The Wage Impact of the *Marielitos*: A Reappraisal

George J. Borjas
Harvard University

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ABSTRACT

This paper brings a new perspective to the analysis of the Mariel supply shock, revisiting the question and the data armed with the accumulated insights from the vast literature on the economic impact of immigration. A crucial lesson from this literature is that any credible attempt to measure the wage impact of immigration must carefully match the skills of the immigrants with those of the pre-existing workers. The *Marielitos* were disproportionately low-skill; at least 60 percent were high school dropouts. A reappraisal of the Mariel evidence, specifically examining the evolution of wages in the low-skill group most likely to be affected, quickly overturns the finding that Mariel did not affect Miami's wage structure. The absolute wage of high school dropouts in Miami dropped dramatically, as did the wage of high school dropouts relative to that of either high school graduates or college graduates. The drop in the relative wage of the least educated Miamians was substantial (10 to 30 percent), implying an elasticity of wages with respect to the number of workers between -0.5 and -1.5. The analysis also documents the sensitivity of the estimated wage impact to the choice of a placebo. The measured impact is much smaller when the placebo consists of cities where pre-Mariel employment growth was weak relative to Miami.

The Wage Impact of the *Marielitos*: A Reappraisal

George J. Borjas*

I. Introduction

The study of how immigration affects labor market conditions has been a central concern in labor economics for nearly three decades. The significance of the question arises not only because of the policy issues involved, but also because the study of how labor markets respond to supply shocks can teach us much about how labor markets work. In an important sense, examining how immigration affects the wage structure confronts directly one of the fundamental questions in economics: What makes prices go up and down?

David Card's (1990) classic study of the labor market impact of the Mariel supply shock stands as a landmark in this literature. On April 20, 1980, Fidel Castro declared that Cuban nationals wishing to move to the United States could leave freely from the port of Mariel, and around 125,000 Cubans quickly accepted the offer. The Card study was one of the pioneering attempts to exploit the insight that a careful study of natural experiments, such as the exogenous supply shock stimulated by Castro's seemingly random decision to "let the people go," can help identify parameters of significant economic interest. In particular, the Mariel supply shock would let us measure the wage elasticity that shows how the wage of native workers responds to an exogenous increase in supply.

Card's empirical analysis of the Miami labor market, when compared to conditions in other labor markets that served as a control group or "placebo," indicated that nothing much happened to Miami despite the very large number of *Marielitos*. Native wages did not go down in the short run as would have been predicted by the textbook model of a competitive labor market. And unemployment, even for groups with low average skills, remained unchanged relative to what was happening in the placebo cities. Card's study has

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been extremely influential, both in terms of its prominent role in policy discussions and its methodological approach.[1]

During the 1980s and 1990s, a parallel (but non-experimental) literature attempted to estimate the labor market impact of immigration by essentially correlating wages and immigration across cities (Grossman, 1982; Borjas, 1987; Altonji and Card, 1991). These spatial correlations have been criticized for two reasons: (1) immigrants are more likely to settle in high-wage cities, so that the endogeneity of supply shocks induces a spurious positive correlation between immigration and wages; and (2) native workers and firms respond to supply shocks by resettling in areas that offer better opportunities, effectively diffusing the impact of immigration across the national labor market.

Card's Mariel study is impervious to both of these criticisms. The fact that the *Marielitos* settled in Miami had little to do with pre-existing wage opportunities, and much to do with the fact that Castro suddenly decided to allow the boatlift to occur and that the Cuban-Americans who organized the flotilla lived in South Florida.[2] Similarly, the very short run nature of Card's empirical exercise, effectively looking at the impact of immigration just a few years after the supply shock, means that we should be measuring the short-run elasticity, an elasticity that is not yet contaminated by labor market adjustments and that economic theory predicts to be negative.

Angrist and Krueger's (1999) analysis of the "The Mariel Boatlift That Did Not Happen" provides the most important conceptual criticism of Card's study to date:[3]

> In the summer of 1994, tens of thousands of Cubans boarded boats destined for Miami in an attempt to emigrate to the United States in a second Mariel Boatlift that promised to be almost as large as the first one...Wishing to avoid the political fallout that accompanied the earlier boatlift, the Clinton Administration interceded and ordered the Navy to divert the would-be

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[1] Studies that examine exogenous supply shocks that are clearly influenced by the Card analysis include Hunt (1992), Carrington and de Lima (1996), Friedberg (2001), Saiz (2003), Borjas and Doran (2012), Glitz (2012), Pinotti et al (2013), and Dustmann, Schönberg, and Stuhler (2015).

[2] Both the 1990 and 2000 censuses report that almost two-thirds of the Cuban immigrants who likely were part of the Mariel influx still resided in the Miami metropolitan area.

[3] There have also been many discussions of the statistical inference difficulties raised by this type of analysis; see Bertrand, Duflo, and Mullainathan (2004), Donald and Lang (2007), and Aydemir and Kırdar (2013).

> immigrants to a base in Guantanamo Bay. *Only a small fraction of the Cuban émigrés ever reached the shores of Miami.* Hence, we call this event, "The Mariel Boatlift That Did Not Happen" (Angrist and Krueger, 1999, p. 1328; emphasis added).

Angrist and Krueger reproduced the methodological design of Card's Mariel article by comparing the labor market in Miami before and after 1994 with the same set of placebo cities. It turned out that this *potential* supply shock made things much worse for some natives. For example, the black unemployment rate in Miami increased from 10 to 14 percent, at a time that the aggregate economy was booming and unemployment was dropping in the placebo cities.

The usual interpretation would have to be that a "phantom menace" of non-existent workers harmed Miami's African-American workforce. It obviously makes no sense to make such a claim, but this raises an important question: Does the evidence from the Mariel boatlift that *did* happen really indicate that immigration had no impact? As Angrist and Krueger (1999, p. 1329) conclude, "Since there was no immigration shock in 1994, this illustrates that different labor market trends can generate spurious findings in research of this type."

In retrospect, however, the Angrist-Krueger claim that "only a small fraction of the Cuban émigrés ever reached the shores of Miami," written before the availability of the 2000 census, was not accurate. As I will show shortly, President Clinton's decision to reroute the potential migrants to Guantanamo seemed to only delay a sizable supply shock of around 50,000 Cubans by only a year or so. As a result, it may be difficult to infer much from the comparison of the Mariel supply shock to the 1994 event that ended up bringing many immigrants to the Miami metropolitan area.

This paper provides a reappraisal of the evidence of how the Miami labor market responded to the influx of *Marielitos*. The paper is not a replication of the earlier studies. Instead, I approach and examine these questions from a fresh perspective, building on what we have learned from the 30 years of research on the labor market impact of immigration. One crucial insight from this research is that any credible attempt to measure the impact must carefully match the skills of the immigrants with the skills of the pre-existing workforce. Borjas (2003), in the study that introduced the approach of correlating wages and immigration across skill groups in the national labor market, found a significant negative correlation between the wage growth of specific skill groups, defined by education and age, and the size of the immigration-induced supply shock into those groups.

The analysis of the available microdata using this new perspective provides a *very* different picture of what happened after Mariel. As is well known, the *Marielitos* were disproportionately low-skill; around 60 percent were high school dropouts and only 10 percent were college graduates. At the time, about a quarter of Miami's pre-existing workers lacked a high school diploma. As a result, even though the Mariel supply shock increased the number of workers in Miami by 8 percent, it increased the number of high school dropouts by almost 20 percent.

The unbalanced nature of this supply shock obviously suggests that we should look at what happened to the wage of high school dropouts in Miami before and after Mariel. Remarkably, this trivial comparison was not made in Card's (1990) study and, to the best of my knowledge, has not yet been conducted.[4] By focusing on this very specific skill group, the finding that the Mariel supply shock did not have any consequences for pre-existing workers immediately disappears. In fact, the absolute wage of high school dropouts in Miami dropped dramatically, as did the wage of high school dropouts relative to that of either high school graduates or college graduates. The drop in the low-skill wage between 1979 and 1985 was substantial, perhaps as much as 30 percent.

The evidence reported in this paper provides an entirely new perspective of how the Miami labor market responded to an exogenous supply shock. At least in the short run, the labor market responded precisely in the way that the "textbook" model predicts: an increase in the number of potential workers lowered the wage of those workers who faced the most competition from the new immigrants. It seems that the short-run labor demand curve, even in the Miami of the early 1980s, was downward sloping after all.

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[4] Table 7 in Card (1980) reports wage and employment changes for the subsample of black high school dropouts, but does not report any other pre-post Mariel differences for the least educated workers. Card's finding that the black wage in Miami declined after Mariel, which he attributes to cyclical fluctuations, will be discussed below. In an unpublished online appendix, Monras (2014) attempts to replicate some of Card's results and also examines wage trends in the sample of workers who have at most a high school diploma. Monras's evidence is very suggestive of the findings reported in this paper.

II. Data

The migration of large numbers of Cubans to the United States began shortly after Fidel Castro's communist takeover on January 1, 1959. By the year 2010, over 1.3 million Cubans had emigrated.

The first large-scale data set that precisely identifies an immigrant's year of arrival is the 2000 decennial census. Prior to 2000, the census microdata reported the year of arrival in intervals (e.g., 1960-1964). I merged the data from various censuses and the American Community Surveys (ACS) to construct a mortality-adjusted number of Cuban immigrants for each arrival year between 1955 and 2010.[5] For example, I used the 1970 census to estimate the number of Cuban immigrants who arrived in the United States between 1960 and 1964, and then used the detailed year-of-migration information in the 2000 census to allocate those early immigrants to specific years within the 5-year band. Figure 1 shows the trend in the number of Cubans migrating to the United States.

Several patterns emerge from the time series. First, it is easy to see the immediate impact of the communist takeover of the island. In 1958, only 8,000 Cubans migrated to the United States. By 1961 and 1962, 52,000 Cubans were migrating annually.[6] The Cuban Missile Crisis abruptly stopped this flow in October 1962, and it took several years for other escape routes to open up. By the late 1960s, the number of Cubans moving to the United States was again near the level reached before the Missile Crisis.

The huge spike in 1980, of course, is *the* Mariel supply shock. Between 1978 and 1980, the number of new Cuban immigrants increased 17-fold, from 6,500 to 110,000. The figure shows yet another spike in 1994 and 1995, coinciding with the period of Angrist and Krueger's (1999) "Mariel Boatlift That Did Not Happen." The census data clearly indicates that somehow the "phantom" Cubans from that boatlift ended up in the United States, making this supply shock a Little Mariel. Although the number of "Little *Marielitos*" pales in comparison to the number of actual *Marielitos*, it is still quite large; the number of migrants arriving in 1995 was similar to that of the early Cuban waves in the 1960s. It is also evident

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[5] In principle, the calculation also adjusts for potential out-migration of Cuban immigrants. I suspect, however, that the number of Cubans who chose to return is trivially small (although a larger number might have migrated elsewhere).

[6] Full disclosure: I am a data point in the 1962 flow.

that there has been a steady increase in the number of Cuban migrants since the early 1980s. By 2010, about 40,000 Cubans were arriving annually.

One last detail is worth noting about the Cuban migration: A disproportionately large number of the immigrants ended up residing in the Miami metropolitan area. The fraction of Cuban immigrants residing in Miami was 50 percent in the 1980 census, 58 percent in the 1990 census, and 60 percent in the 2000 census. Regarding the *Marielitos* themselves, 62.6 percent of the Marielitos resided in Miami in 1990 and 63.4 percent still resided there in 2000.

The main data sets used in the empirical analysis are the 1977-1993 March Supplements of the Current Population Surveys (CPS).[7] These surveys report the annual wage and salary income as well as the number of weeks worked by a respondent in the previous calendar year. The wage analysis will be restricted to men aged 25-59, who are not self-employed, who are not enrolled in school, and who report positive annual earnings, positive weeks worked, and positive usual hours worked.[8] The age restriction ensures that a worker's observed earnings are not contaminated by transitory fluctuations that occur during the transitions from school to work and from work to retirement.

The 1977-1993 period that will be analyzed throughout much of the paper is selected for two reasons. First, although the March CPS data files are available since 1962, the Miami metropolitan area can only be consistently identified in the 1973-2004 surveys. Beginning with the 1977 survey, the CPS began to identify 44 metropolitan areas (including Miami) that can be used in the empirical analysis.[9] Second, my analysis of wage trends will

[7] The March surveys are known as the Annual Social and Economic Supplements (ASEC). The data was downloaded from the Integrated Public Use Microdata Series (IPUMS) website on August 22, 2015. Card (1990) used the CPS Outgoing Rotation Groups (ORG). I will show below that the evidence from the ORG data leads to a similar inference: something did indeed happen to the low-skill labor market in post-Mariel Miami.

[8] In addition, I exclude persons who reside in group quarters or have a negative sample weight. It is tempting to increase sample size by including working women in the study, but female labor force participation was increasing very rapidly in the 1980s, so that wage trends are likely to be affected by the selection that marks women's entry into the labor market. The labor force participation rate of women (aged 18-64) increased from 52.1 to 72.5 percent between 1980 and 1990 in Miami, and from 49.2 to 71.2 percent in all other urban areas.

[9] The Miami-Hialeah metropolitan area is not identified at all before 1973, and is combined with the Fort Lauderdale metropolitan area after 2004. The 1973-1976 surveys identify only 34 metropolitan areas, and one of them (New York City) is not consistently defined throughout the period; the Nassau-Suffolk metropolitan area is pooled with the New York City metro area in 1976.

stop with the 1993 survey to avoid contamination from the Little Mariel supply shock of 1994 and 1995.

The CPS did not report a person's country of birth before 1994, so that it is not possible to measure the wage impact of the Mariel supply shock on the native-born population. I instead examine the impact on non-Hispanic men (where Hispanic background is determined by a person's answer to the Hispanic ethnicity question), a sample restriction that comes close to identifying Miami's native-born population at the time. For example, the 1980 census, conducted days before the Mariel supply shock, reports that 40.7 percent of Miami's male workforce was foreign-born, with 65.1 percent of the immigrants born in Cuba and another 11.2 percent born in other Latin American countries.

The labor market outcome examined throughout the study will be the worker's log weekly earnings, where weekly earnings are defined by the ratio of annual income in the previous calendar year to the number of weeks worked. I use the Consumer Price Index (CPI) for all urban consumers to deflate the earnings data (1980 = 100).[10] For expositional consistency and unless otherwise noted, whenever I refer to a particular calendar year hereafter, it will be the year in which earnings were actually received by a worker, as opposed to the CPS survey year.[11]

Before proceeding to an examination of wage trends, it is important to document what we know about the skill distribution of the *Marielitos*. As noted earlier, the Mariel supply shock began a few days after the 1980 census enumeration, so that the first large survey that contains a large sample of the *Marielitos* themselves is the 1990 census. Nevertheless, a few CPS supplements conducted in the 1980s (including April 1983, June 1986, and June 1988) provide information on a (very) small sample of Cuban immigrants who arrived at the time of Mariel.

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[10] To minimize the problem of outlying observations, I exclude all workers who earn less than $1.50 an hour or more than $40 an hour (in 1980 dollars). This restriction approximately drops workers in the top and bottom 1 percent of the earnings distribution. I replicated the analysis using the log hourly wage as an alternative measure of a worker's income, and the results are similar to those reported in this paper.

[11] For example, a discussion of the earnings of workers in 1985 refers to the data drawn from the 1986 March CPS.

Table 1 presents the education distribution of the sample of adult Cuban immigrants who arrived in 1980 (or in 1980-1981, depending on the data set) and who were enumerated in various surveys sometime between 1983 and 2000. The calculation includes the entire population of *Marielitos* (workers and non-workers, as well as men and women) who were at least 18 years old as of 1980.

The crucial implication of the table is that the Mariel supply shock consisted of workers who were very unskilled, with a remarkably large fraction of the *Marielitos* being high school dropouts.[12] Despite the variation in sample size and the almost 20-year span in the surveys reported in the table, the fraction of *Marielitos* who lacked a high school diploma hovers around 60 percent. Table 1 also shows that a very small fraction of these immigrants were college graduates (around 10 percent).

It is insightful to compare the education distribution of the *Marielitos* with that of the pre-existing workforce. The last row of Table 1 shows that “only” 26.7 percent of labor force participants in the Miami metropolitan area were high school dropouts. In fact, Miami’s workforce was remarkably balanced in terms of its skill distribution, with 20 to 30 percent of workers in each of the four education groups.[13]

Table 2 summarizes what we know about the magnitude of the Mariel supply shock. There were 176,300 high school dropouts in Miami’s labor force just prior to Mariel (out of a total of 659,400). According to the 1990 census, 60,100 Cuban *workers* migrated (as adults) either in 1980 or 1981. If we make a slight adjustment for the small number who entered the country in 1981, Mariel increased the size of the labor force by 55,700 persons, of which almost 60 percent were high school dropouts.[14] Although the Mariel supply shock

[12] The fact that most of the adult *Marielitos* lacked a high school diploma does not necessarily imply that they did not complete compulsory schooling in Cuba. There is also a possibility that the skills of the *Marielitos* were further “downgraded” upon arrival, as in Dustmann, Frattini, and Preston (2013), so that even those immigrants with a high school diploma were still competing with the least educated workers in the pre-existing Miami workforce.

[13] The pre-existing workforce includes all labor force participants in Miami, regardless of where they were born or their ethnicity. The fraction of non-Hispanic workers who were high school dropouts was also very high (19.8 percent).

[14] The 2000 census indicates that approximately 92.8 percent of the Cuban immigrants who entered the country in either 1980 or 1981 actually entered in 1980. It is also important to note that the supply shock was probably slightly larger than indicated in Table 2 because the calculation does not account for mortality through 1990.

increased Miami's workforce by 8.4 percent and increased the number of the most educated workers by 3 to 5 percent, the size of the low-skill labor force rose by a remarkable 18 percent. Moreover, this supply shock occurred almost overnight (Stabile and Scheina, 2015). The first *Marielitos* arrived in Florida on April 23, 1980. The Coast Guard reports that over 100,000 refugees had reached the Florida shores by June 3.

III. Descriptive Evidence

The very low skills of the *Marielitos* indicates that we should perhaps focus our attention on the labor market outcomes of the least educated workers in Miami to get a first-order sense of whether the supply shock had any impact on Miami's wage structure. In fact, the literature sparked by Borjas (2003) suggests that it is important to "match" the immigrants to corresponding native workers by skill groups. Educational attainment is a skill category that would seem to be extremely relevant in an examination of the Mariel supply shock.

Any empirical study of the impact of Mariel encounters an immediate data problem: The number of workers enumerated by the CPS in the Miami labor market is small, introducing a lot of random noise into any calculation. In particular, the number of non-Hispanic men who satisfy the sample restrictions and who are employed in the Miami area was around 90-100 per CPS cross-section in the 1980s, with about a quarter consisting of high school dropouts. The sample size problem, however, becomes particularly acute with the 1991 survey, when the number of non-Hispanic men sampled in Miami falls abruptly (by almost a third), and the number of high school dropouts drops to the single digits. This change in sample size suggests that the evidence is probably most credible when we examine outcomes during the first decade after Mariel.

Nevertheless, it is instructive to start by reporting the evidence from the most straightforward calculation of the potential wage impact that uses all the available data. It turns out that even the most cursory examination of the wage trends reveals a remarkable pattern that immediately overturns the conventional wisdom about Mariel: Something indeed did happen to the wage structure in Miami after 1980. It seems, in fact, as if the

Marielitos may have had a large and adverse wage impact on the wage of comparable Miamians after all.

To easily illustrate the key finding of this paper, I simply calculate the average log weekly wage of high school dropouts in Miami each year between 1972 and 2003, the period for which the March CPS has a consistent time series for the Miami metropolitan area. Figure 2 illustrates the wage trend, using a 3-year moving average to smooth out the noise in the time series. The figure also illustrates the trend for similarly educated non-Hispanic men working outside Miami. It is important to emphasize that this simple exercise does not adjust the CPS data in any way whatsoever (other than taking a moving average), so that it provides a very transparent indication of what happened to wages in Miami pre- and post-Mariel.

It is obvious that despite the similarity in wage trends between Miami and the rest of the United States prior to 1980, something happened in 1980 that caused the two wage series to diverge. Before Mariel, the log wage of high school dropouts in Miami was 0.10 log points below that of workers in the rest of the country. By 1985, the gap had widened to 0.42 log points, implying that whatever caused the divergence had lowered the relative wage of low-skill workers in Miami by around 30 percent. Note that the low-skill wage in Miami fully recovered by 1990, only to be "hammered" again in 1995, coincidentally the time of the Little Mariel supply shock. By 2002, the wage gap between high school dropouts in Miami and elsewhere had returned to its pre-Mariel normal of around 0.11 log points.

Of course, the distinctive wage trend in Miami may not appear quite as distinctive when contrasted with what happened in other specific cities. The comparison of Miami to an aggregate of the U.S. labor market may be masking a lot of the variation that influences particular places and that disappears when averaged out. It is, therefore, important to create a control group of comparable cities unaffected by the Mariel supply shock to determine if the wage trends evident in Miami were due to macroeconomic factors that affected other similar communities as well. Beginning with the 1977 survey, the March CPS data identifies 43 metropolitan areas that can be combined in some fashion to construct a sort of placebo. Card (1980, p. 249; emphasis added) describes the construction of his control group as follows:

> For comparative purposes, I have assembled similar data…in four other cities: Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg. These four cities were selected both because they had relatively large populations of blacks and Hispanics and *because they exhibited a pattern of economic growth similar to that in Miami over the late 1970s and early 1980s*. A comparison of employment growth rates…suggests that economic conditions were very similar in Miami and the average of the four comparison cities between 1976 and 1984.

It is important to emphasize that the four cities in the Card placebo were chosen partly based on employment trends observed *after* the Mariel supply shock. Put differently, if Mariel worsened employment conditions in Miami, the Card placebo is comparing the poorer outcomes of workers in Miami to the outcomes of workers in cities where some other factor worsened their opportunities as well. It is obviously far preferable to exogenize the choice of a placebo by comparing cities that were roughly similar *prior* to the treatment, rather than being similar after one of them was "injected" with a very large supply shock.

The various panels of Figure 3 illustrate the wage trends in Miami and several potential placebos between 1976 and 1992. The top panel shows that the log wage of high school dropouts declined dramatically after 1980 when compared to what happened in the cities that make up the Card placebo. Of course, trends in absolute wages reflect many factors that are specific to local labor markets, so that it is possible that these ups and downs capture idiosyncratic shifts that affected all workers in Miami. The Mariel supply shock, however, specifically targeted the least educated workers and the bottom two panels of the figure show that the relative wage of high school dropouts in Miami—relative to either college graduates or high school graduates—also declined dramatically after Mariel, and also recovered by 1990.[15] In sum, the wage trends observed in Miami—relative to those seen in the cities that make up the Card placebo—consistently indicate that the

[15] The time series of the wage of high school dropouts in Miami relative to high school graduates has a data quirk that is worth noting: High school dropouts earned slightly more than high school graduates prior to Mariel. This anomaly arises because the average wage of high school graduates sampled by the CPS in Miami in 1979 is unusually low, and this data anomaly carries over to neighboring years because of the moving average calculation. All the findings in this paper are invariant to dropping the 1979 observation. The figure also shows that the wage of high school dropouts again exceeds that of high school graduates after 1990. As noted earlier, however, there is a precipitous drop in the number of high school dropouts sampled in Miami after 1990.

economic well-being of the least educated workers in Miami took a downward turn shortly after 1980, reached its nadir around 1985-1986, and did not recover fully until 1990.

As noted above, the cities in the Card placebo do not make up a proper control group because they were chosen, in part, so that post-Mariel employment conditions in the placebo cities resembled those in Miami. To determine the set of cities that had comparable employment growth *prior* to Mariel, I pooled the 1977 and 1978 cross-sections of the CPS, and also pooled the 1979 and 1980 cross-sections.[16] Note that the 1980 CPS data, collected in March, is not affected by the supply shock, as the *Marielitos* did not begin to arrive until late April. I then used the two pooled cross-sections to calculate the log of the ratio of the total number of workers in 1979-1980 to the number of workers in 1977-1978. Column 1 of Table 3 reports the employment growth rate for each of the 44 metropolitan areas, ranked by the growth rate.

It is evident that Miami's pre-Mariel employment conditions were quite robust, ranking 6th in the rate of employment growth. Note that *all* the cities that make up the Card placebo had lower growth rates than Miami between 1977 and 1980. In fact, the average employment growth rate in those four cities (weighted by average employment) was 6.9 percent, less than half the 15.3 percent growth rate in Miami.

I use the rankings reported in Table 3 to construct a new placebo, which I call the "employment placebo," by simply choosing the four cities that were most similar to Miami prior to 1980. Specifically, the employment placebo consists of the four cities (Anaheim, Rochester, Nassau-Suffolk, and San Jose) ranked just above and just below Miami.

Figure 3 clearly shows that the relative decline in the wage of low-educated workers in Miami is much larger when we compare Miami to cities that had comparable employment growth than to the cities that make up the Card placebo. Between 1979 and 1985, for instance, the wage of high school dropouts in Miami relative to the Card placebo fell by about 0.28 log points (or 24 percent), but the decline was about 0.48 log points (38 percent) when compared to the cities in the employment placebo. This difference, of course, is not surprising. The comparison of post-Mariel economic conditions in Miami to that of

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[16] These years refer to the *survey* years and not the calendar years where earnings are observed. A person is employed if he or she works in the CPS reference week.

cities where employment conditions are also poor *by construction* inevitably hides some of the impact of the *Marielitos*.

In short, the choice of a placebo plays a crucial role in determining the wage impact of Mariel. The fact that there are 43 metropolitan areas from which to select a 4-city control group (a number that is itself arbitrary) implies that there are a total of 123,410 potential placebos. In view of the very large number of choices, it might be reasonable to expect a huge dispersion in the estimated wage effect of the *Marielitos* across the 123,410 potential comparisons that can be made. I will report below the distribution of estimated wage impacts across all potential four-city placebos and show that the wage impact of Mariel is significantly larger when the placebo contains cities that better resembled Miami's economic conditions before 1980.

An alternative way of choosing a placebo is to employ the "synthetic control" statistical method developed by Abadie, Diamond, and Hainmueller (2010). The method essentially "searches" across all potential placebo cities and derives a weight that averages cities to create a new synthetic city. This synthetic city is the one that best resembles the pre-Mariel Miami labor market. The synthetic control approach has two beneficial properties. First, it precludes the researcher from making arbitrary decisions about what the proper placebo should be. Second, the weights attached to the potential placebo cities can be based on several economic characteristics.[17]

I defined a "synthetic placebo" by using three such characteristics: the rate of employment growth in the 4-year period prior to Mariel (i.e., the variable used to define the employment placebo); the concurrent rate of employment growth for high school dropouts; and the concurrent rate of wage growth for high school dropouts. The last two columns of Table 3 report these additional characteristics, showing that Miami also had a robust low-skill labor market prior to Mariel. Miami ranked sixth in the growth of low-skill employment and 13th in the rate of wage growth.

Figure 3 also illustrates the wage trends in the "city" that makes up the synthetic placebo. As will be seen throughout the paper, sometimes the trends from the synthetic

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[17] The synthetic control method still requires the researcher to specify the vector of variables (in addition to the labor market outcome of interest) that should be similar between Miami and the synthetic city in the pre-treatment period.

placebo resemble those from the Card placebo; sometimes they resemble those from the employment placebo, and sometimes they resemble neither. Much depends on the labor market characteristic being examined.

It is interesting to examine the weights implied by the synthetic control method (see Appendix Table A-1 for a listing). When the labor market interest of outcome is the log wage of high school dropouts, the synthetic control method assigns the largest weights to Kansas City (with a weight of 0.56), Anaheim (0.27), Sacramento (0.041), and San Diego (0.013). The appendix table also reports the weights assigned by the synthetic control method in the regression analysis reported below. The metropolitan areas with the largest weights are Anaheim (0.372), San Diego (0.239), Rochester (0.159), and San Jose (0.043). By looking at the ranking of all these cities in Table 3, it is obvious that the synthetic control method consistently selects metropolitan areas that had robust labor markets prior to Mariel (generating some overlap between the cities that make up the synthetic control and the cities in the employment placebo). Figure 3 documents that a comparison of wage trends between Miami and the synthetic placebo again suggests that the Miami experience was unique.

The wage comparisons between Miami and the various placebos, however, do not preclude the possibility that other cities in other time periods have experienced equally steep wage cuts. Perhaps there are many documented cases of similar transitory and numerically large wage reductions in other cities that are attributable to sampling error or to factors that have nothing to do with Mariel.

It is easy to establish that the steep drop in the low-skill wage in post-Mariel Miami was a very unusual event. The average wage of high-school dropouts in Miami fell by around 35 percent between 1976-1979 and 1981-1986. We can calculate the comparable wage change in every other metropolitan area for all equivalent time periods between 1976 and 2003 and see if the Miami experience at the time of Mariel stands out.[18] Obviously, if 35-percent wage cuts happen frequently, it would be harder to claim that

[18] Garthwaite, Gross, and Notowidigdo (2014) conduct a similar exercise to examine the distribution of the impact of an experiment in health insurance availability on employment lock. Note that I extend the sample period through 2003 to determine how the steep wage drop observed among low-skill Miamians in the early 1980s compares to the experience of comparable workers in all other metropolitan areas over a two-decade period.

Miami's experience had much to do with the *Marielitos*. Perhaps something else was going on—a "something else" that occurs frequently enough in local labor markets—that just happened to coincide with the timing of Castro's decision.

To assess how Miami's post-Mariel experience compares to that of the *entire* distribution of wage changes, I calculated the wage change between every single "pre-treatment" period τ (1976-1979, 1977-1980,…,1993-1996) and the corresponding "post-treatment" period τ' (1981-1986, 1982-1987,…,1998-2003). Note that to replicate the Mariel experiment, I skip a year between the 4-year pre-treatment span and the 6-year post-treatment span. I conducted this calculation for each metropolitan area, leading to a total of 774 possible "events" outside Miami (43 metropolitan areas and 18 potential treatment years between 1980 and 1997). I also calculated the change in the log wage of the other education groups for all city-year permutations.

The top panel of Figure 4 illustrates the frequency distribution of all observed changes in the wage of high school dropouts *outside* Miami. Table 4 reports summary statistics from the various distributions created by this empirical exercise.

Between 1977-1979 and 1981-1986, the log wage of high school dropouts in Miami fell by 0.439 log points (or 35.5 percent). It is visually obvious from Figure 4 that such a large wage drop was an unusual event. The mean observed wage change across all city-year permutations was only about -0.10 log points. The Mariel experience ranks in the 1.8th percentile of the distribution of all observed wage changes between 1976 and 2003 across all metropolitan areas. Similarly, the frequency distribution of observed wage changes for high school dropouts in the 1980 treatment year shows that the wage drop observed in Miami was the largest wage drop observed among all metropolitan areas.

Equally important, this exercise reveals that more educated workers in Miami did *not* experience a substantial wage decline (see the bottom panel of Figure 4). Although there have been recent claims that perhaps high school dropouts and high school graduates are perfect substitutes and should be pooled to form the "low skill" workforce (more on this in the next section), the data clearly contradicts this conjecture. The mean wage change in the log wage of high school graduates across all city-year permutations in the years 1977 through 2001 was -0.061, and Miami's Mariel experience ranked in the 66th percentile. The value observed in the Miami metropolitan area at the time of Mariel was -0.021, ranking 42nd out of the 44 metropolitan areas in the distribution for treatment year 1980.

In short, something unique happened to the economic well being of high school dropouts in Miami in the early 1980s, but not to high school graduates or to workers with even more education. The event that shocked the wage structure in Miami at the time of Mariel, whatever it happened to be, happens rarely and its adverse consequences were targeted very narrowly on workers who lacked a high school diploma.

IV. Robustness of the Descriptive Evidence

Given the striking picture that the raw data gives about the labor market impact of the *Marielitos*, and given the very contentious debate over immigration policy both in the United States and abroad, it is important to establish that the evidence presented in the previous section is robust.

This section addresses several distinct questions to evaluate the sensitivity of the results. For example, was the decline in the wage of high school dropouts in the Miami of the early 1980s recorded by other contemporaneous data sets, such as the CPS Outgoing Rotation Groups (ORG)? After all, the trends in wage inequality observed in the ORG sometimes differ markedly from those observed in the March CPS.

Similarly, is the evidence robust to alternative definitions of the low-skill workforce? The descriptive analysis, motivated by the education distribution of the *Marielitos*, used the sample of high school dropouts to define the low-skill workforce. Are the wage trends similar if we defined a low-skill worker differently or if we examined the shape of Miami's wage distribution?

1. Results from the CPS-ORG

It is well known (Autor, Katz, and Kearney, 2008; Lemieux, 2006) that wage trends recorded by the March CPS sometimes differ from the "comparable" wage trends recorded by the CPS Outgoing Rotation Groups. Unlike the March CPS, which measures annual earnings in the calendar year prior to the survey, the ORG gives a measure of the hourly wage for respondents who are paid by the hour and of the usual weekly wage for all other workers. The ORG time series begins in 1979, so that the pre-treatment period only contains one year of data. Following Autor, Katz, and Kearney (2008), I extend the pre-treatment period by using the roughly comparable (though smaller) May CPS supplements for 1977 and 1978. [19]

It is important to emphasize that the differences in wage trends between the March CPS and the ORG arise partly because the two surveys measure different concepts of income. The March CPS reports total earnings from all jobs held in the previous calendar year. The ORG measures the wage in the main job held by a person in the week prior to the survey (if working). The ORG does not provide any earnings information for persons who happen not to be working on that particular week, whereas the March CPS would capture the earnings losses associated with jobless periods. From the perspective of determining the labor market impact of the Mariel supply shock, it would seem that the more encompassing measure of labor market outcomes in the March CPS is far preferable.

Before proceeding to examine the potential disparities in wage trends across the two surveys, it is convenient to first adjust the data for differences in the age distribution of workers in different time periods and in different metropolitan areas. To make the analysis transparent, I used a simple regression model to calculate the age-adjusted mean wage of a skill group in a particular market. Specifically, I estimated the following individual-level earnings regression separately in each CPS cross-section:[20]

$$\text{(1)} \qquad \log w_{irst} = \theta_r + \boldsymbol{A_i}\, \gamma_t + \varepsilon,$$

where w_{irst} is the weekly wage of worker *i* in city *r* in education group *s* at time *t*; θ_r is a vector of fixed effects indicating city of residence; and $\boldsymbol{A_i}$ is a vector of fixed effects giving

[19] I use the 1979-2001 ORG files archived at the National Bureau of Economic Research. Because the May supplements before 1977 provide limited information on metropolitan area of residence, the wage series in the ORG begins in 1977 while the comparable series in the March CPS begins in 1976. The wage measure used in the ORG analysis is the recoded usual earnings per week (*earnwke*).

[20] Of course, the regressions are estimated separately in the March CPS and the ORG.

the worker's age.[21] The fixed effects θ_r deflate the log weekly wage for regional wage differences. The average residual from this regression for cell (r, s, t) gives the age-adjusted mean wage of that cell. Unless otherwise specified, I use age-adjusted wages for the remainder of the paper.[22]

Figure 5 illustrates the wage trends calculated in the ORG data for Miami and for the three placebos defined in the previous section.[23] It is again visually evident that something happened to the low-skill labor market in Miami in the early 1980s, particularly when the Miami trend is compared to either the employment or synthetic placebos. The use of the Card placebo in the ORG data often masks much of what went on in post-Mariel Miami.

For example, the wage of high school dropouts in Miami fell by 0.22 log points between 1979 and 1985. Figure 5 indicates that the comparable wage fell by 0.17 log points in the Card placebo, and by -0.10 log points in either the employment or synthetic placebos. The use of the Card placebo would imply that Mariel lowered the wage of high school dropouts in Miami by only about 5 percent, while both the employment and synthetic placebos would imply an impact of around 12 percent. Even more so than the March CPS data, the ORG shows the crucial role that the choice of a placebo plays in any measurement of the wage impact of the *Marielitos*.

2. Other measures of skills

Some recent studies contend that much of the wage impact of immigration disappears when the low-skill group is defined in an alternative way. Card (2009), for example, argues that high school dropouts and high school graduates are perfect substitutes.[24] The pooling of these two groups into a *very large* low-skill workforce

[21] I used seven age groups to create the fixed effects (25-29, 30-34, 35-39, 40-44, 45-49, 50-54, and 55-59).

[22] It is worth noting that the wage trends in the age-adjusted data implied by the March CPS look almost identical to the raw trends documented in Figure 2.

[23] The last column of Appendix Table A-1 shows the weights attached to the different metropolitan areas by the synthetic placebo method in the ORG data. The largest weights were attached to Anaheim (0.396), San Diego (0.234), and Rochester (0.164).

[24] See also Ottaviano and Peri (2012) and Manacorda and Manning (2012).

inevitably dilutes the disparate impact of low-skill immigration on the least skilled workers, and helps to "build in" a conclusion that recent immigration could not have had much of an impact on the wage structure (Borjas, Freeman, and Katz, 1997).

Putting aside whether the two groups are or are not perfect substitutes for the moment, it is nonetheless important to ascertain if the evidence that Mariel seems to have had a substantial wage impact disappears when such an aggregation is conducted. Before proceeding, however, it is worth emphasizing that the raw data in Figure 4 indicated that while the wage of high school dropouts in Miami fell dramatically after Mariel, the wage of high school graduates did not (in fact, it *rose* relative to what happened elsewhere). The two education groups were almost equally sized in pre-Mariel Miami, so that the aggregation will again inevitably dilute the impact of Mariel on the least-educated workers.

Figure 6 uses the March CPS data to illustrate the basic trends in the log weekly wage of the pooled group of high school dropouts and high school graduates. Despite the fact that the aggregation attenuates some of the wage effect, the figure again shows a difference between what happened to this aggregated low-skill workforce in Miami and elsewhere. For example, the wage of the pooled group of high school dropouts and graduates fell by 12 percent in Miami in the early 1980s, but by only 5 percent in the cities that make up the employment placebo and 7 percent in the synthetic placebo.

It is important to note that the observed wage trends *reject* the conjecture that the two education groups should be pooled. If we start with a nested CES production function and if we also assume that wages are equal to the value of marginal product, it is well known that the elasticity of substitution between high school dropouts (group 1) and high school graduates (group 2) can be estimated by the regression:

(2) $$\log\left(\frac{w_1}{w_2}\right) = \lambda - \frac{1}{\sigma}\log\left(\frac{L_1}{L_2}\right),$$

where w_i is the wage of group *i*; L_i gives the number of workers in that group; and σ is the elasticity of substitution. The typical study exploits variation in factor prices and factor quantities across regions or over time (or both) to estimate σ. The intercept λ is a function of technological parameters, and need not be either region- or time-invariant.

The visual evidence (as well as the regression evidence presented in subsequent sections) suggests that there is little need to take the "detour" of estimating equation (2) to determine if high school dropouts and high school graduates are perfect substitutes. If we take the CES framework seriously, equation (2) implies that the wage ratio of the two groups will be uncorrelated with the quantity ratio only if σ equals infinity. However, the data consistently indicates that the wage of high school dropouts in Miami *relative* to that of high school graduates fell dramatically after the Mariel supply shock. This drop in the relative wage of high school dropouts is obviously inconsistent with the hypothesis that the two groups are perfect substitutes. In fact, as we have seen and will see again below, the impact of Mariel on the wage of high school dropouts is consistently negative, while the impact on high school graduates is mostly positive.

The "experimental" way of showing that the two groups are not productive clones is far more convincing than the typical regression approach used to estimate σ. It is well known that the relative demand for low-skill labor fell in recent decades, so that the intercept λ in equation (2) is not constant over time. We obviously do not know how to net out this demand shift in a time-series data set (such as the one that could be constructed from the CPS), so that assumptions must be made about the shape of the unobserved trend in relative demand.[25] Borjas, Grogger, and Hanson (2012) show that estimates of the slope coefficient in equation (2) are *extremely* sensitive to these extraneous assumptions. The estimate of σ can be made positive, zero, or even negative by assuming different functional forms for the unobserved trend, regardless of whether the underlying wage data are a time

[25] See Katz and Murphy (1992) and Autor, Katz, and Kearney (2008). Goldin and Katz (2010) argue that the "preferred" specification for the regression model should include a linear trend as well as a post-1992 spline to account for these unobserved shifts in the relative demand of high school dropouts and high school graduates. If one "buys into" these functional form assumptions, the estimate of $(-1/\sigma)$ using the Goldin-Katz annual CPS data from 1963 through 2005 is -0.135 (with a standard error of 0.027), rejecting the hypothesis that the two groups are perfect substitutes (see Borjas, Grogger, and Hanson, 2012).

series or exploit geographic variation.[26] The Mariel evidence that suggests the two groups are *not* perfect substitutes is not vulnerable to this criticism.

Finally, it is instructive to show that the wage of Miami's most disadvantaged workers behaved differently in the early 1980s even if we dispense completely with the use of educational attainment to define the low-skill workforce. It turns out that Miami also experienced a widening of its wage distribution at the time. The most transparent way of documenting this widening is by examining what happened to the spread of the distribution of log weekly earnings in the various cities.

Figure 7 uses the March CPS to illustrate the trend in both the wage of the worker at the 20th percentile as well as the interquantile range, which I define as the difference in the log weekly wage between the worker at the 20th percentile and the worker at the 80th percentile. The trends are visually striking. It is evident that the economic well being of Miamians in the bottom tail of the wage distribution took a beating post-Mariel. Much of the decline seemed to occur in the first few years after Mariel, at which point both the absolute and relative position of low-skill workers began to recover.

3. Implications for the black-white wage gap

Just days prior to the Mariel supply shock, the 1980 census reported that 25.2 percent of Miami's (male) workforce was African-American. There was, however, a sizable disparity in the black share among education groups; it was 42.5 percent for high school dropouts, but only 6.0 percent for college graduates. This imbalance in the skill distributions of black and white workers in pre-Mariel Miami suggests that a large supply shock of low-skill immigrants would likely have a disproportionately larger effect on the black workforce, and could widen the average wage gap between black and white workers.

The impact of Mariel on Miami's black workforce is of particular interest because racial riots ravaged parts of the city within a month after the Mariel boatlift began, leaving 18 dead and 400 injured. The conditions on the ground were volatile, and the riots were

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[26] The typical regression approach also faces a serious conceptual difficulty: What exactly is the exogenous force that generates changes in relative quantities that somehow *cause* changes in relative wages? Despite the classic supply-demand endogeneity problem with this regression framework, the issue has been almost universally ignored in the literature.

the consequence of a long list of accumulated grievances, particularly the acquittal of four white police officers charged with manslaughter when an African-American man died after a high-speed chase. But, notably, one of the grievances cited by a history of those riots was "the displacement of blacks by Cubans from jobs and other opportunities" (Vogel and Stowers, 1991, p. 120).

Figure 8 illustrates the trend in the black-white wage gap in Miami and the placebos using both the March CPS and the ORG files. It is obvious that the relative black wage declined sharply after Mariel.[27] The March CPS data, for example, indicates that the black relative wage in Miami fell by almost 20 percentage points between 1979 and 1985, showing a very different trend than what occurred elsewhere.

It is interesting to note that the original Card study, which used the ORG files, suggests the possibility that the African-American workforce in Miami was particularly affected by the Mariel supply shock. Card (1990, Table 3, p. 250) reports that the black wage in Miami fell by 11 percentage points between 1979 and 1983, as compared to a drop of only 5 percentage points in the comparison cities. This suggestive evidence, however, was dismissed: "The data do suggest a relative downturn in black wages in Miami during 1982-83. It seems likely, however, that this downturn reflects an unusually severe cyclical effect associated with the 1982-83 recession." Figure 7 shows that the downturn in black wages was not a transitory cyclical deviation. In fact, the bottom panel of the figure shows that the ORG data would have revealed a continuing decline in the economic fortunes of Miami's black workers had the Card study examined the data beyond its stopping point of mid-1985.

In short, it seems as if the impact of the *Marielitos* on relative wages across education groups substantially worsened the relative economic status of the typical African-American in Miami relative to his counterpart in the placebo cities. This disproportionate impact of low-skill immigration on the African-American workforce is consistent with the evidence reported in Borjas, Grogger, and Hanson (2010).

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[27] The calculation of the synthetic placebo was not conducted for the black-white wage gap in the March CPS because the methodology requires a perfectly balanced panel over the relevant sample period. Unfortunately, there were over 10 city-year permutations in the data that did not sample any black workers. There were only 3 city-year permutations without black workers in the ORG (and they were all in 1977 or 1978). I used adjacent-year data to impute the missing information for the ORG.

It would be of great interest to also examine the relative trends in Hispanic wages, but the nature of the available data would make that comparison uninformative. A replication of the analysis illustrated in Figure 8 for the Hispanic population (not shown for the sake of brevity) would show steady wage declines for Hispanic workers throughout the entire period in Miami and in the various placebos. The 1980s and 1990s were a period of substantial Hispanic immigration into many areas of the country, and that influx included millions of undocumented immigrants who are also disproportionately likely to be high school dropouts. Many of the placebo cities also received large numbers of low-skill Hispanic immigrants, diluting their effectiveness as a control group. Moreover, the CPS data does not allow us to create a sample of "pre-existing" Hispanic workers, so that the observed trend in the Hispanic wage is largely reflecting the changing composition of the Hispanic workforce due to the persistent inflow of large numbers of immigrants.

V. Regression Results

To estimate the post-treatment effect of the Mariel supply shock relative to the various placebos, I use the mean age-adjusted wage of high school dropouts in city *r* at time *t*, denoted by $\log \overline{w}_{rt}$. This wage becomes the dependent variable in a traditional difference-in-differences regression model:

(3) $$\log \overline{w}_{rt} = \theta_r + \theta_t + \beta(\text{Miami} \times \text{Post-Mariel}) + \varepsilon,$$

where θ_r is a vector of city fixed effects; θ_t is a vector of year fixed effects; "Miami" obviously represents a dummy variable indicating the Miami-Hialeah metropolitan area; and "Post-Mariel" indicates if time *t* occurs after 1980.

The regression uses annual observations between *t*=1977 and *t*=1992, but excludes 1980, the year of the supply shock.[28] The cities *r* included in the regression are Miami and the cities in a specific placebo. For example, if the Miami experience is being compared to that of cities in the employment placebo, there would be five cities in the data, and each of

[28] This time span allows me to estimate the identical regression model in both the March CPS and ORG samples.

these cities would be observed 15 times between 1977 and 1992, for a total of 75 observations. The regression comparing Miami to the synthetic placebo is similar in spirit, but there are only two "cities" in this regression: Miami and the synthetic city, for a total of 30 observations.

To allow the wage impact of Mariel to vary over time, the "post-Mariel" variable in equation (3) is a vector of fixed effects indicating whether the observation refers to 1981-1983, 1984-1986, 1987-1989, or 1990-1992. Table 5 reports the estimated coefficients in the vector β for various specifications of the regression model using the March CPS data. The table also reports robust standard errors that correct for heteroscedasticity. It is likely that there is serial correlation in outcomes at the city level that would require further adjustments for valid statistical inference, but it is well known (Cameron and Miller, 2015) that clustered standard errors are downward biased when there are few clusters in the data.

Consider initially the regressions reported in Panel A of the table, where the dependent variable is the age-adjusted log weekly wage of high school dropouts in city *r* at time *t*. The various columns of the table use alternative placebos: the Card placebo, the employment placebo, the synthetic placebo, as well as an aggregate placebo composed of all other 43 metropolitan areas. The various rows report the coefficients in the vector β indicating how the wage impact varies during the post-Mariel period. The trend in these coefficients presumably captures the wage effect as the Miami labor market adjusts, and moves from the "short" to the "long" run.

It is evident that the coefficient β estimated immediately after Mariel is negative, indicating an absolute decline in the wage of low-skill workers in the aftermath of the supply shock. However, the effect is much smaller when I use the Card placebo than when I use either the employment or synthetic placebo. The immediate wage cut using the original Card placebo is -0.137 (0.093), while the wage cut implied by the employment placebo is twice as large, with a point estimate of -0.289 (0.090), and the wage cut implied by the synthetic placebo is -0.210 (0.086). It seems, therefore, that the wage of high school dropouts in Miami fell by 20 to 25 percent in the immediate short run (1981-1983). Remarkably, this wage effect *increases* in the next three years, so that the wage for high school dropouts fell by 40 percent within 5 years (using either the employment or synthetic placebos). The wage effect then begins to weaken, and essentially disappears by the 1990-1992 period, when the coefficient in the synthetic placebo regression is 0.021 (0.096).

Panels B and C of the table replicate the analysis using the two alternative measures of relative wages. Both panels suggest that the relative wage of the least educated workers typically fell immediately after the supply shock, with the wage of high school dropouts falling by as much as 30 percent relative to high school graduates. As with the absolute wage results, the relative wage effect *also* disappears by the early 1990s.

Table 6 reports the coefficients from comparable regressions using the ORG data. The immediate effect on the wage of high school dropouts implied by the ORG is roughly similar to that implied by the March CPS when I use either the employment placebo or the synthetic placebo. The log wage of high school dropouts fell by 20 to 25 percent in the March CPS data and by 15 to 20 percent in the ORG. There is one interesting difference in the ORG regression results: The wage effect of the Marielitos does *not* eventually disappear. Both the employment and synthetic placebos indicate that the log wage of high school dropouts in Miami is 10 to 20 percent below that of comparable workers in the placebo cities even a decade years after Mariel (although the effect is not significant with the employment placebo).

Despite the regression finding that the Mariel supply shock harmed low-skill workers in the short run, the overall evidence may not be consistent with the textbook model of factor demand. The evidence consistently suggests that the adverse wage effect of the *Marielitos* initially increased over time before eventually disappearing. This is hard to square with the theoretical prediction that the wage effect would be largest right after the supply shock and would weaken as the capital stock adjusted over time. One possible explanation may be that employers are reluctant to cut wages for pre-existing workers, so that the immigration-induced wage cuts come into play "slowly" as turnover in the low skill labor market allows firms to take advantage of the changed situation.

Equally important, the adjustments induced by the Mariel supply shock probably involved much more than the increase in the capital stock that plays the central role in the neoclassical model of labor demand. As suggested by the racial unrest that shook Miami soon after Mariel, the political and social upheaval created by Castro's decision to open up the port of Mariel affected Miami's economy in ways that extend far beyond what our models capture (Portes and Stepick, 1994). Put differently, the *ceteris paribus* assumption does not really apply. Given these undocumented and unknown reactions, it is difficult to say much about the dynamics of the wage effect from the evidence generated by the Mariel supply shock.

We also do not fully understand the factors responsible for the eventual disappearance of the *relative* wage effect (at least in the March CPS). Economic theory implies that it is the *average* wage in the labor market that will return to its pre-Mariel level if the production function is linear homogeneous (Borjas, 2014). The relative wage effect will not go away unless there has also been a change in the relative quantities of low- and high-skill labor. Card (1990, p. 255) cites evidence that insinuates a possible supply response: "The Boatlift may have actually held back long-run population growth in Miami…the population of Dade County in 1986 was about equal to the pre-Boatlift projection of the University of Florida Bureau of Economic and Business." Although suggestive, this slowdown in population growth cannot explain the absence of a long-term relative wage effect unless the slowdown also resulted in a relative "exodus" of low-skill workers from the Miami labor market.

Finally, Table 7 summarizes regression coefficients from models that define the low-skill workforce in alternative ways. To simplify the presentation, I estimated the regression model in equation (3) using only the years between 1977 and 1986 (excluding 1980), and the short-run wage effect reported in the table is simply the interaction between the indicator for the Miami metropolitan area and the indicator for a post-1980 observation. Although there is obviously a lot of variation in the estimated coefficients (and statistical significance), the thrust of the evidence suggests a negative short-run impact regardless of whether we look at the log wage of high school dropouts, the log wage of the pooled group of high school dropouts and high school graduates, the interquantile range, or the black-white wage gap. The Mariel supply shock typically harmed workers at the bottom end of the wage distribution.[29]

The use of either the employment placebo or the synthetic placebo indicates that the wage of high school dropouts in Miami fell by 10 to 30 percent (depending on the data set used) during the first 6 years after Mariel. As I noted earlier, the supply shock increased the number of high school dropouts by around 20 percent, so that the implied wage elasticity ($d \log w/d \log L$) is between -0.5 and -1.5.

Either of these elasticity estimates is far higher than the typical wage effect estimated in (non-experimental) cross-city regressions that link wages to immigration, an effect that often clusters around a negligible number. They are also higher than the wage elasticities estimated by correlating wages and immigration across skill groups in the national labor market (Borjas 2003), an elasticity that clusters around -0.3 to -0.4. Interestingly, the estimates are close to those reported in Monras (2015) and Llull (2015), who use new instruments (including the Peso Crisis in Mexico, natural disasters, armed conflicts, and changes in political conditions) to correct for the endogeneity of migration flows. Monras reports a wage elasticity of -0.7 and Llull's estimates cluster around -1.2.

There are obviously many caveats that need to be considered regarding the specification of the regression models and the small samples in the CPS data before we fully buy into an elasticity estimate of between -0.5 and -1.5. Nevertheless, the key implication of the evidence is unambiguous. The wage of high school dropouts in the Miami labor market fell significantly after the Mariel supply shock. Any attempt at rationalizing this fact as due to something other than the *Marielitos* will need to specify precisely what those other factors were.

[29] I also estimated specifications of the regression model that used different vectors of variables to predict the synthetic placebo. A very general specification, for example, included the total growth rate of employment in the metropolitan area, the employment and wage growth rates for each of the four education groups, the percent of the workforce that was black, and the percent that was Hispanic. The estimated short-run wage effect was -0.148 (0.059) in the March CPS and -0.134 (0.079) in the ORG. The weighting algorithm in these expanded regressions, particularly when including the percent Hispanic variable, often assigned very large weights to San Diego.

VI. The Choice of a Placebo

The evidence reported in the previous sections suggests that the choice of a placebo matters. The short-run impact of the Mariel supply shock (i.e., the impact on wages between 1981 and 1986) was generally more negative and statistically significant when I used either the employment or synthetic placebo than when I used the Card placebo. It is useful to document in a very simple way how it is possible to "cherry pick" placebos to build in a particular empirical finding. I illustrate this variation by estimating the short-run wage effect using the difference-in-differences regression model in equation (3) in each of the 123,410 possible four-city placebos. Because I am focusing on the short-run wage impact, the regressions only employ the observations between 1977 and 1986 (with the 1980 observation excluded throughout).

The two panels of Figure 9 illustrate the frequency distribution of estimated effects when the dependent variable is the log wage of high school dropouts, while Table 8 reports summary statistics for the various distributions. For comparison purposes, the bottom rows of the table report the actual estimated wage impact (and standard error) when using the Card, employment, and synthetic placebos.

Consider the distribution of estimated effects on the wage of high school dropouts in the March CPS data. The mean effect is -0.243, which is far smaller than the estimate obtained from either the employment placebo (-0.374) or the synthetic placebo (-0.335). Nevertheless, most of the potential placebos would still suggest that the wage effect is significant: over 98 percent of the estimated effects have a t-statistic above 1.6.

Note, however, that if the set of placebos were restricted so that the *average* employment growth in the four placebo cities was roughly similar to that of pre-Mariel Miami, the mean wage effect rises to -0.282. Similarly, if we look at the still smaller subset of placebos where *each* city in the placebo had a similar pre-Mariel employment growth as Miami, the estimated wage effect becomes even stronger; the mean coefficient is -0.333, and *all* of those coefficients are statistically significant. Put differently, the closer we get to a placebo that seems to replicate the pre-existing employment conditions in Miami, the more likely we are to find that the *Marielitos* had a numerically sizable and a statistically significant wage effect on low-skill Miamians. As Table 8 shows, the same general trend is implied by the frequency distribution of wage effects computed in the ORG data.

This type of unusual exercise shows the importance that the *choice* of a placebo plays in generating estimates of the impact of natural experiments. It might be prudent to withhold drawing many substantive inferences from such experiments until we see how the "preferred" estimate of the policy impact compares to the distribution of potential impacts.[30]

It is instructive to conclude by extending the synthetic control approach to show the distribution of wage effects implied by an intriguing counterfactual exercise. What would the distribution of estimated wage effects look like if we "acted as if" a city had experienced a shock in year *t*, and simply calculated the pre-post wage change attributable to this imaginary supply shock?

To be more specific, suppose we define a pre-treatment period of 3 years and a post-treatment period of 6 years. We can imagine that the city of Akron was hit by a phantom supply shock in 1988. We can then calculate the wage change experienced by Akron between 1985-1987 and 1989-2004, and contrast this wage effect with what happened to wages in the synthetic placebo implied by the pre-existing conditions in Akron.[31] Presumably, the wage effect resulting from this exercise should be near zero simply because Fidel Castro did not suddenly decide to relocate over 100,000 Cubans to Akron in 1988. However, other (random) things may have happened in post-1988 Akron that we know nothing about and that may have changed the relative wage of low-skill workers in that city relative to the synthetic placebo.

We can obviously carry out this exercise for every single permutation of a city receiving an imaginary supply shock and every single pre-post period allowed by the data between 1977 and 2003. To generate the synthetic control, I used the city's rate of total employment growth in the 4-year period prior to the treatment, and the rates of employment and wage growth for the specific education group being examined. The top panel of Figure 10 illustrates the distribution of the estimated wage effects of these

[30] There is, in fact, a strong negative correlation between the wage effect estimated in placebo *p* and the mean rate of employment growth in the cities that form that placebo.

[31] To be consistent with the analysis of the Mariel supply shock, the pre-treatment employment growth is measured in the four-year period prior to the hypothetical shock, or 1985-1988 in the Akron example discussed in the text.

hypothetical shocks using the log wage of high school dropouts as the dependent variable, and Table 9 summarizes some of the characteristics of the resulting distributions. The data reported in Table 9, of course, allow a permutation inference analysis of the wage effect of the Mariel supply shock.

There is obviously a lot of dispersion in the estimated wage effects across all these hypothetical shocks. As expected, the mean effect is zero. It is important to emphasize, however, that the March CPS implies that the wage effect induced by the *real* Mariel supply shock in Miami was -0.335 (0.090), which is in the 3rd percentile of the counterfactual distribution where each imaginary shock is effectively being compared to the "best" possible placebo for that city at that time. If we narrow down the comparison to the 1980 treatment year, the Mariel effect is by the most negative across all metropolitan areas.

It is also instructive to document the wage impact on the other education groups resulting from this counterfactual exercise. As the last three columns of Table 9 show, the mean effects across all city-year permutations are always near zero, and the effect observed in Miami at the time of Mariel is typically not significantly different from zero. Nevertheless, as the bottom panel of Figure 9 illustrates, the impact of Mariel on the wage of high school graduates is positive, again rejecting the conjecture that high school graduates and high school dropouts are perfect substitutes.

Finally, Figure 11 illustrates the dynamics of the estimated wage effect resulting from the Mariel supply shock in Miami, and from the hypothetical supply shocks in all other metropolitan areas in the 1980 treatment year. Specifically, I use the synthetic control method and calculate for each metropolitan area in each year through 1992 the pre-post difference between the wage of high school dropouts in a specific metropolitan area and in its synthetic placebo. The calculated double difference, of course, implies that each point in the figure measure the impact of the supply shock on the wage of high school dropouts as of time *t*. For example, the trend in the Miami wage effect implied by the March CPS data indicates that the wage of high school dropouts in Miami relative to the synthetic placebo is around 30 percent lower in 1985 than it was in 1979. Note that the trend for the wage effect in the Miami metropolitan area from a shock in treatment year 1980 forms a lower "envelope" for the entire distribution of potential wage effects resulting from contemporaneous supply shocks in all other metropolitan areas. This exercise again shows that the wage effect of the Marielitos disappears after a decade in the March CPS data, but persists in the ORG.

In sum, the Mariel supply shock had a very specific target and it hit that target with impressive laser-like precision: The *Marielitos* had a substantial depressing effect on the earnings of the least educated workers in Miami.

VII. Conclusion

Card's (1990) classic paper on the labor market impact of the Mariel supply shock stands as a landmark study in labor economics. His finding that the supply shock seemed to have little effect on the labor market opportunities of native workers has profoundly influenced what we think we know about the economic consequences of immigration. The elegance of the methodological approach—the exploitation of a fascinating natural experiment to estimate a parameter of great economic interest—has also influenced the way that many applied economists frame their questions, organize the data, and search for an answer.

This paper brings a new perspective to the analysis of the Mariel supply shock. I revisit the question and the data armed with the insights provided by three decades of research on the economic impact of immigration. One key lesson from this voluminous literature is that the effect of immigration on the wage structure depends crucially on the differences between the skill distributions of immigrants and natives. The direct effect of immigration is most likely to be felt by those workers who had similar capabilities as the *Marielitos*.

It is well known that the Mariel supply shock was composed of disproportionately low-skill workers, and at least 60 percent were high school dropouts. Remarkably, none of the previous examinations of the Mariel experience documented what happened to the pre-existing group of high school dropouts in Miami, a group that composed over a quarter of the city's workforce. Given the literature sparked by Borjas (2003), it seems obvious that a crucial component of any analysis of the Mariel supply shock should focus on the labor market outcomes of these low-skill workers.

The examination of wage trends among high school dropouts quickly overturns the "stylized fact" that the supply shock did not affect Miami's wage structure. In fact, the absolute wage of high school dropouts dropped dramatically, as did their wage relative to that of either high school graduates or college graduates. The drop in the average wage of the least skilled Miamians between 1977-1979 and 1981-1986 was substantial, between 10 and 30 percent (depending on whether the analysis uses the CPS-ORG or the March CPS data). In fact, the examination of wage trends in every single city identified by the CPS throughout the period shows that the steep post-Mariel wage drop experienced by Miami's low-skill workforce was a very unusual event.

The reappraisal presented in this paper also strikingly illustrates that the researcher's *choice* of a placebo is an important component of any such empirical exercise, and that picking the "wrong" placebo can easily lead to a weaker measured impact of immigration. The analysis documented the importance of placebo choice by estimating the impact of Mariel across *all* potential (four-city) placebos allowed by the data. The distribution of estimated wage effects is very informative. The measured wage impact of the supply shock is largest when the comparison group consists of cities that had a similar rate of pre-Mariel employment growth as Miami. The methodological approach of estimating the entire distribution of potential effects across all possible placebos can be a useful component of studies that examine the consequences of natural experiments.

The empirical evidence also has many lessons for the vast literature that purports to measure the wage impact of immigration. For instance, many studies measure the effect by estimating spatial correlations between wages and the number of immigrants in a particular locality. These spatial correlations, many of which cluster around zero, are plagued both by endogeneity problems (i.e. immigrants settle in high-wage regions) and by native adjustments (i.e., firms and workers may respond to the supply shock by relocating to other cities). The fact that the spatial correlation implied by the Mariel supply shock is strongly negative suggests that the existing non-experimental literature has not successfully purged those statistical difficulties. There is still some way to go before non-experimental spatial correlations can be presumed to estimate a parameter of economic interest.

The evidence also has potentially important implications for estimates of the economic benefits from immigration. The benefit that accrues to the native population, or the "immigration surplus," is the flip side of the wage impact of immigration. In fact, it is well known that the greater the wage impact, the greater the immigration surplus. Borjas (2014, p. 151) estimates the current surplus to be around 0.24 percent of GDP (or around $43 billion annually). The fact that there was a much larger reduction in the earnings of the workers most likely to be affected by the *Marielitos* than was previously believed suggests that we may also need to reassess existing estimates of the immigration surplus. That surplus could easily be twice or three times as large if the Mariel context correctly measures the wage impact.

It has been a quarter-century since the publication of Card's Mariel study. More likely than not, that analysis has been replicated often as part of an empirical exercise in an econometrics or labor economics class. The reappraisal of the evidence provided in this paper teaches an important lesson. Although replication obviously serves an extremely useful role in the advancement of applied science, there is much to be gained by revisiting many of those persistent old questions with a new perspective, a perspective that uses the insights accumulated over the years. If nothing else, the reappraisal of the Mariel evidence shows that even the most cursory reexamination of some old data with some new ideas can reveal trends that radically change what we think we know.

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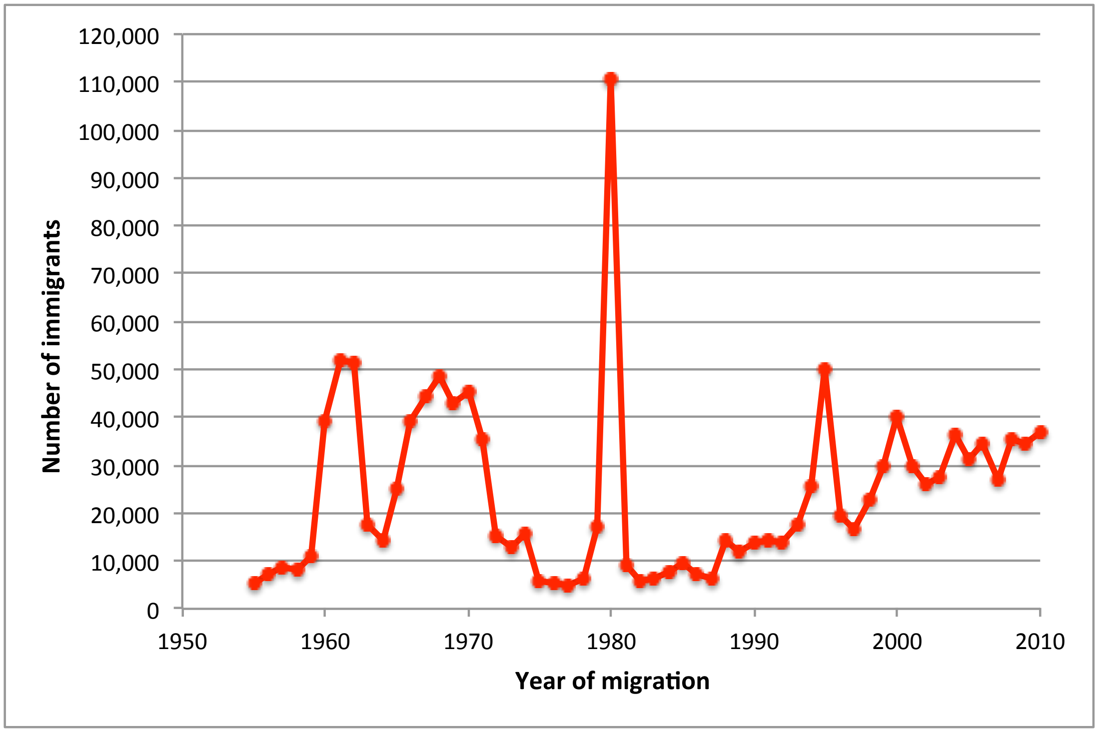
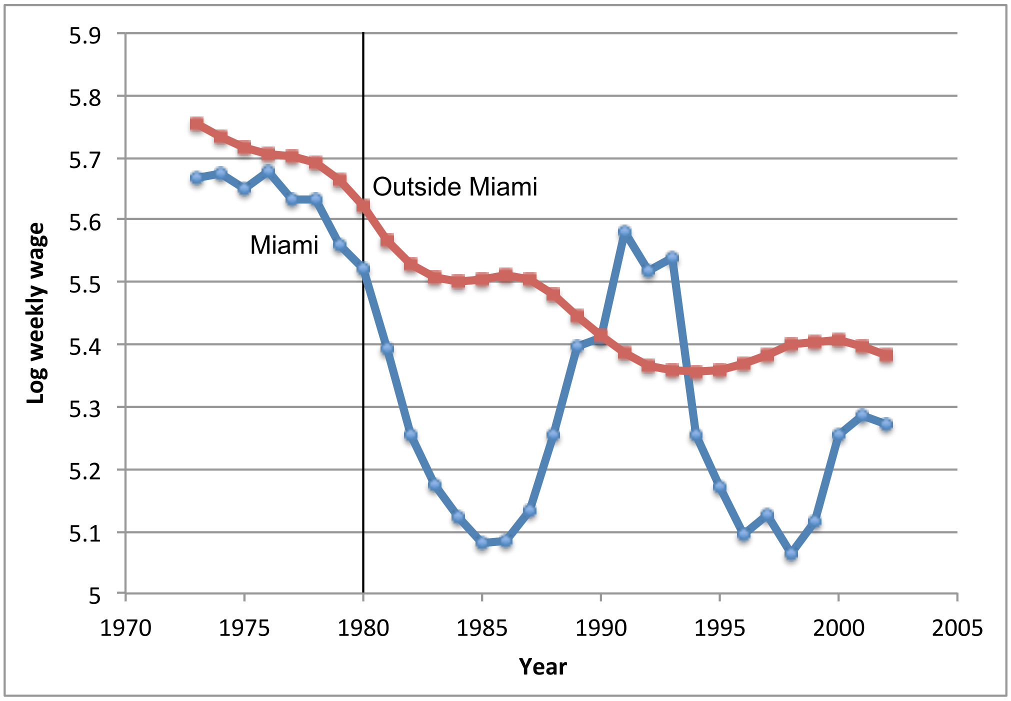
Figure 1. Number of Cuban immigrants, by year of migration, 1955-2010



Notes: The specific year of migration (through 1999) is first reported in the 2000 census. The counts are adjusted for mortality and out-migration by using information on the number of arrivals provided by the 1970 through 1990 censuses; see the text for details. The 2000-2008 counts are drawn from the pooled 2009-2011 American Community Surveys (ACS), while the 2009-2010 counts are drawn from the 2012 ACS.

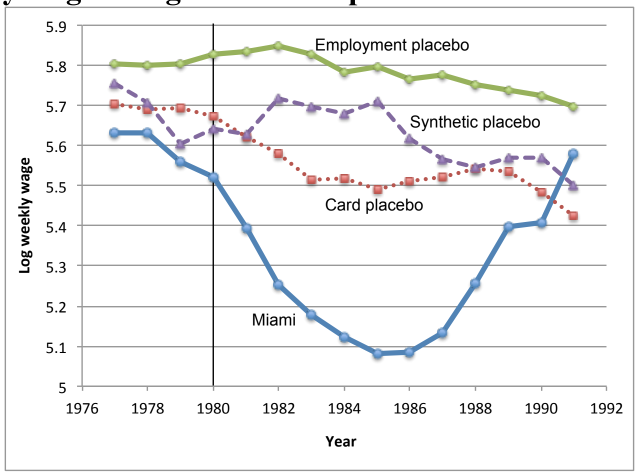
Figure 2. Log wage of high school dropouts, 1972-2003



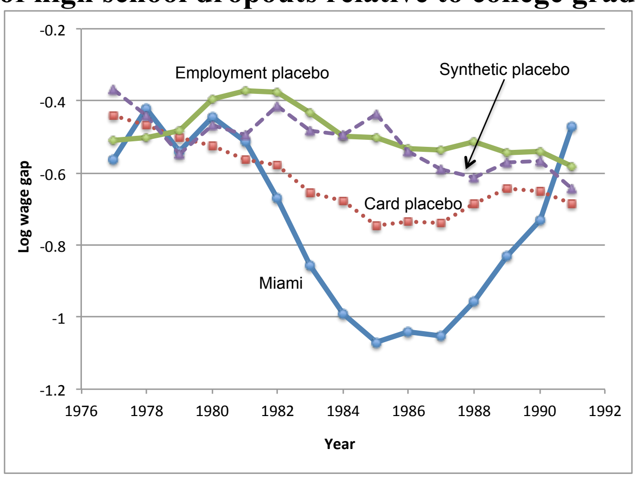
Notes: The log weekly wage is a 3-year moving average of the unadjusted average log wage of high school dropouts in each geographic area. The data are drawn from the March CPS files.

Figure 3. The trend in the wage of low-skill workers, 1976-1992

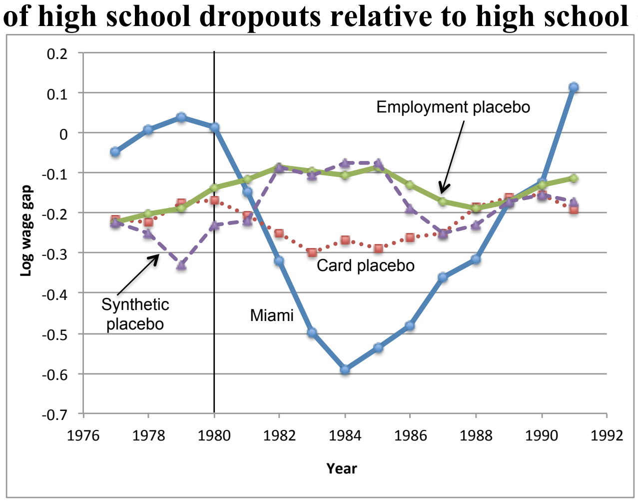
A. Log weekly wage of high school dropouts



B. Log wage of high school dropouts relative to college graduates



C. Log wage of high school dropouts relative to high school graduates

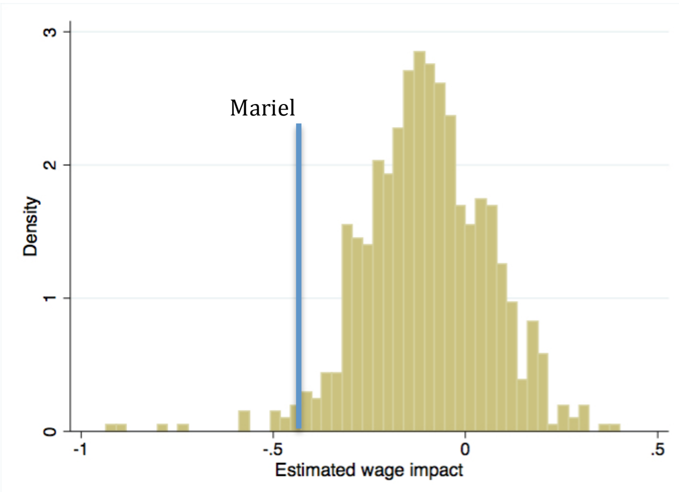


Notes: The figures use a 3-year moving average of the age-adjusted average log wage of high school dropouts, high school graduates, and college graduates in each specific geographic area. The data are drawn from the March CPS files.

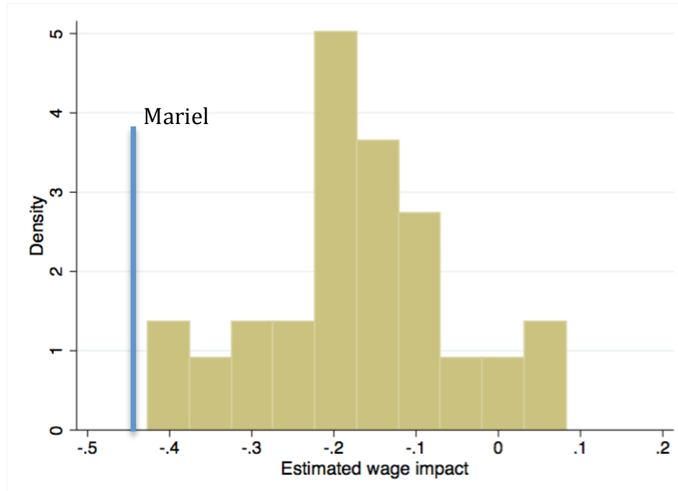
Figure 4. Distribution of pre-post wage changes, 1976-2003

A. Log wages of high school dropouts

Across all city-year permutations

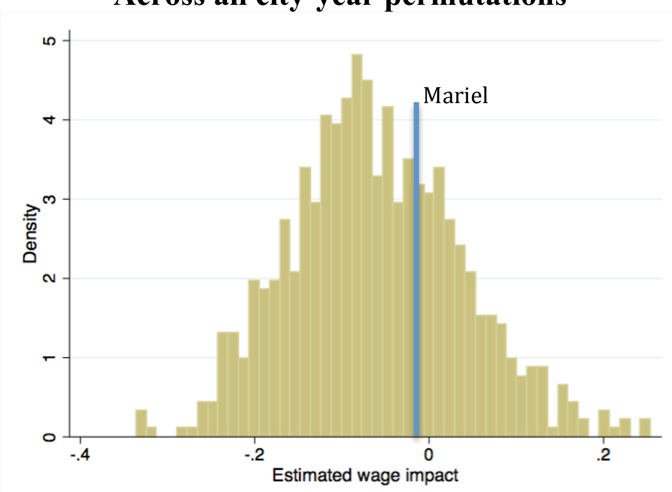


1980 treatment year

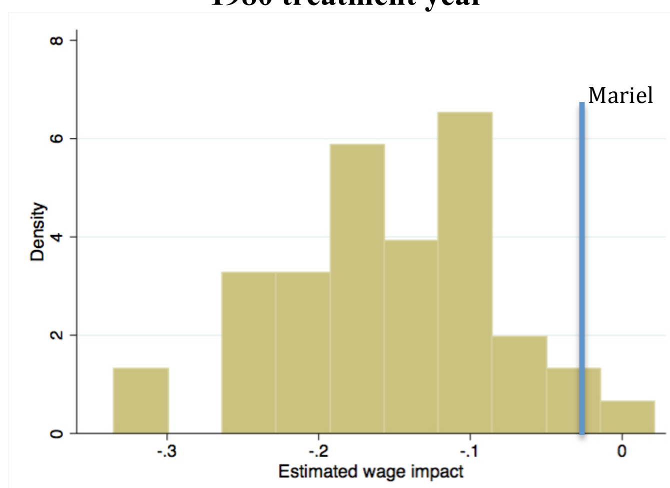


B. Log wage of high school graduates

Across all city-year permutations



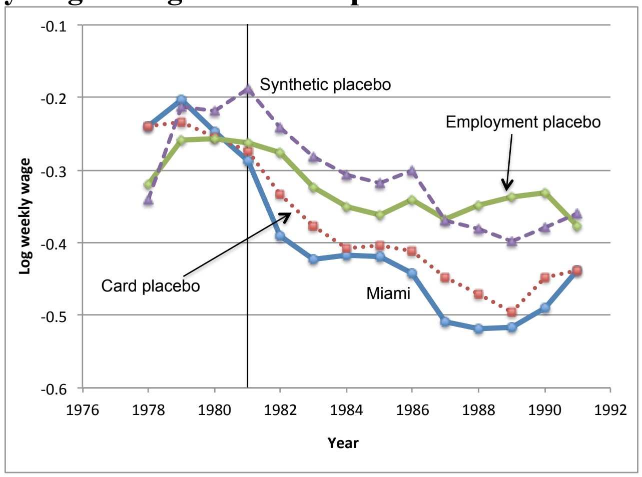
1980 treatment year



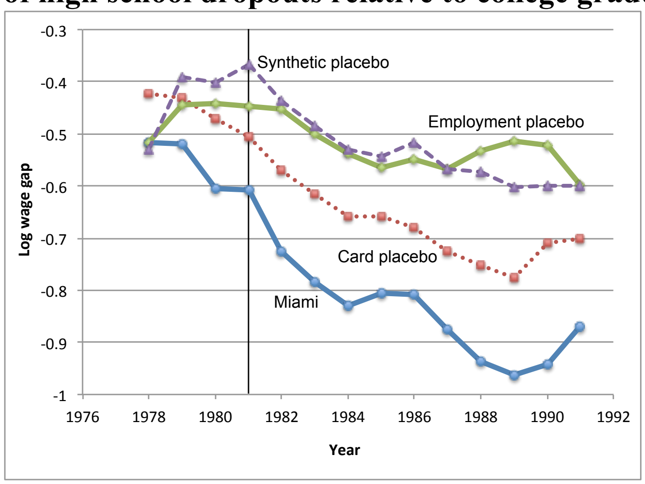
Notes: The pre-treatment period lasts 4 years; the post-treatment period lasts 6 years; and the year of the treatment is excluded from the calculation. The data are drawn from the March CPS files.

Figure 5. The trend in the wage of low-skill workers in the ORG, 1977-1992

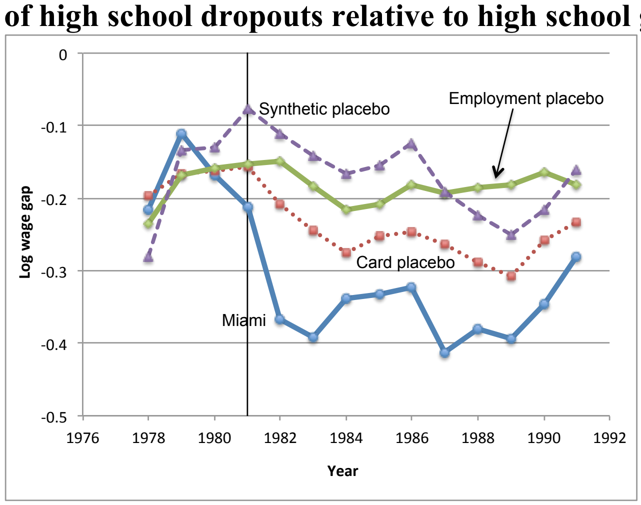
A. Log weekly wage of high school dropouts



B. Log wage of high school dropouts relative to college graduates



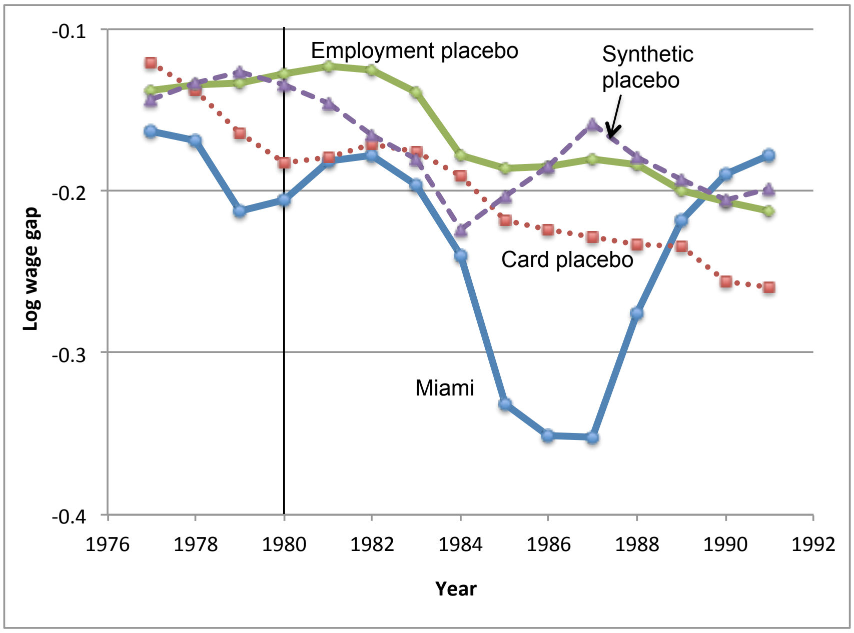
C. Log wage of high school dropouts relative to high school graduates



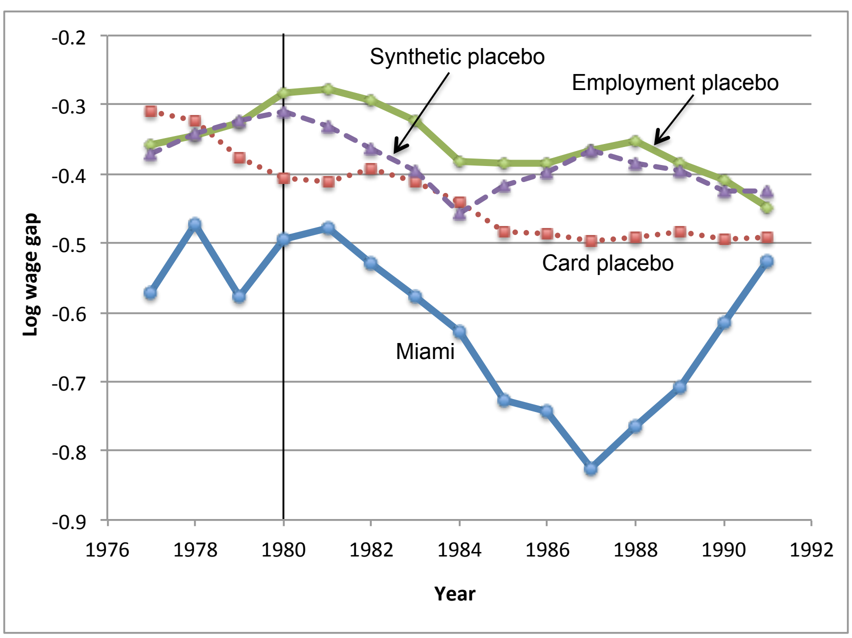
Notes: The figures use a 3-year moving average of the age-adjusted average log wage of high school dropouts, high school graduates, and college graduates in each specific geographic area.

Figure 6. Wage trends in pooled group of high school dropouts and high school graduates, March CPS, 1977-1992

A. Log wage of pooled high school dropouts and high school graduates



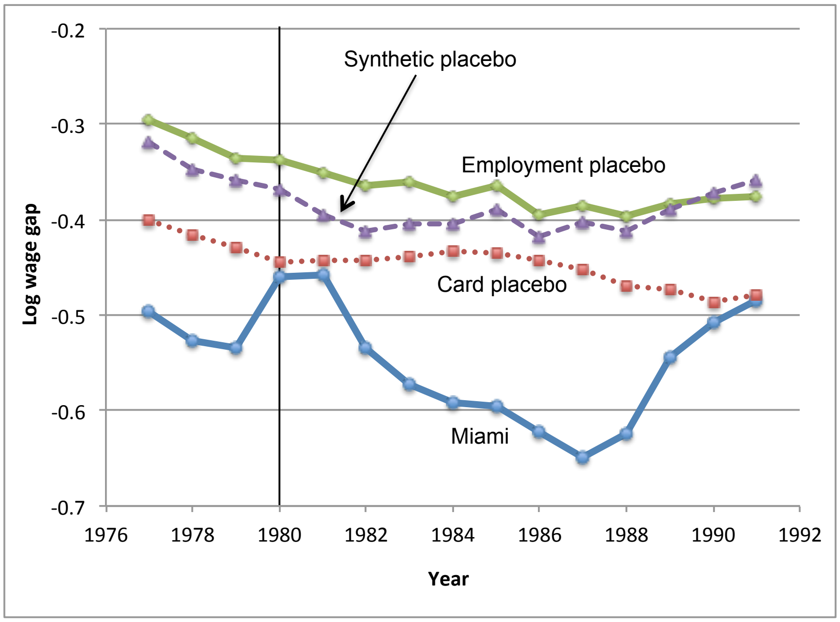
B. Log wage of pooled high school dropouts and graduates relative to college graduates



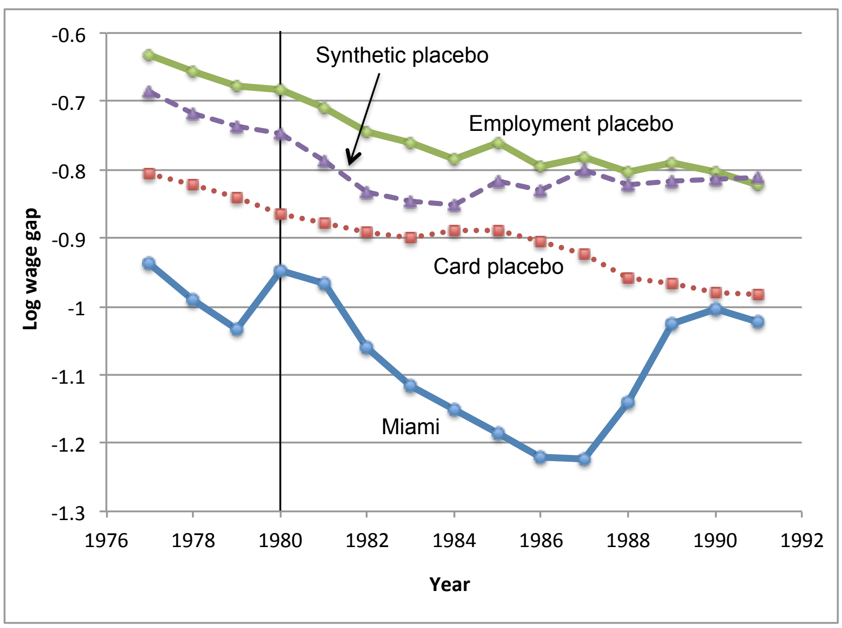
Notes: The figures use a 3-year moving average of the age-adjusted average log wage of the pooled group of high school dropouts and high school graduates, and of college graduates in each specific geographic area.

Figure 7. Trends in the spread of the log weekly wage distribution, March CPS, 1977-1992

A. Log wage of worker at the 20th percentile



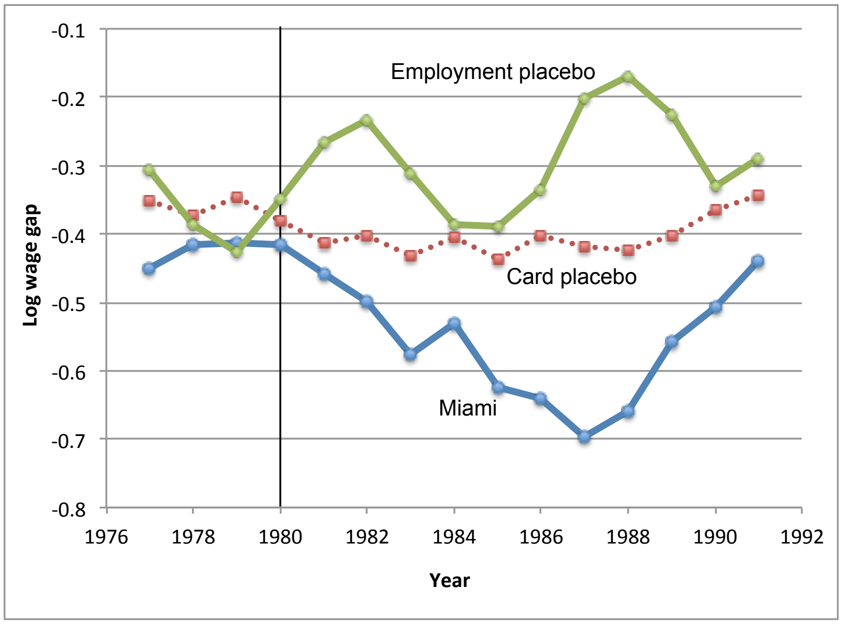
B. Difference in the log wage of workers at the 20th and 80th percentiles



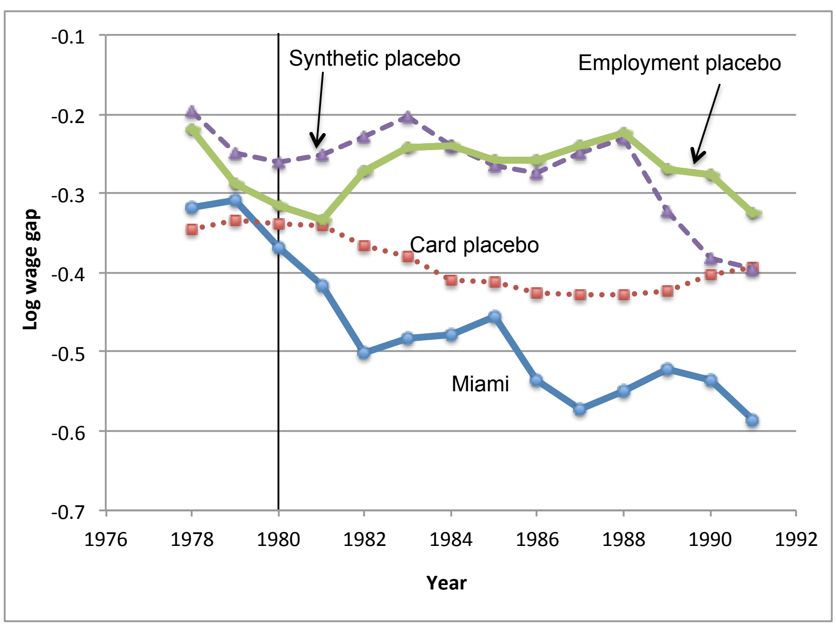
Notes: The figures use a 3-year moving average of the age-adjusted log weekly wage in each specific geographic area for each specific percentile.

Figure 8. Trends in the black-white wage differential, 1976-1992

A. March CPS



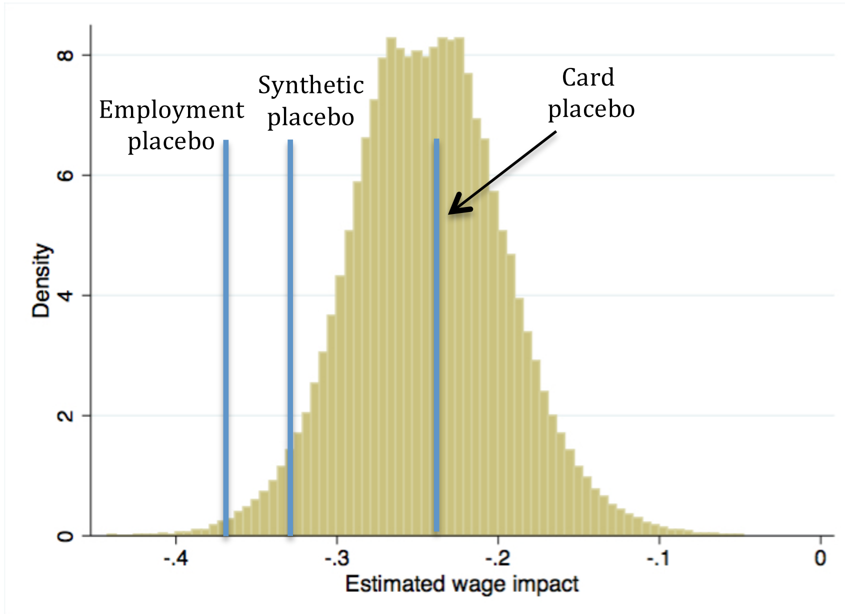
B. CPS-ORG



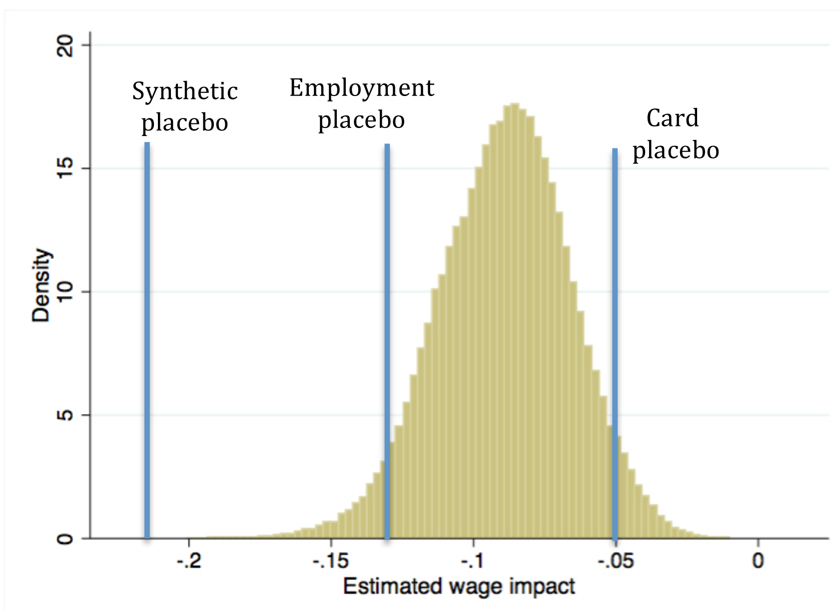
Notes: The figures use a 3-year moving average of the difference in the age-adjusted average log wage between black and white workers in each specific geographic area.

Figure 9. Distribution of short-run impacts across all possible four-city placebos, 1977-1986

A. March CPS



B. CPS-ORG

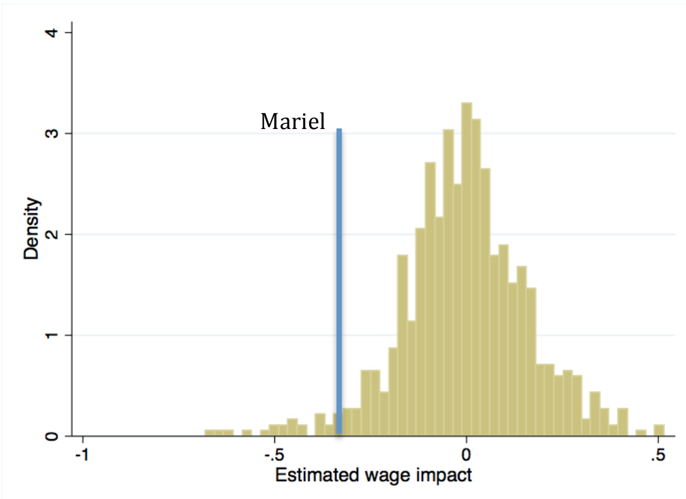


Notes: The figure shows the distribution of the interaction term from the difference-in-differences regression model in equation (3) resulting from comparing Miami to all possible 123,410 placebos in the March CPS data. The regressions use annual observations for each city in the period 1977-1986 (1980 excluded), and the coefficients measure the impact in the "short run" (i.e., 1981-1986). All regressions were weighted by the number of observations used to calculate the mean wage of high school dropouts in city *r* at time *t*.

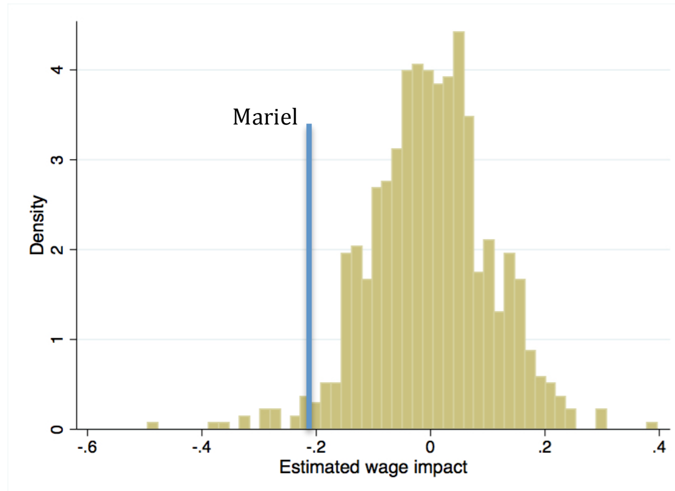
Figure 10. Distribution of hypothetical short-run impacts relative to synthetic placebo, assuming a supply shock hits each city-year permutation

A. Log wage of high school dropouts

March CPS

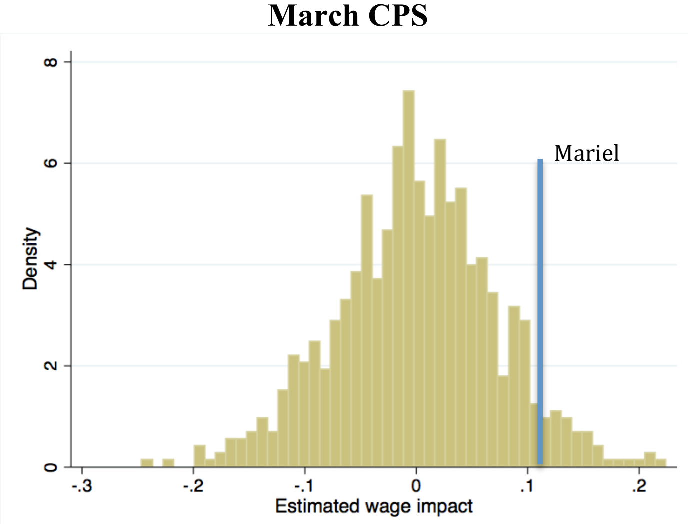


CPS-ORG

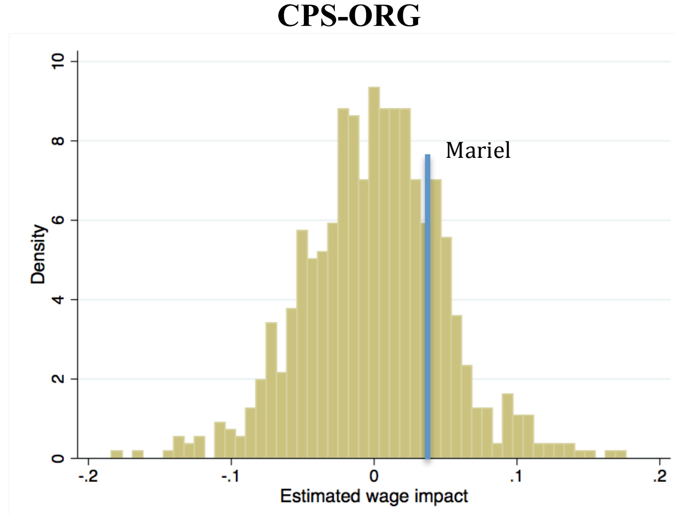


B. Log wage of high school graduates

March CPS



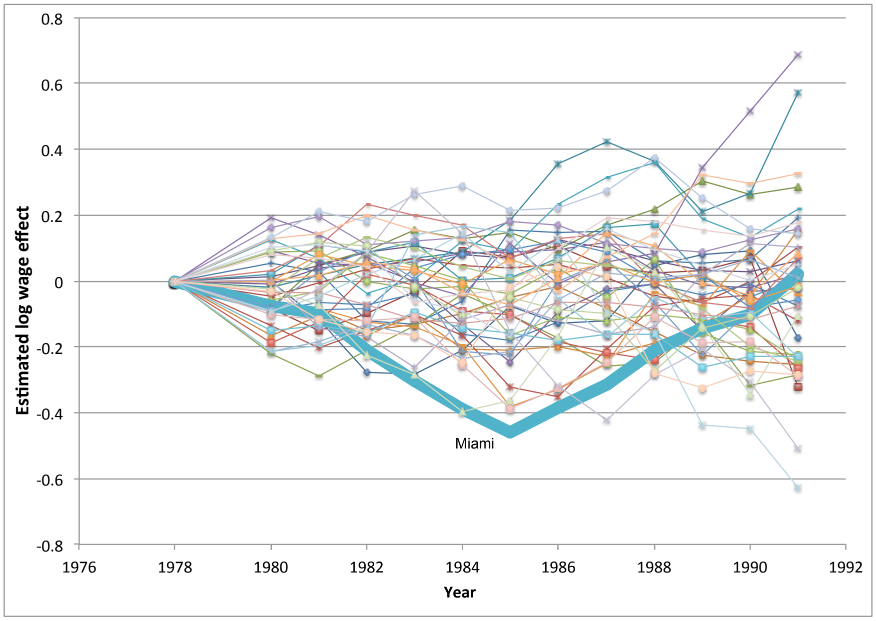
CPS-ORG



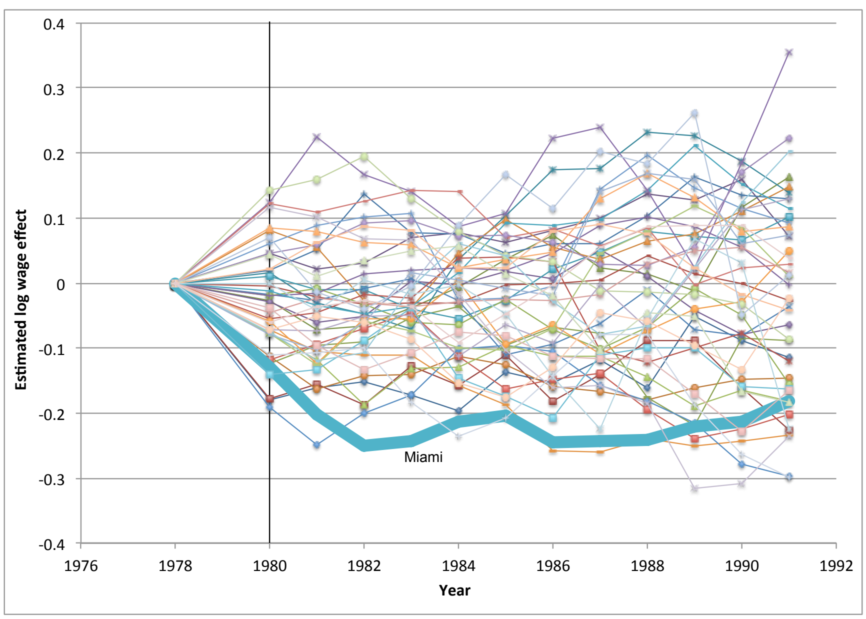
Notes: Each year between 1980 and 1995 is assumed to be a potential treatment year. The pre-treatment period lasts 3 years; the post-treatment period lasts 6 years. The wage effect is estimated from a difference-in-differences regression model that excludes the year of the treatment. The frequency distributions do not include any of the wage effects estimated in the Miami metropolitan area.

Figure 11. Effect of hypothetical supply shock in 1980 on log wage of high school dropouts in each metropolitan area, relative to synthetic placebo

A. March CPS



B. CPS-ORG



Notes: The exercise traces the wage effect of a supply shock in treatment year 1980 in each of the 44 metropolitan areas. The wage effect is defined as the difference-in-differences Δw_{rt} - Δw_{r0}, where Δw_{rt} gives the log wage gap between city *r* and its synthetic placebo at time *t*; and Δw_{r0} gives the equivalent average log wage gap in pre-treatment years 1977-1979. Beginning in 1980, the illustrated wage effects represent a 3-year moving average.

Table 1. Education distribution of adult *Marielitos*

	Years of education				
Sample:	< 12	12	13 - 15	≥ 16	Sample size
Marielitos:					
April 1983 CPS	57.9	25.6	3.5	13.1	31
June 1986 CPS	55.2	28.0	6.4	9.6	31
June 1988 CPS	58.7	26.1	4.4	10.9	46
1990 Census	64.8	15.8	12.9	6.5	4,234
1994 CPS-ORG	61.4	20.5	9.8	8.3	143
2000 Census	59.9	20.0	12.7	7.4	3,301
Miami's pre-existing labor force:					
1980 Census	26.7	28.4	26.0	18.8	32,971

Notes: The statistics are calculated in the sample of persons born in Cuba who migrated to the United States at the time of Mariel and were 18 years old in 1980. In the April 1983 CPS and 2000 census, the Marielitos are identified as persons born in Cuba who migrated to the United States in 1980. In all other samples, the Marielitos are identified as Cubans who entered the country in 1980 or 1981. The pre-existing labor force of Miami includes both natives and immigrants.

Table 2. The size of the Mariel supply shock

Education group:	Size of Miami's labor force in 1980 (1000s)	Number of *Marielitos* in labor force (1000s)	Percent increase in supply
High school dropouts	176.3	32.5	18.4
High school graduates	187.5	10.1	5.4
Some college	171.5	8.8	5.1
College graduates	124.1	4.2	3.4
All workers	659.4	55.7	8.4

Notes: The pre-existing number of native workers in Miami is calculated from the 1980 census; the number of *Marielito* workers (at least 18 years old at the time of Mariel) is calculated from the 1990 census, and a small adjustment is made because the 1990 census reports the number of Cuban immigrants who entered the country in 1980 or 1981.

Table 3. Rates of employment and wage growth before Mariel

Rank	Metropolitan area	Employment growth: all workers	Employment growth: high school dropouts	Wage growth: high school dropouts
1	San Diego, CA	0.194	0.067	-0.093
2	Greensboro-Winston Salem, NC	0.182	-0.063	-0.307
3	Kansas City, MO/KS	0.179	0.052	-0.191
4	Anaheim-Santa Ana- Garden Grove, CA	0.162	0.257	0.067
5	Rochester, NY	0.153	-0.172	0.065
6	**Miami-Hialeah, FL**	**0.153**	**0.086**	**0.014**
7	Nassau-Suffolk, NY	0.151	0.056	-0.057
8	San Jose, CA	0.137	0.130	0.124
9	Albany-Schenectady-Troy, NY	0.130	0.065	0.058
10	Boston, MA	0.121	-0.100	-0.008
11	Milwaukee, WI	0.121	-0.006	0.040
12	Indianapolis, IN	0.115	0.071	-0.032
13	Seattle-Everett, WA	0.110	-0.079	-0.051
14	Norfolk-Virginia Beach-Newport News, VA	0.103	0.052	0.111
15	Philadelphia, PA/NJ	0.102	-0.033	0.002
16	Newark, NJ	0.092	-0.116	-0.089
17	Tampa-St. Petersburg-Clearwater, FL	0.083	0.068	0.129
18	Denver-Boulder-Longmont, CO	0.082	-0.139	-0.012
19	Houston-Brazoria, TX	0.078	0.090	0.004
20	Sacramento, CA	0.078	0.152	-0.004
21	Dallas-Fort Worth, TX	0.076	0.062	-0.037
22	Portland-Vancouver, OR/WA	0.071	-0.074	-0.015
23	Riverside-San Bernardino, CA	0.071	-0.017	0.298
24	Atlanta, GA	0.069	-0.087	-0.062
25	Cincinnati-Hamilton, OH/KY/IN	0.063	0.038	-0.068
26	Washington, DC/MD/VA	0.061	0.028	0.082
27	Detroit, MI	0.060	-0.099	-0.010
28	Fort Worth-Arlington, TX	0.058	-0.006	-0.033
29	Los Angeles-Long Beach, CA	0.056	0.075	-0.112
30	Columbus, OH	0.048	-0.324	-0.016
31	Buffalo-Niagara Falls, NY	0.039	0.040	-0.143
32	Chicago-Gary-Lake IL	0.025	-0.082	-0.017
33	St. Louis, MO/IL	0.019	-0.060	-0.023
34	Bergen-Passaic, NJ	0.015	-0.051	0.011
35	Baltimore, MD	0.012	-0.108	-0.016
36	Minneapolis-St. Paul, MN	0.007	-0.050	-0.010
37	Cleveland, OH	0.001	-0.071	-0.017
38	New York, NY	0.000	-0.146	0.069
39	Pittsburg, PA	-0.013	-0.111	0.127
40	Birmingham, AL	-0.020	-0.172	-0.090
41	San Francisco-Oakland-Vallejo, CA	-0.027	-0.200	-0.102
42	Gary-Hammond-East Chicago, IN	-0.029	0.119	0.042
43	New Orleans, LA	-0.046	-0.313	-0.038
44	Akron, OH	-0.110	-0.351	-0.004

Notes: The rate of employment growth is the log ratio of average employment in 1979-1980 to average employment in 1977-1978, calculated using the March CPS from the 1977-1980 survey years. The rate of wage growth is the difference in the (age-adjusted) log weekly wage between 1978-1979 and 1976-1977.

Table 4. Distribution of wage changes within metro areas across all potential city-year permutations, 1976-2003

	Dependent variable: Log wage of education group			
Characteristics of distribution:	< 12 years	12 years	13-15 years	≥ 16 years
Value for Mariel	-0.439	-0.025	-0.116	-0.062
Distribution across all city-year permutations				
Mean of distribution outside Miami	-0.100	-0.061	-0.024	0.006
Percentile of Mariel effect	1.8	65.6	19.0	20.7
Distribution in treatment year 1980:				
Mean of distribution outside Miami	-0.170	-0.152	-0.092	-0.043
Ranking of Mariel effect	1/44	42/44	14/44	17/44

Notes: The summary statistics are calculated from the distribution of wage changes between the pre- and post-period for all metropolitan areas (excluding Miami) for all possible permutations in the 1976-2003 March CPS data. The pre-treatment period lasts 4 years; the post-treatment period lasts 6 years; and the year of the treatment is excluded from the calculation. The distributions have 774 observations.

Table 5. Difference-in-differences impact of the *Marielitos* on the wage of high school dropouts, March CPS

Dependent variable and treatment period	Card placebo	Employment placebo	Synthetic placebo	All cities
A. Log wage of high school dropouts				
1981-1983	-0.137	-0.289	-0.210	-0.135
	(0.093)	(0.090)	(0.086)	(0.080)
1984-1986	-0.364	-0.495	-0.461	-0.378
	(0.080)	(0.071)	(0.077)	(0.033)
1987-1989	-0.216	-0.251	-0.210	-0.192
	(0.085)	(0.071)	(0.068)	(0.058)
1990-1992	0.188	0.096	0.021	0.188
	(0.158)	(0.136)	(0.096)	(0.111)
B. Log wage relative to college graduates				
1981-1983	-0.168	-0.390	-0.269	-0.180
	(0.187)	(0.164)	(0.193)	(0.170)
1984-1986	-0.387	-0.593	-0.552	-0.453
	(0.159)	(0.154)	(0.193)	(0.135)
1987-1989	-0.340	-0.482	-0.387	-0.357
	(0.154)	(0.159)	(0.195)	(0.132)
1990-1992	0.180	0.084	0.191	0.192
	(0.223)	(0.200)	(0.141)	(0.180)
C. Log wage relative to high school graduates				
1981-1983	-0.276	-0.420	-0.383	-0.285
	(0.131)	(0.146)	(0.104)	(0.136)
1984-1986	-0.490	-0.627	-0.620	-0.470
	(0.114)	(0.093)	(0.097)	(0.094)
1987-1989	-0.344	-0.325	-0.298	-0.254
	(0.098)	(0.071)	(0.041)	(0.082)
1990-1992	0.067	0.016	-0.102	0.122
	(0.195)	(0.144)	(0.101)	(0.143)

Notes: Robust standard errors are reported in parentheses. The data consist of annual observations for each city between 1977 and 1992 (1980 excluded). All regressions include vectors of city and year fixed effects. The table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. The regressions that use the Card or employment placebos have 75 observations; the regressions that use the synthetic placebo have 30 observations; and the regressions in the last column have 658 observations. The regressions in Panel A are weighted by the number of observations size used to calculate the dependent variable. The regressions in Panels B and C are weighted by $(n_1 n_s)/(n_1 + n_s)$, where n_1 is the number of observations used to calculate the mean wage of high school dropouts in city r at time t, and n_s is the respective number of observations used to calculate the mean wage of the more highly educated group. The regressions that use the synthetic placebo are not weighted.

Table 6. Difference-in-differences impact of the *Marielitos* on the wage of high school dropouts, CPS-ORG

Dependent variable and treatment period	Card placebo	Employment placebo	Synthetic placebo	All cities
A. Log wage of high school dropouts				
1981-1983	-0.068	-0.153	-0.240	-0.092
	(0.027)	(0.060)	(0.082)	(0.027)
1984-1986	-0.032	-0.097	-0.203	-0.075
	(0.039)	(0.066)	(0.078)	(0.028)
1987-1989	-0.061	-0.206	-0.241	-0.137
	(0.031)	(0.055)	(0.075)	(0.018)
1990-1992	0.005	-0.105	-0.182	-0.051
	(0.058)	(0.078)	(0.075)	(0.041)
B. Log wage relative to college graduates				
1981-1983	-0.020	-0.171	-0.302	-0.066
	(0.059)	(0.114)	(0.117)	(0.069)
1984-1986	-0.018	-0.130	-0.275	-0.067
	(0.056)	(0.096)	(0.109)	(0.057)
1987-1989	-0.048	-0.291	-0.377	-0.152
	(0.055)	(0.111)	(0.112)	(0.061)
1990-1992	-0.017	-0.173	-0.283	-0.074
	(0.086)	(0.127)	(0.115)	(0.077)
C. Log wage relative to high school graduates				
1981-1983	-0.141	-0.188	-0.320	-0.130
	(0.033)	(0.072)	(0.088)	(0.041)
1984-1986	-0.084	-0.122	-0.242	-0.092
	(0.070)	(0.094)	(0.096)	(0.060)
1987-1989	-0.081	-0.173	-0.222	-0.108
	(0.041)	(0.061)	(0.077)	(0.039)
1990-1992	0.023	-0.082	-0.184	-0.033
	(0.069)	(0.062)	(0.076)	(0.061)

Notes: Robust standard errors are reported in parentheses. The data consist of annual observations for each city between 1977 and 1992 (1980 excluded). All regressions include vectors of city and year fixed effects. The table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. The regressions that use the Card or employment placebos have 75 observations; the regressions that use the synthetic placebo have 30 observations; and the regressions in the last column have 660 observations. The regressions in Panel A are weighted by the sample size used to calculate the dependent variable. The regressions in Panels B and C are weighted by $(n_1 n_S)/(n_1 + n_S)$, where n_1 is the number of observations used to calculate the mean wage of high school dropouts in city r at time t, and n_S is the respective number of observations used to calculate the mean wage of the more highly educated group. The regressions that use the synthetic placebo are not weighted.

Table 7. Difference-in-differences short-run impacts of the *Marielitos*

Sample	Card placebo	Employment placebo	Synthetic placebo	All cities
A. Coefficients from March CPS				
1. Log wage of high school dropouts	-0.237	-0.374	-0.335	-0.237
	(0.088)	(0.078)	(0.090)	(0.076)
2. Log wage of "pooled" high school dropouts and graduates	-0.030	-0.064	-0.035	-0.039
	(0.049)	(0.063)	(0.082)	(0.059)
3. Interquantile range (20th – 80th percentile)	-0.062	-0.029	-0.024	-0.044
	(0.108)	(0.096)	(0.098)	(0.100)
4. Black/white relative wage	-0.107	-0.221	---	-0.092
	(0.072)	(0.098)		(0.061)
B. Coefficients from CPS-ORG				
1. Log wage of high school dropouts	-0.054	-0.130	-0.227	-0.087
	(0.026)	(0.049)	(0.071)	(0.022)
2. Log wage of pooled high school dropouts and graduates	-0.003	-0.052	-0.050	-0.036
	(0.018)	(0.019)	(0.022)	(0.014)
3. Interquantile range (20th – 80th percentile)	0.017	-0.031	-0.063	-0.037
	(0.041)	(0.051)	(0.059)	(0.040)
4. Black/white relative wage	-0.134	-0.135	-0.110	-0.127
	(0.037)	(0.051)	(0.045)	(0.037)

Notes: Robust standard errors are reported in parentheses. The data consist of annual observations for each city between 1977 and 1986 (1980 excluded). All regressions include vectors of city and year fixed effects. The table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and if the observation is drawn from the post-Mariel period. The regressions that use the Card or employment placebos have 45 observations; the regressions that use the synthetic placebo have 18 observations; and the regressions in the last column have 396 observations. See the notes to Table 6 for a description of the weighting used in the regressions.

Table 8. The distribution of estimated short-run wage effects on the log weekly wage of high school dropouts across all four-city placebos, 1977-1986

Characteristics of distribution:	March CPS	CPS-ORG
Mean	-0.243	-0.089
Standard deviation	0.047	0.023
Statistical significance		
Fraction of t-statistics above |1.6|	0.984	0.947
Fraction of t-statistics above |2.0|	0.939	0.845
Average employment growth of placebo cities within 0.5 standard deviations of Miami (N = 5,740)		
Mean	-0.282	-0.105
Fraction of t-statistics above |1.6|	0.998	0.933
Fraction of t-statistics above |2.0|	0.980	0.812
Employment growth for *each* placebo city within 0.5 standard deviations of Miami (N = 126)		
Mean	-0.333	-0.098
Fraction of t-statistics above |1.6|	1.000	0.833
Fraction of t-statistics above |2.0|	1.000	0.619
Actual impact using the Card placebo:		
Coefficient	-0.237	-0.054
Robust standard error	(0.088)	(0.026)
Actual impact using the employment placebo:		
Coefficient	-0.374	-0.130
Robust standard error	(0.078)	(0.049)
Actual impact using the synthetic placebo:		
Coefficient	-0.335	-0.227
Robust standard error	(0.090)	(0.071)

Notes: The table reports the distribution of the interaction coefficient between a dummy variable indicating if the metropolitan area is Miami and if the observation is drawn from the post-Mariel period. The regressions were estimated separately in all possible 123,410 four-city placebos. The regressions use annual observations for each city from 1977 through 1986 (excluding 1980). All regressions have 45 observations and are weighted by the sample size used to calculate the mean log age-adjusted wage of high school dropouts in city r at time t.

Table 9. Distribution of wage effects relative to the synthetic placebo for hypothetical supply shocks

	Dependent variable: Log wage of education group			
Characteristics of distribution:	< 12 years	12 years	13-15 years	≥ 16 years
A. March CPS				
Mean effect	-0.003	-0.000	-0.006	-0.000
Standard deviation	0.163	0.071	0.099	0.072
Fraction of t-statistics above $\lvert 1.6 \rvert$	0.250	0.301	0.262	0.278
Fraction of t-statistics above $\lvert 2.0 \rvert$	0.167	0.205	0.185	0.182
Mariel wage impact with synthetic control:				
Coefficient	-0.335	0.114	-0.042	0.104
Robust standard error	(0.090)	(0.076)	(0.116)	(0.086)
Placement of Mariel effect:				
Percentile in distribution across all years	3.0	94.8	33.2	93.2
Rank in 1980 treatment year	1/44	44/44	14/43	43/44
B. CPS-ORG				
Mean effect	0.001	0.000	0.002	-0.002
Standard deviation	0.104	0.049	0.065	0.053
Fraction of t-statistics above $\lvert 1.6 \rvert$	0.253	0.306	0.292	0.328
Fraction of t-statistics above $\lvert 2.0 \rvert$	0.154	0.218	0.194	0.214
Mariel wage impact with synthetic control:				
Coefficient	-0.227	0.042	0.071	0.052
Robust standard error	(0.072)	(0.021)	(0.057)	(0.061)
Placement of Mariel effect:				
Percentile in distribution across all years	1.8	81.6	89.4	84.8
Rank in 1980 treatment year	1/44	37/44	38/44	36/44

Notes: The pre-treatment period lasts 3 years; the post-treatment period lasts 6 years. Each regression excludes the year of the treatment and has 18 observations. There are 774 hypothetical shocks distributed across 43 metropolitan areas (outside Miami) for treatment years between 1980 and 1997. The predictors used to create the synthetic placebo are the city's rate of total employment growth, the rates of employment and wage growth for the particular education group in the 4-year period preceding the treatment year.

Appendix Table A-1. Weights defining the synthetic control

Rank	Metropolitan area	Measure of wage of high school dropouts: Actual log weekly wage		
			Age-adjusted log weekly wage	
		March CPS	March CPS	CPS-ORG
1	San Diego, CA	0.015	0.239	0.234
2	Greensboro-Winston Salem, NC	0.000	0.006	0.000
3	Kansas City, MO/KS	0.560	0.010	0.016
4	Anaheim-Santa Ana- Garden Grove, CA	0.203	0.372	0.396
5	Rochester, NY	0.004	0.159	0.164
6	**Miami-Hialeah, FL**	**---**	**---**	---
7	Nassau-Suffolk, NY	0.013	0.011	0.013
8	San Jose, CA	0.009	0.043	0.007
9	Albany-Schenectady-Troy, NY	0.006	0.012	0.010
10	Boston, MA	0.005	0.008	0.010
11	Milwaukee, WI	0.005	0.009	0.010
12	Indianapolis, IN	0.008	0.007	0.007
13	Seattle-Everett, WA	0.005	0.006	0.008
14	Norfolk-Virginia Beach-Newport News, VA	0.005	0.008	0.007
15	Philadelphia, PA/NJ	0.005	0.006	0.008
16	Newark, NJ	0.004	0.005	0.006
17	Tampa-St. Petersburg-Clearwater, FL	0.005	0.006	0.006
18	Denver-Boulder-Longmont, CO	0.004	0.005	0.006
19	Houston-Brazoria, TX	0.007	0.005	0.005
20	Sacramento, CA	0.041	0.005	0.002
21	Dallas-Fort Worth, TX	0.007	0.005	0.005
22	Portland-Vancouver, OR/WA	0.004	0.005	0.005
23	Riverside-San Bernardino, CA	0.002	0.000	0.007
24	Atlanta, GA	0.004	0.004	0.005
25	Cincinnati-Hamilton, OH/KY/IN	0.007	0.004	0.005
26	Washington, DC/MD/VA	0.005	0.004	0.005
27	Detroit, MI	0.004	0.004	0.005
28	Fort Worth-Arlington, TX	0.005	0.004	0.005
29	Los Angeles-Long Beach, CA	0.007	0.004	0.004
30	Columbus, OH	0.002	0.004	0.003
31	Buffalo-Niagara Falls, NY	0.006	0.003	0.004
32	Chicago-Gary-Lake IL	0.004	0.003	0.004
33	St. Louis, MO/IL	0.004	0.003	0.003
34	Bergen-Passaic, NJ	0.003	0.003	0.003
35	Baltimore, MD	0.004	0.003	0.003
36	Minneapolis-St. Paul, MN	0.004	0.003	0.003
37	Cleveland, OH	0.003	0.003	0.003
38	New York, NY	0.003	0.003	0.003
39	Pittsburg, PA	0.002	0.003	0.003
40	Birmingham, AL	0.003	0.002	0.002
41	San Francisco-Oakland-Vallejo, CA	0.003	0.002	0.002
42	Gary-Hammond-East Chicago, IN	0.004	0.002	0.002
43	New Orleans, LA	0.002	0.002	0.001
44	Akron, OH	0.001	0.001	0.001

Notes: The metropolitan areas are ranked by the 1977-1980 rate of employment growth.