MIGRANT BUSING AND CRIME: A SYNTHETIC APPROACH

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Matthew Gensburg

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Matthew Gensburg

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Economics

Abstract

This study examines the impact of Texas Governor Greg Abbott's migrant busing policies on urban crime rates. Employing a synthetic control method, I create a counterfactual scenario to isolate the causal effect of this influx of immigrants, controlling for socioeconomic and demographic characteristics. Results indicate no substantial impact on crime rates. This study contributes to the broader study of immigration and crime by focusing on a targeted, mass-immigration event rather than immigration as a whole. Policy implications are discussed, emphasizing the necessity for evidence-based dialogue.

KEYWORDS: (Undocumented immigration, Crime, Synthetic Control Methodology)

JEL CODES: (J15, J24, C32, C52)

ON MY HONOR, I HAVE NEITHER GIVEN NOR RECEIVED

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Signature

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Introduction

"Non-cooperative jurisdictions that do not honor U.S. Immigration and Customs (ICE) detainer requests to hold criminal aliens who are already in their custody, endanger the public and threaten officer safety by releasing criminal aliens back into the community to re-offend. In addition to causing preventable crimes, this creates another pull factor that increases illegal immigration."

-U.S. Department of Homeland Security, 2018

One of the more consistently and increasingly polarizing issues in the United States (U.S.) is that of border security and illegal immigration. An estimated 25% of immigrants in the U.S. were illegal in 2020, meaning that they illegally entered the U.S. (Pew Research Center, 2020). As Donald Trump said in 2015, "They're bringing drugs. They're bringing crime. They're rapists" (Trump, 2015).

Although this fear of crime entering the United States is largely speculative rather than empirically supported, mid-20th-century economic theory bolstered the idea that illegal immigration comes with the burdens of violent crime and drugs. Theory as late as 1998 suggests that illegal immigration and crime rates have a positive relationship (Grogger, 1998).

Many academic articles since then, though, have found scant evidence linking illegal immigration to increasing crime rates within the United States (Lee, Martinez, and Rosenfeld 2001; Ousey & Kubrin 2009; Wadsworth 2010; Lyons, Ve'lez, and Santoro 2013;) and elsewhere (Leiva et al. 2020; Boateng et al. 2020; Rumbaut and Rubén, 2008). In fact, the perception that illegal immigration itself is increasing isn't accurate either. From 2007 to 2016, the number of illegal immigrants in the U.S. actually decreased from 12.2 million to 10.7 million (Passel and Cohn, 2018). Studies have largely focused on the broader impact of illegal immigration rather than targeted mass-immigration events. Texas Governor Greg Abbott's recent migrant busing policies provide a natural experiment for examining this impact, and as such, this paper is the first evaluation to measure the effect of large, concentrated increases in undocumented immigration on crime rates in cities.

Sanctuary cities are cities that do not cooperate with the U.S. Immigration and Customs detainer requests, allowing undocumented immigrants to remain in the country post-arrest rather than deporting them (US Dept of Homeland Security, 2018). These sanctuary policies allow for the exploitation of inflammatory rhetoric, and Texas seized this opportunity. Since 2022, the state of Texas has spent over $148 million busing over 100,000 migrants to sanctuary cities throughout the country (Martínez-Beltrán, 2024). Abbot’s busing policies have primarily been to Washington, D.C.; New York, New York; Philadelphia, Pennsylvania; Los Angeles, California; Denver, Colorado; and Chicago, Illinois (Office of the Texas Governor, 2024).

Synthetic Control Methodology (SCM) seems to be the best tool for measuring this impact, as specific cities are entirely unique in their compositions. SCM allows for the creation of a synthetic counterfactual, where a combination of weighted cities from a donor pool can best represent the unique nature of any given city (Abadie et al. 2010).

SCM is a relatively new tool, first developed in Abadie et al. (2003). In addition to the newness of this methodology, rhetoric on undocumented immigration appears to be perpetually intensifying. Also, migrant busing began in 2022. These three factors provide a unique opportunity for a novel exploration of the effects of immigration on crime, with a specific focus on condensed and controlled increases of undocumented immigrants in cities. I expect to see that, consistent with modern literature, busing of undocumented immigrants causes no statistically significant effects on crime rates in the destination cities.

Literature Review

Immigration

Numerous articles have tackled the traditional economic theory of immigration and crime (Becker, 1968; Ehrlich, 1973), which states that given an immigrant's lower expected returns from legitimate occupations due to a lack of education or opportunity, they are more likely to turn to illicit occupations with a higher expected value. Social disorganization theory (Shaw and McKay, 1969), which posits that increased ethnic diversity destabilizes housing costs and crime rates, has also been examined. The majority of literature concludes that undocumented immigrants commit fewer violent and property crimes than legal immigrants, who, in turn, commit fewer crimes than native-born citizens. Limitations exist for any research in this area, as reliable data on undocumented immigration is hard to come by.

The literature surrounding the issue of immigration and crime is robust, although conflicting, with numerous different conclusions arising from similar studies. This is exemplified by Ousey and Kubrin (2009), who found conflicting evidence as to whether or not immigration and crime are linked. They measured the macro-level effects of immigration and crime in US cities from 1980-2000. They found that there are numerous potential factors both supporting and countering the traditional theory that immigration and crime rates are linked, concluding that no substantive answer to the question of the link between immigration and crime can reasonably exist given the conflicting confounding factors both in support and against the correlation.

Some studies, such as Odabaşi (2021), support the traditional theoretical idea that increases in immigration cause subsequent increases in crime rates. In her study, Odabaşi used a fixed effects approach to observe the correlation between immigration and crime. She found that immigration and unemployment have a direct link, although when controlling for unemployment, there is a limited association between immigration and crime. As immigration can cause unemployment (Ajimotokin et al. 2015), causation can exist for immigration to have a positive correlation with crime rates. Hence, this literature argues that immigration potentially has a positive effect on crime rates.

Other studies have combated this traditional theory. For example, Adelman et al. (2017), looking at a 40-year period, found that immigration is consistently linked to a decrease in crime across the board. Stowell et al. (2009) countered social disorganization theory, finding statistically significant causation between immigrants migrating into cities and no destabilization of crime rates or housing costs in these cities.

Most literature looking at recent immigration finds that newer immigrants, especially undocumented immigrants, tend to commit less crime than more established immigrants or native-born citizens. For example, Light et al. (2020) compared crime rates between undocumented immigrants, legal immigrants, and native-born citizens in Texas. They found that US-born citizens are twice as likely to be arrested for violent crimes, 2.5 times more likely to be arrested for drug crimes, and over 4 times more likely to be arrested for drug crimes than their undocumented immigrant counterparts. Mariani and Mercier (2021), employing a predator/prey model of crime, examined the role of self-selection on crime rates for undocumented immigrants. Imposing a cost of changing careers from legal to illicit activity and vice versa, they found that crime rates are higher for second-generation immigrants than that of first-generation immigrants. Sampson et al. (2008) found that a first-generation immigrant is 45% less likely to commit a crime than a third-generation immigrant.

Given that 94% of undocumented immigrants live in cities (Passel and Cohn, 2009), metropolitan areas are most valuable in examining the impact of undocumented immigrants on outcome measures. Hall and Stringfield (2014) analyzed segregation between Mexicans and other races in cities. They found that with larger undocumented populations, segregation between black people and Mexican people decreases. Similarly, Wadsworth (2010) found that cities with the largest increases in immigration experience the largest decreases in violent and property crime. Additionally, Adelman (2021) analyzed the relationship between undocumented immigration and metropolitan crime. He finds a negative relationship between undocumented immigration and crime.

This project focuses on sanctuary city policies specifically, as migrant busing is aimed at large sanctuary cities. Numerous papers examining this effect find either no correlation or a negative correlation between these sanctuary policies and crime, concurring with most literature about metropolitan areas and undocumented immigrants. Gonzalez et al. (2019) used a causal inference matching strategy to study the effects of sanctuary policies on violent crime, specifically rape, and property crime rates, and they found no differences in cities with these policies versus those without. In addition, they found these cities are larger, more ethnically diverse, have greater rates of poverty, are more democratic, and have greater populations of immigrants. Otsu (2021) analyzed the relationship between sanctuary policy and crime using an event study approach. She found no correlation between these policies and violent crime, although property crime experiences a sharp, statistically significant decline. She also found that this reduction in crime is more likely to be caused by a lower propensity of undocumented immigrants to commit crimes rather than a compositional change within the city. Hausman (2020) studied the effects of sanctuary policies on crime and deportations. He found that deportations decreased by about 33% among those fingerprinted, while deportations decreased by over 50% for those with no convictions. At the same time, he found that sanctuary policies have no effect on crime rates nor clearance rates.

Synthetic Control Methodology

In 2003, Abadie and Gardeazabal (2003) first employed synthetic control methodology, analyzing the effect of conflict on GDP in the Basque Country. This methodology was expanded upon in 2010, when Abadie et al. examined the impact of California's tobacco regulatory policy. Since 2010, the use of synthetic control methodology (SCM) has exploded. SCM has been hailed as "the most important innovation in the policy evaluation literature in the last fifteen years" (Athey and Imbens, 2017). It is employed when there can be uncertainty about any given control group to represent an accurate counterfactual, as no two regions can reasonably be considered the same. It is particularly useful in comparative case studies when some units are exposed while others are not. Throughout its brief history, it has been employed in numerous ways to gather results across the spectrums of demographics and policy.

Karaye et al. (2023) utilized synthetic control methodology to measure the effects of various gun control policy reforms on firearm fatalities in New York. They utilized SCM to create a "Synthetic New York", a representative and relevant counterfactual by which they could perform analyses on. Wu and McDowall (2024) analyzed whether bail policy reform in New York impacts crime rates, using interrupted time series analyses as well as SCM. SCM here was used as a supplement to control for the effects of the COVID-19 pandemic and to test for a causal relationship between bail reform and crime rates. Goin et al. (2017) analyzed the impacts of droughts on crime in California using SCM. Here, they also used a negative control to test for causality, as SCM is difficult to employ to measure causality when there are simultaneously occurring events.

Synthetic controls are also used to measure policies or shocks that affect multiple states. Kagawa et al. (2023) measured the effect of comprehensive background check policies in numerous states on firearm fatalities. Oliphant (2022) estimated the correlation between homicide rates and death penalty execution in multiple states by employing synthetic controls. Harper and Jorgensen (2023) analyzed whether legalizing marijuana in the states of Colorado and Washington has had any effect on crime rates, expanding upon prior research by creating synthetic control states for each state being examined. Jorgenson (2024) built upon previous gun control policy work, examining whether permit-less carry laws in numerous states affect violent crime rates. He corrected for "underdeveloped" research, as other methods for examining policy implications create difficulty in finding an accurate counterfactual.

Synthetic control methodology's scope can vary as well, from individual city blocks to entire countries. Piza and Connealy (2022) looked at Seattle, WA's Capitol Hill Occupation Protest (CHOP) Zone, using microsynthetic control methodology created by Saunders et al. (2015) and Robbins et al. (2017) to examine individual street segments and crime reporting. Meanwhile, Jemberu and Dehning (2023) analyzed Slovakia's entrance into a currency union on financial development, using full countries as synthetic controls. Overall, SCM carries a wide range of applications, especially within the field of policy analysis and infrequent shocks to a region's population.

Although synthetic control methodology is a proven tool to test for correlation for infrequent shocks to a region's population, it requires sensitivity analysis within the context of synthetic control methodology, to check for robustness. Sensitivity analysis helps confirm that results are based on treatment effect rather than random chance.

Data and Methodology

Data

To gather crime data, I used the FBI UCR database (Federal Bureau of Investigation). For socioeconomic and demographic indicators, I used the *getcensus* command in Stata, which links to available data from the United States Census Bureau (U.S. Census Bureau). I also use a database that identifies which counties and states in America have sanctuary policies (Center for Immigration Studies). I use the years 2015, 2016, 2018, 2019, and 2021 as my pre-treatment, and 2022 and 2023 as my post-treatment. This is due to availability and completeness of data. The data only covers about ⅓ of the total population, as a strongly balanced model is required for a synthetic control approach.

For outcome variables in each of my models, the FBI UCR database provides me with violent, property, and drug crime rates. My controls from available census data are Gini coefficients; percentage of white people; percentage of black people; percentage of men between the ages of 18 and 64; percentage of women between the ages of 18 and 64; standardized immigration into the region; standardized median income; standardized per capita income; standardized gross rent; standardized total population; percentage of population employed; and percentage of people who have received a high school diploma. I also attain sanctuary policies. Table 1 displays summaries of each covariate and outcome variable used.

Table 1

Descriptive Statistics

Variable	Mean	Std. Dev.	Min	Max
Gini	.449	.034	.333	.56
White percent	.816	.143	.147	.992
Black percent	.118	.123	.003	.74
Male 18-64 percent	.304	.019	.244	.411
Female 18-64 percent	.304	.018	.25	.377

Standardized Immigration	.003	.999	-3.532	6.722
Standardized Median Income	0	1	-2.62	5.93
Standardized Per Capita Income	0	1	-2.03	5.812
Standardized Gross Rent	0	1	-.628	11.51
Violent rate	.01	.013	0	.423
Property rate	.026	.04	0	1.509
Drug rate	.006	.007	0	.193
Standardized Total Population	0	1	-.54	10.84
High School Rate	.617	.049	.419	.77
Employment Rate	.806	.026	.69	.899

Numerous limitations arose from the data cleaning process. Any observations with missing variables had to be dropped, as was the case with missing years as well. Due to incomplete data, four of the treated cities had to be dropped (New York City, NY; Los Angeles, CA; Philadelphia, PA; Chicago, IL), so only Washington D.C. and Denver, CO could be analyzed. I merged census and FBI data using a crosswalk (Bureau of Justice Statistics).

Methodology

The first approach I employ is Interrupted Time Series Analysis (ITSA). It is used to observe changes in crime rates associated with sanctuary city status before and after treatment, and whether there is a significant shift in trends in crime rates post-treatment. By including a lagged variable for crime rates, ITSA model fit can be improved, leading to a more accurate representation of underlying data and reducing bias in estimating values. ITSA alone cannot fully control for other factors that could be simultaneously influencing crime rates, so it is essential to supplement ITSA with other models like endogenous treatment effects method and synthetic control method.

Endogenous treatment effects method is used to address potential selection bias—that is, the possibility that sanctuary cities are inherently different from non-sanctuary cities in ways that could impact crime rates. The model examines whether certain socioeconomic or demographic factors influence both sanctuary status and crime rates. The model uses a two-stage approach. First, it models the probability of being a sanctuary city as a function of selected socioeconomic and demographic factors. It then uses this probability to control for potential selection bias in estimating the effects of sanctuary status on crime rates. It accounts for both observed and unobserved factors that might be influencing both sanctuary status and crime rates. This model is useful in reducing bias for estimating the effect of sanctuary status on crime. Despite controlling for observed variables, the model might not fully capture all unobserved factors influencing both sanctuary status and crime rates, which is why further validation through synthetic control methodology might be necessary.

Synthetic control methodology is the primary tool of analysis for causal inference used in this study. The method allows for the construction of "synthetic" versions of treated cities by weighing units to match pre-treatment characteristics of the treated unit. Synthetic control methodology is particularly useful in comparative case studies, where an accurate counterfactual doesn't naturally exist. "Synthetic" cities for each treated unit, in this case Denver, CO and Washington D.C., are created by selecting and weighing control units (untreated cities with similar characteristics). Demographic and socioeconomic variables that could reasonably be expected to affect crime rates were selected. The Root Mean Square Prediction Error (RMSPE) is used to examine the strength of fit for "synthetic" cities in the pre-treatment period. Synthetic control methodology allows for the creation of an estimate of the treatment effect by comparing post-treatment outcomes between the treated unit and its synthetic counterpart. By measuring the difference in post-treatment effects, an inference can be made about the effect of migrant busing on cities that are being bused to.

Analysis and Results

Interrupted Time Series Analysis (ITSA)

I first use an interrupted time series analysis to see the effect of sanctuary city status on violent, property, and drug crime rates, controlling for various socioeconomic factors. Table 2 displays the results of my model.

Table 2:

ITSA

	Violent Crime Rate	Property Crime Rate	Drug Crime Rate
Time Trend	<0.001	<.001	<.001***
	(.000)	(.000)	(.000)
Post	.007***	.030***	<.001
	(.002)	(.006)	(.002)
Post Time Trend	-.001***	-.004***	<.001
	(.000)	(.009)	(.000)
Lagged rate	1.045***	1.240***	1.139***
	(.006)	(.007)	(.016)
Gini	.004	-.018	-.003
	(.005)	(.016)	(.004)
Sanctuary City	<.001	<.001	<.001
	(.000)	(.001)	(.000)
White Percent	-.002	-.005	-.003***
	(.001)	(.004)	(.001)
Black Percent	-.003*	-.005	-.002
	(.001)	(.005)	(.001)

Male 18-64 Percent	-.005	-.025	.008
	(.006)	(.022)	(.006)
Female 18-64 Percent	-.004	-.045	-.014*
	(.008)	(.027)	(.008)
Standardized Immigration	<.001*	-.001***	<.001***
	(.000)	(.000)	(.000)
Standardized Median Income	<.001	.001	<0.001
	(.001)	(.002)	(.001)
Standardized Per Capita Income	<0.001	.002	<.001
	(.000)	(.001)	(.000)
Standardized Gross Rent	<.001	<.003	<.001
	(.000)	(.001)	(.000)
High School Rate	-.010***	-.050***	-.001
	(.004)	(.013)	(.004)
Employment Rate	.007	.063***	.006
	(.007)	(.023)	(.007)
Standardized Total Population	<.001	<.001	<.001
	(.000)	(.001)	(.000)
Constant	.003	.009	.002
	(.004)	(.015)	(.004)
Observations	1414	1414	1414

R-squared	.965	.959	.812

Standard errors are in parentheses.

**** p<.01, ** p<.05, * p<.1*

Lagged crime rates have the most substantial effect on crime rates, with highly significant results ($p < 0.01$), indicating that previous crime rates are the most reliable predictors for crime rates.

Sanctuary status, according to this model, has no effect on crime rates. For each outcome variable, an increase in crime rates is uncorrelated to whether a city or county has sanctuary policies. For violent crime, factors such as percentage of black people and high school graduation rate have statistically significant decreases in violent crime rates at the 95% confidence level. According to this model, property crime rates decrease when cities become more populated and when people are more educated. Some counterintuitive results emerge, such as increases in per capita income and employment rates causing increases in property crime. This could indicate that certain economic complexities aren't fully captured using this model, which could indicate that omitted variable bias potentially exists. For drug crime rates, population size changes in cities have no effect on the crime rates, nor does sanctuary city status either.

My post variable represents the immediate effect on crime rates after treatment. Treatment had no statistically significant effect on drug crime, although violent and property crime rates show a statistically significant increase in crime rates following treatment. My post time trend variable represents the change in the trend of crime rates following treatment. For violent and property crime rates, the variables are negative and statistically significant, implying that the increase in crime rates slows down following treatment.

This model explains a substantial portion of violent and property crime rates, with R-squared values of .9648 and .9585, respectively. Drug crime rates, though, are a poorer model fit, with an R-Squared of .8123. This suggests that there could be numerous factors not included in the model that explain the variation in drug crime rates.

There are numerous potential limitations with this model, such as different immigration policies separate from just sanctuary city status. A city or county either does or doesn't have sanctuary policies, although sanctuary policies are not the only policy involving undocumented immigrants. For example, states such as Arizona have policies that allow for the police to ask people within the state to provide identification if 'reasonable suspicion' exists that they can be an undocumented immigrant (American Civil Liberties Union). In addition, most variables are standardized to allow for a better fit with factors such as Gini coefficients or variables in percent form. This leads to most variables having a very small effect on crime rates, if any. Inconsistent with previous literature, numerous factors in this model have little statistically significant effect on crime rates, such as employment status (Ajimotokin et al. 2015).

Overall, this model shows that sanctuary city status has no overall effect on crime rates, although it is likely that this model doesn't accurately display certain factors' effects on crime rates, as the results are inconsistent with previous literature. To better get an idea of whether or not sanctuary policies cause crime rates, and if Greg Abbott's migrant busing policies are 'bringing the crime' to these sanctuary cities, a different model might be required.

Endogenous Treatment Effects Model

This model controls for several socioeconomic factors, allowing for the isolation of the impact of sanctuary city status' effect on treatment selection. It is particularly useful in helping control selection bias by accounting for factors that might simultaneously affect crime rates and sanctuary status. The purpose of this analysis is to assess whether cities chosen for Abbott's busing policies were treated due to sanctuary city status versus other demographic or socioeconomic factors. Table 3 displays the results of the model.

Table 3:

Endogenous Treatment Model Results

	Violent Crime Rate	Property Crime Rate	Drug Crime Rate
Lagged Crime Rates	1.043***	1.238***	1.130***
	(.005)	(.007)	(.015)
Gini	.001	-.027	-.006
	(.004)	(.015)	(.004)
White Percent	-.003*	-.007	-.004***
	(.001)	(.004)	(.001)
Black Percent	-.004**	-.007	-.003*
	(.001)	(.005)	(.001)
Male 18-64 Percent	-.002	-.006	. 011
	(.006)	(.022)	(.006)
Female 18-64 Percent	-.002	-.030	-.004
	(.007)	(.024)	(.006)
Standardized Median Income	<.001*	-.001***	<.001**
	(.000)	(.000)	(.000)
Standardized Immigration	<.001	.001	<.001
	(.001)	(.002)	(.001)
Standardized Per Capita Income	<.001	. 003*	. 001*
	(.000)	(.001)	(.000)
Standardized Gross Rent	<.000	<.000	<.001
	(.000)	(.001)	(.000)

High School Rate	-.007*	-.034**	.003
	(.003)	(.012)	(.003)
Employment Rate	. 001	. 033	-.004
	(.006)	(.020)	(.006)
Standardized Total Population	<.001	<.001	<.001
	(.000)	(.001)	(.000)
Treated	.001	-.003	-.001
	(.002)	(.008)	(.001)
Constant	.006	.018	.005
	(.004)	(.015)	(.004)
Sanctuary City	8.173	7.137	5.683
	(109.592)	(301.267)	(1515.028)
Constant	-10.246	-9.217	-7.752***
	(109.591)	(301.266)	(.127)
Treatment-Outcome Error Correlation	.264	.180	.248***
	(.237)	(.249)	(.061)
Log of Unexplained Crime Variation	-5.770***	-4.493***	-5.774***
	(.0190)	(.190)	(.019)
Chi-squared	38244.36	31871.39	6019.263
p	0	0	0

Standard errors are in parentheses

**** p<.01, ** p<.05, * p<.1*

Given the results of the endogenous treatment effects model, treated cities return statistically insignificant results. Therefore, it is very unlikely that selection of cities in which migrants were bused to was only determined by cities with sanctuary status. The main predictors of crime rates according to the model are population density changes and lagged crime rates. Lagged crime rates cause a statistically significant increase in crime rates at the 99% confidence level. Population density increases cause a marginally significant ($p < 0.1$) decrease in violent crime rates, and a statistically significant decrease in property ($p < 0.01$) and drug ($p < 0.05$) crime rates.

Sanctuary status itself is not statistically significant in predicting any of the crime types, as indicated by the treatment variable's coefficients. This suggests that sanctuary policies do not have a measurable direct impact on crime rates within counties and cities in the sample.

The treatment-outcome error correlations are low and statistically insignificant for violent and property crime. This indicates that for violent and property crime rates, unobserved factors influencing both crime rates and sanctuary status selection are likely limited. For drug crime rates, although the treatment-outcome error correlation is low, it is statistically significant. This suggests a weak but systematic association between unobserved factors influencing both treatment selection and crime rates. The log of unexplained crime variation values are all negative and statistically significant, implying that there is a consistent variation in crime rates that is not explained by this model. The chi-square values are very high, and p-values are essentially zero, indicating that the model is statistically significant.

The absence of certain variables weakens the explanatory power of this model. In addition, socioeconomic and demographic complexities make it difficult to fully isolate the impact of sanctuary policy on crime rates. To further isolate the treatment effect, a synthetic control model is applied. This allows for a more precise comparison by constructing a weighted combination of untreated cities that closely resemble the demographic, crime, and socioeconomic factors of treated cities in the pre-treatment period. This counterfactual can then be tested for causality. This approach will help validate the results of this model and provides a stronger basis for causal inference.

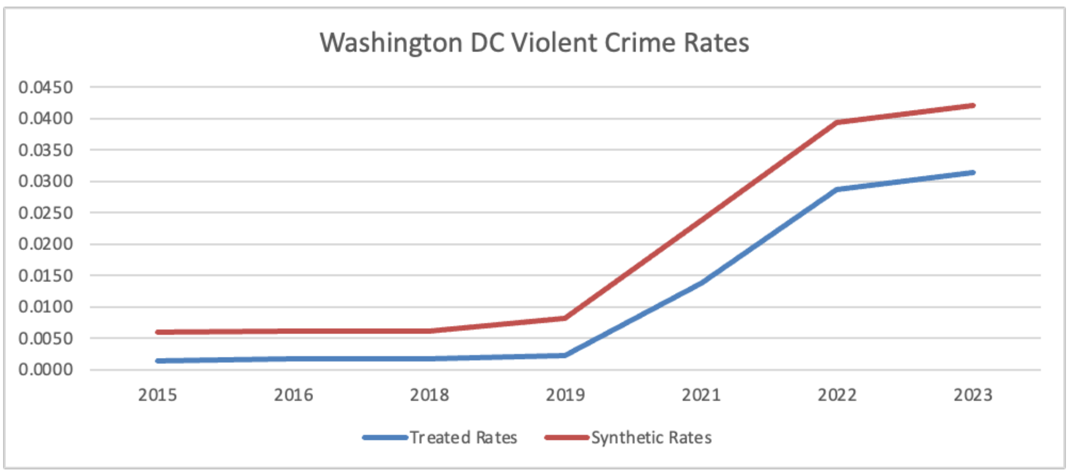
Synthetic Control Model

I employ a synthetic control model as the final stage for my analysis. Synthetic control methodology's value stems from its ability to evaluate infrequent shocks to a region's population changes and demographic effects based on that shock. In this case, migrant busing to Washington D.C. and Denver, Colorado was analyzed using this methodology, as they were the only treated cities with complete data. Counterfactuals were created for Washington D.C. and Denver, CO using weighted units from the donor pool based on best fit of selected demographic and socioeconomic factors. This approach helps create an inference of what would have happened had the treatment not been exacted.

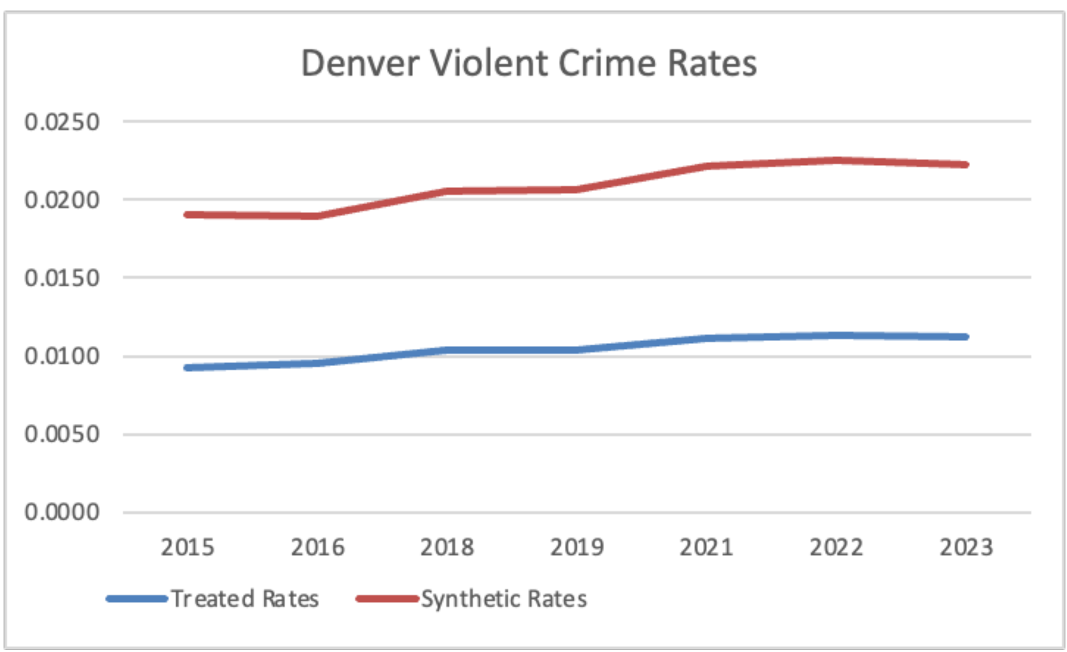
Synthetic controls were constructed by weighing lagged crime rates; Gini coefficients; sanctuary city status; percentage of white people of total population; percentage of black people of total population; percentage of men between the ages 18-64 of total population; percentage of women between the ages 18-64 of total population; standardized median income; standardized per capita income; percentage of population employed; standardized gross rent; standardized population change; percentage of population who have received a high school diploma; and standardized total population.

Violent Crime Rates. For violent crime, "Synthetic Washington D.C." was constructed with .07 King County, WA; .591 Norfolk County, MA; and .339 Suffolk County, MA. The analysis produced a Root Mean Squared Prediction Error (RMSPE) of .0115, indicating a good fit for the model. Lower RMSPEs indicate less error in the model, signifying better fits for the data. "Synthetic Denver" was constructed with .161 Cass County, ND; .155 Fairfax County, VA; .38 Hillsborough County, NH; .137 King County, WA; .043 Maricopa County, AZ; .01 Salt Lake County, UT; .053 Sarpy County, NE; .055 Utah County, UT; and .006 Weber County, UT. With a RMSPE of .0002, this is a very strong fit for the model. For both models, the small RMSPE implies that pre-treatment values of all factors are closely correlated between synthetic and treated. The pre-treatment period was 2015 through 2021, and the post-treatment period spanned 2022 and 2023. Graphs 1 and 2 display violent crime rates for treated and synthetic cities for each of Washington, D.C. and Denver, as well as the magnitude of the difference between the two. The vertical axis on the left represents violent crime rate, and the vertical axis on the right represents the changes in the difference of violent crime between treated and synthetic cities.

Graph 1:



Graph 2:



From the graph above, it is unclear how parallel the two crime rate trends are. In order to test their parallel nature statistically, I ran a hypothesis test. Equation 1 displays the basic equation.

Equation 1:

$$Treat_t = \alpha + \beta_{synth} + \varepsilon_t$$

Where

α = The baseline level of the treated crime rate when the synthetic crime rate is 0

β = The relationship between the treated outcome and the synthetic control outcome

I look at statistical significance and the coefficient. I also observe the F-statistic following a hypothesis test of whether the coefficient is one. If the coefficient is close to 1 and is statistically significant, it implies a parallel relationship between treated and synthetic crime rates. If the F-statistic is large, it indicates that the relationship between treated crime rates and synthetic crime rates is not a one-to-one match.

For Washington D.C., I found that the estimate was significantly different from one at 4.34 at the 99.99% confidence level, indicating that synthetic versus treated violent crime rates are not identical trends. A high F-statistic of 36.64 also implies that there is a significant deviation between synthetic and treated Washington D.C. For Denver, however, the coefficient is close to one, at 1.1203, with a statistically insignificant p-value of 0.3193 and F-statistic of 1.2, implying that the violent crime rate trends are very similar between synthetic and treated Denver.

I then ran an interaction model to examine the effect of treatment on violent crime rate trends. Equation 2 displays the equation.

Equation 2:

$$\text{treated}_t = \alpha + \beta_{1\ synth} + \beta_2 effect_t + \beta_3 \, (synth_t \cdot effect_t) + \varepsilon_t$$

Where

treated_t= The outcome variable for the treated group at time t

α = The intercept, or baseline level of the treated outcome

β_1 = The coefficient on *synth*, representing the relationship between treated and synthetic crime rates through the entire period

β_2 = The coefficient on *effect*, capturing level changes in the treated crime rates following treatment year

β_3 = The coefficient of interest. It is the coefficient on the interaction term ($synth_t$ $effect_t$), representing the post-2022 difference between treated and synthetic outcomes.

I observed the coefficient for the interaction term. A significant coefficient indicates a change in the relationship between treated and synthetic crime rates post-intervention, implying that crime rates do not follow parallel trends following intervention.

In Washington D.C., the coefficient was 1.38 at the 99.7% confidence level, suggesting that following treatment, violent crime rates increased at a greater rate in "Synthetic Washington D.C." versus treated Washington D.C. In Denver, the coefficient was small at -0.0033 with a p-value of 0.91, implying that no change in violent crime rate trends resulted from treatment.

I lastly employed a time trend analysis model and collect Z-scores to assess whether there is a systematic trend in treated versus synthetic cities. Equation 3 displays the equation for the treated group, and equation 4 displays the equation for the synthetic group

Equation 3:

$$treated_t = \alpha_{treated} + \beta_{treated} trend_t + \varepsilon_{treated,\ t}$$

Where

$$Trend_t = \text{year being measured - 2014}$$

$\alpha_{treated}$ = The intercept for the treated group, representing baseline crime rates

$\beta_{treated}$ = The coefficient on trend, indicating yearly changes in violent rates for the treated group

Equation 4:

$$synth_t = \alpha_{synth} + \beta_{synth}\ trend_t + \varepsilon_{synth,\ t}$$

Where

$$Trend_t = \text{year being measured - 2014}$$

α_{synth} = The intercept for the synthetic group, representing baseline crime rates

β_{synth} = The coefficient on trend, indicating yearly changes in violent rates for the synthetic group

I then take these coefficients and their standard errors to produce Z-scores. The equation for Z-scores is displayed in equation 5.

Equation 5:

$$Z= (\beta_{treated} - \beta_{synth}) / SE(\beta_{treated})^2+SE(\beta_{synth})^2$$

Where

$\beta_{treated}$ = Yearly changes in crime rates for the treated group

β_{synth} = Yearly changes in crime rates for the synthetic group

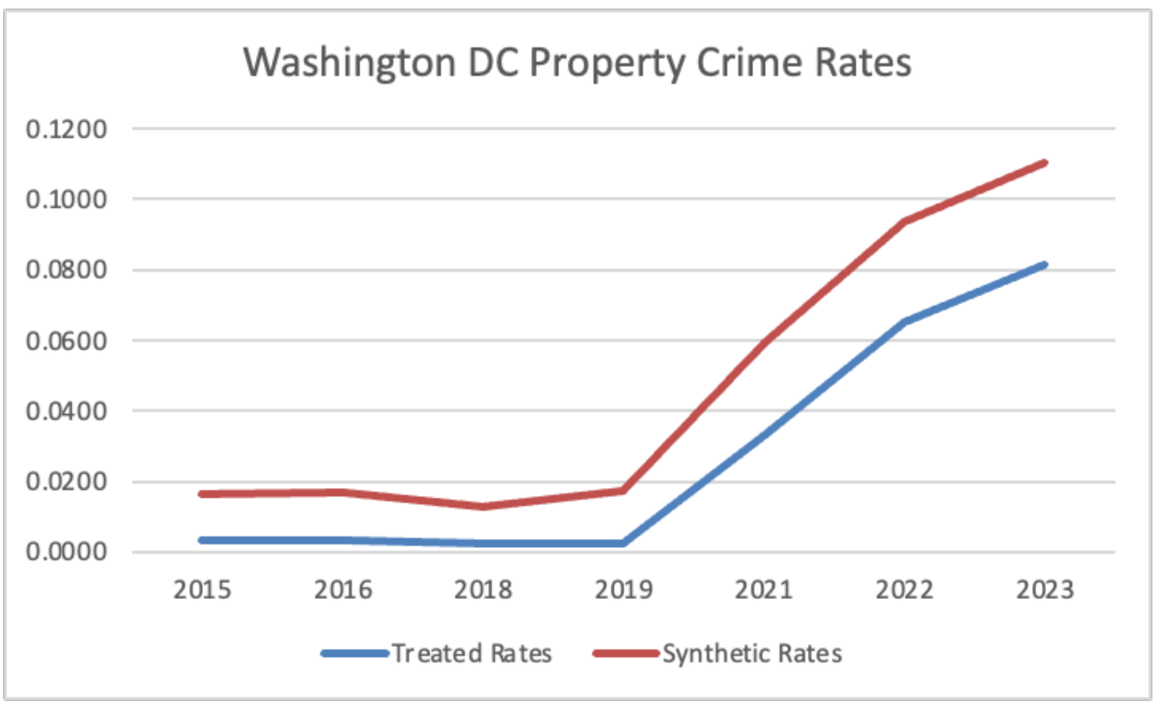
$SE(\beta_{treated})$ and $SE(\beta_{synth})$ = The standard errors of those coefficients

Z-scores are used to observe whether the difference between the two coefficients is statistically significant, or whether there is a statistically significant difference in yearly trends of crime rates between the two groups. A high Z-score indicates that over time, trends between synthetic and treated crime rates are different.

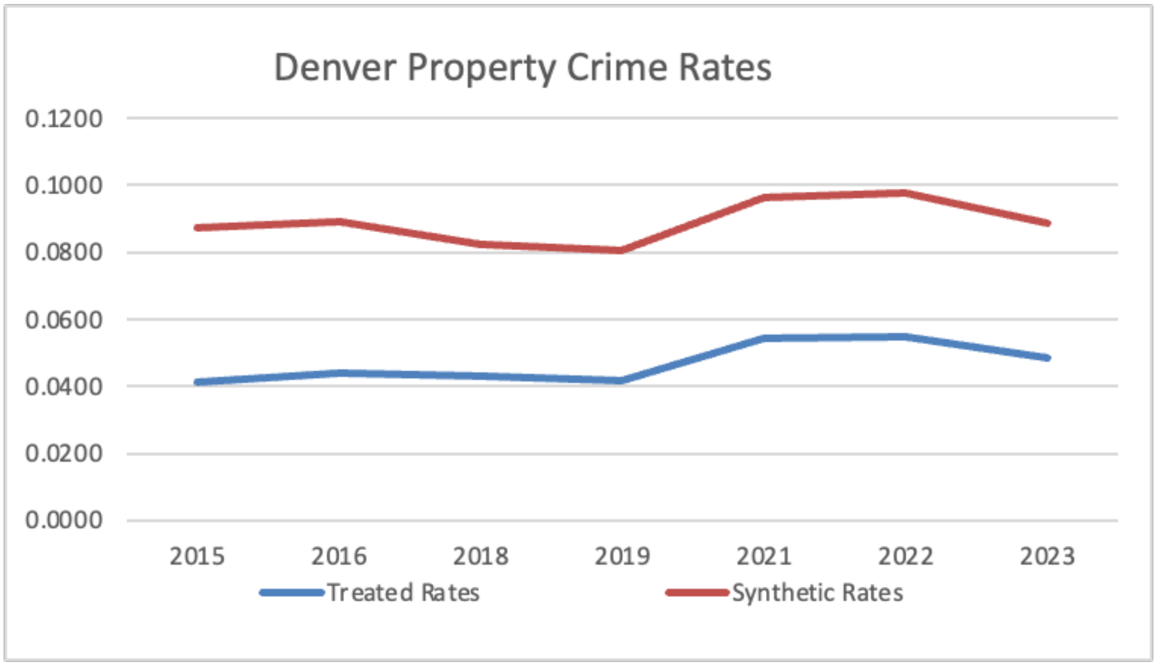
In Washington D.C., the Z-score is significant at 3.204, indicating that violent crime rate trends between synthetic and treated Washington D.C. are significantly different. For Denver, the Z-score is insignificant at 0.365, indicating a parallel violent crime rate pattern between synthetic and treated Denver.

Property Crime Rates. For property crime, "Synthetic Washington D.C." was constructed with .4 Fairfax County, VA; .007 Hinds County, MS; .037 Norfolk County, MA; and .555 Suffolk County, MA. It was constructed with a RMSPE of 0.0007, implying a very strong fit between socioeconomic and demographic indicators pre-treatment. "Synthetic Denver" was constructed with .118 Chittenden County, VT; .109 Davis County, UT; .004 Fairfax County, VA; .313 King County, WA; .026 Maricopa County, AZ; .021 Middlesex County, MA; .045 Pierce County, WA; .158 Rockingham County, NH; and .207 Salt Lake County, UT. Synthetic Denver has a RMSPE of 0.008, showing that it as well has a very strong fit. The same pre- and post-treatment period as violent crime analysis exist. Graphs 3 and 4 display property crime rates for each treated and synthetic city, as well as the changes in the difference between the two. The vertical axis on the left shows the crime rates, and the vertical axis on the right shows the magnitude of the differences.

Graph 3:



Graph 4:



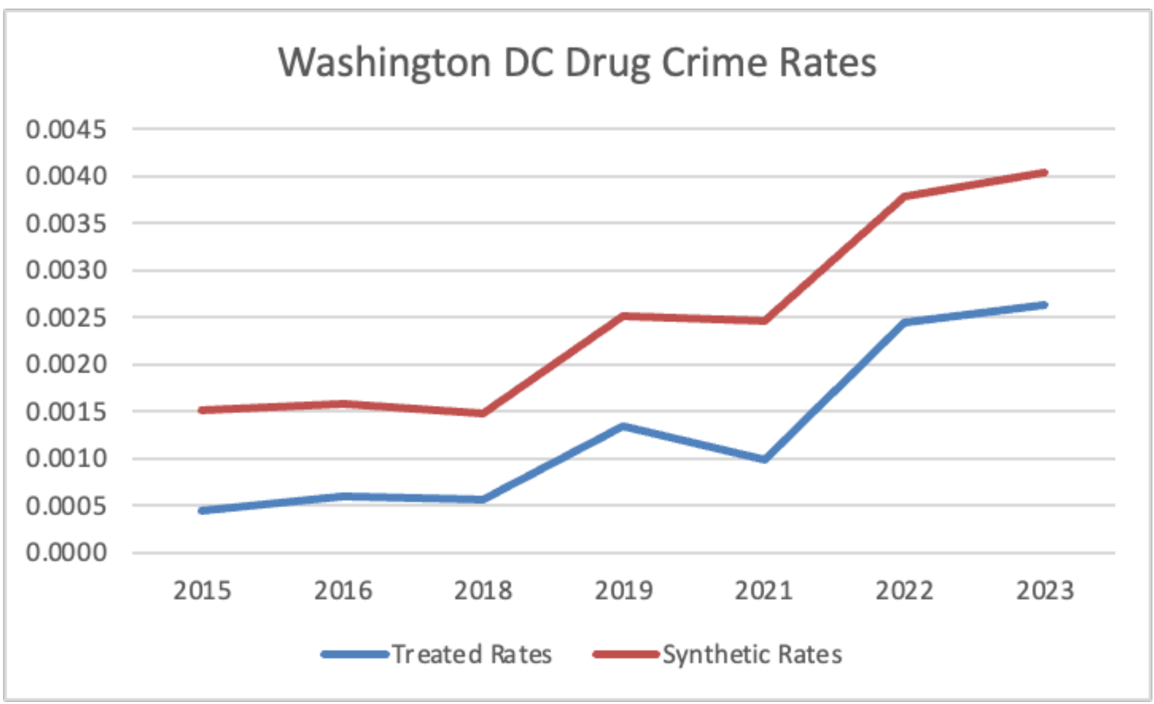
I ran hypothesis tests, interaction models, and time trend analysis models on property and drug crime rates as well. For property crime rates, there is a very similar pattern. For the hypothesis, in Washington D.C., there is a statistically significant and large coefficient of 4.14 at the 99.9% confidence level, with a large F-statistic of 36.37, implying that property crime rates follow different patterns in synthetic and treated Washington D.C. For Denver, there is a statistically insignificant and small coefficient of -0.87 with a small F-statistic of 1.8, implying that the property crime rate trends are very similar between synthetic and treated Denver.

The interaction model also returns similar values. In Washington D.C., there is a large estimate for the interaction term of 1.27 at the 99.1% confidence level, indicating that following treatment, “Synthetic Washington D.C.” experienced a larger change in property crime rates than treated Washington D.C. For Denver, there is a small, insignificant estimate of 0.16, implying that little changed post-treatment for the interaction between synthetic and treated Denver.

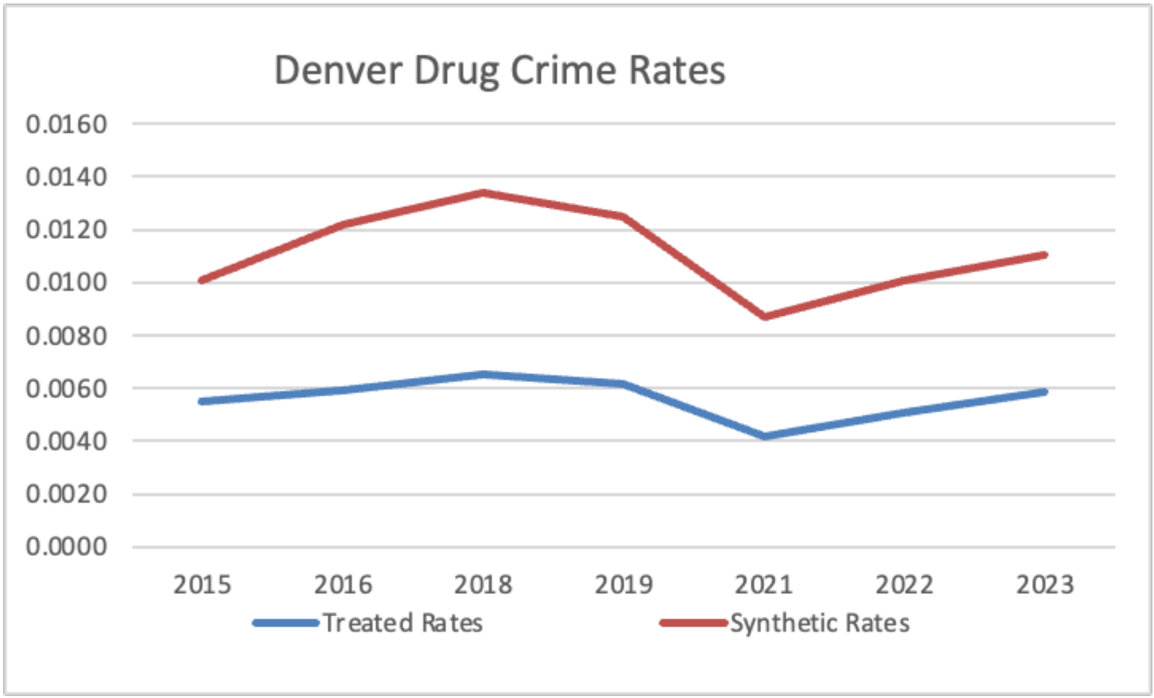
The time trend analysis model shows similar results for Washington D.C. With a Z-score of 3.026, it is inferred that property crime rate trends are significantly different. In Denver, there is a divergence from analysis of violent crime rates. With a Z-score of 2.936, it is inferred that property crime rate trends are significantly different.

Drug Crime Rates. For drug crime, "Synthetic Washington D.C." was constructed with .118 King County, WA; .527 Norfolk County, MA; and .355 Suffolk County, MA, with a RMSPE of .0007. "Synthetic Denver" was constructed with .097 Fairfax County, VA; .079 King County, WA; .172 Larimer County, CO; .054 Maricopa County, AZ; .178 Middlesex County, MA; .132 Salt Lake County, UT; .248 Snohomish County, WA; and .04 Utah County, UT, with a RMSPE of .0004. Both of these RMSPEs indicate a strong fit with the synthetic controls. Graphs 5 and 6 display drug crime rates for synthetic and treated cities, as well as the difference in trends between the two. The vertical axis on the left shows the crime rates, and the vertical axis on the right shows the magnitude of the differences.

Graph 5:



Graph 6:



The hypothesis test shows statistically insignificant results for Washington D.C., with a p-value of 0.14, with an F-statistic. This implies that drug crime rates follow a parallel pattern overall. This change from analysis of other crime rates can be due to drug crimes representing a much smaller share of total crimes, so there is less variation in these crime rates. For Denver, there is a marginally significant small estimate of 0.63, with an F-statistic of 4.67. This implies less correlation between synthetic and treated drug crime rates in Denver.

For the interaction model, there is a large and statistically significant estimate for Washington D.C. of 1.17 at the 99.5% confidence level, indicating that following treatment, drug crime rates increase at a greater rate in “Synthetic Washington D.C.” than that of treated Washington D.C. In Denver, there is a small and statistically insignificant estimate, implying that drug crime rate trends post-treatment follow the same pattern.

For the time trend analysis model, Washington D.C. has a large Z-score of 3.158, indicating significantly different trends for drug crime rates in synthetic versus treated

Washington D.C., corroborating my previous analyses of other crime categories. In Denver, a small Z-score of 0.049 indicates that trends are extremely similar between synthetic and treated Denver.

Sensitivity Analysis. For my sensitivity analysis, I employ two techniques. First, I employ in-time placebo testing, in which I arbitrarily choose an alternative date as my treatment period. If no significant effects are found when using this altered treatment date, it suggests that the original effect is likely due to the intervention rather than underlying trends. I also employ in-space placebo testing, in which I use the most similar cities to Washington, D.C. and Denver based on previous synthetic controls to examine the impact of hypothetical treatment. In this case, King County, WA serves as a proxy for Denver, as it is most similar to Denver using weights of previous synthetic control models, while Norfolk County, MA is a proxy for Washington D.C. If a similar trend emerges from the synthetic control results, it can indicate that the observed effect from synthetic control methodology is due to external factors rather than the treatment itself. I observe the effects of total crime rates.

Washington D.C. experienced a large, statistically significant estimate of 2.46, with a large F-statistic of 8.8, implying that "Synthetic Washington D.C." and treated Washington D.C., implying a lack of correlation between crime rates. This diverges from my drug crime analysis, although the results are similar to violent and property crime rates. With a negative, statistically insignificant coefficient of -0.45 and a small F-statistic of 1.56, Denver also experiences similar results to previous testing, implying that total crime rates follow similar trends. In King County, WA, the proxy for Denver, there is a negative coefficient of -0.82 at the 99.9% confidence level and very large F-statistic of 53.23, suggesting an inverse relationship between synthetic and treated crime rates. In Norfolk County, MA, the proxy for Washington D.C., there is a statistically significant coefficient of 0.67 and large F-statistic of 9.61, implying a strong correlation between synthetic and treated crime rates, a similar result to my synthetic control method.

In Washington D.C., there is a statistically significant coefficient of 1.2919, a very similar result to Washington D.C. at its actual treatment time, implying that crime rates diverged following treatment despite the different period of treatment, which suggests that underlying factors or trends could be influencing the results of the synthetic control model for Washington D.C. Insignificant results for Denver and Norfolk County, MA, the proxy for Washington D.C., are seen, implying that crime rates did not change following treatment in these regions. In King County, WA, there is a small but significant coefficient of 0.03 following treatment, a divergence from the results for Denver, as crime rates did not diverge in my synthetic control model.

Washington, D.C., Denver, and King County, WA, my proxy for Washington, D.C., show statistically significant Z-scores, indicating different trends in synthetic crime rates versus treated ones. For Washington D.C., the results are very consistent with my synthetic control model. However, this is a divergence from previous testing of Denver, in which Z-scores were largely statistically insignificant. Norfolk County, MA, my proxy for Denver, shows an insignificant Z-score.

The placebo testing implies that there are potential underlying factors affecting both Washington, D.C. and "Synthetic Washington D.C." In Denver, in which indeterminate results occur, sensitivity analysis suggests that Denver's crime rate values from my synthetic control are robust.

Discussion

This paper examined the impact of Texas Governor Greg Abbott's migrant busing policies on crime rates in the cities in which migrants were bused to. My hypothesis was that the influx of migrants in selected sanctuary cities would have indeterminate effects on crime rates. This paper is the first to examine a targeted mass-immigration event's effect on crime rates. Broadly speaking, this paper adds to existing literature by looking at one mass immigration rather than a slower, more constant process.

The results of this paper find that this mass migration did not cause a statistically significant effect on crime rates. The ITSA analysis showed small, statistically significant decreases in violent and property crime rates following treatment, although given that the results weren't consistent with previous literature for some of my covariates, it is likely that this model isn't a great fit for examining the effect of treatment. The endogenous treatment effects model showed statistically insignificant impacts on crime rates, with more consistent findings. Although Washington D.C. showed statistically significant decreases in crime rates, sensitivity analysis for Washington D.C. implied that underlying trends other than immigration affected crime rates in both Washington D.C. and "Synthetic Washington D.C." Denver had more robust results in its sensitivity analysis, validating the findings of indeterminate results for crime rates.

The paper aligns with previous literature about sanctuary policy. Hausman (2020), Gonzalez et al. (2019), and Otsu (2021) all examine the effect of sanctuary policies on crime rates and find that for most crime rates, there is an indeterminate effect. For immigration as a whole, this paper conflicts with some findings in both directions. Papers such as Wadsworth (2010) and Adelman (2021) suggest that I should expect to see decreases in crime rates across the board, while papers such as Odabaşi (2021) indicate I should see increases in crime rates.

Numerous limitations exist for this project, so findings should be taken cautiously. Drug crime rates represent a much smaller share of total crime rates than property or violent crime, and this difference in magnitude can induce misleading results. My model fit for drug crime rates is weaker than that of violent or property crime rates, which could indicate potentially inaccurate results for drug crime rates.

The longer-term effect of migrant busing policies remains unclear, as only two years of post-treatment data exist at the time of writing. In addition, this paper examines undocumented rather than illegal immigrants. Undocumented immigrants are still first-generation immigrants, although many of them aren't technically illegal. This distinction is relevant, as undocumented immigrants can have different motivations and behaviors than that of illegal immigrants, which can influence crime dynamics in ways that this study couldn't examine.

Theoretically, this paper adds to the growing literature combatting traditional economic theory of immigration and crime (Becker, 1968; Ehrlich, 1973). In a practical sense, this paper likely doesn't have any implications. Articles since 2009 have been combatting the claim that immigration causes crime, although the wide-scale idea that immigrants cause crime continues to grow. Texas Governor Greg Abbott will likely continue busing migrants to sanctuary cities, and inflammatory rhetoric targeting undocumented immigrants will likely continue to grow despite the expanding collection of literature countering this. Anecdotal evidence rather than empirical analysis continues to dominate the public discourse of undocumented immigration. Nevertheless, research such as this can contribute to a more evidence-based dialogue on immigration and crime over time. Shifts in public perception might be slow, but the accumulation of empirical evidence can build a foundation for which future change in public dialogue or legislative action can be enacted.

Future research can focus more on drug crime, creating models specifically tailored to better capture fluctuations in drug crime rates. A model addressing the unique characteristics of drug crime rates—often less prevalent yet highly variable—could lead to more accurate insights of how undocumented immigration affects this particular crime category.

As time progresses, a more comprehensive dataset will emerge, which will enhance the robustness of findings. Examining the impact on other cities from the ever-expanding list migrants are being bused to will also be helpful in examining the impact of targeted mass-immigration events.

Different methodologies, such as natural experiments and refined synthetic controls, can improve robustness of results as well. Testing the results using different statistical approaches would help validate the conclusions and reveal potential nuances in the relationship between immigration and crime rates.

In conclusion, my study finds that mass-immigration events within the United States have largely indeterminate effects on crime rates in the cities to which immigrants are moved. This is a new phenomenon, so with more time, longer-term impacts on these crime rates will emerge. In addition, a closer examination of drug crime rates could be addressed to enhance the understanding of these policies.

As migration policies continue to evolve—especially with recent developments in immigration policy following Donald Trump's presidential reelection—it remains essential to ground discussions of immigration in rigorous analysis. While this study may not change public perceptions or policy in the short-term, it represents a step towards a more informed and research-oriented approach to immigration policy. Studies like this can help foster a more constructive and empirically based public dialogue in the future.

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