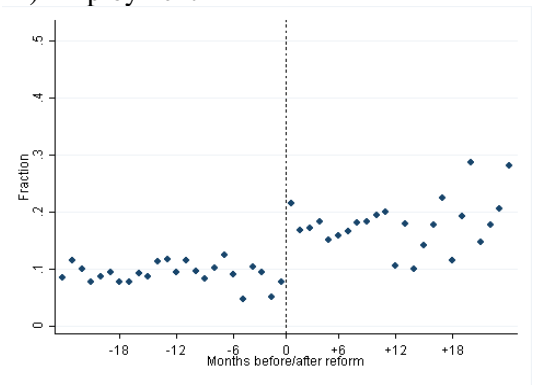
Figure 4. Individual income, the first full year after residency.



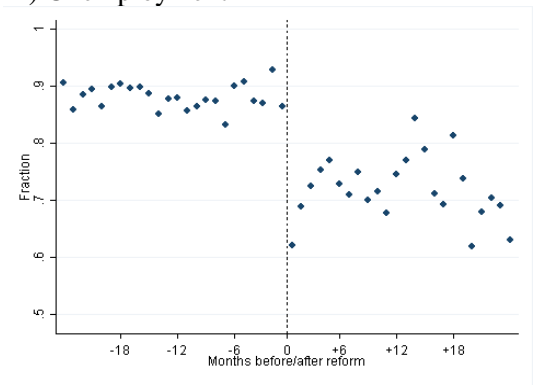
Note: Figure shows individual level transfer income, total gross income and labor earnings by monthly bins of timing of residencyby. The dashed line indicates the timing of the Start Aid introduction in July 2002.

Figure 5. Labor market outcomes first full year after residency.

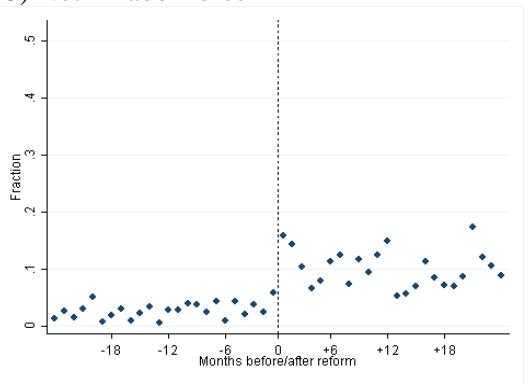
A) Employment



B) Unemployment



C) Not in labor force

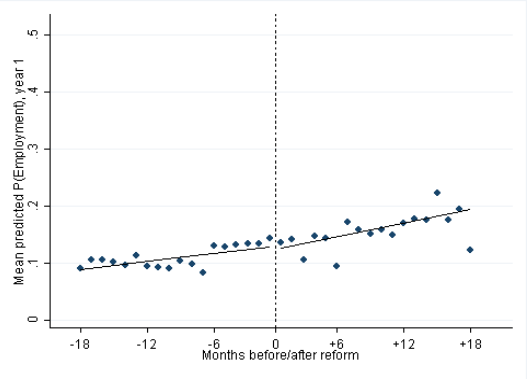


Note: Figure shows average rates of employment (A), unemployment (B), and not in the labor force (C) in the first year following residency by monthly bins. The dashed line indicates the timing of the Start Aid introduction in July 2002.

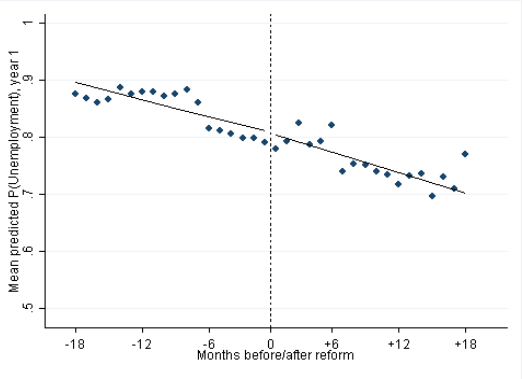
Figure 6. Labor market outcomes and labor earnings 1 year after residency, predicted from background characteristics alone.

Main sample

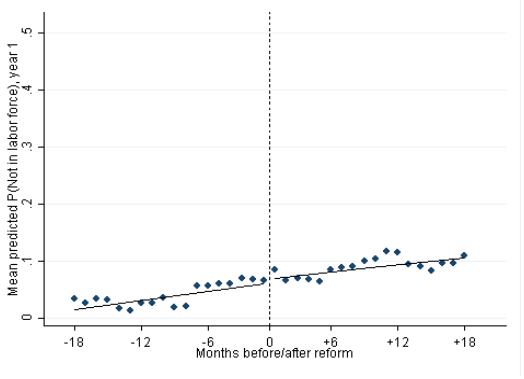
A) Employment



B) Unemployment

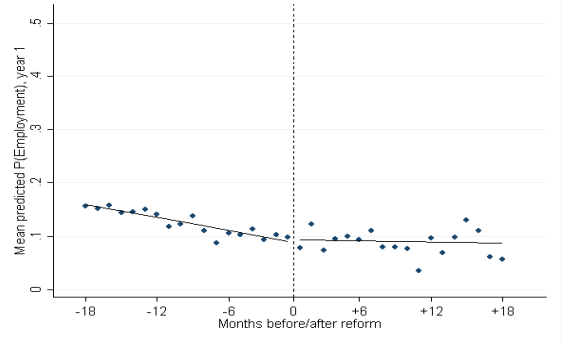


C) Not in labor force

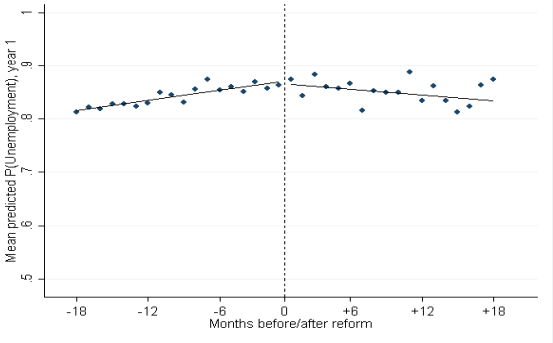


Couples sample

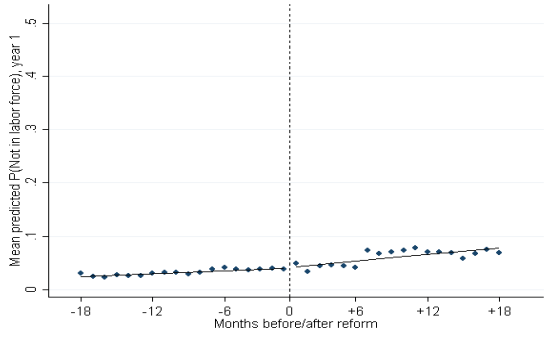
D) Employment



E) Unemployment



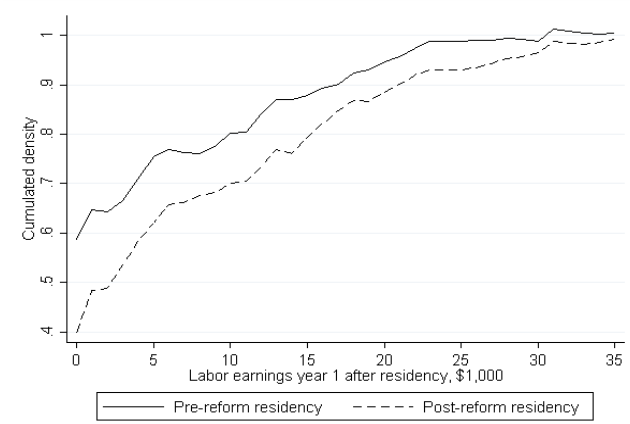
F) Not in labor force



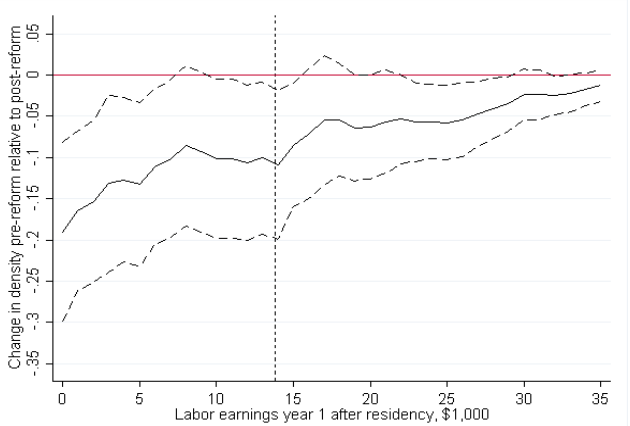
Note: The figure shows levels of labor earnings, employment, unemployment, and not in the labor force in the first year following residency predicted using the full set of covariates. The figure also contains linear slopes of the predictions before and after the timing of the reform, in order to mimic our estimation strategy. Figures A, B, and C show results for the main sample. Figures D, E, and F show results for the couples sample.

Figure 7: Change in labor earnings distribution as result of Start Aid, by household type

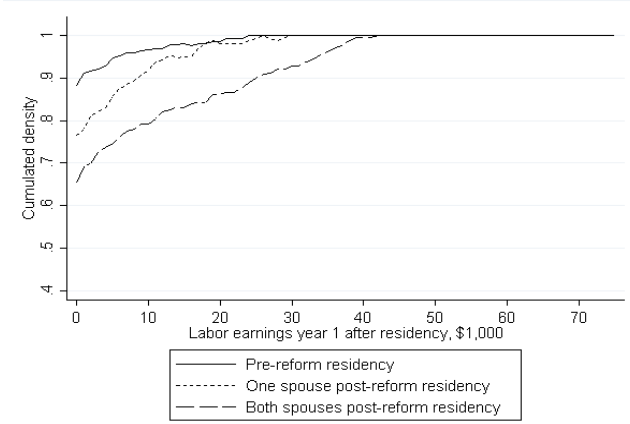
A) Singles, change in cumulated yearly labor earnings distribution



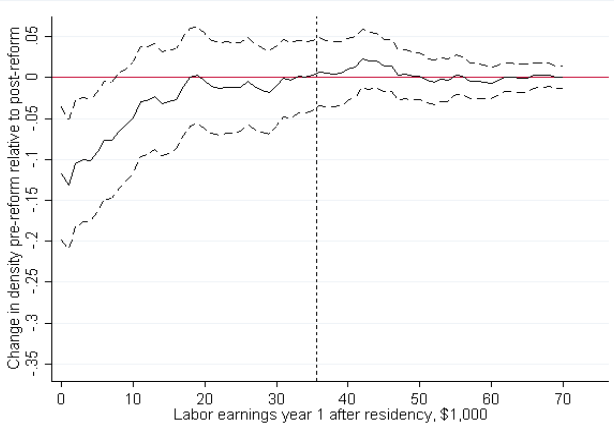
B) Singles, change in density in yearly labor earnings distribution



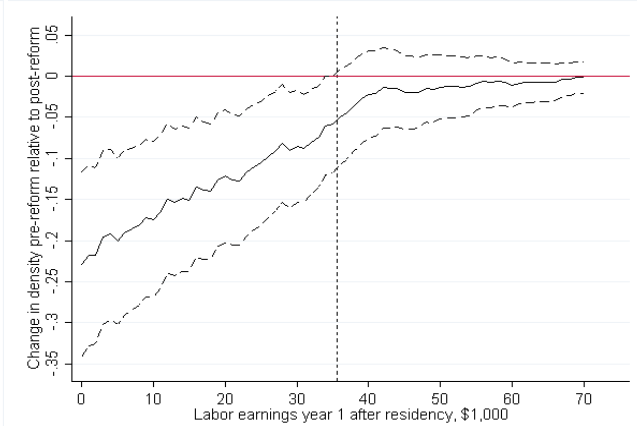
C) Couples, change in cumulated yearly labor earnings distribution



D) Couples change in density in yearly labor earnings distribution, one spouse after reform



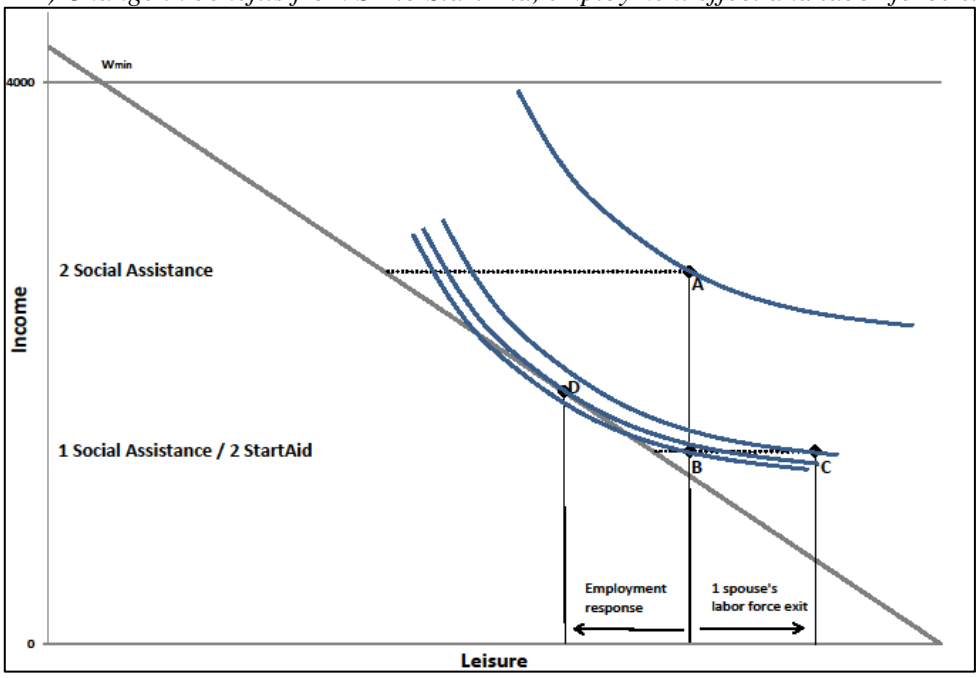
E) Couples, change in density in yearly labor earnings distribution, both after reform



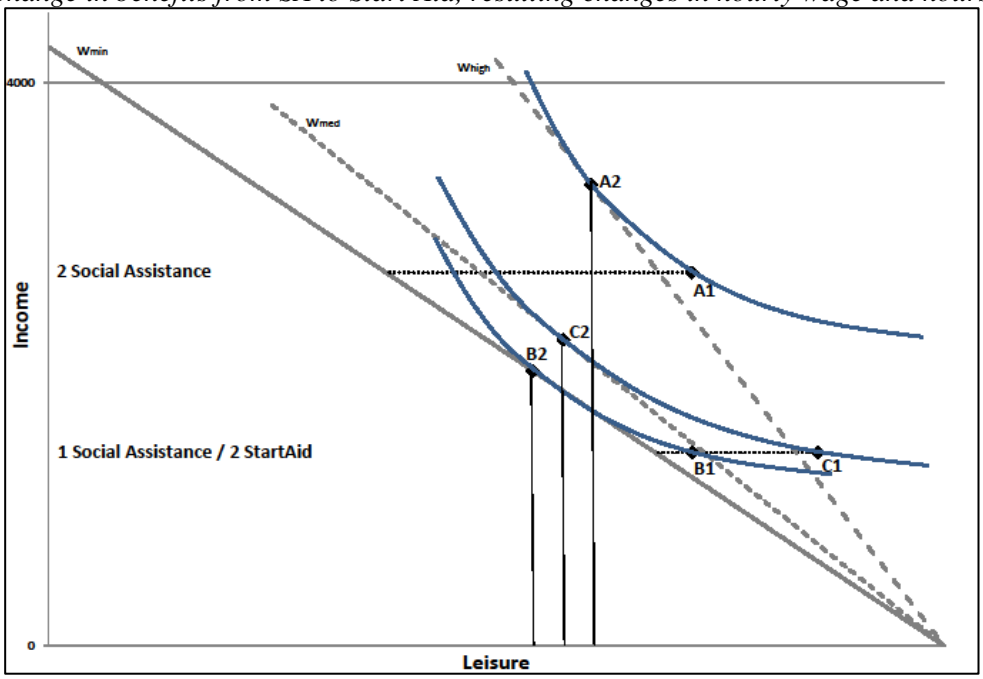
Note: Figure shows the estimated changes in cumulative income distributions for singles and couples on household level in A and C, respectively. B, D, and E show the changes induced by the reform (the vertical differences between the lines in A and C) for singles, couples where one recieve residency after the reform, and couples where both receive recidency after the reform. The dashed vertical line in B marks the income level (break even point) where the increased marginal tax rate from means testing drops to the normal marginal income tax for singles. The dashed vertical line in D and E marks the income level (break even point) where the marginal tax rate from means testing to normal marginal income tax for couples. All income levels are on yearly basis.

Figure 8. Household Budget Constraints and the Impact of the Start Aid Reform, Couples

A) Change in benefits from SA to Start Aid, employment effect and labor force exit



B) Change in benefits from SA to Start Aid, resulting changes in hourly wage and hours worked



Note: Figure illustrates how the reform affects households' behavior treating households as unitary. A) the employment and labor force drop out responses when both spouses and only one received recidency after the reform, respectively. B) why hourly wage and hours worked distributions differ across household.

Figure A: *Point A* shows benefits if both spouses received SA pre-reform. The minimum work required on household level to receive 2*SA is an illustration of both spouses' benefit eligibility being conditional on integration/language course participation etc. *Point B* shows the corresponding benefits after the reform. The horisontal gap between *Points A and B* illustrates the range with marginal tax close to 100% (cf. Figure X). For couples where only one received residency after the reform, it will be optimal to have one spouse drop out of the labor force increasing leisure from *Point B to C*. For couples where both spouses received recidency after the reform Point C is not possible. Instead labor supply is increased until the *Point D*.

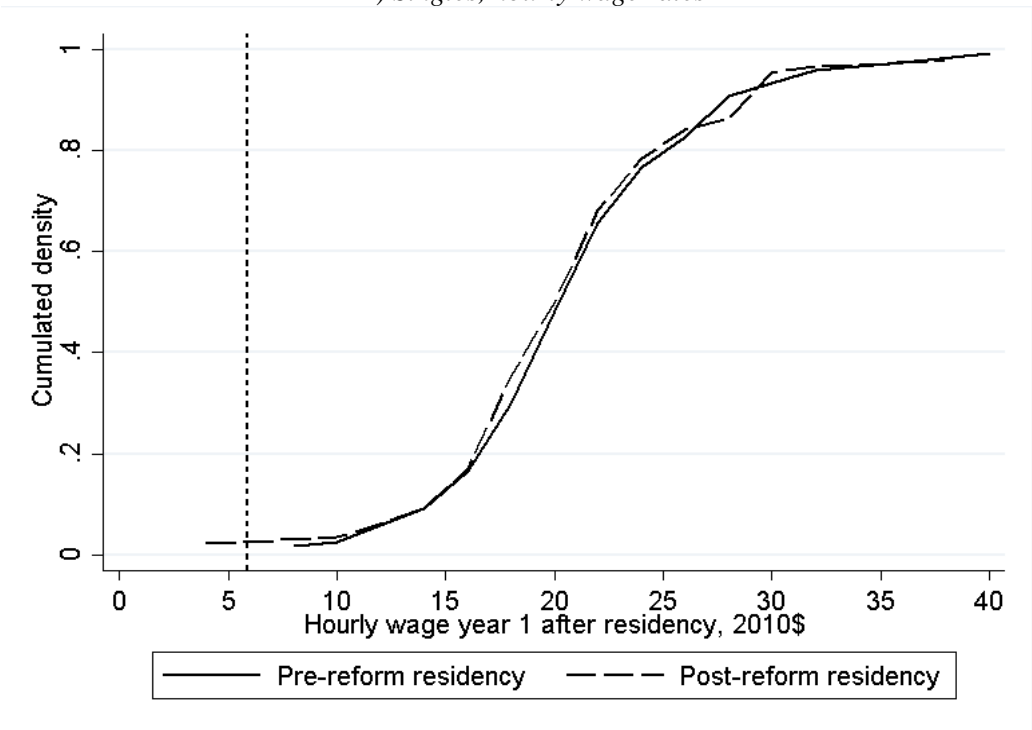
Figure B: *A1, B1, and C1* as above. If both are eligible for SA a high wage is needed for a low labor supply, *A2*. If both are eligible for Start Aid a low wage is needed for a higher labor supply, *B2*. If one exits the labor force without affecting benefits a medium wage is needed for an intermediate labor supply, *C2*.

Figure 9: Change in distributions of hourly wage rates as result of reform, by household type, year 1

A) Males in couples, hourly wage rates



B) Singles, hourly wage rates



Note: Figure shows the distributions of hourly wage rates. Figure A shows the estimated effects using Equation (2) and as outcomes dummy variables indicating a wage rate <= each level from 0 to 40. Figure B shows distribution for the full pre- and post-reform groups and not estimated using the discontunity around the reform due to too few observations. The dashed vertical lines mark the equivalent post-reform break even points; in A for couples on household level (which also coincides almost with the collective bargain minimum wage of $13.3) and in B for singles.

Table 1. Levels of Social Assistance to Refugees by Residency Before / After the Reform.

Status	Age	Children	Before Reform	After reform	Pct. Reduction
Spouse	>= 25	0	1,020	545	47
Spouse	>= 25	1	1,356	682	50
Spouse	>= 25	>= 2	1,356	818	40
Single	>= 25	0	1,020	658	36
Single	>= 25	1	1,356	822	39
Single	>= 25	>= 2	1,356	986	27
Own residence	< 25	0	658	545	17
Spouse	< 25	1	1,356	682	50
Spouse	< 25	>= 2	1,356	818	40
Single	< 25	1	1,356	710	48
Single	< 25	>= 2	1,356	874	36
Parent's residence	< 25	0	317	271	15

Note: The table shows how different demographic types are affected by the reform. All amounts are reported in 2010 PPP-adjusted USD. Young refugees without children are affected the least as they were already entitled to comparatively low levels of SA before the reform. All other groups are entitled to at least 25% lower SA after the reform. Note that couples are affected the most (up to 50% lower SA).

Table 2. Sample Means of Covariates, Main Sample Aged 18-55 at Residency and Couples Sample.

	A) Main sample			*B) Couples sample*				
	All	Pre-reform residency	Post-reform residency	All	Pre-reform residency	Post-reform residency	Males	Females
Reform=1	0.371	0.000	1.000	0.270	0.000	1.000	0.163	0.378
	(0.483)	(0.000)	(0.000)	(0.444)	(0.000)	(0.000)	(0.369)	(0.485)
Age at residency	32.625	32.700	32.498	33.294	33.238	33.446	35.403	31.186
	(8.270)	(8.311)	(8.202)	(8.538)	(8.775)	(7.862)	(8.556)	(7.983)
1 or more siblings	0.017	0.014	0.022	0.002	0.003	0.000	0.001	0.002
	(0.129)	(0.118)	(0.146)	(0.044)	(0.052)	(0.000)	(0.038)	(0.050)
Female	0.507	0.475	0.560	0.500	0.426	0.699	0.000	1.000
	(0.500)	(0.499)	(0.496)	(0.500)	(0.495)	(0.459)	(0.000)	(0.000)
# of children	2.257	2.346	2.106	2.411	2.414	2.405	2.411	2.411
	(1.903)	(1.931)	(1.847)	(1.265)	(1.278)	(1.232)	(1.265)	(1.265)
Single	0.246	0.226	0.279	-	-	-	-	-
	(0.431)	(0.418)	(0.449)					
Muslim countries	0.838	0.878	0.769	0.843	0.859	0.799	0.844	0.841
	(0.369)	(0.327)	(0.422)	(0.364)	(0.348)	(0.401)	(0.363)	(0.365)
Iraq	0.520	0.582	0.416	0.546	0.562	0.505	0.554	0.539
	(0.500)	(0.493)	(0.493)	(0.498)	(0.496)	(0.500)	(0.497)	(0.499)
Iran	0.071	0.045	0.115	0.061	0.050	0.092	0.060	0.062
	(0.257)	(0.208)	(0.319)	(0.240)	(0.218)	(0.289)	(0.237)	(0.242)
Eastern Europe/former USSR	0.055	0.050	0.062	0.042	0.035	0.062	0.034	0.050
	(0.227)	(0.219)	(0.241)	(0.201)	(0.184)	(0.241)	(0.182)	(0.218)
Rest of the world	0.108	0.071	0.170	0.115	0.106	0.139	0.121	0.109
	(0.310)	(0.257)	(0.375)	(0.319)	(0.308)	(0.346)	(0.327)	(0.311)
Refugee permit status	0.618	0.635	0.588	0.527	0.590	0.387	0.847	0.257
	(0.486)	(0.482)	(0.492)	(0.499)	(0.492)	(0.487)	(0.360)	(0.437)
First arriving spouse in couple	-	-	-	0.500	0.601	0.228	0.862	0.138
				(0.500)	(0.490)	(0.420)	(0.344)	(0.344)
Observations	4,843	3,044	1,799	4,072	2,972	1,100	2,036	2,036

Note: The table shows sample means for the main sample of adults receiving recidency +/- 18months around the reform and the balanced couples sample of all couples where at least one spouse received residency +/- 18months around the reform. '# of children' refers to the number of children upon residency. 'Muslim countries' refers to countries where the majority are muslim and not the individual's faith. 'Refugee' refers to the indiviual receving recidency based on refugee status (was residency given on grounds of being a refugee 1, or from being the spouse / child of an individual with refugee status, 0). 'First arriving spouse' likewise refers to being the first spouse in the couple who is granted residency in Denmark. A minor share of spouses do not have the same country of origin. This is almost exclusively couples with origin in two neighboring countries (with loosely enforced borders, low population density) as for example Somalia, Eritrea. 'Single' is left out of the couples sample summary as no one is single by construction. First arriving spouse is left out of the main sample summary, as this is only relevant in the couples sample.

Table 3. Summary of Outcome Variables Following Residency.

Panel A: Income				
Years since residency	Transfers	Labor earnings	Total gross income	Net-of-tax income
1	15.105	2.081	17.391	13.810
	(8.416)	(6.633)	(8.564)	(6.445)
2	14.917	4.834	19.971	15.664
	(9.094)	(10.979)	(10.184)	(7.184)
3-5	13.552	9.606	23.582	17.790
	(8.777)	(13.879)	(10.919)	(6.969)
Observations	4,843	4,843	4,843	4,843
Panel B: Labor market status				
Years since residency	Employment	Unemployment	Not in labor force	Dropped out
1	0.124	0.821	0.053	0.044
	(0.330)	(0.383)	(0.225)	(0.206)
2	0.220	0.697	0.080	0.062
	(0.415)	(0.460)	(0.272)	(0.240)
3-5	0.362	0.381	0.255	0.068
	(0.400)	(0.353)	(0.336)	(0.190)
Observations	4,843	4,843	4,843	4,843
Panel C: Share convicted of crime accumulated from time of residency (age 15-55)				
Years since residency	All crimes	Property crimes	Violent crimes	
1	0.024	0.020	0.004	
	(0.153)	(0.139)	(0.064)	
2	0.045	0.036	0.009	
	(0.208)	(0.187)	(0.094)	
3-5	0.083	0.064	0.019	
	(0.276)	(0.244)	(0.136)	
Observations	5,059	5,059	5,059	
Panel D: Number of crimes accummulated from time of residency (age 15-55)				
Years since residency	All crimes	Property crimes	Violent crimes	
1	0.029	0.025	0.004	
	(0.212)	(0.198)	(0.064)	
2	0.057	0.046	0.009	
	(0.300)	(0.275)	(0.097)	
3-5	0.121	0.093	0.022	
	(0.522)	(0.458)	(0.169)	
Observations	5,059	5,059	5,059	

Note: The table shows summary statistics for the main outcomes in year 1, 2, and the average of year 3-5 since residency with average levels and standard deviations in parentheses. Panel A shows transfer income, labor earnings, total gross income, and net-of-tax income measured on an annual basis. Panel B shows the corresponding for labor market outcomes: Employment, unemployment, not in the labor force (NILF), and dropped out of welfare system (a subset of NILF). Panel C shows the share convicted of a crime for all crimes and then by type of crime. Panel D shows the number of crime convictions for all crimes and then by type of crime. Crime is measured using the exact date of crime relative to the timing of residency and is reported as the accumulated crime level from residency (and not year-by-year as income and labor market outcomes in Panels A and B).

Table 4. Conditional Balancing Test of Covariates Across Reform.

	A) Main sample	*B) Full sample*	*C) Couples sample*
Age at residency	-0.000	-0.000	-0.002**
	(0.001)	(0.001)	(0.001)
Any siblings	0.007	-0.007	-
	(0.030)	(0.022)	
Female	0.015	0.005	0.014
	(0.010)	(0.006)	(0.024)
# of children	-0.007	-	-0.010*
	(0.004)		(0.006)
Single	0.007	-	-
	(0.018)		
Spouse present in Denmark	0.016	-	-
	(0.016)		
Eastern Europe/former USSR	-0.036	(0.027)	-0.043
	(0.028)	-0.015	(0.038)
Rest of the world	-0.013	(0.023)	0.011
	(0.025)	-0.001	(0.041)
Refugee permit status	-0.003	(0.016)	0.023
	(0.018)	(0.057)	(0.037)
First residency in couple	-	-	-0.008
			(0.030)
F	1.010	0.545	1.145
P(F)	0.451	0.794	0.359
Observations	4,843	8,142	4,076
Running variable	X	X	X

Note: Panels A and B show full regression results and F-tests of conditional balancing of covariates across the reform. Panels A and B show results from regressing a dummy indicating whether residency was granted before or after the reform on all covariates and the running variable for the adult sample (age 18-55) and the full sample including children. Panel C shows results from regressing a dummy indicating whether the spouse was granted residency before or after the reform on all covariates and the running variable. I.e. the table reports the individual γ's and a F-test for joint significance of the γ's from the regression:

$reform(0/1) = X'\gamma + f(Z)'\pi$

with standard errors in parentheses. Covariates include age at residency, whether the refugee had any siblings (except for couples in Panel C due to lack of variation in siblings across spouses), gender, number of children (except for Panel B as this sample includes children), marital status (except for Panel B as this sample includes children), spouse present in Denmark (except for Panel B as this sample includes children), country of origin (Eastern Europe/former USSR, predominantly muslim countries is reference category, rest of world), and refugee permit status (was residency given on grounds of being a refugee 1, or from being the spouse / child of an individual with refugee status, 0). The covariates for Panel C also include a dummy for whether spouse was the

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

Table 5. Effect of Reform on Subsequent Income per Year (Log Income).

	A) Total gross income			*B) Total net-of-tax (disposable) income*		
Years since residency						
1-5	**-0.382*** **	**-0.344*** **	**-0.343*** **	**-0.331*** **	**-0.296*** **	**-0.294*** **
	(0.042)	**(0.035)**	**(0.032)**	**(0.038)**	**(0.031)**	**(0.028)**
Observations	4,843	4,843	4,843	4,843	4,843	4,843
Running variable	X	X	X	X	X	X
Covariates		X	X		X	X
Year Fixed Effects			X			X

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on log of subsequent total gross income, and net-of-tax income measured on individual level for the full sample of adults age 18-55 on the time of residency in year 1-5 since residency.

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

Table 6. Effect of Reform on Subsequent Annual Individual Income by Income Type (Measured in USD 1,000).

	A) Transfers		*B) Labor earnings*		*C) Total gross income*		*D) Total net-of-tax (disposable) income*	
Years since residency	Pre-reform mean	Estimate	Pre-reform mean	Estimate	Pre-reform mean	Estimate	Pre-reform mean	Estimate
1	18.431	**-9.775*** **	1.852	**1.144*** **	20.524	**-8.530*** **	15.941	**-6.265*** **
	(7.663)	**(0.407)**	(6.353)	**(0.400)**	(7.071)	**(0.431)**	(5.574)	**(0.330)**
2	17.979	**-8.320*** **	4.182	**1.567*** **	22.387	**-6.748*** **	17.311	**-4.820*** **
	(8.655)	**(0.446)**	(10.401)	**(0.541)**	(8.733)	**(0.637)**	(6.392)	**(0.482)**
3-5	15.849	**-4.956*** **	8.424	**1.070** **	24.752	**-4.163*** **	18.630	**-2.988*** **
	(8.760)	**(0.457)**	(13.273)	**(0.451)**	(9.593)	**(0.549)**	(6.205)	**(0.389)**
Observations	3,044	4,843	3,044	4,843	3,044	4,843	3,044	4,843
Running variable		X		X		X		X
Covariates		X		X		X		X
Year Fixed Effects		X		X		X		X

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on subsequent transfer income, labor earnings, total gross income, and net-of-tax income measured on individual level for the full sample of adults age 18-55 on the time of residency in year 1, 2, and the average of year 3-5 since residency. The table also shows pre-reform means of the outcome variables. Covariates include age at residency, gender, number of siblings in Denmark, number of children, marital status, country of origin, and resident permit status.
* p<0.1; ** p<0.05; *** p<0.01

Table 7. Effect of Reform on Subsequent Labor Market Outcomes.

	A) Employment rate		*B) Unemployment rate*		*C) Not in labor force rate*	
Years since residency	Pre-reform mean	Estimate	Pre-reform mean	Estimate	Pre-reform mean	Estimate
1	0.103	**0.092*** **	0.868	**-0.164*** **	0.028	**0.072*** **
	(0.305)	**(0.022)**	(0.339)	**(0.027)**	(0.164)	**(0.014)**
2	0.188	**0.070*** **	0.755	**-0.158*** **	0.053	**0.093*** **
	(0.391)	**(0.019)**	(0.430)	**(0.028)**	(0.224)	**(0.020)**
3-5	0.323	**0.041***	0.417	**-0.069*** **	0.257	**0.031***
	(0.390)	**(0.021)**	(0.359)	**(0.018)**	(0.334)	**(0.017)**
Observations	3,044	4,843	3,044	4,843	3,044	4,843
Running variable		X		X		X
Covariates		X		X		X
Year Fixed Effects		X		X		X

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on subsequent probability of being in employment, unemployed, or not in the labor force for the main sample of adults age 18-55 on the time of residency in year 1, 2, and the average of year 3-5 since residency. The table also shows pre-reform means of the outcome variables. Covariates include age at residency, gender, number of siblings in Denmark, number of children, marital status, country of origin, and resident permit status.

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

Table 8. Effect of Reform on Labor Market Outcomes, by Gender.

Years	A) Employment rate			B) Unemployment rate			C) Not in labor force rate			D) Dropped out		
since residency	All	Males	Females	All	Males	Females	All	Males	Females	All	Males	Females
1	**0.092*** **	**0.160*** **	0.037	**-0.164*** **	**-0.155*** **	**-0.171*** **	**0.072*** **	-0.004	**0.132*** **	**0.070*** **	0.002	**0.126*** **
	(0.022)	**(0.044)**	(0.022)	**(0.027)**	**(0.045)**	**(0.022)**	**(0.014)**	(0.012)	**(0.021)**	**(0.014)**	(0.010)	**(0.021)**
2	**0.070*** **	**0.132*** **	0.015	**-0.158*** **	**-0.137*** **	**-0.172*** **	**0.093*** **	0.011	**0.162*** **	**0.096*** **	0.020	**0.159*** **
	(0.019)	**(0.035)**	(0.019)	**(0.028)**	**(0.038)**	**(0.029)**	**(0.020)**	(0.023)	**(0.025)**	**(0.019)**	(0.017)	**(0.025)**
3-5	**0.041***	0.042	**0.041** **	**-0.069*** **	**-0.051** **	**-0.079** **	**0.031***	0.012	**0.043***	**0.060*** **	0.013	**0.096*** **
	(0.021)	(0.033)	**(0.016)**	**(0.018)**	**(0.021)**	**(0.029)**	**(0.017)**	(0.030)	**(0.024)**	**(0.010)**	(0.012)	**(0.018)**
Observations	4,843	2,390	2,453	4,843	2,390	2,453	4,843	2,390	2,453	4,843	2,390	2,453
Running variable	X	X	X	X	X	X	X	X	X	X	X	X
Covariates	X	X	X	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X	X	X	X

Note: The table shows the estimated effects, by gender, of being granted residency after the reform relative to before the reform on subsequent probability of being in employment, unemployed, not in the labor force and dropped out of the welfare system completely for the main sample of adults age 18-55 on the time of residency in year 1, 2, and the average of year 3-5 since residency. Covariates include age at residency, gender, number of siblings in Denmark, number of children, marital status, country of origin, and resident permit status.

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

Table 9. Effect of Reform on Subsequent Labor Market Outcomes, Spouse Present in Denmark at Residency, by Gender and Household Type.

Treatment	Year since residency	Employment	Unemployment	NILF	Dropped out
A) Couples, both granted residency after reform					
Males	1	**0.153*** **	**-0.202*** **	0.049	-0.006
		(0.043)	**(0.050)**	(0.037)	(0.026)
	2	**0.095***	**-0.134** **	0.039	0.011
		(0.050)	**(0.058)**	(0.046)	(0.030)
Females	1	**0.079***	**-0.169*** **	**0.090** **	**0.084*** **
		(0.043)	**(0.050)**	**(0.037)**	**(0.026)**
	2	0.081	**-0.110***	0.031	**0.058***
		(0.050)	**(0.058)**	(0.046)	**(0.030)**
Observations		2,036	2,036	2,036	2,036
B) Couples, one granted residency after reform					
Males	1	**0.075** **	**-0.101*** **	0.025	0.020
		(0.031)	**(0.036)**	(0.027)	(0.019)
	2	**0.107*** **	**-0.137*** **	0.029	0.026
		(0.036)	**(0.042)**	(0.033)	(0.022)
Females	1	0.031	**-0.204*** **	**0.173*** **	**0.154*** **
		(0.031)	**(0.036)**	**(0.027)**	**(0.019)**
	2	0.036	**-0.209*** **	**0.176*** **	**0.198*** **
		(0.036)	**(0.042)**	**(0.033)**	**(0.022)**
Observations		2,036	2,036	2,036	2,036
C) Singles (males and females)					
	1	**0.150*** **	**-0.163*** **	0.016	0.015
		(0.042)	**(0.044)**	(0.019)	(0.015)
	2	**0.138*** **	**-0.200*** **	**0.065** **	**0.059*** **
		(0.048)	**(0.050)**	**(0.026)**	**(0.021)**
Observations		1,454	1,454	1,454	1,454

Note: The table shows the estimated effects of being granted residency after the reform on labor market outcomes by family type. Standard errors in parentheses. Panel A shows, by gender, the effects of both spouses being granted residency after the reform relative both spouses being granted residency before the reform on subsequent probability of being in employment, unemployed, or not in the labor force, and/or not in the labor force for unspecified reasons for a balanced sample of couples in year 1 and 2 since last arriving spouse's residency. Panel B shows the same for couples where only one spouses was granted residency after the reform relative to both spouses being granted residency before the reform. Panel C shows the effects for singles in year 1 and 2 since residency for males and females together due to the smaller sample size, relative to singles being granted residency before the reform. Results in panels A and B are estimated as outlined in Equation (2) and Panel C estimated as outlined in Equation (1).

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

Table 10. Effect of Reform on Couples' Annual Income, by Gender and Household Type.

Treatment	Year since residency	Transfers	Labor earnings	Gross income	Net-of-tax income
A) Individual level income in couples, both granted residency after reform					
Males	1	**-9.983*** **	**4.322*** **	**-5.287*** **	**-3.151*** **
		(0.930)	**(1.112)**	**(1.027)**	**(0.756)**
	2	**-6.804*** **	**2.660***	**-3.587*** **	**-1.403***
		(0.994)	**(1.451)**	**(1.183)**	**(0.798)**
	3	**-4.219*** **	1.759	-2.140	-0.383
		(1.028)	(1.681)	(1.325)	(0.848)
Females	1	**-9.175*** **	0.736	**-8.198*** **	**-5.437*** **
		(0.930)	(1.112)	**(1.027)**	**(0.756)**
	2	**-7.668*** **	1.284	**-6.200*** **	**-3.850*** **
		(0.994)	(1.451)	**(1.183)**	**(0.798)**
	3	**-6.705*** **	1.570	**-4.989*** **	**-3.259*** **
		(1.028)	(1.681)	**(1.325)**	**(0.848)**
B) Individual level income in couples, one granted residency after reform					
Males	1	**-1.444** **	1.308	0.080	**1.328** **
		(0.672)	(0.803)	(0.742)	**(0.546)**
	2	-0.567	1.482	**1.515***	**2.723*** **
		(0.718)	(1.048)	**(0.855)**	**(0.576)**
	3	-0.465	1.852	**1.588***	**2.250*** **
		(0.743)	(1.214)	**(0.957)**	**(0.612)**
Females	1	**-11.439*** **	-0.088	**-11.599*** **	**-8.177*** **
		(0.672)	(0.803)	**(0.742)**	**(0.546)**
	2	**-11.414*** **	1.044	**-10.148*** **	**-7.010*** **
		(0.718)	(1.048)	**(0.855)**	**(0.576)**
	3	**-9.606*** **	0.952	**-8.326*** **	**-5.729*** **
		(0.743)	(1.214)	**(0.957)**	**(0.612)**
C) Household income in couples, both granted residency after reform					
	1	**-19.159*** **	**5.058*** **	**-13.485*** **	**-8.589*** **
		(1.458)	**(1.721)**	**(1.382)**	**(1.010)**
	2	**-14.472*** **	**3.944***	**-9.787*** **	**-5.253*** **
		(1.593)	**(2.336)**	**(1.637)**	**(1.058)**
	3	**-10.924*** **	3.330	**-7.129*** **	**-3.642*** **
		(1.645)	(2.668)	**(1.862)**	**(1.125)**
D) Household income in couples, one granted residency after reform					
	1	**-12.883*** **	1.220	**-11.519*** **	**-6.849*** **
		(1.053)	(1.243)	**(0.998)**	**(0.730)**
	2	**-11.981*** **	2.526	**-8.633*** **	**-4.287*** **
		(1.150)	(1.687)	**(1.182)**	**(0.764)**
	3	**-10.070*** **	2.804	**-6.738*** **	**-3.479*** **
		(1.188)	(1.927)	**(1.345)**	**(0.813)**

Note: The table shows the estimated effects of the reform on subsequent transfer income, labor earnings, total gross income, and net-of-tax income (USD 1,000) in year 1, 2, and 3 since last arriving spouse's residency. Standard errors in parentheses. Panel A shows the effects of both spouses receiving residency after the reform relative both spouses receiving residency before the reform on individual income. Panel B shows the same for individuals in couples where only one spouses received residency after the reform relative to both spouses receiving residency before the reform. Panel C shows effect on household income as in Panel A. Panel D shows effect on household income as in Panel B. Observations: 4036 Panels A and B, 2036 Panels C and D.

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

Table 11. Effect of Reform on Tranfers Income and Likelihood of Not Being in the Labor Force and Out of Social Welfare System for Married Refugees and their Spouses, by Years after Residency of Last Arriving Spouse.

		(1)	*(2)*	*(3)*	*(4)*
		Dropped out, actual		*Dropped out, corrected for means testing*	
Treatment	Years since residency	Males	Females	Males	Females
A) Couples, both granted residency after reform					
	1	-0.006	**0.084*** **	-0.004	0.019
		(0.026)	**(0.026)**	(0.019)	(0.019)
	2	0.011	**0.058***	0.013	0.027
		(0.030)	**(0.030)**	(0.021)	(0.021)
	3	0.009	**0.111*** **	-0.010	0.011
		(0.031)	**(0.031)**	(0.020)	(0.020)
Observations		2,036	2,036	2,036	2,036
B) Couples, one granted residency after reform					
	1	0.020	**0.154*** **	0.013	**0.086*** **
		(0.019)	**(0.019)**	(0.013)	**(0.013)**
	2	0.026	**0.198*** **	0.019	**0.086*** **
		(0.022)	**(0.022)**	(0.015)	**(0.015)**
	3	0.007	**0.158*** **	0.007	**0.090*** **
		(0.022)	**(0.022)**	(0.015)	**(0.015)**
Observations		2,036	2,036	2,036	2,036

Note: The table shows the estimated effects, by gender, of the reform on subsequent probability of having dropped out of the welfare system completely in year 1, 2, and 3 since last arriving spouse's residency. Standard errors in parentheses. 'Dropped out, actual' shows results for observed Dropped out rates in the data. 'Dropped out, corrected for means testing' sets levels to 0 if the individual or their spouse are recorded as being in employment during that year (and thus likely to cap transfer-levels by means testing). Panel A shows, by gender, the effects of both spouses being granted residency after the reform relative both spouses being granted residency before the reform. Panel B shows the same for couples where only one spouses was granted residency after the reform relative to both spouses being granted residency before the reform.

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

Table 12. Effect of Reform on Subsequent Criminal Convictions (Accumulated since Residency), Household Type and Gender.

Treatment	Years since residency	Probability of crime conviction		Number of crime convictions	
		Pre-reform mean	Estimate	Pre-reform mean	Estimate
A) Couples, both granted residency after reform					
Males	1	0.025	**0.041****	0.028	**0.043***
		(0.157)	**(0.018)**	(0.179)	**(0.023)**
Females	1	0.006	**0.035****	0.008	**0.046****
		(0.077)	**(0.018)**	(0.112)	**(0.023)**
Observations		4,072	4,072	4,072	4,072
B) Couples, one granted residency after reform					
Males	1	0.025	-0.002	0.028	-0.003
		(0.157)	(0.013)	(0.179)	(0.016)
Females	1	0.006	0.018	0.008	0.024
		(0.077)	(0.013)	(0.112)	(0.016)
Observations		4,072	4,072	4,072	4,072

Note: The table shows the estimated effect of the reform, by gender, on subsequent probability of crime conviction (P(crime)) and number of subsequent crime convictions (#crimes) during the first year following residency. Effects are estimated using the exact date of crime counting 1 year since the last arriving spouse's date of residency. Standard errors in parentheses. Panel A shows, by gender, the effects of both spouses being granted residency after the reform relative both spouses being granted residency before the reform. Panel B shows the same for couples where only one spouse was granted residency after the reform relative to both spouses being granted residency before the reform. Pre-reform mean refers to mean for couples where both spouses received residency before the reform.

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

Table 13: Effect of Reform on Children Age 0-5 at Residency, Probability of Enrolmed in Daycare or Pre-school, and Danish Test Scores During School-Ages

Outcome	Pre-reform mean	Estimate	Observations
A) P(Daycare / prescool use)			
Year 1 since residency	0.558	**-0.157****	950
	(0.497)	**(0.058)**	
Year 2 since residency	0.679	**-0.144****	676
	(0.468)	**(0.066)**	
B) Test scores, Danish, first recorded test following residency			
Test scores, standardized relative to national average	-0.676	0.056	875
	(0.953)	(0.122)	
P(2.5sd below national average or lower)	0.030	**0.052****	875
	(0.171)	**(0.022)**	
P(1sd below national average or lower)	0.313	-0.008	875
	(0.464)	(0.056)	
P(at national average or lower)	0.776	-0.014	875
	(0.417)	(0.060)	

Note: The table shows the estimated effects of being granted residency after the reform relative to the individual being granted residency before the reform for children between age 0 and 5 at time of residency on A) subsequent probability of child being enrolled in daycare/preschool in year 1 and 2 since residency, and B) test scores in the national test in Danish schools, and corresponding probabilities of falling at or below 2.5 standard deviations below the national average, at or below 1 standard deviation below the national average, or at or below the national average. Test scores are measured at different grades-levels as children's time of residency and age at residency vary, and the national test scores are only available from 2009 and onwards. 19% are measured in 2nd grade, 41% in 4th grade, 33% in 6th grade, and 8% in 8th grade. Not all children have recorded test scores (due to absence at the day of test or special needs school / migrant class enrollment). Test for significant differences in missing test scores around the reform: P-value=0.223. Results are robust to limiting the sample to test scores measured in grades 2, 4, or grades 2, 4, and 6, or limiting the sample to individuals age 0-4 at time of residency.

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

Table 14. Effect of Reform on Children's Overall Years of Schooling.

	Pre-reform mean	A) Mean effect	B) Males	C) Females
Reform, all	11.44	-0.093	-0.171	-0.005
	(1.75)	(0.103)	(0.180)	(0.169)
Reform, age at residency<10	11.10	0.083	0.157	0.039
	(1.41)	(0.138)	(0.190)	(0.126)
Reform, age at residency>=10	12.27	**-0.647****	**-0.897*****	-0.270
	(2.17)	**(0.267)**	**(0.292)**	(0.422)

Note: Table shows estimated effects of being granted residency after the reform on years of completed schooling measured in 2016 for children who were between age 2 and 13 at timing of residency. Panel A presents mean results for the full group, and Panels B and C for males and females, respectively. Estimates are evaluated relative to receiving recidency before the reform. In each panel the estimated mean effect is presented in the first and in right column, the estimated effect is presented across age at residency (youger than 10 years / 10 years or older). Of the 2,648 children age 2-13, 1,409 are males and 1,239 are females.
* p<0.1; ** p<0.05; *** p<0.01

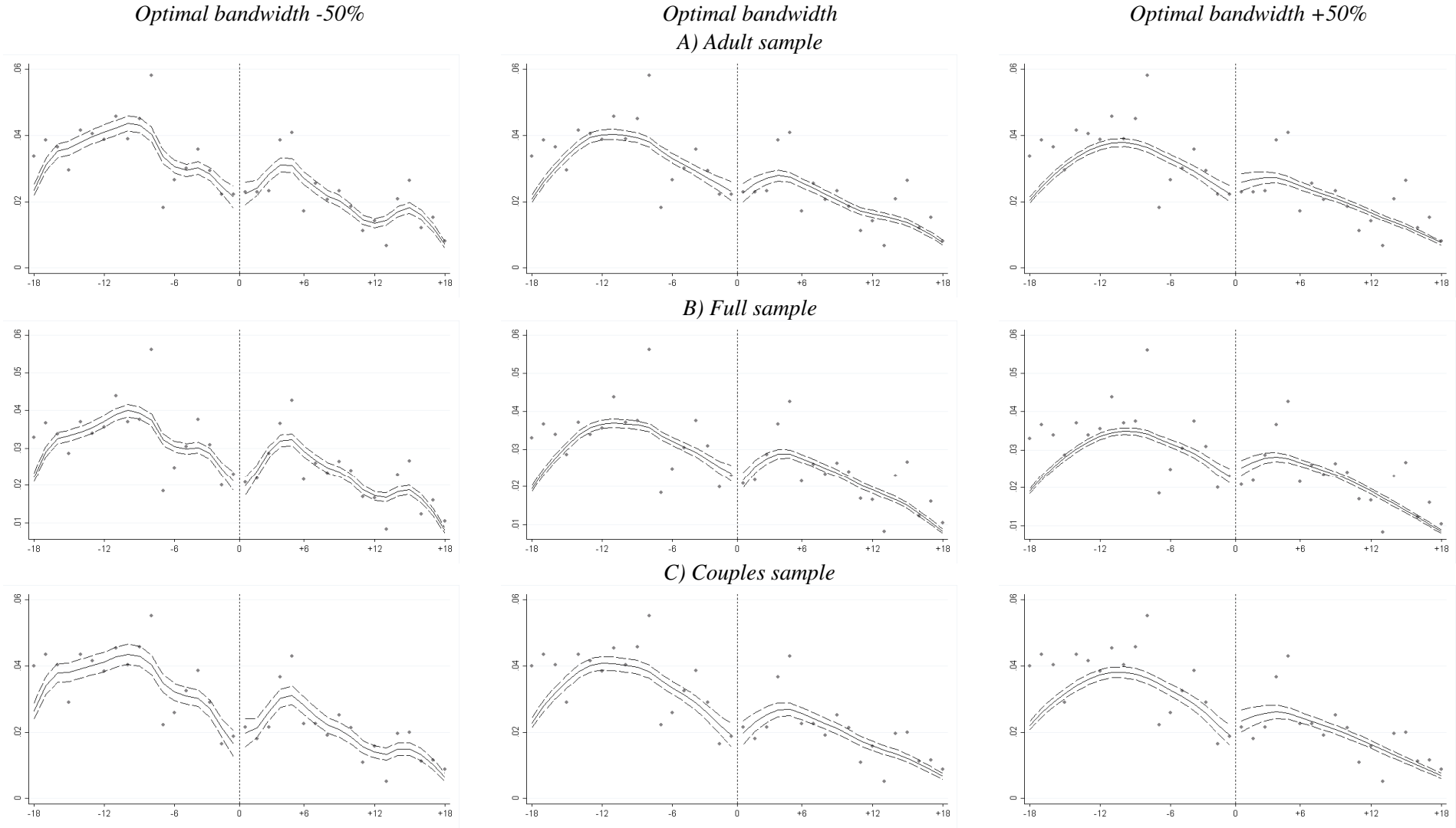
Table 15. Effect of Reform on Crime Convictions at or Before Age 20 for Children Between 11-18 at Reseidency.

	All crime		Property		Theft supermarket		Other shoplifting		Violence	
Treatment	P(convic.)	# convic.	P(convic.)	# convic.	P(convic.)	# convic.	P(convic.)	# convic.	P(convic.)	# convic.
Pre-reform mean	0.229	0.407	0.171	0.261	0.053	0.058	0.021	0.025	0.070	0.102
	(0.420)	(0.993)	(0.377)	(0.719)	(0.224)	(0.276)	(0.144)	(0.176)	(0.256)	(0.412)
A) Full sample	0.065	**0.269***	0.040	0.123	0.013	0.001	0.024	0.036	**0.058*****	**0.078*****
	(0.042)	**(0.151)**	(0.051)	(0.136)	(0.030)	(0.040)	(0.015)	(0.023)	**(0.016)**	**(0.025)**
B) by gender										
Males	0.075	0.416	0.028	0.181	0.012	0.027	0.002	0.016	**0.094*****	**0.140*****
	(0.066)	(0.256)	(0.080)	(0.222)	(0.038)	(0.046)	(0.015)	(0.027)	**(0.028)**	**(0.050)**
Females	0.038	0.041	0.029	0.026	0.001	-0.016	**0.052****	**0.056****	0.007	0.007
	(0.060)	(0.078)	(0.059)	(0.076)	(0.031)	(0.042)	**(0.018)**	**(0.019)**	(0.011)	(0.011)
C) by age at residency										
Age at residency < 14	0.010	0.155	-0.000	0.014	-0.027	-0.049	0.010	0.002	0.041	0.087
	(0.071)	(0.281)	(0.077)	(0.230)	(0.046)	(0.068)	(0.019)	(0.025)	(0.034)	(0.061)
Age at residency >= 14	**0.134*****	**0.444****	0.073	0.254	0.046	0.051	0.032	**0.061***	**0.096*****	**0.114****
	(0.043)	**(0.189)**	(0.060)	(0.156)	(0.035)	(0.039)	(0.022)	**(0.033)**	**(0.027)**	**(0.051)**

Note: The table shows the estimated effects of being granted residency after the reform relative to the individual being granted residency before the reform for children between age 11 and 18 at time of residency on subsequent probability of receiving a crime conviction and the number of crime convictions until age 20 for A) all children age 11-18 at time of residency, B) by gender, C) by age at residency (younger than 14, 14 or older). Panel D) shows results by household type: children where both parents are present in Denmark and both granted residency after the reform, both parents are present in Denmark but only one was granted residency after the reform, or only had a single parent present in Denmark. All estimates are evaluated relative to receiving residency before the reform.
* p<0.1; ** p<0.05; *** p<0.01

Appendix Figures and Tables

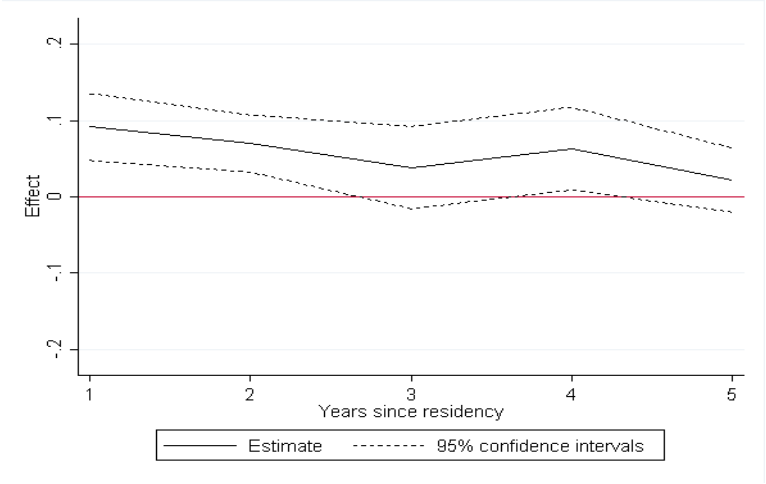
Figure A.1. McCrary tests around the timing of the reform



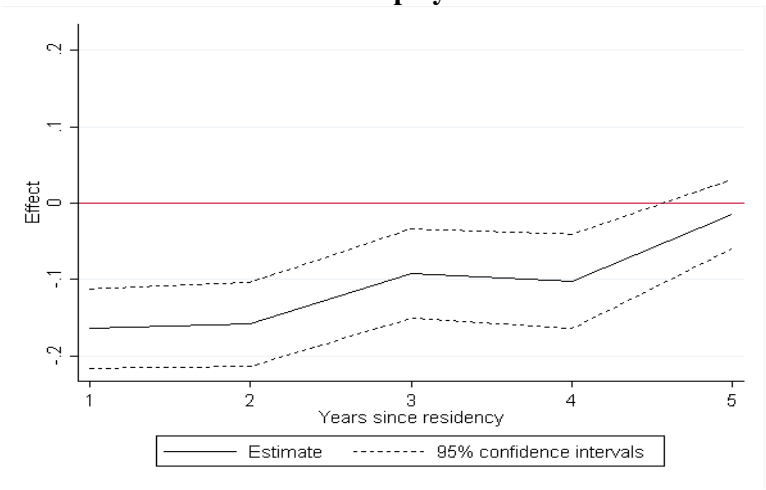
Note: Figure shows McCrary tests of manipulation of the running variable density (month of residency) around the timing of the reform which is one of our identifying assumptions. The scatterplots are densities by monthly bins and the dashed lines show the 95% CIs around the fitted line. Bandwidths are chosen as in McCrary (2008) resulting in an approximately 6.6 months BW. Figure also shows 50% lower and higher BWs illustrating robustness. As the fitted lines are connected and 95% CIs almost entirely overlapping at the time of the reform, we do not find evidence of manipulation in any of our samples.

Figure A.2. Effect of reform on subsequent labor market outcomes

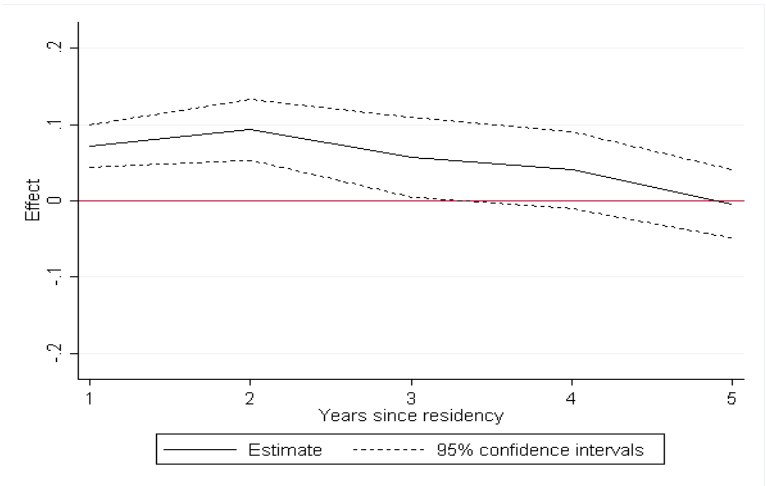
Employment



Unemployment



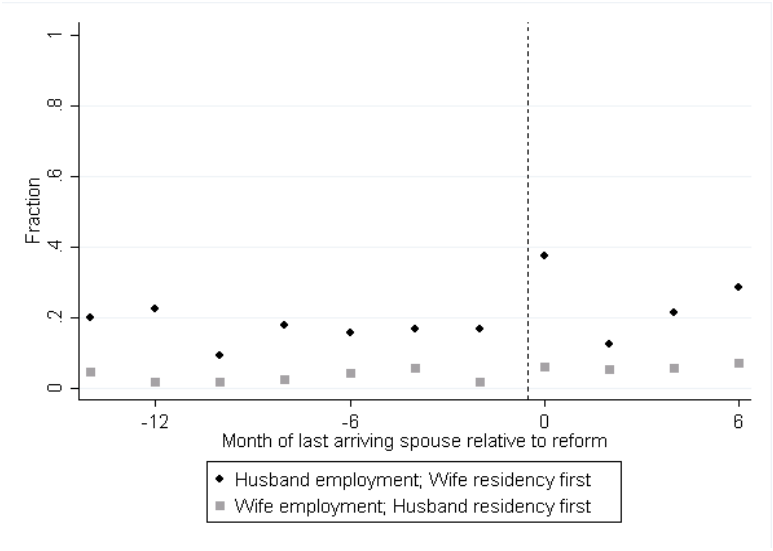
Not in the labor force



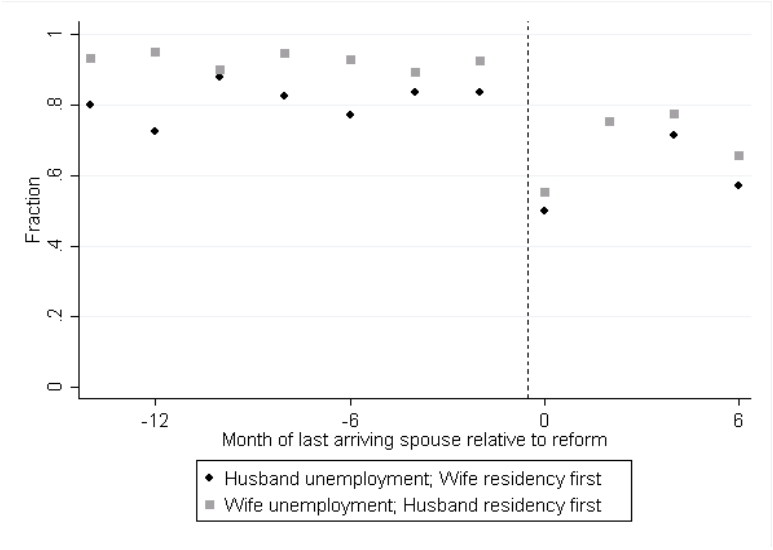
Note: Figures correspond to the estimation results shown in Table 7. Effects estimated as outlined in Equation (1). Covariates include age at residency, gender, number of siblings in Denmark, number of children, marital status, country of origin, and resident permit status. Year 3-5 enter as individual years and not as an average of the years as in Table 7. The solid lines show the point estimates and the dashed lines the 95% confidence intervals.

Figure A.3: Labor market response in households where one spouse receives residency after the reform

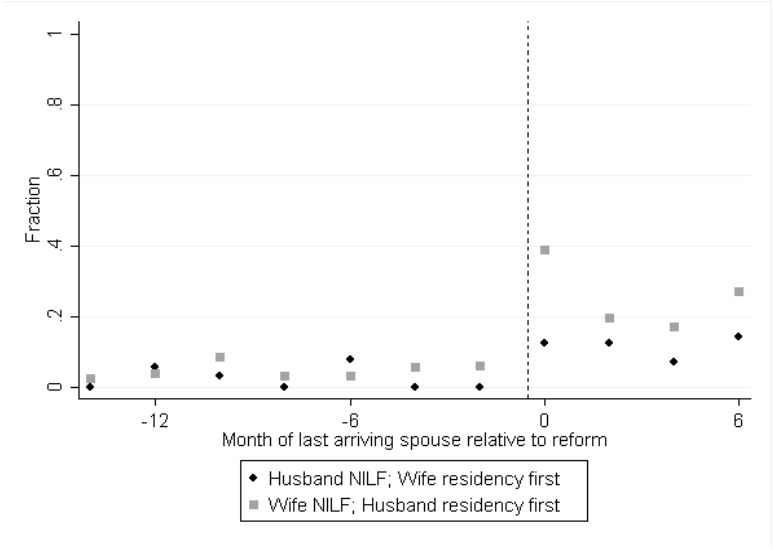
A) Employment, year 1



A) Unemployment, year 1



C) Not in labor force, year 1



Note: Figure shows labor market outcomes of couples who receive residency on each side of the reform by whether the husband or wife received residency first.

Figure A.4. Probability of receiving SA or Start Aid by timing of spouse's residency

A) Males, both spouses granted residency after reform relative to both before the reform



B) Males, one spouse granted residency after reform relative to both before the reform



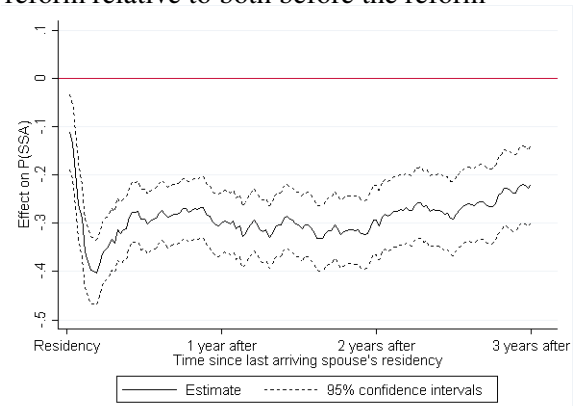
C) Males, difference one-two spouses granted residency after reform



D) Females, both spouses granted residency after reform relative to both before the reform



E) Females, one spouse granted residency after reform relative to both before the reform



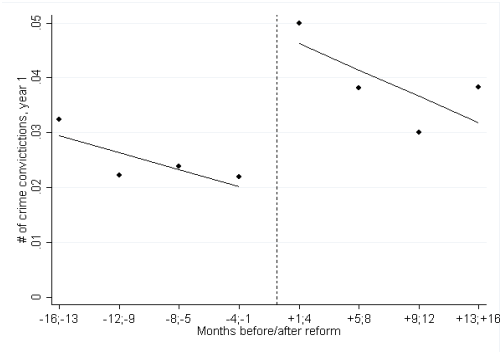
F) Females, difference one-two spouses granted residency after reform



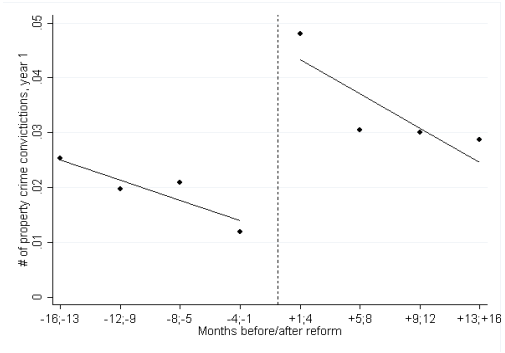
Note: Figures A and D show the effects of the reform on likelyhood of receving Social Assistance/Start Aid for males (A) and females (D) in couples where both spouses received residency after the reform. Figures B and E shows the equivalent couples where only one spouse received residency after the reform. Figures A, B, D, E estimated using Equation (2). Figures C and F shows the difference between males' (C) and females' (F) takeup of social assistance depending on whether one or both of spouses received residency after the reform. These figures (C and F) show the consequence of the assymmetric incentive structure that was built into the reform (for males and females). The solid lines show the point estimates and the dashed lines the 95% confidence intervals.

Figure A.5: Reduced form figures of derived outcomes by residency relative to the reform

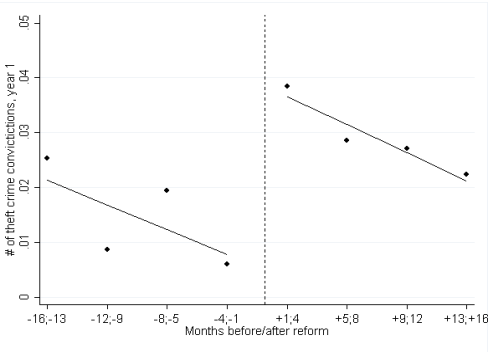
A) # crime convictions year 1



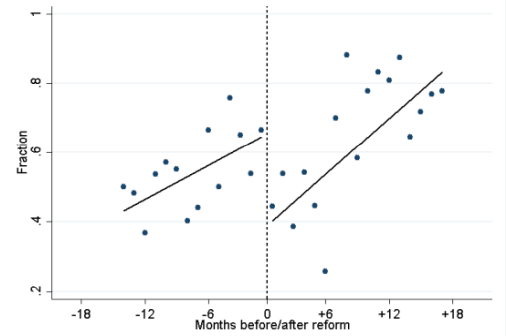
B) # property crime convictions



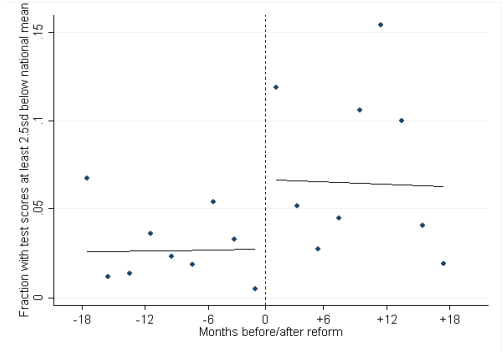
C) # theft crime convictions



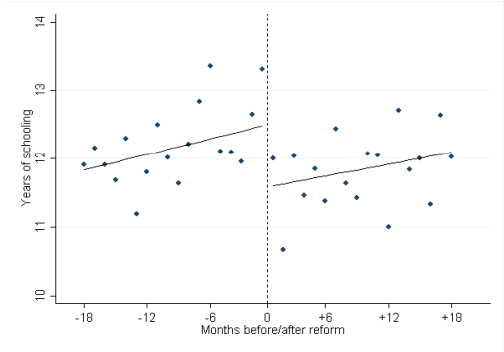
D) Enrolled in daycare/prescool year 1, children age 0-5 at time of residency



E) Danish Test Scores during primary school at or below 2.5 sd.below national average



F) Years of completed schooling in 2016, males age 10-13 at time of residency



G) Fraction with crime conviction until age 20, males age 14-18 at time of residency

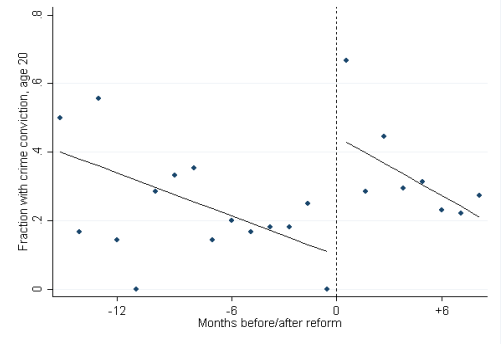
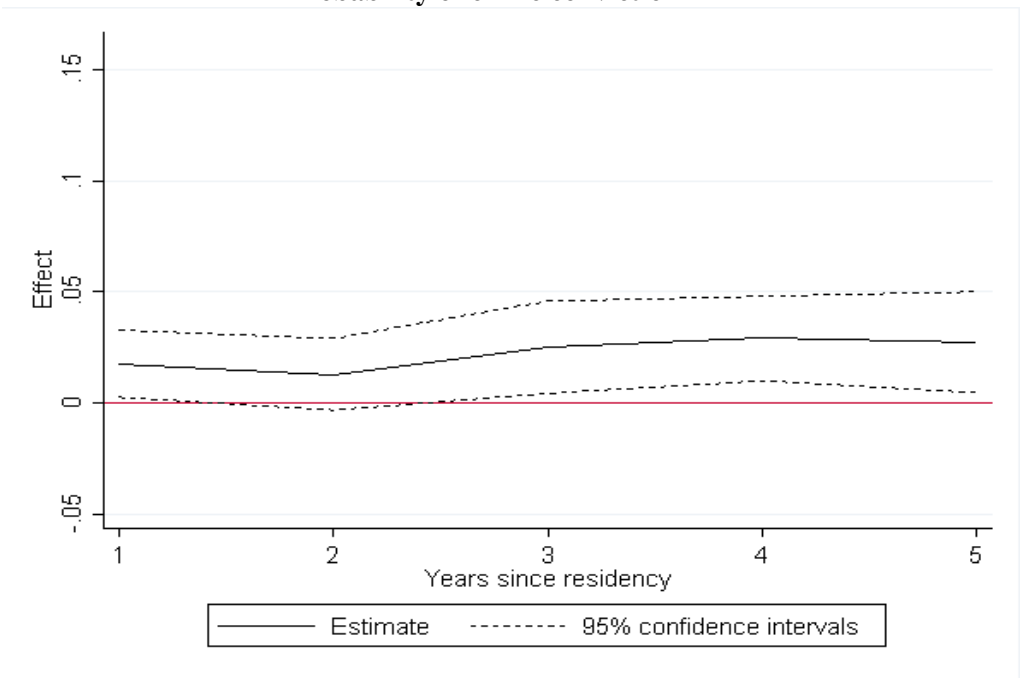
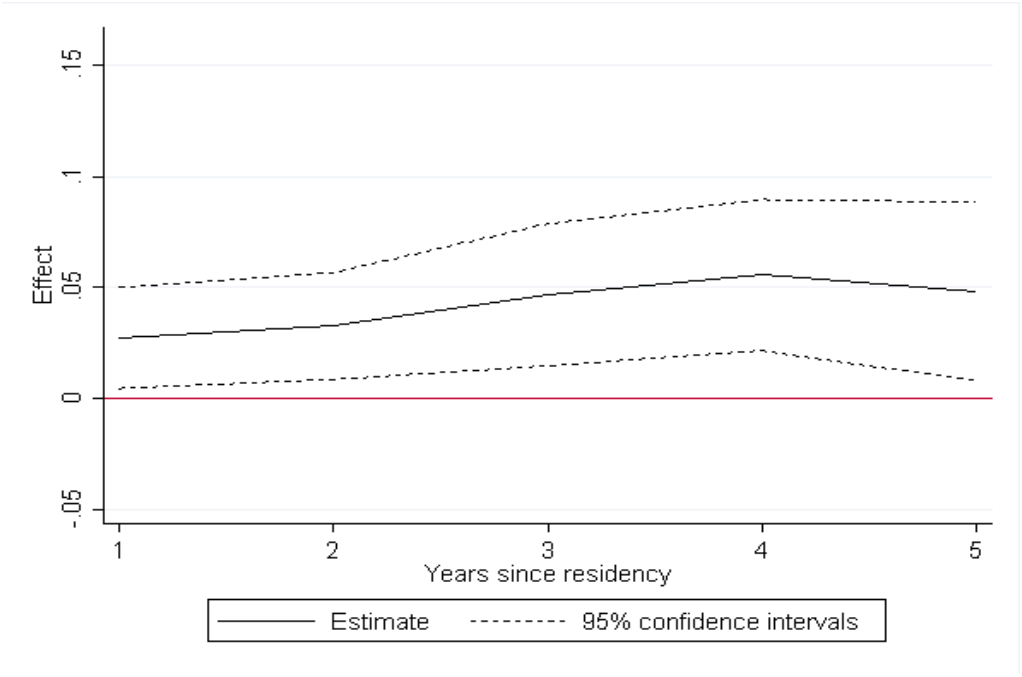


Figure A.6. Effect of reform on crime convictions

Probability of crime conviction



Number of crime convictions



Note: Figures show the estimation results on crime but for the full sample of adults age 18-55 at residency considered together and a wider range of years (year 1-5 and not only year 1). Effects estimated as outlined in Equation (1). Covariates include age at residency, gender, number of siblings in Denmark, number of children, marital status, country of origin, and resident permit status. The solid lines show the point estimates and the dashed lines the 95% confidence intervals.

Table A.1. Implied Marginal Tax Rate at the Participation Margin and Break-Even Point by Residency Before / After the Reform.

			Single / both spouses before reform		*Single / both spouses after reform*		*Spouses on each side of reform*	
Status	Age	Children	Break-even point	Implied marginal tax rate, 1st $ earned	Break-even point	Implied marginal tax rate, 1st $ earned	Break-even point	Implied marginal tax rate, 1st $ earned
Spouse	>= 25	0	2,286	0.935	1,596	0.821	1,233	0.935
Spouse	>= 25	1	2,959	0.935	1,994	0.821	1,542	0.935
Spouse	>= 25	>= 2	2,959	0.935	2,393	0.821	1,850	0.935
Single	>= 25	0	1,152	0.935	961	0.821	-	-
Single	>= 25	1	1,533	0.935	1,201	0.821	-	-
Single	>= 25	>= 2	1,533	0.935	1,441	0.821	-	-
Own residence	< 25	0	744	0.935	798	0.821	617	0.935
Spouse	< 25	1	2,959	0.935	1,994	0.821	1,542	0.935
Spouse	< 25	>= 2	2,959	0.935	2,393	0.821	1,850	0.935
Single	< 25	1	1,533	0.935	1,038	0.821	-	-
Single	< 25	>= 2	1,533	0.935	1,278	0.821	-	-
Parent's residence	< 25	0	359	0.935	393	0.821	-	-

Note: The table shows the implied marginal tax rate on labor earnings at the participation margin (i.e. implied tax of first $ earned) and the break-even point (where one dollar earned returns one dollar in gross income) for different demographic types and by treatment status. All amounts are reported in 2010 PPP-adjusted USD. Calculations are based on the average minimum hourly wage for unskilled workers across several sectors. 'Before reform' are both couples were both received residency before the reform (A=0, B=0, see Figure 2) and singles who received residency before the reform. 'After reform' are couples where both received residecy after the reform (A=0, B=1, see Figure 2) or singles who received residency after the reform. 'On each side of reform' are couples where both received residency before the reform and one after the reform (A=1, B=0, see Figure 2).

Table A.2. Robustness Check of the Impact of Model Specification.

Years since residency	Main estimate	Quad. running var.	Donut sample	Reduced bandwidth	Incl. Afg/ Jugoslav
A) Employment rate					
1	0.090***	0.098***	0.054***	0.085***	0.100***
	(0.029)	(0.035)	(0.016)	(0.030)	(0.018)
2	0.069**	0.070**	0.046**	0.075***	0.094***
	(0.029)	(0.026)	(0.020)	(0.023)	(0.015)
3-5	0.041*	0.039	0.045*	0.085*	0.076***
	(0.021)	(0.031)	(0.021)	(0.030)	(0.016)
Observations	4,843	4,843	4,439	3,362	7,456
B) Unemployment rate					
1	-0.168***	-0.202***	-0.109***	-0.173***	-0.183***
	(0.040)	(0.042)	(0.015)	(0.035)	(0.021)
2	-0.168***	-0.188***	-0.098***	-0.188***	-0.190***
	(0.043)	(0.045)	(0.020)	(0.035)	(0.023)
3-5	-0.069***	-0.065*	-0.061**	-0.071**	-0.062***
	(0.018)	(0.028)	(0.018)	(0.023)	(0.012)
Observations	4,843	4,843	4,439	3,362	7,456
C) Not in labor force rate					
1	0.077***	0.098***	0.056***	0.085***	0.082***
	(0.020)	(0.019)	(0.006)	(0.019)	(0.012)
2	0.104***	0.123***	0.056***	0.116***	0.100***
	(0.026)	(0.030)	(0.009)	(0.029)	(0.018)
3-5	0.031*	0.031	0.020	0.034	-0.012
	(0.017)	(0.024)	(0.020)	(0.022)	(0.016)
Observations	4,843	4,439	4,439	3,362	7,456
Running variable	X	X	X	X	X
Covariates	X	X	X	X	X
Year Fixed Effects	X	X	X	X	X

Note: The table shows robustness tests of the main results for labor market outcomes using alternative specifications. The table shows the estimated effects of being granted residency after the reform relative to being grated residency before the reform. From left: the table shows the main results as reported in Table 6; including a quadratic running variable on each side of the reform along with the linear running variable; a donut sample where we exclude two months on each side of the reform; a reduced bandwidth of +-12 months around the reform; and the sample including Afghan refugees.
* p<0.1; ** p<0.05; *** p<0.01

Table A.3. Unconditional Balancing Test Across Reform.

	A) Main sample	B) Main sample incl. remigrants	C) Full sample	D) Couples sample
Age at residency	-0.873	-0.934	-0.830	-1.904***
	(0.561)	(0.557)	(0.767)	(0.580)
Any siblings	-0.005	-0.002	0.019	-
	(0.007)	(0.007)	(0.035)	
Female	0.056	0.042	0.022	0.062
	(0.040)	(0.036)	(0.027)	(0.047)
# of children	-0.137	-0.104	-	-0.210
	(0.129)	(0.104)		(0.113)
Single	0.008	0.004	-	-
	(0.038)	(0.037)		
Spouse present in Denmark	0.013	0.003	-	-
	(0.030)	(0.028)		
Eastern Europe/former USSR	-0.025	-0.020	-0.023	-0.031
	(0.020)	(0.017)	(0.018)	(0.027)
Muslim countries	0.037	0.028	0.040	0.015
	(0.042)	(0.036)	(0.035)	(0.049)
Rest of the world	-0.012	-0.008	-0.017	0.016
	(0.036)	(0.031)	(0.030)	(0.041)
Refugee permit status	-0.049	-0.033	-0.026	0.013
	(0.056)	(0.058)	(0.057)	(0.073)
Waiting time in asylum center a)	0.943	-	-	-
	(1.641)			
Remigrated	-	-0.030	-	-
		(0.032)		
First residency in couple	-	-	-	-0.044
				(0.045)
Observations	4,843	5,747	8,142	4,076
Running variable	X	X	X	X

Note: The table shows estimation results of each observable characteristic on a dummy indicating whether residency was granted before or after the reform conditional on the running variable. I.e. the table reports the individual γ's from the regressions:

$x = reform'\gamma + f(Z)'\pi$

with standard errors in parentheses. Each cell represents one regression and shows change in the observable characteristic at the time of the reform, by sample. Covariates include age at residency, whether the refugee had any siblings, gender, number of children, marital status, spouse present in Denmark, country of origin ('Muslim countries' refers to countries where the majority are muslim and not the individual's faith), and refugee permit status (was residency given on grounds of being a refugee 1, or from being the spouse / child of an individual with refugee status, 0). Re-migrants are people who left Denmark during the follow up period. We do not consider '# of children', 'single', or 'spouse present in Denmark' for the full sample as the full sample is equal to the main sample plus children where these three variables all take 0. We do not test for siblings in the couples sample as there are less than 10 observations with siblings (cf. summary statistics in Table 2). First residency in couple is a dummy variable measuring whether the individual was the first or last in the couple to receive residency. This is only used for the couples sample.

a) Waiting time in asylum center measures (in weeks) the time spent from asylum application date in Denmark until the residency is granted. This variable is only available for a subsample (N = 3,520) and only for the main sample without re-migrants. The mean waiting time was 15 months and eight days.

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

Table A.4. Effect of Reform on Subsequent Type of Occupation.

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

Note: The table shows the estimated effects of being granted residency after the reform relative to before the reform on subsequent probability of being employed in a given type of occupation for the main sample of adults age 18-55 on the time of residency in year 1, 2, and the average of year 3-5 since residency. Panel A shows results for all individuals in the sample. Panels B and C show results by level of education.

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

Table A.5. Effect of Reform on Labor Market Outcomes, by Education and Gender.

	<12 years of educ.		>=12 years of educ.	
	Males	Females	Males	Females
A) Employment				
1	**0.193*** **	0.034	**0.133** **	0.035
	(0.060)	(0.024)	**(0.051)**	(0.035)
2	**0.127** **	-0.003	**0.141*** **	0.038
	(0.062)	(0.025)	**(0.048)**	(0.043)
3-5	0.034	0.020	0.057	**0.077** **
	(0.039)	(0.033)	(0.047)	**(0.029)**
B) Unemployment				
1	**-0.192*** **	**-0.176*** **	**-0.126** **	**-0.159*** **
	(0.058)	**(0.028)**	**(0.051)**	**(0.034)**
2	**-0.155** **	**-0.145*** **	**-0.128** **	**-0.208*** **
	(0.066)	**(0.037)**	**(0.047)**	**(0.046)**
3-5	**-0.081** **	**-0.075** **	-0.034	**-0.088** *
	(0.030)	**(0.031)**	(0.028)	**(0.047)**
C) Not in labor force				
1	0.001	**0.139*** **	-0.007	**0.124*** **
	(0.019)	**(0.026)**	(0.017)	**(0.045)**
2	0.036	**0.151*** **	-0.010	**0.177*** **
	(0.024)	**(0.028)**	(0.036)	**(0.048)**
3-5	0.049	**0.059** **	-0.021	0.017
	(0.034)	**(0.023)**	(0.038)	(0.045)
D) Dropped out				
1	0.009	**0.130*** **	-0.004	**0.121*** **
	(0.016)	**(0.023)**	(0.012)	**(0.043)**
2	0.028	**0.140*** **	0.014	**0.185*** **
	(0.028)	**(0.026)**	(0.020)	**(0.041)**
3-5	0.025	**0.094*** **	0.005	**0.100*** **
	(0.016)	**(0.021)**	(0.014)	**(0.027)**
Observations	1,199	1,531	1,191	922
Running var	X	X	X	X
Covariates	X	X	X	X
Year FE	X	X	X	X

Note: The table shows the estimated effects of being granted residency after the reform relative to being granted residency before the reform on subsequent probability of being in employment, unemployed, not in the labor force, or dropped out of the welfare system completely, by gender and highest completed grade at time of residency for the main sample of adults aged 18-55 on the time of residency in year 1, 2, and the average of year 3-5 since residency.
* p<0.1; ** p<0.05; *** p<0.01

Table A.6. Effect of Reform on Couples' Annual Income ($1,000) (Corresponding to Table 10), Detailed by Income Type, Gender and Household Type.

Treatment	*Gross income (1+2+3)*	*1) Labor earnings*	*2) Residual earnings*	*3) Transfers (3.1+3.2+ 3.3)*	*3.1) Pensions*	*3.2) Temporary Assistance: UI, SA, Start Aid*	*3.3) Other (3.3.1+ 3.3.2+ 3.3.3)*	*3.3.1) Housing support*	*3.3.2) Child benefits*	*3.3.3) Education support*	*4) Tax payments*	*Net-of-tax income (1+2+3-4)*
A) Individual level income in couples, both granted residency after reform												
Males	**-5.287*** **	**4.322*** **	0.374	**-9.983*** **	0.146	**-10.760*** **	**0.642** **	**0.553*** **	0.032	0.057	**-2.149*** **	**-3.151*** **
	(1.027)	**(1.112)**	(0.278)	**(0.930)**	(0.256)	**(0.846)**	**(0.282)**	**(0.140)**	(0.215)	(0.084)	**(0.362)**	**(0.756)**
Females	**-8.198*** **	0.736	0.241	**-9.175*** **	0.052	**-8.498*** **	**-0.696** **	0.108	**-0.826*** **	0.022	**-2.725*** **	**-5.437*** **
	(1.027)	(1.112)	(0.278)	**(0.930)**	(0.256)	**(0.846)**	**(0.282)**	(0.140)	**(0.215)**	(0.084)	**(0.362)**	**(0.756)**
B) Individual level income in couples, one granted residency after reform												
Males	0.080	1.308	0.217	**-1.444** **	0.035	**-2.225*** **	**0.750*** **	**0.913*** **	-0.127	-0.036	**-1.259*** **	**1.328** **
	(0.742)	(0.803)	(0.201)	**(0.672)**	(0.185)	**(0.611)**	**(0.204)**	**(0.101)**	(0.155)	(0.061)	**(0.261)**	**(0.546)**
Females	**-11.599*** **	-0.088	-0.072	**-11.439*** **	0.008	**-11.543*** **	**0.120**	0.096	-0.015	0.039	**-3.461*** **	**-8.177*** **
	(0.742)	(0.803)	(0.201)	**(0.672)**	(0.185)	**(0.611)**	**(0.204)**	(0.101)	(0.155)	(0.061)	**(0.262)**	**(0.546)**
C) Household income in couples, both granted residency after reform												
	-13.485* **	**5.058*** **	0.145	**-19.159*** **	0.198	**-19.298*** **	-0.058	**0.661*** **	**-0.798*** **	0.079	**-4.912*** **	**-8.589*** **
	(1.382)	**(1.721)**	(0.335)	**(1.458)**	(0.362)	**(1.277)**	(0.450)	**(0.184)**	**(0.304)**	(0.127)	**(0.520)**	**(1.010)**
D) Household income in couples, one granted residency after reform												
	-11.519* **	1.220	0.616	**-12.883*** **	0.042	**-13.793*** **	**0.867*** **	**1.009*** **	-0.145	0.003	**-4.683*** **	**-6.849*** **
	(0.998)	(1.243)	(0.463)	**(1.053)**	(0.261)	**(0.922)**	**(0.325)**	**(0.133)**	(0.220)	(0.092)	**(0.376)**	**(0.730)**

Note: The table shows the estimated effects of the reform on income the first year after the last arriving spouse's residency (corresponding to Table 10), but with income divided into the specific income sources. Standard errors in parentheses. Observations: 4,036 in panels A and B, 2,036 in panels C and D. Panel A shows the effects of both spouses receiving residency after the reform relative both spouses receiving residency before the reform on individual income. Panel B shows the same for individuals in couples where only one of the spouses receives residency after the reform relative to both spouses receiving residency before the reform. Panel C shows effect on household income as in Panel A. Panel D shows effect on household income as in Panel B. Gross income is the sum of Labor earnings (1); Residual earnings (2) (approx. 1.5% of income divided between business income, 1.35%, and capital income, 0.15%); and Transfer income (3). Transfer income is the sum of Pensions (3.1); Temporary assistance (UI, SA Start Aid) (3.2); and 'Other transfers' (3.3). Other transfers are the sum of housing support (3.3.1); child benefits (3.3.2); and educational support (3.3.3). Net-of-tax income is Gross income minus Tax payments (4). The effects on tranfers in (3) capture the direct impact of lower temporary assistance and means testing from increased earnings resulting from the reform (3.2) and the minor changes in 'other transfers' (3.3). Other transfers change as housing benefits increase mechanically as a result of lower income levels and housing benefits and child support is reduced for families where both are eligible to receive Start Aid because of an introduction of a cap in one-time supplementary payments for families with children (Act on special support relating to §34 of Law on Social Benefits: §3 of the act in combination with §6.4)

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

Table A.7. Effect of Reform on Criminal Convictions (Accumulated from Residency), by Crime Type, Household Type and Gender.

Treatment	Years since residency	Property crimes		Larceny/theft		Theft from supermarket		Other shoplifting		Violence	
		P(crime)	#crimes	P(crime)	#crimes	P(crime)	#crimes	P(crime)	#crimes	P(crime)	#crimes
A) Couples, both granted residency after reform											
Males	1	0.016	0.020	0.019	0.023	0.010	0.010	**0.010****	**0.011****	**0.020****	**0.020****
		(0.016)	(0.020)	(0.014)	(0.018)	(0.013)	(0.015)	**(0.005)**	**(0.006)**	**(0.008)**	**(0.008)**
Females	1	**0.031***	**0.042****	0.022	**0.033***	0.022	**0.028***	0.001	0.001	0.005	0.005
		(0.016)	**(0.020)**	(0.014)	**(0.018)**	(0.013)	**(0.015)**	(0.005)	(0.006)	(0.008)	(0.008)
Observations		2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036
B) Couples, one granted residency after reform											
Males	1	-0.000	-0.003	0.003	-0.001	0.001	0.001	-0.004	-0.003	0.005	0.005
		(0.012)	(0.015)	(0.010)	(0.013)	(0.010)	(0.011)	(0.003)	(0.004)	(0.006)	(0.006)
Females	1	0.018	**0.024***	0.014	0.020	0.016	**0.022***	-0.001	-0.001	-0.000	-0.000
		(0.012)	**(0.015)**	(0.010)	(0.013)	(0.010)	**(0.011)**	(0.003)	(0.004)	(0.006)	(0.006)
Observations		2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036	2,036

Note: The table shows the estimated effect of the reform, by gender, on subsequent probability of crime conviction (P(crime)) and number of subsequent crime convictions (#crimes), by crime type, during the first year following residency. Effects are estimated using the exact date of crime counting 1 year since the last arriving spouse's date of residency. Standard errors in parentheses. Panel A shows, by gender, the effects of both spouses being granted residency after the reform relative both spouses being granted residency before the reform. Panel B shows the same for couples where only one spouses was granted residency after the reform relative to both spouses being granted residency before the reform. 'Larceny/theft' is nested in 'Property crime' and 'Theft supermarket' and 'Other shoplifting' are nested in 'Larceny/theft'.

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

Table A.8. Estimated Income Reductions for the Parents in our Sample.

Years since residency	Treatment	Transfers	Wage income	Gross income	Net-of-tax	Ln(gross inc)	Ln(net-of-tax)
				Parents, couples			
1	One parent	**-14.144*** **	1.733	**-12.286*** **	**-6.931*** **	**-0.322*** **	**-0.221*** **
	after reform	**(1.057)**	(1.084)	**(0.966)**	**(0.783)**	**(0.025)**	**(0.024)**
	Both parents	**-15.840*** **	1.658	**-13.879*** **	**-8.031*** **	**-0.389*** **	**-0.289*** **
	after reform	**(1.312)**	(1.191)	**(1.315)**	**(1.137)**	**(0.031)**	**(0.031)**
2	One parent	**-12.453*** **	1.821	**-10.184*** **	**-4.932*** **	**-0.267*** **	**-0.163*** **
	after reform	**(1.425)**	(1.900)	**(1.343)**	**(0.957)**	**(0.033)**	**(0.029)**
	Both parents	**-12.299*** **	1.212	**-10.707*** **	**-5.439*** **	**-0.260*** **	**-0.165*** **
	after reform	**(1.445)**	(2.244)	**(1.535)**	**(1.008)**	**(0.035)**	**(0.030)**
3-5	One parent	**-6.357*** **	-0.493	**-6.600*** **	**-2.856*** **	**-0.148*** **	**-0.076*** **
	after reform	**(1.593)**	(2.802)	**(1.927)**	**(1.153)**	**(0.038)**	**(0.031)**
	Both parents	**-4.283*** **	2.982	-1.778	-0.219	-0.036	-0.002
	after reform	**(1.840)**	(3.396)	(2.401)	(1.405)	(0.044)	(0.036)
Observations		2,964	2,964	2,964	2,964	2,964	2,964
				Single parents			
1	After reform	**-7.945*** **	0.980	**-7.120*** **	**-5.182*** **	**-0.294*** **	**-0.256*** **
		(1.484)	(0.887)	**(1.421)**	**(1.268)**	**(0.078)**	**(0.077)**
2	After reform	**-6.736*** **	1.481	**-5.341*** **	**-3.425*** **	**-0.217*** **	**-0.169*** **
		(1.631)	(1.417)	**(1.576)**	(1.319)	**(0.080)**	**(0.077)**
3-5	After reform	**-4.243*** **	2.247	-2.013	-1.485	0.031	0.017
		(1.780)	(1.883)	(1.773)	(1.384)	(0.062)	(0.055)
Observations		478	478	478	478	475	475

Note: The upper panel of the table shows the estimated effect of one or two parents being granted residency after the reform relative both parents being granted residency before the reform on subsequent household level transfer income, wage earnings total gross income, net-of-tax income, log of total gross income, and log of net-of-tax income in year 1, 2, and the average of year 3-5 since last residency of parents. The lower panel of the table shows the corresponding effects for single parents being grated residency after the reform relative to before the reform.
* p<0.1; ** p<0.05; *** p<0.01

Table A.9. Replication of Effects of the Reform on Relationship Status and Crime Convictions of Parents by Family Type.

		Relationship status		All crime, accumulated				Property, accumulated			
Years since	Treatment	P(cohabi.)	P(married)	P(crime conviction)		# crime convictions		P(crime conviction)		# crime convictions	
residency		Couples	Couples	Fathers	Mothers	Fathers	Mothers	Fathers	Mothers	Fathers	Mothers
					Parents, couples						
1	One parent	**0.055****	-0.010	-0.001	**0.027****	-0.003	0.024	0.005	**0.027****	0.005	0.024
	after reform	**(0.030)**	(0.031)	(0.015)	**(0.015)**	(0.016)	(0.016)	(0.015)	**(0.015)**	(0.015)	(0.016)
	Both parents	-0.018	-0.034	**0.035****	**0.033****	0.030	0.038	0.025	0.028	0.023	0.033
	after reform	(0.031)	(0.025)	**(0.021)**	**(0.020)**	(0.022)	(0.025)	(0.016)	(0.019)	(0.016)	(0.025)
Observations		2,964	2,964	2,964	2,964	2,964	2,964	2,964	2,964	2,964	2,964
					Single parents						
1	After reform	-0.062	**-0.172****	0.023		0.023		0.023		0.023	
		(0.073)	**(0.083)**	(0.031)		(0.031)		(0.031)		(0.031)	
Observations		478	478	478		478		478		478	

Note: Upper panel of the table shows the estimated effect of one or two parents being granted residency after the reform relative both parents being granted residency before the reform on parents' relationship status and mothers' and fathers' probability of crime conviction (P(crime)) and number of crime convictions (#crimes) in year 1 since last residency of parents. The lower panel of the table shows the corresponding effects for single parents being granted residency after the reform relative to before the reform.

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

Table A.10: Isolating Bound of Income Effects of Reform on Enrollment in Daycare/Pre-school, Years of Education, and Crime

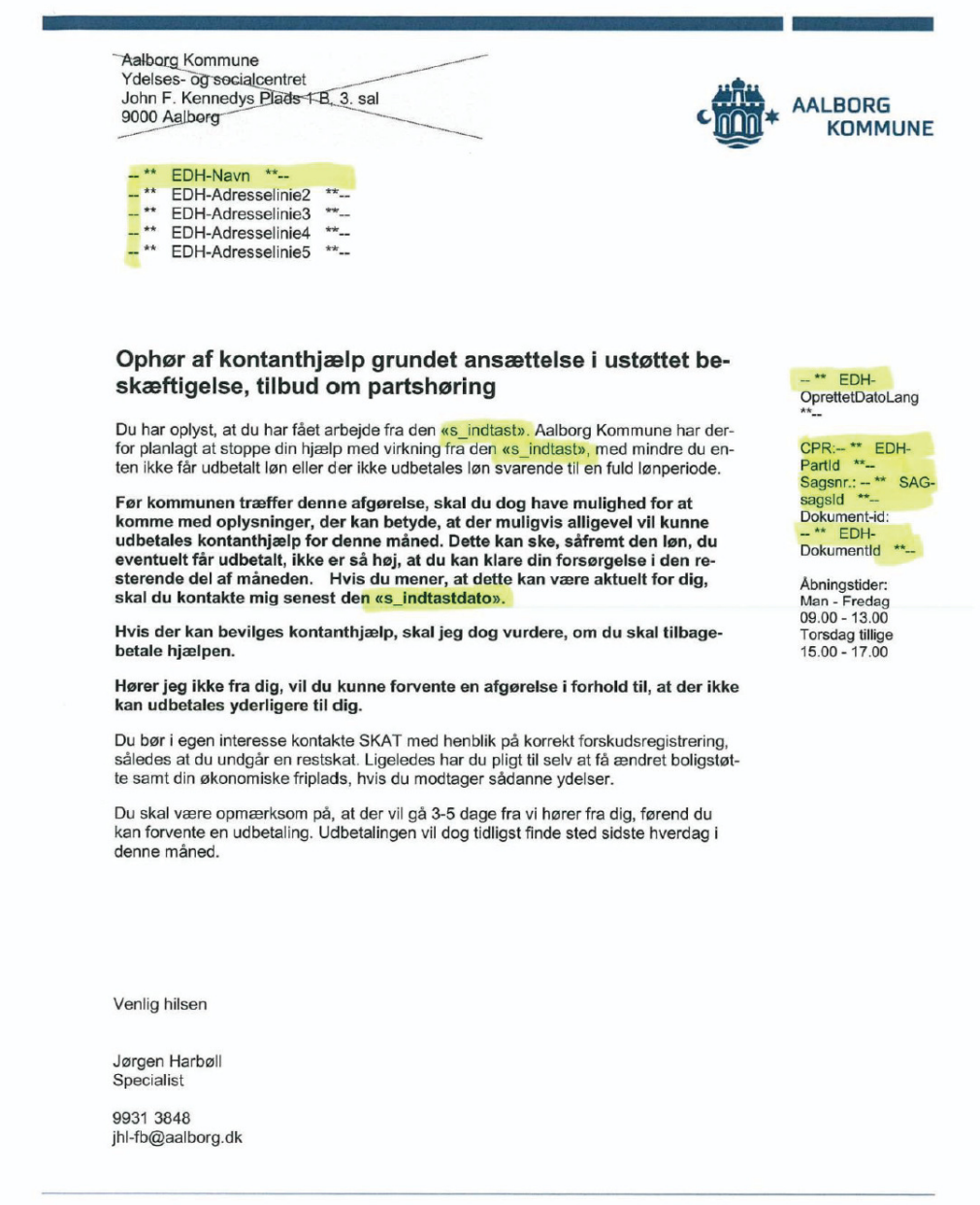
Outcome	(1) Mean effect, original estimate	(2) Mean effect, adjusting for possible subsitution	(3) Young at residency, original estimate	(4) Young at residency, adjusting for possible subsitution	(5) Older at residency, original estimate	(6) Older at residency, adjusting for possible subsitution
Panel A - daycare/preschool use and test scores, age 0-5 at residency						
Daycare, age 1	**-0.157****	**-0.124****	-	-	-	-
	(0.058)	**(0.056)**				
Daycare, age 2	**-0.144****	**-0.117****	-	-	-	-
	(0.066)	**(0.055)**				
2.5sd below national	**0.052****	**0.040****	-	-	-	-
average or lower	**(0.022)**	**(0.016)**				
Panel B - education, age 2-13 at residency						
Years of schooling	-0.093	**-0.196***	0.083	-0.057	**-0.647****	**-0.665****
	(0.103)	**(0.106)**	(0.138)	(0.128)	**(0.267)**	**(0.248)**
Panel C - crime, age 11-18 at residency						
P(crime)	0.065	**0.055***	0.010	0.039	**0.134*****	**0.082****
	(0.042)	**(0.032)**	(0.071)	(0.067)	**(0.043)**	**(0.039)**
# crimes	**0.269***	**0.230***	0.155	0.228	**0.444****	**0.261***
	(0.151)	**(0.130)**	(0.281)	(0.270)	**(0.189)**	**(0.138)**
P(property)	0.040	0.032	-0.000	0.021	0.073	0.038
	(0.051)	(0.040)	(0.077)	(0.069)	(0.060)	(0.046)
# property	0.123	0.108	0.014	0.108	0.254	0.106
	(0.136)	(0.117)	(0.230)	(0.216)	(0.156)	(0.105)
P(theft supermarket)	0.013	0.011	-0.027	-0.012	0.046	0.027
	(0.030)	(0.025)	(0.046)	(0.036)	(0.035)	(0.034)
# theft supermarket	0.001	-0.007	-0.049	-0.034	0.051	0.021
	(0.040)	(0.039)	(0.068)	(0.065)	(0.039)	(0.036)
P(shoplifting)	0.024	**0.026****	0.010	0.029	0.032	0.012
	(0.015)	**(0.011)**	(0.019)	(0.018)	(0.022)	(0.013)
# shoplifting	0.036	**0.035****	0.002	0.032	**0.061***	0.021
	(0.023)	**(0.015)**	(0.025)	(0.019)	**(0.033)**	(0.017)
P(violence)	**0.058*****	**0.047****	0.041	0.035	**0.096*****	**0.078*****
	(0.016)	**(0.022)**	(0.034)	(0.038)	**(0.027)**	**(0.023)**
# violence	**0.078*****	**0.065***	0.087	0.067	**0.114****	**0.104***
	(0.025)	**(0.034)**	(0.061)	(0.069)	**(0.051)**	**(0.052)**

Note: Table shows estimation results from Tables 13, 14, and 15 in columns 1, 3, 5, and the corresponding estimates adjusting for the possible substitution effects arising from lower benefits in columns 2, 4, and 6. The adjustment is computed by replacing outcomes by the pre-reform mean for all children whose parents have aggregated wage earnings on household level > $25,000 during the first three years after the reform. Young at residency refers to age at residency<10 and <14 for years of schooling age crime outcomes, respectively. Older at residency refers to age at residency >= 10 and >= 14 for years of schooling and crime outcomes, respectively.

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

Appendix B – Examples of letters to Social Assistance recipients

Example 1 – Letter informing of means testing



Aalborg Kommune
Ydelses- og socialcentret
John F. Kennedys Plads 1 B, 3. sal
9000 Aalborg

AALBORG KOMMUNE

-- ** EDH-Navn **--
-- ** EDH-Adresselinie2 **--
-- ** EDH-Adresselinie3 **--
-- ** EDH-Adresselinie4 **--
-- ** EDH-Adresselinie5 **--

Ophør af kontanthjælp grundet ansættelse i ustøttet beskæftigelse, tilbud om partshøring

-- ** EDH-OprettetDatoLang **--

CPR:-- ** EDH-PartId **--
Sagsnr.: -- ** SAG-sagsId **--
Dokument-id:
-- ** EDH-DokumentId **--

Åbningstider:
Man - Fredag
09.00 - 13.00
Torsdag tillige
15.00 - 17.00

Du har oplyst, at du har fået arbejde fra den «s_indtast». Aalborg Kommune har derfor planlagt at stoppe din hjælp med virkning fra den «s_indtast», med mindre du enten ikke får udbetalt løn eller der ikke udbetales løn svarende til en fuld lønperiode.

Før kommunen træffer denne afgørelse, skal du dog have mulighed for at komme med oplysninger, der kan betyde, at der muligvis alligevel vil kunne udbetales kontanthjælp for denne måned. Dette kan ske, såfremt den løn, du eventuelt får udbetalt, ikke er så høj, at du kan klare din forsørgelse i den resterende del af måneden. Hvis du mener, at dette kan være aktuelt for dig, skal du kontakte mig senest den «s_indtastdato».

Hvis der kan bevilges kontanthjælp, skal jeg dog vurdere, om du skal tilbagebetale hjælpen.

Hører jeg ikke fra dig, vil du kunne forvente en afgørelse i forhold til, at der ikke kan udbetales yderligere til dig.

Du bør i egen interesse kontakte SKAT med henblik på korrekt forskudsregistrering, således at du undgår en restskat. Ligeledes har du pligt til selv at få ændret boligstøtte samt din økonomiske friplads, hvis du modtager sådanne ydelser.

Du skal være opmærksom på, at der vil gå 3-5 dage fra vi hører fra dig, førend du kan forvente en udbetaling. Udbetalingen vil dog tidligst finde sted sidste hverdag i denne måned.

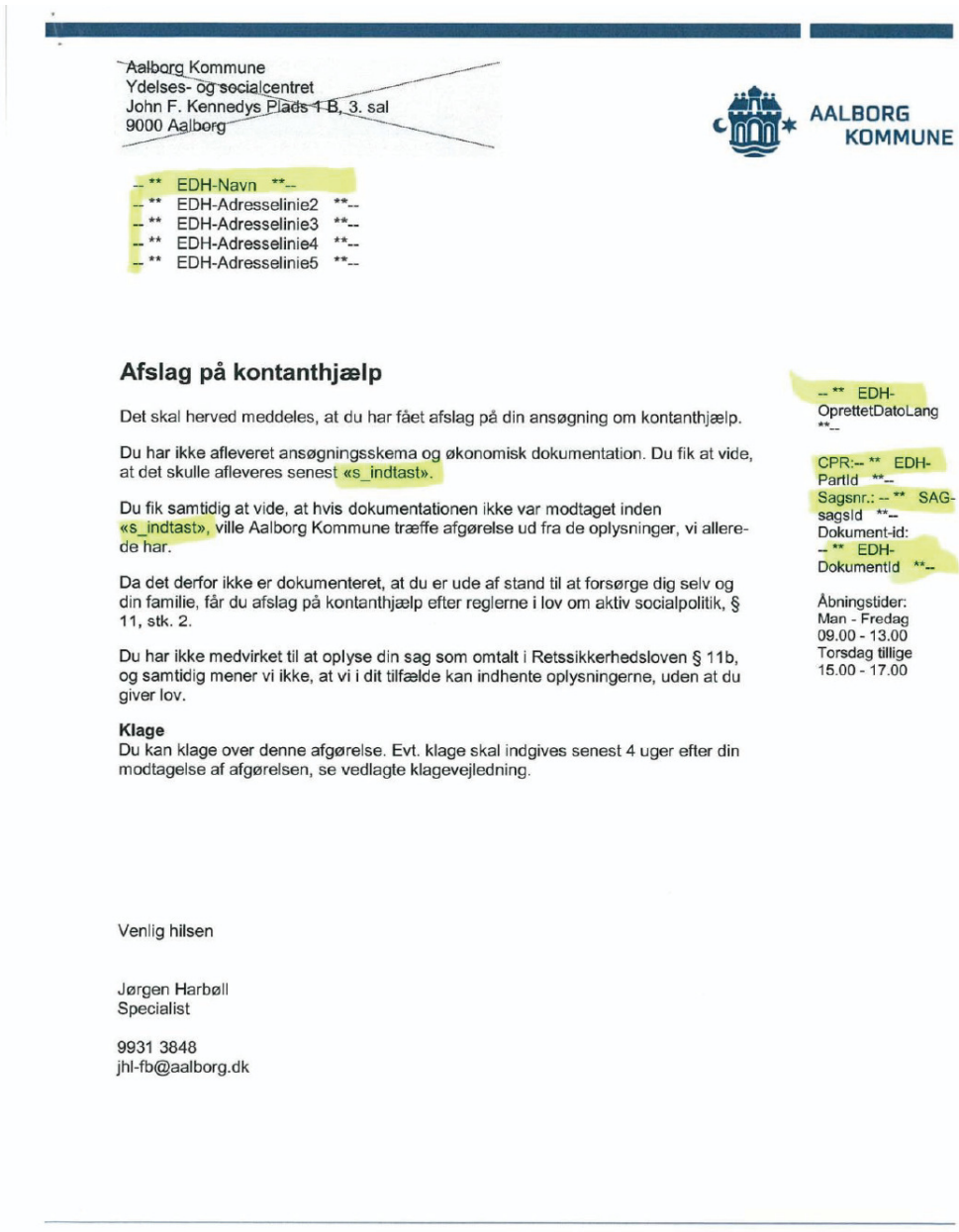
Venlig hilsen

Jørgen Harbøll
Specialist

9931 3848
jhl-fb@aalborg.dk

Note: Example letter from Aalborg Municipality on 'Termination of Social Assistance based on employment'. The first four paragraphs state that:
'*You have informed us that you have found employment from {insert date}. Aalborg Municipality has therefore seized to provide welfare support to you from {insert date} unless your wage income is not equivalent to a full-time job.*
Before the municipality reaches its final conclusion, you are offered the opportunity to provide information which may continue to make you eligible for Social Assistance. This could, for example, be if your wage earnings are below a subsistence minimum for part of the month. If you assess that you could be eligible for such a waiver, please contact me before {insert date}.
If you are found to be eligible for Social Assistance, I will still, however, assess whether you should refund this subsequently.
In case you do not contact me, you should expect transfers to be seized. [own translation]'

Example 2 – Letter informing of termination of benefits due to noncompliance



Aalborg Kommune
Ydelses- og socialcentret
John F. Kennedys Plads 1 B, 3. sal
9000 Aalborg

AALBORG KOMMUNE

-- ** EDH-Navn **--
-- ** EDH-Adresselinie2 **--
-- ** EDH-Adresselinie3 **--
-- ** EDH-Adresselinie4 **--
-- ** EDH-Adresselinie5 **--

Afslag på kontanthjælp

-- ** EDH-OprettetDatoLang **--

CPR:-- ** EDH-PartId **--
Sagsnr.: -- ** SAG-sagsId **--
Dokument-id:
-- ** EDH-DokumentId **--

Åbningstider:
Man - Fredag
09.00 - 13.00
Torsdag tillige
15.00 - 17.00

Det skal herved meddeles, at du har fået afslag på din ansøgning om kontanthjælp.

Du har ikke afleveret ansøgningsskema og økonomisk dokumentation. Du fik at vide, at det skulle afleveres senest «s_indtast».

Du fik samtidig at vide, at hvis dokumentationen ikke var modtaget inden «s_indtast», ville Aalborg Kommune træffe afgørelse ud fra de oplysninger, vi allerede har.

Da det derfor ikke er dokumenteret, at du er ude af stand til at forsørge dig selv og din familie, får du afslag på kontanthjælp efter reglerne i lov om aktiv socialpolitik, § 11, stk. 2.

Du har ikke medvirket til at oplyse din sag som omtalt i Retssikkerhedsloven § 11b, og samtidig mener vi ikke, at vi i dit tilfælde kan indhente oplysningerne, uden at du giver lov.

Klage
Du kan klage over denne afgørelse. Evt. klage skal indgives senest 4 uger efter din modtagelse af afgørelsen, se vedlagte klagevejledning.

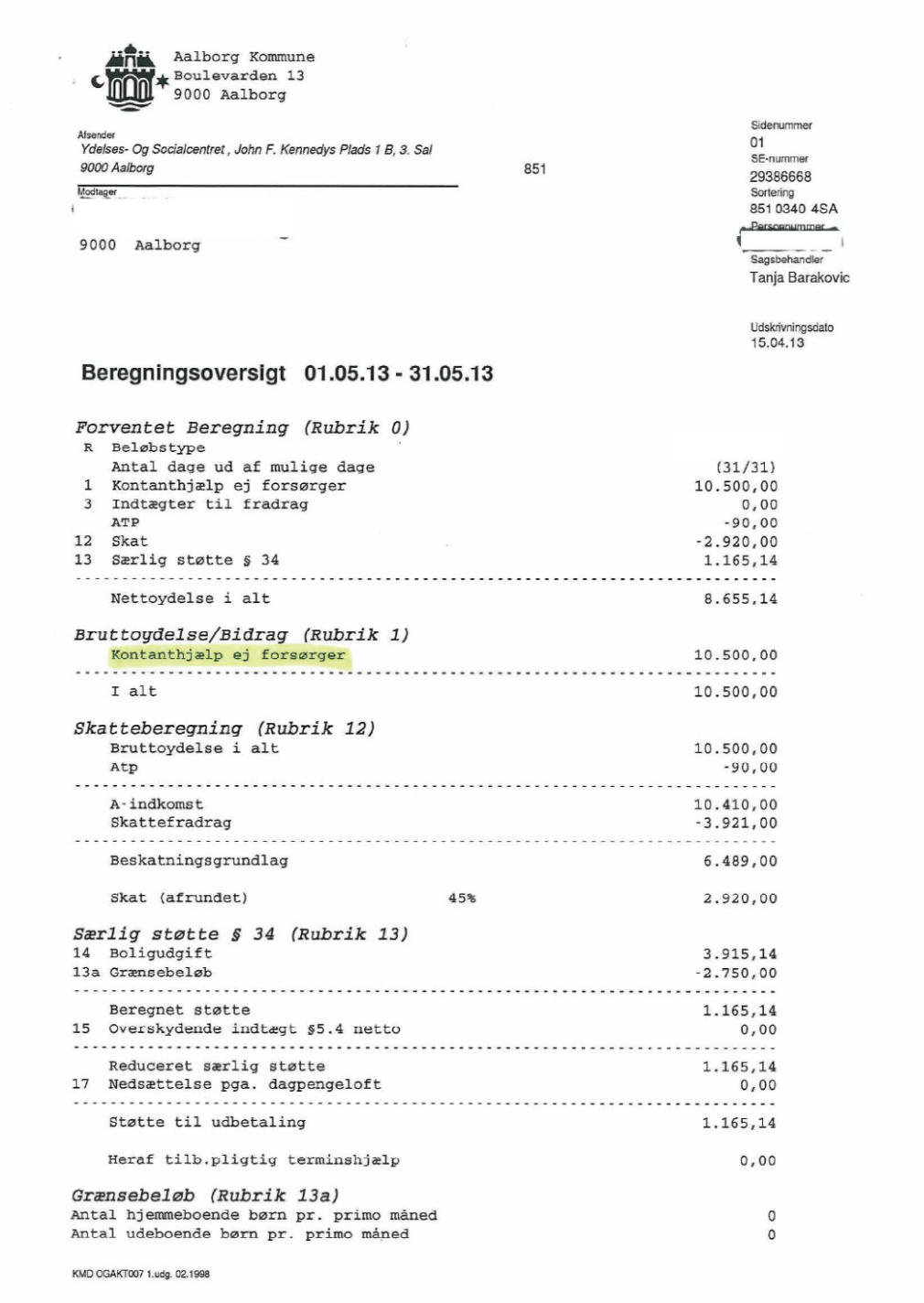
Venlig hilsen

Jørgen Harbøll
Specialist

9931 3848
jhl-fb@aalborg.dk

Note: Example letter from Aalborg Municipality on 'Rejection of Social Assistance. The first two paragraphs state that: '*We hereby inform you that your application for Social Assistance has been rejected.*
You have not handed in your application for and documentation of need. You were notified that this should be handed in by {insert date} at the latest. [own translation]'

Example 3 – Letter detailing benefit payments to a Social Assistance recepient



Aalborg Kommune
Boulevarden 13
9000 Aalborg

Afsender
Ydelses- Og Socialcentret , John F. Kennedys Plads 1 B, 3. Sal
9000 Aalborg

851

Modtager

9000 Aalborg

Sidenummer
01
SE-nummer
29386668
Sortering
851 0340 4SA
Personnummer
[redacted]
Sagsbehandler
Tanja Barakovic

Udskrivningsdato
15.04.13

Beregningsoversigt 01.05.13 - 31.05.13

Forventet Beregning (Rubrik 0)

R	Beløbstype	
	Antal dage ud af mulige dage	(31/31)
1	Kontanthjælp ej forsørger	10.500,00
3	Indtægter til fradrag	0,00
	ATP	-90,00
12	Skat	-2.920,00
13	Særlig støtte § 34	1.165,14
	Nettoydelse i alt	8.655,14

Bruttoydelse/Bidrag (Rubrik 1)

	Kontanthjælp ej forsørger	10.500,00
	I alt	10.500,00

Skatteberegning (Rubrik 12)

	Bruttoydelse i alt		10.500,00
	Atp		-90,00
	A-indkomst		10.410,00
	Skattefradrag		-3.921,00
	Beskatningsgrundlag		6.489,00
	Skat (afrundet)	45%	2.920,00

Særlig støtte § 34 (Rubrik 13)

14	Boligudgift	3.915,14
13a	Grænsebeløb	-2.750,00
	Beregnet støtte	1.165,14
15	Overskydende indtægt §5.4 netto	0,00
	Reduceret særlig støtte	1.165,14
17	Nedsættelse pga. dagpengeloft	0,00
	Støtte til udbetaling	1.165,14
	Heraf tilb.pligtig terminshjælp	0,00

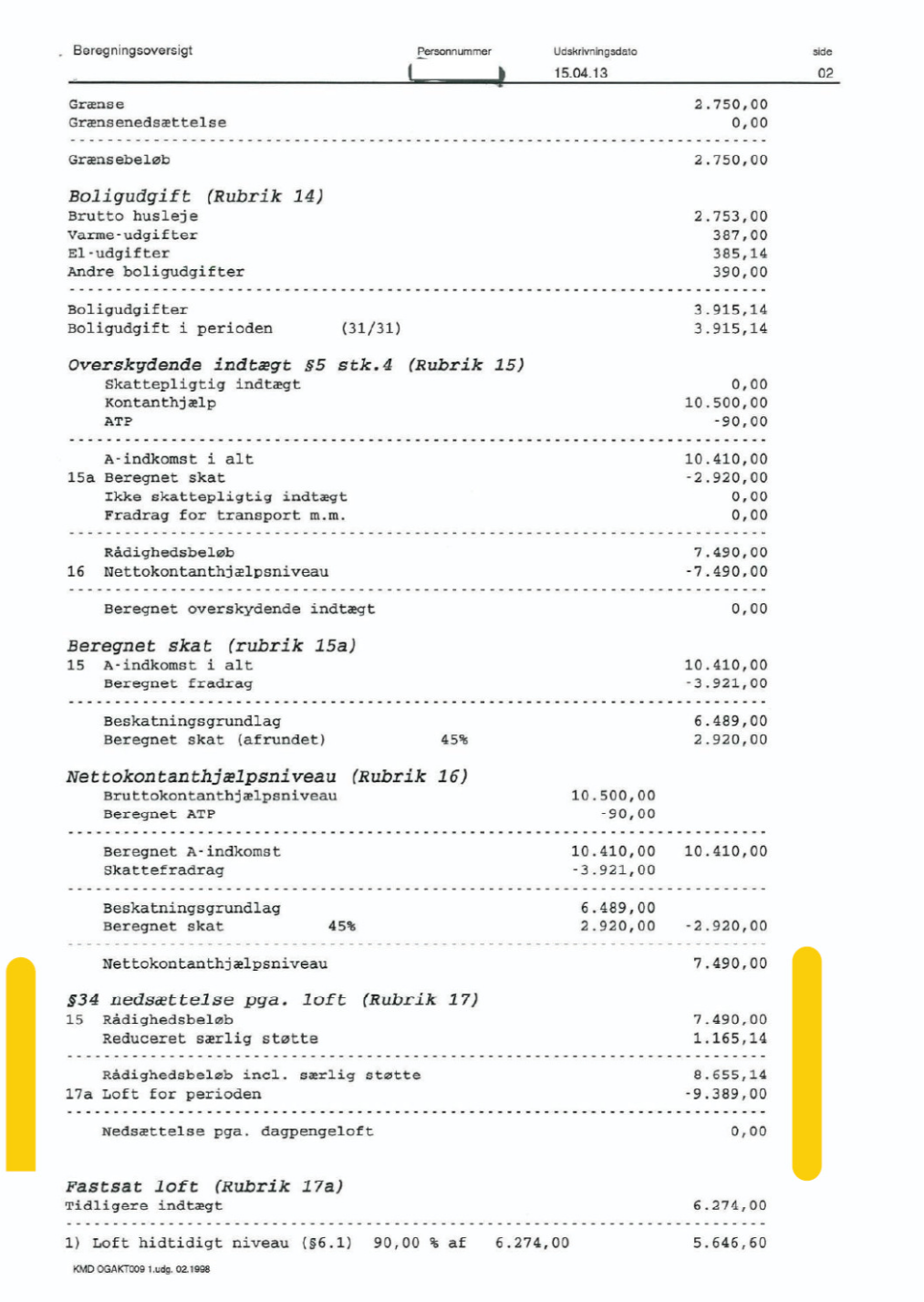
Grænsebeløb (Rubrik 13a)

Antal hjemmeboende børn pr. primo måned	0
Antal udeboende børn pr. primo måned	0

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Note: Continued on next page.

Example 3 – continued



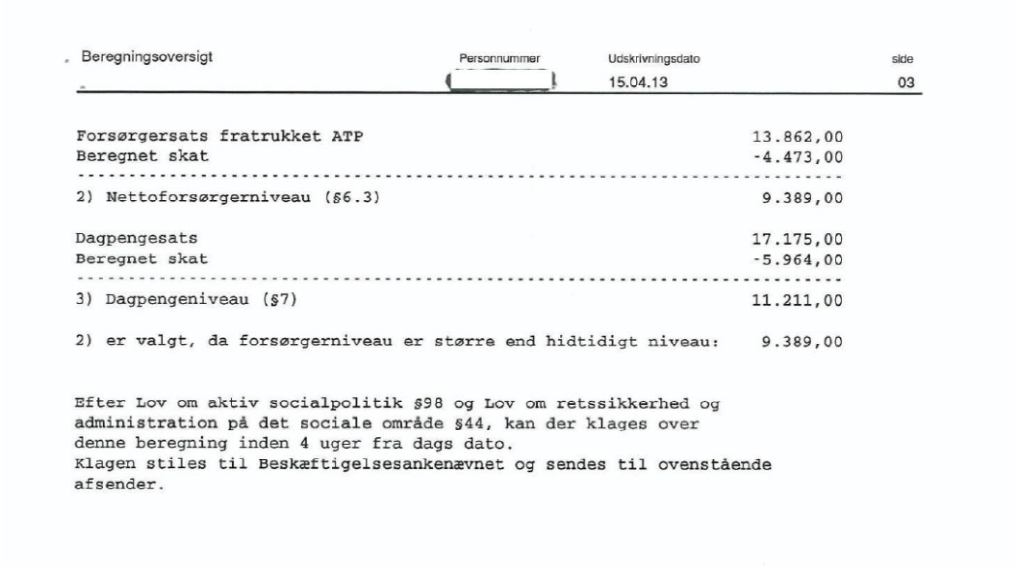
Beregningsoversigt | Personnummer | Udskrivningsdato 15.04.13 | side 02

Grænse			2.750,00
Grænsenedsættelse			0,00
Grænsebeløb			2.750,00
Boligudgift (Rubrik 14)			
Brutto husleje			2.753,00
Varme-udgifter			387,00
El-udgifter			385,14
Andre boligudgifter			390,00
Boligudgifter			3.915,14
Boligudgift i perioden	(31/31)		3.915,14
Overskydende indtægt §5 stk.4 (Rubrik 15)			
Skattepligtig indtægt			0,00
Kontanthjælp			10.500,00
ATP			-90,00
A-indkomst i alt			10.410,00
15a Beregnet skat			-2.920,00
Ikke skattepligtig indtægt			0,00
Fradrag for transport m.m.			0,00
Rådighedsbeløb			7.490,00
16 Nettokontanthjælpsniveau			-7.490,00
Beregnet overskydende indtægt			0,00
Beregnet skat (rubrik 15a)			
15 A-indkomst i alt			10.410,00
Beregnet fradrag			-3.921,00
Beskatningsgrundlag			6.489,00
Beregnet skat (afrundet)	45%		2.920,00
Nettokontanthjælpsniveau (Rubrik 16)			
Bruttokontanthjælpsniveau		10.500,00	
Beregnet ATP		-90,00	
Beregnet A-indkomst		10.410,00	10.410,00
Skattefradrag		-3.921,00	
Beskatningsgrundlag		6.489,00	
Beregnet skat	45%	2.920,00	-2.920,00
Nettokontanthjælpsniveau			7.490,00
§34 nedsættelse pga. loft (Rubrik 17)			
15 Rådighedsbeløb			7.490,00
Reduceret særlig støtte			1.165,14
Rådighedsbeløb incl. særlig støtte			8.655,14
17a Loft for perioden			-9.389,00
Nedsættelse pga. dagpengeloft			0,00
Fastsat loft (Rubrik 17a)			
Tidligere indtægt			6.274,00
1) Loft hidtidigt niveau (§6.1)	90,00 % af	6.274,00	5.646,60

KMD OGAKT009 1.udg. 02.1998

Note: Continued on next page.

Example 3 – continued



Beregningsoversigt | Personnummer [redacted] | Udskrivningsdato 15.04.13 | side 03

Forsørgersats fratrukket ATP	13.862,00
Beregnet skat	-4.473,00
2) Nettoforsørgerniveau (§6.3)	9.389,00
Dagpengesats	17.175,00
Beregnet skat	-5.964,00
3) Dagpengeniveau (§7)	11.211,00

2) er valgt, da forsørgerniveau er større end hidtidigt niveau: 9.389,00

Efter Lov om aktiv socialpolitik §98 og Lov om retssikkerhed og administration på det sociale område §44, kan der klages over denne beregning inden 4 uger fra dags dato.
Klagen stiles til Beskæftigelsesankenævnet og sendes til ovenstående afsender.

Note: Example letter from Aalborg Municipality detailing monthly benefit payments and how they are calculated. The individual in question receives Social Assistance (see highlighted line on first page). The letter then details housing support, and how that is calculated, taxes, before addressing caps of assistance (see highlighted section on second page). This individual is not capped as the total benefits of DKK 8,655.14 do not exceed 9,389.00.