

Letter

Does Halting Refugee Resettlement Reduce Crime? Evidence from the US Refugee Ban

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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. In this article, we find that there is no discernible effect on county-level property or violent crime rates.

INTRODUCTION

The number of refugees globally has reached new highs in the last decade and political conflict over the issue has followed. In the United States and across Europe, refugees and resettlement have become key campaign issues and common targets of resurgent right-wing parties (Dinas et al. 2019; Dustmann et al. 2019). Earlier political debates about immigration typically centered on its consequences for labor markets, government budgets, and crime (e.g., Dancygier and Margalit 2020; Hainmueller and Hopkins 2015). More recently, disagreements over refugee resettlement have focused largely on public safety, with many opponents claiming that refugees put native-born residents at increased risk of crime.

In the US, domestic resettlement agencies administer the placement of refugees, and due to the nonrandom 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. To alleviate selection bias and isolate the causal effect, we require changes in refugee resettlement that are exogenous with respect to local crime trends.

We leverage the large sudden drop in refugee arrivals due to Trump's Executive Order #13769 of

January 2017 (the “refugee ban”) as a natural experiment to study whether reducing refugee resettlement affected crime rates. The ban resulted in much larger reductions in refugee arrivals in counties that had received higher numbers of refugees prior to it. Our difference-in-differences design exploits the fact that this nationwide policy change, based on federal policy considerations rather than local conditions, affected counties very differently in a way that is plausibly uncorrelated with preexisting crime trends. Multiple tests of observable implications of this assumption support the validity of the research design.

The results show that the reduction in refugee arrivals had a precisely estimated null effect on property and violent crime rates. In other words, crime rates would have been similar had arrivals continued at pre-Executive Order levels. In light of several recent studies from Europe suggesting that refugee migration causes a modest rise in crime rates (Dehos 2017; Gehrsitz and Ungerer 2017; Lange and Sommerfeld 2018), our null findings contribute to our understanding of a highly contentious dimension of immigration policy. Our work most directly contributes to the literature on the social and economic effects of immigration (e.g., Borjas 2017; Peri and Yasenov 2019), which in turn has implications for understanding the drivers of political attitudes around migration (e.g., Hainmueller and Hopkins 2015; Scheve and Slaughter 2001).

THEORETICAL EXPECTATIONS ABOUT REFUGEES AND CRIME

Existing empirical research has found varied estimates of the relationship between immigration and crime. Although the evidence suggesting that immigration increases crime, particularly violent crime, is thin (Ousey and Kubrin 2018), there is significant heterogeneity in findings across studies depending on the

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context and research design and types of immigration and crime (see, e.g., Berardi and Bucurius 2014; Shihadeh and Barranco 2010).

Refugees are a particular subset of immigrants and differ from economic migrants in both their observable characteristics and the drivers behind their migration decisions (Dustmann et al. 2017), differences that suggest the need for special consideration of the influence of refugees specifically on crime rates. Recent studies focusing on refugees in Germany find small increases in crime rates due to the inflow of refugee migrants (Dehos 2017; Gehrsitz and Ungerer 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, especially as broader evidence on the immigration-crime relationship shows that it may differ across countries, such as in the US compared with Europe (see, e.g., Berardi and Bucurius 2014; Milton, Spencer, and Findley 2013). There is a paucity of research on the effects of refugee resettlement on crime in the US. The one exception is a recent study that broadly examines data from 2006 through 2014 and finds no evidence of an effect of refugee resettlement on crime or terrorism (Amuedo-Dorantes, Bansak, and Pozo 2020). We build on their work through a novel estimation strategy, analyzing the Executive Order as a natural experiment associated with a large, sharp, and sudden variation in resettlement in 2017.

Existing theory presents ambiguous expectations about the consequences of refugee resettlement on crime. On the one hand, resettlement could increase crime rates if the refugees are a crime-prone demographic. First, people migrating from places where violence is widespread may have a relatively high propensity to commit violent crimes. Living in environments with extreme hardship, such as urban slums and refugee camps, could have long-term psychosocial effects including social disaffection, depression, or posttraumatic stress disorder, which may increase the propensity for antisocial behavior. Traumatic experiences, including living through or witnessing violence, poverty, and sexual violence, have been widely demonstrated to be predictive of aggressive and criminal behavior (Ardino 2012). Looking specifically at refugee resettlement in Switzerland, Couttenier et al. (2019) find evidence that refugees who lived through war as children committed more crimes after resettlement than those who had not experienced war.

Second, even if refugees are not a crime-prone demographic, arriving in a new country could lead people to commit nonviolent crimes due to economic hardship or social alienation. Economic reasons could drive people to turn to illegal market opportunities whether through theft or the drug trade (Ousey and Kubrin 2009). Simmler et al. (2017) find that refugees in Switzerland were more likely to commit property crime (e.g., shoplifting) than Swiss natives and other immigrant populations and argue that the difference is driven by psychosocial challenges of arriving in a new country as a refugee.

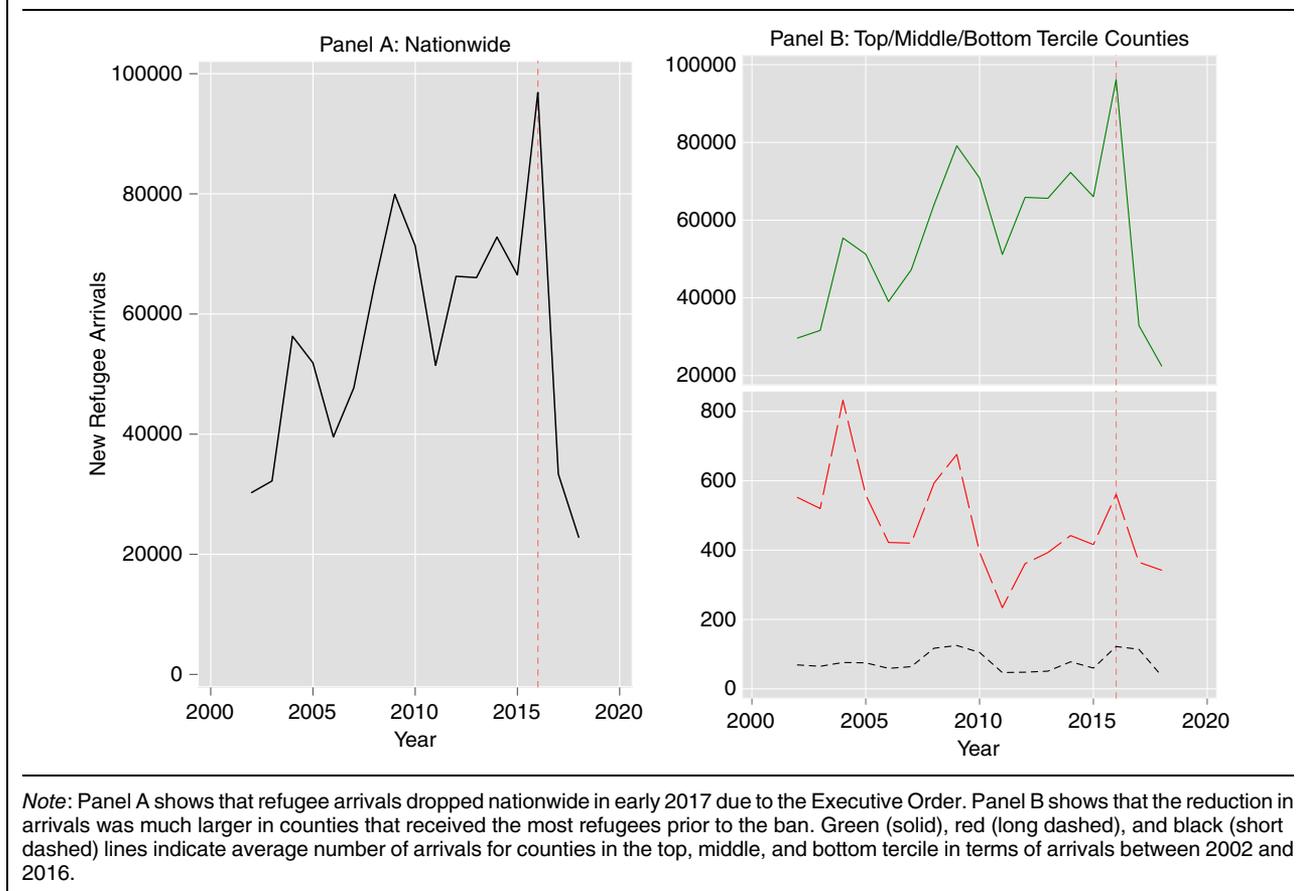
Another pathway that may link refugee resettlement to higher crime is that natives may commit more crimes targeting refugees as resettlement increases. Today refugee resettlement is a highly salient political issue in many countries, and climates of xenophobia and anti-immigrant sentiment may foster a high risk of antirefugee violence. Hangartner et al. (2019) show that exposure to refugees in Greece can increase anti-refugee hostility. There is also evidence from Germany that higher levels of refugee immigration led to more antirefugee violence (Marbach and Ropers 2018). Lastly, Müller and Schwarz (2017) show that right-wing hate speech on social media can incite higher levels of antirefugee violence.

On the other hand, refugees selected for resettlement may be less likely to commit crimes than natives. Formal resettlement systems, including the US program, explicitly attempt to screen out “high-risk” individuals. Multiple agencies such as the Central Intelligence Agency and the Department of Homeland Security screen resettlement applicants and run extensive background checks. Successful applicants are often subjected to further screening once they arrive on US soil. These programs typically prioritize applicants based on family reunion or vulnerability-based criteria, including injuries, medical problems, and other forms of hardship. By “selecting in” family-based and high-vulnerability cases, countries may be indirectly selecting a subpopulation with a low propensity to commit crimes.

Moreover, the structure of refugee resettlement may depress crime rates because many refugees are seeking permanent residency and citizenship and a criminal record would undermine this objective. Refugees are more likely to naturalize within six years than nonrefugee immigrants—45% compared with 29% (Mossaad et al. 2018). In the US, resettled refugees can apply for permanent residency and naturalization one and four years after arrival, respectively. Given that a criminal record could lead to an application being denied, refugees face higher costs for crimes than a similar native. In Germany, Lange and Sommerfeld (2018) compare crime propensity across nationalities of origin and find that asylum seekers who have higher ex ante probability of being granted asylum are less likely to commit crimes.

DATA

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 focus on a sample of 18,172 local law enforcement agencies that consistently report crime statistics throughout the 2010–2018 period. UCR contains information on reported incidents of violent crime (aggravated assault, rape, murder, and robbery) and property crime (burglary, theft, and motor vehicle theft). Following the crime literature, we convert the

FIGURE 1. Refugee Arrivals in the US and Research Design, 2002–2018

reported absolute number of crimes into crime rates per 100,000 population as our main outcome of interest and use a log transformation as an alternative specification.

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 US by country of origin and destination city. WRAPS is managed by the Bureau of Population, Refugees, and Migration and serves to provide a standardized management system and accountability to the US refugee resettlement program.¹ 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. Throughout this period, 787 counties in all 50 states received refugee arrivals.

We merge the data sources together and our analysis focuses on the county-year level covering the 2010–2018 period.² Last, we use county-level population

estimates from the American Community Survey (ACS) from Manson et al. (2020).

EMPIRICAL STRATEGY

Design

Figure 1 illustrates our research design. Panel A shows the large and sudden drop in refugee arrivals following the Executive Order in 2017. We exploit 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 counties that had received larger numbers of refugees prior to the ban. We use two specifications of the difference-in-differences estimator to analyze the effect of reducing refugee resettlement on crime rates. This approach compares changes in crime rates after the Executive Order in counties that received many refugees before the ban to crime rates in counties that received fewer refugees.

The identifying assumption states that in the absence of the policy change, crime in areas with larger drops in resettlement due to the Executive Order would have followed a trajectory (or trend) similar to areas with smaller reductions. See the Appendix for formal tests showing that areas with

¹ See the Appendix for more information on the US Refugee Admissions Program.

² County is the lowest level of geographical aggregation that allows for a consistent merge between the two data sources.

different levels of refugee resettlement were moving along similar crime trends before the ban, supporting our identifying assumption.

First-Differences Model

The first model we estimate is

$$\Delta crime_c^{pre-post} = \alpha_1 + \beta_1 \times \Delta refugees_c^{pre-post} + \phi_{state} + \varepsilon_c, \quad (1)$$

where c denotes county and s indexes states. The outcome variable, $\Delta crime_c^{pre-post}$, measures the 2015/16–2017/18 change in a separate crime type per 100,000 people (or in log number of crimes + 1). Similarly, the independent variable of interest, $\Delta refugees_c^{pre-post}$, is the corresponding change in refugee arrivals per 100 people (or in log number of refugees + 1), where $\Delta > 0 (< 0)$ denotes an increase (decrease) in refugee resettlement in county c from 2015/16 to 2017/18. We pool the 2015 and 2016 (2017 and 2018) data together to form an observation for the “pre” (“post”) Executive Order period. The term ϕ_{state} controls for state fixed effects allowing for state-specific crime trends during this period. The intercept is α_1 and ε_c is the error term. We cluster the standard errors by state. A positive sign on β_1 indicates that refugee resettlement is associated with an increase in crime rates. In the model where both variables are in expressed 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 percentage of change in the number of crimes for a 1% increase in refugee arrivals.

Continuous Difference-in-Differences Model

Second, we move on to a more rigorous model in which we use data from the entire sample period 2010–2018. Specifically, we estimate

$$crime_{ct} = \alpha_2 + \beta_2 \times refugees_c^{2016} \times \mathbf{1}(t > 2016) + \gamma_c + \delta_t + X_{ct} + \varepsilon_{ct}, \quad (2)$$

where c indexes counties, t denotes year, and $\mathbf{1}(t > 2016)$ is an indicator for years 2017 and 2018, corresponding to the period after the Executive Order. The outcome is a separate crime type measured in rate per 100,000 population (or the log number of crimes + 1). The treatment variable, $refugees_c^{2016}$, is the 2016 refugee arrivals per 100 population (or the log number of refugees + 1) and is designed to measure county-level reductions in resettlement due to the Executive Order. We include county fixed effects (γ_c) adjusting for permanent 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 cluster the standard errors by state. The intercept is α_2 and ε_{ct} is the

error term. Note that compared with the model above, the interpretation of β_2 is switched so that a negative sign would indicate that counties with higher levels of refugee resettlement in 2016 experienced larger drops in crime rates in 2017 and 2018. Thus, a negative sign on β_2 is consistent with the hypothesis that refugee resettlement leads to higher crime rates.

RESULTS

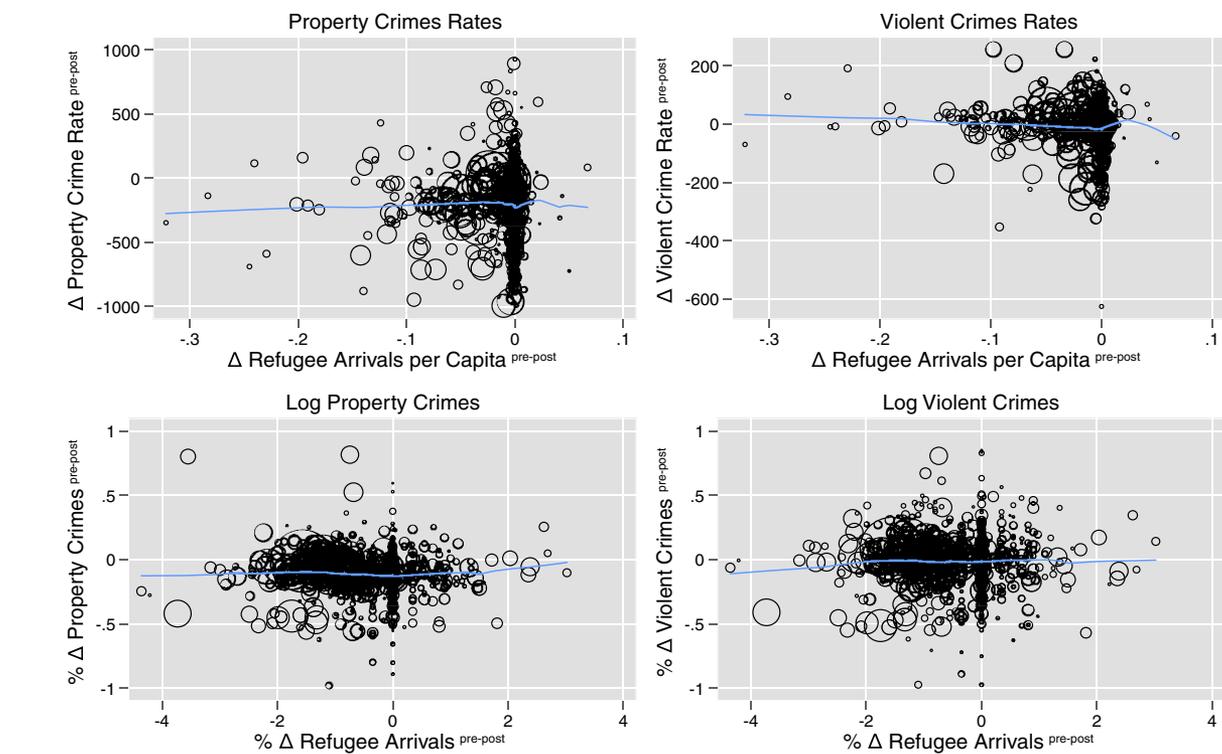
Figure 2 provides a graphical summary of the main findings. It plots the relationship between 2015/16–2017/18 changes in refugee arrivals and contemporaneous changes in crime rates along with the local nonparametric regression (LOESS) fit in blue. If refugee resettlement led to higher crime rates, we would observe an upward sloping regression line. Across both types of crimes (left versus right plots) and when measured in rates and logs (top versus bottom plots), we find no discernible relationship between the reduction in refugee arrivals due to the ban and subsequent changes in the local crime rates.

Table 1 presents the regression results using the first-difference model in Equation 1, which is equivalent to fitting a straight line in these scatterplots (with the potential for controlling for state fixed effects).³ All coefficients are small in substantive terms and none is statistically significant, indicating an absence of a relationship between resettlement and crime. We discuss the magnitude of effect estimates in the next section.

Table 2 presents the results from the continuous difference-in-differences model in Equation 2. Note again that here a positive coefficient indicates that higher refugee resettlement is associated with lower rates of crime. The table layout is similar to the one above with the exception that in the even-numbered columns we control for county-specific crime trends. All point estimates except for one are positive, and the one negative estimate is not statistically significant. The one statistically significant point estimate (in column 5) is positive, indicating that counties with larger reductions in refugee arrivals experienced larger increases in property crime rates following the ban.

Overall, the results provide little evidence that the reduction in refugee resettlement due to the ban had a discernible effect on crime rates. As with any statistical result, point estimates are (weighted) averages across the full sample and may mask variation within subgroups. We present a series of robustness checks in the Appendix such as subsetting to urban counties; using all 3,112 US counties; varying the specification of the statistical model (such as weighting and dropping outliers); adjusting for demographic control variables and spatially lagged crime rates; and testing for effects on internal migration, crime-reporting behavior, and a one-year crime lead. Evidence consistently provides

³ To improve precision, we drop counties with changes in crime rates larger than 1,000 in absolute value, which results in excluding 12 counties, on average. Our results remain qualitatively the same but are estimated less precisely in our full sample.

FIGURE 2. The Effect of the Executive Order on Local Crime Rates

Note: Crime trends and drop in refugee arrivals before (2015/2016) and after (2017/2018) the Executive Order for property (left panels) and violent (right panels) crimes. The top (bottom) two panels show variables measured in rates (logs). Each circle is a single county and its size is proportional to the 2016 population. LOESS fits are shown in blue lines.

no clear indication of a relationship between refugee resettlement and crime.

DISCUSSION

Effect Sizes and Estimates' Precision

To interpret the estimated effect sizes, we first need to consider whether the US program is sufficiently large to provide evidence of a meaningful null relationship. Until 2016, it was the world's largest program, resettling more refugees every year than the rest of the world combined (Connor and Krogstad 2018). The 2016/17 reduction in resettlement was, therefore, approaching the largest possible cut we could observe empirically. In other words, given the historical magnitude of refugee resettlement programs around the world, this case provides a critical test of the relationship between refugees and crime.

Next, we explore how precisely estimated the null effects we report are, thereby testing whether the study has sufficient power to detect reasonably small effects of the intervention. We begin by presenting the expected percentage of change in crime for a 1% increase in resettlement as predicted by our statistical models (corresponding to models in odd-numbered

columns in Tables 1 and 2). We then do the same for a one-standard-deviation increase in resettlement and compare those changes to the median crime rates. Results are presented in Table A19 in the Appendix.

Based on the results from the continuous difference-in-differences model, Panel B, columns 3 and 4, show that the estimated change in crime rates for a 1% increase in pre-ban refugee resettlement is 0.014% [-0.021%, 0.006%] for property crime, and 0.001% [-0.011%, 0.009%] for violent crime. In the rates models (Panel B, columns 1 and 2), the predicted change in property and violent crime rates for a one-standard-deviation shift in refugee resettlement are 14.597 [-15.544, 44.738] and -3.696 [-12.000, 4.608] respectively. These point estimates are very small in comparison with median county-level crime rates: 2,317.855 (property crime) and 254.387 (violent crime). The magnitudes of results in the first-difference models are similar (also presented in Panel B). Overall, the estimated effects are small in magnitude and precisely estimated in levels as well as logs, supporting an interpretation of the results as meaningful null findings.

Internal Migration Following Resettlement

How long do refugees that are resettled to one area reside there before moving elsewhere? If refugees

TABLE 1. The Effect of the Executive Order on Local Crime Rates: First-Differences, 2015–2018

	Crime rates				Log number of crimes			
	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Property	(6) Property	(7) Violent	(8) Violent
Δ refugees per capita ^{pre-post}	171.853 (185.996)	277.294 (175.790)	-59.126 (81.784)	-12.788 (82.462)				
Δ log(refugees) ^{pre-post}					-0.007 (0.010)	-0.012 (0.009)	0.003 (0.011)	-0.002 (0.009)
State FE		X		X		X		X
N	756	756	785	785	773	773	768	768
R ²	0.001	0.216	0.001	0.253	0.001	0.318	0.000	0.262

Note: Each column shows the estimated coefficients from a separate regression model. The unit of observation is a county. The outcome variable is denoted in the column header and expressed in pre–post changes in the crime rate per 100,000 people (columns 1–4) or log of number of crimes (columns 5–8). The independent variable is the pre–post change in refugee arrivals per 100 people (columns 1–4) or log number of refugees resettled (columns 5–8). The preperiod is 2015/2016 and the postperiod is 2017/2018. All regressions control for initial population size. Standard errors are clustered by state and shown in parentheses. **p* < 0.05, ***p* < 0.01, ****p* < 0.001.

TABLE 2. The Effect of the Executive Order on Local Crime Rates: Continuous Difference-in-Differences, 2010–2018

	Crime rates				Log number of crimes			
	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Property	(6) Property	(7) Violent	(8) Violent
Diff-in-Diff	-201.899 (207.559)	200.035 (206.458)	51.126 (57.183)	1.673 (50.694)				
Diff-in-Diff					0.013*** (0.004)	0.006 (0.006)	0.001 (0.005)	0.005 (0.005)
County Trends		X		X		X		X
N	7,065	7,065	7,065	7,065	7,065	7,065	7,065	7,065
R ²	0.910	0.952	0.936	0.965	0.974	0.984	0.975	0.985
\bar{Y}	2,382.4	2,382.4	306.0	306.0	7.9	7.9	5.8	5.8
SD(Y)	1,190.8	1,190.8	243.1	243.1	1.6	1.6	1.7	1.7

Note: Each column shows the estimated coefficients from a separate regression model. The unit of observation is a county-year. The outcome variable is denoted in the column header and expressed in pre–post changes in the crime rate per 100,000 people (columns 1–4) or natural logarithm of number of crimes (columns 5–8). The independent variable is the pre–post change in refugee arrivals per 100 people (columns 1–4) or natural logarithm of refugee resettled (columns 5–8). All regressions control for population size. The preperiod is 2010–2016 and the postperiod is 2017–2018. Standard errors are clustered by state and shown in parentheses. **p* < 0.05, ***p* < 0.01, ****p* < 0.001.

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. We use data from the US Office of Refugee Resettlement's (ORR) annual reports to calculate the number of refugees who made interstate moves in 2013 and 2014 (U.S. Department of Health and Human Services: Office of Refugee Resettlement 2014; 2015). Approximately 3.9% of refugees who had arrived in the past four years (after which they can apply for naturalization) moved per year.^{4,5} This shows that refugees do not relocate at high rates, mitigating the concern that the null result is driven by high secondary migration of resettled refugees. The data currently available from ORR provide estimates at the state level rather than county level, and intrastate mobility among this population could be significantly higher than interstate moves.⁶

CONCLUSION

In this letter we estimate the effect of a large and significant cut to the US refugee resettlement program and find that there is no discernible short-term effect on county-level property or violent crime rates. There are at least three factors that likely contribute to this result. The first is the selection process for refugees and extensive multiagency background checks. Candidates for resettlement go through several interviews and background checks before being admitted and 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, until the Executive Order, the US resettled more refugees each year than the rest of the world combined. Thus, this policy reversal represents about as large of a change as realistically possible. Nevertheless, its size is small relative to the population, so resettlement is unable to ultimately change local crime rates. A much larger resettlement program, which more profoundly altered the demographics of the US population, might have distinct equilibrium effects on the economy and society than what our study identifies.

⁴ We discuss our analysis of ORR data in the Appendix.

⁵ Using a longer time window and earlier study period, Mossaad et al. (2020) estimate that from 2000–2014 17% of refugees moved between states within two years of their arrival. Not only is the time window wider but also interstate mobility has trended downward in the US since 2000 (Molloy, Smith, and Wozniak 2011).

⁶ Based on tests shown in the Appendix, we find no evidence for the possibility that the Executive Order affected the migration choices of natives and other residents.

Third, the demographic composition of people resettled to the US 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 2018). In contrast, our calculations show approximately 14% of the refugees resettled to the US in 2016 were men within a similar age range.

After decades of increasingly liberal immigration policy in much of the Western world, the region appears to now be entering a period of hardening national boundaries. Understanding the effects of such immigration policy reversals will be critical for future research in the political economy of migration. Here we show that restricting refugee resettlement to the US is not an effective policy tool for reducing crime. This finding contributes to our understanding of a central element of political conflict and public opinion related to immigration policy.

SUPPLEMENTARY MATERIALS

To view supplementary material for this article, please visit <http://dx.doi.org/10.1017/S0003055421000150>.

DATA AVAILABILITY STATEMENT

Replication files are available at the American Political Science Review Dataverse: <https://doi.org/10.7910/DVN/KDOM7E>.

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