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 shown that there are divergences 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 education costs increased in Miami in the aftermath of the Boatlift, leading to higher property tax rates and increased state transfers. These effects persisted for at least ten years. The results shed light on the heterogeneous impacts of immigration over time and space.

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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 and employment pressures on the native population, particularly among those subgroups of workers who are exposed to increased labor market competition. Because of these concerns, much of the economic literature on immigration has focused on its effect on labor markets. Though the effects depend considerably on the nature of the migrant inflows and the demographics of the affected workers, much of this work has concluded that fears over wage competition are often misplaced; in fact, numerous studies have found that the average long-run impact of immigration on the wages of native workers may even be positive (Ottaviano and Peri, 2012; Dustmann et al., 2013; Albert, 2021; Dustmann et al., 2017; Abramitzky et al., 2019).

Despite these findings, an analysis of immigration that accounts only for wage and employment effects will be incomplete. A full accounting of the welfare effects of immigration must also account for its fiscal impacts, particularly the near-term effects on local governments that provide services to recent arrivals. Recent work using census data has made progress toward understanding these fiscal effects. A widely cited report by the National Academies of Science, Engineering, and Medicine (NAS) provides a comprehensive set of estimates regarding the 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, by relying on cash flow accounting methods whereby the public services received by immigrants are netted out from their taxes paid, this work suffers from several limitations (Clemens, 2022). First, the estimates are sensitive to how the costs of public goods are allocated (Orrenius, 2017). Second, and perhaps most importantly, they are biased due to their failure to account for immigration's impact on prices or productivity. A separate approach that has attempted to account for these general 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 highly sensitive to modeling choices, particularly the long-run elasticities of labor demand (Clemens, 2022).

Moreover, because most of the major work on immigration takes an approach that is nation-wide and long-term in scope, it may also obscure important heterogeneity across time and space. One of the important questions raised by the National Academy report is how and to what extent the federal government should compensate lower-level governments for the short-term costs they incur in receiving migrant inflows. This is especially relevant as immigrants are unequally distributed geographically and disproportionately cluster in the most heavily populated metro areas (Pew Research Center, 2020). Understanding the implications of these inflows for fiscal federalism requires well-identified estimates at the local level.

To investigate the fiscal effects of immigration on local governments, this paper revisits 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. In keeping with the previous work, this paper exploits the Boatlift as a natural experiment, but focuses on the fiscal impacts on local governments, thereby bringing quasi-experimental methods to a literature whose results otherwise rely on strong modeling assumptions.

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, and the City of Miami – thereby capturing the full effect on the Boatlift on a variety of government services. Drawing on data from the Census of Governments, the analysis first considers the effect of the Boatlift on total revenues and expenditures for each level of government over the ten year period following the Boatlift (1981-1990). To shed light on the exact nature of the expenditure pressures brought about by the population increase, I then further examine specific revenue and expenditure categories to show what is driving the overall effects. Finally, drawing on tax data from the Florida Department of Revenue, an additional analysis examines how the Boatlift affected changes in property tax rates and the taxable value of real estate, thereby shedding light on the mechanism by which any change in property taxes occurred.

The results indicate that education costs increased by more than 20 percent in Miami in the aftermath of the Boatlift, and that these effects persisted for at least ten years. These expenditure pressures led in turn to an increase in state transfers as well as an increase in property taxes, driven by an increase in tax rates of approximately 5 basis points. While the effects were concentrated in the Miami-Dade School District, the results also show that the City of Miami experienced a 20% increase in total spending in the immediate aftermath of the Boatlift owing partly to the construction of resettlement camps. There were no significant fiscal impacts on the county government.

This paper contributes to a broad literature on the economic and fiscal effects of immigration. As noted above, because of concerns about the effects of immigration on native wages and employment, much of the economic literature has focused on the effects of immigration on labor markets (Ottaviano and Peri, 2012; Dustmann et al., 2013; Albert, 2021; Dustmann et al., 2017; Abramitzky et al., 2019; Borjas and Monras, 2017). There has been far less academic work on the fiscal effects; the work that has appeared has focused primarily on Europe and, as with National Academies of Sciences, Engineering, and Medicine (2017), is mostly based on cash-flow accounting (Dustmann and Frattini, 2014; Martinsen and Pons Rotger, 2017; Jofre-Monseny et al., 2016) A more recent body of literature examines the effect of immigration on preferences for redistribution (Alesina et al., 2021, 2023).[1]

A subset of the economic literature on immigration deals with the consequences of large immigration shocks, typically using difference-in-difference or instrumental variable methods and looking at social outcomes, such as marriage (Eriksson et al., 2022), crime rates (Bell et al., 2013), or economic impacts. As with this paper, these studies leverage a particular migrant wave in order to take advantage of the opportunity for improved identification and transparency. While the Mariel Boatlift was the product of a particular leadership decision as well as socioeconomic conditions in the origin country, others have studied immigration shocks that resulted from border closures (Eriksson et al., 2022; Abramitzky et al., 2019), extreme weather events (Peri et al., 2022) and war (Bell et al., 2013; Erten and Keskin, 2021).

This paper proceeds as follows. Section 2 provides background on the Mariel Boatlift. Section 3 discusses the synthetic control approach and the details of its application. Section 4 outlines the data and provides summary statistics. The results are presented in Section 5,

[1] 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.

along with evidence for mechanisms. Section 6 concludes.

2 Background on the 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. 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. Figure 1 shows the total population of Miami-Dade County (a subset of the Metro area) before and after the Boatlift. The population appears to increase by approximately 100,000 by 1981 relative to a linear extrapolation, with the population increase tapering off slightly over time. Figure A1 shows the populations of the counties surrounding Miami, including Monroe County, home to Key West where many of the "Marielitos" landed. None of these bordering counties show the same obvious spike in population experienced by Miami.

2.1 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 critiqued Card's methods, most notably the ad hoc nature of the comparison group, the lack of focus on low-skilled workers, and for his failure to account for non-classical measurement error in his standard errors (Peri and Yasenov, 2019).

Using a restricted subsample of high school dropouts and the Current Population Survey (CPS), Borjas (2017) reconsidered the Boatlift using newer methods. Constructing comparison groups based on employment trends prior to the Mariel shock, he found 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.

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, understanding the economic and political consequences of immigration also requires an understanding 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.

3 Methods

3.1 Synthetic Control

To investigate the fiscal impact of the Mariel shock on local governments in the region, 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. Unlike the earliest studies of the Boatlift that relied on a comparison group that was assembled ad hoc, the advantage of the synthetic control approach is that it constructs a weighted average 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.

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. Nevertheless, recent papers have made strides in advancing a set of best practices for the methodology (McClelland and Mucciolo, 2022; Abadie, 2021). These include: 1) restricting the size of the donor pool, as recommended by Abadie et al. (2015) and Abadie (2021), and 2) reporting results from matching on the basis of all pretreatment outcomes, as suggested by Ferman et al. (2020) and McClelland and Mucciolo (2022). Thus, this paper follows those guidelines by first limiting the donor pool to units of a comparable size to the treated unit, and then matching on the full set of pretreatment outcomes but no other covariates. In a series of robustness tests, I relax these restrictions and explore the sensitivity of the results to expansions of the donor pool and the inclusion of additional time-varying covariates.

3.2 Treated Units

The Miami metropolitan area includes three different counties and more than twenty municipalities, raising the question of which government entities should be considered "treated." The earlier labor market studies focused on the metropolitan area as a result of the sampling practices of the CPS. However, the metro area does not constitute an independent government entity with its own budget. 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. As demonstrated in Figures 1 and A1, Census data suggest that Miami-Dade was the recipient of the majority of refugees that remained in south Florida. As school districts in Florida are based on county lines, the borders of the Miami-Dade School District are coincident with those of the county.

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. For each treated unit, the SCM draws on a separate pool of comparison units ("donor pool"). 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. However, in order to restrict the size of the donor pool such that the comparison group represents a more suitable counterfactual, the comparison set of counties is limited 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 is limited to districts with greater than 50k students (compared with 226K in the Miami-Dade School District)[2], and the comparison set of cities is limited to those with populations between 200 and 500k (compared with 347K in the City of Miami).[3] Any governments from the counties that are contiguous with Miami-Dade are excluded from the donor pool, eg. 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 and Mucciolo, 2022). In section 5.4 I explore the robustness of the results to alternative restrictions on the donor pool.

3.3 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. The actual treated unit is not considered for estimation of the placebo effects. 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, one can compute "standardized" p-values by dividing all effects by the corresponding pretreatment match quality (as measured by the pretreatment RMSE) (Galiani and Quistorff, 2017). Thus, for all of the estimates of individual years, I provide standardized p-values. To produce inference for the postreatment effect across all periods, Abadie et al. (2010) suggest using the posttreatment RMSE. This too can be standardized by pretreatment match quality. Thus, when reporting average effects in the post-treatment period, I report corresponding p-values that represent the proportion of

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

[3] 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.

placebos that have a ratio of posttreatment RMSE to pretreatment RMSE that is at least as large as the ratio for the treated unit.

3.4 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.[4]

4 Data and Variables

To construct the pool of comparison governments for the main set of analyses, this paper draws on data from the Census of Governments. 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, 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

[4]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.

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.

In order to probe whether or not changes in government spending as a result of the Boatlift led to changes in tax rates, the analysis also draws on data from the Florida Department of Revenue. The Department of Revenue provides data on the aggregate taxable values of real and personal property and millage rates by county. The mill rate is the tax rate that is applied per $1,000 of assessed value.

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 in fiscal year 1979. The bulk of the county government's revenues came from charges and miscellaneous revenues (36%), with the remainder coming from intergovernment revenues (32%) and property taxes (21%). The Miami-Dade school district is next largest, collecting $493 million, the majority of which came from intergovernmental revenues (61%) and property taxes (34%). The city government 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%).

5 Results

5.1 Pre-Treatment Balance

Before discussing the findings, in this section I discuss the pretreatment balance between the treated entities (city, school district, or county) and their synthetic controls. 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 (1970-1979). 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). This metric is presented below the RMSE in the tables that follow and labeled as RMSE percentile. It 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.[5]

Figure 2 plots the synthetic control alongside the actuals for the three treated units: the City of Miami, the Miami Dade School District, and Miami-Dade County. For each jurisdiction, there is a separate synthetic control plot for log total revenues and log total expenditures. Based on visual inspection of Figure 2, the City of Miami and the Miami-Dade School District demonstrate a strong fit with their respective synthethic controls. Prior to the red line demarcating the Boatlift in 1980, there is very little separation between the jurisdiction and its synthetic control. On the other hand, the County shows a noticeably weaker fit, particularly in total revenues where there is a fluctuating gap.

These observations are further bolstered by the pretreatment RMSE and RMSE percentile shown in Table 2. For the City of Miami's synthetic control estimates, 89 and 98 percent of the placebos have a pretreatment RMSE that is as least as large as the treated unit. For the School District, these numbers are also relatively high: 63 and 86 percent. On the other hand, for Miami-Dade County, only 13 and 29 percent of placebos have a pretreatment RMSE as large. Taken collectively, these results lend confidence to the estimates for

[5]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.

the City and School District, while suggesting caution for any results for the County, and consequently in the discussion that follows I focus primarily on the City and School District.

5.2 Main Results

The right-hand side of the plots in Figure 2 demonstrate the post-treatment trajectory of all three treated units. Figures 2a and Figure 2b indicate that, while the City of Miami may have expanded slightly faster during specific short intervals following the Boatlift, most notably in total expenditures during the two years immediately following the Boatlift (1981-1982), there is no persistant effect of the Boatlift. 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.[6] Table 2 provides the corresponding synthetic control coefficients. The average effect of the Boatlift on the City's finances over the ten year post-treatment period is 8 percent for total revenues and 6 percent for total expenditures. Neither effect is statistically significant (p-values of 0.30 and 0.19). For the School District, the average effect on revenues is 12 percent, while the average effect on expenditures is 26 percent. The estimate for expenditures has a p-value of 0, indicating that the effect size is larger than all of the 43 placebo estimates. The estimate for revenues is not significant (p-value 0.56).

Effects by Year Table 3 presents the treatment effect estimates for the City and School District by year. (I exclude results for the County because of both the poor pretreatment balance as well as small effect sizes.) The effect on the City's total expenditures spikes shortly after the Boatlift - increasing by 19 percent in 1981 and 20 percent in 1982. Both of these two estimates are statistically significant (p-values of 0.021 and 0.043), however none of the other estimates for the City's revenues or expenditures are, indicating that the City experienced a short-term increase in spending as a result of the Boatlift that swiftly

[6]Figure A2 and A3 also plot the results of permutation tests of the significance between the City/School District and its synthetic control for both total revenues and total expenditures.

dissipated. On the other hand, the effect of the Boatlift on the School District appears to have increased steadily over time – from an increase in spending of 8 percent in 1981 to a 41 percent increase in 1990. With the exception of 1981, all of the spending estimates for the School District have p-values of less than 0.05.

Effects on Individual Line-Items What caused this rise in spending, and how was it financed? Like other states, Florida requires its local governments to balance their budgets, and consequently any increase in service provision (expenditures) must be matched by an increase in revenues, often accomplished by adjusting property tax rates. Figure 3 shows synthetic control plots for several of the larger revenue and expenditure categories for the City of Miami. Two plots stand out: police expenditures and parks and recreation. Figure 3c indicates an increase in police expenditures that grew over time, from 3 percent in 1981 to a high of 43 percent in 1989. Figure 3e shows that spending on parks and recreation also spiked dramatically in the years immediately following the Boatlift (1981-1982). Table 4 shows the corresponding synthetic control estimates for the average effect over the post-treatment period. While the the average effect on police spending (29 percent) is quite large, neither the average effect nor any of the annual estimates (not shown) are statistically significant. Moreover, other crime-related events that took place around the time of the Boatlift, specifically the race riots that took place in Miami in May of 1980 as well as the drug war that began in the late 1970s, raise some concern that the increase in police spending may be due to other causes. On the other hand, while the increase in police spending is both statistically insignificant and potentially spurious, parks and recreation spending shows an increase of 76 and 90 percent in 1981 and 1982 respectively, the latter of which is significant under the classical permutation test (with a p-value of 0), though not significant (p-value of 0.15) using standardized p-values. This large increase in parks & rec spending in 1981-1982 mirrors the uptick in total expenditures for that same period. Equivalant to 23 million dollars between 1980-1982, or approximately one third of the increase in total expenditures, the increase primarily occured via an increase in capital outlays for the parks department (not shown), and thus likely reflects the City's efforts to build resettlment camps to house the refugees (National Archives and Records Administration, 2015). Thus, while the effect is imprecise using standardized p-values, the temporary increase in parks and recreation spending helps to shed light on the increase in total expenditures during that period.

In the School District, where all spending is categorized as education and thus cannot be broken down in the same way, there is a sharp increase in property taxes in 1981 that persists for the duration of the observed post-treatment period, and an increase in intergovernmental revenues that grows over time. A separate analysis (not shown) indicates that the growth in intergovernment revenue is due almost entirely to state rather than federal transfers.[7] As with the effects on other disaggregated categories, these estimates are not statistically significant and thus can only offer a plausible channel by which the main effect on revenues occured. Nevertheless, the evidence is consistent with a story in which the School District's persistent increase in spending was financed by increases in property taxes and intergovernmental revenues from the state.

Effect Sizes How large are these effect sizes? One way to place the overall effect sizes into context is to compare them to the increases in population and student enrollment. While previous work on the labor market impacts of the Boatlift estimate that it increased the labor force in the metro region by eight percent between 1980 and 1990 (Peri and Yasenov, 2019), the data on county-level population presented in Figure 1 suggests that it increased the total population of the county by approximately 100,000, or roughly 6 percent. Appendix Figure A4 plots the enrollment of the Miami-Dade School District. Although it is difficult to infer the exact magnitude of the enrollment increase based on the pre-treatment trend alone, the figure implies an immediate enrollment increase of around six percent in 1981 that widens significantly in the following years.

A further analysis of Census records sheds additional light on the number of school-age

[7] 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, with an especially large effect on intergovernmental expenditures (0.19), none are statistically significant.

children. According to the 5 percent sample of the 1990 Census, 6 percent of school-aged children in Miami-Dade County in 1990 had a Cuban parent that arrived to the country in 1980-1981. Only 3 percent arrived themselves from Cuba at that time. Among 18 year-olds, 8 percent either had a parent or were themselves a part of the refugee wave.

The synthetic control results above show that the Boatlift increased spending in the Miami-Dade school district by 26 percent on average, increasing from a low of eight percent in 1981. However, 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). Assuming an average enrollment increase of 8 percent and an average spending increase of 20 percent, these results would imply that the per pupil costs of educating the Marielitos was 150 percent higher than the average per pupil cost prior to their arrival. 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. They are 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). Not only did the Marielitos continue to have children after their arrival, but many of these children 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.

5.3 Property Tax Rates and Taxable Value

The results above suggest that the increase in education spending was financed through an increase in both property taxes and intergovernmental revenue. To understand the economic effects of the population increase, one would ideally like to measure the incidence of these tax increases. Understanding the incidence of the transfers is complicated by the multiple sources of revenue that the state uses to fund itself. Although sales taxes represent by far the largest tax source for the state (44 percent of revenues in 1979), the state also receives federal transfers and raises revenue through charges and fees. On the other hand, the school district only has one significant "own" source of revenue: the property tax. Thus, one further way to investigate the incidence of the tax increases is to examine the millage rates and taxable values for the school district. These represents the two different channels by which an increase in property taxes may have occurred: as the result of an increase in rates or as the result of an increase in the assessed value of property. If the increase in property taxes resulted from an increase in the value of property in the region, then this would suggest that property owners may have indirectly benefitted from the increase in population and the ensuing increase in housing demand. On the other hand, if the increase in property taxes resulted from an increase in rates, then this would suggest that property owners at least partially bore some of the costs (under the assumption that owners were not able to completely pass on the increase to tenants).

Using tax assessment data from the Florida Department of Revenue, I estimate the effect of the Boatlift on school district millage rates (inclusive of both operating and debt service) and the taxable value of real property. For taxable values, I use county-level data since this overlaps with the taxing jurisdictions of school districts, and I scale taxable value by county expenditures; Miami-Dade County has the highest value of real estate in Florida, and thus without scaling, the County is not within the convex hull of the values for the donor pool.[8] The donor pool consists of all other counties in Florida with complete sets of observations over the pretreatment period, excluding the contiguous counties of Broward, Monroe, and Palm Beach. The results for millage rates are presented in Figure 5 and Table 5, while the results for taxable values are presented in Figure 6 and Table 6. Although neither of these estimates is statistically significant, the results point to an increase in rates

[8]This relies on the finding that the Boatlift did not affect spending among county level governments.

as the cause for the increase in property values. Table 5 indicates that the mill rate increased by .45 mills, which is equivalant to an increase in the tax rate of 0.045 percentage points, while Figure 6 shows that taxable values in the school district were lower on average than its synthetic control. Thus, while not dispositive, these findings suggest that an increase in property tax rates financed the increase in education spending rather than any change in the value of real estate.

5.4 Robustness Tests

Although best practice suggests matching on the basis of all pretreatment outcomes alone and restricting the size of the donor pool, it is possible that different choices regarding matching procedures and the donor pool may improve on the pretreatment fit. 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 (Abadie, 2021). Insofar as the main estimates do not fully capture the structural determinants of spending, including additional demographic variables may potentially provide a better pretreatment fit across the complete parameter space. Thus, to the set of lagged outcomes, I add covariates for population and population growth (for the city and county) and enrollment and enrollment growth (for the school district). Specifically, I add the population/enrollment in 1970 and population/enrollment growth between 1970 and 1980.[9] Next, I reduce the number of lagged outcomes used for matching. Insofar as more recent outcomes provide a better predictor of future trajectories than earlier ones, the optimal approach may be to limit the covariates used for matching to the most recent outcomes, and reducing the number of covariates has the potential to improve on the pretreatment fit in the years immediately preceding the Boatlift. Thus, instead of matching on ten years of lagged outcomes as in the baseline estimate, I match

[9] See footnote 3. A measure of population/enrollment growth that incorporates data from 1980 may contain post-treatment information if the 1980 measure was affected by the Boatlift. However, the growth measure should be only marginally affected by changes in 1980.

only on the most recent five years (and drop earlier years from the sample) in addition to the covariates for population/enrollment growth. 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.[10] Finally, in the spirit of Abadie et al. (2010), I remove comparison units for the donor pool that have poor match quality in the pretreatment period. Specifically, I do not include placebo effects in the calculation of p-values if the preatment RMSE is greater than ten times the pretreatment RMSE of the treated unit. This last test only affects the calculation of p-values; the treatment effect estimates remain unchanged from the baseline.

The results for the five robustness checks are presented in Table 7. Adding covariates to the matching process does little to improve the pretreatment fit and produces estimates that are nearly identical to the baseline estimates. Matching on a smaller number of lagged outcomes does produce some different results; the effect on School revenue more than doubles from 0.12 to 0.27, and the effect on County expenditures also shows a large increase. However the p-values for the County estimates remain large, and qualitatively the overall results are similar. Expanding the size of the donor pool results in estimates that are of similar magnitude but are less precise, with the exception of City spending, which is now statistically significant with a p-value of 0.04. Shrinking the donor pool has little effect; the pretreatment fit is now worse for the City estimates but better for the School estimates. Removing placebos that are of poor match quality has little effect on p-values; all of the estimates that were statistically significant remain so (including the year-specific effects on city spending in 1981-

[10] An alternative approach would be to use the entire donor pool and not place any restrictions, thereby removing researcher discretion entirely. However, not only would this create comparison groups that are extremely distinct from the treated units, but with over 3,300 school districts in the potential donor pool, along with more than 1,700 cities and 1,100 counties, running the necessary permutation tests would be extremely computationally intensive.

1982, not shown in the table). Overall, the robustness checks support the main findings. Total City spending increased sharply in 1981-1982, but was short-lived, while the School District increased spending by an average of 26 percent between 1981-1990 relative to its synthetic control.

Placebo Test Finally, in addition to varying the covariates used for the construction of the synthetic control, 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 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 0.035 with a p-value of 0.22; the corresponding effect for schools is 0.004 with a p-value of 0.78. 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 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. City spending increased by 20% for a brief two year period following the Boatlift while results for the school district show that educational expenditures increased steadily over the ten year period following the Boatlift for an average increase of 26 percent. The paper also presents suggestive evidence that, in order to finance the increase in spending, the school district relied on higher transfers from the state government and increased property tax rates by approximately 5 basis points.

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 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 prior work showing that a large number of refugees settled in Miami permanently. 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 a local property tax increase was nonetheless necessary to finance the expansion of 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 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. 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 6 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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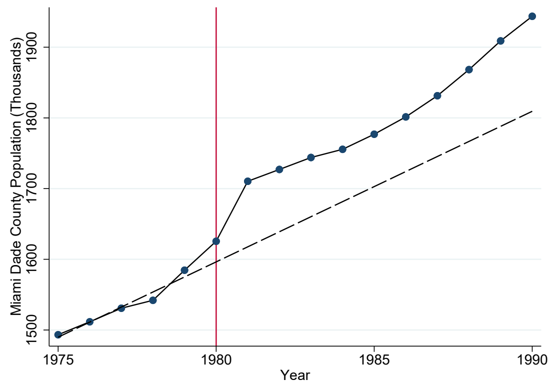
Figure 1: Miami-Dade Population



Note: Source: U.S. Census Bureau. The figures show the population for Miami-Dade County during the years before and after the Boatlift. Estimates are as of July 1. The Boatlift occurred between April and October of 1980. The dotted line is a linear projection based on 1975-1979.

Figure 2: Main Results

City of Miami

Figure 2a: Log Total Revenues

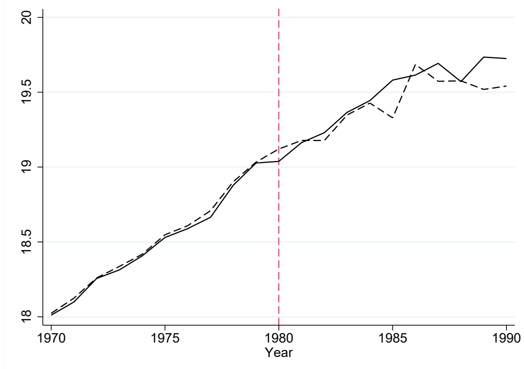
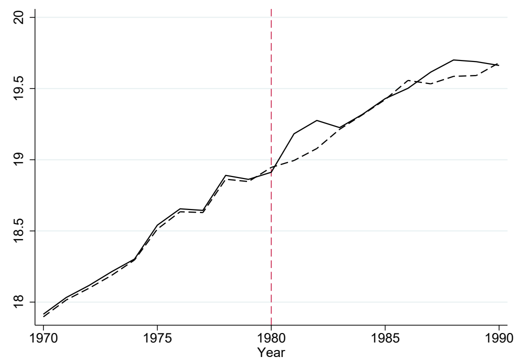


Figure 2b: Log Total Expenditures



Miami-Dade County School District

Figure 2c: Log Total Revenues

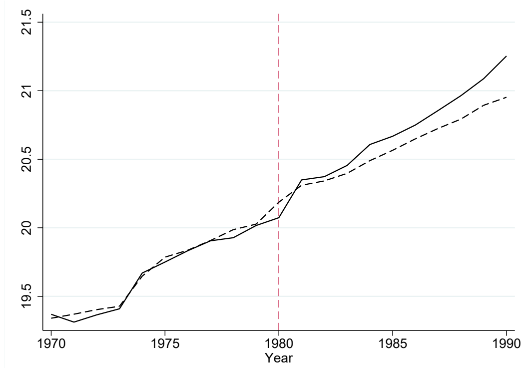
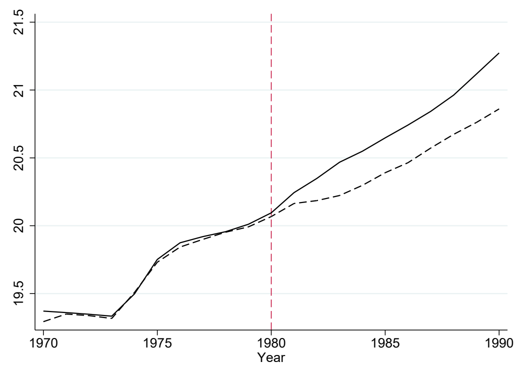


Figure 2d: Log Total Expenditures



Miami-Dade County

Figure 2e: Log Total Revenues

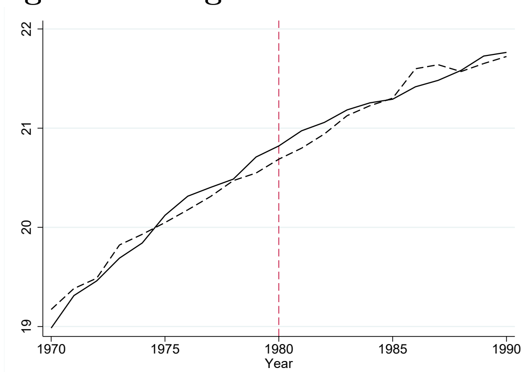
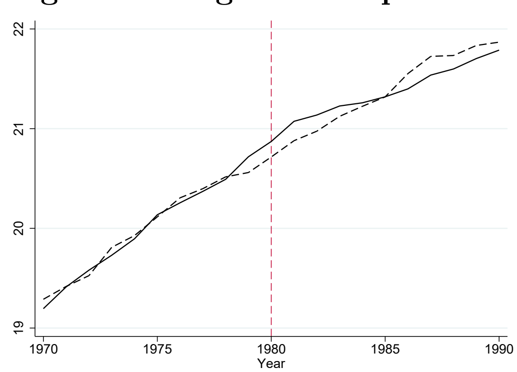


Figure 2f: Log Total Expenditure



Note: The figures plot the synthetic control 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 plots the actual outcomes in the City, while the dotted lines plot the synthetic control 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

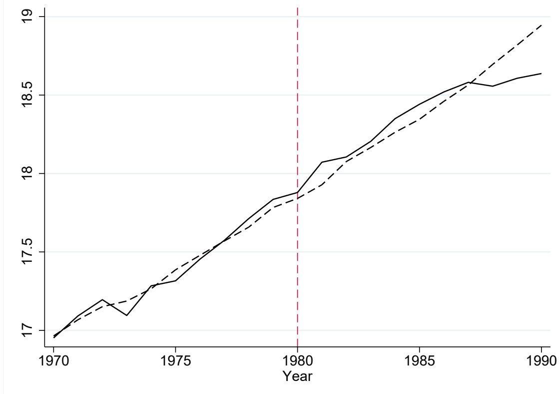


Figure 3b: Log Intergov Revenue

Figure 3c: Log Police Expenditures

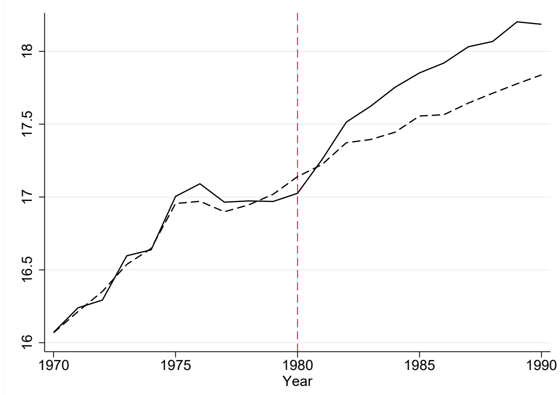


Figure 3d: Log Fire Protection Expend

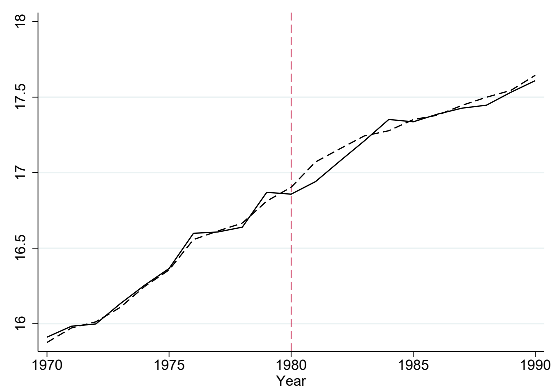
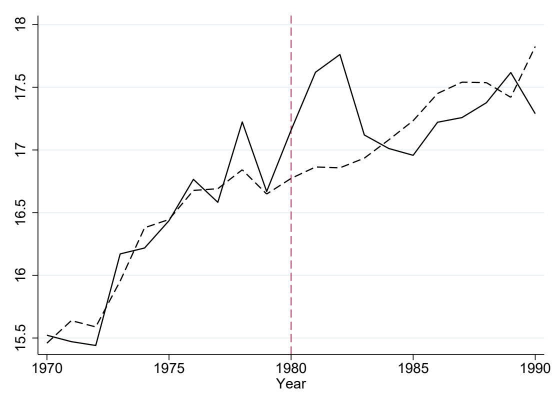


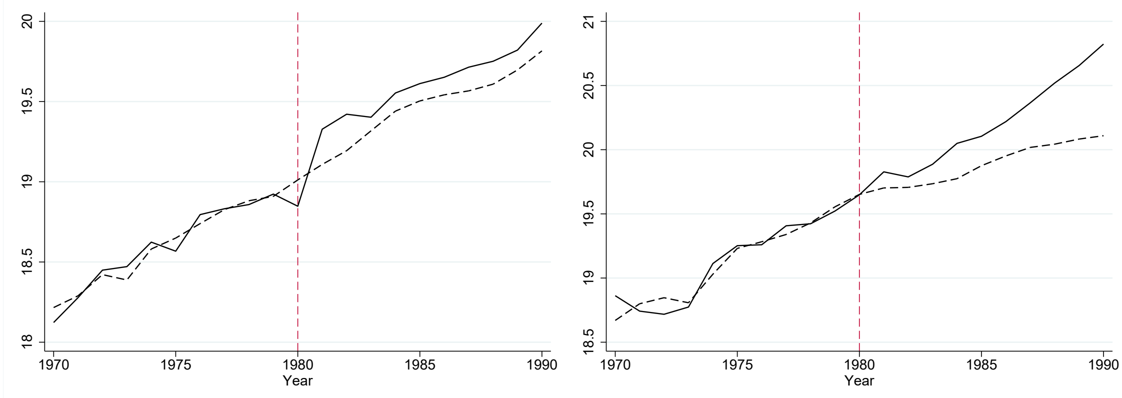
Figure 3e: Log Parks & Rec Expenditures



Note: The figures plot the synthetic control estimates for the City of Miami from 1970 to 1990. The solid lines plot the actual outcomes (log property tax, etc) in the City, while the dotted lines plot the synthetic control estimates. The vertical dash lines indicate 1980, the year of the treatment.

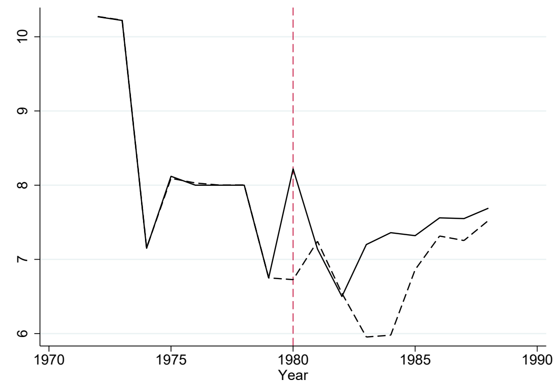
Figure 4: Results for Selected Line Items in the Miami-Dade County School District

Figure 4a: Property Taxes **Figure 4b: Intergov Revenues**



Note: The figures plot the synthetic control estimates for the Miami-Dade School District from 1970 to 1990. The solid lines plots the actual outcomes (log property tax, log intergovernment revenues) in the District, while the dotted lines plot the synthetic control estimates. The vertical dash lines indicate 1980, the year of the treatment.

Figure 5: Miami Dade School District Mill Rate



Note: The figure plots the synthetic control estimate for the effect of the Boatlift on property tax millage rates in the Miami-Date School District from 1976 to 1988. The solid line plots the actual mill rate, while the dotted line plots the synthetic control estimate. The vertical dash line indicates 1980, the year of the treatment. The mill rate is the tax that is applied per $1,000 of assessed value; 1 mill is equal to $1 in property tax per $1,000 of a property's taxable value.

Figure 6: Miami Dade School District Taxable Value

Note: The figure plots the synthetic control estimate for the effect of the Boatlift on the log of the taxable value of real property as a share of county government spending in Miami-Dade County from 1972 to 1990. The solid line plots the actual outcome values, while the dotted line plots the synthetic control estimate. The vertical dash line indicates 1980, the year of the treatment.

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	0.08	0.06
	P-value	0.30	0.19
	# of placebos	47	47
	Preperiod RMSE	0.022	0.017
	RMSE percentile	0.89	0.98
Miami-Dade Public Schools	Estimate	0.12	0.26
	P-value	0.56	0.00
	# of placebos	43	43
	Preperiod RMSE	0.040	0.021
	RMSE percentile	0.63	0.86
Miami-Dade County	Estimate	0.02	-0.02
	P-value	0.98	0.85
	# of placebos	48	48
	Preperiod RMSE	0.11	0.081
	RMSE percentile	0.13	0.29

Note: The table presents 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 3.3 and are standardized based on pretreatment match quality. The root-mean square error (RMSE) is calculated using ten years of pretreatment data, and the percentile is based on a comparison among all placebo estimates.

Table 3: Results By Year

		Total Revenue		Total Expenditure	
		Estimate	P-value	Estimate	P-value
Miami City	1981	-0.014	0.81	0.19	0.021
	1982	0.052	0.62	0.20	0.043
	1983	0.018	0.87	0.012	0.83
	1984	0.017	0.89	0.003	0.94
	1985	0.25	0.15	0.006	0.91
	1986	-0.07	0.53	-0.055	0.49
	1987	0.12	0.26	-0.08	0.34
	1988	-0.005	0.96	0.12	0.19
	1989	0.22	0.26	0.098	0.26
	1990	0.18	0.30	-0.019	0.81
Miami-Dade Public Schools	1981	0.04	0.72	0.08	0.069
	1982	0.03	0.88	0.16	0.047
	1983	0.06	0.77	0.25	0
	1984	0.12	0.58	0.25	0.023
	1985	0.10	0.58	0.26	0
	1986	0.10	0.53	0.28	0
	1987	0.13	0.49	0.27	0.047
	1988	0.17	0.44	0.29	0.047
	1989	0.19	0.47	0.36	0
	1990	0.30	0.33	0.41	0.047

Note: The table presents synthetic control estimates of the effect of the Mariel Boatlift on financial outcomes for the City of Miami and the Miami-Dade County School District by year. The p-values are based on the permutation test described in Section 3.3 and are standardized based on pretreatment match quality.

Table 4: Synthetic Control Estimates for Additional Line Items

		Revenues		Expenditures		
		Log Property Tax	Log Intergov Revenue	Log Police Expenditure	Log Fire Expenditure	Log Parks Expenditure
Miami City	Estimate	-0.02	0.19	0.29	-0.030	0.05
	P-value	0.72	0.74	0.30	0.85	0.64
	# of placebos	47	47	47	47	47
	Preperiod RMSE	0.047	0.117	0.063	0.030	0.20
	RMSE percentile	0.45	0.47	0.34	0.72	0.23
Miami-Dade Public Schools	Estimate	0.14	0.32			
	P-value	0.67	0.23			
	# of placebos	43	43			
	Preperiod RMSE	0.071	0.081			
	RMSE percentile	0.44	0.40			

Note: The table presents synthetic control estimates of the effect of the Mariel Boatlift on additional financial outcomes for the City of Miami and the Miami-Dade County School District. The treatment effect is averaged over the years 1981 to 1990. The p-values are based on the permutation test described in Section 3.3 and are standardized based on pretreatment match quality. The root-mean square error (RMSE) is calculated using ten years of pretreatment data, and the percentile is based on a comparison among all placebo estimates.

Table 5: Mill Rates

		Property Tax Rate
Miami-Dade	Estimate	0.45
Public Schools	P-value	0.83
Mill Rate	Number of placebos	59
	Preperiod RMSE	0.50
	RMSE percentile	0.17

Note: The table presents the synthetic control estimate of the effect of the Mariel Boatlift on property tax mill rates in Miami. The treatment effect is averaged over the years 1981 to 1988. The p-values are based on the permutation test descibed in Section 3.3 and are standardized based on pretreatment match quality. The root-mean square error (RMSE) is calculated using eight years of pretreatment data (1972-1979), and the percentile is based on a comparison among all placebo estimates. The mill rate is the tax that is applied per $1,000 of assessed value.

Table 6: Taxable Value

		Log Taxable Value as Share of County Expenditure
County-Wide	Estimate	-0.02
	P-value	1.00
	Number of placebos	23
	Preperiod RMSE	0.114
	RMSE percentile	0.61

Note: The table presents the synthetic control estimate of the effect of the Mariel Boatlift on the taxable value of real estate in Miami-Dade County. The outcome variable is measured as the log of taxable value as a share of county expenditure. The treatment effect is averaged over the years 1981 to 1990. The p-values are based on the permutation test descibed in Section 3.3 and are standardized based on the pretreatment match quality. The root-mean square error (RMSE) is calculated using eight years of pretreatment data (1972-1979), and the percentile is based on a comparison among all placebo estimates.

Table 7: Robustness Checks

		Baseline		Additional Covariates		Fewer Years		Larger Donor Pool		Smaller Donor Pool		Restricted Placebos	
		Total Revenue	Total Expend	Total Revenue	Total Expend	Total Revenue	Total Expend	Total Revenue	Total Expend	Total Revenue	Total Expend	Total Revenue	Total Expend
City	Estimate	0.08	0.08	0.08	0.09	0.06	0.13	0.10	0.05	0.04	0.14	0.08	0.08
	P-value	0.30	0.19	0.26	0.19	0.57	0.40	0.29	0.04	0.5	0.46	0.30	0.22
	# placebos	47	47	47	47	47	47	70	70	28	28	47	41
	Preperiod RMSE	0.022	0.017	0.021	0.018	0.027	0.034	0.022	0.006	0.033	0.047	0.022	0.017
	RMSE percentile	0.89	0.98	0.91	0.96	0.53	0.85	0.74	1.00	0.75	0.71	0.89	0.98
Schools	Estimate	0.12	0.26	0.14	0.22	0.27	0.32	0.17	0.24	0.15	0.22	0.12	0.26
	P-value	0.56	0.00	0.49	0.00	0.16	0.02	0.34	0.06	0.15	0	0.57	0.00
	# placebos	43	43	43	43	43	43	65	65	20	20	42	42
	Preperiod RMSE	0.040	0.021	0.040	0.021	0.024	0.003	0.036	0.021	0.039	0.029	0.040	0.021
	RMSE percentile	0.63	0.86	0.63	0.86	0.49	0.95	0.55	0.78	0.80	0.95	0.62	0.86
County	Estimate	0.02	-0.02	0.02	-0.02	-0.05	0.29	0.02	-0.02	0.02	-0.02	0.02	-0.02
	P-value	0.98	0.85	0.98	0.85	0.83	0.77	0.97	0.89	0.97	0.76	0.98	0.87
	# placebos	48	48	48	48	48	48	65	65	29	29	47	47
	Preperiod RMSE	0.11	0.081	0.114	0.081	0.053	0.091	0.11	0.081	0.11	0.082	0.11	0.081
	RMSE percentile	0.13	0.29	0.10	0.29	0.33	0.19	0.06	0.20	0.17	0.24	0.11	0.28

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 population/enrollment growth to the baseline set of lagged outcomes. The second, "Fewer Years," uses only five years of lagged outcomes in the matching process rather than ten as in the baseline estimates. 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, "Restricted Placebos," does not include placebo effects in the calculation of p-values if the pretreatment RMSE is greater than 10 times the RMSE of the treated unit.

Figure A1: Population of Surrounding Counties

Figure A1a: Broward

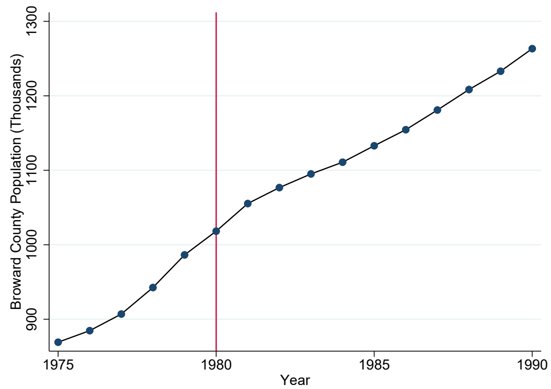


Figure A1b: Monroe

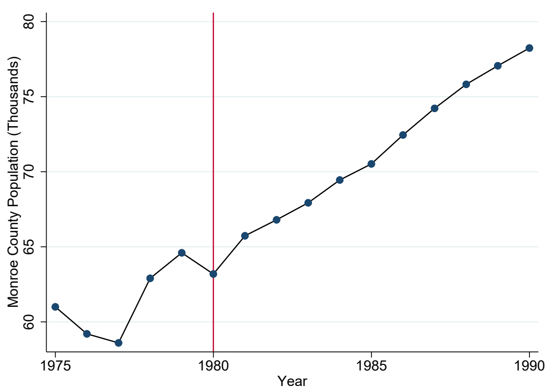
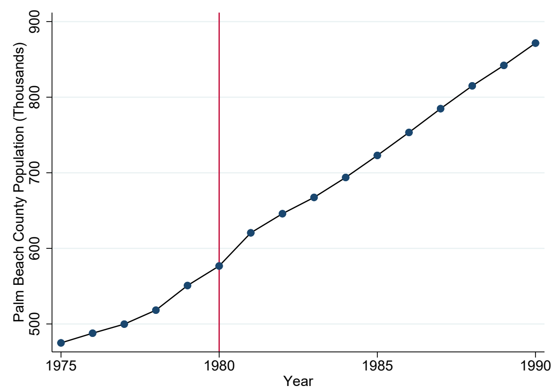


Figure A1c: Palm Beach



Note: Source: U.S. Census Bureau. The figures shows population counts during the years before and after the Boatlift for the counties that surround Miami-Dade County. Estimates are as of July 1. The Boatlift occurred between April and September of 1980.

Figure A2: Miami City - Permutation Tests

Figure A2a: Log Total Revenues

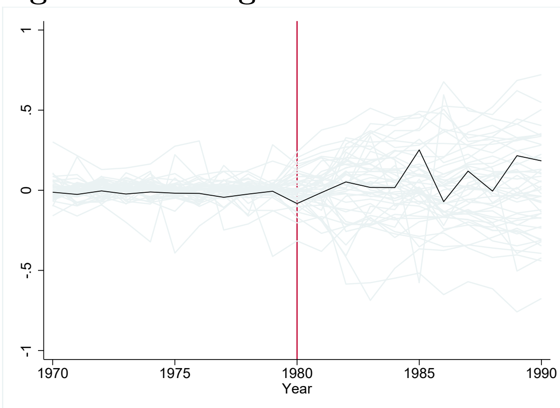
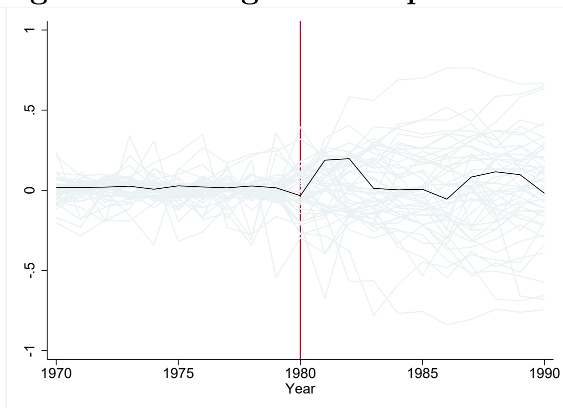


Figure A2b: Log Total Expenditures



Note: The figures plot the results of permutation tests of the significance of the difference between the City of Miami and its synthetic control. The solid, dark line plots the difference for the City of Miami. The light gray lines plot the difference using other cities.

Figure A3: Miami-Dade School District - Permutation Tests

Figure A3a: Log Total Revenues

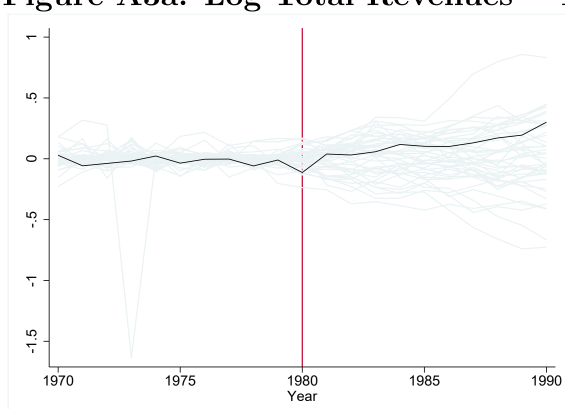
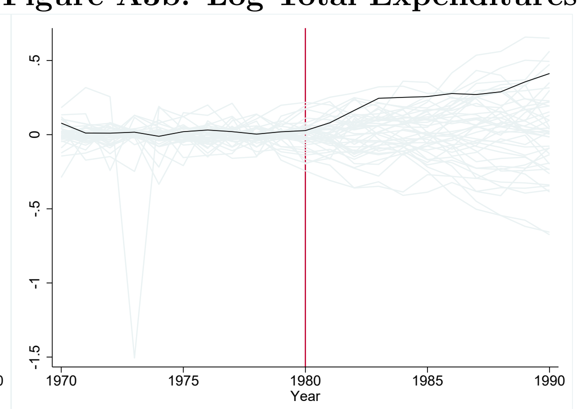
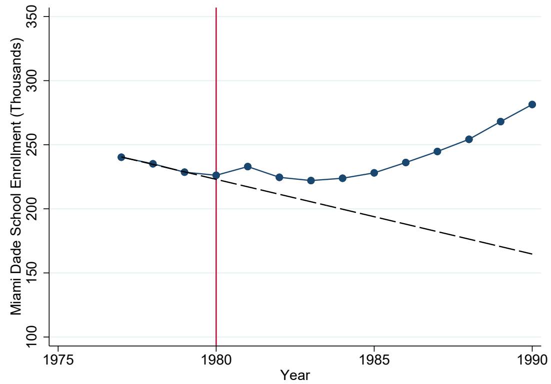


Figure A3b: Log Total Expenditures



Note: The figures plot the results of permutation tests of the significance of the difference between the Miami-Dade School District and its synthetic control. The solid, dark line plots the difference for the Miami-Dade School District. The light gray lines plot the difference using other school districts.

Figure A4: Miami-Dade School District Enrollment, 1977-1985



Note: Source: Miami-Dade School District. The figure shows enrollment figures for the Miami-Dade School District between 1977 and 1990. The dotted line is a linear projection based on 1977-1979.

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	0.437
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	0.015
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	0.085
Greenville County	0	St. Louis	0
Hillsborough County	0	Tucson	0
Houston	0.124	Tulsa	0
Indianapolis	0		0

Note: This paper's analysis looks at three 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 constructed on the basis of school district expenditures. 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	0.07	0.04	0.06	0.10	0.19
	P-value	0.32	0.19	0.19	0.40	0.28
	Number of placebos	47	47	47	47	47
	Preperiod RMSE	0.017	0.008	0.008	0.033	0.017
	RMSE percentile	0.49	0.83	0.85	0.64	0.62

Note: The table presents 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 descibed in Section 3.3 and are standardized based on pretreatment match quality. The root-mean square error (RMSE) is calculated using four years of pretreatment data, and the percentile is based on a comparison among all placebo estimates.

Table A3: Current Operating Expenditures in the School District

		Log Total Current Operating Expenditure
Miami-Dade Public Schools	Estimate	0.20
	P-value	0
	# of placebos	43
	Preperiod RMSE	0.014
	RMSE percentile	0.91

Note: The table presents 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	0.035
	P-value	0.22
	# of placebos	36
	Preperiod RMSE	0.00
	RMSE percentile	0.83
Miami-Dade Public Schools	Estimate	0.004
	P-value	0.78
	# of placebos	60
	Preperiod RMSE	0.019
	RMSE percentile	0.30

Note: The table presents 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.