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

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1 Appendix

1.1 The US Refugee Admission Program

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

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

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

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

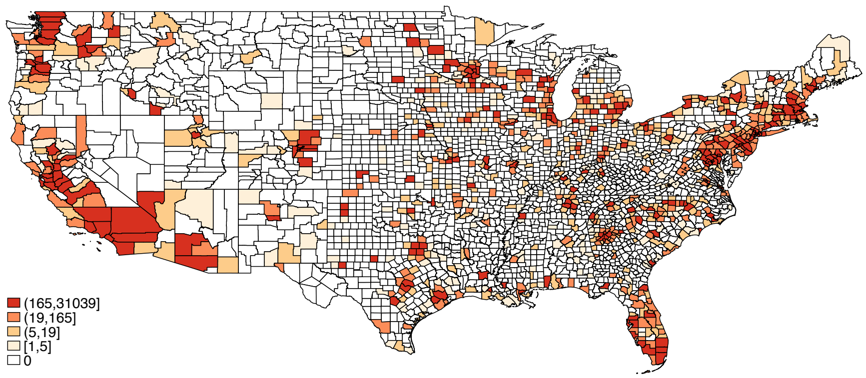
1.2 Descriptive Statistics

Table A1 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 2010–2018, resulting in 7,065 observations. All crime variables are right-skewed. The mean (median) property crime rate per 100,000 population was 2,385.21 (2,213.95) per county per year. The mean (median) violent crime rate per 100,000 population was 1,183.2 (1,065.68) per county per year. Because we use a logarithmic transformation as a robustness check, we present summary statistics for these variables as well. The bottom rows of Table A1 show summary statistics of our refugee arrival variables. Similar to the crime data, these variables are also right-skewed. The average county received 77.46 (2) refugees per year (per 100 people per year).

Figure A1 shows a map of cumulative refugee arrivals to the US in the time period 2002–2016 for each county. During the time period 787 counties, located in all 50 states, received some refugees. Darker shades of red denote higher refugee arrival levels and white denotes counties that received no refugees in our study period. This figure illustrates the non-random allocation of refugees to localities. In particular, refugees are more likely to be resettled in places near major urban centers such as parts of California, Washington, Florida and the Northeast.

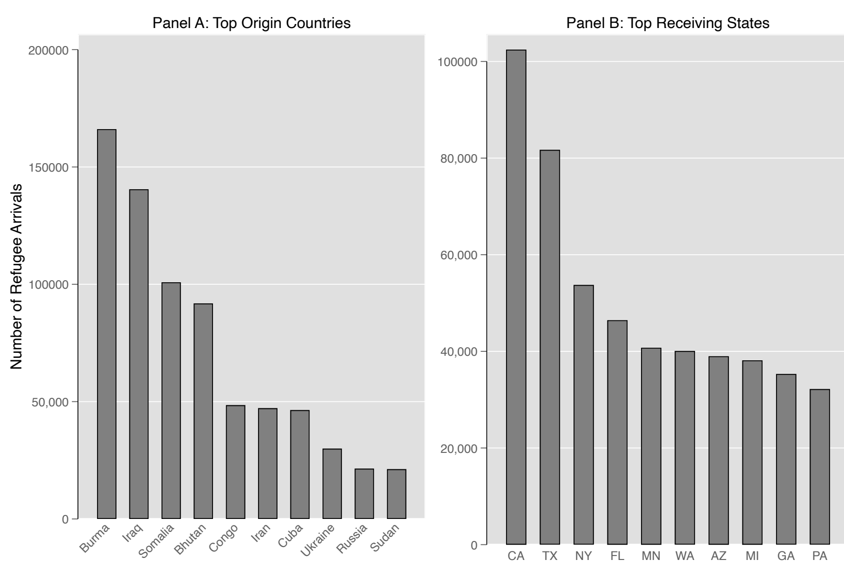
Next, the left panel in Figure A2 shows the top 10 refugee origin countries and the right panel displays the top ten receiving states. All numbers reflect cumulative values for the time period 2002-2016. The three largest sending countries are Burma (166,115), Iraq (140,468) and Somalia (100,850), and the three largest receiving states were California (102,444), Texas (81,697) and New York (53,737).

Figure A1: Cumulative Refugee Arrivals in the US by County, 2002–2016



Notes: Cumulative number refugee arrivals in the US for the period prior to the Executive Order, 2002–2016. Each polygon is a separate county. Darker shades of red correspond to higher number of refugee resettled.

Figure A2: Origins and Destinations for Refugee Arrivals in the US, 2002–2016



Notes: Panel A shows the top ten refugee sending countries and Panel B presents the top ten receiving states. All numbers reflect aggregate arrival values in the period prior to the Executive Order, 2002–2016.

Table A1: Descriptive Statistics

	Mean	Median	SD	Min	Max	Observations
	Crime Variables					
Property crimes rate	2382.41	2211.59	1190.76	0	9657.12	7065
Violent crimes rate	306.04	252.54	243.08	0	3323.85	7065
Log property crimes	7.95	8.04	1.61	0	12.44	7065
Log violent crimes	5.78	5.77	1.73	0	10.98	7065
	Refugee Resettlement Variables					
Refugee arrivals	77.46	0.00	252.93	0	3474.00	7065
Refugee arrivals per 100 people	0.02	0.00	0.07	0	1.78	7065
Log refugee arrivals	1.61	0.00	2.14	0	8.15	7065
Population (in 100,000s)	3.09	1.42	5.84	0	101.06	7065
Observations	7065					

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 2010–2018. Crime data comes from 18,172 local law enforcement units which consistently report data for the entire period without missing entries.

1.3 Pre-trends

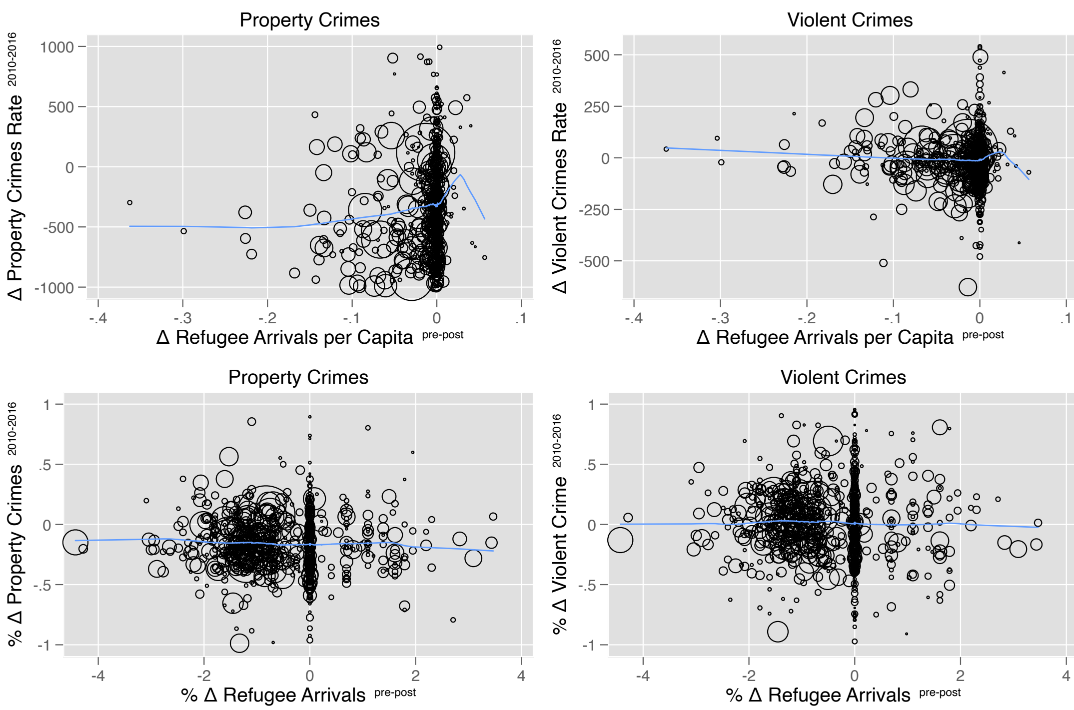
To test for violation of the identifying parallel trends assumption, we correlate the 2010–2016 county-level crime trends with the 2015/16–2017/18 drop in refugee arrivals. This test assesses whether crime trends predating the Executive Order are associated with the drop in arrivals due to the refugee ban. Given that the ban was based on national consideration and not on local conditions, we do not expect the two would be correlated. We cluster standard errors by state.

The results are shown in Table A2 and Figures A3 and A4. We find no meaningful relationship between crime pre-trends and the observed 2015/16–2017/18 change in refugee resettlement. Figure A3 presents the results for crime and resettlement measured in rates (top) and logs (bottom) with scatter plots of pre-existing crime trends and drop in resettlement due to the ban . Figure A4 presents crime trends by high/low/medium refugee receiving counties, to search for visual evidence of parallel time crime trends across refugee resettlement terciles. Figure A5 uses an event-study approach to explore for possible violations of parallel pre-trends, comparing the year-to-year relationship between refugee resettlement and crime rates, relative to 2016.

If anything, two of the regression coefficients in the log-log specification in Table A2 (Columns 6 and 8) are negative and statistically significant indicating that counties that lost more refugees may have been on declining crime trends. If true, this would bias our results in the direction of finding that refugee resettlement increases crimes. Nevertheless, this result is not observed in the other six columns of the table or in any of the plots in Figures A3 and A4.

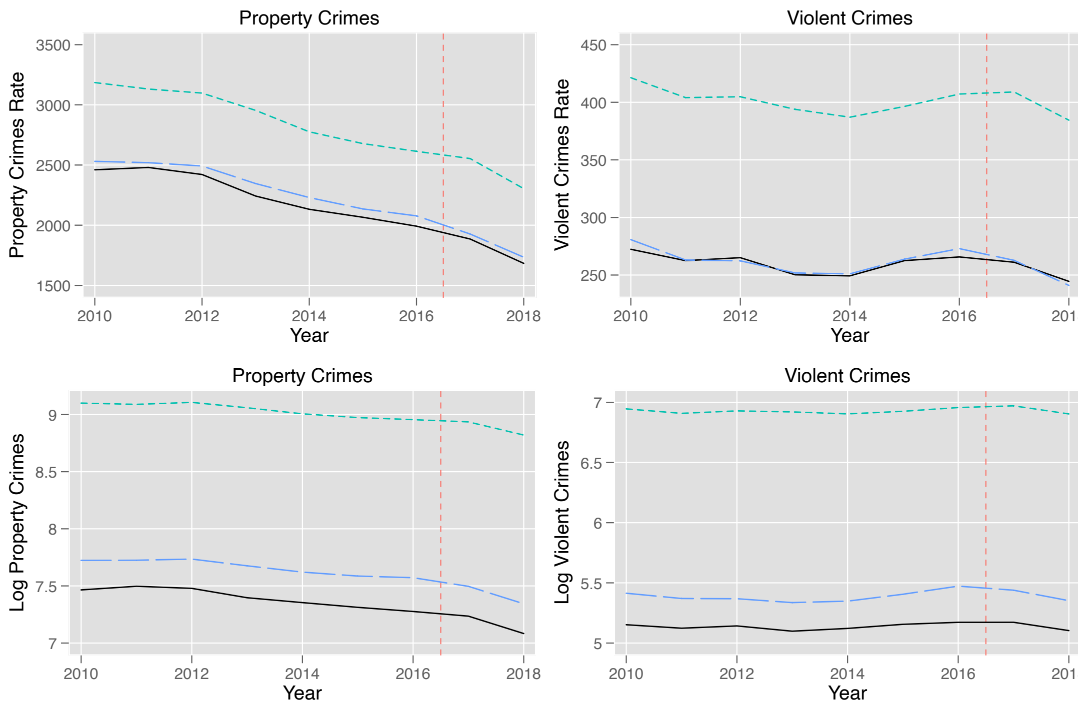
All in all, we conclude that places with differential reductions in resettlement due to the refugee ban were not on different crime trend trajectories before the policy reversal. Consequently, it is reasonable to assume that these counties would have continued on such parallel crime trends had the ban not occurred and that our research design is valid.

Figure A3: Pre-existing Crime Trends and Drop in Refugee Arrivals



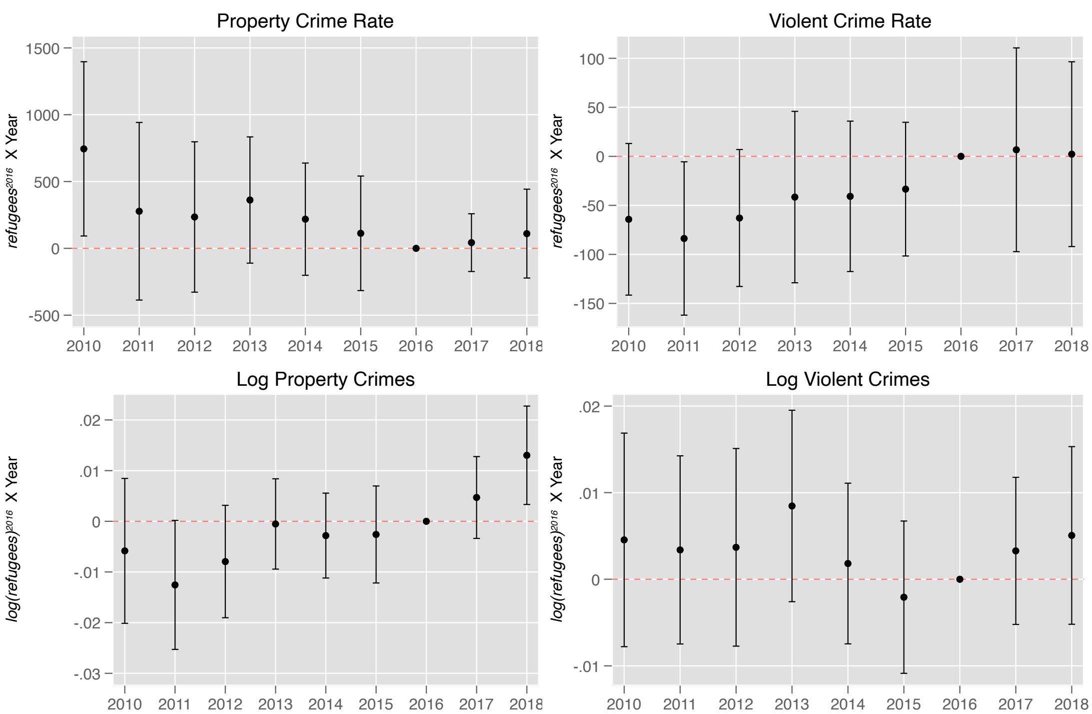
Notes: The top two panels show crime trends between 2010 and 2016 and drop in refugee arrivals due to the Executive Order for property (left) and violent (right) crimes. Local non-parametric regression (LOESS) fits are shown in blue lines. Each circle is a single county and its size is proportional to the 2016 population. The data in the top (bottom) figures are expressed in rates (logarithm). Positive (negative) values in the horizontal axes denote an increase (decrease) in refugee resettlement from 2015/16 to 2017/18.

Figure A4: Pre-existing Crime Trends and Drop in Refugee Arrivals: Robustness Check 1



Notes: Crime trends by top/middle/bottom tercile refugee receiving counties for property (left) and violent (right) crimes. The data in the top (bottom) figures are expressed in rates (logarithm).

Figure A5: Pre-existing Crime Trends and Drop in Refugee Arrivals: Robustness Check 2



Notes: Coefficients of interactions of exposure to the ban (number of refugees in 2016 per 100 people) and year dummies for property (left) and violent (right) crimes. Year 2016 is omitted and serves as the reference point. The regression model is otherwise identical to that shown in Table 2. The data in the top (bottom) figures are expressed in rates (logarithm). Vertical lines correspond to 95% confidence intervals.

Table A2: Pre-existing Crime Trends and Drop in Refugee Arrivals: Regression Results

	Crime Rates				Log Number of Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Property	Property	Violent	Violent	Property	Property	Violent	Violent
Δrefugees per capita$^{\text{pre-post}}$	751.236*	741.580*	-152.333	-97.552				
	(340.437)	(298.238)	(100.035)	(62.687)				
Δlog(refugees)$^{\text{pre-post}}$					0.005	-0.004	0.007	-0.003
					(0.010)	(0.008)	(0.013)	(0.013)
State FE		X		X		X		X
N	685	685	785	785	770	770	759	759
R^2	0.006	0.262	0.005	0.146	0.000	0.277	0.000	0.153

Notes: Each column shows the estimated coefficients from a separate regression model. The outcome variable is denoted in the column header and expressed in 2010–2016 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). The unit of observation is a county. Standard errors are clustered by state and shown in parentheses. All regressions are unweighted. To reduce noise in the data, we drop counties with changes in crime rates larger than 1000 in absolute value; our results remain qualitatively the same but are estimated less precisely in our full sample. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$

Identifying Assumptions in a Difference-in-differences Design

Here we provide a critical discussion of the identifying assumptions in a difference-in-differences research design (see, e.g., Lechner, 2011).

1. SUTVA, no general equilibrium effects or spillover

Previous studies of crime have identified the possibility that policing that reduces crime in targeted communities may push perpetrators into neighboring areas that did not experience increases in policing (Blattman et al. 2017). In our context, we might be concerned that if refugees increase (decrease) crime upon arriving in resettlement counties, they could have a spillover effect on other counties that leads to a similar increase (decrease) in crime there. Such spillover effects could mean that refugee resettlement does in fact increase (decrease) crime rates, but we would fail to detect that change because we difference out the concomitant spillover effects onto comparison counties.

Our main analysis includes all counties that received at least one refugee during the data period (2010–2018) totaling 787 counties. We also run analysis using all 3,112 US counties. The results are qualitatively identical for the regressions with 787 counties and for all counties, suggesting that spillover effects likely are not playing a crucial role.

We also run the test presented in Bianchi et al. (2012) for spatial spillovers, re-running our regression with all counties, but this time adjusting for spatially lagged crime rates. As in Bianchi et al. (2012) this denotes calculating for each county a weighted average of crime rates in neighboring counties, with weights defined by the distance between county centroid. Results from this regression are presented in Table A5. Results are qualitatively identical to our main results, providing further evidence that spatial spillovers do not play an important role for interpreting our results.

2. Exogeneity of conditioning variables

Treatment should not affect conditioning variables. The variables we include, time and county indicators, and a linear time trend that is effectively an interaction of time trends and county indicators, are exogenous by construction.

3. No effect of treatment on the treated before treatment occurs [treatment had no effect on the pre-treatment population]

The treatment in our research design is a cut to refugee resettlement. Our research design would be undermined if the policy impacted outcomes in previous time periods, such as if some individuals, institutions, or markets anticipated the policy and reacted before the policy was implemented. So, did Executive Order 13769 affect crime rates before it was announced in January 2017? On January 27, 2017, President Trump signed the Executive Order, 80 days after he had won the US presidential election on November 8, 2016. Donald Trump did not win the presidential election until nearly the end of our 2010-2016 pre-treatment period. Even after that point although more restrictive immigration policies may have been anticipated after the election, during the final two months of 2016, the speed and scale of the order were a shock when it was announced in January. It is unlikely that any anticipation of the policy would have changed how counties or individuals acted before the ban went into effect due to anticipation of this policy in any way sufficient to change crime rates for our pre-period of 2010-2016.

4. Parallel trends

We provide extensive tests of the parallel trends assumption in Appendix Section 1.3. The parallel trends assumption is ultimately about counterfactual and unobservable behavior of after treatment if units had received different levels of treatment, and therefore cannot be directly tested.

That being said, results from pre-treatment periods support the credibility of the assumption. Our study has the benefit of including data from seven pre-treatment years (2010-2016) increasing our ability to detect differential pre-treatment time trends if they existed.

5. Common support

Although the preceding assumptions are all formulated in terms of unobservable random variables and are not testable, we can in fact test the fifth assumption. With sufficient density in these distributions across treatment levels, we can demonstrate sufficient common support to avoid effect estimates being fragile and model-dependent. We can analyze this by dividing observations into bins with respect to treatment and covariates and checking common support. Reassuringly, the results presented below provide strong evidence of common support with respect to our covariates.

The only covariates in our main specification are state and year fixed effects. By design, year fixed effects are balanced between treatment and control counties. However, this is not necessarily true for the state fixed effects.

Since our treatment variable is continuous, we separate all counties in our sample into two bins depending on the value for our treatment variable for the resettlement rate specification (number of refugees in 2016 per 100 capita)—above median ("treatment") and below median ("control"). We then check whether each state has counties in both bins (i.e., common support). Reassuringly, we find that this is true for all states except for Delaware and Hawaii (only in control) and Nevada and Wyoming (only in treatment).

We then re-run our main regression specification without these four states in the sample. The results are presented in Table A11. Reassuringly, the results align with the results in our main model. If anything, the one statistically significant coefficient (column 5) is consistent with refugees decreasing crime rates.

1.4 Robustness Checks

1.4.1 Adding Demographic Controls

- It is possible that other population changes may have occurred during the study time frame correlated with crime and refugee resettlement.
- Therefore we replicate Tables 1 and 2 and add the following control variables: (log) population, share Black, share White, share Hispanic, share high school dropouts, share high school graduates, unemployment rate, share out of labor force, share males ages 15-34.

Table A3: The Effect of the Executive Order on Local Crime Rates: First Differences, Adding Controls

	Crime Rates				Log Number of Crimes			
	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Property	(6) Property	(7) Violent	(8) Violent
Δrefugees per capita$^{\text{pre-post}}$	314.863	347.484	-30.173	-10.668				
	(186.002)	(190.039)	(78.790)	(69.120)				
Δlog(refugees)$^{\text{pre-post}}$					0.002	0.002	0.008	0.007
					(0.009)	(0.008)	(0.008)	(0.008)
State FE		X		X		X		X
N	756	756	785	785	773	773	768	768
R^2	0.051	0.241	0.052	0.277	0.084	0.360	0.058	0.276

Notes: As in Table 1 except that all regressions control for the 2016 values of (log) population, share Black, share White, share Hispanic, share high school dropouts, share high school graduates, unemployment rate, share out of labor force, share males ages 15-34. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A4: The Effect of the Executive Order on Local Crime Rates: Continuous Difference-in-Differences, Adding Controls

	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	-106.558	107.680	47.041	2.170				
	(196.045)	(230.128)	(53.452)	(45.452)				
Diff-in-Diff					0.010**	0.004	-0.001	0.007
					(0.003)	(0.006)	(0.004)	(0.005)
County Trends		X		X		X		X
N	6232	6232	6232	6232	6232	6232	6232	6232
R^2	0.915	0.954	0.941	0.969	0.977	0.987	0.977	0.986
$\bar{Y}$	2348.1	2348.1	305.0	305.0	8.0	8.0	5.8	5.8
sd(Y)	1174.2	1174.2	242.6	242.6	1.6	1.6	1.7	1.7

Notes: As in Table 2 except that all regressions control for the contemporaneous (log) population, share Black, share White, share Hispanic, share high school dropouts, share high school graduates, unemployment rate, share out of labor force, share males age 15-34. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.4.2 Adding Spatial Spillovers in Crime as a Control

- Previous studies have demonstrated the existence of crime-related spatial spillovers (see, e.g., Blattman et al. 2017). Therefore, we follow the procedure outlined in Bianchi et al. (2012) to control for spatial spillovers in crime.

- Specifically, for each county (and year) in our sample, we calculated a weighted average of all crime variables among the neighboring counties (in the same year) where the weights were the distances between the counties' centroids. We used this and this sources.

- We then ran our main specification with controlling for this spatial crime variable.

Table A5: The Effect of the Executive Order on Local Crime Rates: Controlling for Spatial Spillovers

	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	-430.251*	154.433	36.455	3.212				
	(205.169)	(182.200)	(50.600)	(50.194)				
Diff-in-Diff					0.010*	0.005	0.000	0.004
					(0.004)	(0.006)	(0.005)	(0.005)
County Trends		X		X		X		X
N	6795	6795	6795	6795	6795	6795	6795	6795
R^2	0.920	0.956	0.940	0.967	0.974	0.984	0.975	0.985
$\bar{Y}$	2379.6	2379.6	303.3	303.3	7.9	7.9	5.7	5.7
sd(Y)	1196.4	1196.4	243.5	243.5	1.6	1.6	1.7	1.7

Notes: As in Table 2 except that we control for the spatial crime lag variable defined above. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.4.3 Subset to Urban Counties

- Heterogeneous crime responses to demographic changes may be relevant in the study's context, and a null effect may conceal differential effects within subgroups.
- Therefore, we replicate Table 2 on the subset of counties with above-median population density, to test for evidence of an effect in urban areas.

Table A6: The Effect of the Executive Order on Local Crime Rates: Urban Counties, 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	-290.405	177.183	27.595	-19.312				
	(252.426)	(231.958)	(54.302)	(48.558)				
Diff-in-Diff					0.013***	0.006	-0.000	0.000
					(0.004)	(0.004)	(0.004)	(0.004)
County Trends		X		X		X		X
N	3537	3537	3537	3537	3537	3537	3537	3537
R^2	0.928	0.968	0.960	0.982	0.976	0.989	0.979	0.989
$\bar{Y}$	2624.1	2624.1	355.2	355.2	8.9	8.9	6.7	6.7
sd(Y)	1237.3	1237.3	286.8	286.8	1.2	1.2	1.5	1.5

Notes: As in Table 2 except we focus on our sample of counties with above median population density. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.4.4 Use All US Counties

- Replicating Table 2 with all US counties (regardless of whether they have had a resettled refugee since 2010).
- To accommodate the skewness of the right hand side variable (measure of exposure to the Executive Order) that is introduced by added a large number of counties that never receive refugees, we bin this variable into three groups: counties with no refugees ("No Refugees"), counties with below median number of refugees ("Low Refugees") and counties with above median number of refugees (the omitted group). The table presents the coefficients of the interactions of "No Refugees' and "Low Refugees" variables with year dummies indicating the post-Executive Order period. The hypothesis that refugees increase crime rates would be consistent with negative and statistically significant coefficients.

Table A7: The Effect of the Executive Order on Local Crime Rates: All US Counties, 2010–2018

	Crime Rates				Log Number of Crimes			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Property	Property	Violent	Violent	Property	Property	Violent	Violent
No Refugees × Post	145.093**	-42.441	2.983	-7.763				
	(47.124)	(47.805)	(10.162)	(7.548)				
Low Refugees × Post	9.747	-49.091	-16.446*	-12.306				
	(42.396)	(45.655)	(6.301)	(7.874)				
No Refugees × Post					-0.105*	-0.035	-0.026	-0.009
					(0.041)	(0.039)	(0.036)	(0.036)
Low Refugees × Post					-0.027	-0.014	-0.026	-0.001
					(0.031)	(0.044)	(0.030)	(0.045)
County Trends		X		X		X		X
N	27999	27999	27999	27999	27999	27999	27999	27999
R^2	0.852	0.903	0.819	0.889	0.953	0.974	0.951	0.969
$\bar{Y}$	1794.2	1794.2	241.7	241.7	5.9	5.9	3.9	3.9
sd(Y)	1210.5	1210.5	241.8	241.8	2.1	2.1	2.0	2.0

Notes: As in Table 2 except we include all US counties. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.4.5 Weighted Regressions

- Replicating Tables 1 and 2 with weighting each regression by the 2016 population.

Table A8: The Effect of the Executive Order on Local Crime Rates: First Differences, Weighted Regressions

	Crime Rates				Log Number of Crimes			
	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Property	(6) Property	(7) Violent	(8) Violent
Δrefugees per capita$^{\text{pre-post}}$	163.257	283.024	-74.545	-20.501				
	(203.336)	(184.369)	(81.744)	(80.986)				
Δlog(refugees)$^{\text{pre-post}}$					-0.007	-0.012	0.004	-0.001
					(0.010)	(0.009)	(0.011)	(0.009)
State FE		X		X		X		X
N	756	756	785	785	773	773	768	768
R^2	0.001	0.235	0.002	0.277	0.001	0.341	0.000	0.286

Notes: As in Table 1 except that all regressions are weighted by the 2016 population. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A9: The Effect of the Executive Order on Local Crime Rates: Continuous Difference-in-Differences, Weighted Regressions

	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	-207.877	224.315	62.945	13.150				
	(228.400)	(222.888)	(60.163)	(51.434)				
Diff-in-Diff					0.014***	0.006	0.001	0.005
					(0.003)	(0.005)	(0.005)	(0.004)
County Trends		X		X		X		X
N	7065	7065	7065	7065	7065	7065	7065	7065
R^2	0.912	0.954	0.941	0.969	0.975	0.985	0.977	0.986
$\bar{Y}$	2422.8	2422.8	315.2	315.2	8.1	8.1	5.9	5.9
sd(Y)	1197.9	1197.9	251.7	251.7	1.6	1.6	1.7	1.7

Notes: As in Table 2 except that all regressions are weighted by the 2016 population. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.4.6 First-Differences Model without Dropping Outliers

- Replicating Table 1 without dropping counties with changes in crime rates larger than 1000 in absolute value.

Table A10: The Effect of the Executive Order on Local Crime Rates: First Differences, All Sample Counties

	Crime Rates				Log Number of Crimes			
	(1) Property	(2) Property	(3) Violent	(4) Violent	(5) Property	(6) Property	(7) Violent	(8) Violent
Δrefugees per capita$^{\text{pre-post}}$	1.251	266.795	-59.126	-12.788				
	(281.267)	(266.056)	(81.784)	(82.462)				
Δlog(refugees)$^{\text{pre-post}}$					-0.003	-0.002	-0.004	-0.003
					(0.017)	(0.019)	(0.016)	(0.016)
State FE		X		X		X		X
N	785	785	785	785	785	785	785	785
R^2	0.000	0.225	0.001	0.253	0.000	0.100	0.000	0.174

Notes: As in Table 1 except that we do not drop any observations. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.4.7 Common Support Assumption

- As discussed in the Appendix section titled, "Identifying Assumptions in a Difference-in-differences Design," covariates in empirical models should have common support across treatment levels. The only covariates in our main specification are state and year fixed effects. By design, year fixed effects are balanced between the "treatment" and "control" counties. However, it is not clear that this is true for the state fixed effects.
- To test for common support among the state dummies, we separated all counties in our sample into 2 bins depending on the value of our treatment variable (in the rate specification, number of refugees in 2016 per 100 capita) – above ("treatment") and below ("control") the median value.
- We then checked whether each state has counties in both bins. This was true for all states except for Delaware (contained counties only in the "control" group), Hawaii (only in "control"), Nevada (only in "treatment") and Wyoming (only in "treatment").
- Lastly, we ran our main specification when dropping these four states from the sample.

Table A11: The Effect of the Executive Order on Local Crime Rates: Dropping States Without Common Support

	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	-200.757	207.272	51.031	3.417				
	(208.427)	(208.413)	(57.370)	(51.447)				
Diff-in-Diff					0.013**	0.006	0.001	0.006
					(0.004)	(0.006)	(0.005)	(0.005)
County Trends		X		X		X		X
N	6993	6993	6993	6993	6993	6993	6993	6993
R^2	0.911	0.953	0.936	0.965	0.974	0.984	0.975	0.985
$\bar{Y}$	2377.7	2377.7	305.1	305.1	7.9	7.9	5.8	5.8
sd(Y)	1194.7	1194.7	243.4	243.4	1.6	1.6	1.7	1.7

Notes: As in Table 2 except that the states Delaware, Hawaii, Nevada and Wyoming were dropped from the sample. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.5 Alternative Outcome Variables

1.5.1 Testing for Effects on One-Year Lead Crime Rates

- Crime may take more than a year to manifest an effect from a demographic shock.
- Therefore, we replicate Table 2 with a one-year lead crime as the outcome.

Table A12: The Effect of the Executive Order on Local Crime Rates: Continuous Difference-in-Differences, One-year Lead Crime as Outcome

	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	-67.957	135.274	38.749	-19.157				
	(211.274)	(186.552)	(48.972)	(32.025)				
Diff-in-Diff					0.016***	0.007	0.002	0.005
					(0.004)	(0.004)	(0.005)	(0.004)
County Trends		X		X		X		X
N	6280	6280	6280	6280	6280	6280	6280	6280
R^2	0.913	0.953	0.940	0.968	0.976	0.985	0.976	0.985
$\bar{Y}$	2341.1	2341.1	303.9	303.9	7.9	7.9	5.8	5.8
sd(Y)	1176.4	1176.4	242.3	242.3	1.6	1.6	1.7	1.7

Notes: As in Table 2 except that we use one-year lead crime as the outcome variables. $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.5.2 Share In-Movers

- A potential concern is that natives or other foreigners might migrate internally as a response to lower refugee arrivals induced by the Executive Order. This might bias our results if the internal migration somehow affects crime rates.

- To test for this, we ran our regression models with the outcome variable being the number of people who were in a "different house in US 1 year ago in a different MSA" per 100,000 people and the log number of people who were in a "different house in US 1 year ago in a different MSA". Data come from Manson et al. (2020).

Table A13: The Executive Order and Internal In-migration: First Differences

	Rates		Log	
	(1)	(2)	(3)	(4)
	Δ Share In-movers	Δ Share In-movers	% Δ In-movers	%Δ In-movers
Δrefugees per capita$^{\text{pre-post}}$	73.320	186.099		
	(578.927)	(568.016)		
Δlog(refugees)$^{\text{pre-post}}$			-0.002	-0.000
			(0.006)	(0.007)
State FE		X		X
N	785	785	785	785
R^2	0.000	0.083	0.000	0.219

Notes: As in Table 1 except that the outcome variable is the pre–post change in the number of people who were in a "different house in US 1 year ago in a different MSA" per 100,000 population (columns 1-2) and the change in the log number of people who were in a "different house in US 1 year ago in a different MSA" (columns 3-4). $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A14: The Executive Order and Internal In-migration: Continuous Difference-in-Differences

	Rates		Logs	
	(1)	(2)	(3)	(4)
	Share In-movers	Share In-movers	Log # In-movers	Log # In-movers
Diff-in-Diff	-135.916	-417.353		
	(392.789)	(364.075)		
Diff-in-Diff			-0.013	0.021*
			(0.008)	(0.008)
County Trends		X		X
N	7065	7065	7065	7065
R^2	0.951	0.983	0.968	0.989
$\bar{Y}$	2830.1	2830.1	6.6	6.6
sd(Y)	2448.6	2448.6	4.0	4.0

Notes: As in Table 2 except that the outcome variable is the number of people who were in a "different house in US 1 year ago in a different MSA" per 100,000 population (columns 1-2) and the log number of people who were in a "different house in US 1 year ago in a different MSA" (columns 3-4). $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.5.3 Police Behavior and Crime Reporting

- A potential concern is that the change in refugee resettlement induced by the Executive Order might be correlated with changes in police behavior or effort. For instance, it might be that the documented number of crimes remains unchanged while the actual number of committed crimes increases (or decreases).
- We conducted two tests which might address this concern. First, we ran regressions in which the outcome was the number of "total offenses cleared by arrest or exceptional means divided by the number of actual offenses." Data come from Manson et al. (2020).

Table A15: The Executive Order and the Share of Cleared Crimes: First Differences

	Rates		Log	
	(1)	(2)	(3)	(4)
	Share Cleared Property	Share Cleared Property	Share Cleared Violent	Share Cleared Violent
Δrefugees per capita$^{\text{pre-post}}$	-0.028	-0.006		
	(0.068)	(0.051)		
Δlog(refugees)$^{\text{pre-post}}$			-0.003	-0.004
			(0.003)	(0.004)
State FE		X		X
N	785	785	782	782
R^2	0.000	0.030	0.001	0.106

Notes: As in Table 1 except that the outcome variable is the share of "total offenses cleared by an arrest or exceptional means". $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A16: The Executive Order and the Share of Cleared Crimes: Continuous Difference-in-Differences

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Property	Property	Violent	Violent	Property	Property	Violent	Violent
Diff-in-Diff	-0.023	0.039	0.040	0.030				
	(0.031)	(0.059)	(0.036)	(0.031)				
Diff-in-Diff					-0.001	0.003	0.000	0.002
					(0.002)	(0.004)	(0.001)	(0.002)
County Trends		X		X		X		X
N	7051	7051	7027	7027	7051	7051	7027	7027
R^2	0.182	0.206	0.754	0.813	0.182	0.206	0.754	0.813
$\bar{Y}$	0.2	0.2	0.5	0.5	0.2	0.2	0.5	0.5
sd(Y)	0.3	0.3	0.2	0.2	0.3	0.3	0.2	0.2

Notes: As in Table 2 except that the outcome variable is the share of "total offenses cleared by an arrest or exceptional means". $^*p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

- Second, we ran regressions in which the outcome variable was the share of male law enforcement agents. Data come from Manson et al. (2020).

Table A17: The Executive Order and the Share of Male Law Enforcement Agents: First Differences

	Rates		Log	
	(1) Share Male Police	(2) Share Male Police	(3) Share Male Police	(4) Share Male Police
Δrefugees per capita$^{\text{pre-post}}$	-0.198	-0.221		
	(0.150)	(0.159)		
Δlog(refugees)$^{\text{pre-post}}$			-0.000	0.002
			(0.002)	(0.003)
State FE		X		X
N	785	785	785	785
R^2	0.014	0.084	0.000	0.069

Notes: As in Table 1 except that the outcome variable is the pre-post change in the share of men among all employees in "protective service occupations: law enforcement." $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

Table A18: The Executive Order and the Share of Male Law Enforcement Agents: Continuous Difference-in-Differences

	Rates		Logs	
	(1) Share Male Police	(2) Share Male Police	(3) Share Male Police	(4) Share Male Police
Diff-in-Diff	0.174*	0.162		
	(0.066)	(0.124)		
Diff-in-Diff			0.001	0.000
			(0.001)	(0.002)
County Trends		X		X
N	7062	7062	7062	7062
R^2	0.552	0.771	0.550	0.770
$\bar{Y}$	0.8	0.8	0.8	0.8
sd(Y)	0.1	0.1	0.1	0.1

Notes: As in Table 2 except that the outcome variable is the share of men among all employees in "protective service occupations: law enforcement." $^{*}p < 0.05$, $^{**}p < 0.01$, $^{***}p < 0.001$.

1.6 Secondary migration

- To estimate the magnitude of secondary refugee migration to a different state after arrival, we use ORR data. For reasons of data protection, ORR only publishes aggregate (state-level) statistics on secondary in- and out-migration.

- Ideally, we would use data from 2017 and 2018 but it is not available. The closest time period we found was 2013 and 2014. The only other source of refugee secondary migration we identified were averages from 2000–2014 (Mossaad et al., 2020). Because inter-state migration among the general population has been trending downward since 2000 (Molloy et al., 2011), we sought more recent estimates of migration.

- We use data from the US Office of Refugee Resettlement's (ORR) annual reports to calculate the number of refugees who made inter-state moves in 2013 and 2014 (U.S. Department of Health and Human Services: Office of Refugee Resettlement, 2014, 2015). We estimate that approximately 3.9% of refugees who had arrived in the past four years moved per year.

- We use this four-year time window since after that period, resettled refugees can apply for naturalization.

- ORR publishes how many refugees (including those formally resettled to the US and Cuban and Haitian refugees, which constitute a different program) pre-naturalization moved to a different state each year.

- For each year, 2013 and 2014, we divide the number of movers by the total number of refugees resettled (and Cuban and Haitian refugee arrivals) in the previous 4 years (2010–2013 and 2011–2014, respectively).

- Since the data on arrivals and movers for 2013 (and 2014) include both new arrivals in that year and people who had previously arrived and moved in that year, we estimate inter-state moving rates for each year with and without new arrivals from the year. This produces upper and lower bounds, respectively, for the estimates.

- We take the mean of those two estimates for 2013 and 2014 to calculate our final inter-state moving estimate, 3.9%.

1.7 Precision

- To gauge the precision of our estimates of the impact of halting refugee resettlement on crime, we begin by calculate the change in crime following a one standard deviation (or a one percent) increase in refugee resettlement as predicted by our statistical models presented in the odd-numbered columns in Tables 1 and 2.
- In order to interpret the magnitude of the predicted changes in Columns 1 and 2, we present the median crime rates above in the same columns.
- For the log specification, we present estimates in percent changes for a 1 percent increase in resettlement in Columns 3 and 4.

Table A19: Statistical Precision of the Estimated Impact of Refugee Resettlement on Crime

	Crime Rates		Log Number of Crimes	
	(1) Property	(2) Violent	(3) Property	(4) Violent
	Panel A: Median Crime Values, 2010–2016			
Median Value	2317.855	254.387	–	–
	(19.366)	(3.274)	–	–
	Panel B: Δ Crime for a Given Increase in Resettlement			
	1 SD Increase		1% Increase	
First-Differences	12.425	-4.275	-0.007%	0.003%
Model	[-14.585, 39.435]	[-16.151, 7.602]	[-0.026%, 0.013%]	[-0.019%, 0.025%]
Continuous	14.597	-3.696	-0.014%	-0.001%
Diff-in-Diff Model	[-15.544, 44.738]	[-12.000, 4.608]	[-0.021%, -0.006%]	[-0.011%, 0.009%]

Notes: Panel A shows the median crime values for number of crimes per 100,000 people (columns 1-2) and log absolute number of crimes (columns 3-4) in the pre-Executive Order period, 2010–2016. Standard errors are estimated via quantile (median) regression and shown in parenthesis. Panel B presents the estimated change in crime following a one standard deviation (columns 1-2) or a one percentage point (columns 3-4) increase in refugee resettlement predicted by our first–differences model (Table 2, odd-numbered columns) and our continuous difference–in–differences model (Table 3, odd-numbered columns). 95% confidence intervals are shown in brackets. Values in columns 3 and 4 in Panel B are presented in percent, not log points.

Appendix References

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