

THE WAGE IMPACT OF THE *MARIELITOS*: A REAPPRAISAL

GEORGE J. BORJAS*

This article brings a new perspective to the analysis of the wage effects of the Mariel boatlift crisis, in which an estimated 125,000 Cuban refugees migrated to Florida between April and October, 1980. The author revisits the question of wage impacts from such a supply shock, drawing on the cumulative insights of research on the economic impact of immigration. That literature shows that the wage impact must be measured by carefully matching the skills of the immigrants with those of the incumbent workforce. Given that at least 60% of the *Marielitos* were high school dropouts, this article specifically examines the wage impact for this low-skill group. This analysis overturns the prior finding that the Mariel boatlift did not affect Miami's wage structure. The wage of high school dropouts in Miami dropped dramatically, by 10 to 30%, suggesting an elasticity of wages with respect to the number of workers between -0.5 and -1.5 .

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 because of the policy issues involved and because 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 impact of the Mariel boatlift 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

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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 people go, can help identify parameters of great 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 analysis of the Miami labor market, and of comparable 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. Card's study has 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; Altonji and Card 1991). These spatial correlations are problematic 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.²

Card's Mariel study is impervious to both of these criticisms. The fact that the *Marielitos* settled in Miami probably 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 many of the Cuban Americans who organized the flotilla lived in South Florida.³ Similarly, the short-run nature of Card's empirical exercise, 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

¹Many other studies examine exogenous supply shocks and are clearly influenced by the Card framework; see Hunt (1992), Carrington and de Lima (1996), Friedberg (2001), Angrist and Kugler (2003), Saiz (2003), Borjas and Doran (2012), Glitz (2012), Pinotti, Gazzè, Fasani, and Tonello (2013), and Dustmann, Schoänberg, and Stuhler (2017).

²Beginning with Altonji and Card (1991), the endogenous settlement of immigrants across localities has been addressed by using the geographic sorting of earlier waves of immigrants to predict the sorting of the new arrivals. The validity of this instrument, however, depends on whether economic conditions in local labor markets persist over time; see Jaeger, Ruist, and Stuhler (2016).

³The geographic clustering of the large Cuban community in Miami probably had little to do with the relative economic opportunities offered by the Miami labor market, and much to do with the fact that the flights operated by Pan American Airways that carried almost all of the early refugees out of Cuba had a single destination, the Miami airport. The 1980 census indicates that 50% of Cuban immigrants lived in the Miami metropolitan area. Both the 1990 and 2000 censuses report that more than 60% of the Cuban immigrants who likely were part of the Mariel influx still resided in the Miami metropolitan area.

contaminated by labor market adjustments and that economic theory predicts to be negative.

This article provides a reappraisal of the evidence of how the Miami labor market responded to the influx of *Marielitos*. The article is not a replication of 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 incumbent 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 *Marielitos* were disproportionately low-skill workers; approximately 60% were high school dropouts and only 10% were college graduates. At the time, about a quarter of Miami's 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%, it increased the number of high school dropouts by almost 20%.

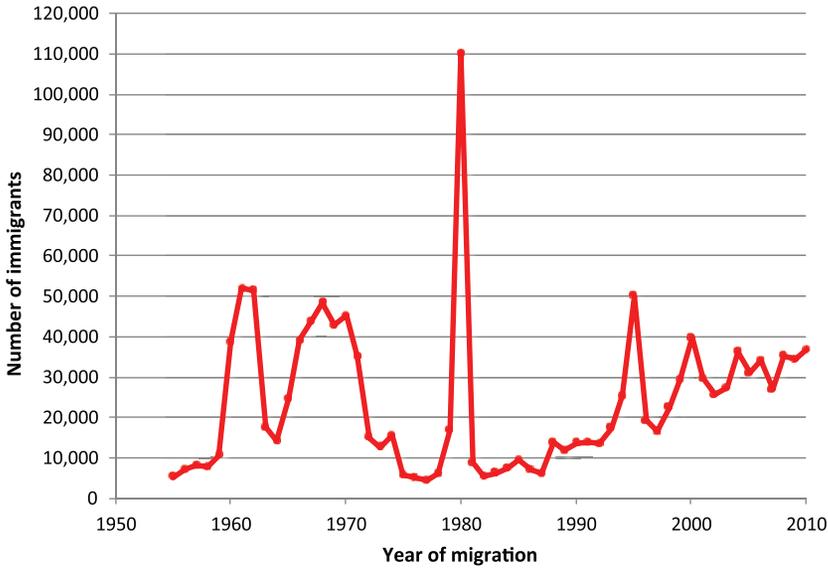
The unbalanced nature of this supply shock suggests that we should look at what happened to the wage of high school dropouts in Miami before and after Mariel. Remarkably, this trivial comparison was not made in Card's (1990) study and, to the best of my knowledge, had not been conducted before the first draft of this article was released.⁴ By focusing on this very specific skill group, we obtain an entirely new perspective of how the Miami labor market responded to an exogenous supply shock.

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, more than 1.3 million Cubans had emigrated.

⁴Card (1990, table 7) reported labor market outcomes for the subsample of black high school dropouts but does not report any other pre- or post-Mariel differences for the least-educated workers. Monras (2014) examined wage trends in the sample of workers who have at most a high school diploma; his evidence is suggestive of the findings reported in this article. Finally, three months after this study was released as an NBER working paper (Borjas 2015), Peri and Yasenov (2015) re-examined the data for high school dropouts and concluded that Card's results stood the test of time. The Peri and Yasenov study, however, looked at wage trends that were contaminated by various sampling decisions. They examined the pooled earnings of men and women, but ignored the changing gender composition of the workforce; included non-Cuban Hispanics in the analysis, even though many of those Hispanics were immigrants who arrived *after* 1980; and included teenagers aged 16 to 18, even though most of these teenagers were enrolled in high school and were classified as high school dropouts because they did not yet have a diploma. Borjas (2016) documented the biases created by these methodological choices.

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. 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), and the 2009–2010 counts are drawn from the 2012 ACS.

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 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 Cubans 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 five-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 the 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.

⁵In principle, the calculation also adjusts for potential out-migration by 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).

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

The huge spike in 1980, of course, is the Mariel boatlift 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 what Angrist and Krueger (1999) erroneously labeled the “Mariel Boatlift That Did Not Happen.”⁷ The census data clearly indicate 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. A steady increase in the number of Cuban migrants since the early 1980s is also evident. By 2010, about 40,000 Cubans were arriving annually.

One last detail is worth noting. A disproportionately large number of the *Marielitos* ended up residing in the Miami metropolitan area: 62.6% resided in Miami in 1990 and 63.4% still resided there in 2000.

The empirical analysis initially uses the 1977 to 1993 March Supplements of the Current Population Surveys (CPS).⁸ These surveys report a person’s annual wage and salary income as well as the number of weeks worked in the previous calendar year. Much of the analysis will be restricted to men aged 25 to 59, who report positive values for wage and salary income, weeks worked, and usual hours worked.⁹ The age restriction implies that a worker’s observed earnings are unaffected by transitory fluctuations that occur during the transitions from school to work and from work to retirement. Similarly, the restriction to working men ensures that wage trends are not distorted by the entry of large numbers of women into the workforce in the 1970s and 1980s.¹⁰

The 1977 to 1993 period analyzed throughout much of the article is selected for two reasons. First, although the March CPS data files are available since 1962, the Miami metropolitan area can be consistently identified only in the 1973 to 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.¹¹ Second, my analysis of wage trends stops with

⁷In 1994, Castro again toyed with the idea of permitting a boatlift, but the United States preemptively responded and rerouted many of the potential migrants to the military base in Guantanamo. Angrist and Krueger (1999), writing *before* the release of the 2000 census, argued that this seemingly phantom flow had an adverse effect on Miami’s black unemployment rate, raising questions about how to interpret the evidence from the Mariel boatlift that did happen. The 2000 census indicates that many of the rerouted immigrants eventually made their way to the United States.

⁸The March surveys are known as the Annual Social and Economic Supplements (ASEC). The data were downloaded from the Integrated Public Use Microdata Series (IPUMS) website on May 9, 2016.

⁹I also exclude persons who reside in group quarters, have a negative sample weight, or have outlying hourly wage rates (less than \$1.50 or more than \$40 in 1980 dollars). This last restriction roughly excludes workers in the top and bottom 1% of the earnings distribution.

¹⁰The sensitivity of the evidence to including working women is discussed below.

¹¹The 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.

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 I cannot measure the wage impact of the Mariel supply shock on the native-born population. I instead examine wage trends among non-Hispanic men (Hispanic background is determined by a person's answer to the Hispanic ethnicity question), a sample restriction that comes close to isolating Miami's native-born population at the time. The 1980 census, conducted days before the Mariel supply shock, reported that 40.7% of Miami's male workforce was foreign-born, with 65.1% of those immigrants born in Cuba and another 11.2% percent born in other Latin American countries. The restriction to non-Hispanics also ensures that the post-Mariel wage trends are not skewed by a composition effect created by the large post-1980 wave of new Hispanic immigrants into many local labor markets.¹²

The dependent variable will be the worker's log weekly earnings, for which weekly earnings are defined as the ratio of annual income in the previous calendar year to the number of weeks worked.¹³ I use the Consumer Price Index (CPI) for all urban consumers to deflate the earnings data (1980 = 100). For expositional consistency and unless otherwise noted, whenever I refer to a particular year hereafter, it will be the year in which earnings were actually received by a worker, as opposed to the CPS survey year.¹⁴

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, which means the first large survey that contains a large sample of the *Marielitos* is the 1990 census. Nevertheless, a few CPS supplements conducted in the 1980s provide information on a very small sample of Cuban immigrants who arrived at the time of Mariel.

Table 1 reports 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

¹²The restriction to non-Hispanics implies that few of the low-skill workers in the sample are foreign-born. According to the 1980 census, only 18.7% of non-Hispanic high school dropouts in Miami were foreign-born (as compared to 6.4% outside Miami). By contrast, 55.1% of the non-Hispanic dropouts in Miami were black (as compared to 17.6% outside Miami). The key native-born group potentially affected by the *Marielitos*, therefore, was the low-skill African American workforce.

¹³I replicated the analysis using the log hourly wage as an alternative measure of a worker's income. Because the measure of usual hours worked weekly in the March CPS is noisy, the results are similar but less precisely estimated.

¹⁴For example, a discussion of the earnings of workers in 1985 refers to the data drawn from the 1986 March CPS.

Table 1. Education Distribution of Adult *Marielitos*

Sample	Years of education				Sample size
	< 12	12	13 – 15	≥ 16	
<i>Marielitos</i>					
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.

being high school dropouts.¹⁵ Despite the variation in sample size and the almost 20-year span in the surveys examined in the table, the fraction of *Marielitos* who lacked a high school diploma hovers around 60%. Table 1 also shows that a small fraction of these immigrants were college graduates (around 10%).

It is insightful to compare the education distribution of the *Marielitos* with that of the incumbent workforce. The last row of Table 1 shows that 26.7% of labor force participants in the Miami metropolitan area were high school dropouts. Miami's workforce was remarkably balanced in terms of its skill distribution, with 20 to 30% of workers in each of the four education groups.¹⁶

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% were high school dropouts.¹⁷ Although the Mariel supply shock increased Miami's workforce by 8.4% and increased the number of the most-educated workers by 3 to 5%, the number of low-skill workers rose

¹⁵The skills of the *Marielitos* may have been further downgraded upon arrival, as found in Dustmann, Fratini, and Preston (2013), so that even those immigrants with a high school diploma were competing with the least-educated workers in the pre-existing Miami workforce.

¹⁶The pre-existing workforce includes all labor force participants in Miami, regardless of where they were born or their ethnicity.

¹⁷The 2000 census indicates that 92.8% of the Cubans who immigrated in either 1980 or 1981 actually entered the country in 1980. Note that the supply shock was probably slightly larger than indicated in Table 2 because the calculation does not account for mortality through 1990.

Table 2. Size of the Mariel Supply Shock

<i>Education group</i>	<i>Size of Miami's labor force in 1980 (1000s)</i>	<i>Number of Marielitos in labor force (1000s)</i>	<i>Percentage increase in supply</i>
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 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.

by a remarkable 18.4%. Moreover, this supply shock occurred almost overnight. The Coast Guard reports that the first *Marielitos* arrived in Florida on April 23, 1980, and that more than 100,000 refugees had been admitted by June 3, 1980 (Stabile and Scheina 2015).

Descriptive Evidence

The very low skills of the *Marielitos* indicate that we should perhaps focus our attention on the labor market outcomes of the least-educated workers in Miami to establish a first-order sense of whether the supply shock had any impact on Miami's wage structure. The literature sparked by Borjas (2003) supports the importance of matching 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 runs into 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 the calculations. The top panel of Table 3 reports the number of men who satisfy the sample restrictions and who were enumerated in the Miami metropolitan area between survey years 1977 and 1993. The average number of observations in each pre-1990 March CPS was 96.5, and the average number of high school dropouts was 19.5. The sample size drops dramatically with the 1991 survey, when the number of non-Hispanic men sampled in Miami falls abruptly (by nearly a third), and the number of high school dropouts sometimes drops to the single digits. The empirical analysis reported below will effectively pool at least three years of the CPS (either by manually aggregating the data or by estimating impacts for three-year intervals) to enable a more precise calculation of the effect.

I begin by carrying out the most straightforward calculation of the potential wage impact that uses all the available data. In particular, I simply calculate the average log weekly wage of high school dropouts in Miami each

Table 3. Number of Observations in the Miami-Hialeah Metropolitan Area

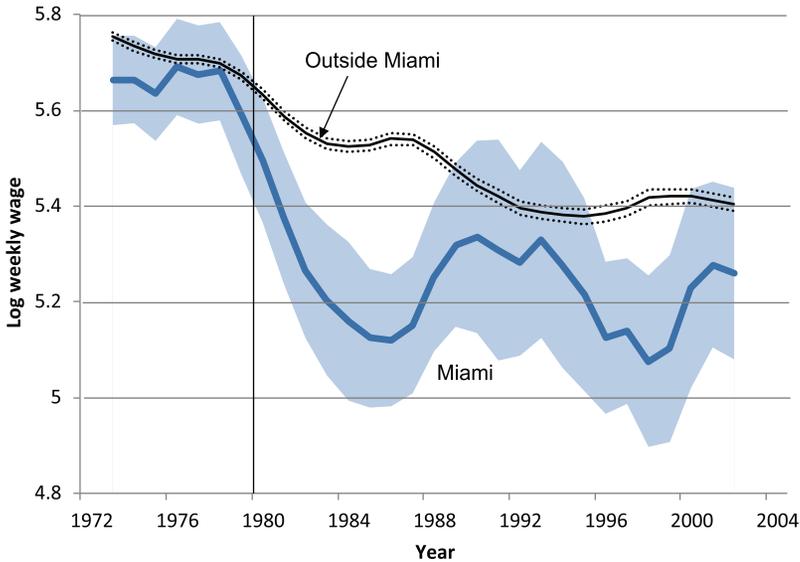
Survey year	March CPS		May CPS and CPS-ORG	
	All workers	High school dropouts	All workers	High school dropouts
A. Men				
1977	104	23	66	16
1978	101	26	66	12
1979	94	22	216	56
1980	88	17	245	55
1981	93	18	237	51
1982	93	20	209	39
1983	93	27	212	50
1984	93	18	209	48
1985	101	16	135	26
1986	99	15	231	36
1987	91	16	234	46
1988	102	18	264	37
1989	102	17	247	37
1990	98	16	223	38
1991	77	4	191	24
1992	84	10	177	20
1993	84	10	154	15
B. Men and women				
1977	190	36	124	24
1978	175	42	117	24
1979	190	40	427	95
1980	180	30	450	88
1981	179	30	447	84
1982	175	37	414	73
1983	157	45	410	88
1984	170	28	420	77
1985	188	27	249	42
1986	205	31	512	59
1987	199	30	496	75
1988	213	37	545	71
1989	198	36	504	77
1990	197	35	475	82
1991	171	17	441	60
1992	178	18	398	44
1993	173	19	348	38

Notes: The table reports the number of observations for the Miami-Hialeah metropolitan area of non-Hispanic workers, aged 25–59, who reported positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly (in the March CPS), or positive usual weekly earnings and positive usual hours worked weekly (in the May/ORG data).

year between 1972 and 2002, the period for which the March CPS has a consistent time series for the Miami metropolitan area.¹⁸ Figure 2 illustrates the wage trend and the 95% confidence band around the mean, using a three-year moving average to smooth out the noise in the time series. The

¹⁸The calculation of the average log weekly wage for each year weighs each individual observation by the product of the person's sampling weight times the number of weeks worked in the previous calendar year.

Figure 2. Log Wage of High School Dropouts, 1972–2003



Source: Data are drawn from the March CPS files.

Notes: 95% confidence band. Log weekly wage is a three-year moving average of the average log wage of high school dropouts in each geographic area.

figure also shows the trend for similarly educated non-Hispanic men working outside Miami. Note that this simple exercise does not adjust the CPS data in any way (other than taking a moving average), so it provides a transparent indication of what happened to low-skill wages in pre- and post-Mariel Miami.

Despite the similarity in wage trends between Miami and the rest of the country prior to 1980, and despite the small sample size for the Miami metropolitan area, it is obvious that something happened in 1980 that caused the two wage series to diverge in a statistically significant way. Before Mariel, the log wage of high school dropouts in Miami was about 0.1 log points below that of workers in the rest of the country. By 1985, the gap had widened to 0.4 log points, implying that whatever caused the divergence had lowered the relative wage of low-skill workers in Miami by about 30%. The low-skill wage in Miami fully recovered by 1990, only to be hammered again in 1995, perhaps coincidentally with 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 about 0.1 log points.

Of course, Miami's distinctive wage trend may not appear quite as distinctive when contrasted with what happened in other specific cities. The comparison of Miami to the aggregate U.S. labor market may be masking part of the variation that influences particular localities and that disappears when averaged out. It may be important, therefore, to create a control group of comparable cities unaffected by the Mariel supply shock to

determine if the wage trends evident in Miami were attributable to macroeconomic factors that affected other similar communities as well.

Beginning with the 1977 survey, the March CPS identifies 43 other metropolitan areas that can be combined in some fashion to construct a sort of placebo. Card (1990: 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*.

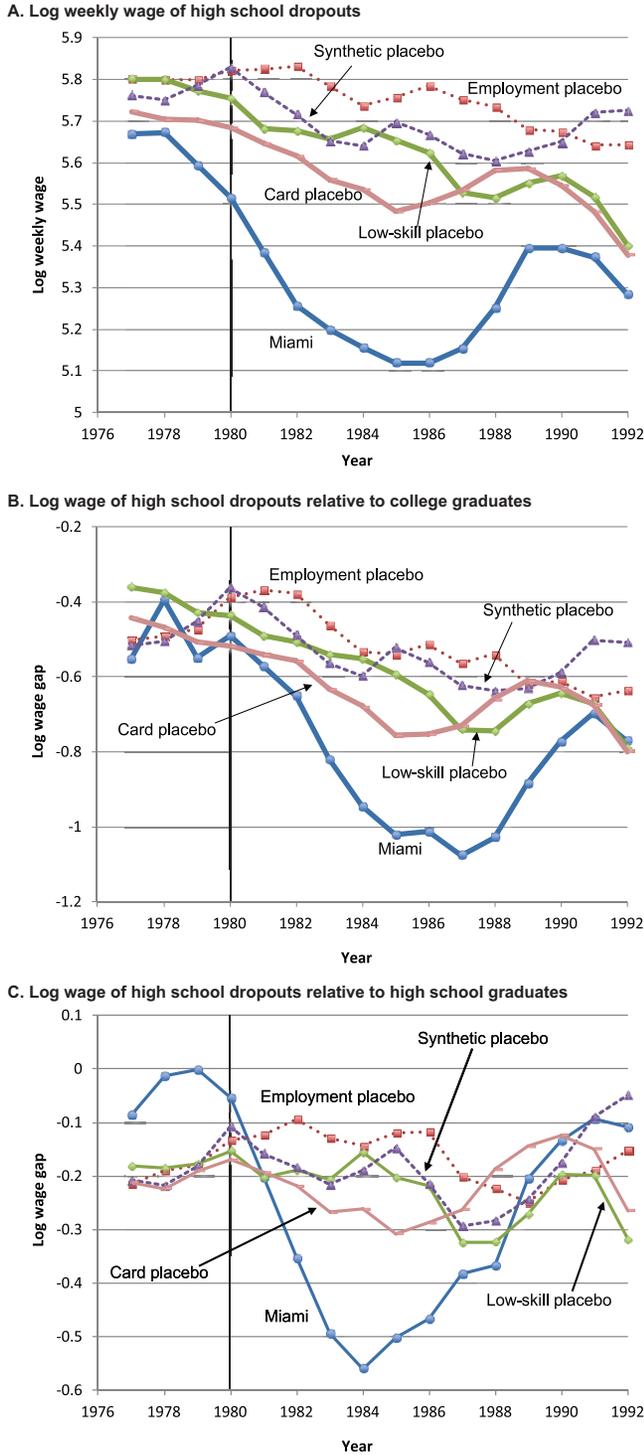
It is important to emphasize that the four cities in the Card placebo were chosen based partly on employment trends observed *after* the Mariel supply shock. In other words, if Mariel worsened employment conditions in Miami, the Card placebo is comparing the poorer outcomes of workers in Miami to the outcomes of workers in cities where some other factor worsened their opportunities as well. It is 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 panels of Figure 3 illustrate the wage trends in Miami and several potential placebos between 1976 and 1992.¹⁹ 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 these ups and downs may 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. In sum, the wage trends observed in Miami consistently indicate that the 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

¹⁹The calculation of wage trends in Figures 2 and 3 differs slightly. To calculate the standard error of the (moving) average in Figure 2, I computed the average by pooling three years of micro data. The data in Figure 3, which are used in the regression analysis reported below, calculate the mean log wage for each survey and then take a simple moving average of those means.

Figure 3. Trends in the Wage of Low-Skill Workers in the March CPS, 1977–1992



Notes: Figures use a three-year moving average of the average log wage of high school dropouts, high school graduates, and college graduates in each specific geographic area.

Mariel, I pooled the 1977 and 1978 surveys of the CPS and also pooled the 1979 and 1980 surveys. I then used the pooled surveys to calculate the log of the ratio of the total number of workers in 1979–1980 to the number of workers in 1977–1978.²⁰ Column 1 of Table 4 reports the employment growth rate for each of the 44 metropolitan areas, ranked by the growth rate.

Miami's pre-Mariel employment conditions were quite robust, ranking 6th in the rate of employment growth. Note that all the cities that made up the Card placebo had lower growth rates than Miami had between 1977 and 1980. The average employment growth rate in those four cities (Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg) (weighted by the city's employment) was 6.9%, less than half the 15.3% growth rate in Miami.

I use the rankings reported in Table 4 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 made 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 0.25 log points (or 22%), but the decline was 0.43 log points (35%) when compared to the cities in the employment placebo. This difference is not surprising; by construction, the comparison of post-Mariel economic conditions in Miami to that of cities where employment conditions are also poor inevitably hides some of the impact of the *Marielitos*.

I also constructed an alternative "low-skill placebo" by choosing the four cities that had similar pre-Mariel growth for *low-skill* employment. Column 2 of Table 4 reports the rate of employment growth for high school dropouts. Miami also ranked 6th by this metric. Coincidentally, two of the cities with similar low-skill employment growth are in the Card placebo (Los Angeles and Houston; the other two are Gary and Indianapolis). Figure 3 shows that the post-1980 Miami experience was also unusual when compared to this placebo, with a wage drop of 30% (which lies in between the effects implied by the Card and employment placebos).

The choice of a placebo obviously plays a crucial role in determining the magnitude of the wage impact of Mariel. An element of arbitrariness occurs in how a placebo is created, so the researcher's choice of a particular placebo can exaggerate or attenuate the wage effect. For example, 123,410 potential four-city placebos can be created from 43 metropolitan areas. It

²⁰A person is employed if he or she works in the CPS reference week. The 1980 survey, collected in March, is not affected by the supply shock, as the *Marielitos* did not begin to arrive until late April.

Table 4. Rates of Employment and Wage Growth before Mariel

<i>Rank</i>	<i>Metropolitan area</i>	<i>Employment growth: all workers</i>	<i>Employment growth: high school dropouts</i>	<i>Wage growth: high school dropouts</i>
1	San Diego, CA	0.194	0.067	-0.127
2	Greensboro-Winston Salem, NC	0.182	-0.063	-0.336
3	Kansas City, MO/KS	0.179	0.052	-0.178
4	Anaheim-Santa Ana-Garden Grove, CA	0.162	0.257	0.068
5	Rochester, NY	0.153	-0.172	0.019
6	Miami-Hialeah, FL	0.153	0.086	0.012
7	Nassau-Suffolk, NY	0.151	0.056	-0.047
8	San Jose, CA	0.137	0.130	0.138
9	Albany-Schenectady-Troy, NY	0.130	0.065	0.065
10	Boston, MA	0.121	-0.100	-0.024
11	Milwaukee, WI	0.121	-0.006	0.056
12	Indianapolis, IN	0.115	0.071	-0.048
13	Seattle-Everett, WA	0.110	-0.079	-0.051
14	Norfolk-Virginia Beach-Newport News, VA	0.103	0.052	0.110
15	Philadelphia, PA/NJ	0.102	-0.033	0.012
16	Newark, NJ	0.092	-0.116	-0.124
17	Tampa-St. Petersburg-Clearwater, FL	0.083	0.068	0.120
18	Denver-Boulder-Longmont, CO	0.082	-0.139	0.027
19	Houston-Brazoria, TX	0.078	0.090	0.003
20	Sacramento, CA	0.078	0.152	-0.034
21	Dallas-Fort Worth, TX	0.076	0.062	-0.050
22	Portland-Vancouver, OR/WA	0.071	-0.074	-0.020
23	Riverside-San Bernardino, CA	0.071	-0.017	0.254
24	Atlanta, GA	0.069	-0.087	0.014
25	Cincinnati-Hamilton, OH/KY/IN	0.063	0.038	-0.032
26	Washington, DC/MD/VA	0.061	0.028	0.090
27	Detroit, MI	0.060	-0.099	0.010
28	Fort Worth-Arlington, TX	0.058	-0.006	-0.059
29	Los Angeles-Long Beach, CA	0.056	0.075	-0.128
30	Columbus, OH	0.048	-0.324	-0.004
31	Buffalo-Niagara Falls, NY	0.039	0.040	-0.144
32	Chicago-Gary-Lake, IL	0.025	-0.082	-0.017
33	St. Louis, MO/IL	0.019	-0.060	-0.060
34	Bergen-Passaic, NJ	0.015	-0.051	0.007
35	Baltimore, MD	0.012	-0.108	-0.001
36	Minneapolis-St. Paul, MN	0.007	-0.050	-0.012
37	Cleveland, OH	0.001	-0.071	-0.014
38	New York, NY	0.000	-0.146	0.074
39	Pittsburg, PA	-0.013	-0.111	0.123
40	Birmingham, AL	-0.020	-0.172	-0.124
41	San Francisco-Oakland-Vallejo, CA	-0.027	-0.200	-0.105
42	Gary-Hammond-East Chicago, IN	-0.029	0.119	0.042
43	New Orleans, LA	-0.046	-0.313	-0.017
44	Akron, OH	-0.110	-0.351	-0.005

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

might be reasonable to expect a huge dispersion in the estimated wage effect of the *Marielitos* across the 123,410 potential comparisons. I will report below the distribution of estimated wage impacts across all potential

four-city placebos and show that Mariel had a negative impact regardless of which placebo is chosen.

Alternatively, one can employ the synthetic control method developed by Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010). The method essentially searches across all potential placebo cities and derives a weight that combines cities to create a new synthetic city. This synthetic city is the one that best resembles the pre-Mariel Miami labor market along some set of pre-specified conditions. Unlike the hands-on method I used to create the employment and low-skill placebos, the synthetic control allows the construction of the synthetic city to be based on several characteristics. The synthetic control approach seems to limit the researcher's ability to make arbitrary decisions about what the proper placebo should be. Note, however, that an element of arbitrariness still transpires. The researcher must specify the vector of variables that should be comparable between Miami and the placebo in the pre-treatment period. As I show below, different choices of control variables lead to different estimates of the wage impact.

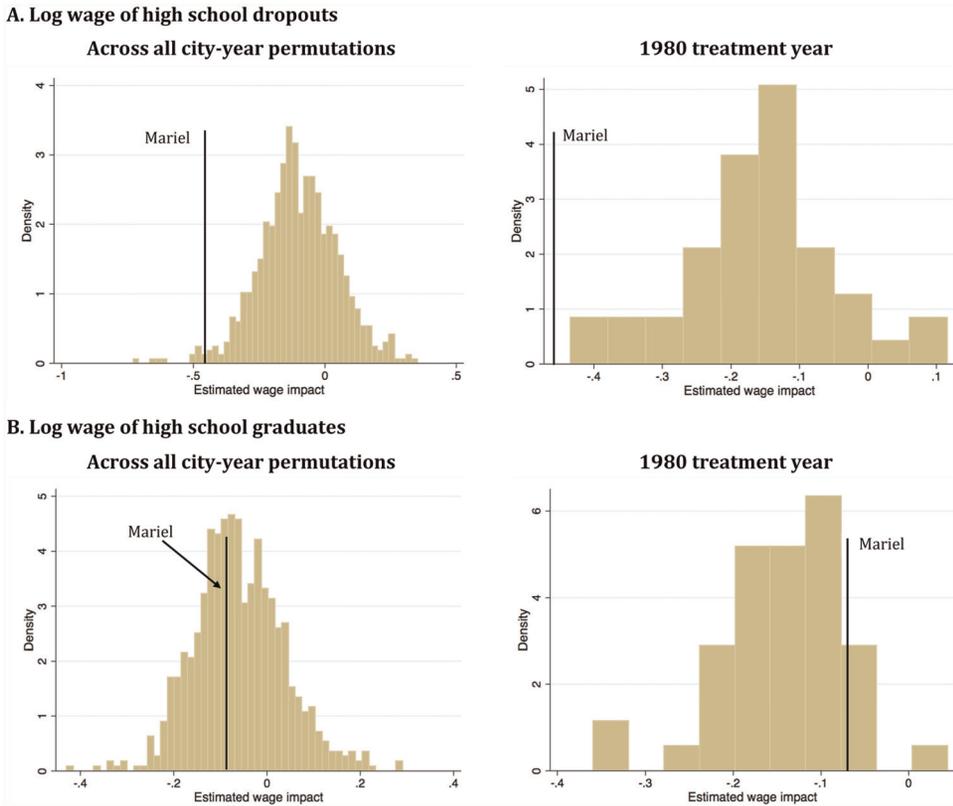
Initially, I construct the synthetic city by using three such control variables: the rate of employment growth in the four-year period prior to Mariel (i.e., the variable used to define the employment placebo); the concurrent rate of employment growth for high school dropouts (the variable used to define the low-skill placebo); and the concurrent rate of wage growth for high school dropouts. The last column of Table 4 shows that the low-skill market in Miami also had robust wage growth prior to Mariel, ranking 13th in the country. Figure 3 illustrates the wage trends in the "city" that makes up the synthetic control. The post-1980 Miami experience differed markedly from that of the synthetic control.

It is interesting to examine the weights attached to each city by the synthetic control method. When looking at the log wage of high school dropouts, the method assigns the largest weights to Anaheim (a weight of 0.40), Rochester (0.20), San Diego (0.18), and San Jose (0.06). The ranking of those cities in Table 4 indicates that the synthetic control method consistently selects metropolitan areas that had robust labor markets prior to Mariel.

Finally, I can establish that the steep drop in the low-skill wage in post-Mariel Miami was a *very* unusual event. The average wage of male high school dropouts in Miami fell by about 37% 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 to see if the Mariel experience stands out.²¹ If 37% wage cuts happen frequently in local labor markets, it would be harder to claim that Miami's experience had much to do with the *Marielitos*. Perhaps something else was going on—a

²¹Garthwaite, Gross, and Notowidigdo (2014) conducted a similar exercise to examine the distribution of the impact of an experiment in health insurance availability on employment lock.

Figure 4. Distribution of Pre- and Post-Mariel Wage Changes, 1976–2003



Source: Data are drawn from the March CPS files.

Notes: Pre-treatment period is four years; post-treatment period is six years; and year of the treatment is excluded from the calculation.

something else that other cities experience often enough at different times—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). Specifically, I pooled the March CPS data for the four years in each pre-treatment period and the six years in each post-treatment period, and calculated the average log wage in each city–period permutation. To duplicate the Mariel experiment, I skip a year between each four-year pre-treatment span and each six-year post-treatment span. The exercise generates a total of 774 possible events outside Miami (43 metropolitan areas and 18 potential treatment years between 1980 and 1997).

The top panel of Figure 4 illustrates the frequency distribution of all observed changes in the wage of male high school dropouts *outside* Miami. Between 1976–1979 and 1981–1986, the log wage of high school dropouts

in Miami fell by 0.463 log points (or 37%). Visually, we see that such a large wage drop was a rare event. The mean observed wage change across all city–period permutations was only about -0.16 log points. The Mariel experience ranks in the first percentile of the distribution of all observed wage changes between 1976 and 2003 across all metropolitan areas. And the frequency distribution for the 1980 treatment year shows that the wage drop observed in Miami at the time of Mariel was the largest wage drop observed among *all* metropolitan areas.

Equally important, the bottom panel of Figure 4 reveals that more-educated workers in Miami did *not* experience a substantial wage drop. Although it has been claimed that perhaps high school dropouts and high school graduates are perfect substitutes and should be pooled to form the low-skill workforce, the Mariel data clearly contradict this conjecture.²² The mean wage change in the log wage of high school graduates across all city–period permutations in the years 1976 through 2003 was -0.060 , and Miami’s Mariel experience ranked in the 43rd percentile. The value observed in the Miami metropolitan area at the time of Mariel was -0.080 , ranking 37th out of the 44 metropolitan areas in the distribution for treatment year 1980.

In short, something unique happened to the economic status of high school dropouts in Miami in the early 1980s. The event that shocked the wage structure in Miami at the time of Mariel, whatever it happened to be, occurs rarely and its adverse consequences were narrowly targeted on workers who lacked a high school diploma.

Robustness of the Descriptive Evidence

Given the striking picture that the raw data imply 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 is robust. I now address two distinct issues to evaluate the sensitivity of the results. First, 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)? Second, is the evidence robust to the inclusion of women in the analysis?

Results from the CPS-ORG

It is well known that wage trends recorded by the March CPS sometimes differ from the comparable wage trends recorded by the CPS ORG. Unlike the March CPS, which reports 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

²²See the contrasting arguments of Ottaviano and Peri (2012) and Borjas, Grogger, and Hanson (2012).

workers. The ORG time series begins in 1979, so that the pre-treatment period contains only one year of data. Following Autor, Katz, and Kearney (2008) and Lemieux (2006), I extend the pre-treatment period back by using the roughly comparable May CPS supplements for the pre-1979 years.²³

One key advantage of the ORG data is that they contain much larger samples, by construction, roughly three times the size of the March CPS. This advantage is somewhat neutralized, however, by the need to use the May CPS files to create a longer pre-treatment period. The May files, like the March CPS, are monthly surveys and both have equally small samples. In fact, as Table 3 shows, the number of high school dropouts enumerated in pre-Mariel Miami who satisfy the sample restrictions is *smaller* in the May CPS than in the March file. The presumed advantage of the ORG file is further neutralized because, in practice, the ORG does not triple the number of observations in the March CPS. The average ORG sample between 1981 and 1989 is only 2.2 times as large as the March file (an annual average of 41.1 compared to 18.3 observations). The fact that using the ORG does not triple the sample size indicates that some workers go missing in the ORG analysis.

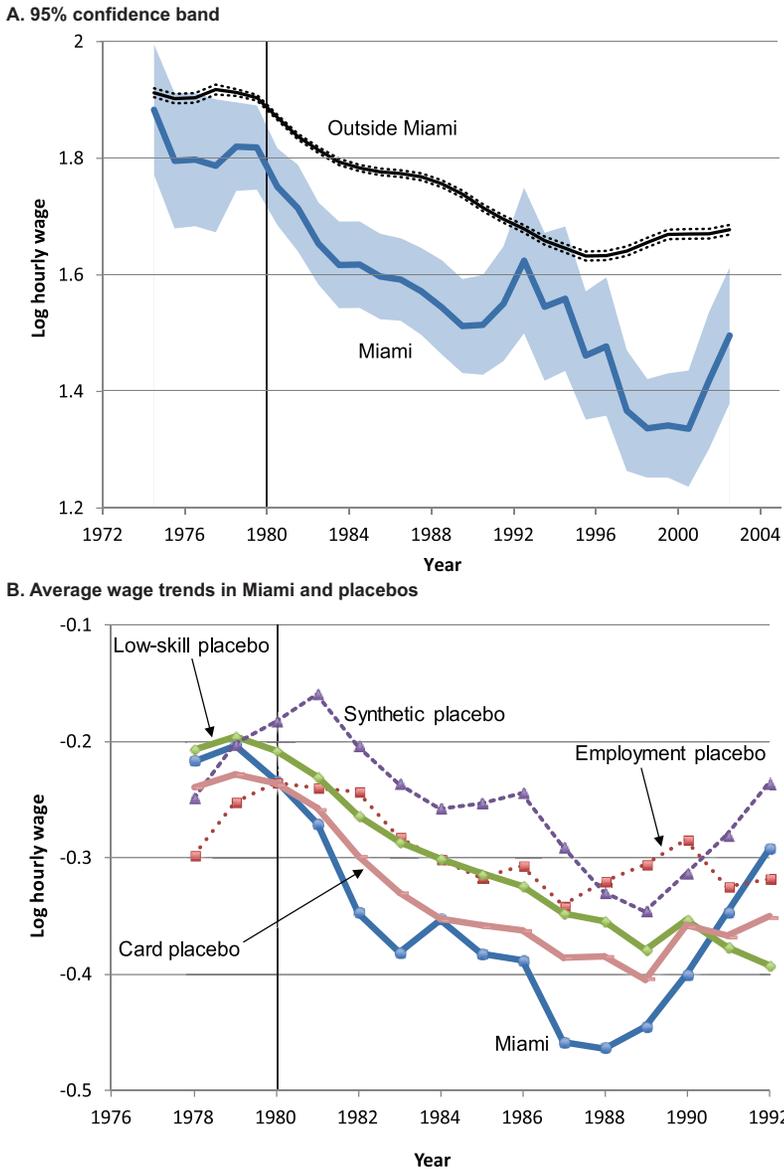
Those workers disappeared because the March CPS and the ORG measure different concepts of income, creating very different samples that can be used to analyze wage trends. The March CPS reports wage and salary income from all jobs held in the previous calendar year. The ORG measures the wage in the main job held by a person in the week prior to the survey—if *working*. The exclusion of persons who happen not to be working in that particular week (but worked sometime during the year) leads to a noticeable decline in the potential number of observations in the ORG data. At the same time, however, the ORG does allow a more precise calculation of the price of skills. I use the log hourly wage rate of a worker, defined as the ratio of usual weekly earnings to usual hours worked per week, as the dependent variable in the ORG analysis.

The top panel of Figure 5 uses all the available May/ORG surveys in which the Miami metropolitan area is consistently defined (1973 through 2003) and illustrates the 95% confidence bands for the trends in the average log hourly wage of low-skill workers in Miami and in the rest of the country.²⁴ As with the March CPS, the two wage series are roughly similar before Mariel. Miami's low-skill wage then tumbled after 1980 and recovered by 1990. The figure also shows a significant wage drop in the mid-1990s, coinciding with the arrival of the Little *Marielitos*.

²³I use the pre-1979 May files and the 1979–2003 ORG files archived at the National Bureau of Economic Research.

²⁴To increase sample size in the pre-treatment period, the January, February, and March samples of the 1980 ORG are added to the 1979 data, as those early months are unaffected by Mariel. The calculation of the average log wage in the cell weighs each individual observation by the product of the person's earnings weight times the usual number of hours worked weekly.

Figure 5. Trend in the Wage of High School Dropouts in the CPS-ORG, 1973–2003



Source: Data are drawn from the May/CPS-ORG files.

Notes: Log hourly wage is a three-year moving average of the average log wage of high school dropouts in each geographic area in Figure 5A, and of the age-adjusted wage in Figure 5B.

Before proceeding to further document the potential disparities in wage trends across the different surveys, I adjust the data for differences in the age distribution of workers in different time periods and in different metropolitan areas. I use a simple regression model to calculate the age-adjusted mean wage of a skill group in a particular market. Specifically, I estimate

the following individual-level earnings regression separately in each CPS cross-section:

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

where w_{irst} is the 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 A_i is a vector of fixed effects giving the worker's age.²⁵ The fixed effects θ_r deflate the log 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 article.²⁶

The bottom panel of Figure 5 illustrates the wage trends for Miami and the various placebos over the 1976–1992 period. It is visually evident that something happened to the low-skill labor market in Miami in the early 1980s, particularly when Miami's trend is compared to the employment, low-skill, or synthetic placebos. The use of the Card (1990) placebo in the ORG data tends to mask much of what went on in post-Mariel Miami.

For example, the wage of high school dropouts in Miami fell by 0.18 log points between 1979 and 1985. The comparable wage fell by 0.13 log points in the Card placebo, but by only 0.06 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%, while both the employment and the synthetic placebos would imply an impact of more than 10%.

Inclusion of Women

The number of observations available to examine wage trends in Miami before and after Mariel could be increased by including women in the analysis. Female labor force participation was increasing very rapidly in the 1970s and 1980s, however, so wage trends are likely to be affected by the changing gender composition of the workforce as well as by the selection that marks women's entry into the labor market. Moreover, female participation increased differentially across cities. For example, the 1970 decennial census data reported that 35.3% of Anaheim's workforce was female; by 1990, this fraction had risen by almost 10 percentage points to 45.1%. The increase in San Diego was similar, from 34.0 to 42.5%. In Miami, however, the increase was far smaller, from 40.2 to 42.1%.

The bottom panel of Table 3 shows that including women would indeed increase the sample size of low-skill workers in Miami, from an average of 19.5 in each pre-1990 March CPS cross-section to an average of 34.5.

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

²⁶Note that the wage trends in the age-adjusted data implied by the March CPS look almost identical to the raw trends illustrated in Figure 3.

Because of the male–female wage gap (which may itself be changing over time), the differential rates of growth in female labor force participation across cities distort relative wage trends between Miami and any placebo. Therefore, an analysis that examines the pooled earnings of men and women must (at the very least) account for the changing gender composition of the workforce. I adjust for this composition effect by running the following wage regression in the pooled sample of men and women in each cross-section of the CPS:

$$(2) \quad \log w_{i,t} = \theta_r + A_i \gamma_t + F_i \delta_t + \varepsilon,$$

where F_i is a dummy variable set to unity if the worker is female. The residual from this regression gives the age- and gender-adjusted wage of worker i in year t .

The two panels of Figure 6 illustrate the trends in the average adjusted wage observed in the pooled sample of men and women in both the March CPS and the ORG files. Adding women to the sample does not change the insight that something happened in Miami after 1980. It is important to stress, however, that the gender-adjusted wage trends are still contaminated by (unknown) differences across cities in the nature of the selection that motivates only some women to enter the labor market. The statistical difficulties associated with purging the data from this type of selection bias suggest that the most credible evidence of the Mariel impact on the price of skills is likely drawn from samples that examine the wage trends among prime-age men.²⁷

Regression Results

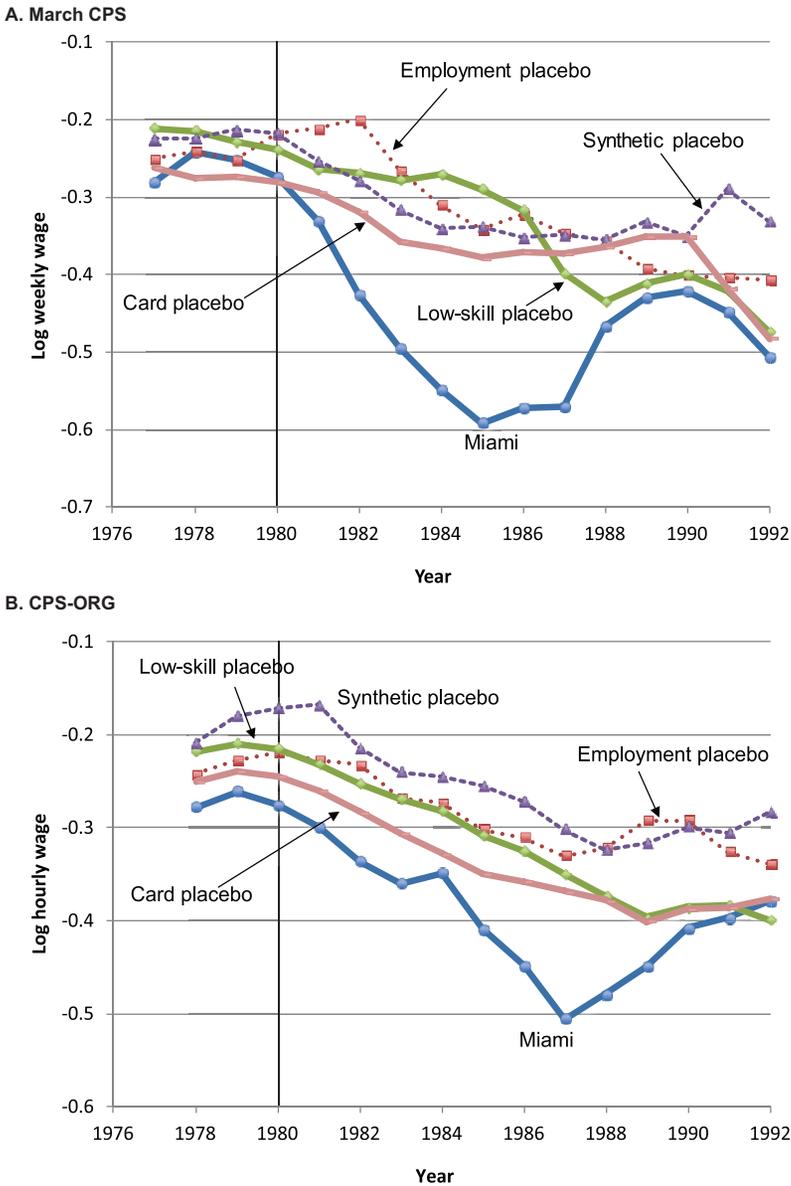
To estimate the impact of the Mariel supply shock relative to the various placebos, I use the mean age-adjusted log wage of male high school dropouts in city r at time t , denoted by $\log \bar{w}_{rt}$. This wage becomes the dependent variable in a generic difference-in-differences regression model:

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

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

²⁷My analysis uses the sample of non-Hispanic men aged 25 to 59. It may seem that an “easy” way of increasing sample size is to include non-Cuban Hispanics in the calculations. This solution, however, is very problematic. There was a rapid increase in Hispanic immigration in the 1980s, so that many of the observations added to the sample would be Hispanics (mainly from Mexico) who migrated to the United States *after* Mariel. The 1990 census implies that if one were to add non-Cuban Hispanics to the sample, 52% of the new observations are immigrants who arrived in the 1980s. The presence of such a large number of post-1980 immigrants, who typically have very low entry wages, distorts the post-1980 wage trend in many of the placebo cities, such as Los Angeles, seriously contaminating any measurement of the wage impact of the Mariel supply shock and biasing it toward zero.

Figure 6. Trends in the Wage of High School Dropouts in Pooled Sample of Men and Women, 1977–1992



Notes: Figures use a three-year moving average of the age- and gender-adjusted average log wage of the pooled group of men and women in each specific geographic area.

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

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

Table 5. Difference-in-Differences Impact of the *Marielitos* on the Wage of High School Dropouts

<i>Sample/Variable</i>	<i>Card placebo</i>	<i>Employment placebo</i>	<i>Low-skill placebo</i>	<i>Synthetic control</i>	<i>All cities</i>
A. March CPS, men					
1981–1983	−0.204 (0.076)	−0.290 (0.073)	−0.180 (0.081)	−0.257 (0.077)	−0.191 (0.064)
1984–1986	−0.368 (0.060)	−0.454 (0.059)	−0.301 (0.054)	−0.458 (0.075)	−0.393 (0.022)
1987–1989	−0.328 (0.081)	−0.303 (0.072)	−0.228 (0.060)	−0.293 (0.070)	−0.286 (0.059)
1990–1992	−0.026 (0.072)	−0.056 (0.123)	0.039 (0.065)	−0.188 (0.155)	−0.003 (0.032)
B. CPS-ORG, men					
1981–1983	−0.075 (0.026)	−0.140 (0.049)	−0.079 (0.024)	−0.154 (0.026)	−0.083 (0.012)
1984–1986	−0.069 (0.057)	−0.116 (0.065)	−0.076 (0.054)	−0.157 (0.056)	−0.086 (0.046)
1987–1989	−0.106 (0.036)	−0.175 (0.064)	−0.101 (0.035)	−0.150 (0.023)	−0.141 (0.022)
1990–1992	0.019 (0.041)	−0.070 (0.062)	0.036 (0.038)	−0.079 (0.016)	−0.008 (0.023)

Notes: Robust standard errors are reported in parentheses. 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. Regressions that use the Card, employment, or low-skill placebos have 75 observations; regressions that use the synthetic placebo have 30 observations; and regressions in the last column have 658 observations. All regressions are weighted by the number of observations used to calculate the dependent variable. The number of observations of the synthetic control is a weighted average of the sample size in the actual cities that make up the synthetic city.

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 these cities would be observed 15 times between 1977 and 1992, for a total of 75 observations. The regression comparing Miami to the synthetic control is similar in spirit, but only two cities are 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 (and to partially alleviate the problem of small samples), the post-Mariel variable in Equation (3) is initially a vector of fixed effects indicating whether the observation refers to the three-year intervals 1981–1983, 1984–1986, 1987–1989, or 1990–1992.

Table 5 presents the estimated coefficients in the vector β (and robust standard errors) for various specifications of the model.²⁹ Consider initially the coefficients reported in Panel A, drawn from regressions estimated in

²⁹The presence of serial correlation in outcomes at the city level requires further adjustments for valid statistical inference, but clustered standard errors are downward biased when the data have few clusters (Cameron and Miller 2015).

Table 6. Sensitivity of Estimates of the Short-Run Wage Impact, 1977–1986

<i>Placebo</i>	<i>Men</i>		<i>Men and women</i>	
	<i>March CPS</i>	<i>CPS-ORG</i>	<i>March CPS</i>	<i>CPS-ORG</i>
1. Card placebo	−0.280 (0.069)	−0.074 (0.031)	−0.189 (0.061)	−0.040 (0.024)
2. Employment placebo	−0.358 (0.064)	−0.131 (0.044)	−0.220 (0.048)	−0.069 (0.033)
3. Low-skill placebo	−0.269 (0.072)	−0.078 (0.028)	−0.182 (0.056)	−0.040 (0.022)
4. Synthetic control	−0.343 (0.081)	−0.155 (0.028)	−0.175 (0.051)	−0.075 (0.024)
5. All cities placebo	−0.279 (0.062)	−0.086 (0.023)	−0.184 (0.051)	−0.036 (0.020)

Notes: Robust standard errors are reported in parentheses. Data consist of annual observations for each city between 1977 and 1986 (1980 excluded). 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. See the notes to Table 5 for more details on the regression specification.

the March CPS sample. The various columns of the table use the alternative placebos introduced in the previous section as well as an aggregate placebo composed of all other 43 metropolitan areas. The various rows show how the wage impact varies during the post-Mariel period. The variation in these coefficients presumably captures the wage effect as the Miami labor market adjusts and moves from the short to the long run.

Regardless of the placebo used, the wage effect immediately after Mariel is negative, indicating an absolute decline in the wage of low-skill workers in Miami. All the regressions also suggest that the wage impact was stronger between 1983 and 1986 than between 1981 and 1983. On average, it seems that the low-skill wage in the first six years after Mariel fell by at least 20 or 30%. Finally, the evidence indicates that the wage effect weakened after 1986 and essentially disappeared by 1990.

Panel B reports comparable coefficients from regressions estimated in the ORG data. Although the wage impact in the first six years after Mariel is again consistently negative, it is numerically smaller, hovering around 10%. The ORG regressions also suggest an eventual attenuation of the impact.

To easily illustrate the sensitivity of the regression evidence, the remainder of the analysis uses a simpler form of the model in Equation (3), focusing on the years between 1977 and 1986 (excluding 1980). The short-run wage effect reported in Table 6 is simply the interaction between the indicator for the Miami metropolitan area and the indicator for a post-1980 observation. Among men, the short-run impact ranges from about 10 to 30%. Table 6 also reports the analogous coefficient estimated using the age- and gender-adjusted wage in the pooled sample of men and women. Although these effects are weaker (between 5 and 20%), they are always negative and significant.

The ORG wage effects are about one-half to one-third the size of the corresponding effects in the March CPS, but the standard errors imply that the difference is often not statistically significant. A large part of the difference in point estimates between the two data sets can be attributed to labor supply effects and to measurement issues related to labor supply. For example, the short-run wage impact for men using all other metropolitan areas as a placebo is -0.279 (0.062) in the March CPS and -0.086 (0.023) in the ORG. The estimated coefficient in the March CPS would fall to -0.216 (0.059) if I used the log hourly wage rate instead of log weekly earnings as the dependent variable and the average log hourly wage in a cell was calculated as in the ORG, using a weight equal to the sampling weight times the number of hours worked weekly. Finally, the coefficient would fall even more, to -0.167 (0.075), if the March sample was limited to persons who worked in the reference week (as in the ORG). In short, conceptual and measurement issues related to labor supply account for about half the difference in point estimates.

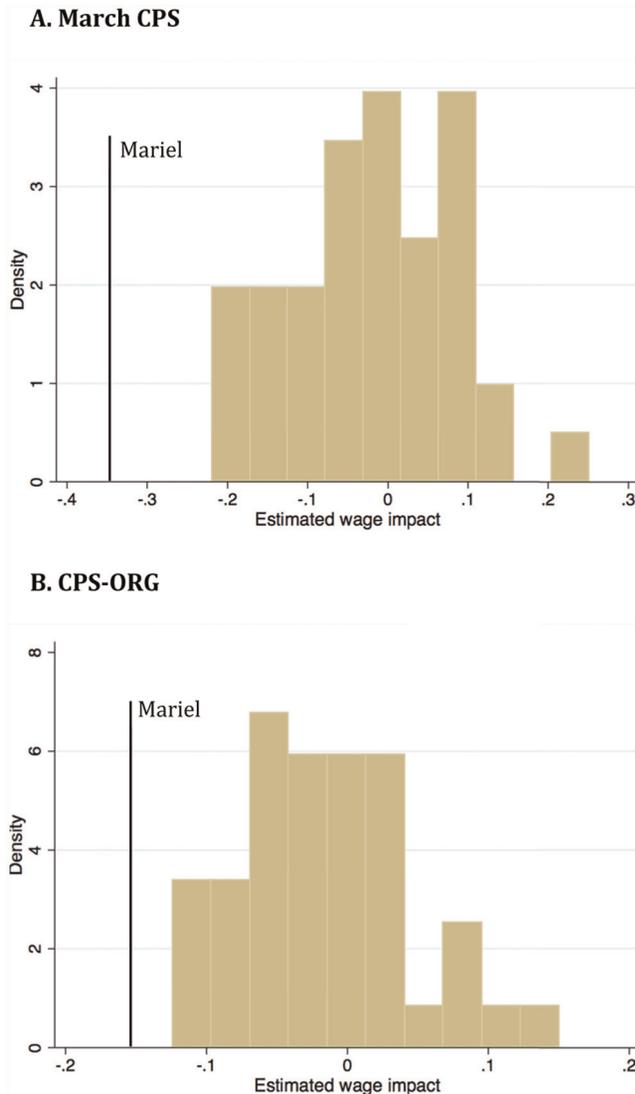
It is also instructive to extend the synthetic control approach to estimate the distribution of short-run wage effects implied by an intriguing counterfactual exercise. What would the distribution of estimated effects look like if we acted as if each city had experienced a shock in 1980 and simply calculated the pre- and post-wage change attributable to that imaginary supply shock *relative* to each city's synthetic control?³⁰ Would the wage effect estimated for the actual Mariel supply shock look all that unusual when compared to the entire distribution of hypothetical wage effects?

To be more specific, I again define the pre-treatment period as 1977 to 1979 and the post-treatment period as 1981 to 1986. Imagine that the city of Akron was hit by a phantom supply shock in 1980. We can then calculate the wage trends in Akron *and* in Akron's synthetic control in the pre- and post-treatment periods and estimate the difference-in-differences regression model in Equation (3). Presumably, the wage effect resulting from this exercise should be near zero because Fidel Castro did not suddenly decide to relocate more than 100,000 Cubans to Akron in 1980. However, other (random) things may have happened in post-1980 Akron that we know nothing about and that may have changed the relative wage of low-skill workers in that city relative to its synthetic control.

I construct each city's synthetic control by using the same control variables introduced earlier: the city's rate of employment growth and the concurrent rates of employment and wage growth for the low-skill workforce. I then estimate the regression model in Equation (3) to calculate the short-run wage impact for each city relative to its synthetic control. Figure 7 illustrates the distribution of the estimated effects on the log wage of male high school dropouts. Although there is a lot of dispersion across all the

³⁰This exercise effectively extends the distributional analysis summarized in Figure 4 by contrasting what actually happened in city r with what happened in city r 's synthetic control.

Figure 7. Distribution of Hypothetical Short-Run Impacts Relative to Synthetic Placebo, Assuming a Supply Shock Hits Each City in 1980



Notes: Pre-treatment period is three years; post-treatment period is six years. Wage effect is estimated from a difference-in-differences regression model that excludes 1980, the year of the treatment.

hypothetical shocks, the mean effect is numerically equal to zero.³¹ Both the March CPS and the ORG imply that the wage effect induced by the *real* Mariel supply shock was the most negative wage impact observed during the period.

³¹The mean of the distribution is -0.018 in the March CPS and -0.014 in the ORG.

Although the regression results unambiguously indicate that the Mariel supply shock harmed low-skill workers, the overall evidence may not be consistent with the textbook model of factor demand. The March CPS data suggest that the adverse wage effect of the *Marielitos* initially increased over time before eventually disappearing. This finding is hard to square with the theoretical prediction that the wage effect should be largest right after the supply shock and would weaken as the capital stock adjusted over time.

There has been little research on the dynamics of immigration-induced supply shocks. But there has been much work on the dynamics of demand shocks. The presumption that wages are sticky downward is a common feature in business cycle models. Many studies, such as those that examine the impact of oil shocks (Hamilton 1983), recognize that the largest effects of demand shocks do not happen immediately. It typically takes a few years for the adverse shock to reverberate through the labor market. In the Mariel context, it may also be that employers exploited sticky nominal wages during the high inflation of the early 1980s as a way of passing through the wage cuts. Given the well-documented lags between demand shocks and their consequences, we do not yet know if the dynamics of the wage data in post-Mariel Miami are consistent with the expected effects of a supply shock.³²

We also do not fully understand why the *relative* wage of low-skill workers in Miami recovered after a decade (as illustrated in Figure 3). Economic theory implies that it is the *average* wage 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.

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. Within a month after the Mariel boatlift began, racial riots ravaged parts of Miami, leaving 18 people dead and 400 injured. The conditions on the ground were volatile, and the riots were the consequence of a long list of accumulated grievances. Nevertheless, one of the grievances was “the displacement of blacks by Cubans from jobs and other opportunities” (Vogel and Stowers 1991: 120).

The political and social upheaval ignited by Castro’s decision to open up the port of Mariel, as well as the increasingly important role that the Miami

³²Another potential explanation for the delayed impact is that perhaps Miami continued to be hit by large supply shocks, which eventually subsided by the late 1980s. The data from the 1990 census, however, is not consistent with this conjecture. After the entry of the *Marielitos*, there was a steady flow of low-skill immigrants into Miami, increasing their number by 9.1% in 1982–1984, by 8.6% in 1985–1986, and by 11.8% in 1987–1990. The Miami experience was not unusual. There was a surge in low-skill immigration in the 1980s throughout the country, so that many of the cities that often end up in the placebos experienced similar supply shocks. In Anaheim, new arrivals increased the number of low-skill immigrants by 11.1% in 1982–1984, by 11.1% in 1985–1986, and by 17.9% in 1987–1990. The statistics for the cities in the low-skill placebo are 8.6%, 7.7%, and 10.0%, respectively.

of the 1980s played in the market for illicit narcotics, affected Miami's economy in ways that extend far beyond what our models capture. Put bluntly, the *ceteris paribus* assumption in the neoclassical model does not apply. As a result, it is difficult to infer much about the dynamics of the wage effect from the evidence provided by the Mariel supply shock.

The coefficients reported in Table 6 suggest that the wage of male high school dropouts in Miami fell by 10 to 30% during the first six years after Mariel (depending on the placebo and data set used). The supply shock increased the number of high school dropouts by approximately 20%, 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 (which sometimes cluster 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 . The estimates are close, however, 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 reported a wage elasticity of -0.7 while Llull estimated an elasticity of about -1.2 .

Granted, many caveats need to be resolved regarding the specification of the regression model and the external validity of the Mariel experience 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 caused by something other than the *Marielitos* will need to specify precisely what those other factors were.

The Choice of a Placebo

One lesson from the evidence presented in the previous sections is that the choice of a placebo matters. To easily document this sensitivity, I estimate the short-run wage impact using alternative specifications of the synthetic control method, for which I use different sets of control variables to create the synthetic city.

Specifically, row 1 of Table 7 again reports the impact estimated with the controls introduced earlier: employment growth, low-skill employment growth, and low-skill wage growth. The wage impact (in the sample of men) is -0.34 in the March CPS and -0.16 in the ORG. The control variables in row 2 are those used by Card (1990) to (manually) define his placebo: employment growth, percentage of the workforce that is Hispanic, and percentage that is black. Note that the implied wage impact in the March CPS is about the same, but the wage impact in the ORG drops by almost half to -0.09 .

Table 7. Sensitivity of Short-Run Wage Effects to the Definition of a Synthetic Control

Set of control variables	Men		Men and women	
	March CPS	CPS-ORG	March CPS	CPS-ORG
1. $\Delta \log E$, $\Delta \log E_{L^S}$, $\Delta \log w_U$	-0.343 (0.081)	-0.155 (0.028)	-0.175 (0.051)	-0.075 (0.024)
2. $\Delta \log E$, percentage Hispanic, percentage black	-0.367 (0.108)	-0.092 (0.032)	-0.264 (0.065)	-0.057 (0.023)
3. $\Delta \log E$, percentage Cuban, percentage black	-0.317 (0.080)	-0.134 (0.039)	-0.251 (0.064)	-0.070 (0.039)
4. $\Delta \log E$, $\Delta \log E_{L^S}$, $\Delta \log w_U$, percentage Hispanic, percentage Cuban, percentage black, percentage female	-0.301 (0.084)	-0.072 (0.037)	-0.221 (0.051)	-0.057 (0.023)
5. Same as row 4, plus percentage low-skill and industry mix	-0.325 (0.052)	-0.097 (0.033)	-0.254 (0.044)	-0.048 (0.024)

Notes: Robust standard errors are reported in parentheses. Data consist of annual observations for each city between 1977 and 1986 (1980 excluded). The control variable $\Delta \log E$ is the city's employment growth rate between 1977 and 1979; $\Delta \log E_U$ is the concurrent growth rate of low-skill employment; and $\Delta \log w_U$ is the concurrent growth rate of the low-skill wage. 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. See the notes to Table 5 for more details on the regression specification.

Although it may seem sensible to include percentage Hispanic as a control variable, the use of this variable could be problematic. A lot of heterogeneity occurs within the Hispanic population; the labor market opportunities available to low-skill Cubans probably have little in common with those available to low-skill Mexicans. Rows 2 and 3 show that the estimated wage effect can vary by 5 percentage points (in *either* direction) depending on whether the control variable is the fraction that is Hispanic or the fraction that is Cuban. The implied synthetic city in row 2, which uses percentage Hispanic, is an amalgam of Greensboro-Winston (with a weight of 0.44), Los Angeles (0.53), and San Diego (0.03). Simply replacing percentage Hispanic with percentage Cuban changes the synthetic city to Greensboro-Winston (0.33), Newark (0.46), and Tampa (0.21). Not surprisingly, the implied synthetic city is very sensitive to the choice of control variables, so careful consideration needs to be given to which set of variables should be included in the model.

Row 5 of Table 7 presents the most general specification, which adds variables denoting the fraction of the workforce in each of 11 industries, as well as the fraction of the workforce that is female or low skill.³³ The estimated

³³The industries are agriculture or mining, construction, manufacturing, transportation, wholesale or retail trade, finance, business and repair services, personal services, entertainment services, professional services, and public administration. The control variables giving the change in local employment or wages were calculated using the March CPS; all other variables were calculated using the 1980 decennial census.

wage impact is -0.33 in the March CPS and -0.10 in the ORG. Put bluntly, the construction of the placebo is *not* an innocuous decision—even when the methodology used to construct the placebo is thought to be relatively free of researcher intervention (as the synthetic control method is sometimes advertised).

One alternative (and perhaps preferable) way of determining whether the key finding of a negative wage impact is sensitive to the choice of placebo is to estimate the wage impact for *every* potential placebo, and then examine the resulting distribution of potential wage effects. I illustrate this approach by using the regression model in Equation (3) to estimate the short-run effect in each of the 123,410 possible four-city placebos.

The two panels of Figure 8 illustrate the frequency distribution of estimated effects when the dependent variable is the log wage of male high school dropouts, while Table 8 reports summary statistics for the distributions. Consider the density of estimated effects in the March CPS data. The mean effect is -0.283 , and almost all of the effects are statistically significant.

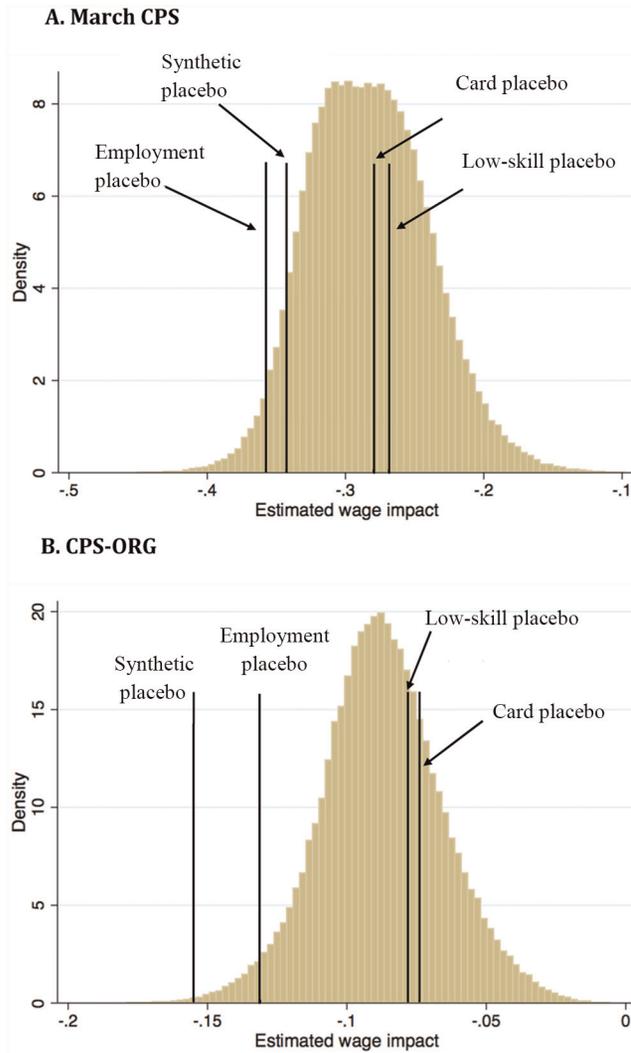
Note that if the set of placebos is restricted to those in which the average employment growth in the four placebo cities was roughly similar to that of pre-Mariel Miami, the mean wage effect rises to -0.319 . If we look at the smaller subset, with both average employment growth and average low-skill employment growth similar to that of Miami, the mean wage effect rises further to -0.330 . Put differently, the closer we get to a placebo that seems to replicate the pre-existing conditions in Miami, the more likely we are to find that the *Marielitos* had a larger wage effect on low-skill Miamians. Table 8 shows that the same trend is implied by the frequency distribution of wage effects computed in the ORG data.

Conclusion

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

This article 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 the vast 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

Figure 8. Distribution of Short-Run Wage Impacts across All Possible Four-City Placebos, 1977–1986



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. 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 .

likely to be felt by those workers who had similar capabilities as the *Marielitos*.

The Mariel supply shock was composed disproportionately of low-skill workers; approximately 60% were high school dropouts. Remarkably, none of the previous examinations of the Mariel experience documented what happened to the pre-existing high school dropouts in Miami, a group that composed over a quarter of the city's workforce. Given the literature

Table 8. Distribution of Estimated Short-Run Wage Effects across All Four-City Placebos

<i>Characteristics of distribution</i>	<i>March CPS</i>	<i>CPS-ORG</i>
Mean	-0.283	-0.087
Standard deviation	0.043	0.022
Statistical significance: Fraction of <i>t</i> -statistics above 2.0	0.998	0.845
Average employment growth of placebo cities within 0.1 standard deviations of Miami (<i>N</i> = 1,148)		
Mean	-0.319	-0.108
Fraction of <i>t</i> -statistics above 2.0	1.000	0.863
Average low-skill employment growth of placebo cities within 0.1 standard deviations of Miami (<i>N</i> = 1,096)		
Mean	-0.297	-0.105
Fraction of <i>t</i> -statistics above 2.0	1.000	0.973
Average total and low-skill employment growth for placebo cities within 0.1 standard deviations of Miami (<i>N</i> = 74)		
Mean	-0.330	-0.120
Fraction of <i>t</i> -statistics above 2.0	1.000	0.986

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. Regressions were estimated separately in all possible 123,410 four-city placebos. See the notes to Table 5 for more details on the regression specification.

sparked by Borjas (2003), 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%. The examination of wage trends in every other city identified by the CPS 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 article also illustrates that the researcher's *choice* of a placebo is an important element of any such empirical exercise, and that picking a different placebo can easily lead to a weaker or stronger measured impact of immigration. The synthetic control method, for example, can generate disparate wage effects depending on the set of variables the researcher uses to construct the synthetic city. Similarly, the magnitude of the wage effect differs substantially across all potential four-city placebos that can be constructed in the CPS data. Note, however, that despite the variation in the magnitude of the wage effects across placebos, the evidence consistently indicates that the *Marielitos* had a sizable negative effect on the wage of competing workers.

The evidence has potentially important implications for the literature that purports to measure the wage impact of immigration. Many studies measure the effect by estimating spatial correlations between wages and the

number of immigrants in a particular locality. These spatial correlations 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 indicates that the existing non-experimental literature may not have successfully overcome those statistical difficulties. Researchers still have some way to go before non-experimental spatial correlations can be presumed to estimate a parameter of economic interest.

The evidence also has 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. The greater the wage impact, the greater the immigration surplus. Borjas (2014: 151) estimated the current surplus to be approximately 0.24% of GDP, or around \$43 billion annually. Because there was a much larger reduction in the earnings of the workers most likely to be affected by the *Marielitos* than was previously believed, we may need to reassess existing estimates of the immigration surplus. That surplus could easily be two 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 (1990) Mariel study. The reappraisal of the evidence presented in this article suggests that much can be gained by revisiting some 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 earlier data with some new ideas can reveal trends that radically change what we think we know.

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