

Welfare Programs and Crime Spillovers*

David Jenkins[†]

Copenhagen Business School

and IZA

Elira Kuka[‡]

The George Washington University,

IZA and NBER

Claudio Labanca[§]

Monash University and IZA

May 1, 2026

Abstract

Research on the social safety net examines 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 cut welfare benefits for refugees, increasing crime among affected individuals. Linking refugees to neighbors, we find increased crime among non-Danish neighbors, with spillovers persisting even after direct effects stabilize. Accounting for these spillovers raises the marginal value of public funds by approximately 20%. We explore several mechanisms and find evidence consistent with peer effects in crime.

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, Pham Tram, Anna Zhu and seminar participants at the Australian Public Economics Exchange, Bristol Workshop on Economic Policy, Copenhagen Business School, Conference on Immigration in OECD Countries, Dondena Workshop on Public Policy, George Mason University, the George Washington University, HELP workshop, Labour Econometric Workshop, Monash University, National Taiwan University, Penn State, the Rockwool Foundation, SOLE Meetings, Texas Economics of Crime Workshop, Tulane University, Virtual Crime Economics seminar, the University of Maryland, the University of Turin, and the University of Wisconsin IRP for helpful feedback. This research was supported by the Rockwool Foundation (Project 3045: Connecting Copenhagen). 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' behavior across various dimensions such as crime (e.g. [Damm and Dustmann, 2014](#)) and 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. However, analyzing these spillover effects can be challenging 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 behavior 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 behavior 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, a source of variation used in prior work. We find large spillover effects from the welfare cut on criminal behavior 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.

¹Spending 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 1st 2002, many of whom had applied for residency long before the reform was announced. This feature 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 and their children have been previously studied (Dustmann et al., 2023; Dustmann et al., 2024), 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 1st 2002. In order to reduce the concerns associated with the endogenous sorting of individuals across buildings in anticipation of the reform, we assign neighbors 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, it is strongest in the first three 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 behavior 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. Unlike the effects on crime of refugees that stabilize after three 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 57% (8.9

²Our 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 magnitude of these effects, combined with the evidence that the spillover effects intensify over time while direct effects remain stable, suggests that further 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 2.7 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 reducing welfare benefits may entail non-negligible costs due to spillover effects. To assess the relevance of these costs for welfare program design, 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-Keysler, 2020](#)). Accounting for the costs associated with neighbors’ crime convictions, we estimate a 21% increase in the MVPF due to spillovers on neighbors. If we base the MVPF calculations on reported crimes rather than convictions, the increase in MVPF rises to 66%.

In the final part of the study, we analyze potential mechanisms to explain the spillover effects. In line with the hypothesis that spillovers are driven by peer effects due to social interactions among similar individuals (homophily, e.g. [Bell and Machin, 2013](#)), we find the strongest effects when the refugee and the neighbor either come from countries where the primary language belongs to the same language family or share the same country of origin, making interactions arguably easier. The effects are also more pronounced when both individuals are young, unmarried, or childless – characteristics associated with a higher likelihood of committing crimes.

Next, we investigate what drives the stronger effects among similar individuals. One possibility is that, as refugees are induced by the reform to commit crimes, those who evade convictions may share their experiences with similar neighbors, lowering their perceived risks and increasing their perceived benefits of criminal activity ([Sah, 1991](#)). Supporting this hypothesis, we find stronger spillover effects in municipalities where a lower share of reported crimes ultimately results in convictions.

Another possibility is that refugees and neighbors with similar characteristics commit crimes together (i.e., a “partners in crime” mechanism, e.g. [Billings et al., 2019](#)). 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 three 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.

As neighbors exposed to crime-committing refugees (peers) engage in criminal activity themselves, they may become more likely to live in high-crime neighborhoods, further increasing their exposure to crime-prone peers. Consistent with this mechanism, we find that the reform increased the likelihood that non-Danish neighbors remained in the buildings to which refugees were assigned and led them to ultimately reside in buildings with a higher share of peers with a prior criminal history.

We explore mechanisms beyond peer effects but find little evidence that they are the primary drivers of the spillovers. First, we investigate whether changes in welfare payments and labor force participation among refugees influenced their neighbors' welfare receipt and work choices. These effects could arise from peer influences in welfare use 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 government transfer income, labor force participation or labor income of non-Danish neighbors, suggesting that this channel is unlikely to play a major role.

Finally, we consider whether increased crime among refugees in a specific area could have led to a rise in policing efforts, and consequently, to more crimes being detected rather than committed. Since our specifications include municipality fixed effects and Danish and non-Danish neighbors are, *ex ante*, similarly likely to commit crimes, increased policing would need to be both highly localized and specifically targeted at non-Danish neighbors to explain our findings. In contrast, we do not find that spillover effects are particularly concentrated in municipalities where the refugees reside, nor in areas with high shares of anti-immigrant votes or where police tend to overcharge immigrants relative to Danes.

This is the first study to analyze the spillover effects of welfare spending on criminal behavior. Despite a recent and growing literature showing that changes in welfare benefits have important effects on the criminal behavior of welfare recipients and their families ([Deshpande and Mueller-Smith, 2022](#); [Dustmann et al., 2023](#) ; [Dustmann et al., 2024](#)), 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 behavior 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 behavior 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 behavior 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 behavior 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 remainder 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 evaluates the welfare implications of the reform based on the main results. Section 7 discusses potential mechanisms. Section 8

provides a number of robustness checks of our baseline results. Finally, Section 9 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 1st, 2002, and the law was passed on May 1st, 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 1st 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. At the time of the Start Help reform, after applying for asylum refugees faced an average wait of around 16 months for a decision on their status (Hvidtfeldt and Schultz-Nielsen, 2018). This is important for our empirical design: asylum decisions around our threshold were for applications filed long before asylum seekers would have known about the reform, meaning refugees were unable to change their immigration behavior in response to it.

If an asylum seeker is granted refugee status, the Danish Immigration Service will decide

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

⁴The 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.

⁵We thus have three types of couples in our data. Type A couples, where both spouses arrived before July 1st 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 1st 2002, their combined benefits were capped to the Start Help amounts, hence they also received the reduced transfers in Start Help.

in which municipality the refugee shall live.⁶ They base this decision on preferences the refugee may have expressed during the asylum application process, annual quotas for refugees placed in each Danish municipality, and other considerations such as special medical or educational needs. Refugees are expected to remain in their placement municipality for three years under threat of losing their monthly benefit payment (Farrokhi and Jinkins, 2023).

The municipality is required to find housing for assigned refugees (DK-, 2001). While each municipality can create its own procedures for this process, typically they place refugees in social housing (*almen boliger*) through the standard municipal procedures for finding housing for people in need. In Denmark, new social housing 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 need, such as those with inability to find housing through ordinary channels and few economic resources (Anker et al., 2002). These could be homeless people, people fleeing from violent partnerships, or people with disabilities. The exact housing unit into which a refugee is placed depends on which housing fitting family-size and income needs is available to the municipality at the time (Billings et al., 2024).

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.

A week after the Start Help reform was passed by parliament, several additional reforms to the Danish asylum system were enacted. These reforms tightened the qualifications for refugee status, imposed stricter constraints on family reunification, and specified who could be denied refugee status due to connections with third countries. These rules applied only to

⁶Additional details of the asylum process can be found in Dustmann et al. (2023).

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

asylum seekers who *applied* for refugee status after July 1st, 2002. These reforms are unlikely to pose a threat to our empirical approach, since the Start Help reform applied to all asylum seekers who were *granted* refugee status after July 1st, 2002. As noted above, during this period, the average waiting time from application to decision was 16 months. Consistent with this hypothesis, we do not observe significant differences in the characteristics of refugees who were granted residency before or after the July 1st cutoff (see Section 4.1).⁸

3 Data

This study relies on data from the Danish registers. 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, KRAN). These data are collected by the government to provide services, assess tax liability, make sure people are legally in Denmark, and to create 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 B.

We focus on adult refugees and their neighbors who are not part of their family. Our refugee sample includes refugees and their spouses with family reunification visas who were granted residency within 16 months of July 2002, when the new policy took effect.⁹ Under Start Help, a couple received lower transfers if at least one spouse arrived after July 1st, 2002. Therefore, we assign both spouses the latest date of residence permit within the couple if either one spouse received residency before and the other after July 1st, 2002, or if both spouses received residency before that date. If both spouses arrived after July 1st, 2002, we assign them the earliest date of residency. Following [Dustmann et al. \(2023\)](#), we include only refugees who were between 18 and 55 years old at the time they were granted residency and we exclude refugees from Afghanistan and the Balkans. The Danish immigration service temporarily halted the processing of Afghan asylum cases following the fall of the Taliban regime in late 2001, and Kosovo was deemed safe in early 2002. These changes affected the granting of refugee permits to people from these two regions around the time of the Start Help reform. For more details on data construction, we refer to Appendix B.

⁸For more information about these reforms and other reforms affecting refugees over the past several decades, see [Hvidtfeldt and Schultz-Nielsen \(2018\)](#). For detailed implementation dates, see the Danish Aliens Act of 2002 ([Folketinget, 2002](#)).

⁹A Danish family reunification (familiesammenføring) visa is for spouses and children of Danish residents.

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.¹⁰ 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 to avoid possibly endogenous sorting into buildings after refugee arrival. We discuss this choice in further detail in the next section. We drop anyone who was younger than 16 or older than 55 years of age at the time the refugee was placed,¹¹ 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 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

¹⁰In Appendix Table A.1 we show summary statistics for the buildings in our sample. We have 1985 buildings, which contain a little over 11 neighbors on average, of whom an average of 7 are adults and thus in our sample. Around 60% of neighbors are of Danish origin.

¹¹The 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.

outcomes of refugees who were granted residency just after and just before the reform cutoff date. The estimating equation takes the following form:

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

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.¹² 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 behavior 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 neighboring 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:

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

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 variable; and Z is a vector of predetermined controls.¹³ 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

¹² We include the following controls, measured as of the time of residency permit: age, gender, marital status, and fixed effects for number of children (up to four), continent of origin and municipality. In Section 8, we show that the results are robust to excluding these controls.

¹³ We include the following neighbors' characteristics, as of the year in which the refugee's arrived in the building: age, gender, marital status, and fixed effects for the number of children, continent of origin, and municipality. Since for neighbors we also have information on Y in the two years prior to refugee arrival, we also control for this variable. 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 8).

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 8). 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.¹⁴ 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 8 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.¹⁵ Even under this restriction, however, the many-to-one problem persists in 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 8, 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

¹⁴We 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.

¹⁵This restriction limits the set of refugees who can potentially affect a given neighbor, but it comes at the cost of ignoring spillovers across buildings. However, this is unlikely to be a major concern in our setting, as [Billings et al. \(2024\)](#) have shown that neighborhood effects in Denmark tend to be stronger among individuals living within a two-minute walk of each other. In particular, neighborhood effects on crime convictions—which is the focus of our analysis—although insignificant, exhibit a large gradient in distance. To the extent that spillover effects extend across buildings, our estimates can be interpreted as a lower bound for the overall spillovers from the reform.

construct the running variable. In this analysis, we find similar spillover effects.

Second, neighbors may endogenously sort across buildings in anticipation of the reform. To address this concern, we assign neighbors to buildings based on their residence information in the 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, or whether the refugee will receive residency rights just before or just after July 1st. 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. As a result of this restriction, our main analysis focuses on pre-existing, as opposed to current, neighbors. When exploring the mechanisms behind the main effects, we then investigate whether the composition of current neighbors changes as an effect of the reform (see Section 7.1.2).

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 behavior of refugees. This approach, however, would require the reform to be a strong predictor of a refugee’s criminal behavior 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. An instrumental variable approach would also require a monotonicity assumption, namely that the reform shifts refugees’ criminal behavior in the same direction for all individuals. This assumption may be difficult to justify in our setting, where responses to the reform appear 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, marital status, number of children, and region of

origin (panel A),¹⁶ 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 1st 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

¹⁶We note that Table 2 shows a marginally significant (at 10% level) imbalance for gender. While our baseline specifications control for gender, we obtain similar results when excluding control variables (see Section 8). This suggests that the gender imbalance is unlikely to be problematic in our setting.

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 discuss balance tests performed on non-Danish neighbors and neighboring refugees, for whom we find significant spillovers, we reach similar conclusions from balance tests conducted on the Danish neighbors and their neighboring refugees (see Appendix Table B.1).¹⁷

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 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.¹⁸ Appendix Table A.2 presents the corresponding RDD estimates obtained from estimating equation (1).

Refugees arriving after July 1st 2002 experience a decrease of \$9,159 in welfare benefits in their first full year after receiving a permit, equivalent to a substantial 32% decrease relative

¹⁷Of the 26 variables included in Table B.1, two—refugee age and continent of origin—exhibit some imbalances. While our baseline specification controls for refugees’ age and continent of origin, the fact that we continue to find insignificant effects on Danish neighbors even when excluding these controls suggests that these characteristics are unlikely to be a major source of concern in our setting (see Section 8).

¹⁸In 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 1st 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 1st 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.

to refugees being granted a permit before Start Help was enacted. The difference in transfers grows over time, although at a decreasing pace, and four years after arrival, the accumulated difference in transfers doubles to \$16,407. After those first four years, the difference in total benefits remains stable, suggesting no differences in yearly transfers survive beyond the first few years.¹⁹ [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.²⁰ 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 increase in the likelihood of refugees being convicted of any crime, a 33% 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 approximately 70% relative to the control mean in the likelihood and number of property crimes. 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

¹⁹Even though treated refugees have higher average labor income than untreated refugees in the years following residency, both groups see quickly increasing labor income and labor force participation. The gap in transfer income between groups falls not because treated refugees receive more benefits over time, but rather untreated refugees also enter the workforce and therefore receive less transfer income. In addition, Start Help no longer applies to refugees once they have lived in Denmark for seven years. After that, benefits rise to the level of refugees who were granted residence before July 1st, 2002.

²⁰Our findings are in line with those of [Dustmann et al. \(2023\)](#). We find a large first-year drop in transfers and a positive but modest labor income response at the threshold, as well as higher conviction risk concentrated in property crime. Our results are not numerically identical to those in [Dustmann et al. \(2023\)](#) for the following reasons: our running variable is measured in days rather than months; our sample window is 16 rather than 18 months around the cutoff; we do not exclude refugees who remigrate within nine years, as we do not have information on permanent exits; our crime results are for refugees who were 18-55 years old at residency rather than 18-45 years old with children; and our crime results are for 10 years of accumulated convictions rather than five.

exposed to. These results are in line with the results of panel A and indicate an 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 are also less precise due to the smaller sample size. Overall, the findings suggest that Start Help led to substantial increases in property crime among its recipients. While this section focuses on the overall effects within the 10 years following the reform, in Section 5.4 we examine how these effects evolved over time during the same period.

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 behavior 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 5.4, 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 insignificant effects. In Section 8 we show that both the insignificant effects on Danish neighbors and the significant effects on non-Danish neighbors are robust to various specifications and robustness checks. Given the insignificance of the effects on Danish neighbors, the following discussion will focus on the effects observed among non-Danish neighbors. In Section 7, we return to the question of why these effects are observed for non-Danes but not for Danes.

The effects on non-Danish neighbors are large: being exposed to a refugee that arrived after July 1st 2002, and who thus received fewer transfer benefits, leads to a 8.9 p.p. (57% of the control mean) and a 0.42 unit (110% 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.²¹

²¹Appendix 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 weapon-related crimes, crimes against public order or police, and drug-related crimes, although the coefficient on drug-related crimes is not statistically significant. Moreover, the last column shows that Start help did not affect traffic crimes, which we exclude from our baseline measures of crime.

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 investigates the heterogeneity of spillover effects among non-Danish neighbors by the crime propensity of the neighboring refugee. Specifically, we construct a crime propensity index for each refugee in the sample, capturing the predicted likelihood of committing a crime based on predetermined characteristics.²² Refugees are ranked by this predicted measure and assigned a crime index ranging from 0 to 1. Following previous work in the Danish setting (see Dustmann and Landersø, 2021), we classify as crime-prone those refugees with an index above 0.6. Crime-prone refugees are, on average, younger, more likely to be male, less likely to be married, and have fewer children than the average refugee in our sample (see Appendix Table A.5). Column 1 of Table A.4 reports the baseline effects on all neighbors from Table 4, while Columns 2 and 3 present estimates for neighbors of crime-prone and non-crime-prone refugees, respectively. The results indicate that spillover effects are stronger among neighbors of refugees with higher crime propensity.

Appendix Table A.6 further 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 and above the median age of 32 years old at the time of the refugee’s arrival (columns 2 and 3), males and females (columns 4 and 5), parents and childless individuals (columns 6 and 7), and married and unmarried individuals (columns 8 and 9). We find that treatment effects are heterogeneous and concentrated among traditionally “crime-prone” groups, such as males, the youth, the childless, and the unmarried.

In our baseline specification, we follow the literature on spillovers from government interventions and estimate regressions at the individual level (see, for instance, Dahl et al., 2014a; Dustmann et al., 2024). Under this approach, buildings with a larger number of neighbors receive greater weight in determining average effects. In Appendix Table A.7, we present results from an alternative specification in which we weight observations by the inverse of the number of individuals residing in each building, so that all buildings receive equal weight. We find effects that are consistent with our baseline results and, if anything, larger in magnitude. In the next section, we discuss the magnitude of the spillover effects

²²We use as predictors the following characteristics measured at the time of residency: age, gender, marital status, number of children, continent of origin, and municipality.

estimated under both the baseline and alternative specifications.

5.3 Magnitude of Spillover Effects

How large are the estimated effects, and what do they imply about the social multiplier of criminal activity? Our baseline results imply that 2.72 aggregate crimes among neighbors are associated with each property crime induced by the reform among refugees, leading to a social multiplier effect of 3.72.²³ These estimates capture both direct and indirect effects on neighbor crime. Direct effects arise when crimes committed by affected refugees directly influence neighbors' criminal behavior, for example through peer effects. Indirect effects may result from mechanisms such as snowball dynamics among neighbors or changes in neighborhood composition, potentially driven by rising poverty rates among refugees. These mechanisms may amplify the overall magnitude of the observed spillovers.²⁴

The multiplier figure reported above includes spillover effects on both property and non-property crimes, providing a comprehensive measure of the size of spillovers. When we restrict attention to spillovers in property crimes, where the estimated effects among refugees are statistically significant, the implied multiplier decreases to 2.75. Another implicit assumption behind our baseline multiplier estimates is that larger buildings receive more weight, since the regressions are estimated at the individual level. In Section 5.2, we also report estimates from regressions in which observations are weighted by the inverse of building size, thus assigning equal weight to each building regardless of the number of residents. Under this alternative specification, the implied social multiplier ranges between 3.16 and 4.06, depending on whether we consider only property crimes or all crimes.²⁵

Finally, it is worth noting that, for consistency with existing estimates of crime multipli-

²³We calculate the social multiplier as follows: based on Table 3, Start Help increases the likelihood of committing a property crime by 0.047 percentage points among the 2,636 refugees in buildings with only one refugee, resulting in approximately 124 additional crimes. We focus on the effects of the reform on property crime among refugees as, differently from the effects on any crime, these are statistically significant. Based on Table 4, Start Help also increases the likelihood of committing any crime by 0.089 percentage points among the 3,797 non-Danish neighbors, resulting in approximately 337.9 additional neighbor crimes. Hence, 2.72 neighbor crimes arise for every crime induced by the reform among refugees.

²⁴This approach abstracts from the strength of social ties when estimating the social multiplier. Accounting for such ties requires specifying and estimating a model of crime with social interaction, such as the one proposed by [Dustmann and Landersø \(2021\)](#). Re-estimating their model in our setting, we obtain a (noisy) estimate of the social multiplier of 2.42 (see Appendix A.1 for details). Although imprecisely estimated, this value is comparable to those reported in this section and smaller than the multiplier of 5 obtained by [Dustmann and Landersø \(2021\)](#) using the same approach in the Danish context.

²⁵We estimate these alternative multipliers using the same method as in the previous paragraph, focusing either on property crime effects alone (Table 4, Panel B, column 3), or on reweighted estimates for neighbors (Appendix Table A.7). For the effects on refugees, all multiplier calculations are based on the estimated impact on property crimes, as reported in Table 3, Panel B, column 3. We focus on property crimes throughout, as these are the only crime categories for which the estimated effects are statistically significant.

ers, our discussion so far has focused on aggregate effects. However, one can also calculate how many additional crimes per neighbor arise for every additional crime that the policy induces per refugee. In our setting, this ratio is 1.89 for spillovers in all crimes and 1.21 for property crimes, pointing, consistent with the aggregate estimates, to sizable spillover effects.²⁶

Independently of the specific measure we select, crime multipliers implied by our estimates are within the range of other estimates in the literature, as shown in Appendix Figure A.8.²⁷

5.4 Effects over Time

The above results 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 cumulative criminal convictions in the first full year after a refugee’s residency is granted, the first two full years, the first three full years, etc. For the neighbors’ analysis, we can also create total crime in the year of exposure, in the year prior to exposure, and in the two years prior to exposure.²⁸ We then estimate the RDD specification (2) for each of these outcomes,²⁹ 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 property 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. The effect stabilizes and remains of similar magnitude in the subsequent years.³⁰ These results suggest that Start Help mainly affected refugee’s likelihood of committing

²⁶These estimates are calculated as the ratio of the reform’s effect on the probability of committing a crime among neighbors, reported in Table 4 (columns 1 and 3 in Panel B for all crimes and property crimes, respectively), to the corresponding effect among refugees, reported in Table 3 (column 3 in Panel B).

²⁷ The estimated social multiplier tends to be smaller in papers analyzing effects on arrests or prison and larger for convictions – which in Denmark mostly result in fines – and school misbehavior, suggesting larger effects for less severe outcomes.

²⁸We cannot analyze convictions prior to residency for immigrants, because they were not yet in Denmark, and hence in our Danish data.

²⁹In this set of results for neighbors’ crimes, we do not control for crime in the two years prior (as in our baseline specification) to be able to directly interpret the effect of Start Help.

³⁰Appendix Figure A.9 presents results for the number of crime convictions instead of the likelihood of convictions and confirms this pattern of results.

crimes in the first few years after residency, but that it did not lead to a long-term increase in crime for these individuals relative to the control group. 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.

The results for non-Danish neighbors, on the right side of Figure 6, display several patterns. First, the figure shows that non-Danish neighbors exposed to refugees who received lower welfare benefits due to Start Help have similar crime convictions in the two years prior to refugee arrival to neighbors who received higher benefits, confirming that exposure to refugees eligible for Start Help is exogenous to prior criminal convictions. Second, it shows that non-Danish neighbors exposed to Start-Help refugees are almost four percentage points more likely to be convicted of a crime in the two full years after exposure, compared to neighbors not exposed to start help refugees. This gap increases over time, reaching 8.9 percentage points ten years after exposure. Third, the effects on overall crime are driven by both property and non-property crimes, unlike the effects observed among refugees themselves.

That spillover effects on neighbors continue to increase over time raises the question of whether this growth is driven by crime spreading to a larger number of treated buildings or by more individuals being convicted within the same buildings where crime initially increases following the reform. To investigate this, Appendix Figure A.10 presents the effect of Start Help on the probability that any neighbor in a building is convicted of a crime, effectively measuring crime at the building rather than the individual level. The results show that, while individual-level effects continue to increase over the 10-year period (Panel A), building-level effects stabilize after the first two years (Panel B). Although the building-level effects are noisy and should be interpreted with caution, this pattern suggests that the growth in individual-level effects is driven by an increase in the likelihood of convictions among individuals originally living in buildings where crime increased in the first few years after exposure to (treated) refugees.

Overall, the evidence that crime effects among neighbors are more persistent than effects among refugees, and that these effects intensify within originally affected buildings over time, suggests that the impact of the reform may self-reinforce among neighbors.³¹ 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. For a dedicated discussion of potential mechanisms

³¹We abstract from modeling the causal chain through which spillover effects cascade in the network, as such estimation would require additional assumptions about the sequence in which neighbors are affected by refugees. These assumptions would be difficult to justify in our setting, where we do not observe the chronological order in which neighbors are affected.

behind the spillover effects, we refer to Section 7.

6 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). Following an approach similar to other related studies (e.g., Deshpande and Mueller-Smith, 2022), we estimate the MVPF of increasing reduced Start Help welfare benefits to their pre-reform level. That is, we compare society’s willingness to pay for restoring higher benefits to the cost of funding them. In line with our baseline results, we evaluate benefits and costs over a 10-year period. In what follows, we first describe our cost calculations, then our willingness-to-pay calculations, and finally we discuss the resulting MVPF estimates.³²

6.1 Net cost of higher welfare benefits

The reform generated fiscal savings for the government by reducing welfare expenditures, which we estimate directly for the average refugee using equation (1) (see Table A.2). From these savings, we subtract the forgone tax revenues on welfare transfers, since such transfers are taxable in Denmark, and add the additional tax revenues resulting from the increase in labor-force participation and earnings induced by the reform. We estimate the effects on earnings based on equation (1) (see Table A.2). Thus, we apply a tax rate of 38 percent, the bottom-bracket rate at the time of the reform, to both welfare benefits and labor income to compute tax revenues. Panel A of Table 5 (MVPF components 1 to 3) presents the resulting fiscal savings, with detailed calculations provided in Appendix Table A.8.

From these savings, we deduct the costs associated with increased crime. We consider two broad cost categories: costs to taxpayers and costs borne by crime victims. Following Deshpande and Mueller-Smith (2022), we treat the former as part of the net fiscal cost and present them in this section, while the latter enter the MVPF as an increase in society’s willingness to pay for welfare and are therefore discussed in the next section.

Taxpayer costs in our setting consist of enforcement and prosecution expenses.³³ We obtain crime-specific estimates of such costs from the Danish State Prosecutor’s Office and ag-

³²All costs and benefits are expressed in 2021 US dollars. To ensure comparability across the separate samples of refugees and neighbors, we consider total rather than average changes in costs and benefits.

³³We find no significant effect on imprisonment. These results are not shown but are available upon request.

gregate them to the property-crime and non-property-crime levels by weighting each crime’s unit cost by its sample frequency among refugees or neighbors affected by the reform. Panel A in Table 6 reports the cost estimates for each detailed crime type, the frequency weight associated with each crime type, and the resulting weighted costs of property and non-property crime.

We compute separate cost totals for crimes committed by refugees and by non-Danish neighbors, based on the estimated reform effects on property and non-property crime counts and the unit costs from Table 6. We include only crime categories for which the reform has a statistically significant effect: property crime for refugees, and both property and non-property crime for non-Danish neighbors. Panel A of Table 5 (MVPF components 4 and 5) shows the resulting total cost figures, with further details provided in Appendix Table A.9.

6.2 Willingness to pay for higher welfare benefits

We define a recipient’s willingness to pay as the reduction in transfers resulting from the reform, following a similar approach to [Deshpande and Mueller-Smith \(2022\)](#). This is interpreted as the amount a recipient would be willing to pay to retain the welfare benefits reduced by the reform. Since welfare transfers are taxed in Denmark, we deduct from this amount the taxes that the recipient would have paid on the benefits if received.

To obtain society’s total willingness to pay for higher benefits, we add the costs borne by crime victims. In the baseline, most conservative, scenario we include only crimes that result in convictions. Unit victim-cost figures by crime type are taken from [Deshpande and Mueller-Smith \(2022\)](#) for the United States, as equivalent estimates are not available for Denmark. We aggregate these costs to the property-crime and non-property-crime levels by weighting each unit cost by the frequency of that crime among refugees or their neighbors affected by the reform.

We also consider broader cost estimates that account for crimes without convictions, which may still result in costs for victims. In this case, we scale conviction-based victim costs by the inverse of the conviction rate, defined as the share of reported crimes that result in conviction. Reported crime counts are obtained from Danish registers. Panel B of Table 6 reports the victim cost estimates by detailed crime type, the corresponding frequency weights and conviction rates, and the resulting weighted costs of property and non-property crime.

As with enforcement and prosecution costs, we calculate separate totals for crimes committed by refugees and by non-Danish neighbors, using the estimated effects of the reform on crime counts. Only crime categories with statistically significant reform effects are included. Panel A of Table 5 (MVPF components 6 to 9) shows the resulting total cost figures, with

further details provided in Appendix Table A.9.

6.3 Marginal value of public funds

Table 5 summarizes the MVPF estimates obtained by taking the ratio of willingness-to-pay and net cost figures. Panel B presents results for our most conservative approach, which includes only victim costs from convicted crimes. Panel C presents results that also account for victim costs from reported crimes.

Abstracting from the costs associated with increased crime from the reform, we estimate an MVPF of 0.976. That is, society is willing to pay only 0.976 dollars for an additional dollar of welfare support. When we account for the costs of convicted crimes committed by refugees and their neighbors, the MVPF increases to 1.191. This is approximately 21% higher than the MVPF of 0.983, which excludes spillover effects but accounts for victims' costs from refugees' crime, and 22% higher than the MVPF of 0.976 obtained by excluding all crime-related costs. When victim costs for reported crimes are included, the MVPF increases further to 1.791. This is approximately 66% higher than the corresponding MVPF of 1.078, which includes only refugees' crime costs and does not account for spillover effects on neighbors.³⁴

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).³⁵ Since our focus is on the contribution of spillovers to the MVPF, as long as crime spillovers impose additional costs on the government and victims, our conclusion that the MVPF inclusive of spillovers is higher than without spillovers remains valid, even if the specific MVPF values may change when accounting 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.10 we provide MVPF estimates that, under some assumptions, factor in 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). MVPF estimates remain 16% to 59% higher with spillovers than without, depending on whether victim costs are based on

³⁴In line with the existing literature, the analysis in this section abstracts from the fact that costs to victims of property crimes may represent benefits to offenders. Assuming that the entirety of victims' costs is transferred to offenders and incorporating these benefits, we estimate that neighbors' spillovers increase the MVPF by 20% to 44%, depending on whether convicted or reported crimes are considered.

³⁵Figure 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.

convictions or reported crimes (see Appendix Table A.10).

Finally, we note that the cost-benefit analysis so far assumes, in line with the significance of the baseline coefficients (see Table 4), that the reform has no effect on the criminal behavior of Danish neighbors. To account for the fact that some of the coefficients estimated for Danish neighbors in Table 4 are not exactly zero, we present MVPF calculations in Appendix Table B.2, assuming that the insignificant effects on Danish neighbors are different from zero and evaluating them at their point estimates. Since the estimated effects on Danes have a negative sign, the benefits from reduced crime among Danish neighbors partially offset the costs resulting from increased crime among non-Danish neighbors. Nonetheless, the MVPF that includes spillovers remains 3% to 24% higher than the MVPF without spillovers, depending on whether victim costs are based on convictions or reported crimes, respectively.

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.

7 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.

As a first step in determining whether a link exists between criminal activity among refugees and their neighbors, Appendix Table A.11 tests whether increases in non-Danish neighbors' criminal activity occur among neighbors of refugees who also increased their criminal activity.³⁶ We find that the increase in non-Danish neighbors' likelihood of crime convictions over 10 years (column 1) is driven by the likelihood that both the neighbors and the refugees they were exposed to were convicted of crimes (column 2).³⁷ This suggests an important link between refugees' and their neighbors' criminal activities.³⁸

³⁶ To do so we cannot simply estimate the effects on neighbors' crime among those whose refugee did or did not commit a crime, as we would be selecting the sample on an endogenous outcome. Instead, we create new outcomes by interacting the likelihood that neighbors and refugees did or did not commit a crime.

³⁷ On average, buildings in which a treated refugee (i.e., one who received residency on or after July 1, 2002) committed a crime housed approximately 50 residents and 10 non-Danish neighbors in our sample. Among these neighbors, approximately 20% were convicted within 10 years of the refugee's arrival in the building.

³⁸ In Table A.12, we further break down the effects from column 2 of Table A.11, distinguishing between larger (above-median) and smaller (below-median) buildings, defined either by total resident population (columns 2 and 3) or by the number of non-Danish neighbors in our sample (columns 4 and 5). Consistent with the sizable multiplier effects, we find stronger impacts in larger buildings, where relatively more neighbors may have been exposed to the criminal behavior of the refugees.

Why are non-Danish neighbors more likely to be convicted of crimes when exposed to crime-committing refugees? There are at least three possible explanations for this finding. First, the reform may have increased crime among neighbors through peer effects in criminal behavior. Second, changes in transfers and labor supply among refugees may have affected transfers and labor supply among their neighbors, potentially leading to increased criminal activity. Third, the rise in crime among refugees could have prompted heightened policing in specific areas, such that the observed increase in convictions reflects increased enforcement rather than a rise in underlying criminal activity.

Below we discuss each of these three mechanisms in detail.

7.1 Peer Effects in Crime

One potential explanation for our findings on criminal convictions among non-Danish neighbors is the presence of peer effects in crime. These may arise both from direct interactions with refugees and from changes in the composition of peers to whom neighbors are exposed over time. In what follows, we explore these two channels in detail.

7.1.1 Peer Effects between Refugees and Neighbors

The findings so far point to a significant link between the criminal activity of refugees and their neighbors. To investigate whether this association reflects peer effects, we re-estimate our main effects on non-Danish neighbors separately for neighbors who do 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,³⁹ 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 crimes. At the same time, the effects are smaller if both the refugee and the neighbor are married or have children, suggesting that peer effects are weaker among individuals who are less likely to commit crimes.⁴⁰

³⁹We group countries into language families based on Lewis (2009).

⁴⁰In 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 crimes than other Danes (see Appendix Table A.13).

This evidence leaves open the question of what ultimately drives peer effects among similar individuals. The literature has proposed several potential explanations (for a review, see, for instance, [Lindquist and Zenou, 2019](#)). First, having criminal peers could influence perceptions of the benefits and costs of crime (e.g., [Sah, 1991](#)). In our context, as refugees are induced by the reform to commit crimes, they may share their experiences with similar neighbors, altering the perceived risks associated with criminal activity. Unfortunately, the available data do not allow us to directly measure perceived risks or benefits of crime. However, if this mechanism is driving our results, we would expect stronger spillover effects in cities where a lower share of crimes ultimately results in a conviction, and therefore where the perceived risk associated with committing crimes may be lower. To investigate this hypothesis, Appendix Table [A.14](#) presents separate estimates for areas where the ratio of crimes convicted to crimes reported is above versus below the median or the 75th percentile, in panels A and B respectively. Consistent with this hypothesis, the table shows that the spillover effects of Start Help are stronger in areas where crime is more likely to go undetected.⁴¹

Alternatively, criminal peers might provide crime-specific human capital (e.g., [Bayer et al., 2009](#)), or they could create opportunities to commit crimes together (i.e. *partners in crime* story, see for instance [Billings et al., 2019](#)). Our findings do not support these two alternative explanations. First, we only find evidence of refugees committing more property crimes, while our results suggest that neighbors commit both more property and non-property crimes. This indicates that refugees are unlikely to have provided non-property crime human capital to neighbors. Second, by linking criminal cases together, Appendix Table [A.15](#) shows that our effects remain 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.

7.1.2 Residential Mobility and Composition of Peers

As neighbors exposed to crime-committing refugees engage in criminal activity themselves, they may become less able to afford, or less likely to be offered, housing in low-crime neighborhoods. This can induce residential sorting into higher-crime environments, thereby increasing exposure to crime-prone peers and potentially amplifying the observed effects on crime.

⁴¹An alternative mechanism consistent with the fact the the reform might have changed the perception of costs and benefits associated with criminal activity is whether the exposure to refugees receiving lower welfare might have generated a sense of injustice among immigrants which would have triggered more crime. Unfortunately, the data at hand do not allow to test the importance of such mechanism, that we consider however as broadly consistent with changes in crime perception associated with refugee contact.

To investigate this possibility, we begin by estimating the effect of the reform on the likelihood that neighbors move out of the buildings to which refugees were originally assigned. Panel A of Figure 8 presents the results. It shows that the reform significantly reduced the likelihood that neighbors move out of these buildings, with effects that intensify over time. We estimate that, four to ten years after exposure, neighbors are 10 percentage points less likely to have moved out of the building. This is a sizable reduction, given that most (90%) neighbors in our sample move out of the original building within ten years of the reform.⁴² Such a reduction may have prolonged neighbors' exposure to crime-prone peers.⁴³

The fact that a large share of neighbors move out of the buildings to which refugees were originally assigned raises the question of whether the reform may have changed the type of buildings or neighborhoods in which these neighbors ultimately reside. Such effects could arise, for instance, if neighbors who, as a result of the reform, are more likely to have a criminal history are less likely to find housing in better neighborhoods. To investigate this possibility, we identify the buildings in which non-Danish neighbors reside in each year after refugee exposure and measure the share of building residents with prior (1998–2000) crime convictions. Panel B of Figure 8 reports the effect of the reform on the share of residents with prior convictions in the buildings where neighbors reside after the reform. We find that, over time, non-Danish neighbors affected by the reform become more likely to live in buildings with residents who have a prior criminal history, and these effects are statistically significant ten years after the reform. The magnitude is sizable: exposure to a Start Help refugee leads to living in buildings with a 2.1 percentage point (approximately 60 percent) higher share of individuals with past crime convictions.⁴⁴

Overall, these findings indicate that exposure to refugees affected by the reform induced changes in housing among neighbors that led to additional exposure to crime-prone peers. These housing-mediated peer effects may have amplified the direct effects from refugees, helping to explain the increasing spillover effects over time documented in Section 5.4.

⁴²The high rate of out-mobility in our setting likely reflects the fact that many residents consider public housing a temporary rather than permanent accommodation.

⁴³ We do not find significant effects of the reform on refugees' probability of moving out of the original buildings to which they were allocated when focusing on buildings that host at most one refugee family, which are more directly relevant for the analysis of spillovers on neighbors. By contrast, we find positive and significant effects when considering all refugees affected by the reform, including those in buildings with multiple refugee families (see results shown in Figure A.11). Moreover, buildings that host both refugees and neighbors tend to have higher crime rates than other buildings and may constitute relatively crime-prone environments for neighbors.

⁴⁴ The fact that, in a separate analysis, we fail to find effects of the reform on the composition of the buildings in which refugees were originally placed suggests that the patterns in Panel B are driven by differences in the types of buildings neighbors eventually live in (see Figure A.12).

7.2 Changes in Work and Transfers

A second 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.16 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 1st 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.

7.3 Changes in Policing

A third explanation is that the observed increase in neighbors' crime conviction is driven by changes in policing and not changes in criminal activity (e.g. Levitt, 1997). 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 1st 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. Moreover, crime rates among Danes and non-Danes in our sample are similar prior to the reform (Table 4), making them equally likely to be detected under intensified policing.

For changes in policing to drive our effects, the policing response would need to be both highly localized and specifically targeted at non-Danes. In Appendix Section A.2, we examine this hypothesis in detail. We show that spillover effects are not only concentrated in municipalities where refugees are assigned after obtaining residency (Appendix Table A.17), nor in areas with high shares of anti-immigrant votes or where police are more likely to overcharge immigrants relative to Danes (Appendix Table A.18). Overall, the findings from this section suggest that localized changes in policing are unlikely to be the primary or sole driver of the observed spillover effects.

8 Robustness Checks and Placebos

Table 7 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 nearly 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 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 Figure A.13 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 Figure A.14 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.14. The two-sided p-values for all crimes (panel A), property crimes (panel B) and non-property crime (panel C) are 0.002, and 0.012 and 0.028 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 Table A.19 shows the effects obtained by using the likelihood of being charged, rather than convicted, for a crime as an alternative measure of criminal behavior. 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.20 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 1st 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.

In Appendix C, we present analogous robustness checks for the spillover effects on Danish neighbors, finding results that are generally consistent with the baseline results of insignificant spillovers on Danes.⁴⁵

⁴⁵An exception to these trends is the effect observed from very short windows, such as those obtained from the optimal bandwidth or RD robust approach in Appendix Table B.4, which are based on a window of 2-3 months, or those derived from windows shorter than 4 months in Appendix Figure B.3. The significance of these effects is driven by the fact that these specifications are more sensitive to the higher crime rates observed in month -1 (see Appendix Figure B.2), which can in turn be traced back to a single building, arguably an outlier.

9 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 three 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 are more consistent with the existence of peer effects in crime.

With a more theoretical approach, future research could explore the channels through which policies might reduce the negative spillovers from welfare programs. This could include, for example, the design of more targeted allocation policies of refugees across buildings or neighborhoods. In addition, examining how changes in welfare benefits for natives, rather than immigrants, spill over to their neighbors could provide valuable insights into the broader functioning of social interactions and spillover effects in different contexts.

References

- (2001). Lov om integration af udlændinge i danmark (integrationsloven). Lovbekendtgørelse (LBK) nr. 643 af 28. juni 2001. Konsolideret udgave.
- 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.
- Anker, J., Deding, M., and Nørgaard, L. (2002). Boligsocial anvisning i fire kommuner. Arbejdspapir 24:2002, Socialforskningsinstituttet (SFI), København.
- Ballester, C., Calvó-Armengol, A., and Zenou, Y. (2006). Who's who in networks. wanted: The key player. *Econometrica*, 74(5):1403–1417.
- 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.
- Bell, B. and Machin, S. (2013). Immigrant enclaves and crime. *Journal of Regional Science*, 53(1):118–141.
- 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., Deming, D. J., and Ross, S. L. (2019). Partners in crime. *American Economic Journal: Applied Economics*, 11(1):126–150.
- Billings, S. B., Hoekstra, M., and Rotger, G. P. (2024). The scale and nature of neighborhood effects on children. *Journal of Public Economics*, 240:105260.
- 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.
- Calvó-Armengol, A., Patacchini, E., and Zenou, Y. (2005). Peer effects and social networks in education and crime. Technical report, IUI Working Paper.
- 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.
- Dustmann, C., Landersø, R., and Andersen, L. H. (2024). Unintended consequences of welfare cuts on children and adolescents. *American Economic Journal: Applied Economics*.
- 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.
- Folketinget (2002). LOV nr 365 af 06/06/2002: Lov om ændring af udlændingeloven og ægteskabsloven m.fl. Accessed: 2025-02-06.
- 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.
- Glaeser, E. L., Sacerdote, B. I., and Scheinkman, J. A. (2003). The social multiplier. *Journal of the European Economic Association*, 1(2-3):345–353.

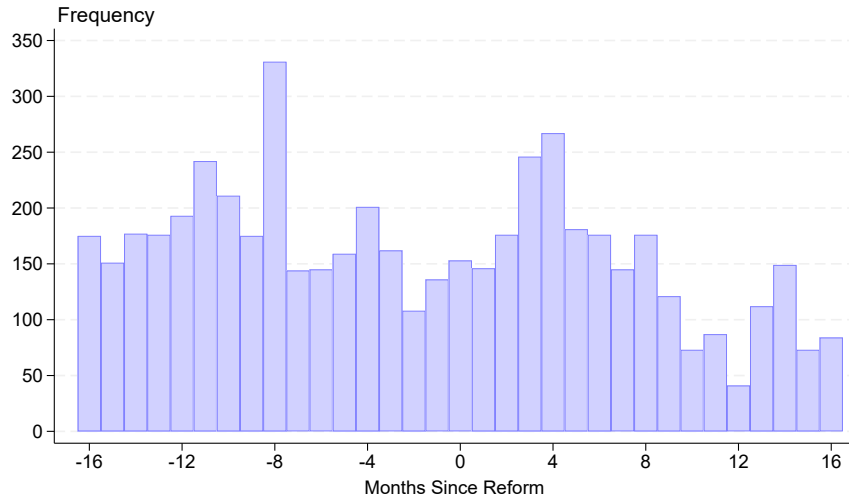
- Hendren, N. and Sprung-Keyser, B. (2020). A unified welfare analysis of government policies. *The Quarterly Journal of Economics*, 135(3):1209–1318.
- Hoynes, H. (2019). The earned income tax credit. *The Annals of the American Academy of Political and Social Science*, 686(1):180–203.
- Hvidtfeldt, 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.
- Levitt, S. D. (1997). Using electoral cycles in police hiring to estimate the effect of police on crime. *The American Economic Review*, 87(3):270.
- 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.
- Sah, R. K. (1991). Social osmosis and patterns of crime. *Journal of political Economy*, 99(6):1272–1295.
- 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



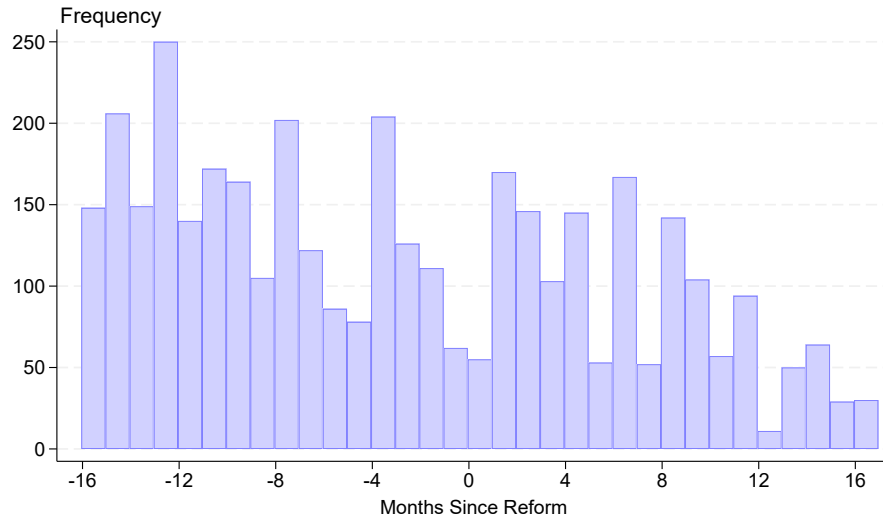
B: Effect of Start Help on Number of Refugees Per Day



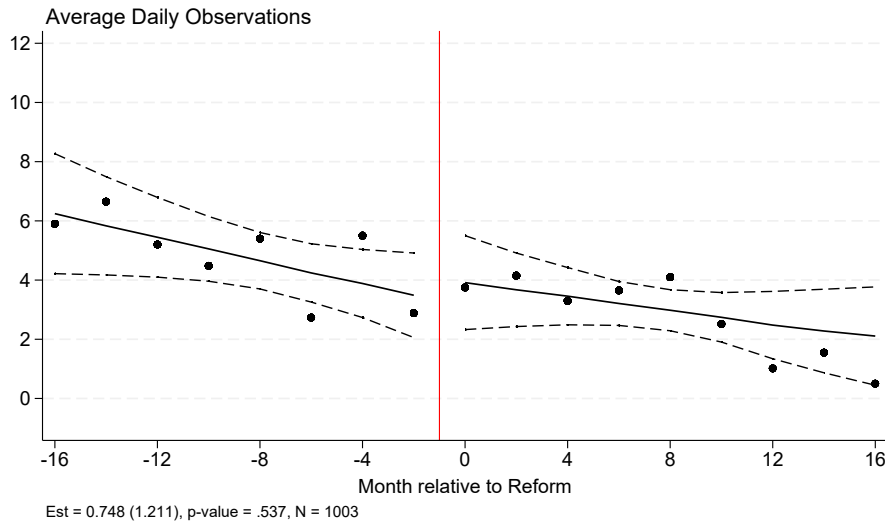
Notes: This figure shows whether there is extra density of refugees around the cutoff date of Start Help reform. Panel A presents a histogram of total number of refugees in two-month bins. Panel B present the effect of Start Help on the average number of refugees who received a residency permit in each 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, including days with zero refugees. 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 1st 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. These black circles would be identical to the histograms in panel A if we averaged them at the monthly instead of bimonthly 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.

Figure 2: Non-Danish Neighbor Density Around the Cutoff

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

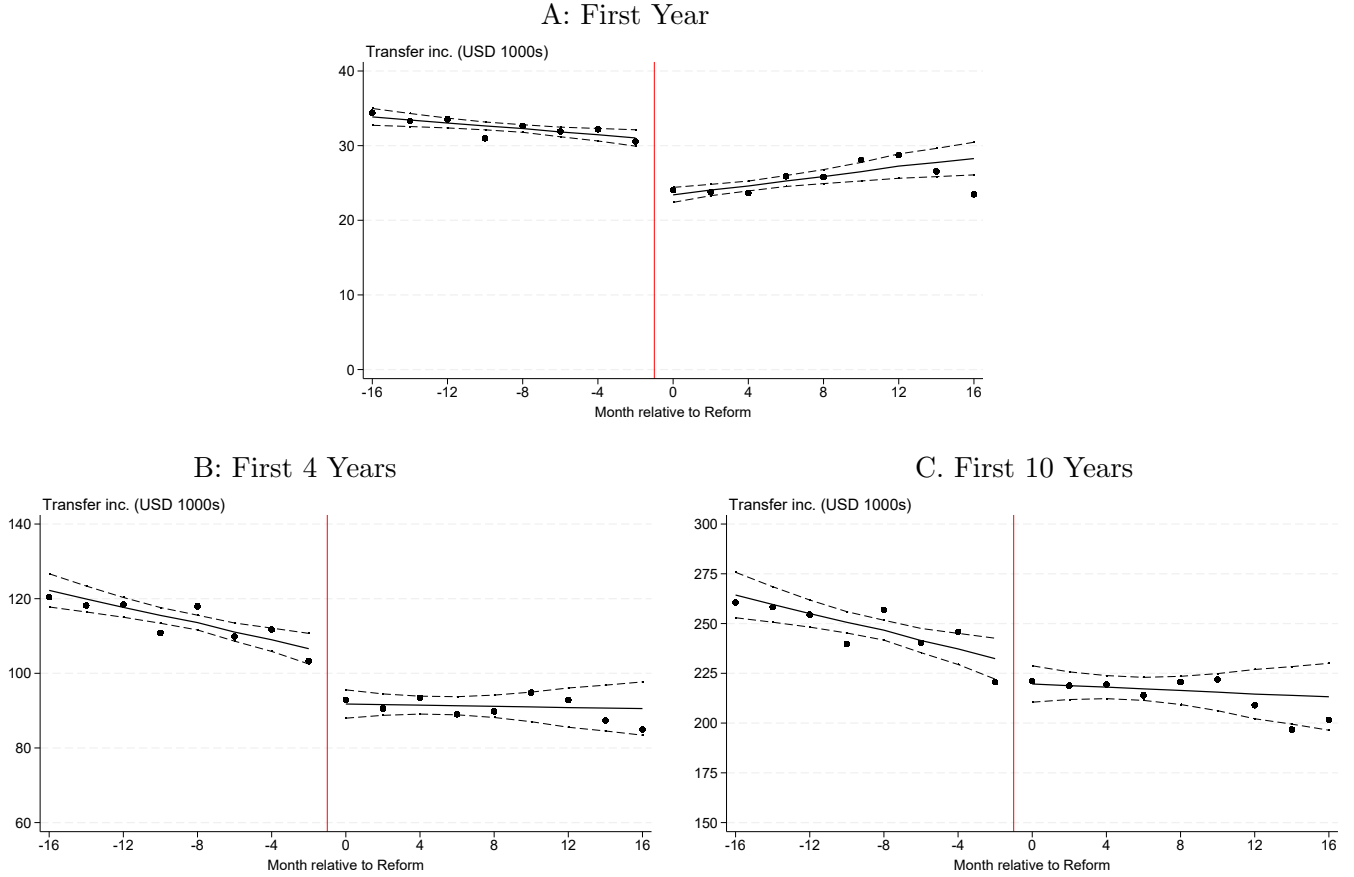


B: Effect of Start Help on Number of Non-Danish Neighbors Per Day



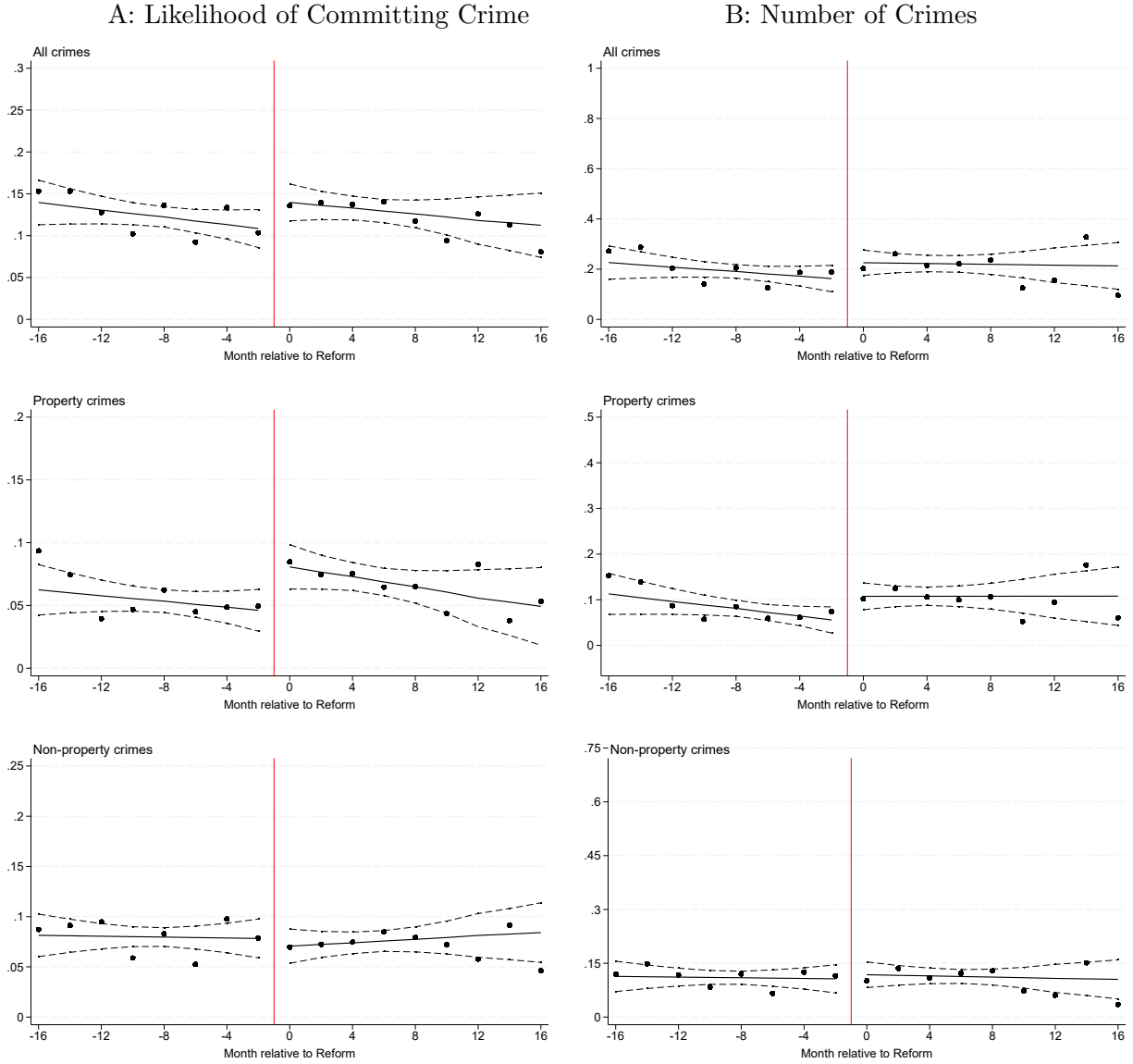
Notes: This figure shows whether there is extra density of refugees' non-Danish neighbors around the Start Help reform. Panel A presents a histogram of total number of neighbors in two-month bins. Panel B present the effect of Start Help on the average number of non-Danish neighbors with a refugee who received a residency permit in each day relative to July 1st 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, including days with zero non-Danish neighbors. 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 1st, 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. These black circles would be identical to the histograms in panel A if we averaged them at the monthly instead of bimonthly 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.

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



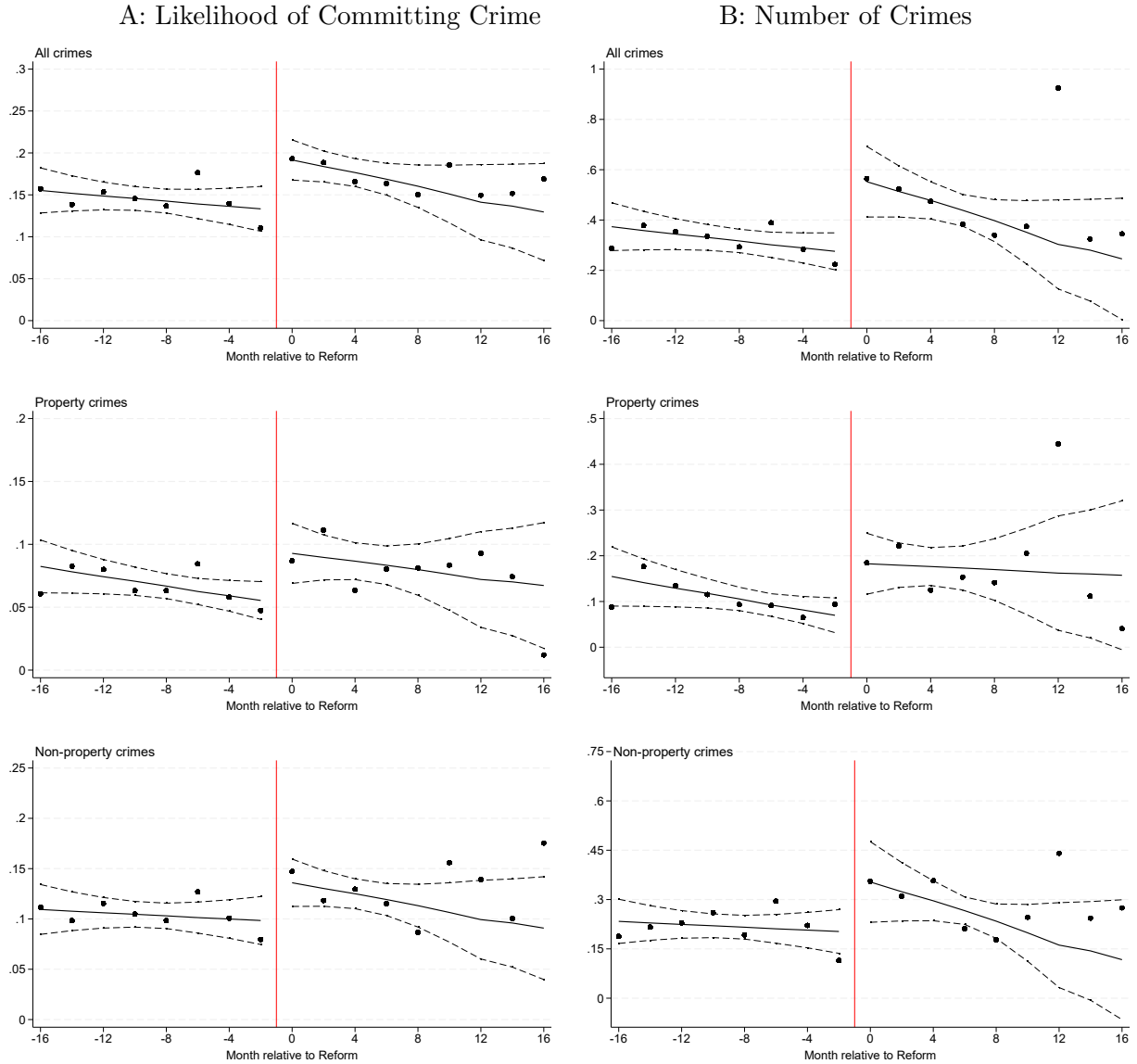
Notes: These figures present the effect of Start Help on refugees' total transfers in the first year, the first four years, and the first ten years since receiving a residency permit, in panels A to C respectively. We create these figures to mimic our estimation strategy. We first create residualized 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 1st 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



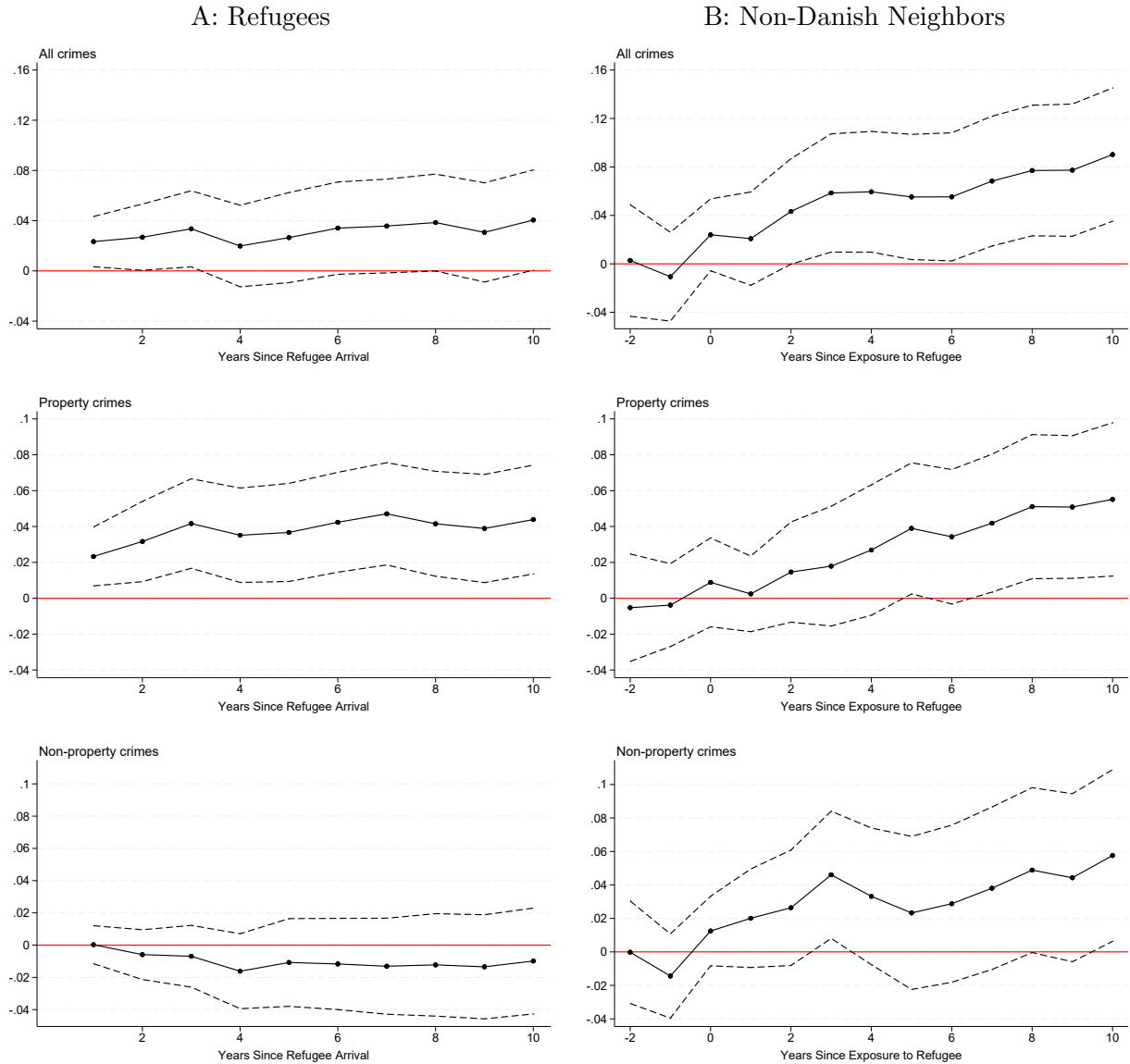
Notes: These figures present the effect of Start Help on refugees' crime convictions in the first ten years since receiving a residency permit. Panel A presents results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). 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 1st 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



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 1st 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



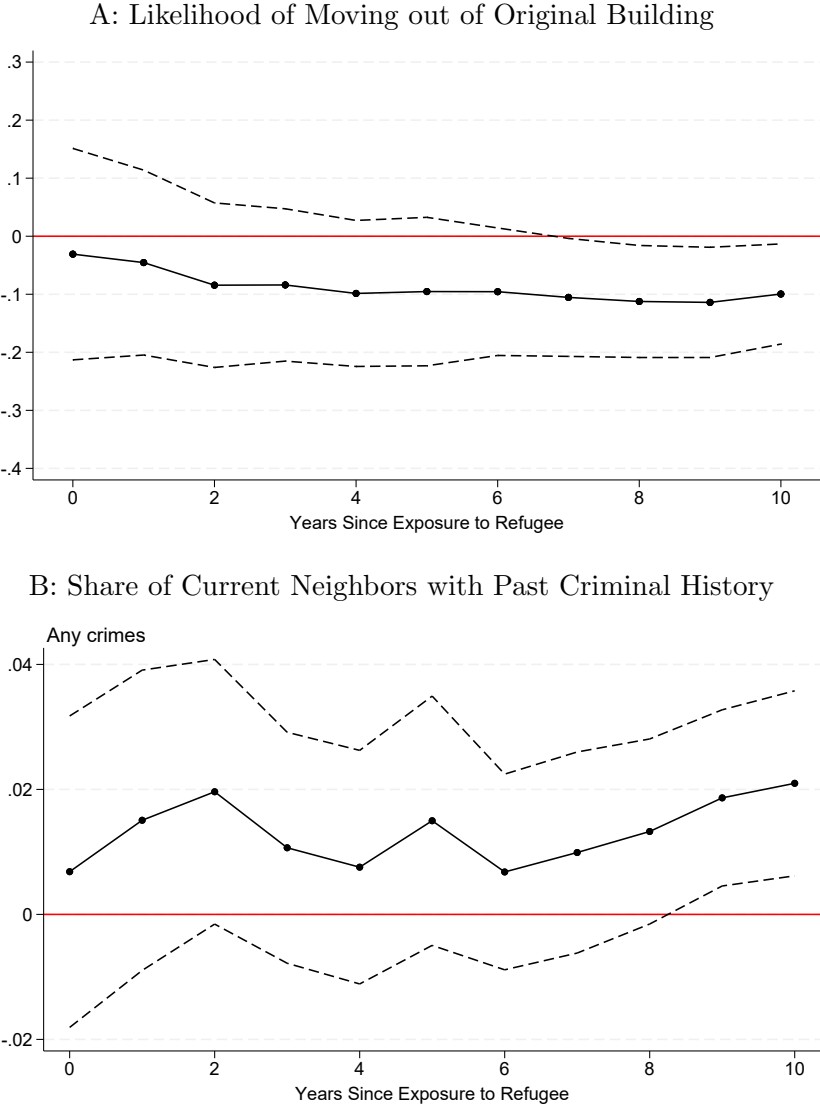
Notes: These figures present the effect of Start Help on refugees' (panel a) and their non-Danish neighbors' (panel b) likelihood of crime convictions over time. For both groups, we present results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and non-property 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.

Figure 8: Effect of Start Help on Non-Danish Neighbors' Housing Choices



Notes: These figures present the effect of Start Help on non-Danish neighbors' housing choices over time. Panel A presents the effect of Start Help on the likelihood that non-Danish neighbors have moved out of the original building to which the refugees were originally assigned, measured each year after the refugee's arrival. Panel B presents the effect of Start Help on the share of residents with prior crime convictions (between 1998 and 2000) in the buildings where non-Danish neighbors reside in each year after the refugee's arrival. To create these figures, we first estimate equation (2), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 4, and clustering the errors at the building level. The black circles show the estimated effect of Start Help in each year after refugee arrival, along with its 95% confidence interval. 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.

Tables

Table 1: Descriptive Statistics

	Refugees	Danish Neighbors	Non-Danish Neighbors
<i>Panel A: Demographic Characteristics</i>			
Age at policy	32.908 (8.326)	32.238 (10.428)	32.666 (9.629)
Female	0.452 (0.498)	0.420 (0.494)	0.431 (0.495)
Married	0.749 (0.434)	0.124 (0.330)	0.558 (0.497)
Children at Home	0.967 (1.330)	0.335 (0.738)	1.063 (1.313)
Capital Region	0.199 (0.399)	0.331 (0.470)	0.411 (0.492)
<i>Panel B: Crime Convictions</i>			
Any Crime	0.132 (0.338)	0.147 (0.354)	0.170 (0.376)
Any Property Crime	0.070 (0.254)	0.080 (0.272)	0.084 (0.277)
Any Non-Property Crime	0.075 (0.264)	0.110 (0.313)	0.120 (0.325)
Number of All Crimes	0.218 (0.751)	0.447 (1.748)	0.453 (1.648)
Number of Property Crimes	0.101 (0.482)	0.206 (1.061)	0.164 (0.847)
Number of Non-Property Crimes	0.116 (0.514)	0.241 (0.984)	0.289 (1.156)
<i>Panel C: Welfare Benefits and Labor Market Outcomes</i>			
Transfer inc. (USD 1000s)	218.033 (145.132)	132.743 (142.205)	175.117 (153.650)
Labor inc. (USD 1000s)	118.815 (168.873)	314.957 (267.444)	169.259 (210.243)
Years with Labor Income > 0	3.753 (3.490)	6.914 (3.814)	4.665 (3.962)
Observations	5292	9890	3797

Notes: This table presents averages and standard deviations 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

<i>Panel A: Own Demographics</i>						
	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
<i>Panel B: Refugee and Building Characteristics</i>						
	Refugee Age	Refugee Female	Refugee Married	Number of Kids	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
<i>Panel C: Own Income Pre-Exposure</i>						
	Transfers	Earnings	Earnings>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		
<i>Panel D: Own Crime Pre-Exposure</i>						
	All		Property		Non-Property	
	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
<i>Panel E: Predicted Crime</i>						
	All		Property		Non-Property	
	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	0.170	0.453	0.084	0.164	0.120	0.289
Number of Neighbors	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. OLF in Panel C stands for out of labor force. 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
<i>Panel A: All Refugees</i>						
Start Aid	0.041** (0.020)	0.079* (0.047)	0.044*** (0.015)	0.064** (0.026)	-0.010 (0.017)	0.015 (0.035)
Mean Y	0.132	0.218	0.070	0.101	0.075	0.116
Mean Y Pre Start Help	0.126	0.204	0.062	0.092	0.077	0.112
Number of Refugees	5292	5292	5292	5292	5292	5292
<i>Panel B: Buildings with 1 Refugee</i>						
Start Aid	0.028 (0.031)	-0.014 (0.066)	0.047* (0.024)	0.038 (0.038)	-0.035 (0.023)	-0.052 (0.050)
Mean Y	0.132	0.203	0.071	0.098	0.073	0.106
Mean Y Pre Start Help	0.122	0.196	0.059	0.087	0.074	0.108
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
<i>Panel A: All Neighbors</i>						
Start Aid	0.016 (0.018)	0.078 (0.075)	0.009 (0.013)	0.034 (0.041)	0.006 (0.015)	0.021 (0.054)
Mean Y	0.154	0.448	0.081	0.194	0.113	0.254
Mean Y Pre Start Help	0.144	0.417	0.078	0.183	0.104	0.234
Number of Neighbors	13687	13687	13687	13687	13687	13687
<i>Panel B: Non-Danish Neighbors</i>						
Start Aid	0.089*** (0.026)	0.424*** (0.121)	0.057*** (0.021)	0.168*** (0.053)	0.058** (0.025)	0.235** (0.107)
Mean Y	0.170	0.453	0.084	0.164	0.120	0.289
Mean Y Pre Start Help	0.157	0.384	0.075	0.134	0.112	0.249
Number of Neighbors	3797	3797	3797	3797	3797	3797
<i>Panel C: Danish Neighbors</i>						
Start Aid	-0.013 (0.021)	-0.052 (0.089)	-0.009 (0.015)	-0.004 (0.053)	-0.015 (0.018)	-0.075 (0.058)
Mean Y	0.147	0.447	0.080	0.206	0.110	0.241
Mean Y Pre Start Help	0.140	0.429	0.079	0.201	0.101	0.228
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

	<i>Amount</i>	<i>Notes</i>
<i>Panel A: MVPF components</i>		
1. Change in welfare transfers net of taxes	41307615.801	See Appendix Table A.8
2. Changes in tax revenues from labor income	1003350.224	See Appendix Table A.8
3. Total savings to taxpayers	42310966.025	1+2
4. Enforcement and prosecution costs from refugees	54624.594	See Appendix Table A.9
5. Enforcement and prosecution costs from neighbors	1920305.856	See Appendix Table A.9
6. Costs to victims from convicted crime of refugees	235057.733	See Appendix Table A.9
7. Costs to victims from convicted crime of non-Danish neighbors	6496613.387	See Appendix Table A.9
8. Costs to victims from reported crime of refugees	4254708.130	See Appendix Table A.9
9. Costs to victims from reported crime of non-Danish neighbors	26674123.395	See Appendix Table A.9
<i>Panel B: MVPF including costs to victims of convicted crime</i>		
Base MVPF (ignore effects on crime)	0.976	1 over 3
MVPF adding refugees' crime	0.983	(1 + 6) over (3 - 4)
MVPF adding also neighbors' crime	1.191	(1 + 6 + 7) over (3 - 4 - 5)
<i>Panel C: MVPF including costs to victims of reported crime</i>		
Base MVPF (ignore effects on crime)	0.976	1 over 3
MVPF adding refugees' crime	1.078	(1 + 8) over (3 - 4)
MVPF adding also neighbors' crime	1.791	(1 + 8 + 9) 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.8](#) and [A.9](#) provide detailed calculations behind each component reported in panel A.

Table 6: Weighted Cost of Crime Estimates

	Cost amount	Frequency of crime	Costs weighted by frequency	Conviction share of reported crimes
<i>Panel A.1: Costs to Taxpayers from Refugees' property crime</i>				
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	-
<i>Panel A.2: Costs to Taxpayers from Neighbors' property crime</i>				
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	-
<i>Panel A.3: Costs to Taxpayers from Neighbors non-property crime</i>				
Economic Crime	699.130	0.05	33.179	-
Drugs related crime	1,864.257	0.16	301.757	-
Sexual Offenses	2,777.923	0.004	11.771	-
Violence	4,009.891	0.07	265.061	-
Other criminal offenses	2,596.807	0.04	103.432	-
Road traffic legislation	987.514	0.58	575.771	-
Violations of other regulation	1,325.760	0.10	129.205	-
Total weighted cost			1420.176	-
<i>Panel B.1: Costs to Victims from Refugees' property crime</i>				
Burglary	2,626.198	0.00	0	0.022
Theft	694.024	1.00	694.024	0.055
Robbery	15,008.868	0.00	0	0.225
Total weighted cost of convicted crime			694.024	
Weighted average crime conviction share				0.055
Total weighted cost of reported crime				12562.323
<i>Panel B.2: Costs to Victims from Neighbors' property crime</i>				
Burglary	2,626.198	0.10	272.427	0.022
Theft	694.024	0.90	622.030	0.055
Robbery	15,008.868	0.00	0	0.225
Total weighted cost of convicted crime			894.457	
Weighted average crime conviction share				0.052
Total weighted cost of reported crime				17275.415
<i>Panel B.3: Costs to Victims from Neighbors non-property crime</i>				
Fraud/forgery	0.000	0.17	0.000	0.020
Drugs related crimes	0.000	0.58	0.000	0.285
Sexual assault	163,222.025	0.02	2473.061	0.002
Assault	17,635.065	0.24	4168.288	0.071
Total weighted cost of convicted crime			6641.349	
Weighted average crime conviction share				0.379
Total weighted cost of reported crime				17543.755

Notes: This table details the calculations behind the weighted cost per conviction in Table A.9. Panel A shows taxpayer costs from prosecution data provided by the Danish State Prosecutor (DSP), adjusted from 2012 DKK to 2021 USD using CPI (1.073) and exchange rates (6.289). Panel B presents victim costs from [Deshpande and Mueller-Smith \(2022\)](#) (Table B.21), adjusted for US CPI (1.181). Costs are aggregated into property and non-property crime categories, weighted by crime frequencies among refugees or neighbors in the treatment group (granted residency post-July 1, 2002). The 'Total weighted cost of reported crime' is derived by dividing the 'Total weighted cost of convicted crime' by the conviction share of reported crimes, weighted by crime type frequencies. In accordance with Statistics Denmark's confidentiality rules, frequency counts based on fewer than 5 observations are not reported. For refugees, only property crime costs are included due to insignificant effects on non-property crime; for neighbors, both crime categories are considered.

Table 7: 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
<i>Panel A: All crimes</i>							
Start Aid	0.089*** (0.026)	0.075** (0.030)	0.089*** (0.026)	0.136*** (0.038)	0.071*** (0.026)	0.203*** (0.062)	0.206*** (0.037)
Mean Y	0.170	0.170	0.170	0.170	0.168	0.183	0.181
Mean Y Pre Start Help	0.157	0.157	0.157	0.157	0.157	0.153	0.151
Number of Neighbors	3797	3797	3797	3797	3797	897	897
<i>Panel B: Property crimes</i>							
Start Aid	0.057*** (0.021)	0.043** (0.020)	0.058*** (0.020)	0.085*** (0.030)	0.048** (0.020)	0.090 (0.058)	0.083*** (0.029)
Mean Y	0.084	0.084	0.084	0.084	0.085	0.088	0.092
Mean Y Pre Start Help	0.075	0.075	0.075	0.075	0.075	0.066	0.072
Number of Neighbors	3797	3797	3797	3797	3797	858	858
<i>Panel C: Non-property crimes</i>							
Start Aid	0.058** (0.025)	0.052* (0.030)	0.058** (0.025)	0.099*** (0.038)	0.040* (0.024)	0.137** (0.060)	0.161*** (0.037)
Mean Y	0.120	0.120	0.120	0.120	0.117	0.136	0.136
Mean Y Pre Start Help	0.112	0.112	0.112	0.112	0.112	0.114	0.1114
Number of Neighbors	3797	3797	3797	3797	3797	962	962

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

A Additional Results and Discussion

A.1 Social Multiplier Estimates in a Crime Model of Social Interactions

In order to account for the strength of social ties in estimating the social multiplier of crime, we follow the approach of [Dustmann and Landersø \(2021\)](#) and estimate the multiplier implied by a model of crime with social interactions. This framework builds on earlier models of peer effects in crime (e.g., [Ballester et al., 2006](#); [Calvó-Armengol et al., 2005](#)) and of social multipliers (e.g., [Glaeser et al., 2003](#)), and it is estimated in the same Danish institutional context, making their multiplier more comparable to ours.

The model characterizes the crime best-response function of each individual to the criminal behavior of others in the network of fathers (refugees in our setting) and their neighbors. They then derive the Nash equilibrium of this system of best-response functions. They show that, in equilibrium, the functions describing criminal activity of fathers (refugees in our setting) and their neighbors can be expressed as a function of an exogenous crime shifter (see their equations (5a) and (5b)). They show that this shifter can be used to identify the structural parameters of the model, including the strength of social ties. In their context, the exogenous shifter is the child’s gender, which affects both the father’s and the neighbors’ propensity to commit crime. In our setting, the exogenous shifter is whether the refugee is eligible for StartHelp and thus faces lower welfare benefits.

Important for us, [Dustmann and Landersø \(2021\)](#) also show that the social multiplier can be expressed as a function of the parameter capturing the strength of social ties, γ (see their equation (6)). Following their approach, we estimate γ in our setting using nonlinear least squares, obtaining a noisily estimated 0.587 (see Appendix Table [A.21](#)). Consistent with our baseline results, and with the notion that equilibrium conditions may take time to materialize, we obtain this estimate using cumulative crime numbers over the 10 years after the reform, focusing on total (property and non-property) crime. To account for time trends in crime, and to align the specification more closely with our baseline RDD, we also include a linear time trend for refugees.⁴⁶

⁴⁶We also experimented with including a time trend for neighbors, but the estimator failed to converge when both time trends were included. We therefore include only the refugee time trend, as refugee numbers and crime may vary over time in response to international crises. By contrast, we have less reason to expect

Substituting the point estimate γ into equation (6) of [Dustmann and Landersø \(2021\)](#) yields a social multiplier of 2.420.

A.2 Changes in Policing: Additional Results

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 crime 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.17](#), 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 percentage 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.⁴⁷ [Appendix Table A.18](#)

that neighbor crime exhibits sizable trends over our sample period.

⁴⁷For each municipality, we first calculate the ratio of charges to convictions among Danes and immigrants

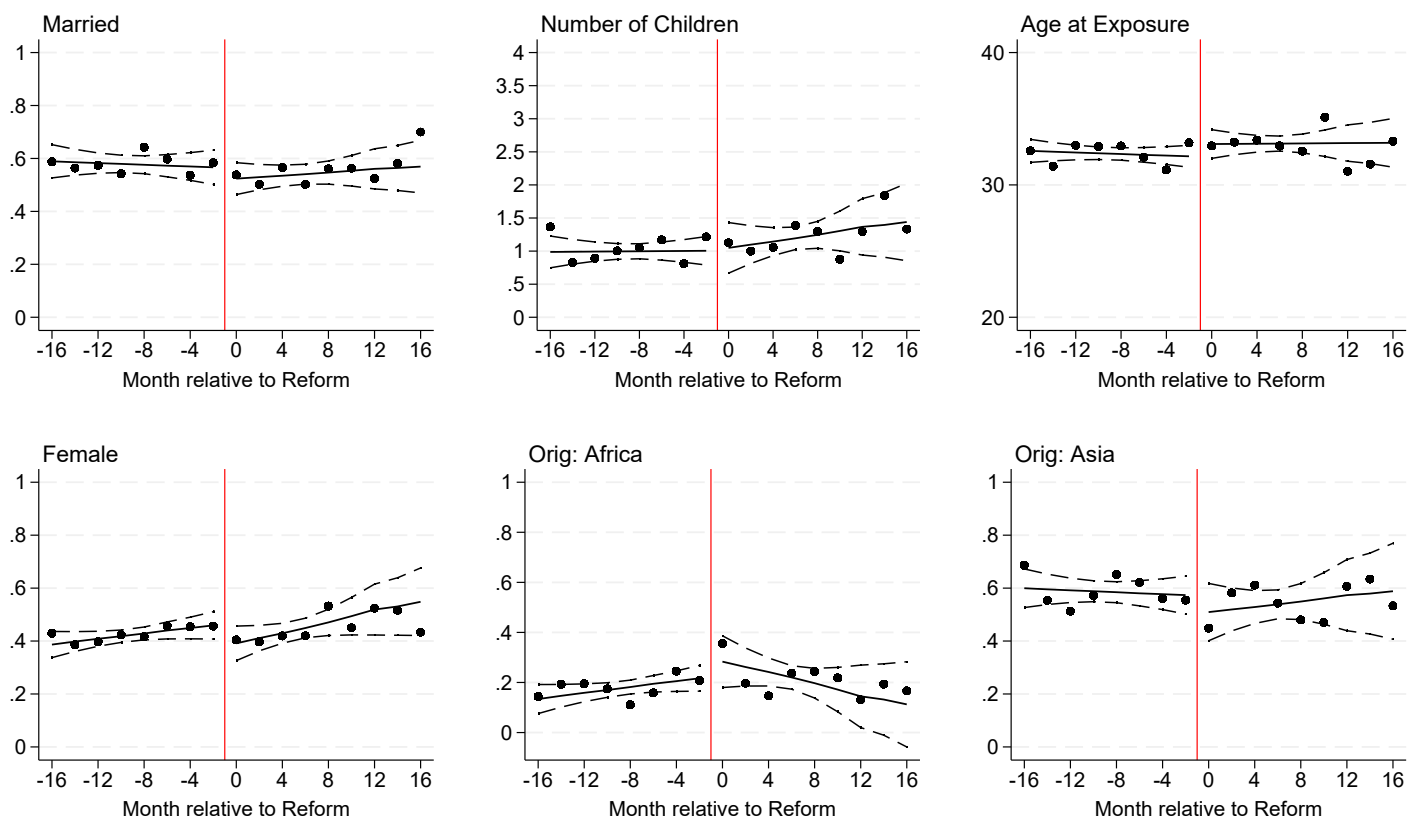
contains the results of this analysis and shows evidence that this is unlikely to be the case. Treatment effects at the intensive margin of crime are generally similar across municipalities with high and low anti-immigrant party vote shares, as well as across municipalities with low and high racist-police indices. At the extensive margin of crime, effects are also similar when splitting municipalities by anti-immigrant party vote share (Panel A, columns 1 and 2). By contrast, extensive-margin effects appear larger in municipalities with a high relative to a low racist-police index, although the latter estimates are imprecise.

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

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.

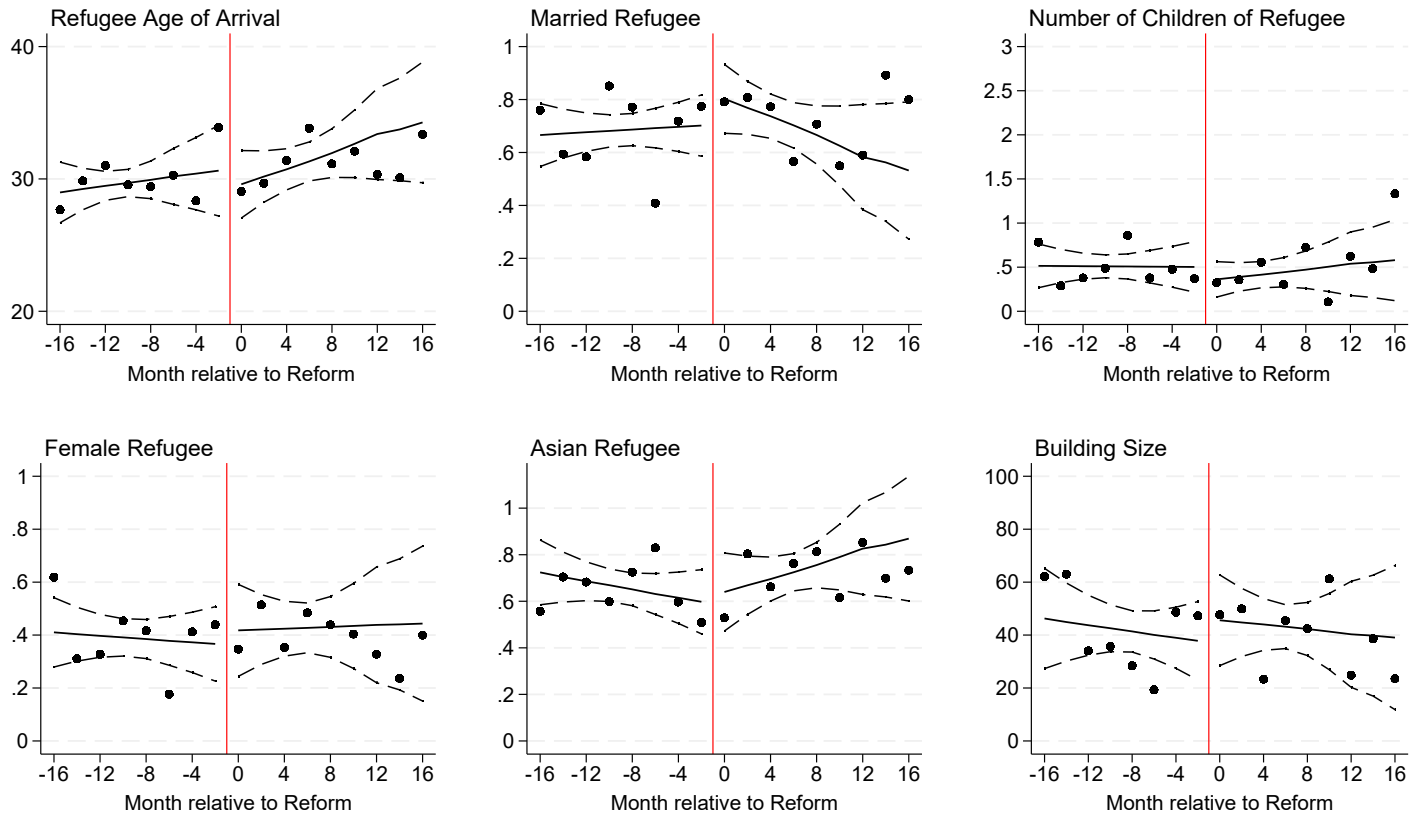
Appendix Figures

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



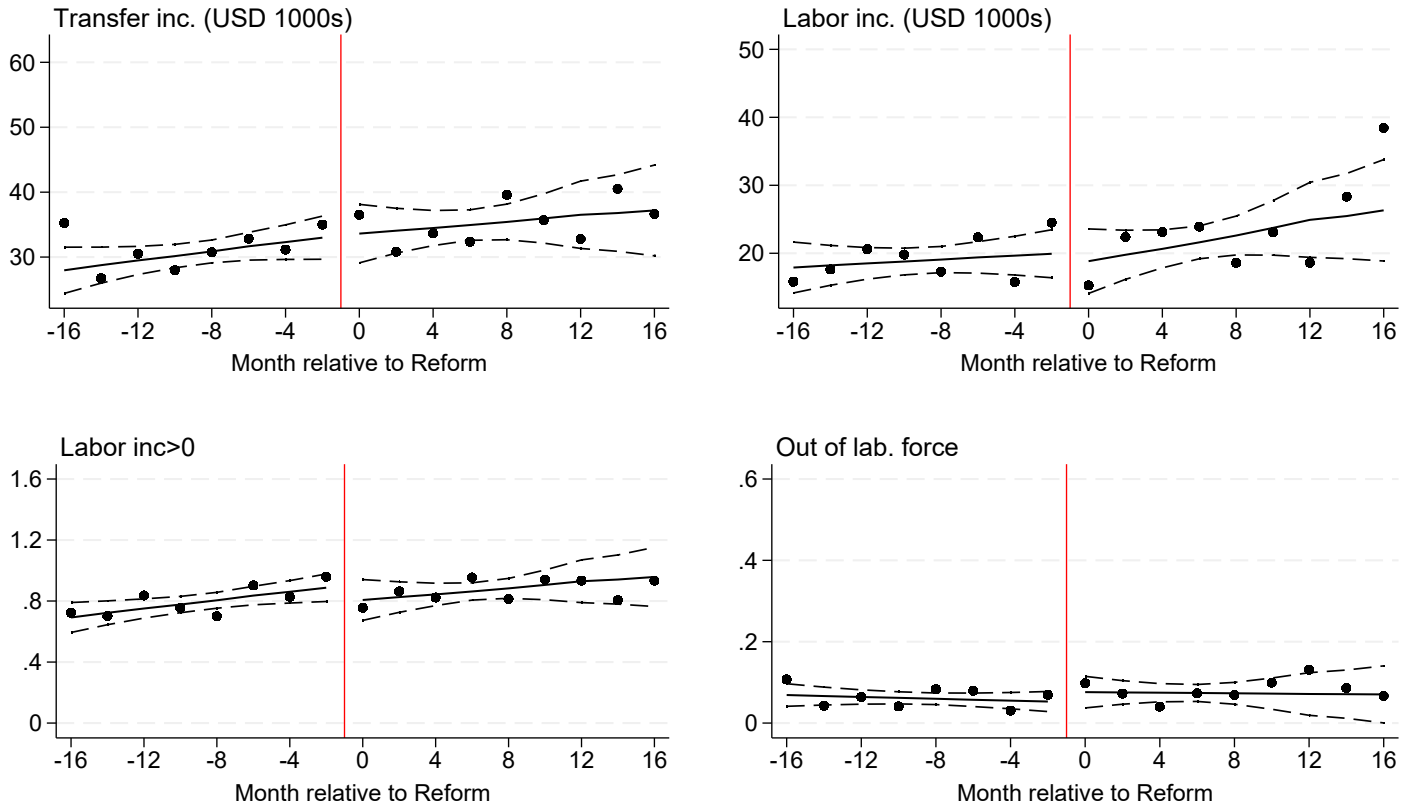
Notes: These figures show balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' characteristics, all measured at the year of arrival of the refugee. We test whether Start Help affected the likelihood of being married (top left), average number of children (top), average 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



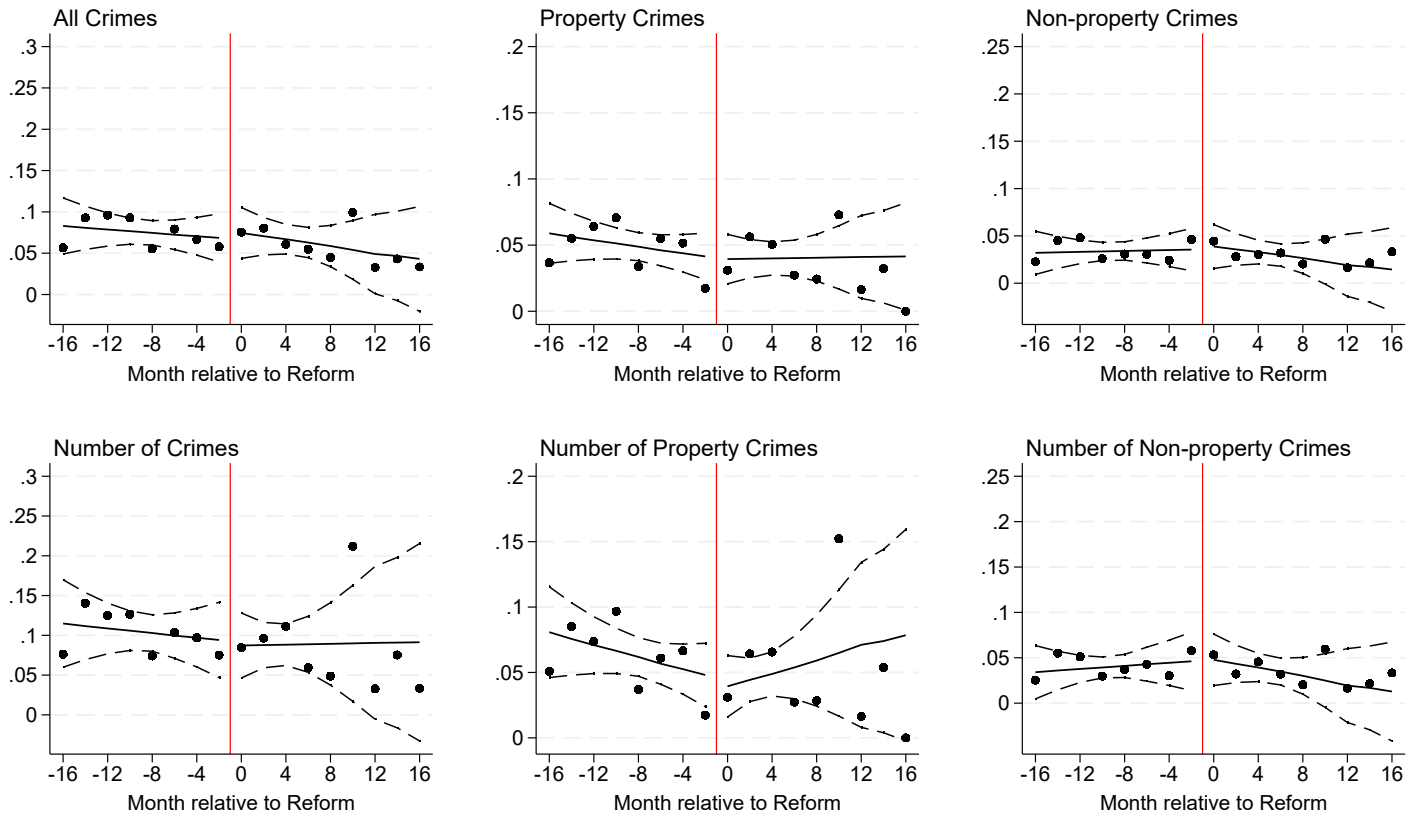
Notes: These figures show balance tests for the characteristics of refugees and buildings, all measured at the year of arrival of the refugee. We test whether Start Help affected refugees' age (top left), marital status (top), number of children (top right), likelihood of being a woman (bottom left), 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



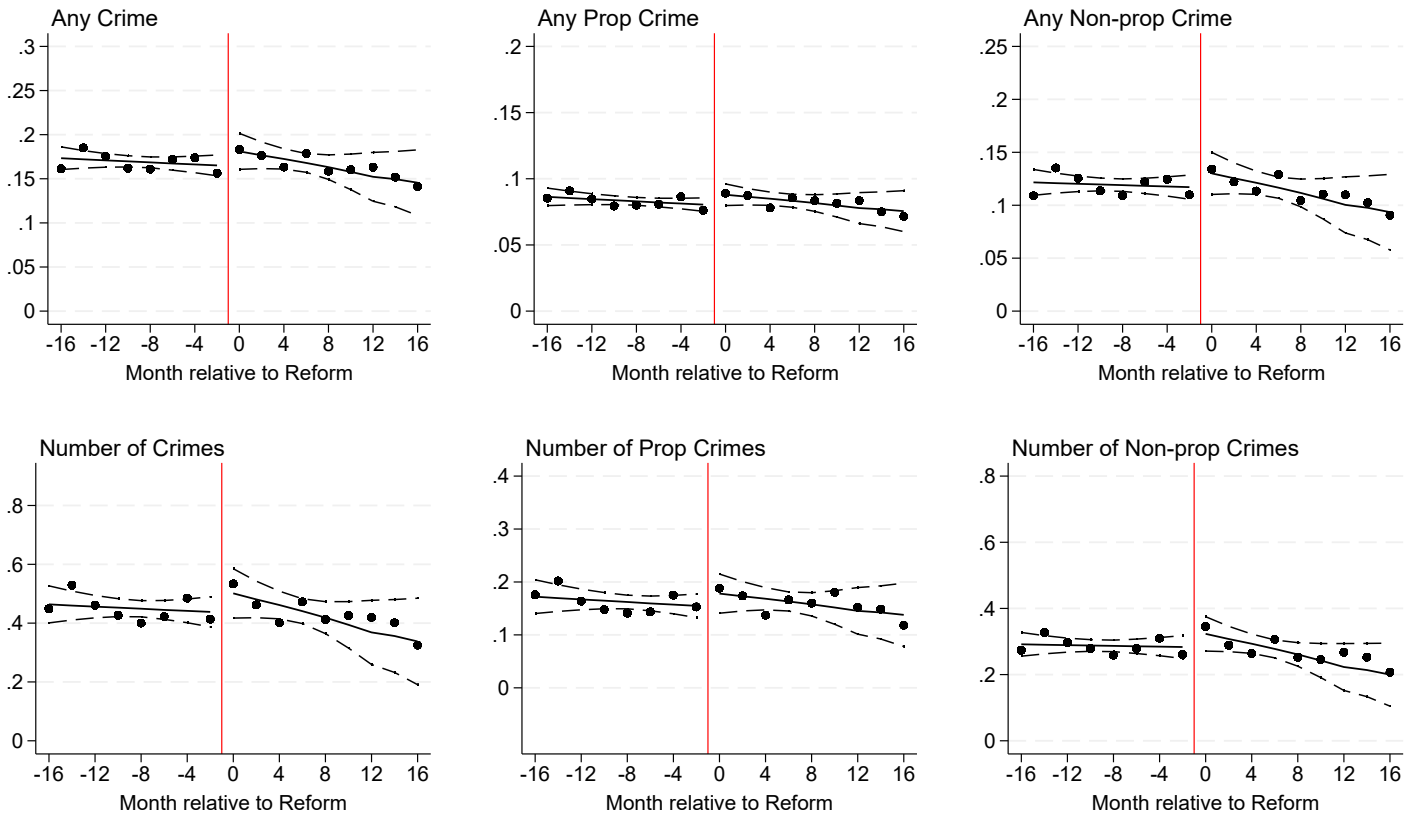
Notes: These figures show balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' labor market outcomes in the two years prior to refugee arrival. We test government transfer income in 1000s of US dollars (top left), labor income in 1000s of US dollars (top right), a 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.

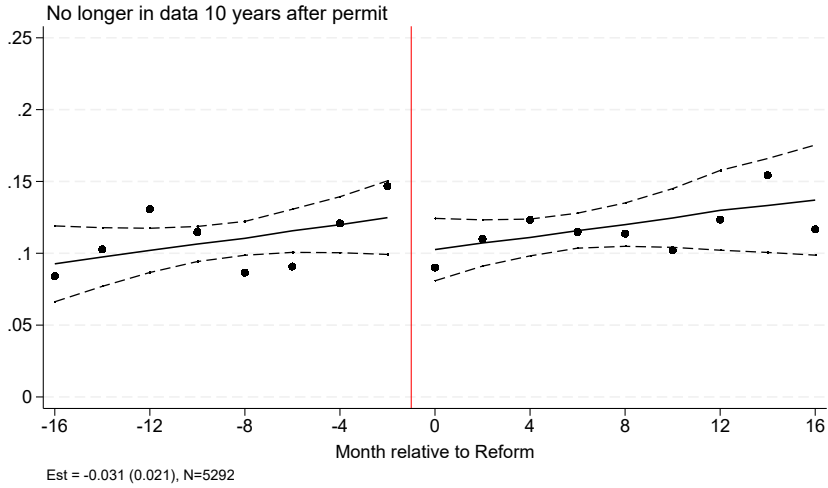
Figure A.5: Balance Test for non-Danish Neighbors' Predicted Crime



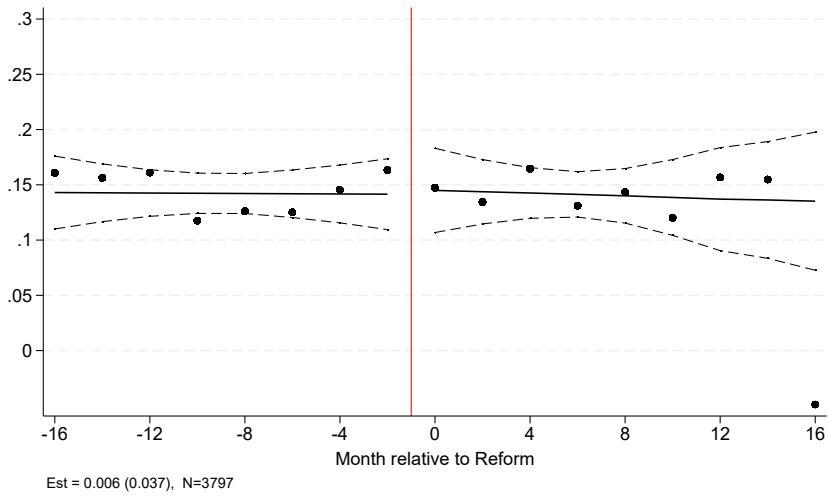
Notes: These figures show balance tests for Non-Danish neighbors by showing the effect of Start Help on neighbors' predicted crime. We use all refugees' and neighbors' demographic characteristics as well as neighbors labor market outcomes in the two years prior to refugee arrival to predict crime convictions. 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

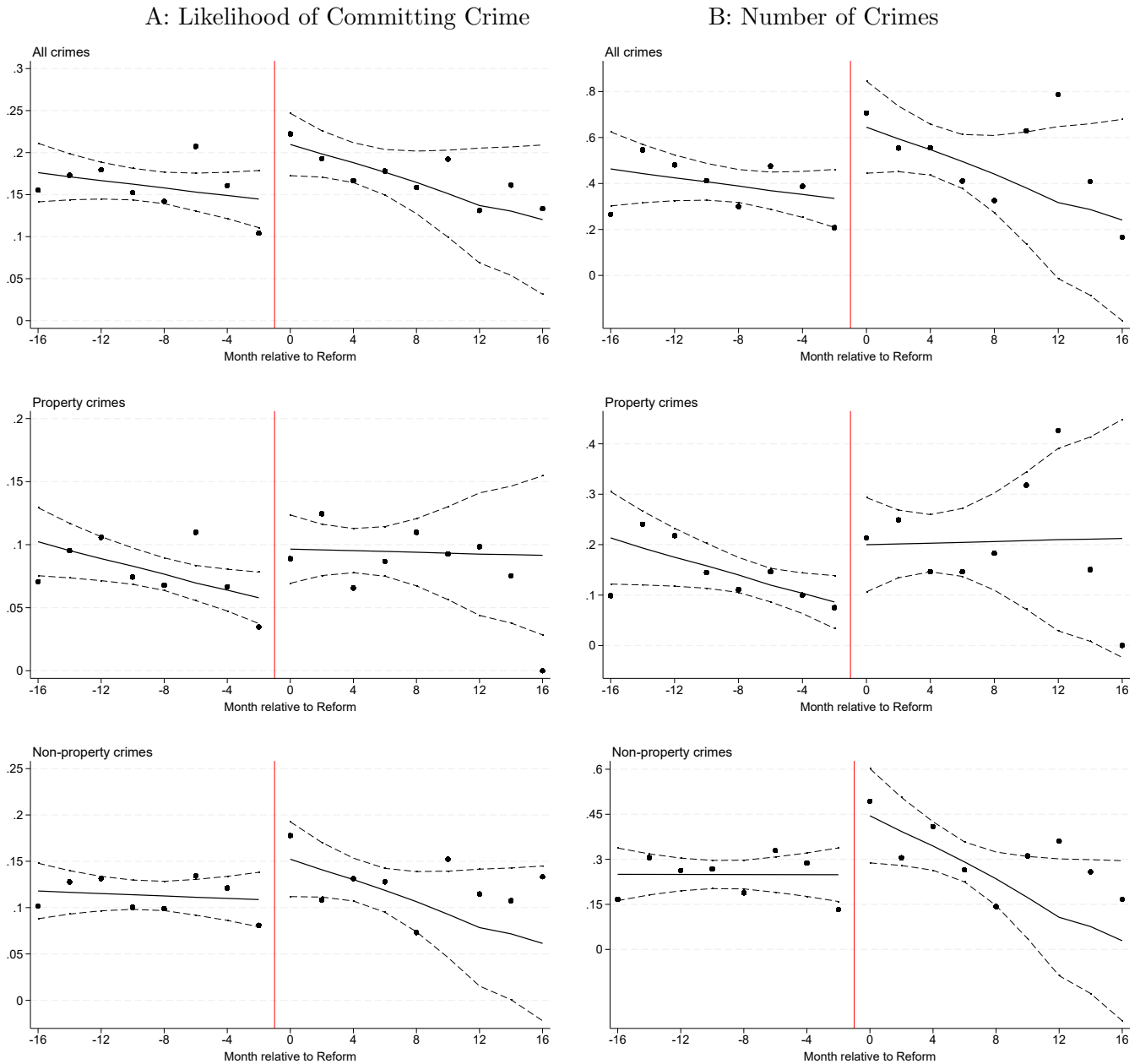


B: Non-Danish Neighbors



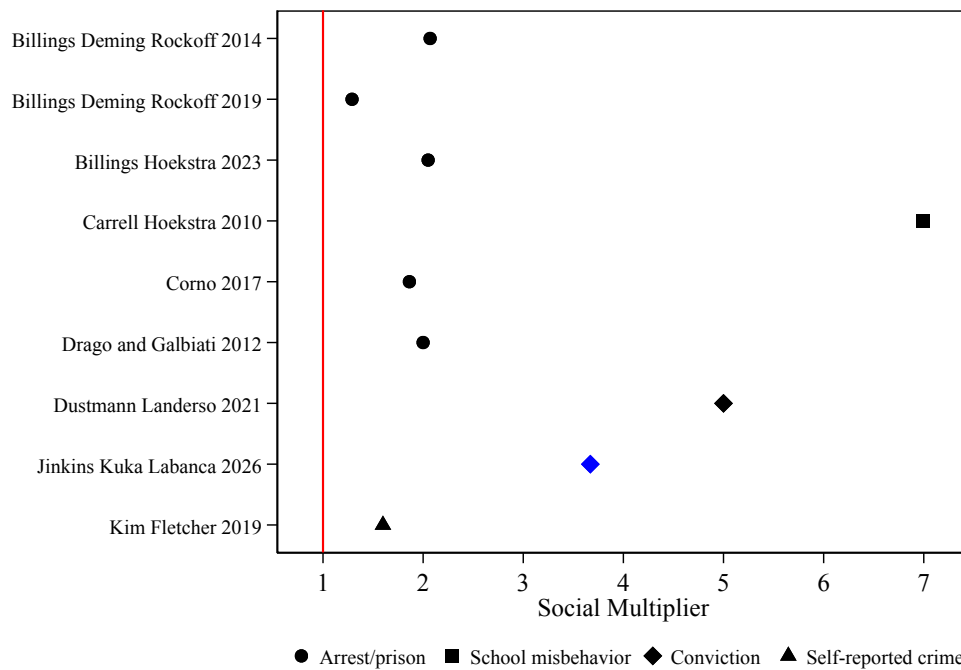
Notes: These figures present the effect of Start Help on refugees' (panel a) and their non-Danish neighbors' (panel b) attrition rate in the first ten years since the refugee's arrival in the building. Attrition is defined as an indicator for absence in the administrative registers in any of the 10 years 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 1st 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



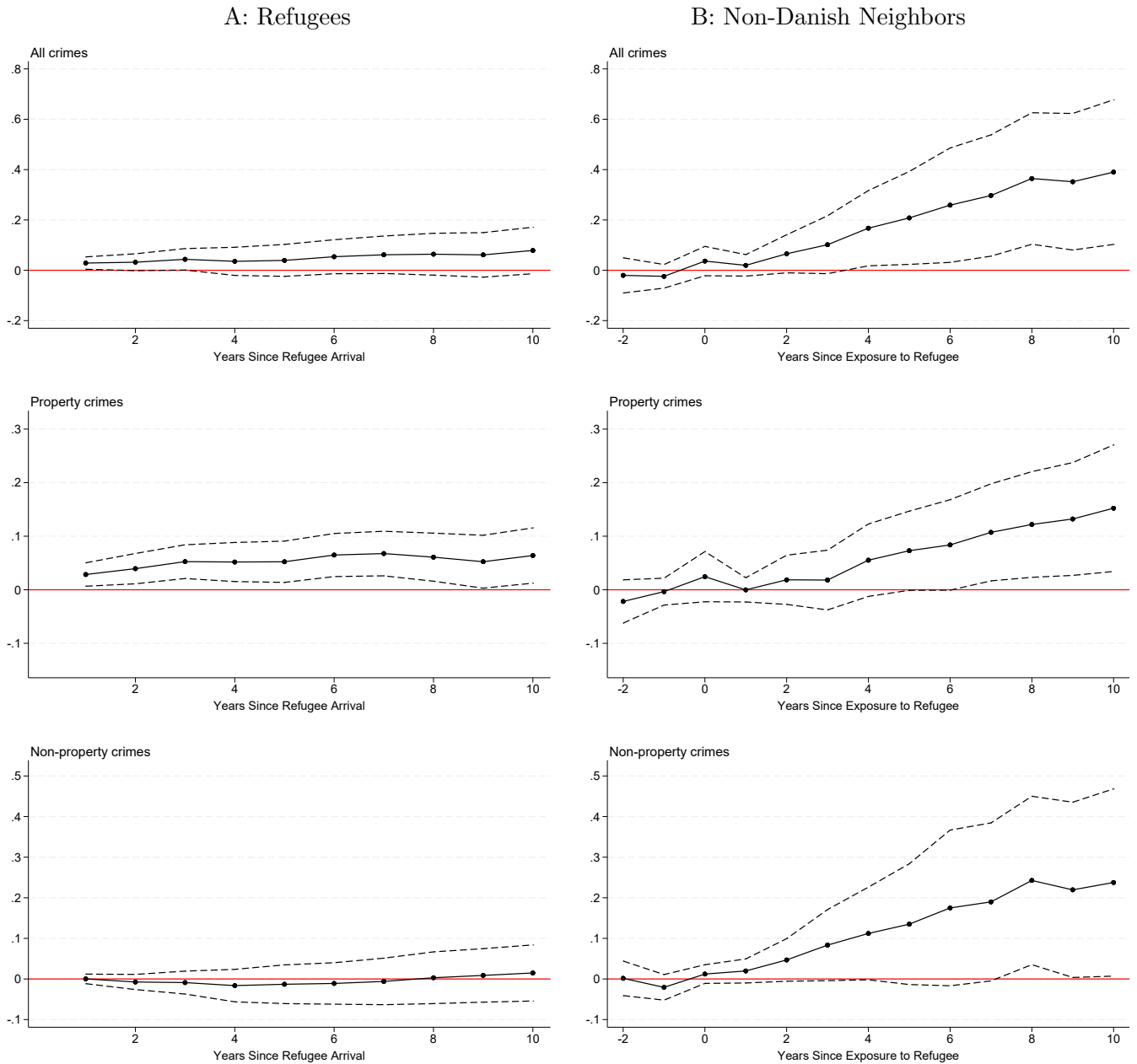
Notes: These figures present the effect of Start Help on refugees' non-Danish neighbors' crime convictions in the first ten years since the refugee's arrival in the building. Panel A presents results for the likelihood of being convicted of any (non-traffic) crimes (top), property crimes (middle) and 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 1st 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: 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.

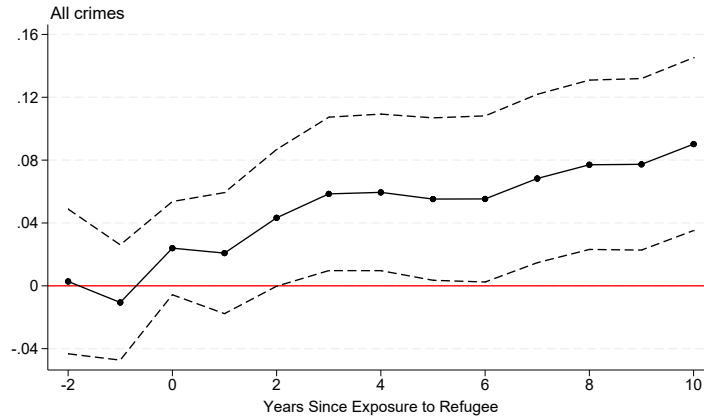
Figure A.9: Effect of Start Help on Refugees' and Neighbors' Number of Crimes – Treatment Effect Over Time



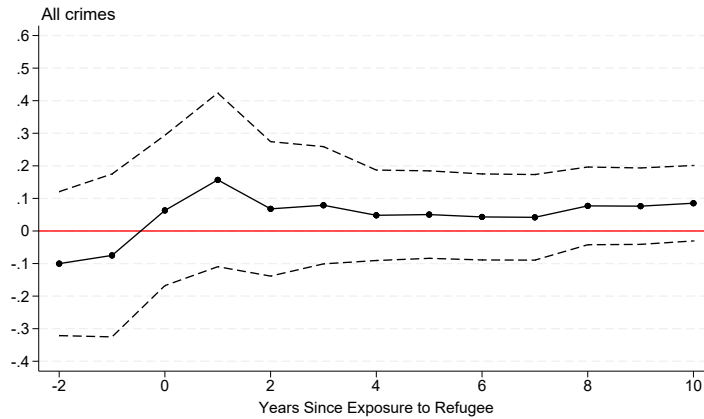
Notes: These figures present the effect of Start Help on refugees' (panel a) and their non-Danish neighbors' (panel b) number of crime convictions over time. For both groups, we presents results for the number of any (non-traffic) crimes (top), property crimes (middle) and non-property crimes (bottom). 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.10: Effect of Start Help on Likelihood of Crime Among Non-Danish Neighbors – Individual vs. Building Level

A: Own Likelihood of Crime



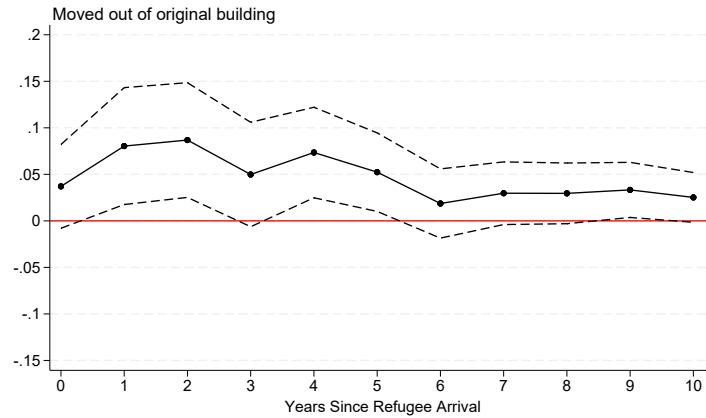
B: Likelihood of Any Crime in the Original Building



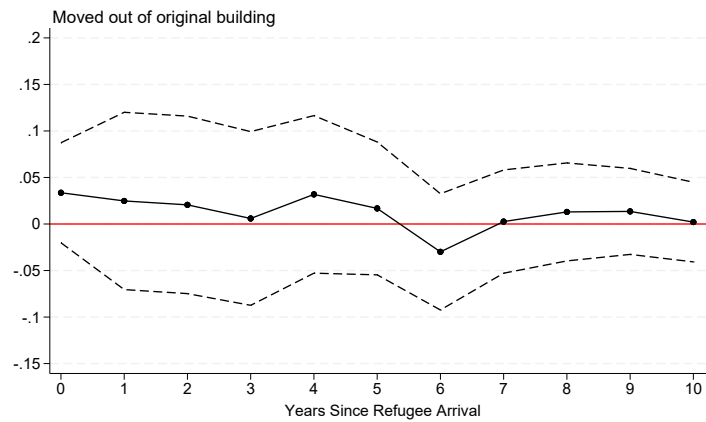
Notes: These figures present the effect of Start Help on the likelihood of a crime conviction measured at the individual and building levels over time. Panel A presents the effect of Start Help on the likelihood that a non-Danish neighbor is convicted of any crime in each year since exposure to the refugee, replicating the individual-level results from Figure 6. Panel B presents the effect of Start Help on the likelihood that the original building the non-Danish neighbors lived in has at least one crime conviction among non-Danish neighbors in each year since exposure to the refugee. To create these figures, we estimate equation (2), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 4, and clustering the errors at the building level. In Panel B, the dependent variable varies at the building level, but equation (2) is estimated using individual-level observations for consistency with Panel A. The black circles show the estimated effect of Start Help in each year after refugee arrival, along with its 95% confidence interval. 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.11: Effect of Start Help on Refugees' Likelihood of Moving out of Original Building

A: All

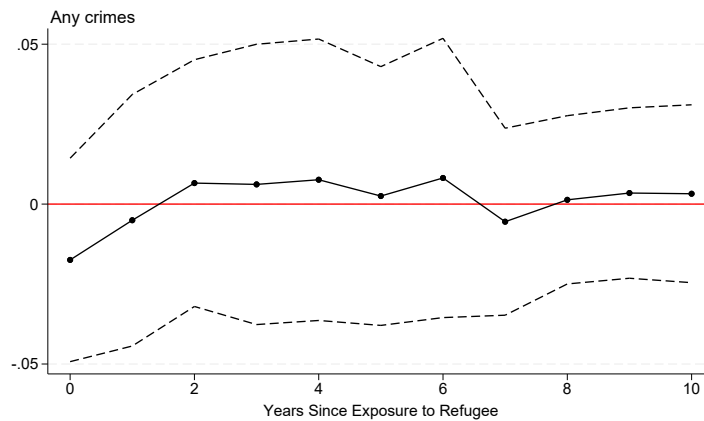


B: Buildings with One Refugee



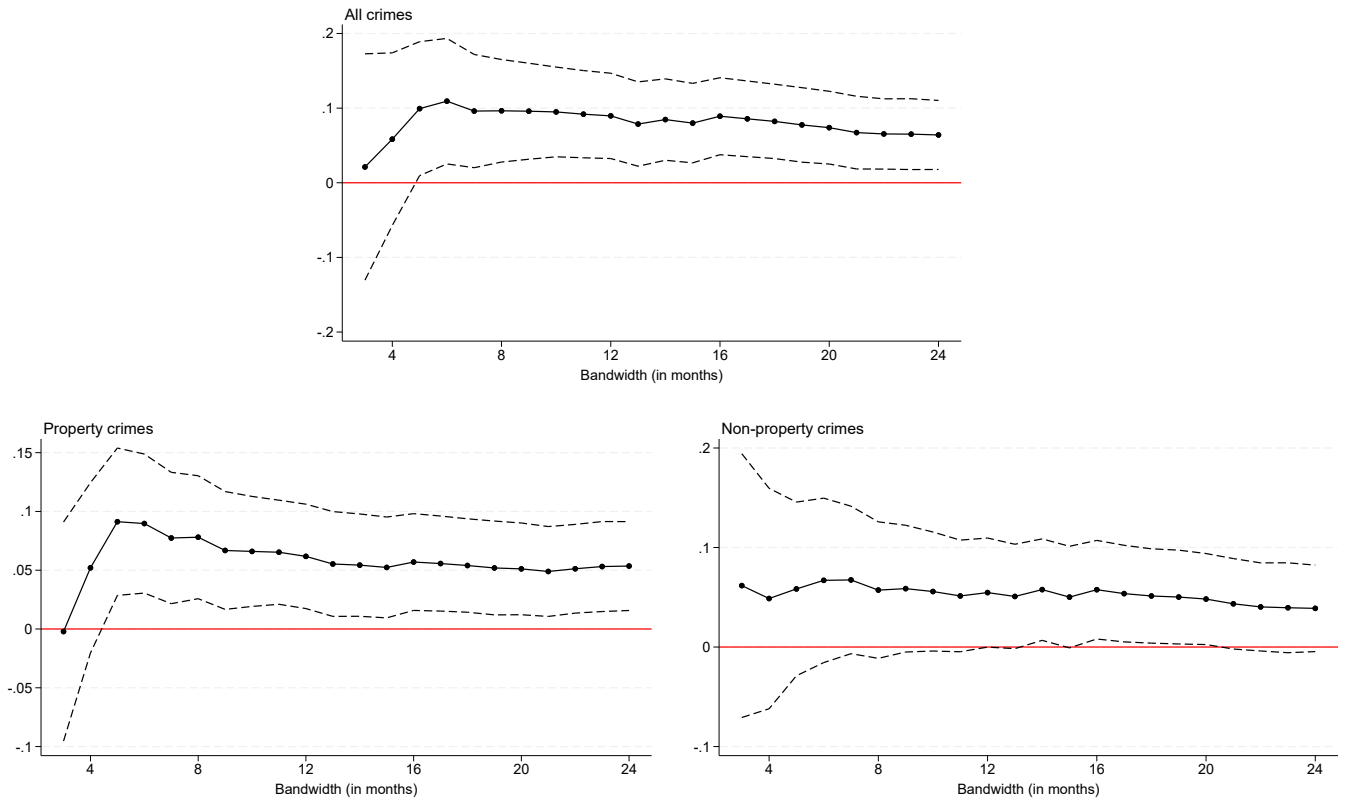
Notes: These figures present the effect of Start Help on refugees' likelihood of moving out of the building to which they were originally assigned, measured in each year after the refugee's arrival. Panel A presents results for all refugees. Panel B restricts the sample to refugees assigned to buildings that received only one refugee family during the estimation window. To create these figures, we first estimate equation (2), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 4, and clustering the errors at the building level. The black circles show the estimated effect of Start Help in each year after refugee arrival, along with its 95% confidence interval. *Sample:* The sample includes 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.

Figure A.12: Effect of Start Help on Non-Danish Neighbors' Building Composition: Share of Individuals with Past Crime Convictions in Original Building



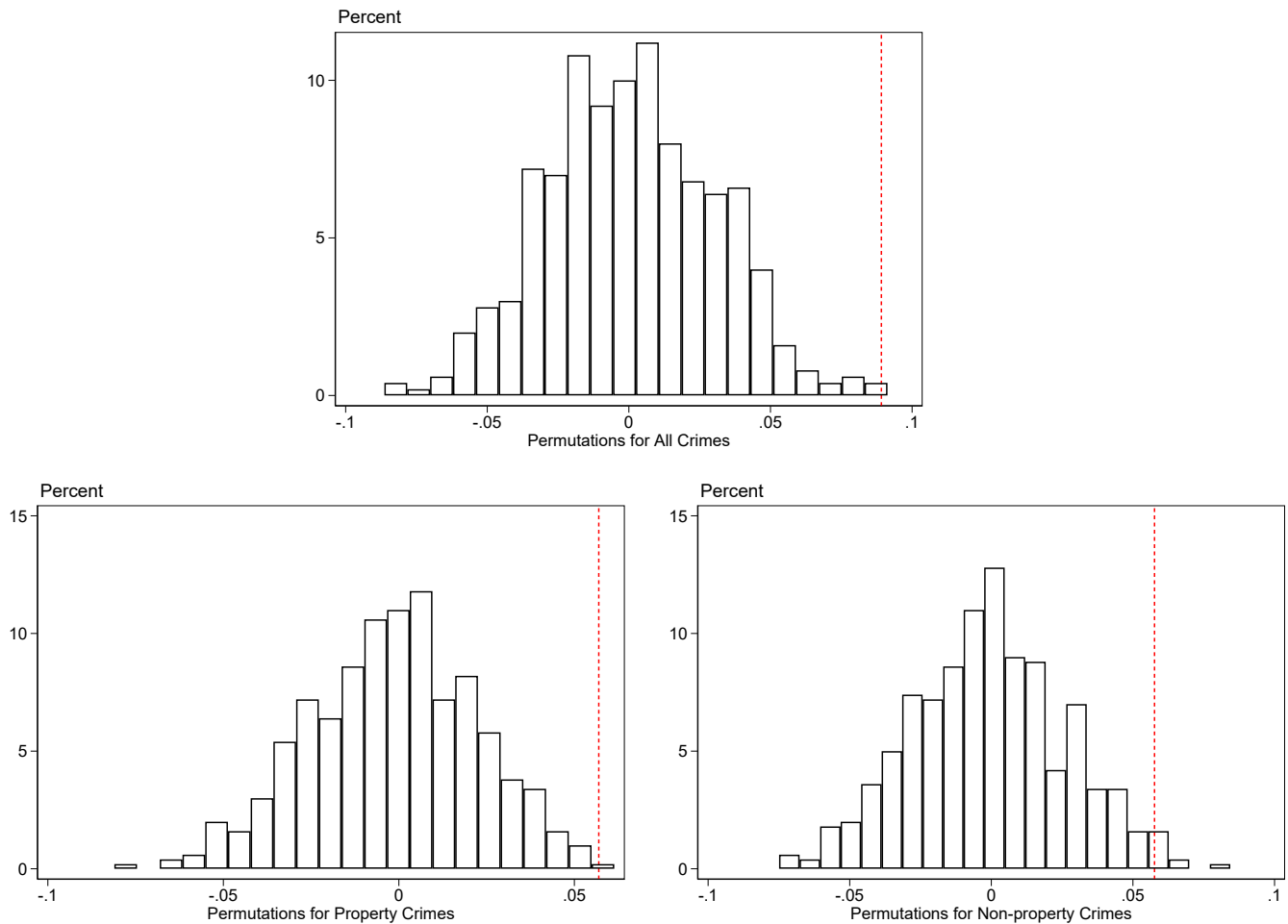
Notes: These figures present the effect of Start Help on the share of non-Danish neighbors' co-residents with prior crime convictions (between 1998 and 2000) in the original buildings refugees were assigned to, in each year after refugee arrival. To create this figure, we first estimate equation (2), controlling for linear functions of the running variable, using triangular weights, controlling for the demographics listed in section 4, and clustering the errors at the building level. The black circles show the estimated effect of Start Help in each year after refugee arrival, along with its 95% confidence interval. *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.13: 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 4, 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.14: 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 4, 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 effect 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	11.556 (18.994)
Average Sample (age 18-55) Residents per Building	7.397 (14.651)
Average Danish Neighbors per Building	4.460 (11.550)
Average Non-Danish Neighbors per Building	1.751 (4.284)
Average Refugees per Building	1.580 (1.247)
Buildings in Sample	4139
Municipalities in Sample	262
Buildings with only 1 Refugee Family in Sample	1985
Municipalities with Buildings with only 1 Refugee Family in Sample	246

Notes: This table shows average values and standard errors (in parentheses) for building characteristics in our sample. The sample used for the statistics includes only buildings with at most one refugee family.

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

	Yr 1	Yrs 1-4	Yrs 1-10
<i>Panel A: Transfer Income (1000s USD)</i>			
Start Aid	-9.159*** (0.964)	-16.407*** (3.525)	-12.620 (8.797)
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
<i>Panel B: Labor Income (1000s USD)</i>			
Start Aid	0.465 (0.598)	1.937 (3.345)	0.497 (10.277)
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
<i>Panel C: Years with Labor Income > 0</i>			
Start Aid	0.057** (0.022)	0.141* (0.084)	0.254 (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 4, 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	Shoplift	Other	Violent	Sexual	Drug	Weapon	Immigration	Public order/ police	Financial	Breaking of misc. laws	Traffic
<i>Panel A: Likelihood of Any Crime</i>												
Start Aid	0.089*** (0.026)	0.057*** (0.018)	0.013 (0.015)	0.014 (0.019)	0.002 (0.003)	0.027 (0.021)	0.020* (0.011)	0.003 (0.004)	0.024** (0.011)	0.005 (0.005)	0.019 (0.016)	0.034 (0.030)
Mean Y	0.170	0.047	0.050	0.040	0.004	0.050	0.018	0.003	0.029	0.005	0.029	0.240
Mean Y Pre Start Help	0.157	0.042	0.044	0.039	0.003	0.041	0.016	0.003	0.024	0.004	0.026	0.234
<i>Panel B: Number of Crimes</i>												
Start Aid	0.424*** (0.121)	0.116*** (0.044)	0.051* (0.030)	0.015 (0.029)	0.002 (0.003)	0.044 (0.058)	0.021* (0.012)	0.005 (0.005)	0.079 (0.054)	0.005 (0.005)	0.048 (0.034)	0.116 (0.081)
Mean Y	0.453	0.085	0.079	0.058	0.004	0.115	0.020	0.004	0.045	0.005	0.040	0.466
Mean Y Pre Start Help	0.384	0.068	0.067	0.060	0.004	0.096	0.019	0.003	0.032	0.004	0.034	0.445
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 column and then restricting it to various subcategories in the remaining columns. Columns 2 and 3 present results for property crimes, divided into shoplifting and other property crimes. Columns 4-11 present results for non-property crimes, divided into crimes that are: violent, sexual, drug-related, weapon-related, immigration-related, "public order/police" (such as insulting public authority, evidence tampering, trespassing, violation of public orders and police regulations), financial, and violations of miscellaneous Danish laws. In the last column, we show results for traffic-related crimes. 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 4, 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: Start Help and Neighbors' Crime Convictions:
Heterogeneity by Refugees' Crime Propensity

	All	High Crime Index	
		Yes	No
<i>Panel A: All Crimes</i>			
Start Aid	0.089*** (0.026)	0.085*** (0.033)	0.061 (0.053)
Mean Y	0.170	0.175	0.162
Mean Y Pre Start Help	0.157	0.158	0.154
<i>Panel B: Property Crimes</i>			
Start Aid	0.057*** (0.021)	0.068** (0.030)	0.048 (0.034)
Mean Y	0.084	0.082	0.086
Mean Y Pre Start Help	0.075	0.070	0.082
<i>Panel C: Non-Property Crimes</i>			
Start Aid	0.058** (0.025)	0.051 (0.031)	0.042 (0.050)
Mean Y	0.120	0.126	0.110
Mean Y Pre Start Help	0.112	0.117	0.104
Number of Neighbors	3797	2390	1407

Notes: This table presents the effect of Start Help on the likelihood that non-Danish neighbors are convicted of any, property, and non-property crimes, reported in Panels A to C, respectively. We distinguish between neighbors of refugees with high versus low crime propensity. Column 1 reproduces the baseline effects from Table 4, where the outcome variable is the likelihood that a neighbor is convicted of (any, property, or non-property) crimes within ten years of the refugee's arrival in the building. Column 2 presents estimates for neighbors of refugees with high crime propensity, while Column 3 reports estimates for neighbors of refugees with low crime propensity. Refugees are classified as high or low crime propensity as follows. For each refugee in the sample, we predict the likelihood of committing a crime within ten years after being granted residency. The prediction is based on a linear regression using the following refugee characteristics measured at the time of residency: gender, marital status, number of children, continent of origin, and municipality. These characteristics are measured at the time of residency. Refugees are excluded from the estimation of their own predicted crime. Once predicted values are obtained, we rank refugees and assign them a crime index ranging from 0 to 1. We classify as high-crime-index those refugees with an index above 0.6. The specifications are otherwise identical to those of Table 4.

Table A.5: Descriptive Statistics: High-Crime vs. All Refugees

	High Crime Index			All Sample		
	Mean	SD	N	Mean	SD	N
<i>Panel A: All Refugees</i>						
Average age	29.206	6.837	2099	32.957	8.415	5292
Share of females	0.152	0.359	2099	0.446	0.497	5292
Share married	0.529	0.499	2099	0.740	0.439	5292
Average # of children	0.210	0.594	2099	0.955	1.321	5292
<i>Panel B: Buildings with 1 Refugee</i>						
	Mean	SD	N	Mean	SD	N
Average age	28.983	6.724	1020	32.639	8.262	2636
Share of females	0.190	0.393	1020	0.475	0.499	2636
Share married	0.571	0.495	1020	0.768	0.422	2636
Average # of children	0.259	0.634	1020	1.021	1.302	2636

Notes: This table presents descriptive statistics distinguishing between the sample of high-crime-index refugees (columns 1–3) and the full sample of refugees, including both high- and low-crime-index individuals (columns 4–5). Refugees are classified as having high or low crime propensity as follows. For each refugee in the sample, we predict the likelihood of committing a crime within ten years after being granted residency. The prediction is based on a linear regression using the following characteristics measured at the time of residency: gender, marital status, number of children, continent of origin, and municipality. Each refugee is excluded from the estimation of their own predicted crime. Once predicted values are obtained, we rank refugees and assign them a crime index ranging from 0 to 1. Refugees with an index above 0.6 are classified as high-crime-index. Panel A reports statistics for all refugees, while Panel B reports statistics for refugees assigned to a building with no other refugee family arriving in the estimation window.

Table A.6: 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
<i>Panel A: All Crimes</i>									
Start Aid	0.089*** (0.026)	0.154*** (0.035)	0.040 (0.038)	0.090** (0.044)	0.056 (0.041)	0.042 (0.038)	0.118*** (0.039)	0.007 (0.039)	0.182*** (0.038)
Mean Y	0.170	0.189	0.149	0.250	0.065	0.140	0.200	0.131	0.219
Mean Y Pre Start Help	0.157	0.164	0.148	0.228	0.062	0.142	0.171	0.133	0.189
<i>Panel B: Property Crimes</i>									
Start Aid	0.057*** (0.021)	0.061** (0.029)	0.052* (0.031)	0.037 (0.028)	0.075* (0.039)	0.064** (0.029)	0.039 (0.034)	0.017 (0.028)	0.087*** (0.033)
Mean Y	0.084	0.092	0.074	0.111	0.047	0.070	0.098	0.061	0.112
Mean Y Pre Start Help	0.075	0.077	0.072	0.098	0.043	0.062	0.087	0.059	0.095
<i>Panel C: Non-Property Crimes</i>									
Start Aid	0.058** (0.025)	0.131*** (0.040)	0.008 (0.034)	0.080* (0.043)	-0.001 (0.018)	0.002 (0.035)	0.093*** (0.036)	-0.016 (0.030)	0.158*** (0.041)
Mean Y	0.120	0.142	0.096	0.193	0.023	0.090	0.150	0.084	0.165
Mean Y Pre Start Help	0.112	0.123	0.099	0.180	0.024	0.095	0.129	0.089	0.144
Number of 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 4, 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.7: Effect of Start Help on Neighbors' 10-Year Crime Convictions – Sensitivity to Weighting by the Inverse of Building Size

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
<i>Panel A: All Neighbors</i>						
Start Aid	0.018 (0.020)	0.054 (0.072)	0.002 (0.014)	-0.027 (0.034)	0.007 (0.018)	0.068 (0.055)
Mean Y	0.141	0.335	0.068	0.133	0.101	0.202
Mean Y Pre Start Help	0.133	0.313	0.065	0.129	0.096	0.184
Number of Neighbors	13687	13687	13687	13687	13687	13687
<i>Panel B: Non-Danish Neighbors</i>						
Start Aid	0.102** (0.042)	0.601** (0.234)	0.072** (0.029)	0.119* (0.061)	0.073* (0.039)	0.444*** (0.170)
Mean Y	0.157	0.369	0.072	0.119	0.114	0.251
Mean Y Pre Start Help	0.139	0.296	0.064	0.097	0.108	0.199
Number of Neighbors	3797	3797	3797	3797	3797	3797
<i>Panel C: Danish Neighbors</i>						
Start Aid	-0.000 (0.022)	-0.065 (0.072)	-0.016 (0.015)	-0.052 (0.039)	-0.008 (0.019)	-0.026 (0.053)
Mean Y	0.135	0.323	0.067	0.137	0.097	0.185
Mean Y Pre Start Help	0.130	0.319	0.066	0.140	0.091	0.178
Number of Neighbors	9890	9890	9890	9890	9890	9890

Notes: This table re-estimate the specifications of Table 4 weighting each observations by the inverse of building size. Practically, this is done by dividing the triangular weight associated with each observation by the number of neighbors living in the building in a given year. The table is otherwise identical to Table 4. *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.8: Benefits and Costs of Start Help Excluding Crime Effects

	<i>Amount</i>
<i>Panel A: Change in welfare transfers net of taxes</i>	
Change in transfers (Appendix Table A.2)	12620
Marginal tax rate on low incomes	0.381
Average change in welfare transfers net of taxes	7805.672
Number of refugees	5292
Total change in welfare transfers net of taxes	41307615.801
<i>Panel B: Change in tax revenues from labor income</i>	
Change in labor income (Appendix Table A.2)	497
Marginal tax rate on low incomes	0.381
Average change in tax revenues	189.598
Number of refugees	5292
Total change in tax revenues from labour income	1003350.224

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.9: Costs of Start Help from Crime Effects

	Costs to taxpayers	Costs to victims	
		Convicted crimes	Reported crimes
<i>Panel A: Costs from refugees' crimes</i>			
Change in number of property crimes (Table 3)	0.064	0.064	0.064
Weighted cost of property crime (Appendix Table 6))	161.283	694.024	12562.323
Average change in costs from property crime	10.322	44.418	803.989
Number of refugees	5292	5292	5292
Total change in costs from refugees' crime	54624.594	235057.733	4254708.130
<i>Panel B: Costs from non-Danish neighbors' property crimes</i>			
Change in number of property crimes (Table 4)	0.168	0.168	0.168
Weighted cost of property crime (Appendix Table 6)	1023.819	894.457	17275.415
Average change in costs from property crime	172.002	150.269	2902.270
Number of non-Danish neighbors	3797	3797	3797
Total change in costs from property crime	653089.941	570570.740	11019918.260
<i>Panel C: Costs from non-Danish neighbors' non-property crimes</i>			
Change in number of non-property crimes (Table 4)	0.235	0.235	0.235
Weighted cost of non-property crime (Appendix Table 6)	1420.176	6641.349	17543.755
Average change in costs from non-property crime	333.741	1560.717	4122.782
Number of non-Danish neighbors	3797	3797	3797
Total change in costs from non-property crime	1267215.915	5926042.647	15654205.135
Total change in costs from non-Danish neighbors' crime	1920305.856	6496613.387	26674123.395
<i>Panel D: Costs from Danish neighbors' property crimes</i>			
Change in number of property crimes (Table 4)	-0.004	-0.004	-0.004
Weighted cost of property crime (Appendix Table B.3)	1083.472	1046.538	21295.651
Average change in costs from property crime	-4.334	-4.186	-85.183
Number of Danish neighbors	9890	9890	9890
Total change in costs from property crime	-42862.140	-41401.041	-842455.937
<i>Panel E: Costs from Danish neighbors' non-property crimes</i>			
Change in number of non-property crimes (Table 4)	-0.075	-0.075	-0.075
Weighted cost of non-property crime (Appendix Table B.3)	1487.330	8021.848	21743.198
Average change in costs from non-property crime	-111.550	-601.639	-1630.740
Number of Danish neighbors	9890	9890	9890
Total change in costs from non-property crime	-1103227.089	-5950206.099	-16128017.101
Total change in costs from Danish neighbors' crime	-1146089.229	-5991607.140	-16970473.038

Notes: This table shows the details behind the calculations of MVPF components 4 to 9 in panel A of Table 5 and components 4 to 12 in Table B.2. 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 6 and Table B.3. To allow for a comparison between effects obtained on different samples of refugees, non-Danish and 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 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 non-Danish neighbors. For neighbors, we sum the total change in costs across property and non-property crime to obtain total costs.

Table A.10: Cost-Benefit Analysis of Start Help with Reduced Migration

	<i>Amount</i>	<i>Notes</i>
<i>Panel A: MVPF components</i>		
1. Change in welfare transfers net of taxes	41307615.801	See Appendix Table A.8
2. Changes in tax revenues from labor income	1003350.224	See Appendix Table A.8
3. Total savings to taxpayers	42310966.025	1+2
4. Enforcement and prosecution costs from refugees	54624.594	See Appendix Table A.9
5. Enforcement and prosecution costs from neighbors	1920305.856	See Appendix Table A.9
6. Costs to victims from convicted crimes of refugees	235057.733	See Appendix Table A.9
7. Costs to victims from convicted crimes of non-Danish neighbors	6496613.387	See Appendix Table A.9
8. Costs to victims from reported crimes of refugees	4254708.130	See Appendix Table A.9
9. Costs to victims from reported crimes of non-Danish neighbors	26674123.395	See Appendix Table A.9
<i>Panel B: Effects of reduced migration on government spending and revenues</i>		
10. Estimated overall reduction in migration flows over 10 years	50000	5000 per year over 10 years
11. Estimated reduction in number of refugees	2800	See table's notes for details
12. Estimated reduction in number of immigrants	47200	See table's notes for details
<i>Government Spending</i>		
13. Share of refugees who receive some welfare over 10 years	0.96	See table's notes for details
14. Share of immigrants who receive some welfare over 10 years	0.93	See table's notes for details
15. Average welfare transfers per refugee net of taxes over 10 years	123689.593	See table's notes for details
16. Average welfare transfers per immigrant net of taxes over 10 years	109613.4055	See table's note for details.
17. Estimated savings on welfare transfers from reduced migration	5144067673.744	$(15 \times 11 \times 13) + (16 \times 12 \times 14)$
<i>Tax revenues from labor income</i>		
18. Share of refugees who receive some labor income over 10 years	0.665	See table's notes for details
19. Share of immigrants who receive some labor income over 10 years	0.704	See table's notes for details
20. Average tax revenues per refugee over 10 years	49211.436	See table's notes for details
21. Average tax revenues per immigrant over 10 years	64492.92207	See table's notes for details
22. Reduction in tax revenues due to reduced migration	2234654102.778	$(20 \times 11 \times 18) + (21 \times 12 \times 19)$
<i>Panel C: MVPF including costs of convicted crimes to victims and effects of reduced migration</i>		
Base MVPF (ignore effects on crime)	0.01399	1 over (3+17-22)
MVPF adding refugees' crime	0.01407	(1+6) over (3-4+17-22)
MVPF adding also neighbors' crime	0.01629	(1+6+7) over (3-4-5+17-22)
<i>Panel D: MVPF including costs of reported crimes to victims and effects of reduced migration</i>		
Base MVPF (ignore effects on crime)	0.01399	1 over (3+17-22)
MVPF adding refugees' crime	0.01544	(1+8) over (3-4+17-22)
MVPF adding also neighbors' crime	0.02449	(1+8+9) over (3-4-5+17-22)

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, 2002, 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.8 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.11: Effect of Start Help on Joint Likelihood of Crime Convictions for Refugees and Their Neighbors

	All	Refugee	
		Convicted	Not Convicted
<i>Panel A: All Crimes</i>			
Start Aid	0.089*** (0.026)	0.080** (0.037)	0.009 (0.046)
Mean Y	0.170	0.028	0.142
Mean Y Pre Start Help	0.157	0.014	0.142
<i>Panel B: Property Crimes</i>			
Start Aid	0.057*** (0.021)	0.035** (0.017)	0.022 (0.028)
Mean Y	0.084	0.014	0.069
Mean Y Pre Start Help	0.075	0.008	0.067
<i>Panel C: Non-Property Crimes</i>			
Start Aid	0.058** (0.025)	0.063** (0.030)	-0.006 (0.035)
Mean Y	0.120	0.021	0.099
Mean Y Pre Start Help	0.112	0.011	0.102
Number of Neighbors	3797	3797	3797

Notes: This table presents the effect of Start Help on the likelihood that refugees and their non-Danish neighbors are convicted of any, property and non-property crimes, in panel A to C respectively. In column 1 the outcomes is the likelihood the that neighbor is convicted of (any, property, non-property) crimes. In column 2 the outcome is the likelihood that both the neighbor is convicted of (any, property, non-property) crimes and the refugee the neighbor was exposed to is convicted of property crimes. In column 3 the outcome is the likelihood that the neighbor is convicted of (any, property, non-property) crimes but the refugee the neighbor was exposed to is not convicted of property crimes. 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 4, 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.12: Effect of Start Help on Joint Likelihood of Crime Convictions for Refugees and Their Neighbors: Heterogeneity by Building Size

	Building Size		No. Danish Neighbors	
	Below Median	Above Median	Below Median	Above Median
<i>Panel A: Likelihood of Any Crime</i>				
Start Aid	0.011 (0.026)	0.166*** (0.059)	0.001 (0.021)	0.148*** (0.052)
Mean Y	0.024	0.033	0.022	0.033
Mean Y Pre Start Help	0.019	0.007	0.018	0.010
Number of Neighbors	1926	1871	1746	2051
<i>Panel B: Likelihood of Property Crime</i>				
Start Aid	-0.004 (0.012)	0.084*** (0.026)	-0.005 (0.010)	0.075*** (0.023)
Mean Y	0.012	0.017	0.013	0.016
Mean Y Pre Start Help	0.011	0.004	0.011	0.005
Number of Neighbors	1926	1871	1746	2051
<i>Panel C: Likelihood of Non-Property Crime</i>				
Start Aid	0.008 (0.020)	0.131*** (0.048)	-0.001 (0.019)	0.116*** (0.043)
Mean Y	0.017	0.025	0.016	0.025
Mean Y Pre Start Help	0.014	0.005	0.014	0.006
Median Building Size	27.667	27.667	27.667	27.667
Median No. Danish Neighbors	7.000	7.000	7.000	7.000
Number of Neighbors	1926	1871	1746	2051

Notes: This table re-estimates the specification of column 2 in Table A.11, distinguishing between larger (above-median) and smaller (below-median) buildings, defined either by total resident population (columns 1 and 2) or by the number of non-Danish neighbors in our sample (columns 3 and 4). The specification is otherwise the same as in column 2 of Table A.11.

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 (0.048)	-0.150 (0.254)	0.002 (0.037)	-0.002 (0.151)	-0.025 (0.042)	-0.164 (0.165)
Mean Y	0.246	0.977	0.157	0.498	0.188	0.479
Mean Y Pre Start Help	0.234	0.926	0.151	0.483	0.171	0.443
Number of Neighbors	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 4, 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 – Heterogeneity by the Municipality's Crime Conviction Rates

	All						Property						Non-Property								
	Any		Number		Number		Any		Number		Number		Any		Number		Any		Number		
	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes	No	
<i>Panel A: Municipality Above or Below Median</i>																					
Start Aid	0.083*** (0.032)	0.051 (0.045)	0.418*** (0.157)	0.222* (0.129)	0.066** (0.027)	0.018 (0.032)	0.187*** (0.063)	0.071 (0.085)	0.055* (0.031)	0.015 (0.038)	0.188 (0.139)	0.141 (0.097)									
Below Median																					
Mean Y Pre Start Help	0.160	0.152	0.392	0.377	0.077	0.072	0.139	0.129	0.109	0.117	0.253	0.248									
Mean Share Convicted	0.060	0.083	0.060	0.083	0.060	0.083	0.060	0.083	0.060	0.083	0.060	0.083									
Number of Neighbors	2172	1625	2172	1625	2172	1625	2172	1625	2172	1625	2172	1625									
<i>Panel B: Municipality Above or Below 75 Percentile</i>																					
Start Aid	0.078*** (0.028)	0.038 (0.055)	0.407*** (0.136)	0.053 (0.140)	0.073*** (0.023)	-0.043 (0.047)	0.198*** (0.057)	-0.068 (0.083)	0.045 (0.028)	0.029 (0.045)	0.176 (0.118)	0.111 (0.116)									
Below 75 Percentile																					
Mean Y Pre Start Help	0.160	0.148	0.386	0.381	0.073	0.081	0.133	0.139	0.114	0.106	0.253	0.242									
Mean Convicted	0.064	0.090	0.064	0.090	0.064	0.090	0.064	0.090	0.064	0.090	0.064	0.090									
Number of Neighbors	2911	886	2911	886	2911	886	2911	886	2911	886	2911	886									

Notes: This table presents the heterogeneous effects of Start Help on refugees' non-Danish neighbors' crime convictions by their municipalities' crime conviction rates. The columns indicate the outcome variables such as the likelihood of being convicted and number of convictions for any (non-traffic) crimes (columns 1–4), property crimes (columns 5–8) and non-property crimes (columns 9–12). Odd columns in panel A (B) contain results for neighbors in municipalities with below the median (75th percentile) conviction rates. Even columns in panel A (B) contain results for neighbors in municipalities with above the median (75th percentile) conviction rates. 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 4, 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.15: 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
<i>Panel A: All Convictions</i>						
Start Aid	0.089*** (0.026)	0.424*** (0.121)	0.057*** (0.021)	0.168*** (0.053)	0.058** (0.025)	0.235** (0.107)
Mean Y	0.170	0.453	0.084	0.164	0.120	0.289
Mean Y Pre Start Help	0.157	0.384	0.075	0.134	0.112	0.249
<i>Panel B: Convictions without Refugees</i>						
Start Aid	0.095*** (0.026)	0.415*** (0.120)	0.059*** (0.021)	0.164*** (0.054)	0.063** (0.025)	0.239** (0.107)
Mean Y	0.163	0.436	0.080	0.160	0.114	0.276
Mean Y Pre Start Help	0.156	0.379	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 4, 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.16: Effect of Start Help on Non-Danish Neighbors' Transfers and Work

	Yrs -2/-1	Yr 1	Yrs 1-4	Yrs 1-10
<i>Panel A: Transfer income (1000s USD)</i>				
Start Aid	-0.359 (2.024)	-2.009* (1.118)	-4.351 (4.830)	-4.904 (13.764)
Mean Y	32.586	18.246	71.813	175.117
<i>Panel B: Labor income (1000s USD)</i>				
Start Aid	-2.187 (3.524)	-1.409 (1.241)	-2.689 (4.766)	-10.199 (14.571)
Mean Y	19.841	13.038	58.980	169.259
<i>Panel C: Years with labor income > 0</i>				
Start Aid	-0.115 (0.089)	-0.018 (0.037)	-0.061 (0.118)	-0.252 (0.290)
Mean Y	0.827	0.458	1.899	4.665
<i>Panel D: Years Out of Labor Force</i>				
Start Aid	-0.005 (0.026)	-0.006 (0.010)	-0.009 (0.045)	0.087 (0.169)
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 4, 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.17: Effect of Start Help on non-Danish Neighbors' 10-Year Crime –
By Crime Location

	Municipality		
	Any	Residence	Non-Residence
<i>Panel A: Likelihood of Any Crime</i>			
Start Aid	0.089*** (0.026)	0.084*** (0.026)	0.022* (0.013)
Mean Y	0.170	0.159	0.041
Mean Y Pre Start Help	0.157	0.146	0.036
<i>Panel B: Number of Crimes</i>			
Start Aid	0.424*** (0.121)	0.395*** (0.111)	0.025 (0.025)
Mean Y	0.453	0.389	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 4, 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.18: Effect of Start Help on non-Danish Neighbors' 10-Year Crime –
By Municipality Sentiment

	Anti-Immigrant Vote		Racist Police Index	
	Low	High	Low	High
<i>Panel A: Likelihood of Any Crime</i>				
Start Aid	0.083*** (0.032)	0.106** (0.046)	0.061 (0.038)	0.128*** (0.045)
Mean Y	0.178	0.159	0.181	0.157
<i>Panel B: Number of Crimes</i>				
Start Aid	0.461*** (0.152)	0.272* (0.154)	0.403** (0.168)	0.408** (0.179)
Mean Y	0.499	0.389	0.466	0.438
Mean Anti-Immigrant Vote Share	0.101	0.132	0.117	0.111
Mean Racist Police Index	1.190	1.114	0.994	1.349
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 4, 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.19: Effect of Start Help on non-Danish Neighbors' 10-Year Crime – Convictions vs. Charges

	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
<i>Panel A: Convictions</i>						
Start Aid	0.089*** (0.026)	0.424*** (0.121)	0.057*** (0.021)	0.168*** (0.053)	0.058** (0.025)	0.235** (0.107)
Mean Y	0.170	0.453	0.084	0.164	0.120	0.289
Mean Y Pre Start Help	0.157	0.384	0.075	0.134	0.112	0.249
<i>Panel B: Charges</i>						
Start Aid	0.133*** (0.029)	1.042 (0.655)	0.096*** (0.026)	0.660* (0.357)	0.097*** (0.028)	0.473 (0.372)
Mean Y	0.217	1.253	0.125	0.585	0.152	0.666
Mean Y Pre Start Help	0.200	1.046	0.115	0.471	0.139	0.574
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 4, 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.20: 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
<i>Panel A: Likelihood of Any Crime</i>						
Any crimes	0.089*** (0.027)	0.074*** (0.027)	0.073*** (0.027)	0.077*** (0.028)	0.076*** (0.027)	0.077*** (0.028)
Property crimes	0.057*** (0.021)	0.051** (0.020)	0.052** (0.020)	0.055*** (0.020)	0.054*** (0.020)	0.055*** (0.020)
Non-property crimes	0.058** (0.027)	0.042 (0.026)	0.038 (0.025)	0.040 (0.026)	0.040 (0.026)	0.040 (0.026)
<i>Panel B: Number of Crimes</i>						
Any crimes	0.424*** (0.128)	0.371*** (0.126)	0.355*** (0.128)	0.357*** (0.128)	0.358*** (0.128)	0.361*** (0.128)
Property crimes	0.168*** (0.062)	0.140** (0.064)	0.143** (0.064)	0.142** (0.064)	0.143** (0.065)	0.144** (0.065)
Non-property crimes	0.235** (0.103)	0.218** (0.099)	0.203** (0.100)	0.212** (0.101)	0.214** (0.100)	0.215** (0.100)
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 4, 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.

Table A.21: Structural Spillover Estimates and Implied Social Multiplier

γ	0.587 (2.328)
$1/(1 - \gamma)$	2.420 (13.630)
Observations	4,274
Clusters	783

Notes: This table reports the parameter estimates from structural spillover model based on the Dustmann-Landersø (2021). We report the spillover parameter γ , and the implied social multiplier $1/(1 - \gamma)$. Clusters are made up of a focal refugee and a group of neighbors. Standard errors for the social multiplier are delta-method standard errors.

B 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 1st, 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:

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

²Because 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.

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

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

⁵We 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 1st 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 1st, 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 1st 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.

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

⁸In a robustness check, we relax this restriction considering buildings with up to refugee families (see Section 8 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 (*skat-mvialt_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. We categorize crimes based on the Danish seven digit code for law violations *afg-ger7*. For our main analysis, we generally follow Danish two digit classifications (sexual crime, violent crime, property crime, etc). We do, however, marginally adjust some fine crime categories. For example, we classify theft of drugs as a drug crime, even though in the Danish classification it is a property crime. When we turn to our MVPF calculations, we reclassify crimes, mainly because our data on the relative cost of criminal activity are categorized differently than the two digit Danish codes. It was straightforward, but, sometimes required judgment. Worth mentioning, we classify “assault” in our MVPF calculations as all violent crimes except for manslaughter, and “fraud and forgery” as all economic crime. 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. 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.

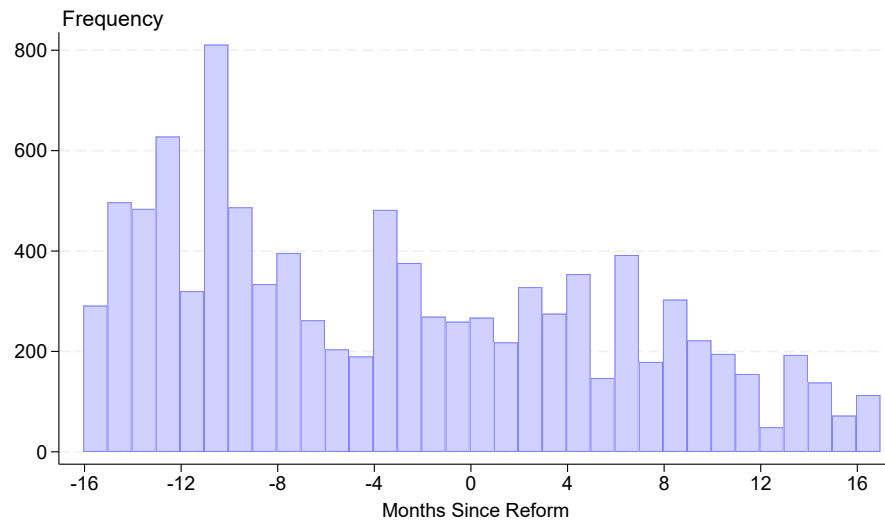
⁹It is possible, but unusual, that a neighbor may move from a building in which a single refugee family is placed to another building where a different refugee family is subsequently placed.

C Results on Danish Neighbors

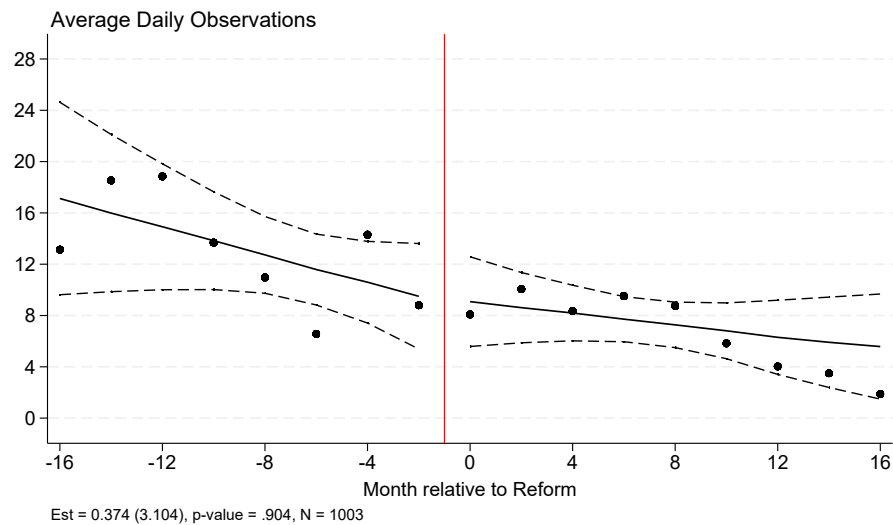
Figures

Figure B.1: Danish Neighbors Density Around the Cutoff

A: Number of Danish Neighbors By Month of Refugee Permit

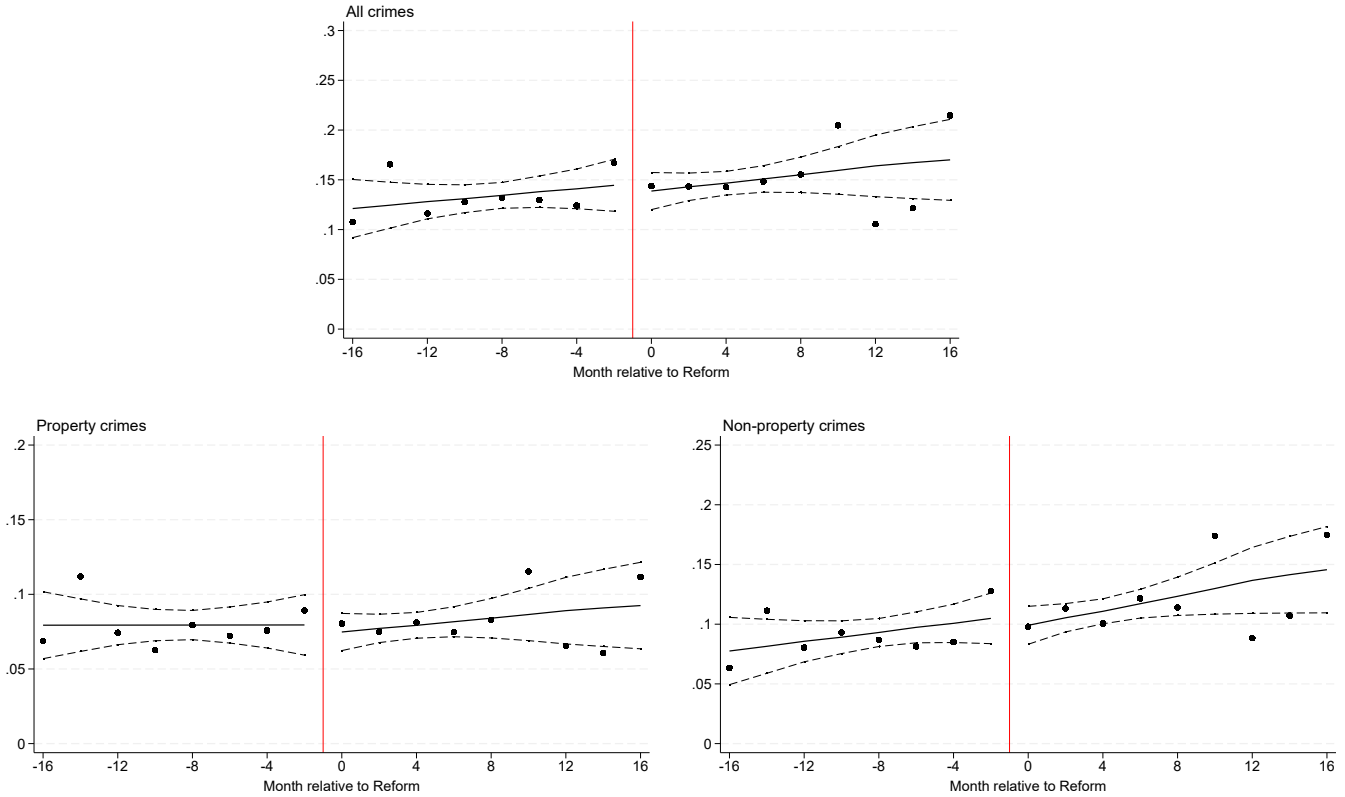


B: Effect of Start Help on Number of Danish Neighbors Per Day



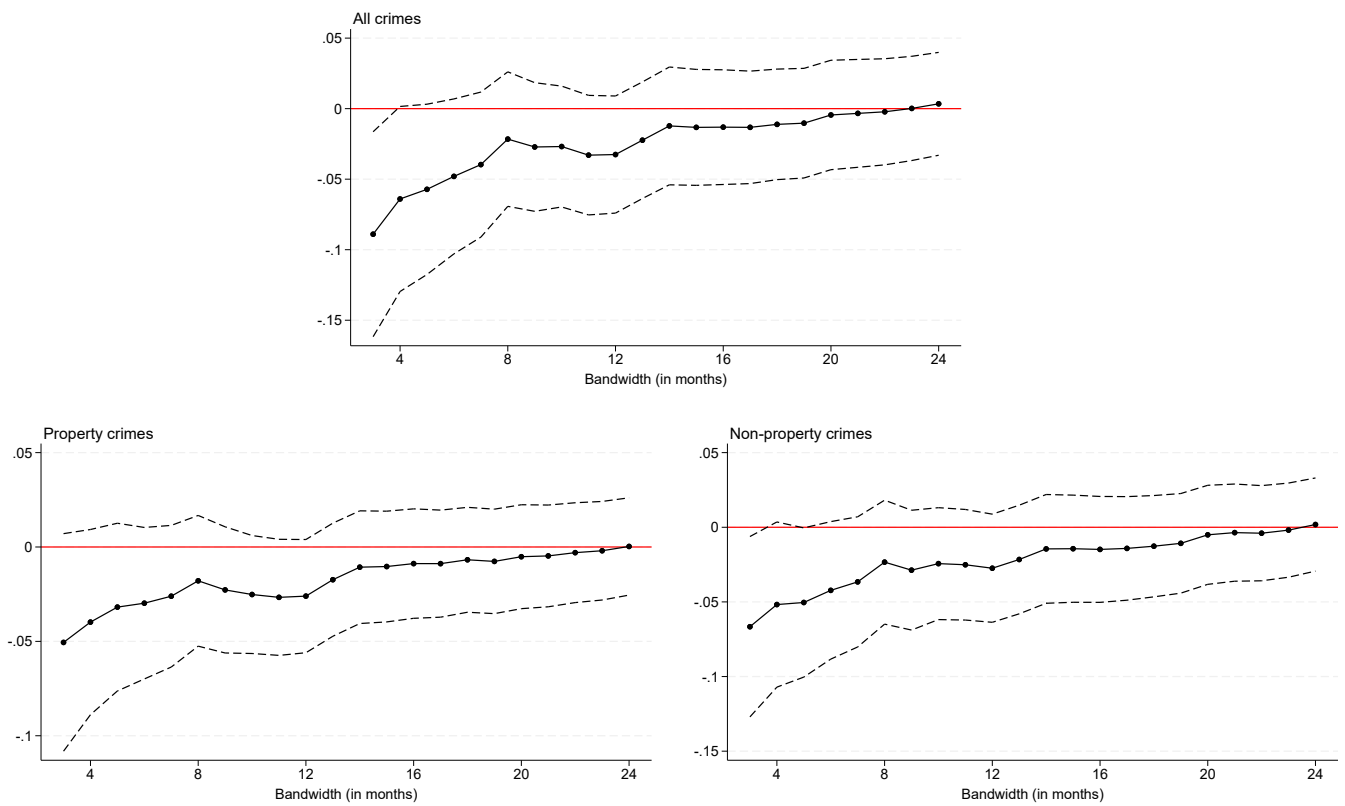
Notes: This figure shows whether there is extra density of refugees' Danish neighbors around the Start Help reform. The figure is otherwise identical to Figure 2 to which refer for further details.

Figure B.2: Effect of Start Help on Danish Neighbors' Likelihood of 10-Year Crime



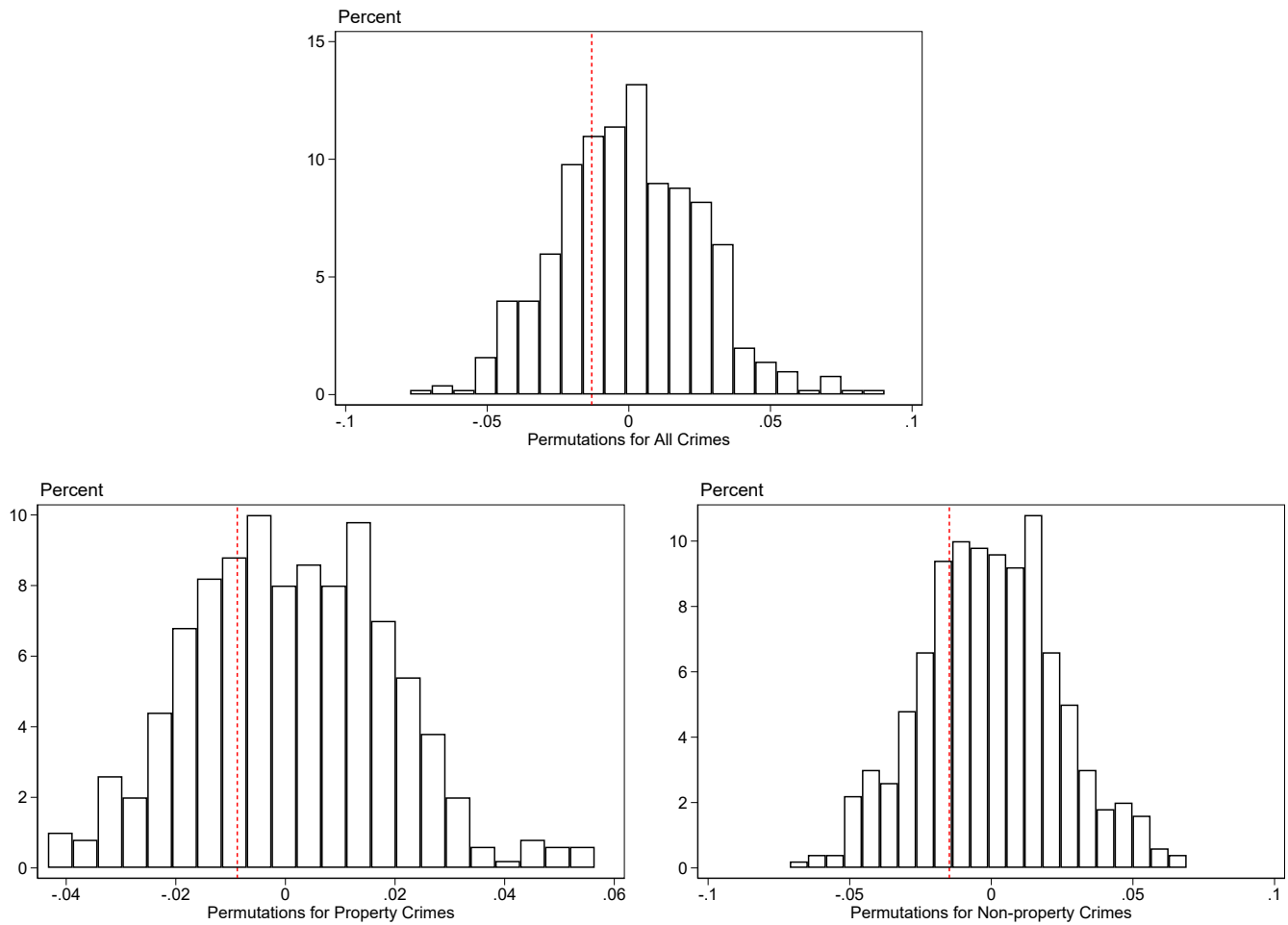
Notes: These figures present the effect of Start Help on refugees' Danish neighbors' likelihood of crime convictions in the first ten years since the refugee's arrival in the building. The top figure presents results for the likelihood of being convicted of any (non-traffic) crimes, while the bottom 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 4 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 1st 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 B.3: Effect of Start Help on Danish Neighbors' Likelihood of 10-Year Crime – Sensitivity to Bandwidth Choice



Notes: These figures re-estimates the specifications of Figure A.13 on the sample of Danish neighbors. We refer to the footnote of Figure A.13 for further details.

Figure B.4: Effect of Placebo Start Help on Danish Neighbors' Likelihood of 10-Year Crime – Randomization Inference



Notes: These figures re-estimates the specifications of Figure A.14 on the sample of Danish neighbors. We refer to Figure A.14 for further details.

Tables

Table B.1: Balancing Tests of Danish Neighbors

<i>Panel A: Own Demographics</i>						
	Age	Female	Married	Number of Kids	From Asia	From Africa
Start Aid	-0.270 (0.992)	0.032 (0.044)	0.016 (0.022)	-0.001 (0.076)	-	-
Mean	32.238	0.420	0.124	0.335	0.000	0.000
<i>Panel B: Refugee and Building Characteristics</i>						
	Refugee Age	Refugee Female	Refugee Married	Number of Kids	Asia	Building Size
Start Aid	-2.972* (1.684)	0.072 (0.104)	0.058 (0.085)	-0.078 (0.117)	0.266*** (0.097)	-0.320 (12.743)
Mean	30.726	0.346	0.696	0.417	0.646	46.306
<i>Panel C: Own Income Pre-Exposure</i>						
	Transfers	Earnings	Earnings>0	OLF		
Start Aid	-1.612 (2.428)	4.476 (4.709)	0.063 (0.078)	-0.064 (0.049)		
Mean	22.989	45.691	1.478	0.189		
<i>Panel D: Own Crime Pre-Exposure</i>						
	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Start Aid	-0.017 (0.036)	-0.035 (0.073)	-0.020 (0.029)	-0.028 (0.048)	-0.003 (0.022)	-0.008 (0.027)
Mean	0.079	0.130	0.054	0.080	0.039	0.050
<i>Panel E: Predicted Crime</i>						
	All		Property		Non-Property	
	Any	Number	Any	Number	Any	Number
Start Aid	-0.018 (0.014)	-0.082 (0.075)	-0.010 (0.010)	-0.038 (0.041)	-0.015 (0.012)	-0.044 (0.035)
Mean	0.147	0.447	0.080	0.206	0.110	0.241
Number of Neighbors	9890	9890	9890	9890	9890	9890

Notes: This table presents balance tests for 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. The specifications estimated are identical to Table 2 to which we refer for further details.

Table B.2: Cost-Benefit Analysis of Start Help with Effects on Danish Neighbors

	<i>Amount</i>	<i>Notes</i>
<i>Panel A: MVPF components</i>		
1. Change in welfare transfers net of taxes	41307615.801	See Appendix Table A.8
2. Changes in tax revenues from labor income	1003350.224	See Appendix Table A.8
3. Total savings to taxpayers	42310966.025	1+2
4. Enforcement and prosecution costs from refugees	54624.594	See Appendix Table A.9
5. Enforcement and prosecution costs from non-Danish neighbors	1920305.856	See Appendix Table A.9
6. Enforcement and prosecution costs from Danish neighbors	-1146089.229	See Appendix Table A.9
7. Costs to victims from convicted crime of refugees	235057.733	See Appendix Table A.9
8. Costs to victims from convicted crime of non-Danish neighbors	6496613.387	See Appendix Table A.9
9. Costs to victims from convicted crime of Danish neighbors	-5991607.140	See Appendix Table A.9
10. Costs to victims from <i>reported crime of refugees</i>	4254708.130	See table's notes for details
11. Costs to victims from <i>reported crime of non-Danish neighbors</i>	26674123.395	See table's notes for details
12. Costs to victims from <i>reported crime of Danish neighbors</i>	-16970473.038	See table's notes for details
<i>Panel B: MVPF including costs to victims of convicted crime</i>		
Base MVPF (ignore effects on crime)	0.976	1 over 3
MVPF adding refugees' crime	0.983	(1 + 7) over (3 - 4)
MVPF adding also neighbors' crime	1.014	(1 + 7 + 8 + 9) over (3 - 4 - 5 - 6)
<i>Panel C: MVPF including costs to victims of reported crime</i>		
Base MVPF (ignore effects on crime)	0.976	1 over 3
MVPF adding refugees' crime	1.078	(1 + 10) over (3 - 4)
MVPF adding also neighbors' crime	1.332	(1 + 10 + 11 + 12) over (3 - 4 - 5 - 6)

Notes: This table shows the details behind the marginal value of public funds (MVPF) calculation in the case in which we also consider as significant the effects on crime of Danish neighbors. The table is otherwise identical to Table 5. Appendix Tables A.8 - 6 and Table B.3 provide detailed calculations behind each component reported in panel A.

Table B.3: Weighted Cost of Danish Neighbors' Crime Estimates

	Cost amount	Frequency of crime	Costs weighted by frequency	Conviction share of reported crimes
<i>Panel A.1: Costs to Taxpayers from Danish Neighbors' property crime</i>				
Burglary	1,703.085	0.18	310.718	-
Theft	945.200	0.82	772.754	-
Robbery	5,996.778	0.00	0	-
Total weighted cost			1083.472	-
<i>Panel A.2: Costs to Taxpayers from Danish Neighbors non-property crime</i>				
Economic Crime	699.130	0.06	39.356	-
Drugs related crime	1,864.257	0.17	323.827	-
Sexual offenses	2,777.923	0.006	17.872	-
Violence	4,009.891	0.09	353.103	-
Other criminal offenses	2,596.807	0.04	103.371	-
Road traffic legislation	987.514	0.57	563.443	-
Violations of other regulation	1,325.760	0.07	86.358	-
Total weighted cost			1487.330	-
<i>Panel B.1: Costs to Victims from Danish Neighbors' property crime</i>				
Burglary	2,626.198	0.18	479.134	0.022
Theft	694.024	0.82	567.404	0.055
Robbery	15,008.868	0.00	0	0.225
Total weighted cost			1046.538	
Weighted average crime conviction share				0.049
Total weighted cost of reported crime				21295.651
<i>Panel B.2: Costs to Victims from Danish Neighbors non-property crime</i>				
Fraud/forgery	0.000	0.17	0.000	0.119
Drugs related crimes	0.000	0.54	0.000	0.492
Sexual assault	163,222.025	0.02	3236.124	0.161
Assault	17,635.065	0.27	4785.724	0.302
Total weighted cost			8021.848	
Weighted average crime conviction share				0.369
Total weighted cost of reported crime				21743.198

Notes: This table shows the details behind the calculations of the weighted cost per conviction displayed in Table A.9. Panel A displays costs to taxpayers. Panel B shows costs to victims. 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 Danish neighbors in the treatment group (i.e., those whose neighboring refugee was granted residency after July 1, 2002). Apart from the Danish-specific weights, the table is otherwise identical to panels A.2, A.3, B.2 and B.3 of Table 6. We refer to Table 6 for further details.

Table B.4: Effect of Start Help on 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
<i>Panel A: All crimes</i>							
Start Aid	-0.013 (0.021)	-0.019 (0.051)	-0.012 (0.021)	-0.056* (0.029)	-0.010 (0.022)	-0.034 (0.037)	-0.057* (0.030)
Mean Y	0.147	0.147	0.147	0.147	0.148	0.156	0.156
Mean Y Pre Start Help	0.140	0.140	0.140	0.140	0.140	0.156	0.156
Number of Neighbors	9890	9890	9890	9890	9890	2452	2452
<i>Panel B: Property crimes</i>							
Start Aid	-0.009 (0.015)	-0.009 (0.038)	-0.008 (0.015)	-0.031 (0.020)	-0.005 (0.016)	-0.006 (0.025)	-0.005 (0.026)
Mean Y	0.080	0.080	0.080	0.080	0.083	0.081	0.081
Mean Y Pre Start Help	0.079	0.079	0.079	0.079	0.079	0.083	0.083
Number of Neighbors	9890	9890	9890	9890	9890	2965	2965
<i>Panel C: Non-property crimes</i>							
Start Aid	-0.015 (0.018)	-0.008 (0.042)	-0.014 (0.018)	-0.054** (0.024)	-0.011 (0.019)	-0.080** (0.035)	-0.107*** (0.022)
Mean Y	0.110	0.110	0.110	0.110	0.110	0.131	0.131
Mean Y Pre Start Help	0.101	0.101	0.101	0.101	0.101	0.135	0.135
Number of Neighbors	9890	9890	9890	9890	9890	1170	1170

This table re-estimate the specifications of Table 7 considering effects on Danish neighbors only. We refer to the footnote of Table 7 for further details.