The Fiscal Effects of Immigration on Local Governments: Revisiting the Mariel Boatlift

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Abstract

Immigration raises important political and economic questions, yet there remains considerable disagreement about its short- and long-term consequences. This paper examines the fiscal consequences of immigration for local governments. Previous work has highlighted the gap between the long-term economic benefits of immigration and the short-term fiscal burden posed by recent arrivals, however several influential estimates based on cash-flow accounting suffer from potential bias. I use a quasi-experimental approach to re-examine a famous case: the large wave of Cuban refugees that landed in Miami in 1980, otherwise known as the Mariel Boatlift. Using a synthetic control design, I find that per-pupil education costs increased in Miami in the aftermath of the Boatlift, financed by an increase in state transfers. These effects persisted for at least ten years. The results shed light on the heterogeneous impacts of immigration over time and space.

JEL Codes: H72, J15, H77

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1 Introduction

Immigration remains a perennial source of political disagreement, even as it promises important economic benefits. Due to the responsiveness of immigrants to economic conditions (Basso and Peri, 2020) and the large share of college-educated immigrants with degrees in science and engineering (Hunt and Gauthier-Loiselle, 2010), immigration has the potential to increase productivity and innovation, not to mention expand total economic output. These forces are particularly pronounced in the United States, where the foreign born population has a higher labor force participation rate than the native-born (Bureau of Labor Statistics, 2022). Nevertheless, immigration raises concerns about wage pressures on the native population and the fiscal burden of providing services for non-taxpayers. While there exists an extensive economic literature on the labor market impacts of immigration,[1] there is much less research on the fiscal impacts, particularly the near-term effect on local governments that provide services to recent arrivals. Without accounting for these fiscal impacts, it is impossible to provide a full accounting of the welfare effects of immigration.

This paper examines the fiscal impacts of immigration on local governments by revisiting the Mariel Boatlift, the large wave of Cuban refugees that landed in Miami in 1980. Due to the size and unexpected nature of the shock that it posed to the local labor market, a previous literature has investigated its effect on local wages and employment (Card, 1990; Borjas, 2017; Peri and Yasenov, 2019; Clemens and Hunt, 2019). However, to date the *fiscal* consequences of the Boatlift remain underexplored. To examine the impact of the Boatlift on local government budgets, I employ a synthetic control design. The design compares budgetary outcomes in Miami to a synthetic control group constructed from ten years of pretreatment data and a nationwide pool of possible comparison units. Due to the overlapping and fragmented nature of local governments in the United States, I consider outcomes across three levels of government – Miami-Dade County, the Miami-Dade School District,

[1]See, for example, Abramitzky et al. (2019), Albert (2021), Dustmann et al. (2013), Dustmann et al. (2017), and Ottaviano and Peri (2012), among others.

and the City of Miami – thereby capturing the full effect of the Boatlift on a variety of government services. Drawing on fiscal data from the Census of Governments and county-level population data, the analysis considers the effect of the Boatlift on population demographics as well as revenues and expenditures for each level of government over the ten year period following the Boatlift (1981-1990).[2]

This paper builds on an earlier body of work that uses an accounting methodology to estimate the fiscal effects of immigration. This work typically uses individual-level census data to calculate the net fiscal impact of immigrants relative to natives on the basis of their tax contributions and benefit take-up. Prominent among this literature is a widely cited report by the National Academies of Science, Engineering, and Medicine (NAS) that provides a comprehensive set of estimates regarding fiscal impacts, concluding that immigrants have a positive fiscal impact on the federal government, but a negative fiscal impact on state and local governments, largely owing to the cost of educating immigrant children (Blau and Hunt, 2019; National Academies of Sciences, Engineering, and Medicine, 2017). Owing to the richness of the data and precision of the estimates, these estimates have largely come to inform the public debate. However, estimates based on cash flow accounting suffer from several limitations (Clemens, 2022).[3] First, the estimates are sensitive to how the costs of public goods are allocated, eg. the extent to which immigrants increase the cost of road maintenance (Orrenius, 2017). Second, and perhaps most importantly, they are biased due to their failure to account for the indirect effects of immigration, such as its impact on prices or productivity. A separate approach that has attempted to account for these general

[2]To my knowledge, there is only one other piece of research that considers the fiscal effects of the Boatlift. In a concurrent paper, Yao et al. (2022) look at how the Boatlift affected a set of fiscal outcomes in the city of Miami that are based on an economic freedom index compiled by Stansel (2019). Importantly, the outcomes that they consider, including sales tax revenue and property tax revenue, are scaled by personal income. Since personal income was likely affected by the Boatlift, these outcomes might more properly be considered economic rather than budgetary outcomes. In contrast, this paper uses budgetary data as compiled by the Census and considers the effect of the Boatlift on a wide variety of revenue and expenditure measures, not just for the city of Miami, but for several distinct government entities in the Miami metropolitan area, including the school district.

[3]Other studies utilizing an accounting methodology include Bratsberg et al. (2010), Bratsberg et al. (2014), Dustmann and Frattini (2014), Hansen and Lofstrom (2003), Jofre-Monseny et al. (2016), Martinsen and Pons Rotger (2017), and Ruist (2014).

equilibrium effects by modeling immigration's effects on productivity and the prices of labor and capital (see, eg. Chojnicki (2013)) also suffers from shortcomings, namely that its estimates are also highly sensitive to modeling choices (Clemens, 2022). The sensitivity of these estimates to a range of untestable assumptions limits their ability to inform the policy debate.

By exploiting the Boatlift as a natural experiment, this paper documents the causal impact of an immigrant wave on local government finances and avoids the strong modeling assumptions of this earlier literature. While the synthetic control design requires certain assumptions as with any quasi-experimental approach, the analysis does not require any additional assumptions about the distribution of public goods provision or the general equilibrium impact of immigration on secondary markets. Instead, the reduced-form approach captures the indirect fiscal impacts of immigration on local government budgets, such as those operating through labor or housing markets. Additionally, the paper avoids some of the difficulties associated with the aggregation of individual-level survey data, such as measurement error or nonresponse bias (Bollinger et al., 2018), by looking directly at government finances as the outcome of interest.

In examining the fiscal impacts of an immigration shock to a particular region, this study also joins recent work in highlighting heterogeneity across time and space (Card et al., 2007; Mayda et al., 2023) rather than focusing on long-term, nation-wide impacts. Immigrants are unequally distributed geographically and disproportionately cluster in the most heavily populated metro areas (Pew Research Center, 2020; Sharpe, 2019). Understanding the implications of these diverse inflows for fiscal federalism requires well-identified estimates at the local level. Not only does this paper examine how the fiscal consequences of the Boatlift for the Miami region evolved over time, but by separately estimating the effect on three different layers of government, it also sheds light on the specific fiscal and administrative channels that were affected as well as the specific revenue and expenditure categories driving the overall effects.

The findings highlight the disparate impact that immigration has on different types of local governments. The largest estimated effects are evident for the Miami-Dade School District, which experienced an expenditure increase of 25 percent, reflecting a 50 percent marginal increase in per-pupil expenditures. The City of Miami experienced a 19% increase in total spending in the immediate aftermath of the Boatlift that quickly tapered off, with precise effects for the parks and recreation department, consistent with a short-turn rise in the cost of providing shelter and relief. In contrast, the county government experienced no fiscal consequences. An examination of revenues indicates that the school district financed its increase in expenditures through an increase in state transfers. A supplemental analysis shows that the school district in Palm Beach County, the other county in the metropolitan area to experience a large increase in its student-age population, similarly experienced a sharp increase in per-pupil expenditures.

The paper proceeds as follows. Section 2 provides background on the Mariel Boatlift and local governments in the Miami region. Section 3 discusses the synthetic control approach and the details of its application. Section 4 outlines the data. The results are presented in Section 5. Section 6 presents additional results for surrounding counties. Section 7 concludes.

2 Background

2.1 Mariel Boatlift

Under a backdrop of housing and job shortages that resulted from the struggling Cuban economy, on April 20, 1980 the Castro regime announced that any Cubans wishing to emigrate to the United States were free to board boats at the port of Mariel. This unexpected announcement precipitated a wave of approximately 125,000 Cuban refugees that fled to U.S. shores between April and October in what became known as the Mariel boatlift. The exodus concluded by mutual agreement between the Castro and Carter administrations in

October 1980. In 1984, Congress amended the 1966 Cuban Adjustment Act, thereby placing the recent Cuban arrivals on a path to citizenship.

At the same time that the situation in Cuba was escalating, the Carter administration was negotiating the legal status of Haitian refugees, who had been arriving by boat for years and claiming political persecution by the Duvalier regime. The influx of Cuban refugees brought the issue to a head, and under pressure from members of Congress not to treat the two groups differently, the administration agreed to afford Cuban and Haitian refugees the same legal status (Engstrom, 1997). Approximately 25,000 Haitians would also enter the United States during the Boatlift.

Most of the refugees were processed at camps in the greater Miami area. A large majority were unskilled and without a high school diploma (Peri and Yasenov, 2019). Based on careful examination of the 1980 and 1990 Censuses, Peri and Yasenov (2019) concluded that approximately sixty percent of the refugees remained in the Miami metropolitan area as of 1990 and thus had likely settled there permanently, ultimately increasing the Miami labor force by approximately 54,000 or 8 percent.

2.2 Effect on the Miami Labor Market

Card (1990) was the first to exploit the Boatlift as a large, exogenous shock to the Miami labor market. Using difference-in-difference methods and a comparison group of large cities, Card concluded that the Boatlift had no economically significant impact on the wages and employment of low-skilled non-Cubans in Miami. The study was an early example of how to construct a quasi-experimental comparison group. Nevertheless, later researchers reconsidered the Boatlift using newer methods. Using a restricted subsample of high school dropouts and the Current Population Survey (CPS), Borjas (2017) constructed comparison groups based on employment trends prior to the Mariel shock, finding that there was in fact a large and lasting effect on the wages of low-skilled workers in Miami. In the following years, two other papers, Peri and Yasenov (2019) and Clemens and Hunt (2019) replicated

Borjas' findings, but argued that the results were an artifact of a shift in the composition of certain small subsamples of workers in the CPS that was specific to Miami. Using a synthetic control approach, Peri and Yasenov (2019) reached a similar conclusion as Card (1990), namely that there was no statistically significant effects of the Boatlift on the wages of high school dropouts in Miami.[4]

Notably, all of these studies were focused on labor market outcomes, and in particular on the labor market outcomes of low-skilled workers. This paper proposes instead to examine the effect of the Mariel shock on the finances of local governments in the Miami region. While the labor market consequences of immigration are of first-order importance to understanding the political perceptions of native workers, a full accounting of the economic consequences of immigration also requires knowledge of the short and long-run fiscal impacts, and in particular the heterogeneity of those impacts on governments at different levels within a federalist system.

2.3 Local Governments in Miami

In addition to focusing on labor market outcomes, the vast majority of work on the Boatlift has focused on the metropolitan area. However, the metro area does not constitute an independent government entity with its own budget. Like all regions in the United States, the Miami metropolitan area consists of a large number of overlapping local governments, each with a distinct set of responsibilities and capacity to raise revenue. The metro area includes three county governments and more than 20 cities, including the City of Miami, Fort Lauderdale, and West Palm Beach. Much like in other states, Florida's school districts function as independent local governments. Statewide these districts align their boundaries with county borders, resulting in three distinct school districts within the metro area.

The large number of independent government entities raises the question of which gov-

[4]Using a different data set, the Conference Board's Help-Wanted Index (HWI), Anastasopoulos et al. (2021) find a short-term decrease in low-skilled vacancies in the city of Miami followed by a full recovery.

ernment entities should be considered "treated." To understand the effect of the Boatlift on public finances, it is necessary to isolate the government entities most directly affected. This paper focuses on the largest county, school district, and municipality in the region: Miami-Dade County, the Miami-Dade School District, and the City of Miami respectively. Thus, this paper assesses the effect of the Boatlift on three different treated units, each a different form of government entity. This helps to further shed light on the heterogeneous effects of the Boatlift and on the overlapping nature of tax bases. In supplemental tables, I also present results for the other two counties in the Miami metropolitan area, Broward and Palm Beach.

Each of the three types of government – county, city/municipality, and school district – has different fiscal responsibilities and constraints. County governments are broader in geography and in scope than cities, providing a range of public services that are more efficiently provided over a wider area of service, such as infrastructure, utilities, and public health. City governments, in contrast, provide public services that are more narrowly tailored to the demand from residents of their jurisdiction. These include public safety, parks, and community development. School districts are responsible solely for public education. All three types of governments in Florida can levy property taxes and have independent authority to set property tax rates, with residents then subject to a combined rate. Only county governments can levy discretionary sales surtaxes on top of the state's sales tax, however city governments receive a portion of sales tax revenue through state and county revenue-sharing. All three types of governments rely on intergovernmental revenues from the state and federal government, with school districts in particular relying on state transfers for a large portion of their budgets and county governments receiving significant funds from federal grant programs related to health and transportation.

Table 1 provides budget profiles for the three treated governments in 1979, the year before the Boatlift occurred. The county government is the largest of the three government entities, collecting $986 million in revenues. The bulk of the county government's revenues come from charges and miscellaneous revenues (36%), with the remainder coming from intergovernment revenues (32%) and property taxes (21%). Owing to its more specialized function, the Miami-Dade school district is roughly half the size of the county government, collecting \$493 million in 1979, the majority coming from state transfers (57%) and property taxes (34%). With a population roughly one fifth the size of the county, the city is significantly smaller than the other two governments, collecting \$183 million in total, a third (30%) from property taxes and a third (30%) from intergovernmental revenues. While the county government provides services across a large number of domains, the largest of which are utilities (16% of expenditures), hospitals (14%), and sewers (11%), the school district by definition is focused solely on providing education services. The city spent a relatively higher percentage of its budget in 1979 on police (15%), fire (14%), and parks and recreation (11%).

3 Methods

3.1 Synthetic Control

The fundamental challenge to assessing the causal impact of the Boatlift, or any policy change that impacts a single region, is identifying a suitable counterfactual for the affected entity. Card's (1990) early study of the Boatlift attempted to overcome this challenge by assembling a comparison group of metro regions with similar characteristics to the Miami metro area, namely Los Angeles, Houston, Atlanta, and Tampa Bay-St.Petersburg. The study was an early example of how to construct a quasi-experimental comparison group. Nevertheless, later researchers critiqued Card's methods, most notably the ad hoc nature of the comparison group and a failure to formally validate the comparison group as a suitable counterfactual (Peri and Yasenov, 2019).

To investigate the fiscal impact of the Mariel shock, this paper employs the synthetic control method (SCM). First developed in a series of papers by Abadie and co-authors (Abadie et al., 2010, 2015; Abadie and Gardeazabal, 2003), the SCM is a data-driven procedure that assesses the effect of a policy change on a single unit of interest, eg. a city or state. The synthetic control approach offers several advantages over the traditional approach to comparative case studies. First, the SCM constructs a linear combination of the available comparison units by minimizing the root mean square error (RMSE) of the predictor variables, thereby offering the best possible fit to the pretreatment period and a more suitable counterfactual than an ad hoc set of comparison units. Second, it is straightfoward to check the validity of the comparison group by comparing the pretreatment differences between the treated unit and its synthetic control. Finally, the SCM offers an intuitive way to conduct quantitative inference; by calculating the synthetic control for each unit in the comparison group, the researcher can observe the distribution of outcomes and calculate p-values using the set of placebos.

Despite being data-driven, the SCM is not without researcher discretion. As Borjas (2017) and Ferman et al. (2020) point out, the researcher must still select the vector of covariates that will serve as the basis for building the synthetic control. In order to reduce specification searching, one standard practice is to report results from matching on the basis of all pretreatment outcomes (Ferman et al., 2020; McClelland and Mucciolo, 2022). However, this approach is not necessarily ideal. As Kaul et al. (2022) point out, "using all outcome lags as separate predictors renders all other covariates irrelevant...irrespective of how important these covariates are for accurately predicting posttreatment values of the outcome." Ideally, researchers must consider the theoretical foundations of the data-generating process and the availability of covariates with predictive power on the outcome of interest. In this particular setting, data availability is a challenge. Not only is there a limited amount of administrative data available from the 1970s, but the task is further complicated by the difficulty of selecting and identifying covariates that are appropriate for three different types of treated local governments. Thus, for its main analyses, this paper matches on the full set of pretreatment outcomes but not other covariates. Nevertheless, in a robustness test, I explore the sensitivity of the result to the inclusion of a limited set of covariates that can be measured across all three governments.

In addition to selecting predictor variables, researchers also exercise discretion over the size of the "donor pool". For each treated unit, the SCM draws on a separate pool of comparison units ("donor pool"). Although it may seem counterintuitive to limit the size of the donor pool, a large donor pool both increases the chance of overfitting and raises the probability that the weighting scheme used for the synthetic control will not be unique (McClelland and Mucciolo, 2022; Abadie et al., 2015; Abadie, 2021). As a result, best practice recommends restricting the donor pool to units with characteristics that are similar to the treated unit (Abadie, 2021). In this case, there are three donor pools. The donor pool for Miami-Dade County consists of all counties in the country, while the pools for the school district and the municipality include all school districts and municipalities respectively. In order to restrict the donor pools such that the comparison groups represent more suitable counterfactuals, I restrict the comparison set of counties to those counties with populations greater than 600k in 1980 (compared with 1,625k in Miami-Dade, then called Dade County), the comparison set of school districts to districts with greater than 50k students (compared with 226K in the Miami-Dade School District)[5], and the comparison set of cities to those with populations between 200 and 500k (compared with 347K in the City of Miami).[6] I also exclude all other governments from the the Miami-Dade metropolitian area as well as those from the southern tip of the state, i.e. municipalities in Monroe, Palm Beach, and Broward counties. These restrictions reduce the size of the comparison groups to 47, 48, and 43 units respectively, small enough to create groups with characteristics similar to the affected units but not so small that they "reduce the granularity of possible p-values" (McClelland

[5]I also exclude community college districts, some of which have notably lower spending per pupil.

[6]Insofar as the Boatlift affected population and enrollment measures in 1980, it is possible that the 1980 population count in Miami contains post-treatment information, and thus should not be used to place restrictions on the sample. However, relative to the breadth of the restrictions, these effects are likely to be extremely small and should have little bearing on the analysis.

and Mucciolo, 2022). These groups are also similar in size to the donor pool used in Peri and Yasenov (2019). In section 5.4 I explore the robustness of the results to alternative restrictions on the donor pool.

One recent advancement in the synthetic control literature is the use of bias-correction procedures to address discrepancies between the predictor variable values for each treated unit and its synthetic control (Abadie and L'hour, 2021; Ben-Michael et al., 2021). If the pretreatment fit between the predictor values of a treated unit and its synthetic control donors is poor, then such procedures can improve on the classical SCM by reducing the estimated bias from predictor variable discrepancies. I produce bias-corrected estimates using the allsynth command in STATA (Wiltshire, 2022).

3.2 Inference

To produce quantitative inference, the SCM conducts placebo tests in space by computing the treatment effect for every potential comparison unit in the donor pool over the same treatment period. P-values are based on the size of the treatment effect estimate relative to the distribution of placebo effects. One disadvantage of this approach is that some placebo effects may be quite large if certain units from the donor pool cannot be matched well in the pretreatment period. To adjust for this, researchers typically compute "standardized" p-values by dividing all effects by the corresponding pretreatment match quality (as measured by the pretreatment RMSE). Due to the nature of the Boatlift, because it is implausible that the size of government could shrink in response to a large inflow of population, I report one-sided rather than two-sided p-values. Thus, for effects averaged over the post-treatment time period, I calculate the ratio of the posttreatment RMSE to the pretreatment RSME, and I restrict the placebo-year estimates that contribute to the posttreatment RMSE to those with positive values, as suggested by Abadie (2021). Then, I calculate p-values as $\frac{R}{J+1}$where J is the number of placebo units and R is the ranking of the treated unit's ratio of posttreatment RMSE to pretreatment RMSE. For annual effect sizes, R is based on the ratio of the estimated treatment effect (rather than the absolute value of the effect size) to the pretreatment RMSE relative to the distribution of that same metric for the placebo units.[7]

3.3 Timing

The Boatlift took place between April and October of 1980. Because both Miami-Dade County and the City of Miami have fiscal years that end on September 30, and thus the fiscal year 1980 would have encompassed almost the entirety of the refugee wave, the analysis treats fiscal year 1981 as the first "post-treatment" year. The matching process treats all years prior to fiscal year 1980 as "pre-treatment" and does not use fiscal year 1980 information in either matching or the treatment effect estimation. The Miami-Dade School District on the other hand has a fiscal year that ends June 30. Since the bulk of the refugees had arrived by June 30 (Larzelere, 1988), the analysis similarly treats fiscal year 1981 as the first post-treatment year for the school system as well. Thus, 1970-1979 constitutes the pre-treatment period for all three governments, and results cover the period 1981-1990.[8]

4 Data

The primary data source for the financial outcomes in this paper is the Census of Governments. For population data, I also draw on historical county-level population data compiled by the National Cancer Institute's Surveillance, Epidemiology, and End Results (SEER) Program. Every five years the Census collects a full survey of state and local governments in the United States, collecting information about the range of government financial activities, including detailed revenue and expenditure categories. Census workers clean the responses and compare them to audited financial statements. In non-census years,

[7]Using all outcome lags as predictors with bias-corrected estimation results in a pretreatment RMSE of zero, which I adjust to one for the standardization of effect sizes. Consequently, for most of the estimates in this paper, the pretreatment RMSE does not impact the calculation of p-values.

[8]Because the Census of Governments reports fiscal data according to "survey year" rather than the fiscal year of the reporting governments, I re-structure the data such that the temporal variation is by fiscal year rather than survey year.

the surveys are stratified by government type, with the probability of selection proportional to size. Although the lack of full coverage can pose challenges for research designs that require broader coverage of smaller governments, due to Miami's relatively large population and the restrictions on the size of the donor pool, in this case the coverage of the survey does not pose a problem as larger governments are surveyed every year.

For key outcomes, this paper focuses on (log) total revenues and (log) total expenditures so that the estimates are easier to interpret and compare across outcomes. Although it might be preferable to focus on per-capita outcomes, the lack of annual, nationwide school enrollment and municipal population data going back to 1970 make this impossible. Nevertheless, I discuss how to interpret the main findings in per capita terms below.

5 Results

5.1 Validation of Population Effects

Before discussing the fiscal results, first I provide evidence of the population shock that resulted from the Boatlift. Figure 1 shows synthetic control estimates for the effect of the Boatlift on the population of Miami-Dade County. Figure 1a shows the treatment effect estimates over time for the total population, while Figures 1b and 1c show the treatment effect estimates for the working age (ages 20-64) and school-age populations (ages 5-19).[9] In all three figures the treatment effect estimates for the pretreatment period (1970-1979) are precisely zero since I use all pretreatment outcomes as predictors and the figure shows bias-corrected gaps. Figure 1a shows that while the total population increased in the immediate aftermath of the Boatlift, this effect remained relatively constant through most of the 1980s. However, the effects on the working-age and school-age populations are more pronounced and

[9] As in the main analysis, I restrict the donor pool for the analysis of total population to counties with less than 600k in 1980. For the analyses of the working-age and school-age population, I restrict the donor pools to counties with working age populations of less than 400k and counties with school age populations of less than 150k. These result in donor pools of 58, 43, and 50 respectively.

continued to grow over time. They are also clearly larger than any of the placebo estimates (shown in grey). The working age population increased by approximately 50k (5%) in 1981. This is in line with the work by Peri and Yasenov (2019) documenting an increase in the metropolitan-area labor force. The school-age population increased by approximately 25k (7%) in 1981 from a pretreatment baseline of 350k, an effect that increased to 125k (36%) in 1990. In section 6, I explore the population effects on the other two counties in the Miami metropolitan area.

5.2 Pre-Treatment Balance

Having provided evidence of the population shock, in this section I discuss diagnostics for the main analysis. There are three basic diagnostic checks for assessing pretreatment balance in the SCM. The most straightforward is to visually inspect the overlap between the treated unit and its synthetic control over the pre-treatment period. The second is to calculate the root-mean-square-error (RMSE) in the pretreatment period, which provides an absolute measure of the fit. Finally, one can also inspect the distribution of root-mean-square-errors (RMSEs) among the placebos and compare the proportion of control units that have values at least as high as the treated unit (Cavallo et al., 2013; Galiani and Quistorff, 2017). As noted above, I use a bias-correction procedure that adjusts for differences in the pretreatment fit of the predictor variables, and in the baseline specification I use all outcome lags as predictors. This obviates the need to examine the pre-treatment balance in the outcome variables as the *bias-corrected* RMSE is zero by construction. Nevertheless, in order to provide a measure of pretreatment match quality, I present measures of the pretreatment balance that results from the classic synthetic control estimation strategy (without bias correction) since this highlights the extent of bias-correction that is necessary. Thus, in the tables that follow, I present bias-corrected estimates and their associated p-values alongside the preatment RMSE and the "RMSE percentile" from the classic synthetic control estimator. The "RMSE percentile" represents the proportion of placebos that have a pretreatment

RMSE at least as large as the treated unit; the higher the measure (the closer to 1), the better the relative fit of the treated unit.

For the City of Miami's (classic) synthetic control estimates, 85 and 90 percent of the placebos have a pretreatment RMSE that is as least as large as the treated unit (Table 2). For the School District, these numbers are also relatively high: 75 and 84 percent. On the other hand, for Miami-Dade County, only 6 and 31 percent of placebos have a pretreatment RMSE as large. Taken collectively, these results lend confidence to the estimates for the City and School District, while suggesting caution for the County results.

One other possible validation check is to consider the units in the donor pool that receive positive weight in the synthetic control. This helps to ensure that the comparison pool is qualitatively similar to the treated unit. In this analysis, there are three treated units and multiple outcomes, leading to a large number of weighting schemes. However, Table A1 shows the school districts receiving positive weight in the synthetic control constructed on the basis of total school district expenditures. As expected, the schools districts receiving positive weight are predominantly in other large urban metro areas.

5.3 Main Results

Figure 2 shows the evolution of the treatment effect over time for all three treated units. Figures 2a and Figure 2b indicate that, while the City of Miami grew slightly during specific short intervals following the Boatlift, including in total expenditures during the two years immediately following the Boatlift (1981-1982), there is no persistent effect. Simiarly, Figures 2e and 2f shows that the county government did not experience any growth in revenues or spending. On the other hand, the synthetic control plots for the School District (Figures 2c and 2d) demonstrate that the School District did experience sustained growth in revenues and expenditures, consistent with the enduring growth in the school-age population evident in Figure 1c. Table 2 provides the corresponding synthetic control coefficients. The estimated average effect of the Boatlift on the City's finances over the ten year post-treatment period is a 10 percent increase in total revenues and a 5 percent increase in total expenditures. Neither effect is statistically significant (p-values of 0.31 and 0.42); nor are the effects for the county. For the School District, the estimated average effect on revenues is a 7 percent increase, while the estimated average effect on expenditures is a 25 percent increase. The estimate for expenditures has a p-value of 0.045, indicating that the effect size (as measured by the ratio of the post-treatment RMSE to the pre-treatment RMSE) is larger than all but one of the 43 placebos.

Effects by Year Table 3 presents the treatment effect estimates by year. The estimated effect on the City's total expenditures spikes shortly after the Boatlift - increasing by 18 percent in 1981 and 19 percent in 1982. However, none of the annual estimates are statistically significant. On the other hand, the estimated effect of the Boatlift on the School District appears to have increased steadily over time – from an increase in spending of 5 percent in 1981 to a 40 percent increase in 1990 – consistent with the persistent increase in the school-age population, as seen in Figure 1c. Four of the spending estimates for the School District have p-values of less than 0.05, and six have p-values less than 0.10. None of the county estimates are significant.

Effects on Individual Line-Items What caused this rise in spending - an apparent temporary rise in 1981-1982 for the City, and a persistent rise for the School District? Figure 3 shows synthetic control plots for several of the larger revenue and expenditure line items for the City of Miami. Two plots stand out: police expenditures and parks & recreation. While there appears to be an increase in police spending that grows over time, this cannot explain the overall rise in spending for the City, which peaked only briefly in the immediate aftermath of the Boatlift, and the average treatment effect is not significant (p-value 0.19). However, the sharp rise in parks and recreation spending in 1981-1982 almost perfectly mirrors the uptick in total expenditures for that same period, and the annual estimates are statistically significant (p-values of 0.042 and 0.021) for 1981-1982. Equivalant to approximately 50 million dollars between 1981-1982, or roughly three quarters of the increase in total expenditures, the increase primarily occured via an increase in capital outlays for the parks department, reflecting the City's efforts to set up temporary shelters and processing centers, including one at the city-owned Orange Bowl Stadium (Chardy, 2010).[10]

Since all spending in the School District is categorized as education and thus cannot be broken down in the same way, Figure 4 shows synthetic control plots for the District's larger revenue categories. Like other states, Florida requires its local governments to balance their budgets, and consequently any persistent increase in service provision (expenditures) that cannot be financed through reserves must be matched by an increase in revenues, often accomplished by adjusting property tax rates. Figure 4a shows no effect on property tax revenues, but Figure 5b shows a dramatic effect on intergovernmental revenues from the state that grows over time.[11] The average estimated effect is a 41 percent increase, larger than any of the placebos (equivalent to a p-value of 0.023), thereby providing a plausible channel through which the School District's persistent increase in spending was financed.[12]

Effect Sizes How large is the increase in education spending? In the absence of annual school enrollment data for the donor pool, one way to place the effect size into context is to compare the total increase in spending to the corresponding increase in the school-age

[10]Because the Census of Government data is based on a uniform set of expenditure categories that do not necessarily align with the breakdown of a city government's different agencies and budget responsibilities, it's not possible to definitively tie the increase in capital outlays to the cost of shelters and processing centers for refugees. However, contemporaneous accounts suggest that city funds were deployed for this purpose, and as a recreation facility, the Stadium would have fallen under the budget of parks and recreation (Chardy, 2010). Moreover, between 1970-1979, the city did not appropriate any funding for housing and community developing, suggesting that the responsibility to set up processing centers may have fallen on a city department with more capacity and resources.

[11]The increase in state transfers suggests that the Boatlift may have had an effect on spending at the state level. In Table A2, I use state-level data to estimate the impact of the Boatlift on revenues and expenditures for the state of Florida. While all of the estimates are positive, including an estimated 12 percent increase in intergovernmental expenditures, none are statistically significant.

[12]The federal government also devoted significant resources to the Boatlift; one estimate from the state department in 1981 suggests that the Boatlift cost the federal government \$700 million (Larzelere, 1988, p. 380). However, much of this money consisted of appropriations to FEMA and consequently little flowed directly through local government budgets. The share of revenues coming from federal funds actually declined for all three of the treated governments between 1979 and 1981, from 14 percent to 12 percent for the City of Miami, from 4 percent to 2 percent for the Miami-Dade School District and from 24 percent to 23 percent for the county. By 1990, these shares had only fallen further to 4 percent, 0.2 percent, and 8 percent respectively.

population in Miami. The estimates corresponding to Figure 1 indicate that the school-age population increased by an average of 16% over the period 1981-1990 relative to the synthetic control. The results in Tables 2-3 show that the Boatlift increased spending in the Miami-Dade school district by 25 percent on average, increasing from a low of five percent in 1981.[13] Assuming an average enrollment increase of 16 percent and an average spending increase of 25 percent, these results would imply that the per pupil costs of educating the Marielitos was 50 percent higher than the average per pupil cost prior to their arrival, or alternatively that the overall per-pupil cost increased by approximately 8 percent. While high, these estimates are not outside the range of those found in previous studies that examine the cost of education to English language learners; according to Jimenez-Castellanos and Topper (2012), the most common approach to costing out education has produced weight recommendations for English language learners that range from 1.39 times base cost to 3.0 times base cost. It is also consistent with higher per pupil spending allocated to students of limited English proficiency (LEP) and higher learning needs under Florida's school funding formulas (Florida Department of Education, 2021). Many of the children arriving as refugees had more limited English than their peers in the school system and potentially higher learning needs, needs that are reflected in the disproportionate spending increase.

5.4 Robustness Tests and Alternative Specifications

Alternative Specifications Although the baseline specifications show strong pretreatment balance both with and without bias correction, it is possible that different choices regarding matching procedures and the donor pool may improve on the estimation of the counterfactual. Thus, in a series of robustness checks, I vary the estimation of treatment effects and quantitative inference in five ways. First, I add additional covariates that might explain the trajectory of government revenues and expenditures. Insofar as the main estimates do

[13] This estimate includes capital spending; some of the additional expenditures during this period appear to have gone toward the construction and rehabilitation of school buildings. When looking only at current operating spending, the effect shrinks to 20 percent (Table A3).

not fully capture the structural determinants of spending, including additional demographic variables may potentially provide a more accurate prediction of the posttreatment outcomes (Abadie, 2021). Thus, I add a covariate for the school-age population at the county-level (for the city and county) and enrollment (for the school district) to a more limited set of lagged outcomes. Specifically, I add the average value of school-age-population/enrollment across the pretreatment period 1970-1979, and I include lagged outcomes only for the period 1970-1975.[14] Next, to address any concerns about the log transformation of the outcome variables, I demean the lagged outcomes over the pre-treatment period. Next, I vary the restrictions on the size of the donor pool. I restrict the size of the city donor pool first to cities with 1980 population larger than 200k and then to cities with population greater than 250k and less than 450k. I restrict the size of the school district donor pool first to districts with 1980 enrollment greater than 40k and then to enrollments with greater than 75k, and I restrict the county donor pool first to counties with 1980 population greater than 500k and then to counties with population larger than 800k. Finally, I employ an alternative estimator. Specifically, I employ the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021), as implemented in Stata by Clarke et al. (2023).

The results for the five robustness checks are presented in Table 4. Overall, the robustness checks support the main findings. In particular, all six of the estimates for school district expenditures are statistically significant at the 5 percent level. One other finding of note is that the estimates for school district revenues show substantial variation, with several estimates similar in magnitude to the expenditure estimates.

Placebo Test Finally, in addition to varying the specification choices, I also estimate a placebo in time. Rather than using 1970-1979 as the pretreatment period and 1981-1990 as posttreatment, I instead use 1970-1974 as the pretreatment period and explore the effect of a placebo shock in 1975. Given that no such shock occurred, a non-zero treatment effect

[14] As referenced above, the Census of Governments does not report school enrollment on an annual basis for this period. Thus, for school districts the covariates takes the average value of the observations in the data.

estimate would cast doubt on the use of the synthetic control in this context. Using the same sample restrictions as in the original analysis except with 1970 data, I estimate the placebo test for total expenditures in both the City and School District and calculate the average effect over the 1975-1979 period. The results are in Table A4. The effect on total expenditures in the City is 0.037 with a p-value of 0.57; the corresponding effect for schools is 0.023 with a p-value of 0.36. Not only are the average effect estimates not statistically significant, but neither are any of the estimates for individual years, further validating the use of the SCM in this setting.

6 Effects on Other Counties

Figure 5 shows the estimated effects over time of the Boatlift on the population of the two other counties in the Miami metro area: Broward and Palm Beach. Together with Miami-Dade, these counties comprise the three most populated counties in the state. There is no evidence of an effect on the population of Broward County; in fact, other than a brief positive effect in the early 1980s on the working age population, the majority of the estimated effects are negative. There is, however, strong evidence of an effect on Palm Beach County. There are large positive effects on all three population groups, with the effect on the total population increasing gradually throughout the 1980s to approximately 150k (a 27% increase over the pretreatment baseline), while the effect on the working-age population increases to 88k (31%), and the effect on the school-age population increases to 23k (22%). Thus, while Miami-Dade saw a more immediate impact on the size of its working-age population, Palm Beach county experienced higher growth as a percentage of its total population by the end of the decade. As a result, I explore the effects on these two other counties to validate the main findings.

To do so, I estimate the effect of the Boatlift on the largest city, the school district, and the county government of the two counties. Table 5 shows the results. Figures A1 and A2 show the corresponding synthetic control plots. None of the results for Broward are significant, consistent with the lack of any population effects. However, as in Miami-Dade, the fiscal effects on the Palm Beach school district are large and precise. Revenue in the school district increased by 49 percent, while expenditures increased by 39 percent. Both estimates have a p-value of 0.023. Assuming that the district population increased on average by 13 percent (as shown in Figure 5), then this translates to a near tripling of the marginal per-pupil cost of educating the new arrivals, even larger than the estimate for Miami. Thus, these results provide further evidence that the Boatlift's effect on public finances in the region operated primarily through increases in per-pupil educational expenditures.

7 Conclusion

This paper investigates the impact of an immigration shock on the finances of the local governments in the affected region. Using synthetic control methods, it shows that revenues and expenditures in both the City of Miami and the Miami-Dade School District increased following the wave of Cuban refugees that arrived in south Florida in 1980, commonly known as the Mariel Boatlift. Educational expenditures increased steadily over a ten year period for an estimated average increase in per-pupil expenditures of eight percent. City spending increased by an estimated 19% for a brief two year period following the Boatlift, driven by a precisely estimated increase in parks and recreation spending. There was no effect on the finances of Miami-Dade County. In order to finance the increase in spending, the school district relied on a steady increase in state transfers.

The results build on a recent body of literature investigating the fiscal and economic effects of immigration. Unlike prior work that relies on census records and cash flow accounting, this paper draws on a natural experiment and government financial records and thus avoids the pitfalls associated with allocating the cost of public goods across taxpayers. Despite the difference in methods however, this work reaches conclusions that are broadly similar to that earlier work. Immigration increases spending at the local level, primarily as a result of higher educational expenditures.

In addition to confirming some of the findings of this earlier literature, the analysis also fleshes out our understanding of heterogeneity, specifically the distributional effects over time and space. While the results for the City of Miami indicate a sharp rise in spending that quickly dissipated, the spending effects on the School District were persistent and actually increased over time, consistent with the effect that the Boatlift had on the population demographics of the area. Moreover, while education in the Unites States is financed by multiple levels of government - with local entities receiving both federal and state funds – the results here show that state transfers were the primary means by which the Miami-Dade School district was able to expand services.

These findings highlight the need for a greater federal role in smoothing out the fiscal impact of immigration flows, which may place an undue fiscal burden on the states and local communities that host recent arrivals. Balanced budget requirements require that local governments immediately raise revenue in order to finance additional services. On the other hand, prior works suggests that investments in education disproportionately benefit federal coffers in the long-run because of the federal government's reliance on a progressive income tax (National Academies of Sciences, Engineering, and Medicine, 2017; Rueben and Gault, 2017). This imbalance suggests that increased federal transfers in the wake of immigration flows would provide a more equitable way of financing the necessary increase in short-term spending.

How generalizable are these findings? The fiscal effects of immigration are of course highly dependent on the demographic make-up of the foreign-born (Mayda et al., 2023). As noted above, a large majority of Mariel Cubans were unskilled and without a high school diploma. As Peri and Yasenov (2019) document, the Boatlift produced an 18 percent increase in the number of high school dropouts, compared with an overall increase in the labor force of approximately 8 percent. The Marielitos possessed similar levels of education as other migrants from Mexico and Central America, who are less likely to be high school graduates than the U.S. born and who have historically represented the largest share of immigrants (Pew Research Center, 2020). Another crucial demographic characteristic, which this paper highlights, is the age of arrival. Because the fiscal effects are driven largely by educational expenditures, the age profile of immigrants is crucial to understanding the fiscal effects.

Other important sources of variation may be access to health care and housing availability. A small number of states have expanded Medicaid and CHIP coverage for low-income residents regardless of immigration status. As of December 2022, eight states provide comprehensive state-funded coverage to all income-eligible children regardless of immigration status, while a few states, including California and New York, have also expanded coverage to adults (Kaiser Family Foundation, 2022). While Medicaid is primarily funded at the state and federal level, states have some flexibility to pass on certain costs to local governments, and in New York, local governments add roughly a quarter to the total amount of state spending (Empire Center, 2022). The cost of providing shelter may also be an important margin on which localities differ; because New York provides a legal "right to shelter," New York City's recent response to an influx of asylum-seekers has caused the city comptroller to raise the alarm over the cost of shelter provision (New York City Comptroller, 2023). Thus, while education spending appears to be the key factor in the distribution of the fiscal effects of immigration, this may shift as more state and local governments expand the eligibility for health care services and face a shortage of affordable housing.

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Figure 1: Miami-Dade County Population

Figure 1a: Total Population

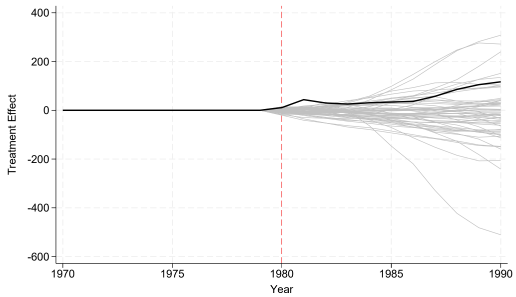


Figure 1b: Working-Age Population (20-64)

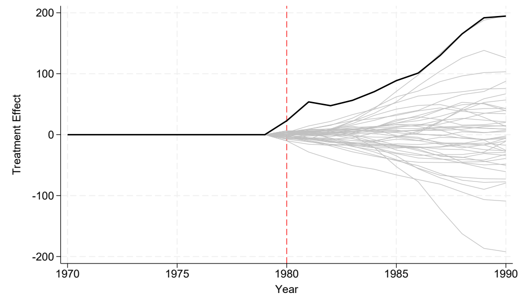
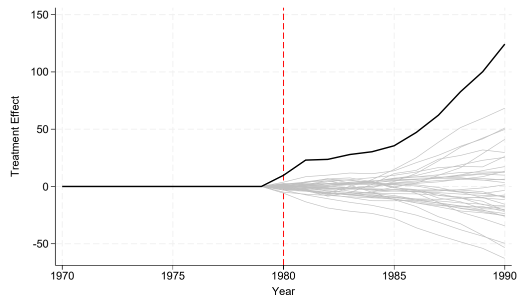


Figure 1c: School-Age Population (5-19)



Note: The figures plot the estimated treatment effect estimates for the effect of the Boatlift on the population of Miami-Dade County. All population figures are in thousands. Figure 1a plots the treatment effect estimates for the total population. Figure 1b plots the treatment effect estimates for the working-age population (aged 20-64). Figure 1c plots the treatment effect estimates for the school-age population (5-19). Data on historical county-level population estimates come from SEER. The solid lines plot the bias-corrected treatment effect estimates, while the grey lines reflect the bias-corrected placebo estimates. The vertical dash line indicate 1980, the year of the treatment.

Figure 2: Main Results

City of Miami

Figure 2a: Log Total Revenues

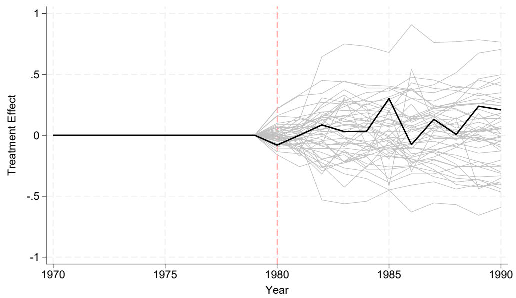
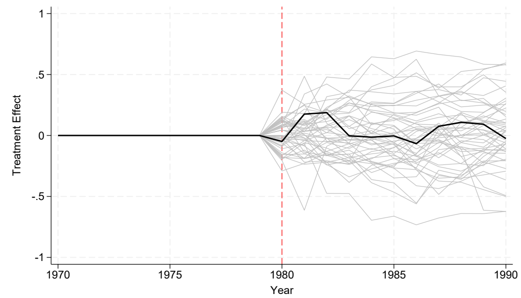


Figure 2b: Log Total Expenditures



Miami-Dade County School District

Figure 2c: Log Total Revenues

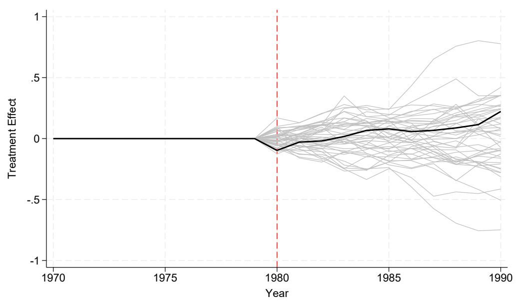
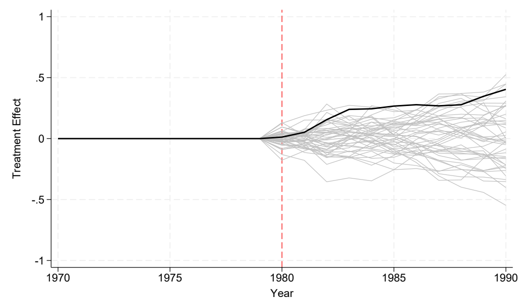


Figure 2d: Log Total Expenditures



Miami-Dade County

Figure 2e: Log Total Revenues

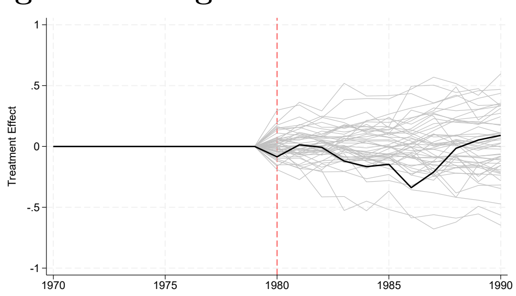
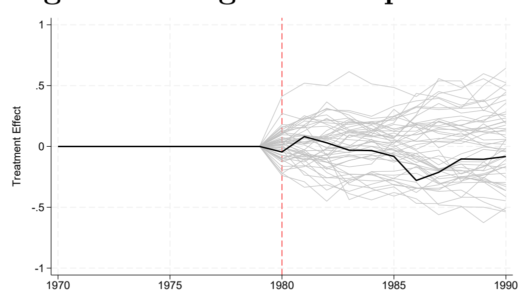


Figure 2f: Log Total Expenditure



Note: The figures plot the estimated treatment effect estimates for the City of Miami, the Miami-Dade County School District, and Miami-Dade County from 1970 to 1990. In each case, there are separate figures for log total revenues and log total expenditures. The solid lines plot the bias-corrected treatment effect estimates, while the grey lines reflect the bias-corrected placebo estimates. The vertical dash lines indicate 1980, the year of the treatment.

Figure 3: Results for Selected Line Items in City of Miami

Figure 3a: Log Property Tax

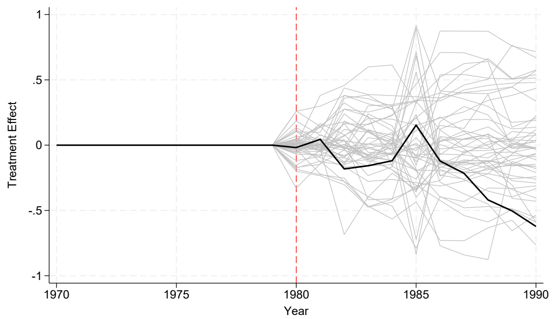


Figure 3b: Log Intergov Revenue

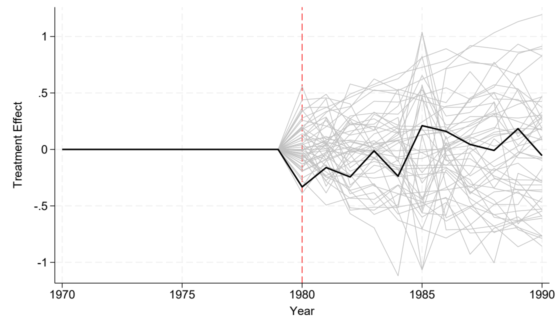


Figure 3c: Log Police Expenditures

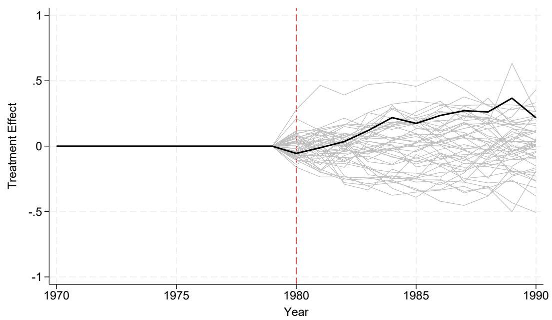


Figure 3d: Log Fire Protection Expend

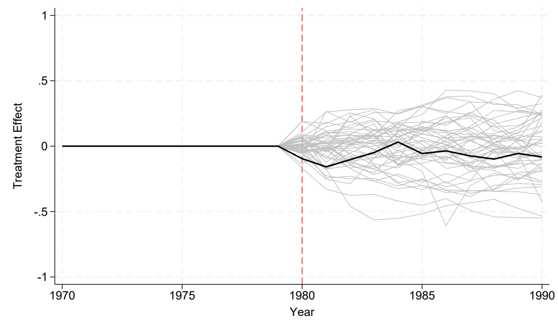
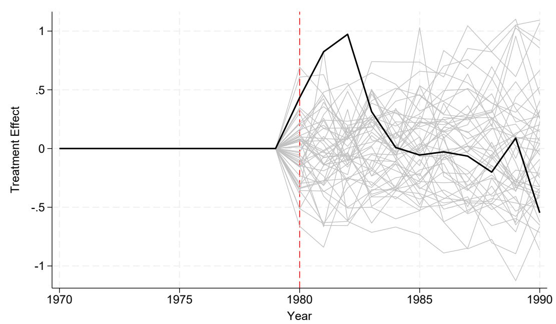


Figure 3e: Log Parks & Rec Expenditures



Note: The figures plot the estimated treatment effect estimates for the City of Miami from 1970 to 1990. The solid lines plot the bias-corrected treatment effect estimates for each outcome, while the grey lines reflect the bias-corrected placebo estimates. The vertical dash lines indicate 1980, the year of the treatment. Figures 3a and 3b omit several placebo-year observations in 1985 that are outside the range of the graph.

Figure 4: Results for Selected Line Items in the Miami-Dade County School District

Figure 4a: Log Property Taxes

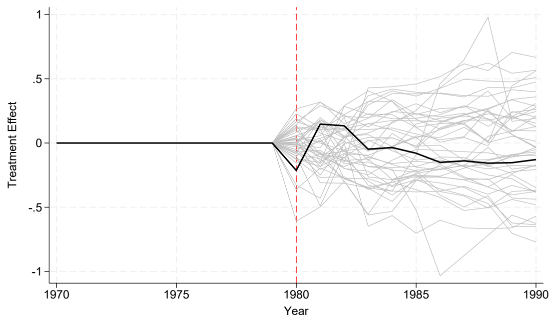
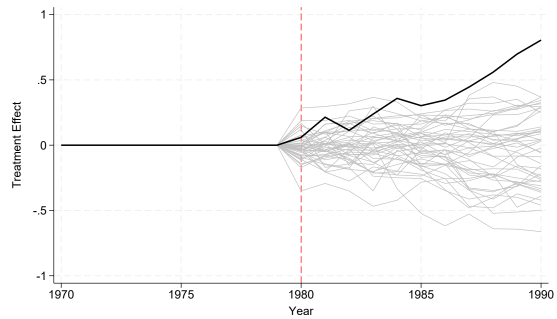
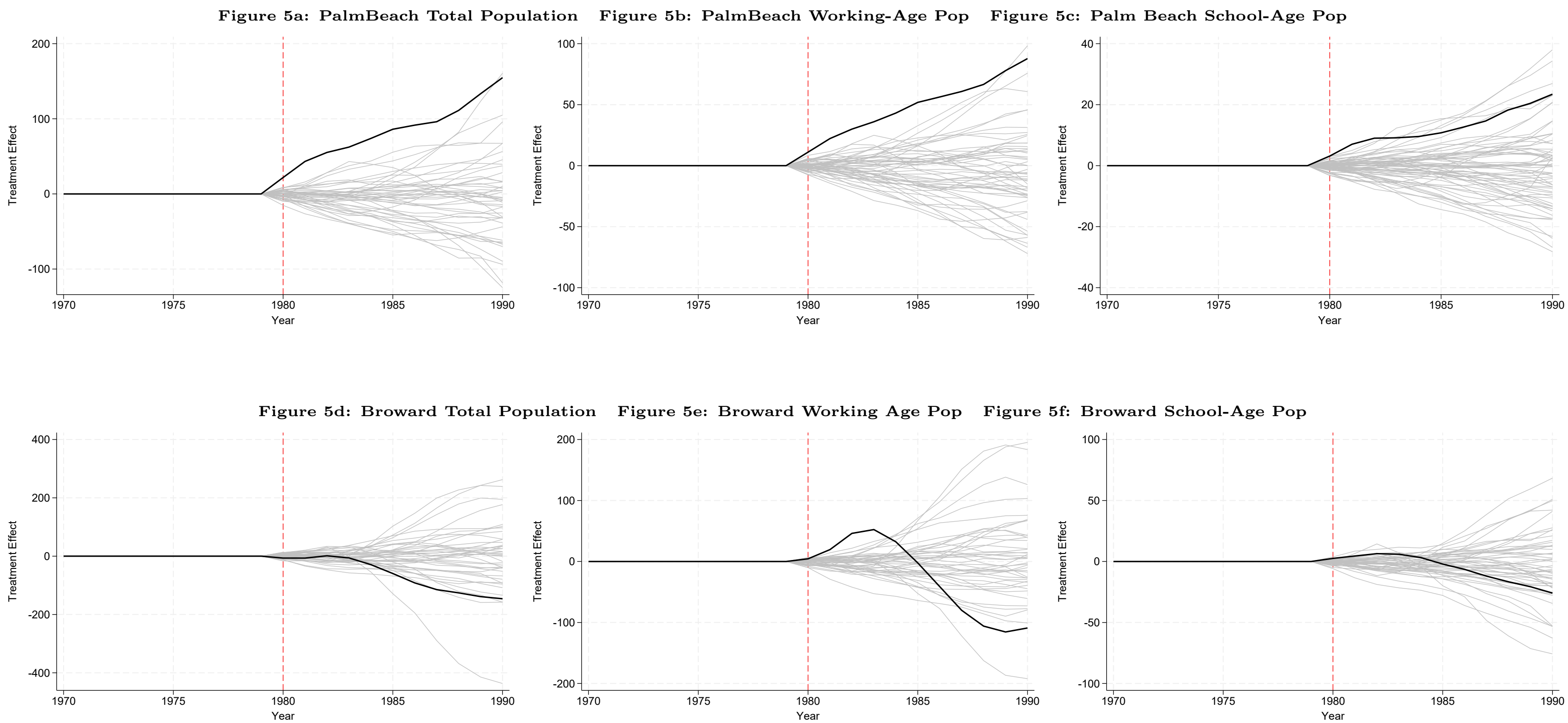


Figure 4b: Log State Intergov Revenues



Note: The figures plot the estimated treatment effect estimates for the Miami-Dade School District from 1970 to 1990. The solid lines plot the bias-corrected treatment effect estimates for each outcome, while the grey lines reflect the bias-corrected placebo estimates. The vertical dash lines indicate 1980, the year of the treatment. Figure 4a omits two placebo-year observations that are outside the range of the graph.

Figure 5: Population of Other Counties in Miami Metropolitan Area



Note: The figures plot the estimated treatment effect estimates for the effect of the Boatlift on the populations of Palm Beach County and Broward County. All population figures are in thousands. Figures 5a-c plot the estimates for the total population, working-age population (20-64), and school-age population (5-19) of Palm Beach County. Figures 5d-f plot the estimates for the total population, working-age population (20-64), and school-age population (5-19) of Broward County. Data on historical county-level population estimates come from SEER. The solid lines plot the bias-corrected treatment effect estimates, while the grey lines reflect the bias-corrected placebo estimates. The vertical dash line indicate 1980, the year of the treatment. The donor pool of Palm Beach County was restricted to counties with total population between 500-800k, working-age population between 250-400k, and school-age population between 90-150k. The donor pool of Broward County was restricted to counties with total population greater than 700k, working-age population greater than 400k, and school-age population greater than 150.

Table 1: 1979 Budget Profiles

		City of Miami		Miami-Dade School District		Miami-Dade County	
		1979 value	% of Total	1979 value	% of Total	1979 value	% of Total
Revenues	Total Revenue	183	100%	493	100%	986	100%
	Property Tax	56	30%	165	34%	210	21%
	Sales Tax	24	13%	0	0%	44	4%
	Charges & Misc Rev	26	14%	27	6%	359	36%
	Intergov Rev	55	30%	301	61%	310	32%
	IG Revenue - State	27	15%	282	57%	73	7%
	IG Revenue - Fed	25	14%	18	4%	238	24%
Expenditures	Total Expenditure	155	100%	490	100%	995	100%
	Education	0	0%	484	99%	0	0%
	Public Welfare	0	0%	0	0%	16	2%
	Parks & Rec	17	11%	0	0%	46	5%
	Housing & Comm Dev.	9	6%	0	0%	78	8%
	Police	23	15%	0	0%	68	7%
	Fire	21	14%	0	0%	24	2%
	Sewerage	9	6%	0	0%	106	11%
	Hospitals	0	0%	0	0%	138	14%
	Utilities	0	0%	0	0%	157	16%

Note: All variables in millions. Source: Census of Governments.

Table 2: Main Results - Synthetic Control Estimates

		Log Total Revenue	Log Total Expenditure
Miami City	Estimate	0.10	0.05
	P-value	0.31	0.42
	# of placebos	47	47
	Preperiod RMSE	0.021	0.021
	RMSE percentile	0.85	0.90
Miami-Dade Public Schools	Estimate	0.07	0.25
	P-value	0.45	0.045
	# of placebos	43	43
	Preperiod RMSE	0.029	0.022
	RMSE percentile	0.75	0.84
Miami-Dade County	Estimate	-0.08	-0.08
	P-value	0.61	0.53
	# of placebos	48	48
	Preperiod RMSE	0.11	0.07
	RMSE percentile	0.06	0.31

Note: The table presents bias-corrected synthetic control estimates of the effect of the Mariel Boatlift on financial outcomes for three governments: the City of Miami, the Miami-Dade County School District, and Miami-Date County. The treatment effect is averaged over the years 1981 to 1990. The p-values are based on the permutation test described in Section 3.2. The root-mean square error (RMSE) and percentile are calculated using ten years of pretreatment data and are based on the synthetic control without bias correction.

Table 3: Results By Year

		Log Total Revenue		Log Total Expenditure	
		Estimate	P-value	Estimate	P-value
Miami City	1981	0.00	0.50	0.18	0.19
	1982	0.08	0.40	0.19	0.21
	1983	0.03	0.52	-0.00	0.46
	1984	0.03	0.54	-0.01	0.50
	1985	0.30	0.15	-0.00	0.50
	1986	-0.08	0.63	-0.07	0.58
	1987	0.13	0.38	0.08	0.38
	1988	0.01	0.52	0.11	0.33
	1989	0.24	0.27	0.09	0.42
	1990	0.21	0.27	-0.03	0.56
Miami-Dade Public Schools	1981	-0.03	0.68	0.05	0.20
	1982	-0.02	0.57	0.16	0.14
	1983	0.02	0.50	0.24	0.045
	1984	0.07	0.43	0.24	0.068
	1985	0.08	0.39	0.27	0.023
	1986	0.06	0.43	0.28	0.023
	1987	0.07	0.41	0.27	0.11
	1988	0.09	0.41	0.28	0.16
	1989	0.11	0.39	0.35	0.045
	1990	0.22	0.30	0.40	0.091
Miami-Dade County	1981	0.01	0.47	0.08	0.35
	1982	-0.01	0.57	0.03	0.47
	1983	-0.12	0.86	-0.03	0.61
	1984	-0.17	0.90	-0.03	0.61
	1985	-0.15	0.86	-0.08	0.71
	1986	-0.34	0.94	-0.28	0.84
	1987	-0.21	0.84	-0.21	0.76
	1988	-0.02	0.57	-0.10	0.55
	1989	0.05	0.37	-0.11	0.57
	1990	0.09	0.35	-0.08	0.65

Note: The table presents bias-corrected synthetic control estimates of the effect of the Mariel Boatlift on financial outcomes for the City of Miami, the Miami-Dade County School District, and Miami-Dade County by year. The p-values are based on the permutation test described in Section 3.2.

Table 4: Robustness Checks

		Log Total Revenue						Log Total Expenditure					
		Baseline	Additional Covariates	Demeaned	Larger Donor Pool	Smaller Donor Pool	Synthetic DiD	Baseline	Additional Covariates	Demeaned	Larger Donor Pool	Smaller Donor Pool	Synthetic DiD
City	Estimate	0.10	0.021	0.11	0.09	-0.08	0.10	0.05	-0.01	0.05	0.07	0.12	0.12
	P-value	0.31	0.50	0.33	0.28	0.52	0.24	0.42	0.56	0.50	0.35	0.48	0.27
	# placebos	47	47	47	70	28		47	47	47	70	28	
	Preperiod RMSE	0.021	0.051	0.037	0.021	0.026		0.021	0.084	0.049	0.018	0.050	
	RMSE percentile	0.85	0.69	0.65	0.75	0.90		0.90	0.65	0.60	0.92	0.59	
Schools	Estimate	0.07	0.30	0.23	0.15	-0.028	0.26	0.25	0.21	0.21	0.28	0.25	0.22
	P-value	0.45	0.14	0.14	0.14	0.62	0.038	0.045	0.045	0.023	0.030	0.048	0.007
	# placebos	43	43	43	65	20		43	43	43	65	20	
	Preperiod RMSE	0.029	0.041	0.030	0.029	0.035		0.022	0.057	0.010	0.022	0.029	
	RMSE percentile	0.75	0.86	0.66	0.65	0.81		0.84	0.59	0.95	0.73	0.86	
County	Estimate	-0.08	0.30	-0.01	-0.10	-0.14	-0.05	-0.08	0.66	0.10	-0.05	0.03	-0.03
	P-value	0.61	0.43	0.51	0.68	0.43	0.36	0.53	0.10	0.24	0.53	0.43	0.40
	# placebos	48	48	48	65	29		48	48	48	65	29	
	Preperiod RMSE	0.11	0.13	0.06	0.11	0.11		0.07	0.14	0.03	0.07	0.07	
	RMSE percentile	0.06	0.16	0.20	0.05	0.17		0.31	0.24	0.84	0.27	0.30	

Note: The table presents the baseline synthetic control estimates for total revenues and total expenditures alongside the results of five different robustness checks. The first, "Additional Covariates" adds covariates for school-age population/enrollment to a set of lagged outcomes for the earlier years of the sample period (1970-1975). The second, "Demeaned," demeans the outcome variable in the pre-treatment period. The third and fourth, "Larger Donor Pool" and "Smaller Donor Pool" place different sets of restrictions on the donor pool, resulting in more and less placebos respectively. The fifth "Synthetic DiD" uses the synthetic difference-in-differences estimator of Arkhangelsky et al. (2021), as implemented in Stata by Clarke et al. (2023).

Table 5: Results for Palm Beach County and Broward County

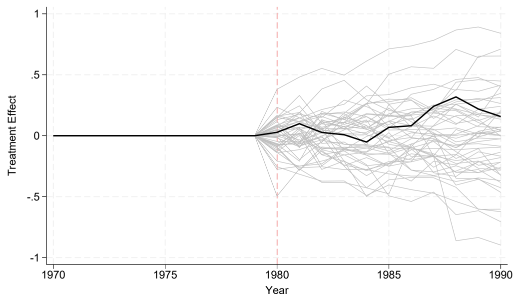
Palm Beach				Broward			
		Log Total Revenue	Log Total Expenditure			Log Total Revenue	Log Total Expenditure
City of West Palm Beach	Estimate	0.12	0.18	Fort Lauderdale	Estimate	0.12	0.06
	P-value	0.29	0.29		P-value	0.35	0.33
	# of placebos	48	48		# of placebos	45	45
	Preperiod RMSE	0.049	0.048		Preperiod RMSE	0.030	0.023
	RMSE percentile	0.41	0.63		RMSE percentile	0.83	0.96
Palm Beach Public Schools	Estimate	0.49	0.39	Broward County Schools	Estimate	-0.056	0.11
	P-value	0.023	0.023		P-value	0.69	0.24
	# of placebos	43	43		# of placebos	53	53
	Preperiod RMSE	0.014	0.036		Preperiod RMSE	0.038	0.041
	RMSE percentile	0.93	0.57		RMSE percentile	0.56	0.44
Palm Beach County	Estimate	0.10	0.14	Broward County	Estimate	0.04	0.09
	P-value	0.39	0.16		P-value	0.35	0.39
	# of placebos	56	56		# of placebos	53	53
	Preperiod RMSE	0.042	0.07		Preperiod RMSE	0.083	0.15
	RMSE percentile	0.42	0.26		RMSE percentile	0.09	0.06

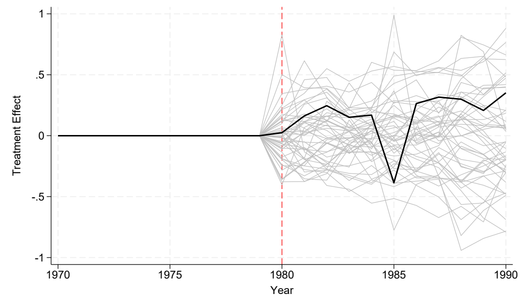
Note: The table presents bias-corrected synthetic control estimates of the effect of the Mariel Boatlift on financial outcomes for governments in Palm Beach County and Broward County. The treatment effect is averaged over the years 1981 to 1990. The p-values are based on the permutation test described in Section 3.2. The root-mean square error (RMSE) and the percentile are calculated using ten years of pretreatment data and are based on the synthetic control without bias correction. The donor pools for Palm Beach County were restricted to cities with 1980 population in 1980 between 60-70k, districts with 1980 enrollment greater than 50k, and counties with 1980 population between 400-700k. The donor pools for Broward County were restricted to cities with 1980 population in 1980 between 110-160k, districts with 1980 enrollment greater than 45k, and counties with 1980 population greater than 650k.

Figure A1: Results for Palm Beach County

City of West Palm Beach

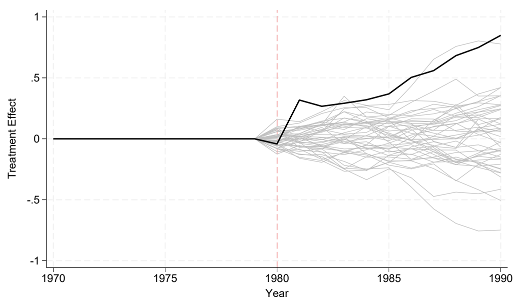
Figure A1a: Log Total Revenues **Figure A1b: Log Total Expenditures**

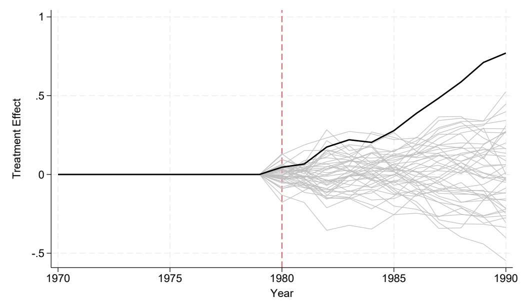




Palm Beach County School District

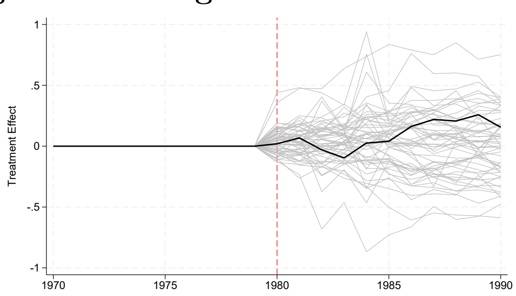
Figure A1c: Log Total Revenues **Figure A1d: Log Total Expenditures**

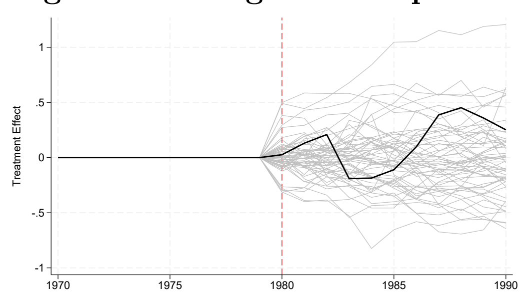




Palm Beach County

Figure A1e: Log Total Revenues **Figure A1f: Log Total Expenditure**





Note: The figures plot the estimated treatment effect estimates for the City of West Palm Beach, the Palm Beach County School District, and Palm Beach County from 1970 to 1990. In each case, there are separate figures for log total revenues and log total expenditures. The solid lines plot the bias-corrected treatment effect estimates, while the grey lines reflect the bias-corrected placebo estimates. The vertical dash lines indicate 1980, the year of the treatment. Figure A1e omits three placebo-year observations that are outside the range of the graph.

Figure A2: Results for Broward County

City of Fort Lauderdale

Figure A2a: Log Total Revenues

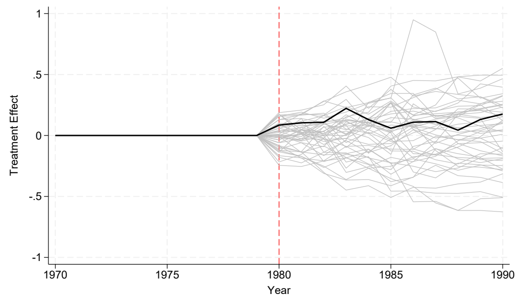
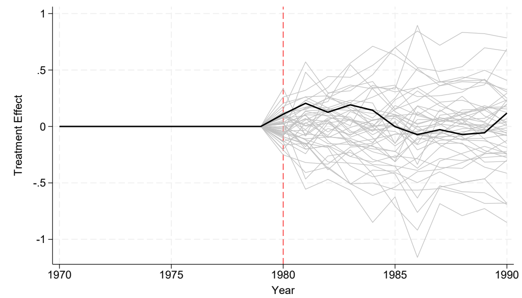


Figure A2b: Log Total Expenditures



Broward County School District

Figure A2c: Log Total Revenues

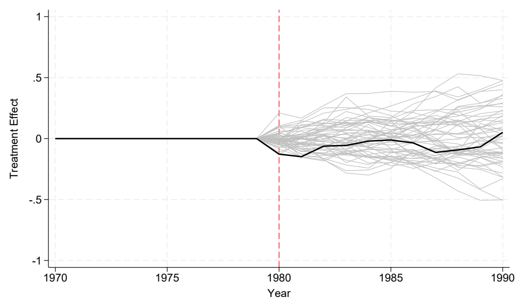
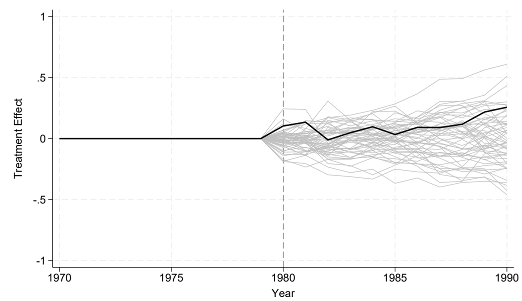


Figure A2d: Log Total Expenditures



Broward County

Figure A2e: Log Total Revenues

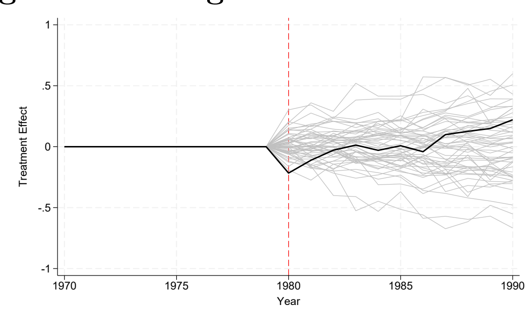
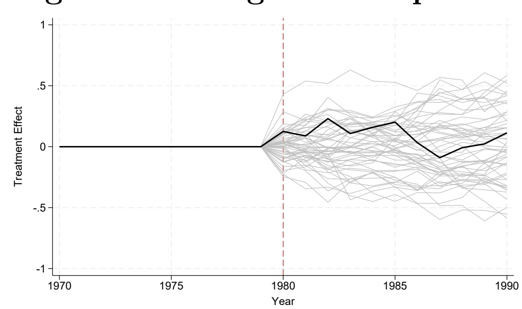


Figure A2f: Log Total Expenditure



Note: The figures plot the synthetic control estimates for Fort Lauderdale, Broward County Schools, and Broward County from 1970 to 1990. In each case, there are separate figures for log total revenues and log total expenditures. The solid lines plots the actual outcomes, while the dotted lines plot the synthetic controls. The vertical dash lines indicate 1980, the year of the treatment.

Table A1: Weights in the Synthetic Control for School District Expenditures

District	Weight	District	Weight
Albuquerque	0	Jefferson County (AL)	0
Atlanta	0.106	Jefferson County (CO)	0
Austin	0	Jefferson County (KY)	0
Chicago	0	Jefferson Parish	0
Cincinatti	0	Long Beach	0
Clark County	0	Los Angeles	0.435
Cleveland	0	Milwaukee	0
Cobb County	0	Mobile County	0
Columbus	0.167	Oakland	0
Dallas	0	Orange County	0
Dekalb County	0	Orleans Parish	0
Denver	0	Philadelphia	0.018
Detroit	0	Pinellas County	0
Duval County	0	Polk County	0
East Baton Rouge Parish	0	Portland	0
El Paso	0	San Antonio	0
Ft. Worth	0	San Diego	0.065
Granite	0	San Francisco	0.084
Greenville County	0	St. Louis	0
Hillsborough County	0	Tucson	0
Houston	0.125	Tulsa	0
Indianapolis	0		0

Note: This paper's analysis looks at several different treated units and multiple outcomes, leading to a large number of weighting schemes used in synthetic control estimation. This table shows one of those weighting schemes; specifically, it shows the weights used in the synthetic control for school district expenditure in the Miami-Dade School District. The table lists the school districts in the donor pool along with their weights. The donor pool is limited to school districts with enrollments in 1980 of greater than 50,000 students.

Table A2: Synthetic Control Estimates for the State of Florida, 1981-1990

		Revenues	Expenditures			
		Log Total Revenue	Log Total Expenditure	Log Education Expenditure	Log Health Expenditure	Log Intergov Expenditure
Florida	Estimate	0.09	0.10	0.08	0.12	0.12
	P-value	0.06	0.15	0.23	0.33	0.29
	Number of placebos	47	47	47	47	47
	Preperiod RMSE	0.022	0.024	0.022	0.007	0.059
	RMSE percentile	0.52	0.33	0.44	0.90	0.27

Note: The table presents bias-corrected synthetic control estimates of the effect of the Mariel Boatlift on financial outcomes for the State of Florida. The donor pool consists of all state governments. The vector of pretreatment outcomes available for matching includes 1972 and 1977-1979 and is thus more limited than that for local governments. The treatment effect is averaged over the years 1981 to 1990. The p-values are based on the permutation test described in Section 3.2. The root-mean square error (RMSE) and the percentile are calculated using four years of pretreatment data and are based on the synthetic control without bias correction.

Table A3: Current Operating Expenditures in the Miami-Dade School District

		Log Total Current Operating Expenditure
Miami-Dade Public Schools	Estimate	0.20
	P-value	0.09
	# of placebos	43
	Preperiod RMSE	0.023
	RMSE percentile	0.75

Note: The table presents bias-corrected synthetic control estimates that are similar to those in Table 2 for the Miami-Dade School District, except that instead of looking at total expenditures, it looks at current operating expenditures.

Table A4: Placebo in Time

		Log Total Expenditure
Miami City	Estimate	0.037
	P-value	0.57
	# of placebos	36
	Preperiod RMSE	0.002
	RMSE percentile	0.84
Miami-Dade Public Schools	Estimate	0.023
	P-value	0.36
	# of placebos	60
	Preperiod RMSE	0.019
	RMSE percentile	0.66

Note: The table presents bias-corrected synthetic control estimates from a placebo test that estimates the "effect" of an immigration shock that occurs in 1975. The synthetic control is matched on the basis of pretreatment outcomes between 1970-1974. The treatment effect is averaged over the years 1975 to 1979.