Welfare Program Spillovers*

David Jinkins†
Copenhagen Business School and IZA

Elira Kuka‡
George Washington University, IZA and NBER

Claudio Labanca§
Monash University and IZA

July 2024

Abstract

Most research on the social safety net focuses on understanding its effects on recipients and their families. We show that these effects extend beyond recipients' families. Using a regression discontinuity design and administrative data, we study a Danish policy that drastically cut welfare benefits for refugees, leading to increased crime among the affected individuals. We link refugees to their neighbors and show large increases in crime among non-Danish neighbors. These spillover effects intensify over time and persist even when the direct effects on refugees' crime stop growing. Due to spillovers, we estimate an increase of up to 23% in the marginal value of public funds associated with leaving welfare benefits to their pre-reform level. We explore several mechanisms for the spillovers, such as changes in employment opportunities, welfare use, and policing. We find evidence most consistent with peer effects in crime among young, unmarried peers from the same country of origin.

JEL Classifications: I38, K42
Keywords: welfare programs, crime, spillovers

*We thank Lars Højgaard Andersen, Gordon Dahl, Tommaso Frattini, Rasmus Landersø, Mikkel Mertz, Ismir Mulalic, Dario Pozzoli, Kevin Schnepel, Marie Shultz-Nielsen and seminar participants at the Australian Public Economics Exchange, Bristol Workshop on Economic Policy, Copenhagen Business School, George Mason University, the George Washington University, HELP workshop, Monash University, National Taiwan University, the Rockwool Foundation, Texas Economics of Crime Workshop, Tulane University, Virtual Crime Economics seminar, the University of Maryland, and the University of Wisconsin IRP for helpful feedback. This research was supported by the Rockwool Foundation "Connecting Copenhagen" grant. All errors are our own.

†Department of Economics. Email: dj.eco@cbs.dk.

‡Department of Economics. Email: ekuka@gwu.edu.

§Department of Economics. Email: claudio.labanca@monash.edu.

1 Introduction

Social safety spending accounts for the largest share of overall government spending in many countries,[1] making welfare programs one of the primary means through which governments redistribute resources within a country. The design of effective welfare programs hinges on the accurate assessment of their impacts. While a substantial literature is dedicated to evaluating the effects of welfare programs on the recipients and their immediate families (e.g. Eissa and Liebman, 1996; Dahl and Lochner, 2012; Hoynes, 2019; Bitler et al., 2021), little is known on the impact of these programs beyond recipients' households.

A growing strand of research, however, indicates that social interactions, particularly among neighbors, can influence individuals' behaviour across various dimensions such as, for instance, crime (e.g. Damm and Dustmann, 2014) or human capital formation and accumulation (e.g. Chetty et al., 2016; List et al., 2020). The findings of this literature suggest that the effects of welfare programs may extend to individuals residing in proximity to the recipients. Nevertheless, analyzing these spillover effects poses considerable challenges as it requires plausibly exogenous variation in welfare payments and detailed information to link welfare recipients to individuals around them.

This paper studies the spillover effects of a welfare cut on the criminal behaviour of the recipients' neighbors in Denmark. To overcome the challenges usually associated with the study of welfare spillovers, we connect several administrative data sources in Denmark that allow us to link welfare recipients to individuals residing in the same building (i.e. neighbors), and to track the criminal behaviour of these individuals for over a decade. We combine these newly linked data on neighbors with the plausibly exogenous variation in welfare benefits deriving from a 2002 reform that sharply cut welfare benefits to refugees. We find large spillover effects from the welfare cut on criminal behaviour of neighbors. These effects are concentrated among non-Danish individuals, materialize soon after the welfare cut and persist for up to 10 years after the reform. Starting from these findings, we discuss several potential explanations for the spillover effects, including peer effects in crime, localized changes in policing efforts and spillover effects on welfare access and labor market outcomes. We find evidence consistent with the hypothesis that welfare spillovers are driven by peer effects in crime.

Denmark serves as an ideal setting for our study for several reasons. First, the distinctive characteristics of Danish data enable us to connect individuals residing in the same building.

[1] Spending on social protection accounts for roughly 35% of total government spending across OECD countries. The second-largest expenditure category is health, which represents 16% of overall spending (OECD, 2023).

Second, in 2002, Denmark implemented a reform that significantly reduced welfare benefits to refugees. Crucially, this welfare cut applied only to refugees granted residency on or after July 1^{st} 2002, many of whom had applied for residency long before the reform was announced. This aspect of the reform provides plausibly exogenous variation, allowing us to assess its impact on refugees' neighbors. While the direct effects of this reform on refugees have been previously examined (Dustmann et al., 2023), we explore the spillover effects.

Our empirical strategy consists of a regression discontinuity design based on the neighbors of refugees who received residency in the 16 months around the reform cutoff date. Our main estimates capture differences in crime outcomes of neighbors of refugees who were granted residency just after and just before July 1^{st} 2002. In order to reduce the concerns associated with the endogenous sorting of individuals across buildings in anticipation of the reform, we assign neighbours to buildings based on their residence information in the year prior to refugee arrival into the building. Consistent with the fact that neighbors so-defined would be unlikely to anticipate the reform, we find smooth neighbors' observables around the reform cutoff date.

In the first part of the analysis, we confirm the results in Dustmann et al. (2023) by studying the effects of the reform on individuals who were directly affected by the welfare cut. Consistent with Dustmann et al. (2023), we find that the reform leads to increased employment of refugees in the short run.[2] This increase in employment, however, is not large enough to offset the loss of income deriving from the cut of welfare benefits leading to a decrease of disposable income and increased crime among refugees. This increase in criminal activity is concentrated in property crimes (and specifically shoplifting), it is strongest in the first 3 years after the welfare cut, and it remains significant at least 10 years after the reform.

Next, we analyze the spillover effects on neighbors. We start by estimating spillover effects across all neighbors, finding small and insignificant effects. The overall effects however, hide substantial heterogeneity among neighbors. A breakdown of the effects between neighbors of Danish and non-Danish origin reveals large and significant effects on criminal behaviour of non-Danes, while the effects on Danes are insignificant. The spillover effects on non-Danes are significant for both property and non-property crime. Differently from the effects on crime of refugees that stabilize after 3 years, the spillover effects on non-Danes continue to increase in magnitude over time.

We estimate that a reduction in welfare benefits to refugees of 32% causes a 60% (9.5

[2]Our findings of positive effects on employment and labor income are also consistent with those of Rosholm and Vejlin (2010), who find an increase in job-finding rates and a reduction in labor force exit rates following the reform.

percentage points) increase relative to the control mean in the probability that a refugee's neighbor commits any crime within 10 years of the refugee's arrival. The large magnitude of these effects, combined with the evidence that the spillover effects intensify over time while direct effects remain stable, suggest that further peer effects among neighbors may amplify spillovers. Combining direct and spillover effects of the reform, our estimates imply that for each additional refugee who commits a crime due to the reform, there are approximately 3 non-Danish neighbors who commit a crime due to spillovers within a 10-year period. This finding aligns with the sizable social multipliers associated with criminal activity reported in the literature (e.g. Drago and Galbiati, 2012; Dustmann and Landersø, 2021).

The magnitude of these effects suggests that there may be non-negligible costs to reducing welfare benefits due to spillovers. To assess the relevance of these costs for the design of welfare programs, we calculate the social value of welfare benefits per government dollar spent, a measure known as the Marginal Value of Public Funds (MVPF) (Hendren and Sprung-Keyser, 2020). By factoring in the costs associated with neighbors' crime, we estimate a 5% to 23% increase in the MVPF, depending on whether victim costs are included, solely due to spillovers on neighbors' crime.

In the final part of the study, we analyze potential mechanisms to explain the spillover effects. First, we investigate whether changes in transfer payments and labor force participation among refugees influenced their neighbor' transfers receipt and work choices. These effects could arise from peer influences in welfare utilization and labor market engagement (e.g. Dahl et al., 2014a) or from heightened competition between refugees and non-Danes in the job market (e.g. Beaman, 2012). Changes in government transfers or labor market outcomes of neighbors may, in turn, result in increased crime among this group. We find no significant effects of the reform on transfer income, labor force participation or labor income of neighbors suggesting that this channel is unlikely to play a major role.

Second, we consider whether increased crime among refugees in a specific area could have led to an increase in policing efforts in that area, and therefore to more crime being detected (and not committed) among non-Danish neighbors, particularly if the police target non-Danes. In order to investigate this possibility, we estimate separate regressions for crimes committed in the same municipality where the refugee resides versus other municipalities. We find increased crimes in both the same and other municipalities, suggesting that increased localized policing effort cannot explain the totality of our effects. Consistent with this finding, we also find that the spillover effects are not only concentrated in municipalities with high shares of anti-immigrant votes or where police tend to discriminate more against immigrants.

Next, we consider whether the effects are driven by refugees and neighbors committing crimes together (i.e. a "partners in crime" story). Using detailed data linking all individuals involved in a crime record, we exclude neighbor crimes committed together with a refugee. We find similar sized-effects on crimes committed without a refugee, suggesting that spillovers are unlikely to be driven by a partners in crime story. This interpretation is also consistent with the fact that spillover effects continue to increase over time, while the direct effects of the reform on crime of refugees stabilize after 3 years; and with the evidence of significant spillovers in property and non-property crime while the direct effects on refugees tend to concentrate in property crimes.

Finally, we explore whether spillover effects are due to peer effects on crime that are transmitted through social interactions. As refugees are induced by the reform to commit crimes, those who are not caught may share their experiences with neighbors, potentially lowering the perceived risk and raising the perceived benefits of committing crime among neighbors. In line with this mechanism, we find that the effects are stronger among crime-prone peers who are more likely to interact with refugees. We observe the strongest effects when the refugee and neighbor are either from countries where the primary language belongs to the same language family or originate from the same country of origin, making interactions arguably easier. The effects are also pronounced if both individuals are young, unmarried, or childless, and thus more likely to commit crimes.

This is the first study to analyze the spillover effects of welfare spending on criminal behaviour. Despite a recent and growing literature showing that changes in welfare benefits have important effects on the criminal behavior of welfare recipients (Deshpande and Mueller-Smith, 2022; Dustmann et al., 2023), little is known about the effects of welfare on crime beyond the recipients and their families. We provide causal evidence of sizeable and persistent effects of a welfare cut on the criminal behaviour of recipients' neighbors. These findings have important implications for the evaluation and design of welfare programs. In our setting, taking spillovers into account leads to a sizeable increase of the marginal value of public fund associated with the reform.

Our findings of spillover effects from welfare reforms on criminal behaviour of recipients' neighbors complement the existing literature on spillover effects from government interventions (e.g. Bitler et al., 2021). While this literature focuses on the analysis of spillover effects among family members (e.g. Dahl et al., 2014a; Mueller-Smith et al., 2023) or coworkers (e.g. Dahl et al., 2014b; Labanca and Pozzoli, 2022), we show that spillovers from government interventions extend also to individuals who live in proximity to those directly affected by the intervention. We find that the spillovers on crime occur even in the absence of direct effects on the welfare take-up or the labor market outcomes of neighbors, which tend to be the main outcomes of interest in existing studies on spillovers. This finding underscores the importance of accounting for the effect of public policies on criminal behaviour in order to obtain a better assessment of the overall impact of government interventions.

Finally, we contribute to the literature on peer effects in crime (see Gavrilova and Puca, 2022 for a review). This literature largely focuses on the analysis of peer effects from the exposure to crime-prone peers in a variety of settings, such as schools (e.g. Billings et al., 2014), neighborhoods (e.g. Damm and Dustmann, 2014) or prisons (e.g. Bayer et al., 2009; Stevenson, 2017). Differently from this literature that relies on variation in the share of crime-prone peers to identify peer effects, we exploit plausibly exogenous variation in individuals' incentives to commit crime deriving from the welfare reform for the identification of peer effects. This allows us to isolate the peer effects on criminal behaviour of neighbors from other factors or peer characteristics that may vary with the composition of crime-prone peers in a neighborhood.

An exception to the aforementioned literature is Dustmann and Landersø (2021), which leverages the plausibly exogenous variation in individuals' criminal behavior resulting from the birth of a son compared to a daughter to estimate spillover effects on crime among neighbors. Unlike Dustmann and Landersø (2021), our focus centers on spillovers stemming from government-controlled welfare programs, bearing direct implications for policies aimed at reducing crime. Similar to Dustmann and Landersø (2021), our findings reveal substantial social multipliers in criminal activity within a neighborhood. By digging into the mechanisms underlying the spillovers, our study provides new evidence suggesting that these effects are unlikely to be driven by changes in police enforcement or labor market dynamics but rather by peer effects in crime among crime-prone peers who are more likely to interact with each other.

The reminder of the paper is organized as follows. Section 2 describes the institutional setting and the welfare reform. Section 3 describes the data. Section 4 outlines the empirical strategy. Section 5 presents our main results. Section 6 discusses potential mechanisms. Section 7 provides a number of robustness checks of our baseline results. Finally, Section 8 concludes.

2 Background on Refugees and Start Help

The Danish parliamentary election in November of 2001 was a sea change. For the first time in the modern era, right-leaning parties won an outright majority in parliament. Immigration had grown as a political issue, and immigration policy was a major contributor to the right-wing victory (Lidegaard, 2009). A wave of refugees in the 1990's had put pressure on the Danish welfare state, with welfare outlays to immigrants comprising 3.4% of total public spending (Matthiessen, 2009). The newly formed government proposed a reform to reduce welfare benefits for immigrants on March 1^{st}, 2002, and the law was passed on May 31^{st}, 2002 (Frederiksen, 2002). We will call this law the Start Help (*Starthjælp*) reform, because that was the name of the new, lower welfare payments applied to immigrants.

In Denmark, cash benefits (*kontanthjælp*) are paid to residents who do not have the means to support themselves. In 2002, the level of these benefits for a married parent of two children was $1,368 per month.[3] The Start Help reform reduced these benefits for Danish residents who had not been in Denmark for a total of seven of the last eight years (Frederiksen, 2002), the great majority of whom were immigrants. The reduced level of benefits for a married parent of two children was $847 per month – a 38% reduction in benefits.[4] The reform applied to all people who were granted Danish residency after July 1^{st} 2002. Those who had earlier residence were grandfathered into the old system of cash benefits. Couples arriving separately into Denmark received reduced transfers if at least one spouse in the couple was granted residency after that date.[5] While all new immigrants were affected by Start Help, our paper focuses on refugees who, due to the specific features of the Danish setting and the refugee protection program explained below, were less likely to be able to change their immigration behaviour (and thus residency date) in response to the reform.

In order to explain how refugees were affected by the Start Help reform, we briefly describe the process through which an asylum seeker becomes a refugee in Denmark.[6] In order to apply for refugee status, an asylum seeker must be physically in Denmark (Service, 2023). After registering with the police and a brief interview with the Danish Immigration Service, the asylum seeker is housed in an asylum center while they wait for a decision on whether they will receive refugee status. During this period, an asylum seeker is not allowed to work, although he may be offered an unpaid internship or Danish language classes. The average wait for a decision on refugee status at the time of the start help reform was around 16 months (Hvidtfeldt and Schultz-Nielsen, 2018). It is important for our empirical design that asylum decisions around our threshold were for applications lodged long before asylum seekers would have known about the Start Help reform.

If an asylum seeker's application is rejected, he will be moved into a process for repatriation. If the application is accepted, the asylum seeker will be granted refugee status.

[3]Figures based on the Danish kroner to US dollar exchange rate of 7.49 on July 1^{st} 2002 (Dreesen, 2023).

[4]The exact size of the benefit reduction varied in size according to marital status and number children. We directly estimate the drop in benefits for our sample in Section 5.

[5]We thus have three types of couples in our data. Type A couples, where both spouses arrived before July 1^{st} 2002, were unaffected by the reform. In type B couples, where both spouses arrived after that date, both spouses received the reduced transfers in Start Help. In type C couples, where one spouse arrived before and one after July 1^{st} 2002, their combined benefits were capped to the Start Help amounts, hence they also received the reduced transfers in Start Help.

[6]Here we merely sketch the process. For further details, see Bendixen (2023).

The Danish Immigration Service will also decide in which municipality the refugee shall live. They base this decision on both preferences the refugee may have expressed during the asylum application process, as well as annual quotas for refugees placed in each Danish municipality. Refugees are expected to remain in their placement municipality for three years under threat of losing their monthly benefit payment (Farrokhi and Jinkins, 2023).

In Denmark, new social housing (*almen boliger*) is built with a public subsidy, and then run by non-profit organizations which rent the apartments out at cost. A single non-profit organization often runs several buildings. In principle anyone can choose to live in these apartment buildings, but many of them are oversubscribed, with waitlists for many units measured in decades. As part of the regulations, either the fourth or the fifth empty apartment is given to the municipality rather than the next in line on the waitlist.[7] The municipalities then distribute these units to people with emergency need. Refugees needing a place to stay were placed in public housing through these municipal emergency need lists. The exact housing unit which a refugee was placed into was not influenced by the refugee's preferences or the characteristics of neighbors, but rather depended on which housing fitting family-size needs was the next to become available (Billings et al., 2022).

Refugees and other immigrants may lose their residency status and be deported if convicted of a crime. The rules governing deportation are complex and follow a step system based on how long an immigrant has resided in Denmark (Udlændinge- og Integrationsministeriet, 2019). The longer an immigrant has resided in Denmark, the more serious the crime must be for deportation to occur. Consistent with the fact that refugees and their neighbors in our setting are involved in relatively minor crimes (if any), in Section 4.1, we provide evidence suggesting that departure from Denmark is unlikely to be a relevant effect of the reform in our setting.

3 Data

This study relies on data from the Danish register. Danish register data is collected via various government bodies, and is made available to researchers through Denmark Statistics. The most important feature of the register data is that records can be linked via unique id numbers for individuals and residence addresses. The primary registers we use for this project are the census register (BEF), the income register (IND), the residency type register (OPHG), and the judicial registers (KRAF, KRSI). These data are collected by the government to provide services, assess tax liability, make sure people are legally in Denmark, and to create

[7] The rule on whether it is the fourth or fifth apartment allocated to the municipality varies across municipalities.

criminal records. Except for scrambling the personal identification numbers and addresses, the data are not top-coded or manipulated in any way. Below we briefly describe our sample. For a detailed description of how we construct it, see Appendix A.

We are interested in refugees and their non-family neighbors. In our refugee sample we include refugees and their spouses with family reunification visas who were granted residency within 16 months of July 2002, when the new policy went into effect. We keep only those that were 18 to 55 years old at the time they were granted residency. Following Dustmann et al. (2023), we drop refugees from Afghanistan and the Balkans. The Danish immigration service temporarily stopped processing Afghan asylum cases following the fall of the Taliban regime in late 2001, and they deemed Kosovo safe at the beginning of 2002. This affected the granting of refugee permits to people from these two regions around the time of the start help reform.

For reasons we will discuss further in the next section, we include in our sample of neighbors only those in a building where exactly one refugee family was placed within our time period.[8] We link every refugee to the building in which they are placed at the time they are granted residency. For each of these buildings, we define a refugee's neighbors as individuals who have been living in the building the year before the placement and who were between 16 and 55 years of age at the time the refugee was placed.[9] We drop anyone who was ever married to a refugee, as well as neighbors with a recorded immigration date after February 1st, 2001, the start of our window of analysis (2% of our observations). Our final sample includes 5,292 refugees and 13,687 neighbors, of whom 3,797 are either immigrants themselves or children of immigrants. We refer to this latter category as "non-Danes".

Table 1 contains descriptive statistics on demographic characteristics (Panel A), probabilities and average number of crime convictions (Panel B), and welfare usage and labor market participation (Panel C) for refugees and neighbors in our sample. We split neighbors into Danish (column 2) and non-Danish (column 3) groups. The table shows that refugees and immigrant neighbors are more likely to be married, have children and live outside the capital region than Danish neighbors. In terms of criminal activity, refugees are on average less likely to commit crimes than neighbors. Among neighbors, non-Danish neighbors are slightly more likely to commit crimes. However, these statistics include potential effects of the reform. Differences in crime rates between Danish and non-Danish neighbors are

[8] In Appendix Table A.1 we show summary statistics for the buildings in our sample. We have 1646 buildings, which contain a little over 13 neighbors on average, of whom an average of 8.6 are adults and thus in our sample. Around 50% of neighbors are of Danish origin.

[9] The crime registers only record convictions of people 15 and older. We use neighbors 16 and above at the time the refugee was placed so that we can have at least one year of data prior to refugee arrival for all neighbors.

minimal in the pre-reform period (see Section 5). Finally, regarding welfare use and labor market participation, refugees tend to rely more on welfare payments than neighbors and have lower earnings and labor market tenure rates. Among neighbors, Danes tend to have higher earnings and stronger labor market attachment than non-Danes.

4 Empirical Strategy

We use a regression discontinuity design (RDD) to estimate the spillover effects of Start Help. We proceed in 2 steps. In the first step, we confirm Dustmann et al. (2023)'s results by estimating the direct effects of the reform on refugees. We do so by comparing the outcomes of refuges who were granted residency just after and just before the reform cutoff date. The estimating equation takes the following form:

$$Y_{i\tau} = \beta_0 + \beta_1 1\left(t_i \geq c\right) + 1\left(t_i \geq c\right) g\left(t_i - c\right) + g\left(t_i - c\right) + X_i' \beta_2 + \epsilon_i \tag{1}$$

where $Y_{i\tau}$ is the outcome of refugee i measured τ years after residency; c is the cutoff date for eligibility to Start Help; t_i is the date in which refugee i is granted residency; $g()$ is a control function of the running variable; and X_i is a set of pre-determined controls.[10] The coefficient of interest, β_1, captures the average difference in the outcome Y between refugees who were granted residency just after and just before the reform cutoff date. As in Dustmann et al. (2023), we consider measures of labor supply and criminal behaviour as our key outcome variables. To confirm that the reform lowered welfare payments to refugees, we also examine the effect of the reform on the amount of transfers received from the government .

In the second step of the analysis, we estimate the spillover effects of the reform on refugees' neighbors. To identify the spillover effects, we compare outcomes of neighbors whose neighbouring refugee was granted residency just after the cutoff date to those whose neighboring refugee was granted residency just before the cutoff date. The estimating equation takes the following form:

$$Y_{-i\tau} = \gamma_0 + \gamma_1 1\left(t_i \geq c\right) + 1\left(t_i \geq c\right) f\left(t_i - c\right) + f\left(t_i - c\right) + Z_{-i}' \gamma_2 + \nu_{-i} \tag{2}$$

where $Y_{-i\tau}$ is the outcome of a neighbor of refugee i measured τ years after the refugee's arrival in the building; c and t_i are defined as in equation (1); $f()$ is a function of the running

[10] Following Dustmann et al. (2023) we include the following controls: age, gender, marital status, and fixed effects for number of children (up to four), continent of origin and municipality. In Section 7, we show that the results are robust to excluding these controls.

variable; and Z is a vector of predetermined controls.[11] The coefficient of interest in equation (2), γ_1, captures the average difference in the outcome Y between neighbors of refugees who were granted residency just after and just before the reform cutoff date. Since neighbors have citizenship (Danes) or residency (non-Danes) at the time of the reform, evidence of significant effects would imply that Start Help had spillover effects on the outcomes of individuals who were not directly targeted by the reform.

Our main outcome variables of interest are the likelihood of conviction (i.e., the extensive margin of criminal behavior) and the number of convictions (i.e., the intensive margin of criminal behavior) within 10 years from a refugee's arrival in the building. As part of our discussion of mechanisms, we also examine effects on labor supply and welfare payments received by neighbors.

When estimating equations (1) and (2), we control for linear functions of the running variable and we assign a greater weight to observations that are closer to the cutoff through triangular weighting. In a set of robustness checks, we show that the results are robust to alternative weighting and functional form assumptions on $g()$ and $f()$ (see Section 7). We measure the running variable at the highest available frequency of days, and cluster standard errors at the building level to account for correlated unobservables among refugees or neighbors in the same building.[12] As discussed in the previous section, our baseline sample comprises refugees who are granted residency within a window of 16 months around the cutoff date. In Section 7 we consider a range of alternative windows and find similar results.

In our context, there are additional challenges to estimating spillover effects. First, the same neighbor may be affected by multiple refugees. In such a many-to-one setting, it is unclear how to define the running variable especially for neighbors who are affected by refugees on both sides of the threshold. As a way to reduce this concern, we restrict our analysis to spillovers within a building which is the most detailed geographic unit available in our data.[13] Even under this restriction, however, the many-to-one problem persists in

[11] We include the following controls: age upon the refugee's arrival in the building, gender, marital status, and fixed effects for the number of children, continent of origin, and municipality. Furthermore, we also control for the following refugee characteristics: gender, age, marital status, and fixed effects for the number of children and continent of origin. Results are robust to excluding these controls from the analysis (see Section 7).

[12] We use the running variable at the highest available frequency of days to minimize potential concerns about inference from using discrete running variables (Kolesár and Rothe, 2018). However, we obtain similar results when using months relative to the cutoff date, as done in Dustmann et al. (2023), to construct the running variable.

[13] This restriction limits the set of refugees who can potentially affect a given neighbor but it comes at the cost of ignoring spillovers across buildings. This is however unlikely to be a major concern in our setting as past studies have shown that spillovers in crime in Denmark are stronger among people living within a two-minute walk from each other (Billings et al., 2022).

buildings that host multiple refugee families. Following an approach similar to those of other studies in the literature (see for instance, Dahl et al., 2014a), we further restrict the main analysis to buildings with one refugee family only. This comes at the cost of restricting the analysis to a subset of refugees. In Section 5 we show that the direct effects of the reform on refugees are consistent across refugees residing in buildings with only one refugee family and the entire sample of refugees. Then, in Section 7, we relax the assumption by examining the effects for buildings housing multiple refugees, focusing on those where all refugees are on the same side of the cutoff, and using the average date of residency among refugees to construct the running variable. In this analysis, we find quantitatively similar spillover effects.

Second, neighbors may endogenously sort across buildings in response to the reform. To address this concern, we assign neighbors to buildings based on their residence information one year prior to the arrival of the refugee. Consequently, at the time they are linked to the building, neighbors do not know whether or when their building will receive a refugee, and if the refugee will receive residency right before or right after July 1^{st}. Since not all neighbors may still reside in the same building at the time of the refugee's arrival, our estimates should be interpreted as an "intention to treat" effect of the reform on neighbors' behavior.

An alternative approach to the estimation of spillover effects would be to regress a neighbor's outcome on the refugee's outcomes predicted from equation (1) (i.e. 2SLS approach). In this case, the reform would act as an instrument for criminal behaviour of refugees. This approach, however, would require the reform to be a strong predictor of a refugee's criminal behaviour in the first-stage regression (1). As it will become clear in the next section, due to the limited number of refugees in our estimating sample, the effects of the reform obtained from equation (1) are not always precise enough to allow for reliable estimations in a 2SLS model (i.e. the F-stat of the excluded instrument is below 4). An additional advantage of using a reduced form approach of the type presented in this section is that it requires fewer assumptions. Specifically, it does not require to assume that spillovers only occur through a refugee's response to the reform (i.e. the exclusion restriction) allowing us to explore a large range of potential mechanisms for the estimated effects on neighbors. A 2SLS approach also requires the assumption that all affected refugees are affected in the same way by the reform (i.e. the monotonicity assumption), an assumption that is unlikely be satisfied in our setting where effects appear to be rather heterogeneous across refugees (see also Dustmann et al., 2023). For all these reasons, we base the analysis on a reduced form approach.

4.1 Identification

A causal interpretation of the direct effects, β_1, and indirect effects, γ_1, of Start Help requires that no other factors vary discontinuously at the cutoff date of the reform. Table 2 presents RDD balance tests based on equation (2). It shows smoothness at the cutoff of non-Danish neighbors' characteristics related to age, gender, marital status, number of children, and region of origin (Panel A); characteristics of the neighboring refugees or of the building (Panel B); the amount of government transfers received and labor market outcomes prior to the reform (Panel C); criminal convictions prior to refugee arrival in the building (Panel D); and criminal convictions as predicted by demographic characteristics of neighbors and refugees (Panel E). In Appendix Figures A.1 to A.5 we provide a graphical representation of the results presented in Table 2.

The evidence that emerges from these balance tests is consistent with the fact that the allocation of refugees to a building was primarily driven by the availability of suitable housing in our period of interest and, therefore, independent of the amount of welfare benefits received (see Section 2 for details). It also reflects the fact that we focus on neighbors who reside in a building the year before the refugee is assigned to the building, thus reducing the concerns related to the potential sorting of neighbors across buildings in response to the reform.

A causal interpretation of the RDD effects also requires that being granted residency just before or after July 1^{st} 2002 is out of the direct control of refugees or neighbors. Under this assumption, the density of refugees and their neighbors around the reform cutoff date should be smooth.

To check if this assumption holds in our setting, Panel A of Figure 1 plots the number of refugees granted residency each month from 16 months before to 16 months after the reform. In Panel B, we formally test for differences in the density of refugees around the reform cutoff date by estimating a version of equation (1) with the number of refugees granted residency each day as the outcome variable. Since this outcome variable does not refer to any particular refugee, we exclude individual control variables from this specification. In both figures, we fail to detect significant differences in the density of refugees at the cutoff. This is consistent with the fact that asylum decisions around the threshold were for applications lodged long before Start Help was announced, leaving refugees with no room for manipulation (see also Dustmann et al., 2023). Figure 2, analogous to the previous figure for refugees, shows no significant difference in the density of non-Danish neighbors whose neighboring refugee received residency before or after the cutoff.

Finally, it is important to note that, consistent with Dustmann et al. (2023), the absence of structural breaks in the characteristics or density of refugees and their neighbors at the cutoff suggests that the long-term effects of the reform on migration flows documented in Agersnap et al. (2020) are unlikely to pose identification issues in our setting. However, this does not rule out the possibility that the reduced generosity of welfare benefits may have prompted refugees and/or their neighbors to leave Denmark, potentially serving as a mechanism for our observed effects on crime. In Appendix Figure A.6, we find no evidence of a discontinuity at the reform cutoff date in the probability of attrition, defined as exiting the administrative records within 10 years from the refugee's residency date. This suggests that this type of response is unlikely to play a major role among immigrants in our analysis, who were already in Denmark at the time of the reform.

Overall, the results of this section indicate that manipulation or confounding factors are unlikely to be an issue in our setting. While we focus this analysis on non-Danish neighbors for whom we find significant spillovers, we reach similar conclusions from balance tests performed on the whole sample of neighbors and only neighboring refugees.

5 Results

5.1 Direct Effects on Refugees

We begin our discussion of the results by confirming that the introduction of Start Help did lead to lower welfare transfers to refugees. Figure 3 presents graphical evidence that mimics our estimated effects of Start Help on total transfer income (in thousands of 2021 US dollars) received in the first full year (Panel a), the first four years (Panel b), and the first ten years (Panel c) after residency. In each figure, the vertical red line just before July 1^{1st} 2002 separates the treatment from the control period. Evidence of a significant change in the outcome variable of interest at this cutoff capture the treatment effect of Start Help.[14] Appendix Table A.2 presents the corresponding RDD estimates obtained from estimating equation (1).

Refugees arriving after July 1^{st} 2002 experience a decrease of $9,211 in welfare benefits in their first full year since receiving a permit, equivalent to a substantial 32% decrease relative to refugees being granted a permit before Start Help was enacted. The difference in transfers

[14]In order to create figures that mimic corresponding RDD estimates from baseline specifications in Figure 3 and all similar-looking RD figures in the paper, we first create residualized outcome variables by regressing our outcome variables on the controls listed in section 2 and then adding back average outcome for untreated observations, following (Deshpande and Mueller-Smith, 2022). We then estimate equation (1) without the control variables. In doing so, we estimate separate linear functions of the running variable before and after July 1^{st} 2002, using triangular weights and clustering the errors at the building level. Based on these estimates, we predict transfers according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot these predicted transfers along with its 95% confidence interval. The black circles show average residualized transfers in two months bins, to present the underlying data.

grows over time, although at a decreasing pace, and four years after arrival, the accumulated difference in transfers doubles to $16,588. After those first four years, the difference in total benefits remains stable, suggesting no differences in yearly transfers survive beyond the first few years. Dustmann et al. (2023) shows that this decrease in transfer led to increased labor income and probability of working. Panels B and C of Appendix Table A.2 confirm these findings. However, the increase in labor income is small in magnitude (and insignificant) relative to the loss in transfer income, and many refugees remain unemployed. Overall, these findings indicate that refugees are economically worse off due to the Start Help reform.

Next, we analyze the effect of Start Help on refugees' criminal behavior, as also shown in Dustmann et al. (2023). Figure 4 presents graphical evidence of the effects on the likelihood of being convicted for a crime (i.e. extensive margin) and the total number of crime convictions (i.e. intensive margin) in the first ten years since residence in Panels A and B, respectively. We present the effects on both the extensive and intensive margins of criminal behavior in three rows. The first row displays effects on all non-traffic crime convictions, the second row on property crime convictions, and the last row on non-property crime convictions. Panel A of Table 3 presents the corresponding RDD estimates obtained from equation (1).

The results show that Start Help led to a 4.1 percentage point (p.p.) increase in the likelihood of refugees being convicted of any crime, a 32% increase relative to the control mean. These effects are statistically significant at the 5% level. They are driven by property crimes, where we find significant effects both at the intensive and extensive margins of crime convictions. These effects are relatively large in magnitude, with an estimated increase of 70% and 64% relative to the control mean in the likelihood and number of property crimes, respectively. The effects on non-property crimes are smaller and not statistically significant.

Panel B of Table 3 presents results from restricting the sample to refugees residing in buildings with only one refugee family, representing the peers our ”neighbors” sample is exposed to. These results are in line with the results of Panel A and indicate a significant increase in property crime. The magnitudes of the effects are similar in Panel A and Panel B, especially on the extensive margin. However, the effects in Panel B also are more imprecise due to the smaller sample size. Overall, the results suggest that Start Help led to substantial increases in property crime among its recipients.

5.2 Main Effects: Spillovers on Neighbors

In this section we present the main results of our analysis on the spillover effects of Start Help on criminal behaviour of individuals living in the same buildings as the affected refugees (i.e. neighbors). Table 4 presents results obtained from estimating equation (2) for all neighbors, as well as their non-Danish and Danish subsets in Panels A, B and C, respectively. We focus on neighbors aged 16-55 at the time of the refugee's arrival and consider crime convictions occurring within the initial 10 years after the refugee's arrival in the building. In Section 5.3, we examine the evolution of these effects over time.

The results show that Start Help did not significantly affect all neighbors' crime convictions. However, these overall effects mask important heterogeneity in effects across neighbors, with non-Danish neighbors experiencing significant increases in crime and Danish neighbors experiencing small and insignificant effects. The effects on non-Danish neighbors are large: being exposed to a refugee that arrived after July 1^{st} 2002, and who thus received fewer transfer benefits, leads to a 9.5 p.p. (60% of the control mean) and a 0.4 unit (104% of the control mean) increase in the likelihood and number of convictions for any crimes, respectively. Differently from the effects on refugees, the spillover effects on neighbors stem from rises in both property and non-property crime convictions.[15] Given that the effects are concentrated among non-Danish neighbors, we mainly focus on this group for the remainder of the paper. In Section 6, we return to the question of why these effects are observed for non-Danes but not for Danes.

Figure 5 provides graphical evidence of the effects on non-Danish neighbors, corresponding to panel B of Table 4. The figure shows a sharp and significant increase in crime convictions for non-Danes exposed to refugees that were impacted by Start Help relative to those exposed to refugees who were not affected by the reform at the cutoff. This increase is evident both on the intensive and extensive margins. In Appendix Figure A.7 we exclude controls for covariates from the estimation and find similar results.

Appendix Table A.4 explores the heterogeneity of the main effects across subgroups of non-Danish neighbors. The table presents results on the likelihood of being convicted of a crime within 10 years of a refugee's arrival. The subgroups include all individuals (column 1), those below vs. above the median age of 32 years old at the time of the refugee's arrival (columns 2 and 3), males vs. females (columns 4 and 5), parents vs. childless individuals (columns 6 and 7), and married vs. unmarried individuals (columns 8 and 9). We find that treatment effects are quite heterogeneous and concentrated among traditionally "crime-prone" groups, such as males, the youth, the childless, and the unmarried.

How large are the estimated effects, and what do they imply about the social multiplier of criminal activity? Our results indicate that for each additional refugee who commits a crime due to Start Help, a total of 2.9 additional neighbors also commit a crime, leading to

[15] Appendix Table A.3 presents results for subcategories of crimes, and shows that property crime convictions are driven by shoplifting and non-property crime convictions are driven by drug-related convictions. Moreover, the last column shows that Start help did not affect traffic crimes, which we exclude from our baseline measures of crime.

a social multiplier effect of 3.9.[16] This large multiplier is within the range of other estimates in the literature, as shown in Appendix Figure A.9. In particular, Dustmann and Landersø (2021), using a different methodology and quasi-experiment, estimate a social multiplier of 5 in a Danish setting.[17]

5.3 Effects over Time

The results above focus on refugees' and neighbors' criminal convictions in the first 10 years since the refugees were granted residency and assigned to the neighbors' building. Our data also allow us to study how these crime effects evolved over time. For both refugees and neighbors, we create outcomes that measure criminal convictions in the first full year, the first two full years, the first three full years, etc., since refugees' residency. We then estimate the RDD specification (2) for each of these outcomes, and plot the estimated coefficients against years since residency. Figure 6 presents results for refugees' and non-Danish neighbors' likelihood of a conviction, in Panels A and B, respectively. For both groups, we first study all crimes, then study property and non-property crimes separately.

Focusing on refugees (i.e., the left side of the figure), Figure 6 indicates that refugees who are eligible for lower welfare benefits as an effect of Start Help, exhibit a statistically significant two percentage point higher likelihood of being convicted of a crime in the first full year following residency, compared to refugees arriving before that date. This effect increases to four percentage points at three years post-residency for refugees eligible for Start Help. However, the effect stabilizes and remains of similar magnitude in the subsequent years.[18] These results suggest that Start Help mainly affected refugee's likelihood of committing crimes in the first few years after residency, but that it did not lead to a long-term increase in crime for these individuals. The other two figures that present results for refugees show that the overall crime dynamics reflect the dynamics of property crime convictions. Start Help does not appear to have had a significant impact on the likelihood of refugees committing non-property crimes in any of the years examined.

[16] We calculate the social multiplier in the following way: being exposed to Start Help leads to a 0.048 percentage point increase in the likelihood of any property crime among the 2,636 refugees in buildings with one refugee only, resulting in a total of 126.5 additional criminal refugees. Being exposed to Start Help also leads to a 0.095 percentage point increase in the likelihood of any crime among the 3,797 non-Danish neighbors, resulting in a total of 360.7 criminal neighbors. Hence, each additional refugee criminal leads to 2.9 additional criminal neighbors.

[17] Unlike Dustmann and Landersø (2021), we do not account for the strength of social ties in the estimation of the social multiplier. Accounting for such ties would require formulating and estimating a social interaction model, a task that is beyond the scope of this study.

[18] Appendix Figure A.10 presents results for the number of crime convictions instead of the likelihood of convictions and confirms this pattern of results.

The results for non-Danish neighbors, on the right side of Figure 6, reveal several patterns. First, the figure shows that non-Danish neighbors exposed to refugees eligible for lower welfare benefits due to Start Help are almost four percentage points more likely to be convicted of a crime in the two full years after exposure, compared to neighbors exposed to refugees eligible for higher benefits. This gap increases over time, reaching 9.5 percentage points ten years after exposure. Secondly, the effects on overall crime are driven by both property and non-property crimes, unlike the effects observed among refugees themselves. Specifically, focusing on property crimes, neighbors exposed to refugees arriving after July 1^{st} 2002 had only a slightly higher likelihood of committing crimes in the first few years after exposure. The size of these effects was smaller for neighbors compared to refugees during the first three years. However, while the effects stabilized among refugees from year 4 onward, they continued to increase among neighbors, ultimately leading to larger treatment effects for neighbors than for refugees. Turning to non-property crimes, we observe higher crime convictions among treated neighbors immediately after exposure to treated refugees. Non-property crimes appear to be the primary drivers of the overall crime effects in these initial years.

Overall, the fact that crime effects among neighbors are more persistent than effects among refugees suggests that the impact of the reform may self-reinforce over time among neighbors. This underscores the importance of considering spillover effects when evaluating the costs and benefits of a welfare reform. In the next section, we provide a comparison of these costs and benefits for the case of Start Help.

5.4 Cost-benefit Analysis of Start Help

Our analysis so far reveals important spillover effects from Start Help. Such effects may alter the balance between the costs and benefits associated with the reform. In this section, we use the marginal value of public funds (MVPF) framework to assess the impact of spillovers on social welfare. The MVPF offers a unifying approach for welfare analysis that is used to consistently evaluate government interventions (Hendren and Sprung-Keyser, 2020). In this section, we consider the MVPF of increasing reduced Start Help welfare benefits to their pre-reform level. That is, we compare social willingness to pay for the increased welfare payments to the government costs of funding them.

Following an approach similar to other related studies (e.g., Deshpande and Mueller-Smith, 2022), we define a recipient's willingness to pay as the decrease in transfers resulting from the reform. This is interpreted as the amount that a recipient would be willing to pay to receive the welfare benefits reduced by the reform. Since welfare transfers are taxed in

Denmark, we deduct from this amount the taxes that a recipient would have paid on the transfers if received. As for the costs of the program, the reform generated savings for the government due to reduced welfare transfers. From these savings, we deduct the missed tax revenues on welfare transfers and the small reduction in tax revenues from the labor income of refugees that resulted from the reform. In line with our baseline results, we evaluate benefits and costs over a 10-year period. Table 5 provides summary figures for these MVPF components, with detailed calculations shown in Appendix Tables A.5.

Taking the ratio of willingness to pay and savings for taxpayers, we estimate an MVPF of 0.972 associated with the reform. This estimate abstracts from the costs associated with increased crime resulting from the reform. To account for these additional costs, we distinguish between two different scenarios (see also Deshpande and Mueller-Smith, 2022). First, we consider only the costs of crime to taxpayers. Since we do not find significant effects on imprisonment, in our setting these costs consist of enforcement and prosecution expenses. Second, we add to government costs the costs to society of increased crime implied by the costs incurred by crime victims. These costs enter the MVPF calculation as an increase in the willingness to pay for welfare.

Table 5 summarizes the results of the analysis, with more details provided in Appendix Tables A.5 - A.7. If we include direct and spillover effects on crime but exclude costs to crime victims, we obtain an MVPF of 1.019. This is 5% higher than the MVPF of 0.973 obtained by excluding spillover effects. When we also consider costs to victims, we estimate an MVPF of 1.196, which is approximately 22% higher than the MVPF of 0.979 obtained by excluding spillovers while still considering costs to victims from refugees' crime, and 23% higher than the MVPF of 0.972 obtained by excluding all costs associated with crime.

It is worth noting that the analysis presented in this section aims to measure the costs and benefits for individuals physically in Denmark during the period of interest, thus abstracting from the effects associated with the reduced migration that resulted from Start Help (Agersnap et al., 2020).[19] Since our focus is on the contribution of spillovers to the MVPF, as long as crime spillovers generate additional costs for the government and victims, our conclusion that the MVPF inclusive of spillovers is higher than the MVPF without spillovers remains valid, although the specific values of the MVPF may change, if we account for reduced migration. While a full evaluation of the potential benefits (e.g., Agersnap et al., 2020) and costs (e.g., Foged et al., 2022) of reduced migration is beyond the scope of our analysis, in Appendix Table A.8 we provide MVPF estimates that, under some assumptions, factor in

[19]Figure A.6 rules out a significant increase in attrition among immigrants and refugees in our sample following the reform (see Section 4.1), suggesting that the reduced generosity of welfare did not have a substantial effect on migration decisions of immigrants who were already in Denmark.

the costs and benefits for the government associated with a reduction of 5000 immigrants per year from the reform, as estimated in Agersnap et al. (2020). In this scenario, we find that spillovers led to an increase of 17% in the MVPF inclusive of costs to victims relative to the case in which only the effects of the reform on crime of refugees are considered.

Overall, spillover effects on neighbors appear to play a significant role in shaping the costs and benefits associated with the welfare reform, suggesting that such effects should be accounted for when crafting and assessing welfare programs.

6 Mechanisms

The results so far indicate that refugees eligible for the reduced Start Help welfare benefits had higher property crime convictions in the 10 years subsequent to their arrival in Denmark. Furthermore, our analysis reveals that not only did refugees eligible for Start Help experience an uptick in criminal behavior, but so did non-Danish individuals residing in their buildings.

Why are non-Danish neighbors more likely to be convicted of crimes when exposed to crime-committing refugees? There are many possible explanations for this finding. First, changes in transfers and work among refugees might have led to changes in transfer and work among their neighbors as well, leading to increased criminal activity. Second, the rise in crime among refugees could have prompted heightened policing efforts in specific areas. Consequently, our observed increase in crime convictions might be attributed to this intensified policing rather than an actual increases in criminal activity. Third, increased criminal activity among refugees might have led to increased crime among neighbors through peer effects in criminal behaviour.

Below we discuss each of these three mechanisms in detail.

6.1 Changes in Work and Transfers

One potential explanation for the increased crime among the non-Danish neighbors of refugees is that the neighbors also experienced negative changes in transfers and/or labor market outcomes that led them to commit more crime. This could stem from peer effects influencing the uptake of welfare transfers (Dahl et al., 2014a) or from heightened competition in the labor market for neighbors resulting from the increased labor market participation of refugees (Beaman, 2012). These changes in transfers and work might have led to increased crime. To test this hypothesis, Appendix Table A.9 presents results obtained when comparing transfers and labor market outcomes among neighbors living in a building with a refugee that received residency just after July 1^{st} 2002 relative to just before. We do not find significant effects on total transfers, total earnings, net (transfers+earnings) income, nor likelihood of working or being out of the labor force, suggesting that this channel is unlikely to drive our results.

6.2 Changes in Policing

A second explanation is that the observed increase in neighbors' crime conviction is driven by changes in policing and not changes in criminal activity. Our identification strategy already rules out aggregate policing responses as a driver of our effects. Because we control for municipality fixed effects, in fact, our estimates effectively compare the crime convictions of neighbors within the same municipality. Hence, if a municipality increases policing after July 1^{st} 2002, due to increased crime by refugees, this increased policing would equally affect buildings and neighbors on both sides of the threshold within the municipality, making such a response irrelevant for our estimates.

For changes in policing to drive our effects, the policing response must be both very local and targeted at non-Danes. One such example is the following: assume individuals primarily shoplift in shops located close to their residence. When refugees eligible for Start Help begin committing property crimes in nearby supermarkets, the police may respond by assigning more officers to guard those supermarkets. This might lead to an increase in convictions for neighbors shoplifting in the supermarket due to the higher probability of detection, even in the absence of an actual change in criminal activity among neighbors. However, since the crime rates of Danes and non-Danes in our sample are similar (Table 4), for this to explain our results of spillovers on non-Danes only, the increased policing effort would also need to be specifically targeted at immigrants. In what follows, we explore this type of mechanism in more detail.

First, while we have no detailed data on the location where a crimes is committed within a municipality, we have data on its municipality. We can thus test whether the increased crime convictions among neighbors occurred in the municipalities to which the refugee is assigned after residency, or other municipalities. We present these results in Appendix Table A.10, for the likelihood of being convicted of any crimes as well as number of crimes. Looking at the means of the outcome variables, one can see that the majority of crimes are committed in one's municipality of residence, but that nevertheless some crime is committed outside. Moreover, the results show that having as neighbor a refugee that received Start Help led to increased crime committed both in one's municipality of residence and in other municipalities. While the treatment effect coefficients are larger for own municipality crimes, the effects are similar in percent terms when one takes into consideration the lower mean in other municipalities.

Second, the policing story above relies on the fact that the increased police effort is targeted at immigrants. If this were true, we might see more crime happening in municipalities with stronger anti-immigrant sentiment, where the administration in power may put more emphasis on detecting and punishing immigrants' crime. We test this hypothesis by analyzing whether our treatment effects are larger in municipalities where the voting share for anti-immigrant parties in 2001 was above the median, or in municipalities where the police are more likely to over-charge immigrants relative to Danes.[20] Appendix Table A.11 contains the results of this analysis and shows no evidence that this is the case. Treatment effects for both the extensive and intensive margins are generally similar in municipalities with low anti-immigrant party vote share, and for municipalities with low and high racist-police indices.

Overall, the findings of this section suggest that spillover effects are unlikely to be uniquely or primarily driven by localized changes in policing efforts.

6.3 Peer Effects in Crime

The third explanation for our findings on criminal convictions of non-Danish neighbors is the existence of peer effects in crime. The literature has proposed several mechanisms that could lead to peer effects in crime (for a review, see for instance Lindquist and Zenou, 2019 and Gavrilova and Puca, 2022). First, having criminal peers could affect perceptions about the benefits and costs of crime or could shift norms around crime. Second, criminal peers might provide crime-specific human capital. Finally, they could provide opportunities to commit crime together.

Our findings are not consistent with the last two explanations as the sole drivers of the peer effects. First, we only have evidence for refugees commiting more property crimes, while our evidence suggests that neighbors commit both more property and non-property crimes. Hence, refugees are unlikely to have provided non-property crime human capital to neighbors. Second, by linking criminal cases together, in Appendix Table A.12 we find that our effects are unchanged when we exclude crime convictions in which a refugee was convicted along with the neighbor, ruling out a *partners in crime* explanation for our peer effects.

The remaining hypothesis is that refugees who committed crimes as a result of the reform changed their neighbors' perceptions of the costs and benefits of committing crime, and

[20]For each municipality, we first calculate the ratio of charges to convictions among Danes and immigrants separately. This measure captures policing quality. We then create a measure of anti-immigrant policing by dividing the immigrant quality measure by the Danes' measure. We then divide municipalities into those with above- and below-median anti-immigrant policing.

led to increased crime. This hypothesis suggests that peer effects stem from interactions between refugees and neighbors, particularly those at higher risk of committing crimes who may be more sensitive to marginal changes in the perceived benefits of criminal activity. In line with this hypothesis, we find evidence suggesting that peer effects are strongest among groups more likely to routinely interact together, and those more likely to commit crimes. Specifically, we re-estimate our main effects on non-Danish neighbors separately for neighbors who match or do not match the refugee in five characteristics: whether they are from a country with a primary language that belongs to the same language family,[21] whether they are from the same country of origin, whether they were both young (32 or below, the median age) when the refugee arrived, whether they are both married, and whether they both have children. Figure 7 presents the estimated effects (height of the bar) as well as their confidence intervals. It shows that the effects of Start Help are larger if both the refugee and the neighbor are from countries with a primary language that belongs to the same language family, or are from the same country of origin, or if they are both young and therefore more likely to commit crime. At the same time, the effects are smaller if both refugee and neighbor are married or have children, suggesting that peer effects are weaker among individuals who are less likely to commit crimes.[22]

7 Robustness Checks and Placebos

Table 6 shows a set of robustness checks to the baseline specification of Table 4. In this analysis, we focus on the effects on non-Danes for whom we find significant spillovers, and we distinguish between spillover effects on all crimes (Panel A), property crimes (Panel B) and non-property crimes (Panel C). In order to allow for a direct comparison, column 1 reports the baseline effects on non-Danes also presented in Table 4.

Column 2 shows the results obtained from excluding pre-determined neighbors' controls from the baseline specification of equation (2). In this specification, we obtain qualitatively similar results suggesting that the main findings are not sensitive to pre-determined controls. In column 3, we use months – instead of days – since residence permit as the running variable, following (Dustmann et al., 2023). Our results are practically unchanged.

Column 4 presents the effects estimated while controlling for quadratic functions of the running variable on each side of the cutoff. These effects tend to be greater in magnitude and more significant than those obtained under the linear specification of column 1. Column 5

[21] We group countries into language families based on Lewis (2009).

[22] In line with the fact that spillovers stem from interactions and that refugees mainly interact with non-Danes, we fail to find significant spillover effects on low income Danes, who are more likely to commit crime than other Danes (see Appendix Table A.13).

presents the effects estimated from a specification in which we assign the same weight to all observations (i.e. uniform weighting). These effects are generally in line with the baseline results obtained under triangular weights, but tend to be slightly smaller in magnitude. Column 6 shows the effects obtained from restricting the analysis to observations within a window around the cutoff (i.e. bandwidth) selected using the data-driven procedure of Calonico et al. (2014a) (see also Calonico et al., 2014b; Imbens and Kalyanaraman, 2012). We find qualitatively similar results in this specification.

Finally, column 7 presents results when we estimate our model with the *rdrobust* command developed by Calonico et al. (2014b), which estimates treatment effects using local polynomials. The estimated effects are larger with this model. Taken together, the results of columns 3 and 7 suggest that, if anything, the effects obtained from the linear specification may provide a conservative measure of the spillover effects from Start Help.

In the appendix we present an additional set of robustness checks. In particular, to further assess the sensitivity of the results to the choice of the estimation window, Appendix Fig A.11 presents estimates of the spillover effects obtained from a range of windows spanning from 3 to 24 months around the cutoff date. With the exception of very small windows (3 to 4 months) for which effects are positive but noisy, the estimated effects tend to be positive and significant independently of the specific window used for the estimation. The magnitude of the effects tend to decrease with the length of the estimation window consistent with the fact that the negative effects of the reform are less severe among refugees further away from the cutoff date leading to lower spillover effects.

Appendix Fig A.12 presents the results of a placebo test in which we assign to each neighbor a random residency date drawn without replacement from all possible dates in the 16 months around the cutoff date of Start-Help. We estimate a placebo effect using this definition of treatment 500 times for each of our main outcomes, and plot the resulting distribution of estimates in Figure A.12. The one-sided p-values for all crimes (Panel A), property crimes (Panel B) and non-property crime (Panel C) are 0.002, and 0.01 and 0.016 respectively, suggesting that the estimated effects are unlikely to be driven by random factors.

For comparison with the related literature, in our baseline specifications we measure crime based on convictions (see also Dustmann et al., 2023). Appendix Tab A.14 shows the effects obtained by using the likelihood of being charged, rather than convicted, for a crime as an alternative measure of criminal behaviour. We find effects that are in line with the baseline suggesting that the difference between crime charges and convictions is limited in our setting.

Finally, we test the sensitivity of our results by including buildings with more than one refugee family. The first column of Appendix Table A.15 presents our baseline results, where we restrict the sample to buildings with at least one refugee family, for the likelihood of committing crimes and the number of crimes, in Panels A and B, respectively. In columns 2 to 6, we relax this restriction and include buildings with up to 6 families, as indicated in the column headers, as long as all refugee families have a permit date before or after July 1^{st} 2002. When there are multiple refugee families, the neighbors are assigned a date of residency permit (our running variable) equal to the average of the permit dates of all refugee families moving in the building in that same year. These estimated effects are similar in magnitude and significance to our baseline results.

8 Conclusion

In this paper, we investigate whether welfare programs' effects extend to the neighbors of welfare recipients. Using a regression discontinuity design, we find that Denmark's 32% reduction in welfare benefits for refugees resulted in a significant increase in property crime among refugees during their initial 10 years after the reform. Connecting these refugees to other individuals in their residential buildings, we find substantial and statistically significant increases in 10-year property and non-property crime among non-Danish neighbors. Notably, while the crime effects peak within the first 3 years for refugees, they persistently increase over time for their non-Danish neighbors, indicating a lasting shift in their criminal behavior.

We investigate various mechanisms that could underlie these effects. Our analysis dismisses changes in other transfers or labor market responses, as well as shifts in policing behavior, as the primary drivers of our observed effects. Instead, our findings appear to be more consistent with the existence of peer effects in crime. Future research could further examine the mechanisms driving spillover effects from welfare programs in different settings. For example, examining how changes in welfare payments may spill over from changes in welfare benefits of natives, rather than immigrants, could offer valuable insights into the functioning of spillovers through social connections.

References

Agersnap, O., Jensen, A., and Kleven, H. (2020). The welfare magnet hypothesis: Evidence from an immigrant welfare scheme in denmark. *American Economic Review: Insights*, 2(4):527–542.

Bayer, P., Hjalmarsson, R., and Pozen, D. (2009). Building criminal capital behind bars: Peer effects in juvenile corrections. *The Quarterly Journal of Economics*, 124(1):105–147.

Beaman, L. A. (2012). Social networks and the dynamics of labour market outcomes: Evidence from refugees resettled in the us. *The Review of Economic Studies*, 79(1):128–161.

Bendixen, M. C. (2023). The three phases of the asylum procedure. Accessed: 23 Aug 2023.

Billings, S. B., Deming, D. J., and Rockoff, J. (2014). School segregation, educational attainment, and crime: Evidence from the end of busing in charlotte-mecklenburg. *The Quarterly journal of economics*, 129(1):435–476.

Billings, S. B., Hoekstra, M., and Rotger, G. P. (2022). The scale and nature of neighborhood effects on children: Evidence from a danish social housing experiment. Technical report, National Bureau of Economic Research.

Bitler, M., Gennetian, L. A., Gibson-Davis, C., and Rangel, M. A. (2021). Means-tested safety net programs and hispanic families: Evidence from medicaid, snap, and wic. *The ANNALS of the American Academy of Political and Social Science*, 696(1):274–305.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014a). Robust data-driven inference in the regression-discontinuity design. *The Stata Journal*, 14(4):909–946.

Calonico, S., Cattaneo, M. D., and Titiunik, R. (2014b). Robust nonparametric confidence intervals for regression-discontinuity designs. *Econometrica*, 82(6):2295–2326.

Chetty, R., Hendren, N., and Katz, L. F. (2016). The effects of exposure to better neighborhoods on children: New evidence from the moving to opportunity experiment. *American Economic Review*, 106(4):855–902.

Dahl, G. B., Kostøl, A. R., and Mogstad, M. (2014a). Family welfare cultures. *The Quarterly Journal of Economics*, 129(4):1711–1752.

Dahl, G. B. and Lochner, L. (2012). The impact of family income on child achievement: Evidence from the earned income tax credit. *American Economic Review*, 102(5):1927–56.

Dahl, G. B., Løken, K. V., and Mogstad, M. (2014b). Peer effects in program participation. *American Economic Review*, 104(7):2049–2074.

Damm, A. P. and Dustmann, C. (2014). Does growing up in a high crime neighborhood affect youth criminal behavior? *American Economic Review*, 104(6):1806–1832.

Deshpande, M. and Mueller-Smith, M. (2022). Does welfare prevent crime? the criminal justice outcomes of youth removed from ssi. *The Quarterly Journal of Economics*, 137(4):2263–2307.

Drago, F. and Galbiati, R. (2012). Indirect effects of a policy altering criminal behavior: Evidence from the italian prison experiment. *American Economic Journal: Applied Economics*, 4(2):199–218.

Dreesen, J. S. (2023). Exchange rates. Accessed: 10 Aug 2023.

Dustmann, C. and Landersø, R. (2021). Child's gender, young fathers' crime, and spillover effects in criminal behavior. *Journal of Political Economy*, 129(12):3261–3301.

Dustmann, C., Landersø, R., and Andersen, L. H. (2023). Refugee Benefit Cuts. *American Economic Journal: Economic Policy*, forthcoming.

Eissa, N. and Liebman, J. B. (1996). Labor supply response to the earned income tax credit. *The quarterly journal of economics*, 111(2):605–637.

Farrokhi, F. and Jinkins, D. (2023). Root growing and path dependence in location choice: Evidence from danish refugee placements. Technical report, Working paper.

Foged, M., Kreuder, J., and Peri, G. (2022). Integrating refugees by addressing labor shortages? a policy evaluation. Technical report, National Bureau of Economic Research.

Frederiksen, C. H. (2002). Lov om ændring af lov om aktiv socialpolitik og integrationsloven. Accessed: 10 Aug 2023.

Gavrilova, E. and Puca, M. (2022). 10. peer effects in crime. *A Modern Guide to the Economics of Crime*, page 227.

Hendren, N. and Sprung-Keyser, B. (2020). A unified welfare analysis of government policies. *The Quarterly Journal of Economics*, 135(3):1209–1318.

Hoynes, H. (2019). The earned income tax credit. *The Annals of the American Academy of Political and Social Science*, 686(1):180–203.

Hvidtfeldt, C. and Schultz-Nielsen, M. L. (2018). Refugees and asylum seekers in denmark 1992-2016. Rockwool Foundation Report.

Imbens, G. and Kalyanaraman, K. (2012). Optimal bandwidth choice for the regression discontinuity estimator. *The Review of economic studies*, 79(3):933–959.

Kolesár, M. and Rothe, C. (2018). Inference in regression discontinuity designs with a discrete running variable. *American Economic Review*, 108(8):2277–2304.

Labanca, C. and Pozzoli, D. (2022). Constraints on hours within the firm. *Journal of Labor Economics*, 40(2):473–503.

Lewis, M. P., editor (2009). *Ethnologue: Languages of the World*. SIL International, Dallas, TX, USA, sixteenth edition. Accessed in 2011.

Lidegaard, B. (2009). *Denmark in the 20th Century*. Gyldendal.

Lindquist, M. J. and Zenou, Y. (2019). Crime and networks: Ten policy lessons. *Oxford Review of Economic Policy*, 35(4):746–771.

List, J. A., Momeni, F., and Zenou, Y. (2020). The social side of early human capital formation: Using a field experiment to estimate the causal impact of neighborhoods. Technical report, National Bureau of Economic Research.

Matthiessen, P. C. (2009). *Immigration to Denmark*. University Press of Southern Denmark.

Mueller-Smith, M. G., Reeves, J. M., Schnepel, K., and Walker, C. (2023). The direct and intergenerational effects of criminal history-based safety net bans in the us. Technical report, National Bureau of Economic Research.

OECD (2023). General government spending (indicator). *doi: 10.1787/a31cbf4d-en (Accessed on 26 October 2023)*.

Rosholm, M. and Vejlin, R. (2010). Reducing income transfers to refugee immigrants: Does start-help help you start? *Labour Economics*, 17(1):258–275.

Service, D. I. (2023). Adult asylum seeker. Accessed: 23 Aug 2023.

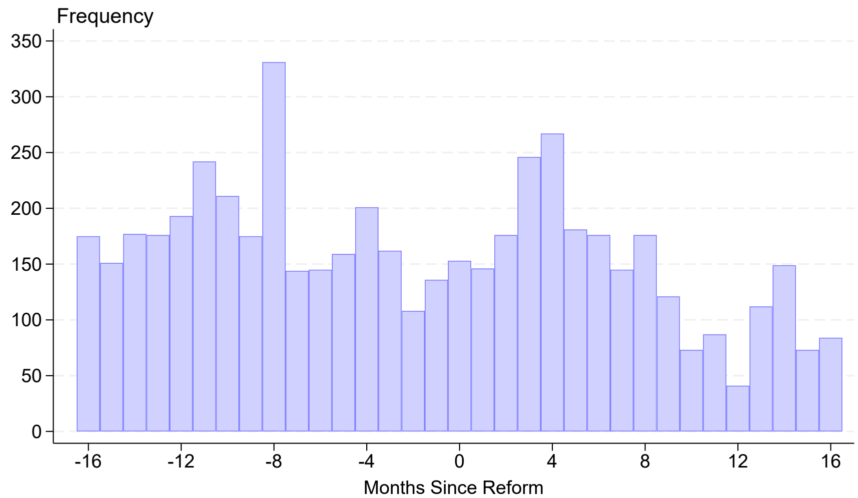
Stevenson, M. (2017). Breaking bad: Mechanisms of social influence and the path to criminality in juvenile jails. *Review of Economics and Statistics*, 99(5):824–838.

Udlændinge- og Integrationsministeriet (2019). Udlændingeloven, kapitel 4: Udvisning. LBK nr 1022 af 02/10/2019.

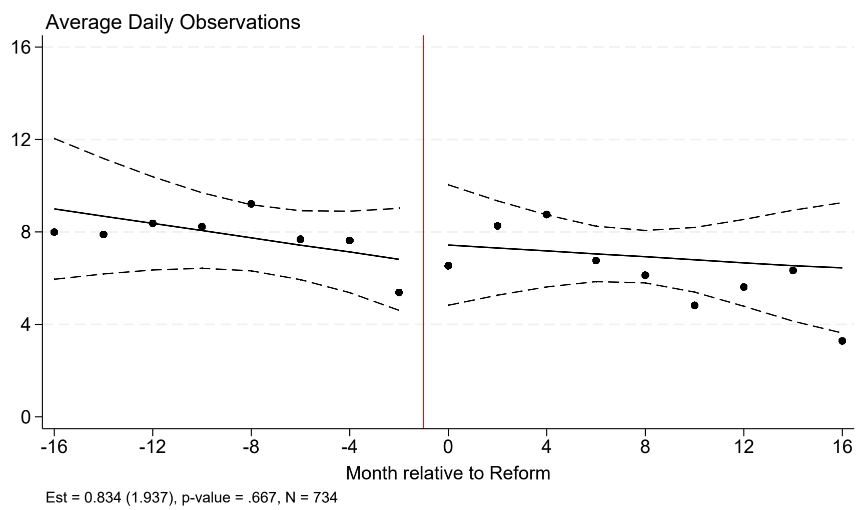
Figures

Figure 1: Refugee Density Around the Cutoff

A: Number of Refugees By Month of Residency Permit



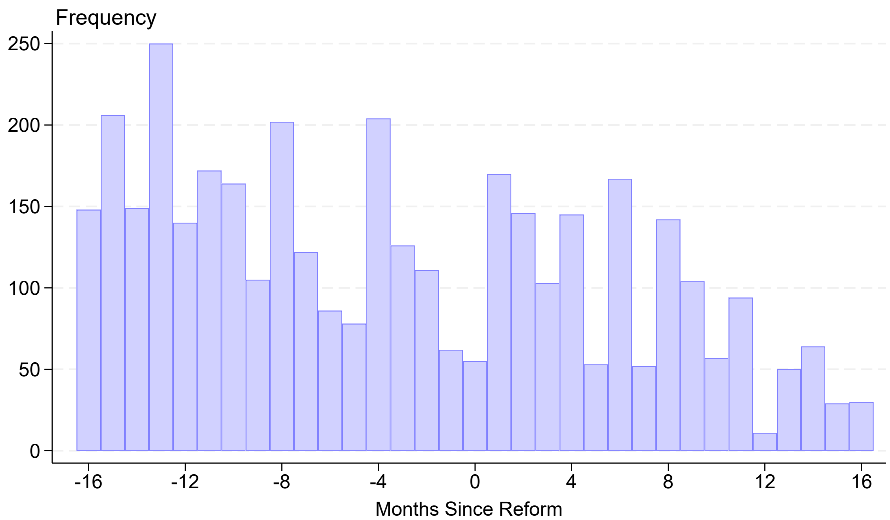
B: Effect of Start Help on Number of Refugees Per Day



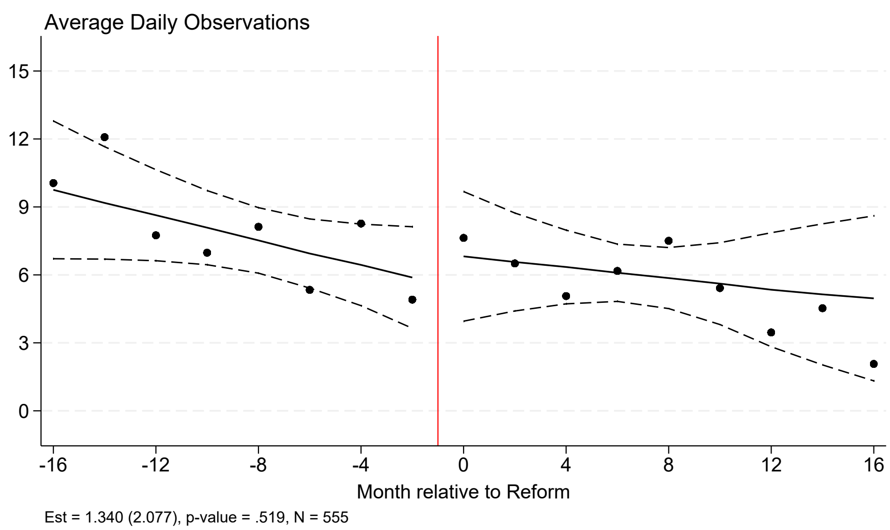
Notes: This figure shows whether there is extra density of refugees around the cutoff date of Start Help reform. Panel A presents a histogram of total number of refugees in two-month bins. Panel B present the effect of Start Help on the average number of refugees who received a residency permit in each day relative to July 2002. To create this figure, we first collapse the data at the day of residence permit level and capture the number of refugees who received the residence permit in each day. Second, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. We do not include any controls. Third, we predict number of refugees according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot these predicted number along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average number of individuals in two months bins, to present the underlying data. *Sample:* The sample includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan.

Figure 2: Non-Danish Neighbor Density Around the Cutoff

A: Number of Non-Danish Neighbors By Month of Refugee Permit

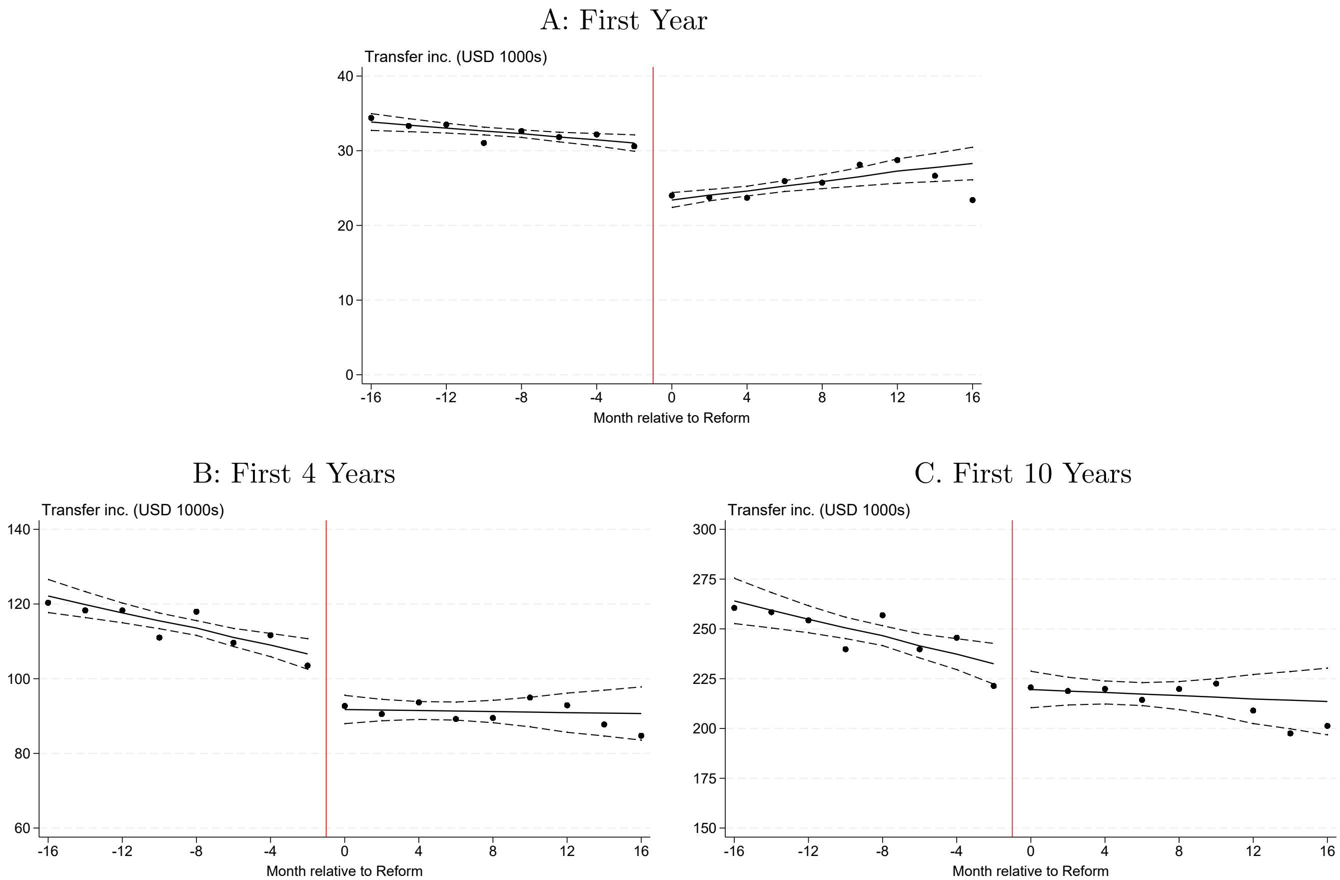


B: Effect of Start Help on Number of Non-Danish Neighbors Per Day



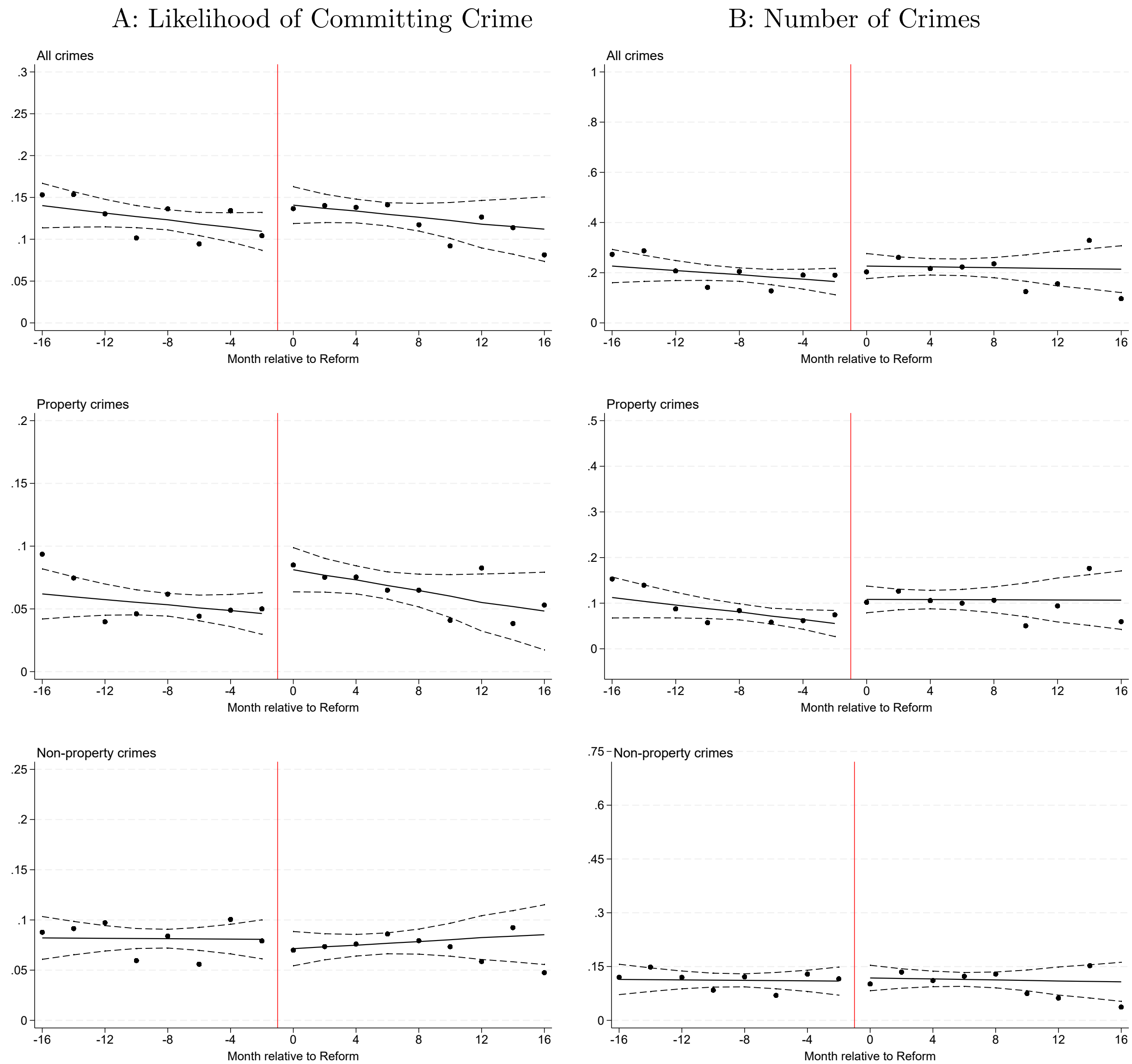
Notes: This figure shows whether there is extra density of refugees' non-Danish neighbors around the Start Help reform. Panel A presents a histogram of total number of neighbors in two-month bins. Panel B present the effect of Start Help on the average number of non-Danish neighbors with a refugee who received a residency permit in each day relative to July 1^{st} 2002. To create this figure, we first collapse the data at the day of the refugees' residence permit level and capture the number of neighbors in each day. Second, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. We do not include any controls. Third, we predict number of neighbors according to the number of days from the refugees' date of residence permit relative to July 1^{st}, and then plot these predicted number along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average number of individuals in two months bins, to present the underlying data. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure 3: Effect of Start Help on Refugees' Transfer Income



Notes: These figures present the effect of Start Help on refugees' total transfers in the first year, the first four years, and the first ten years since receiving a residency permit, in panels A to C respectively. We create these figures to mimic our estimation strategy. We first create residualized outcome variables – by regressing our outcome variables on the controls listed in section 2 and then adding back the control mean. We then estimate equation (1) without the control variables, hence just controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. We then predict transfers according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot these predicted transfers along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average residualized transfers in two months bins, to present the underlying data. *Sample:* The sample includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan.

Figure 4: Effect of Start Help on Refugees' 10-Year Crime

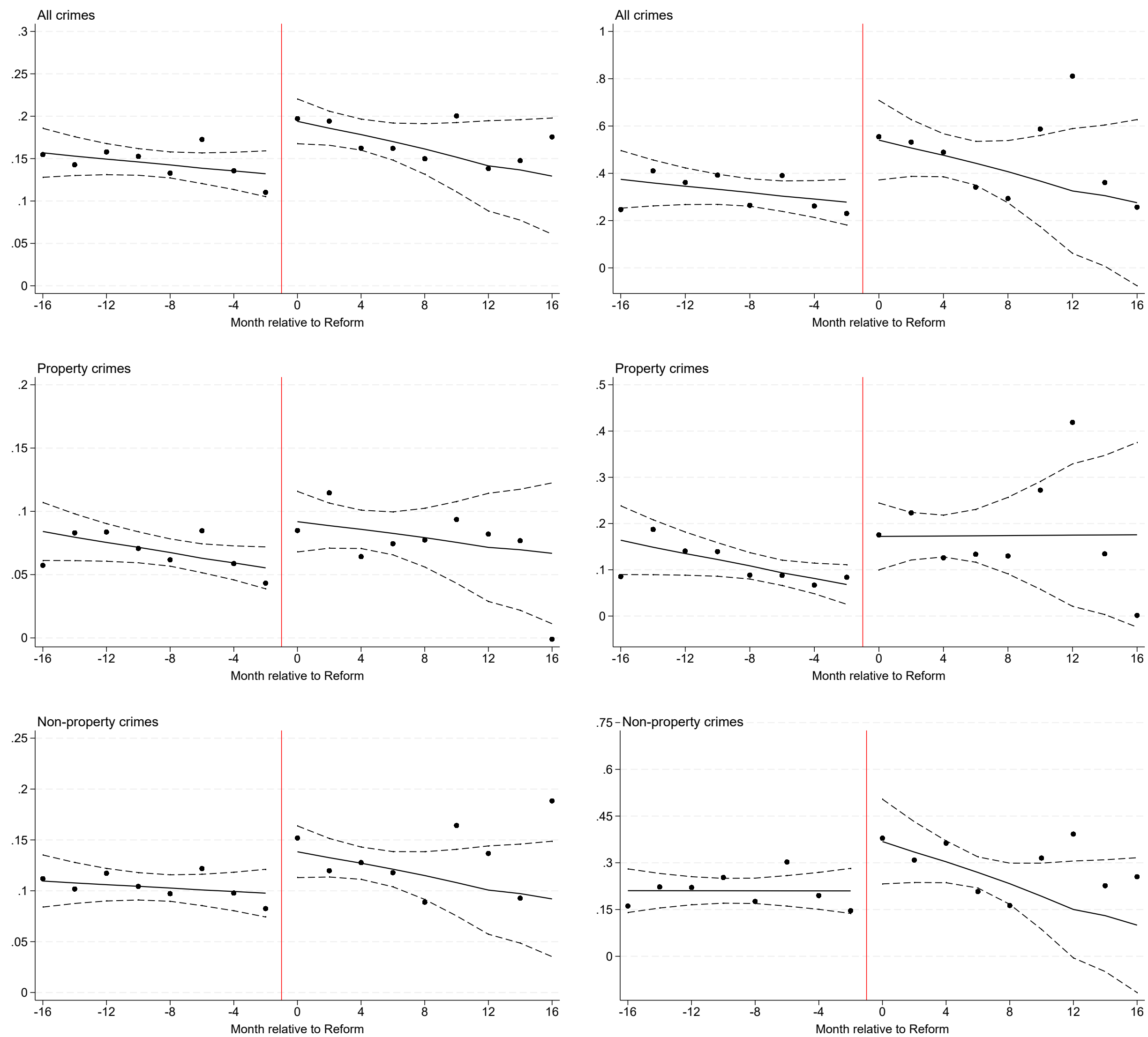


Notes: These figures present the effect of Start Help on refugees' crime convictions in the first ten years since receiving a residency permit. Panel A presents results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). Panel B presents results for total number of convictions instead of likelihood of convictions, for the same types of crime as Panel A. We create these figures to mimic our estimation strategy. We first create residualized outcome variables – by regressing our outcome variables on the controls listed in section 2 and then adding back the control mean. We then estimate equation (1) without the control variables, hence just controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. We then predict crime according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot the predicted outcomes along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average residualized crime convictions in two months bins, to present the underlying data. *Sample:* The sample includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan.

Figure 5: Effect of Start Help on Non-Danish Neighbors' 10-Year Crime

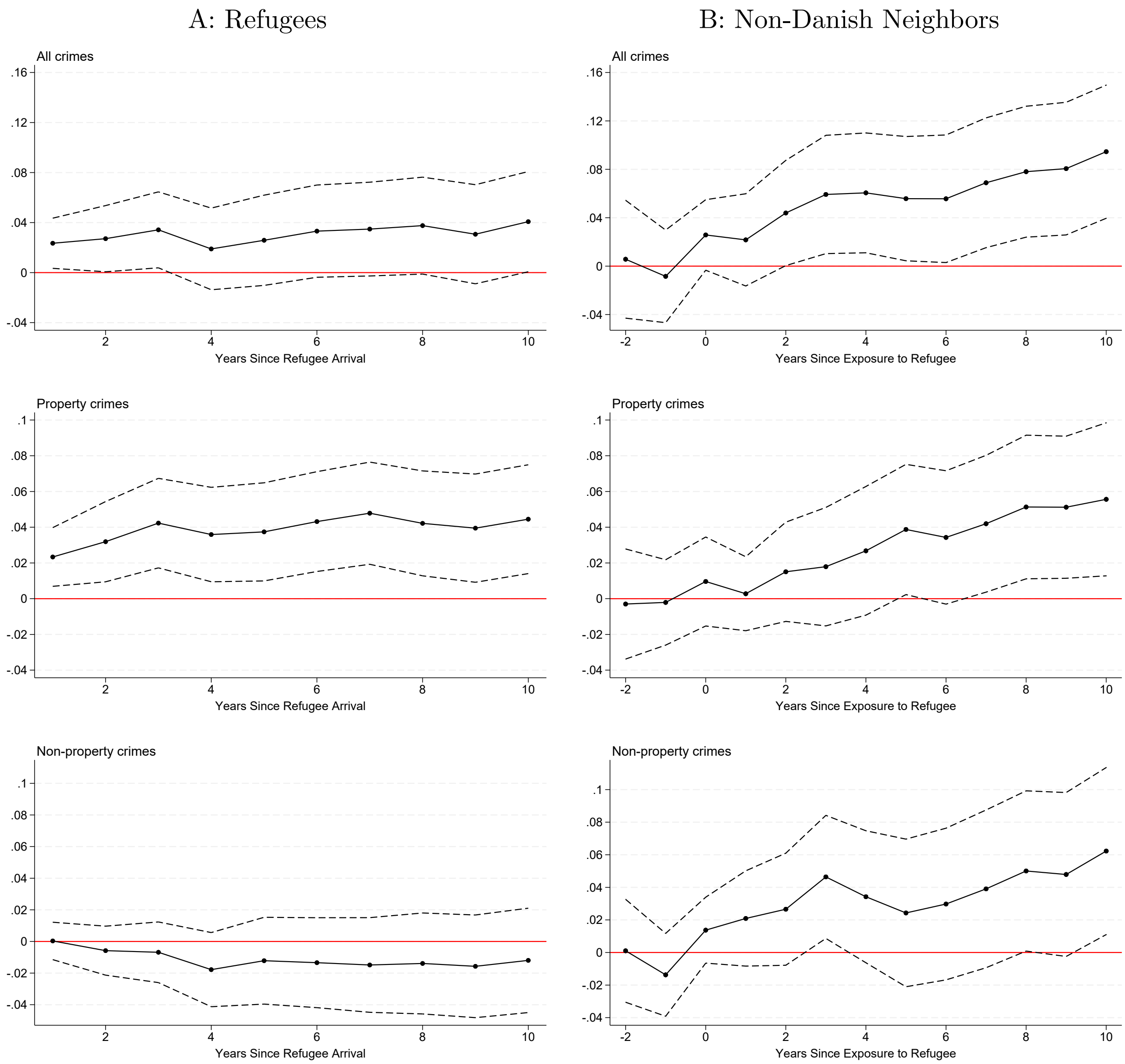
A: Likelihood of Committing Crime

B: Number of Crimes



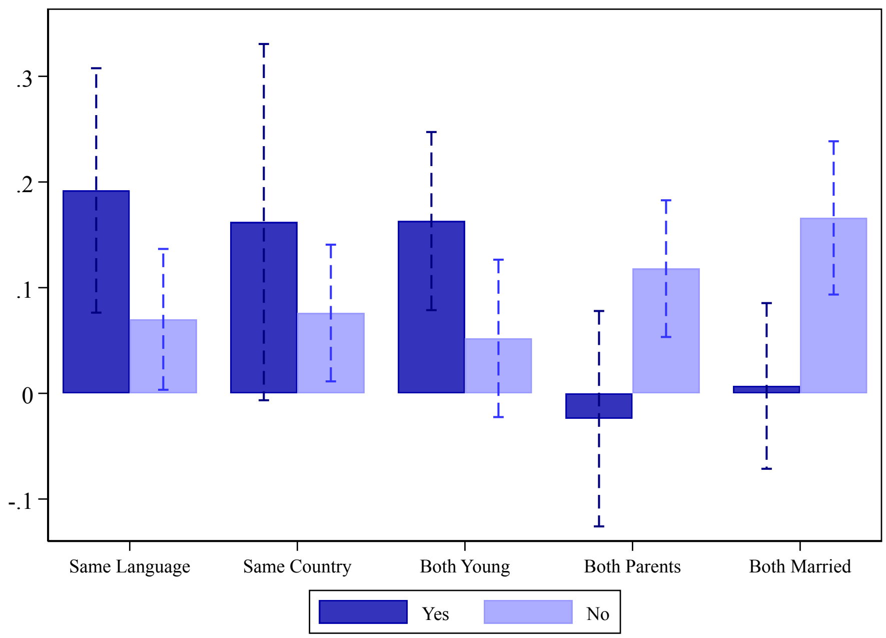
Notes: These figures present the effect of Start Help on refugees' non-Danish neighbors' crime convictions in the first ten years since the refugee's arrival in the building. Panel A presents results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). Panel B presents results for total number of convictions instead of likelihood of convictions, for the same types of crime as Panel A. We create these figures to mimic our estimation strategy. We first create residualized outcome variables – by regressing our outcome variables on the controls listed in section 2 and then adding back the control mean. We then estimate equation (1) without the control variables, hence just controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. We then predict crime according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot the predicted outcomes along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average residualized crime convictions in two months bins, to present the underlying data. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure 6: Effect of Start Help on Refugees' and Neighbors' Likelihood of Crime – Treatment Effects Over Time



Notes: These figures present the effect of Start Help on refugees' (Panel a) and their non-Danish neighbors' (Panel b) likelihood of crime convictions over time. For both groups, we presents results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). To create these figures, we first estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. The black circles show the estimated effect of Start Help one to ten years after refugee residence permit (as indicated by the x-axis), along with its 95% confidence interval. *Sample:* The sample for Panel A includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan. The sample for Panel B includes the neighbors of non-Danish origin of the refugees in Panel A. We also exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure 7: Effect of Start Help on non-Danish Neighbors' 10-Year Likelihood of Crime – Heterogeneity by Refugee-Neighbor Match



Notes: This figures present the heterogeneous effect of Start Help on non-Danish neighbors' 10-year likelihood of crime convictions. The height of each bar represents the effect of Start Help from estimating equation (1) on the sample of neighbors that match or do not match the refugee on the following characteristics: language, country of origin, young (median age of 32 or younger), parent, married. The bars represent the 95% confidence intervals. Same language refers to the case in which refugees and neighbors are from countries where the primary language belongs to the same language family. We group countries into language families based on Lewis (2009). *Sample:* The sample includes neighbors of non-Danish origin and their neighboring refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Tables

Table 1: Descriptive Statistics

	(1) Refugees	(2) Danish Neighbors	(3) Non-Danish Neighbors
Panel A: Demographic Characteristics			
Age at policy	32.908	32.238	32.666
	(8.326)	(10.428)	(9.629)
Female	0.452	0.420	0.431
	(0.498)	(0.494)	(0.495)
Married	0.749	0.124	0.558
	(0.434)	(0.330)	(0.497)
Children at Home	0.967	0.335	1.063
	(1.330)	(0.738)	(1.313)
Capital Region	0.199	0.331	0.411
	(0.399)	(0.470)	(0.492)
Panel B: Crime Convictions			
Any Crime	0.132	0.148	0.172
	(0.339)	(0.355)	(0.377)
Any Property Crime	0.070	0.080	0.084
	(0.254)	(0.272)	(0.277)
Any Non-Property Crime	0.076	0.112	0.121
	(0.265)	(0.315)	(0.327)
Number of All Crimes	0.218	0.448	0.455
	(0.751)	(1.748)	(1.648)
Number of Property Crimes	0.101	0.206	0.164
	(0.482)	(1.061)	(0.847)
Number of Non-Property Crimes	0.117	0.242	0.291
	(0.513)	(0.984)	(1.157)
Panel C: Welfare Benefits and Labor Market Outcomes			
Transfer inc. (USD 1000s)	218.033	132.743	175.117
	(145.132)	(142.205)	(153.650)
Labor inc. (USD 1000s)	118.815	314.957	169.259
	(168.873)	(267.444)	(210.243)
Years with Labor Income > 0	3.753	6.914	4.665
	(3.490)	(3.814)	(3.962)
Observations	5292	9890	3797

Notes: This table presents averages and standard errors for demographic characteristics (Panel A), crime convictions (Panel B), and labor market outcomes (Panel C) within the first ten years of residency for refugees and their neighbors. Column 1 shows statistics for refugees, while columns 2 and 3 show statistics for Danish and non-Danish neighbors, respectively. To ensure consistency with averages shown in other tables in the paper, observations are weighted using triangular weights. *Sample*: The refugee sample includes individuals (and their spouses) who received a residence permit from 16 months before to 16 months after July 2002, were aged 18-55 at the time of residency, and were not from the Balkans or Afghanistan. Neighbor samples include individuals living near these refugees, excluding those in buildings with multiple refugee families or aged outside 16-55.

Table 2: Balancing Tests of Non-Danish Neighbors

Panel A: Own Demographics	Age Exposed	Female	Married	Number Of Kids	From Asia	From Africa
Start Aid	0.953	-0.087*	-0.045	0.010	-0.068	0.074
	(0.809)	(0.050)	(0.052)	(0.262)	(0.077)	(0.069)
Mean	32.666	0.431	0.558	1.063	0.560	0.209
Panel B: Refugee and Building Characteristics	Refugee Age	Refugee Female	Refugee Married	Number Kids	From Asia	Building Size
Start Aid	-1.558	0.053	0.121	-0.158	0.033	9.000
	(2.493)	(0.132)	(0.103)	(0.203)	(0.128)	(13.467)
Mean	30.339	0.401	0.708	0.468	0.668	42.306
Panel C: Own Income Pre-Exposure	Transfers	Earnings	Earn>0	OLF		
Start Aid	-0.090	-1.880	-0.109	0.025		
	(3.296)	(3.486)	(0.095)	(0.027)		
Mean	32.586	19.841	0.827	0.066		
Panel D: Own Crime Pre-Exposure	All		Property		Other	
	Any	Number	Any	Number	Any	Number
Start Aid	0.010	-0.006	-0.001	-0.009	0.005	0.004
	(0.025)	(0.038)	(0.015)	(0.021)	(0.019)	(0.026)
Mean	0.071	0.096	0.045	0.056	0.034	0.040
Panel E: Predicted Own Crime	All		Property		Other	
	Any	Number	Any	Number	Any	Number
Start Aid	0.020	0.079	0.009	0.028	0.016	0.051
	(0.014)	(0.059)	(0.006)	(0.026)	(0.014)	(0.037)
Mean	0.172	0.455	0.084	0.164	0.121	0.291
Obs.	3797	3797	3797	3797	3797	3797

Notes: This table presents balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' own demographic characteristics (Panel A), the characteristics of the refugees they are exposed to and of the building they live in (Panel B), their income and earnings and labor force participation (Panel C) and their crime convictions in the two years prior to being exposed to the refugee (Panel D). In Panel (E) we use all refugees' and neighbors' demographic characteristics as well as neighbors labor market outcomes in the two years prior to refugee arrival to predict crime convictions and estimate the effect of Start Help on this predicted crime. The columns headings list the specific outcome variable. For all these results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. Here we do not control for the demographics listed in section 2. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table 3: Effect of Start Help on Refugees' 10-Year Crime Convictions

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Panel A: All Refugees						
Start Aid	0.041**	0.077	0.045***	0.065**	-0.012	0.011
	(0.020)	(0.047)	(0.016)	(0.027)	(0.017)	(0.035)
Mean Y	0.132	0.218	0.070	0.101	0.076	0.117
Mean Y Pre Start Help	0.127	0.205	0.062	0.092	0.078	0.113
Number of Refugees	5292	5292	5292	5292	5292	5292
Panel B: Buildings with 1 Refugee						
Start Aid	0.028	-0.021	0.048**	0.040	-0.040*	-0.061
	(0.031)	(0.066)	(0.024)	(0.038)	(0.023)	(0.050)
Mean Y	0.133	0.204	0.071	0.098	0.074	0.106
Mean Y Pre Start Help	0.123	0.198	0.059	0.087	0.077	0.111
Number of Refugees	2636	2636	2636	2636	2636	2636

Notes: This table presents the effect of Start Help on refugees' crime convictions in the first ten years since receiving a residency permit. Panel A presents results for all refugees and Panel B presents results for refugees who were assigned to buildings with no other refugee family arriving in the same window. The columns indicate the outcome variables such as the likelihood of being convicted and number of convictions for any (non-traffic) crimes (columns 1 and 2), property crimes (columns 3 and 4) and non-property crimes (columns 5 and 6). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan.

Table 4: Effect of Start Help on Neighbors' 10-Year Crime Convictions

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Panel A: All Neighbors						
Start Aid	0.004	-0.020	-0.002	-0.012	-0.001	-0.008
	(0.022)	(0.108)	(0.017)	(0.057)	(0.018)	(0.061)
Mean Y	0.155	0.450	0.081	0.194	0.114	0.256
Mean Y Pre Start Help	0.146	0.418	0.078	0.183	0.105	0.235
Number of Neighbors	13687	13687	13687	13687	13687	13687
Panel B: Non-Danish Neighbors						
Start Aid	0.095***	0.400***	0.056**	0.154**	0.062**	0.246**
	(0.028)	(0.145)	(0.022)	(0.060)	(0.026)	(0.116)
Mean Y	0.172	0.455	0.084	0.164	0.121	0.291
Mean Y Pre Start Help	0.157	0.384	0.075	0.134	0.113	0.250
Number of Neighbors	3797	3797	3797	3797	3797	3797
Panel C: Danish Neighbors						
Start Aid	-0.035	-0.191	-0.026	-0.071	-0.029	-0.120*
	(0.026)	(0.131)	(0.019)	(0.073)	(0.021)	(0.068)
Mean Y	0.148	0.448	0.080	0.206	0.112	0.242
Mean Y Pre Start Help	0.141	0.431	0.079	0.201	0.103	0.230
Number of Neighbors	9890	9890	9890	9890	9890	9890

This table presents the effect of Start Help on neighbors' crime convictions in the first ten years since being exposed to a refugee. Panel A presents results for all neighbors, Panel B presents results for neighbors of non-Danish origin, and Panel C presents results for Danish neighbors. The columns indicate the outcome variables such as the likelihood of being convicted and number of convictions for any (non-traffic) crimes (columns 1 and 2), property crimes (columns 3 and 4) and non-property crimes (columns 5 and 6). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table 5: Cost-Benefit Analysis of Start Help

	Amount	*Notes*
Panel A: MVPF components		
1. Change in welfare transfers net of taxes	42548153.549	See Appendix Table A.5
2. Changes in tax revenues from labor income	1213306.810	See Appendix Table A.5
3. Total savings to taxpayers	43761460.359	1+2
4. Total enforcement and prosecution costs from refugees	55478.103	See Appendix Table A.6
5. Total enforcement and prosecution costs from neighbors	1971640.762	See Appendix Table A.6
6 Costs to victims from crime of refugees	238730.510	See Appendix Table A.6
7. Costs to victims from crime of neighbors	7122973.830	See Appendix Table A.6
Panel B: MVPF excluding costs to victims		
Base MVPF (ignore effects on crime)	0.972	1 divided by 3
MVPF adding refugees' crime	0.974	1 divided by (3 + 4)
MVPF adding also neighbors' crime	1.019	1 divided by (3 + 4 - 5)
Panel C: MVPF including costs to victims		
Base MVPF (ignore effects on crime)	0.972	1 over 3
MVPF adding refugees' crime	0.979	(1 + 6) over (3 - 4)
MVPF adding also neighbors' crime	1.196	(1 + 6 + 7) over (3 - 4 - 5)

Notes: This table shows the details behind the marginal value of public funds (MVPF) calculation. Panel A details the MVPF components. Panels B and C describe how to combine these components to obtain the MVPF. The amounts in Panel A are in 2021 US dollars. To allow for a comparison between average effects obtained on different samples of refugees and non-Danish neighbors, we consider total amounts, rather than average amounts, obtained by multiplying average amounts by the number of refugees (5292 individuals) or neighbors (3797 individuals) in our sample. Appendix Tables A.5 - A.7 provide detailed calculations behind each component reported in Panel A.

Table 6: Effect of Start Help on non-Danish Neighbors' Likelihood of Committing Crime Within 10 Years – Sensitivity to Specification

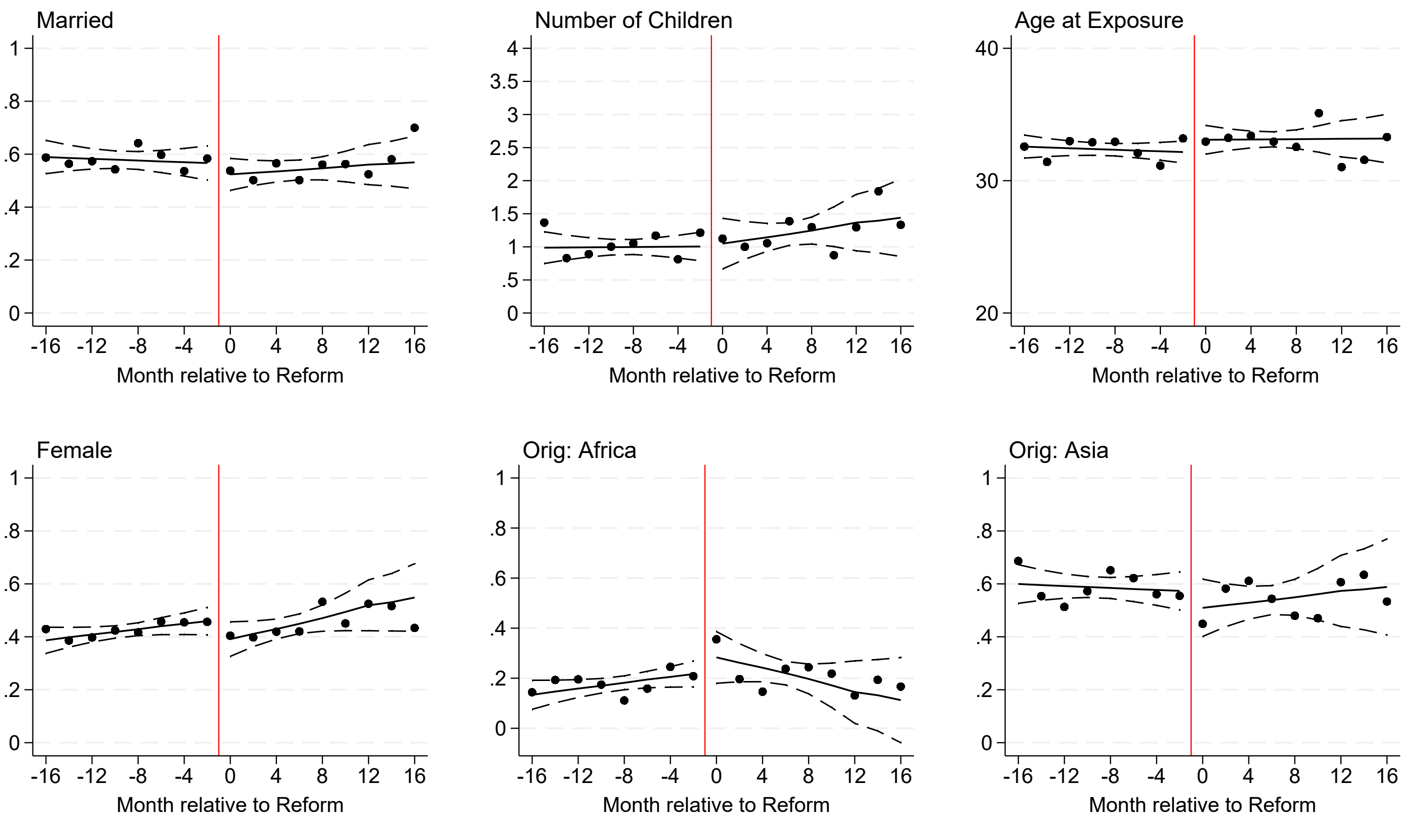
	Baseline Model	No Controls	Run Var: Months	Quadratic Spline	No Weights	Optimal Bdwdth	RD-Robust
Panel A: All crimes							
Start Aid	0.095***	0.077**	0.095***	0.145***	0.076***	0.216***	0.255***
	(0.028)	(0.030)	(0.028)	(0.039)	(0.028)	(0.071)	(0.042)
Mean Y	0.172	0.172	0.172	0.172	0.170	0.181	0.181
Mean Y Pre Start Help	0.157	0.157	0.157	0.157	0.157	0.151	0.151
N Neighbors	3797	3797	3797	3797	3797	856	856
Panel B: Property crimes							
Start Aid	0.056**	0.043**	0.057***	0.088***	0.046**	0.126**	0.116***
	(0.022)	(0.020)	(0.021)	(0.030)	(0.021)	(0.053)	(0.030)
Mean Y	0.084	0.084	0.084	0.084	0.085	0.088	0.088
Mean Y Pre Start Help	0.075	0.075	0.075	0.075	0.075	0.070	0.070
N Neighbors	3797	3797	3797	3797	3797	888	888
Panel C: Non-property crimes							
Start Aid	0.062**	0.054*	0.062**	0.102***	0.045*	0.118*	0.200***
	(0.026)	(0.030)	(0.025)	(0.038)	(0.025)	(0.061)	(0.045)
Mean Y	0.121	0.121	0.121	0.121	0.120	0.136	0.136
Mean Y Pre Start Help	0.113	0.113	0.113	0.113	0.113	0.112	0.112
N Neighbors	3797	3797	3797	3797	3797	924	924

This table presents the sensitivity of the effect of Start Help on non-Danish neighbors' likelihood of crime convictions in the first ten years since refugee arrival. We present results for the likelihood of any crime, property crimes and non-property crimes, in Panels A to C respectively. Column 1 presents results from our baseline specification, where we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. Column 2 shows results when we do not include any demographic controls. In Column 3 we show sensitivity to using "months since July 2002" as the running variable, similar to Dustmann et al. (2023). In Column 4 we allow for a quadratic function of our running variable. In Column 5 we do not use triangular weights. In Column 6 we estimate our baseline model after we restrict the analysis to observations within a window around the cutoff (i.e. bandwidth) selected using the data-driven procedure of Calonico et al. (2014a). Finally, in Column 7 we estimate our model using the *rdrobust* command from Calonico et al. (2014a). *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

ONLINE APPENDIX:
Welfare Program Spillovers

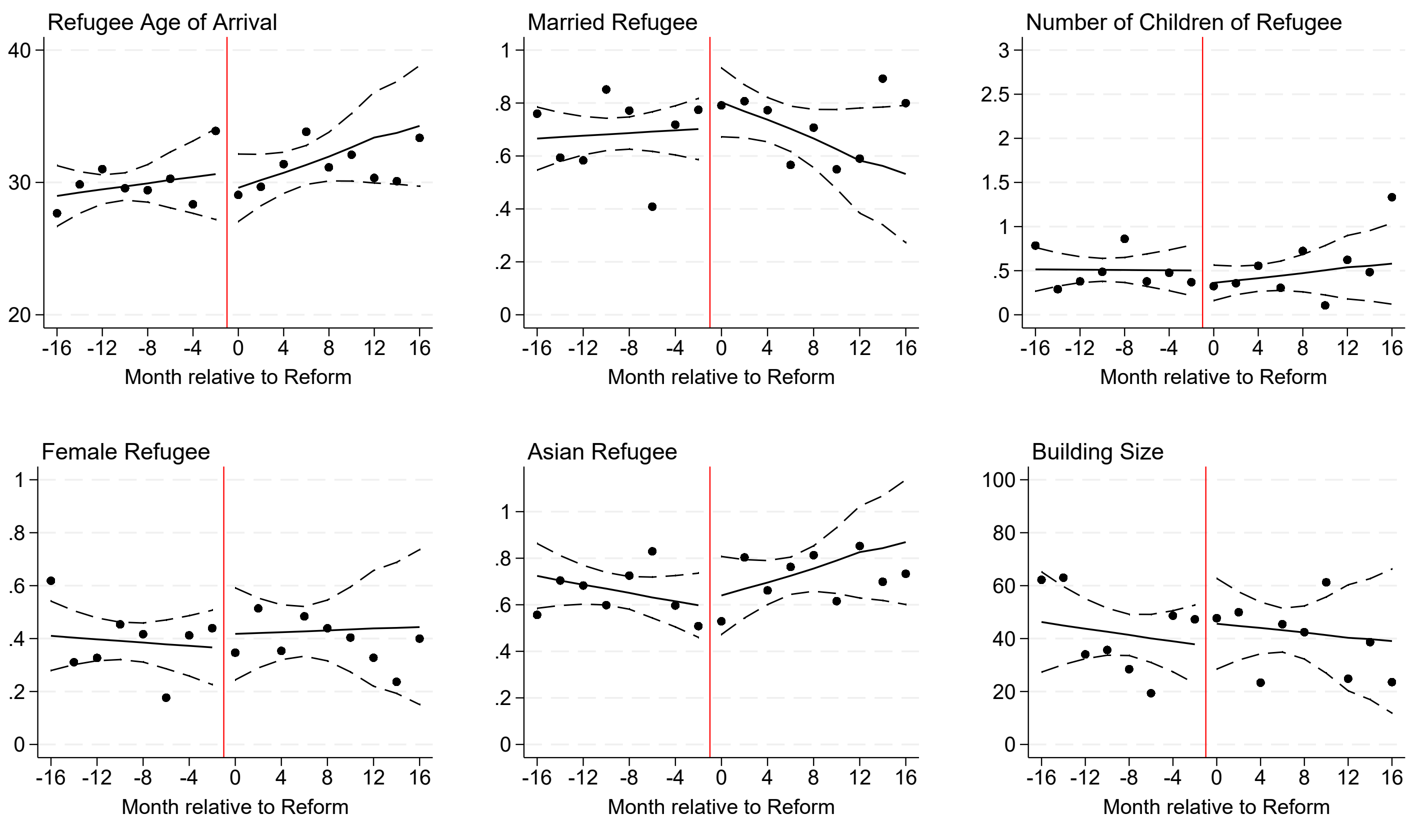
Appendix Figures

Figure A.1: Balance Tests for non-Danish Neighbors' Characteristics



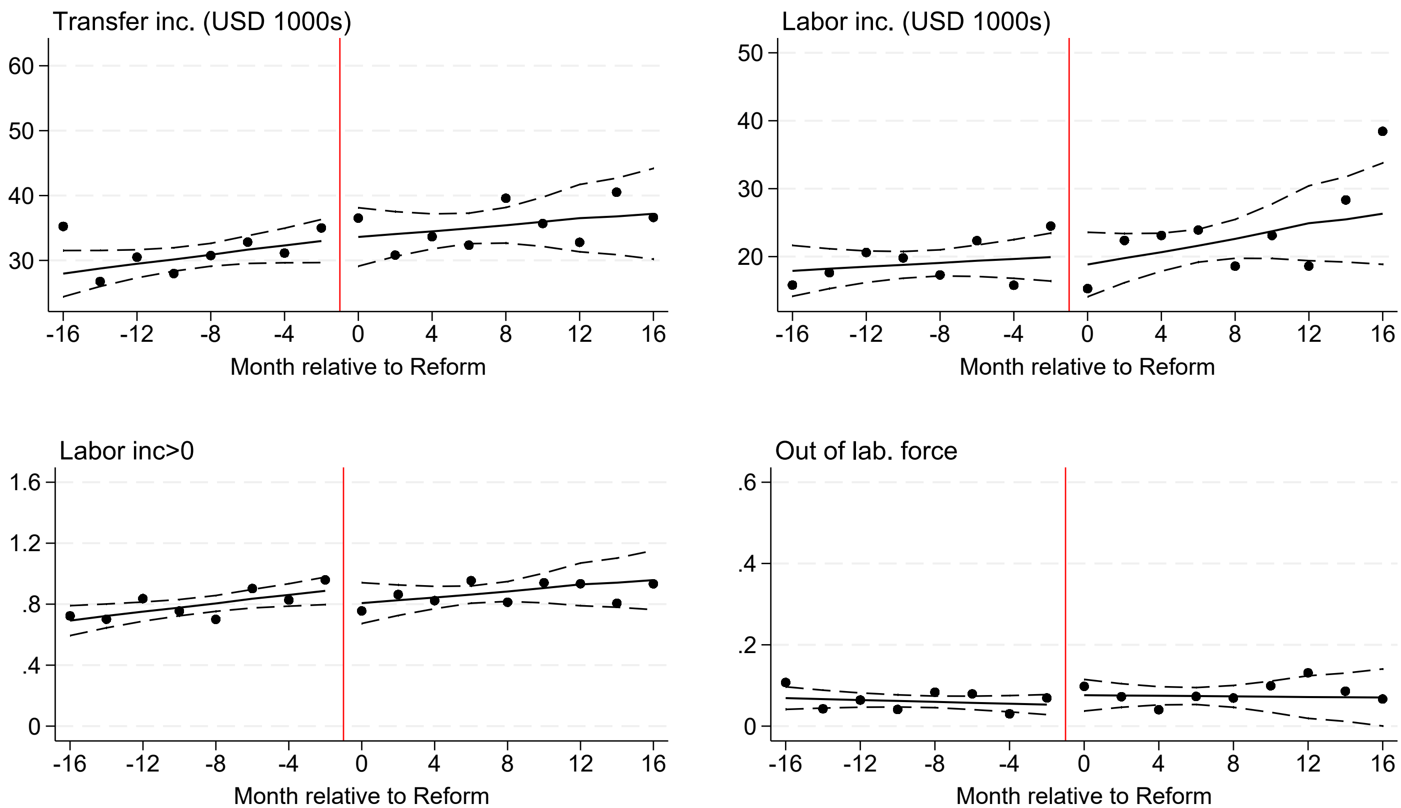
Notes: These figures show balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' characteristics, all measured at the year of arrival of the refugee. We test whether Start Help affected the likelihood of being married (top left), average number of children (top), average age (top right), likelihood of being a woman (bottom left), likelihood of having an African country of origin (bottom), and likelihood of having an Asian country of origin (bottom right). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. Here we do not control for the demographics listed in section 2. The black circles show average values of characteristics in two months bins, to present the underlying data. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.2: Balance Test for Refugee and Building Characteristics



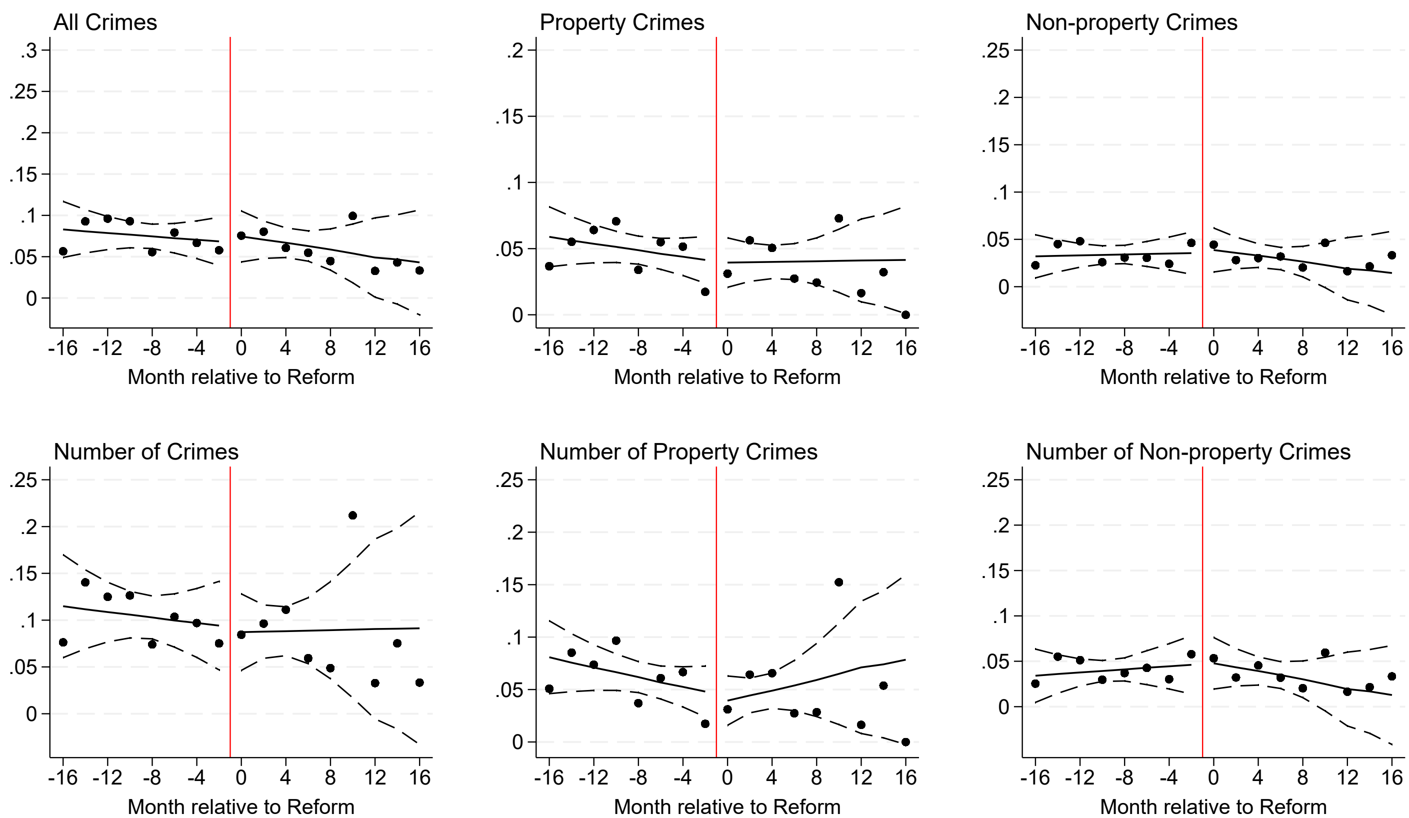
Notes: These figures show balance tests for the characteristics of refugees and buildings, all measured at the year of arrival of the refugee. We test whether Start Help affected refugees' age (top left), marital status (top), number of children (top right), likelihood of being a woman (bottom left), as well as the probability of an Asian origin (bottom), and the number of residents in the building (bottom right). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. Here we do not control for the demographics listed in section 2. The black circles show average values of characteristics in two months bins, to present the underlying data. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.3: Balance Test for non-Danish Neighbors' Pre-Exposure Labor Market Outcomes



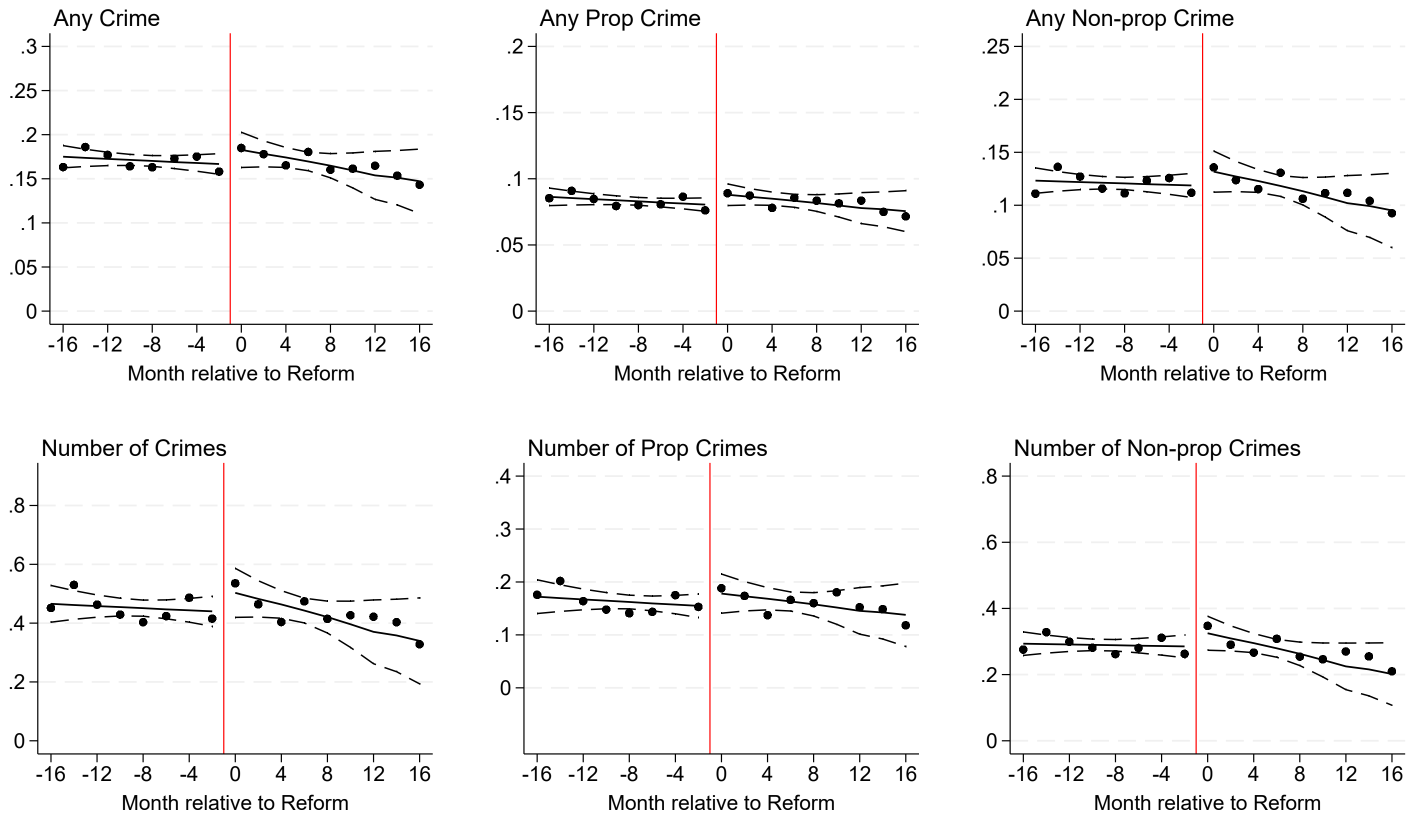
Notes: These figures shows balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' labor market outcomes in the two years prior to refugee arrival. We test government transfer income in 1000s of US dollars (top left), labor income in 1000s of US dollars (top right), a dummy for having positive labor income (bottom left), and a dummy for being out of the labor force (bottom right). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. Here we do not control for the demographics listed in section 2. The black circles show average values of characteristics in two months bins, to present the underlying data. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.4: Balance Test for non-Danish Neighbors' Pre-Exposure Crime



Notes: These figures shows balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' crime convictions in the two years prior to refugee arrival. In the first row, we show results for likelihood of being convicted for any (non-traffic) crimes (top left), property crimes (top), and non-property non-traffic crimes (top right). The second row shows counts of the same crime types. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. Here we do not control for the demographics listed in section 2. The black circles show average values of characteristics in two months bins, to present the underlying data. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

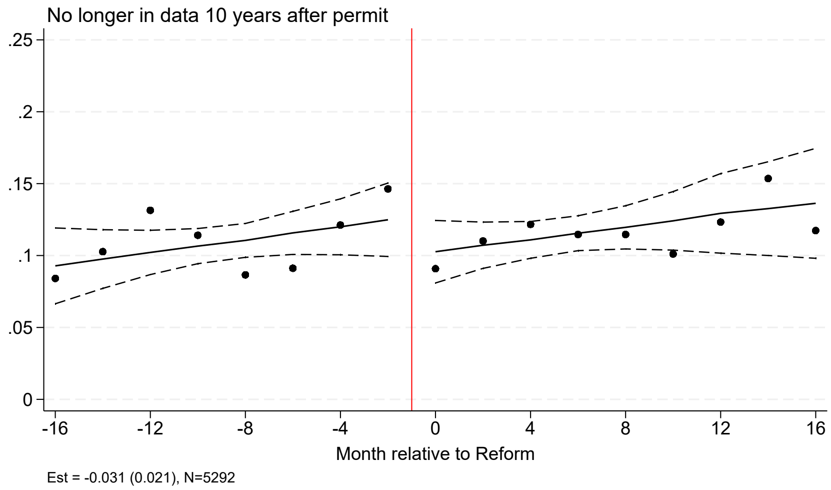
Figure A.5: Balance Test for non-Danish Neighbors' Predicted Crime



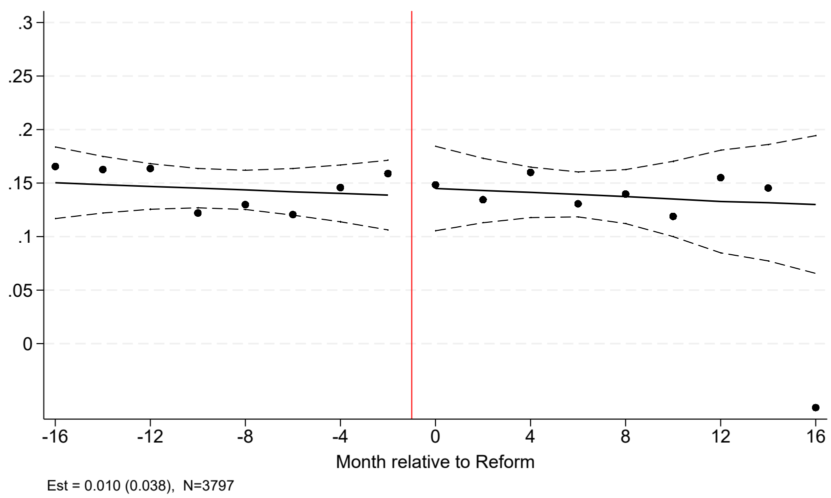
Notes: These figures shows balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' predicted crime. We use all refugees' and neighbors' demographic characteristics as well as neighbors labor market outcomes in the two years prior to refugee arrival to predict crime convictions. In the first row, we show results for predicted likelihood of being convicted for any (non-traffic) crimes (top left), property crimes (top), and non-property non-traffic crimes (top right). The second row shows predicted counts of the same crime types. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. Here we do not control for the demographics listed in section 2. The black circles show average values of characteristics in two months bins, to present the underlying data. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.6: Effects of Start Help on Attrition Rates

A: Refugees



B: Non-Danish Neighbors

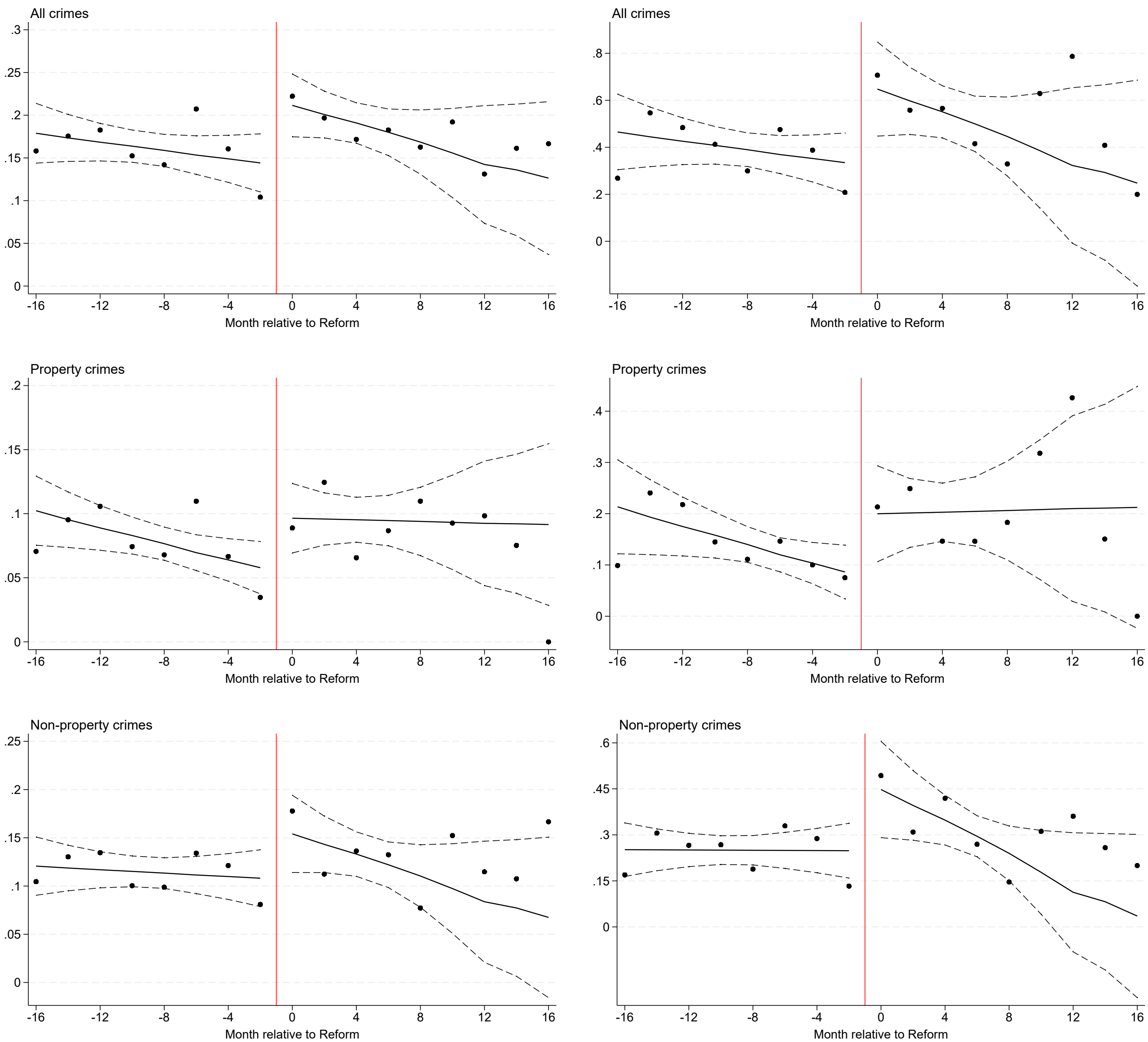


Notes: These figures present the effect of Start Help on refugees' (panel a) and their non-Danish neighbors' (panel b) attrition rate in the first ten years since the refugee's arrival in the building. Attrition is defined as an indicator for absence in the administrative registers in any of the 10 years after a refugee's arrival in the building. To create these figures, we first estimate equation (1), controlling for linear functions of the running variable, using triangular weights and clustering the errors at the building level. We do not control for the demographics listed in Section 4. We then predict the dependent variable according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot these predicted transfers along with its 95% confidence interval. The black circles show average of the outcome variable in two months bins, to present the underlying data. "Est" reported at the bottom of each figure refers to the estimated effect at the cutoff and standard errors (in parenthesis) based on underlying data. *Sample:* The sample includes neighbors of non-Danish origin and their neighboring refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.7: Effect of Start Help on non-Danish Neighbors' 10-Year Crime - Raw Data

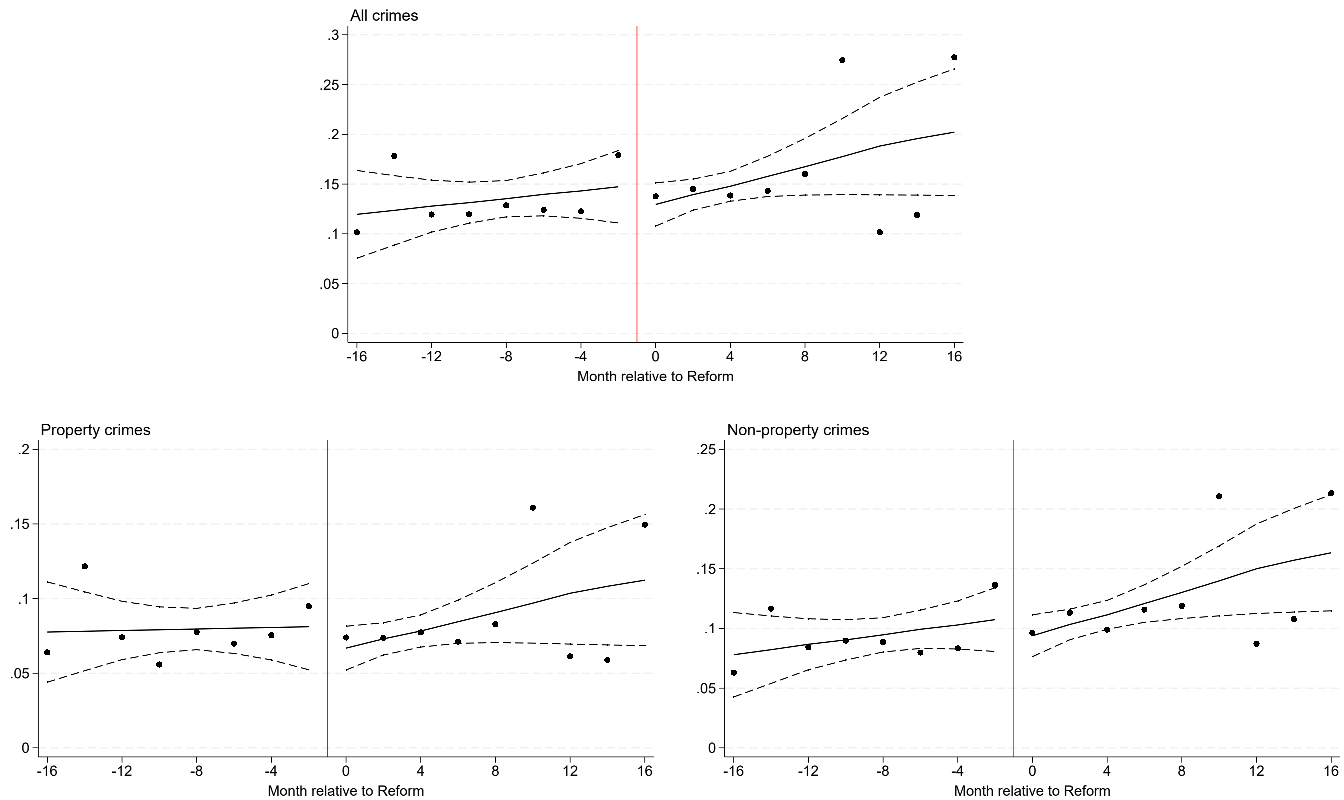
A: Likelihood of Committing Crime

B: Number of Crimes



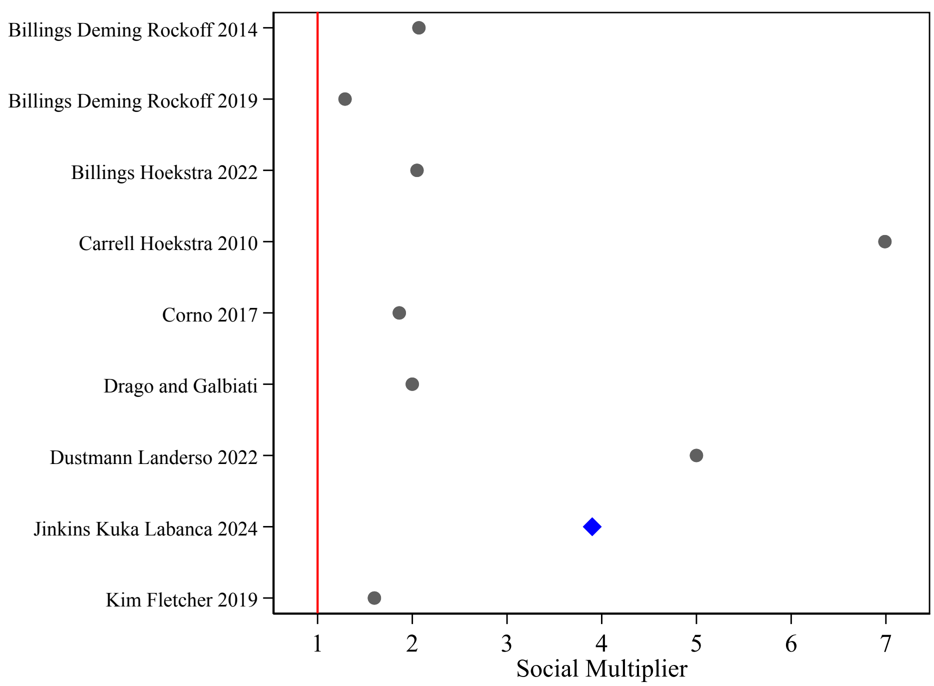
Notes: These figures present the effect of Start Help on refugees' non-Danish neighbors' crime convictions in the first ten years since the refugee's arrival in the building. Panel A presents results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). Panel B presents results for total number of convictions instead of likelihood of convictions, for the same types of crime as Panel A. To create these figures, we first estimate equation (1), controlling for linear functions of the running variable, using triangular weights and clustering the errors at the building level. Differently from our baseline results, here we do not control for the demographics listed in section 2. We then predict transfers according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot these predicted transfers along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average crime outcomes in two months bins, to present the underlying data. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.8: Effect of Start Help on Danish Neighbors' Likelihood of 10-Year Crime



Notes: These figures present the effect of Start Help on refugees' Danish neighbors' likelihood of crime convictions in the first ten years since the refugee's arrival in the building. The top figure presents results for the likelihood of being convicted of any (non-traffic) crimes, while the bottom two figures present results for property crimes (left) and non-property crimes (right). We create these figures to mimic our estimation strategy. We first create residualized outcome variables – by regressing our outcome variables on the controls listed in section 2 and then adding back the control mean. We then estimate equation (1) without the control variables, hence just controlling for linear functions of the running variable, using triangular weights, and clustering the errors at the building level. We then predict crime according to the number of days from date of residence permit relative to July 1^{st} 2002, and then plot the predicted outcomes along with its 95% confidence interval. The jump at the threshold represents the estimated treatment effect of Start Help. The black circles show average residualized crime convictions in two months bins, to present the underlying data. *Sample:* The sample includes neighbors of Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

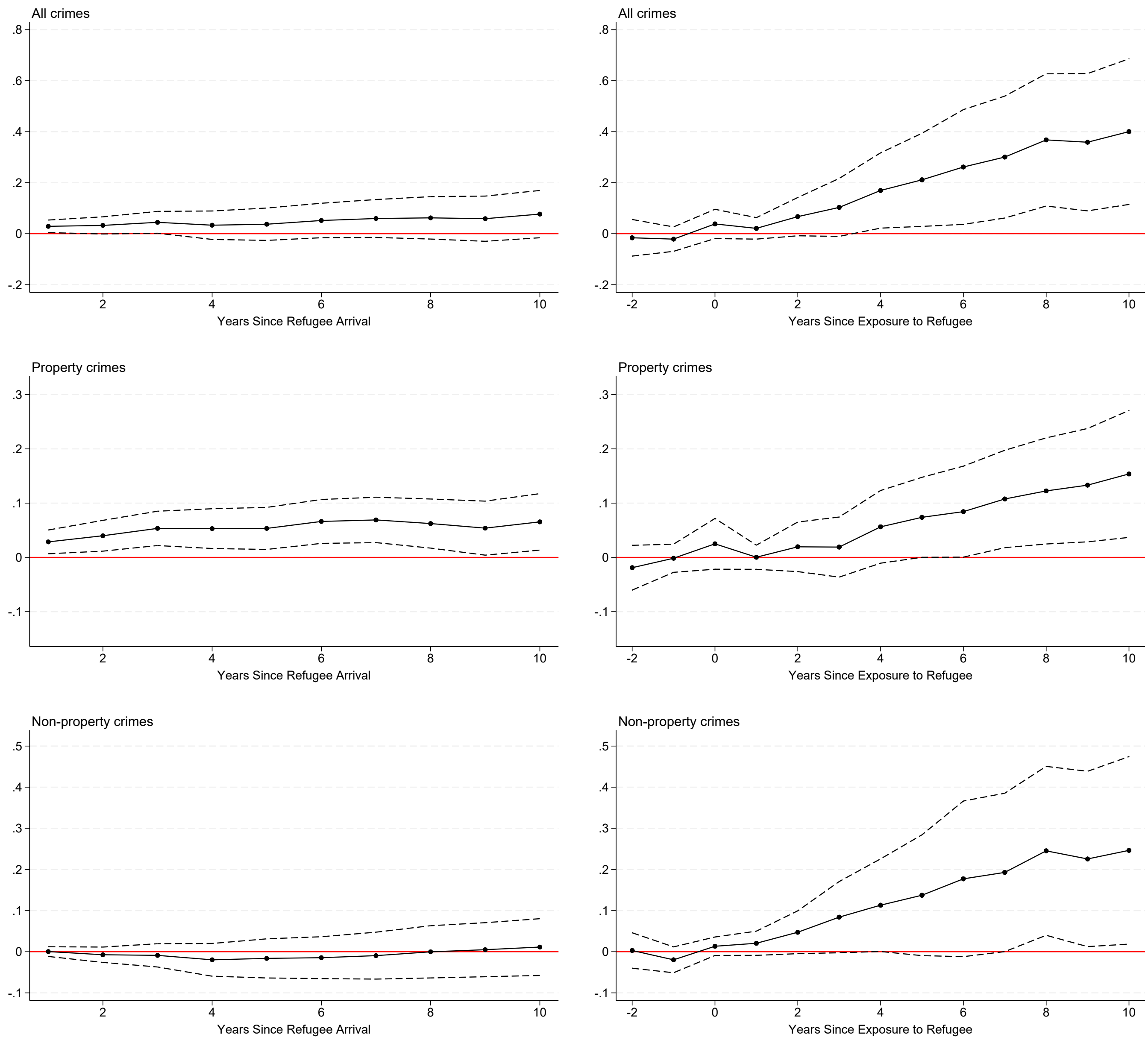
Figure A.9: Social Multiplier Estimates from the Literature on Crime Peer Effects



Notes: This figure presents calculated social multipliers for several papers analyzing peer effects. To calculate the multiplier we use own and peer estimated effects as well as own and peer group size.

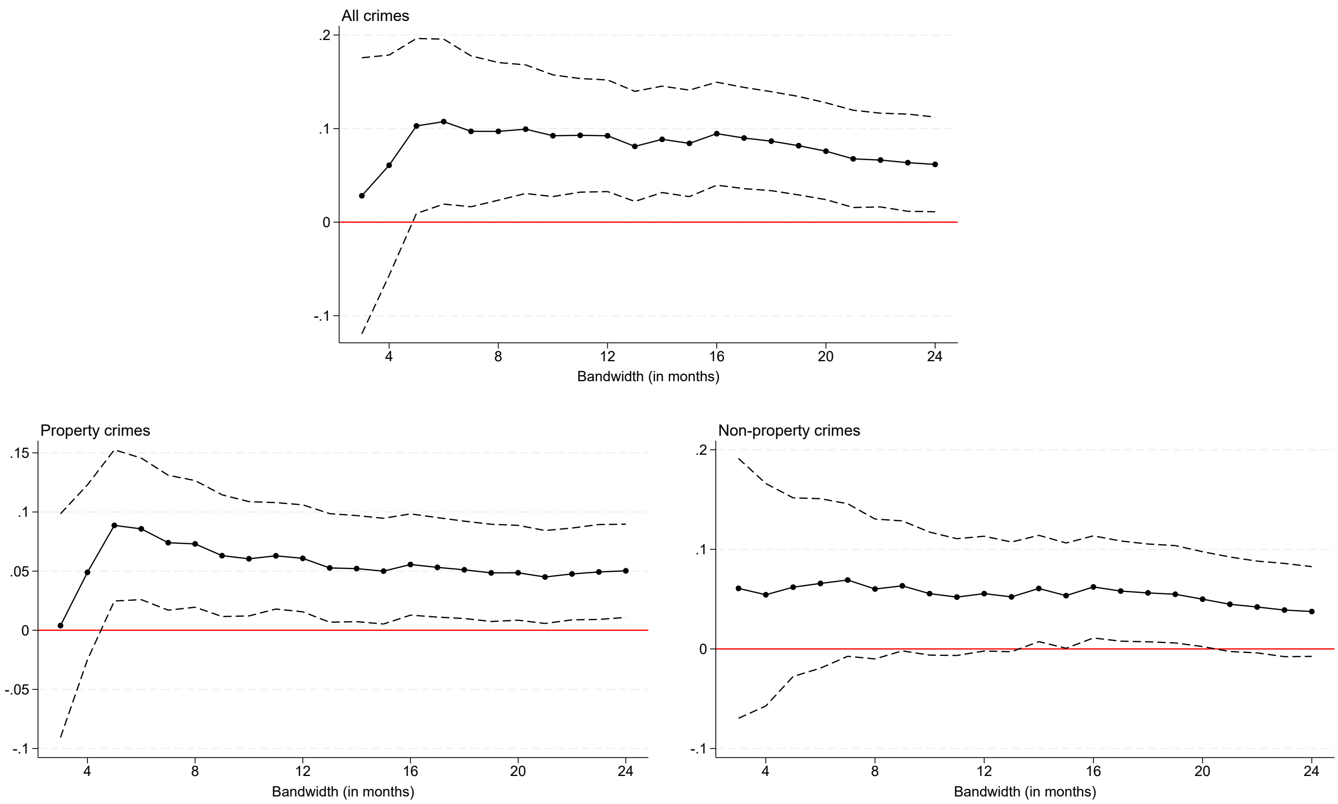
Figure A.10: Effect of Start Help on Refugees' and Neighbors' Number of Crimes – Treatment Effect Over Time

A: Refugees | B: Non-Danish Neighbors



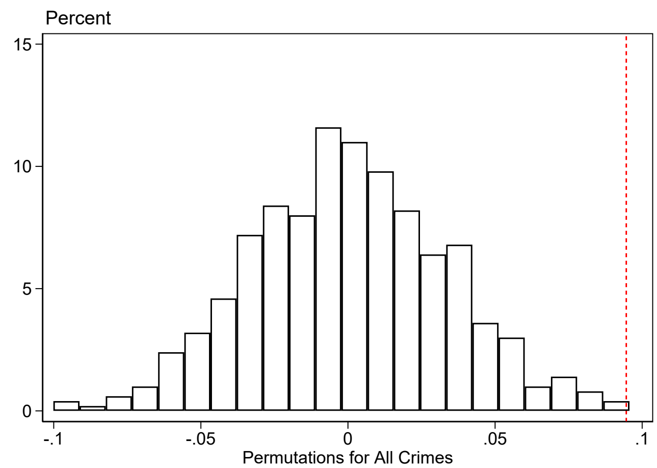
Notes: These figures present the effect of Start Help on refugees' (Panel a) and their non-Danish neighbors' (Panel b) number of crime convictions over time. For both groups, we presents results for the number of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). To create these figures, we first estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. The black circles show the estimated effect of Start Help one to ten years after refugee residence permit (as indicated by the x-axis), along with its 95% confidence interval. *Sample:* The sample for Panel A includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan. The sample for Panel B includes the neighbors of non-Danish origin. We also exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

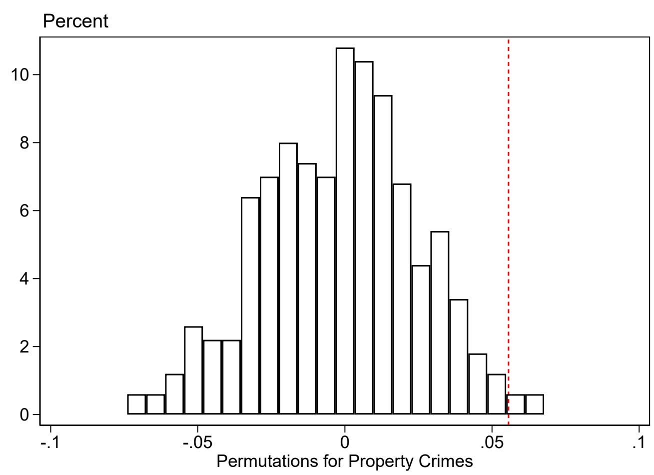
Figure A.11: Effect of Start Help on non-Danish Neighbors' Likelihood of 10-Year Crime – Sensitivity to Bandwidth Choice

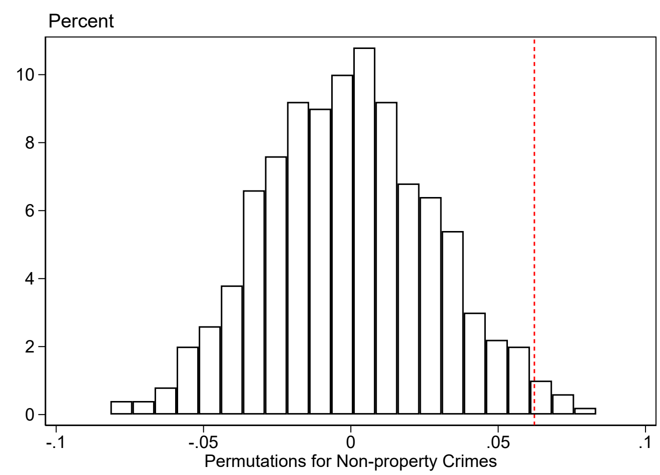


Notes: These figures presents the sensitivity to the choice of bandwidth of the effect of Start Help on non-Danish neighbors' likelihood of crime convictions in the first ten years since being exposed to a refugee. The top figure presents results for the likelihood of being convicted of any (non-traffic) crimes, while the bottom two figures present results for property crimes (left) and non-property crimes (right).To create these figures, we first create a sample of refugees arriving in the relevant bandwidth (3 to 24 months around July 2002) and then find their non-Danish neighbors. With this new sample, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. The black circles show the estimated effect of Start Help, along with its 95% confidence interval. *Sample:* The sample for includes non-Danish neighbors of refugees (and their spouses) who received a residence permit X (3 to 24, as indicated on the X-axis) months before to X months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan. We also exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Figure A.12: Effect of Placebo Start Help on non-Danish Neighbors' Likelihood of 10-Year Crime – Randomization Inference







Notes: These figures present randomization inference results for non-Danish neighbors' likelihood of 10-year crime convictions. The top figure presents results for the likelihood of being convicted of any (non-traffic) crimes, while the bottom two figures present results for property crimes (left) and non-property crimes (right). To create these figures, we take our baseline sample of non-Danish neighbors and then assign each building a random date of refugee permit, following a uniform distribution. We then estimate equation (1) with the new running variables, controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. We repeat this process 500 times, and then plot the distribution of the estimated effects. The red line indicates the efect estimated with the true running variable. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Appendix Tables

Table A.1: Summary Statistics of Building Characteristics

	Sample Statistics
Average Total Residents per Building	13.100
	(20.456)
Average Sample (age 18-55) Residents per Building	8.623
	(16.189)
Average Danish Neighbors per Building	2.235
	(8.629)
Average Non-Danish Neighbors per Building	2.054
	(3.277)
Average Refugees per Building	1.196
	(1.514)
Buildings in Sample	4425
Municipalities in Sample	262
Buildings with only 1 Refugee Family in Sample	1716
Municipalities with Buildings with only 1 Refugee Family in Sample	73

Notes: This table shows average values and standard errors (in parentheses) for building characteristics in our sample. The sample used for the statistics includes all buildings independently of whether they host one or more than one refugee family.

Table A.2: Effect of Start Help on Refugees' Transfer Income and Work

	Yr 1	Yrs 1-4	Yrs 1-10
Panel A: Transfer Income (1000s USD)			
Start Aid	-9.211***	-16.588***	-12.999
	(0.964)	(3.529)	(8.806)
Mean Y	24.466	91.513	218.033
Mean Y Pre Start Help	28.529	102.429	231.764
Number of Refugees	5292	5292	5292
Panel B: Labor Income (1000s USD)			
Start Aid	0.464	1.979	0.601
	(0.601)	(3.354)	(10.306)
Mean Y	3.163	32.435	118.815
Mean Y Pre Start Help	2.655	27.846	112.312
Number of Refugees	5292	5292	5292
Panel C: Years with Labor Income>0			
Start Aid	0.057**	0.142*	0.260
	(0.022)	(0.084)	(0.210)
Mean Y	0.177	1.262	3.753
Mean Y Pre Start Help	0.141	1.100	3.492
Number of Refugees	5292	5292	5292

Notes: This table presents the effect of Start Help on refugees' total transfer income and work in the first year, the first four years, and the first ten years (in columns 1 to 3 respectively) since residence permit. Panel A presents results for total transfer income, Panel B presents results for labor income, and Panel C presents results for the number of years working, defined as having positive labor income. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002. We exclude individuals younger than 18 or older than 55 and individuals who arrived from the Balkans or Afghanistan.

Table A.3: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – Subcategories

		Property			Non-Property							
	All	All	Shoplift	Other	Violent	Sexual	Drug	Weapon	Laws	Fin	Misc	Traffic
Panel A: Likelihood of Committing a Crime												
Start Aid	0.095***	0.056**	0.050**	0.015	0.013	0.002	0.035*	0.015	0.036**	0.005	0.016	0.029
	(0.028)	(0.022)	(0.019)	(0.015)	(0.019)	(0.003)	(0.020)	(0.011)	(0.017)	(0.004)	(0.011)	(0.031)
Mean Y	0.172	0.084	0.047	0.050	0.040	0.004	0.049	0.015	0.043	0.003	0.023	0.238
Mean Y Pre Start Help	0.157	0.075	0.042	0.044	0.039	0.003	0.041	0.013	0.038	-	0.021	0.234
Number of Neighbors	3797	3797	3797	3797	3797	3797	3797	3797	3797	3797	3797	3797
Panel B: Number of Crimes												
Start Aid	0.400***	0.154**	0.095*	0.059*	0.015	0.002	0.065	0.014	0.129**	0.005	0.017	0.115
	(0.145)	(0.060)	(0.050)	(0.032)	(0.029)	(0.003)	(0.059)	(0.013)	(0.059)	(0.004)	(0.016)	(0.086)
Mean Y	0.455	0.164	0.085	0.079	0.058	0.004	0.115	0.017	0.066	0.003	0.028	0.464
Mean Y Pre Start Help	0.384	0.134	0.068	0.067	0.060	0.004	0.095	0.016	0.049	-	0.026	0.444
Number of Neighbors	3797	3797	3797	3797	3797	3797	3797	3797	3797	3797	3797	3797

Notes: This table presents the effect of Start Help on refugees' non-Danish neighbors' likelihood of crime convictions (Panel A) and number of crimes (Panel B) if the first 10 years since residence permit. Each column represents a different type of crime, starting with any (non-traffic) crime in the first two column and then restricting it to various subcategories in the remaining columns. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.4: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – Heterogeneity

		Young		Male		Parent		Married	
	All	Yes	No	Yes	No	Yes	No	Yes	No
Panel A: All crimes									
Start Aid	0.095***	0.153***	0.046	0.113**	0.044	0.039	0.145***	0.009	0.190***
	(0.028)	(0.037)	(0.040)	(0.046)	(0.041)	(0.040)	(0.041)	(0.037)	(0.041)
Mean Y	0.172	0.190	0.152	0.252	0.066	0.142	0.201	0.133	0.220
Panel B: Property crimes									
Start Aid	0.056**	0.064**	0.044	0.048	0.061	0.053*	0.052	0.006	0.091**
	(0.022)	(0.030)	(0.031)	(0.030)	(0.039)	(0.029)	(0.035)	(0.029)	(0.036)
Mean Y	0.084	0.092	0.074	0.111	0.047	0.070	0.098	0.061	0.112
Panel B: Non-property crimes									
Start Aid	0.062**	0.123***	0.017	0.090**	0.002	0.011	0.101***	-0.008	0.157***
	(0.026)	(0.039)	(0.036)	(0.043)	(0.020)	(0.036)	(0.036)	(0.031)	(0.040)
Mean Y	0.121	0.143	0.098	0.195	0.025	0.092	0.151	0.086	0.166
N Neighbors	3797	1976	1821	2157	1640	1947	1850	2140	1657

Notes: This table presents the effect of Start Help on non-Danish neighbors' likelihood of crime convictions in the first ten years since being exposed to a refugee. Panel A presents results for any (non-traffic) crimes, Panel B for property crimes, and Panel C for non-property crimes. The first column presents our baseline results, while the remaining columns present results when we estimate our model in the sub-samples listed in the column headers (neighbors exposed before or after age 32, male and female neighbors, neighbors with or without children, married or unmarried neighbors). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.5: Benefits and Costs of Start Help Excluding Crime Effects

	Amount
Panel A: Change in welfare transfers net of taxes	
Change in transfers (Appendix Table A.2)	12999
Marginal tax rate on low incomes	0.381
Average change in welfare transfers net of taxes	8040.089
Number of refugees	5292
Total change in welfare transfers net of taxes	42548153.549
Panel B: Change in tax revenues from labor income	
Change in labor income (Appendix Table A.2)	601
Marginal tax rate on low incomes	0.381
Average change in tax revenues	229.272
Number of refugees	5292
Total change in tax revenues from labour income	1213306.81

Notes: This table shows the details behind the calculations of MVPF components 1 and 2 in Panel A of Table 5. The amounts displayed in the table are in 2021 US dollars. In each panel, the average change in welfare transfers net of taxes is obtained as the product of the change in transfers times 1 minus the marginal tax rate on low incomes. To allow for a comparison between effects obtained on different samples of refugees and non-Danish neighbors, we consider total amounts, rather than average amounts, obtained by multiplying the average amounts by the number of refugees in our sample. The 38% tax rate on low incomes in the bottom tax bracket is determined based on Table C.8 of Labanca and Pozzoli (2022), as the sum of regional tax rate (33.38%) and bottom tax rate (3.64%) net of EITC contributions (4.25%). For labor income, there is a labor market contribution of 8 percent on top of the above taxes, but at the same time, labor income enters all the other tax bases net of the labor market contribution. The effective tax rate is therefore given as $(33.38 + 3.64 - 4.25) \times (1 - 0.08) + 8 = 38.148$. The upper limit for the bottom tax bracket in 2011 is 389,900 Danish Krone (DKK), and the average income among refugees in our sample, 66,135 (2011) DKK, falls in this bracket.

Table A.6: Costs of Start Help from Crime Effects

	Costs to taxpayers	Costs to victims
Panel A: Costs from refugees' crimes		
Change in number of property crimes (Table 3)	0.065	0.065
Weighted cost of property crime (Appendix Table A.7)	161.283	694.024
Average change in costs from property crime	10.483	45.112
Number of refugees	5292	5292
Total change in costs from refugees' crime	55478.103	238730.510
Panel B: Costs from neighbors' property crimes		
Change in number of property crimes (Table 4)	0.154	0.154
Weighted cost of property crime (Appendix Table A.7)	1023.819	894.457
Average change in costs from property crime	157.668	137.746
Number of neighbors	3797	3797
Total change in costs from property crime	598665.779	523023.178
Panel C: Costs from neighbors' non-property crimes		
Change in number of non-property crimes (Table 4)	0.253	0.253
Weighted cost of non-property crime (Appendix Table A.7)	1429.228	6870.361
Average change in costs from non-property crime	361.595	1738.201
Number of neighbors	3797	3797
Total change in costs from non-property crime	1372974.983	6599950.651
Total change in costs from neighbors' crime	1971640.762	7122973.830

Notes: This table shows the details behind the calculations of MVPF components 4 to 7 in Panel A of Table 5. The amounts displayed in the table are in 2021 US dollars. In each panel, the average change in costs of (property/non-property) crime is obtained as the product of the change in the number of crimes committed due to the reform (from Tables 3 and 4) and the weighted cost of crime. The weighted costs of crimes are obtained as shown in Appendix Table A.7. To allow for a comparison between effects obtained on different samples of refugees and non-Danish neighbors, we consider total, rather than average, changes in costs. This is done by multiplying the average change in costs by the number of refugees or non-Danish neighbors in our sample. For refugees, we consider effects on property crimes only, as the effects on non-property crime are insignificant. For neighbors, we consider both property and non-property crime effects, as both are significant. For neighbors, we sum the total change in costs across property and non-property crime to obtain total costs.

Table A.7: Weighted Cost of Crime Estimates

	Cost amount	Frequency of crime	Costs weighted by frequency
Panel A.1: Costs to Taxpayers from Refugees' property crime			
Burglary	290.604	0.00	0
Theft	161.283	1.00	161.283
Robbery	1,023.252	0.00	0
Total weighted cost			161.283
Panel A.2: Costs to Taxpayers from Neighbors' property crime			
Burglary	1,703.085	0.10	176.669
Theft	945.200	0.90	847.150
Robbery	5,996.778	0.00	0
Total weighted cost			1023.819
Panel A.3: Costs to Taxpayers from Neighbors non-property crime			
Economic Crime	699.130	0.04	27.051
Drugs related crime	1,864.257	0.16	306.168
Sexual Offences	2,777.923	0.004	11.943
Violence	4,009.891	0.07	268.935
Other criminal offences	2,596.807	0.04	104.944
Road traffic legislation	987.514	0.59	579.093
Violations of Other regulation	1,325.760	0.10	131.094
Total weighted cost			1429.228
Panel B.1: Costs to Victims from Refugees' property crime			
Bulgary	2,626.198	0.00	0
Theft	694.024	1.00	694.024
Robbery	15,008.868	0.00	0
Total weighted cost			694.024
Panel B.2: Costs to Victims from Neighbors' property crime			
Bulgary	2,626.198	0.10	272.427
Theft	694.024	0.90	622.030
Robbery	15,008.868	0.00	0
Total weighted cost			894.457
Panel B.3: Costs to Victims from Neighbors non-property crime			
Fraud/forgery	0.000	0.14	0.000
Drugs related crimes	0.000	0.60	0.000
Sexual assult	163,222.025	0.02	2558.339
Assault	17,635.065	0.24	4312.022
Total weighted cost			6870.361

Notes: This table shows the details behind the calculations of the weighted cost per conviction displayed in Table A.6. Panel A displays costs to taxpayers; Panel B shows costs to victims. Taxpayer costs consist of prosecution costs provided by the Danish State Prosecutor (DSP) under a Freedom of Information Request. These were originally provided in 2012 Danish Krone (DKK) and then adjusted to 2021 USD by multiplying by the Danish CPI increase between 2012 and 2021 (1.073) and then dividing by the DKK to USD exchange rate in 2021 (6.289). Costs to victims are from Deshpande and Mueller-Smith (2022) (Table B.21). These were adjusted to 2021 USD by multiplying by the US CPI change between 2012 and 2021 (1.181). In both panels, we aggregate detailed crime costs into the broader categories of property and non-property crime, weighting costs by observed crime frequencies among refugees or neighbors in the treatment group (i.e., those granted residency after July 1, 2002). We focus only on crimes for which both cost estimates from DSP or Deshpande and Mueller-Smith (2022) and frequencies from Danish registers could be determined. For refugees, we consider the costs of property crimes only, as the effects on non-property crime are insignificant. For neighbors, we consider both property and non-property crime costs, as we find significant effects on both types of crime.

Table A.8: Cost-Benefit Analysis of Start Help with Reduced Migration

	Amount	*Notes*
Panel A: MVPF components		
1. Change in welfare transfers net of taxes	42548153.549	See Appendix Table A.5
2. Changes in tax revenues from labor income	1213306.810	See Appendix Table A.5
3. Total savings to taxpayers	43761460.359	1+2
4. Total enforcement and prosecution costs from refugees	55478.103	See Appendix Table A.6
5. Total enforcement and prosecution costs from neighbors	1971640.762	See Appendix Table A.6
6 Costs to victims from crime of refugees	238730.510	See Appendix Table A.6
7. Costs to victims from crime of neighbors	7122973.830	See Appendix Table A.6
Panel B: Effects of reduced migration on government spending and revenues		
8. Estimated overall reduction in migration flows over 10 years	50000	5000 per year over 10 years
9. Estimated reduction in number of refugees	2800	See table's notes for details
10. Estimated reduction in number of immigrants	47200	See table's notes for details
Government Spending		
11. Share of refugees who receive some welfare over 10 years	0.96	See table's notes for details
12. Share of immigrants who receive some welfare over 10 years	0.93	See table's notes for details
13. Average welfare transfers per refugee net of taxes over 10 years	123703.2	See table's notes for details
14. Average welfare transfers per immigrant net of taxes over 10 years	109613.4055	See table's note for details.
15. Estimated savings on welfare transfers from reduced migration	5144104250.306	(13×9×11) + (14×10×12)
Tax revenues from labor income		
16. Share of refugees who receive some labor income over 10 years	0.665	See table's notes for details
17. Share of immigrants who receive some labor income over 10 years	0.704	See table's notes for details
18. Average tax revenues per refugee over 10 years	49211.436	See table's notes for details
19. Average tax revenues per immigrant over 10 years	64492.92207	See table's notes for details
20. Reduction in tax revenues due to reduced migration	2234654102.778	(18×9×16) + (19×10×17)
Panel C: MVPF including costs to victims and effects of reduced migration		
Base MVPF (ignore effects on crime)	0.01444	1 over (3+15-20)
MVPF adding refugees' crime	0.01449	(1+6) over (3-4+15-20)
MVPF adding also neighbors' crime	0.01691	(1+6+7) over (3-4-5+15-20)

Notes: This table presents the details of the marginal value of public funds (MVPF) calculation, inclusive of costs and benefits from reduced migration. Panel A details MVPF components, excluding the effects of reduced migration, which aligns with Table 5. Panels B describe additional components related to reduced migration. Panel C explains how to combine components from Panels A and B to derive the MVPF. Amounts in Panels A and B are in 2021 US dollars. In Panel B, we estimate the proportion of refugees and immigrants among the total reduction in migration flows, assuming, based on Statistics Denmark data on migration flows by visa type (years 2002-2011), that 5.6% of the total inflow consists of refugees. We estimate average transfers received by refugees and immigrants, average labor income earned by refugees and immigrants, as well as the percentages of refugees and immigrants receiving welfare transfers and labor income based on refugees in our sample who obtained residency after July 1, 2022, and their non-Danish neighbors. To calculate average welfare transfers per refugee/immigrant net of taxes, we subtract taxes from welfare transfers assuming a tax rate of 38.1% (see footnote in Appendix Table A.5 for details on this tax rate). Similarly, we estimate tax revenues assuming that average labor income per refugee/immigrant is taxed at a rate of 38.1%.

Table A.9: Effect of Start Help on Non-Danish Neighbors' Transfers and Work

	Yrs -2/-1	Yr 1	Yrs 1-4	Yrs 1-10
Panel A: Transfer inc. (1000s USD)				
Start Aid	-0.359	-1.952	-4.164	-4.012
	(2.024)	(1.295)	(5.438)	(15.109)
Mean Y	32.586	18.246	71.813	175.117
Panel B: Labor inc. (1000s USD)				
Start Aid	-2.187	-2.365	-6.358	-18.994
	(3.524)	(1.810)	(6.898)	(19.774)
Mean Y	19.841	13.038	58.980	169.259
Panel C: Years with Labor inc>0				
Start Aid	-0.115	-0.048	-0.158	-0.444
	(0.089)	(0.050)	(0.158)	(0.375)
Mean Y	0.827	0.458	1.899	4.665
Panel C: Years Out of Lab Force				
Start Aid	-0.005	-0.007	-0.010	0.095
	(0.026)	(0.017)	(0.070)	(0.217)
Mean Y	0.066	0.057	0.291	1.166
Number of Neighbors	3797	3797	3797	3797

Notes: This table presents the effect of Start Help on refugees' non-Danish neighbors' transfer income and work in the two years prior, the first year, the first four years, and the first ten years (in columns 1 to 4 respectively) since refugee arrival. Panel A presents results for total transfer income, Panel B presents results for labor income, Panel C presents results for the number of years working, defined as having positive labor income, and Panel D presents results for the number of years being out of the labor force. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.10: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – By Crime Location

	Municipality		
	Any	Residence	Non-Residence
Panel A: Likelihood of Committing a Crime			
Start Aid	0.095***	0.086***	0.025*
	(0.028)	(0.028)	(0.013)
Mean Y	0.172	0.161	0.041
Mean Y Pre Start Help	0.157	0.147	0.036
Panel B: Number of Crimes			
Start Aid	0.400***	0.371***	0.029
	(0.145)	(0.132)	(0.025)
Mean Y	0.455	0.390	0.065
Mean Y Pre Start Help	0.384	0.331	0.053
Number of Neighbors	3797	3797	3797

Notes: This table presents the effect of Start Help on refugees' non-Danish neighbors' likelihood of any crime convictions (Panel A) and number of crimes (Panel B) in the first 10 years since residence permit. The first column presents results for all crimes independent of their location, the second column presents crimes committed in the municipality where the neighbor resides, and the third column presents crimes committed in another municipality. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.11: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – By Municipality Sentiment

	Anti-Immigrant Vote		Racist Police Index	
	Low	High	Low	High
Panel A: Likelihood of Committing a Crime				
Start Aid	0.093***	0.097**	0.073*	0.137***
	(0.036)	(0.044)	(0.040)	(0.048)
Mean Y	0.179	0.161	0.182	0.160
Panel B: Number of Crimes				
Start Aid	0.486***	0.209	0.401*	0.399*
	(0.186)	(0.165)	(0.210)	(0.218)
Mean Y	0.500	0.393	0.468	0.441
Mean Anti-Immigrant Vote Share	0.101	0.132	0.117	0.111
Mean Racist Police Index	1.190	1.113	0.994	1.348
Number of Neighbors	2243	1554	1933	1864

Notes: This table presents the effect of Start Help on refugees' non-Danish neighbors' likelihood of crime convictions (Panel A) and number of crimes (Panel B) in the first 10 years since residence permit. The first two columns contain results when we stratify the sample by municipality with below or above median anti-immigrant vote share. In the last two columns we stratify by municipalities more or less likely to over-charge immigrants relative to Danes. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes neighbors of non-Danish origin of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.12: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – Excluding Crimes Committed with Refugees

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Panel A: All Convictions						
Start Aid	0.095***	0.400***	0.056**	0.154**	0.062**	0.246**
	(0.028)	(0.145)	(0.022)	(0.060)	(0.026)	(0.116)
Mean Y	0.172	0.455	0.084	0.164	0.121	0.291
Mean Y Pre Start Help	0.157	0.384	0.075	0.134	0.113	0.250
Panel B: Convictions without Refugees						
Start Aid	0.098***	0.402***	0.060***	0.157***	0.064**	0.246**
	(0.028)	(0.145)	(0.022)	(0.060)	(0.026)	(0.116)
Mean Y	0.163	0.436	0.080	0.160	0.114	0.276
Mean Y Pre Start Help	0.156	0.378	0.077	0.144	0.110	0.235
Number of Neighbors	3797	3797	3797	3797	3797	3797

Notes: This table presents the effect of Start Help on non-Danish neighbors' crime convictions in the first ten years since being exposed to a refugee. Panel A presents our baseline results, while Panel B excludes all crimes for which the neighbor was convicted together with the refugee. The columns indicate the outcome variables such as the likelihood of being convicted and number of convictions for any (non-traffic) crimes (columns 1 and 2), property crimes (columns 3 and 4) and non-property crimes (columns 5 and 6). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.13: Effect of Start Help on Danish Neighbors' Likelihood of 10-Year Crime – Low-Earning Danish Neighbors

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Start Aid	-0.031	-0.186	-0.011	-0.011	-0.023	-0.175
	(0.055)	(0.335)	(0.044)	(0.198)	(0.046)	(0.175)
Mean Y	0.246	0.977	0.157	0.498	0.188	0.480
Mean Y Pre Start Help	0.234	0.926	0.151	0.483	0.172	0.443
Observations	2473	2473	2473	2473	2473	2473

Notes: This table presents the effect of Start Help on low-earning Danish neighbors' likelihood of crime convictions in the first ten years since being exposed to a refugee. The columns indicate the outcome variables such as the likelihood of being convicted and number of convictions for any (non-traffic) crimes (columns 1 and 2), property crimes (columns 3 and 4) and non-property crimes (columns 5 and 6). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes low-earning Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. Low-earning is defined as being in the bottom one fourth of the earnings distribution in the two years prior to refugee arrival. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.14: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – Crime Charges

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Panel A: Convictions						
Start Aid	0.095***	0.400***	0.056**	0.154**	0.062**	0.246**
	(0.028)	(0.145)	(0.022)	(0.060)	(0.026)	(0.116)
Mean Y	0.172	0.455	0.084	0.164	0.121	0.291
Mean Y Pre Start Help	0.157	0.384	0.075	0.134	0.113	0.250
Panel B: Charges						
Start Aid	0.144***	0.099***	0.108***	1.281**	0.676*	0.605
	(0.032)	(0.027)	(0.032)	(0.644)	(0.361)	(0.371)
Mean Y	0.217	0.122	0.155	1.253	0.573	0.681
Mean Y Pre Start Help	0.197	0.109	0.140	1.014	0.412	0.601
Number of Neighbors	3797	3797	3797	3797	3797	3797

Notes: This table presents the effect of Start Help on non-Danish neighbors' crime in the first ten years since being exposed to a refugee. Panel A presents our baseline results on convictions, while Panel B presents results for crime charges. The columns indicate the outcome variables such as the likelihood of being convicted and number of convictions for any (non-traffic) crimes (columns 1 and 2), property crimes (columns 3 and 4) and non-property crimes (columns 5 and 6). For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. *Sample:* The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if multiple refugee families moved into their building, and we exclude neighbors younger than 16 or older than 55.

Table A.15: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – Sensitivity to Including Buildings with More than 1 Refugee Families

	Keep Buildings with up to X Refugee Families					
	1	2	3	4	5	6
Panel A: Likelihood of Committing a Crime						
Any crimes	0.095***	0.079***	0.075**	0.080***	0.079***	0.080***
	(0.030)	(0.030)	(0.029)	(0.030)	(0.030)	(0.030)
Property crimes	0.056**	0.049**	0.050**	0.054**	0.054**	0.055**
	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)	(0.022)
Non-property crimes	0.062**	0.046*	0.039	0.041	0.041	0.041
	(0.027)	(0.026)	(0.026)	(0.026)	(0.026)	(0.026)
Panel B: Number of Crimes						
Any crimes	0.401**	0.353**	0.328**	0.337**	0.340**	0.341**
	(0.156)	(0.153)	(0.153)	(0.154)	(0.154)	(0.154)
Property crimes	0.154**	0.131*	0.135*	0.139**	0.140**	0.141**
	(0.069)	(0.069)	(0.070)	(0.070)	(0.070)	(0.070)
Non-property crimes	0.247**	0.222**	0.193*	0.198*	0.200*	0.200*
	(0.118)	(0.113)	(0.112)	(0.113)	(0.112)	(0.112)
Number of Neighbors	3797	4097	4185	4235	4254	4258

Notes: This table presents the effect of Start Help on non-Danish neighbors' crime in the first ten years since being exposed to a refugee. Panel A presents our baseline results on likelihood of convictions, while Panel B presents results for number of convictions. Each row contains results from a separate outcome: convictions for any (non-traffic) crimes, property crimes and non-property crimes. Each column presents results when we keep buildings with 1 (our baseline) to 6 refugee families in our sample. For all results, we estimate equation (1), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 2, and clustering the errors at the building level. If there are multiple refugees, both the date of residency (our running variable) and the refugee-level controls are averages of the same variables of refugees in the building.
Sample: The sample includes non-Danish neighbors of the refugees (and their spouses) who received a residence permit 16 months before to 16 months after July 2002, were between 18 and 55 when they received residency, and were not from the Balkans or Afghanistan. We exclude neighbors if more than 1 to 6 refugee families moved into their building (as indicated in the columns), and we exclude neighbors younger than 16 or older than 55.

A Data Appendix

The confidential register data used in this paper were obtained from Statistics Denmark, the national Danish statistical agency.[1] We used the following registers: IND (income), BEF (demographics),[2] KRSI (criminal citations), KRAF (judicial decisions), and OPGH (visas). Our data period for all these registers is 1997-2019. In addition to the register data, we used information on Danish election results from "Den Danske Valgdatabase", which is publicly accessible through Denmark Statistics. We also used Denmark Statistics series on CPI (PRIS8) and exchange rates (DNVALD).

We start by using the OPGH (visas) register to identify all immigrants who obtained a visa in our period. Since an individual gets an entry in the OPHG register every time they renew their visa, we keep only each person's first observation.[3] We then use the visa type variable (*kategori*) to restrict our sample to refugees or those who arrived through a family reunification.

Next, we need to identify couples in our data for two purposes. First, we need to identify immigrants who had a family reunification visa to a refugee, who we also consider as part of our refugee sample. Second, we need to link couples together because both spouses' immigration dates determine whether they were affected by Start Help. To identify each individual's first spouse, we use the BEF data to identify the first observed non-missing spouse ID (*aegte_id*). We then link two individuals in our sample (refugees or immigrants with a family reunification) as spouses if their registered marriage date is no more than one year after their residency date.[4] We then restrict our sample to either single immigrants who arrived as refugees, or married couples in which either both were refugees or one was a refugee and the other was family-reunified.[5]

Once we have a pool of refugees, we restrict the sample to those who were granted residency within an interval of 16 months around the Start Help cutoff date, July 1^{st}, 2002. We use the BEF register to extract information on each individual's date of residency permit, which we define as the first non-missing date reported by any of the three immigration variables:

[1] For more information on how to access the Danish register data, visit the Danish statistical agencies website: https://www.dst.dk/en/TilSalg/Forskningsservice.

[2] Because annual BEF demographic information is recorded on January 1st, we treat an individual's information in the BEF (location, age, marriage, etc) as applying to the previous calendar year. This is important for refugees, because they would otherwise have missing values in the year they are granted residence.

[3] In our sample, 81% of immigrants have only one observation, 14% had two, and the remaining 5% had 3 observations or more.

[4] If two individuals married 2 or more years after arrival, we consider them as two single refugees.

[5] We thus drop refugees married to either Danish nationals or immigrants with other visa types. We also drop refugees if the spouse with the family reunification visa had residency before the refugee, which is only a few observations.

van_vtil, foerste_indvandring, seneste_indvandring.[6]
Since under Start Help a couple received lower transfers if at least one person arrived after July 1^{st} 2002, we create a "joint" residency date for couples arriving together. This joint residency variable is equal to the latest date of residence permit date of the two if either both spouses arrived before July 1^{st}, 2002, or if one arrived before and the other after. It is equal to the earliest date of residency of the two if both spouses arrived after that date. If two spouses arrive more than 24 months apart, we consider them as separate and thus use each individual's own permit date (and not the joint date) as the date of residency. We do so because, for instance, if the first spouse arrives in June 2002 and the second spouse arrives after June 2004, the first spouse was not affected by Start Help—hence received high transfers—for at least 2 years before entering the lower transfer regime. For the purpose of our study, we consider these individuals as untreated. Once we have a residency date for single refugees and refugee families, we restrict the sample to individuals with a residency date within an interval of 16 months around July 1^{st} 2002.

For the reasons explained in Section 3, we drop refugees from Afghanistan and the Balkans using information about country of origin (*opr_land*) from BEF.[7] Finally, we restrict the sample to adults, defined as refugees who received residency permit while aged 18 to 55. Our final sample of refugees consists of 5,292 individuals.

With the refugee sample in hand, we then proceed to identify their neighbors. We start by identifying the first year and address (*opgikom*) where a refugee appears. For couples arriving within 24 months of each other, we take the first address of the spouse who immigrated first. This address refers to an entrance to a housing unit, so even though we refer to them as buildings in the text, one large apartment building may have several *opgikom* codes. We drop any building that has more than 300 residents on average between 1999 and 2001. This is done to avoid labeling as neighbors individuals who happen to be in non-standard residential buildings devoted to public services, such as prisons, boarding schools, or long-term care hospitals.

For each building and year combination, we count the number of unique refugee families who resided in the building. In the baseline analysis we restrict the analysis to buildings with at most one refugee family.[8] Thus we identify all individuals who lived in that building

[6] *van_vtil* is available until 2003, and the other two variables are available after 2003. While *foerste_indvandring* is in principle the date of an immigrant's first residency in Denmark, its value is often missing. In the case that the value is missing, we use the value of *seneste_indvandring*, the immigrant's latest date of residency.

[7] The excluded countries are Afghanistan, Albania, Serbia and Montenegro, Yugoslavia, Bosnia and Herzegovina, North Macedonia, Serbia, The Federal Republic of Yugoslavia, Montenegro, and Kosovo.

[8] In a robustness check, we relax this restriction considering buildings with up to refugee families (see Section 7 for details.

the year before the refugee (family) arrived. We define these individuals as neighbors for our analysis. For the small share of neighbors who were exposed to more than one refugee, we only keep information about the first refugee in our sample they were exposed to.[9] We exclude from the neighbors sample all individuals who ever shared a family id with one of the refugees in our sample or who has an immigration date 16 months before to 16 months after July 2002, as they could be potentially affected by Start Help directly. Finally, we restrict the neighbors sample to adults, defined as those who were exposed to a refugee while they were aged 16 to 55. Our final neighbors sample consists of 13,687 individuals, 3,797 of which are Non-Danes – that is either immigrants themselves or children of immigrants.

Finally, we match refugees and neighbors in our sample to data from the IND and KRAF registers to measure our main outcomes of interest. For income, we collect data on labor income (*loenm_13*), transfer income (*off_overforsel_13+skatfriyd*), and taxes paid (*skatmvialt_13*) from IND. We call an individual out of the labor force if they have positive amount of public pension (*folkefortid_13*). If someone has positive labor earnings and no public pension, we consider them employed.

In our baseline analysis, we measure crime based on crime convictions. We use the the KRAF variable *afg_ger7* to classify crime types, and the KRAF variable *afg_afgtyp3* to flag convictions. Values of *afg_afgtyp3* between 100 and 300 correspond to convictions with punishments that involve a fine, probation, and/or prison. In a robustness check, we also consider crime charges as an alternative measure of crime. These are obtained based on the variable *sig_ger7* reported in the KRSI register. The crime classification variables consists of a seven digit identifier of the most serious charge in a case complex. A case complex consists of all charges related to a single criminal episode. We classify charges into crime types generally using the first two digits of *afg_ger7*, although more digits were required to identify narrow crime types like shoplifting. In the analysis of potential mechanisms, we consider two people to have been involved in the same crime if their person IDs are are both linked to the same *afg_journr*, an ID for each case complex.

[9]It is possible, but unusual, that a neighbor may move from a building in which a single refugee family is placed to another building where a different refugee family is subsequently placed.