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ABSTRACT

We apply the Synthetic Control Method to re-examine the labor market effects of the Mariel Boatlift, first studied by David Card (1990). This method improves on previous studies by choosing a control group of cities that best matches Miami's labor market trends pre-Boatlift and providing more reliable inference. Using a sample of non-Cuban high-school dropouts we find no significant difference in the wages of workers in Miami relative to its control after 1980. We also show that by focusing on small sub-samples and matching the control group on a short pre-1979 series, as done in Borjas (2017), one can find large wage differences between Miami and control because of large measurement error.

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1 Introduction

How do receiving countries absorb sudden waves of immigrants? What are their immediate effects on wages and employment? How long do these effects last? Sudden and unexpected refugee waves may provide a cleaner source of exogenous supply shocks, relative to the inflow of economic migrants which are more gradual, predictable and driven by local economic conditions.[1]

One such episode has held a special place in the minds of the American citizens and economists as an example of an unexpected and large refugee inflow. On April 20, 1980, Fidel Castro announced he would open the ports of Mariel, in Cuba, enabling anyone who wanted to leave the country to do so. Consequently, between April and September of the same year, almost 125,000 Cubans fled to the United States' shores in what is known as the Mariel Boatlift. The majority of them settled in Miami, increasing its labor force by about 8 percent. Because most of these immigrants had little schooling, the relative increase of workers with no high school degree was much larger (around 18 percent). This event provides a quasi-experimental environment to test theories about the effects of a sudden change in the supply of immigrants. If all other factors of the economy (e.g., technology, productivity, physical capital, etc.) remain fixed, this sudden inflow generates high potential for negative short-run impacts on wages and/or for displacement of existing workers.

An early study by David Card (1990) analyzed the Mariel Boatlift. His results showed that the impact on employment and wages of low-skilled non-Cubans in Miami was insignificant. This study became a prominent example of how the predictions of the simplistic canonical model of labor demand and labor supply do not work well in analyzing the consequences of immigration even in the short run.[2] From a methodological point of view, the

[1] Several studies have examined the impact of sudden inflows, often of refugees, in Europe. Examples include Hunt (1992), Carrington and De Lima (1994), Friedberg (2001) and Borjas and Monras (2016) among others.

[2] Other studies have suggested how different channels for absorbing the Mariel Cubans might have worked, rationalizing the results within richer models. Lewis (2004) showed that less skilled Cubans were absorbed by industries that chose more "unskilled-intensive" technology and less automation. In addition, Bodvarsson et al. (2008) argued that the immigrants increased significantly local demand for services, shifting the labor

design of Card's paper profoundly influenced the direction of research in labor economics (see Angrist and Pischke 2010). Card (1990) has represented the "word" on this episode for 25 years. However, his analysis has important econometric limitations that modern-day methods can significantly improve upon. First, when constructing standard errors Card (1990) did not take into account city-level idiosyncratic shocks to labor market outcomes. In other words, he treated data from all workers within a city over time as independent observations, accounting only for classical measurement error on wages. Second, the choice of a control group of cities for Miami, consisting of Los Angeles, Houston, Atlanta, and Tampa Bay-St. Petersburg, was somewhat arbitrary and ad-hoc. Its validity was never formally tested. Finally, he analyzed only a small set of labor market outcomes.

Given the historical importance of the Mariel Boatlift and of the Card (1990) study, one reason to revisit it is that, since then, we have improved our methodological toolbox. The Synthetic Control Method (SCM), an econometric technique developed and used in a series of papers by Alberto Abadie and coauthors (Abadie and Gardeazabal 20003; Abadie, Diamond and Heinmueller 2010, 2015), is better suited than the classic difference-in-differences methodology to address these type of case-studies. The main underlying idea is that a linear combination of labor markets is a better control group for Miami than any single one. The method, rather than arbitrarily choosing a single (or a group of) labor market as a control, identifies an "optimal" control group by minimizing the pre-1979 difference with Miami for a given set of relevant labor market characteristics. This creates a "Synthetic" city which serves as the control group.

This method has several advantages which address the shortcomings of the original Mariel Boatlift study. First, the formalized procedure reduces the "ad-hoc" nature of choosing the control group. Second, we validate its quality by checking the pre-treatment differences of the outcome variable (wages or unemployment) between the treated and the Synthetic Control units. Finally, by constructing a synthetic control for every available city we can

demand and not only labor supply.

obtain a distribution of observed effects. Then we can calculate a p-value for how significant the post-treatment difference is compared to the pre-treatment one for Miami relative to the whole distribution, thus conducting inference with idiosyncratic city-specific shocks.

Two relevant data sources are available, the May-ORG (Outgoing Rotating Group) and the March extracts from the Current Population Survey (CPS). We analyze the statistical power of both datasets and we conclude that the ORG-CPS is superior in measuring average weekly wage of subgroups in metropolitan areas. This is due to the much larger sample size, beginning in 1979, and hence smaller measurement error in reporting "point-in-time" weekly wages (May-ORG), as opposed to retrospective yearly wages (March-CPS). This is especially relevant for the main group of interest, low-skilled workers, who are often paid by the hour. Before 1979 both the March and the May CPS have small samples and large measurement error. Hence when selecting a control group we emphasize the importance of using a long pre-1979 sample, inclusive of as many years as possible. To further alleviate the measurement error problem, which is the main concern in this analysis, we include the largest group potentially affected by the competition of Mariel immigrants. Ideally one would want to study all low-skilled native workers in Miami at the time of arrival of the Mariel Cubans. A natural way to do this is to include all prime-age non-Cuban workers with no high school degree.[3] We also analyze heterogeneous responses by separating the main sample into subgroups of workers who could be affected differently due to their different skills and attachment to the labor market. The labor market outcomes we analyze are log wages (weekly and hourly) and unemployment rates.

Our results show no significant difference in the post-1979 labor market outcomes of high school dropouts between Miami and its Synthetic Control. Neither wages (annual, weekly or hourly) nor unemployment rates of high school dropouts differ significantly between Miami and its control group during the 1981-1983 period. The point estimate of the Miami-control

[3] Because several Hispanic non-Cubans could also be immigrants, an alternative is to include only non-Hispanic individuals. We therefore perform extensive analysis on sub-groups as well. Note that the CPS began reporting country of birth for the survey respondents only after 1994.

difference in log wages is actually positive after 1979 in most samples. We reach the same conclusion when we consider wages in the bottom 15th or 20th percentile of the distribution for all non-Cuban workers. We also run "difference-in-differences" type of regressions for Miami and the Synthetic Control and we provide the simulated significance level of the Synthetic control method estimates to formally show the lack of statistically significant differences. In essence our results confirm the early findings of Card (1990).

We then move on to reconcile our results with those presented in Borjas (2017). Using a restricted subsample of high school dropouts and the March-CPS[4], he finds a large and long lasting negative difference in wages between Miami and its control in the 1982-1985 period. We begin by reproducing his finding and then we show that these results hold only in small sub-groups when using the March-CPS dataset. They are strongest in the sub-sample obtained including only non-Hispanic men and selecting a short age range of prime-age workers (25-59 years old) among high school dropouts. We consider all possible 27 combinations of high school dropouts sub-groups along the gender (male, female and both), ethnicity, (non-Cuban Hispanic, non-Hispanic and all non-Cubans) and age (prime age, young-old, and all working age) dimensions and we show that smaller subgroups exhibit very large fluctuations of average weekly wages in Miami, around the long-run trend 1972-91. These yearly fluctuations are both positive and negative, occur over the whole period, and are not particularly large after 1979. Larger samples, instead, show much smaller variations away from the trend and no significant differences after 1979. We also highlight the importance of considering a long pre-1979 period to match the labor market trends in Miami. Given the small size of all samples before 1979, matching on only two years (1977-79) of data can be very risky and may result in selecting a control group which does not mimic the 1972-79 labor market behavior of Miami.

Finally, we address the criticism of Angrist and Krueger (1999) who show the risk of

[4]Perhaps due to the reasons outlined in this paper, virtually all previous studies analyzing labor market aspects of the Mariel Boatlift also choose the ORG-CPS. These include Card (1990), Angrist and Krueger (1999), Bodvarsson et al. (2008), Laing (2011), Borjas (2012), Cahuc et al. (2014), Monras (2015), and Doudchenko and Imbens (2016).

drawing inference from analyzing an event with a small number of treatment and control units. They imagine a "non-existent" Mariel Boatlift in 1994 and analyze it the same way Card (1990) did, erroneously finding a statistically significant increase in unemployment for black workers. They argue that in small samples differences may arise by pure chance. While we agree with their general point, we also show that the Synthetic Control analysis obviates the problem in this case. In fact we find that a well-constructed control group for Miami in 1994 shows no significant difference of relevant outcomes between Miami and control in the years after 1994.

More broadly, this paper contributes to the literature on the effects of immigrants on wages and labor market outcomes of less-educated native workers. Some recent papers analyzing US labor markets have found negative wage effects, as predicted by a simple canonical model (e.g., Borjas 2017, Monras 2015) while others have not (e.g., Card 2001, 2009, Ottaviano and Peri 2012). The latter have argued that several adjustment margins and some degree of complementarity between natives and immigrants reduce the wage impact of immigrants (Peri 2016). The results of this paper are in line with the second set of findings.

2 Data, Variables and Samples

Our preferred data source is the combination of the May extracts from the Current Population Survey (CPS) for years 1973 until 1978, with the Merged Outgoing Rotational Group (ORG) data from the same survey, starting from 1979 (the first year available). This sample allows us to construct a measure of average log weekly and hourly wages, unemployment rate, and wages at different percentiles of the distribution for workers in Miami and in 43 other metropolitan areas. The survey records information on the amount the respondents usually earned per week at their current job before deductions as well as the usual number of hours worked for the same period. We construct hourly wages by dividing the former by the latter. Alternatively, we also analyze wage outcomes using the March-

CPS data. This survey collects information on the respondent's total pre-tax salary and wage income for the previous calendar year as well as the number of weeks worked last year. We construct weekly wages by dividing the former by the latter.

To evaluate the impact of the sudden inflow of Cuban immigrants on the labor market outcomes of Miami workers, we should focus on studying the wage and employment of the group that was more likely to be affected by their competition and was working in Miami in 1980. Given that-as we will document below-a large majority of Mariel Cubans were unskilled and without a high school diploma, we analyze wage and labor market outcomes of working-age high school dropouts in Miami, excluding any Cuban individuals. To approximate this group and maintain the largest sample of potentially affected workers our preferred sample includes non-Cuban[5] high school dropouts, between 19 and 65 years of age, in the labor force, and not self-employed, with positive earnings and sample weight.

2.1 Sample Size and Measurement Error

Table 1 shows the number of observations for our preferred sample (all non-Cuban high school dropouts age 19-65) in Miami. Using the March-CPS, (first column) it includes between 60 and 80 observations per year. The May-CPS dataset, available between 1973 and 1978, is somewhat smaller, with about 40 observations per year. However, beginning in 1979 and spanning the years during and after the Mariel Boatlift, the ORG-CPS sample usually consists of around 150 observations per year. In Section 5 we discuss in detail how the small size of the March-CPS dataset affects the analysis of Borjas, (2017) who focuses on a small sub-group of the population of high school dropouts. On average, the size of Borjas' (2017) sample is between a quarter to a third of our preferred sample. He justifies the choice of such a restricted sample as measuring wages more precisely without the contamination from

[5] We want to focus only on US-born workers. However, the CPS began identifying birthplace only after 1994. One option is to use the information about "Hispanic" origin and only include non-Hispanics in the sample to exclude first generation Hispanic immigrants from previous waves. However this will also eliminate a potentially large group of US-born second generation Hispanics. Hence, in our preferred sample, we include Hispanic non-Cuban workers and then we provide robustness checks including only non-Hispanic workers.

trends in women's participation rates and transitions in early and late working life. However, those were national trends, unlikely to affect Miami in particular or to change abruptly after 1980. More importantly, an aggregate regression including ethnic, gender, and age controls can address those differences and, using city-specific residuals (as we will do below in our empirical methodology), the analysis should not be affected by those demographic trends.

A second important lesson from Table 1 is that the pre-1979 samples are especially small and likely noisy. This suggests that when matching Miami to a control group of cities, whose labor market trends were similar before 1979, one should include as many years as possible to reduce the impact of measurement error. Data for Miami and other 43 metropolitan areas exist since 1973, hence we choose that as the initial year of analysis.

Another argument in favor of relying on the May-ORG dataset is that the hourly and weekly wages figures are more accurate than the corresponding ones from the March CPS. The variation of measurement error in average wages across cities in the March-CPS sample is much larger than in the May-ORG CPS sample. One way to show this is to assume that, for year 1979, the "true" average city log wages for the group of high school dropouts can be calculated using the 5% sample of the 1980 Census. From the Census dataset, we calculate the log wage measure analogously to the March-CPS by dividing the respondent's total pre-tax wage and salary income for the past year by the number of weeks worked for the same period. First, we calculate average log weekly wages for our preferred sample in each of the 32 metropolitan areas available in all three datasets. Then, we calculate the difference of these average log wages in each metropolitan area, between the March-CPS and the Census and between the ORG-CPS and the Census. The measurement error in the March-CPS has a standard deviation of 0.12 logarithmic points (about 12%) while for the ORG-CPS it is equal to about half of that (0.07 log points, 7%).[6] Consequently, differences in average wage across cities within 15-20% will be impossible to separate from simple measurement error in the March-CPS.

[6] Assuming normal and independent measurement errors across cities, the mean absolute difference between measurement errors for two randomly chosen cities will be equal to $\approx 2/\sqrt{\pi} * Std.dev.$

A final reason to be skeptical about the use of the March-CPS in measuring weekly wages, especially of low-skilled individuals, was proved by Bollinger (1998) and Lemieux (2006). They showed that the March-CPS wage data, based on the recollection of previous year annual salary and weeks worked, compounds recall and division errors which are particularly severe for people who are paid by the hour; this includes a large fraction of the high school dropouts who are the main subject of this analysis. People often have a difficult time recalling the exact actual number of hours worked last year, a figure used in constructing weekly wages. The impact of this measurement error will certainly be magnified in small samples, such as metropolitan-area-level subgroups in the March-CPS. To the contrary, the May-ORG CPS sample, based on weekly wages recall from the last week of work, produces a less noisy and more reliable estimate of earnings for less educated individuals who are usually paid by the hour.

2.2 Number and Demographics of the Mariel Cubans

Table 2 shows the summary statistics as well as the aggregate count of immigrants and existing workers in Miami around the time of the Boatlift. In the first column, we present data relative to the labor force in Miami as of 1980 as measured in the Census. In the second one, we display the same characteristics for all Mariel Cubans identified in the 1990 Census as Cubans who arrived in the United States in 1980 and were 19 or older at arrival. In the last column, we do the same for Mariel Cubans who were still living in Miami as of 1990, and hence likely settled there permanently.

We obtain a count-total of 54,196 working-age Mariel Cubans, 56% of whom lacked a high school degree. Of those, only 62% were in still in Miami as of 1990. Hence, either at arrival or in the successive years, about two out of five Mariel Cubans relocated to other places. The share of Cubans in Miami, in fact, peaks in 1981 and then declines between 1981 and 1985. As we show below, there is additional evidence in the CPS data for Miami that some of the Cubans who arrived in 1980 might have left the city in the following 2-3 years.

Overall, the statistics of Table 2 imply that the Mariel Boatlift produced an 18% increase in the number of high school dropouts in the Miami labor market, while it increased the other education groups by only 5% and the total Miami labor force by 8.4%. The last three lines of Table 2 show that among dropouts, the Mariel Cubans had a comparable share of women and young individuals to those in the existing Miami labor force.[7]

2.3 Measured Labor Supply Shift in Miami

Figure 1 shows the share of Cubans in Miami's population, age 19-65 (black lines), and in the population with no high school degree, age 19-65 (lighter gray line), between 1973 and 1985. Panel A shows the March-CPS data, while Panel B uses the May-ORG CPS. In both panels we observe a clear jump upwards of the Cuban share from "1979-Pre" to "1981-Post", which is the mark of the Boatlift. In all figures, we adopt the convention to call "1979-Pre" the data relative to the last observation before the Mariel Boatlift and "1981-Post" the first data point certainly after all Mariel Cubans had arrived.[8] The 1980 data in the May-ORG are collected year round and include pre- and post-Boatlift information. Similarly for the March-CPS the wage data are yearly averages and in 1980 include pre and post. Hence, the reader should look at the difference 1979-1981 as the short-run impact of the Boatlift, with the idea that the 1980 data point is "during" the Boatlift.[9]

Three facts emerge from Figure 1. First, the March-CPS and May-ORG CPS data show

[7]The numbers presented in this Section are in accordance with previous studies including Card (1990) and Borjas (2017). As this supply shock took place over the course of a few months it was certainly exceptional. The most significant change analyzed in other "quasi-experiments" literature is the inflow of Russians to Israel (Friedberg 2001) which was equal to 12% of initial population but took place over 5 years (between 1989 and 1994).

[8]The former one is usually year 1979. For wages and unemployment in the March-CPS we use data collected in 1980, which is relative to the previous year. For the ORG-CPS we use data collected in year 1979. We also take the convention, in each figure, of showing a vertical bar exactly at the last pre-shock period (hence on "1979-Pre"). This notation helps to visually identify the last period of the status quo, right before the shock. To the immediate right of the bar we can see the impact of the sudden shock. To its left we can see the trend and variation during the pre-treatment period.

[9]Notice that as the demographic data for the March-CPS are relative to the month of March, the last pre-treatment observation is the one collected in March 1980, and it is called ("1979-Pre"), and it is differentiated from the 1979 (March-CPS 1979). The 1981-Post is the observation for March-CPS 1981, while "1980-shock" is simply the linear interpolation of 1979 and 1981. This is done only for this graph, due to the timing of the March-CPS enumeration, that in 1980 was just before the Boatlift.

similar shares of Cubans in the total population, but they are less consistent with each other for the share among high school dropouts. This indicates that there is significant noise and discrepancies between the May-ORG and the March-CPS statistics when the sample size is small. Second, both time series and samples show an increase between the "1979-Pre" and "1981-Post" data points. The 1979-1981 increase does not seem particularly large compared to the trends and year-to-year movements before and after. Considering May-ORG CPS figures, the 1979-1981 increase as a percentage of the population equals about six points and as a percentage of high school dropouts the increase was around 12 points.[10] Third, after initially increasing between 1979 and 1981, the share of Cubans decreased in the following four years (in both samples) and this effect was larger for the share among high school dropouts. In fact, in 1985 the share of Cubans among high school dropouts was back at percentage levels comparable to those of the pre-Boatlift period. This emphasizes the temporary nature of the shock.[11]

3 Empirical Methods

3.1 The Synthetic Control Method

The Synthetic Control Method (SCM), first introduced by Abadie and Gardeazabal (2003) and then further developed in Abadie et al. (2010, 2015), provides a systematic way of analyzing the impact of case-study events such as the Mariel Boatlift. Typically in these settings a single unit (often represented by a city or a region) experiences, at a point in time, an event (or treatment) while the rest do not. In order to evaluate whether the

[10] These figures are broadly in line with the ones obtained from the Census and described in Section 2.2.

[11] One can also show that the inflow of Cuban high school dropouts did not move the non-Cuban high school dropouts out of Miami. Figure A1 in the Online Appendix shows that in the period 1979-83, non-Cuban high school dropouts (dashed line) remained flat, as share of the Miami population, while Cuban dropouts as share of the population (dotted line) temporarily increased between 1979 and 1981, right after the Boatlift. Hence, the local supply of all high school dropouts (solid line) increased between 1979 and 1981 as a result of Mariel, with virtually no change in the non-Cuban dropouts, revealing no offsetting outmigration of natives.

treatment had an impact on some outcomes in the treated unit, relative to what would have happened in its absence, the method formally identifies a control group called the Synthetic Control unit.

In our case, we consider $J+1$ metropolitan areas indexed by $j = 0, 1, 2 \ldots J$ and denote Miami as 0 while we call the group of all the rest the "donor pool". This is the group of 43 other cities (31 for the March-CPS) in the United States for which data is consistently available in the Current Population Survey for the relevant time period. Then, we define a vector G_0 of dimension $k \times 1$ whose elements are equal to the values of variables that help predict the wages of high school dropouts in Miami between 1972 and 1979. Similarly, we define a $k \times J$ Matrix, G_J, in which row j is the sequence of values for the same variables and years relative to city j in the "donor pool".

The SCM then identifies the vector of non-negative weights $W^* = (w_1, \ldots, w_J)$ that produce a convex combination of variables in cities in the donor pool, G_J, to approximate as close as possible, in terms of a quadratic error, the pre-treatment vector of variables chosen for metropolitan area 0, G_0. In other words, it minimizes the difference between G_0 and $G_J W$:

$$W^* = \arg\min (G_0 - G_J W)' V (G_0 - G_J W) \quad \text{subject to} \quad \sum_{j=1}^{J} w_j = 1, w_j \geq 0 \tag{1}$$

Once we have identified W^* we can use it to calculate the post-treatment outcome variables for the Synthetic Control unit by weighting each city appropriately.[12] Comparing the pre-post 1979 change in the outcome variable for Miami relative to the pre-post change for the Synthetic Control is the basis to evaluate if the treatment has had any effect.

The Synthetic Control Method hence produces a figure with two time series, one for the treated unit (Miami) and one for the synthetic control. In Section 4.1 we show these two

[12] V is a $k \times k$ diagonal, positive-definite matrix that determines the weight for each element of the vector in the objective function. We use STATA's default option for the matrix V which is chosen among all diagonal and positive definite matrices to minimize the average squared prediction error of the outcome variable during the pre-shock period.

lines, for several wage and unemployment outcomes and we visually examine the post-1979 differences in order to assess whether they are large relative to the pre-event differences (1972-79).

We minimize the distance for the following variables in the pre-treatment period: the outcome variable itself for selected pre-1979 years (and/or its average value), the share of low-skilled workers, the share of Hispanics, and the share of manufacturing workers in the labor force. These are all important characteristics in predicting the labor market outcomes of low-skilled workers.[13] In robustness checks, we also match alternative variables such as overall employment, wages growth, and low skilled employment growth, which mirrors the analysis of Borjas (2017).

Given the noise and the small samples it is crucial to allow for as long a pre-treatment period as possible. The most influential studies using the Synthetic Control Method include more than 20 pre-event years (e.g., Abadie et al.2010, Abadie et al.2014). In our case as data for Miami begins in 1973 the best we can do is to include 6 pre-event years of data. Most results are robust to variations in the selection of the Synthetic Control Group, as long as one matches the whole 1973-1979 period. However, matching only on a very short pre-1979 period may produce control groups with very large pre-event differences between the treated unit and control, reducing the validity of the method.

3.2 Accounting for Differential Demographic Composition

It is well-known that the average wages of different demographic groups such as women, men, young, old, Hispanic, Black, and White workers had different national trends in the 1970s and 1980s. Depending on the demographic composition, these trends may affect labor markets differentially, introducing confounding factors in the analysis. The commonly used method for reducing the potential confounding effects from differential demographic characteristics (age, gender and ethnicity) is to adjust individual log wages by running the

[13]The share of Hispanic and pre-1979 employment are also among the variables used by Card (1990) to choose the control group.

following regression:

$$
\begin{aligned}
\ln w_{ijt} = \quad & \alpha + \beta_1 Age_{it} + \beta_2 Female_{it} + \beta_3 Ethnic_{it} + \beta_4 Year_t + \beta_5 \times Age_{it} \times Year_t \\
+ \quad & \beta_6 \times Female_{it} \times Year_t + \beta_7 \times Ethnic_{it} \times Year_t + \varepsilon_{ijt}
\end{aligned}
$$

The wage of individual i in metropolitan area j in year t is regressed on a set of five-year age dummies Age_{it}, on a female dummy, $Female_{it}$, dummies for Hispanic and Black, $Ethnic_{it}$, three-year bin dummies, $Year_t$, as well as the first three variables interacted with $Year_t$. This produces the residual, ε_{ijt}, that captures individual log wage variation once those aggregate trends are accounted for. We then average these residuals by city-year and treat them as outcome variables in several specifications of the empirical analysis.

3.3 Inference

A main drawback of the original analysis of the Mariel Boatlift by Card (1990) is neglecting aggregate city-specific idiosyncratic shocks in conducting statistical inference. This is problematic because it treats contemporaneous observations from different workers within the same labor market (and hence subject to the same labor demand shocks) as independent data while in fact they could be strongly correlated due to city-specific idiosyncratic shocks.[14] We address this issue within the context of the SCM in two ways.

3.3.1 Regression-based Standard Errors

One approach we undertake in this direction is to use classic regression analysis for the two time series, Miami and its control city. The main goal of this exercise is to quantify the noise in the data and give a sense of statistically significant difference. The pre-Boatlift differences Miami-Control in this regression will also provide a validity test for the comparison group

[14] For instance, in modern-day regression analysis it is common to apply a "cluster-robust" adjustment to the standard errors of the estimated coefficients to account for aggregate level shocks which are common to all observations within the "cluster" (e.g., Cameron and Miller 2015).

selected by the Synthetic Control Method. We estimate the following regression:

$$y_{it} = \alpha + \sum_{P \in PRE-79} \beta_P (D_P \times Miami_i) + \sum_{P \in POST-79} \beta_P (D_P \times Miami_i) + \sigma_c + \gamma_t + \varepsilon_{it} \quad (2)$$

The variable y_{it} is the outcome of interest (e.g., average log of weekly wages of high school dropouts) in unit i which is either Miami or its Synthetic Control. Next, σ_c and γ_t are city and year fixed effects. The variable $Miami_i$ is a dummy equal to one for Miami and zero for the Synthetic Control. Next, D_P is a set of 3-year dummies that span the whole period but omit 1979, which is absorbed in the constant and hence serves as a reference year. In the pre-1979 period the dummies are D_{73-75} and D_{76-78} in the post-1979 period they are $D_{81-82}, D_{83-85}, D_{86-88}$ and D_{89-91} and they equal one in the years indicated in the subscript and zero otherwise. We drop the 1980 observation as it is "during the Boatlift" and it is hard to classify it as either "before" or "after". Next, β_P is the set of coefficients associated to the interaction between the city dummy $Miami_i$ and the time period dummies. The term ε_{it} is the classical city-level error term, uncorrelated with the observables. The main coefficient of interest in this regression is β_{81-82} which captures the wage difference Miami-Control right after the Boatlift.

The method of estimation is Feasible Generalized Least Squares (FGLS) allowing the errors to be autocorrelated in an AR(1) process. The city weights comprising the synthetic control are estimated in a first step, there is dependence among observations across units, the sample size is small and hence the limiting distribution of the estimated coefficients is unclear. Therefore, one has to be careful in doing inference on the estimated coefficients.[15] We report the estimated standard errors and comment on their size relative to the coefficients in section 4.3 but we do not attach specific significance levels.

[15] It would be likely more accurate to bootstrap the standard errors. However, to maintain comparability with the estimates of Borjas (2017), who uses this method, we calculate robust standard errors.

3.3.2 Placebo Permutations

An alternative and more accurate way of doing inference with the Synthetic Control Method proposed by Abadie et al. (2010) is based on permutations. The core idea is to simulate a distribution of differences between each city in the donor pool and its Synthetic Control and examine whether Miami shows a post-1979 difference from its Synthetic Control, relative to its pre-1979 difference, that is large vis-a-vis the whole distribution. We apply this method graphically showing the treatment-control differences for all cities and we also provide the p-value of a test of significance for the outcome (wages or unemployment) in Miami.

3.4 Others Methodological Considerations

The Mariel Boatlift was a one-time, unexpected shift in supply that took place mostly between March and July 1980. It was not a persistent policy change or a prolonged increase in immigration rates from Cuba. As shown in Figure 1, the share of Cubans among high school dropouts in Miami was back to pre-Boatlift levels by 1985. Hence the bulk of the effect on employment and/or wages should be detected in years 1981 and 1982. After that, only transitional dynamics would be detected and should imply smaller effect than in 1981-82 as the shock was temporary. Moreover, several other economic shocks (including a significant recession and a worsening of the war on drugs in 1982 which actively and differentially involved Miami) took place during the following decade and Miami could have responded differently from any chosen control group. Differences between Miami and the control city after year 1983 are likely caused by factors unrelated to the Boatlift. Consequently, any study of a labor market impact of the Mariel Boatlift should focus on the years 1981 and 1982.

4 Empirical Estimates

4.1 Main Results

We show our main results in Figures 2 and 3. In each panel we present two time series lines, one for Miami (solid) and one for its synthetic control (dashed). To learn about the possible impact of the Boatlift, our focus will be on two features of the graphs. First, we assess the magnitude of the differences between Miami and the control between 1972 and 1979. This indicates how well our control group matches Miami pre-Boatlift. Second, relative to those, we eyeball the magnitude of the difference in the outcomes in the 1980-1983 period, when the Boatlift should have had its largest potential effect. [16] In Panels A-C of Figure 2 and all Panels of Figure 3, we present various wage measures, and in Panel D of Figure 2 we show the unemployment rate for low skilled workers.

We do not observe a drop in wages in Miami relative to its control in the 1980-1983 period in any of the Panels in Figures 2 and 3. Panels A and B of Figure 2 show (log) weekly wage and hourly wages, respectively. One may argue that the second measure is closer to capturing the marginal productivity (and hence price) of labor. However, we follow previous studies and consider weekly wages as our main variable of interest. These two panels show a reasonable, but noisy, Miami-Control match for the pre-1979 period, and a small positive difference between 1980 and 1983 for Miami relative to its control. This difference is magnified in the longer run, after 1985. Panel C shows the path for the 15th (log) wage percentile of non-Cuban workers in Miami and its control. An advantage of using the wage percentile, relative to looking at the average wage of a small group (such as the high school dropouts), is that the sample used in estimating the statistic is larger. Hence, the results are less sensitive to extreme values and therefore less volatile. Confirming this, Panel

[16] The footnotes to each figure indicate the sample and the cities which enter the Synthetic Control and their associated weights. Notice that in every Panel the Synthetic Control is constructed (at least partly) from cities not included in Card (1990)'s control group (i.e., Los Angeles, Houston, Atlanta, Tampa Bay-St. Petersburg). By construction we improve on his identification strategy because we match similar variables as the ones he based his choice of control group and we make no arbitrary decision regarding which cities enter the control.

C exhibits a better Miami-control match pre-1979 and also show small positive differences between 1980-83.

A possible explanation for the small wage effects is that wages were rigid in the years 1979-1982 and hence a negative demand shock for native workers did not translate into lower wages in Miami.[17] In the presence of a supply shock and rigid wages, the result would be displacement and non-employment of natives. If this explanation is correct we would expect the inflow of Mariel Cubans to be associated with an increase in the unemployment rate of non-Cuban high school dropouts in Miami. Panel D of Figure 2 shows the unemployment rate of this group for Miami and the Synthetic Control. In the years after the shock, between 1980 and 1983, no significant difference between the unemployment rate in Miami and Synthetic Control arises. A caveat in this case is that the year-to-year volatility of unemployment in Miami before 1979 was quite large.

Next, in Figure 3, Panel A we show the behavior of the 20th (log) weekly wage percentile in Miami and Synthetic Control, which is another measure of wage among less skilled workers in Miami. In Panels B and C we present the results for the regression-adjusted weekly and hourly wages, respectively (as described in section 3.2). Any spurious behavior driven by national trends in the wages of women or Hispanics/Black or Young/Old workers affecting the previous figures is controlled for. All three panels show very small differences in the outcomes for 1980-83 and a reasonably good pre-1979 match. Finally, in Panel D of Figure 3, we show the log weekly wage graph for high school dropouts, as in Panel A of Figure 2, using the control group of cities as used in Card (1990) (Los Angeles, Houston, Atlanta, and Tampa Bay-St. Petersburg). Again, no systematic difference between Miami and Control is discernible for the period 1980-83. We also see that the control group chosen by Card (1990) produces a reasonably good pre-1972 match of low-skilled wages.

The key takeaway from Figures 2 and 3 is that the average labor market outcomes of low-wage workers and high school dropouts in Miami do not show any negative break or

[17] As inflation in the early 1980s was high, nominal wages were rather flexible. It is unlikely, therefore, that the rigidity of real wages is a significant concern in this setting.

jump in correspondence of the Mariel Boatlift relative to the control group. The pre-1972 match between the constructed control group and Miami is quite noisy and hence one has to take these results with a grain of salt. There is, however, no unusually large difference in any outcome for Miami and Control during the 1980-83 period, corresponding to the aftermath of the Boatlift.

4.2 Robustness Checks

4.2.1 Subsamples

In this section we begin exploring the sensitivity of the results to the selection of subsamples. A more systematic analysis of how choosing small subsamples affects the results is carried out in Section 5. By restricting the focus to sub-samples of high school dropouts we face the risk of introducing measurement error as the sample size drops. However, when we consider the May-ORG sample, provided we do not restrict on too many dimensions, we still have samples with more than 50 observations in Miami. Hence, even in the absence of evidence of an aggregate effect on high school dropouts, it can be interesting to separate across various demographic sub-groups to check whether there is evidence of heterogeneous effects.

It is worth noting that, while the Mariel Cubans were disproportionately high school dropouts, this group was rather similar to the corresponding native group in term of its age and gender composition. Hence there is no clear reason why the Mariel Cubans should compete more closely in the labor market with one of these sub-groups. Still, one may think that their impact was different for men than for women, for incumbent Hispanic (whose culture and language was similar) or Black workers. This may be the case if, for instance, the Mariel Cubans specialized in occupations that were in direct competition with males and with Hispanics. Figure 4 shows first the Synthetic Control analysis, when separating the labor force by gender between non-Hispanic Men (Panel A) and Women (Panel B). The outcome is log weekly wages of high school dropouts. While the time profiles of the wages of the two groups are different, and the year-to-year variation is large, we do not observe negative differences between Miami and the Control between 1980 and 1983 in either group.

Next, one may think that the non-Cuban Hispanics in Miami were likely to be prior immigrants and they should be separated when evaluating the impact of Mariel Cubans. In Panels C and D of Figure 4 we perform the analysis for Hispanic and African-American workers. This second group could be particularly exposed to the new immigrant competition as they may be more similar in terms of skills and occupational choices. Even in these cases, Miami and the control exhibit small differences (positive in the case of Hispanics) for wages between 1980 and 1983 and similar, albeit noisy, trends of average log wages in the 1970s. Overall, selecting the sample on one dimension (ethnic or gender) at a time does not produce evidence of a negative post-1979 difference in wages between Miami and Control.

4.2.2 Different Control Groups

In the Panels of Figures 2-4 we have chosen the synthetic control so as to best match a common set of variables and the outcome pre-1980. This results in a somewhat different synthetic control group for each Panel. We now show that small variations in the choice of the control group do not change the visual impression obtained from those figures. In Figure 5 we present the same outcomes as in Figure 2, but now comparing Miami to a fixed set of control cities (New York, Nassau-Suffolk, New Orleans and Tampa St. Petersburg) which is the set of cities most frequently selected by the Synthetic control method (for both the ORG-CPS and the March-CPS) when matching share of high school dropouts, share of Hispanics, share of manufacturing, plus outcome variables in the 1972-79 period. The figures show, as expected, a worse match of the pre-1980 trend on average, but they confirm the result of no negative difference (rather a positive one) between Miami and Control log wages between 1980 and 1983. Similarly, only small differences in unemployment rates are observed between Miami and this fixed control group between 1980 and 1983 and they are comparable with those pre-1979. Although the choice of the control group is important, as one needs to approximate as closely as possible the pre-1979 trends in labor market outcomes, Figure 5 shows that the results are not sensitive to small differences in the choice of the best matching set for each variable. Nevertheless, as we will see in Section 5 below, one can be misled when selecting a control group that only matches a very short pre-event period (such as 1977-79), as short-run idiosyncratic noise may be masking the pre-event trend behavior of labor market variables. It is always important, as a check of validity to look at the 1972-79 match of the outcome variables between Miami and the Synthetic Control.

4.2.3 Using March-CPS data

In Section 2 we argued that the small size and the measurement and recall error in the March-CPS may introduce significant noise in the average wages in Miami. Nevertheless, in this section, we show how the synthetic control analysis performs when we use our sample in the March-CPS. A systematic analysis of the consequences of using small sub-samples of high school dropouts in the March-CPS data to calculate the average wages will be performed in section 5 below.

In Figure 6, we simply show the Miami and Control comparison for some outcomes as we did in Figures 2 and 3, but now measured in the March-CPS. The sample consists of non-Cuban high school dropouts in the 19-65 age range. We observe similar patterns as the ones from ORG-CPS. Panel A represents log weekly wages and shows a negative difference for Miami relative to control in 1981-1982, no difference in 1983, and then a quite large difference in 1986. While the difference in 1981-82 seems larger than in pre-1979, it seems also driven by an odd increase of average wage in the control, as much as by a decrease of average wage in Miami. Panels B and C show that such difference is not visible in the 15th and 20th wage percentile time series. Moreover, even in Panel A a simple continuation of the pre-1979 trend for Miami would show little difference with the actual wages in Miami up to 1985. Some more substantial differences between Miami and Control appear in Panels A, B, and C after 1985, five years after the Boatlift. The unemployment rate in Panel D shows no post-1979 difference between Miami and Control. We can observe, however, sizable differences in Panel D before 1979, which imply that the noise in the data prevent a precise pre-1979 match for the unemployment rate. In sum, the preponderance of evidence in Figures 2-5 does not show differences in the labor market outcomes of less skilled incumbent workers of Miami relative to the synthetic control during the post-Mariel period.

4.3 Regression Results

In Table 3 we show the β_P coefficients from regression (2) with their respective standard errors . If the Mariel shock had any labor market effect, this should be captured primarily by the coefficient β_{81-82} that shows the average difference between Miami and Synthetic Control arising in 1981 and 1982 once the 1979 difference is set to zero, as the reference year, and we exclude the 1980 observation.[18] Just as importantly, our framework allows us to quantify the pre-1979 differences between the two cities. Specifically, the estimates of β_{73-75} and β_{76-78} provide validation for how well the two cities track each other before the shock. Statistically significant pre-1979 differences would cast doubt on our control group as they will imply systematic differences between the Miami and control, even before the Boatlift. The subsequent coefficients $\beta_{83-85}, \beta_{86-88}$ and β_{89-91} complete the picture.

Each column in Table 3 corresponds to an outcome variable/specification. The header indicates the Panel and Figure corresponding as well as the outcome variable in the regression. Some consistent features of the estimates are worth pointing out. First, none of the β_{81-82} coefficients for the wage variables is negative and larger than its standard error. Most point estimates (Columns 2-5) are positive and they reveal that Miami had a small positive difference relative to its Synthetic Control after the Boatlift. Given the estimated coefficients and standard errors, there is no support for a significant negative effect. For the unemployment rate (column (6)) we estimate a positive coefficient β_{81-82} after the Boatlift However this value is not very different from the estimates of β_{73-75} and β_{76-78}. This suggest similar

[18]These choices are consistent with Borjas (2017).

behavior of dropouts' unemployment before and after 1980 in Miami and Control. Second, the estimated coefficients β_{76-78} in Columns (2)-(5) are, for the most part, smaller than their standard errors. This is a more formal way of checking that Miami and its Synthetic Control move together to a reasonable extent, in the pre-Boatlift period, validating our identification strategy. The pre-1979 coefficients are smaller for the percentile wages (column 3 and 4), implying that for those variables the pre-1979 Miami-Control fit is better. Note that the estimates are simply a quantification of the differences between the Miami-Control time series represented in Figures 2 and 3 standardizing the difference to 0 the one in 1979, (which the graphs do not necessarily do). Third , the standard errors for the wage regressions are not small (between 0.03 and 0.04 log points) and differences in the order of few percentage points would be difficult to measure precisely. Year-to-year fluctuations of 5-6 per cent seem common both before and after 1979 and it is hard to say whether that are actually due to the Boatlift or measurement error.

4.4 Inference Results

Panels A-D of Figure 7 show the simulated differences treatment-control for Miami and the other 43 cities, analyzing non-Cuban high school dropouts log weekly and hourly wages (Panels A and B), log wages at the 15th percentile of wage distribution (Panel C) and unemployment rate of non-Cuban high school dropouts (Panel D). The dark line in each Panel corresponds to Miami-Control differences, while each of the lighter ones corresponds to one of the 43 cities' difference from their Synthetic Control.[19] Panels A and B show that Miami's average wage had a positive difference relative to its synthetic control, and large vis-a-vis the simulated range, in 1981-1982 while its differences look within the range of idiosyncratic variation after that year. Panels C and D show that for the 15th wage percentile and for the unemployment rate, Miami is rather average and within the range of simulated differences any year post-1979. Notice that the range of simulated idiosyncratic

[19] Note that when simulating the Synthetic control method with other cities we do not include Miami in the donor pool. This avoids contaminating the control group if there is any effect of the Boatlift in Miami.

noise in the sample is large. For instance, log weekly and hourly wages show a range of noise spanning the interval between -20% and +20%. Let us emphasize once again that with this degree of noise it may be hard to identify effects on the order of five or six percentage points.[20]

5 Reconciling Our Results with Borjas (2017)

Readers who are familiar with Borjas (2017) are likely puzzled by the disagreement of the results presented here and those contained in that paper. In this section we bridge the differences between the two. First, we show that the measurement error for average wages of sub-samples in the March-CPS, as chosen by Borjas (2017), can be very large. Specifically, we show that the key result of Borjas (2017) arises when focusing on the small sub-group of male, non-Hispanic, 25-59 years of age, as representative of all native high school dropouts in Miami. By embedding this sample in the 27 possible alternative samples obtained partitioning age, gender and ethnicity (each in two groups and including all possible combinations) we observe the magnitude of the fluctuations of average wages across subgroups and the number of observations in each of those subsamples. Small samples, such as the one chosen by Borjas (2017), display large fluctuations in all periods (not just post-1979) and have the markings of measurement error rather than the consequence of any specific event.

Even more importantly, when we embed Borjas' (2017) sample in the 1972-91 period, rather than starting in 1977, we show the strong negative pre-1979 trend of low-skilled wages in Miami. This leads us to discuss the importance of choosing an appropriate control

[20] In Appendix Table A1 we show test statistics based on the simulations reported in Figure 7. We first calculate the Pre-Post ratio in the average absolute difference of Miami from its control, considering 1980-82 as the post-period and, alternatively, either the 1972-79 (upper Panel) or the more recent 1977-79 interval (lower Panel) as the Pre-period. This procedure adjusts the post-period differences for the idiosyncratic deviations experienced in the pre-1979 period. We then do the same for all other 43 cities in the sample. In the table we show the rank of Miami in the distribution of 44 cities and the probability that a random city in the distribution has a statistics larger than Miami (i.e., p-value). Along the way we implement the correction technique of Ferman and Pinto (2015) who derive conditions under which inference in Synthetic Control corrects for heteroskedasticity. A low value of the rank and a value of the p-statistics higher than 0.10 indicates that Miami-Control differences are not unusual relative to the other cities. The results are in accordance with the placebo simulation graphs.

group with labor market trends more similar to Miami over the whole 1972-79 period, rather than only in 1977-79. Once a longer pre-trend is introduced and less noisy samples are considered, there is no evidence of a post-1979 drop of Miami wages from the pre-existing trend.

5.1 The Figures

5.1.1 March-CPS Sample

Panels A and B of Figure 8 show a simple way of visualizing the key finding in Borjas (2017) and reconciling it with our and Card's (1990) results. The red line shows the average log weekly wage of high school dropouts in Miami from the March-CPS sample that includes male, non-Hispanic workers, between 25 and 59 years old. In Panel A it reproduces exactly the Miami wage path reported in Figure 6 Panel A in Borjas (2017) with two small modifications. First, we do not "smooth" the data with a moving average. The goal of the paper is to find a sharp short-run change in wages right around year 1980. Therefore, we do not want to contaminate the data by averaging observations before and after the Mariel Boatlift. Second, we draw the vertical bar in 1979, the last observation before the Boatlift. The red line in Panel A shows the very large dip in wages, which occurs slowly after 1980. This is the fundamental feature of the data that drives Borjas (2017) to argue in favor of a large effect of the Boatlift. The pre-1979 line is too short to infer the pre-1979 behavior (1977-79) of wages. However, the fact that one observation (1977) is above and the other one (1978) below the 1979 point produces an impression of no clear (or rather flat) wage trend before 1979.

Panel B reproduces the exact same wage path as Panel A, as a thick red line, but now it embeds it into more information about the average wages of high school dropouts in Miami before and after the Boatlift. Specifically, we extend the sample back to 1972, and we add the paths of average log wages of all the comparable sub-groups among workers with no high school degree in Miami, standardized to be equal in 1979. Namely, we consider partitions of gender into male and female, of ethnicity into Hispanic-non-Cuban and non-Hispanic, and of age into Prime (age 25-59) and Marginal (age 19-24 and 59-64). Then we consider all 27 possible subgroups within the population of dropout workers in Miami that one can obtain selecting all possible combinations of individuals in one or both of the subgroups for each of the three characteristics.[21] We plot the average weekly log-wage of each of these sub-groups using light shades of gray for the subgroups with 20 observations or fewer, with mid-gray shades those with 20 to 40 observations, and with dark gray those with 40 or more observations. The only lines in different colors are Borjas' (2017) sample, in red, and our preferred sample in blue. Borjas' (2017) sample belongs to the sub-groups with less than 20 observations (light gray). Our sample, by including both groups for each of the three characteristics, is the largest one and belongs to the sub-groups with 40 plus observations (dark gray). Finally, with a thick black dashed line, we denote the linear pre-1979 trend for the wage of the largest (and least noisy) dropout group extended to 1991.

Three clear facts emerge from Panel B of Figure 8. First, all subgroups with fewer than 40 observations (mid-gray and light -gray) and particularly those with 20 observations or less (light gray plus the red Borjas' (2017) sample) show very large fluctuations, before and after 1979, above and below the average trend of the group. The great dispersion and year-to-year variability of those averages seem to derive from measurement error, as they are more extreme the smaller those groups are and are largest in 1984 and 1985, when the Miami March-CPS sample is smallest (see Table 1). The larger groups, including our sample, have much smaller year to year variation and follow a downward trend with much smaller fluctuations around it. Borjas' (2017) sample happens to be one with a large negative difference relative to the trend both before and after 1979, especially in the 1985-86 period, and then a dramatic reversal with a positive difference in 1989-91. Other small subgroups have similarly large differences, but some of them are positive in the 1985-86 period. The Hispanic women group,

[21] Table A2 in the Online Appendix shows the number of observations for each of those sub-groups in Miami (averaging the yearly observation between 1978 and 1982) in the March (first column) and in the May-ORG CPS (second column) samples.

for instance, is the one with a large double-spiked positive jump in 1983 and 1985. Let us also emphasize that the small sample bias problem, when considering subsamples, applies also to the control cities, not just to Miami. In the Borjas (2017) sample some of the cities in the control group (San Diego and San Jose) have samples with fewer than 15 observations between 1978 and 1982.

Second, Panel B of Figure 8 shows that the average wage of high school dropouts in Miami was on a significant downward trend from 1972 to 1979. Importantly, in order to capture such a trend, especially for the small and noisy subgroups, it is crucial to consider the whole 1972-79 period. Limiting one's attention to the behavior of Miami's labor markets in the 1977-79 period, as Borjas (2017) does when selecting his control groups, implies that short-term fluctuations and measurement error can obscure the actual longer-run trend.

Third, if one can identify a group of 2-3 years in which the differences of the wage of some sub-groups, above or below the average preexisting trend, are largest, those would be the years 1984-1986. Some sharp spikes (both upwards and downwards) emerge in those years. The years 1980-82, which should show the largest short-run effect of the Mariel Boatlift, show instead smaller differences especially if one excludes the very small subgroups. The years 1984-86 were those when the Miami sample of the March-CPS was smallest (fewer than 60 including all high school dropouts) and hence larger variation may simply come from smaller samples[22].

In summary, the drop in wages for male, non-Hispanic, 25-59 years old in March-CPS relative to its pre-1979 trend, which constitutes Borjas' (2017) key piece of evidence, appears, when embedded in a complete picture of all subgroups, as a likely manifestation of measurement error that affects the smaller sub-groups[23].

[22]Clemens and Hunt (2017) suggest that a change in the March CPS sampling data in 1980, to include a larger fraction of black Haitians in Miami, can be one of the sources of measurement error in small subgroups.

[23]Roodman (2015) had already noted the sensitivity of Borjas' results to the choice of sub-sample when separately analyzing men and women.

5.1.2 May-ORG Sample

Panels C and D of Figure 8 perform the same reconciliation between Borjas' (2017) key result and our analysis using the May-ORG data mirroring Panels A and B. Both graphs show very clearly that the Borjas' (2017) figure creates an impression of a downward dip after 1979. Extending it to include the 1972-79 data shows that wages for the Borjas' (2017) sample continued along the pre-1979 trend with only small differences from it. With the May-ORG data the year-to-year fluctuations of the wages for the sub-samples are much smaller than in the March-CPS as their sample size are larger (notice the difference in the range of the vertical log scale). In particular, only few sub-samples show large negative or positive wage differences from the pre-1979 trend. Ours and Borjas' (2017) sub-samples look much closer to each other. In fact, in Panel D the average wage for the Borjas' (2017) sample is pretty close to its pre-1979 trend. This is clearly visible even in Figure 6 Panel B in Borjas (2017). In it, if we align the time series for Miami and the "Card-" or "Employment-" or the "Low Skill-" placebo at 1979, we do not detect any noticeable difference between Miami and the placebos post-1979, up to 1985. The only exception is what Borjas calls the "Synthetic Placebo" obtained matching variables in the 1977-79 period and that looks diverging from Miami in the post-1980 period. We will discuss this placebo and its consequences in the next two sections.

5.2 Choice of the control group

The remaining differences between our's and Borjas' (2017) results, which are relevant when looking at the regression analysis particularly for the May-ORG sample, derive from the choice of the control group. Borjas (2017) matches either overall or low-skilled employment growth, or these along with low skilled wage growth between Miami and Controls during the 1977-79 period. Namely, he pools the 1977-78 data and the 1979-80 data and estimates the growth between these two data points. The four cities most similar to Miami are then chosen as controls. The year-to-year variability of wage data shown above, however, should warn us of the risk of basing the match only on the two years before the event.

Table 4 illustrates the sensitivity of the relative performance of Miami in terms of employment growth with respect to the choice of the period: 1977-78 to 1979-80, a one-year-earlier comparison 1976-77 to 1978-79, or the whole 1972-79 period. The first three columns of Table 4 rank cities according to those three different employment growth rates. Borjas (2017) insists that Miami had robust labor market performance before 1979. However this is only true when using the 1977-78 to 1979-80 growth (first column of Table 4, Miami is 9th out of 32 cities). The one-year-earlier rate (second column of Table 4) shows much slower growth of Miami (19th out of 32 cities) and the overall 1972-79 rate shows Miami as one of the slower performers (25th out of 32). Notice also that the four closest cities to Miami (underlined in Table 4) are different depending on which period we consider. A control group based on only 2 years before 1979 risks selecting cities that do not match at all the Miami labor market trends before 1979. In addition, Miami was the worst performer in the 1972-79 period among all the available 32 cities in terms of average wage growth (last column of the Table 4). Hence, when considering the full 1972-79 trend, Miami seems to show slow employment growth, slow overall wage growth, and declining dropout wages.

The Synthetic Control method, matching variables during the 1972-79 period, captures a group of cities with similarly slow labor markets. The control group in Borjas (2017), based on two years only, selects cities that are very different from Miami when the whole 1972-79 performance is considered. The highlighted in gray cities (Anaheim, San Diego and San Jose) appear both in Borjas' (2017) Synthetic and Employment placebos.[24] However, they experience really strong labor market indicator growth according to all measures except for the one used in his paper. It is then not surprising that wages in Miami are on a lower trajectory post-1980, relative to those.

To show that this matters in the May-ORG analysis, Figure 9 shows the log hourly wages (Panel A) and the log weekly wages (Panel B) for Miami using Borjas' (2017) sample (black

[24] The other cities comprising these placebos are Rochester and Nassau-Suffolk but the CPS does not identify these cities consistently back to 1972 and hence they are excluded from this table.

line). We also present the behavior of our synthetic control, based on matching variables in the whole 1972-79 period (blue line), and Borjas' (2017) synthetic control based on matching variables in the 1977-79 period (red line). While our control matches reasonably well the pre-1979 trend and fluctuations of the Miami wages, Borjas' (2017) synthetic control shows very large pre-1979 differences with Miami, especially in 1976 and 1977. These differences continue after 1979 and the Borjas (2017) control group deviates upwards from the continued downward trend that both Miami and our control, once aligned at the 1979 value, show after 1979. Therefore, matching only on the 1977-79 period tends to select cities whose labor market outcomes and wages do not match properly the negative 1972-79 Miami trend before the event.

5.3 The Regressions

In Table 5, we use the insight gained from the comparisons above to reconcile the regression estimates in Table 5 of Borjas (2017), showing large negative coefficients for the (Miami)x(81-83) dummy with our null or positive coefficient shown in Table 2 above. The table shows the estimates of the Miami-period dummies for the March-CPS sample (upper panel of the Table) and the May-ORG (lower panel). The first column shows our replication of exactly the regressions performed by Borjas estimating differences of Miami dropouts wages from the synthetic control in the post-1980 sub-periods. We include initially, in column (1), exactly the Borjas sample and specification including the moving-average smoothing, the omission of year 1980 from the regression and the choice of control group (which we select to be his synthetic control group). The estimate of column (1) reproduces the large negative estimate for the 1981-83 coefficient (and following years) with relatively small standard errors. We focus on the (81-83) coefficient as this would be the most likely to be affected by the Boatlift. The estimate of column (1) is -0.327 (s.e. 0.068) close to Borjas (2017) result of Table 6, Panel A, column 4 (which equal -0.257, with s.e. 0.077). Then, in Column (2) we extend the data back to 1972 and we include dummies to capture differences between Miami and Control in the pre-1979 period, relative to 1979. This establishes 1979 as a reference year and allows us to evaluate whether there were large Miami-Control differences before the Boatlift, which is a diagnostic measure on the quality of the control group. The ('81-'83) coefficient becomes -0.247 (s.e. 0.052) and we estimate a large positive coefficient with a small standard error on ('72-'75), denoting a quite imprecise match of the pre-79 trend. Then, in column (3), we remove the moving-average smoothing of the data. This further reduces the estimate of the ('81-'83) coefficient to -0.147, and most importantly increases significantly the standard error to 0.103. The data are now much more variable year-to-year and this is reflected by the standard errors. The moving-average smoothing was artificially reducing those errors by smoothing the year-to-year variation of the data. In column (4), we introduce our sample including male and female, non-Cuban workers age 19-65 and, in Column (5), we introduce our synthetic control group, based on 1972-79 matching of variables. In both cases the estimates of the ('81-'83) coefficient are smaller than in column (3) and the pre-79 coefficients are much smaller and within their standard error. The final specification of Column (5) shows a point estimate of -0.11 for the '81-'83 coefficient and a standard error of 0.068. While likely the standard error is underestimated as we are not accounting for the fact that the variables are constructed, it is clear that such coefficient would not be significant at standard confidence levels.

The lower part of the table shows the same progression for the estimates obtained from the May-ORG data. Column (1) reproduces the result of Table 6 Panel B Column 4 of Borjas (2017) obtaining a point estimate of -0.200 (s.e. 0.029) on the '81-'83 coefficient. Then, we see a progressively smaller negative estimate when including the pre-trend coefficients (column 2) and dropping the moving-average smoothing (column 3). These specifications also show the large pre-1979 differences between Miami and the control captured by the large point estimates of the dummies. Afterward, using May-ORG we see that the point estimate of the (81-83) coefficient become positive in column (4) and (5). Finally, the last coefficient is similar to column (1) of Table 3 (the differences are due to a slightly different definition of the time dummies). To emphasize the improvement from the introduction of our control, notice that in Column (5) the estimates on both the pre-1979 coefficients is small and non-significant. The final specification shows a positive difference Miami-Control between 1981 and 1983 of about 4.9 percent with a standard error of 4.8 percent.

6 The Boatlift That Did Not Happen

A subsequent potential flow of Cubans in 1994, announced by Castro but eventually diverted to the naval base in Guantanamo Bay, provided an opportunity for a falsification of the Card's (1990) results . Angrist and Krueger (1999) show that between 1993 (pre-non-shock) and 1995 (post-non-shock) the unemployment rate for Black workers in Miami increased by 3.6 percentage points, while in the control group of cities in Card (1990) it decreased by 2.7 percentage points. Hence, if a researcher were to analyze the impacts of this *non-event*, she would estimate a fake treatment effect of +6.3 percentage points. They argue that this "false positive" is a cautionary tale when utilizing a very small number of units in case studies like this one, where differences can occur by pure chance.[25]

We are sympathetic to this methodological caveat and illustrate that the Synthetic Control Method can significantly reduce this problem. By construction, the SCM eliminates the arbitrary choice of a control group (as done by Angrist and Krueger (1999), who simply use Card's (1990) control group) and allows validation of the newly-constructed one by checking the fit in the pre-1994 period. Similarly to the results presented in Figures 2, 3, and 4, we apply the Synthetic Control Method using high school dropouts' weekly and hourly wages as well as weekly and hourly wages at the 15th percentile of the respective distribution. We

[25] Angrist and Krueger's (1999) argument is somewhat crude and it is simply a cautionary tale. Any serious researcher will at least dig into validating the parallel trends of Miami and the control group assumptions. Reassurance that the control group and Miami had similar labor markets in the mid 1990s (e.g., in terms of occupational structure, demographics, etc.) is also necessary for the credibility of this strategy. Importantly, let us also point out that in 1994 the CPS underwent a major redesign and several measures of employment, especially for males and subgroups were significantly affected (see Polivka and Miller, 1995). Hence focusing on changes exactly around 1994 can be very risky.

use the ORG-CPS dataset and our preferred sample of workers.[26]

Figure 10 shows the results. Panels A and B show the behavior of hourly and weekly wages of high school dropouts in Miami and Synthetic Control between 1989 and 2001. The rest of the Panels do the same for the 15th percentile of the natives' hourly (Panel C) or weekly (Panel D) wage distribution. We include a vertical line on the year 1993, the last year before this non-shock. The solid line shows Miami and the dashed one is the constructed control labor market. The footnote lists all the cities with positive weights in the SCM control group. In all graphs of Figure 10, we see a good fit between Miami and its respective control labor market in the years leading up to 1993 and no significant difference immediately afterwards. Overall, these figures do not produce any false evidence of a downward wage movement. Looking at them, we recognize significant noise in the data but we do not to identify erroneous signs of an effect on wage and employment from the non-existent 1994 Boatlift.[27]

7 Conclusions

In this paper we apply the Synthetic Control Method to the well-known Mariel Boatlift episode with the goal of improving on Card's (1990) methods. We analyze a wide variety of labor market outcomes for high school dropouts and for low-wage non-Cubans in Miami, as well as for several sub-groups. We look for a significant and sudden difference of Miami labor markets' outcomes from those of its Synthetic Control in 1981-1983, as a potential evidence of an effect of the Boatlift on local labor markets. We do not find any consistent evidence of

[26] In this case, to keep computational time within a reasonable amount, we limit the "donor pool" for the control group to cities with at least 20 observations in the relevant group of high school dropouts per year. This produces a pool of about 40 cities. Moreover, in order to have a balanced panel of control cities we keep the pre-1994 period to six years only. Regression analysis of the presented time series and placebo simulations (not shown here) further confirm the obtained results.

[27] We show the behavior of the unemployment rate of minorities (Black and Hispanics) vis-a-vis the Synthetic Control in Figure A2 in the Online Appendix. While the unemployment rate of Black workers still shows an increase relative to the Synthetic Control in 1994 and 1995, that difference is less dramatic and it is reversed by 1996. The unemployment of Hispanic individuals experienced actually a decline relative to the Synthetic Control in 1994-1995.

a short-run depressing effect on low-skilled labor demand nor any lasting effect later on. The contribution of this paper is to put the Card (1990) estimates on sounder ground, showing their robustness and plausibility. Moreover, in the light of recent criticism by Borjas (2017) who finds, instead, a large and delayed wage effect, we show that choosing small sub-samples and matching Miami with a control group based only on two years of pre-Boatlift history can be misleading.

The lack of a significant wage effect, while in part attributable to measurement error which contributes to the noise of average wage data, is also consistent with the recent literature emphasizing mechanisms that allow absorption of immigrants. These may take place through complementarity, technology adjustment, increases in demand, and efficiency. We also show that, when dealing with small samples, one needs to analyze varying samples, outcome variables, control groups, and perform of an extensive set of robustness checks. Our analysis exposes the fragility of the results and criticism by Borjas (2017) and Angrist and Krueger (1999). We find that their claims are predicated on narrow choices.

In conclusion, we think that a reasonable re-assessment of the labor market effects of the Mariel Cubans (and the findings of Card (1990)) which accounts for idiosyncratic and measurement error cannot rule out small wage effects in the order of 3-4%. However, the point estimates in most of the samples using the May-ORG data are slightly positive and do not suggest any negative impact. Certainly, there is no consistent evidence of large negative effects such as the ones presented in Borjas (2017), although some specific sub-samples or specifications may generate such an impression.

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Tables and Figures

Table 1: Number of Observations, High School Dropouts Sample in Miami

Year	Non-Cuban Dropouts 19-65, March-CPS	Non-Cuban Dropouts 19-65, May-ORG
1973	70	42
1974	69	32
1975	66	41
1976	62	43
1977	64	39
1978	61	37
1979-Pre	62	145
1980-Shock	68	161
1981-Post	72	145
1982	62	135
1983	59	149
1984	55	145
1985	55	72
1986	61	183
1987	66	221
1988	86	222
1989	96	222
1990	74	250
1991	72	175

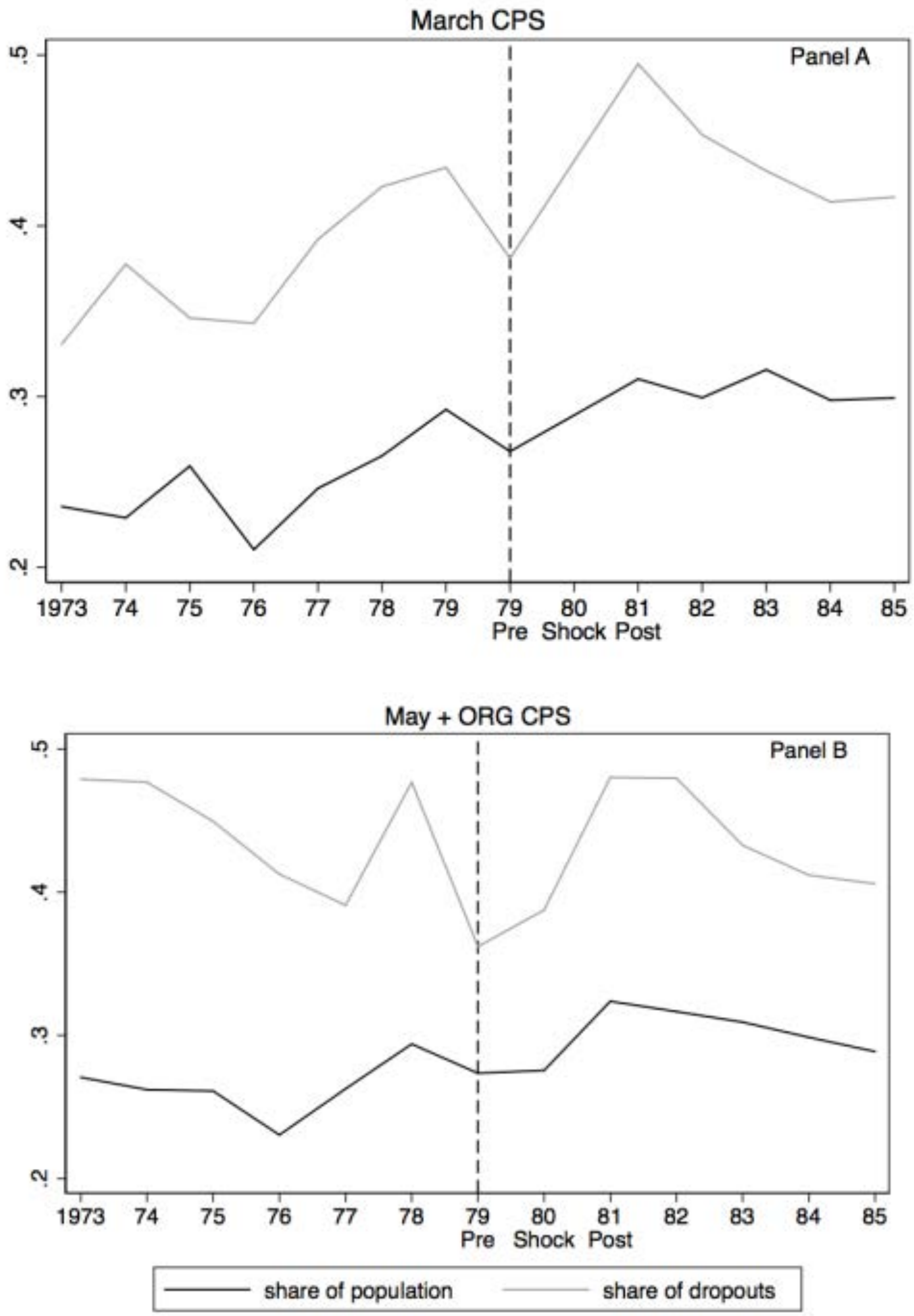
Notes: Our sample includes individuals with no high school degree, non-Cuban, with positive earnings, not self-employed, in the labor force and in the age range 19-65. A one year adjustment is made to the March CPS numbers as previous year earnings are reported.

Table 2: Demographics of Mariel Immigrants and of Existing Labor Force in Miami, 1980

	Miami Labor Force in 1980	Mariel immigrants, measured from the 1990 Census	Mariel Immigrants still in Miami as of 1990
Total in Labor Force (16 to 65 years of age)	644, 860	87, 347	54,196
Share with no HS degree	26.28	55.77	56.34
Share with HS degree	32.11	25.18	24.22
Share with some college	22.37	12.53	12.47
Share with college	19.24	6.52	6.97
Share of female	45.79	37.80	41.97
Share of young (<25 years old)	16.35	16.34	14.01
Only individuals with no High School degree			
Total in labor force	169,440	48,714	30,532
Percentage female	43.33	39.79	44.41
Percentage young (<25 years old)	11.36	12.90	10.24

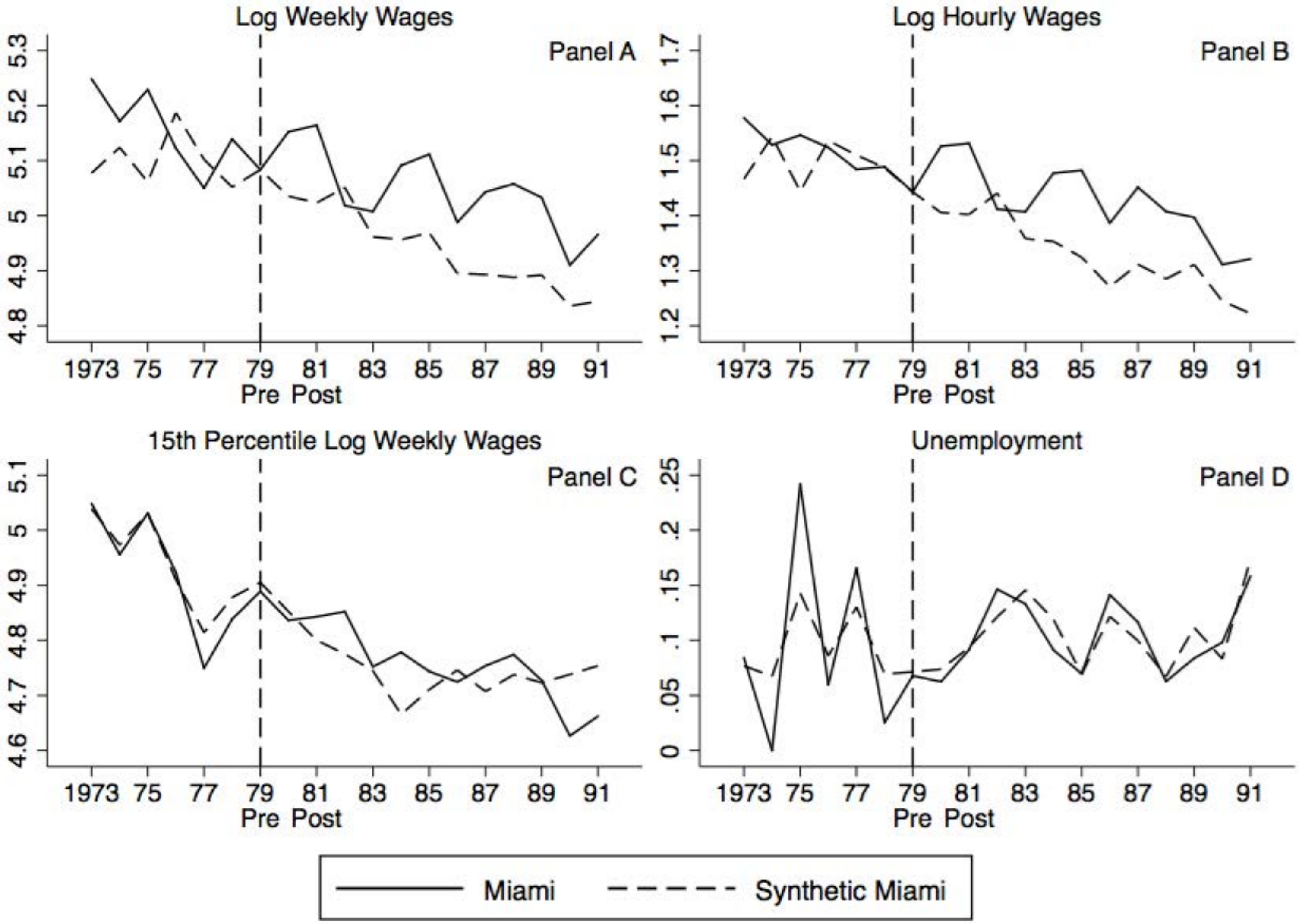
Notes: The values for the Miami Labor force are obtained from the 1980 census. Those on the Mariel Immigrants are obtained from the 1990 census as people born in Cuba who arrived in the US in 1980 and 1981 and were at least 19 years of age at the time of arrival. Labor force is defined as individual 19-65, not in school, and working or looking for a job.

Figure 1: Cubans in Miami as a Share of Total and of High School Dropout Population



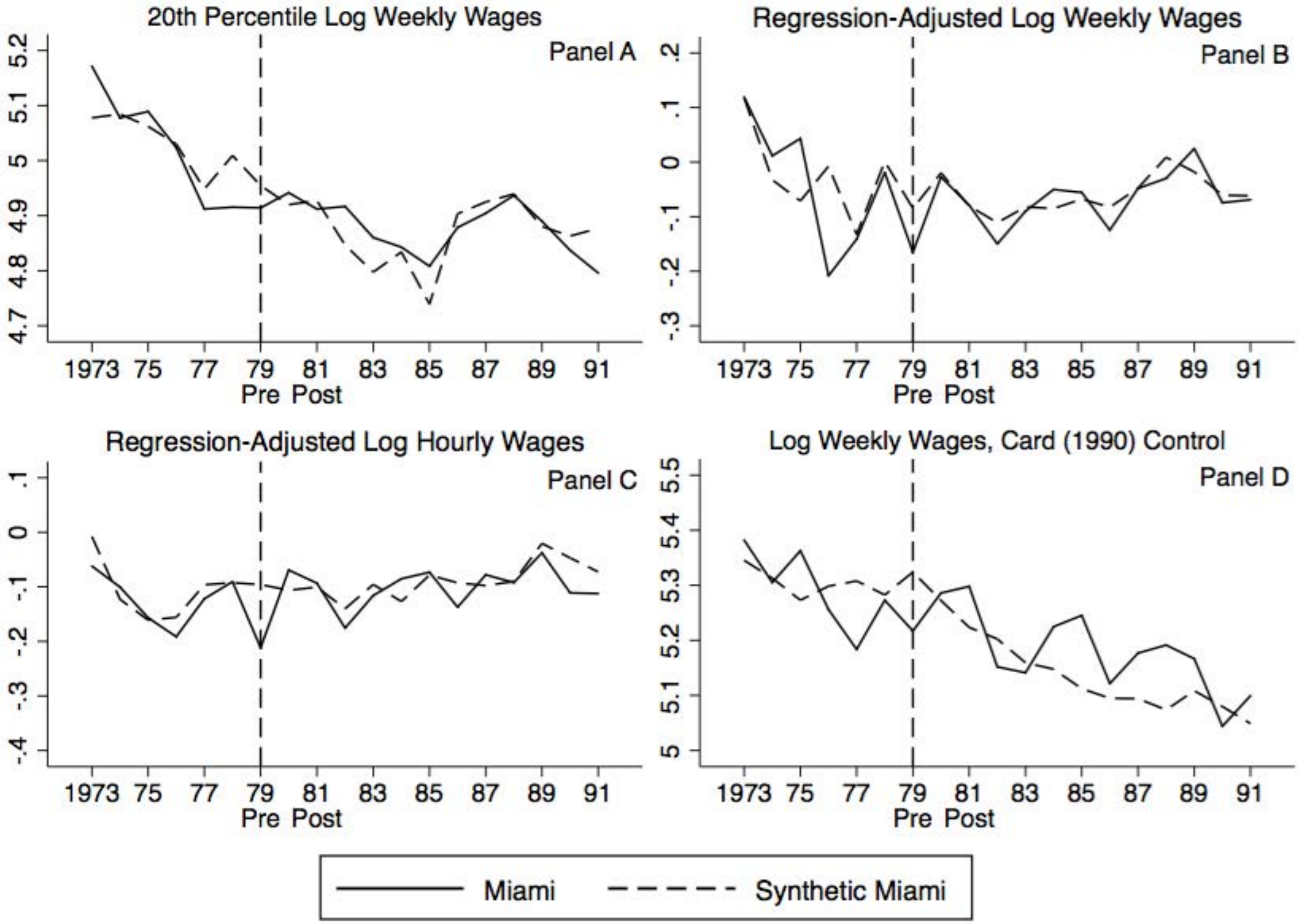
Notes: We calculate the share of all those who define themselves as "Cuban" in the ethnicity question of the CPS. The population considered is the total number of individuals between 19 and 65. The high school dropout population is constituted by those who do not have a high school degree in the age range 19 to 65. For the March CPS, we include the figure for March 1980 as "1979-Pre" and we interpolate the figure for "1980-Shock", between 1979-Pre and 1981-Post. The vertical dashed bar corresponds to the last observation before the Mariel Boatlift happened.

Figure 2: Miami and Synthetic Control, Wages and Unemployment of Low Skilled



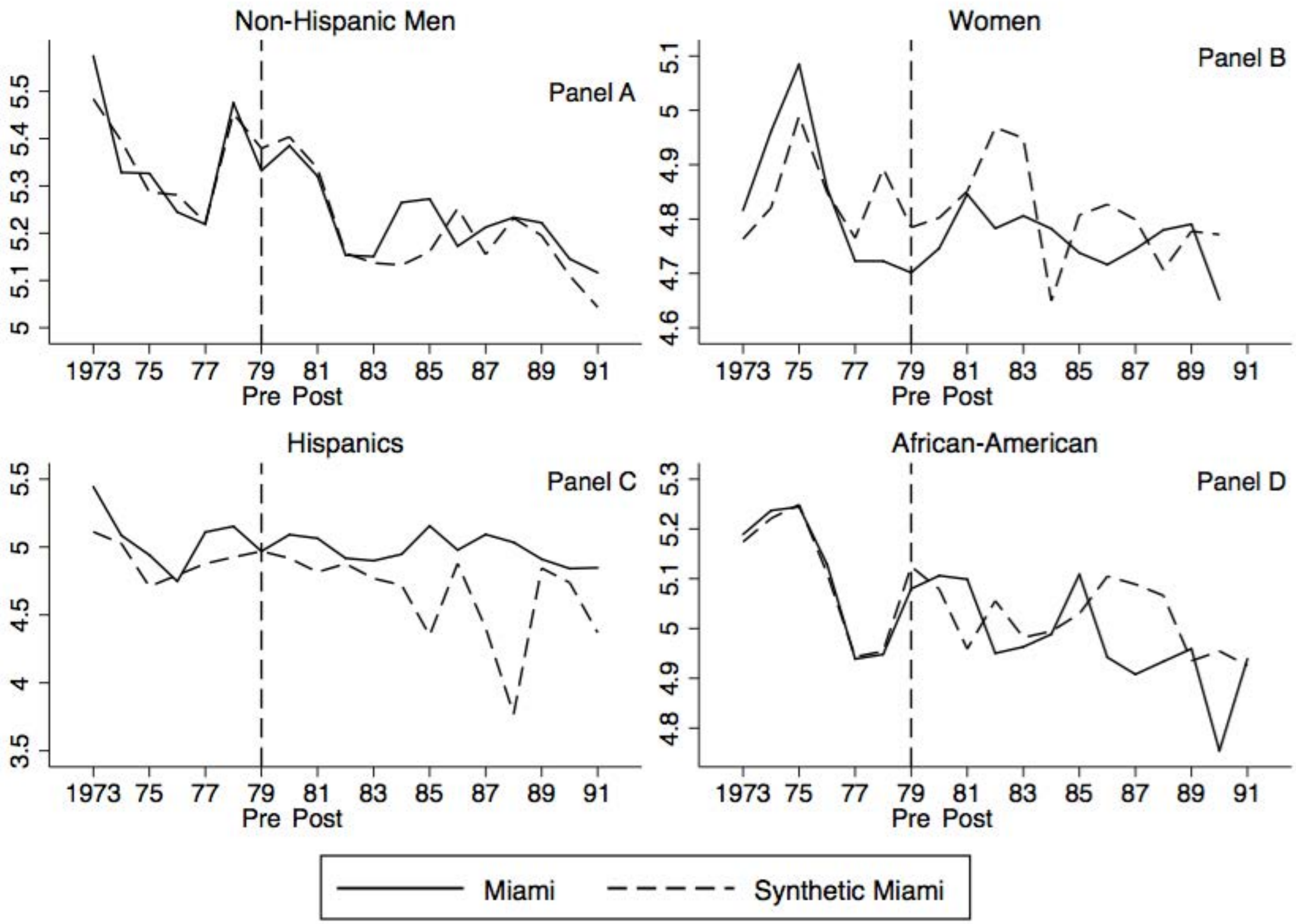
Notes: The data source is May-ORG CPS. Each Panel shows the outcome variable for Miami (solid line) and Synthetic control (dashed line) in the period 1972-1991. The outcome variable is noted in the title of each panel. The sample is non-Cubans, not self-employed individuals, in the labor force, age 19-65 for Panels A, B and D and non-Cubans, not self-employed, in the labor force, age 19-65 for Panel C. Some time series are standardized to the same value in 1979. The cities with positive weight in the synthetic control are as follows. Panel A: New Orleans, LA 43.9%, New York City, NY, 29.9%, Baltimore, MD 24.8%; Panel B: New Orleans, LA 43.2%, New York City, NY, 30.1%, Baltimore, MD 24.9%; Panel C: Birmingham, AL 60.6%, Rochester, NY 28.6%, Nassau-Suffolk, NY 10.4%; Panel D: New Orleans, LA 48.4%, New York City, NY 30.9%, Albany-Schenectady-Troy, NY 19.5% Cincinnati, OH 1.1%;

Figure 3: Miami and Synthetic Control, Additional Outcomes of High School Dropouts



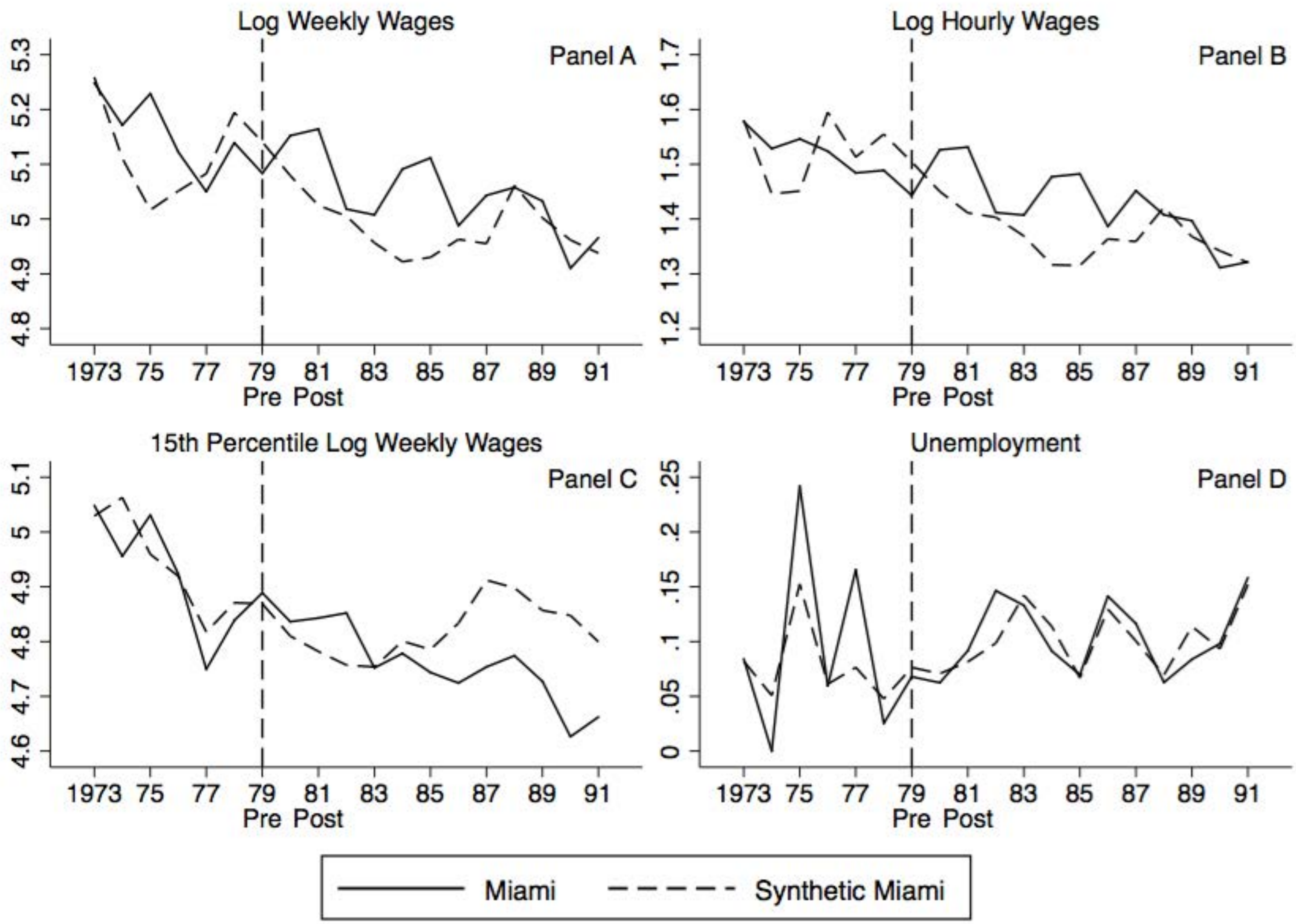
Notes: The data source is May-ORG CPS. Each Panel shows the outcome variable for Miami (solid line) and Synthetic control (dashed line) in the period 1972-1991. The outcome variable is noted in the title of each panel. The sample is non-Cubans, not self-employed individuals, in the labor force, age 19-65. See Section 3.2 in the text for an explanation of the regression adjustment. The cities with positive weight in the synthetic control are as follows. Panel A: San Diego, CA 57.7%, Birmingham, AL 28.4%, Nassau-Suffolk, NY 13.8%; Panel B: Tampa-St. Petersburg, FL 64.6%, Anaheim, CA 25.2%; Panel C: Greensboro, NC 70.5%, Norfold-Portsmouth, VA 18.4%, Cincinnati, OH 7.3, Buffalo, NY 3.8%. Panel D: Los Angeles, CA, Tampa Bay-St. Petersburg, FL; Houston, TX, Anaheim, CA.

Figure 4: Miami and Synthetic Control, Sub-samples of High School Dropouts



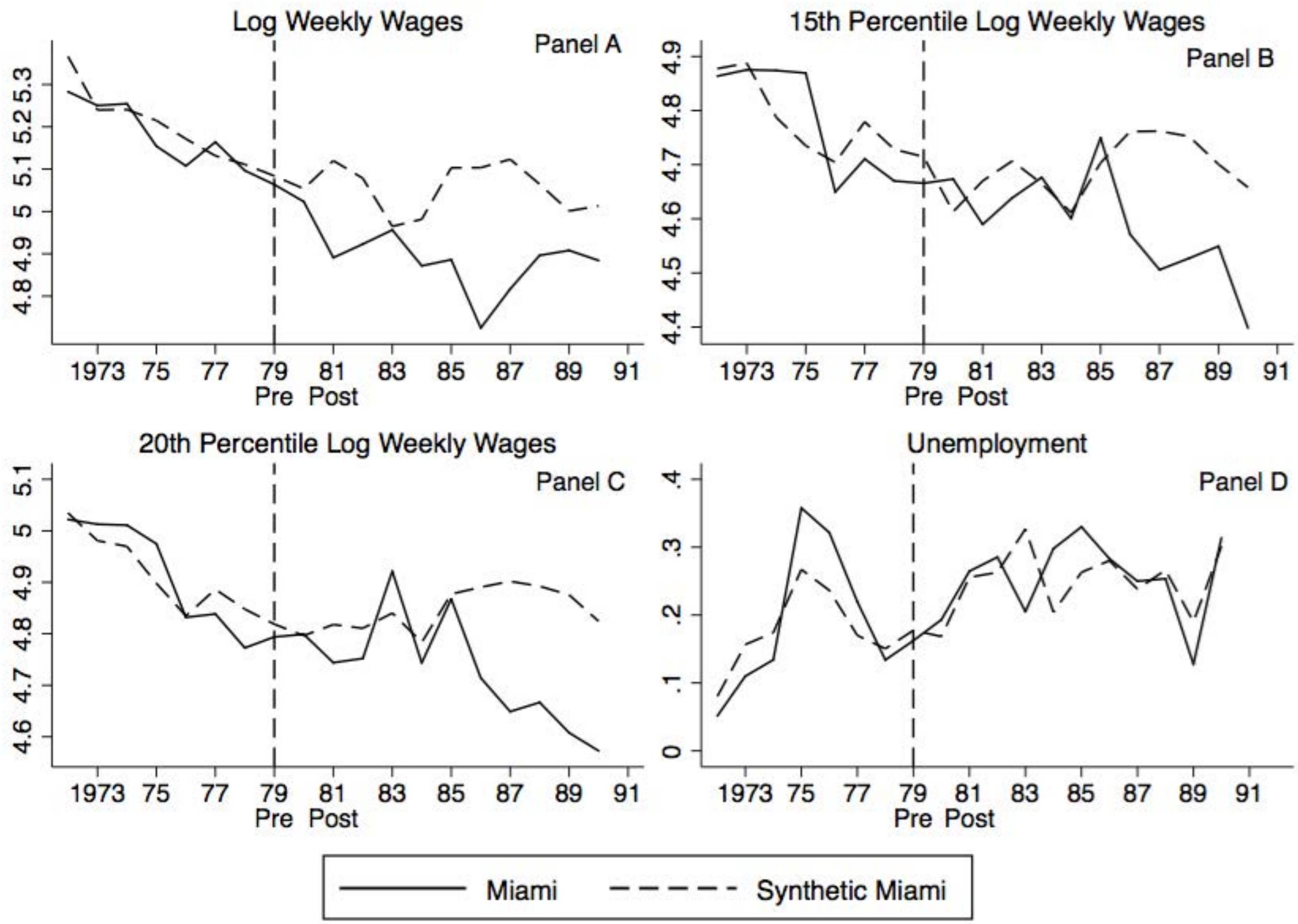
Notes: The data source is May + ORG CPS. Each Panel shows the outcome variable for Miami (solid line) and Synthetic control (dashed line) in the period 1972-1991. The outcome variable is noted in the title of each panel. The sample is a separate subgroup of not self-employed individuals, in the labor force, age 19-65. Panel A and B restrict the sample to males and females only, respectively. Panel C and Panel D further restrict it to Hispanics and Blacks only. Some time series are standardized to the same value in 1979. The cities with positive weight in the synthetic control are as follows. Panel A: Tampa-St Petersburg, FL 92.6%, Greensboro, SC 6.3%, New York City, NY 1.1%; Panel B: Cincinnati, OH 66.8 %, Pittsburgh, PA 29.4%, Indianapolis, IN 3.8%; Panel C: Sacramento, CA 49.8%, Houston, TX 30.1%, Philadelphia, PA 20.1%; Panel D: Greensboro, NC 39.6%, Cincinnati, OH 19.8%, New York City, NY 15.8%, Seattle, WA, 9.7%, Birmingham, AL, 8.5%, New Orleans, LA 6.7%.

Figure 5: Miami and Fixed Control, Wages and Unemployment of High School Dropouts



Notes: The data source is May + ORG CPS. Each Panel shows the outcome variable for Miami (solid line) and Synthetic control (dashed line) in the period 1972-1991. The outcome variable is noted in the title of each panel. The sample is a non-Cubans, not self-employed individuals, in the labor force, age 19-65. The set of available cities in the control group is restricted to New York, Nassau-Suffolk, New Orleans and Tampa St. Petersburg and the synthetic control method attaches different weights of these on the different panels.

Figure 6: Miami and Synthetic Control, Wages and Unemployment of Low Skilled, March CPS Sample



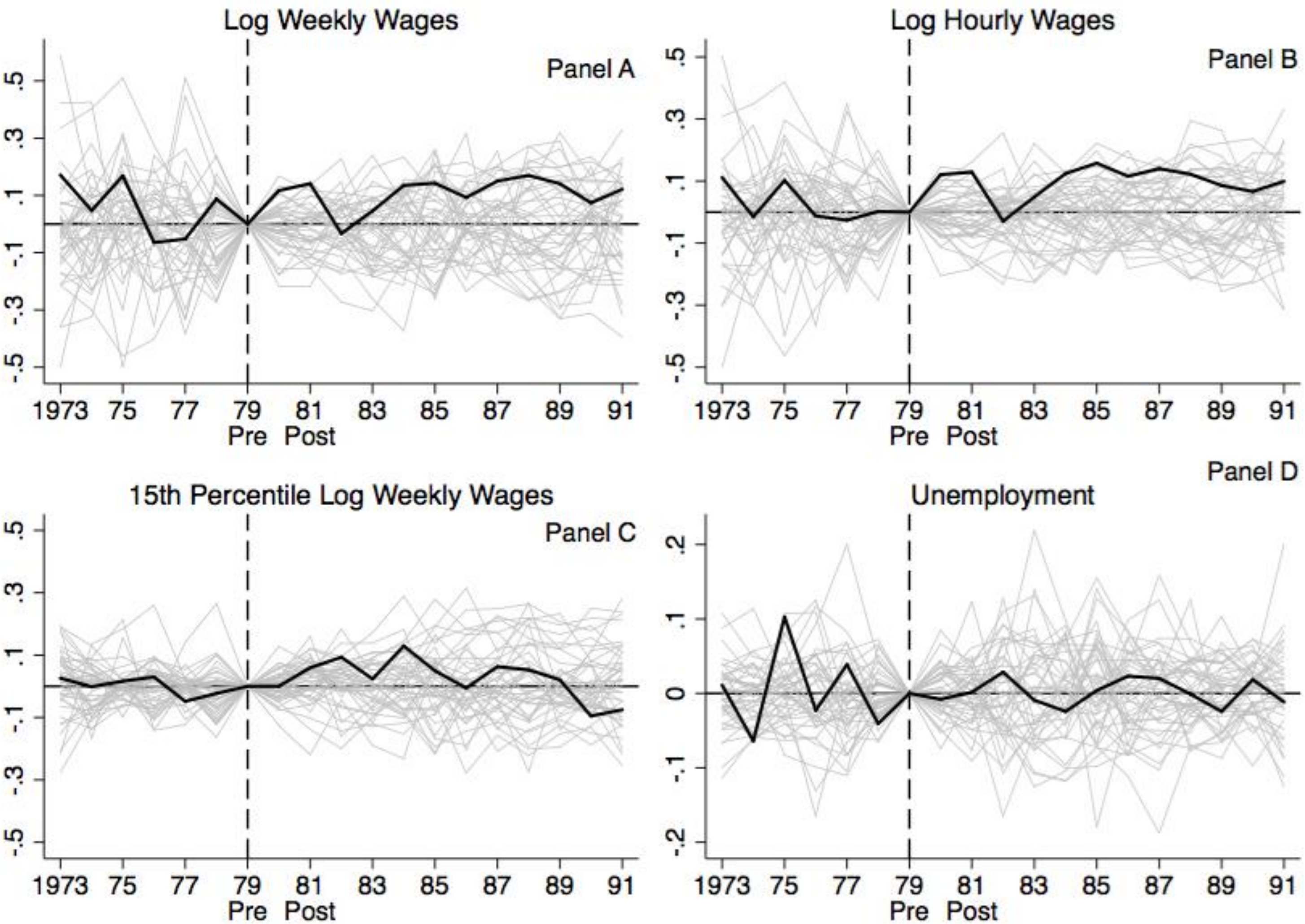
Notes: The data source is March CPS. Each Panel shows the outcome variable for Miami (solid line) and Synthetic control (dashed line) in the period 1972-1991. The outcome variable is noted in the title of each panel. The sample is non-Cubans, not self-employed individuals, in the labor force, age 19-65 for Panels A, B and D and non-Cubans, high school dropouts not self-employed, in the labor force, age 19-65 for Panel C. The cities with positive weight in the synthetic control are as follows. Panel A: Cincinnati-Hamilton,OH/KY/IN 37.1%, Atlanta, GA 32.3%; New Orleans, LA 18.5%, Tampa-St. Petersburg, FL 12.0%; Panel B: New York City, NY 51.8%, San Diego, CA 38.2%; Panel C: San Diego, CA 50.7%, New York, NY 41.9%, Tampa-St. Petersburg, FL 4.6%, Los Angeles-Long Beach, CA 2.8%; Panel D: Denver, CO 65.8%,: Cincinnati-Hamilton,OH/KY/IN 30.3%, San Diego, CA 3.95.

Table 3: Miami and Synthetic Control, Regression Estimates 1973-1991

	(1) Figure 2 Panel A	(2) Figure 2 Panel B	(3) Figure 2 Panel C	(4) Figure 3 Panel A	(5) Figure 3 Panel D	(6) Figure 2 Panel D
Dependent Variable:	**Ln (Weekly Wages) of HSD**	**Ln (Hourly Wages) of HSD**	**Ln Wages, 15th percentile**	**Ln Wages, 20th percentile**	**Ln (Weekly Wages) of HSD, Card Control**	**Unemployment Rate of HSD**
Miami X ('72-'75)	.062	.030	.022	.102	.115	.034
	(.037)	(.029)	(.024)	(.024)	(.035)	(.018)
Miami X ('76-'78)	-.070	-.042	-.010	.022	.016	.034
	(.039)	(.030)	(.025)	(.025)	(.037)	(.019)
Miami X ('81-'82)	-.015	.011	.078	.097	.079	.046
	(.042)	(.033)	(.027)	(.027)	(.040)	(.021)
Miami X ('83-'85)	.044	.076	.076	.112	.135	.018
	(.037)	(.029)	(.024)	(.024)	(.035)	(.018)
Miami X ('86-'88)	.087	.101	.049	.053	.160	.041
	(.037)	(.029)	(.024)	(.024)	(.035)	(.018)
Miami X ('89-'91)	.054	.052	-.046	.035	.099	.023
	(.037)	(.029)	(.024)	(.024)	(.035)	(.018)
Observations	36	36	36	36	36	36

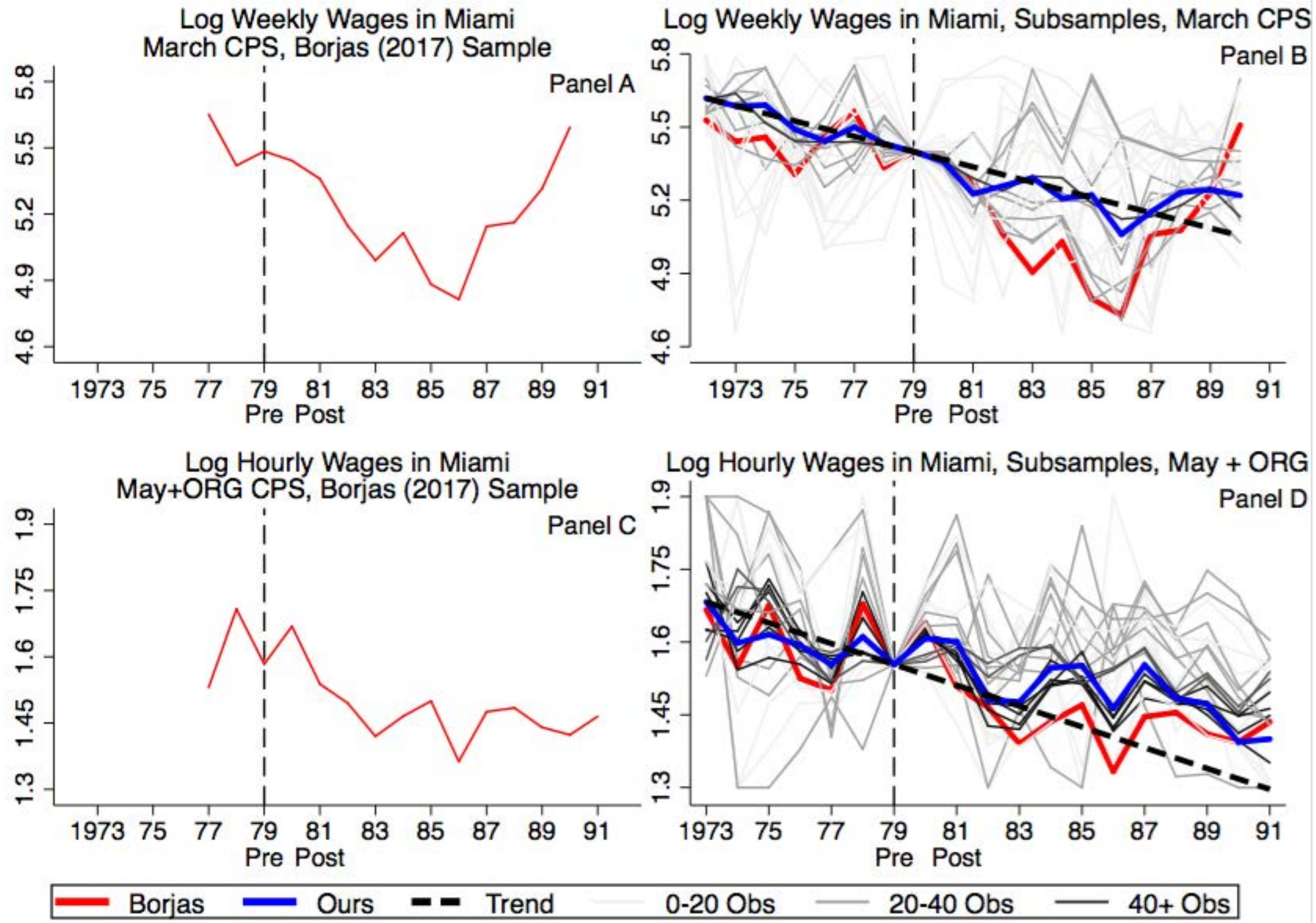
Notes: "HSD" stands for high school dropouts. Each column represents a regression of annual observations for Miami and the corresponding synthetic counterfactual between 1973 and 1991. Each specification includes vectors of city and year bins dummies. Each period dummies extends for three years. The bin for 1979 is excluded so as to standardize the value of that interaction to 0. Data for year 1980 is removed before estimation. The interaction coefficients between a dummy variable for Miami and a corresponding year bin are reported. The method of estimation is Feasible Generalized Least Squares (FGLS) with AR1 process for the error term assumed.

Figure 7: Miami and 31 Metro Areas, Simulated permutations



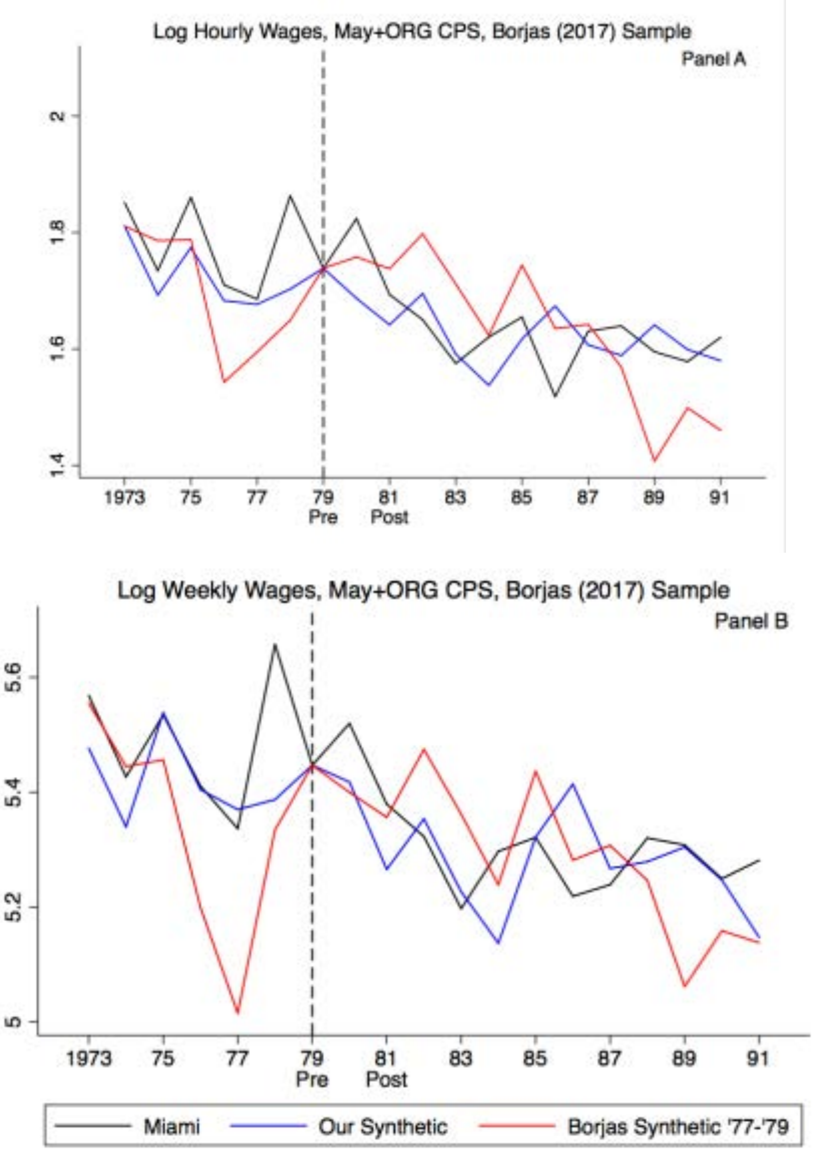
Notes: The data source is May + ORG CPS. Each graph reports deviations between synthetic control and treated group, assuming a treatment in 1980, for 44 metropolitan areas. The bold line represents Miami. Panel A shows the graph for the logarithm of weekly wages, Panel B shows it for the logarithm of hourly wages. Panel C for the 15th percentile of log weekly wages and Panel D the unemployment rate. The sample in Panel A, B and D includes non-Cuban, high school dropouts, 19-65 years old from the May and ORG CPS. In panel C the 15th percentile is calculated on all non-Cuban workers between 19 and 65 years old from the May and ORG CPS. Miami is excluded from constructing the synthetic controls. The lines are standardized to the same value in 1979.

Figure 8: Reconciling Borjas (2017)'s visual evidence with ours, March-CPS and May-ORG



Notes: Panels A and C show the log wages (weekly for Panel A and hourly for panel C) for high school dropouts in Miami as presented in Borjas (2017). The sample is non-Hispanic men, not self-employed individuals, in the labor force, age 25-59. Panel B shows the same time series of 27 different subsamples of dropouts, based along the gender (male, female or both), ethnicity (Hispanic, non-Hispanic or both) and age (you/old, prime age or both) dimensions. Borjas (2017)'s sample choice is displayed in red color, ours in blue color and the rest 25 subsamples are split depending on the average number of observations for Miami in the period 1978-1982 as follows: very light gray (0-20 observations), gray (20-40 observations) and dark gray (40+ observations). The time series are standardized to the same value in 1979.

Figure 9: Comparison between Borjas (2017)'s and Our Synthetic Controls



Notes: Each Panel shows the outcome variable for Miami (black line), Borjas (2007)'s Synthetic control (red line) and our synthetic control (blue line) in the period 1972-1991. The outcome variable and the dataset are noted in the title of each panel. The sample is non-Hispanic men, not self-employed individuals, in the labor force, age 25-59. The time series are standardized to the same value in 1979.

Table 4: City Rankings According to Various Growth Measures

City Ranking	Employment Growth (77-'78 vs '79-'80)	City Ranking	Employment Growth (76-'77 vs '78-'79)	City Ranking	Employment Growth (72-'73 vs '78-'89)	City Ranking	Ave. Wage Growth (72-'73 vs '78-'89)
Riverside, CA	0.21	Anaheim, CA	0.18	Anaheim, CA	0.65	New Orleans, LA	0.01
Denver, CO	0.2	Portland, CO	0.17	San Diego, CA	0.59	Anaheim, CA	-0.02
Philadelphia, PA/NJ	0.14	San Diego, CA	0.15	Seattle-Everett, WA	0.4	Kansas City, MO/KS	-0.08
Portland, CO	0.12	Boston, MA	0.13	Cincinnati,OH/KY/IN	0.35	Pittsburg, PA	-0.09
Boston, MA	0.1	San Jose, CA	0.13	Dallas-Fort Worth, TX	0.33	Bergen-Passaic, NJ	-0.15
Minneapolis, MN	0.09	Philadelphia, PA/NJ	0.12	Tampa, FL	0.29	Houston-Brazoria,TX	-0.17
Indianapolis, IN	*0.09*	Riverside, CA	0.12	Denver, CO	0.27	San Francisco, CA	-0.17
Chicago-Gary-Lake IL	*0.09*	Seattle-Everett, WA	0.11	San Jose, CA	0.27	Seattle-Everett, WA	-0.18
Miami-Hialeah, FL	**0.08**	Tampa, FL	0.1	Indianapolis, IN	0.25	San Jose, CA	-0.19
San Diego, CA	*0.07*	Dallas-Fort Worth, TX	0.1	Houston-Brazoria,TX	0.24	Denver, CO	-0.21
Baltimore, MD	*0.07*	Indianapolis, IN	0.09	Portland, CO	0.22	Washington, DC	-0.22
Seattle-Everett, WA	0.07	Denver, CO	0.09	Washington, DC	0.19	Indianapolis, IN	-0.22
Kansas City, MO/KS	0.05	Newark, NJ	0.09	Minneapolis, MN	0.18	Dallas-Fort Worth, TX	-0.24
Los Angeles, CA	0.05	Washington, DC	0.08	New Orleans, LA	0.16	Baltimore, MD	-0.24
Newark, NJ	0.05	Kansas City, MO/KS	0.08	Riverside, CA	0.16	Philadelphia, PA/NJ	-0.25
Houston-Brazoria,TX	0.05	*Detroit, MI*	*0.05*	Atlanta, GA	0.16	Portland, CO	-0.26
Tampa, FL	0.04	*Cincinnati,OH/KY/IN*	*0.05*	Pittsburg, PA	0.14	Chicago-Gary-Lake IL	-0.26
St. Louis, MO/IL	0.04	**Miami-Hialeah, FL**	**0.05**	Baltimore, MD	0.12	Newark, NJ	-0.3
Anaheim, CA	0.04	*Buffalo, NY*	*0.04*	Detroit, MI	0.11	Tampa, FL	-0.31
Buffalo, NY	0.03	*Baltimore, MD*	*0.04*	Boston, MA	0.1	St. Louis, MO/IL	-0.32
Cleveland, OH	0.03	Atlanta, GA	0.04	San Francisco, CA	0.08	Buffalo, NY	-0.32
San Francisco, CA	0.02	Chicago-Gary-Lake IL	0.04	Los Angeles, CA	0.07	Cleveland, OH	-0.33
Dallas-Fort Worth, TX	0.02	New York, NY	0.03	*Kansas City, MO/KS*	*0.07*	Detroit, MI	-0.34
Bergen-Passaic, NJ	0.02	St. Louis, MO/IL	0.03	*Buffalo, NY*	*0.05*	Cincinnati,OH/KY/IN	-0.35
Detroit, MI	0.01	Houston-Brazoria,TX	0.02	**Miami-Hialeah, FL**	**0.05**	San Diego, CA	-0.35
San Jose, CA	0	Los Angeles, CA	0.02	*Philadelphia, PA/NJ*	*0.04*	New York, NY	-0.35
Washington, DC	0	Minneapolis, MN	0.01	*Newark, NJ*	*0.03*	Los Angeles, CA	-0.39
New York, NY	-0.01	San Francisco, CA	-0.02	Cleveland, OH	0.02	*Atlanta, GA*	*-0.4*
Pittsburg, PA	-0.02	Cleveland, OH	-0.03	St. Louis, MO/IL	0.01	*Boston, MA*	*-0.4*
New Orleans, LA	-0.05	Bergen-Passaic, NJ	-0.04	Chicago-Gary-Lake IL	-0.02	*Minneapolis, MN*	*-0.41*
Atlanta, GA	-0.06	New Orleans, LA	-0.05	New York, NY	-0.05	*Riverside, CA*	*-0.43*
Cincinnati,OH/KY/IN	-0.11	Pittsburg, PA	-0.05	Bergen-Passaic, NJ	-0.13	**Miami-Hialeah, FL**	**-0.59**

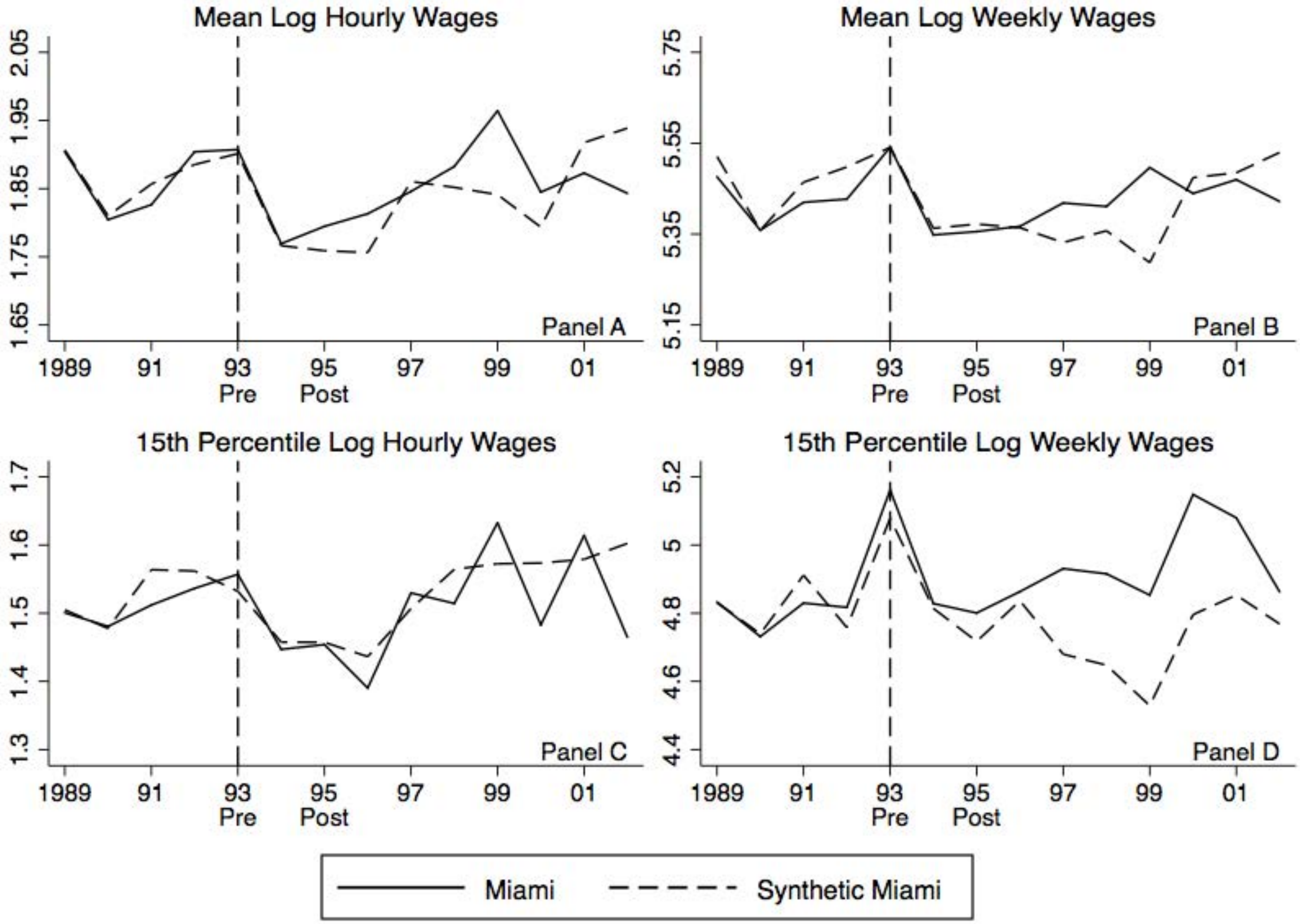
Note: We calculate the employment and average wage growth rate for Miami and the other 31 metropolitan areas using the March-CPS sample. Miami is indicated in bold. The four cities closest to Miami, which would be chosen as control group when minimizing difference in that specific growth rate, are underscored. The grey-shaded cities Anaheim and San Jose are included in the Borjas (2017)'s synthetic and employment controls.

Table 5: Reconciling Borjas (2017) and Our Regression Results

	(1) Borjas	(2) + Pre-77 Data	(3) + No Smooth	(4) + Our sample	(5) + Our control
MARCH CPS					
Dependent Variable:	**Ln (Weekly Wages) of HSD**	**Ln (Weekly Wages) of HSD**	**Ln (Weekly Wages) of HSD**	**Ln (Weekly Wages) of HSD**	**Ln (Weekly Wages) of HSD**
Miami X ('72-'75)		.094 (.029)	.240 (.087)	-.011 (.029)	.005 (.029)
Miami X ('76-'78)		-.022 (.018)	.053 (.039)	.032 (.040)	-.010 (.027)
Miami X ('81-'83)	-.327 (.068)	-.247 (.052)	-.147 (.103)	-.022 (.043)	-.111 (.068)
Miami X ('84-'85)	-.512 (.069)	-.433 (.054)	-.337 (.090)	-.108 (.112)	-.143 (.049)
Miami X ('86-'88)	-.143 (.138)	-.064 (.132)	.036 (.287)	-.212 (.077)	-.264 (.065)
Miami X ('89-'91)	.024 (.043)	.104 (.003)	.201 (.042)	-.049 (.027)	-.091 (.016)
MAY-ORG CPS					
Miami X ('72-'75)		.030 (.018)	.157 (.037)	.061 (.011)	-.013 (.008)
Miami X ('76-'78)		-.067 (.021)	.020 (.039)	.018 (.020)	.066 (.043)
Miami X ('81-'83)	-.200 (.029)	-.176 (.021)	-.109 (.034)	.120 (.021)	.049 (.048)
Miami X ('84-'85)	-.190 (.021)	-.166 (.003)	-.046 (.039)	.165 (.030)	.142 (.015)
Miami X ('86-'88)	-.121 (.051)	-.096 (.047)	-.019 (.057)	.173 (.027)	.127 (.007)
Miami X ('89-'91)	.008 (.022)	.032 (.009)	.142 (.034)	.222 (.010)	.086 (.010)
Observations	28	36	36	36	36

Notes: "HSD" stands for high school dropouts. Each column represents a regression of annual observations for Miami and the corresponding synthetic counterfactual between 1973 and 1991. Each specification includes vectors of city and year dummies. Each period dummies extends for three years. The bin for 1979 is excluded so as to standardize the value of that interaction to 0. Data for year 1980 is removed before estimation. The interaction coefficients between a dummy variable for Miami and a corresponding year bin are reported. The method of estimation is Feasible Generalized Least Squares (FGLS) with AR1 process for the error term assumed. In the first column we attempt to replicate the results of Borjas (2017). In each subsequent one we make a small change (denoted in the header) to the previous specification.

Figure 10: The Non-Event of 1994, Wages of Low-Skilled

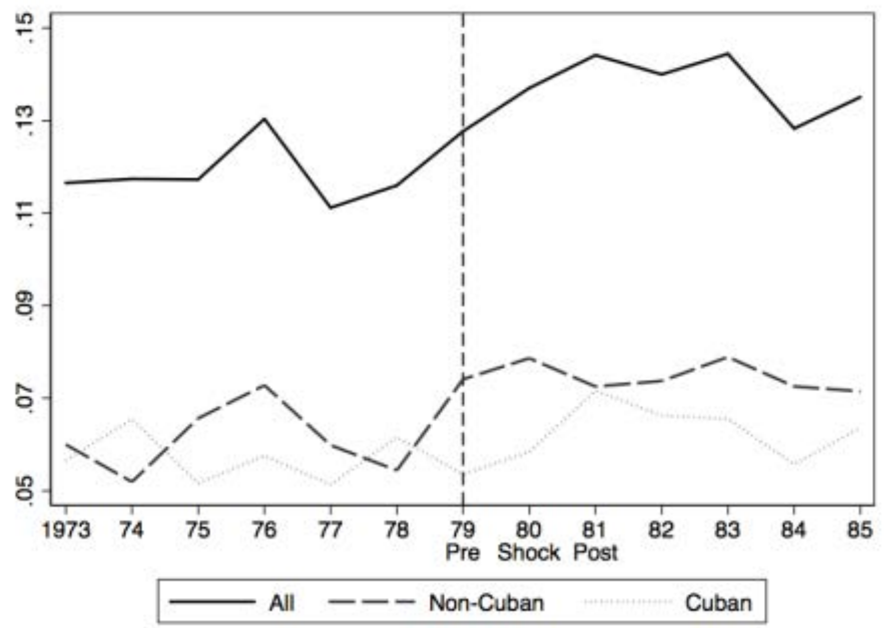


Notes: The data source is May + ORG CPS. Each Panel shows the outcome variable for Miami (solid line) and Synthetic control (dashed line) in the period 1989-2002. The outcome variable is noted in the title of each panel. The sample is non-Cubans, not self-employed individuals, in the labor force, age 19-65. Some time series are standardized to the same value in 1979. The cities with positive weight in the synthetic control are as follows. Panel A: Bakersfield, CA 35.4%, New Orleans, LA 21.8%, El Paso, TX 16.8%, Jackson, MS 13.1%, Visalia-Tulare-Porterville, CA 12.9%; Panel B: Jersey City, NJ 31.8%, Sioux Falls, SD 30.4%, Bakersfield 27.6%, San Antonio, TX 10.2%; Panel C: Bakersfield, CA 38.4%, Lakeland-Winter Haven, FL 28.6%; New Orleans, LA 17.5%, El Paso, TX 10.9%, Jackson, MS 4.4%; Panel D: Jersey City, NJ 50.5%, Sioux Falls, SD 32.1%, San Antonio, TX 17.4%.

Online Appendix

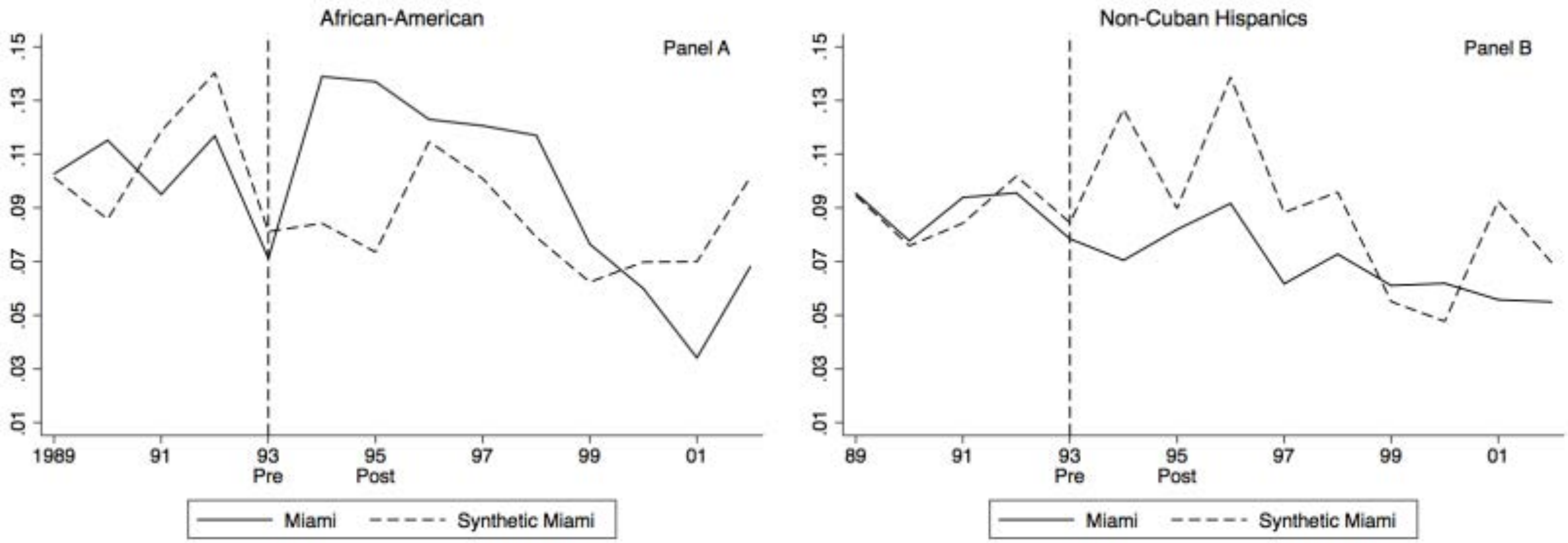
(For online publication only)

Figure A1: High School Dropouts as Share of the Population in Miami



Notes: The solid line displays the share of high school dropouts in Miami. The long- and short-dashed ones show the share of Non-Cuban and Cuban high school dropouts respectively. All statistics are calculated from May and ORG CPS.

Figure A2: The Non-Event of 1994, Unemployment in Miami and in the Synthetic Control



Notes: The data source is ORG CPS. Each Panel shows the behavior of the outcome variable for Miami (solid line) and for the synthetic control (dashed line) in the period 1989-2001. The vertical line corresponds to year 1993, immediately before the non-event of 1994. The variables are noted in the title of each panel. The sample includes all non-Cubans, in the labor force, age 19-61 either of Black ethnicity (Panel A) or of Hispanic ethnicity (Panel B).

Table A1: Distribution of 1980-82 Deviations of City Outcomes from their Synthetic Control

	Outcome variable			
	(1) Figure 5 Panel A	**(2) Figure 5 Panel B**	**(3) Figure 5 Panel C**	**(4) Figure 5 Panel D**
Dependent Variable	**Ln Weekly Wages of HSD**	**Ln Hourly Wages of HSD**	**Ln Weekly 15th Percentile**	**Unemployment Rate of HSD**
		Analysis relative to Pre-period 72-79		
Ratio of Post-Pre MSPE	1.09	3.14	6.08	0.11
Rank, lowest to highest	36/42	42/43	40/43	1/44
P-value, one tailed test $P(\Delta>\Delta_{MIAMI})$	0.14	0.02	0.07	0.98
		Analysis relative to Pre-period 77-79		
Ratio of Post-Pre MSPE	3.34	47.69	4.26	0.28
Rank, lowest to highest	39/42	43/43	28/43	4/44
P-value, one tailed test $P(\Delta>\Delta_{MIAMI})$	0.07	0.00	0.35	0.91

Notes: "HSD" stands for high school dropouts. The "Ratio of Post-Pre" equals the absolute value of the ratio of the average Miami-Synthetic control square deviation in 80-82 divided the average Miami-Synthetic control square deviation in the pre-period. In the upper panel the pre-period is the whole period 72-79, in the lower panel it is the last two years 77-79. We also calculate the same ratio for each city in the donor pool and construct a distribution of the 32 ratio statistics. The "rank" entry shows were Miami ranks in the distribution of 44 values (bottom to top) the p-value is a test of the probability that a random draw from the donor pool takes a higher than Miami value. Miami is excluded from constructing the synthetic controls.

Table A2: Average Number of Observations for the Years 1978-1982 in Subgroups of Miami Dropouts

	May + ORG CPS	March CPS
Non-Hispanic & Men & PrimeAge	47.4	19.4
Non-Hispanic & Men & MarginalAge/PrimeAge	67.2	28.0
Non-Hispanic & Men & MarginalAge	22.4	10.0
Non-Hispanic & Women/Men & PrimeAge	79.8	34.2
Non-Hispanic & Women/Men & MarginalAge/PrimeAge	107.4	46.8
Non-Hispanic & Women/Men & MarginalAge	31.4	14.6
Non-Hispanic & Women & PrimeAge	32.4	14.8
Non-Hispanic & Women & MarginalAge/PrimeAge	40.2	18.8
Non-Hispanic & Women & MarginalAge	9.0	4.6
Hispanic/Non-Hispanic & Men & PrimeAge	60.8	60.8
Hispanic/Non-Hispanic & Men & MarginalAge/PrimeAge	85.0	39.6
Hispanic/Non-Hispanic & Men & MarginalAge	27.8	12.6
Hispanic/Non-Hispanic & Women/Men & PrimeAge	105.8	51.6
Hispanic/Non-Hispanic & Women/Men & MarginalAge/PrimeAge	138.8	68.4
Hispanic/Non-Hispanic & Women/Men & MarginalAge	38.2	19.4
Hispanic/Non-Hispanic & Women & PrimeAge	45.0	23.0
Hispanic/Non-Hispanic & Women & MarginalAge/PrimeAge	53.8	28.8
Hispanic/Non-Hispanic & Women & MarginalAge	10.4	6.8
Hispanic & Men & PrimeAge	13.4	9.2
Hispanic & Men & MarginalAge/PrimeAge	17.8	11.6
Hispanic & Men & MarginalAge	5.4	2.6
Hispanic & Women/Men & PrimeAge	26.0	17.4
Hispanic & Women/Men & MarginalAge/PrimeAge	31.4	21.6
Hispanic & Women/Men & MarginalAge	6.8	4.8
Hispanic & Women & PrimeAge	12.6	8.2
Hispanic & Women & MarginalAge/PrimeAge	13.6	10.0
Hispanic & Women & MarginalAge	1.4	2.2

Notes: We present the average number of observations for Miami over the period 1978-1982 for various subsamples. “PrimeAge” refers to workers age 25-59 and “MarginalAge” to age 19-24 and 60-65.