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Daniel Masterson and Vasil I. Yasenov

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

Daniel Masterson†[1] and Vasil I. Yasenov[1,2]

[1]Immigration Policy Lab, Stanford University
[2]IZA – Institute of Labor Economics

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Abstract

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. We provide evidence on the effects of refugee resettlement on crime, leveraging a natural experiment in the United States, where an Executive Order by the president in January 2017 halted refugee resettlement. We find that, despite a 65.6% drop in refugee resettlement, there is no discernible effect on county-level crime rates. These null effects are consistent across all types of crime. Overall, the results suggest that crime rates would have been similar had refugee arrivals continued at previous levels.

Keywords: Refugees, immigration, crime
JEL Codes: F22, J15, K42

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†Corresponding author: Daniel Masterson, Immigration Policy Lab, Stanford University, 417 Galvez Mall, Encina Hall West, Suite 100. Email: dmasters@stanford.edu.

Both the scale of refugee crises and political conflict around the issue have reached a high-point 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 [1]. 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, bringing in thousands of refugees each year [2]. Canada, another major resettlement country, has welcomed some 700,000 refugees over the past four decades [3]. And European countries have received millions of asylum seekers in recent years [4]. Despite these efforts, however, an estimated 1.4 million individuals who in need of permanent resettlement to a safe country [5].

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 an 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 [6]." 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 between 2016 and 2017. Consequently, admissions in 2017 were among the lowest since the beginning of USRAP (33,368 individuals) [2]. Resettlement numbers for 2018 were even lower, with only

21,148 refugees admitted as of early December [7].

Given these consequential concerns about a link between refugees and crime, it is important to gather systematic empirical evidence on the issue. Previous studies have found that immigration more generally does not have discernible effects on crime rates [e.g., 8, 9, 10, 11] although some studies find modest decreases [12, 13] and others modest increases in crime due to immigration [14, 15, 16, 17]. There exists less evidence on the effect of refugees and asylum seekers specifically, but some studies from Europe suggest similar null effects or a small increase in crime rates, based on evidence in Germany [18, 19, 20]. There is a paucity of research on the effects of refugee resettlement on crime in the United States. One exception is a recent study by [21] who examine data from 2006 through 2014 and find no evidence of an effect of refugee resettlement on crime and terrorism related incidents.

One methodological challenge in estimating the effect of refugees on crime is the non-random selection of refugees to locations. For example, in the United States domestic resettlement agencies administer the allocation of refugees. While refugees with family ties in the United States are typically assigned to locations close to their family members, refugees without family ties are allocated 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 exogenous changes in refugee resettlement that are uncorrelated with local crime trends.

In this study we build on [21] to examine the link between refugee resettlement and crime rates in the context of the United States resettlement program. 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. This design allows us to overcome some of the methodological challenges that make it difficult to isolate the effect of refugees on crime because, as we show below, the reduction in arrivals caused by the ban was uncorrelated with pre-existing local crime trends. To our knowledge, this is the first study to examine the effects of this sudden policy reversal.

Our analysis focuses on the county-year level. Our outcome of interest is crime rates measured as the number of crimes in a given year per 100,000 county population. We use the Uniform Crime Reports (UCR) database published by the Federal Bureau of Investigation (FBI) for the period 2010-2017. We measure refugee arrivals using data from the Department of State's Worldwide Refugee Admissions Processing System (WRAPS). Overall our sample covers 6,296 county-year observations. Descriptive statistics (Table S1 and Figures S1, S2, S3 and S4) and details about the data, sample, and statistical analysis are reported in the Supplementary Materials (SM).

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 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 leverage this exogenous variation in the reduction of arrivals in a difference-in-differences design that allows us to estimate the effect of reducing refugee arrivals on crime rates. We compare crime trends in counties that experienced large drops in arrivals with counties that experienced much smaller or no reduction in arrivals.

Importantly, 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 pre-existing trends in county crime rates. Panels C-F of Figure 1 show that there is no discernible relationship between the reduction in arrivals and historical trends in crimes rates of murder, rape, assault, and burglary. This pattern supports the parallel trends assumption for the difference-in-differences design.

Given that high- and low-receiving counties had similar crime trends prior to the ban, it is reasonable to assume that these counties would have continued on such parallel crime trends had the ban not occurred. Under this parallel trends 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 (see Figures S5–S9 and Table S2 for further evidence on parallel trends).

Results

Did halting refugee resettlement reduce crime rates? Figure 2 provides a graphical summary of the main findings from our natural experiment. 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. This indicates that 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. The results are similar for other crime types, including theft, motor vehicle theft, and robbery (Figure S10); when using log transformation (Figures S11 and S12; and when fitting linear models (Tables S3–S6).

Next we turn to estimating the difference-in-differences models. Pooling the data from the 2010-2017 period, we regress crime rates on the interaction between a measure of exposure to the Executive Order and the indicator for the year 2017, which marks the post-ban period. The coefficient of interest is the interaction term that identifies the differential change in crime rates between counties that experienced large and small reductions in arrivals due to the ban. We use two specifications. In the first, the measure of exposure is the number of arrivals per capita in 2016. In the second, we relax the linearity assumption on the interaction and measure exposure with three dummy variables, which differentiate counties with no arrivals, a low number of arrivals per capita, or a high number of arrivals per capita. The split between low and high is based on the median number of arrivals per capita among counties that received a non-zero number of refugees in 2016. All models control for time-invariant county characteristics with county fixed-effects, common temporal shocks with year fixed-effects, and linear county-specific trends in crime rates.

Table 1 presents the results from our difference-in-differences models. If the Executive Order decreased crime rates we would expect a negative interaction effect. This would indicate 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 that there is no discernible relationship between exposure to the Executive Order and changes in crime rates. For the linear specification 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. The results are similar for the delinearized specification. Again, the point estimates for three of the four crime types are positive, and one is statistically significant. Overall, these results show that the ban's reduction in refugee resettlement had no discernible impact on crime rates.

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.

In the SM we present various checks that support the robustness of these null findings. We find that the null effects also hold for other types of crime, including theft, motor vehicle theft, and robbery (Table S7); after log transformations (Tables S8 and S9); when using alternative independent variables (Tables S10 and S11 and Figure S13) and when focusing on high crime areas (Tables S12 and S13). Additionally, the null findings hold when we allow for differential changes prior to the Executive Order by interacting the exposure variables with each year (Figures S14–S17).

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 uncorrelated with pre-existing 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 an 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. This null finding is consistent with and adds to the small but growing literature suggesting that refugee arrivals have, at most, modest effects on crime rates [18, 19, 20].

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 multilayered vetting that involves multiple agencies running extensive background checks. 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. While the United States resettlement program is larger than its counterparts in other countries in terms of absolute numbers, admitted refugees make up a small fraction of the United States population. For example, across the 2000-2016 period the average county received about two refugees per 100,000 persons per year, and the maximum was 178 refugees per 100,000 persons per year. Given this, the impact of refugees on the crime rate is likely to be limited compared to the impact of the native population.

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 [22]. For example, in 2016, 34% of asylum seekers in Germany were men between the ages of 18 and 35 [23]. In contrast, approximately 14% of the refugees resettled to the United States in 2016 were men within a similar age range [24].

Our findings have important implications for refugee policy, suggesting that restricting resettlement to the United States is unlikely to yield benefits in terms of reducing the crime rate. In fact, our results suggests that changes in crime rates would have been similar had arrivals continued at pre-ban levels.

Our study is not without limitations. Given that our data ends in 2017, we can only examine the short-term effects of the Executive Order. Also, 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 develop a more comprehensive evidence base about how refugees affect receiving communities.

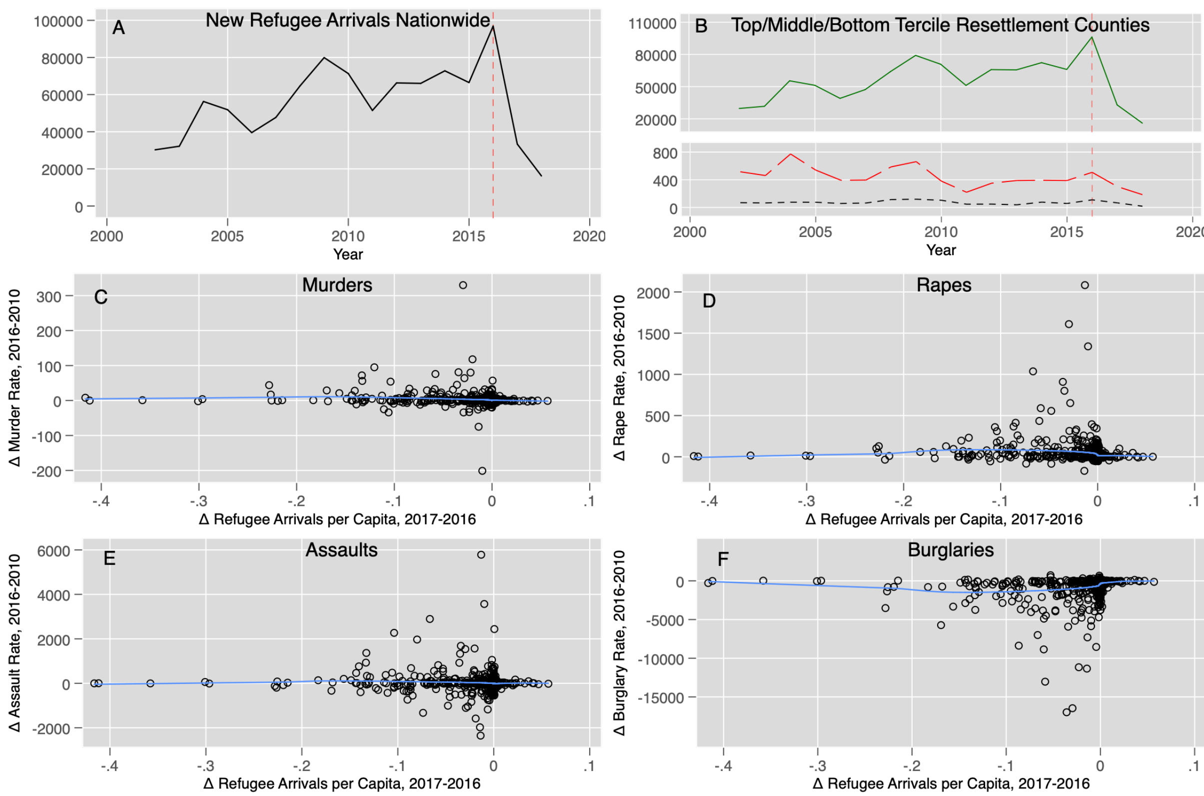

Figure 1: **Research Design: Comparing Counties with Low and High Exposure to Executive Order #13769.** *A*: Refugee arrivals dropped nationwide in early 2017 due to the Executive Order. *B*: The reduction in arrivals was much larger in counties that received the most refugees prior to the ban. Green (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. *C*-*F*: There is no detectable relationship between the 2016–2017 change in refugee arrivals per capita and the 2010–2016 changes in local crime rates. Blue lines are local linear regression (LOESS) fits.

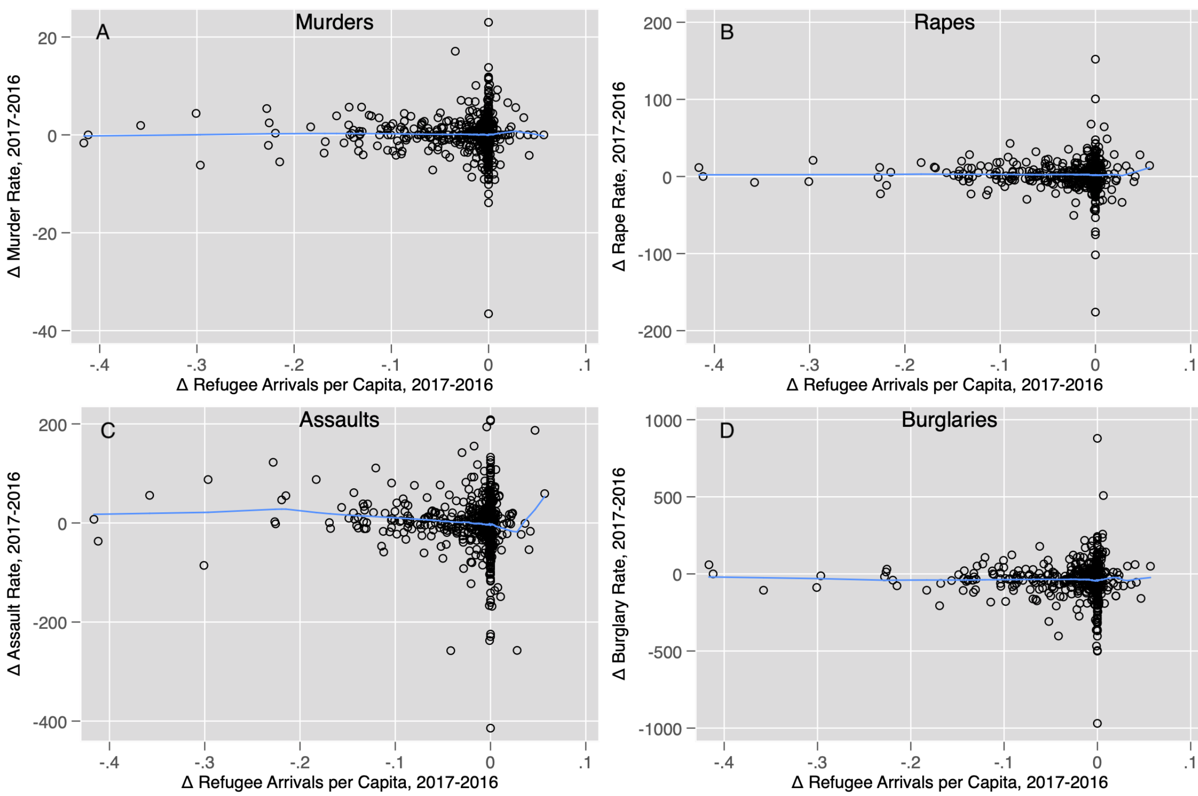

Figure 2: **The Effect of the Executive Order on Local Crime Rates.** Plots show the relationship between the 2016–2017 drop in refugee resettlement due to the Executive Order and the 2016–2017 changes in crime rates across counties. The flat LOESS lines demonstrate that there is no discernible relationship between the reduction in refugee resettlement and local crime rates for murder (A), rape (B), aggravated assault (C), and burglary (D).

	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

Table 1: **Difference-in-Differences Results for the Effect of the Executive Order on Local Crime Rates.** Each entry presents the difference-in-differences estimate comparing crime rates in counties with a high and low exposure to the Executive Order. See SM for details of the empirical strategy. We find no discernible relationship between exposure to the Executive Order and changes in local crime rates.

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Supplementary Materials

Materials and Methods

Data

We use the Federal Bureau of Investigation's (FBI) Uniform Crime Reports (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). We downloaded these series for years 2010–2017 from Jacob Kaplan's OpenICPSR repository [25].

Following the crime literature, we convert the reported absolute number of crimes into crime rates per 100,000 population 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. We focus on all 50 states and the District of Columbia, excluding all 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 out of 3,142 counties had at least one local agency reporting crime statistics, covering the majority of the United States.

We obtain refugee resettlement data from the Worldwide Refugee Admissions Processing System (WRAPS) database from Refugee Processing Center's website [2]. It contains yearly information on refugee arrivals to the United States. The level of observation in the raw dataset is year-origin-city. We convert the refugee flow numbers to shares per 100 population and 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. Throughout this period, 787 out of 3,142 counties in all states received refugee arrivals.

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) [26]. Because estimates for year 2017 are not available, we assign 2016 population values to all counties in year 2017.

Statistical Analysis

We use multiple specifications of the difference-in-differences estimator to analyze the effect of reducing refugee resettlement on crime rates. Our research design compares crime rates in counties that received many refugees before 2017 to crime rates in counties that received fewer refugees, in the time after the Executive Order relative to the years prior. We estimate each regression separately for each of the seven main crime types: murder, rape, aggravated assaults, burglary, robbery, theft and motor vehicle theft. We begin with evaluating the underlying parallel trends assumption invoked throughout our analysis.

Parallel Trends Assumption

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

First, we correlate the 2010–2016 county-level crime trends with the 2016–2017 drop in refugee arrivals (Figure 1 bottom panels and Figure S5). This test assesses whether crime trends predating the Executive Order are associated with the drop in arrivals due to the refugee ban. We find no meaningful relationship between crime pre-trends and the observed 2016–2017 change in refugee resettlement.

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

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

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 the share of the populations 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 statistically indistinguishable from zero.

The results are shown in Table S2. Standard errors are clustered by state. Note that positive γ_i coefficients suggests counties higher exposure to the Executive Order were on an upward crime trend from 2016–2017, which would make us more likely to estimate that refugee resettlement increased crime rates. None of the estimated coefficients is large, and none is negative and significant.

Second, 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 the per (100) capita refugee arrivals 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 S6, S7, S8 and S9. Again, we find that crime trends are similar regardless of exposure to the policy. While the levels are different, the trajectories seem to be very close to parallel across county groups.

All in all, there is no clear evidence of a violation of the parallel trends assumption. The weak evidence that suggests any difference in trends would make us more likely to identify a positive relationship between refugee resettlement and crime. 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. Alternatively, we use log absolute number of crimes and log refugee arrivals in 2016 as a robustness check, which we present in Tables S4, S6 and Figures S10, S12 (First-differences) 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, but regardless, a positive sign indicates that refugee resettlement is associated with an increase in crime rates. For instance, 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 S3, S4, S5, and S6. All standard errors are clustered by state. More scatter plots are shown in Figures S10, S11 and S12. There appears to be no robust and statistically significant relationship between refugee resettlement and crime rates.

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. 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) controlling 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 using the actual 2016–2017 drop in refugee arrivals, using arrivals in the entire 2010–2016 period, using 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 sign interpretation of β_2 is switched so that a negative one 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 1, S7, S8, S9. Standard errors are clustered by county. We find no robust relationship between drops in refugee resettlement and crime rates.

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 impact of refugee flows on crime rates in each year. Refugees causing crime would result in the coefficient β_{2017} being statistically significantly smaller than β_{2016} because this corresponds to counties with higher exposure to refugee flows experiencing lower 2017 crime rates.

Additionally, this specification allows for further verification of the underlying parallel trends assumption. If we were to estimate significant difference between the coefficients $\beta_{2011}, \ldots, \beta_{2016}$ this would undermine the validity of our empirical strategy.

Figures S14, S15, S16 and S17 show the β_τ coefficients results for various crime types in rates and logs. Standard errors are clustered by county. These results further confirm our tests of parallel trends prior to 2016. Moreover, we find no discernible evidence that refugee resettlement affect crime rates.

Supplementary Text

Descriptive Statistics

Crime

Table S1 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. We also present descriptive statistics of the logarithmic transformations.

Next, Figure S1 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.

Lastly, Figure S2 displays the ten states with the highest crime rates per 100,000 people by crime type. All crime statistics in our analyses line up nearly exactly with official crime summary data published by the FBI [27]. 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.

Refugee Resettlement

The bottom rows of Table S1 show summary statistics of our refugee arrival variables. Similarly to the crime data, these variables are also right-skewed. The level of observation is county–year, the sample covers 2010–2017 and the sample size is 6,296. The mean (median) county received 83.34 (1.00) refugees.

The left panel in Figure S3 shows the top 10 refugee origin counties and the right one displays the top 10 receiving states. 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 S4 shows a map of cumulative refugee arrivals to the United States in the time period 2002–2017 for each United States county. As mentioned above, only 787 counties received refugees during the time period. Darker shades of red denote higher refugee arrival levels and white denotes counties with no data on refugee resettlement.

Robustness Checks

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 test three alternative treatment variables.

First, we use the observed (i.e., actual) 2016–2017 county-level drop in refugee resettlement as a treatment variable. The results are presented in Table S10 and S11.

Second, to flexibly accommodate the skewness of the refugee resettlement variable we split all 787 counties in our analysis into three groups depending on 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 ("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 Tables 1 and S7.

Lastly, we took the average refugee arrivals in the entire sample pre-period 2010–2016. In Figure S13 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¡0.000) indicating strong autocorrelation in refugee flows across United States counties over time.

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.

Robustness to Focusing on Other Crime Types

While in the main text we focus on four crimes (murder, rape, assault and burglary), FBI's UCR database contains information on three other major crime types - theft, robbery and motor vehicle theft. We conducted all statistical analyses for these additional crime types.

The results are presented in Figures S5, S7 and S9 (Parallel Trends); Tables S4, S6 and Figures S10, S12 (First-differences); Tables S7 and S9 (Continuous Difference-in-differences); and Figures S15 and S17 (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.

Robustness to 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 S8 and S9 (Parallel Trends); Tables S5, S6 and Figures S11, S12 (First-differences); Tables S8 and S9 (Continuous Difference-in-differences); and Figures S16 and S17 (Generalized Continuous Difference-in-differences). Similar to our main results, we find no evidence of a discernible relationship between refugee resettlement and crimes.

Robustness to Focusing on High Crime Areas

We conducted subgroup analysis focusing on localities 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 counties with above median crime activity.

The results are shown in Tables S12 and S13 (Continuous Difference-in-differences). We find no evidence that refugee resettlement significantly impacted crime rates in these high crime areas.

Tables

Table S1: 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 S2: 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S3: 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S4: 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S5: 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S6: 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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S7: 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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S8: 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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S9: 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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S10: 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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S11: 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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table S12: 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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

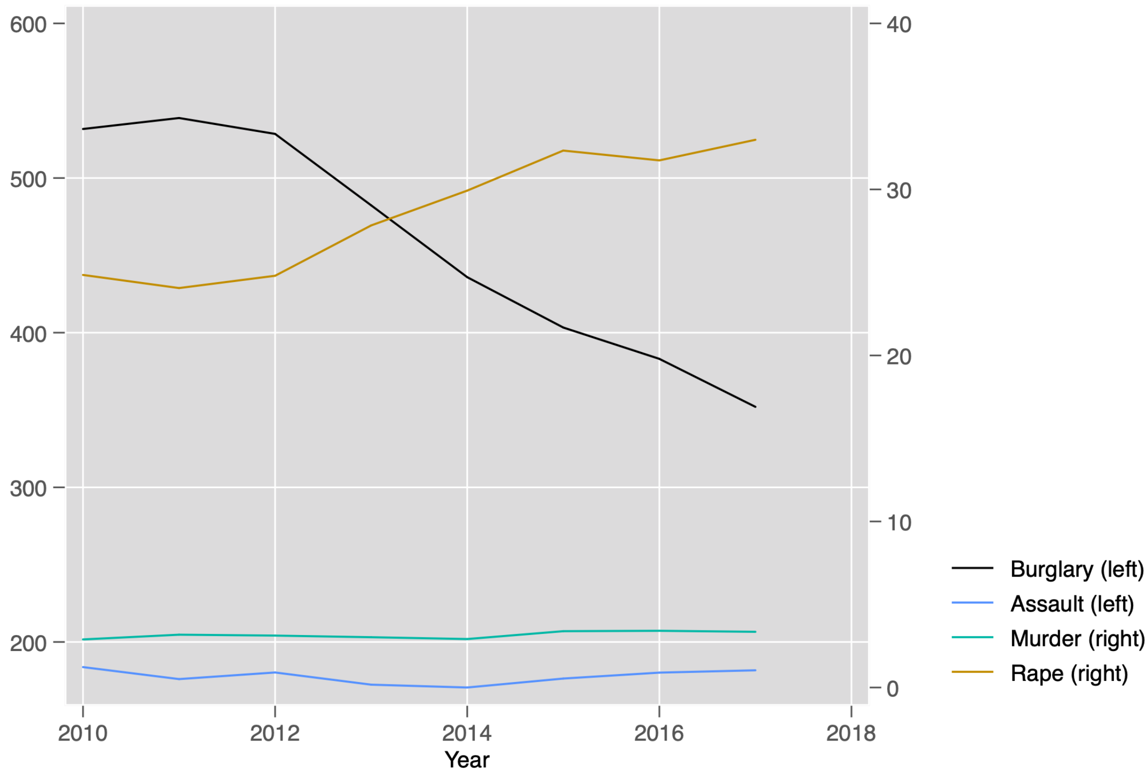
Table S13: Continuous Difference-in-Differences Results: Additional Crime Types, High Crime Areas

	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 control for county and year fixed effects. Standard errors are shown in parentheses and are clustered by county. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$..

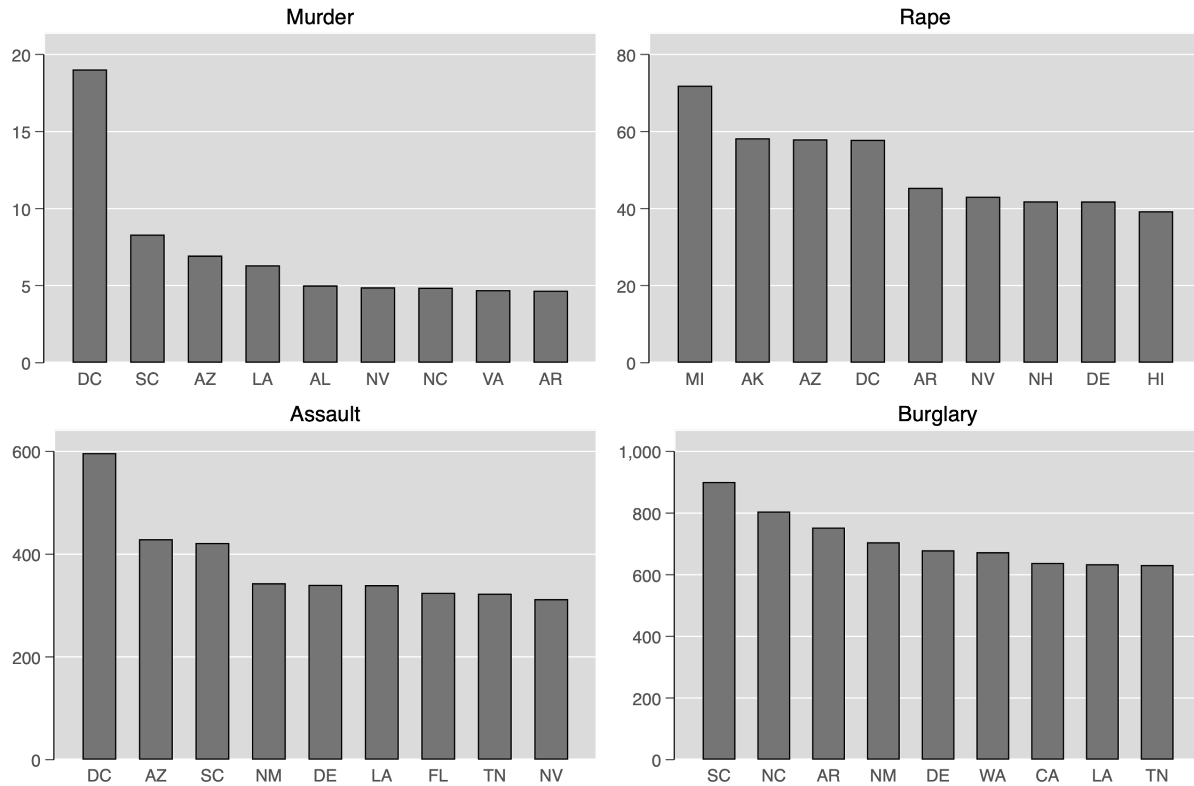
Figures

Figure S1: National Crime Rates per 100,000 People



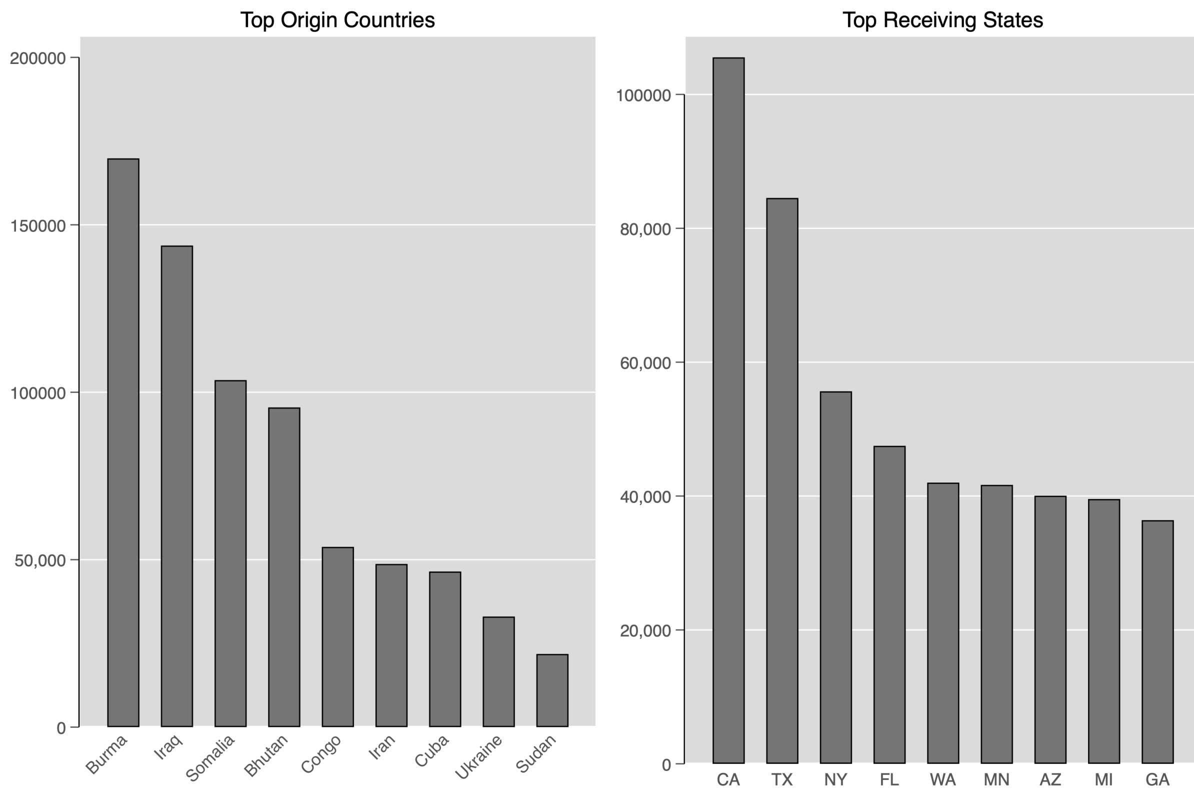
Notes: Aggregate crime rates in the United States by crime type in the period 2010–2017.

Figure S2: States with Highest Average Crime Rates per 100,000 People, 2010–2017



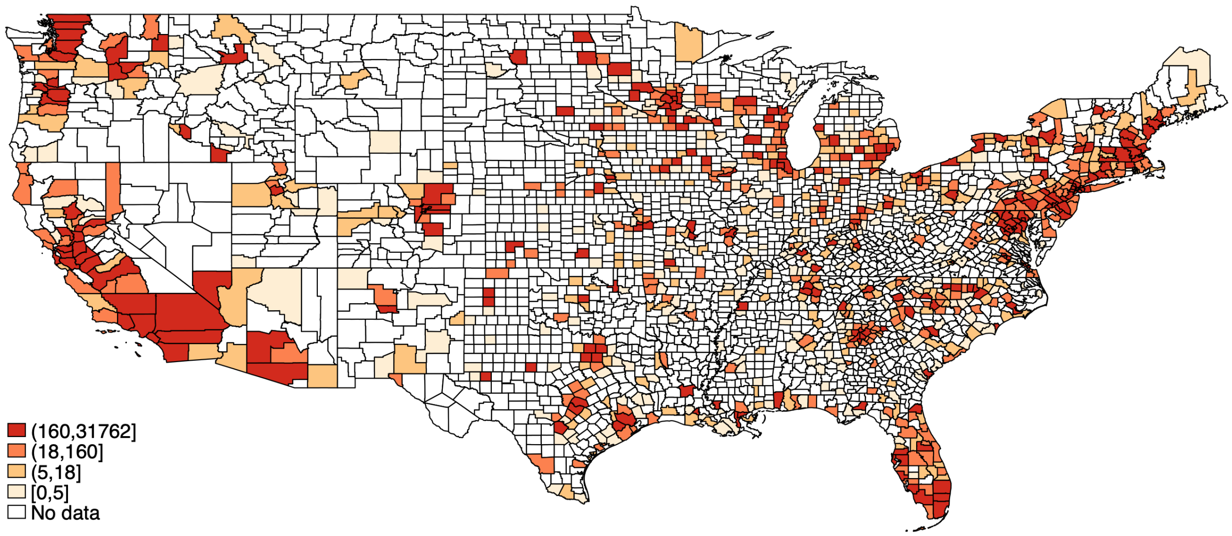
Notes: All numbers reflect 2010–2017 averages.

Figure S3: Origins and Destinations for Refugees in the United States, 2002–2017



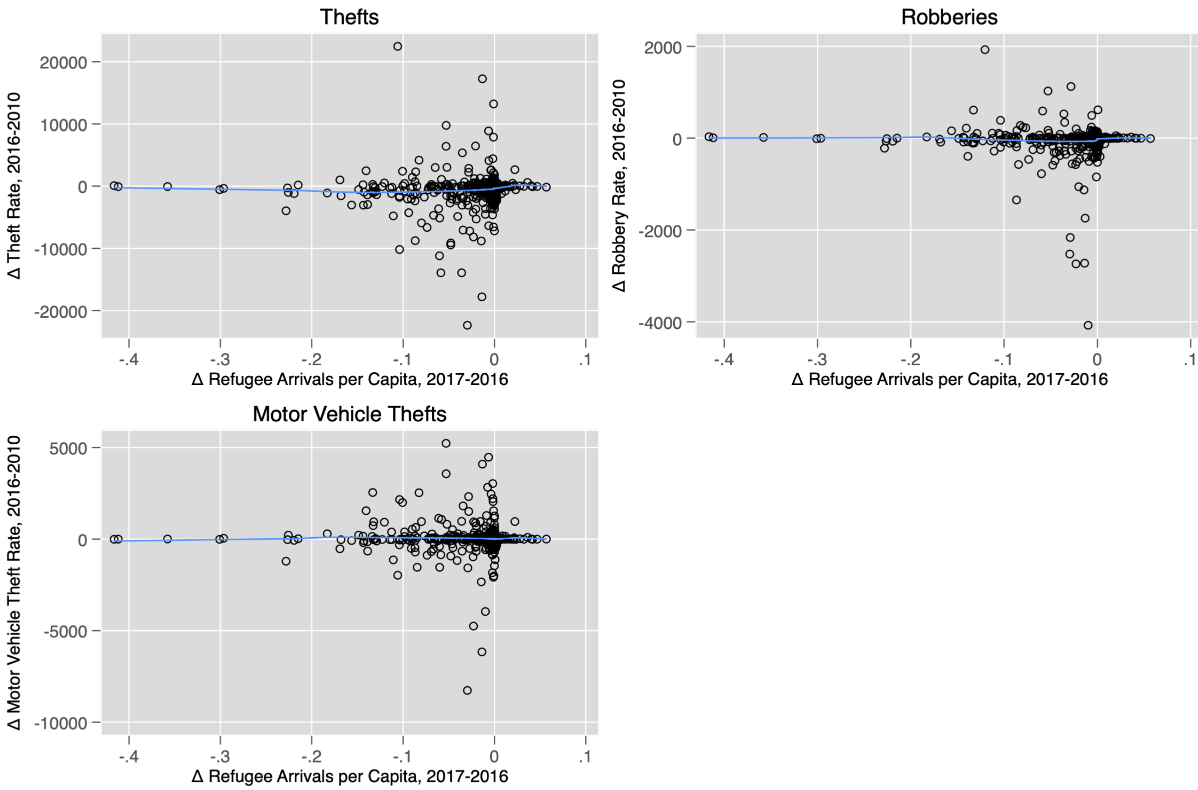
Notes: List of the ten largest refugee sending countries (left panel) and the top ten receiving states (right panel). All numbers reflect 2002–2017 aggregate values.

Figure S4: Cumulative Refugee Arrivals in the United States, 2002–2017



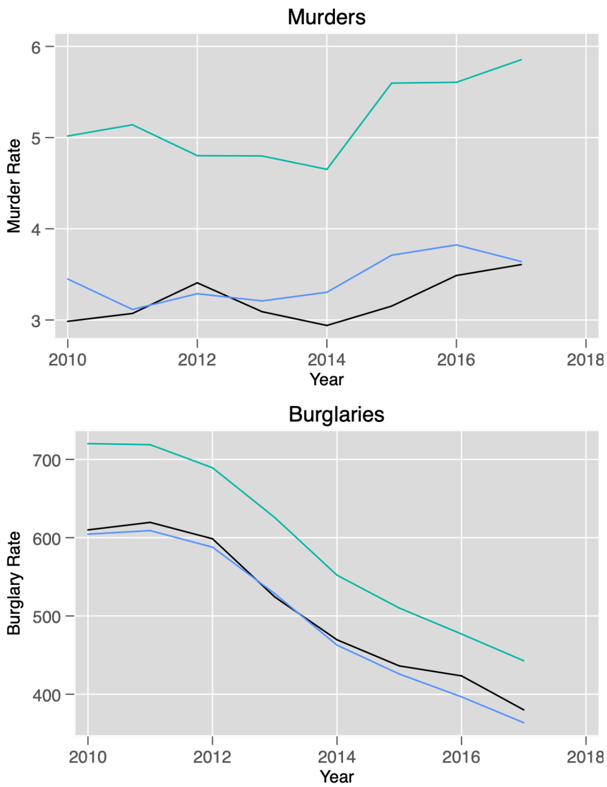
Notes: Cumulative refugee arrivals in the United States for the period 2002–2017. Each observation is a county. Darker shades of red correspond to higher number of refugee resettled.

Figure S5: Pre-ban Crime Trends and Drop in Refugee Arrivals: Additional Crime Types



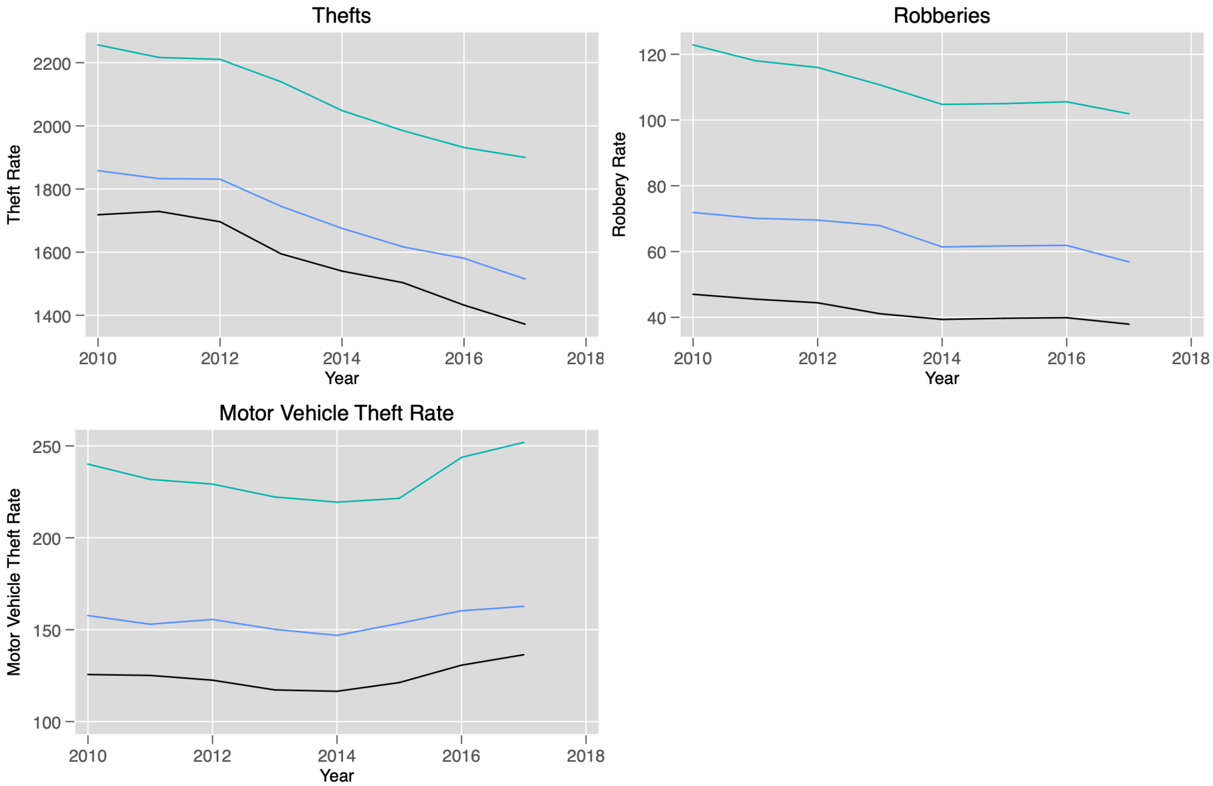
Notes: Crime trends between 2010 and 2016 and drop in refugee arrivals due to the Executive Order by crime type. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure S6: Crime Trends by High/Low/Very Low Receiving Counties: Main Crime Types



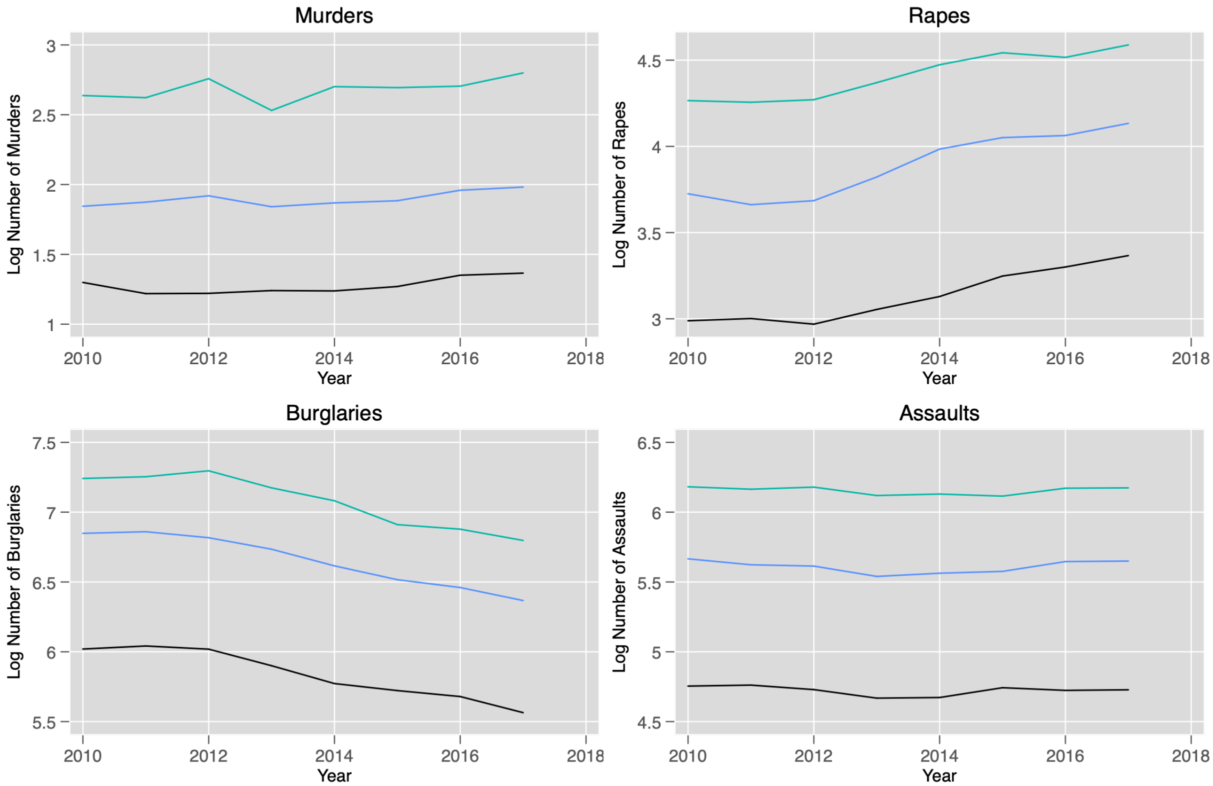
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are that received no refugees in 2016. The other two groups are split in two groups of equal size – above median are high receiving counties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

Figure S7: Crime Trends by High/Low/Very Low Receiving Counties: Additional Crime Types



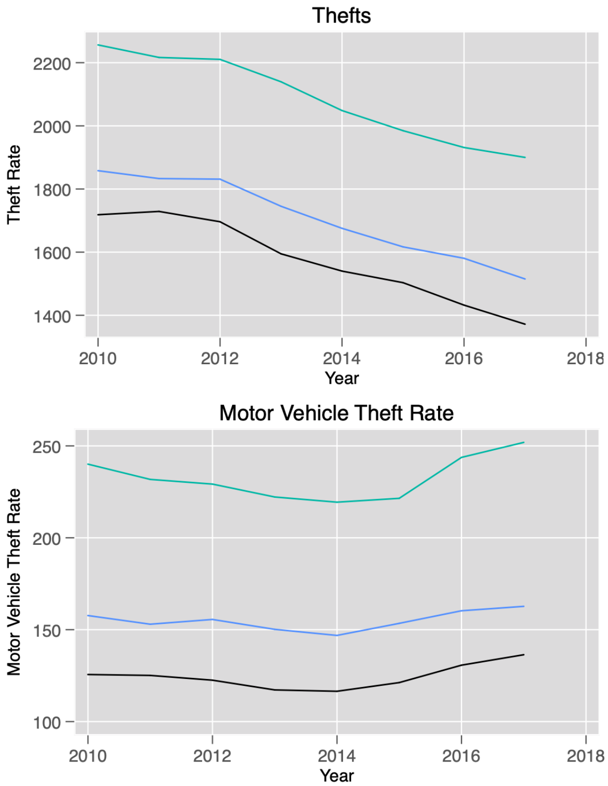
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are ones with no refugee arrivals in 2016. The other two groups are split in two groups of equal size – above median are high receiving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

Figure S8: Crime Trends by High/Low/Very Low Receiving Counties: Main Crime Types, Logs



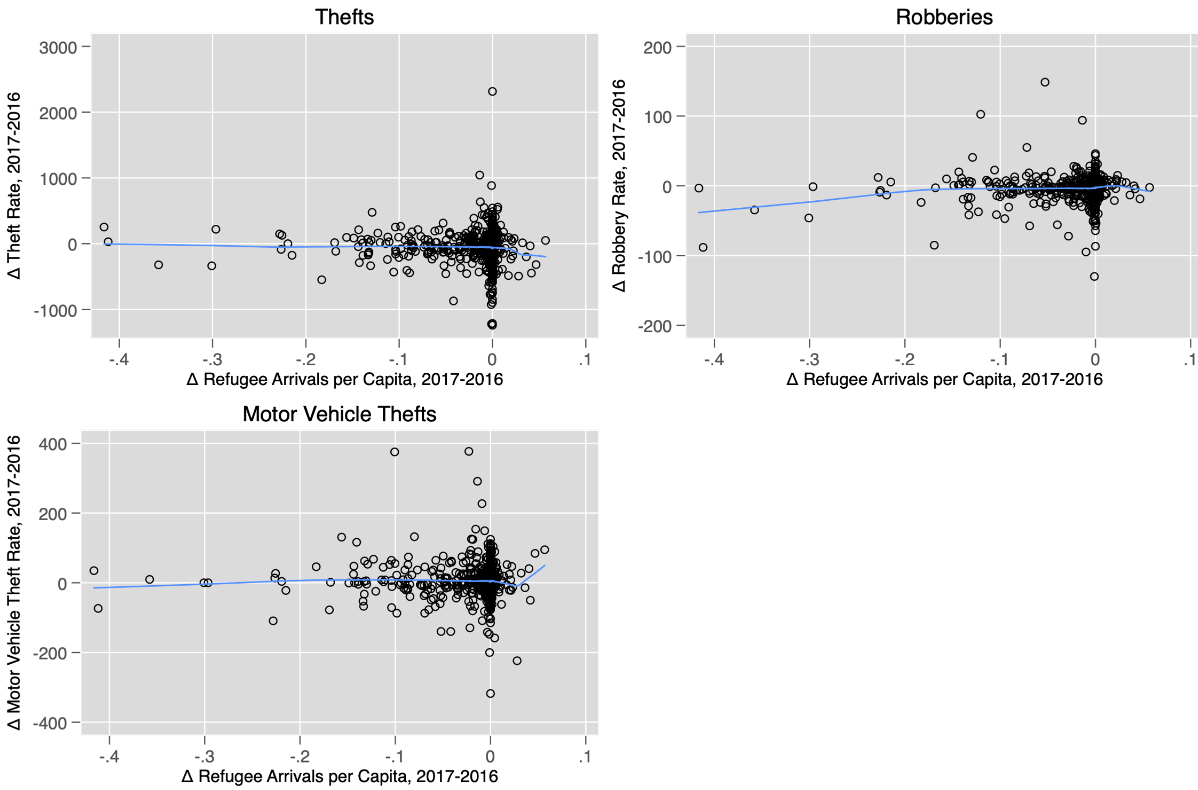
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are ones with no refugee arrivals in 2016. The other two groups are split in two groups of equal size – above median are high receiving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

Figure S9: Crime Trends by High/Low/Very Low Receiving Counties: Additional Crime Types, Logs



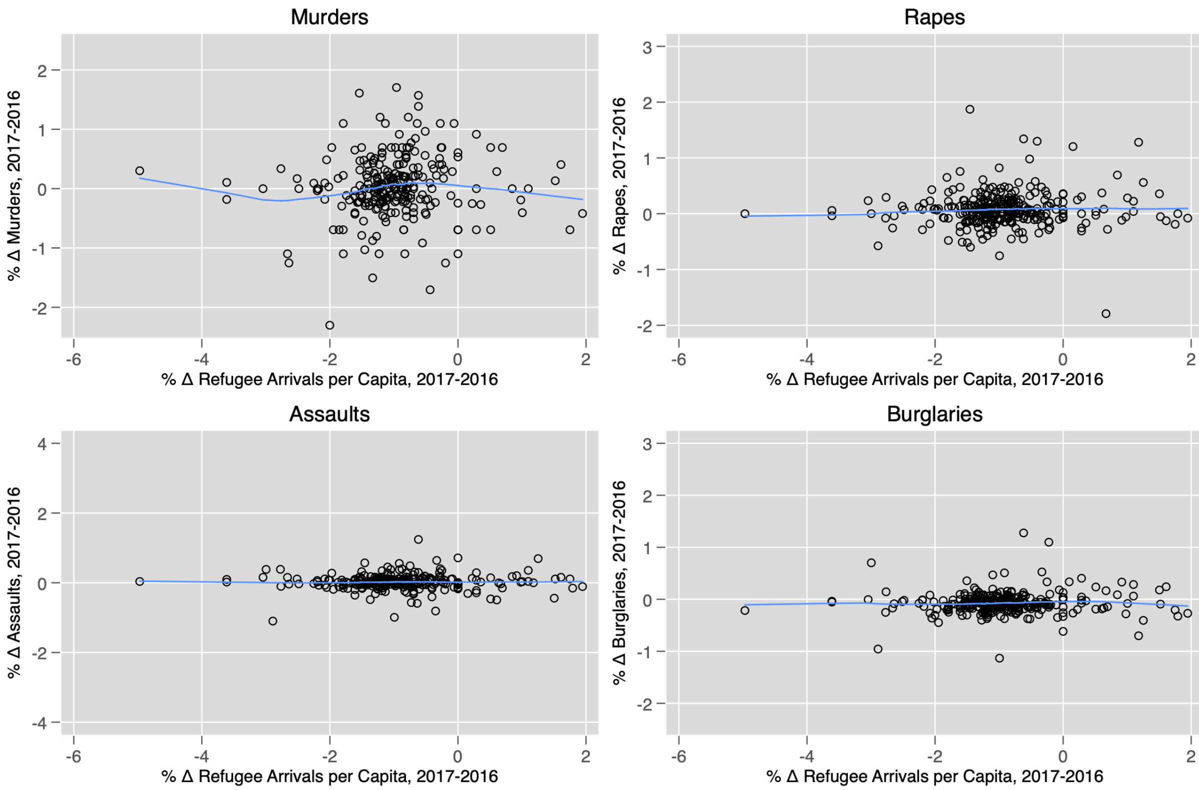
Notes: Trends in crime behavior by high (green line), low (blue line), and very low (black line) refugee receiving counties over time. Very low receiving localities are ones with no refugee arrivals in 2016. The other two groups are split in two groups of equal size – above median are high receiving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

Figure S10: First-Differences Results: Additional Crime Types



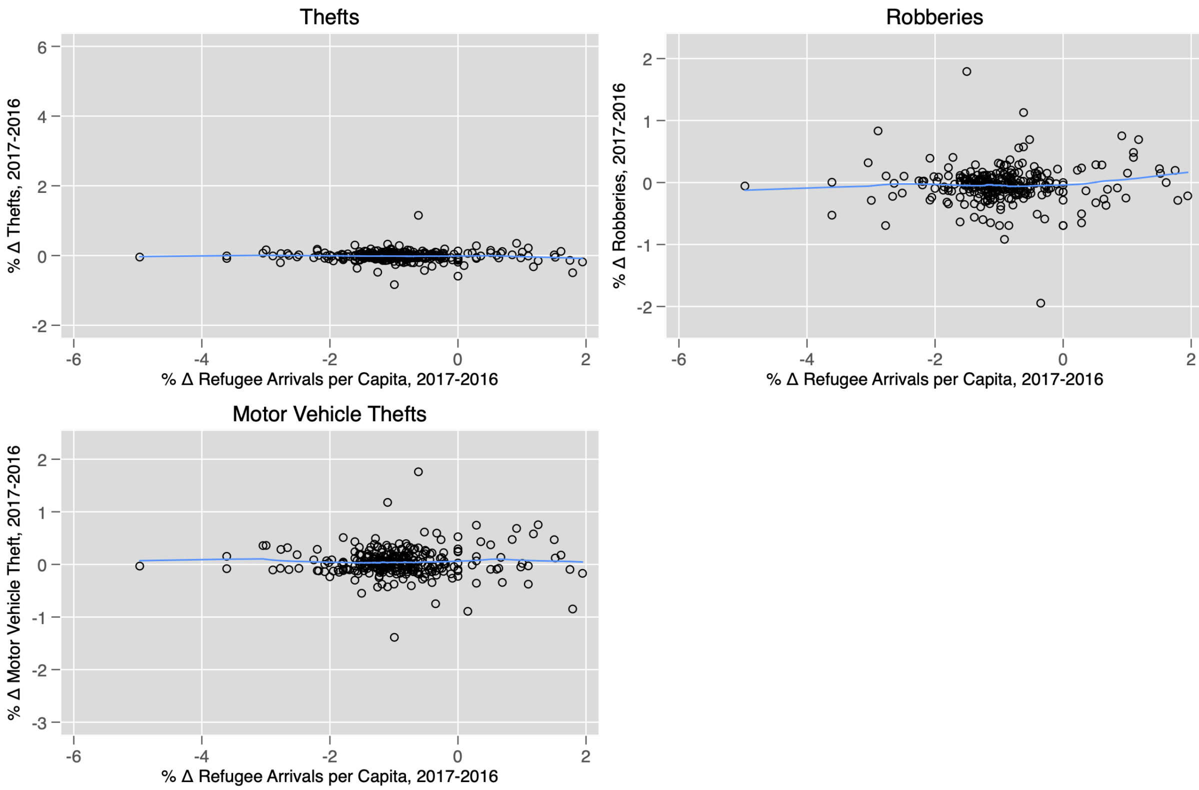
Notes: Scatter plot of 2016–2017 change in refugee arrivals per 100 population and 2016–2017 changes in crime rate per 100,000 people. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure S11: First-Differences Results: Main Crime Types, Logs



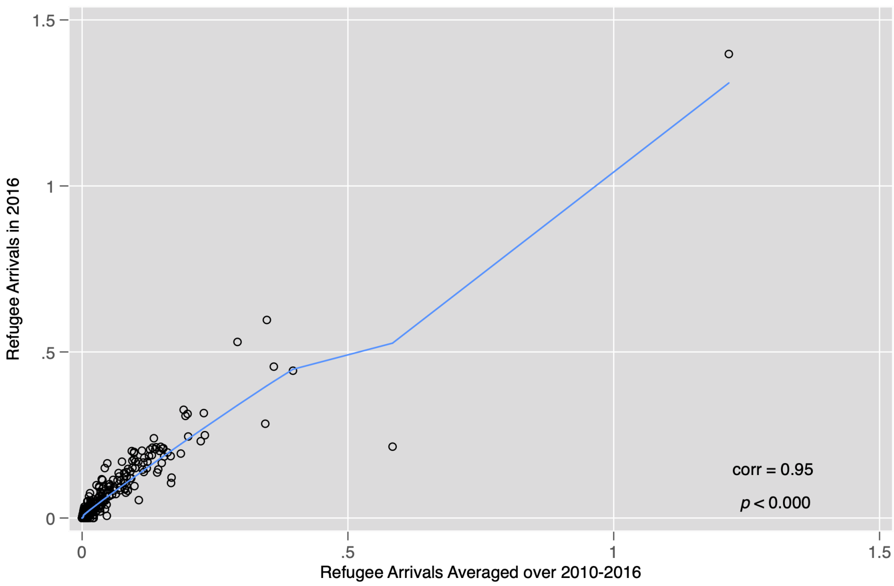
Notes: Scatter plot of 2016–2017 percent change in refugee arrivals and 2016–2017 percent changes in absolute crimes. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure S12: First-Differences Results: Additional Crime Types, Logs



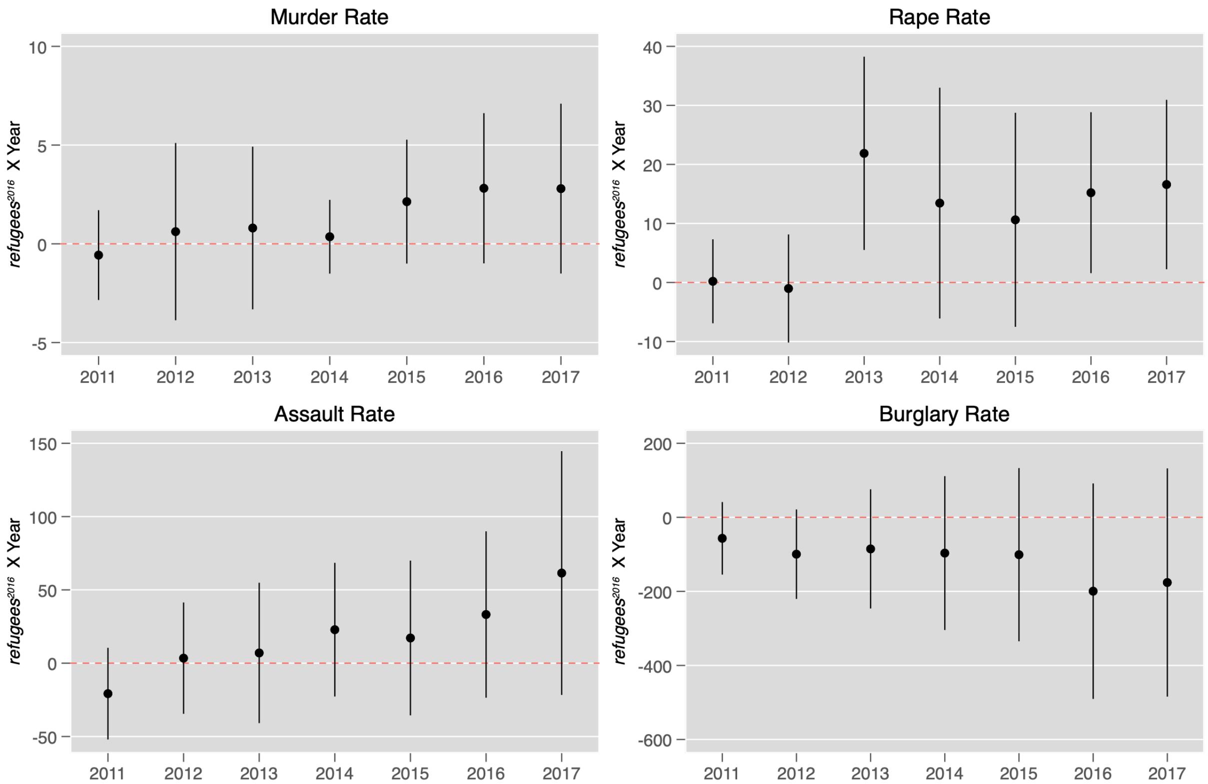
Notes: Scatter plot of 2016–2017 percent change in refugee arrivals and 2016–2017 percent changes in absolute crimes. Local regression (LOESS) fit is shown in blue line. Each observation is a single county.

Figure S13: Treatment Variable Robustness Check



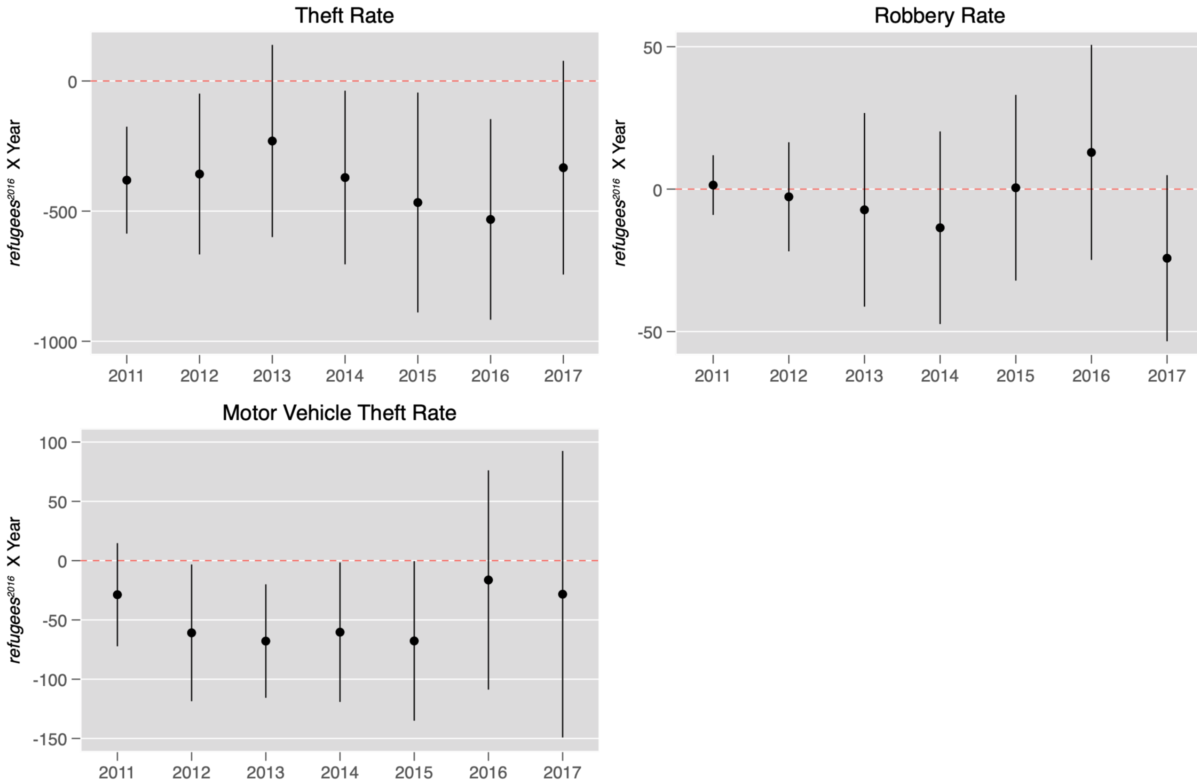
Notes: Scatter plot of refugee resettlement per 100 people in 2016 and aggregated 2010–2016 values. Blue line is local regression (LOESS) fit. Each observation is a single county.

Figure S14: Generalized Continuous Difference-in-Differences Results: Main Crime Types



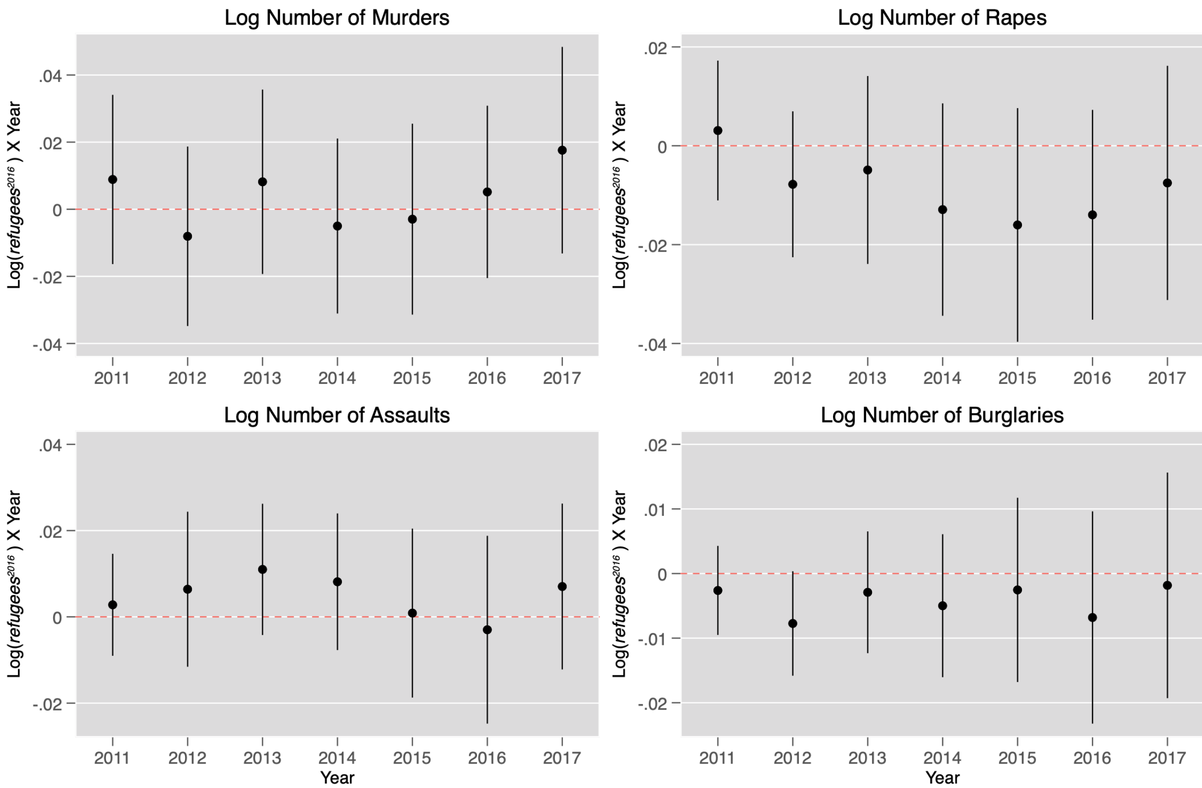
Notes: Estimated regression coefficients of year dummies interacted with number of refugee arrivals in 2016 per 100 people from a generalized continuous difference-in-differences model. See the text in the SM for details on the regression specification. The outcome variable is expressed in crime rate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

Figure S15: Generalized Continuous Difference-in-Differences Results: Additional Crime Types



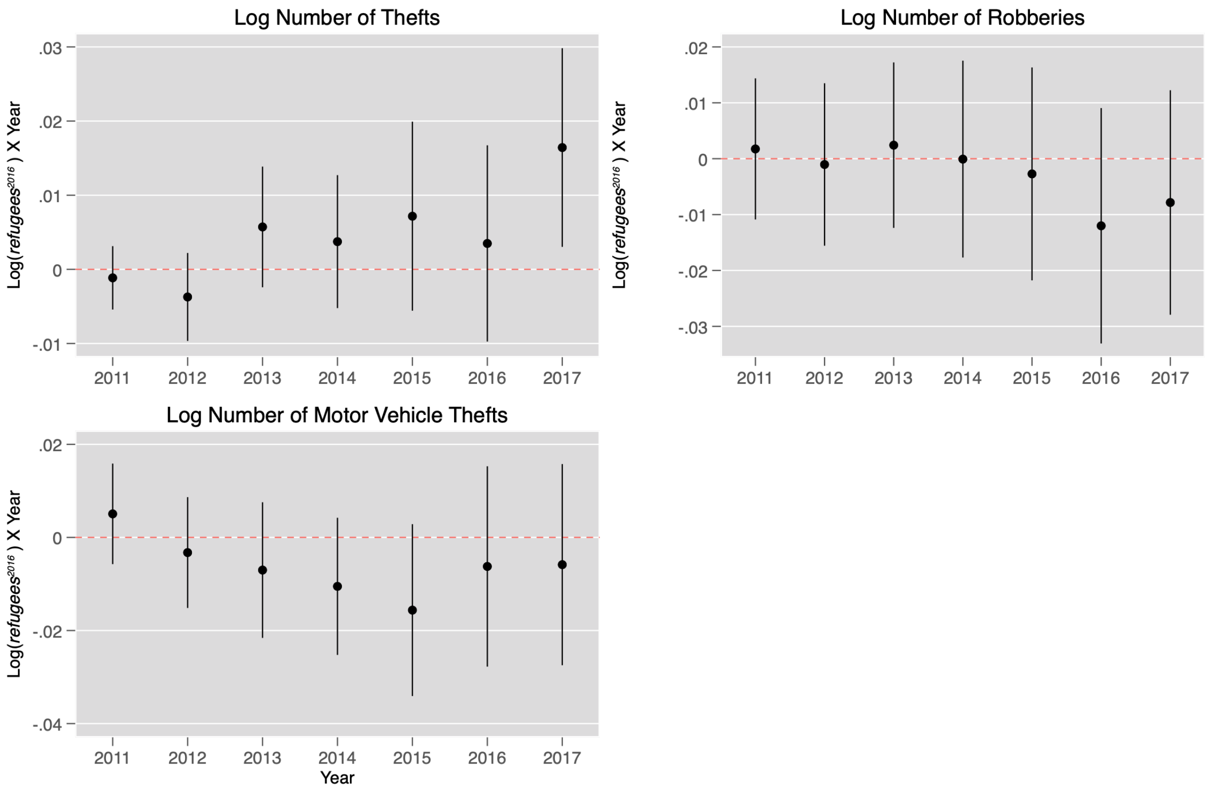
Notes: Estimated regression coefficients of year dummies interacted with number of refugee arrivals in 2016 per 100 people from a generalized continuous difference-in-differences model. See the text in the SM for details on the regression specification. The outcome variable is expressed in crime rate 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 S16: 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 S1). Standard errors are clustered by county and 95% confidence intervals are shown as vertical lines.

Figure S17: 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 S1). Standard errors are clustered by county and 95% confidence intervals are shown as vertical lines.