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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 workforce. 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. In fact, comparing the magnitude of the steep post-Mariel drop in the low-skill wage in Miami with that observed in all other metropolitan areas over an equivalent time span between 1977 and 2001 reveals that the change in the Miami wage structure was a very unusual event. 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.

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The Wage Impact of the *Marielitos*: A Reappraisal

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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. By September 1980, around 125,000 Cubans had accepted the offer. The Card study was one of the pioneering attempts to exploit the conceptual 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 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 been extremely

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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

[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), 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 shock 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 factually incorrect. As I will show shortly, President Clinton's decision to reroute many of the potential migrants to Guantanamo seemed to only delay the supply shock by a year or so. As a result, it is difficult to infer much from the comparison of the Mariel supply shock to the 1994 event that ended up bringing real human beings to the Miami metropolitan area anyway.

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 in a matter of months.

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 perception 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 of a competitive labor market predicts: an increase in the number of potential workers lowers the wage of those workers who face the most competition from the additional immigrants. It seems that the short-run labor demand curve, even in the Miami of the early 1980s, was downward sloping after all.

[4] Table 7 in Card (1980) reports wage and employment changes for the subsample of black high school dropouts. Card's paper does not report any other pre-post Mariel differences for the least educated workers in Miami. 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. His 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 2000, nearly 1 million Cubans had migrated.

The first large-scale data set that precisely identifies an immigrant's year of arrival is the decennial 2000 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 (from 1970 through 2000) to construct a mortality-adjusted number of Cuban immigrants for each arrival year between 1955 and 1999.[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 interval. Figure 1 shows the trend in the number of Cubans arriving each year.

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. Finally, 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."

One last detail is worth noting about the Cuban migration: A disproportionately large number of Cubans ended up residing in the Miami metropolitan area. The fraction of

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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.

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 1977-1992 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 time period analyzed throughout much of the paper (1977-1992) is selected for two reasons. First, it avoids contamination from the Little Mariel supply shock of 1994 and 1995. Second, as will be discussed below, the number of persons sampled by the CPS in the Miami-Hialeah area dropped sharply in 1990, leading to much noisier wage data in the 1990s. It is tempting to increase sample size by including working women, 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 obviously marks women's entry into the labor market.[9]

Before 1994, the CPS did not report a person's country of birth, 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

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[7] The March surveys are known as the Annual Social and Economic Supplements (ASEC). The microdata 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 who have a negative sample weight.

[9] The labor force participation 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.

sample restriction that comes close to identifying the native-born population in Miami at the time.[10]

The key labor market outcome used throughout the analysis 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).[11] For expositional consistency and unless otherwise noted, whenever I refer to a particular calendar year it will be the year in which earnings were actually received by a worker, as opposed to the CPS survey year.[12]

Before proceeding to an examination of wage trends, it is important to document what we know about the skill distribution of the *Marielitos*. The Mariel supply shock began a few days after the enumeration conducted by the 1980 census, so that the first large survey that contains a large number of observations on 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 in 1980 or in 1980-1981.

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. By construction, the Cubans included in the calculation were at least 18 years old as of 1980. The calculation includes the entire population of *Marielitos* (workers and non-workers, as well as men and women).

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

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[10] The 1980 census 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.

[11] 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.

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

high school dropouts.[13] 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 pre-existing workers in the Miami labor market at the time. The last row of Table 1 shows that "only" 25.9 percent of workers in the Miami metropolitan area were high school dropouts prior to Mariel. In fact, the pre-existing workforce in Miami was remarkably balanced in terms of its skill distribution, with 20 to 30 percent of workers in each of the four education groups.[14]

Table 2 summarizes what we know about the size of the Mariel supply shock on Miami's workforce. There were 162,900 high school dropouts working in Miami just days prior to Mariel (out of a total workforce of 627,900 thousand). According to the 1990 Census, 53,800 thousand Cuban-born adult *workers* entered the United States in 1980 or 1981. If we make a slight adjustment for the small number of Cubans who entered the country in 1981, the predicted number of working Marielitos was around 49,800 thousand, of which almost 60 percent, or 28,500 thousand, were high school dropouts.[15] As a result, although the Mariel supply shock increased the size of Miami's workforce by 7.9 percent and increased the number of the most educated workers by 3 to 5 percent, the size of the low-skill workforce increased by almost 18 percent.

The very low skills of the *Marielitos* indicates that we should perhaps focus our attention on the labor market outcomes of the least skilled workers in Miami in order to get

[13] The fact that most of the adult *Marielitos* lacked a high school diploma does not necessarily imply that they did not complete their compulsory schooling in Cuba. There is also a possibility that the skills of the *Marielitos* were "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 Miami workforce.

[14] The pre-existing workforce includes all workers in Miami, regardless of where they were born or their ethnicity. The fraction of non-Hispanic workers in Miami in 1980 who are high school dropouts was also very high (19.8 percent).

[15] The 2000 census indicates that approximately 92.5 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.

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 is extremely relevant in any 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 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 in the 1990s. Perhaps due a CPS redesign (or a redefinition of the metropolitan area), the number of non-Hispanic high school dropouts sampled in Miami drops abruptly (to single digits) in 1990. This change in sample size suggests that the evidence is probably most credible when we examine outcomes in the first decade after Mariel. As a result, almost all of the analysis of Miami's wage trends will focus on the period 1977-1992.

It is best to start by reporting the evidence that results from the most straightforward calculation of the potential wage impact. It turns out that even the most cursory examination of the wage trends reveals a remarkable pattern that 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.

In particular, consider the following exercise. For each calendar year, let's calculate the average log wage of each education group in the Miami labor market. Panel A of Figure 2, for example, shows what happened to the average log wage of high school dropouts in Miami between 1977 and 1992, using a 3-year moving average to smooth out the noise in the data.[16] It is important to emphasize that the simple exercise I am proposing does *not* adjust the CPS data in any way whatsoever (other than taking a moving average), so that it

[16] All calculations of the data point for the moving average in 1992 will use data from the 1993 CPS survey to avoid losing an observation.

provides a very transparent indication of what happened to wages in Miami pre- and post-Mariel.

It is visually obvious that the low-skill wage in Miami behaved strangely in the early 1980s, falling steeply between 1980 and 1985-1986. By 1990, the wage of high school dropouts had fully recovered. Of course, trends in absolute wages reflect many factors that are specific to local labor markets, so that it is entirely possible that these ups and downs capture idiosyncratic shifts that affected all workers in Miami. We have seen, however, that the Mariel supply shock specifically targeted the least skilled workers so that it is important to determine if the *relative* wage of high school dropouts also moved dramatically over time.

Apart from the potential complementarities between low- and high-skill workers, the Mariel supply shock had so few high-skill workers that it should have had little impact on the earnings of college graduates. Moreover, although such complementarities are likely to exist, most numerical simulations of factor demand models predict that the "cross" effects tend to be small (Borjas, 2003), so that the expected wage impact of Mariel on the wage of college graduates was probably negligible.

I calculated the difference between the average log wage of high school dropouts in Miami and either college graduates or high school graduates. To smooth out the random noise in the data, I again constructed a three-year moving average of each relative wage series. Panels B and C of Figure 2 illustrate the trends in the smoothed data. Both panels clearly show that the relative wage of high school dropouts in the Miami labor market, just like the absolute wage, had ups and downs in the sample period, and it most certainly had a very noticeable "down" between 1980 and 1985-1986.

We obviously need a control group of cities unaffected by the Mariel supply shock to determine whether these wage trends were due to macroeconomic trends affecting many other communities as well. The available March CPS data in the 1977-1980 period, *before* the Mariel supply shock, identifies 43 metropolitan areas that can be combined in some fashion to construct some sort of placebo.[17] Card (1980, p. 249; emphasis added) describes the construction of his placebo 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 labor market 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 employment conditions 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 2 also illustrate the trends in the Card placebo. It is visually obvious that the wage of high school dropouts in Miami—relative to the cities in the Card placebo—took a downward turn shortly after 1980, reached its nadir around 1985-1986, and did not recover fully until 1990.

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.[18] 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

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[17] The 1976 March CPS identifies 10 fewer metropolitan areas and the 1972 CPS identifies only 19 metropolitan areas.

[18] 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.

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 interesting 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 employment in 1977) was 6.5 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 2 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 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 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 technique developed by Abadie, Diamond, and Hainmueller (2010). Their 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 Abadie-Diamond-Hainmueller 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.

I defined a new "synthetic placebo" by using two such characteristics: the rate of employment growth in the city between 1977 and 1979 (i.e., the variable used to define the employment placebo) and the concurrent rate of employment growth in the low-skill labor market (i.e., of high school dropouts).[19] Column 2 of Table 3 reports the growth of low-skill employment in each metropolitan area, showing that Miami also had a robust low-skill labor market prior to Mariel. Coincidentally, Miami ranked sixth in growth of low-skill employment as well.

Figure 2 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 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.

As an example, consider the weights implied by the approach when the dependent variable is the log weekly wage of high school dropouts. The Abadie-Diamond-Hainmueller method then assigns the largest weights to two metropolitan areas: Anaheim (with a weight of 0.27) and San Diego (0.40). In contrast, a comparable analysis that examines the trend in the wage of high school dropouts relative to that of high school graduates assigns the largest weights to Greensboro (0.26), Anaheim (0.39) and San Diego (0.10). It is worth noting that the synthetic placebo consistently assigns large weights to cities where employment growth was at least as robust as in Miami (see Table 3). Regardless of the weights that define the synthetic placebo, all the wage trends illustrated in Figure 2 again suggest that the Miami experience of the early 1980s seems unique.

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[19] I also determined the synthetic placebo using only the total rate of employment growth and the results do not change any of the findings discussed below.

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 an unusual event. The wage of high-school dropouts in Miami fell by over 30 percent between 1977-1979 and 1981-1986. We can calculate the comparable wage change in every other metropolitan area for all equivalent time periods between 1977 and 2001 and see if the Miami experience at the time of Mariel stands out.[20] Obviously, if 30-percent wage cuts happen frequently, it would be much harder to claim that Miami's experience had much to do with the *Marielitos*. Perhaps something else was going on—a "something else" that occurs often 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 (1977-1979, 1978-1980,…,1992-1994) and the corresponding "post-treatment" period τ' (1981-1986, 1982-1987,…,1996-2001). Note that to replicate the Mariel experiment, I skip a year between the 3-year pre-treatment span and the 6-year post-treatment span. I conducted this calculation for every single metropolitan area, leading to a total of 688 possible "events" outside the Miami metropolitan area (43 metropolitan areas and 16 potential treatment years between 1980 and 1995). I also calculated the change in the log wage of the other education groups for all city-year permutations.

Panel A of Figure 3 illustrates the frequency distribution of all observed changes in the wage of high school dropouts *outside* Miami. Table 4 reports some of the summary statistics from the various distributions created by this empirical exercise.

[20] 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 2001 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.

Between 1977-1979 and 1981-1986, the log wage of high school dropouts in Miami fell by 0.447 log points (or 36 percent). It is visually obvious from Figure 3 that a decline that large was indeed an unusual event. The mean observed wage change across all city-year permutations was only about -0.07 log points, with a standard deviation of 0.14. The Miami experience at the time of Mariel ranks in the 0.8th percentile of the distribution of observed wage changes between 1977 and 2001 across all metropolitan areas.

Equally important, Panel B of Figure 3 illustrates the frequency distribution of observed wage changes for high school *graduates*. 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 distributions in Figure 3 clearly contradict 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 -0.070. The value observed in the Miami metropolitan area at the time of Mariel was -0.021, and lies in the 71st percentile of the distribution. Put differently, something unique happened in the economic well being of high school dropouts in Miami in the early 1980s, but not to high school graduates. In fact, Table 4 shows that the corresponding Mariel value for every single education group in Miami—*other than high school dropouts*—was relatively close to the mean wage change observed in all other cities throughout the entire 1977-2001 period.

In sum, the event that shocked the wage structure in Miami at the time of Mariel, whatever it happened to be, happens rarely and its consequences were targeted very narrowly on workers who lacked a high school diploma.

III. 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, it is well known that the wage inequality trends 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. Would the wage trends be 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 observed in the March CPS sometimes differ from the "comparable" wage trends observed in the CPS Outgoing Rotation Groups (ORG).[21] 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, however, 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 1977 and May 1978 CPS supplements. This allows me to create a pre-treatment time span for the ORG that is the same as that available in the March CPS.[22]

It is important to emphasize that the differences in wage trends between the March CPS and the ORG partly arise 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

[21] I use the 1979-2001 ORG files maintained at the National Bureau of Economic Research (NBER). The wage measure used in the analysis is the recoded usual earnings per week (*earnwke*).

[22] One benefit of the ORG surveys is that the construction of the outgoing rotation groups generates samples that are roughly three times the size of the March CPS. To increase the size of the pre-treatment sample, I also included workers who were interviewed by the CPS-ORG in January, February, or March of 1980 (prior to the Mariel supply shock) as part of the 1979 sample.

the earnings losses associated with jobless periods. From the perspective of determining the labor market impact of the Mariel supply shock, it seems 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:[23]

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

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 the worker's age.[24] 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.[25]

Figure 4 illustrates the wage trends calculated in the ORG data for Miami and for the three placebos defined in the previous section. 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 the employment or synthetic placebo. The use of the Card placebo in the ORG data often masks much of what went on in post-Mariel Miami.

For instance, the wage of high school dropouts in Miami fell by -0.22 log points between 1979 and 1985. Figure 4 indicates that the comparable wage fell by -0.17 log

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

[24] 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).

[25] 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.

points in the Card placebo, by -0.10 log points in the employment placebo, and by -0.06 log points in the synthetic placebo. 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 at least 10 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.[26] The pooling of these two groups into a *very large* low-skill workforce 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. Figure 5 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. One crucial detail about this aggregation is worth noting. Table 2 implies that 54.5 percent of non-Hispanic workers in pre-Mariel Miami were "low skill" by this definition. The fact that the majority of Miami's workforce (as recently as 1980) can be classified as such obviously suggests that the aggregation may not be all that sensible at least in this metropolitan area and in this period.

Despite this conceptual problem, Figure 5 again shows a striking 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.

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

Although the pooling of high school dropouts and high school graduates does not evaporate the wage impact in the Mariel context, the observed wage trends provide strong evidence *against* the conjecture that the groups should be pooled. The standard method for ascertaining whether any two groups are perfect substitutes is to estimate a regression relating the ratio of log wages to the ratio of log quantities. 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 the next section) 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 one takes 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. In fact, the frequency distributions in Figure 3 dramatically showed that the steep drop in the log wage of high school dropouts was a unique event among all possible city-year permutations observed between 1977 and 2001, while the change in the change of high school graduates in post-Mariel Miami was "average." This difference in wage trends is obviously inconsistent with the conjecture that the two groups are perfect substitutes.

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.[27]

However, Borjas, Grogger, and Hanson (2012) show that estimates of the slope coefficient in equation (3) 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 data are a time series or exploit geographic variation in relative wages and quantities.[28] The Mariel evidence that suggests the two groups are *not* perfect substitutes is not vulnerable to this criticism.

Finally, it is helpful to document that Miami's low-skill market 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 6 uses the March CPS to illustrate the trends in both the wage of the worker at the 20^{th} percentile as well as the interquantile range, which I define as the difference in the log weekly wage between the worker at the 20^{th} percentile and the worker at the 80^{th} percentile. The trends are again 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

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[27] 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 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/σ) 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).

[28] 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 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. However, 42.5 percent of high school dropouts were black and only 6.0 percent of college graduates were black. 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 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 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 during his arrest 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).

Panel A of Figure 7 illustrates the trend in the black-white wage gap in Miami and the placebos throughout the period. It is obvious that the relative black wage declined sharply after Mariel.[29] The black relative wage in Miami fell by almost 20 percentage points between 1979 and 1985, showing a very different trend than what occurred in the cities that compose either the Card of the employment placebo.

In short, it seems as if the impact of the *Marielitos* on relative wages across skill 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

[29] The calculation of the synthetic placebo was not conducted for the black-white wage gap because the methodology requires a perfectly balanced panel over the relevant sample period. Unfortunately, there were 13 city-year permutations in the March CPS that did not sample any black workers.

reported in Borjas, Grogger, and Hanson (2010). Their analysis documents a strong negative correlation between the earnings of black workers and low-skill immigration in the national labor market.

In fact, it is easy to document that the widening of the black-white wage gap in the Miami area in the early 1980s can be *entirely* attributed to the decline in the wage of high school dropouts—the group most likely to be affected by the *Marielitos*. Panel B of Figure 7 illustrates the trends in the black-white wage gap among workers who have at least a high school education. The visual evidence clearly shows that the relative wage of these more highly skilled blacks barely changed in Miami between 1979 and 1985. The empirical evidence, therefore, strongly suggests that the Mariel supply shock worsened the average economic status of blacks in Miami *because* the labor market impact was concentrated at the bottom end of the education distribution, and almost half (47.0 percent) of the black workforce lacked a high school diploma.

It would be of great interest to also examine the relative trends in Hispanic wages, but the nature of the available data makes that comparison uninformative. A replication of the analysis illustrated in Figure 7 for the Hispanic population (not shown for the sake of brevity) would show steady wage declines for Hispanic workers throughout the entire 1977-1992 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 these immigrants.

IV. 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. 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 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, or 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, and, for comparison purposes, an aggregate placebo composed of all other 43 metropolitan areas. The various rows of the panel 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 impact 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 -0.206 (0.072). 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), depending on how the placebo is defined. Remarkably, this wage effect actually 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 employment placebo regression is 0.096 (0.136).

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 low-skill workers typically declined after the supply shock, with relative wages 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 short-run effect on the wage of high school dropouts implied by the ORG is 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 in the first three years after Mariel. Either of these effects is far stronger than the impact calculated using the Card 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 repeatedly suggests that the adverse wage effect of the *Marielitos* 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 labor demand model. As suggested by the racial unrest that shook Miami soon after Mariel, the upheaval created by Castro's decision to open up the port of Mariel (and the subsequent political and cultural fallout) 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 of immigration 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. 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 hints at 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) across the different placebos, 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 relative wage of the worker at the 20th percentile of the wage distribution, or the black-white wage differential. In the vast majority of cases, the Mariel supply shock harmed workers at the bottom end of the wage distribution.[30]

In sum, the use of either the employment placebo or the synthetic placebo in the available CPS data indicates that the wage of high school dropouts in Miami fell by between 10 and 30 percent during the first 6 years after the Mariel supply shock. As I noted earlier, the Mariel supply shock increased the supply 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 infinitely higher than the typical wage effect estimated in (non-experimental) cross-city regressions that link wages to immigration, an effect that 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, an elasticity that clusters around -0.3 to -0.4 (Borjas 2014). 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.

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[30] There are only 3 city-year permutations without black workers in the ORG (and they are all in 1977 or 1978). I used adjacent year data to impute the missing information.

V. The Choice of a Placebo

The evidence reported in the previous sections repeatedly 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.[31] Because I am focusing on the short-run wage impact, the regressions only employ the observations between 1977 and 1986 in the March CPS and 1979-1986 in the ORG (with the 1980 observation excluded throughout).

The two panels of Figure 8 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 with either the employment placebo (-0.374) or the synthetic placebo (-0.344). 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 -0.283. 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

[31] I also conducted preliminary runs using all potential 5-city and 6-city placebos, and the results are very similar to those reported below.

placebo that seems to replicate the pre-existing 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 difference-in-difference 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.[32]

It is instructive to conclude by extending the Abadie-Diamond-Hainmueller approach of constructing synthetic placebos 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, I will use the March CPS and 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.[33] Presumably, the wage effect resulting from this exercise should be near zero simply because Fidel Castro did not suddenly decide to relocate 125,000 new Cuban immigrants 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.

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[32] 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. Cities with rapidly growing employment prior to Mariel likely experienced rapidly growing employment after Mariel as well, and this rapid growth might "protect" the wages of low-skill workers. In contrast, if we compared low-skill wages in Miami to a placebo that consisted of cities with stagnant employment (and wages), the wage impact of Mariel would tend to "fade away" because everyone—in Miami and in the placebo—is doing poorly.

[33] 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.

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 2001. To predict the synthetic control, I used both the city's rate of total employment growth in the 4-year period prior to the treatment and the rate of employment growth for the specific education group being examined. The top panel of Figure 9 illustrates the distribution of the difference-in-differences wage effects of these 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 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 -0.344 (0.106), 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.

It is also very instructive to document the wage impacts 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 never statistically significantly different from zero. As an illustration, the bottom panel of Figure 9 shows the distribution of wage effects for high school graduates. Despite all the recent conjectures about high school graduates and high school dropouts being perfect substitutes, the evidence is unequivocal and contradicts those conjectures. In fact, the Mariel supply shock was associated with a greater than average increase in the wage of high school graduates in Miami. It is important to note, however, that this positive wage effect was insignificantly different from zero.

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.

VI. 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 research 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 comparable group of high school dropouts in Miami, a group that composed 25 percent of the city's pre-existing 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 what happened to the labor market outcomes of high school dropouts in Miami.

The examination of wage trends in this education group 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 in Miami 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). In fact, the examination of wage trends in every single city identified by the CPS throughout the 1977-2001 period shows that the steep wage drop experienced by Miami's low-skill workforce immediately after Mariel 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 may be an extremely useful component of studies that examine the consequences of natural experiments.

The empirical evidence also has many lessons for the vast literature that attempts 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 cluster around zero, but they are plagued both by endogeneity problems (i.e. immigrants settle in high-wage regions) and 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 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 immigration surplus to be around 0.24 percent of GDP (or around $43 billion annually). The fact that there was a sizable reduction in the earnings of the workers most likely to be affected by the *Marielitos* suggests that we may also need to reassess existing estimates of the immigration surplus. That surplus could easily be at least twice or three times as large as is currently believed.

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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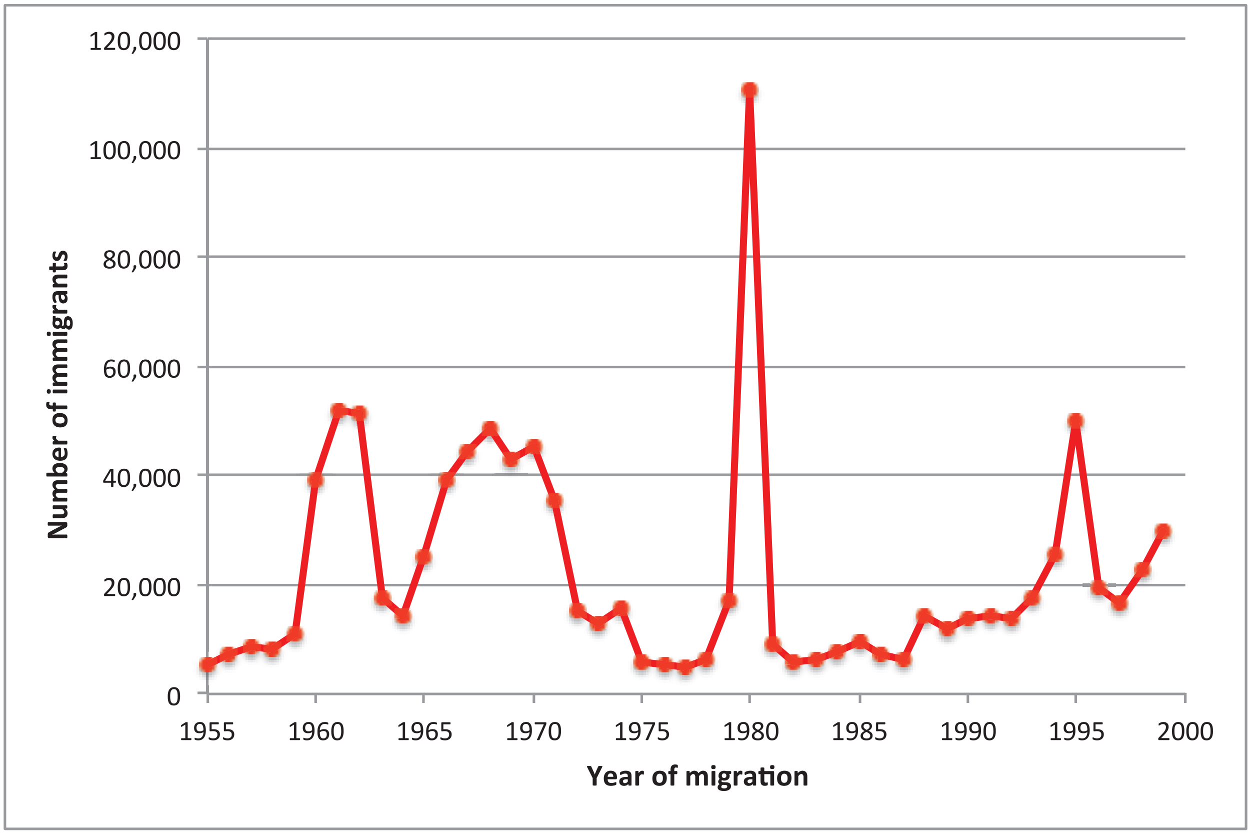
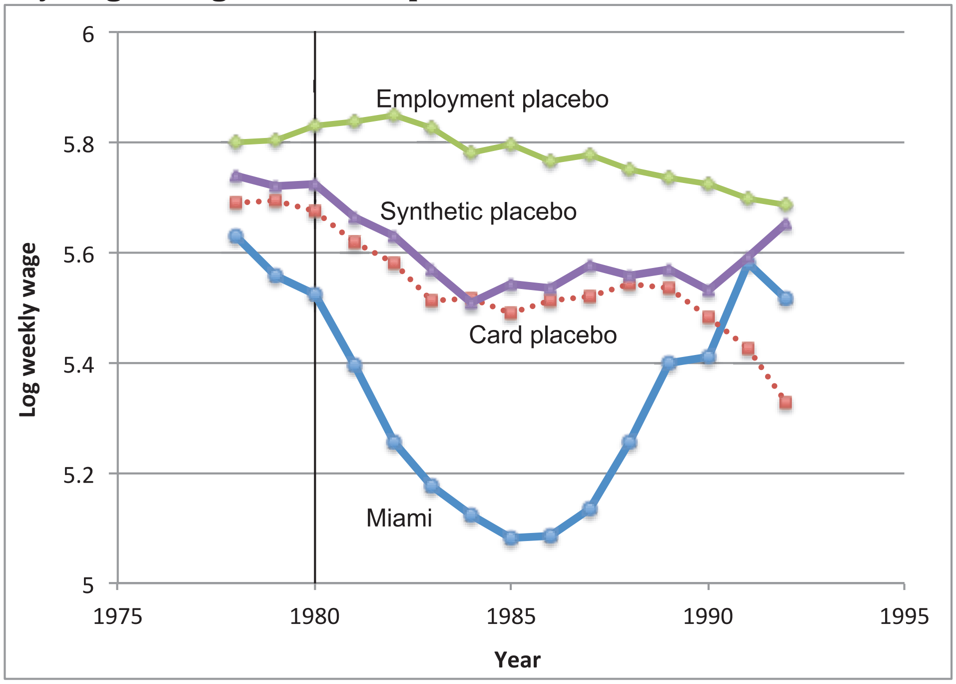
Figure 1. Number of Cuban immigrants, by year of migration, 1955-1999



Notes: The specific year of migration 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.

Figure 2. The trend in the wage of low-skill workers, 1977-1992

A. Log weekly wage of high school dropouts



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

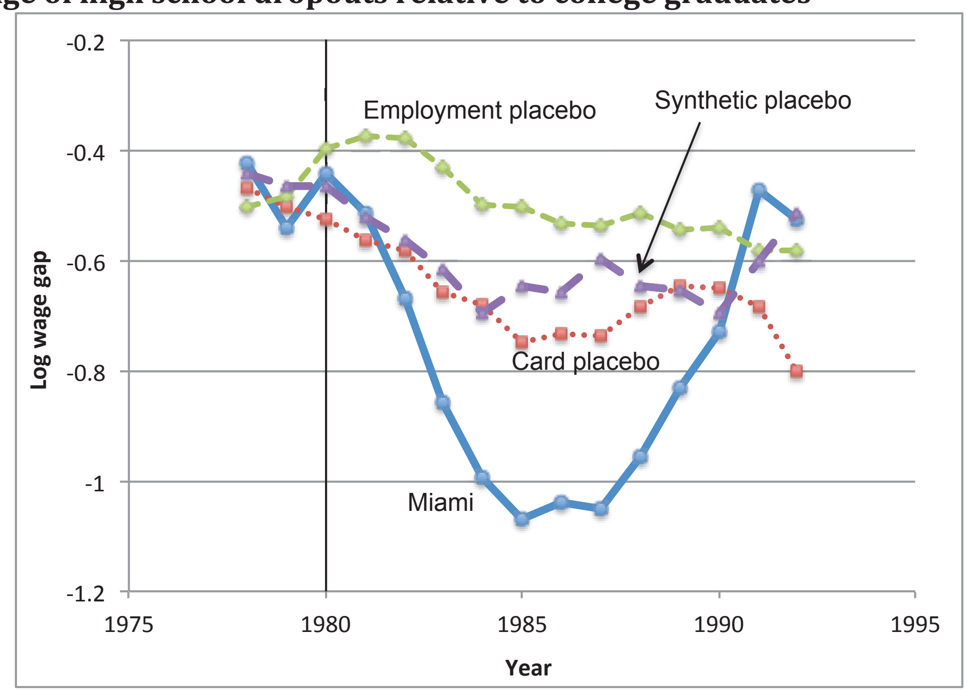
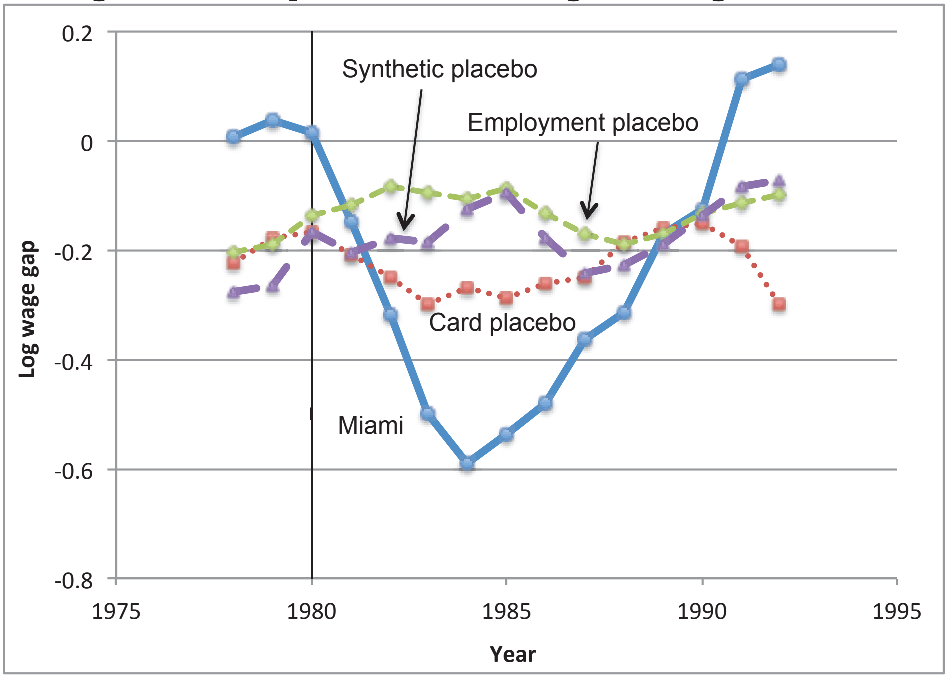


Figure 2 (Continued)

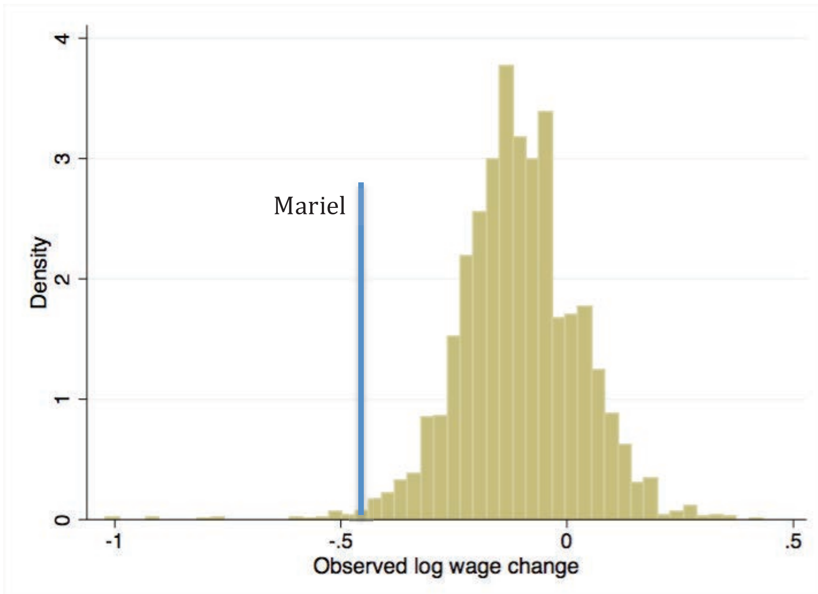
C. Log wage of high school dropouts relative to high school graduates



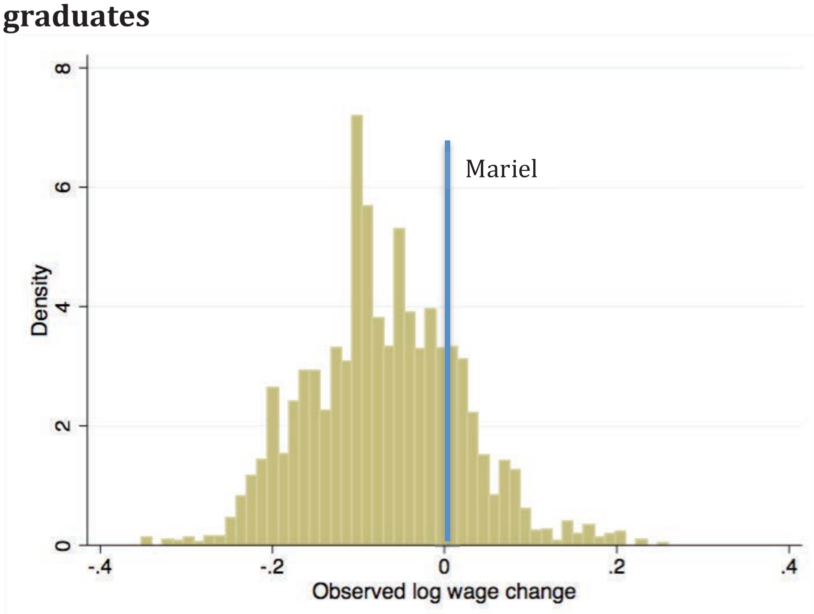
Notes: The figures use a 3-year moving average of the unadjusted average log wage of high school dropouts, high school graduates, and college graduates in each specific geographic area. The data are drawn from the March CPS files.

Figure 3. Distribution of pre-post wage changes across all year-city permutations outside Miami, 1977-2001

A. High school dropouts



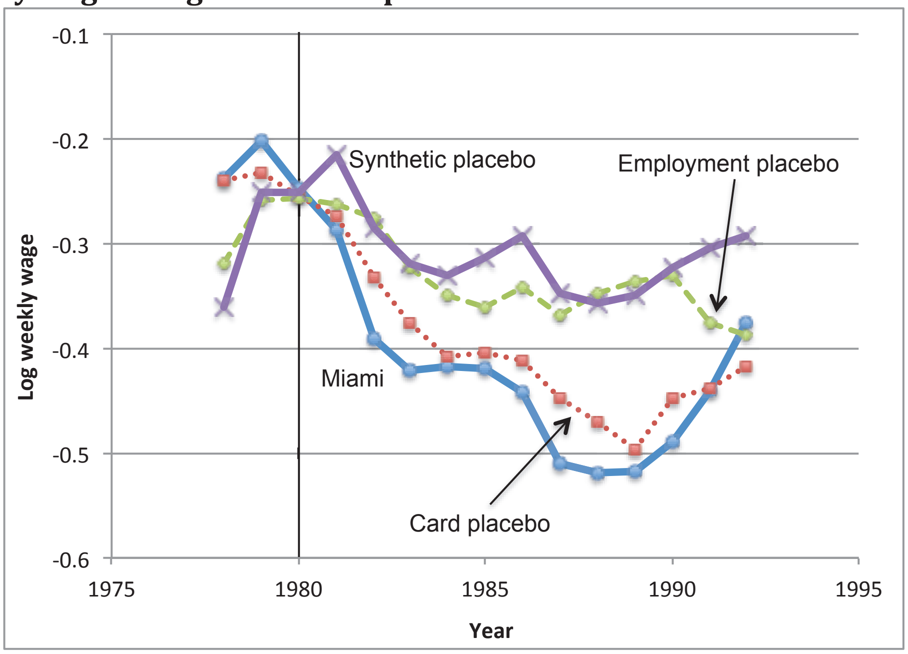
B. High school graduates



Notes: The pre-treatment period lasts 3 years; the post-treatment period lasts 6 years; and the year of the treatment is excluded from the calculation. The density functions are weighted are weighted by $(n_0 n_1)/(n_0 + n_1)$, where n_0 is the number of observations used to calculate the mean log wage of high school dropouts for a particular city in the pre-period, and n_1 is the respective number of observations used to calculate the mean wage in the post-period. The data are drawn from the March CPS files.

Figure 4. 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

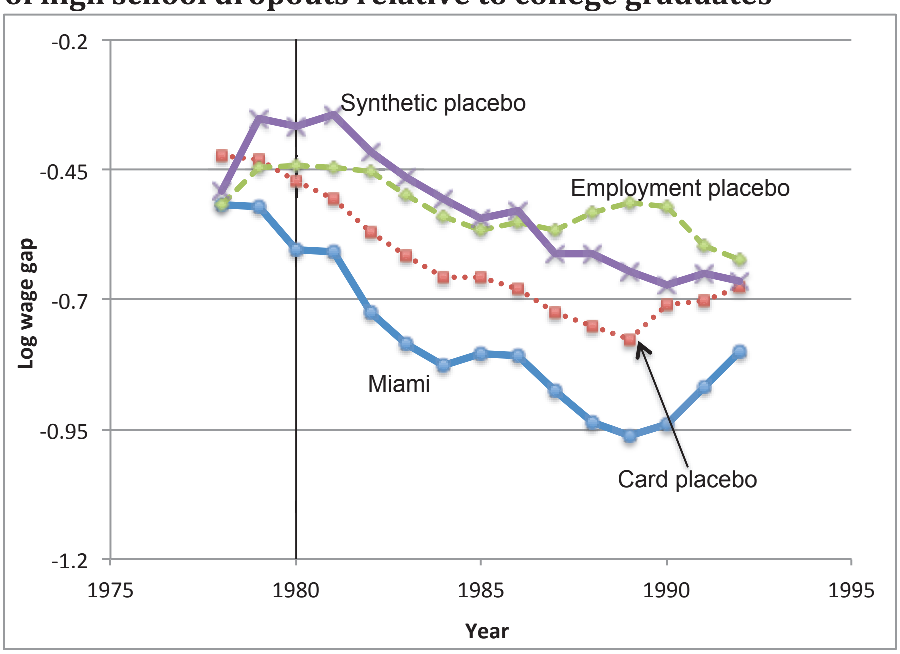
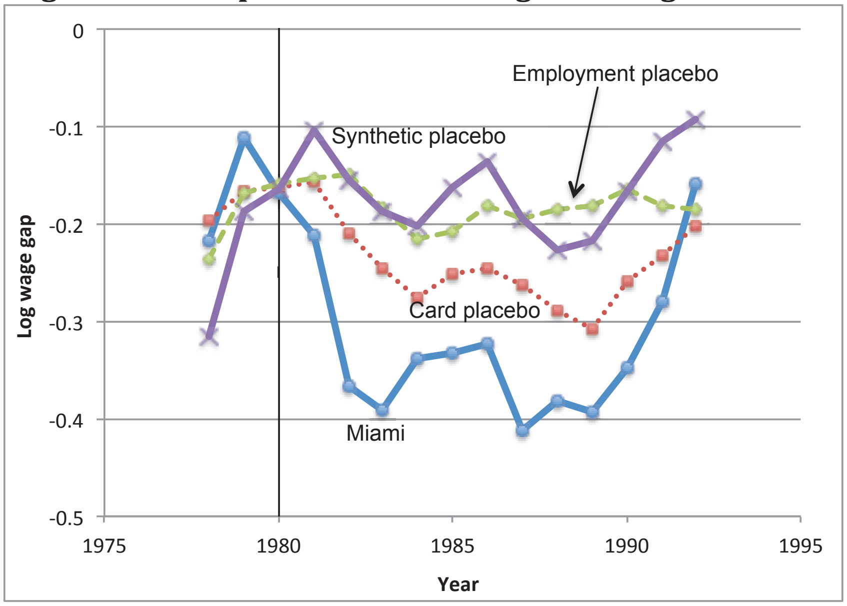


Figure 4 (Continued)

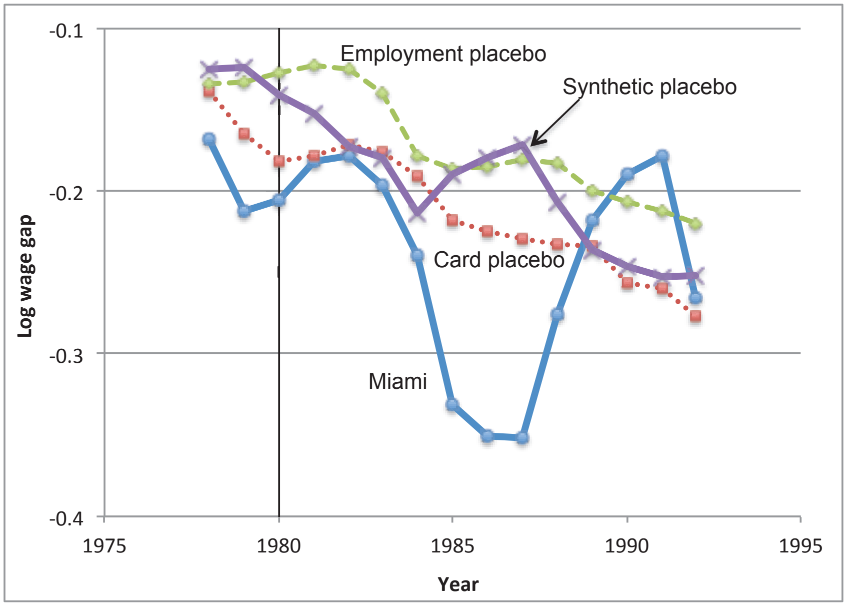
C. Log wage of high school dropouts relative to high school graduates



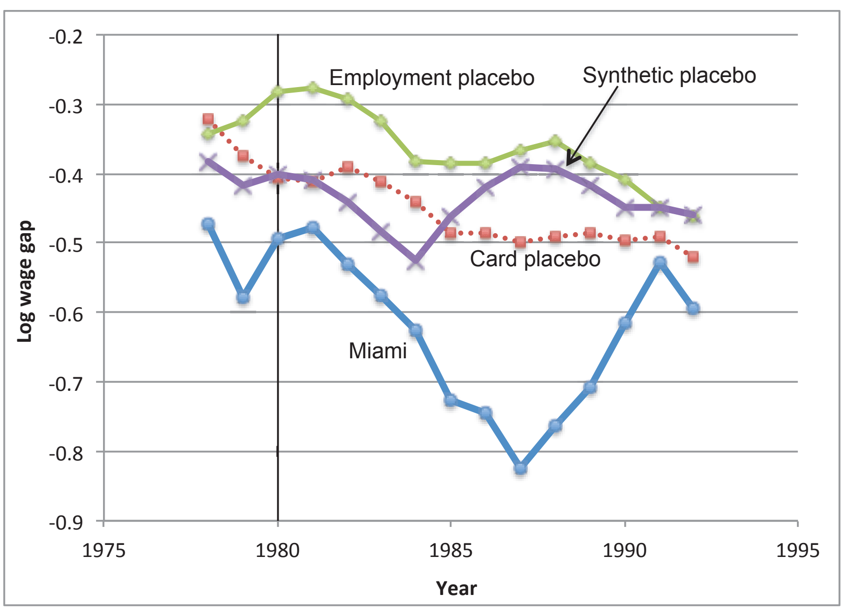
Notes: The figures use a 3-year moving average of the age-adjusted average log wage of high school dropouts, high school graduates, and college graduates in each specific geographic area.

Figure 5. 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



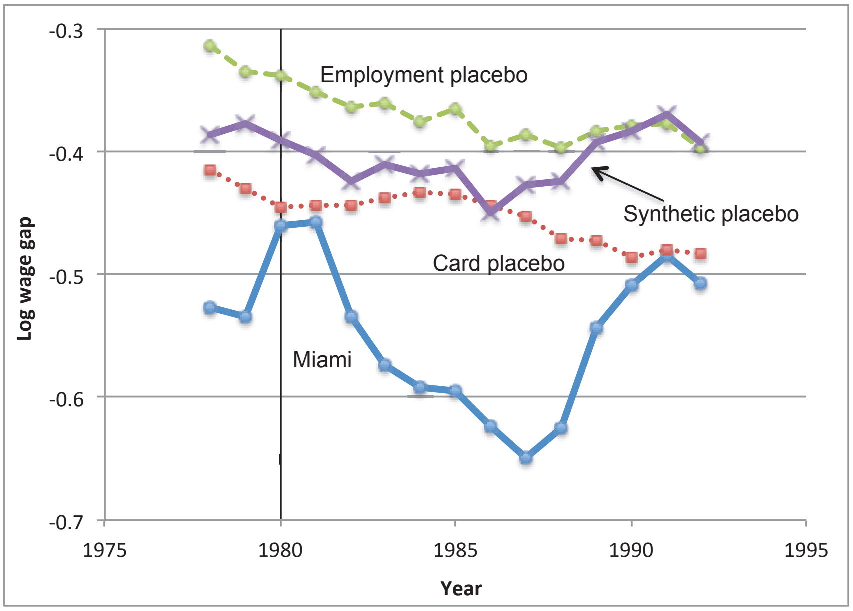
B. Log wage of pooled high school dropouts and graduates relative to college graduates



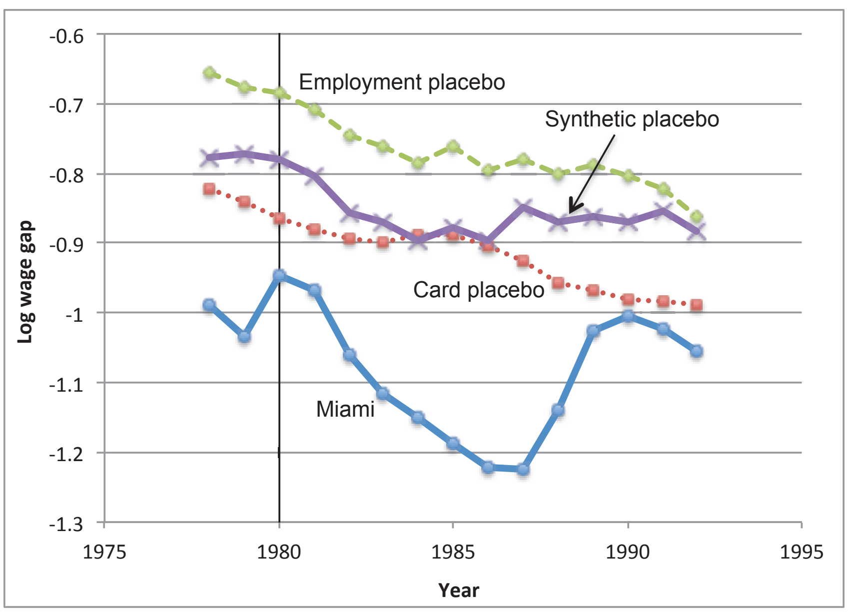
Notes: The figures use a 3-year moving average of the age-adjusted average log wage of the pooled group of high school dropouts and high school graduates, and of college graduates in each specific geographic area.

Figure 6. Trends in the spread of the log weekly wage distribution, March CPS, 1977-1992

A. Log wage of worker at 20th percentile



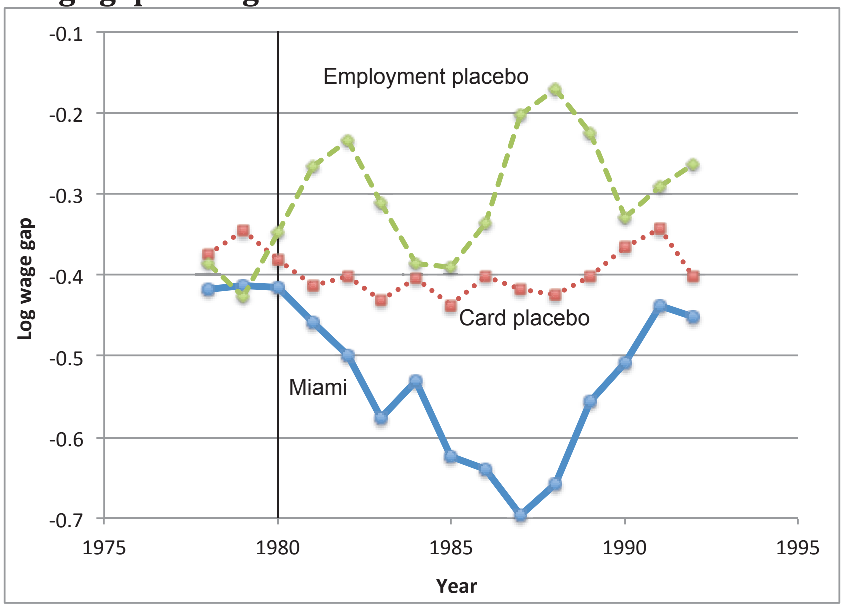
B. Difference in the log wage of workers at 20th and 80th percentiles



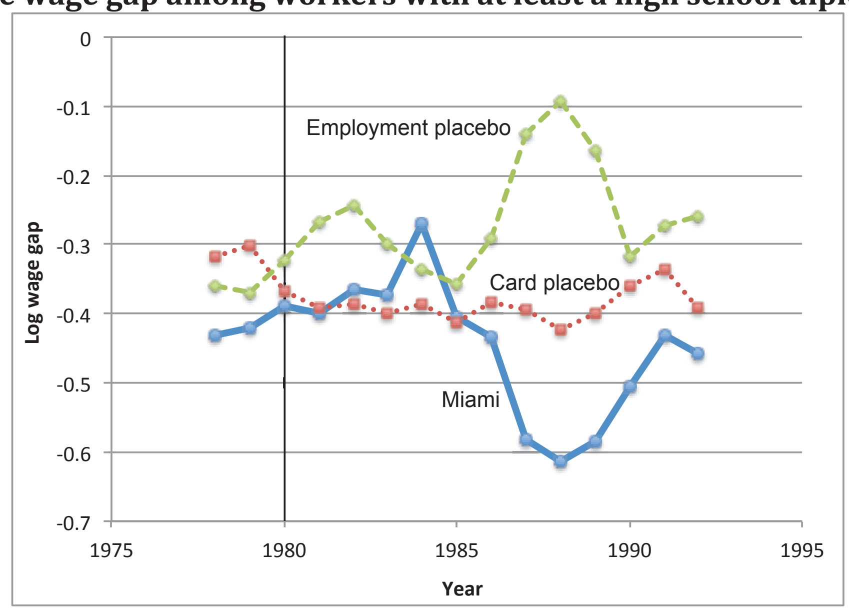
Notes: The figures use a 3-year moving average of the age-adjusted log weekly wage in each specific geographic area for each specific percentile.

Figure 7. Trends in the black-white wage differential, March CPS, 1977-1992

A. Black-white wage gap among all workers



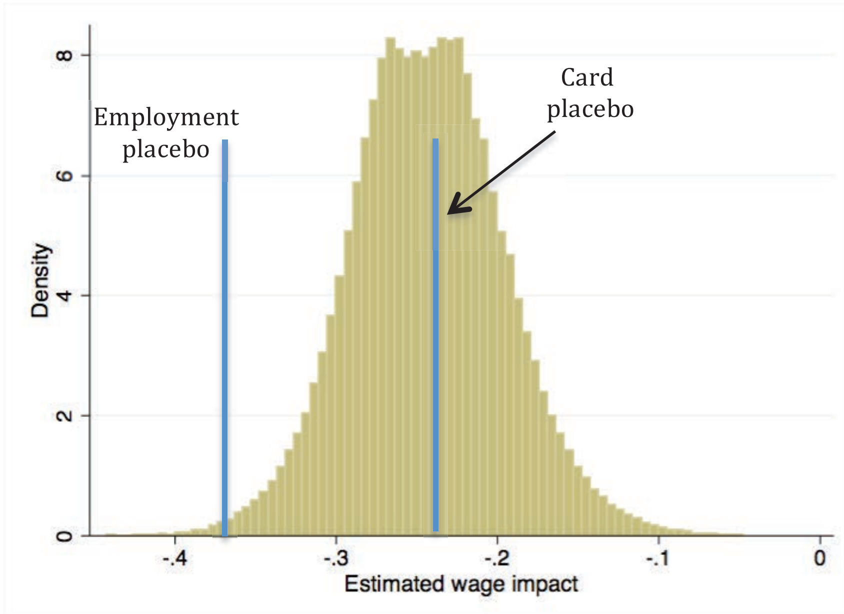
B. Black-white wage gap among workers with at least a high school diploma



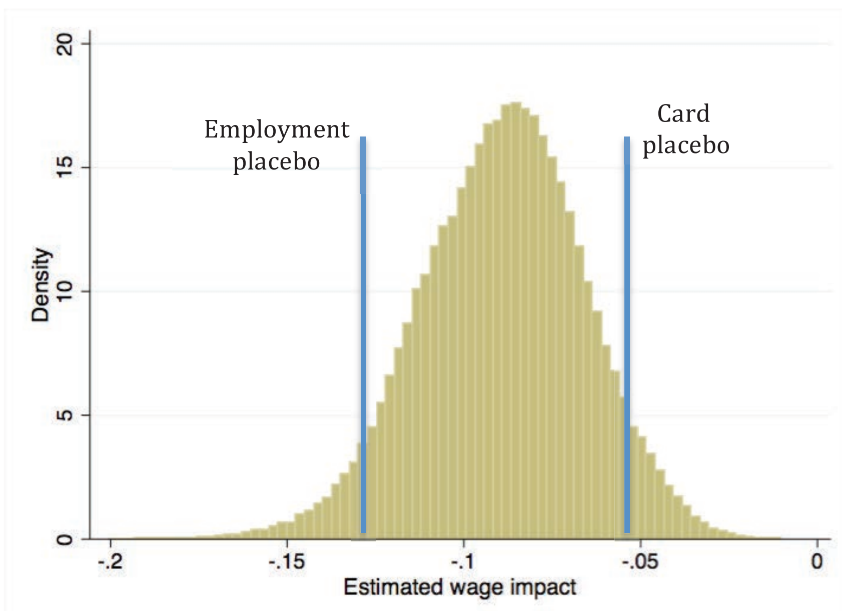
Notes: The figures use a 3-year moving average of the difference in the age-adjusted average log wage between black and white workers in each specific geographic area.

Figure 8. Distribution of short-run impacts across all possible four-city placebos, 1977-1986

A. March CPS

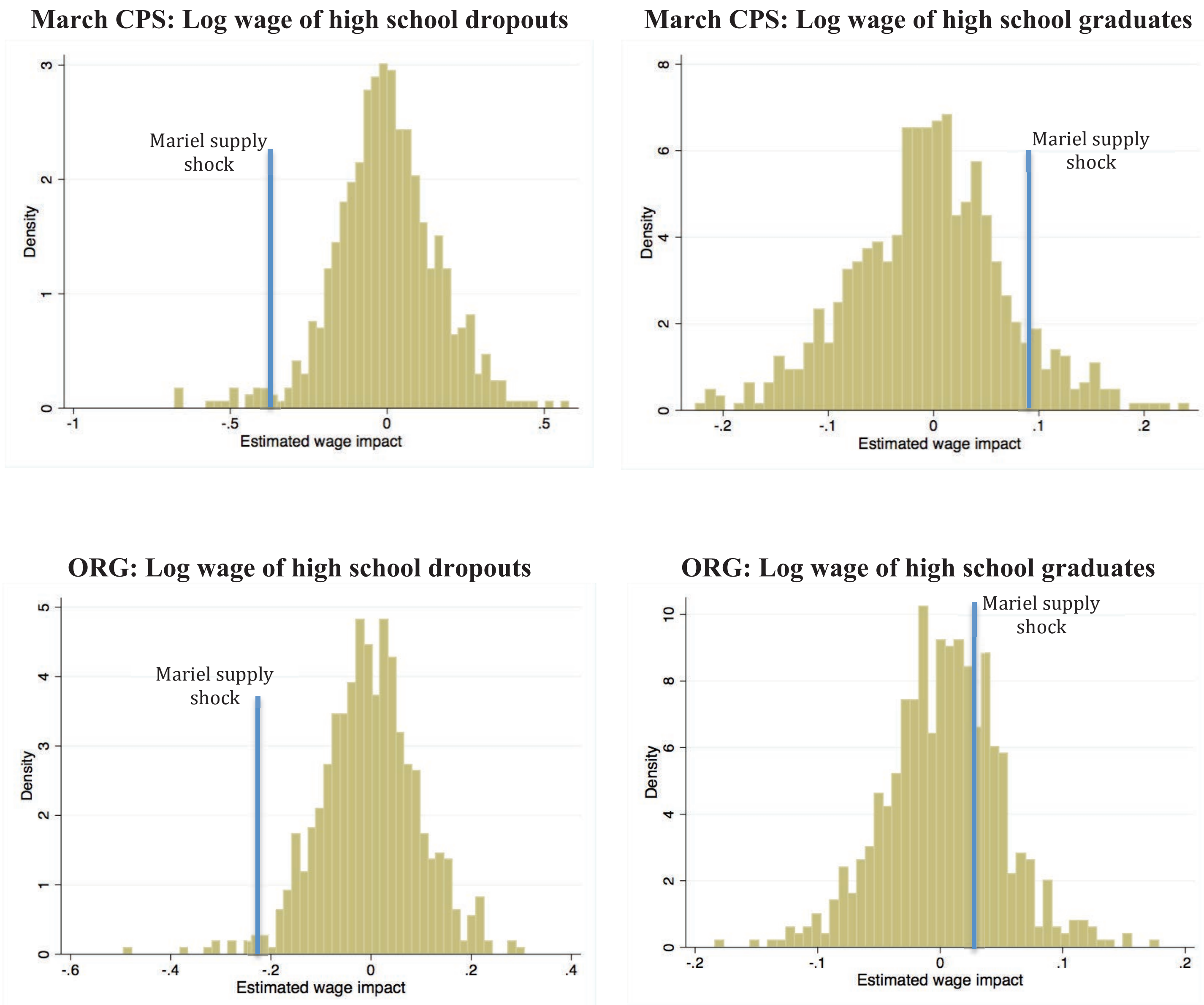


B. CPS-ORG



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



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 distribution does not include any of the wage effects estimated in the Miami metropolitan area.

Table 1. Education distribution of adult *Marielitos*

Sample:	Years of education < 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	4.3	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 workforce:					
1980 Census	25.9	28.5	26.2	19.3	31,393

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 workforce of Miami includes both natives and immigrants.

Table 2. The size of the Mariel supply shock

Education group:	Number of workers in Miami in 1980 (1000s)	Number of working *Marielitos* (1000s)	Percent increase in supply
High school dropouts	162.9	28.5	17.5
High school graduates	179.0	9.4	5.3
Some college	164.6	8.0	4.9
College graduates	121.3	3.9	3.0
All workers	627.9	49.8	7.9

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. Rate of employment growth before Mariel, 1977-1980

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

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.

Table 4. Distribution of wage changes within metro areas across all potential city-year combinations, 1977-2001

	Dependent variable: Log wage of education group			
Characteristics of distribution:	< 12 years	12 years	13-15 years	≥ 16 years
Mean of distribution outside Miami	-0.104	-0.070	-0.034	-0.003
Standard deviation of distribution	0.130	0.088	0.098	0.084
Value for Mariel	-0.447	-0.021	-0.111	0.024
Percentile of Mariel effect	0.8	70.8	21.8	67.8

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 1977-2001 March CPS data. The pre-treatment period lasts 3 years; the post-treatment period lasts 6 years; and the year of the treatment is excluded from the calculation. The data are weighted by $(n_0 n_1)/(n_0 + n_1)$, where n_0 is the number of observations used to calculate the mean wage for a particular city in the pre-period, and n_1 is the respective number of observations used to calculate the mean wage in the post-period. The distributions have 688 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	-0.206	-0.135
	(0.093)	(0.090)	(0.072)	(0.080)
1984-1986	-0.364	-0.495	-0.482	-0.378
	(0.080)	(0.071)	(0.062)	(0.033)
1987-1989	-0.216	-0.251	-0.266	-0.192
	(0.085)	(0.071)	(0.040)	(0.058)
1990-1992	0.188	0.096	0.003	0.188
	(0.158)	(0.136)	(0.075)	(0.111)
B. Log wage relative to college graduates				
1981-1983	-0.168	-0.390	-0.176	-0.180
	(0.187)	(0.164)	(0.167)	(0.170)
1984-1986	-0.387	-0.593	-0.435	-0.453
	(0.159)	(0.154)	(0.185)	(0.135)
1987-1989	-0.340	-0.482	-0.371	-0.357
	(0.154)	(0.159)	(0.165)	(0.132)
1990-1992	0.180	0.084	0.070	0.192
	(0.223)	(0.200)	(0.141)	(0.180)
C. Log wage relative to high school graduates				
1981-1983	-0.276	-0.420	-0.413	-0.285
	(0.131)	(0.146)	(0.128)	(0.136)
1984-1986	-0.490	-0.627	-0.687	-0.470
	(0.114)	(0.093)	(0.101)	(0.094)
1987-1989	-0.344	-0.325	-0.357	-0.254
	(0.098)	(0.071)	(0.057)	(0.082)
1990-1992	0.067	0.016	-0.104	0.122
	(0.195)	(0.144)	(0.112)	(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	-0.228	-0.092
	(0.027)	(0.060)	(0.062)	(0.027)
1984-1986	-0.032	-0.097	-0.228	-0.075
	(0.039)	(0.066)	(0.062)	(0.028)
1987-1989	-0.061	-0.206	-0.285	-0.137
	(0.031)	(0.055)	(0.048)	(0.018)
1990-1992	0.005	-0.105	-0.258	-0.051
	(0.058)	(0.078)	(0.059)	(0.041)
B. Log wage relative to college graduates				
1981-1983	-0.020	-0.171	-0.286	-0.066
	(0.059)	(0.114)	(0.124)	(0.069)
1984-1986	-0.018	-0.130	-0.238	-0.067
	(0.056)	(0.096)	(0.124)	(0.057)
1987-1989	-0.048	-0.291	-0.298	-0.152
	(0.055)	(0.111)	(0.127)	(0.061)
1990-1992	-0.017	-0.173	-0.193	-0.074
	(0.086)	(0.127)	(0.136)	(0.077)
C. Log wage relative to high school graduates				
1981-1983	-0.141	-0.189	-0.311	-0.130
	(0.033)	(0.072)	(0.072)	(0.041)
1984-1986	-0.084	-0.122	-0.269	-0.092
	(0.070)	(0.094)	(0.087)	(0.060)
1987-1989	-0.081	-0.173	-0.253	-0.108
	(0.041)	(0.061)	(0.054)	(0.039)
1990-1992	0.023	-0.082	-0.264	-0.033
	(0.069)	(0.062)	(0.067)	(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	-0.344	-0.237
	(0.088)	(0.078)	(0.082)	(0.076)
2. Log wage of "pooled" high school dropouts and graduates	-0.030	-0.064	-0.030	-0.039
	(0.049)	(0.063)	(0.078)	(0.059)
3. Interquantile range (20^{th} – 80^{th} percentile)	-0.062	-0.029	-0.044	-0.044
	(0.108)	(0.096)	(0.099)	(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	-0.228	-0.087
	(0.026)	(0.049)	(0.051)	(0.022)
2. Log wage of pooled high school dropouts and graduates	-0.003	-0.052	-0.047	-0.036
	(0.018)	(0.019)	(0.026)	(0.014)
3. Interquantile range (20^{th} – 80^{th} percentile)	0.003	-0.035	-0.093	-0.045
	(0.035)	(0.048)	(0.056)	(0.034)
4. Black/white relative wage	-0.134	-0.135	-0.129	-0.127
	(0.037)	(0.051)	(0.055)	(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 7 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	-0.088
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	0.844
Average employment growth of placebo cities within 0.5 standard deviations of Miami (*N* = 5,270)		
Mean	-0.283	-0.105
Fraction of *t*-statistics above |1.6|	0.998	0.932
Fraction of *t*-statistics above |2.0|	0.982	0.808
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	-0.344	-0.228
Robust standard error	(0.082)	(0.051)

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

Characteristics of distribution:	Dependent variable: Log wage of education group			
	< 12 years	12 years	13-15 years	≥ 16 years
A. March CPS				
Mean effect	-0.003	-0.004	-0.003	0.003
Standard deviation	0.164	0.072	0.101	0.072
Fraction of *t*-statistics above |1.6|	0.240	0.273	0.250	0.265
Fraction of *t*-statistics above |2.0|	0.157	0.214	0.170	0.160
Actual impact of Mariel using synthetic control:				
Coefficient	-0.344	0.097	-0.031	0.044
Robust standard error	(0.082)	(0.091)	(0.115)	(0.104)
Percentile of actual Mariel effect	3.4	91.6	36.8	73.4
A. CPS-ORG				
Mean effect	-0.002	0.003	0.005	-0.002
Standard deviation	0.098	0.048	0.065	0.053
Fraction of *t*-statistics above |1.6|	0.235	0.305	0.301	0.310
Fraction of *t*-statistics above |2.0|	0.131	0.221	0.208	0.192
Actual impact of Mariel using synthetic control:				
Coefficient	-0.228	0.035	0.072	0.051
Robust standard error	(0.051)	(0.024)	(0.064)	(0.061)
Percentile of actual Mariel effect	1.6	75.8	88.2	85.0

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 688 hypothetical shocks distributed across 43 metropolitan areas (outside Miami) for treatment years between 1980 and 1995. The predictors used to create the synthetic placebo are the city's rate of total employment growth and the rate of employment growth for the particular education group in the 4-year period culminating in the treatment year.