Crime and the Mariel Boatlift

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Abstract

Our paper describes crime effects attributable to the Mariel Boatlift, the 1980 Cuban refugee crisis that increased Miami's population by nearly 10%. Using synthetic control methods to match Miami with cities that exhibit similar pre-intervention crime patterns, we find strong evidence the phenomenon comparatively increased property crime and murder rates; we also document weaker but suggestive relative growth in per capita violent crime linked with the influx of Cubans. Compositional features of the newcomers seemingly drive our results; the disproportionately young, male Mariel Cubans' characteristics highly correlate with illicit activity. Given the unique composition of the group and the absence of rigorous screening, our findings likely constitute the "upper bound" of crime caused by migration.

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1 Introduction

Few natural experiments have garnered the breadth of coverage as the Mariel Boatlift. This diaspora of approximately 120,000 Cubans from the port of Mariel into Miami began in April 1980, and continued until that October.[1] The phenomenon famously fueled a debate surrounding labor market effects of immigration. Research began with Card (1990), who found low-skilled workers were virtually unaffected by the Mariel labor supply shock. In a reappraisal of Card's seminal study, Borjas (2017) refined the original difference-in-differences approach. Namely, he applied matching methods to identify cities with similar pre-Boatlift labor market conditions as Miami; relative to these counterfactuals, Borjas found low-skilled workers' wages in Miami fell considerably after the arrival of flotilla. This response ignited controversy amongst labor economists. Clemens and Hunt (2017) and Peri and Yasenov (2019) argued sample selection sensitivity drove Borjas's estimates; accounting for compositional changes in the population – unrelated to the Boatlift – led both to arrive at Card's result. A separate strand of empirical research examined the quasi-experiment with alternative focuses; these papers evaluated innovation and political backlash spurred by the Boatlift (Harris, 2015; Thompson, 2019).

Despite its prevalence in the applied literature, no causal analysis of the effects of the refugee influx on crime exists. The absence of research on this topic is surprising for myriad reasons. For one, the link between immigration and crime was salient to Miami residents in the 1980s. The arrival of Cuban refugees coincided with a spike in crime rates, which local politicians and journalists quickly pointed out.[2] Additionally, records reveal Fidel Castro exploited the flotilla as a means of ridding Cuba of its mentally ill and criminal populations.[3]

[1]For an in depth analysis of the historical events, see Stephens (2016).

[2]Eleventh Judicial Circuit of Florida's Grand Jury Report on Immigration Issues

[3]Perry Rivkind, Miami district director of the U.S. Immigration and Naturalization Service at time, stated, "The whole situation [the Boatlift] has presented tremendous problems to the safety and health of the community," See the Sun Sentinel article here. Aguirre, Saenz, and James (1997) cast doubt on estimates of troubled migrants central to worries espoused by Rivkind and others. They argue Castro might have intentionally mixed in a very small cohort of criminals and mentally ill individuals to shape the image of the rest of the Marielitos. This belief is supported by Bach, Bach, and Triplett (1981) who estimate 16% of Mariels had served prison terms in Cuba for ambiguous reasons. Likewise Portes and Stepick (1985) find

While individuals with such backgrounds would generally be barred from entry, political tensions between the US and Cuba virtually eliminated the option to deport undesirable newcomers. To exacerbate matters, federal authorities were both unaware and unprepared to interview Mariel Cubans.[4]

Our paper addresses the gap between descriptive research on Mariel related crime and the relevant economics literature. Using Uniform Crime Reports (UCR) compiled by the Federal Bureau of Investigations (FBI), we quantify the effects of the Boatlift on several dimensions of criminal activity. To measure these causal impacts, we apply the synthetic control methods developed by Abadie, Diamond, and Hainmueller (2010, 2015) and Abadie and Gardeazabal (2003).

This estimation procedure requires extensive crime data, which are not readily available. Accessible records suffer from miscalculation and missing data issues. While the FBI requests local law enforcement agencies to submit crime records on a monthly basis, some fail to do so. These agencies may send aggregated annual or semi-annual statistics. In other cases, individual months are missing. Leveraging archival works by Maltz and Weiss (2006) and Maltz (2006), we carefully apply an approach to correct inappropriately aggregated and missing UCR statistics. Specifically, we linearly interpolate aggregate crime based on records before and after the appearance of gaps; then, we allocate data to individual categories according to time-invariant, law enforcement agency-specific crime distributions.

To ensure we make like-for-like comparisons between locations with different population levels, we pull population data to construct per 100,000 crime measures. Yet accurate annual records covering police department jurisdictions are irregularly obtainable. While the FBI estimate agency-level populations, it is well-known they are incorrect in non-census years. To overcome this hurdle, we propose and apply a parsimonious linear interpolation method with US Census Bureau Data to correct for UCR population estimates.[5] Namely, we compute

criminals, homosexuals, or mentally ill individuals composed no more than 5% of the Marielitos.

[4]NY Department of Correctional Service's Executive

[5]Chalfin and McCrary (2018) and Maltz (2006) note the FBI inappropriately measures agency-level populations. While the former implement a procedure to adjust populations, the approach is not outlined.

jurisdictional population weights in census years as shares of county populations. Then, we linearly interpolate jurisdictional populations with these weights, combining them with county level interncensal population estimates. Since our units of analysis are metropolitan statistical areas (MSAs) and our agencies' jurisdictions do not span entire MSAs, identifying accurate population data is integral to the validity of our per capita crime rates. These corrections coupled with the use of synthetic control methods enable us to estimate crime effects attributable to the Mariel Boatlift.

Our analyses indicate Marielitos' arrival led to a temporary surge in violent crime and a long-term increase in property crime relative to similar MSAs. Murder rates, however, remain significant as other violent crime effects disappear; our preferred estimates indicate murders per 100,000 comparatively rose by 41.2% in the seven years after the arrival of the flotilla. While less persuasive, we observe a relative increase in aggregate violent crime rates of 43-53% on average following April 1980, though this effect dissipates after five quarters. Our property crime and robbery estimates are more sustained; we find that these comparative measures grew nearly 25-32% and 70%, respectively, and persist until 1990.

While we cannot control for every unobservable variable, we conduct numerous robustness checks. Several phenomena hypothetically confound our findings. Miami's centrality to cocaine distribution between 1970 and 1990 is one such idiosyncrasy. However, the inclusion of drug trafficking proxies in synthetic control methods leaves our results virtually unchanged. Additionally, one could argue our population estimates undercount the arrival of Cubans and, thus, mechanically produces large crime effects. To address this, we show even the most conservative adjustment to the Miami population does not substantively affect our findings.

Additional support for our estimates comes from a placebo test borrowed from the difference-in-differences literature. Specifically, we drop data after the arrival of the flotilla, and estimate the associated results with an artificial Boatlift beginning in 1977. That exercise yields no significant differences between Miami and its comparators; therefore, it supports the internal validity of the natural experiment.

Accounting for migrant selection lies outside the scope of our project and the available data. Nonetheless, this is an unignorable aspect of the Mariel refugee crisis. Our results are at least – in part – driven by negative selection of Cubans. Marielitos were disproportionately young men with low levels of education; a segment of this group even held felony records by US standards. These features suggest the group possessed high proclivity for criminal activity. Back-of-the-envelope calculations that consider changes in Miami's age and gender profiles induced by the Boatlift capture only 12-14% of our estimates. Therefore, underlying propensity for illicit activity or immersion difficulties explain the majority of our effects.

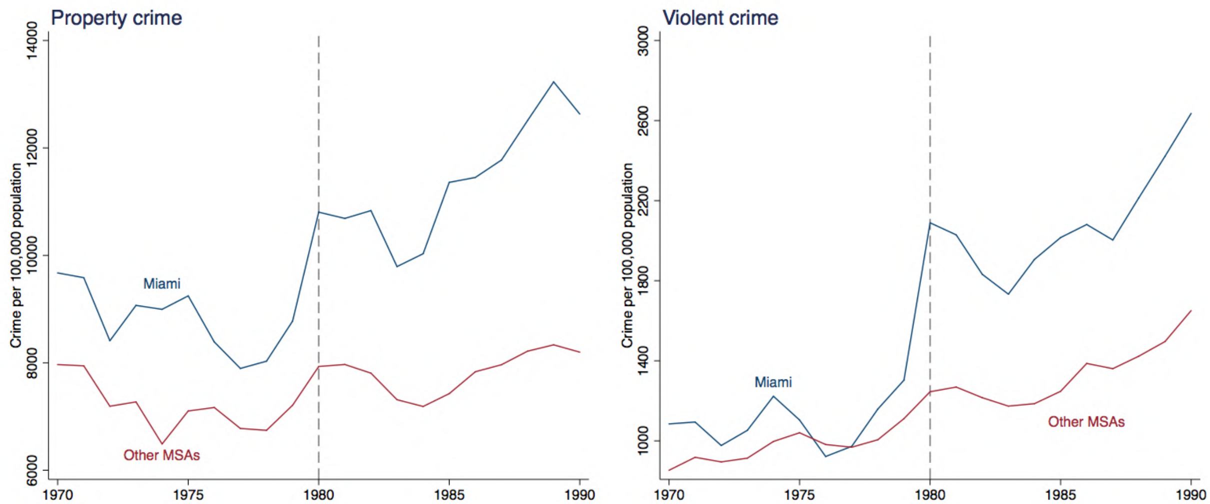
Given the negative selection of newcomers, the lack of a rigorous vetting process, and a virtually non-existent resettlement process, there are restrictions on external validity. This case study does not capture features of more recent refugee and immigration waves. Despite those limitations, our findings are useful for policymakers. Our project identifies downside risks of poorly managing refugee and immigration policies and a likely "upper-bound" of the negative effects of immigration on crime. Faced with similar crises, our results support policies that first vet refugees, then distribute them according to their abilities and communities' needs. Allocating migrants as such has the potential to eliminate the deleterious crime effects we calculate.

2 Background

Between 1970 and 1990, crime rates steadily grew throughout America. Miami's rates outpaced those of other American cities during this period, especially following 1980; it boasted the highest nationwide city-level murder rate in 1980-1981 and again in 1984-1985.[6]

[6]See the LA Times Article on this issue here.

Figure 1: Annual Crime Rates: 1970-1990



Notes: The crime rates depicted above come from aggregated agency-level UCR data; these entities receive weights proportional to their smoothed population shares. The pool of cities consists only of MSAs where the largest jurisdiction has population greater than or equal to 100,000 in 1980. Population data were pulled from from IPUMS and Census repositories.

Many in Miami – including city police – posited recent arrivals from Cuba were the source of public safety concerns.[7] The ingress of Cubans were relatively young, uneducated, predominantly male, and weakly attached to formal labor markets.[8] Table 1 compares demographic characteristics between non-Mariel Miami residents and Mariel Cubans in 1980. Marielitos' characteristics strongly correlate with criminal activity. Reports Fidel Castro forced those with mental illnesses and felony records into Miami further stoked fears.[9] Therefore, it is unsurprising the arrival of the flotilla triggered widespread panic.[10]

[7]Ibid.
[8]Card (1990)
[9]Bach et al. (1981), Portes and Stepick (1985)
[10]Freeman (1999), Hirschi and Gottfredson (1983), Mustard (2010), Taft (1936)

Table 1: Demographic Characteristics - 1980 Miami

Population - All Ages			Population - 18-64		
	Average Miami Resident	Mariel Cuban		Average Miami Resident	Mariel Cuban
14 and under	18.3	20.8	HS Dropout	26.9	53.7
15 to 24	15.6	16.0	HS Grad	36.2	26.5
25 to 34	14.4	24.7	Some college	21.0	13.1
35 to 44	11.2	17.4	College degree	15.9	6.7
45 to 64	22.1	16.4	No English	3.9	28.5
65 and older	18.4	4.7	English, not well	5.9	34.8
Male	47.0	58.1	English, well	90.1	36.8

Table 1 contains demographic information pulled from the 1980 and 1990 Censuses. The 1980 Census captures the United States prior to the arrival of Mariel Cubans in April. The Mariel characteristics come from the 1990 Census; we subtract ten from age figures to reflect age in 1980. Because Mariel Cubans likely accumulated more education and more English proficiency in the ten years since arrival, these values are over estimates of the education and English proficiency of Mariel Cubans at arrival.

The apathetic screening procedure administered by the federal government did little to assuage public anxiety. At the time of arrival, a recession constrained federal funding; the incomprehensive screening that took place relied on dubious, self-reported information.[11] Roughly 1,000 Mariel Cubans were flagged upon entry for prior criminal or mental health issues, and this reinforced the belief they held a high propensity to commit crimes. A comparable number of those permitted to enter the United States were later arrested for crimes; likewise, they contributed to the negative perception surrounding Mariels.[12]

Limited evidence from the Criminal Justice Council of Miami found Mariel Cubans committed more crimes than pre-Mariel Cuban-Americans. Though they represented 5% of the Miami-Dade County population in 1985, Marielitos were booked for 10% of felonies and 22% of misdemeanors; nearly half of these defendants were deemed mentally unfit for trial.[13] Reports from the Department of Correctional Services of New York and the Board of County Commissioners for Miami-Dade County intimate Mariel Cubans disproportionately repre-

[11] NY Department of Correctional Service's Executive

[12] Card (1990)

[13] See the Sun Sentinel article here.

sented the number of incarcerated Cuban-Americans.[14] Sociologists such as Aguirre et al. (1997) corroborate these findings.

That said, this evidence, inherently more descriptive in nature, might constitute statistical artifacts. Total crime clearly increases with population growth. Plausibly, this mechanical response and systematic violence in the 1980s prompted residents to inappropriately ascribed blame to Mariels. This begs the question: was the malaise Miami was experiencing throughout the 1980s caused by the influx of Cubans?

3 Literature

Sociological research on the relationship between immigration and crime spans back at least a century.[15] American sociologists in the early 20th century were interested in tenement dwelling Southern and Eastern Europeans; speculation at the time suggested crime enabled newcomers to skip rungs of the social ladder. To substantiate these claims, scholars compared rates of illicit activity in foreigners' homelands with measures from their American counterparts. Other studies compared crime rates between native and foreign-born residents. Results from these analyses were inconclusive; the direction of correlations hinged on context.[16] Skeptics pointed out that these studies did not account for differences in the age, gender, or rural-urban distribution of natives and immigrants [17]. While these pieces influenced – and were influenced by – cultural perceptions of immigrants, most lacked credibility expected of modern empirical research.[18]

Limitations in records and statistical inference techniques led to the brevity of compelling evidence on the topic. The primary hurdle came by way of the absence of available data.[19]

[14] NY Department of Correctional Service's Executive, Crime Statistics for Dade County, Florida: 1979-1985

[15] Abbott (1915), Taft (1936)

[16] Hagan and Palloni (1998)

[17] Sutherland (1924)

[18] Hagan and Palloni (1998)

[19] Martinez Jr. and Lee (2000) and Clemens and Hunt (2017) note CPS Data do not report country of birth until 1994. This challenges those interested in separately identifying Marielitos with their Haitian

The FBI's Uniform Crime Report (UCR) — the most reliable source of crime statistics — does not capture ethnicity, race, or immigration status. While UCR data indicate age, race, and gender, most agencies and years do not report offenders' ethnic groups; no agencies document immigration status. For this reason, researchers turned to alternative sources. While selection problems arise in surveys of prisoners, nearly every scholarly article using these data report migrants – both legal and illegal – are less likely to be incarcerated relative than native-born Americans (Hagan & Palloni, 1998; Kubrin & Ishizawa, 2012; Landgrave & Nowrasteh, 2017).

More recently, economists and sociologists have employed econometric techniques in order to uncover a causal relationship between immigration and overall crime rates.[20] This research tends to mirror those studying the effects of immigration on local labor markets by relying on spatial and secular variation. Results from this strand of the literature tend to be mixed. MacDonald et al. (2013) and Spenkuch (2013) both use Bartik instruments to purge estimation of immigrants' endogenous spatial choices. While the former find a reduction in crime associated with immigration, Spenkuch (2013) documents no effect on violent crimes but a minor increase in property crimes. Despite the appeal of this identification strategy, serial correlation associated with early newcomers' decision-making or unobserved location-specific characteristics could potentially bias estimates. Recent papers by Goldsmith-Pinkham, Sorkin, and Swift (2018) and Jaeger, Ruist, and Stuhler (2018) highlight potential shortcomings.

Chalfin (2014) reconsiders the role of share-shift instruments in this context. Instead of exploring factors that pull migrants to locations, he leverages mechanisms that push potential migrants to leave their homes. To do so, he exploits historic migration routes between Mexico and the US as well as the randomness of rainfall. Specifically, he argues precipitation in Mexico does not noticeably correlate with crime or labor market patterns

and Jamaican counterparts. In general, the paucity of crime data surprise many. See Episode 9 of Jennifer Doleac's Probable Causuation Podcast on available crime records.

[20] See, for example, Bell, Fasani, and Machin (2013); Chalfin (2014); Chalfin and Deza (2019); Gehrsitz and Ungerer (2017); MacDonald, Hipp, and Gill (2013); Spenkuch (2013).

in the US; however, abnormal weather does impact the decision of many to emigrate from Mexico. Therefore, rainfall serves as a natural instrument for Mexican migration. Chalfin finds no robust effect on either violent or property crime attributable to Mexican migrants. Given these immigrants - on average - tend to have characteristics highly correlated with illicit activity, he argues his estimates serve as an "upper bound" on the causal relationship between immigration and crime.[21]

Labor market conditions — tied to Chalfin's claim — have been understood to drive criminal tendencies as early as Becker (1968). A series of recent papers highlights the influence of these channels on immigrants. Pinotti (2015) and Mastrobuoni and Pinotti (2015) explore the relationship between legal status and propensity to commit crime in Italy. They find legal rights to participate in the formal labor market significantly reduce migrants' willingness to engage in illicit activities. Bell et al. (2013) study two separate but similar migration waves in the UK. In one, they find small, significant increases in property crime; the other cohort did not yield any perceptible crime effects. They attribute this contrast to differences in labor market opportunities faced by each group. Thus, even for identical cohorts of immigrants, propensities to commit crime may differ within the same settings.

Like Chalfin (2014), we see this project as establishing a ceiling on the estimate of crime induced by immigrants. Similar to the Mexican-American population considered by Chalfin, our cohort possessed low levels of education and skills; they, too, faced cultural barriers to formal labor markets. Unlike Mexican migrants, the sudden nature of the Mariel Boatlift combined with its scale depressed opportunities for entrants in the local labor market. Additionally, the Mariel wave differs from average Mexican migrants in that a non-negligible portion were involved in violent criminal activity prior to arrival in Miami. Chalfin considers a restricted definition of newcomers, too. Our population of interest entered as refugees with less oversight than Mexican migrants face. Thus, positive selection of entrants does not factor into our approach; rather, we have *negative selection* of migrants. Consequently, our

[21] Braun (2019), Freeman (1999), Glaeser and Sacerdote (1999), Grogger (1998)

estimates likely contain stronger case for identifying an "upper bound."

Our empirical strategy, too, differs. We draw upon the rich labor economics literature addressing the effects the 1980 Cuban migrant wave imposed on low-skilled, non-Mariel Miami residents. With the tools of synthetic controls developed by Abadie et al. (2010, 2015) and Abadie and Gardeazabal (2003), this quasi-experiment enables us to obtain estimates under weaker conditions. While this approach has been employed in multiple immigration and crime studies, we believe this is the first implementation at the intersection of the two.[22]

4 Data

In this study, we construct quarterly crime rates across four dimensions for a number of metropolitan statistical areas (MSAs).[23] Monthly crime data come from the FBI's UCR. These reports are captured at the police agency level, and date back to 1960; agencies' jurisdictions are defined by FIPS places. Our primary outcome variables are violent and property crimes; the FBI include murder, non-negligent manslaughter, rape, robbery, and aggravated assault in the former and burglary, larceny-theft, and motor vehicle theft in the latter. Given the prevalence of reporting issues prior to 1970, we restrict our study period from 1970 to 1990.

Despite improved reporting from 1970 onward, missing UCR data entail practical challenges for empiricists. Submission of crime statistics is not mandatory.[24] While data covering all major police departments' are essentially complete, smaller agencies occasionally fail to report their UCR records. These delinquent law enforcement entities eventually submit aggregated statistics covering multiple months. Consequently, each UCR edition contains monthly gaps. We pull partially cleaned crime records from Maltz and Weiss (2006) who indicate reasons for missing observations. Using data preceding and following gaps, we first

[22] Relevant immigration studies include Borjas (2017), Clemens and Hunt (2017), Peri and Yasenov (2019), Nowrasteh, Forrester, and Blondin (2019). Applications in the crime literature using synthetic controls can be found in Robbins, Saunders, and Kilmer (2017), Pinotti (2015), Donohue, Aneja, and Weber (2017)

[23] Property crime, violent crime, murder and homicide, and robbery

[24] Some states require UCR statistics. See the repository of Crime in Maryland Reports here.

linearly interpolate aggregate crime rates. Subsequently, we distribute statistics to individual categories such as forcible rape based on their historic shares of overall crime within that agency. Appendix A.1 details our imputation method in greater depth.

Another challenging aspect of the UCR involves its population estimates. Although the FBI includes population figures, these statistics are incorrectly calculated.[25] Crime researchers typically identify agencies' jurisdictions, then match UCR data with the American Community Survey (ACS). Given the ACS did not exist during our study period, we are unable to implement this strategy. Using US Census Bureau data, we develop a method to obtain agency-level population statistics. First, we link agencies to their corresponding county. In census years, we estimate the share of county population within FIPS places. Then, we linearly interpolate those weights in intercensal years. Finally, we calculate agency-level population estimates by multiplying the shares by county-level populations. A thorough discussion of this process can be found in Appendix A.2.

We link agency-level records with various correlates of criminal activity. We pull annual law enforcement employment statistics from the FBI's Law Enforcement Officers Killed in Action program and various demographic characteristics from 1970 and 1980 census tables provided by IPUMS NHGIS (Manson, Schroeder, Van Riper, & Ruggles, 2018).[26]

We aggregate these data at the MSA level and the quarterly frequency to obtain a balanced panel. We define MSAs according to the NBER guide outlined by Jean Roth.[27] This method conforms with the labor economics literature on the Mariel Boatlift. This approach is especially important considering multiple, relatively large agencies lie within the Miami MSA; many of the associated locations were destinations for Cuban refugees.[28]

Performing our analyses at the MSA level requires us to aggregate across agencies within a given MSA. We include only entities with complete data for the study duration. The

[25] See Chalfin and McCrary (2018) or Maltz (2006) for a discussion on this issue

[26] Variables include: shares of the African American and Hispanic population, share age 18 to 24, share HS dropout, median income, poverty rate, and population density.

[27] See NBER guide here.

[28] The next 6 largest agencies — Ft. Lauderdale, Hialeah, Hollywood, Miami Beach, West Palm Beach, and Pompano Beach — contain over 600,000 residents.

reporting problems described above require us to drop a large number of agencies. This choice does not affect the inclusion of almost all large law enforcement entities, including all major agencies in the Miami area. Nonetheless, five agencies located in a place with a population over 100,000 are dropped. See Table 2 for an accounting of this. Missing data are especially prevalent for certain types of agencies. Therefore, we also do not include any of the following: county police departments, university police departments, agencies ever covering populations fewer than 2,500 people, as well as other miscellaneous agencies (for example, city transportation police).

Table 2: Number of agencies with complete data

Agency population in 1980	Total	Complete crime data	Complete covariate data	Final sample
500,000 and greater	22	22	22	22
250,000 to 499,999	32	31	32	31
100,000 to 249,999	109	105	108	104
25,000 to 99,999	592	430	429	350
Less than 24,999	2,696	1127	1551	917

Table shows the number of agencies by data availability and size in potential donor MSAs (and Miami)

We apply a population criterion to restrict our donor set — entities that could comprise the counterfactual group — to MSAs in large urban environments.[29] Specifically, we include MSAs where the largest agency has a population at least as large as 100,000. Elimination of smaller urban areas is convenient for computational purposes and inference. Though unreported in this paper, we check the sensitivity of our results to the population threshold. Findings from alternative specifications are available upon request; to remain transparent, we note meaningful differences in estimates prompted by each choice.[30] Our preferred estimates come from a final sample of 112 MSAs.

[29]For consistency, we also exclude Honolulu, HI and Las Vegas, NV from our analysis. Both city agencies also covers the surrounding county.

[30]Alternative population criterion levels include 50,000, 150,000, and 200,000.

Table 3 shows basic summary statistics for the Miami MSA as well as among donor MSAs. The Miami-Fort Lauderdale-West Palm Beach, FL MSA, which we will refer to as Miami, was among the highest crime MSAs. It ranked 13th and 19th in terms of average violent and property crime indices, respectively. A full listing of crime rates across the period of interest can be found in Appendix B.

Table 3: Population and crime in Miami and donor MSAs

	Miami	Donor MSAs				
		p10	p25	p50	p75	p90
MSA Population	1,564.6	152.9	191.3	331.5	755.2	1,322.2
Largest Agency	335.7	112.3	149.2	203.1	425.1	698.8
Viol. Crime Rate	846.8	305.8	392.1	545.0	735.1	866.0
Prop. Crime Rate	8,468.2	5,404.0	5,977.2	6,763.2	7,844.5	8,798.4
Share Black	15.5	2.5	6.4	13.2	26.5	35.8
Poverty Rate	14.5	9.7	11.0	13.9	16.0	19.2
HS dropout Rate	36.3	21.7	26.5	33.0	37.6	41.3

The table above shows Miami and donor pool characteristics. Populations and non-crime variables are from 1980 census data, and are listed in the thousands. Crime rates reflect average annual per 100,000 crime rates from 1970 to 1979.

5 Empirical Approach

5.1 Overview and Mechanics

Synthetic control methods (SCMs), developed by Abadie et al. (2010, 2015) and Abadie and Gardeazabal (2003), endow researchers with a tool to quantitatively analyze comparative case studies. This data-driven procedure generalizes the difference-in-differences framework. Specifically, the SCM constructs a control group comprised of a weighted combination of comparator units; this "synthetic" unit functions as the counterfactual from which to draw causal inference.

SCMs boast attractive features relative to alternative approaches. For one, SCMs can calculate treatment effects with a single unit exposed to the intervention; regression would be underpowered in this context. Further, SCMs add a layer of transparency to counterfactual selection processes relative to other procedures. SCM algorithms choose counterfactual units by an exact rule (that will be described below in more detail). Dissimilar to an approach like propensity-score matching, this method neither relies on heuristics to determine counterfactuals nor obfuscates which entities best track treated units in pre-intervention period. SCMs produce explicit weights indicating the contribution of each counterfactual constituent .[31]

From our perspective, the greatest benefit of SCM comes involves its ability to make statistical inferences. This method eliminates uncertainty which plagues comparative case studies. Difference-in-differences with one treated unit tend to lack power; further, the treated unit might exhibit different pre-intervention patterns relative to potential comparators. SCMs overcome both shortcomings. First, it affords researchers a tool to construct a counterfactual nearly identical to the treated group prior to intervention; moreover, robust and conservative test statistics can be derived from SCMs with just one treated entity.

We now walk through the construction of the synthetic control. Define $Y_{j,t}$ as the outcome for unit j of $J+1$ total units at time t.[32] $j=0$ indicates the treated unit. Each member of set of potential comparator units – called the donor pool, is indexed $j=1,...,J$. The synthetic control is then calculated as a weighted sum of all the units in the donor pool.

[31]That said, this process is still vulnerable to manipulation during the selection of donor units and predictor variables.

[32]Abadie et al. (2010) set $Y_{j,t} = \alpha_{i,t}D_{i,t} + \theta_t Z_i + \lambda_t \mu_i + \delta_t + \epsilon_{i,t}$. Here, δ_t captures an unknown systematic factor with constant factor loadings, Z_i is a $(r \times 1)$ vector of observables left unchanged by intervention, and θ_t identify unknown parameters. The additional term, $\lambda_t \mu_i$, is the product of unobserved time-factors and factor loadings. This addition of last component invalidates standard difference-in-differences assumptions.

Specifically, we define counterfactuals as,

$$\tilde{Y}_t = \sum_{j=1}^{J} w_j Y_{j,t}, \text{where } w_j \geq 0 \text{ and } \sum_{j=1}^{J} w_j = 1.^{33}$$

Define X_0 and X_J as a $k \times 1$ vector and a $k \times J$ matrix that contain our k predictor variables for the treatment unit and J donor units, respectively. The choice of the weighting vector, $W^* = (w_1, ..., w_J)$, is chosen such that the weights minimize a weighted norm between treatment and synthetic values of our predictor variables.

$$W^* = \ argmin \ (X_0 - X_J W)^T V^* (X_0 - X_J W)$$

The weighting matrix, V, is a $k \times k$ diagonal matrix with each value representing the weight applied to a predictor variable. V is chosen such that it minimizes the mean squared predicted error (MSPE) of the outcome over the pre-treatment period:

$$V^* = \ argmin \ \frac{1}{T_0} \sum_{t=1}^{T_0} (Y_{0,t} - \tilde{Y}_t(V))^2, \tag{1}$$

where T_0 indicates the final period before treatment.[34]

5.2 Quality of Matches and Inference

The SCM is an inherently visual tool; it produces two time series, raw data for the treated unit and its synthetic control. The treatment effect is calculated as the difference between

[33] Although SCM constraints have come under scrutiny, their imposition is essential to external validity and generation of a single set of weights (Abadie et al., 2010; Doudchenko & Imbens, 2016). Alternative approaches lose these features and require difficulty to verify linearity assumptions. Regardless, SCM assumptions appear to justified in our context.

[34] While SCMs can search across all positive definite, diagonal matrices that minimize the mean squared predition error of the pre-intervention outcome variable to find V^*, an alternative mechanism to calculate predictor weights exists. Essentially, this second method regresses the outcome variable on all predictors period-by-period; here, weights are assigned according to predictive power (Kaul, Klößner, Pfeifer, & Schieler, 2015). In fact, this second approach is the default in the "synth" Stata package.

the two trends following intervention. The ocular nature of SCM facilitates the ability of researchers to gauge goodness of fit and perform statistical inference.

Two standard approaches help to determine quality of matches between the treated unit and its synthetic control. The reliability of the synthetic unit can be visually assessed; prior to intervention, the two trends should overlap. Additionally, one can compare the balance among our predictors between the treated unit and its synthetic control.

To make statistical inferences with SCMs, one must determine how likely they would observe calculated treatments effects if the method were repeated on entities not exposed to intervention. We conduct this exercise by following a procedure outlined by Abadie et al. (2010). Specifically, we create placebo effects by iteratively swapping the treated unit with donor pool members and re-estimating SCM treatment effects; since these units are not exposed to intervention, their placebo effects collectively capture idiosyncratic variation in the outcome variable. By comparing the variation between each donor pool member and its synthetic control in the post-treatment period relative to the pre-treatment phase, we identify inherent randomness in the outcome variable following intervention. We use root mean square predicted errors (RMSPEs) to capture variation. We take the ratio of post-to-pre-intervention RMSPEs, and rank them from largest-to-smallest; this creates an empirical distribution. From this distribution, we can calculate the likelihood the actual treatment would be observed in the absence of an intervention. In other words, this distributional knowledge allows us to calculate a two-sided, exact p-value.

As proposed by Galiani and Quistorff (2017), we also calculate p-values for individual quarters; to do this, we swap the numerator from the standard approach — the RMSPEs for the entire post-intervention phase — with RMSPEs for each period following treatment. We plot these values to explore the secular trend associated with treatment. This approach constitutes a family-wise error rate; this test statistic, thus, it enables us to view the relationship between treatment and outcomes over time.[35]

[35]Firpo and Possebom (2018)

6 Empirical Evidence

In this section, we estimate the causal effects of the Mariel Boatlift on per capita crime rates. We first describe our baseline results. Subsequently, we explore historically accurate indices of crime—murder and robbery—to add nuance. We close by conducting a series of placebo exercises and sensitivity checks to evaluate the strength of our findings.

6.1 Synthetic Controls

To construct a counterfactual for Miami, we match on MSA-level crime correlates prior to the second quarter of 1980. These include property and violent crime rates, racial compositions, population densities, police force sizes and gender ratios, age profiles, unemployment and poverty rates, median incomes, and shares of high school dropouts. Additionally, we include migrants per capita as predictor given the magnitude of flows into Miami during the 1970s.[36] Predictor balance tables in Appendix C suggest synthetic Miami tracks well with Miami. The matched MSAs and their respective weights can be found in Table 4.

[36] Haitian refugees appeared on the Miami coastline throughout the 1970s. Successful asylum seekers numbered no more than 3,000 per year until Haitian émigrés saw an opportunity to join the Mariel Boatlift. An estimated 15,000 Haitians joined their Cuban counterparts. Additionally, "freedom flights" between 1965-1973 from Cuba brought some 300,000 migrants to Miami (Clemens & Hunt, 2017). We follow an approach described by Borjas (2017) to identify annual MSA-level migration.

Table 4: Donor Weights - Property and Violent Crime

MSA	Property	Violent
Atlanta, GA	-	0.023
Bakersfield, CA	0.374	-
Baton Rouge, LA	0.023	-
Los Angeles - Long Beach, CA	-	0.044
Modesto, CA	-	0.01
New York - Newark- Jersey City, NY-NJ	0.266	0.227
Orlando, FL	0.194	0.464
Phoenix - Mesa, AZ	-	0.03
Sacramento - Arden Arcade - Roseville, CA	0.143	-
Tucson, AZ	-	0.202

Table 4 lists the weights ascribed to counterfactual constituents in our principal estimates. We only include MSAs which receive a positive weight in at least one specification.

Figure 2 captures outcome variables for Miami and its synthetic control for both our property and violent crime measures. Prior to the Mariel Boatlift, the event marked by the vertical lines, the trends for Miami and its counterfactual track closely with each other. This leads us to believe our predictors suitably match Miami to the counterfactual. Immediately after the second quarter of 1980, both outcome trends separate from their synthetic controls. The property crime difference exhibits growth over time. In contrast, violent crime rates in Miami appear to jump, relative to its counterfactual, and these differences remain roughly constant for the following decade.

Figure 2: SCM Results - Property and Violent Crime

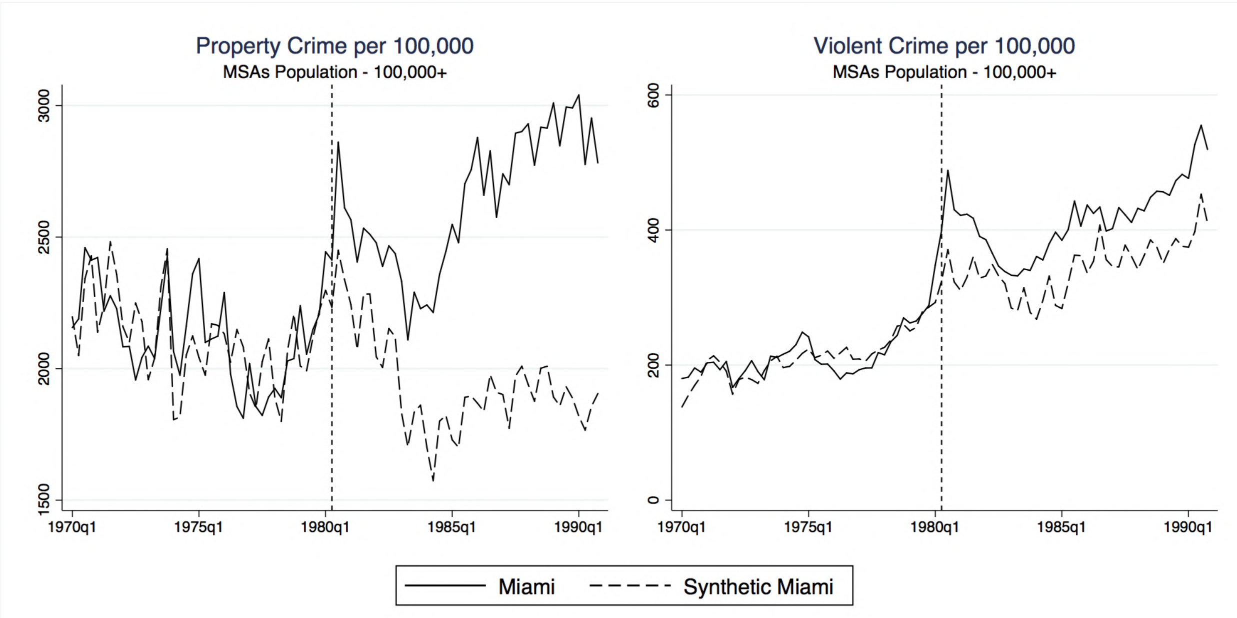


Figure 2 compares raw Miami crime data relative to its synthetic controls. Prior to intervention, the trends virtually overlap; this implies the counterfactuals track well with Miami, and reinforces confidence in our ability to make statistical inferences. Following the second quarter of 1980, these series diverge. These differences between Miami and its synthetic controls implies the existence of substantial crime effects attributable to the Mariel Boatlift.

Following the work of Abadie and Gardeazabal (2003) and Abadie et al. (2010), we assess significance by comparing our estimated treatment effects against an estimated distribution of placebo effects. Specifically, we implement identical synthetic control analyses on each potential donor unit—those who did not experience the Boatlift—and use this distribution to assess whether the probability that we would observe our treatment effects simply by chance. Figure 3 displays SCM treatment effects for every city within the panel. Each grey line is a placebo effect calculated by repeating our SCM on untreated MSAs. The dark trends list the treatment effects for Miami.

Figure 3: Treatment Distribution - Property and Violent Crime

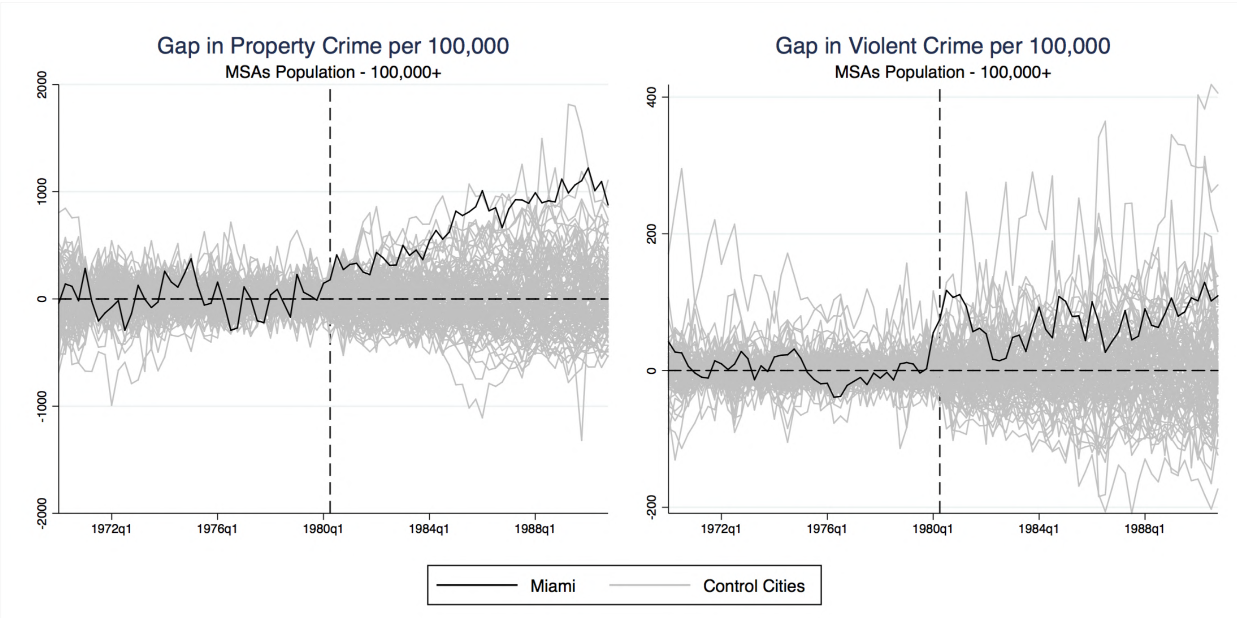


Figure 3 compares estimated treatment effects for Miami and all donor pool members. The dark line refers to Miami's treatment effect, while each of the grey lines is a placebo treatment effect for an untreated unit. Relative to placebo estimates, Miami's property treatment effect lies near the upper envelope. Therefore, it is highly unlikely the actual treatment effect would be observed in the absence of the Mariel Boatlift. The violent crime treatment effect provides less perspicuity.

Consistently, Miami's property crime effects lie on the upper envelope of the distribution of placebo effects. Consequently, it is improbable we would calculate these values in the absence of the Boatlift. The aggregate RMSPE ratio ranking confirms this; we calculate the relevant property crime p-value to be 0.018 (2^{nd} out of 112).

The distribution of violent crime effects offers less clarity. Directly following the arrival of the flotilla, Miami's violent crime treatment effects surge to the top of the distribution; after several quarters, they settle within the collective set of placebo effects for the remainder of the panel duration. We estimate Miami's aggregate p-value to be 0.16 (18^{th} out of 112).

Figure 4: P-Values - Property and Violent Crime

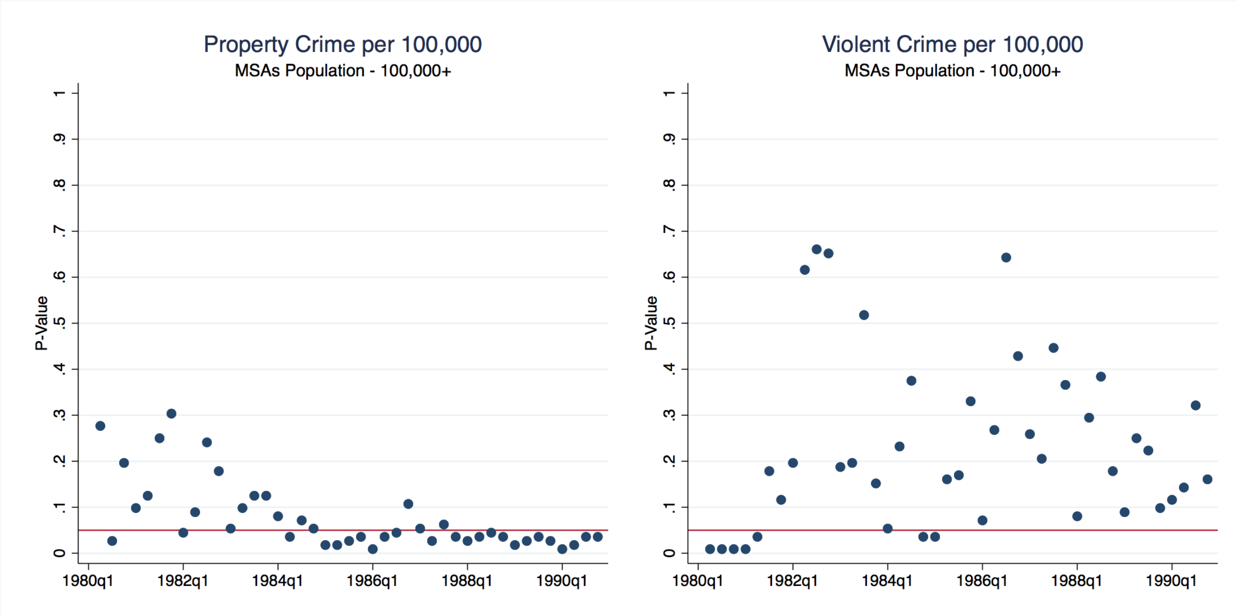


Figure 4 contains secular p-values for treatment effects. The underlying distribution comes from ranking the ratio of post-Boatlift RMSPEs relative to the entire pre-intervention RMSPEs for all panel members. Each p-value identifies Miami's position in the empirical distribution for every period following quarter one 1980. The property crime p-values show a sharpening of statistical significance with time. In contrast, only the violent crime p-values treatment effects in the first five quarters following the Boatlift are statistically significant.

Figure 4, which contains quarterly p-values, affords greater insight. The p-values associated with the property crime effect exhibit a strengthening of statistical significance with time. Immediately after the influx of migrants, only one test statistic is significant at the 95% level of confidence; by 1984, however, essentially all p-values achieve significance.

The disaggregated violent crime test statistics likewise illuminate the secular nature of the Boatlift's effects. Those p-values reveal a statistically significant violent crime effect occurred exactly after the second quarter of 1980. The test statistics then dramatically fade, which explains the value of the aggregate p-value.

Together, the evidence points to a delayed but appreciable increases in long-term property crime compared to the counterfactual. Our property crime specification — along with unreported estimates associated with alternative population criteria — suggest the Boatlift comparably generated 25-32% higher quarterly per capita property crime.

Aggregating over the entire post-treatment phase suggests the flotilla comparatively prompted a 34% quarterly increase in per capita violent crime. If we home in on the first five quarters following the Boatlift, for which we observe statistically significant effects, this short-lived increase becomes 43%.[37]

6.2 Auxiliary Exercises

We now switch outcome variables to two other per capita crime metrics, murder and robbery. For one, both measures are consistently recorded. Unlike forcible rape, murder and robbery reporting depend less on societal contexts. Criminal homicide data are virtually insusceptible to manipulation. Likewise, robbery victims have a vested interest in contacting law enforcement. For these reasons, we repeat our SCM with murder and robbery rates as dependent variables. Our predictors now include pre-Boatlift outcomes. For consistency and robustness, we also estimate results using the exact set of matching variables previously employed; the results are practically identical.

Before delving into the results, we list our matched MSAs in Table 5. The counterfactual constituents closely resemble those of our baseline findings.

[37]Again, we can estimate a range from unlisted specifications. Our estimates live in the range of 43-53%. These are available upon request. See Appendix D for more details about the decision to include the point estimate rather than a range of values.

Table 5: Donor Weights - Murder and Robbery

MSA	Murder	Robbery
Atlanta, GA	0.097	-
Bakersfield, CA	0.117	0.129
Houston - Sugar Land - Baytown, TX	0.065	0.064
Los Angeles - Long Beach, CA	-	0.067
Modesto, CA	0.027	-
New Orleans, LA	-	0.04
New York - Newark- Jersey City, NY-NJ	0.276	0.196
Orlando, FL	0.067	0.344
Phoenix - Mesa, AZ	0.245	0.16
Tucson, AZ	0.106	-

Table 5 lists the weights ascribed to counterfactual constituents when the outcome variables are criminal homicide and robbery. We only include MSAs which receive a positive weight in at least one specification.

Figure 5 displays the outcome variables for Miami and its synthetic controls.

Figure 5: SCM Results - Murder and Robbery

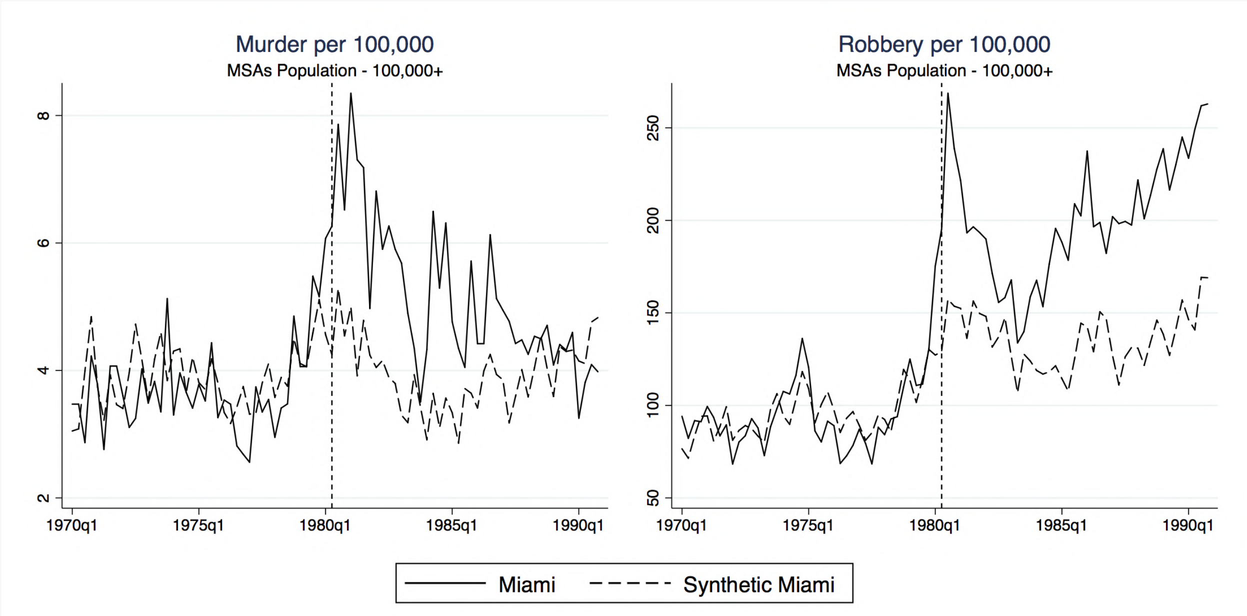


Figure 5 compares raw Miami crime data relative to its synthetic controls. As with our primary results, the two trends virtually overlap prior to the Boatlift. Therefore, synthetic Miami serves as a good counterfactual. Murder rates separate after the arrival of the refugees, and do not converge until 1987. In contrast, the robbery series — barring a period in 1983 — diverge and never rejoin. Both images indicate the existence of substantive treatment effects.

Visual investigation of the pre-treatment phase for the two series imply good counterfactual matches for both murder and robbery. Both graphs also indicate the presence of substantial treatment effects. While murder effects persist until 1987, robbery treatment effects remain throughout the remainder of the panel. We present the distribution of treatment effects in Figure 6.

Figure 6: Treatment Distribution - Murder and Robbery Crime

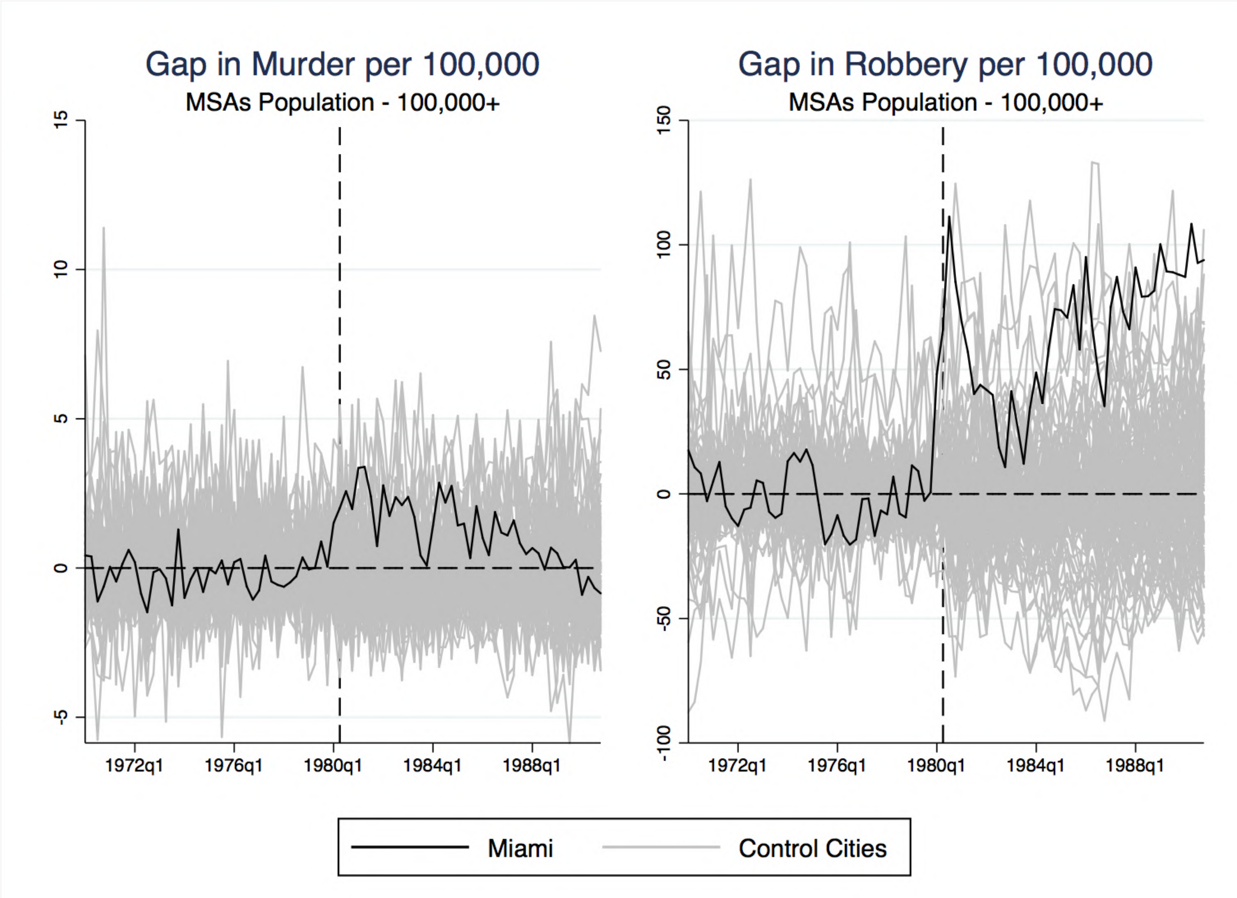


Figure 6 examines estimated murder and robbery treatment effects for all panel units. Given the noise in criminal homicide data, the first graph is deceptive; Miami seemingly lies within the distribution of treatment effects. In contrast, Miami's robbery effect lies toward the top end of the distribution of placebo effects. Clearly, robbery treatment effects reject the null hypothesis that the observed treatment effects could be calculated in the absence of the Mariel Boatlift.

Murder is a noisy variable; therefore, its treatment effects' distribution offers little clarity. Although the treatment effects for Miami seemingly lie nowhere near the upper envelope of quarterly murder effects, we calculate the relevant p-value as 0.0089 (1^{st} of 112). The statistical significance is more straightforward with respect to robbery. Miami's treatment effects essentially compose the upper bound of all effects after the arrival of the Mariel Cubans. Here, we estimate the aggregate p-value at 0.0089 (1^{st} of 112).

The temporal murder and robbery test statistics are captured in Figure 7.

Figure 7: P-Values - Murder and Robbery

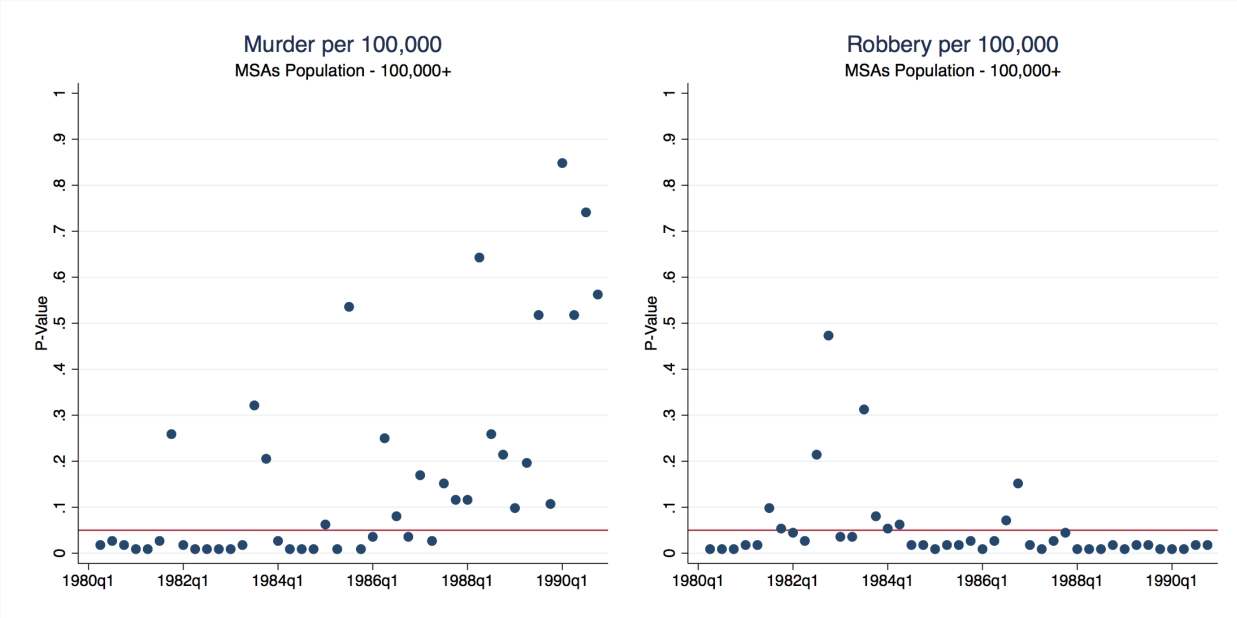


Figure 7 depicts the evolution of statistical significance of murder and robbery effects. The p-values reveal murder effects are strongly significant until 1987. Robbery effects are persistently significant after the arrival of the flotilla.

Both sets of p-values contain noise. Nonetheless, murder treatment effects are strongly significant until 1987. Robbery effects, on the other hand, exhibit statistical significance in nearly every period.

Collectively, the evidence points to a persist relative growth in criminal homicide until 1987 and a long-term comparative increase in robberies. We estimate murder and robbery treatment effects at 41.2% and 70% for the duration of the panel, respectively.

6.3 Robustness Checks

6.3.1 Falsification Tests

To check the internal validity of the quasi-experiment with respect to crime, we replicate placebo exercises typically used to scrutinize difference-in-differences. Specifically, we drop data exposed to the influx of Mariel Cubans, and estimate SCM results with an artificial Boatlift in 1977. Given no actual treatment occurs in 1977, we expect to see no statistically meaningful estimates.

Figure 8 shows the distribution of effects using 1977 as a “placebo boatlift”. These results confirm the internal validity of the main results; Miami’s artificial treatment effects lie nowhere near the upper envelopes.

Figure 8: Placebo Test - Treatment Distribution

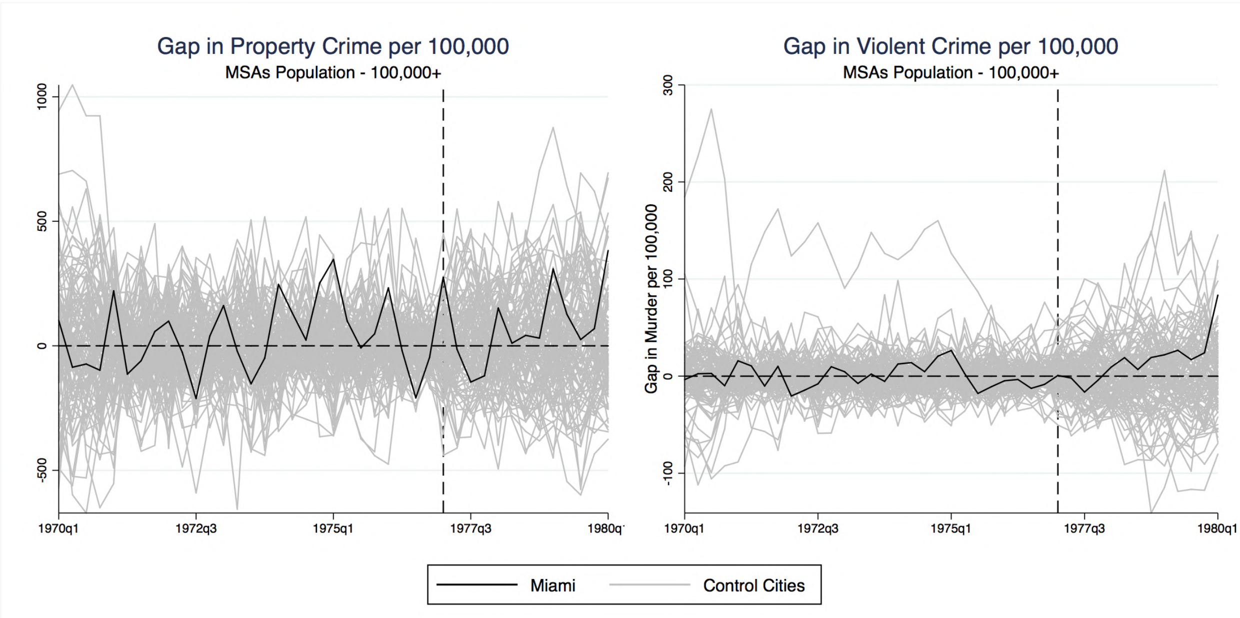


Figure 8 captures property and violent crime treatment effects associated with a hypothesized Boatlift in 1977. We drop all data actually exposed to the Mariel Boatlift. Miami’s artificial treatment effects lie within the distribution of placebos. This evidence supports the internal validity of our primary findings.

More telling evidence comes from aggregate p-values. We find property and violent crime test statistics to be 0.44 (49^{th} of 112) and 0.22 (25^{th} of 112), respectively.

Further justification for the baseline estimates comes from Figure 9.

Figure 9: Placebo Test - P-Values - Property and Violent Crime

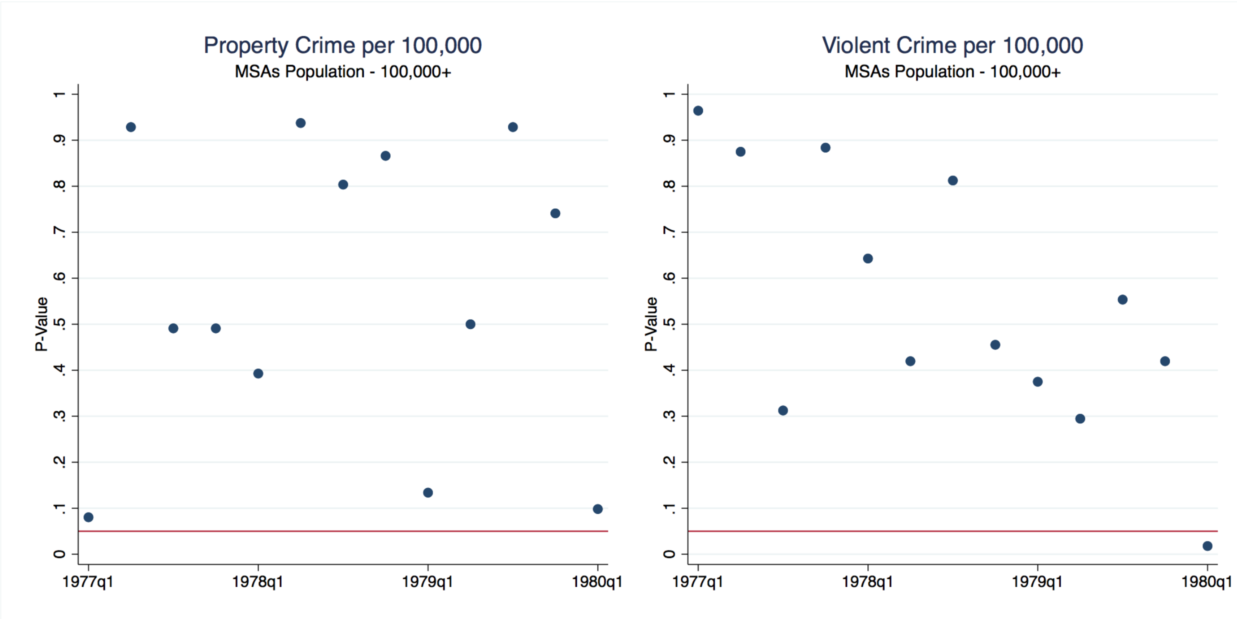


Figure 9 lists test statistics associated with our placebo Boatlift beginning in 1977. Nearly all p-values are insignificant at conventional levels. Although the violent crime p-value leading up to the actual Boatlift is significant, this is likely a statistical anomaly. Given the sudden nature of the refugee crisis, we do not expect to see any anticipatory crime effects.

All but one of the p-values listed above are insignificant. We cannot explain the singular idiosyncrasy. The Boatlift was unexpected; therefore, we doubt this constitutes an anticipatory effect. Given the number of p-values calculated, it is unsurprising to find one significant value. Nevertheless, we remain confident in our ability to make statistical inferences for both violent and property crimes in light of the collective evidence; of course, this holds true to lesser extent for the former.

We close this segment by briefly outlining murder and robbery results connected with similar placebo analyses. Because of their resemblance to previous exercises, we refrain from including all details; we highlight the material aspects.

In this setting, the aggregate p-values for murder and robbery are 0.44 (49^{th} of 112) and 0.32 (36^{th} of 112). This evidence dispels fears our natural experiment lacks internal validity. Even more weight comes from Figure 10, which explores the secular trend of placebo p-values.

Figure 10: Placebo Test - P-Values - Murder and Robbery

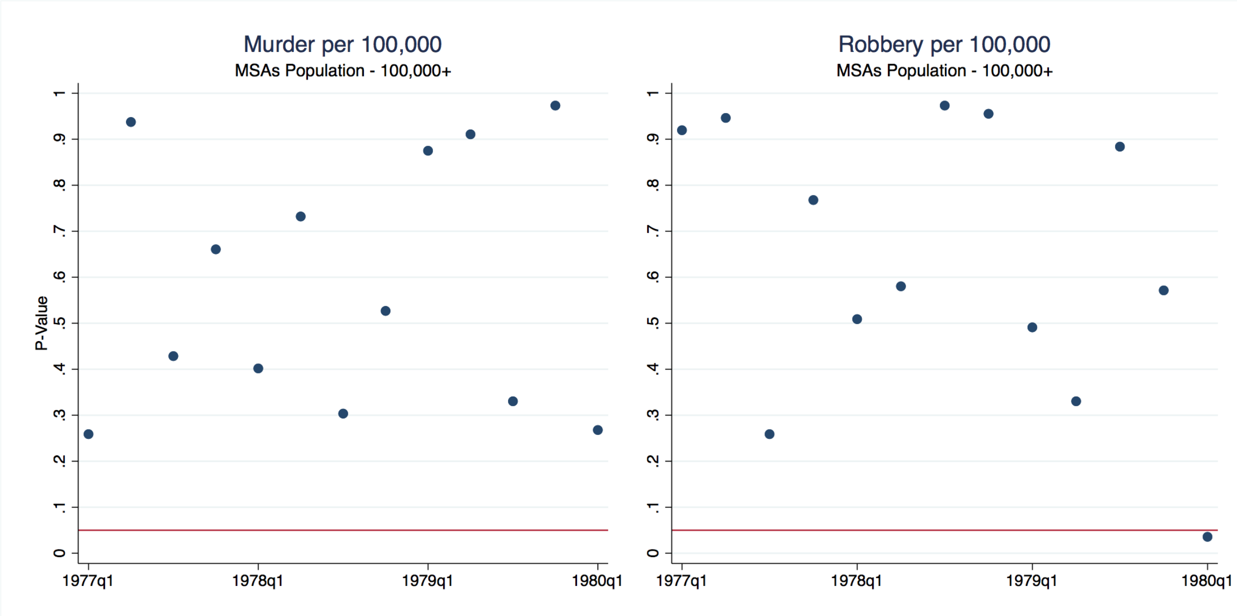


Figure 10 depicts p-values associated with murder and robbery from our placebo exercises. The evidence strongly supports the internal validity of our auxiliary findings given all but one p-value are insignificant.

This evidence is reassuring, especially for our analysis of murder. As with the parallel placebo test for violent crime placebo effects, one robbery p-value is significant; while this result is likely an anomaly, it does marginally dampen confidence in our robbery treatment effects.

6.3.2 Drug Trade

In the 1970s and 1980s, Miami served as the nexus of cocaine distribution in the United States; this phenomenon potential confounds our SCM results. Its proximity to production centers in Latin America and the presence of Spanish speaking connections transformed Miami into a hotbed of drug-related crime.[38] The availability of cocaine eventually sparked the crack epidemic of the mid-1980s. Crack, a blend of cocaine and baking soda, became popular among less well-off users of the pure substance. Research from Fryer, Heaton, Levitt, and Murphy (2005) has shown the importance of the crack cocaine epidemic on violence black

[38] See the Miami Herald article on shopping mall shooting.

youth. Therefore, Miami's relationship with drugs threatens our identification strategy,

However, we note these fears are partially unfounded. By 1975, the Medellín Cartel monopolized and flooded the US cocaine market. Drug-related violence soared in the aftermath, and spread throughout major US cities.[39] Additionally, the crack epidemic did not begin until 1985, well after our significant treatment effects appear.[40] Because we match on violent crime trends, this hypothetically confounding dimension of Miami is likely already captured. For those reasons our synthetic units will implicitly account for the role of early cocaine access on crime rates in the 1980s.

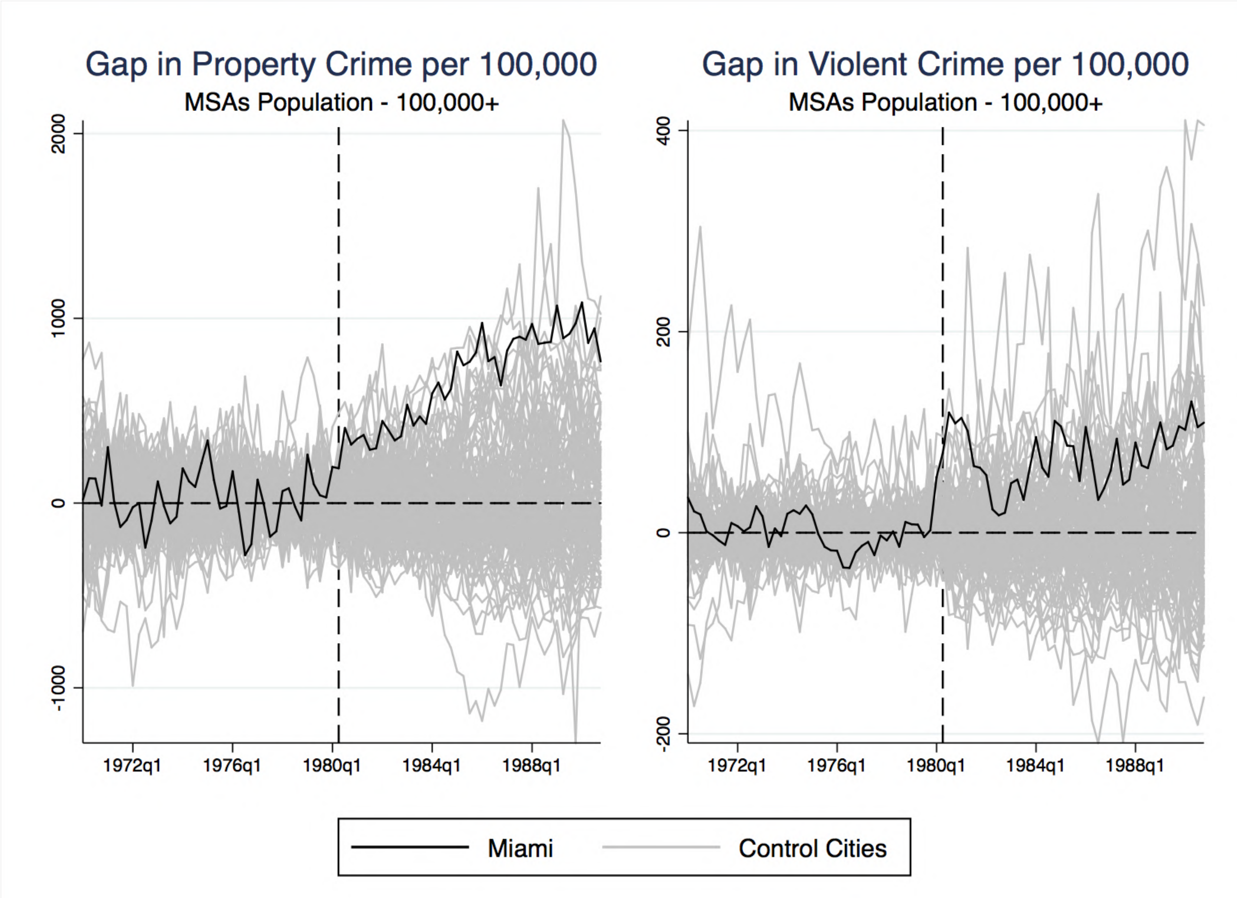
Nonetheless, we attempt to further allay fears by including Underlying Cause of Death data from the Center for Disease Control (CDC) in our SCMs. These records identify cocaine-related fatalities not encompassed by UCR statistics. We construct quarterly per capita cocaine-related deaths for every MSA in our panel; we then integrate this variable into our predictor set.

Figure 11 captures the distribution of treatment effects from our drug-adjusted SCMs.

[39]Read the WSJ's exposé on cocainenomics. See a New York Times article on drug-related violence in Miami.

[40]Fryer et al. (2005)

Figure 11: Specification with ICD-9 Drug Deaths Predictor - Treatment Distribution



The treatment effects distribution above includes drug-related fatalities as a predictor variable in our SCMs. The results are virtually identical to those of our baseline estimates. We follow Fryer et al. (2005) by using ICD-9 entries 8552, 3042, 3056, 8501-8699, 9501-9529, 9620-9629, 972, 9801-9879, 3050-3054,3057-3059, and 9685. Another specification using ICD-282 codes with the word "drug" in their descriptions and elimination of categories not clearly linked with drug-abuse produces results which mirror those we present.

The graphics mirror those associated with the primary estimates. Our property crime treatment effects continue to lie on the upper envelope of placebo effects. Therefore, they remain statistically significant. Though Miami's violent crime effects do not form the upper bound of the treatment distribution, their location does not change relative to our principal effects. The aggregate p-values for property and violent crime confirm this; those values are 0.036 (4^{th} of 112) and 0.16 (18^{th} of 112), respectively.[41]

Figure 12 plots the p-values associated with this matching selection.

[41] Additionally, we reproduce falsification tests but match on drug fatalities prior to the artificial Boatlift. Given the similarity of the results, we suppress their graphics. We estimate for violent and property crime p-values of 0.46 (51^{st} of 112) and 0.22 (26^{th} of 112)

Figure 12: Specification with ICD-9 Drug Deaths Predictor - P-Values

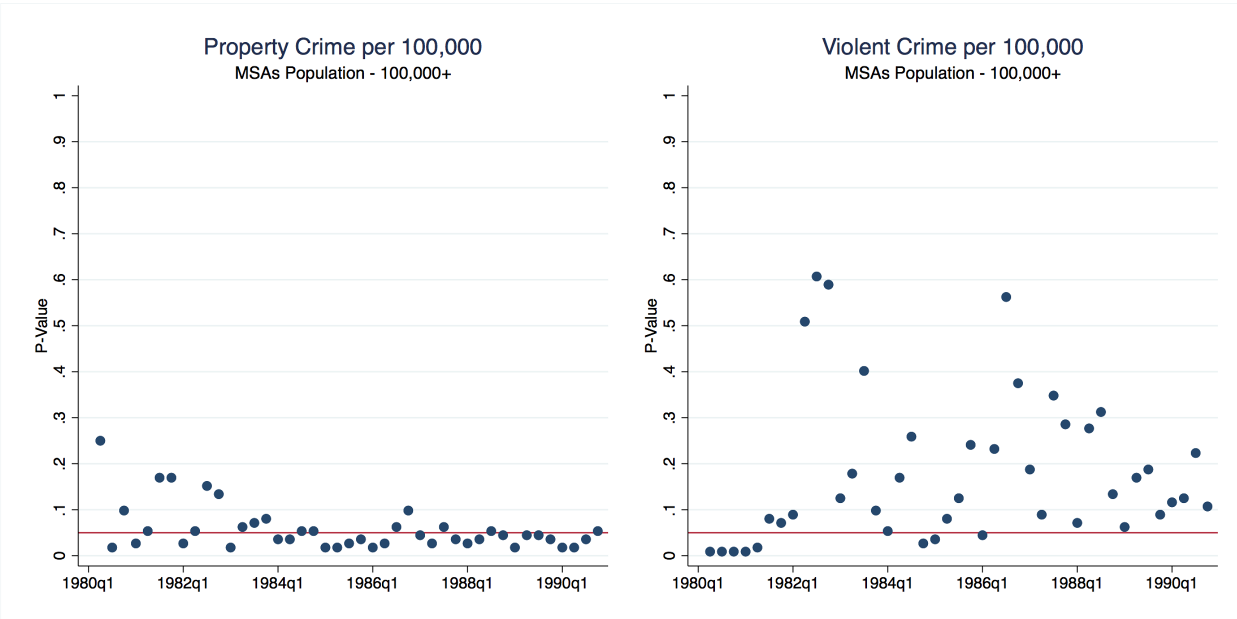


Figure 12 shows property and violent crime p-values associated with the inclusion of ICD drug-related fatalities in our predictor set. The addition of this cocaine trafficking proxy leaves the results virtually unchanged relative to our primary findings.

The visuals follow Figure 4 almost exactly. Therefore, the inclusion of drug trafficking proxies do not substantially alter any aspect of our baseline findings.

6.3.3 Population Mechanism

Our population estimates factor into every single outcome variable. If we incorrectly calculate these figures, we misrepresent per capita crime statistics, and risk drawing erroneous conclusions.

Though we verified our population estimates with data from the Florida Office of Economic and Demographic Research, underlying Census mechanics may prove problematic.[42] Specifically, intercensal records allocate migrants who arrive after April to the following year. Therefore, population calculations do not include Mariel Cubans until 1981. The short-term spike in relative violent crime we observed might be a consequence of this. Moreover, the

[42]See the Florida EDR estimates here.

Census may fail to capture the entire Mariel Boatlift even in subsequent years. Our 1981 MSA estimates indicate Miami grew by 60,000 individuals; this estimate lies below the reported 120,000 Cubans who arrived.

Some of this difference may be accounted for by out-migration, it is well documented that substantial "white flight" in the early 1980s occurred.[43] Additionally, many Marielitos were sent to camps in rural Arkansas, Pennsylvania, Puerto Rico, and Wisconsin.[44] Together, these phenomena likely account for the difference.

To address this we take the most conservative approach and simply assume that, apart from the 60,000 spike in population observed in 1981, the Mariel Cubans were unaccounted for in population statistics. We increase the Miami MSA population by 120,000 individuals in the final three quarters of 1980 (after the April 1980 arrival of Cubans). After 1981, and continuing to the end of our panel, we add 60,000 people to our population statistics. We leave the rest of the data unchanged. In essence, we increase the population of Miami by 120,000 after the arrival of Mariel Cubans until the end of our panel; this figure certainly overstates net population growth within the MSA, and generously increases the denominator for all per capita crime rates. We repeat our primary analyses and compare the results below. Figures 13 and 14 contain the distribution of treatment effects and secular p-values, respectively.

[43]See Miami Herald article on the subject.

[44]Statistics scraped from a Miami Herald database that aims to centralize information on Mariels reveals 50% of refugees were at some point sent to locations outside of Miami. Though many returned, no reliable data can document the length of time it took or how many actually went back.

Figure 13: Specification with Population Adjustments - Treatment Distribution

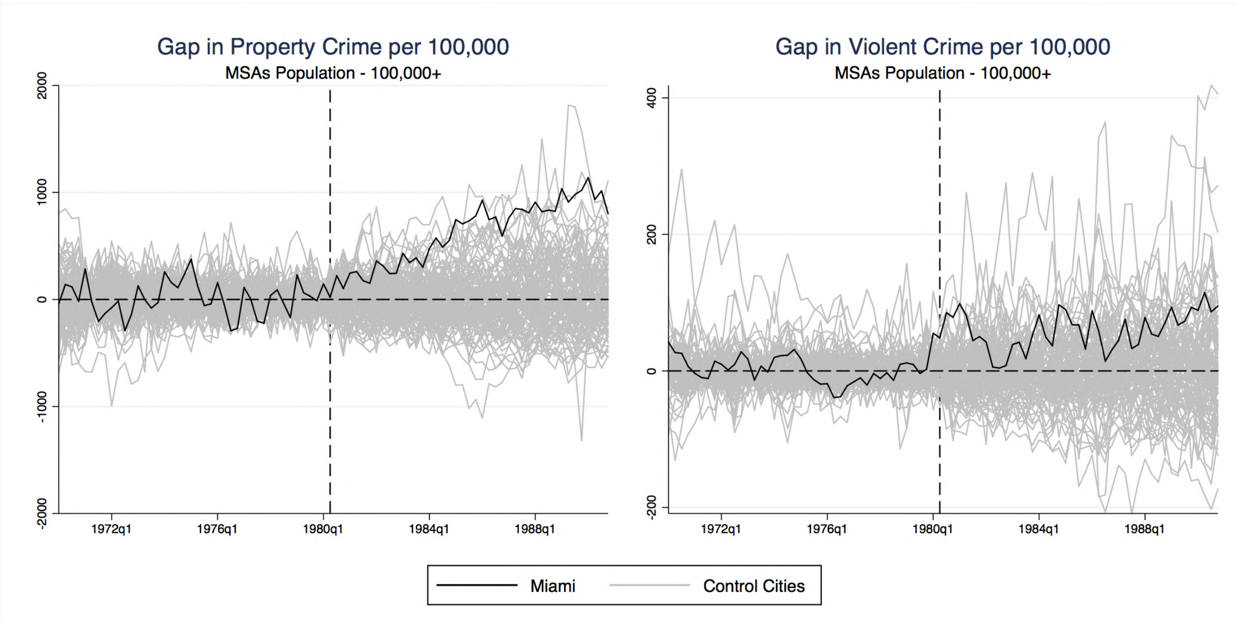
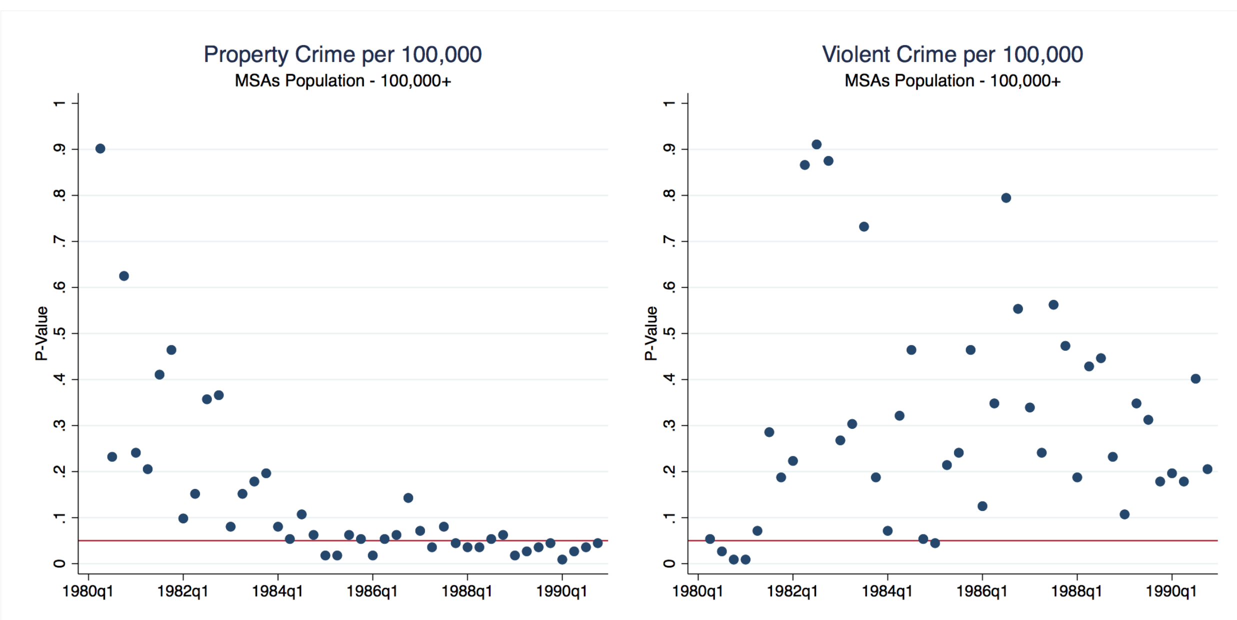


Figure 8 depicts property and violent crime treatment effects generated with adjusted population data. These population statistics include 110,000 and 50,000 more individuals in 1980 and 1981, respectively, relative to the baseline values. Despite this, Miami's treatment effects seemingly retain their position in the distribution compared to the one generated by unaltered data.

Figure 14: Specification with Population Adjustments - P-Values



In Figure 14, we show the secular progression of property and violent crime p-values associated with our population adjustment specification. Despite dramatically increasing the denominators of per capita crime statistics immediately after the arrival of the Boatlift, the test statistics exhibit the same pattern as our baseline specification.

Both figures mirror those found in baseline analysis. We respectively calculate the aggregate p-values associated with property and violent crime at 0.035 (4^{th} of 112) and 0.1875 (21^{st} of 112). Despite dramatically increasing the denominator in 1980 and 1981, the temporary surge in violent crime persists. In general, the findings are virtually identical to those previously discussed. This exercise shows sensitivity to population estimates does not factor into our estimates.

The weight of the robustness checks strongly indicates our results are internally valid and insensitive to potential confounding phenomena. Neither cocaine trade nor mechanical problems with SCMs or underlying data appear to drive our findings.[45]

7 Discussion and Conclusion

Our results present a marginally darker scenario than the preexisting literature portrays. Chalfin (2014), who claimed to identify an upper-bound, found no robust link between immigration and crime. The closest calculations come from Spenkuch (2013). He estimated a 10% increase in the share of immigrants led to a 1.2% rise in property crime. In contrast, we find the 10% increase in Miami's population from the Boatlift prompted a nearly 25-32% expansion in property crime. Moreover, we observe violent crime and murders effects that have not been seen in other research.

Unlike the population of interest for Chalfin (2014), the majority of Mariel immigrants were not properly screened and included individuals with mental illnesses and histories of convictions. Furthermore, the demographic characteristics of Mariel Cubans highly correlate with criminal activity. Events unfolded so quickly federal authorities were unaware of a need to vet the newcomers until they arrived. According to Justice Department officials, nearly 5,000 felons arrived on the flotilla.[46] Normally, these migrants would not receive permanent residence status. However, a repatriation agreement between the US and Cuba did not

[45] While we exclude them for the sake of brevity, additional checks — such as leave-one-out SCMs and estimates that take into account racial tensions within Miami — produce almost identical results.

[46] NY Department of Correctional Service's Executive

exist; thus, deportation was restricted. Though some 2,000 individuals with criminal records were eventually deported, this process was costly. Consequently, the negative selection of Mariel Cubans and the lack of proper oversight constitutes the ideal context for studying the "upper-bound" of crime engendered by immigration.[47]

To give a sense of welfare costs implied by our calculations, we combine our effects with damage valuations from Chalfin and McCrary (2018); their estimates come from a survey of empirical crime research and the statistical value of life literature. They value average property crime, robberies, and homicides — each per capita — at $2,788, $12,624, and $7 million, respectively. Collectively, these suggest a mean per capita cost annually for Miami residents of $68. While we identify public welfare costs, benefits provided by these immigrants have been ignored; that issue is beyond the intent of this project. Our paper identifies one piece of a larger picture and, therefore, do not account for the many ways in which migrants benefit localities.

7.1 Demographic drivers

It is entirely possible the increases in crime we document are compositional. Relative to the rest of Miami residents, Marielitos were disproportionately young, working age men.[48] Roughly 60% were male, and nearly 48% of those arrived between the ages of 15-34. Empirically, males commit crimes at significantly higher rates than females; young men are especially predisposed to crime until they "age out"" in their late twenties.[49]

Assuming Mariel Cubans follow a similar demographic evolution in crime-propensities, we assess the extent to which demographics explain our measured treatment effects. To do so, we estimate predicted crime propensities for both Marielitos and Miami residents based solely on their age and gender distributions. These measures, then, allow us to calculate the expected effects on crime due to changes in age and gender compositions.

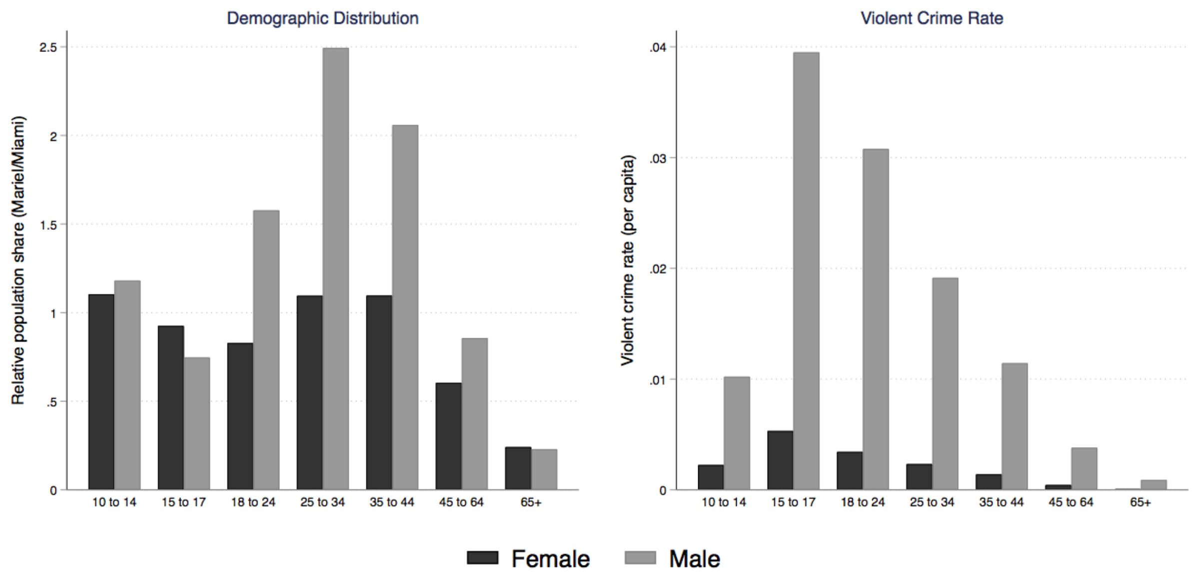
[47]Ibid.

[48]See Table 1

[49]Ellis, Farrington, and Hoskin (2019); Steffensmeier and Streifel (1991)

To construct these propensities, we first pull 1980 and 1990 Census data to sketch age and gender profiles.[50] Next, we calculate crime propensities for our outcome variables using 1980 UCR data on arrests by age and gender.[51] We combine the two datasets, and construct crime rates for pre-Mariel Miami residents and Mariel Cubans by age, gender, and offense type.

Figure 15: Demographic Composition and Relative Crime Rates



Population shares across demographic groups for pre-Mariel Miami residents and Mariel Cubans are calculated from the 1980 and 1990 US Censuses. Violent crime propensities calculated using 1980 UCR of arrests and 1980 US Census. Figure 15 depicts Mariel-to-Miami resident relative population shares on the left panel and per capita violent crime rates on the right. The graphics reveal Mariel Cubans tended to be young, working age men; these features correlate with the criminal propensities in peak crime ages.

Figure 15 depicts Mariel-to-Miami resident population ratios and per capita violent crime rates across demographic groups. The pattern – consistent across crime types – indicates Marielitos occupied age and gender baskets with relatively high propensities for crime.[52]

[50]Because Marielitos arrived after Census enumeration, they do not appear in the 1980 Census. To resolve this, we examine at all 1980-1981 Cuban arrivals in the 1990 Census. We then calculate their age in 1980. Limiting only to Cubans residing in Miami in 1990 provides nearly identical results.

[51]Our primary analyses in this paper rely on reported UCR crime. Unfortunately these reports do not contain information on offender demographics. In their place, we pull UCR arrests. This is an imperfect substitute; arrests do not encompass all crimes committed, and will not perfectly align with the underlying reported crime data.

[52]While not shown, the same pattern emerges among our three other crime measures.

The underlying population shares by age and gender along with the crime rates from Figure 15 allow us to estimate the demographic change on crime induced by the Mariel Boatlift.[53] These results are presented in Table 6.

Table 6: Predicted Treated Effects due to Age and Gender Dynamics

Crime Type	Predicted relative crime rate	Predict increase in crime (%)	Share of treatment effect (%)
Property per 100,000	1.35	3.2	12.7
Violent per 100,000	1.53	4.8	14.2
Robbery per 100,000	1.43	3.9	5.5
Murder per 100,000	1.62	5.6	13.8

Table 6 contains estimates of expected crime effects due to the demographic changes prompted by the 1980 Cuban migration to Miami. The effects capture only a fraction of SCM measures.

This analysis implies age and gender alone predict Marielitos were 35-62% more likely to be arrested than Miami residents. These differences explain only a small portion of our observed effects. Specifically, the predictions suggest demographics differences would cause 3.2% and 4.8% increases in property and violent crime per 100,000, respectively. Correspondingly, these calculations represent 12.7% and 14.2% of our estimated property and violent crime effects.

7.2 Concentration Effects

Another plausible explanation of our treatment effects stems from Mariel Cubans' geographic proximity. Our findings and the extant literature align on this issue with respect to both property and violent crimes.

Theory suggests Mariels' labor market opportunities shaped the group's proclivity for the former.[54] Despite receiving legal access to markets, the cohort faced substantial barriers to employment. For one, Mariel Cubans' characteristics primarily suited them for low-

[53]Note: to ease calculations, we assume the inflow of Cubans in 1980 is equal to 10% of the Miami population.

[54]Becker (1968)

skilled occupations; 54% of the group held the equivalent of a high school level education. Communication skills placed this group at a relative disadvantage within low-skilled sectors; even ten years after their arrival, nearly two-thirds of the group lacked English proficiency.[55] Considering the inflow of refugees constituted a 10% increase in the working-age population, the labor supply shock disproportionately affected low-skilled occupations. Beyond that, the arrival of Mariel Cubans coincided with a recession.[56] Together, these features prolonged already arduous labor market searches. Unsurprisingly, Card (1990) noted almost 40% of Mariels were either unemployed or out of the labor force in 1985.

Therefore, stiff competition between non-Mariel Miami residents and refugees in an anemic labor market likely made illicit activity comparably attractive. Our primary results substantiate this narrative; we consistently find significant property crime and robbery effects, the most likely to arise from economic hardships. Moreover, our pecuniary linked crime effects emerge contemporaneously with the elimination of refugee camps. This further suggests migrants had difficulties settling into their new lives.

Concentration of such a large number of refugees in one city likely induced other crime effects. This hypothesis comes from Martinez Jr. and Lee (2000), who observed high incidence of within-group violence in Miami criminal homicide data. In particular, they found Mariel Cubans lost their lives at the hands of cohort member in 47% of cases. This implies our calculations overstate the extent the public safety decline experienced by the overall community. Furthermore, our findings seemingly point toward this conjecture; the violent crime effects we observe are strongest around the time temporary refugee camps were operational. Grudges held from experiences in Cuba and cramped, inhumane living conditions prompted violent encounters.[57] The collective evidence, consequently, suggests a relationship between migrants' locations within Miami and crime.

We now consider correlational evidence to examine these hypotheses. Namely, we explore

[55] See Table 1

[56] Antón, Antón, and Hernández (2002)

[57] See Palm Beach Post interview with Cesar Odio, assistant city manager at the time of the Boatlift.

the spatial relationship between likely Mariel settlement patterns and crime within the city. Due to data limitations, we do not observe Marielitos' true locations; instead, we proxy these by allocating the inflow of 120,000 refugees according to the pre-Boatlift, non-Mariel Cuban pattern.

We estimate the relationship between Mariel locations and crime via the following equation.

$$y_{i,t} = \alpha + \sum_{t=1970}^{1990} \beta_t S_i^{Mariel} + \gamma_t + \eta_i + \epsilon_{i,t} \text{ for t} \neq 1979\text{, where} \tag{2}$$

$$S_i^{Mariel} = \frac{\frac{n_{i,1980}^{Cuban}}{\sum n_{j,1980}^{Cuban}} \times 120,000}{Pop_i^{1980}}$$

In this event study specification, $y_{i,t}$ captures crime rates per 100,000 in FIPS place i at year t. The variable of interest, S_i^{Mariel}, is constructed by first dispersing 120,000 Mariels via the distribution of non-Mariel Cubans in 1980 within the city; then, we calculate the share of Mariels within a FIPS place. We omit 1979 as it serves as the reference year. We include time and entity fixed effects as well.

Figure 16 captures results associated with Equation 2 across all four outcome variables.

Figure 16: FIPS Place Crime Effects and Relative Exposure to Mariel Cubans

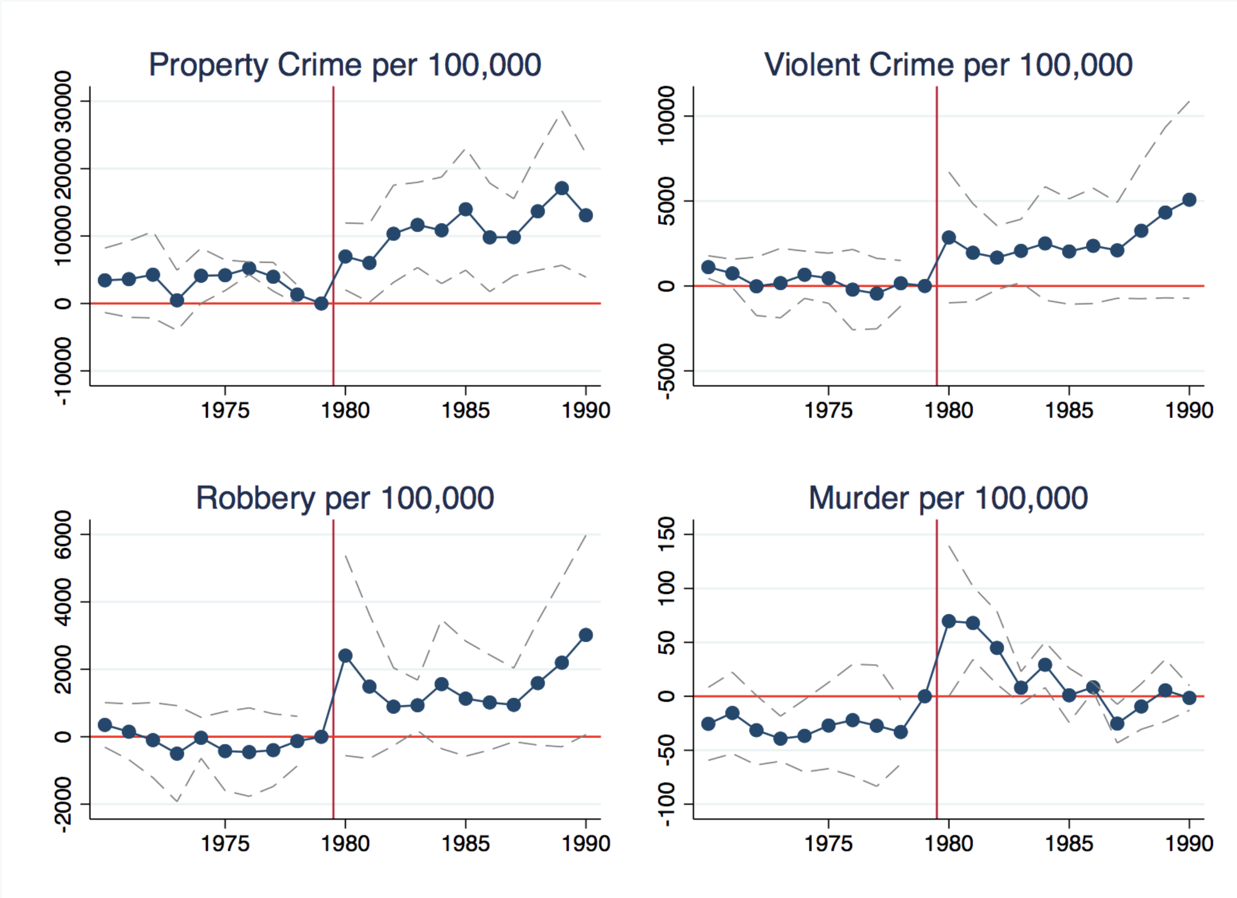


Figure 16 shows estimates of Equation 2 using annual crime per 100,000 residents as outcome variables. Solid line shows coefficients, while 95% confidence intervals are indicated by dashed lines. Observations are weighted by 1970 population; standard errors are two-way clustered by FIPS place and year.

We observe a significant increase in property crime in locations with high concentrations of Mariel Cubans. Likewise, we find similar suggestive evidence for the other three crime metrics. Predictably, the clearest effects — those associated with property crime and robbery — align directly with our primary estimates. Collectively, this evidence corresponds with the argument labor market conditions drove economic crimes. Moreover, it also lends some support to the idea violent crime victims were primarily Marielitos. Finally, the results further indicate our main specifications were not driven by confounding factors.[58]

[58]Results are invariant to monthly or quarterly specifications.

7.3 Policy Implications

Our findings provide little support for current migration fears.[59] More recent cohorts tend to be better educated and thoroughly vetted. Therefore, selection mechanically eliminates the crime effects we document. Though we observe a spike in violent crime, this almost certainly resulted from a combination of addressable factors. Those include poor federal management of a refugee crisis and intended harm from the Castro regime. Despite the greatest efforts to eliminate unauthorized immigration, it will still occur. Barriers to enter labor markets might actually be counteractive, as our results suggest; the inability to immerse into the new culture could lead to substantial increases in property crime. Thus, in the face of similar refugee crises, our findings imply authorities should conduct background investigations and work with communities to assimilate its new members. These actions will expel the inimical effects we observe.

[59] White House Executive Order: Enhancing Public Safety in the Interior of the United States

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Table 7: Accounting of data cleaning

Population group	No. Agencies	Share Missing		Complete Agencies	
		Before	After	Before	After
100,000 and greater	163	0.019	0.001	86	158
25,000 to 99,999	593	0.046	0.025	131	430
Less than 24,999	2696	0.090	0.068	415	1127

A Technical Notes on UCR Cleaning Process

A.1 UCR Adjustments

We identify three types of missing data in the UCR, and address each differently. First, some agencies send aggregated quarterly, semi-annual, or annual statistics rather than monthly records; these data show missing entries leading up to the final month, and then list an aggregated value. For example, annually submitted records show missing observations in all months besides December. Other agencies neglect to submit data for some months; these missing observations may include all categories of crime or a subset.

To ameliorate the issues with the first set of missing observations, we assign aggregated crime data for each type to individual months based on the distribution of crime across non-missing monthly observations for that agency. In the second case — when crime is missing for a series of consecutive months within a year — we linearly interpolate the records using the complete data before and after a given spell. We apply this method when gaps last 4 or fewer months in length; for longer spells of absent data, those agencies are dropped. Generally, our crime data are complete across agencies. Maltz and Weiss (2006) note 90% of agencies submit crime statistics to the FBI. Therefore, these adjustments account for a small share of crime within a given agency. Table 7 shows the share of missing data and the number of eligible sample agencies for which we have complete crime data before and after cleaning.

A.2 Adjusting population data

FBI population estimates are inappropriately calculated. While correct in census years, in non-census years the FBI derives their calculations from contemporaneous projections; these estimates are not retroactively adjusted in cases of inaccurate predictions. These statistics are analogous to the postcensal population projections provided by the US Census Bureau; postcensal estimates calculate population changes based on predicted births and deaths as well as estimates of internal migration derived from administrative sources (e.g., address changes, drivers licenses, tax records, etc.). While not shown here, we discovered FBI population estimates track closely with postcensal population estimates of census places. We also find that FBI population estimates tend to display larger fluctuations over time than those of census places.

Our approach takes advantage of retroactively adjusted county-level population estimates provided by the US Census Bureau. We match those to our agency populations, which overlap with FIPS places. Following a census year, the Census Bureau releases intercensal estimates of populations; these are uniform adjustments of population projections such that the end of decade postcensal population will match that of the following census. Because we were unable to track down intercensal estimates of census places for the 1970s and 1980s, we resorted to matching agencies to counties. This permits us to identify dynamic agency-level population figures. Specifically, we allow an agency's population share of a county to change over time. In each census year (1970, 1980, and 1990) we calculate an agency's share of its corresponding county population ($\theta_{a,t}$). Then, we calculate non-census shares as linear interpolations from the surrounding decades. Let s be the time to the next census, and t_T be the year of the upcoming census.

$$\theta_{a,t} = \frac{s\theta_{a,t_0} + (10 - s)\theta_{a,t_T}}{10}, \text{ where } s = t_T - t$$

We then calculate an agency's population by multiplying agency-county shares by the intercensal county population estimate:

$$p_{a,t} = \theta_{a,t} \times P^{intercensal}_{c(a),t}.$$

Figure 17 shows year-on-year population changes for a handful of MSAs before and after adjustment.

Figure 17: Population Estimates and FBI Population Statistics

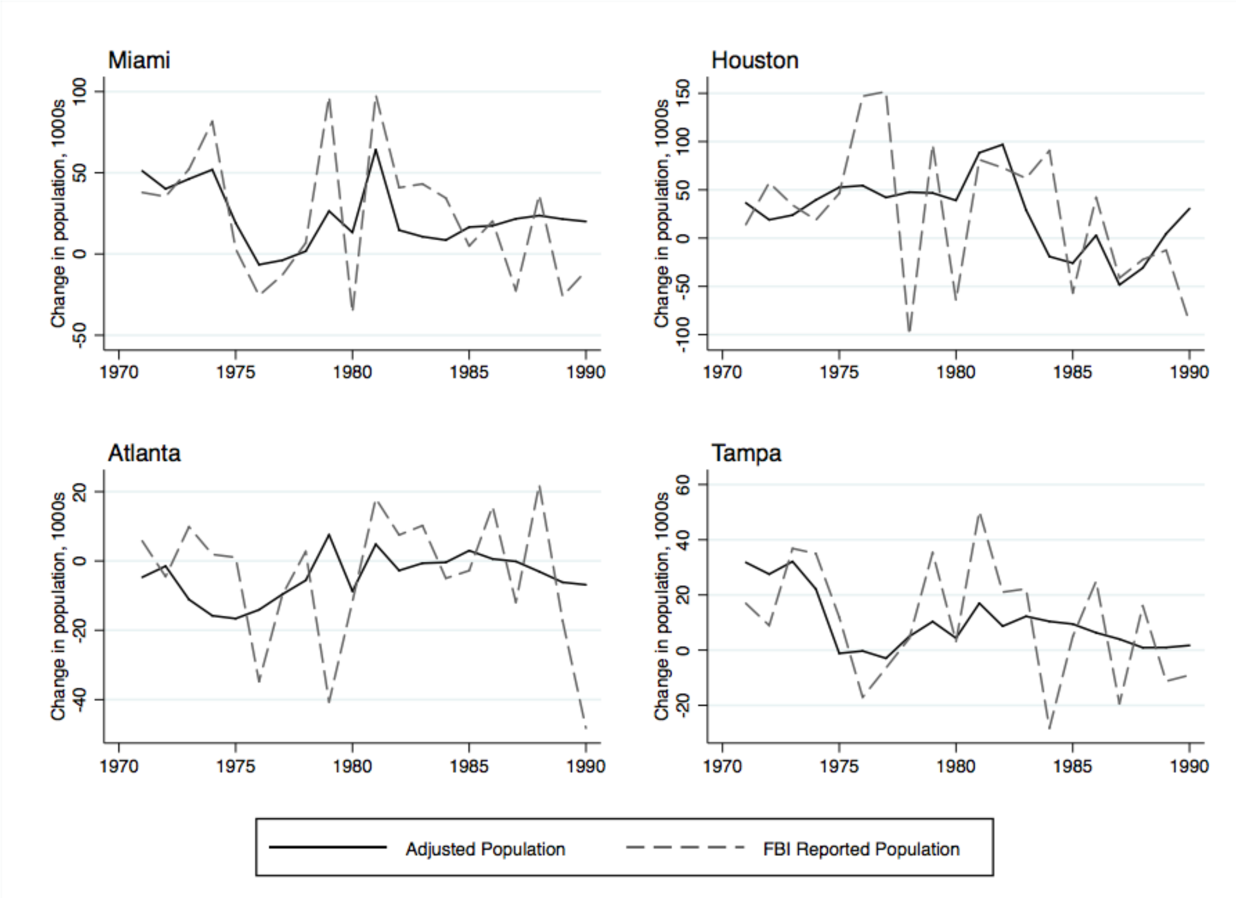

Figure 17 compares our population estimates with those of the FBI. In general, our series exhibit substantially less variation. Furthermore, the FBI data are incorrectly calculated. This is best evidenced in the upper-left panel, which contains Miami population statistics; FBI records show an increase in population equal to the Mariel Boatlift two years prior to the arrival of the flotilla. There are no records this actually occurred, and aggregated census data contradict this observation.

Our adjusted population values display considerably less variation over time relative to the FBI statistics. We test our approach by comparing RMSEs from regressions of population on agency specific quadratic time trends; we find that the RMSE from a regression on adjusted populations is 40% smaller.

It could be the case, though, that the FBI provided populations are actually correct. To test this, we check whether our adjusted crime rates also display less variation. We run identical regressions as above except on annual per capita property crime, using each population value as our denominator; in these regressions we find our approach reduces RMSE by 4.9%. It is also worth noting FBI population statistics indicate the existence of a population surge two years prior to the arrival of the Mariel Boatlift in Miami; this population increase never actually occurred.

B MSA Ranked by Violent Crime Index

Crime by MSA

Violent crime rank	Property crime rank	MSA	Violent crime	Property crime
1	42	BALTIMORE, MD	1,755.50	7,270.5
2	16	ATLANTA, GA	1,334.10	8,609.6
3	92	NEW YORK-NEWARK-JERSEY CITY, NY-NJ-PA	1,305.70	5,828.0
4	7	FLINT, MI	1,268.70	9,482.9
5	53	WASHINGTON, DC-MD-VA-WV	1,213.20	6,904.7
6	63	NEW ORLEANS, LA	1,099.40	6,633.9
7	40	DETROIT, MI	1,082.50	7,293.9
8	21	ST. LOUIS, MO-IL	969.5	8,367.9
9	17	LITTLE ROCK-NORTH LITTLE ROCK, AR	944.1	8,555.2
10	45	LOS ANGELES-LONG BEACH, CA	940.9	7,174.0
11	67	PEORIA-PEKIN, IL	882.4	6,493.8
12	2	BATON ROUGE, LA	866	9,784.6
13	**19**	**MIAMI-FORT LAUDERDALE-WEST PALM BEACH, FL**	**846.8**	**8,468.2**
14	26	SPRINGFIELD, MA	843	8,048.1
15	56	BIRMINGHAM, AL	823.6	6,775.6
16	18	SAN FRANCISCO-OAKLAND, CA	812.6	8,515.9
17	34	HARTFORD, CT	804.9	7,427.8
18	51	JACKSONVILLE, FL	802.9	6,974.1
19	46	RICHMOND-PETERSBURG, VA	792.8	7,106.3
20	28	DAYTON-SPRINGFIELD, OH	792.7	7,876.1
21	85	GREENSBORO–WINSTON-SALEM–HIGH POINT, NC	772.4	5,977.2
22	22	ORLANDO, FL	767.3	8,254.5
23	15	ALBUQUERQUE, NM	758.8	8,629.2
24	14	PORTLAND-VANCOUVER,OR-WA	750.9	8,644.2
25	31	TAMPA-ST. PETERSBURG-CLEARWATER, FL	747.7	7,715.8
26	68	KANSAS CITY, MO-KS	744.8	6,488.5
27	108	PHILADELPHIA, PA-NJ	736.1	4,408.3
28	37	WACO, TX	735.7	7,346.2
29	75	MEMPHIS, TN-AR-MS	735.1	6,342.4
30	5	SACRAMENTO-ARDEN ARCADE-ROSEVILLE, CA	734	9,544.5
31	93	CHICAGO, IL	732.7	5,739.4
32	105	CLEVELAND-LORAIN-ELYRIA, OH	727.1	5,163.8
33	13	DENVER, CO	715.1	8,667.1
34	66	CHARLOTTE-GASTONIA-ROCK HILL, NC-SC	702.8	6,548.4
35	77	BOSTON-WORCESTER-LAWRENCE-LOWELL-BROCKTON, MA-NH	696.6	6,271.2
36	50	CHATTANOOGA, TN-GA	678.2	6,985.7
37	84	MOBILE, AL	669.2	6,068.1
38	102	NASHVILLE, TN	661.9	5,378.8
39	96	LOUISVILLE, KY-IN	657.2	5,626.2
40	62	HOUSTON-SUGAR LAND-BAYTOWN, TX	651.9	6,650.4

Crime by MSA

Violent crime rank	Property crime rank	MSA	Violent crime	Property crime
41	88	GARY, IN	641.8	5,934.1
42	109	PITTSBURGH, PA	637.5	4,287.2
43	90	EVANSVILLE-HENDERSON, IN-KY	630	5,910.9
44	1	BAKERSFIELD, CA	613.8	9,896.5
45	97	BUFFALO-NIAGARA FALLS, NY	611.5	5,609.4
46	9	STOCKTON-LODI, CA	608.8	9,191.0
47	55	CORPUS CHRISTI, TX	601	6,785.4
48	35	DALLAS-FORT WORTH-ARLINGTON, TX	590.1	7,416.0
49	95	BEAUMONT-PORT ARTHUR, TX	582.9	5,695.8
50	24	SEATTLE-BELLEVUE-EVERETT, WA	582.5	8,153.7
51	4	PHOENIX-MESA, AZ	577	9,555.9
52	30	RIVERSIDE-SAN BERNADINO, CA	576.1	7,791.1
53	94	INDIANAPOLIS, IN	559.4	5,707.4
54	80	PUEBLO, CO	557.3	6,160.8
55	59	CINCINNATI, OH-KY-IN	555.3	6,720.2
56	83	RALEIGH-DURHAM-CHAPEL HILL, NC	552.7	6,089.8
57	32	TACOMA, WA	545	7,652.0
58	10	FRESNO, CA	541.3	9,064.8
59	48	TOPEKA, KS	535	7,043.1
60	27	ROCHESTER, NY	530.8	7,958.5
61	99	NORFOLK-VIRGINIA BEACH-NEWPORT NEWS, VA-NC	521.7	5,514.4
62	98	YOUNGSTOWN-WARREN, OH	520.6	5,569.5
63	74	OMAHA, NE-IA	519.2	6,364.3
64	103	KNOXVILLE, TN	518.4	5,232.9
65	57	COLUMBUS, OH	504.5	6,763.2
66	52	SAN ANTONIO, TX	501.6	6,915.8
67	69	GRAND RAPIDS-MUSKEGON-HOLLAND, MI	501.3	6,483.2
68	61	LUBBOCK, TX	499.5	6,688.1
69	71	TULSA, OK	491.3	6,427.7
70	3	MODESTO, CA	472.9	9,625.1
71	44	ANCHORAGE,AK	470	7,183.8
72	58	ROCKFORD, IL	469.5	6,753.1
73	12	ANN ARBOR, MI	468.7	8,798.4
74	8	TUCSON, AZ	464.4	9,204.0
75	76	AKRON, OH	462.6	6,281.3
76	78	OKLAHOMA CITY, OK	462.1	6,211.6
77	38	COLORADO SPRINGS, CO	447.3	7,344.1
78	20	RENO, NV	442	8,407.4
79	43	SAN DIEGO, CA	437.3	7,232.8
80	91	SHREVEPORT-BOSSIER CITY, LA	434.8	5,866.1

Crime by MSA

Violent crime rank	Property crime rank	MSA	Violent crime	Property crime
81	101	MACON, GA	428.6	5,404.0
82	41	AUSTIN-SAN MARCOS, TX	427.8	7,282.0
83	82	LEXINGTON, KY	405.5	6,104.2
84	11	SALT LAKE CITY-OGDEN, UT	405.1	8,841.0
85	73	DAVENPORT-ROCK ISLAND-MOLINE, IA-IL	392.1	6,385.0
86	54	EL PASO, TX	390.2	6,881.1
87	33	WICHITA, KS	390	7,557.3
88	70	NEW HAVEN-BRIDGEPORT-STAMFORD-WATERBURY-DANBU	369	6,469.7
89	87	JACKSON, MS	368.1	5,963.0
90	39	ORANGE COUNTY, CA	366.8	7,295.0
91	25	SPOKANE, WA	364.2	8,115.5
92	81	AMARILLO, TX	363.4	6,141.3
93	89	SYRACUSE, NY	361	5,922.2
94	65	LANSING-EAST LANSING, MI	358.5	6,550.7
95	72	PROVIDENCE-WARWICK-PAWTUCKET, RI	341	6,417.7
96	6	EUGENE-SPRINGFIELD, OR	336.9	9,527.5
97	111	COLUMBUS, GA-AL	334.9	3,827.0
98	29	SAN JOSE, CA	334.1	7,844.5
99	107	ERIE, PA	330.8	4,471.9
100	60	DES MOINES, IA	311.3	6,698.2
101	86	HUNTSVILLE, AL	305.8	5,969.0
102	79	MONTGOMERY, AL	275.8	6,209.4
103	112	ALBANY-SCHENECTADY-TROY, NY	273.4	3,764.0
104	49	BOISE CITY, ID	266	7,042.2
105	36	FORT WAYNE, IN	259.7	7,363.0
106	23	SPRINGFIELD, MO	258.9	8,163.6
107	106	MILWAUKEE-WAUKESHA, WI	227.9	4,785.7
108	100	MINNEAPOLIS-ST. PAUL, MN-WI	227.6	5,455.1
109	110	ALLENTOWN-BETHLEHEM-EASTON, PA	216.3	4,257.1
110	104	LINCOLN, NE	205	5,181.8
111	64	CEDAR RAPIDS, IA	173.5	6,603.9
112	47	MADISON, WI	127.5	7,060.1

C Predictor Balance

Predictor Balance for Primary Estimates - Part 1

Variable		Property Crime	Violent Crime
	Miami	Synthetic Miami	Synthetic Miami
Property Crime - 1971	2286.55	2305.33	2036.31
Property Crime - 1973	2195.58	2191.35	2059.25
Property Crime - 1975	2188.49	2088.19	2006.26
Property Crime - 1977	1897.32	1974.07	1900.97
Property Crime - 1978	1970.32	1991.03	1910.72
Property Crime - 1979	2162.27	2084.74	2026.54
Property Crime - 1980q1	2444.13	2298.05	2163.60
Violent Crime - 1971	201.55	185.86	20.04
Violent Crime - 1973	198.36	186.13	196.02
Violent Crime - 1975	213.25	236.82	217.58
Violent Crime - 1977	200.87	230.81	213.47
Violent Crime - 1978	240.96	254.81	245.36
Violent Crime - 1979	273.16	272.32	268.13
Violent Crime - 1980q1	348.02	266.36	292.76
Migrants per Capita - 1971	300.51	242.29	228.36
Migrants per Capita - 1973	204.90	229.22	223.57
Migrants per Capita - 1975	2003.86	2372.02	2465.12
Migrants per Capita - 1977	2097.00	2207.24	2153.25
Migrants per Capita - 1978	3350.41	2987.47	2928.05
Migrants per Capita - 1979	5708.00	5305.83	6039.16

The table above shows predictor variables for Miami and its synthetic controls. Column 1 captures raw Miami statistics. Columns 2 and 3 reflect the weighted predictors values for synthetic controls when the outcome variables are property and violent crime, respectively. Differences in crime in the quarter just prior to the Boatlift exist between the two groups. However, we average annual crime rates for every other period; the largest differences occur in exactly that quarter. The cyclical and idiosyncratic nature of crime could mechanically produce this. We note the differences between the two groups amount to no more than 5% of property crime and 16% of violent crime for Miami; these figures lie well below our estimated effects. Since the Boatlift was unexpected, there should be no anticipatory impact.

Predictor Balance for Primary Estimates - Part 2

Variable	Miami	Property Crime Synthetic Miami	Violent Crime Synthetic Miami
Share Black - 1980q1	0.15	0.17	0.19
Share HS Dropout - 1980q1	0.36	0.34	0.32
Unemployment Rate - 1980q1	0.02	0.04	0.03
Share 18-14 - 1980q1	0.11	0.14	0.15
Share Hispanic - 1980q1	0.25	0.15	0.12
Poverty Rate - 1980q1	0.14	0.15	0.16
Median Income - 1980q1	15119.92	15959.03	14488.17
Population Density - 1980q1	5806.28	6262.40	5906.83
Female Officers per Capita - 1975	6.85	4.58	7.12
Female Officers per Capita - 1979	10.77	7.56	11.65
Officers per Capita - 1975	218.52	246.79	263.91
Officers per Capita - 1979	222.72	229.89	250.35

The table above shows predictor variables for Miami and its synthetic controls. Column 1 captures raw Miami statistics. Columns 2 and 3 reflect the weighted predictors values for synthetic controls when the outcome variables are property and violent crime, respectively. Hispanic shares of population differ different between Miami and its synthetic controls. This discrepancy is probably a matter of geography, and therefore is likely innocuous. Sociological evidence tends to show Latinos commit crimes at lower rates than the rest of the American population. Therefore, we underestimate our effects if this predictor substantively factors into the construction of SCM weights (Feldmeyer, 2009; Hagan & Palloni, 1998; Ulmer, Harris, & Steffensmeier, 2012).

D Preferences for 100,000

Our preference for the 100,000 threshold comes from differences in counterfactual constituents between this cutoff value and alternative ones. Specifically, the 100,000 criterion — and lower levels — ascribe weight to Orlando. We believe this inclusion eliminates state-level time-trends. In general, the outcome variables track closely across all major Florida MSAs. This fact is documented by Figure 18

Figure 18: Crime Rates in Florida MSAs

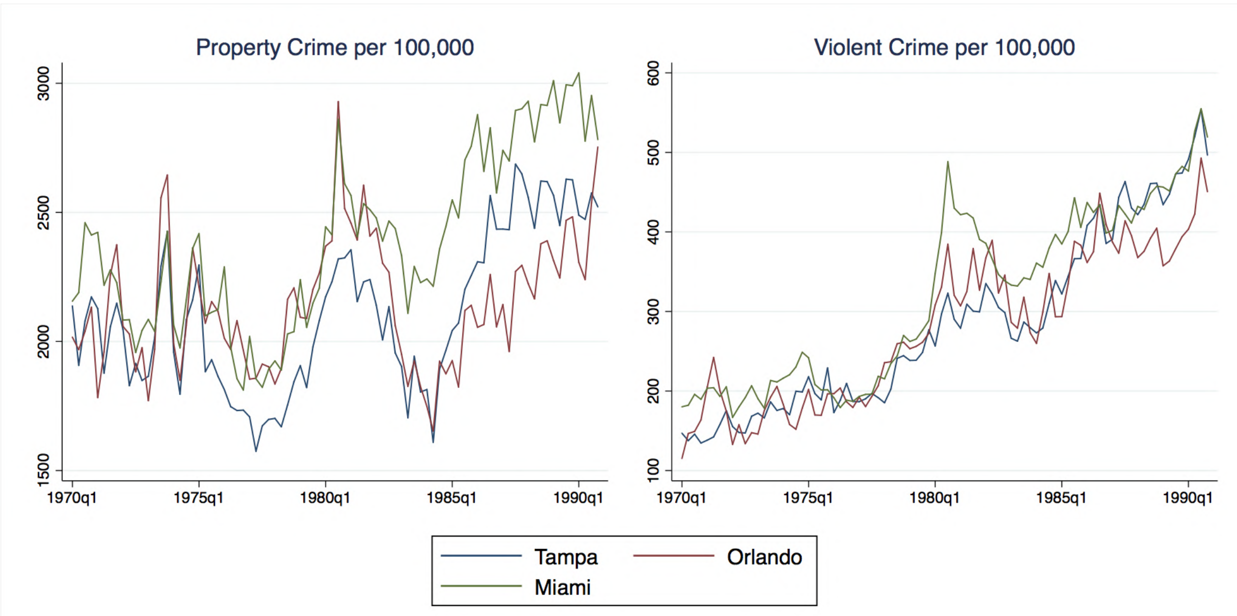

Figure 18 captures secular plots of the outcome variables of interest for the three largest Florida MSAs. These trends are remarkably similar. Therefore, counterfactuals which include either Tampa or Orlando should resemble Miami.

Difference-in-differences estimates reinforce this visual evidence. The associated results in Table 8 are generally consistent with SCM treatment effects.

Table 8: DiD Estimates - Treatment Effect

	FL MSAs		Tampa and Orlando	
	Viol. Crime Rate	Prop. Crime Rate	Viol. Crime Rate	Prop. Crime Rate
Miami × Post-Mariel	64.35***	196.66**	26.44***	275.19***
	(15.29)	(81.82)	(7.12)	(37.01)
Observations	1,344	1,344	252	252
Time Dummy	✓	✓	✓	✓

Standard errors in parentheses. Each rate is reported per 100,000.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Evidence in favor of the parallel trends assumptions can be found in Table 9. The second and third columns include all Florida MSAs excluding Miami as the counterfactual group; the fourth and fifth rely on just Tampa and Orlando as comparators.

Table 9: Placebo Tests

	FL MSAs		Tampa and Orlando	
	Viol. Crime	Prop. Crime	Viol. Crime	Prop. Crime
Miami × Post-Mariel	77.88**	281.29**	31.30**	293.16***
	(24.57)	(131.57)	(11.32)	(59.58)
Miami × Pre-Mariel 1	39.77	251.22	14.86	85.86
	(30.58)	(163.75)	(14.09)	(74.15)
Miami × Pre-Mariel 2	31.36	99.14	18.55	-30.71
	(31.30)	(167.60)	(14.42)	(75.90)
Miami × Pre-Mariel 3	-19.65	-28.47	-14.98	9.96
	(31.30)	(167.60)	(14.42)	(75.90)
Observations	1,344	1,344	252	252
Time Dummy	✓	✓	✓	✓

Standard errors in parentheses. Each rate is reported per 100,000.

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Concern emerges if Marielitos migrated to Orlando. While we cannot directly test this, we note two facts. For one, the Cuban-American population within Orlando is historically low relative to Miami and as a whole.[60] Secondly, the 1990 Census indicates only 1,000 Cubans who arrived between 1980 and 1981 lived in Orlando; this constitutes less than 1% of the flotilla and Orlando's population at the time. In contrast, the 1990 Census finds nearly 66% of Cuban immigrants who arrived around the Boatlift reside in Miami. Thus, "contamination" of other Florida MSAs seems unlikely.

[60]See the Migration Policy Institute's report on this item here.