THE LABOR MARKET EFFECTS OF REFUGEE WAVES: RECONCILING CONFLICTING RESULTS

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Studies have reached conflicting conclusions regarding the labor market effects of exogenous refugee waves such as the Mariel Boatlift in Miami. The authors show that contradictory findings on the effects of the Mariel Boatlift can be explained by a large difference in the pre- and post-Boatlift racial composition in certain very small subsamples of workers in the Current Population Survey. This compositional change is specific to Miami and unrelated to the Boatlift. They also show that conflicting findings on the labor market effects of other important refugee waves are caused by spurious correlation in some analyses 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 it fails to substantiate claims of large detrimental effects on workers with less than a high school education.

A long literature in labor economics has reached something of a consensus that the effects of immigration on average native workers'

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wages and employment is generally small or zero.[1] There is less agreement on the narrower question of the impact of immigration on less-skilled workers: The committee that authored the consensus report published as Blau and Mackie (2016) concluded that the effect of immigration on wages of US workers with less than a high school education is negative, but it did 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. This research has debated whether the findings for Miami missed impacts on subgroups of natives, such as the least skilled (Borjas 2017; Peri and Yasenov 2018), or were contaminated by spurious labor-market trends (Angrist and Krueger 1999: 1328). Borjas and Monras (2017) found that prior analysis of all four of these refugee episodes missed important impacts on subgroups and lacked adequate causal identification. In some cases, these later papers did find sizeable detrimental effects on natives. The discordant findings in this literature have not been reconciled.

We offer two new explanations for the conflicting results in all of the above studies. One explanation is large compositional changes in the US survey data introduced by the selection of narrow subgroups of workers to study; 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.

In this article, we review and interpret the discrepant analyses of the Mariel Boatlift in Miami, followed by the discrepant results on the effects of three other refugee waves—in Israel, in France, and across Europe.

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 7%. Card (1990: 255) compared trends in Miami to trends in four unaffected control cities and concluded that "the Mariel immigration had essentially no effect on the 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, Carcillo, and Zylberberg 2014).

[1]See the National Academies consensus report for the United States (Blau and Mackie 2016: 204), or the survey by Kerr and Kerr (2011) including both the United States and Northern Europe.

Conflicting Reanalyses

Recent, concurrent reanalyses have reached contrasting conclusions about the robustness of the original Card (1990) study. Both Peri and Yasenov (2018) and Borjas (2017) reanalyzed the Card result. While Card (1990) had studied the effects of the Boatlift on natives with an education level of *high school or less*, both of the new reanalyses study the impact on natives with *less than high school*. Borjas (2017: 1077) found that the Boatlift caused the wages of males in this latter subgroup to fall "dramatically, by 10 to 30%," whereas Peri and Yasenov found "no significant difference in the wages of workers in Miami relative to its control" (2018: 1).[2] The studies stress different extracts from the Current Population Survey (CPS), use different weighting variables to construct synthetic control cities, and choose different groups of "natives" to study. Borjas focused on non-Hispanic male workers with less than high school, except those under 25 or over 59. Peri and Yasenov focused on all non-Cuban workers age 19–65 with less than high school.

Several findings in these two conflicting reanalyses have not been adequately explained. We seek to explain the following five empirical puzzles. First, no detrimental effect is observed on workers with high school or less, or on workers with exactly high school. The estimated wage effect of the Mariel Boatlift is indistinguishable from zero for workers with high school *or less* (Card 1990), and positive for workers with high school only when considered separately, as we show below. This sharp contrast with the results for less than high school is somewhat at odds with the fact that the Mariel Boatlift created a large positive shock to Miami's supply of workers with high school only *and* workers with less than high school: Almost half of the Mariel migrants did have a high school degree (Borjas 2017: table 1). Workers with high school only and less than high school are close substitutes in the US labor market (Card 2009). 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.

Second, 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: 1) the Current Population Survey (CPS) March Supplement, and 2) a combination of the CPS May supplement (through 1978) and the CPS Merged Outgoing Rotating Groups (MORG) from 1979.[3] Borjas (2017: tables 5–6) found effects three times larger in the

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[2]Borjas (2017) studied wage effects and did not reanalyze Card's null result on employment effects. Borjas and Monras (2017) did reanalyze Card's null result on employment, and confirmed it, as did Peri and Yasenov (2018).

[3]An issue faced by all studies is that the CPS did not collect country of birth at this time (pre-1994), so the impact on "natives" is inferred from estimated impacts on groups likely to be predominantly natives. In both Borjas (2017) and Peri and Yasenov (2018), "worker" means a person 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 data) positive usual weekly earnings and positive usual hours worked weekly.

March CPS data than in the May data. Peri and Yasenov (2018) attributed this large difference to sampling error (the March CPS sample is smaller than the MORG sample) 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) persisted across several years, it appears unlikely to arise from pure sampling error or measurement error.[4]

Third, no negative effect is observed on US Hispanics. No reanalysis of the Mariel Boatlift finds negative wage impacts for samples that include non-Cuban Hispanics, nor is there a negative wage impact for non-Cuban Hispanics separately, as we show below. Borjas (2017) argued that omitting US Hispanics is necessary because some of the control cities were experiencing a contemporaneous influx of non-Cuban Hispanics. But Peri and Yasenov (2018) showed there is no break in the *Miami-only* wage trend for workers with less than high school—when Hispanics are included—between 1972–1979 and 1980–1990. That is, with Hispanics in the sample, there is no post-Boatlift change in Miami's wage trend that would be concealed, in a differences-in-differences analysis, by a compositional wage decline in the control cities. Though excluding Hispanics is consistent with attempting to study impacts on a predominantly native-born sample, the lack of an 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.[5]

Fourth, the wage effects estimated by Borjas reached their largest size years after the supply shock ended. Peri and Yasenov (2018) observed that after 1984, the share of Cubans among workers with less than high school in Miami returned to pre-Boatlift levels in the CPS data. But the wage effects estimated by Borjas's (2017) method are very large for several years after 1984, peaking in 1986 and lasting through the end of the decade. 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 increase only slowly during the supply shock but persist long after it ends—but these mechanisms are unclear.

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[4]The March CPS sample is indeed small at 17–27 workers in each year 1977–1983, though the May sample is even smaller (12 and 16 during the 1977–1978 surveys). The annual MORG sample falls in the range of 31–56; see Borjas (2017: table 3).

[5]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) found that Cuban immigrants were complements to black and white natives, as well as to black and white immigrants. He also found that Hispanic immigrants in general were 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."

Fifth, there is no observed 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; Borjas and Monras 2017; Peri and Yasenov 2018). It is theoretically possible for Cubans flooding the Miami labor market to have large effects on wages—in some Borjas specifications −0.30 to −0.45 log points—but no effects on unemployment. This would, however, 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).[6]

Sensitivity to Subgroup Selection

We begin by illustrating the known sensitivity of the recent Mariel Boatlift reanalyses to subgroup selection (Peri and Yasenov 2018: figure 8). Thereafter we propose a mechanism by which this subgroup selection can generate discordant results.

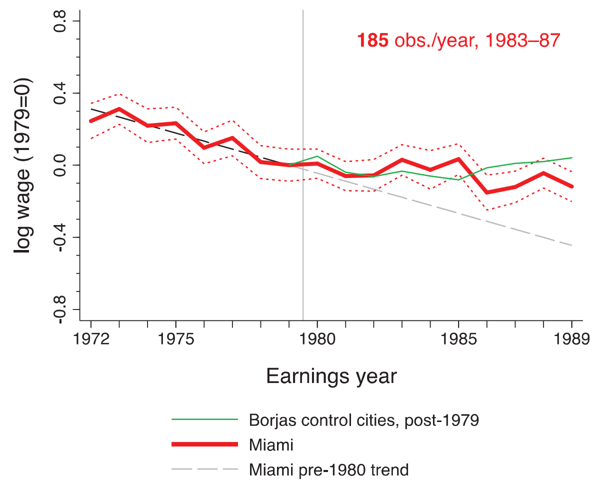
Figure 1 illustrates the original Card result: There was no fall in low-skill wages in Miami after 1980 relative to pre-trends or control cities. Here we define "low-skill" as workers with high school or less, the canonical definition in the labor literature.[7] In the figure, all wage averages are normalized so that the 1979 average equals zero. The thick, solid (red) line shows the annual average wage in Miami for low-skill workers—that is, all non-Cubans age 19–65 with high school or less, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly, in the March CPS data. The dotted lines show the confidence interval around each year's mean. The dashed line after 1979 shows a linear extrapolation of the Miami trend from 1972 up to and including 1979. The thin, solid (green) line shows the annual average wage in the control cities preferred by Borjas (2017).[8] Miami wages stagnated after 1979, rather than

[6]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." We do not investigate the sensitivity of the results to the exclusion of women. Unlike the preliminary versions of his work, the published Borjas paper (2017: table 6) does contain some results based on a sample including women as well as men. The wage impacts are one-half to two-thirds the size of those found for the male sample. This sensitivity was first pointed out by Roodman (2015). See https://blog.givewell.org/2015/10/21/why-a-new-study-of-the-mariel-boatlift-has-not-changed-our-views-on-the-benefits-of-immigration/ (accessed December 16, 2018).

[7]Acemoğlu and Autor (2011: 1101) described this definition as "canonical." The early modern immigration literature, as well, used "low skill" or "less skilled" as a synonym for workers without specialized training (Johnson 1980), usually taken to mean workers with no college (e.g., Card 1990; Altonji and Card 1991).

[8]The Card-preferred control cities (1990), 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-preferred control cities (2017), chosen to resemble pre-1980 employment growth in Miami, are Anaheim, Rochester, Nassau-Suffolk, and San Jose. The results in Figures 1 and 2 are not substantially different when the Card controls are used.

Figure 1. Average Low-Skill Wages in Miami: March CPS

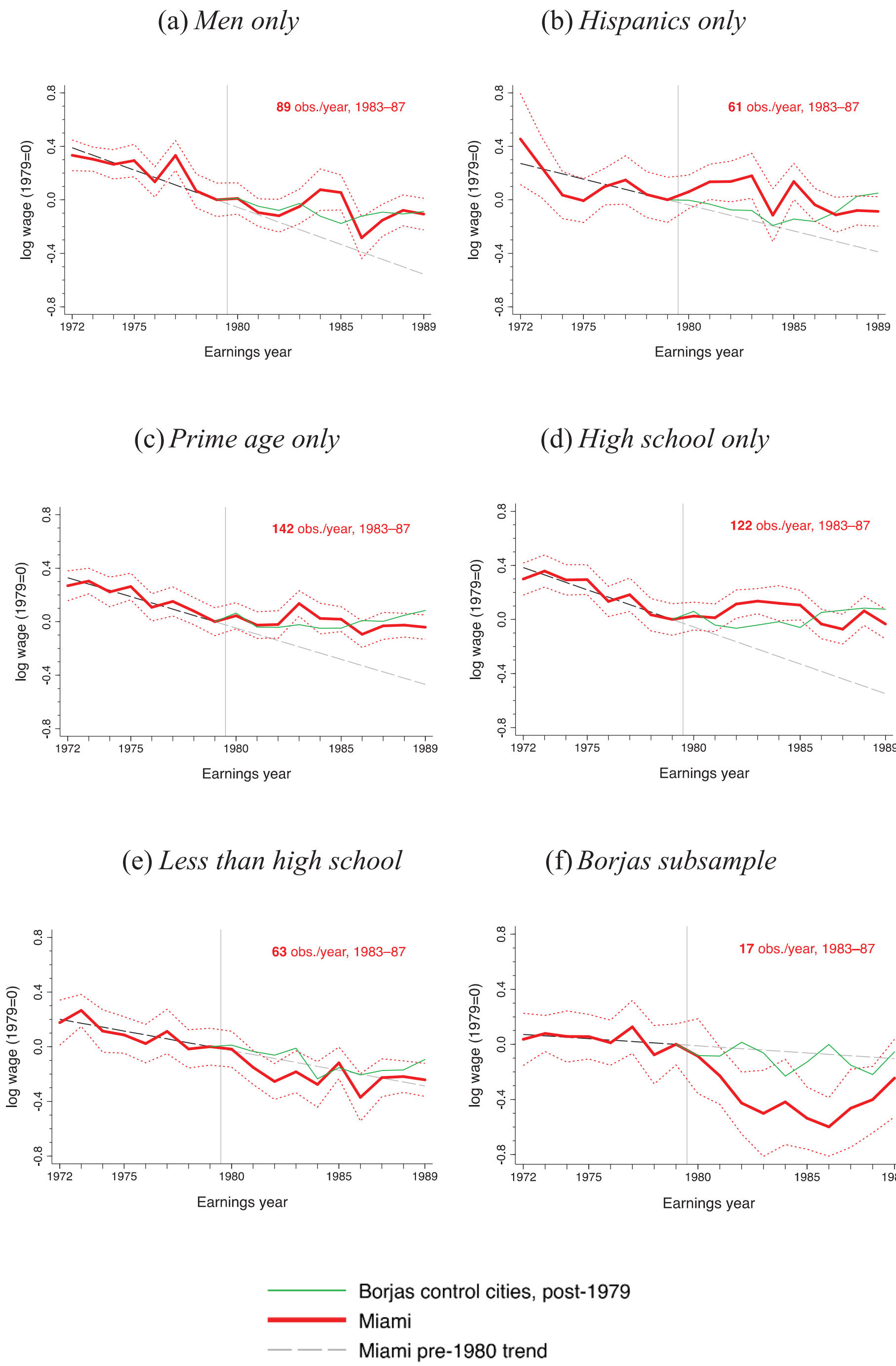


Notes: Thick, solid (red) line shows average wage in Miami, with dotted line showing 95% confidence interval for the annual mean, calculated using Supplement Weight. Dashed line shows post-1980 continuation of pre-1980 linear trend in Miami. Thin, solid (green) line shows average wage of same type of workers in the control cities preferred by Borjas (2017): Anaheim, Rochester, Nassau-Suffolk, and San Jose, calculated using Supplement Weight. March CPS data. "Earnings year" is the year prior to the survey year, following Borjas (2017). "Low-skill" means workers with high school or less. Wages are for non-Cuban workers age 19–65 with high school or less, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly.

declining as before 1979, and the subsequent wage evolution in Miami closely resembled that in the control cities preferred by Borjas from 1979–1985.

One might be concerned that the Card analysis obscures wage competition between the Mariel migrants and the Miami residents who closely resembled them. Figure 2 shows the same analysis suggests no negative effect of the Boatlift on various subgroups of low-skill Miami workers competing most closely with the Mariel migrants, and even a positive effect on Hispanics. The Mariel migrants were predominantly men, essentially all Hispanic, largely prime age. They were roughly evenly split between workers with high school only and those with less than high school. Figure 2a shows low-skill workers who are men. Figure 2b shows low-skill workers who are Hispanic. Figure 2c shows low-skill workers who are prime age (25–59). In all cases, as in Figure 1, Miami wages rise relative to the pre-trend, in 1980 and every year thereafter. This rise is statistically significant in 1982 and every year thereafter. The rise is economically substantial, with Miami wages 30% above the pre-trend by 1985. A large rise (about + 10–20%) relative to average wages also occurs in the Borjas control cities and is statistically significant in some years. When the subgroup of workers with high school only are analyzed separately (Figure 2d), a large and statistically significant rise is seen in wages relative to the pre-trend (about + 30–40%) and relative to

Figure 2. Average Low-Skill Wages in Miami: March CPS Subgroups



Notes: Thick, solid (red) line shows annual average wage in Miami, with 95% confidence interval, using Supplement Weight. Dashed line shows pre-1980 linear trend in Miami. Thin, solid (green) line shows average wage of same type of workers in Borjas control cities, using Supplement Weight. March CPS data. "Earnings year" is year before survey year. "Low-skill" means workers with high school or less. "Borjas subsample" omits Hispanics, females, age < 25, age > 59, and high school only. "Workers" report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly.

the Borjas control cities (about + 10–20%). When the subgroup of low-skill workers with less than high school are analyzed separately (Figure 2e), there may be a short-lived fall (about −10 to −20%) in wages relative to the pre-trend and the Borjas control cities—but it is not statistically precise (both are significant in 1982 only). This closely corresponds to the core result in Peri and Yasenov (2018).

Borjas (2017) reached a sharply different conclusion by heavily truncating the March CPS data on low-skill workers. The Borjas sample omits women, Hispanics, workers under age 25, workers over age 59, and workers who have finished high school or its equivalent. This leaves an average of 17 observations per year during the period in which he found the largest effect (1983–1987); that is, he omitted 91% of the observations of low-skill workers in Miami during that period (average 185 observations per year). In Borjas's subgroup, there is a large fall in Miami wages (about −30 to −50%) that lasted several years after 1980, both relative to the pre-1980 Miami trend and relative to the Borjas control cities (Figure 2f). Borjas estimated the peak effect occurred six years after the Boatlift. Peri and Yasenov (2018) attributed this fall in wages to measurement error arising from the small size of the selected subsample.[9]

An Explanation for Subgroup Sensitivity: Sample Composition Change

We propose that all substantial disagreement in these prior studies can be explained by previously unreported changes in the underlying survey data. There was a sharp increase in the number of black workers with less than high school sampled by the CPS in Miami, coincident with the Mariel Boatlift but unrelated to it. Because black workers with less than high school earned much less than did non-black workers at the same education level, this compositional effect generated a spurious wage decline among Miami workers with less than high school.

Table 1 shows the number of blacks and non-blacks in the CPS samples of Miami workers with less than high school used by Borjas (2017): male non-Hispanics age 25–59, in columns (2)–(4). Column (1) is the year of each CPS survey, as in Borjas (2017: table 3). Between the 1977–1979 surveys and the post-1980 surveys, there were large increases in the fraction of black men in the sample, both in the March CPS (upper panel) and the May/ORG CPS extracts (lower panel)—but much larger in the March CPS.

[9]An important source of measurement error in this setting could arise from match bias in the CPS (Hirsch and Schumacher 2004): Many wage observations in the CPS are imputed from wages earned by a matched "donor" worker, and the donor can be a worker from a different metropolitan area and ethnicity in both the March CPS and the MORG. In the MORG, the donor can even be a worker from a previous month or a previous year. In principle, this could introduce substantial measurement error and attenuate the coefficient estimates, such that the estimated wage effect would rise in absolute value if estimated only on workers with directly observed wages. But the opposite is true in the present case: The core wage effects measured by Borjas (2017: table 5) declined slightly in absolute value when workers with imputed wages were dropped (results not shown, available on request).

Table 1. Blacks in CPS Subsamples

(1)	(2)	(3)	(4)	(5)	(6)	(7)
		Miami		Card control cities	Borjas control cities	Miami
	Less than high school					HS or less
Survey year	N black	N non-black	Fraction black			
			March CPS			
1977	11	12	0.479	0.267	0.028	0.408
1978	10	16	0.372	0.343	0.086	0.394
1979	8	14	0.363	0.304	0.135	0.442
1980	9	8	0.630	0.317	0.107	0.505
1981	11	7	0.663	0.279	0.073	0.512
1982	11	9	0.653	0.341	0.019	0.456
1983	17	10	0.673	0.284	0.000	0.465
1984	11	7	0.634	0.266	0.135	0.363
1985	14	2	0.910	0.276	0.180	0.541
1986	11	4	0.753	0.295	0.098	0.569
1987	13	3	0.822	0.335	0.052	0.605
1988	13	5	0.767	0.198	0.237	0.543
1989	9	8	0.610	0.289	0.229	0.533
1990	10	6	0.728	0.327	0.115	0.541
			May/ORG CPS			
1977	10	6	0.660	0.348	0.097	0.509
1978	6	6	0.534	0.322	0.121	0.474
1979	34	22	0.600	0.290	0.123	0.471
1980	35	20	0.644	0.299	0.109	0.534
1981	34	17	0.703	0.301	0.091	0.596
1982	27	12	0.720	0.300	0.068	0.551
1983	33	17	0.679	0.311	0.095	0.474
1984	28	20	0.604	0.309	0.163	0.449
1985	22	4	0.862	0.292	0.121	0.650
1986	27	9	0.783	0.307	0.078	0.567
1987	28	18	0.646	0.332	0.055	0.533
1988	26	11	0.745	0.255	0.087	0.571
1989	24	13	0.683	0.321	0.041	0.567
1990	20	18	0.572	0.287	0.112	0.519

Notes: Year shows the year the survey was conducted, matching Borjas (2017: table 3A). All samples include only male non-Hispanic workers, age 25–59. March CPS black fraction calculated using Supplement Weight; May/ORG calculated using earnings weight. Unweighted fractions are similar. Card-preferred control cities are Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg; Borjas-preferred control cities are Anaheim, Rochester, Nassau-Suffolk, and San Jose.

In the March CPS, the fraction of black workers roughly tripled between the survey conducted in 1979 and the survey in 1985, rising 55 percentage points (column (4)). The increase in the May/ORG was roughly one-third as large, with the main increase in 1985. The table shows that no such increase in sampled blacks occurred in the control cities preferred by Card or the control cities preferred by Borjas (columns (5) and (6)). There was also no such increase in sampled blacks for workers in Miami with high school or less—the group analyzed by Card (column (7)).

This compositional change could produce a spurious decline in the average wage of Miami workers with less than high school for three reasons. The clearest reason is that Miami experienced a large influx of low-income black Haitian workers precisely in 1980 (Portes and Stepick 1985; Stepick and Portes 1986). Almost all 15,000 of these Haitians were prime-age workers with less than high school (Portes and Stepick 1985: 495–97), and approximately 6,150 were male (41%; Stepick and Portes 1986: 332).[10] Before they arrived, only 16,940 male black workers with less than high school resided in Miami in the subpopulation analyzed by Borjas.[11] Thus, the arrival of Haitians in 1980 alone raised the number of blacks in the Borjas subpopulation by approximately 36%. The overall compositional change through this channel is likely greater, since Haitians continued to arrive during the early 1980s (Portes and Stepick 1985: 495, footnote 3).

These Haitian blacks who arrived during 1980–1982 cannot be distinguished from US blacks in the data,[12] and they had extremely low incomes. Individual earnings for typical newly arrived, employed Haitian workers were $105 per week in 1983–1984 (Portes, Stepick, and Truelove 1986: 88), compared to $263 for the Borjas March CPS subpopulation of otherwise observably identical black men with less than high school in Miami during 1977–1979. This would produce a purely compositional change in earnings for black men with less than high school of -0.25 log points.[13] Since blacks comprise more than two-thirds of the average Miami sample of less than high school men during 1980–1988, the compositional effect on the wages of the whole sample would be on the order of at least -0.17 log points. That finding is large enough to explain approximately half of the treatment effect in the March CPS data (see Table 2a) and the entire treatment effect in the ORG data (see Table 2b).

A second reason for a decline in the average wage is the increased coverage around 1980 in surveys run by the Census Bureau of low-skill US black men, who had lower average incomes than non-blacks. In 1978, the Levitan Commission had quantified major undercoverage of black men in the 1970 census (Levitan and the Commission 1979: 142), which in the run-up to the

[10]The Haitians who arrived in Miami in 1980–1982 had an age distribution similar to that of the Mariel migrants. The Haitians' principal difference was that they were much less educated and were much more likely to originate from rural areas than were the Mariel migrants (Portes, Stepick, and Truelove 1986).

[11]In the 1980 Census 5% sample microdata (Ruggles et al. 2015), there are 847 observations in the Miami-Hialeah metropolitan area of black, male, non-Hispanics age 25–59 with less than high school who report positive income and positive weeks worked last year, living in households (pre-1990 definition) rather than group quarters. The sampling weight implies that these observations represent a subpopulation of 16,940 black men in Miami.

[12]Until 1994, the Current Population Survey did not regularly report an individual's country of birth, so US native blacks cannot be distinguished from immigrant blacks in the 1970s and 1980s.

[13]Since $\left(\frac{\ln(263.4) \times 16,940 + \ln(105) \times 6,150}{16,940 + 6,150}\right) - \ln(263.4) = -0.25$.

Table 2. Reanalysis of Borjas (2017), Table 5: With Black Indicator

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
			Add indicator for black					
	Exact replication		*Nationwide*		*By city*		*By city–less than HS*	
Control cites:	*Card*	*Borjas*	*Card*	*Borjas*	*Card*	*Borjas*	*Card*	*Borjas*
(a) March CPS extract								
1981–83	−0.204***	−0.290***	−0.121	−0.194***	−0.096	−0.174**	0.001	−0.078
	(0.076)	(0.073)	(0.078)	(0.070)	(0.081)	(0.072)	(0.062)	(0.061)
1984–86	−0.368***	−0.454***	−0.202***	−0.301***	−0.137	−0.227***	0.109*	−0.001
	(0.060)	(0.059)	(0.072)	(0.053)	(0.083)	(0.058)	(0.063)	(0.059)
1987–89	−0.329***	−0.303***	−0.202**	−0.237***	−0.135	−0.149**	−0.025	−0.049
	(0.081)	(0.072)	(0.093)	(0.071)	(0.089)	(0.066)	(0.098)	(0.070)
1990–92	−0.026	−0.056	0.094	0.025	0.105	0.037	0.220	0.121
	(0.072)	(0.123)	(0.086)	(0.117)	(0.089)	(0.112)	(0.134)	(0.103)
N	75	75	75	75	75	75	75	75
(b) May/ORG CPS extract								
1981–83	−0.075***	−0.140***	−0.047	−0.104**	0.005	−0.056	0.036	−0.025
	(0.026)	(0.049)	(0.029)	(0.046)	(0.034)	(0.040)	(0.034)	(0.044)
1984–86	−0.069	−0.116*	−0.024	−0.079	−0.001	−0.053	0.016	−0.033
	(0.057)	(0.065)	(0.067)	(0.060)	(0.063)	(0.047)	(0.051)	(0.047)
1987–89	−0.106***	−0.175***	−0.074*	−0.137**	−0.046	−0.101	0.051	0.015
	(0.036)	(0.064)	(0.039)	(0.064)	(0.042)	(0.067)	(0.053)	(0.074)
1990–92	0.019	−0.070	0.069	−0.041	0.105*	−0.004	0.162**	0.039
	(0.041)	(0.062)	(0.044)	(0.070)	(0.055)	(0.076)	(0.061)	(0.078)
N	75	75	75	75	75	75	75	75

Notes: Difference-in-difference regressions with pre-treatment period 1977–79. Year 1980 omitted since it includes both pre-treatment and post-treatment observations, as in Borjas (2017: table 5). Unit of observation is city-year. Dependent variable is log weekly real wage. Robust standard errors in parentheses. Sample is working non-Hispanic males, age 25–59, with less than high school. "HS" is high school. Card-preferred control cities are Atlanta, Los Angeles, Houston, and Tampa-St. Petersburg; Borjas-preferred control cities are Anaheim, Rochester, Nassau-Suffolk, and San Jose. "Nationwide" means coefficient on black indicator is constrained to take the same value for all cities and education levels; "by city" means coefficient on black indicator takes a unique value for all blacks in each city; "by city–less than HS" means coefficient on black indicator takes a unique value for blacks with less than high school in each city.

*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

1980 census brought on national political 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. Pressure was particularly high in Miami and included a lawsuit led by then-mayor Maurice Ferré who had joined leaders of 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."[14] The backdrop for these pressures was 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 and the Commission 1982).

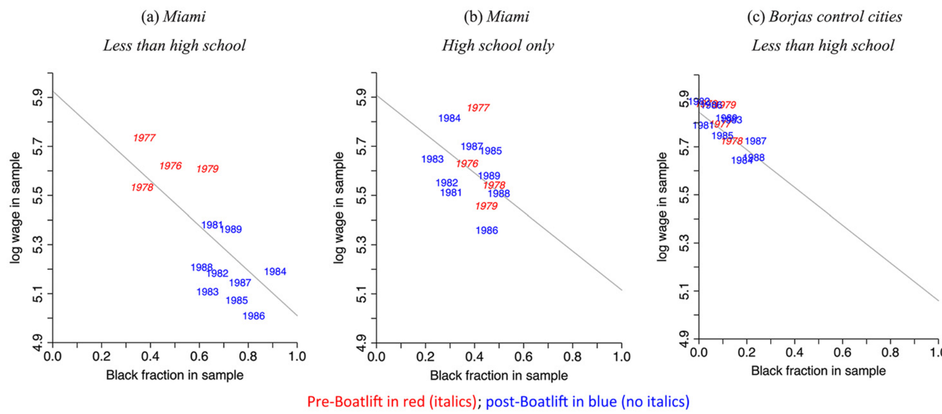
Many of the Levitan Commission's recommendations were implemented immediately in and after 1980 (Hamel and Tucker 1985), in the census and the CPS. These changes included additional census coverage samples to capture more low-income black residences and greater efforts by enumerators to identify all of the people residing in a visited residence (Brooks and Bailar 1978). Starting in the March 1981 CPS, 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 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. Because low-skill black men earned less than low-skill non-black men earned, this would reduce average wages of sampled blacks. The Online Appendix elaborates on the change in sampling procedures.

The above reason is reinforced by a third reason: Increases in coverage of low-skill blacks in the surveys tended to include, at the margin, lower-income blacks relative to previously covered blacks. Contemporary efforts to improve coverage among blacks in and after 1980 clearly focused on the poorest blacks (Levitan and the Commission 1979: 139; U.S. Senate 1981: 82–83; Durant and Jack 1993). Ethnographers at the time found that marginal blacks added through more extensive survey efforts tended 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, Hines, Martin, and Shapiro 1988: 514). Indirect evidence for negative selection of this kind is that within sampled workers with less than high school, years of education fell in Miami after 1980 relative to the control cities (see Online Appendix).[15]

[14]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).

[15]Another mechanism that could spuriously produce wage declines in Miami at this time, in principle, would be a sudden influx of US blacks into the city coincidentally occurring in 1980. But tabulations by the Census Bureau show no important change to the rate of increase of Miami's overall population of US blacks (at all skill levels) in the years after 1980 relative to the years before 1980 (data from the full census 1960–1980 in Bureau of the Census [1982: 22]; data from the CPS 1980–1985 in 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).

Figure 3. Changes in Black Fraction and Average Wage, March CPS



Notes: The line in each panel is a separate least-squares fit to the data points in that panel alone. All data from March CPS. Year shows the earnings year, that is, the year prior to the survey year (following Borjas 2017). Subfigure (a) shows the exact subsamples used by Borjas (2017): Miami employed male non-Hispanic workers age 25–59 with less than high school. Subfigure (b) shows Miami subsamples identical to (a) except for workers with high school only. Subfigure (c) shows subsamples identical to (a) except for the control cities preferred by Borjas (2017): Anaheim, Rochester, Nassau-Suffolk, and San Jose. All scatters omit the year 1980, to correspond to the regressions in Borjas (2017: table 5).

Testing for Spurious Wage Effects

Figure 3a illustrates how this compositional change in the samples coincides closely with large changes in the average wage of workers with less than high school. The figure plots the annual average wage for Miami workers in the Borjas sample against the fraction of the sample that is black in each year.[16] A very strong, negative association is seen between the black fraction and the average wage. The line in the figure shows a simple least-squares fit through all the data points. The doubling of the black fraction between the pre-Boatlift years and the post-Boatlift years is associated with approximately a 40% decline in the average wage (the log weekly wage falls from about 5.6 to 5.1, that is, from \$270 to \$164). We interpret this association as largely a causal relationship, since it is not plausibly coincidental. The change in the black fraction of the sample could not be a compositional effect of the Boatlift itself, since all samples in the figure omit Hispanic blacks.

Figure 3b shows a very similar negative relation between the wage and the share black for Miami workers with exactly high school. Yet there is no fall in the average wage over time because the share black is not increasing over time.

Figure 3c returns to the Borjas subpopulation of workers with less than high school and tests for compositional change for samples of those workers

[16] In Table 1 above—following Borjas (2017: table 3)—the "Survey Year" is the year in which the survey was conducted. Here, in Figure 3, the "Earnings Year" is the year in which reported earnings in the March CPS were earned: the year prior to the survey year—following Borjas (2017: table 5). The reason for this difference is that the March CPS asks workers about their earnings in the preceding year.

in the control cities preferred by Borjas. Also among these workers, we see neither a substantial change in the race composition of the samples around 1980 nor a substantial fall in the wage, despite a relationship between share black and the wage that is similar to Miami's. The graph likewise suggests that if a large change in the race composition of the samples in the control cities had occurred, it would have produced a large fall in the average wage. But no such change occurred in the control cities, as shown in the figure and in Table 1. In all three panels of the figure, the least-squares fit line shows a negative relationship between the fraction black and the average wage. But only in panel (a) does the fraction black jump between the pre- and post-Boatlift years (from 40–50% to 70–80%), and only in that panel does the wage drastically fall (by about 0.5 log points).

This evidence suggests that the wage effects of the Mariel Boatlift estimated by Borjas (2017) are severely biased and that a substantial portion of those estimates is spurious. The most straightforward approach to estimating the degree of the bias is to adapt the analysis to adjust for the share of blacks and compare with the original results. Borjas's (table 5) core results begin with yearly individual-level regressions (for workers of all education levels in Miami and the control cities) that adjust wages for age and city, and then average the adjusted wages for workers with less than high school by city and year. He then performs differences-in-differences regressions at the city-year level, dropping 1980 earnings because in both CPS extracts, 1980 is a mix of pre- and post-Boatlift data. The covariates are the interactions of a dummy for Miami and dummies for three-year time periods: Their coefficients would ideally be zero for years before the Boatlift (indicating that the controls were similar to Miami), and their coefficients for years after the Boatlift would indicate any effect of the Boatlift. Because the city-year averages are pre-adjusted by city and year, the resulting regressions run by Borjas test not for a difference-in-difference of the average wage level, as Borjas incorrectly states, but instead for a difference-in-difference of the *relative* wage of workers with less than high school (compared to the average worker at any other education level).

We adapt this procedure to include controls for black in the individual-level regressions. Table 2a shows the results for the March CPS data. The first two columns are an exact replication of the corresponding columns of Borjas (2017: table 5), with very large estimated treatment effects on wages relative to both the Card and the Borjas control cities. In columns (3) and (4), prior to averaging within city-period cells, we adjust the wage of each individual using a black indicator variable whose coefficient is constrained to take the same value for all cities, education levels, and ages, but is unique for each year. This change reduces the magnitude of the treatment effect by approximately one-third.

It is a strong assumption to constrain the black indicator coefficient to be identical for all cities and skill levels. Empirically, the black–non-black wage gap does vary by city and skill level (Table 3). Theoretically, there are important reasons the black–non-black wage gap in Miami would differ

Table 3. 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*
Dependent variable: log wage						
Black	−0.487***	−0.465***	−0.219***	−0.215***	−0.285***	−0.261***
	(0.0737)	(0.0728)	(0.0315)	(0.0321)	(0.0760)	(0.0770)
Dependent variable: Unemployed (0,1)						
Black	−0.00480	−0.00345	0.0688***	0.0513***	0.0528***	0.0515***
	(0.0320)	(0.0302)	(0.0158)	(0.0155)	(0.0122)	(0.0124)

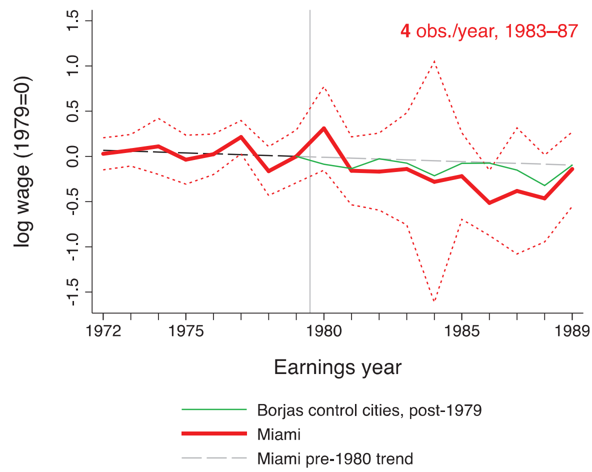
Notes: Regressor is an indicator variable equal to 1 for black, 0 otherwise. Coefficients are from a pooled OLS regression of each outcome on the black dummy and a constant term, and nothing else. Standard errors in parentheses. Weighted regressions use the March Supplement Weight. *Wage regressions:* 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. Unemployment regressions cannot be run using data from Borjas (2017) because that article considers only employed workers and thus tests for wage effects conditional on employment.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

from the control cities. In the 1980s, blacks constitute more than two-thirds of Miami's non-Hispanic males with less than high school, but less than 10% of the same group in the Borjas control cities (see Table 1). Cities with sharply different populations of low-skill blacks exhibit disparate patterns of geographic and occupational segregation that can shape the black–non-black wage gap. Thus, in columns (5) and (6) of Table 2a, we allow the coefficient on the black indicator to take unique values for each city, but still constrain it to take the same value for all education levels. The treatment effect is more heavily attenuated—by more than half of its original value—and is no longer statistically significant relative to the control cities preferred by Card.

In columns (7) and (8), the coefficient on the black indicator is allowed to take unique values for workers with less than high school in each city. The coefficient estimates in these columns therefore represent the effect on non-blacks with less than high school. Estimates in these columns have the disadvantage of controlling away any additional effect that the Mariel Boatlift might have had on the wages of blacks with less than high school compared to non-blacks, but they have the advantage of being unaffected by changes in the racial composition of the subsample with less than high school. In these columns the wage effects are neither economically nor statistically significant.

Table 2b repeats the above analysis using the May/ORG CPS extract—in which larger samples represent the same population. In these data, when we control for a black indicator that is specific to each city, but identical across education levels (columns (5) and (6)), there is no statistically significant treatment effect relative to either the Card or the Borjas control cities.

Figure 4. Miami, March CPS: Average Wage in Borjas Sample without Blacks



Notes: Thick, solid (red) line shows average wage in Miami, with dotted line showing 95% confidence interval for the annual mean, calculated using Supplement Weight. Dashed line shows post-1980 continuation of pre-1980 linear trend in Miami. Thin, solid (green) line shows average wage of same type of workers in the control cities preferred by Borjas (2017): Anaheim, Rochester, Nassau-Suffolk, and San Jose, calculated using Supplement Weight. March CPS data. "Earnings year" is the year prior to the survey year, following Borjas (2017). "Borjas sample" is non-Hispanic male workers age 25–59 with high school or less, who report positive annual wage and salary income, positive weeks worked, and positive usual hours worked weekly.

The same is true when the city-specific black indicator is allowed to take a unique coefficient for workers with less than high school (columns (7) and (8)).

The most direct way to test for wage effects in the absence of an increase in low-income blacks in the sample, in principle, would be to simply exclude blacks from the sample and repeat the analysis. But the already small size of the samples makes this approach impossible in practice. Figure 4 modifies Figure 2f to show average wages for non-blacks only in the March CPS extract. Extremely few observations remain—an average of only four observations per year in the years when Borjas finds the largest treatment effect (1983–1987, see Table 1)—and the confidence intervals are very large. At this point, more than 98% of the original sample of low-skill workers has been discarded, and statistical noise prevents any conclusion about Miami wage trends.

In sum, the core regressions of Borjas (2017) are fragile when adjusted to control for a city-specific black indicator. When this is done, there is only a statistically significant wage effect in the March CPS extract but not in the May/ORG, and even in the March extract, the significance of the effect depends critically on the choice of control cities. When the city-specific black indicator is allowed to vary by education group, the treatment effect is not significant at all. This finding suggests a high degree of bias in the original Borjas (2017) results. The analysis cannot be carried out separately for the subsample of blacks because that subsample is greatly contaminated by simultaneous, unrelated compositional change, and the analysis cannot be carried out separately for non-blacks because minuscule sample sizes prevent it. Overall, the evidence is compatible with a model in which the Mariel Boatlift caused a modest fall in the wages of this subpopulation of roughly −2% to −8% in the few years immediately after the Boatlift, but it is also compatible with a model in which this effect was zero.[17]

Reconciliation of Prior Findings

Prior studies of the Mariel Boatlift emphasize different subgroups. Card (1990) focused on workers with high school or less. Borjas (2017) focused on a small subset of those workers, non-Hispanic men with less than high school, except those under 25 or over 59. Peri and Yasenov (2018) focused on all non-Cubans with less than high school. Their sharply different results can be explained by large contemporaneous changes in the composition of the small subsample used by Borjas (2017), changes that were not substantial in the larger subsamples used by Card (1990) and by Peri and Yasenov (2018).

This can also explain several other features of these previous findings. 1) It can explain why Borjas (2017) and Peri and Yasenov (2018) found a wage effect approximately three times larger in the March CPS than in the May/ORG extract: The change in racial composition is about three times larger in the March CPS than in the May/ORG extract (see Table 1). 2) It can explain why all prior studies found no effect of the Mariel Boatlift on unemployment: There was no difference between black and non-black unemployment rates among male, non-Hispanic workers with less than high school in Miami, despite the large difference between black and non-black wages there (see Table 3), 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) persisted 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) found 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.

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

[17]The coefficient estimate in Table 2a, column (8), row 1981–83 is –8%. This value is the most negative coefficient estimate in columns (7) or (8) in either Table 2a or Table 2b. The corresponding coefficient in Table 2b is –2%. Several of the coefficient estimates in columns (7) and (8) of both tables are positive. None of the coefficients is statistically significant.

(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) sought 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 ran a series of regressions of the form

$$\Delta \log w_{rs} = \theta_r + \theta_s + \eta m_{rs} + \varepsilon_{rs}, \tag{1}$$

where w_{rs} is the wage or other labor market outcome for native workers with skill s in region r; θ_r and θ_s are region and skill fixed effects; $m_{rs} \equiv \frac{M_{rs1}}{L_{rs1}}$ is the size of the refugee supply shock relative to the native population of skill s in region r at time 1, where L_{rst} is the native population with skill s in region r at time t; time 1 is after the refugee influx, time 0 is before it; the coefficient η is to be estimated; and ε is an error term. In one of the reanalyses, r indexes occupations rather than geographic areas.[18] Because refugees' choice 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). Their samples include males only, though the original studies include women.

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, because of their common divisor.[19]

This problem, highlighted more recently by Kronmal (1993), affects instrumental variables as well as standard regression analysis (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 that framework could show spuriously that babies *cause* any regional outcome that is correlated with the number of women in the region.

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[18]This specification varies between the reanalyses. In the France reanalysis, for example, location fixed effects θ_r are omitted (see discussion below). 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: 397). Also in the France reanalysis, the labor market outcome is employment but not wage (wage is unavailable in the original data); in the Israel reanalysis it is wage but not employment. In the Israel reanalysis the index r is across occupations rather than regions, given Israel's small geographic extent. We run alternative forms of all regressions, controlling as well for the term $\eta \log \frac{L_{rs1}}{L_{rs0}}$, motivated by theory, but all results are substantively unaffected.

[19]See also inter alia Pendleton, Warren, and Chang (1979), Pendleton, Newman, and Marshall (1983), Jackson and Somers (1991), Wiseman (2009).

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 it does show 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 a half of instrumental variable results published in leading journals falsely rejecting the null due to their treatment of standard errors (Young 2017).

Kronmal (1993) proposed a specification correction for this problem in an Ordinary Least Squares (OLS) setting that we here adapt to the instrumental variables setting. The robustness test he proposed was to simply split the ratio variable into two separate variables, while accounting for the nonlinear relationship between numerator and denominator.[20] In the stork example, a regression of $\log\left(\frac{\text{babies}}{\text{woman}}\right)$ on $\log\left(\frac{\text{storks}}{\text{woman}}\right)$ will give a spurious coefficient, but a regression of log(babies) on both log(storks) and log(women) will give the correct positive coefficient on women and the correct null coefficient on storks. We modify that correction in one way: Because here the refugee shock variable frequently takes value zero (or occasionally a negative value), the log transformation would censor those observations, so we instead use the inverse hyperbolic sine transformation.[21] The results are invariant to the use of this transformation versus the more traditional $\ln(1 + x)$ transformation (see the Online Appendix).

We therefore modify the regression from Equation (1) with the Kronmal correction to

$$\Delta\log w_{rs} = \theta_r + \theta_s + \eta(\text{asinh}M_{rs1}) + \eta'(\text{asinh}L_{rs1}) + \varepsilon_{rs}, \tag{2}$$

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 ($\text{asinh}M_{rs0}$).

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[20]Kronmal (1993) considered a simple ratio and proposed controlling for the reciprocal of the denominator. In the present case the ratio is logged, so the equivalent is to control for the log of the denominator (which is equivalent to controlling for the log of the reciprocal of the denominator).

[21]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 $\frac{d}{dx} asinh x = \frac{1}{\sqrt{1+x^2}} \approx \frac{1}{x} = \frac{d}{dx} ln x, \forall x \gtrsim 2$. But unlike the log transformation, the inverse hyperbolic sine has desirable properties near zero and is defined at zero ($asinh\ 0 = 0$). See Burbidge, Magee, and Robb (1988) and MacKinnon and Magee (1990).

Table 4. Israel: Placebo Regressions, Borjas and Monras (2017) Model

(a) Israel: Mean 1983 Soviet stock by education, real vs. placebo

Education group	*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

Notes: 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 (2017: table 6, columns (3) and (4))

	Dependent variable: Δ *native wage*			
Lagged Soviet fraction IV:	*(1)* *Real*	*(2)* *Placebo*	*(3)* *Real*	*(4)* *Placebo*
Émigré supply shock/population	−0.616*	−0.820***	−0.611*	−0.873*
	(0.316)	(0.315)	(0.334)	(0.473)
Change in native population			−0.00352	0.0229
			(0.0707)	(0.0976)
N	32	32	32	32
Adjusted R^2	0.286	0.289	0.258	0.257
Kleibergen-Paap F	27.37	5.059	23.19	3.728
p value Anderson-Rubin F-test	0.0985	0.0272	0.113	0.0880

Notes: Robust standard errors in parentheses. Instrument in each column: (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.
$*p < 0.10$; $**\ p < 0.05$; $***\ p < 0.01$.

Israel Reanalysis

Friedberg (2001) studied a large and sudden influx of Soviet refugees to Israel between 1990 and 1994—an influx large enough to raise Israel's population by 12%. She used information on migrants' former occupations in their home countries to construct an instrument for the occupations they take in Israel and found "no adverse impact of immigration on native outcomes" within occupations (p. 1373). Borjas and Monras (2017) reanalyzed the episode using instead the Altonji and Card instrument based on prior migration flows into education-occupation cells inside Israel and instead found large detrimental effects of the migration on Israel natives' wages.

Table 4 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 4a. 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 $\tilde{m}_{rs0}$ contains no information about prior migration flows.

Second, we replicate the Borjas and Monras (2017: table 1, columns (1), (3)) reanalysis of the Israel case. In Table 4b, 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. 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.[22]

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 outcome 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).

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 5 shows that the second-stage coefficient on the émigré supply shock is statistically indistinguishable from zero under the Kronmal correction. Column (1) of the table precisely replicates the original result in Borjas and Monras (2017: table 6), as in Table 4b. Column (2) shows that the result is nearly identical when the ratio measure of the supply shock undergoes the inverse hyperbolic sine transformation. Column (3) shows the results of estimating Equation (2), 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.[23] Instrumentation remains very strong in column (3), with a Kleibergen-Papp (2006) F statistic greater than 14, but the weak-instrument robust Anderson-Rubin (1949)

[22]The Anderson and 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).

[23]This adjustment 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 Online Appendix for proof. The results are substantively unchanged when controlling for log $\frac{L_{rs1}}{L_{rs0}}$, as Borjas and Monras (2017) did in some specifications.

Table 5. Israel: Kronmal Specification Correction to Borjas and Monras

	(1)	(2)	(3)	(4)
Dependent variable:		*Δ native wage*		*asinh émigré supply shock*
Estimator:		*2SLS*		*OLS*
Covariates				
Émigré supply shock/population	−0.616*			
	(0.316)			
asinh émigré supply shock/population		−0.642**		
		(0.325)		
asinh émigré supply shock			−0.0348	
			(0.0443)	
			[−0.284]	
asinh total population			0.0426	1.154***
			(0.0443)	(0.196)
N	32	32	32	32
Adjusted R^2	0.286	0.297	0.156	0.717
Kleibergen-Paap *F*	27.37	31.27	14.41	
p value Anderson-Rubin *F*-test	0.0985	0.0995	0.548	

Notes: 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). Instrument in each column: (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.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

F-test fails by a wide margin to reject the hypothesis that the second-stage coefficient on the refugee shock is zero.[24]

Column (4) of Table 5 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 second-stage coefficient that rejects the null of zero. When this possibility is eliminated by the Kronmal (1993) correction, in column (3) of Table 5, 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

[24]This is the proper test for the present just-identified setting of one endogenous variable and a single instrument.

Table 6. Israel: Placebo Regressions, Friedberg (2001) Model

(a) Israel: Mean pre-migration stock per occupation, by education: real vs. placebo

Years of education	*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

Notes: 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 3, row (4))

Dependent variable: Δ native wage		
Lagged Soviet fraction IV:	*(1)* *Real*	*(2)* *Placebo*
Émigré supply shock/population (‘r’)	0.718*	0.402
	(0.339)	(0.807)
N	8,353	8,353
Adjusted R^2	0.520	0.523
Kleibergen-Paap F	42.23	3.570
p value Anderson-Rubin F-test	0.0195	0.594

Notes: 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 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 divided by the number of Israelis in the cell in 1989. Robust standard errors in parentheses. All specifications include education and occupation fixed effects.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Tables 6 and 7, 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 6a shows the means, by education group, of the Poisson-distributed white noise generated to replace the numerator of the instrument.

In Table 6b, 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

Table 7. Israel: Kronmal Specification Correction to Friedberg

	(1)	(2)	(3)	(4)
Dependent variable:		*Δ native wage*		*asinh émigré in cell, 1994*
Estimator:		*2SLS*		*OLS*
Covariates				
Émigré supply shock/population ('*r*')	0.718**			
	(0.339)			
asinh émigré supply shock/population		0.742**		
		(0.358)		
asinh émigrés in cell, 1994			0.0780	
			(0.0666)	
			[0.572]	
asinh Israelis in cell, 1994			−0.0531	
			(0.0529)	
asinh Israelis in cell, 1989				0.629***
				(0.00233)
N	8,353	8,353	8,353	8,353
Adjusted R^2	0.520	0.520	0.519	0.897
Kleibergen-Paap *F*	42.23	38.37	5.819	
p value Anderson-Rubin *F*-test	0.0195	0.0211	0.115	

Notes: 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). The émigré supply shock r in the original study is Soviet émigrés in 1994, per Israeli in 1994 in each skill-occupation cell. Instrument in each column: (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.
$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

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 7. There, as in Table 5, column (1) replicates the original result and column (2) shows that it is unchanged under the inverse hyperbolic sine transformation of the immigration shock ratio. Column (3) 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 2001 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.[25]

This outcome 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."

France Reanalysis

Hunt (1992) studied 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 approximately 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 found 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 found a 1 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 1% due to the repatriates (see the Online Appendix). Hunt did 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) reanalyzed the impact of the repatriates and attempted to analyze for the first time the effect of the Harkis. Whereas Hunt (1992) had access to data at only the department level, Borjas and Monras (2017: 5) used individual-level data. They found that a 1% 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 2017: table 10, columns (3) and (4)). Although the effect estimated is conceptually slightly different (see the Summary section below), 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.[26]

[25] This is calculated, as above, by: $0.0780 \cdot \frac{1-0.12}{0.12} = 0.572$.

[26] This similarity may be coincidental, however, since the data used by Borjas and Monras (2017) identified 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.

Borjas and Monras also found that a 1% 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 (2017: table 10, columns (3) and (4)). This result is almost three times the effect they found 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 emphasized 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 of the authors' sample of 160,000 Algerians is actually Harkis, since they count 84,000 Harkis as repatriates, with the remainder being economic migrants from Algeria.[27]

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 articles, beginning with Borjas and Monras. We first repeat the reanalysis of the Israel Reanalysis above, focusing on the Algerian nationals. In Table 8a 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 8b shows that the detrimental effects estimated are larger using the placebo instrument $\tilde{m}_{rs0}$, as in the Israel case. Column (1) 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 column (2), we replace the true Algerian prior stock instrument with the placebo: The coefficient estimate rises to 0.42 and retains high statistical significance.[28]

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. That is, the original measured effect from Algerian nationals does not depend on controlling for the repatriate shock. Note that the first-stage F-statistic is very high, suggesting the correlation between the instrument and the endogenous variable is based on an artefact. 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

[27]Economic migrants are described by, for example, Roux (1991: 255).

[28]The standard error on the placebo instrument of column (2) is much higher than in the Borjas and Monras original, but the weak instrument-robust Anderson-Rubin F-test strongly rejects the hypothesis that the second-stage coefficient is zero.

Table 8. France: Placebo Regressions, Borjas and Monras (2017) Model

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

Education group	*Real*	*Placebo*
Less than primary completed	8020.0	8032.8
Primary completed	263.6	263.0
Secondary completed	83.6	81.8
University completed	30.3	29.4

Notes: 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, column (3))

Dependent variable: Δ native unemployment

Lagged Algerian fraction IV:	*(1)* *Real*	*(2)* *Placebo*	*(3)* *Real*	*(4)* *Placebo*	*(5)* *Placebo*
Repatriate supply shock/population	0.0887**	0.04888			
	(0.0384)	(0.0502)			
Algerian supply shock/population	0.247***	0.419***	0.282***	0.437***	0.443***
	(0.0667)	(0.126)	(0.0669)	(0.117)	(0.118)
Change in native population					0.00279
					(0.0123)
N	88	88	88	88	88
Adjusted 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 value Anderson-Rubin F-test	0.000122	0.0000466	0.000542	0.00351	0.00382

Notes: Robust standard errors in parentheses. Instrument set in each column: (1) 1962 repatriate 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.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Israel, therefore, the strategy for identifying the causal relationship between the refugee inflow and native labor market outcomes is potentially flawed.

In Table 9, we carry out the Kronmal specification correction using Equation (2) just as was done for Israel above. Column (1) precisely replicates the core result using the original analytic methods for Algerian nationals in isolation (Table 8b, column (3)). Column (2) shows that this finding is identical when the ratio measure of the migrant shock (and its instrument) undergoes 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 (1993)-corrected equivalent of Table 8b, column (1). The coefficient on the Algerian shock is positive and statistically significant but falls to 0.0018; this implies that a 1% increase in the population due to the Algerians raised unemployment by about 0.23 percentage point.[29] We convert this to a value comparable to the Borjas and Monras (2017) coefficient, shown in square brackets: If Algerians increase the population by 1%, unemployment rises 0.23 percentage point.[30] The adjusted coefficient for repatriates is also similar to the Borjas and Monras (2017) specification estimate of 0.04.

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 9 imply that if Algerians increase the population by 1%, 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 1% increase in population due to repatriates raised native unemployment by 0.09 percentage point, though the latter 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 used 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. In addition to this instrument, however, 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 1992: table 3, column (4)) in our Table 10, column (1).[31] An obvious robustness check is simply to drop the lagged migration instrument and instrument with temperature alone, as 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

[29] As above, this is calculated as $0.00182 \cdot \frac{1-0.008}{0.008} = 0.226$.

[30] 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 ($\text{asinh}L_{rs1}$). Of the variance in the refugee shock, 81% is explained by the size of the cell.

[31] This estimation uses robust standard errors, which Hunt did not.

Table 9. France: Kronmal Specification Correction to Borjas and Monras

	(1)	(2)	(3)	(4)	(5)
Dependent variable:			*Δ native unemployment*		*asinh Algerian supply stock*
Estimator:			*2SLS*		*OLS*
Covariates					
Algerian supply shock/population	0.282***				
	(0.0669)				
asinh Algerian supply shock/population		0.282***			
		(0.0669)			
asinh Algerian supply shock			0.00234***	0.00182**	
			(0.000836)	(0.000904)	
				[0.226]	
asinh repatriate supply shock				0.00151	
				(0.00249)	
				[0.044]	
asinh total native population			−0.00172	−0.00228	2.193***
			(0.00193)	(0.00226)	(0.141)
N	88	88	88	88	88
Adjusted R^2	0.432	0.432	0.301	0.344	0.808
Kleibergen-Paap F	247.7	248.2	42.44	13.82	
p value Anderson-Rubin F-test	0.000542	0.000543	0.0147	0.0154	

Notes: 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). Instruments in each column: (1) 1962 Algerian stock/population; (2) *asinh* of 1962 Algerian stock/population; (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.
$*p < 0.10$; $**p < 0.05$; $***p < 0.01$.

hyperbolic sine of both the repatriate share and its instrument and 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 1%, the unemployment rate increases by 0.16 percentage point. This Kronmal (1993)-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.[32]

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

Table 10. France: Re-Estimation of Hunt (1992) with Alternative Instrument

	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
Dependent variable:		*Δ native unemployment*				*asinh of Repatriates supply stock*
Estimator:		*2SLS*				*OLS*
Covariates						
Repatriate share	0.195**	0.120	0.209***	—	—	—
1968 labor force		(0.096)	(0.076)			
	(0.080)	[0.116]	[0.202]			
	[0.189]					
asinh repatriate share	—	—	—	0.209***	—	—
1968 labor force				(0.076)		
				[0.202]		
asinh number of repatriates 1968	—	—	—	—	0.00254***	—
					(0.00117)	
					[0.156]	
asinh size of labor force 1968	—	—	—	—	−0.00342	—
					(0.00207)	
asinh size of labor force 1962	—	—	—	—	—	1.056***
						(0.041)
N	88	88	88	88	88	88
Adjusted R^2	0.78	0.79	0.78	0.78	0.78	0.80
Other covariates	Yes	Yes	Yes	Yes	Yes	—
Instruments for repatriates						
Temperature	Yes	Yes	—	—	—	—
1962 repatriate share	Yes	—	Yes	—	—	—
asinh 1962 repatriate share	—	—	—	Yes	—	—
asinh 1962 repatriate number	—	—	—	—	Yes	—

Notes: 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, column (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. 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; weights in column (6) are the 1962 labor force.
$^*p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

Europe Reanalysis

Angrist and Kugler (2003) studied the effects of an influx of refugees from the Balkan War on 18 European countries during the 1990s. They found that a sudden increase in the migrant stock of one percentage point raises native unemployment by 0.83 percentage point. Borjas and Monras (2017) reanalyzed the episode in seven of those European countries (Austria, Greece, Ireland, Portugal, Romania, Spain, and Switzerland), again using the prior-migration instrument. They likewise found a detrimental effect on

Table 11. Europe: Placebo Regressions, Borjas and Monras (2017) Model

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

Country	*Real*	*Placebo*
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

Notes: 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, columns (3) and (4)

Dependent variable: Δ native unemployment				
Lagged Balkan fraction IV:	*(1)* *Real*	*(2)* *Placebo*	*(3)* *Real*	*(4)* *Placebo*
Balkan supply shock/population	0.456	0.583*	0.487	0.657
	(0.311)	(0.323)	(0.376)	(0.510)
Change in native population			−0.00266	−0.00426
			(0.0165)	(0.0181)
N	195	195	195	195
Adjusted R^2	0.741	0.740	0.739	0.737
Kleibergen-Paap F	17.72	6.189	16.34	5.219
p value Anderson-Rubin F-test	0.122	0.0204	0.187	0.149

Notes: Robust standard errors in parentheses. Instrument in each column: (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.
*$p < 0.10$; ** $p < 0.05$; ***$p < 0.01$.

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, little disagreement arises between the original study by Angrist and Kugler (2003) and the reanalysis by Borjas and Monras (2017). Both found that the refugee wave they studied substantially displaced natives in the labor market, though estimates in both are statistically imprecise. Angrist and Kugler (2003: F328) warned of identification problems and statistical imprecision in their instrumental variables estimates and recommended interpreting those estimates as an upper bound on the true effect. The instrumental variable estimates of Borjas and Monras (2017) for unemployment were statistically insignificant even at the 10% level.

Nevertheless, we proceed with the same placebo test as above to test the robustness of these findings. Table 11a shows the means of the placebo numerator $\tilde{M}_{rs0}$ by country. Table 11b replicates the core result in Borjas

Table 12. Europe: Kronmal Specification Correction to Borjas and Monras

	(1)	(2)	(3)	(4)
Dependent variable:		*Δ native unemployment*		*asinh Balkan supply shock*
Estimator:		*2SLS*		*OLS*
Covariates				
Balkan supply shock/population	0.456			
	(0.311)			
asinh Balkan supply shock/population		0.459		
		(0.314)		
asinh Balkan supply shock			−0.0132	
			(0.0119)	
			[−0.26]	
asinh total population (without Balkan)			−0.00992	0.955***
			(0.0125)	(0.201)
N	195	195	195	195
Adjusted R^2	0.741	0.741	0.339	0.509
Kleibergen-Paap F	17.72	17.31	1.498	
p value Anderson-Rubin F-test	0.122	0.122	0.0209	

Notes: 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). Instrument in each column: (1) 1990 Balkan fraction; (2) *asinh* of 1990 Balkan fraction; (3) *asinh* of 1990 Balkan stock; (4) none. "*asinh* total population without Balkan" means *asinh* of 2000 total population minus Balkan inflow 1990–2000. All specifications include education and country fixed effects.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

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 12 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 changes sign to become negative and statistically insignificant, with a magnitude (adjusted in square brackets, as above, to be comparable to the column (1) coefficient) of −0.26.[33] 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-state coefficient and failing to show that the Balkan supply shock raised unemployment in refugees' destination regions.

[33]Calculated as above by: $-0.0132 \cdot \frac{1-0.05}{0.05} = -0.26$.

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 found, "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.

The Mariel Boatlift Again

Borjas and Monras (2017) also revisited 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 that were used in Equation (1). They concur with Card (1990) that the Mariel Boatlift had no detrimental impact on native employment. But like Borjas (2017), they found 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 13 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 (column (3)) or without the less than high school skill group in any city (column (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)—the core problems of subsample sensitivity and compositional changes discussed early in this article equally explain 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 13 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

Table 13. Miami: Sensitivity Analysis for Borjas and Monras (2017)

	Dependent variable: Δ log weekly wage					
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
					Only Miami and other cities with wage data on Hispanics	
City-skill cells:	*Original*	*Omit less than HS in Miami only*	*Omit Miami, all skill levels*	*Omit less than HS, all cities*		
Workers:	*Original*	*Original*	*Original*	*Original*	*Original*	*Hispanic only*
Material supply shock/population	−1.263***	2.897**	5.049	3.527***	−1.338***	0.0507
	(0.320)	(1.302)	(5.289)	(1.244)	(0.305)	(0.135)
N	152	151	148	114	110	110
Adjusted 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 value Anderson-Rubin F-test	0.00325	0.0714	0.412	0.0227	0.00219	0.762

Notes: Column (1) of this table is an exact replication of Borjas and Monras (2017: table 3, column (3)). Robust standard errors in parentheses. 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 column (6) above this is changed to *Hispanic* male workers age 25–59. "HS," high school.
*$p < 0.10$; **$p < 0.05$; ***$p < 0.01$.

Table 14. Comparison of Coefficients

Coefficients	*(1)* *Miami wage*	*(2)* *Israel wage*	*(3)* *France unemployment*	*(4)* *France "Algerians" unemployment*	*(5)* *Europe unemployment*
A. Kronmal coefficients					
Borjas and Monras	0.00066	−0.035	0.0015	0.0018**	−0.0132
Friedberg	—	0.078	—	—	—
Hunt (no temperature instrument)	—	—	0.0025**	—	—
B. Borjas and Monras (BM) (2017)	−1.26**	−0.62*	0.09**	0.25**	0.46
C. Coefficients comparable to BM					
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	—	—
D. Comparable to BM, immigrant share = 0.1					
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.
$^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$.

has a p value of 0.76. We perform the placebo and Kronmal analysis for this case in the Online Appendix.

Summary: Comparing Coefficient Estimates in the Various Studies

Table 14 collects the key coefficient estimates from reanalysis of the four studies in comparable terms.[34] Panel A of the table shows raw, untransformed, Kronmal-corrected coefficients from each study (rows) and each historical episode (columns). Panel B shows the corresponding coefficient from Borjas and Monras. Panel C shows the Kronmal-corrected coefficients transformed to units comparable to the Borjas and Monras estimates in

[34]The Online Appendix explains how we adjust the coefficient estimates for comparability.

panel B. Finally, panel D calculates the estimated impact of an immigrant inflow share of 0.1, using the coefficient estimates in panel C.

The corrected, comparable coefficient estimates in panel C of Table 14 are mostly statistically insignificant, and many have a different sign than those in panel B. The important exception is the case of France, which robustly shows that a 1% rise in population due to refugees caused a 0.2 percentage point rise in unemployment (columns (3)–(4)). The cases of Miami and Europe show no deleterious effects; the case of Israel shows that at worst a 1% rise in population due to refugees reduced wages by 0.3% but may also have raised wages by 0.5–0.7%. The Kronmal correction affects the estimates most when serial correlation in the denominators (L_{rs0}, L_{rs1}) is highest relative to serial correlation in the numerators (M_{rs0}, M_{rs1}) and when variance of each denominator in cross section is largest relative to the variance of the numerator, as would be expected.[35]

Conclusions

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.

The discrepancy between Card's (1990) and Borjas's (2017) analyses of the 1980 Mariel Boatlift can be explained by a simultaneous large increase in the share of blacks in the small subgroup of Miami workers of concern to Borjas. The fraction of blacks is much higher in the post- than in the pre-Boatlift years in Borjas's Miami sample of prime-age, male, non-Hispanic workers with less than high school, whereas no such difference is observed in Card's Miami sample of non-Cuban workers with high school or less, nor in the control cities favored by either Card or Borjas. This compositional change offers an explanation for the finding of Peri and Yasenov (2018), that the Borjas result is sensitive to selecting a small subset of workers. Possible reasons for the compositional change include the 1980 arrival of black Haitians with less than high school and improved survey coverage of low-wage black males already in the United States.

Because both Haitian blacks and US blacks had lower wages than other workers with less than high school, this compositional change tends to produce a spurious fall in average wages for workers with less than high school. Our reanalysis of Borjas's sample with an adjustment for the share of blacks yields results similar to those of Card (1990) and Peri and Yasenov (2018):

[35]This analysis is presented in the Online Appendix. We thank an anonymous referee for suggesting the exercise.

Little to no wage impact of the Mariel Boatlift is discernable. The change in share of blacks can also explain other features of the recent reanalyses. For example, both Borjas (2017) and Peri and Yasenov (2018) estimated wage effects of the Boatlift using two different extracts of the Current Population Survey (CPS). They found wage declines roughly three times larger in one extract than in the other extract. The post-Boatlift rise in the black fraction of the survey subsample is, likewise, about three times larger in one extract than in the other.

We also show that understanding a particular type of spurious correlation between endogenous variable and instrument reconciles the findings in the cases of the Soviet refugees in Israel, the French repatriates from Algeria, and the Balkan refugees in Europe. The instrument used by Borjas and Monras (2017), with which they found larger harmful effects on native workers than were found in some of the original studies, gives results that can be reproduced with a placebo instrument. The Borjas and Monras (2017) instrument rests on the attraction of new migrants to the locations of prior migrant inflows (Altonji and Card 1991); the placebo instrument is merely white noise, but gives similar results. This similarity is a consequence of spurious correlation between the instrument and the endogenous variable introduced by applying a common divisor to both (Bazzi and Clemens 2013). We propose a specification that avoids spurious correlation.

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. Thus, the correct specification yields results substantially the same as in the original instrumental-variables 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.

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. Evidence does not support claims of large detrimental impacts on workers with less than high school education.

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