I Z A Institute
of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12551

Does Halting Refugee Resettlement Reduce Crime? Evidence from the United States Refugee Ban

Daniel Masterson
Vasil Yasenov

AUGUST 2019

I Z A Institute of Labor Economics

Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 12551

Does Halting Refugee Resettlement Reduce Crime? Evidence from the United States Refugee Ban

Daniel Masterson
Stanford University

Vasil Yasenov
Stanford University and IZA

AUGUST 2019

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Does Halting Refugee Resettlement Reduce Crime? Evidence from the United States Refugee Ban*

Many countries have reduced refugee admissions in recent years, in part due to fears that refugees and asylum seekers increase crime rates and pose a national security risk. Existing research presents ambiguous expectations about the consequences of refugee resettlement on crime. We leverage a natural experiment in the United States, where an Executive Order by the president in January 2017 halted refugee resettlement. This policy change was sudden and significant – it resulted in the lowest number of refugees resettled on US soil since 1977 and a 66% drop in resettlement from 2016 to 2017. We find that there is no discernible effect on county-level crime rates. These null effects are consistent across all types of crime and precisely estimated. Overall, the results suggest that crime rates would have been similar had refugee arrivals continued at previous levels.

JEL Classification: F22, J15, K42
Keywords: refugees, immigration, crime

Corresponding author:
Vasil Yasenov
Immigration Policy Lab
Stanford University
Encina Hall West, 616 Serra St.
Stanford, CA 94305
USA
E-mail: yasenov@stanford.edu

* We thank Alexandra Blackman, Francesco Fasani, Tomasso Frattini, Jens Hainmueller, Jacob Kaplan, Duncan Lawrence, Jonathan Mummolo, Jeff Paller, Giovanni Peri, Matti Sarvimäki, Dan Thompson, Jeremy Weinstein as well as members of the Immigration Policy Lab at Stanford University for helpful suggestions and comments. We have also benefited from seminar participants at the University of San Francisco, UC Davis Economics Alumni Conference and the EARN Workshop for Integration hosted by the University of Copenhagen.

1 Introduction

Both the scale of refugee crises and political conflict around the issue have escalated in recent years. The United Nations High Commissioner for Refugees (UNHCR) reports that a record high of 68.5 million people are currently globally displaced, including 3.1 million asylum seekers and 25.4 million refugees (UNHCR, 2018a). Many displaced people seek a new home in a safe host country, either through asylum or refugee resettlement. The United States alone has resettled nearly a million refugees since 2002 (Portes and Rumbaut, 2006; Waters et al., 2009; WRAPS, 2018). Canada, another major resettlement country, has welcomed some 700,000 refugees over the past four decades (UNHCR, 2017). And European countries have received millions of asylum seekers in recent years (Eurostat, 2018b). Despite these efforts an estimated 1.4 million individuals are in need of permanent resettlement to a safe country (UNHCR, 2018b).

As the demand for resettlement has reached a historic high, there has been growing opposition to refugees in the West, and several major host countries have begun to close their doors to asylum seekers and refugees. These policy reversals are motivated in part by a concern, often voiced by opponents of refugee resettlement, that refugees put native-born residents at increased risk of crime and terrorism. Across Europe, leaders of resurgent far-right movements regularly blame refugees for crime. Similarly, in the United States President Trump argued during his presidential campaign that refugees pose a threat to native-born citizens, and shortly after taking office he took immediate steps to considerably reduce refugee resettlement.

On January 27, 2017, President Trump signed Executive Order #13769, which suspended the United States Refugee Admissions Program (USRAP) for 120 days to allow his administration to review the application process and ensure "that those approved for refugee admission do not pose a threat to the security and welfare of the United States (Trump, 2018)." In addition, the administration cut the admission ceiling by more than half. Overall, these efforts led to about a 65.6% drop in the number of refugees resettled to the United

States from 2016 to 2017. Consequently, admissions in 2017 reached the lowest level since 1977 (WRAPS, 2018). The United States is by far the world's largest refugee resettlement destination, which presents an ideal and important context to study the effect of restricting refugee resettlement. Since the inception of USRAP in 1980 up to 2016, the United States resettled more refugees each year than the rest of the world combined (Connor and Krogstad, 2018).

Existing research has found varied estimates of the relationship between immigration more broadly and crime. Many studies find that immigration does not have a discernible impact on crime rates (e.g., Butcher and Piehl, 1998; Lee et al., 2001; Chalfin, 2013; Miles and Cox, 2014; Simes and Waters, 2014), whereas some papers report modest decreases in crime due to immigration (Zhang, 2014; Adelman et al., 2017) and others identify modest increases (Bianchi et al., 2012; Bell et al., 2013; Spenkuch, 2013; Piopiunik and Ruhose, 2017). There is significant heterogeneity in findings across studies depending on the context and research design (Ousey and Kubrin, 2018) and types of immigration and crime (Shihadeh and Barranco, 2010).

Refugees are a special subset of immigrants and differ from economic migrants in both their observable characteristics and the drivers behind their migration decisions (Dustmann et al., 2017). Therefore, it is unclear whether the estimates from the broader immigration and crime literature would generalize to refugee resettlement in particular. Recent studies focusing on refugees in Germany suggest a small increase in crime rates due to the inflow of refugee migrants (Gehrsitz and Ungerer, 2017; Dehos, 2017; Lange and Sommerfeld, 2018). Although the evidence is still too limited and provisional for clear conclusions, it highlights the importance of studying the question elsewhere. There is a paucity of research on the effects of refugee resettlement on crime in the United States. The one exception is a recent study that examines data from 2006 through 2014 and finds no evidence of an effect of refugee resettlement on crime and terrorism related incidents (Amuedo-Dorantes et al., 2018).

This paper contributes to the literature by leveraging a natural experiment to identify the effect of halting refugee resettlement on crime. Finding a plausibly exogenous source of variation in refugee resettlement is essential for estimating the effect of refugees on crime due to the non-random selection of refugees to locations. In the United States, domestic resettlement agencies administer the allocation of refugees. While refugees with family ties are typically assigned to locations close to their family members, the rest are distributed based on local capacity. Due to this non-random allocation process we cannot simply infer the effect of refugees on crime by comparing areas that receive many refugees to those that receive few. If we find that high-receiving areas have lower crime rates, this might just reflect the fact that resettlement agencies are reluctant to send refugees to areas with high crime rates. In order to alleviate this selection bias and isolate the causal effect of refugees from the influence of unmeasured confounding factors that are correlated with both refugee resettlement and crime rates, we require changes in refugee resettlement that are exogenous with respect to local crime trends.

In this study we leverage the large sudden drop in refugee resettlement due to Executive Order #13769 (the "refugee ban") as a natural experiment to study whether reducing refugee resettlement led to a reduction in crime rates. Our design exploits the fact that this nationwide reduction affected counties very differently in a way that is uncorrelated with pre-existing crime trends. The ban resulted in much larger reductions in refugee arrivals in those counties that had received higher numbers of refugees prior to the ban. We exploit this exogenous variation in a difference-in-differences design. To our knowledge, this is the first study to examine the effects of this sudden and significant policy reversal. This setting enables us to overcome some of the methodological challenges that make it difficult to isolate the effect of refugees on crime. Specifically, given that the Executive Order was based on federal policy considerations rather than local conditions, the resulting variation in the reduction in arrivals should be unrelated to preexisting trends in county crime rates. As shown in the body of the paper, multiple tests of observable implications of this assumption support the validity of the research design.

To test for a link between resettlement and local crime, we use the Federal Bureau of Investigation's (FBI) Offenses Known to Law Enforcement series from the Uniform Crime Reports (UCR) database. UCR provides a nationwide statistical effort to collect and report data on crimes brought to the attention of various law enforcement agencies. We supplement this with refugee resettlement data from the Worldwide Refugee Admissions Processing System (WRAPS) database from the Refugee Processing Center which contains yearly information on refugee arrivals to the United States by country of origin. WRAPS is managed by the Bureau of Population, Refugees, and Migration and serves to provide a standardized management system and accountability to USRAP. We merge both data sources together and our analysis focuses on the county-year level covering the 2010-2017 time period.[1]

Our difference-in-differences analyses uncover no discernible effects on county-level crime rates. Counties with higher exposure to the policy reversal did not experience drops in crime in 2017 relative to those counties with less exposure. This null result holds across three distinct estimators and all seven types of crimes in the FBI data. Our results suggest that crime trends would have been similar had refugee arrivals continued at previous levels. More broadly, this finding indicates that restricting refugee resettlement is not an effective policy tool for reducing crime in the United States.

The rest of the paper is organized as follows. We continue with briefly describing USRAP in Section 2. We then outline and data sources and summarize our sample in Section 3. Next, in Section 4 we describe our empirical strategy and the regressions we estimate. We present our results in Section 5. Finally, we discuss the findings in Section 6 and Section 7 concludes.

2 The United States Refugee Admission Program

Each year the President of the United States and the Congress discuss the worldwide refugee situation and determine the numerical ceiling for refugee admissions. These admissions are

[1] County is the lowest level of geographical aggregation which allows for a consistent merge between the two data sources.

then handled and processed by USRAP. USRAP is a collaborative effort between government agencies and nonprofit organizations to identify, admit, and resettle refugees to the United States. The program is not hosted by any one particular department of the federal government but, rather, it is spread between various agencies. First, the United States Citizenship and Immigration Services (USCIS), within the Department of Homeland Security, is responsible for refugee applications, admissions, and related legal issues. In parallel, the Bureau of Population, Refugees, and Migration, within the Department of State, runs USRAP's operations abroad and plays more of a humanitarian role. For instance, it collaborates with nonprofits on the ground to provide services and aid to refugees. Lastly, the Office of Refugee Resettlement's (ORR), within the Department of Health and Human Services, works with admitted refugees to maximize their potential in the United States, assisting new refugees with adapting to living and working in their new home.

For a refugee to be considered for admission by USRAP they have to first have been referred by UNHCR, a United States embassy abroad, or a designated nonprofit organization. They need to fit the definition of a refugee as described in section 101(a)(42) of the Immigration and Nationality Act. The main condition is that they are unable to return to their country of origin because of a well-founded fear of persecution stemming from their race, religion, political affiliation or membership in any other social group. Once they are deemed eligible and referred to USRAP, a lengthy admission process ensues. It may involve multiple interviews, background checks, and health exams with numerous government agencies including the Department of Homeland Security and . Cases based on special humanitarian concern (largely based on nationality) or family reunification are given higher priority. The length of time it takes to complete this screening varies from case to case but sometimes takes multiple years (of State, 2018).

Refugees admitted to the United States are assigned to one of nine domestic resettlement agencies (e.g., International Rescue Committee, Lutheran Immigration and Refugee Services, United States Conference of Catholic Bishops). The agency then chooses the destination where the refugee will be resettled with the goal of maximizing the probability of a successful economic and social integration. Factors affecting this choice may include the presence of family members, the size of the local co-ethnic group or proximity to a major health center. The ORR then works with local agencies to provide the newly-admitted refugees with services including cultural orientation, language instruction, and job training.

Note that refugees are sometimes confused with asylum-seekers. Strictly speaking, the latter constitute a group of people who have fled their home country but whose claims for refugee status have not yet been verified. In the United States these two groups are strictly distinct as asylum-seekers make it to the US prior to filing for asylum while refugees file for resettlement from overseas. Throughout the whole paper we focus on refugees only and do not analyze data on asylum-seekers.

3 Data

3.1 Data Sources

We make use of several data sources. First, we utilize FBI's UCR database, which serves as the official data on crime in the United States. The underlying sources are nearly 18,000 local, state and federal law enforcement agencies which voluntarily report detailed crime statistics for their jurisdiction to the FBI each year. More specifically, we use the Offenses Known to Law Enforcement series that records information on four violent crimes (aggravated assault, forcible rape, murder, and robbery) and three property crimes (burglary, larceny-theft, and motor vehicle theft). In the main text we present results for murder, rape, assault, and burglary and show the rest in the Appendix. We downloaded the data for years 2010–2017 from Jacob Kaplan's OpenICPSR repository (Kaplan, 2018).

Following the crime literature, we convert the reported absolute number of crimes into crime rates per 100,000 population as our main outcome of interest and use a log transformation as a robustness check. The level of observation in the raw database is agency-month and we aggregate this to the county–year level. County is the smallest consistently defined geographic unit that allows for merging the refugee data and crime data. We focus on all 50 states and the District of Columbia, excluding other United States territories. To avoid changes in local crime rates due to compositional changes in the reporting local entities, we focus on the 21,771 agencies that consistently report statistics throughout the entire sample period. In our sample, 3,137 counties had at least one local agency reporting crime statistics, covering the majority of the United States.

Second, we obtain the WRAPS database from the Refugee Processing Center's website (WRAPS, 2018). It contains yearly information on refugee arrivals to the United States. We convert the refugee flow numbers to shares per 100 population as our main explanatory variable of interest, and use logarithmic transformation as a robustness check. The level of observation in the raw dataset is year-origin-city which we aggregate to year-county using Google Maps application programming interface (API) to match each city to a county. Again, we focus on all 50 states and the District of Columbia, excluding all other United States territories and covering years 2010–2017.

Lastly, we use county-level population estimates from the American Community Survey (ACS) from the Integrated Public Use Microdata Series (IPUMS) published by the National Historical Geographic Information System (NHGIS) (Manson et al., 2018). Because estimates for year 2017 are not available, we assign 2016 population values to all counties in year 2017.

3.2 Descriptive Statistics

Our sample consists of 787 counties in all 50 states which received at least one refugee between 2010 and 2016. Table 1 shows summary statistics for the main variables of interest in our analysis. The data is at the county–year level and the time period is 2010–2017, resulting in 6,296 observations. All crime and refugee variables are right-skewed. The mean (median) murder rate per 100,000 population was 3.81 (2.51) per county per year; the average rape rate was 34.05 (29.51); for assaults it was 202.85 (168.80) and for burglaries 527.87 (462.06). Thefts were the most common type of crime in our dataset with an average rate of 1,749.08 (1,634.87); there were 66.89 (40.67) robberies per 100,000 people on average and 162.12 (115.72) motor vehicle thefts. Negative values are very rare and reflect adjustments to prior reported criminal activity. Because we use a logarithmic transformation as a robustness check, we present summary statistics for these variables as well.

Figure 2 displays the ten states with the highest crime rates per 100,000 people by crime type. All crime summary statistics drawn from our analyses line up nearly exactly with official crime summary data published by the FBI (FBI, 2018). Murder rates are highest in the District of Columbia, South Carolina, and Arizona; rapes were most common in Michigan, Alaska, and Arizona; assaults were most prevalent in the District of Columbia, Arizona, and South Carolina; burglaries were highest in South Carolina, North Carolina, and Arkansas. Next, Figure A1 presents national crime rates per 100,000 population for selected crime types. Over time, rape rates (right y-axis) have increased, while the burglary rate has decreased (left y-axis). There is less aggregate variation in assaults (left y-axis) and murders (right y-axis), with their values close to the overall sample mean. Note these numbers are higher than the summary statistics because they reflect aggregated (i.e., summed) values over the entire 2010–2016 period.

The bottom rows of Table 1 show summary statistics of our refugee arrival variables. Similarly to the crime data, these variables are also right-skewed. The mean county received 83.34 refugees in a given year. Next, the left panel in Figure 3 shows the top 10 refugee origin counties and the right panel displays the top ten receiving states. All numbers reflect cumulative values for the time period 2002-2017. The three largest sending countries are Burma (172,646), Iraq (143,867) and Somalia (103,746), and the three largest receiving states were California (106,586), Texas (85,710) and New York (56,561).

Finally, Figure 4 shows a map of cumulative refugee arrivals to the United States in the time period 2002–2017 for each county. As mentioned above, only 787 counties received refugees during the time period. These counties are located in all 50 states. Darker shades of red denote higher refugee arrival levels and white denotes counties with no data on refugee resettlement. This figure illustrates the non-random allocation of refugees to localities. In particular, refugees are more likely to be resettled in places near major urban centers such as parts of California, Washington, Florida and the Northeast.

4 Empirical Strategy

Research Design

We use multiple specifications of the difference-in-differences estimator to analyze the effect of reducing refugee resettlement on crime rates. Figure 1 illustrates our research design. Panel A shows the large and sudden drop in refugee arrivals following the Executive Order in 2017. Our design exploits the fact that this nationwide reduction effort affected counties very differently. As shown in Panel B, the ban resulted in much larger reductions in refugee arrivals in those counties that had received higher numbers of refugees prior to the ban. We compare changes in crime rates after the Executive Order in counties that received many refugees before 2017 to crime rates in counties that received fewer refugees. We separately estimate a regression for each of the seven crime types: murder, rape, aggravated assaults, burglary, robbery, theft and motor vehicle theft. In the main text we present results for murder, rape, assault, and burglary and in the Appendix we show the findings for the remaining three.

Parallel Trends Assumption

We assume that, in the absence of the policy change, crime in areas with higher exposure to the Executive Order would have followed a similar trajectory (or trend) to less exposed areas.[2] We test a number of observable implications of this assumption in order to evaluate

[2]The reader should note that we are discussing crime trends and not levels.

its credibility.

First, we correlate the 2010–2016 county-level crime trends with the 2016–2017 drop in refugee arrivals. This test assesses whether crime trends predating the Executive Order are associated with the drop in arrivals due to the refugee ban. The results are shown in Figures 5 and A2. We find no meaningful relationship between crime pre-trends and the observed 2016–2017 change in refugee resettlement. In other words, places with differential exposure to the refugee ban were not on different crime trends trajectories before the policy reversal. Consequently, it is reasonable to assume that these counties would have continued on such parallel crime trends had the ban not occurred.

Second, we test for parallel trends in a regression framework. In particular, we estimate the following equation:

$$refugees_c^{2016} = \alpha_0 + X_c^{2016\prime}\beta_0 + CrimeGrowth_c^{2010-2016\prime}\gamma + \epsilon_c^{2016}, \tag{1}$$

where c denotes county. The outcome variable $refugees_c^{2016}$ is the refugee flows in 2016 per 100 population and serves as a measure of exposure to the Executive Order. The vector X_c^{2016} controls for county-level demographic characteristics affecting crime rates and state fixed effects, including such characteristics as the share of the population that is female, married, young, white, black, high school dropouts, high school graduates, college dropouts, unemployed, and out of the labor force. The vector $CrimeGrowth_c^{2010-2016}$ contains the 2010–2016 growth rates for the seven major crime types. The intercept is α_0 and ϵ_c^{2016} is the error term. The parallel trends assumption implies that the vector of coefficients γ should be close to zero. Standard errors are clustered by state. The results are shown in Table 2. We find that the estimated γ coefficients are substantively small and none is statistically significant, supporting the validity of the parallel trends assumption.

Third, we visually assess crime trends for each crime type and for counties differentially exposed to the Executive Order. We split all 787 counties in our sample into three groups depending on refugee arrivals per 100 people in 2016. The first group is comprised of localities with no refugee arrivals in 2016 and we refer to it as "very low receiving counties." Note that, since they are in our sample, these counties have at least one arrival in the period 2010–2017. Next, we split the rest of the sample into equal parts – localities with below median ("low receiving") and above median ("high receiving") refugee arrivals in the same year. Similarly to the test above, differential trends by treatment group in the pre-2016 period would undermine our difference-in-differences strategy. The results are presented in Figures 6 and A3. Again, we find that crime trends up to 2016 appear similar regardless of exposure to the policy reversal. We highlight here that the identifying assumption for the design pertains to crime trends and not crime levels. While crime levels are different across exposure to the policy reversal, the trajectories seem to be very close to parallel across county groups.

All in all, we do not find clear evidence of a violation of the parallel trends assumption underlying our econometric models. Under this assumption, the crime trends in low-receiving locations that experienced little change in new arrivals provide a valid estimate of the unobserved counterfactual crime trends we would have observed in the high-receiving locations had the ban not occurred. We now move on to presenting three difference-in-differences specifications leveraging the Executive Order as a natural experiment to test for a causal link between refugee resettlement and crime rates.

First-Differences

The first model we estimate is:

$$\Delta crime_c^{2016-2017} = \alpha_1 + \beta_1 \times \Delta refugees_c^{2016-2017} + \epsilon_c, \tag{2}$$

where c again denotes county. The outcome variable $\Delta crime_c^{2016-2017}$ measures the 2016–2017 change in a separate crime type per 100,000 people. Similarly, the independent variable of interest, $\Delta refugees_c^{2016-2017}$, measures the change in refugee arrivals per 100 people. The intercept is α_1 and ϵ_c is the error term.

This empirical strategy compares the 2016–2017 change in crime in counties that experienced larger declines in new refugee arrivals relative to areas with lower drops. The exact interpretation of β_1 depends on the specification (i.e., level or log), but regardless, a positive sign indicates that refugee resettlement is associated with an increase in crime rates. In a model where both variables are in rates, β_1 is interpreted as the change in crime rate for each additional refugee arrival per 100 people. Similarly in the log-log model it is the percent change in crime for a one percent increase in refugee arrivals. This model can be viewed as fitting a straight line with slope β_1 to the scatter plots in Figure 2. The results are shown in Tables 3 and A1 and Figures 7 and A6. Standard errors are clustered by state.

Continuous Difference-in-Differences

Next, we move on to a more rigorous model in which we use data from the entire sample period 2010–2017. In particular, we estimate:

$$crime_{ct} = \alpha_2 + \beta_2 \times refugees_c^{2016} \times \mathbf{1}(t = 2017) + \gamma_c + \delta_t + X_{ct} + \epsilon_{ct} \tag{3}$$

where c indexes counties, t denotes year and $\mathbf{1}(t = 2017)$ is an indicator for year 2017, which corresponds to the period after the Executive Order. The outcome is a separate crime type measured in rate per 100,000 population. The treatment variable $refugees_c^{2016}$ is the 2016 refugee arrivals per 100 population and is designed to measure exposure to the Executive Order. We include county fixed effects (γ_c) adjusting for permanent time-invariant county-level characteristics affecting crime rates and refugee arrivals and year fixed effects (δ_t) accounting for nationwide crime trends. The term X_{ct} captures county-specific linear time trends allowing for idiosyncratic trends across localities. We experiment with several alternative treatment variables, including the actual 2016–2017 drop in refugee arrivals, arrivals in the entire 2010–2016 period, and delinearized (see below) and log-log specifications.

The intercept is α_2 and ϵ_{ct} is the error term.

This specification compares crime trends before and after the Executive Order in counties with higher exposure relative to other ones with lower exposure. Note that compared to the model above, the interpretation of β_2 is switched so that a negative sign would indicate that counties with larger exposure to refugee resettlement in 2016 experienced larger drops in crime rates in 2017. Thus, a negative sign on β_2 would mean that refugee resettlement leads to higher crime rates.

Alternatively, motivated by the skewness of the refugee resettlement variable, we relax the linearity assumption embedded in Equation (3). To do so we include indicators for counties in the 'low receiving' and 'high receiving' groups (see the subsection above). Note the excluded category (i.e., the reference group) here consists of counties with no refugee arrivals in 2016, and at least one arrival in the other years in the dataset, 2010–2017 (hence, included in the WRAPS dataset). The coefficients' interpretation should be adjusted slightly to account for the fact that they reflect pre-post differences in crime trends between the excluded and each group of counties. The results are shown in Tables 4 and A4. Standard errors are clustered by county.

Generalized Continuous Difference-in-Differences

Finally, we estimate a model in which we interact our treatment variable with an indicator for each year in our sample:

$$crime_{ct} = \alpha_3 + \sum_{\tau=2011}^{2017} \beta_\tau \times refugees_c^{2016} \times \mathbf{1}(t=\tau) + \gamma_c + \delta_t + \epsilon_{ct} \tag{4}$$

The notation and variable definitions are the same as in the previous model. The year 2010 is omitted from the regression and serves as the reference category. The coefficients β_τ indicate the relationship between exposure to the ban (i.e., number of refugees resettled in 2016 per 100 people) and crime in each year. Refugees increasing crime rates would result in the coefficient β_{2017} being statistically significantly smaller than β_{2016} as this corresponds to counties with higher exposure to refugee flows in 2016 experiencing lower 2017 crime rates. Additionally, this specification allows for further verification of the underlying parallel trends assumption. In particular, statistically significant coefficients $\beta_{2011}, \ldots, \beta_{2016}$ would undermine the validity of our empirical strategy as this would indicate that counties with differential exposure to the ban were on different crime trends pre-2017. Figures 8 and A9 show the β_τ coefficients results for various crime types in rates and logs. Standard errors are clustered by county.

5 Results

Did halting refugee resettlement reduce crime rates? We begin with presenting the results on the main crime types for each of the three variations of the empirical strategy described above.

5.1 Main Results

First, Figure 7 provides a graphical summary of the main findings from our natural experiment. It plots the relationship between 2016-2017 changes in refugee arrivals and 2016-2017 changes in crime rates along with the LOESS fit in blue. If refugee resettlement leads to higher crime rates, we would observe an upward sloping LOESS fit. Across all four types of crime, we find no discernible relationship between the reduction in refugee arrivals per capita and the change in the local crime rates when comparing the years before and after the ban. Table 3 presents the regression results using the first-difference model in Equation 2, which is equivalent to fitting a straight line in these scatterplots (with the potential for controlling for state fixed effects). For instance, the coefficient in the first column suggests that an increase of one refugee per 100 people is associated with a reduction of 0.8 murders per 100,000 population, although the relationship is not statistically distinguishable from zero. Again, we find that only one of the coefficients is positive and none is statistically significant, providing no evidence of a relationship between resettlement and crime rates. Halting refugee resettlement had no discernible effect on trends in local crime rates compared to the counterfactual trends the counties would have experienced had the ban not been implemented.

Next, Table 4 presents the results from the difference-in-differences model in Equation 3. If the Executive Order decreased crime rates we would expect a negative interaction effect. This would signal that counties with higher levels of exposure, and therefore higher reductions in arrivals, experienced larger decreases in crime rates between the pre- and post-ban period. Instead, we find no discernible relationship between exposure to the Executive Order and changes in crime rates. For the linear specification presented in Panel A of Table 4, all the interaction terms are statistically insignificant at conventional levels. The point estimates for three of the four crime types are positive, indicating that counties with larger reductions in refugee arrivals experienced larger increases in crime rates. For instance, the coefficient in the first column suggests that a reduction of one refugee per 100 people induced by the ban is associated with an increase of 0.7 murders per 100,000 population in 2017 relative to the 2010-2016 period.

The results are similar for the delinearized specification presented in Panel B. The point estimates for six of the eight coefficients are positive, and one of those is statistically significant. Note that in this regression model, a positive coefficient indicates that higher refugee resettlement is associated with *lower* levels of crime. Overall, these results provide no evidence that the ban's reduction in refugee resettlement had a discernible impact on crime rates, let alone a *decrease* in crime rates.

Lastly, Figure 8 displays the coefficients from the generalized continuous difference-in-differences model presented in Equation 4. Each point estimate is an interaction of the number of refugees in 2016 (i.e., exposure to the ban) with an indicator for the respective year shown in the horizontal axis. Vertical bars correspond to 95% confidence intervals with standard errors clustered by county. The year 2010 is omitted from the sample and serves as a reference. Under the parallel trends assumption, the coefficients for interactions terms for years prior to 2017 should be close to zero. The coefficient on the interaction with the year 2017 provides a test of whether the Executive Order affected crime rates. A positive relationship between refugee resettlement and crime would manifest in the interaction for year 2017 being statistically significantly smaller than that of 2016. The generalized continuous difference-in-differences results both further validate the research design and provide no evidence that the refugee ban had a discernible effect on crime rates.

5.2 Robustness Checks

We continue with presenting several robustness checks of this main result.

Measuring Exposure to the Executive Order

Our primary variable measuring exposure to the Executive Order (i.e., treatment variable) is the 2016 refugee arrivals per 100 population. We perform three robustness checks by changing the way we identify policy exposure in our setting. First, we use the observed (i.e., actual) 2016–2017 county-level drop in refugee resettlement as a treatment variable. All other aspects of the estimation remain the same. The results are presented in Tables 5 and A7.

Second, to flexibly accommodate the skewness of the refugee resettlement variable we split all 787 counties in our analysis into three groups according to their 2016 level of refugee arrivals. The first group of counties called "very low receiving" had no arrivals in 2016. Among counties with non-zero refugee arrivals in 2016, we define the second group as those that received fewer refugees than the median among receiving counties ("low receiving counties") and the last group as those that received more refugees than the median ("high receiving"). We then ran our regression analysis by adding indicators for low and high receiving areas and excluding the first group. The results are shown in Panel B of Table 4.

Lastly, we took the average refugee arrivals in the entire sample pre-period 2010–2016. In Figure A12 we present the correlation between this variable, $refugees_c^{2010-2016}$, and our primary treatment measure, $refugees_c^{2016}$. The correlation coefficient is very high (0.95, $p < 0.001$) indicating strong autocorrelation in refugee flows across United States counties over time. All our results hold with this alternative specification of treatment intensity.

All in all, our main conclusion is robust to any of these choices for measuring county-level exposure to the Executive Order. We find no evidence that refugee resettlement affected crime rates.

Focusing on Other Crime Types

While in the main text we focus on murder, rape, assault and burglary, FBI's UCR database contains information on three other crime types – theft, robbery and motor vehicle theft. We conducted all statistical analyses for these additional crime types. The results are presented in Figures A2, A3 and A5 (Parallel Trends); Tables A1, A3 and Figures A6, A8 (First-differences); Tables A4 and A6 (Continuous Difference-in-differences); and Figures A9 and A11 (Generalized Continuous Difference-in-differences). Our conclusion of no statistically detectable relationship between crime rates and refugee resettlement remains valid for thefts, robberies and motor vehicle thefts as well.

Using Logarithmic Transformation

Our primary regression specification measures the impact of refugee arrivals per 100 people on the crime rates per 100,000 population. We replicate this analysis with a log-log specification in which the independent variable is log refugee arrivals in 2016 and the outcome is log absolute number of crimes. The results are shown in Figures A4 and A5 (Parallel Trends); Tables A2, A3 and Figures A7, A8 (First-differences); Tables A5 and A6 (Continuous Difference-in-differences); and Figures A10 and A11 (Generalized Continuous Difference-in-differences). Similarly to our main results, we find no evidence of a discernible relationship between refugee resettlement and crimes.

Focusing on High Crime Areas

We conducted subgroup analysis focusing on areas with high crime rates. To identify these areas we summed the total number of crimes for all counties across the entire period and selected the set of counties with above-median crime activity. We then conducted our analysis on this subsample only. The results are shown in Tables 6 and A8 (Continuous Difference-in-differences). We find no evidence that refugee resettlement significantly impacted crime rates in these high crime areas.

6 Discussion

6.1 Estimates Precision and Effect Sizes

How precisely estimated are these null effects? First, consider the linear specification. Note that the average number of refugee arrivals per 100 population is 0.02, with a standard deviation of 0.07. For burglary, the most common of the four types of crime, our estimates suggest that counties that had a one standard deviation higher exposure to the Executive Order experienced about a 0.78 higher change in the rate of burglaries per 100,000 population. Based on our 95% confidence interval for this effect, we can rule out the possibility that a one standard deviation higher exposure to the ban led to a change in the burglary rate that was larger than a decrease of 5.5 or an increase of 7.1. These are substantively small changes given that the median burglary rate is about 462. The corresponding confidence intervals for murder, rape, and assault are (-0.14, 0.24), (-1.19, 0.78), and (-2.45, 7.83), respectively.

The results are similar for the delinearized specification. For burglary, the estimate suggests that the differential change between high-receiving counties and those that had no exposure was 8.1 burglaries per 100,000 population. Based on our 95% confidence interval for this effect, we can rule out the possibility that the effect of the Executive Order was larger than a decrease of 14.3 or an increase of 30.4 in the burglary rate. The corresponding confidence intervals for murder, rape, and assault are (-0.47, 0.73), (-3.80, 2.46) and (2.38, 26.15), respectively. Overall, the non-rejected effect sizes are small compared to the median crime rates, which supports an interpretation of the results as meaningful null findings.

Above we presented various checks that support the robustness of these null findings. In particular, we find that the null effects also hold for other types of crime, including theft, motor vehicle theft, and robbery; after log transformations; when using alternative independent variables and when focusing on high crime areas. Additionally, the null findings remain unchanged when we allow for differential changes prior to the Executive Order by interacting the exposure variables with indicators for each year.

6.2 Internal Migration Following Resettlement

How long do refugees resettled to one area reside there before moving elsewhere? If refugees move quickly from their initial destination to other locations, we would likely not expect to find any relationship between initial location of resettlement and crime, or any other outcome for that matter.

Using data published by ORR (2014), we calculated the share of refugees who moved within 24 months of arriving at their initial destination to be between 7% and 10%. This shows that after resettlement refugees do not relocate at high rates, mitigating the concern that the null result is driven by high secondary migration of resettled refugees. Although the data provides estimates at the state-level rather than county-level, mobility among this population is clearly not high.

6.3 Potential Explanations for the Null Finding

There are at least three factors that likely contribute to the minimal impact of reducing refugee resettlement on crime rates in the United States. The first is the selection process of refugees, in which applicants pass through multi-layered vetting that involves multiple agencies running extensive background checks. This may involve several interviews and background checks with numerous different agencies. Successful applicants are often subjected to further screening once they arrive on US soil. In addition, refugees are typically selected on vulnerability-based criteria, which prioritize people with injuries and other forms of hardship. Given this selection process, it appears likely that admitted refugees are on average no more prone to engage in criminal activity than the general native population.

The second factor involves the scale of refugee resettlement. Historically, up until the Executive Order, the United States resettled more refugees each year than the rest of the world combined. In terms of refugee resettlement scale, USRAP has been by far the largest single program. Hence, from the perspective of a policymaker concerned with using refugee resettlement to reduce crime rates, this policy reversal presents about as large and drastic of a change as virtually possible. Nevertheless, the admitted refugees make up a small fraction of the United States population. Given this, the impact of refugees on the crime rate is likely to be limited compared to the impact of the native population. This being said, in the previous subsection we documented that our estimates are precisely estimated around zero and hence lack of statistical power is not an issue in our setting.

Third, the demographic composition of people resettled to the United States differs from that of asylum seekers in Europe. The recent group of asylum seekers in Germany consists predominantly of young men, the demographic group that is considered at highest risk to commit crimes (Freeman, 1999). For example, in 2016, 34% of asylum seekers in Germany were men between the ages of 18 and 35 (Eurostat, 2018a). In contrast, approximately 14% of the refugees resettled to the United States in 2016 were men within a similar age range.

7 Conclusion

In recent years policymakers have grown increasingly concerned about a potential link between refugees and crime. In response, Western host countries have reduced refugee admissions. In this study we leverage a major policy reversal in the United States—Executive Order #13769—as a natural experiment to examine whether halting refugee resettlement reduced local crime rates. The ban triggered a reduction in refugee arrivals that was unprecedented in scale and uncorrelated with preexisting local crime trends. This design enables us to improve on existing work in isolating the effect of reducing refugee resettlement from other confounding characteristics.

We find that despite a 65.6% overall drop in refugee arrivals, the Executive Order had no discernible impact of on local crime rates. Instead, the estimates suggest that the reduction in refugee arrivals had a precisely estimated null effect on crime rates, and this result is robust across different types of crime and alternative specifications. Furthermore, we showed that the null effect is precisely estimated. In other words, crime rates would have been similar had arrivals continued at pre-Executive Order levels. In the light of several recent studies from Europe suggesting that refugee arrivals cause a modest rise in crime rates (Gehrsitz and Ungerer, 2017; Dehos, 2017; Lange and Sommerfeld, 2018), our null findings contribute to this small literature about an understudied, yet politically salient, immigrant group.

From a policymaker's perspective, given the scale of USRAP relative to resettlement programs throughout the world, the 2016-2017 reduction in arrivals constitutes virtually as sizable of a change as practically possible. Our null findings have, therefore, important implications for refugee policy, indicating that restricting resettlement to the United States is not an effective policy tool for reducing crime.

Our study is not without limitations. Given that our data ends in 2017 as information for 2018 is not yet available, we are only able to examine the short-term effects of the policy reversal. Moreover, our results are limited to the context of the United States resettlement program and might not apply to European countries, where most refugees enter initially as asylum seekers after crossing the border. Further research on this topic is needed to further develop the evidence base about how refugees affect receiving communities.

References

Adelman, Robert, Lesley Williams Reid, Gail Markle, Saskia Weiss, and Charles Jaret, "Urban crime rates and the changing face of immigration: Evidence across four decades," *Journal of ethnicity in criminal justice*, 2017, *15* (1), 52–77.

Amuedo-Dorantes, Catalina, Cynthia Bansak, and Susan Pozo, "Refugee Admissions and Public Safety: Are Refugee Settlement Areas More Prone to Crime?," *IZA Discussion Paper*, 2018, *11612.*

Bell, Brian, Francesco Fasani, and Stephen Machin, "Crime and immigration: Evidence from large immigrant waves," *Review of Economics and statistics*, 2013, *21* (3), 1278–1290.

Bianchi, Milo, Paolo Buonanno, and Paolo Pinotti, "Do immigrants cause crime?," *Journal of the European Economic Association*, 2012, *10* (6), 1318–1347.

Butcher, Kristin F and Anne Morrison Piehl, "Cross-city evidence on the relationship between immigration and crime," *Journal of Policy Analysis and Management*, 1998, *17* (3), 457–493.

Chalfin, Aaron, "What is the contribution of Mexican immigration to US crime rates? Evidence from rainfall shocks in Mexico," *American Law and Economics Review*, 2013, *16* (1), 220–268.

Connor, Phillip and Jens Manuel Krogstad, "For the first time, U.S. resettles fewer refugees than the rest of the world," *Office for Refugee Resettlement*, 2018.

Dehos, Fabian T, "The refugee wave to Germany and its impact on crime," Technical Report, Ruhr Economic Papers 2017.

Dustmann, Christian, Francesco Fasani, Tommaso Frattini, Luigi Minale, and Uta Schönberg, "On the economics and politics of refugee migration," *Economic policy*, 2017, *32* (91), 497–550.

Eurostat, "Asylum Seeker Data. http://appsso.eurostat.ec.europa.eu/nui/submitViewTableAction.do. Accessed December 16, 2018.," 2018.

_ , "Asylum Statistics.," 2018.

FBI, "Crime in the United States. ," 2018.

Freeman, Richard B, "The economics of crime," *Handbook of labor economics*, 1999, *3*, 3529–3571.

Gehrsitz, Markus and Martin Ungerer, "Jobs , Crime , and Votes : A Short-run Evaluation of the Refugee Crisis in Germany.," 2017, (10494).

Kaplan, Jacob, "Uniform Crime Reporting Program Data: Offenses Known and Clearances by Arrest, 1960-2017 ," 2018.

Lange, Martin and Katrin Sommerfeld, "Causal Effects of Immigration on Crime: Quasi-Experimental Evidence from a Large Inflow of Asylum Seekers.," *Working paper*, 2018.

Lee, Matthew T, Ramiro Martinez, and Richard Rosenfeld, "Does immigration increase homicide? Negative evidence from three border cities," *The Sociological Quarterly*, 2001, *42* (4), 559–580.

Manson, Steven, Jonathan Schroeder, David Van Riper, and Steven Ruggles, "IPUMS National Historical Geographic Information System: Version 13.0 [Database], University of Minnesota," 2018.

Miles, Thomas J and Adam B Cox, "Does immigration enforcement reduce crime? evidence from secure communities," *The Journal of Law and Economics*, 2014, *57* (4), 937–973.

of State, US Department, "Refigee Admissions," 2018.

ORR, "Statistical Abstract for Refugee Resettlement Stakeholders," *Office for Refugee Resettlement*, 2014.

Ousey, Graham C and Charis E Kubrin, "Immigration and crime: Assessing a contentious issue," *Annual Review of Criminology*, 2018, *1*, 63–84.

Piopiunik, Marc and Jens Ruhose, "Immigration, regional conditions, and crime: Evidence from an allocation policy in Germany," *European Economic Review*, 2017, *92*, 258–282.

Portes, Alejandro and Rubén G Rumbaut, *Immigrant America: a portrait*, Univ of California Press, 2006.

Shihadeh, Edward S and Raymond E Barranco, "Latino employment and Black violence: The unintended consequence of US immigration policy," *Social Forces*, 2010, *88* (3), 1393–1420.

Simes, Jessica T and Mary C Waters, "The politics of immigration and crime," *The Oxford Handbook of Ethnicity, Crime, and Immigration*, 2014, pp. 476–478.

Spenkuch, Jörg L, "Understanding the impact of immigration on crime," *American law and economics review*, 2013, *16* (1), 177–219.

Trump, Donald J., "Executive Order #13769.," 2018.

UNHCR, "UNHCR news. https://www.unhcr.org/news/press/2017/4/58fe15464/canadas-2016-record-high-level-resettlement-praised-unhcr.html. Accessed December 10, 2018.," 2017.

_ , "Figures at a Glance. https://www.unhcr.org/figures-at-a-glance.html. Accessed December 10, 2018.," 2018.

— , "Projected Global Resettlement Needs 2019.," 2018.

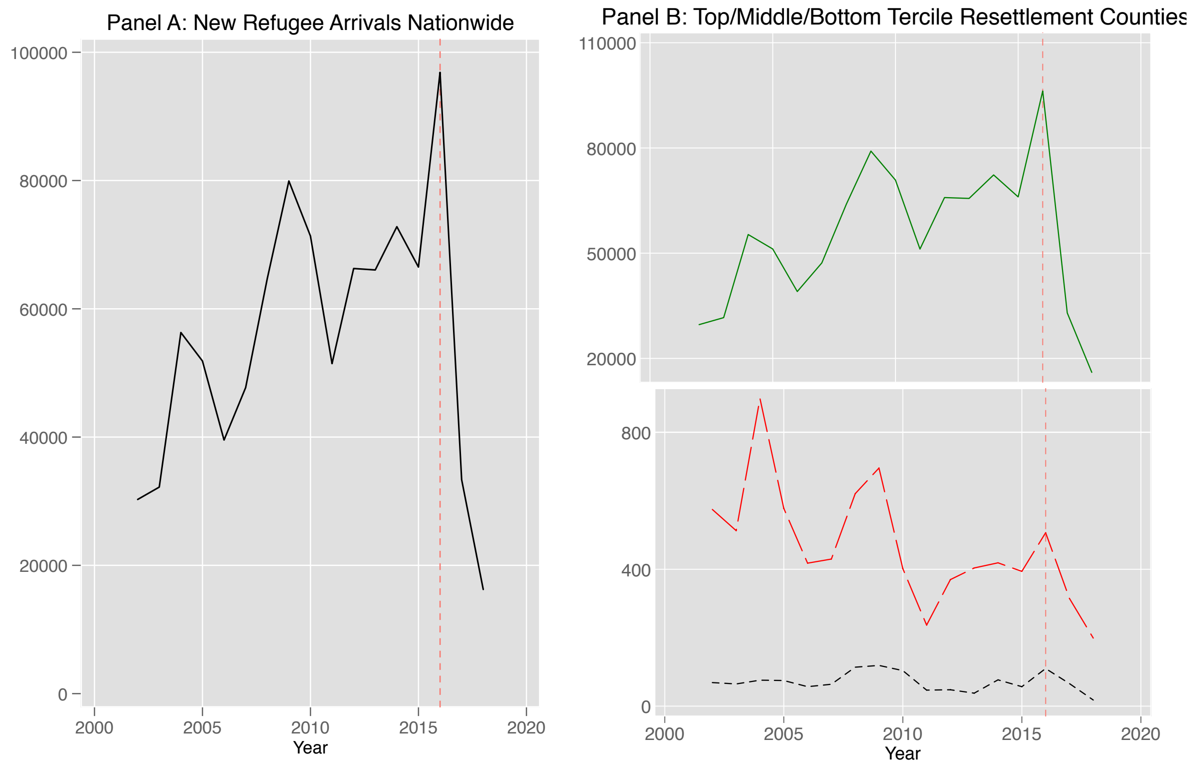
Waters, Mary C, Reed Ueda, and Helen B Marrow, *The new Americans: A guide to immigration since 1965*, Harvard University Press, 2009.

WRAPS, "Worldwide Refugee Admissions Processing System database. Accessed on October 10, 2018.," 2018.

Zhang, Haimin, "Immigration and Crime: Evidence from Canada," Technical Report, Vancouver School of Economics 2014.

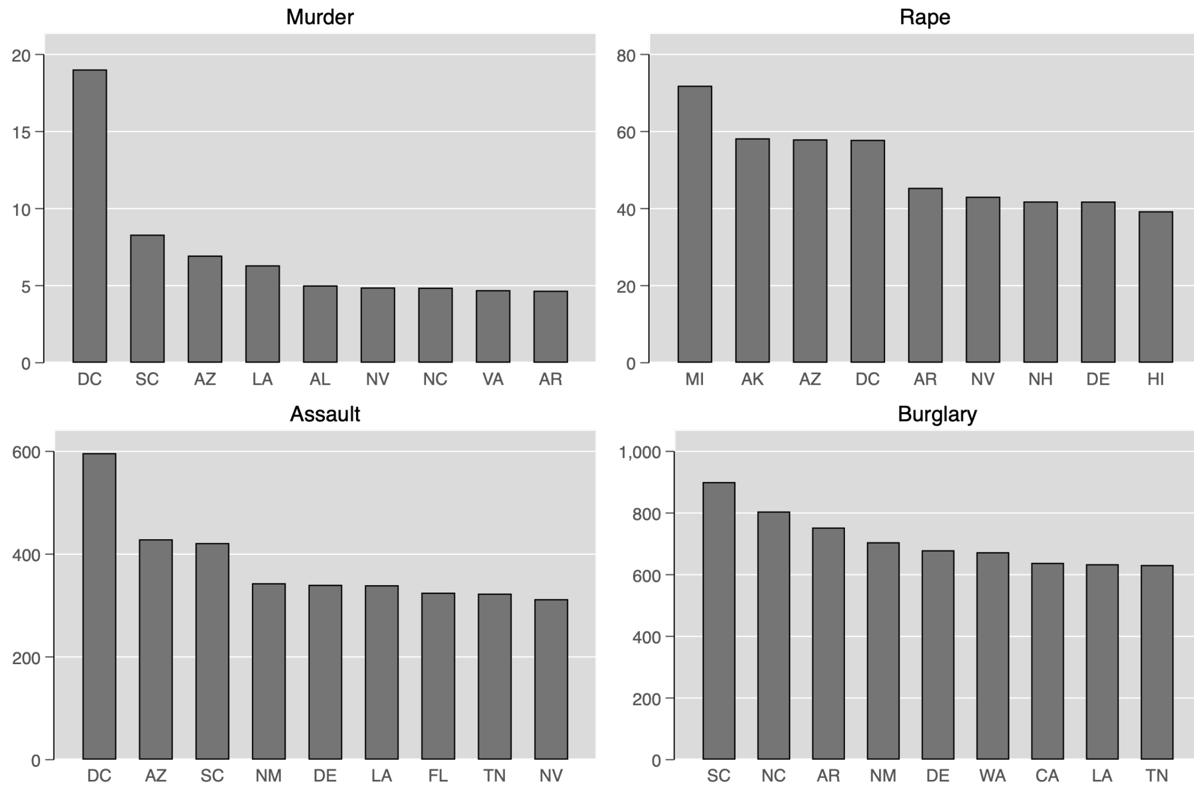
Figures and Tables

Figure 1: Research Design: Comparing Counties with Low and High Exposure to Executive Order #13769.



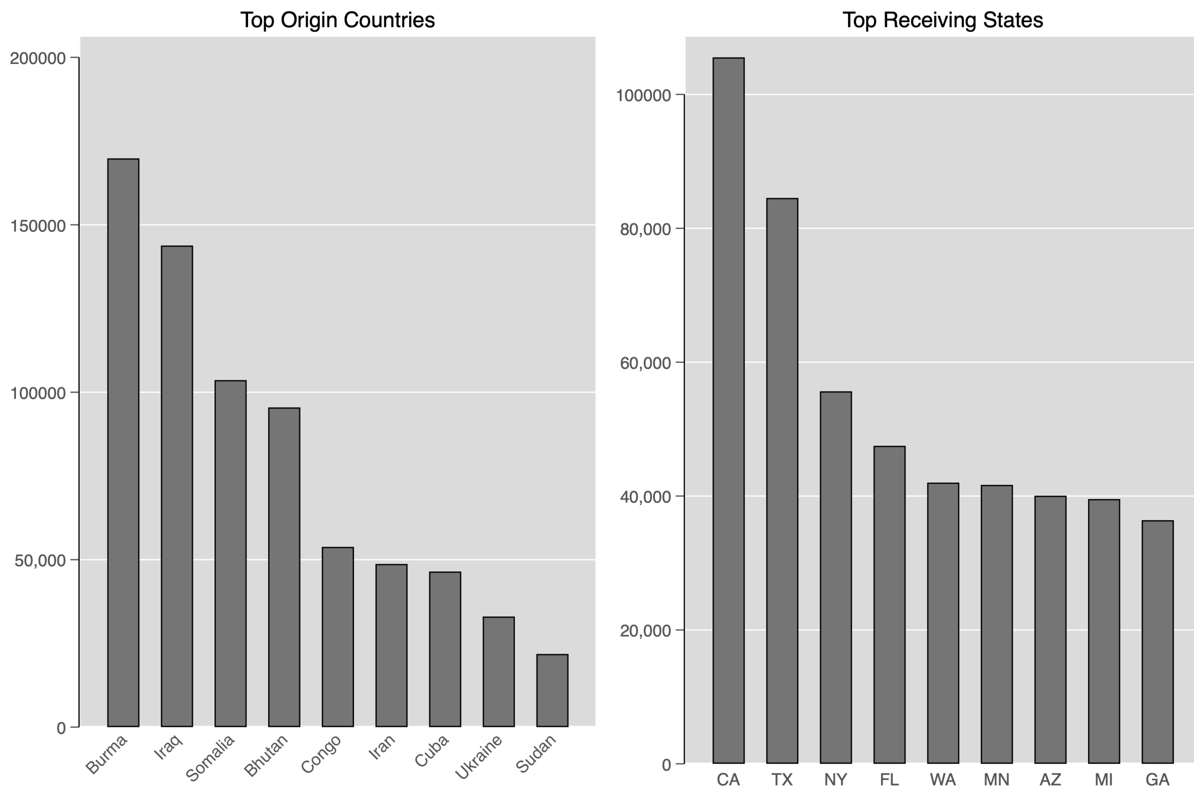
Notes: Panel A shows that refugee arrivals dropped nationwide in early 2017 due to the Executive Order. Panel B visualizes the reduction in arrivals was much larger in counties that received the most refugees prior to the ban. Green (solid), red (long dashed), and black (short dashed) lines indicate average number of arrivals for counties that are in the top, middle, and bottom tercile in terms of arrivals between 2002 and 2016.

Figure 2: States with Highest Average Crime Rates per 100,000 People, 2010–2017



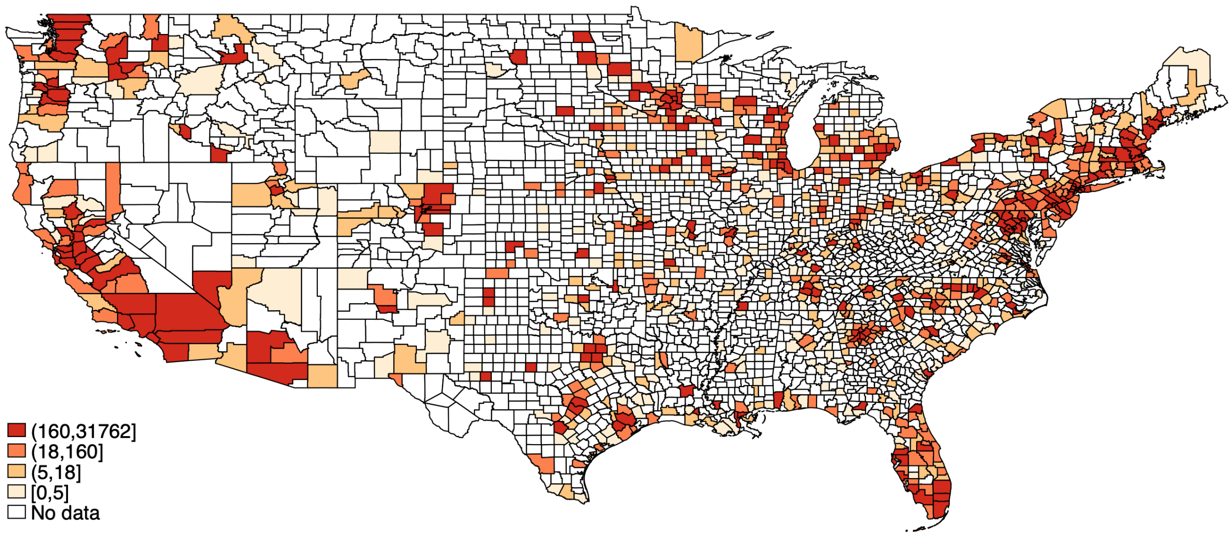
Notes: All numbers reflect 2010–2017 averages.

Figure 3: Origins and Destinations for Refugees in the United States, 2002–2017



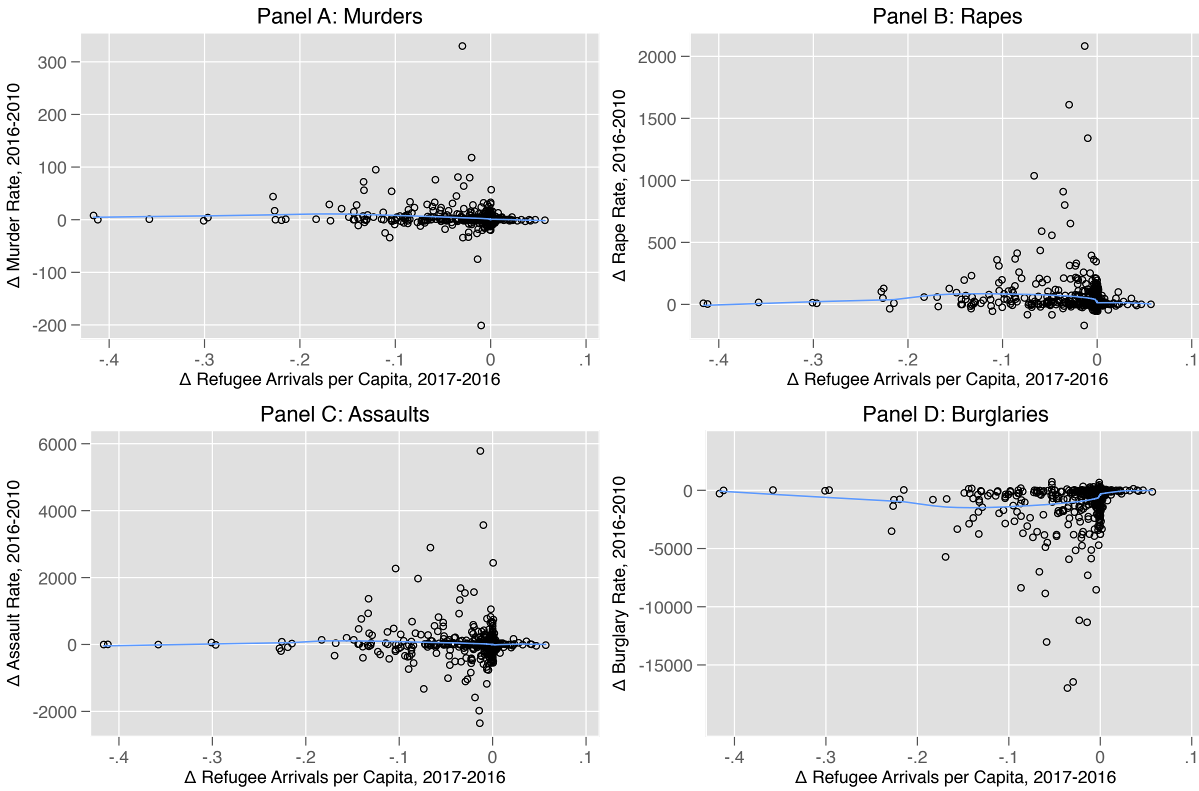
Notes: List of the ten largest refugee sending countries (left panel) and the top ten receiving states (right panel). All numbers reflect 2002–2017 aggregate values.

Figure 4: Cumulative Refugee Arrivals in the United States, 2002–2017



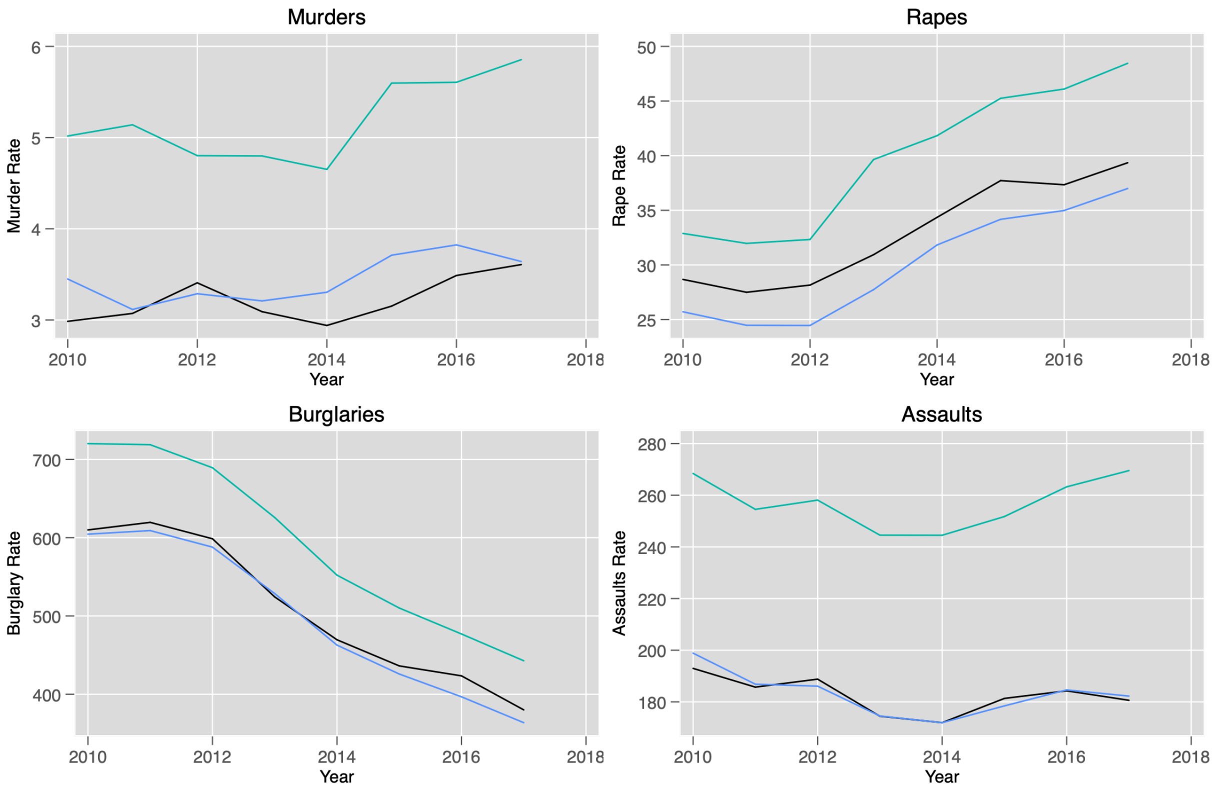
Notes: Cumulative refugee arrivals in the United States for the period 2002–2017. Each observation is a county. Darker shades of red correspond to higher number of refugee resettled.

Figure 5: Pre-ban Crime Trends and Drop in Refugee Arrivals: Main Crime Types



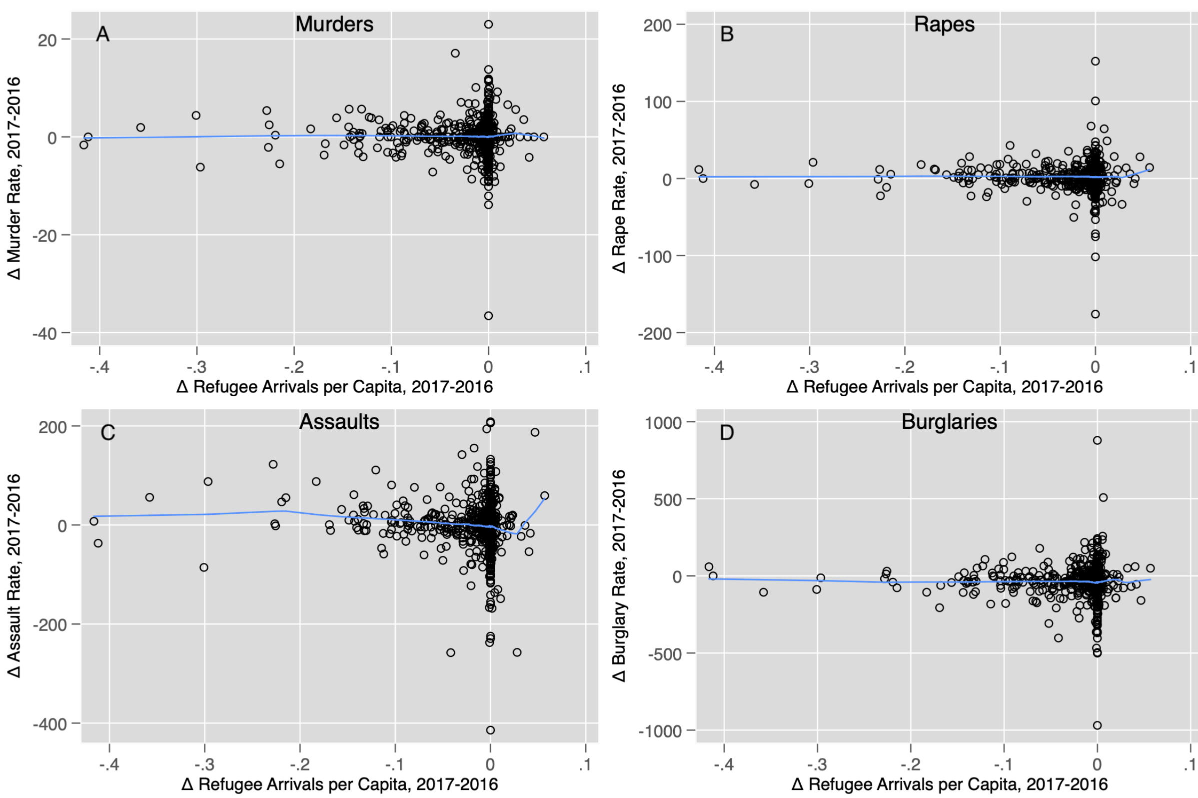
Notes: Crime trends between 2010 and 2016 and drop in refugee arrivals due to the Executive Order by crime type. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure 6: Crime Trends by High/Low/Very Low Receiving Counties: Main Crime Types



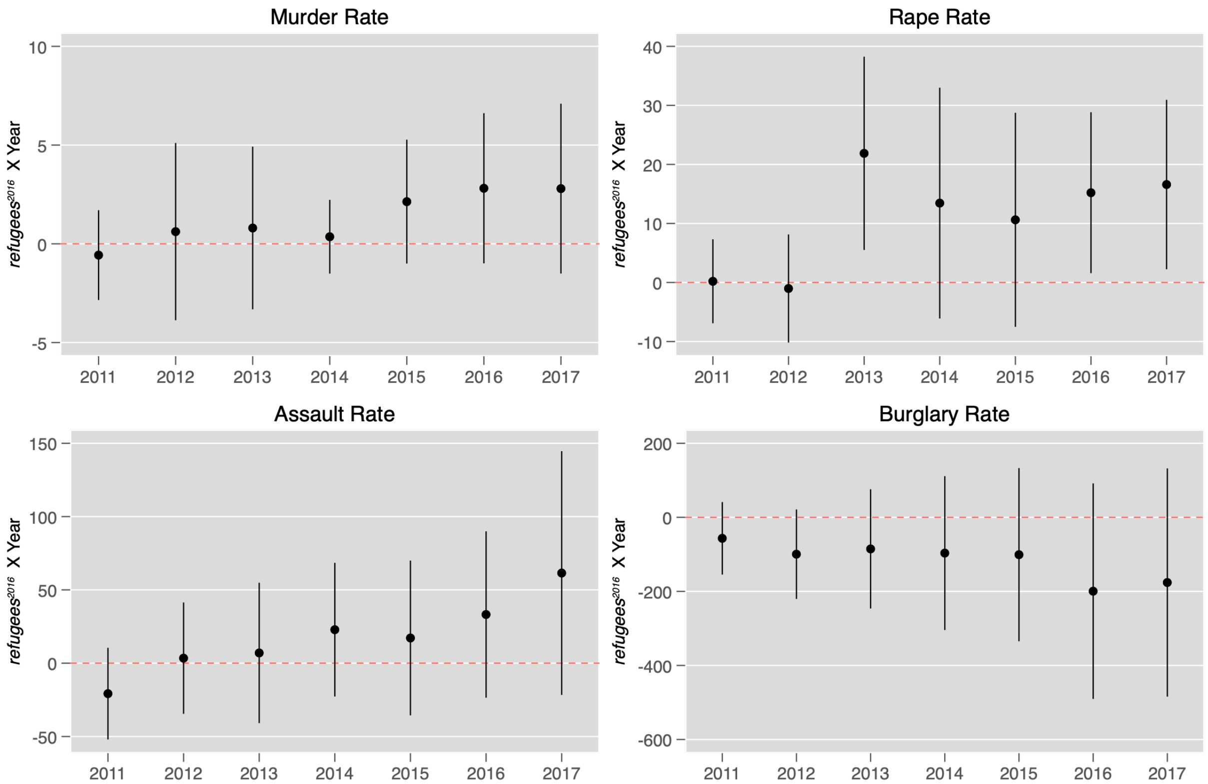
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are that received no refugees in 2016. The other two groups are split in two groups of equal size – above median are high receiving counties and below median are low receiving ones.

Figure 7: The Effect of the Executive Order on Local Crime Rates.



Notes: Plots show the relationship between the 2016–2017 drop in refugee resettlement due to the Executive Order and the 2016–2017 changes in crime rates across counties.

Figure 8: Generalized Continuous Difference-in-Differences Results: Main Crime Types



Notes: Estimated regression coefficients of year dummies interacted with number of refugee arrivals in 2016 per 100 people from a generalized continuous difference-in-differences model. See the text in the SM for details on the regression specification. The outcome variable is expressed in crime rate per 100,000 population. The sample size is 6,296. Standard errors are clustered by county and 95% confidence intervals are standardized by population.

Table 1: Descriptive Statistics for Crime, Refugee Arrivals, and Population

	Mean	Median	SD	Min	Max	Observations
			Crime Variables			
Murder rate	3.81	2.51	4.97	0	64.87	6296
Rape rate	34.05	29.51	24.50	0	320.92	6296
Assault rate	202.85	168.80	162.31	0	1980.41	6296
Burglary rate	527.87	462.06	329.08	0	2251.21	6296
Theft rate	1749.08	1634.87	836.60	0	7392.95	6296
Robbery rate	66.89	40.67	86.75	-3	1267.90	6296
Motor vehicle theft rate	162.12	115.72	151.21	0	1338.42	6296
Log number of murders	1.86	1.61	1.41	0	6.73	4962
Log number of robberies	4.14	4.06	2.03	0	9.99	5981
Log number of assaults	5.35	5.38	1.69	0	10.40	6240
Log number of burglaries	6.40	6.47	1.57	0	10.81	6257
Log number of thefts	7.66	7.78	1.57	0	11.97	6265
Log number of rapes	3.69	3.69	1.45	0	8.37	6147
Log number of motor vehicle thefts	5.10	5.03	1.77	0	10.77	6229
			Resettlement Variables			
Refugees arrivals	83.34	1.00	265.65	0	3474.00	6296
Refugee arrivals per 100 people	0.02	0.00	0.07	0	1.78	6296
Log number of refugees	3.10	2.71	2.16	0	8.15	3212
Population (in 100,000s)	3.10	1.41	5.84	0	100.57	6296

Notes: Crime rates are expressed in absolute number of crimes per 100,000 people. The unit of observation is a county and the time period is 2010–2017.

Table 2: Pre-ban Crime Trends: Regression Results

	(1)	(2)	(3)	(4)
Murder rate growth	-0.000	-0.001	0.001	0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Rape rate growth	0.004*	0.005*	0.002	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
Assault rate growth	0.001	0.001	0.001**	0.000
	(0.000)	(0.000)	(0.000)	(0.000)
Burglary rate growth	-0.029	-0.027	-0.030	-0.030
	(0.021)	(0.024)	(0.019)	(0.020)
Theft rate growth	0.000	-0.018	0.005	-0.005
	(0.013)	(0.013)	(0.015)	(0.015)
Robbery rate growth	-0.003	-0.002	-0.001	-0.002
	(0.005)	(0.006)	(0.004)	(0.005)
Motor vehicle theft rate growth	0.010	0.013*	0.009	0.010
	(0.005)	(0.006)	(0.006)	(0.007)
Observations	602	602	602	602
Adjusted R^2	-0.000	0.050	0.149	0.199
County Controls			X	X
State Fixed Effects		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the Supplementary Materials for details on the regression specification. The outcome variable is 2016 refugee arrivals per (100) capita. Crime growth rates reflect 2010–2016 values. The unit of observation is a county. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table 3: First-Differences Results: Main Crime Types

	Murder	Murder	Rape	Rape	Assault	Assault	Burglary	Burglary
$\Delta refugee^{2017-2016}$	-0.800	-2.160	-4.026	-8.830	-91.676	-85.292	-23.884	12.403
	(2.532)	(2.194)	(11.978)	(12.943)	(48.360)	(50.465)	(51.275)	(55.687)
N	787	787	787	787	787	787	787	787
R-sq	0.000	0.040	0.000	0.066	0.007	0.105	0.000	0.065
$\bar{Y}$	0.073	0.073	2.104	2.104	-0.612	-0.612	-38.091	-38.091
sd(Y)	3.456	3.456	16.446	16.446	49.443	49.443	103.633	103.633
State FE		X		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the 2016–2017 change in refugee arrivals per 100 population. The unit of observation is a county. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table 4: Difference-in-Differences Results for the Effect of the Executive Order on Local Crime Rates.

	Murder	Rape	Assault	Burglary
Panel A: Linear Specification				
Difference-in-Differences	0.734	-2.918	38.410	11.245
	(1.392)	(7.146)	(37.420)	(45.656)
Panel B: Delinearized Specification				
Low Receiving Counties	-0.379	0.413	4.516	1.662
	(0.282)	(1.434)	(5.260)	(10.584)
High Receiving Counties	0.132	-0.669	14.266**	8.070
	(0.304)	(1.594)	(6.053)	(11.374)
Observations	6296	6296	6296	6296
Mean Crime Rate	3.814	34.049	202.847	527.871
SD Crime Rate	4.972	24.502	162.314	329.079
County Trends	X	X	X	X

Notes: Each entry presents the difference-in-differences estimate comparing crime rates in counties with a high and low exposure to the Executive Order. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 5: Continuous Difference-in-Differences Results: Main Crime Types, Using Actual Drop in Refugees

	Murder	Murder	Rape	Rape	Assault	Assault	Burglary	Burglary
$\Delta refugees^{2016-2017} \times \mathbf{1}(t = 2017)$	6.958**	2.046	15.013	-6.525	-238.005*	42.120	137.081***	83.795
	(3.520)	(2.581)	(10.364)	(14.890)	(130.133)	(93.112)	(52.181)	(56.543)
N	6296	6296	6296	6296	6296	6296	6296	6296
$\bar{Y}$	3.814	3.814	34.049	34.049	527.871	527.871	202.847	202.847
sd(Y)	4.972	4.972	24.502	24.502	329.079	329.079	162.314	162.314
County Trends		X		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the interaction of a dummy for year 2017 and county-level 2016–2017 change in refugee arrivals. The unit of observation is a county–year and the time period is 2010–2017. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

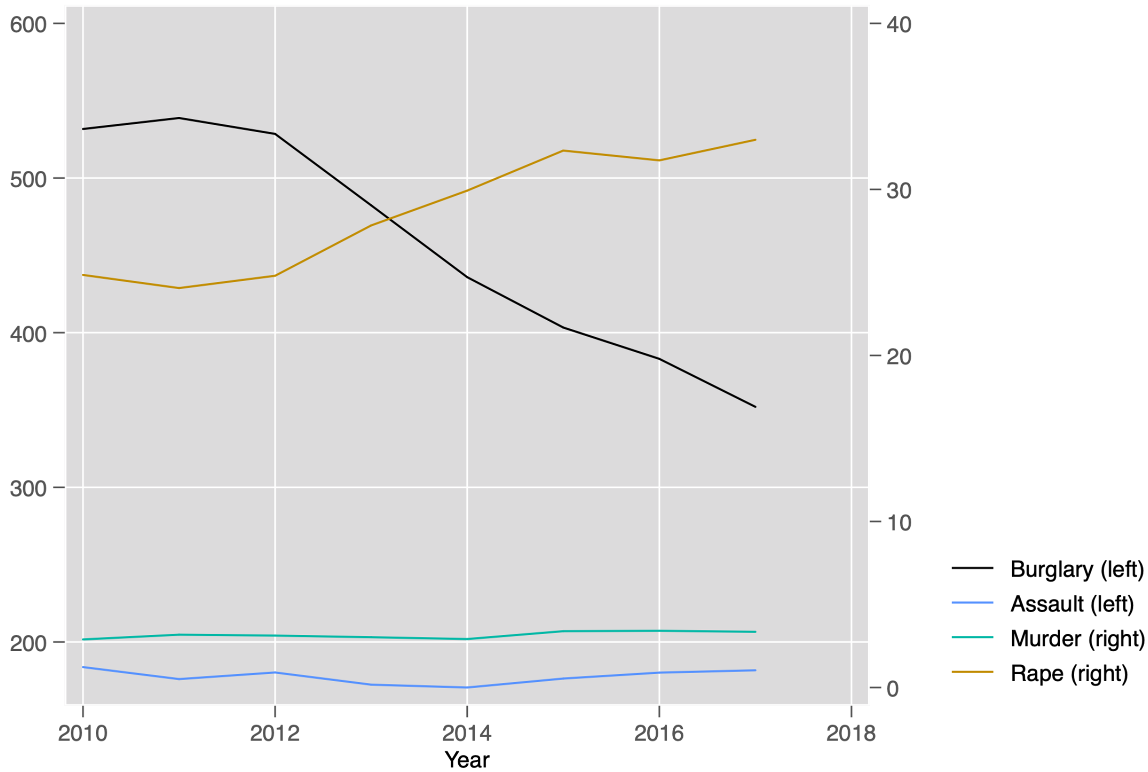
Table 6: Continuous Difference-in-Differences Results: Main Crime Types, High Crime Areas

	Murder	Murder	Rape	Rape	Assault	Assault	Burglary	Burglary
$refugees^{2016} \times \mathbf{1}(t = 2017)$	7.035**	1.553	9.069	-13.292	143.428**	108.457**	-132.959	73.941
	(3.471)	(1.940)	(11.058)	(14.117)	(55.489)	(50.481)	(129.870)	(74.309)
N	3144	3144	3144	3144	3144	3144	3144	3144
R-sq	0.890	0.961	0.821	0.971	0.946	0.991	0.907	0.992
$\bar{Y}$	5.185	5.185	36.537	36.537	259.504	259.504	635.131	635.131
sd(Y)	5.680	5.680	22.037	22.037	182.721	182.721	339.944	339.944
County Trends		X		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the interaction of a dummy for year 2017 and county-level refugee arrivals in 2016 per 100 population. The unit of observation is a county–year and the time period is 2010–2017. The sample is restricted to counties with above median total number of crimes for the entire sample period. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

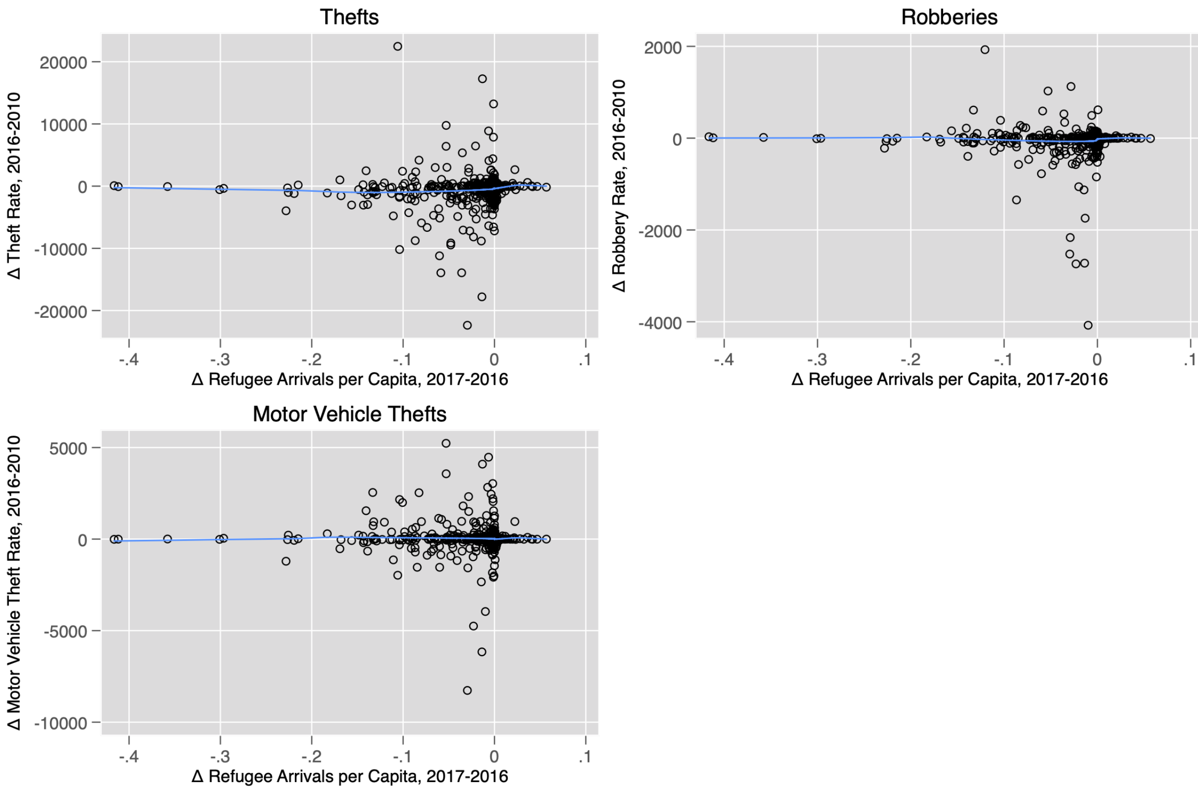
Appendix

Figure A1: National Crime Rates per 100,000 People



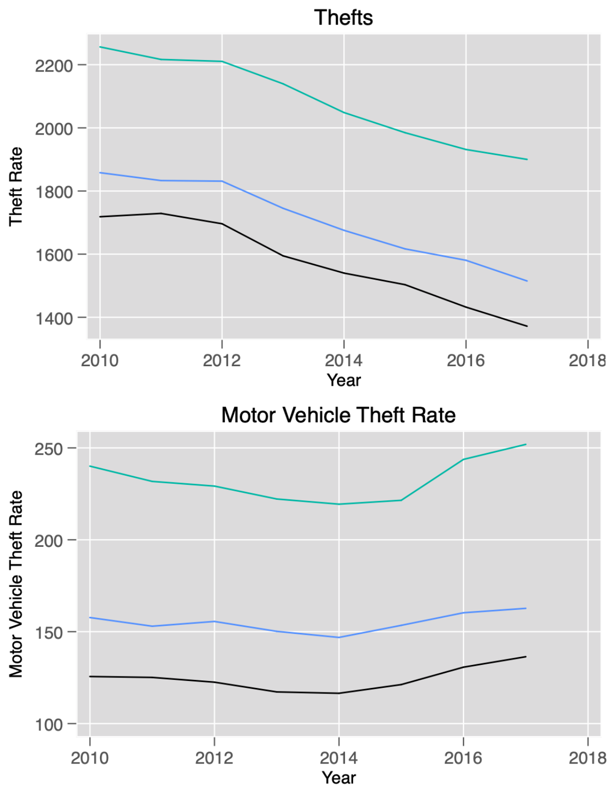
Notes: Aggregate crime rates in the United States by crime type in the period 2010–2017.

Figure A2: Pre-ban Crime Trends and Drop in Refugee Arrivals: Additional Crime Types



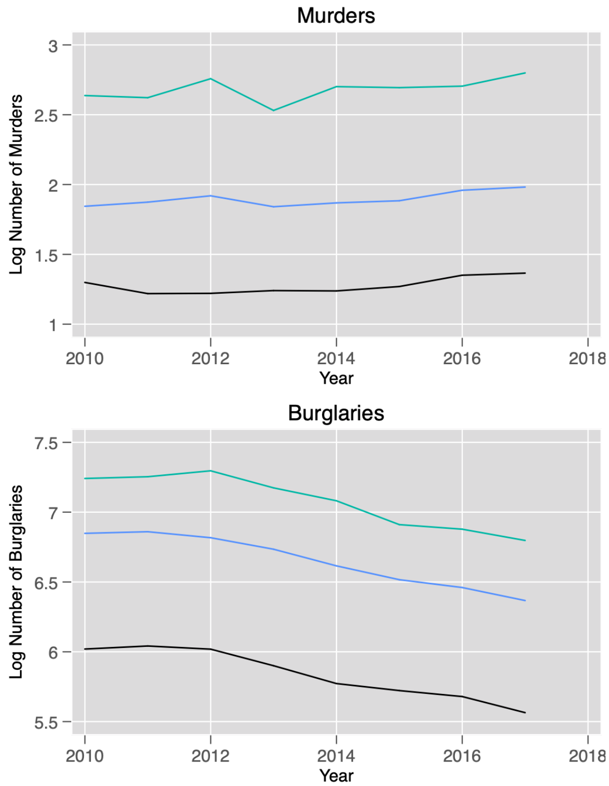
Notes: Crime trends between 2010 and 2016 and drop in refugee arrivals due to the Executive Order by crime type. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure A3: Crime Trends by High/Low/Very Low Receiving Counties: Additional Crime Types



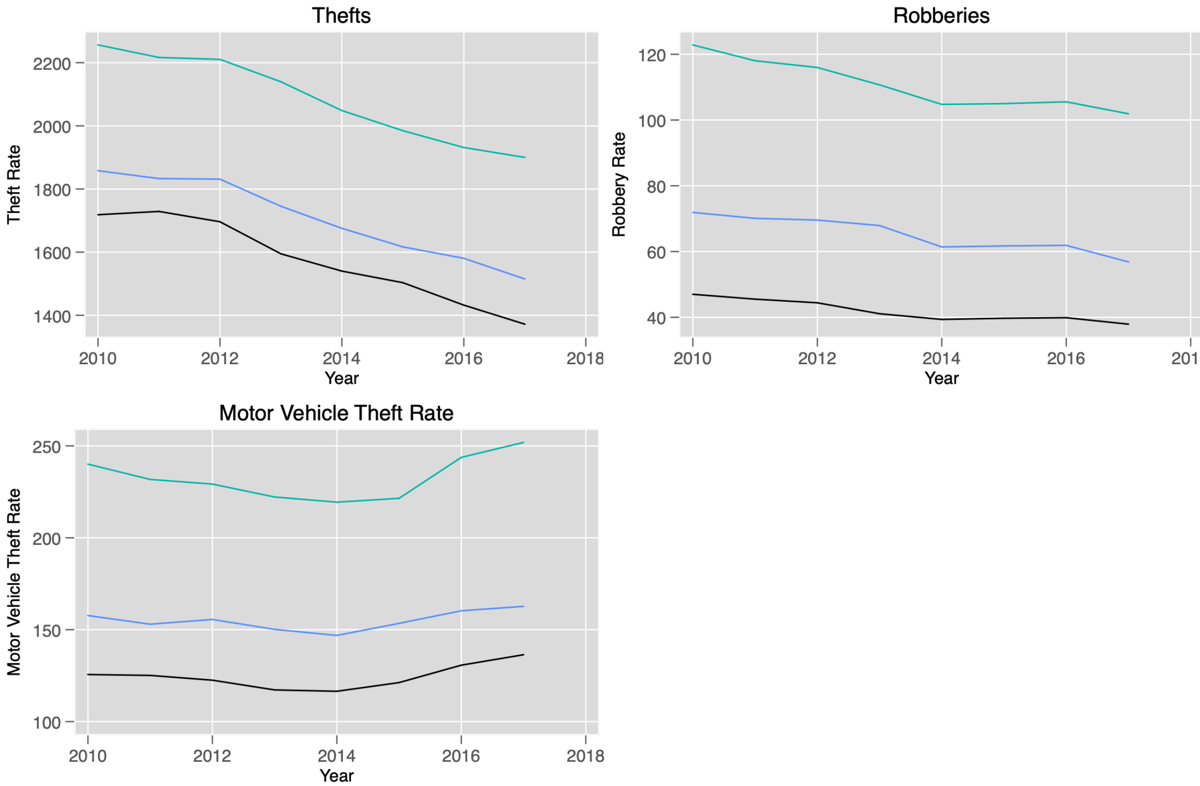
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are ones with no refugee arrivals in 2016. The other two groups are split in two groups of equal size – above median are high receiving counties and below median are low receiving ones.

Figure A4: Crime Trends by High/Low/Very Low Receiving Counties: Main Crime Types, Logs



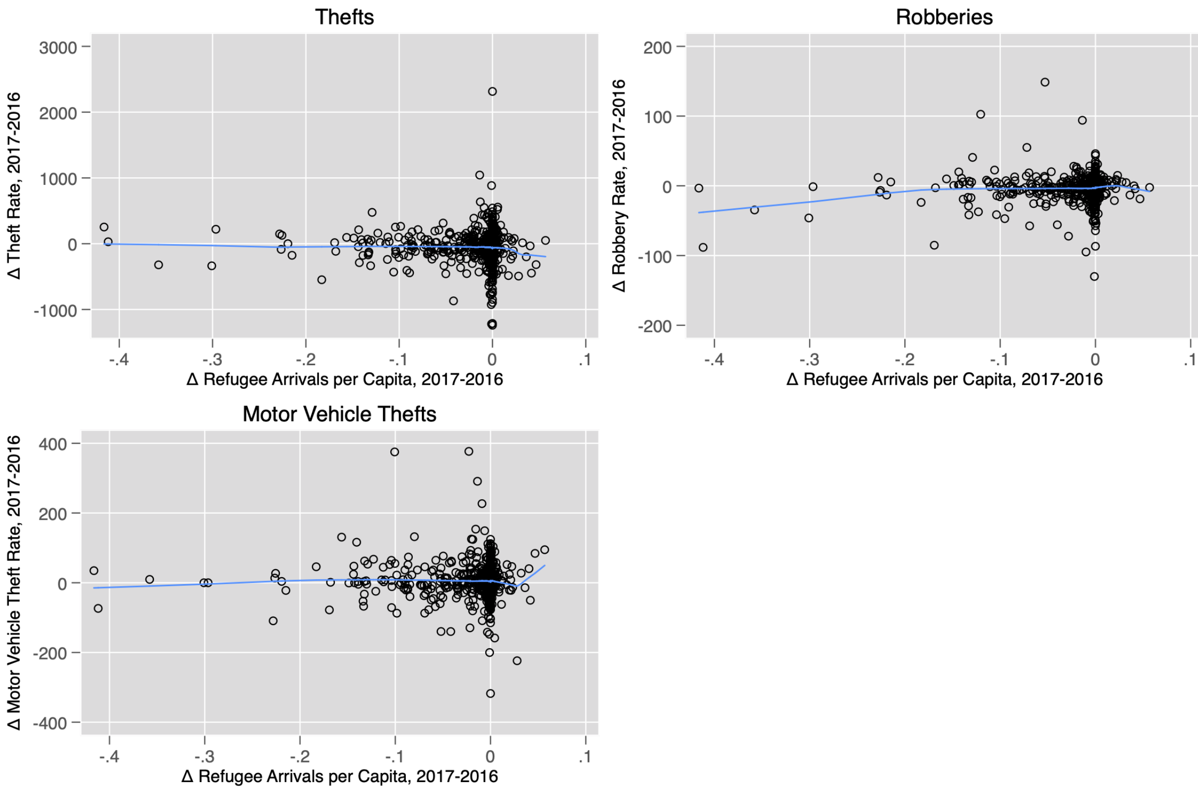
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are ones with no refugee arrivals in 2016. The other two groups are split in two groups of equal size – above median are high receiving counties and below median are low receiving ones.

Figure A5: Crime Trends by High/Low/Very Low Receiving Counties: Additional Crime Types, Logs



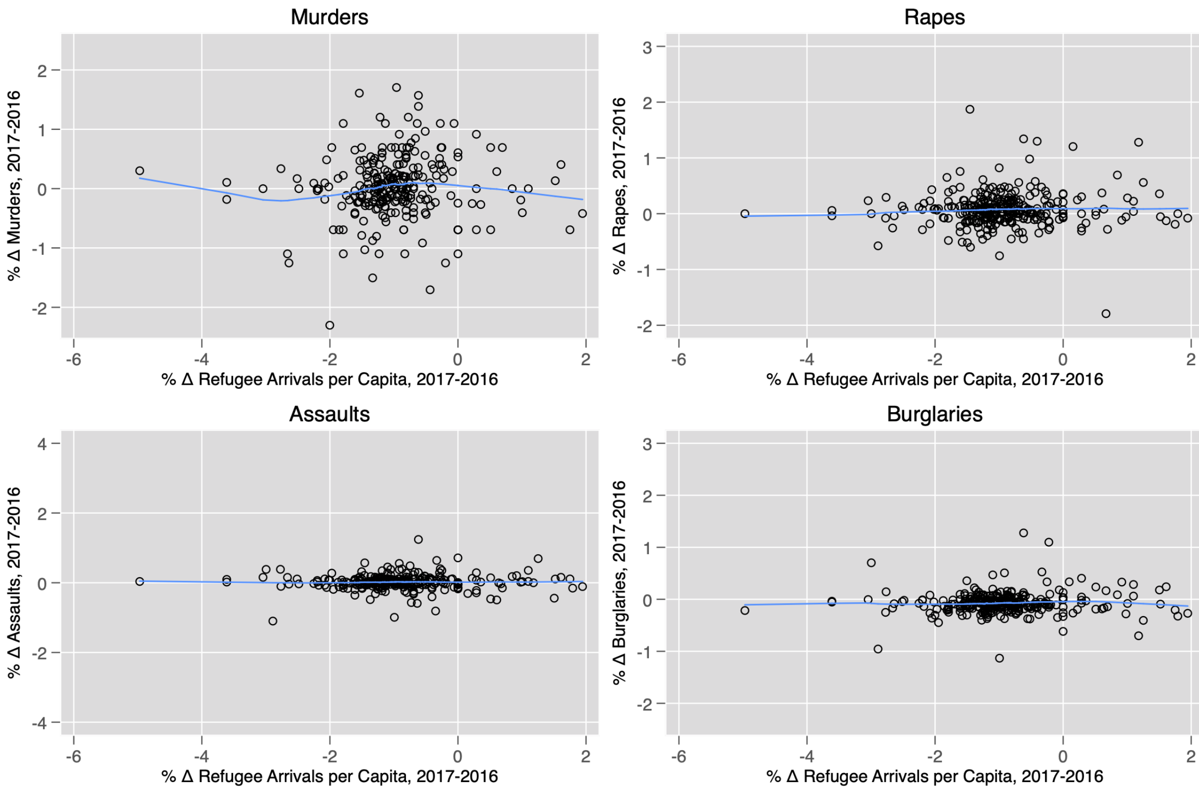
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are ones with no refugee arrivals in 2016. The other two groups are split in two groups of equal size – above median are high receiving counties and below median are low receiving ones.

Figure A6: First-Differences Results: Additional Crime Types



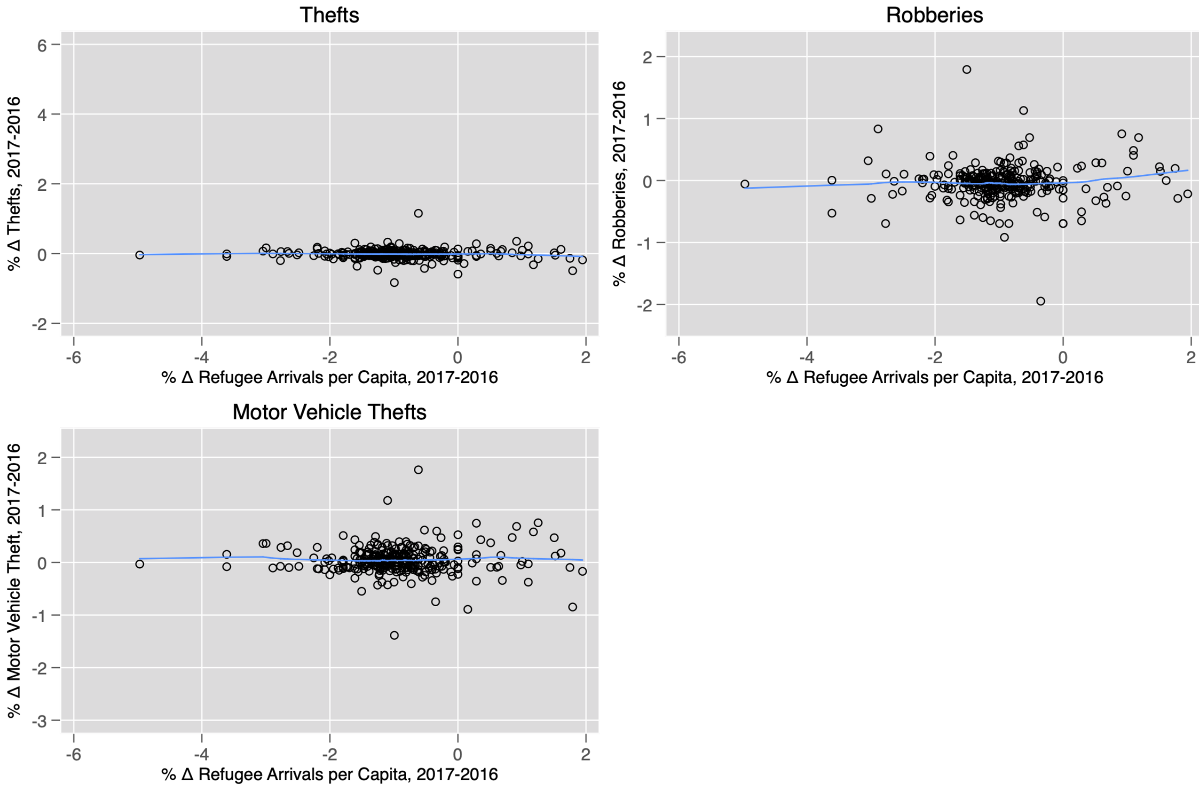
Notes: Scatter plot of 2016–2017 change in refugee arrivals per 100 population and 2016–2017 changes in crime rate per 100,000 people. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure A7: First-Differences Results: Main Crime Types, Logs



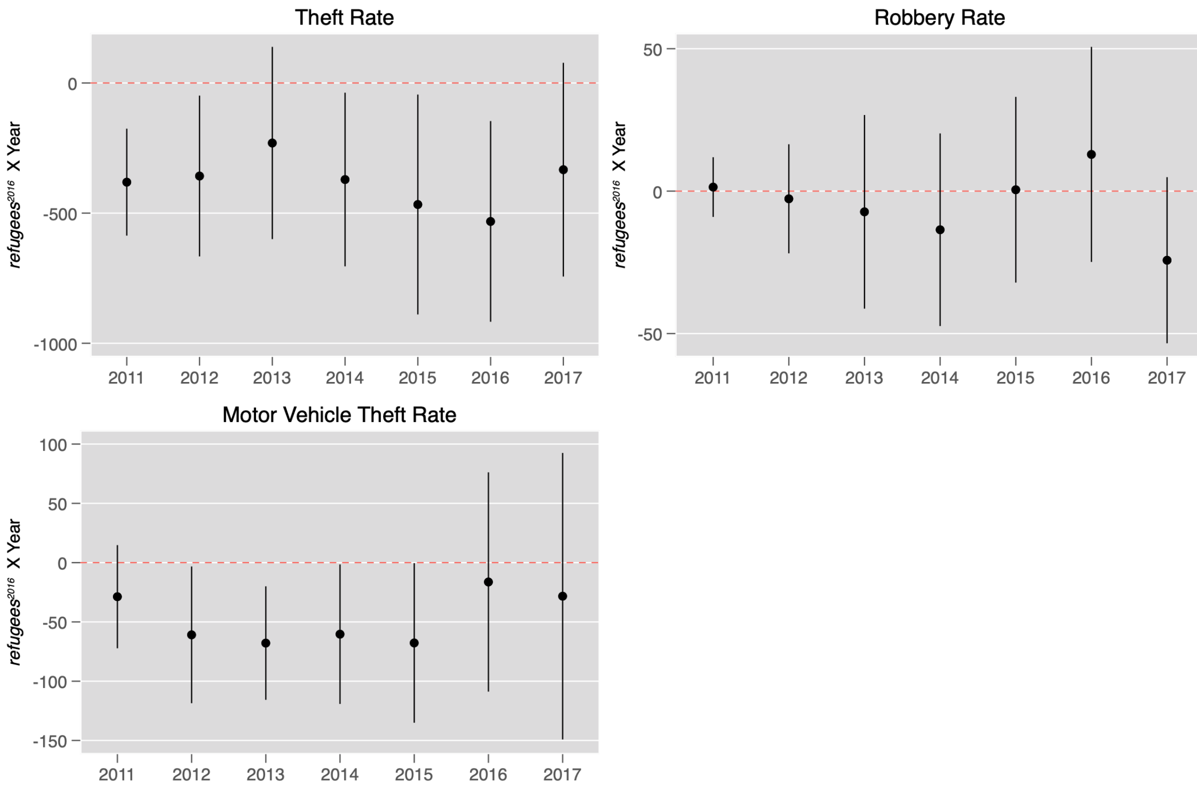
Notes: Scatter plot of 2016–2017 percent change in refugee arrivals and 2016–2017 percent changes in absolute crimes. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure A8: First-Differences Results: Additional Crime Types, Logs



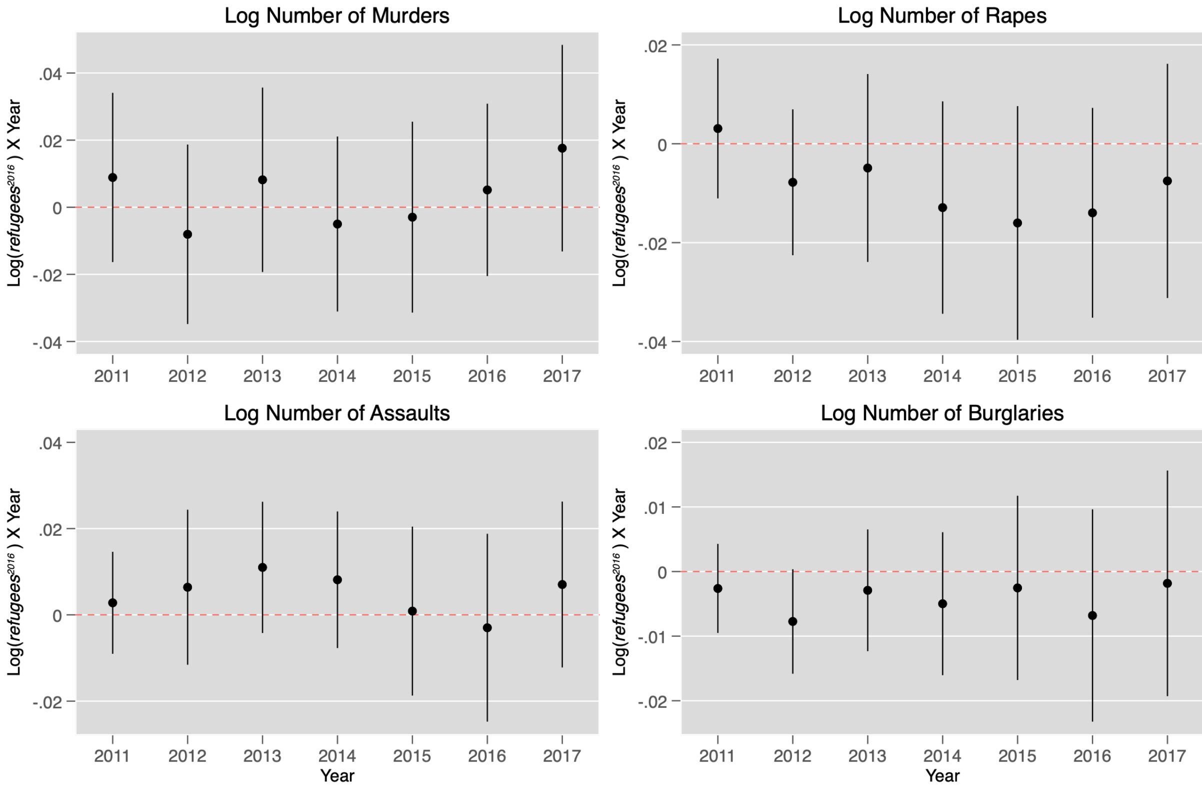
Notes: Scatter plot of 2016–2017 percent change in refugee arrivals and 2016–2017 percent changes in absolute crimes. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure A9: Generalized Continuous Difference-in-Differences Results: Additional Crime Types



Notes: Estimated regression coefficients of year dummies interacted with number of refugee arrivals in 2016 per 100 people from a generalized continuous difference-in-differences model. See the text in the SM for details on the regression specification. The outcome variable is expressed in crime rate per 100,000 population. The sample size is 6,296. Standard errors are clustered by county and 95% confidence intervals are standardized by population.

Figure A10: Generalized Continuous Difference-in-Differences Results: Main Crime Types, Logs

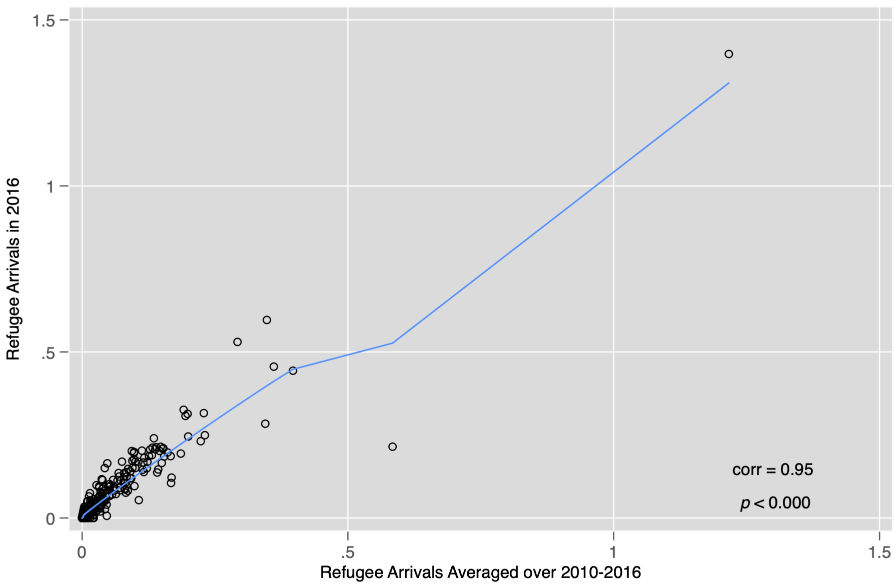


Notes: Estimated regression coefficients of year dummies interacted with log number of refugee arrivals in 2016 from a generalized continuous difference-in-differences model. See the text in the SM for details on the regression specification. The outcome variable is expressed in log absolute number of crimes. The sample size varies by crime type (Table 1). Standard errors are clustered by county and 95% confidence intervals are shown as vertical lines.

Figure A11: Generalized Continuous Difference-in-Differences Results: Additional Crime Types, Logs

Notes: Estimated regression coefficients of year dummies interacted with log number of refugee arrivals in 2016 from a generalized continuous difference-in-differences model. See the text in the SM for details on the regression specification. The outcome variable is expressed in log absolute number of crimes. The sample size varies by crime type (Table 1). Standard errors are clustered by county and 95% confidence intervals are shown as vertical lines.

Figure A12: Treatment Variable Robustness Check



Notes: Scatter plot of refugee resettlement per 100 people in 2016 and aggregated 2010–2016 values. Blue line is local regression (LOESS) fit. Each observation is a single county.

Table A1: First-Differences Results: Additional Crime Types

	Robbery	Robbery	Theft	Theft	Motor Vehicle Theft	Motor Vehicle Theft
$\Delta refugee^{2017-2016}$	39.256	48.152	-163.116	-112.618	-5.143	-1.205
	(31.156)	(31.358)	(181.311)	(176.965)	(41.242)	(46.214)
N	787	787	787	787	787	787
R-sq	0.010	0.221	0.001	0.088	0.000	0.117
$\bar{Y}$	-3.263	-3.263	-53.863	-53.863	5.467	5.467
sd(Y)	17.290	17.290	232.115	232.115	48.177	48.177
State FE		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the 2016–2017 change in refugee arrivals per 100 population. The unit of observation is a county. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A2: First-Differences Results: Main Crime Types, Logs

	Murder	Murder	Rape	Rape	Assault	Assault	Burglary	Burglary
$\Delta Log(refugee^{2017-2016})$	0.040	0.066	0.019	0.047	0.006	0.018	0.014	0.029
	(0.047)	(0.060)	(0.026)	(0.033)	(0.020)	(0.027)	(0.017)	(0.023)
N	253	253	294	294	293	293	295	295
R-sq	0.004	0.185	0.003	0.230	0.001	0.133	0.003	0.165
$\bar{Y}$	0.033	0.033	0.077	0.077	0.018	0.018	-0.074	-0.074
sd(Y)	0.538	0.538	0.317	0.317	0.215	0.215	0.211	0.211
State FE		X		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed as the 2016–2017 change in log absolute number of crimes. The independent variable is the 2016–2017 change in log refugee arrivals. The unit of observation is a county. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A3: First-Differences Results: Additional Crime Types, Logs

	Robbery	Robbery	Theft	Theft	Motor Vehicle Theft	Motor Vehicle Theft
$\Delta Log(refugee^{2017-2016})$	0.012	0.023	-0.009	0.004	0.006	0.040
	(0.025)	(0.031)	(0.011)	(0.014)	(0.022)	(0.027)
N	286	286	296	296	294	294
R-sq	0.001	0.164	0.003	0.178	0.000	0.222
$\bar{Y}$	-0.042	-0.042	-0.018	-0.018	0.039	0.039
sd(Y)	0.291	0.291	0.140	0.140	0.265	0.265
State FE		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed as the 2016–2017 change in log absolute number of crimes. The independent variable is the 2016–2017 change in log refugee arrivals. The unit of observation is a county. Standard errors are clustered by state and shown in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A4: Continuous Difference-in-Differences Results: Additional Crime Types

	Theft	Theft	Robbery	Robbery	Motor Vehicle Theft	Motor Vehicle Theft
Panel A: Linear Specification						
$refugees^{2016} \times \mathbf{1}(t = 2017)$	-132.200	122.203	-19.285	-22.994	339.753	300.107
	(145.325)	(131.187)	(15.557)	(20.376)	(254.866)	(241.384)
Panel B: Delinearized Specification						
Low Receiving Counties	10.974	4.486	-4.999***	-2.575*	6.912	-28.122
	(27.220)	(28.233)	(1.895)	(1.455)	(20.273)	(24.116)
High Receiving Counties	17.747	36.900	-5.417*	1.894	106.114*	135.055***
	(28.545)	(29.365)	(3.126)	(2.257)	(56.791)	(51.506)
N	6296	6296	6296	6296	6296	6296
$\bar{Y}$	1749.082	1749.082	66.889	66.889	801.984	801.984
sd(Y)	836.603	836.603	86.749	86.749	2422.082	2422.082
County Trends		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the interaction of a dummy for year 2017 and county-level refugee arrivals in 2016 per 100 population. The unit of observation is a county–year and the time period is 2010–2017. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A5: Continuous Difference-in-Differences Results: Main Crime Types, Logs

	Murder	Murder	Rape	Rape	Assault	Assault	Burglary	Burglary
$Log(refugees^{2016}) \times \mathbf{1}(t = 2017)$	0.017	0.022	-0.000	0.012	0.003	0.005	0.002	0.005
	(0.012)	(0.014)	(0.009)	(0.009)	(0.006)	(0.007)	(0.007)	(0.006)
N	2869	2869	3360	3360	3387	3387	3396	3396
$\bar{Y}$								
sd(Y)								
County Trends								

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in log absolute number of crimes. The independent variable is the interaction of a dummy for year 2017 and county-level log refugee arrivals in 2016. The unit of observation is a county–year and the time period is 2010–2017. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. ${}^{*}p < 0.1$, ${}^{**}p < 0.05$, ${}^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A6: Continuous Difference-in-Differences Results: Additional Crime Types, Logs

	Theft	Theft	Robbery	Robbery	Motor Vehicle Theft	Motor Vehicle Theft
$Log(refugees^{2016}) \times \mathbf{1}(t = 2017)$	0.014***	0.009**	-0.006	0.001	-0.000	0.009
	(0.005)	(0.004)	(0.008)	(0.009)	(0.008)	(0.007)
N	3404	3404	3309	3309	3383	3383
$\bar{Y}$						
sd(Y)						
County Trends						

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in log absolute number of crimes. The independent variable is the interaction of a dummy for year 2017 and county-level log refugee arrivals in 2016. The unit of observation is a county–year and the time period is 2010–2017. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A7: Continuous Difference-in-Differences Results: Additional Crime Types, Using Actual Drop in Refugees

	Theft	Theft	Robbery	Robbery	Motor Vehicle Theft	Motor Vehicle Theft
$\Delta refugees^{2016-2017} \times \mathbf{1}(t = 2017)$	-220.115	231.619	-20.632	-16.546	53.439	85.937
	(270.547)	(284.891)	(33.376)	(34.364)	(77.147)	(55.290)
N	6296	6296	6296	6296	6296	6296
$\bar{Y}$	1749.082	1749.082	66.889	66.889	162.119	162.119
sd(Y)	836.603	836.603	86.749	86.749	151.210	151.210
County Trends		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the interaction of a dummy for year 2017 and county-level 2016–2017 change in refugee arrivals. The unit of observation is a county–year and the time period is 2010–2017. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.

Table A8: Continuous Difference-in-Differences Results: Additional Crime Types, High Crime Areas

	Theft	Theft	Robbery	Robbery	Motor Vehicle Theft	Motor Vehicle Theft
$refugees^{2016} \times \mathbf{1}(t = 2017)$	-41.205	219.847	18.551	14.562	55.291	55.900
	(288.739)	(252.268)	(34.246)	(29.305)	(96.726)	(69.170)
N	3144	3144	3144	3144	3144	3144
R-sq	0.920	0.995	0.957	0.992	0.905	0.985
$\bar{Y}$	2098.141	2098.141	108.241	108.241	229.342	229.342
sd(Y)	804.591	804.591	105.096	105.096	179.856	179.856
County Trends		X		X		X

Notes: Each column shows the estimated coefficients from a separate regression model. See the text in the SM for details on the regression specification. The outcome variable is denoted in the column header and expressed in crime rate per 100,000 population. The independent variable is the interaction of a dummy for year 2017 and county-level refugee arrivals in 2016 per 100 population. The unit of observation is a county–year and the time period is 2010–2017. The sample is restricted to counties with above median total number of crimes for the entire sample period. All regressions adjust for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$ correspond to two-sided hypothesis tests.