

# The Labor Market Effects of Refugee Waves: Reconciling Conflicting Results

**Michael Clemens and Jennifer Hunt**

## Abstract

An influential strand of research has tested for the effects of immigration on natives' wages and employment using exogenous refugee supply shocks as natural experiments. Several studies have reached conflicting conclusions about the effects of noted refugee waves such as the Mariel Boatlift in Miami and post-Soviet refugees to Israel. We show that conflicting findings on the effects of the Mariel Boatlift can be explained by a sudden change in the race composition of the Current Population Survey extracts in 1980, specific to Miami but unrelated to the Boatlift. We also show that conflicting findings on the labor market effects of other important refugee waves can be produced by spurious correlation between the instrument and the endogenous variable introduced by applying a common divisor to both. As a whole, the evidence from refugee waves reinforces the existing consensus that the impact of immigration on average native-born workers is small, and fails to substantiate claims of large detrimental impacts on workers with less than high school.

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## Introduction

A long literature in labor economics has reached something of a consensus that the effects of immigration on average native workers' wages and employment is generally small or zero.<sup>1</sup> There is less consensus on the narrower question of the impact of immigration on less-skilled workers: [Blau and Mackie, eds \(2016\)](#) conclude that the effect of immigration on wages of native-born workers with less than high school is negative, but do not reach consensus on the magnitude of the effect.

An influential strand of research has tested for labor market effects on natives using exogenous refugee supply shocks as natural experiments. Small or null effects on average native workers have been found following large refugee inflows such as those in 1980s Miami ([Card 1990](#)), 1960s France ([Hunt 1992](#)), 1990s Israel ([Friedberg 2001](#)), and in the 1990s across Europe ([Angrist and Kugler 2003](#)). But a subsequent and important strand of research has revisited those earlier works—debating whether they missed impacts on subgroups of natives such as the least skilled ([Borjas 2017](#); [Peri and Yasenov 2016](#)), relied on inadequate causal identification ([Angrist and Krueger 1999](#)), or both ([Borjas and Monras 2017](#)). The discordant findings in this literature have not been reconciled.

In this paper we offer two new explanations for the conflicting results in all of the above studies. One is large compositional changes in the underlying survey data; the other is specification choices in the use of instrumental variables. Accounting for these differences can reduce or even eliminate substantial disagreement on the labor market effects of refugee waves in this literature.

First, we show that the discrepancy between [Card's \(1990\)](#), [Borjas's \(2017\)](#), and [Peri and Yasenov's \(2016\)](#) analyses of the Mariel Boatlift can be fully explained by a large, simultaneous, and hitherto unreported change in the composition of the survey subsamples. In 1980, coinciding exactly with the Boatlift, the fraction of non-Hispanic blacks suddenly doubles in the subgroup of Miami workers with less-than-high-school analyzed by [Borjas](#)

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<sup>1</sup>See the National Academies consensus report for the United States ([Blau and Mackie, eds 2016](#), 204), or the survey by [Kerr and Kerr \(2011\)](#) including Europe.

(2017). Thereafter it rises still further, to compose over 90 percent of the sample—almost triple its level just before the Boatlift. No such increase occurs in the subgroup of natives with high-school-or-less analyzed by Card (1990) in the same dataset, nor in the control cities favored by either Card or Borjas.

Due to the large wage difference between black and non-black workers with less than high school, this sharp shift from majority non-black in 1979 to majority black in 1980—and almost entirely black by 1985—can account entirely for the magnitude of the effect measured by Borjas (2017) relative to the null result of Card (1990). It can also explain several other findings in the reanalyses of Borjas (2017) and Peri and Yasenov (2016). For example, they find that the estimated wage effect of the Mariel Boatlift is roughly three times larger in the March Current Population Survey (CPS) extracts than in the Merged Outgoing Rotating Group CPS extracts, and the racial composition shift we demonstrate is likewise three times larger in one extract than the other. We describe three separate mechanisms by which this sample-composition effect could arise, all irrelevant to the Mariel Boatlift: 1) a sharp increase after the 1980 census in surveyors' efforts to cover black male Americans, 2) relatively low incomes among marginal blacks thereby added to the sample, and 3) the simultaneous arrival in Miami of very low-income non-Hispanic blacks from Haiti.

Second, we show that recent applications of instrumental variables to revisit the effects of the Mariel Boatlift and three other refugee waves—in France, Israel, and across Europe—give similar results to the original studies after a specification correction. First, we show that the instrument used by Borjas and Monras (2017), with which they find larger harmful effects on native workers than found in some of the original studies, gives results that can be reproduced with a placebo instrument. The Borjas and Monras instrument rests on the attraction of new migrants to the locations of prior migrant inflows (Altonji and Card 1991); the placebo instrument replaces information on prior migrant flows with white noise, but gives similar results. This is a consequence of spurious correlation between the instrument and the endogenous variable introduced by applying a common divisor to both of them (Bazzi and Clemens 2013). The problem is addressed with a specification correction due to Kronmal (1993), after which otherwise identical methods give the same

results as the original instrumental-variable studies: a positive but statistically insignificant effect on native wages in Israel, a small detrimental and statistically significant effect on native unemployment in France, and an unstable, statistically insignificant effect on native unemployment in Europe.

Overall, we conclude that the evidence from refugee waves reinforces the existing consensus that the impact of immigration on average native-born workers is small, and fails to substantiate claims of large detrimental impacts on workers with less than high school. The paper begins in [Section 1](#) by reviewing discrepant analyses of the Mariel Boatlift in Miami, and showing that a sharp and simultaneous shift in the subsample composition of the underlying survey data can account for the discrepancies. It proceeds in [Section 2](#) to review discrepant results on the effects of the three other refugee waves, and show that a specification correction can reconcile the results. In [Section 3](#) it concludes by discussing the interpretation of this literature.

## 1 A refugee wave from Cuba: The Mariel Boatlift

In mid-1980, a sudden and unexpected influx of refugees from Mariel Bay, Cuba raised the labor supply in Miami, Florida by seven percent. [Card \(1990\)](#) compares trends in Miami to trends in four unaffected control cities and concludes that “the Mariel immigration had essentially no effect on wages or employment outcomes of non-Cuban workers.” This study has become influential in labor economics research methods and in immigration policy debate, as well as in graduate economics education ([Cahuc et al. 2014](#)).

### 1.1 Conflicting reanalyses

Recent, concurrent reanalyses have reached contrasting conclusions about the robustness of the original [Card](#) study. Both [Peri and Yasenov \(2016\)](#) and [Borjas \(2017\)](#) challenge the original method for selecting control cities and use recently developed methods to construct a synthetic control more transparently ([Abadie et al. 2010](#)). While [Card](#) had studied the effects of the Boatlift on natives with *high school or less*, both of the new reanalyses study

the impact on natives with *less than high school*. But in this latter subgroup, [Borjas](#) finds that the Boatlift caused the wages of males in this subgroup to fall “dramatically, by 10 to 30 percent,” and [Peri and Yasenov](#) find instead “no significant departure between Miami and its control.”<sup>2</sup> The studies stress different extracts from the Current Population Survey (CPS), use different weighting variables to construct the synthetic control city, and choose different groups of ‘natives’ to study.<sup>3</sup>

Several findings in these two conflicting reanalyses have not been adequately explained. These include:

- *There is no observed effect on workers with high school or less, or workers with exactly high school.* The estimated wage effect of the Mariel Boatlift is absent for workers with high school *or less* ([Card 1990](#)), as well as for workers with high school only considered separately.<sup>4</sup> This sharp contrast versus the results for less-than-high-school is somewhat at odds with evidence that workers with high-school-only and less-than-high-school are close substitutes in the United States ([Card 2009](#)), and the fact that the Mariel boatlift likewise created a large positive shock to the supply of workers with a high school degree in Miami: Almost half of the Mariel migrants did have a high school degree ([Borjas 2017](#), Table 1). It is of course possible in principle that the Mariel migrants with less than high school complemented natives with high school only, to a degree that just offset the substitution effect created by Mariel migrants with a high school degree.
- *The observed effect size depends on the CPS extract used.* Two nationally representative wage survey samples cover the years before and after the Mariel Boatlift: a) the Current Population Survey (CPS) March Supplement, and b) a combination of the CPS May supplement (through 1978) and the CPS Merged Outgoing Rotating Groups (MORG) from 1979. [Borjas \(2017, Tables 5–6\)](#) finds effects three times larger

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<sup>2</sup>[Borjas \(2017\)](#) studies wage effects and does not reanalyze [Card](#)’s null result on employment effects. [Borjas and Monras \(2017\)](#) do reanalyze [Card](#)’s null result on employment, and confirm it, as do [Peri and Yasenov \(2016\)](#). Both [Borjas](#) and [Peri and Yasenov](#) test the robustness of their findings to samples that, like [Card](#)’s, include women. Note that ‘natives’ cannot be strictly identified in the data samples used by [Card](#) and [Borjas](#) because the CPS does not report country of birth, but both studies focus on non-Hispanic subgroups that are likely to consist of primarily natives.

<sup>3</sup>An issue faced by all studies is that the CPS did not collect country of birth at this time, so the impact on ‘natives’ is imputed from estimated impacts on groups likely to be predominantly natives.

<sup>4</sup>The latter finding is below, in [Table 12](#).

in the March CPS data than in the May data. [Peri and Yasenov \(2016\)](#) attribute this large difference to sampling error (the March CPS sample is smaller than the MORG sample)<sup>5</sup> and recall bias (the March CPS asks about earnings in the prior year, the MORG in the survey week). But because the effect estimated by [Borjas \(2017\)](#) persists across several years, it appears unlikely to arise from pure sampling error or measurement error.<sup>6</sup>

- *There is no observed effect on U.S. Hispanics.* All reanalysis of [Card](#)'s results confirms that the Mariel migrants had no effect on the labor market outcomes of U.S. Hispanic workers, nor of native workers with or without a high school degree collectively in samples that include Hispanics.<sup>7</sup> [Borjas \(2017\)](#) argues that omitting U.S. Hispanics is necessary because many U.S. cities were experiencing a contemporaneous influx of non-Cuban Hispanics. Though excluding Hispanics is consistent with attempting to study impacts on a predominantly native-born sample, the lack of effect on Hispanics is nonetheless a puzzle. Theory does not suggest a clear reason why Cubans would compete directly with non-Hispanic workers while not competing at all with other Hispanics. English language skill is an important segmenter of the labor market ([McManus 1990](#); [Peri and Sparber 2009](#); [Lewis 2013](#)), suggesting that newly arrived Cubans could substitute for newly arrived non-Cuban Hispanics at the same low skill level.<sup>8</sup>
- *The [Borjas](#) estimated wage effect keeps increasing for years after the supply shock, and long outlasts the supply shock.* [Peri and Yasenov \(2016\)](#) observe that after 1984, the share of Cubans among workers with less than high school in Miami returned

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<sup>5</sup>The March CPS sample is indeed small at 17–27 workers in each year 1977–1983, though the May/MORG sample is even smaller (12 and 16) during the 1977–1978 surveys and the annual MORG sample falls in the range 31–56 ([Borjas 2017](#), Table 3).

<sup>6</sup>The subsample is male non-Hispanic workers, age 25–59, reporting positive annual wage and salary income, positive weeks worked, as well as (in the March CPS) reporting positive usual hours worked weekly, or (in the MORG) positive usual weekly earnings and positive usual hours worked weekly.

<sup>7</sup>[Peri and Yasenov \(2016\)](#) show a null result for Hispanics only in a framework similar to [Borjas's \(2017\)](#), in either March or May data. For our own reanalysis of the [Borjas and Monras \(2017\)](#) results on the Mariel Boatlift for Hispanics only, see [subsection 2.4](#) below.

<sup>8</sup>The finding that the Mariel Cubans strongly substituted for non-Hispanics, but not for Hispanics, contradicts contemporary evidence from nationwide census data. Using 1980 national census data, [Borjas \(1987, 390\)](#) finds that Cuban immigrants are complements to black and white natives, as well as to black and white immigrants. He also finds that Hispanic immigrants in general are complements to black natives, concluding that in 1980, “Cubans have not had an adverse impact on the earnings of any of the native-born male groups. In fact, a significant complementary relationship exists between Cuban men and white, black, and Asian native-born men.”



to pre-Boatlift levels in the CPS data. [Borjas \(2017\)](#) estimates that the Mariel shock depressed wages to a greater degree in each passing year up to 1984, and then continued to depress wages in Miami for several years after 1984. There may be wage adjustment mechanisms that would lead to such delayed and persistent effects on wages—so that the principal effects of the shock only increase slowly during the supply shock but persist long after it ends—but these mechanisms are unclear.

- *All studies agree there was no effect on unemployment.* The various studies’ disagreement on wage effects of the Boatlift is more striking given their agreement that the Boatlift had no detectable effect on native unemployment ([Card 1990](#); [Peri and Yasenov 2016](#); [Borjas and Monras 2017](#)). It is theoretically possible for Cubans flooding the Miami labor market to have large effects on wages but no effects on unemployment, though this would seem to require a high degree of downward flexibility in low-skill wages that is not supported by all strands of the labor literature (e.g. [Altonji and Devereux 2000](#)).<sup>9</sup> Given that the wage effect found by [Borjas \(2017\)](#) is so large (perhaps  $-30\%$ ), and given that [Borjas and Monras \(2017\)](#) find both wage and unemployment effects of other refugee wages, it is something of a puzzle that the wage and unemployment effects are found to be disjoint in Miami.

## 1.2 Compositional change in the survey data

We propose that all of these discrepancies can be explained by compositional changes in the underlying survey data: a sharp increase in the number of sampled blacks coincident with the Mariel Boatlift but unrelated to it. This compositional effect can generate spurious wage impacts of the Mariel Boatlift of the same magnitude estimated in the literature. It could arise by three mechanisms:

MECHANISM A: *Increased coverage of blacks, who earn less than non-Hispanic whites with the same education—but do not have higher unemployment.* Major efforts to raise coverage of blacks, especially males, in nationally representative surveys were spurred by political

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<sup>9</sup>The higher estimates of the wage impact of the Boatlift would require flexibility in nominal wages, not only in real wages. Cumulative consumer price inflation in Miami from July 1980 to July 1983 was 20.4%. From: U.S. Bureau of Labor Statistics series CUUSA320SA0, “CPI—All Urban Consumers, All items in Miami-Fort Lauderdale, FL, not seasonally adjusted.”

pressure in the run-up to the 1980 census. In 1978, the Levitan Commission had quantified major undercoverage of black men in the 1970 census (Levitan et al. 1979, 142), raising national pressure to raise coverage of that group in particular. By 1980, Senate hearings described the Census Bureau as “embattled” and engaged in “massive efforts to improve coverage” (U.S. Senate 1981, 1–2, 48). Efforts to respond by improving coverage focused on low-income black men. There was particular pressure in Miami, including a lawsuit led by then-mayor Maurice Ferré that joined a handful of other cities in alleging large undercounts of low-income urban blacks due to “negligence or malfeasance attributable to local Census Bureau officials.”<sup>10</sup> The backdrop for these pressures was the the May 1980 riots in the Liberty City and Overtown sections of Miami, which had led to a widespread perception that Miami’s low-income blacks had been ignored by the government (Pendleton et al. 1982).

Many of the Levitan Commission’s recommendations were implemented immediately in and after 1980 (Hamel and Tucker 1985). These changes included additional ‘coverage samples’ to capture more low-income black residences and greater efforts by enumerators to identify all of the people residing in a visited residence (Brooks and Bailar 1978). Starting in the March 1981 CPS (representing 1980 in the figures below, following Borjas 2017), the Current Population Survey extracts changed the treatment of race, because “[a]nalysis of results from the 1980 census indicated that reporting of race was not directly comparable with CPS because of different data collection procedures.” The degree to which this altered CPS coverage of different black subpopulations is not recorded in publicly available documents, but these measures were taken in order to arrive at “more precise estimates . . . for black and non-black populations” (Census Bureau 1982, 13)—that is, to reduce undercounts of blacks.

MECHANISM B: *Income-composition effects among blacks*. Increases in coverage of blacks in the surveys tended to include, at the margin, more relatively low-income blacks. Contemporary efforts to improve coverage among blacks in and after 1980 clearly focused on the poorest blacks (Levitan et al. 1979, 139; U.S. Senate 1981, 82–83; Durant and Jack 1993). Ethnographers at the time found that marginal blacks added through more exten-

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<sup>10</sup>Maurice A. Ferré, et al. v. Philip M. Klutznick, et al. C.A. No. 80-2933, Southern District of Florida, October 30, 1980; *In Re* 1980 Decennial Census Adjustment Litigation., 506 F.Supp. 648 (JPML 1981).

sive survey efforts would tend to be the poorest blacks—those who had been concealed from surveyors in order to preserve welfare benefits, or those whose “transiency and mobility” in the poorest inner-city black neighborhoods “does not fit the Census Bureau assumption of a ‘usual residence’” (Hainer et al. 1988, 514).

MECHANISM C: *Immigration by poor blacks.* Until 1994, the Current Population Survey did not regularly report an individual’s country of birth, so U.S. native blacks cannot be distinguished from immigrant blacks in the 1970s and 1980s. There was a large increase in immigration to Miami by black Haitians precisely in 1980 (Portes and Stepick 1985; Stepick and Portes 1986). “Haitian boat arrivals had been detected by the Immigration and Naturalization Service previously, but they did not exceed an average of 3,000 per year. In 1980, however, the number swelled to over 15,000” (Portes and Stepick 1985, 496). This was smaller than the Mariel Cuban shock of 125,000 in 1980, but its relative size was larger in the less-than-high-school subgroup: Almost half of the Mariel migrants had a high school degree (Borjas 2017, Table 1), but almost none of the 1980 Haitians did (Portes and Stepick 1985, 495, 497). That is, for the less-than-high-school subpopulation as a whole this Haitian shock was roughly one fourth the size of the Mariel migrant shock. Many of the Haitians arriving in and after 1980 moved into residences previously occupied by native blacks (Wingerd 1992), but had much lower average earnings, even within skill groups (Portes and Stepick 1985; Portes et al. 1986). This too would generate an income-composition effect among non-Hispanic blacks.

All of these effects would tend to produce important shifts in the racial composition of CPS samples over time, particularly low-skill blacks, and complicate efforts to measure changes in wages over time. They would tend to create discontinuities in those measures in 1980 that do not arise from the effects of the Mariel Boatlift, and do not arise from random sampling error. Random sampling error is an unlikely cause of sudden changes in sample composition that persist over several years, even when each sample is small.<sup>11</sup>

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<sup>11</sup>Another mechanism that could spuriously produce wage declines in Miami at this time, in principle, would be a suddenly influx of U.S. blacks into the city coincidentally occurring in 1980. But census data show no important change to the rate of increase of Miami’s overall population of U.S. blacks (at all skill levels) in the years after 1980 relative to the years before 1980 (Bureau of the Census 1982, 22; Starsinic and Forstall 1989, 40–41). And histories of Miami’s black population mention no large and sudden surge in overall native-born black migration to Miami in 1980 that would cause a discontinuity in the true population of native-born blacks there (Dunn 1997).

### 1.3 Spurious wage changes due to compositional effects

Here we show that all substantial differences between the wage effects measured in [Card \(1990\)](#), [Peri and Yasenov \(2016\)](#), and [Borjas \(2017\)](#) can be explained by large compositional changes in the underlying CPS data on workers with less than high school, driven in turn by a jump in the share of black men, coincident with the Mariel Boatlift. There was a sudden and persistent doubling, in 1980, of the fraction of black men covered by the Miami CPS subsample of men with less than high school. Thereafter, the black fraction rose even further until blacks constituted almost the entire sample in 1985. Because blacks typically earned much less than non-blacks, this change in survey coverage can explain why these three studies reach conflicting conclusions—as well as the magnitudes of their various findings.

[Figure 1a](#) shows the fraction black, according to the March CPS subsamples, in the exact samples and subpopulation studied by [Borjas \(2017, Table 3A\)](#).<sup>12</sup> These are weighted averages of an indicator variable taking the value 1 for a black man, 0 otherwise, weighted by the CPS Supplement-specific weight. Thus changes in the black fraction reported here cannot be accounted for by increased coverage of blacks in the population that is considered in the weights.

The change in 1980 is sudden and very large: the sample suggests that blacks suddenly go from 36.3% of this subpopulation to 63.0%—in a single year. Then this fraction keeps rising, to 67.3% by 1983 and 91.0% in 1985. [Table 1](#) shows the raw counts of blacks and non-blacks, and shows that the sharp rise in the weighted fraction black is similar in the unweighted fraction.

This does not arise from a nationwide increase in survey coverage of blacks in this subpopulation. It is specific to Miami. [Figure 1a](#) also shows that the estimated fraction black

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<sup>12</sup>This subpopulation is male non-Hispanic workers, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly. Following [Borjas](#), the years in the graph refer to the year in which earnings were earned, not the year of the survey. Thus for example the earnings of workers in 1980 references data taken from the 1981 March CPS. The average sample size in each year is 20 individuals, with a maximum of 27 (in 1983) and a minimum of 15 (in 1985). Data are from IPUMS-USA ([Ruggles et al. 2015](#)). [Table 1](#) also shows that the large, persistent rise in the black fraction is present in both the weighted and unweighted fractions.

in this subpopulation in the March CPS remained stable in the group of control cities preferred by Card and notably *fell* in the control cities preferred by Borjas—where it actually reaches zero in 1983.<sup>13</sup> Note that the change in black fraction exhibits a similar, large, Miami-specific jump in 1980 (not graphed here) among all male non-Hispanics aged 25–59—including those not working—thus the jump arises not from a sudden shift in labor force participation by this group but in survey coverage.

This sudden change in Miami relative to the controls can account fully for the post-1980 estimated change in wages in the March CPS subsample in Miami relative to other cities. To show this, in Table 2 we first regress the log wage on an indicator variable for black, in the March CPS samples of this subpopulation across 1977–1986.<sup>14</sup> The coefficient estimate  $\hat{\beta}^{\text{Miami}} = -0.487$ . For the subpopulation in the control cities, the corresponding coefficient is  $\hat{\beta}^{\text{Control}} = -0.219$  for the Card controls and  $\hat{\beta}^{\text{Control}} = -0.285$  for the Borjas controls.<sup>15</sup> We can then estimate what change in the average log wage in this subpopulation, in and after 1980 relative to 1979, would arise exclusively from the changing racial composition of each CPS sample. This is:

$$\Delta \ln \tilde{w}_t = \hat{\beta}^{\text{Miami}} \left( b_t^{\text{Miami}} - b_{1979}^{\text{Miami}} \right) - \hat{\beta}^{\text{Control}} \left( b_t^{\text{Control}} - b_{1979}^{\text{Control}} \right), \quad (1)$$

where  $b$  is the fraction black in the subpopulation analyzed by Borjas (2017) (that is, male non-Hispanic workers 25–59 with less than high school), weighted by the sampling weight. The estimates of  $\Delta \ln \tilde{w}_t$  for the March CPS are shown in Figure 2a for both sets of control cities. With the Card controls, this compositional change in the sample by itself

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<sup>13</sup>In this paper, references to individuals in the Card or Borjas control cities refer to people with the specified traits in the control cities favored by either author, not the exact samples they use; for example in Figure 1a the black fraction in the Card control cities refers to male non-Hispanics age 25–59 with less than high school in the same cities used by Card, not to the same survey sample used by Card. The fraction black for control cities refers to the average of an indicator variable equal to 1 for black and 0 otherwise, across individual 25–59 year old non-Hispanic male workers with less than a high school degree, weighted by the sampling weight, for individuals residing in any of the control cities pooled. The Card control cities, chosen because they resembled Miami in employment growth “over the late 1970s and early 1980s,” are Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg. The Borjas control cities, chosen to resemble pre-1980 employment growth in Miami, are Anaheim, Rochester, Nassau-Suffolk, and San Jose.

<sup>14</sup>This period is chosen to match the years covered by Borjas (2017, Tables 6–7).

<sup>15</sup>The use of sampling weights in regressions of this type is controversial (Solon et al. 2015), but the Miami coefficient estimate on the black dummy is similar when the regression is unweighted (−0.465). If we isolate the years during this period that postdate the survey changes (1981–1986), the coefficient estimate is slightly more negative:  $\hat{\beta}^{\text{Miami}} = -0.531$ .

produces a change of  $-0.15$  log points by 1983, and  $-0.25$  log points by 1985. With the Borjas controls, because the black fraction is falling in those control cities, the effect is even larger:  $-0.18$  by 1983, and  $-0.27$  by 1985.

This sample-composition effect on the average wage is comparable in magnitude to the entire treatment effect estimated for this subpopulation by Borjas. He attributes to the Mariel Boatlift a fall in wages of “10 to 30 percent” for natives with less than high school—the same magnitude as the wage changes that would arise purely from increased coverage of blacks in the March CPS after 1980 (Figure 2a), which would produce declines of 18 to 27 percent. The compositional effects are contained in the confidence interval of all the corresponding treatment effects estimated in Borjas (2017, Table 6), which range from  $-0.27$  to  $-0.36$  and do not allow one to reject values that are between 0.12 and 0.16 smaller in absolute value than those point estimates.

In the May/MORG samples, too, there is a substantial compositional change: the estimated black fraction of this subpopulation rises after 1980 in Miami, while remaining stable in the Card control cities and falling in the Borjas control cities (Figure 1b and Table 1).<sup>16</sup> But the magnitude is smaller than in the March CPS, and thus so is the predicted log wage change arising exclusively from compositional effects. Figure 2b repeats the above exercise for the May/MORG samples, again for precisely the samples and the subpopulation considered in Borjas (2017, Table 3A). The spurious wage effect is on the order of  $-0.07$  by 1983 and  $-0.12$  by 1986, for both sets of control cities (Figure 2b). This compositional effect in the May/MORG is roughly one third the size of the corresponding effect in the March CPS. This change, too, can fully explain the wage decline observed by Borjas (2017, Table 6) in the May/MORG data, which is roughly one third the size of the wage change in the March CPS data.<sup>17</sup>

Next we return to the March CPS and repeat the exercise for the samples of a different

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<sup>16</sup>In the May/MORG data, the coefficient estimate in a regression of log wage on the black dummy  $\hat{\beta}$  in the Borjas (2017) subpopulation 1977–1986 is:  $-0.353$  in Miami,  $-0.319$  in the Card control cities, and  $-0.177$  in the Borjas control cities.

<sup>17</sup>In Borjas (2017, Table 6), the average ratio of the men-only treatment effect estimate in the March CPS data to the treatment effect estimate in the CPS-ORG data is 3.08. In Appendix A we discuss reasons why the racial composition might have changed so much more in the March CPS extracts after 1980 than in the May/MORG extracts.

subpopulation: workers with high school *or less*. This is the low-skill group considered by Card. The strong compositional effects seen above are absent in the samples of this subpopulation (Figure 1c). There is no jump in the black fraction in Miami between 1979 and 1980. The black fraction drifts slightly up from 1979 to 1981 and then down in the years following, while the fraction remains stable in the control cities.<sup>18</sup> For this reason the estimated compositional effect on the average log wage is very small, with the wage effect due to composition just  $-0.02$  to  $-0.03$  in 1981 (depending on the control cities) before rising back to zero and slightly above (Figure 2c). The absence of substantial composition effects in the subpopulation studied by Card offers a clear explanation for why his results differ from those of Borjas, and can explain the full magnitude of the discrepancy.

Finally, we show that earlier findings of a negative wage effect are not robust to controlling for the racial composition of the subsamples, in Figure 4. The figure begins by showing the average wages for male non-Hispanics with less than a high school degree, which fell faster after 1980 in Miami relative to the control cities (Figure 4a, which corresponds to Borjas 2017, Figures 5–6 without smoothing). Figure 4b shows the implied differences between wages in Miami versus the controls (with 1979 normalized to zero), which might suggest a negative wage effect of the Boatlift. Figure 4c makes a single change to Figure 4a: the mean log wages in each city-year are calculated controlling for the average black-nonblack wage gap in that city-year.<sup>19</sup> Figure 4d shows the wage differences implied by Figure 4c. There is no longer a relative fall in average wages after the Mariel Boatlift. In fact, wages the year after the Boatlift are unchanged in Miami relative to the control cities preferred by Borjas, and rise relative to the control cities preferred by Card. The following year, wages rise greatly in Miami relative to both groups of control cities. In the years after that, wages are consistently higher in Miami than in the Card control cities. They are slightly lower in Miami than in the Borjas control cities, but only starting three years after the Boatlift—when this difference would be difficult to attribute to the Boatlift. Comparing Figure 4b and Figure 4d shows that simply controlling for the racial composition change

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<sup>18</sup>The reason that the black fraction can rise sharply in the less-than-high-school subgroup but barely rise in the high-school-or-less subgroup is that the black fraction exhibits a large *fall*, specific to Miami, in the high-school-only subgroup. This is shown in Appendix Figure 1. In other words, in the years after 1980 in Miami—but not in the control cities—there was a large shift of coverage among blacks with high school or less, away from high-school-only and toward less-than-high-school.

<sup>19</sup>That is, Figure 4c reports the coefficient on the constant term from an OLS regression of log wage on an indicator variable that is 1 for black and 0 otherwise, run separately for each city-year.

in this subsample of natives fundamentally alters the result.

#### 1.4 Reconciliation of prior findings

The 1980 increase in the share of blacks among male, non-Hispanic, less-than-high-school workers in Miami can fully explain why [Borjas \(2017\)](#) finds that wages in this group fell relative to wages in the control cities. This change in racial composition of the sample occurred simultaneously with, but independently of, the Mariel Boatlift. It can explain why [Card \(1990\)](#) finds no wage effect among men with high school or less, because no large change in racial composition occurred in that group. Before 1980, blacks with less than high school had been undercovered relative to blacks with exactly high school. After 1980 this was rectified: coverage of blacks increased among workers with less than high school, decreased among workers with exactly high school, and changed little for the combined high-school-or-less group.

This can also explain several other previous findings. 1) It can explain why [Borjas \(2017\)](#) and [Peri and Yasenov \(2016\)](#) find a wage effect three times larger in the March CPS than in the MORG: The change in racial composition is about three times larger in the March CPS than in the MORG. 2) It can explain why all prior studies find no effect of the Mariel Boatlift on unemployment: There was no difference between black and nonblack unemployment rates among male non-Hispanic less-than-high-school workers in Miami ([Table 2](#)), so a change in racial composition would not change average unemployment in the sample. 3) It can explain why the wage effects estimated by [Borjas \(2017\)](#) persist into the period 1985–1988, by which time the supply shock of Cubans had subsided. The shift in racial composition of the sample continues—and in fact increases—through the years 1985–1988. 4) It can explain why [Borjas \(2017\)](#) finds larger effects in his preferred control cities than in [Card’s \(1990\)](#) preferred control cities: Coverage of blacks fell in the Borjas control cities, even hitting zero in 1983, but did not fall in the Card control cities. 5) It can explain why there is no apparent effect of the Mariel Boatlift on the wages of Hispanics or women: No such shift in the racial composition of the CPS sample occurred for those groups in Miami.<sup>20</sup>

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<sup>20</sup>[Peri and Yasenov](#) and [Borjas](#) find much smaller wage effects when women are included in the sample,



Another mechanism is possible in theory: Let mechanism  $D$  denote a change in the black coefficient  $\beta$  arising because the Mariel immigrants competed more with blacks than with non-blacks in this subpopulation. This is very unlikely to explain a substantial portion of the post-1980 wage drop because it does not fit the facts above, as detailed in [Appendix B](#).

## 2 Comparing the Mariel Boatlift to other refugee waves in Israel, France, and across Europe

Recent reanalysis has also challenged earlier results on the labor market impacts of three other large refugee waves—in France ([Hunt 1992](#)), Israel ([Friedberg 2001](#)), and across Europe ([Angrist and Kugler 2003](#))—alongside the Mariel Boatlift in a parallel instrumental-variables framework. For all four of these cases, [Borjas and Monras \(2017\)](#) seek to improve on causal identification in the original studies with an instrumental variable closely related to the instrument introduced by [Altonji and Card \(1991\)](#). They run a series of regressions of the form

$$\Delta \log w_{rs} = \theta_r + \theta_s + \eta m_{rs} + \varepsilon_{rs}, \quad (2)$$

where  $w_{rs}$  is the wage or other labor market outcome for native workers with skill  $s$  in region  $r$ ;  $\theta_r$  and  $\theta_s$  are region and skill fixed effects;  $L_{rst}$  is the native population with skill  $s$  in region  $r$  at time  $t$ ,  $m_{rs} \equiv \frac{M_{rs1}}{L_{rs1}}$  is the size of the refugee supply shock relative to the native population of skill  $s$  in region  $r$  at time 1; time 1 is after the refugee influx, time 0 is before it; the coefficient  $\eta$  is to be estimated and  $\varepsilon$  is an error term. In one of the reanalyses,  $r$  indexes occupations rather than geographic areas.<sup>21</sup> Because refugees’ choice

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[Card](#) finds no effect on men and women pooled, and [Peri and Yasenov](#) find no effects on women separately. There is no substantial increase in and after 1980 in the coverage of otherwise identical black *females* in either the March CPS or MORG in Miami relative to the control cities ([Figure 3](#)). Here, ‘otherwise identical’ means non-Hispanic, age 25–59, with less than a high school degree and working. Undercoverage in the census data had been three times greater for black men than for black women ([Levitan et al. 1979, 142](#)), thus the historical sources make it clear that efforts to improve coverage focused on black men.

<sup>21</sup>This specification varies between the reanalyses. In the France reanalysis, for example, location fixed effects  $\theta_r$  are omitted (see discussion in [subsection 2.2](#)). The reason given for omitting these fixed effects in the France reanalysis is that including them affects the results: it “makes the coefficients for the French repatriates supply shock very unstable” ([Borjas and Monras 2017, 44](#)) Also in the France reanalysis, the labor market outcome is employment rather than wage because wage is unavailable in the original data; but in the Israel reanalysis it is wage but not employment. In the Israel reanalysis the index  $r$  is across occupations rather than regions, due to Israel’s small geographic extent. Alternative forms of all regressions are run controlling as well for the term  $\eta \log \frac{L_{rs1}}{L_{rs0}}$ , motivated by theory, but all results are substantively

of geographic destination can be endogenous, the authors instrument for the refugee shock  $\frac{M_{rs1}}{L_{rs1}}$  with prior migration to that region  $\frac{M_{rs0}}{L_{rs0}}$ , resting on the idea that previous migrants attract new migrants to the same area (following [Altonji and Card 1991](#)).

A potential weakness of this instrumental variables approach lies in the fact that the native population of each region changes little over the short time periods in question, thus both the instrument and the endogenous variable have a common divisor ( $L_{rs1} \approx L_{rs0}$ ). This can generate spurious correlation between the ratios  $m_{rs1}$  and  $m_{rs0}$  regardless of the numerator, as first observed by [Pearson \(1896\)](#). In the colorful example of [Neyman \(1952, 143\)](#), one could conclude that storks bring babies by correlating storks-per-woman with babies-per-woman across any set of geographic areas. The variables would correlate well by construction, due to their common divisor.<sup>22</sup>

This problem, highlighted more recently by [Kronmal \(1993\)](#) for standard regression analysis, affects instrumental variables as well ([Bazzi and Clemens 2013](#)). One would find storks-per-woman to be a strong instrument for babies-per-woman even if storks are irrelevant to babies, and could use that framework to spuriously show that babies *cause* any regional outcome that is correlated with the number of women in the region.

The problem can be most simply revealed by taking an instrumental variable regression of this type with an economically meaningful variable in the numerator of the instrument, and replacing that numerator with storks—or any other irrelevant placebo. Robustness to such a change is a telltale indicator of a spurious result in the original instrumental variables regression, one form of what has been called the “blunt instruments” problem ([Bazzi and Clemens 2013](#)). Robustness to this placebo substitution does not invalidate the result, but demonstrates that the result requires further scrutiny to demonstrate that the original instrument contains identifying information beyond variance in the denominator (which may not be a valid instrument by itself). A recent and more general literature suggests that instrumental variable results in practical application are often spurious, with between a third and half of instrumental variable results published in leading journals falsely rejecting the null due to their treatment of standard errors ([Young 2017](#)).

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unaffected.

<sup>22</sup>See also *inter alia* [Pendleton et al. \(1979, 1983\)](#); [Jackson and Somers \(1991\)](#); [Wiseman \(2009\)](#).

[Kronmal \(1993\)](#) proposes a specification correction for this problem in an Ordinary Least Squares setting that we here adapt to the instrumental variables setting. The robustness test he proposes is to simply split the ratio variable into two separate variables, while accounting for the nonlinear relationship between numerator and denominator with the log transformation. In the stork example, a regression of  $\log(\text{babies})$  on both  $\log(\text{storks})$  and  $\log(\text{women})$  will give the correct positive coefficient on women and the correct null coefficient on storks.

We modify [Kronmal's](#) method in one way: Because here the refugee shock variable frequently takes value zero, the log transformation would truncate those observations, so we instead use the inverse hyperbolic sine transformation.<sup>23</sup>

We therefore modify the regression (2) with the [Kronmal](#) correction to

$$\Delta \log w_{rs} = \theta_r + \theta_s + \eta(\text{asinh}M_{rs1}) + \eta'(\text{asinh}L_{rs1}) + \varepsilon_{rs}, \quad (3)$$

where *asinh* is the inverse hyperbolic sine and where the endogenous refugee supply shock ( $\text{asinh}M_{rs1}$ ) is instrumented by the predetermined stock of prior migrants ( $\text{asinh}M_{rs0}$ ).<sup>24</sup>

## 2.1 Israel reanalysis

[Friedberg \(2001\)](#) studies a large and sudden influx of Soviet refugees to Israel between 1990 and 1994, large enough to raise Israel's population by 12 percent. She uses information on migrants' former occupations in their home countries to construct an instrument for the occupations they take in Israel, and finds "no adverse impact of immigration on native outcomes" within occupations. [Borjas and Monras \(2017\)](#) reanalyze the episode using instead the [Altonji and Card](#) instrument based on prior migration flows into education-occupation cells inside Israel, and instead find large detrimental effects of the migration

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<sup>23</sup>Regression coefficients on variables transformed with the inverse hyperbolic sine can be interpreted identically to those using the traditional log transformation (as approximating percent changes) since  $\frac{d}{dx} \text{asinh}x = \frac{1}{\sqrt{1+x^2}} \approx 1/x = \frac{d}{dx} \ln x, \forall x \gtrsim 2$ . But unlike the log transformation, the inverse hyperbolic sine has desirable properties near zero and is defined at zero ( $\text{asinh}0 = 0$ ). See [Burbidge et al. \(1988\)](#); [MacKinnon and Magee \(1990\)](#).

<sup>24</sup>Note again that in this Israel case only, subscript *r* indexes occupations rather than regions.

on Israel natives' wages.

Table 3 carries out the placebo test described above on the Borjas and Monras application of the prior-flows instrument to the Israel refugee wave. First, we construct a placebo instrument that contains no information about prior flows of migrants into the education-by-occupation cells in the reanalysis. We take the pre-influx Soviet immigrant stock across occupations, by skill group—and generate Poisson-distributed white noise with the same mean as the real numerator  $M_{rs0}$ . The means of these placebo numerators  $\tilde{M}_{rs0}$  for each skill group are shown in Table 3a. We then construct a placebo instrument by dividing that white-noise numerator  $\tilde{M}_{rs0}$  by the same divisor as the true instrument ( $L_{rs0}$ ). The resulting placebo instrument  $\tilde{m}_{rs}$  contains no information about prior migration flows.

Second, we replicate the Borjas and Monras (2017, Table 1, cols.1, 3) reanalysis of the Israel case. In Table 3b, columns 1 and 3, we exactly replicate Borjas and Monras's (2017) findings with their original instrument. Third, we replace the Borjas and Monras instrument with our placebo instrument. Columns 2 and 4 use the placebo instrument, without changing anything else in the analysis. The placebo instrument achieves a result that is similar—and actually increases in magnitude and statistical significance. In both columns the estimated detrimental effect of Soviet migrants on native wages is a bit larger in absolute value than in the original study; in column 2 the result is more statistically significant than in the original.<sup>25</sup>

Identification of the effect of the refugee shock ostensibly rests on the distribution of prior migration across occupations within skill cells. But when all information about prior migration is purged from the instrument, the result stands, and in fact grows stronger. This suggests that the original result could be spurious, driven by irrelevant relationships between wage trends for natives in different skill cells and the population size of those cells (the denominator of the instrument).

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<sup>25</sup>Instrumentation is not as strong using the placebo instrument. But the degree of potential bias from weak instrumentation is around 25% of the coefficient estimate (Stock et al. 2005)—in the absence of which the placebo coefficient would match the original coefficient even more closely. Furthermore the Anderson-Rubin (1949)  $F$ -test for the significance of the refugee shock in the second stage, a test that is robust to weak instrumentation, rejects the hypothesis that the coefficient is zero at the 5% level in column 2.

We apply the [Kronmal](#) specification correction to the instrumental variables regressions, splitting the refugee shock numerator and the population size into two separate variables in the second stage. Then the absolute magnitude of the refugee shock (in number of people) can be strongly and validly instrumented with the absolute magnitude of lagged migration to the region or occupation in question.

[Table 4](#) shows that the second-stage coefficient on the émigré supply shock is statistically indistinguishable from zero under the [Kronmal](#) correction. The first column of the table precisely replicates the original result in [Borjas and Monras \(2017, Table 6\)](#). The second column shows that the result is nearly identical when the ratio measure of the supply shock undergoes the inverse hyperbolic sine transformation. The third column shows regression (3), in which the current migration shock is instrumented with true lagged migration into the cell. The coefficient on the refugee supply shock becomes indistinguishable from zero, and its magnitude—adjusted to be comparable to column 1 (in square brackets)—falls in absolute value from  $-0.616$  to  $-0.284$ .<sup>26</sup> Instrumentation remains very strong in column 3, with a [Kleibergen-Paap \(2006\)](#)  $F$  statistic over 14, but the weak-instrument robust [Anderson-Rubin \(1949\)](#)  $F$ -test fails by a wide margin to reject the hypothesis that the second-stage coefficient on the refugee shock is zero.<sup>27</sup>

The last column of [Table 4](#) offers an explanation for this pattern, by simply regressing the absolute magnitude of the refugee supply shock ( $\text{asinh}M_{rs1}$ ) on the absolute magnitude of the population in each cell ( $\text{asinh}L_{rs1}$ ). The coefficient of 1.15 is indistinguishable from unity, and 72% of the variance in the size of the refugee supply shock is explained simply by the size of the native population in each education-occupation cell. That is, most of the information contained in the size of the refugee supply shock is contained in the size of the native population in each cell; the denominator of the original instrument contains almost all information about prior flows. Thus any coincidental relationship between wage trends in some occupation-skill cells and the absolute size of that cell could produce a

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<sup>26</sup>This is done, here and in the tables to follow, by multiplying the Kronmal coefficient by  $\frac{1-p}{p}$ , where  $p$  is the immigrant share: here  $-0.0348 \cdot \frac{1-0.109}{0.109} = -0.284$ . See [Appendix C](#) for proof. The results are substantively unchanged when controlling for  $\log \frac{L_{rs1}}{L_{rs0}}$ , as [Borjas and Monras \(2017\)](#) do in some specifications.

<sup>27</sup>This is the proper test for the present just-identified setting of one endogenous variable and a single instrument.

second-stage coefficient that rejects the null of zero. When this possibility is eliminated by the [Kronmal](#) correction, in column 3 of [Table 4](#), the second stage coefficient cannot be distinguished from zero.

Of course the original instrument used by [Friedberg \(2001\)](#), also constructed from a ratio, could be vulnerable to the same problem. Thus in [Tables 5](#) and [6](#) we repeat the placebo test and [Kronmal](#) correction for [Friedberg's](#) original result. [Friedberg's](#) instrument is not the lagged ratio in Israel of Soviet émigrés in each skill-occupation cell per Israeli in that cell used by [Borjas and Monras](#), but rather the number of émigrés in each skill-occupation cell *prior to migration*, in their home countries, per lagged Israeli in that cell in Israel. [Table 5a](#) shows the means, by education group, of the Poisson-distributed white noise generated to replace the numerator of the instrument.

In [Table 5b](#), column 1 we then exactly replicate [Friedberg's](#) core instrumental variable result, a positive impact on native wages that is statistically significant at the 5% level. Column 2 shows that [Friedberg's](#) result, unlike [Borjas and Monras's](#), cannot be reproduced with the placebo instrument. The second-stage coefficient now fails to reject a wide range of negative and positive effects on wages. The weak-instrument robust [Anderson-Rubin](#)  $F$ -test fails by a wide margin to reject the hypothesis that the second-stage coefficient is zero. This implies that the original instrumental variable used by [Friedberg](#) contained identifying information in the numerator, as intended.

Thus we do not expect the [Kronmal](#) specification correction to greatly alter [Friedberg's](#) core result, and this is indeed what we observe in [Table 6](#). There, as in [Table 4](#), the first column replicates the original result and the second shows that it is unchanged under the inverse hyperbolic sine transformation of the immigration shock ratio. The third column splits that ratio into its numerator and denominator (each in inverse hyperbolic sine transformation,  $asinh$ ), instrumenting for the numerator only ( $asinh$  of the 1994 Soviet émigré stock in Israel in each cell) with the  $asinh$  of the pre-migration size of that cell given the migrants' previous occupations in their home countries (the numerator of the [Friedberg](#) instrument). The second-stage coefficient remains positive but is no longer statistically significant—though the weak-instrument robust [Anderson-Rubin](#)  $F$ -test does reject at the

11% level the hypothesis that the second-stage coefficient on the endogenous migrant shock is zero. The magnitude of the coefficient estimate in column 3, adjusted to be comparable with column 1 (in square brackets), is 0.572.<sup>28</sup>

This departs slightly from [Friedberg](#)'s original finding that any zero or negative wage impact could be rejected at the 5% level. But it does remain in accordance with [Friedberg](#)'s (2001, 1403) interpretation of that finding: “we cannot reject the hypothesis that the mass migration of Russians to Israel did not affect the earnings or employment of native Israelis.”

## 2.2 France reanalysis

[Hunt](#) (1992) studies the effects on French wages and native unemployment of the arrival from Algeria in 1962 of 900,000 people of European (and Jewish) origin (e.g. [Festy 1970](#); [Guillon 1974](#)). Algeria's independence in that year led almost the entire population of European origin to flee to France, along with about 140,000 Muslims of Arab and Berber origin ([Roux 1991](#), 230), mostly illiterate “Harkis” who faced reprisals for having fought for France in the war of independence. Using variation by department (province) and time, she finds that the arrival of the repatriates raised French native unemployment by “at most 0.3 percentage points.” This result stems from the fact that the repatriates represented 1.6% of the 1968 labor force and that [Hunt](#) finds a one percentage point increase in repatriates in the labor force increased the unemployment rate by 0.19 percentage point; to a very close approximation 0.19 is also the effect of an increase in the labor force of one percent due to the repatriates (see [Appendix C](#)). [Hunt](#) does not study the effect of the Harkis.

Using variation across region (each containing several departments), education and time, along with the prior-flows instrumental variable strategy, [Borjas and Monras](#) (2017) reanalyze the impact of the repatriates and attempt to analyze for the first time the effect of the Harkis. While [Hunt](#) had access to data only at the department level, [Borjas and Monras](#) (2017, 5) use individual-level data. They find that a one percent increase in population due to repatriates raised the unemployment rate of similarly educated male natives by a statistically significant 0.09 percentage point ([Borjas and Monras](#) Table 10, cols. 3 and 4).

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<sup>28</sup>This is calculated, as above, by:  $0.0780 \cdot \frac{1-0.12}{0.12} = 0.572$ .

Although the effect estimated is conceptually slightly different (see [Appendix C](#)), it seems reasonable to consider this result to be similar to [Hunt](#)'s, given standard errors, suggesting [Borjas and Monras](#)'s exclusion of native women may be innocuous.<sup>29</sup>

[Borjas and Monras](#) also find that a one percent increase in the population due to the arrival of Algerian nationals raised the unemployment rate of similarly educated male natives by a statistically significant 0.25 percentage point ([Borjas and Monras](#) Table 10 columns 3 and 4). This is almost three times the effect they find for the imputed repatriates, though similar in magnitude to the [Hunt](#) estimate of the impact of the repatriates (0.19), given standard errors. This similarity is obscured in the discussion in [Borjas and Monras](#), as the magnitude the authors emphasize is the product of the 0.25 coefficient and the share of Algerians in the lowest education group in the most affected cities, a much larger number. Probably only half the authors' sample of 160,000 Algerians are actually Harkis, since they count 84,000 Harkis as repatriates, with the remainder being economic migrants from Algeria.<sup>30</sup>

The estimated effects thus do not differ statistically significantly between [Hunt \(1992\)](#) and [Borjas and Monras](#), nor are the differences in the point estimates very large. Nevertheless, we scrutinize the use of lagged migration as an instrument in both papers, beginning with [Borjas and Monras](#). We first repeat the reanalysis of [subsection 2.1](#), above, focusing on the Algerian nationals. In [Table 7a](#) we show the means of Poisson-distributed white noise  $\tilde{M}_{rs0}$  with the same mean as the prior stocks of Algerian migrants  $M_{rs0}$  across French regions within skill cells. [Table 7b](#) shows that the detrimental effects estimated for natives are larger using the placebo instrument  $\tilde{m}_{rs0}$ , as in the Israel case. The first column exactly replicates the core result in [Borjas and Monras](#) Table 10: IV coefficients of 0.09 for the repatriates and 0.25 for the Algerians. In the second column, we replace the true Algerian prior stock instrument with the placebo: the coefficient estimate rises to 0.42 and retains high statistical significance.<sup>31</sup>

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<sup>29</sup>This similarity may be coincidental, however, since the data used by [Borjas and Monras](#) identify neither the repatriates nor the Harkis. Repatriates are imputed from the data rather than indicated in the data. The authors' imputation identifies 1.4 million repatriates, more than 50% too many, with their sample including more than half the Harkis—the 84,000 who had acquired French nationality by 1968 ([Roux 1991](#), 226)—and non-repatriate French nationals returning from residence abroad.

<sup>30</sup>Economic migrants are described by e.g. [Roux \(1991, 255\)](#).

<sup>31</sup>The standard error on the placebo instrument of column 2 is much higher than in the [Borjas and](#)



In column 3, we drop the French-citizen repatriate shock from the original regression in column 1, showing that the coefficient estimate on the Algerian national shock is essentially unchanged at 0.28. In column 4, we then replace the true Algerian prior migrant stock instrument with the placebo instrument. Again the coefficient estimate retains high statistical significance, rising in magnitude to 0.44, with instrumentation stronger than in column 2 (the [Kleibergen-Paap](#)  $F$ -statistic rises to 5.3). Column 5 shows that this result is unaltered by controlling for the interperiod change in the native population (following [Borjas and Monras](#)). As in the case of Russian migration to Israel, therefore, the strategy for identifying the causal relationship between the refugee inflow and native labor market outcomes is potentially flawed.

In [Table 8](#), we carry out the Kronmal specification correction using equation (3) just as was done for Israel above. The first column precisely replicates the core result using the original analytic methods for Algerian nationals in isolation ([Table 7b](#), column 3). Column 2 shows that this finding is identical when the ratio measure of the migrant shock (and its instrument) undergo the inverse hyperbolic sine transformation. Column 3 shows that under the Kronmal specification correction, with strong instrumentation (a [Kleibergen-Paap](#)  $F$ -statistic of 42), the coefficient on the Algerian shock is still positive and statistically significant, with a magnitude of 0.0023. Column 4 then controls for the concurrent repatriate shock, instrumented by the lagged repatriate stock: this is the [Kronmal](#)-corrected equivalent of [Table 7b](#), column 1. The coefficient on the Algerian shock is positive and statistically significant but falls to 0.0018; this implies that a one percent increase in the population due to the Algerians raised unemployment by about 0.23 percentage point.<sup>32</sup> In square brackets we convert this to a value comparable to the [Borjas and Monras](#) coefficient: if Algerians increase the population by one percent, unemployment rises 0.23 percentage point.<sup>33</sup> The adjusted coefficient for repatriates is also similar to the [Borjas and Monras](#) specification estimate of 0.04.

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Monras original, but the weak instrument-robust [Anderson-Rubin](#)  $F$ -test strongly rejects the hypothesis that the second-stage coefficient is zero.

<sup>32</sup>As above, this is calculated as  $0.00182 \cdot \frac{1-0.008}{0.008} = 0.226$ .

<sup>33</sup>In the last column, we regress the absolute magnitude of the Algerian supply shock ( $\text{asinh}M_{rs1}$ ) on the absolute magnitude of the population in each cell ( $\text{asinh}L_{rs1}$ ). 81% of the variance in the refugee shock is explained by the size of the cell.

There is thus general agreement between all of the findings considered: the original results of [Hunt](#), the reanalysis of [Borjas and Monras](#), and the results with the specification correction. For the Algerian-national shock, not studied by [Hunt](#), both [Borjas and Monras](#) and [Table 8](#) here imply that if Algerians increase the population by one percent, unemployment rises by 0.23–0.24 percentage point. For the French-national repatriate shock, [Hunt](#)'s findings cannot be statistically distinguished from [Borjas and Monras](#)'s finding that a one percent increase in population due to repatriates raised native unemployment by 0.09 percentage point, though this result is not robust to the inclusion of the same regional fixed effects used in the other reanalyses, and the result is not statistically significant in the [Kronmal](#)-corrected specification used here.

[Hunt](#) also uses a potentially problematic instrument: the share of early (1954–1962) repatriates as a share of the population, used to instrument 1962–1968 repatriates as a share of the labor force. Like the original instrument used in [Friedberg](#)'s study of Israel, this ratio instrument could be subject to the same problems considered above. However, in addition to this instrument, [Hunt](#) uses the department average temperature: repatriates tended to settle in southern France where the climate was more similar to that of Algeria. We reproduce [Hunt](#)'s main unemployment coefficient of 0.195 ([Hunt](#) Table 3 column 4) in our [Table 9](#) column 1.<sup>34</sup> An obvious robustness check is simply to drop the lagged migration instrument and instrument with temperature alone: this is shown in our column 2. The coefficient drops to 0.120 with a slightly larger standard error, which renders the coefficient statistically insignificant. Nevertheless, we can rule out that a percentage point increase in repatriate's share in the population increases unemployment by more than 0.31 percentage point. To render the coefficient comparable to the [Borjas and Monras](#) coefficient an adjustment is necessary (multiplying the coefficient by one minus the share of repatriates), an almost identical value shown in square brackets in column 2.

Instead of dropping the lagged migration instrument, we can instead drop the temperature instrument so as to pursue comparisons with [Borjas and Monras](#). Column 3 shows that the coefficient of 0.209 is similar to [Hunt](#)'s original coefficient of 0.195. In column 4, we take the inverse hyperbolic sine of both the repatriate share and its instrument, and

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<sup>34</sup>This uses robust standard errors, which [Hunt](#) did not.

obtain the same result as in column 3. In column 4, we control separately for the inverse hyperbolic sine of the 1968 number of repatriates and the 1968 labor force, instrumenting the former with the inverse hyperbolic sine of the 1962 number of repatriates. The coefficient of interest is a statistically significant 0.00254. Thus, as shown in square brackets, if repatriates increase the size of the labor force by one percent, the unemployment rate increases by 0.16 percentage point. This [Kronmal](#)-corrected specification using the lagged migration instrument thus yields a point estimate very similar to the point estimate using temperature as an instrument in column 2, a slightly smaller effect than found by [Hunt](#) or by [Borjas and Monras](#). The estimates are all statistically similar, however, given standard errors.<sup>35</sup>

### 2.3 Europe reanalysis

[Angrist and Kugler \(2003\)](#) study the effects of an influx of refugees from the Balkan War on 18 European countries during the 1990s. They find that a sudden increase in the migrant stock of one percentage point raises native unemployment by 0.83 percentage point. [Borjas and Monras \(2017\)](#) reanalyze the episode in seven of those European countries (Austria, Greece, Ireland, Portugal, Romania, Spain, and Switzerland), again using the prior-migration instrument. They likewise find a detrimental effect on native unemployment, though much smaller in magnitude: an increase in the migrant stock of one percentage point raises native unemployment by 0.49 percentage points. This is larger than the effect found for France.

In this case there is little disagreement between the original study by [Angrist and Kugler \(2003\)](#) and the reanalysis by [Borjas and Monras \(2017\)](#). Both find that the refugee wave they study substantially displaced natives in the labor market, though estimates in both are statistically imprecise. [Angrist and Kugler \(2003, F328\)](#) warn of identification problems and statistical imprecision in their instrumental variables estimates and recommend interpreting those estimates as an upper bound on the true effect. The instrumental variable estimates of [Borjas and Monras \(2017\)](#) for unemployment are statistically insignificant

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<sup>35</sup>In column 6, we regress the transformed number of 1962–1968 repatriates ( $\text{asinh}M_{rs1}$ ) on the transformed number in the labor force ( $\text{asinh}L_{rs1}$ ). 80% of the variance in the refugee shock is explained by the size of the department labor force.

even at the 10% level.

Nevertheless, we proceed with the same placebo test as above to test the robustness of these findings. [Table 10a](#) shows the means of the placebo numerator  $\tilde{M}_{rs0}$  by country. [Table 10b](#) replicates the core result in [Borjas and Monras \(2017, Table 13\)](#), and then reproduces it using the placebo instrument  $\tilde{m}_{rs0}$ . As in the Israel and France cases above, the result *strengthens* when the placebo is used: the coefficient estimate rises somewhat, and it is statistically significant at the 10% level in column 2 (unlike the estimate in the original).

[Table 11](#) carries out the [Kronmal](#) correction as above. Column 1 replicates the original result, column 2 shows that it is identical under the inverse hyperbolic sine transformation, and column 3 carries out the [Kronmal](#) correction instrumenting with the lagged migrant stock. The coefficient estimate is negative and statistically insignificant, with a magnitude (adjusted in square brackets, as above, to be comparable to the column 1 coefficient) of  $-0.26$ .<sup>36</sup> Instrumentation is quite weak, with a [Kleibergen-Paap](#)  $F$  statistic of just 1.5. However the weak instrument-robust [Anderson-Rubin \(1949\)](#)  $F$ -test strongly rejects the hypothesis that the second-stage coefficient is zero—suggesting that some information is indeed contained in the negative second-stage coefficient, and failing to show that the Balkan supply shock raised unemployment in refugees’ destination regions.

This result is discordant with [Angrist and Kugler](#), but only to a limited degree. Their OLS regressions find a “small” but statistically significant detrimental effect of Balkan refugees on native unemployment, but their instrumental variables estimates are mostly statistically insignificant and exhibit a fragility that, the authors find, “suggests these estimates are probably driven by forces other than increased immigration” ([Angrist and Kugler 2003, F302, F322](#)). The result here is also only mildly discordant with the effect estimated by [Borjas and Monras \(2017\)](#), which is detrimental but statistically insignificant.

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<sup>36</sup>Calculated, as above by:  $-0.0132 \cdot \frac{1-0.05}{0.05} = -0.26$ .

## 2.4 The Mariel Boatlift again

[Borjas and Monras \(2017\)](#) also revisit the impact of the Mariel Boatlift—differently from [Borjas \(2017\)](#)—adding variation across education groups and using the same instrumental variables regression specification across area-skill cells in equation (2). They concur with [Card \(1990\)](#) that the Mariel Boatlift had no detrimental impact on native employment. But like [Borjas \(2017\)](#), they find large negative wage impacts.

The above reanalysis of the Mariel Boatlift episode, however, likewise implies that the [Borjas and Monras \(2017\)](#) results on the Mariel Boatlift are explained by irrelevant compositional shifts in the underlying data. This is because the entire result in [Borjas and Monras](#)’s city-skill cell regressions depends on a single cell: workers with less-than-high-school in Miami. [Table 12](#) demonstrates this: Column 1 exactly replicates the core Mariel Boatlift result in [Borjas and Monras](#). Column 2 shows that the negative effect on native wages vanishes if a single data point is dropped: less-than-high-school in Miami. The same is true without any skill cell in Miami (col. 3) or without the less-than-high-school skill group in any city (col. 4).

Because the [Borjas and Monras](#) result for the Mariel Boatlift depends entirely on the decline in wages observed for the less-than-high-school subpopulation in Miami—which is identical to the subpopulation in [Borjas \(2017\)](#)—this means that the core problem of compositional changes discussed in [section 1](#) equally explains the different estimates of wage impacts in [Borjas and Monras \(2017\)](#) relative to [Card \(1990\)](#). And for employment impacts there is no discrepancy to explain: both studies find no impact on employment.

[Table 12](#) furthermore shows that the effect in Miami estimated by [Borjas and Monras](#) is absent among Hispanics. Column 5 reproduces their original result when city-skill cells with no Hispanic observations are omitted from the regression, and column 6 shows that—in the same sample—the result is absent when the wage changes within city-skill cells are estimated for Hispanic workers only. Instrumentation remains very strong (Kleibergen-Paap  $F$  statistic of 1965) but the second-stage coefficient on the supply shock is statistically insignificant, and the [Anderson-Rubin](#)  $F$ -test has a  $p$ -value of 0.76.

### 3 Discussion

Reanalysis of prior results often advances social science (Clemens 2017). Recent reanalyses of four early results on the labor market effects of refugee waves have reached conclusions markedly different from the original studies in two cases (Miami and Israel) and similar to the original studies in the other two cases (France and Europe). The origin of the discrepancies has not previously been clarified in a way that can assist researchers in reconciling these findings.

We offer simple and transparent methodological reasons for the discrepancies in this refugee-wave literature. For the Mariel Boatlift, all important discrepancies between the original analysis and reanalyses can be explained by a large, simultaneous, and irrelevant change in the racial composition in subgroups of the original survey data. For the arrival of Soviet refugees in Israel, all important discrepancies between the original analysis and reanalysis can be explained by specification choices in the construction of the instrumental variable. For the refugee waves from Algeria to France and from the Balkans throughout Europe, there is little substantive discrepancy between the original studies and reanalyses. But even the limited discrepancies are reduced by the same specification correction to the construction of the instrumental variable, as shown in [Appendix Table 1](#).

After accounting for the potential for spurious results from compositional change within subgrouped data and from the ratio-correlation problem for constructed instruments, the evidence from refugee waves collectively supports the existing consensus that the impact of immigration on average native-born workers is small. It does not support claims of large detrimental impacts on workers with less than high school education.

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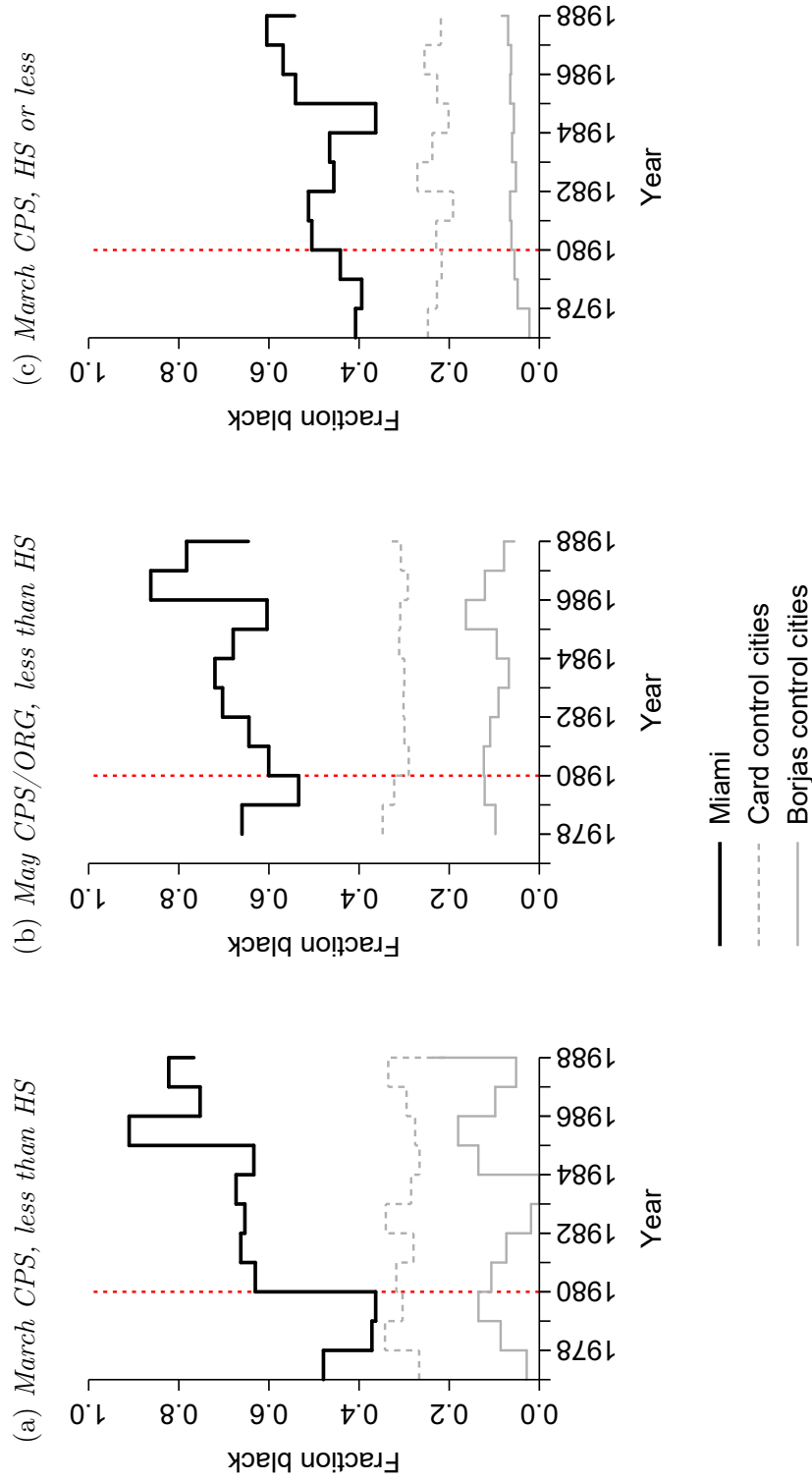
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**Table 1:** BLACKS IN THE MIAMI CPS SUBSAMPLE

Year	March CPS				May CPS/ORG			
	Black		Black fraction		Black		Black fraction	
	Yes	No	Weighted	Unweighted	Yes	No	Weighted	Unweighted
1977	11	12	0.479	0.478	—	—	—	—
1978	10	16	0.372	0.385	10	6	0.660	0.625
1979	8	14	0.363	0.364	6	6	0.534	0.500
1980	9	8	0.630	0.529	34	22	0.600	0.607
1981	11	7	0.663	0.611	35	20	0.644	0.636
1982	11	9	0.653	0.550	34	17	0.703	0.667
1983	17	10	0.673	0.630	27	12	0.720	0.692
1984	11	7	0.634	0.611	33	17	0.679	0.660
1985	14	2	0.910	0.875	28	20	0.604	0.583
1986	11	4	0.753	0.733	22	4	0.862	0.846
1987	13	3	0.822	0.813	27	9	0.783	0.750
1988	13	5	0.767	0.722	28	18	0.646	0.609

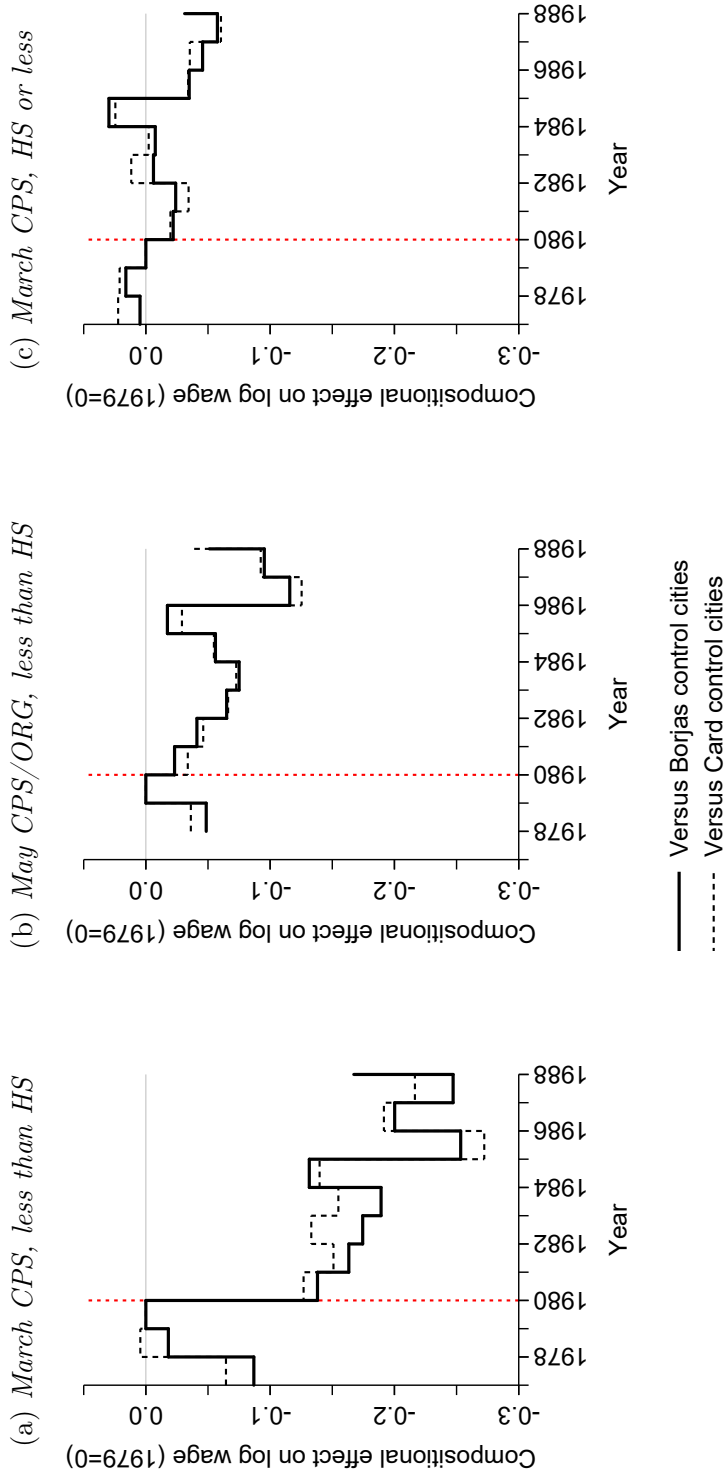
Gray rows show pre-treatment period. These subsamples of non-Hispanic male workers age 25–59 with less than high school are identical to those in [Borjas \(2017, Table 3A\)](#). Following [Borjas](#), the years in the graph refer to the year in which earnings were earned, not the year of the survey (e.g. 1980 data for March CPS are from the 1981 March CPS). March CPS weighted estimates use the Supplement weight, May/ORG weighted estimates use the earnings weight.

**Figure 1: FRACTION BLACK IN POPULATION REPRESENTED BY SAMPLE**



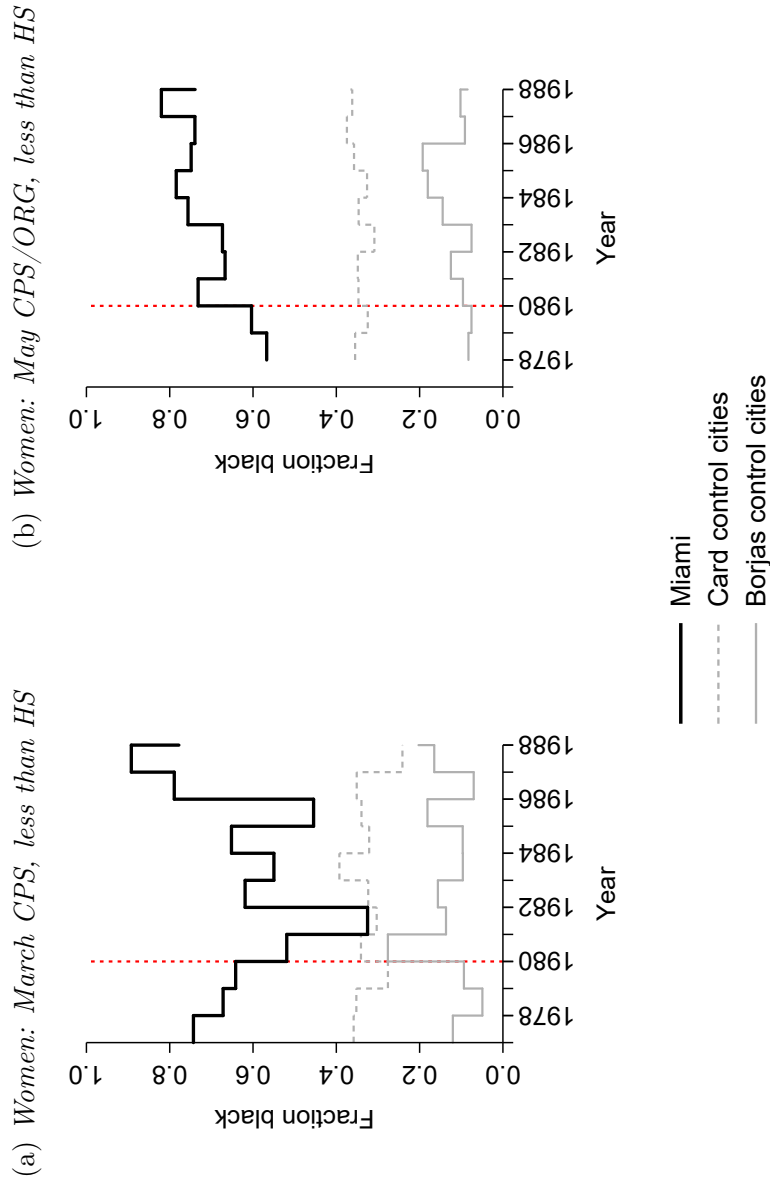
'HS' is high school. The Miami samples in (a) and (b) are identical to those in Borjas (2017, Table 3A). This figure includes only male non-Hispanic workers, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly (and, in ORG, the additional condition of positive usual weekly earnings). Fraction black in (a) and (c) weighted by March Supplement weight, in (b) weighted by ORG earnings weight. Following Borjas, the years in the graph refer to the year in which earnings were earned, not the year of the survey (e.g. 1980 data for March CPS are from the 1981 March CPS).

**Figure 2:** SPURIOUS WAGE EFFECT DUE TO COMPOSITIONAL CHANGE



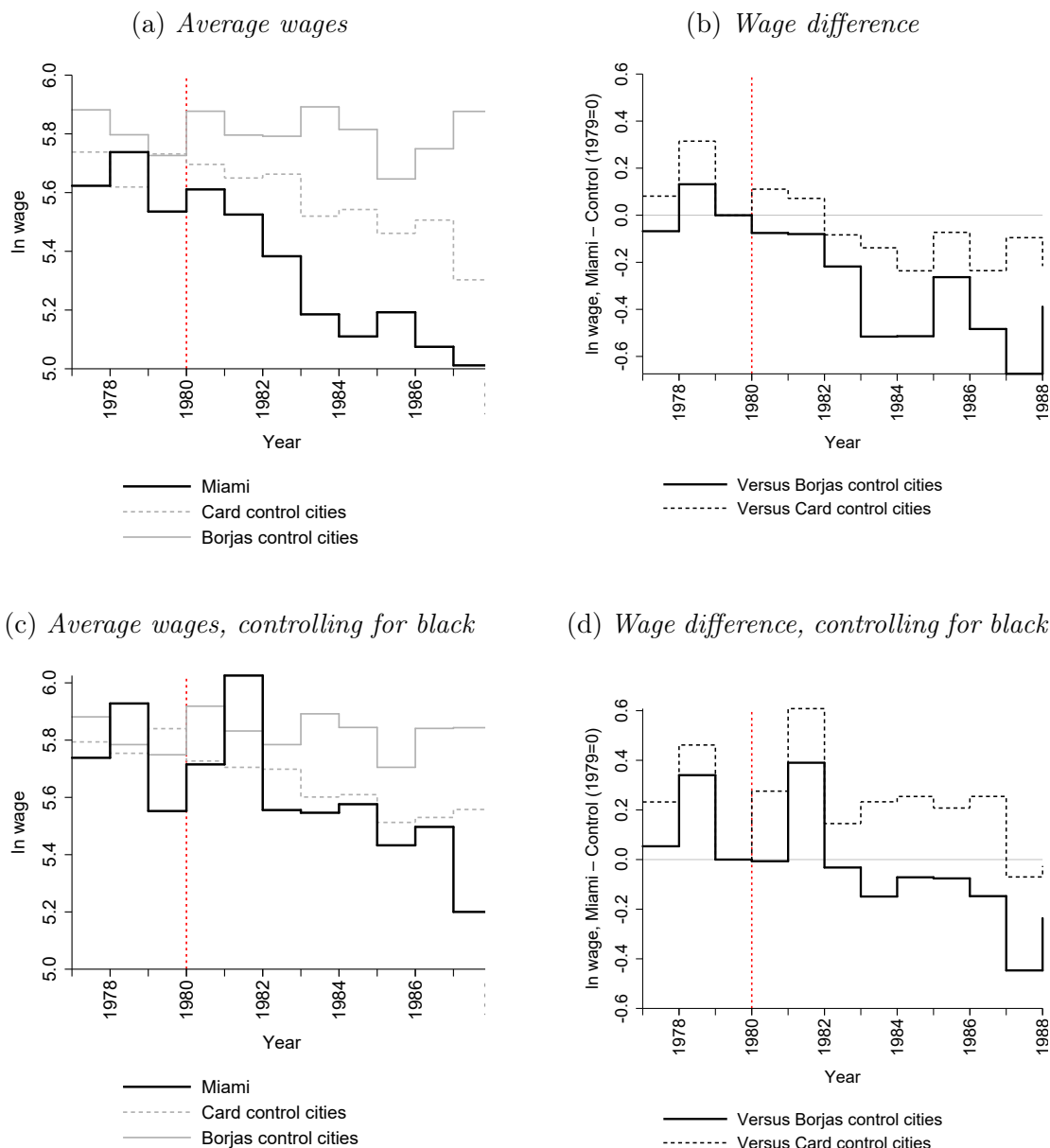
'HS' is high school. The Miami samples in (a) and (b) are identical to those in [Borjas \(2017, Table 3A\)](#). This figure includes only male non-Hispanic workers, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly (and, in ORG, the additional condition of positive usual weekly earnings). Following [Borjas](#), the years in the graph refer to the year in which earnings were earned, not the year of the survey (e.g. 1980 data for March CPS are from the 1981 March CPS).

**Figure 3: WOMEN ONLY: FRACTION BLACK IN POPULATION REPRESENTED BY SAMPLE**



'HS' is high school. The Miami samples of women only in (a) and (b) are identical to the women in [Borjas \(2017, Table 3A\)](#). This figure includes only female non-Hispanic workers, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly (and, in ORG, the additional condition of positive usual weekly earnings). Fraction black in (a) weighted by March Supplement weight, in (b) weighted by ORG earnings weight. Following [Borjas](#), the years in the graph refer to the year in which earnings were earned, not the year of the survey (e.g. 1980 data for March CPS are from the 1981 March CPS).

**Figure 4:** EFFECT OF ADDING A CONTROL FOR RACE OF NATIVES IN SUBSAMPLE



All results use March CPS sample identical to [Borjas \(2017, Table 3A\)](#): male non-Hispanic workers, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly. Panel (a) shows simple average log wage. Panel (b) shows difference in log wage between Miami and controls, 1979 normalized to 0. Panel (c) shows coefficient on the constant term of a regression, separately for each year and city group, of log wage on an indicator variable that is 1 for black and 0 otherwise, weighted by March Supplement weight. That is, it shows the average log wage controlling black-nonblack wage differences in each city-year separately. Panel (d) shows log wage differences implied by panel (c). Following [Borjas](#), the years in the graph refer to the year in which earnings were earned, not the year of the survey (e.g. 1980 data for March CPS are from the 1981 March CPS).

**Table 2:** WAGE AND EMPLOYMENT DIFFERENCES BY RACE, MARCH CPS 1977–1986

	(1)	(2)	(3)	(4)	(5)	(6)
	Miami		Card control cities		Borjas control cities	
	Weighted	Unweighted	Weighted	Unweighted	Weighted	Unweighted
<i>Dep. var: log wage</i>						
Black	-0.487*** (0.0737)	-0.465*** (0.0728)	-0.219*** (0.0315)	-0.215*** (0.0321)	-0.285*** (0.0760)	-0.261*** (0.0770)
<i>Dep. var: Unemployed (0,1)</i>						
Black	-0.00480 (0.0320)	-0.00345 (0.0302)	0.0688*** (0.0158)	0.0513*** (0.0155)	0.0528*** (0.0122)	0.0515*** (0.0124)

Regressor is an indicator variable equal to 1 for black, 0 otherwise. Coefficients are from a pooled OLS regression of each outcome on the black dummy and a constant term, and nothing else. Standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The weighted regressions use the March Supplement weight. *Wage regressions*: The sample is identical to the March CPS sample in [Borjas \(2017, Table 3A\)](#), and includes only male non-Hispanic workers with less-than-high-school education, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly. *Unemployment regressions*: The sample is identical to the March CPS sample in [Borjas and Monras \(2017\)](#), and includes only male non-Hispanic workers with less-than-high-school education, aged 25–59. The unemployment regressions cannot be run using data from [Borjas \(2017\)](#) because that paper considers only employed workers, and tests for wage effects conditional on employment.

**Table 3:** ISRAEL: PLACEBO REGRESSIONS, BORJAS AND MONRAS (2017) MODEL(a) *Israel: Mean 1983 Soviet stock by education, real vs. placebo*

<i>Education group</i>	Real	Placebo
Less than primary completed	478.8	486.1
Primary completed	742.5	743.4
Secondary completed	1735.0	1736.1
University completed	1116.3	1116.3

The placebo is a randomly-generated variable drawn from a Poisson distribution with the same mean as the real variable.

(b) *Israel: Placebo reanalysis of Borjas and Monras Table 6, cols. 3 and 4*

<i>Dep. var.: <math>\Delta</math> native wage</i>	(1)	(2)	(3)	(4)
<i>Lagged Soviet fraction IV:</i>	Real	Placebo	Real	Placebo
Émigré supply shock/pop.	-0.616* (0.316)	-0.820*** (0.315)	-0.611* (0.334)	-0.873* (0.473)
Change in native population			-0.00352 (0.0707)	0.0229 (0.0976)
<i>N</i>	32	32	32	32
adj. <i>R</i> <sup>2</sup>	0.286	0.289	0.258	0.257
Kleibergen-Paap <i>F</i>	27.37	5.059	23.19	3.728
<i>p</i> -val. Anderson-Rubin <i>F</i> -test	0.0985	0.0272	0.113	0.0880

Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument in each column is: (1) real 1983 Soviet fraction, (2) placebo 1983 Soviet fraction, (3) real 1983 Soviet fraction, (4) placebo 1983 Soviet fraction. All specifications include education and occupation fixed effects.



**Table 4:** ISRAEL: KRONMAL SPECIFICATION CORRECTION TO [BORJAS AND MONRAS](#)

	(1)	(2)	(3)	(4)
<i>Dependent variable:</i>		$\Delta$ native wage		<i>asinh</i> émigré supply shock
<i>Estimator:</i>		2SLS		OLS
Émigré supply shock/pop.	-0.616* (0.316)			
<i>asinh</i> émigré supply shock/pop.		-0.642** (0.325)		
<i>asinh</i> émigré supply shock			-0.0348 (0.0443) [-0.284]	
<i>asinh</i> total pop.			0.0426 (0.0443)	1.154*** (0.196)
<i>N</i>	32	32	32	32
adj. $R^2$	0.286	0.297	0.156	0.717
Kleibergen-Paap $F$	27.37	31.27	14.41	—
$p$ -val. Anderson-Rubin $F$ -test	0.0985	0.0995	0.548	—

*asinh* is inverse hyperbolic sine. Robust standard errors in parentheses. Square brackets show column 3 coefficient adjusted to be comparable to column 1 coefficient (dividing by immigrant fraction of population). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument in each column is: (1) 1983 Soviet fraction, (2) *asinh* of 1983 Soviet fraction; (3) *asinh* of 1983 Soviet stock; (4) none. All specifications include education and occupation fixed effects.

**Table 5:** ISRAEL: PLACEBO REGRESSIONS, [FRIEDBERG \(2001\)](#) MODEL(a) *Israel: Mean pre-migration stock per occupation, by education: real vs. placebo*

<i>Years of educ.</i>	Real	Placebo
Less than primary (0–8)	7.257	7.199
Primary completed (9–11)	14.806	14.953
Secondary completed (12)	18.376	18.398
Some college (13–14)	29.206	29.295
Tertiary completed (15–26)	48.545	48.470

The placebo is a randomly-generated variable drawn from a Poisson distribution with the same mean as the real variable.

(b) *Israel: Placebo reanalysis of core result in [Friedberg](#), Table III, row 4*

<i>Dep. var.: <math>\Delta</math> native wage</i>	(1)	(2)
<i>Lagged Soviet fraction IV:</i>	Real	Placebo
Émigré supply shock/pop. ( $r$ )	0.718** (0.339)	0.402 (0.807)
$N$	8353	8353
adj. $R^2$	0.520	0.523
Kleibergen-Paap $F$	42.23	3.570
$p$ -val. Anderson-Rubin $F$ -test	0.0195	0.594

The émigré supply shock  $r$  in the original study is Soviet émigrés in 1994, per Israeli in 1994 in each skill-occupation cell. The instrument in column 1, as in the original study, is the number of the Soviet émigrés who were in each skill-occupation cell prior to migration, per Israeli who was in that cell in 1989. The instrument in column 2 is the Poisson white noise from [Table 5a](#) divided by the number of Israelis in the cell in 1989. Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All specifications include education and occupation fixed effects.

**Table 6:** ISRAEL: KRONMAL SPECIFICATION CORRECTION TO FRIEDBERG

	(1)	(2)	(3)	(4)
<i>Dependent variable:</i>		$\Delta$ native wage		<i>asinh</i> émigrés in cell, 1994
<i>Estimator:</i>		2SLS		OLS
Émigré supply shock/pop. ( <i>r</i> )	0.718** (0.339)			
<i>asinh</i> émigré supply shock/pop.		0.742** (0.358)		
<i>asinh</i> émigrés in cell, 1994			0.0780 (0.0666) [0.572]	
<i>asinh</i> Israelis in cell, 1994			-0.0531 (0.0529)	
<i>asinh</i> Israelis in cell, 1989				0.629*** (0.00233)
<i>N</i>	8353	8353	8353	8353
adj. <i>R</i> <sup>2</sup>	0.520	0.520	0.519	0.897
Kleibergen-Paap <i>F</i>	42.23	38.37	5.819	
<i>p</i> -val. Anderson-Rubin <i>F</i> -test	0.0195	0.0211	0.115	—

*asinh* is inverse hyperbolic sine. Robust standard errors in parentheses. Square brackets show column 3 coefficient adjusted to be comparable to column 1 coefficient (dividing by immigrant fraction of labor force). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The émigré supply shock  $r$  in the original study is Soviet émigrés in 1994, per Israeli in 1994 in each skill-occupation cell. The instrument in each column is: (1) the number of the Soviet émigrés who were in each skill-occupation cell prior to migration, per Israeli who was in that cell in 1989 (as in original); (2) *asinh* of the instrument in column 1; (3) *asinh* of the number of the Soviet émigrés who were in each skill-occupation cell prior to migration; (4) none. All specifications include education and occupation fixed effects.

**Table 7:** FRANCE: PLACEBO REGRESSIONS, [BORJAS AND MONRAS \(2017\)](#) MODEL

(a) *France: Mean of 1962 Algerian stock by education, real vs. placebo*

<i>Education group</i>	Real	Placebo
Less than primary completed	8020.0	8032.8
Primary completed	263.6	263.0
Secondary completed	83.6	81.8
University completed	30.0	29.4

The placebo is a randomly-generated variable drawn from a Poisson distribution with the same mean as the real variable.

(b) *France: Placebo reanalysis of [Borjas and Monras Table 10, col. 3](#)*

<i>Dep. var.:</i>	(1)	(2)	(3)	(4)	(5)
$\Delta$ native unemployment					
<i>Lagged Algerian fraction IV:</i>	Real	Placebo	Real	Placebo	Placebo
Repatriate supply shock/pop.	0.0887** (0.0384)	0.0488 (0.0502)			
Algerian supply shock/pop.	0.247*** (0.0667)	0.419*** (0.126)	0.282*** (0.0669)	0.437*** (0.117)	0.443*** (0.118)
Change in native population					0.00279 (0.0123)
<i>N</i>	88	88	88	88	88
adj. $R^2$	0.460	0.392	0.432	0.368	0.355
Kleibergen-Paap $F$	54.23	2.440	247.7	5.285	5.116
$p$ -val. Anderson-Rubin $F$ -test	0.000122	0.0000466	0.000542	0.00351	0.00382

Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument set in each column is: (1) 1962 repatrié fraction and *real* 1962 Algerian fraction; (2) 1962 repatriate fraction and *placebo* 1962 Algerian fraction; (3) *real* 1962 Algerian fraction; (4) and (5) *placebo* 1962 Algerian fraction. All specifications include education fixed effects.

**Table 8:** FRANCE: KRONMAL SPECIFICATION CORRECTION TO BORJAS AND MONRAS

	(1)	(2)	(3)	(4)	(5)
<i>Dependent variable:</i>	$\Delta$ native unemployment				<i>asinh</i> Algerian supply shock
<i>Estimator:</i>	2SLS				OLS
Algerian supply shock/pop.	0.282*** (0.0669)				
<i>asinh</i> Algerian supply shock/pop.		0.282*** (0.0669)			
<i>asinh</i> Algerian supply shock			0.00234*** (0.000836)	0.00182** (0.000904) [0.226]	
<i>asinh</i> repatriate supply shock				0.00151 (0.00249) [0.044]	
<i>asinh</i> total native population			-0.00172 (0.00193)	-0.00228 (0.00226)	2.193*** (0.141)
<i>N</i>	88	88	88	88	88
adj. <i>R</i> <sup>2</sup>	0.432	0.432	0.301	0.344	0.808
Kleibergen-Paap <i>F</i>	247.7	248.2	42.44	13.82	—
<i>p</i> -val. And.-Rub. <i>F</i> -test	0.000542	0.000543	0.0147	0.0154	—

*asinh* is inverse hyperbolic sine. Robust standard errors in parentheses. Square brackets show column 4 coefficients adjusted to be comparable to column 1 coefficient (dividing by immigrant fraction of labor force). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instruments in each column is: (1) 1962 Algerian stock/pop., (2) *asinh* of 1962 Algerian stock/pop.; (3) *asinh* of 1962 Algerian stock; (4) *asinh* of 1962 Algerian stock and *asinh* of 1962 repatriate stock (in the second stage, both 1968 stocks are considered endogenous); (5) none. All specifications include education fixed effects. '*asinh* total native population' means *asinh* of 1968 total population minus the inflow of repatriates and Algerians.

**Table 9:** FRANCE: RE-ESTIMATION OF HUNT (1992) WITH ALTERNATIVE INSTRUMENT

	(1)	(2)	(3)	(4)	(5)	(6)
<i>Dependent variable:</i>	$\Delta$ native unemployment		2SLS		Repatriates supply shock OLS	
<i>Estimator:</i>						
Repatriate share, 1968 labor force	0.195** (0.080) [0.189]	0.120 (0.096) [0.116]	0.209*** (0.076) [0.202]	—	—	—
<i>asinh</i> repatriate share, 1968 labor force	—	—	0.209*** (0.076) [0.202]	—	—	—
<i>asinh</i> number of repatriates 1968	—	—	—	—	0.00254*** (0.00117) [0.156]	—
<i>asinh</i> size of labor force 1968	—	—	—	—	-0.00342 (0.00207)	—
<i>asinh</i> size of labor force 1962	—	—	—	—	—	1.056*** (0.041)
<i>N</i>	88	88	88	88	88	88
adj. <i>R</i> <sup>2</sup>	0.78	0.79	0.78	0.78	0.78	0.80
Other covariates	Yes	Yes	Yes	Yes	Yes	—
<i>Instruments for repatriates:</i>						
Temperature	Yes	Yes	—	—	—	—
1962 repatriate share	Yes	—	Yes	—	—	—
<i>asinh</i> 1962 repatriate share	—	—	—	Yes	—	—
<i>asinh</i> 1962 repatriate number	—	—	—	—	Yes	—

Robust standard errors are in parentheses and coefficients converted to the effect of a change in the labor force due to migration in square brackets. Column 1 exactly replicates Hunt (1992) Table 3, col. 4 but with robust standard errors; though this specification is invariant to the use of percentages (0–100) or shares (0–1), for comparability of the other specifications with those in other tables, Hunt’s percentages are transformed to shares. There is one observation per French department (province). *asinh* denotes inverse hyperbolic sine. The unreported covariates are seven regional dummies and the 1968–1962 differences in the share of the labor force aged 15–24 and the share with a baccalaureate (high school) degree, and the differences in the employment shares in seven industries. 1962 repatriate covariates refer to the population, 1968 to the labor force. Equations are estimated using  $1/(1/w_{62} + 1/w_{68})$  as weights in columns 1–5, where  $w_{62}$  and  $w_{68}$  are the 1962 and 1968 non-repatriate labor forces respectively; the weights in column 6 are the 1962 labor force. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$

**Table 10:** EUROPE: PLACEBO REGRESSIONS, [BORJAS AND MONRAS \(2017\)](#) MODEL

(a) *Europe: Mean 1990 Balkan stock by country, real vs. placebo*

<i>Country</i>	Real	<b>Placebo</b>
Austria	2612.2	2615.0
Greece	6.2	6.3
Ireland	0.0	0.0
Portugal	2.9	2.5
Romania	11.3	11.4
Spain	0.0	0.0
Switzerland	3082.9	3096.1

The placebo is a randomly-generated variable drawn from a Poisson distribution with the same mean as the real variable.

(b) *Europe: Placebo reanalysis of [Borjas and Monras Table 13](#), cols. 3 and 4*

<i>Dep. var.: <math>\Delta</math> native unemployment</i>	(1)	(2)	(3)	(4)
<i>Lagged Balkan fraction IV:</i>	Real	<b>Placebo</b>	Real	<b>Placebo</b>
Balkan supply shock/pop.	0.456 (0.311)	0.583* (0.323)	0.487 (0.376)	0.657 (0.510)
Change in native pop.			-0.00266 (0.0165)	-0.00426 (0.0181)
<i>N</i>	195	195	195	195
adj. $R^2$	0.741	0.740	0.739	0.737
Kleibergen-Paap $F$	17.72	6.189	16.34	5.219
$p$ -val. Anderson-Rubin $F$ -test	0.122	0.0204	0.187	0.149

Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument in each column is: (1) real 1990 Balkan fraction, (2) placebo 1990 Balkan fraction, (3) real 1990 Balkan fraction, (4) placebo 1990 Balkan fraction. All specifications include education and country fixed effects.

**Table 11:** EUROPE: KRONMAL SPECIFICATION CORRECTION TO BORJAS AND MONRAS

	(1)	(2)	(3)	(4)
<i>Dependent variable:</i>	$\Delta$ native unemployment			<i>asinh</i> Balkan supply shock
<i>Estimator:</i>	2SLS			OLS
Balkan supply shock/pop.	0.456 (0.311)			
<i>asinh</i> Balkan supply shock/pop.		0.459 (0.314)		
<i>asinh</i> Balkan supply shock			-0.0132 (0.0119) [-0.26]	
<i>asinh</i> total pop. (without Balkan)			0.00992 (0.0125)	0.955*** (0.201)
<i>N</i>	195	195	195	195
adj. $R^2$	0.741	0.741	0.339	0.509
Kleibergen-Paap $F$	17.72	17.31	1.498	—
$p$ -val. Anderson-Rubin $F$ -test	0.122	0.122	0.0209	—

*asinh* is inverse hyperbolic sine. Robust standard errors in parentheses. Square brackets show column 3 coefficient adjusted to be comparable to column 1 coefficient (dividing by immigrant fraction of population). \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument in each column is: (1) 1990 Balkan fraction, (2) *asinh* of 1990 Balkan fraction; (3) *asinh* of 1990 Balkan stock; (4) none. ‘*asinh* total pop. without Balkan’ means *asinh* of 2000 total population minus Balkan inflow 1990–2000. All specifications include education and country fixed effects.



**Table 12: MIAMI: SENSITIVITY ANALYSIS FOR BORJAS AND MONRAS (2017)**

<i>Dep. var.: <math>\Delta \log</math> weekly wage</i>	(1)	(2)	(3)	(4)	(5)	(6)
<i>City-skill cells:</i>	Original	Omit less than HS in Miami only	Omit Miami, all skill levels	Omit less than HS, all cities	Only Miami and other cities with wage data on Hispanics	
<i>Workers:</i>	Original	Original	Original	Original	Original	Hispanic only
Marinel supply shock/pop.	-1.263*** (0.320)	2.897** (1.302)	5.049 (5.289)	3.527*** (1.244)	-1.338*** (0.305)	0.0507 (0.135)
<i>N</i>	152	151	148	114	110	110
adj. $R^2$	0.479	0.482	0.479	0.472	0.515	-0.047
Kleibergen-Paap $F$	563.9	25.29	113.9	92.47	565.0	1964.7
$p$ -val. Anderson-Rubin $F$ -test	0.00325	0.0714	0.412	0.0227	0.00219	0.762

Column 1 of this table is an exact replication of Borjas and Monras (2017, Table 3, col. 3). Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . All columns identical to original analysis except for sample. All specifications include metropolitan area (3-digit) and education fixed effects. 'Original' workers used to calculate wages in each city-skill cell are *non-Hispanic* male workers age 25–59; in col. 6 above this is changed to *Hispanic* male workers age 25–59.

# Appendix

## A Compositional change in the March CPS versus May/MORG

Why would CPS coverage of low-skill blacks rise more in the March CPS than in the MORG? The March CPS Annual Social and Economic Supplement (ASEC) data come from a one-off survey that is not repeatedly applied to the same households. The MORG data, in contrast, are gathered from households that have been interviewed repeatedly in a panel over a period of time that extends over either 4 or 16 months.<sup>37</sup> It is well known that households in the CPS panel exhibit a *net* reduction in reported members during the panel: “people leaving a household are correctly identified, but new people entering a household are not always recorded” (Hainer et al. 1988, 517). This non-replaced individual attrition is highest for those only loosely connected to the reference person. In CPS data from 2006–2012, it is 11% for nonrelatives of the reference person who start out the panel living at the residence without their own relatives (Robison and Grieves 2014, 1344).

Such attrition would likely have been larger in the 1980s, and almost certainly larger in low-skill black households than the average. Low-skill black men in inner-city households are much more likely than others to exhibit individual attrition from the CPS panel (Neumark and Kawaguchi 2004). These are many of the same low-skill black men in poor, inner-city neighborhoods who “have tenuous or irregular ties with one or more households, but do not ‘usually’ live anywhere” (Hainer et al. 1988, 525). Many are not reported at all by the overwhelmingly female survey respondents in those neighborhoods—including almost a quarter of 25 year-old black men in the early 1980s (Hainer et al. 1988)—unless probing questions are asked by specially-trained interviewers.

In other words, the undercount of low-skill black men would be most responsive to efforts to increase coverage by more probing initial interviews about who lives at the residence, such as in the one-off March CPS or at the initial interview of an incoming rotation group. But the same people are among the most likely to disappear from the household roster by the time that earnings questions are asked of the panel’s outgoing rotation group.

## B Effects on the black-nonblack wage gap

We have shown that the black fraction ( $b$ ) rose at the right time and to the right degree to explain the fall in wages observed after the Mariel Boatlift, through mechanisms  $A$  (simultaneous changes in survey coverage),  $B$  (relatively low wages for marginal blacks covered), and  $C$  (the simultaneous wave of Haitian black immigrants). The data and the literature demonstrate empirically that all three of these mechanisms were active. Another mechanism is possible in theory: Let mechanism  $D$  denote a large change in the black-nonblack wage gap caused by the Boatlift.

Note that competition between the Mariel migrants and *all* native workers in this subpopulation of less-than-high-school would not affect the black coefficient  $\beta$ . Combined with the above evidence that there was no wage competition between the Mariel migrants and U.S. Hispanics, mechanism  $D$  posits that Mariel migrants only competed substantially with blacks, but not with whites or Hispanics at the same skill level.

Here we discuss why the finding that mechanisms  $A$ ,  $B$ , and  $C$  can generate spurious estimates of

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<sup>37</sup>After rotating into the panel housing units’ occupants are interviewed once a month for four months, ignored for eight months, then again interviewed once a month for four months. Questions about weekly earnings are only asked of the outgoing rotation groups at the fourth interview (month four) and eighth interview (month 16). Housing units are followed in the panel rather than people, so that if a new family moves into the address during the panel they become the survey respondents.

the wage effect of the Mariel Boatlift is robust to the existence of mechanism  $D$ .

First, even under conservative assumptions, most of the estimated treatment effect is accounted for by race composition effects. Suppose that we assume away mechanisms  $B$ ,  $C$ , and  $D$  entirely. That is, assume that marginal blacks added to the March CPS sample when coverage of blacks doubled and then tripled after 1980 had the same average incomes as blacks already in the sample, and restrict there to be no effect on the black coefficient  $\beta$  from the Haitian immigrant shock of 1980 or the Mariel Boatlift. Both of these can be done by imputing to Miami the black coefficient from the control cities ( $\beta^{\text{Miami}} \equiv \beta^{\text{Control}}$ ) in calculating equation (1).<sup>38</sup> Even under these assumptions, using the Borjas control cities, for example  $\Delta \ln \tilde{w}_{1985} = \hat{\beta}^{\text{Control}} \times (b_{1985}^{\text{Miami}} - b_{1979}^{\text{Miami}}) - \hat{\beta}^{\text{Control}} \times (b_{1985}^{\text{Control}} - b_{1979}^{\text{Control}}) = -0.285 \times (0.910 - 0.363) - (-0.285 \times (0.180 - 0.135)) = -0.143$ . Given that Borjas finds a treatment effect in the range of  $-10\%$  to  $-30\%$ , this means that the most conservative estimate of the spurious wage effect ( $-14\%$ ) explains somewhere between half of the effect estimated by Borjas (if his estimate is  $-30\%$ ) and all of that effect (if his estimate is  $-10\%$ ). This would arise from pure race-composition changes, through mechanism  $A$  alone: changes in  $b$  but not  $\beta$ .

That is, the most conservative assumptions only leave something less than half of the estimated wage effect of the Mariel Boatlift to be explained by mechanisms  $B$ ,  $C$ , and  $D$  put together, and anything other than such assumptions leaves much less than half of the estimated wage effect to be explained by mechanisms  $B$ ,  $C$ , and  $D$  put together. This bounds the quantitative importance of mechanism  $D$ .

Beyond this, the above findings contain information about the relative importance of mechanism  $D$ . Most notably, mechanism  $D$  offers no explanation for why the estimated wage effect of the Boatlift would be three times larger in the March CPS extract than in the MORG extract. If the Mariel migrants competed more with blacks than with nonblacks, both CPS extracts should show this in equal measure. The other mechanisms, as discussed above, do offer an explanation for this result. Mechanism  $D$  can only explain the absence of an effect on Hispanics by simply positing that the Mariel immigrants competed only with blacks but 1) not with whites (competition with both whites and blacks would not change  $\beta$ ), and 2) not with Hispanics within the less-than-high-school subpopulation. Mechanism  $D$  offers no clear reason why a supply shock of Cubans that had subsided after 1984 would have its largest effects on the black-white wage gap several years after the shock. Mechanism  $D$  can only explain the absence of an effect of the Boatlift on women by simply positing that the Mariel immigrants competed with black men but not with black women (as well as neither with white non-Hispanic women nor white non-Hispanic men).

This evidence does not rule out the theoretical possibility of nonzero competition effects via mechanism  $D$ . But it does indicate that compositional effects via mechanisms  $A$ ,  $B$ , and  $C$  are large enough to fully explain the substantial discrepancies between prior studies of the Mariel Boatlift, even if wage competition is nonzero and specific to blacks.

## C Comparing coefficient estimates in the various studies

Friedberg (2001) and Borjas and Monras (2017) employ what is known as the skill-cell approach to the impact of immigration. This uses variation in the density of immigrants across worker groups defined by education and another dimension (geography, in the case of Borjas and Monras, occupation in the case of Friedberg). The resulting regression coefficient should be interpreted as

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<sup>38</sup>In principle, another way to rule out mechanisms  $B$ ,  $C$ , and  $D$  would be to estimate  $\beta$  on pre-1980 data only, but in practice the available samples are too small. For example, in the May CPS there are only 12 non-blacks in the pre-1980 samples, as well as only 16 blacks.

the impact of immigrants of a particular skill on immigrants with the same skill, averaged across skills. It does not represent the impact of all immigrants on all natives (the effect studied by Hunt 1992), because it omits the impacts of immigrants of a particular skill on natives with different skills. We ignore this difference when comparing results from skill-cell studies with the Hunt (1992) coefficients. On the other hand, we do adjust the Hunt (1992) coefficient, which reflects the impact of the share of immigrants (in the labor force), to correspond to the Borjas and Monras and Friedberg coefficients, which reflect the impact of the ratio of immigrants to natives (in the skill-cell). To do so, we multiply the Hunt coefficients ( $\gamma$ ) by  $(1 - p)^2$ , where  $p = 0.016$  is the aggregate share of immigrants in the French labor force; in practice, this multiplication by 0.97 makes little difference. Proof:

$$\begin{aligned}
\frac{\partial u}{\partial(M/N)} &= \frac{\partial u}{\partial(M/(M+N))} \frac{\partial(M/(M+N))}{\partial(M/N)} \\
&= \gamma \frac{\partial}{\partial(M/N)} \frac{M/N}{1+M/N} \\
&= \gamma \frac{1}{(1+M/N)^2} \\
&= \gamma(1-p)^2.
\end{aligned} \tag{A.1}$$

A more quantitatively important adjustment is made to the coefficients from the Kronmal-corrected specifications to make them comparable to the Borjas and Monras and Friedberg coefficients. To do so, we multiply the coefficient on the inverse hyperbolic sine of immigrants ( $\theta$ ) by  $(1 - p)/p$ , where  $p$  is the immigrant share. Proof:

$$\begin{aligned}
\frac{\partial u}{\partial(M/N)} &= \frac{\partial u}{\partial \log M} \frac{\partial \log M}{\partial(M/N)} \\
&= \frac{\theta}{M} \frac{\partial M}{\partial(M/N)} \\
&= \theta/(M/N) \\
&= \theta \frac{1-p}{p}.
\end{aligned} \tag{A.2}$$

[Appendix Table 1](#) uses these relationships to show the coefficient estimates from the main text in comparable terms.

APPENDIX TABLE 1: COMPARISON OF COEFFICIENTS

	(1)	(2)	(3)	(4)	(5)
	<i>Miami</i>	<i>Israel</i>	<i>France</i>	<i>France "Algerians"</i>	<i>Europe</i>
	Wage	Wage	Unemployment	Unemployment	Unemployment
<b>A. Kronmal coefficients</b>					
Borjas and Monras	0.00066	-0.035	0.0015	0.0018**	-0.0132
Friedberg	—	0.078	—	—	—
Hunt (no temperature instrument)	—	—	0.0025**	—	—
<b>B. Borjas and Monras (2017), 'BM'</b>					
	-1.26**	-0.62*	0.09**	0.25**	0.46
<b>C. Coefficients comparable to BM</b>					
Transformed Kronmal coefficients					
Borjas and Monras	0.005	-0.28	0.04	0.23**	-0.26
Friedberg	—	0.57	—	—	—
Hunt (no temperature instrument)	—	—	0.16**	—	—
Friedberg (2001)	—	0.72	—	—	—
Transformed Hunt coefficients					
Hunt (1992)	—	—	0.19**	—	—
Hunt (temperature instrument only)	—	—	0.12	—	—
<b>D. Comparable to BM, immigrant share = 0.1</b>					
Transformed Kronmal coefficients					
Borjas and Monras	0.006	-0.31	0.01	0.02**	-0.12
Friedberg	—	0.70	—	—	—
Hunt (no temperature instrument)	—	—	0.02**	—	—
Friedberg (2001)	—	0.80	—	—	—
Transformed Hunt coefficients					
Hunt (1992)	—	—	0.16**	—	—
Hunt (temperature instrument only)	—	—	0.10	—	—

Notes: Panel C transforms coefficients on variables not defined as the ratio of immigrants to natives to be consistent with a coefficient on this ratio, using the share of immigrants in the study in question. This transformation has a trivial effect for the Hunt coefficients, so we do not report the (identical) original coefficients. Panel D makes the transformation assuming the ratio is 0.10, which is the approximate share for Israel and Mariel. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ .

# Online Supplement

## “The Labor Market Effects of Refugee Waves Reconciling Conflicting Results”

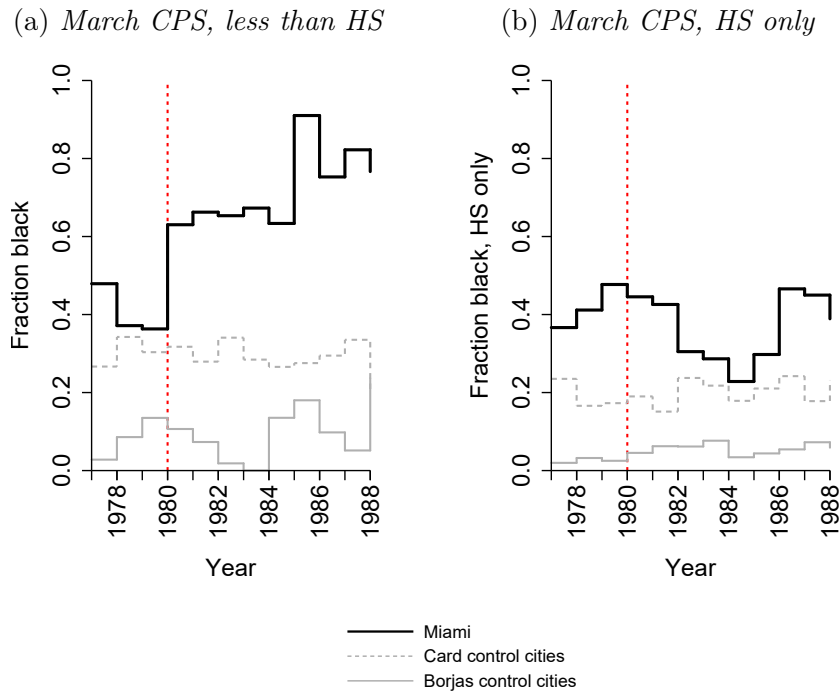
### A Fraction black in high-school-only subgroup

Supplement Figure 1 compares the black fraction of the population implied by the less-than-high-school subgroup used by Borjas (2017) and the otherwise identical high-school-only subgroup.

### B Placebo and Kronmal-corrected regressions for Miami

Here we present reanalysis of the Miami regressions in Borjas and Monras (2017) mirroring the reanalyses in the main text of the Israel, France, and Europe studies. Supplement Table 1 shows the placebo regressions. Supplement Table 2 shows the Kronmal specification correction.

**SUPPLEMENT FIGURE 1: FRACTION BLACK IN POPULATION REPRESENTED BY SAMPLE: LESS-THAN-HS VS. HS-ONLY**



‘HS’ is high school. The Miami less-than-high-school sample in (a) is identical to the sample in Borjas (2017, Table 3A); the sample in (b) is for high-school-only but otherwise identical. This figure includes only male non-Hispanic workers, aged 25–59, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly. Fraction black in (a) and (b) weighted by March Supplement weight. Following Borjas, the years in the graph refer to the year in which earnings were earned, not the year of the survey (e.g. 1980 data for March CPS are from the 1981 March CPS).

**SUPPLEMENT TABLE 1: MIAMI: PLACEBO REGRESSIONS**

(a) *Miami: Mean 1977–1979 Cuban stock by education, real vs. placebo*

<i>Education</i>	Real	<b>Placebo</b>
Less than high school	4912.2	4913.6
High school	2594.9	2599.2
Some college	1693.0	1698.3
College graduate	1293.6	1300.5

The placebo is a randomly-generated variable drawn from a Poisson distribution with the same mean as the real variable.

(b) *Miami: Placebo reanalysis of Borjas & Monras Table 3, cols. 3 and 4*

<i>Dep. var.: <math>\Delta \log</math> weekly wage</i>	(1)	(2)	(3)	(4)
<i>Lagged Cuban fraction IV:</i>	Real	<b>Placebo</b>	Real	<b>Placebo</b>
Mariel supply shock/pop.	−1.263*** (0.320)	−6.058 (10.58)	−1.310*** (0.322)	−4.350 (8.250)
Change in native population			0.0385 (0.0382)	0.0606 (0.0739)
<i>N</i>	152	152	152	152
adj. $R^2$	0.479	0.063	0.478	0.311
Kleibergen-Paap $F$	563.9	1.609	561.3	1.450
$p$ -val. Anderson-Rubin $F$ -test	0.00325	0.608	0.00265	0.652

Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument set in each column is: (1) real 1977–79 Cuban fraction, (2) placebo 1977–79 Cuban fraction, (3) real 1977–79 Cuban fraction, (4) placebo 1977–79 Cuban fraction. All specifications include metropolitan area (3-digit) and education fixed effects.

**SUPPLEMENT TABLE 2: MIAMI: KRONMAL SPECIFICATION CORRECTION**

	(1)	(2)	(3)	(4)
<i>Dependent variable:</i>		$\Delta \log$ weekly wage		<i>asinh</i> Mariel supply shock
<i>Estimator:</i>		2SLS		OLS
Mariel supply shock/pop.	-1.263*** (0.320)			
<i>asinh</i> Mariel supply shock/pop.		-1.274*** (0.330)		
<i>asinh</i> Mariel supply shock			0.000662 (0.000618)	
<i>asinh</i> total pop. (without Mariel)			0.00487 (0.00806)	2.144 (2.691)
<i>N</i>	152	152	152	152
adj. $R^2$	0.479	0.479	0.591	0.160
widstat	563.9	486.3	13.37	
arfp	0.00325	0.00419	0.354	

*asinh* is inverse hyperbolic sine. Robust standard errors in parentheses. \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . The instrument set in each column is: (1) 1977–79 Cuban stock/pop., (2) *asinh* of 1977–79 Cuban stock/pop.; (3) *asinh* of 1977–79 Cuban stock; (4) none. ‘*asinh* total pop. (without Mariel)’ means *asinh* of 1981–1984 total population minus Mariel inflow. All specifications include metropolitan area (3-digit) and education fixed effects.