THE WAGE IMPACT OF THE *MARIELITOS*: THE ROLE OF RACE

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The author's 2017 reappraisal of the impact of the Mariel supply shock revealed that the wage of low-skill workers declined in post-Mariel Miami. Clemens and Hunt (2019) assert that a data quirk in the March CPS—specifically, a substantial increase in the black share of Miami's low-skill workforce in the period—implies that those wage trends do not correctly measure the impact of the *Marielitos*. Because blacks earn less than whites earn, the increased black share would spuriously reduce the average low-skill wage in Miami. The author examines the sensitivity of the evidence to the change in the racial composition of the sample. The Clemens and Hunt assertion is demonstrably false. The timing of the post-Mariel decline in Miami's wage does not coincide with the increase in the black share. And sensible adjustments for racial composition do not change the finding that Miami's low-skill wage fell after 1980.

Since its public release in September 2015, my reappraisal of the wage impact of the Mariel supply shock (Borjas 2017) has attracted considerable attention. My study showed that the long-believed conjecture that the *Marielitos* did not have a wage impact on Miami's workers, first reported in Card (1990), did not truly represent what the data actually showed. Instead, my reappraisal indicated that the wage of Miami's high school dropouts, the group most affected by the low-skill *Marielitos*, declined substantially after 1980 but had fully recovered by 1990.

Perhaps not surprisingly, my reappraisal inspired a number of re-examinations and critiques. Peri and Yasenov (2015), for example, argued that my results were sensitive to sample selection. I examined wage trends in the subsample of non-Hispanic men aged 25 to 59 who did not have a high school diploma. Peri and Yasenov argued that the sample should

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include women, non-Cuban Hispanics, both younger and older workers, and even high school graduates.

The sample selection issue is indeed important. For instance, the critique by Clemens and Hunt (2019: 5) in this *Review* states: "Here we define 'low-skill' as workers with high school or less, the canonical definition in the labor literature." Although the pooled group of high school dropouts and high school graduates forms the "high-school-equivalent" sample that is "canonical" in the wage structure literature, it is far from clear that this is the population we should look at to document the impact of a supply shock that was predominantly composed of workers without a high school diploma. After all, the so-called canonical definition of "high-skill" in the wage structure literature is the group of workers with at least some college education. And this surely would not be the group we would look at to document the impact of, say, a supply shock of Soviet mathematicians (Borjas and Doran 2012) or a supply shock of H-1B visa holders (Kerr and Lincoln 2010). Put bluntly, the sample selection issue cannot be resolved by blindly adopting conventions that may be useful in some contexts but that may mask the impact of supply shocks that affect very specific groups of native workers.[1]

This rejoinder, instead, focuses on the cornerstone of the Clemens–Hunt critique of my 2017 *ILR Review* article, a claim that is refutable by looking at the raw data. They write:

> 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. (Clemens and Hunt 2019: 8; emphasis added)

In other words, there is a sampling problem in the March Current Population Survey (CPS) that can easily contaminate the pre- and post-Mariel wage comparison in Miami. There was a substantial increase in the black share of the low-skill workforce in Miami in the relevant period. And because African American men earn less, the increase in the black share spuriously produced a drop in the average low-skill wage in post-Mariel Miami—a wage drop that Clemens and Hunt posit I incorrectly attributed to the Mariel supply shock.[2]

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[1]The sensitivity of my original results to sample selection suggests that it may be instructive to look for alternative measures of labor market conditions in pre- and post-Mariel Miami. Anastasopoulos, Borjas, Cook, and Lechanski (2018) took a first step in that direction. They examined the number of help-wanted ads published in the *Miami Herald* and found a sizable relative decline in the number of classifieds after the supply shock.

[2]This current article focuses on the material presented in the first half of the Clemens and Hunt (2019) article, which critiques the analysis in my Mariel paper published in the *Review* in October 2017. The second half of the Clemens–Hunt critique addresses issues related to the instrumental variable used in Borjas and Monras (2017) in the context of both Mariel and other refugee supply shocks. Curiously, Clemens and Hunt do not cite the important work of Jaeger, Ruist, and Stuhler (2018), which questions the validity of the often-used lagged-immigrant-share instrument and raises doubts about *all* of the IV estimates reported in the literature.

This current article examines the sensitivity of the Mariel evidence reported in Borjas (2017) to the change in the racial composition of the low-skill CPS sample in Miami. The evidence reported below uses the exact same sample examined in my earlier article: non-Hispanic men aged 25 to 59 who do not have a high school diploma and who live in one of the 44 metropolitan areas identified by the CPS at the time. Paying closer attention to the timing of the rise in the black share and the trend in the relative wage of Miami's low-skill workers, however, provides a very different perspective on the empirical relevance of the Clemens–Hunt assertion.

The Change in the Racial Composition of the Low-Skill Workforce

A very simple and intuitive method documents that the abrupt rise in the relative number of black workers sampled by the March CPS cannot be the reason for the average wage drop observed among low-skill workers in post-Mariel Miami. Contrary to the Clemens–Hunt assertion, the timing of the two events simply does not coincide.

As in Borjas (2017), the dependent variable is the age-adjusted log weekly wage (in the March CPS) and the age-adjusted hourly wage (in the CPS Outgoing Rotation Group [CPS-ORG]). The age-adjusted wage was calculated as the residual from a regression estimated in each CPS cross section:

$$\log w_{it} = \alpha_t + \beta_t A_{it} + \lambda_t C_{it} + \varepsilon_{it}, \qquad (1)$$

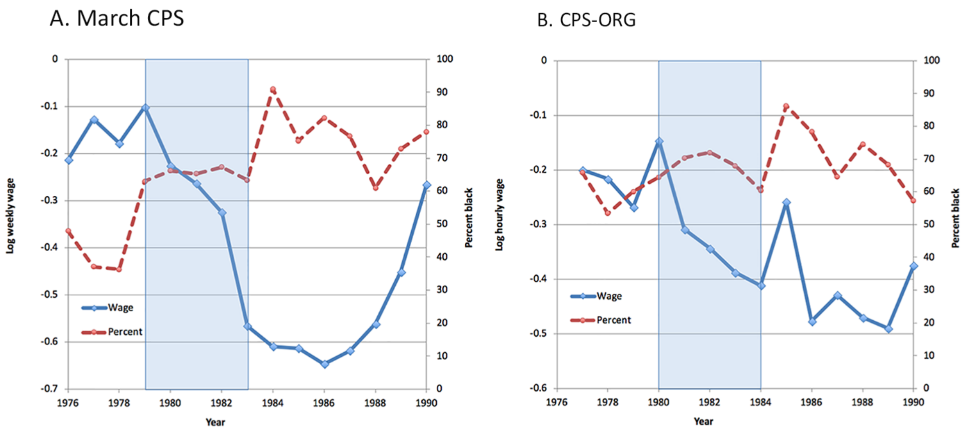
where w_{it} is the wage of worker i in cross-section t; A_{it} is a vector of fixed effects indicating the worker's age; C_{it} is a vector of fixed effects indicating the city of residence. The average of the residuals in a city-education group estimates the age-adjusted wage for the particular cell.[3]

Figure 1, panel A, shows the year-by-year trend in the age-adjusted wage (left y axis) for high school dropouts in the March CPS between 1976 and 1990, and panel B shows the respective trend in the CPS-ORG. The shaded area highlights the post-Mariel time frame when the wage of low-skill workers fell most. The figures also plot the fraction of the workers in the sample who are black (right y axis). Table 1 reports the raw data underlying these figures to allow readers to further examine and investigate the potential link between race and the trend in Miami's low-skill wage.

One crucial property of the March CPS data is worth emphasizing, as it affects the very core of the Clemens–Hunt critique: The March CPS reports earnings for the *previous* calendar year. As a result, the statistics reported in Table 1 for calendar year x refer to data drawn from the survey in year $x + 1$. Hence, the March data reported for, say, 1979 are the earnings reported in the 1980 survey *and* the racial composition of the sample used

[3] The average wage in a cell is a weighted average of the residuals for which the weight is the sampling weight times the number of weeks worked in a year (in the March CPS) or the number of usual hours worked weekly (in the CPS-ORG).

Figure 1. Year-to-Year Trends in the Age-Adjusted Wage and Racial Composition of High School Dropouts in Miami



Notes: The March CPS statistics for calendar year x refer to data drawn from the survey in year $x + 1$ (which reports the earnings in the previous calendar year and the racial composition of the sample used to calculate those earnings). The age-adjusted wage for a given year is the weighted average of the residuals in the sample of high school dropouts in Miami from the regression in Equation (1). The weight is the product of the CPS sampling weight $\times$ weeks worked (in the March CPS) or usual hours worked weekly (in the CPS-ORG). The "percent black" variable gives the weighted fraction of black workers in the sample used to calculate the wage for a particular calendar year and uses the CPS sampling weight. Wage = solid (blue) lines; percent = dashed (red) lines.

to calculate those earnings.[4] Clemens and Hunt (2019: table 1, p. 9) also report the identical data for the black share in Miami. Note, however, that their presentation of the data leaves the misleading impression that the rise in the black share is associated with the year 1980, rather than with the earnings measured in calendar year 1979.

Something peculiar happened to the black share in the March CPS in *calendar year* 1979. The black share rose from 36.3% in 1978 to 63.0% in 1979. It is improbable that this shift reflects what actually happened in Miami's labor market. In fact, the CPS-ORG does not report such a large increase in the black share, as the ORG black share was already high prior to 1979. As a result, the jump in the March statistic likely reflects a sampling issue, an imputation problem, or an error in the construction and manipulation of the data by the Bureau of Labor Statistics (BLS) or the Integrated Public Use Microdata Series (IPUMS).[5]

Note, however, that the black share in the March CPS was stable between calendar years 1979 and 1983. These are the years that witnessed the largest

[4]This sampling peculiarity does not affect the CPS-ORG data, as the calendar year and the survey year in which the wage and black share are measured are identical.

[5]The 1980 census, conducted on April 1, 1980, just weeks before the Mariel supply shock, reports that the black share in the sample of low-skill, non-Hispanic working men aged 25 to 54 was 54.5%. This evidence is consistent with the data from the 1980 March CPS and the corresponding data from the CPS-ORG. The consistency suggests that the problem may lie with a systematic undercounting or misallocation of low-skill black workers in the March CPS *prior* to the 1980 survey year.

Table 1. Age-Adjusted Wage and Racial Composition of High School Dropouts in Miami Labor Market, 1976–1990

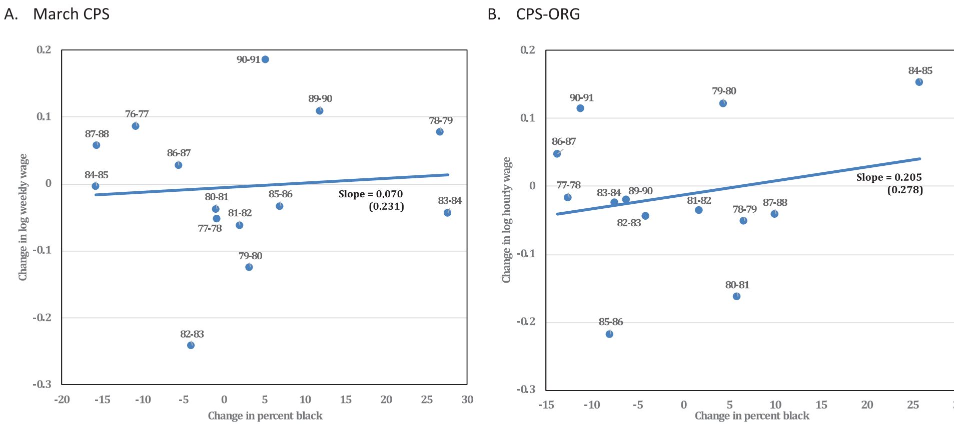
	March CPS			*CPS-ORG*		
Calendar year	*Log wage*	*Percentage black*	*Sample size*	*Log wage*	*Percentage black*	*Sample size*
1976	−0.213	47.9	23	—	—	—
1977	−0.127	37.2	26	−0.200	66.0	16
1978	−0.179	36.3	22	−0.217	53.4	12
1979	−0.101	63.0	17	−0.268	60.0	56
1980	−0.226	66.3	18	−0.146	64.4	55
1981	−0.263	65.3	20	−0.309	70.3	51
1982	−0.325	67.3	27	−0.344	72.0	39
1983	−0.566	63.4	18	−0.388	67.9	50
1984	−0.609	91.0	16	−0.411	60.4	48
1985	−0.613	75.3	15	−0.259	86.2	26
1986	−0.646	82.2	16	−0.476	78.3	36
1987	−0.618	76.7	18	−0.429	64.6	46
1988	−0.561	61.0	17	−0.470	74.5	37
1989	−0.451	72.8	16	−0.490	68.3	37
1990	−0.265	78.1	4	−0.375	57.2	38

Notes: March CPS statistics for calendar year x refer to data drawn from the survey in year $x + 1$ (which reports the earnings in the previous calendar year and the racial composition of the sample used to calculate those earnings). The age-adjusted wage for a given year is the weighted average of the residuals in the sample of high school dropouts in Miami from the regression in Equation (1). The weight is the product of the CPS sampling weight times weeks worked (in the March CPS) or usual hours worked weekly (in the CPS-ORG). The "percentage black" variable gives the weighted fraction of black workers in the sample used to calculate the wage for a particular calendar year and uses the CPS sampling weight.

drop in Miami's low-skill wage. To emphasize: The wage trend in Figure 1 is the wage trend reported in the Borjas (2017) analysis—*it includes blacks.* The figure unequivocally shows *no relationship whatsoever* between the average low-skill wage and the black share in the March CPS sample used to calculate that wage in the post-Mariel years, when the wage dropped dramatically. In other words, it is incorrect to explain the steep wage drop reported in Borjas (2017) in terms of a statistical spuriousness created by a rising black share in the low-skill workforce between calendar years 1978 and 1979.

In short, the Clemens–Hunt assertion is simply not consistent with the *timing* of the increase in the black share and the drop in the average low-skill wage. The Clemens and Hunt (2019: 8; emphasis added) claim that "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" is simply false. The sharp increase in the number of black workers used to calculate the average low-skill wage *preceded* the Mariel supply shock. And the claim that "this compositional effect generated a spurious wage decline among Miami workers with less than high school"

Figure 2. Scatter of First-Differenced Age-Adjusted Wage and Black Share



Notes: Each point in the scatter gives the year-to-year difference in the age-adjusted wage and in the percentage black for high school dropouts in Miami.

(p. 8) is equally false. The steepest wage decline in the March CPS occurred at a time the black share was roughly constant.

The CPS-ORG trends are equally striking. The average low-skill wage in Miami (including blacks) fell dramatically between 1980 and 1984, and the black share rose slightly and then declined slightly over the same period—ending approximately where it started. In short, the large wage drop observed in the first few years after Mariel cannot be related to a change in the racial composition of the low-skill sample in the ORG.

This point is reinforced by examining the first difference of the data reported in Table 1. If the Clemens–Hunt hypothesis was valid, we would expect to find that the average wage fell most in those years when the black share of the sample increased substantially. The two panels of Figure 2 illustrate the first-differenced data and show the regression line that best fits the scatter. It is visually obvious that no systematic relationship exists between the year-to-year change in the average low-skill wage and the change in the black share. The estimates of the slope of the regression line (also illustrated in the figure) confirm the visual observation. The slopes are actually positive (contradicting the Clemens–Hunt assertion), but statistically insignificant.[6]

In sum, not only does the March CPS wage fall most in those years when the racial composition for the sample was constant, but the correlation between the annual change in the wage and the annual change in the black share over the entire 14-year sample period shows no systematic relationship

[6]The slope estimates reported in Figure 2 are from an unweighted regression. The point estimates are also positive and insignificant when the regression is weighted by the sample size used to construct the average wage in the cell: 0.054 (0.223) in the March CPS and 0.168 (0.301) in the CPS-ORG.

between the racial composition of the sample and the average wage of Miami's high school dropouts.

Changing the Pre-Treatment Period

As Figure 1 documents, the peculiar increase in the black share of the March CPS sample between calendar years 1978 and 1979 may be a potential problem when comparing averaged pre-1980 wage data with the averaged post-1980 data. To determine whether this data quirk seriously biases the estimates of the wage impact of Mariel reported in Borjas (2017), I simply discard all the March CPS data prior to calendar year 1979. By starting the time series in 1979, the huge rise in the black share between 1978 and 1979 is no longer able to contaminate the average pre-treatment wage in any way, as the pre- and post-Mariel short-run wage effect is now calculated in a time frame when the racial composition of the sample was roughly constant.

This solution requires that the pre-treatment period be defined differently from how it was in my original (2017) article. The simplest approach is to use the CPS data for calendar years 1979 and 1980 to construct the pre-treatment sample. The 1980 data are obviously contaminated by Mariel, as half that year's earnings were observed after the Mariel refugees arrived. But including the 1980 wage data as part of the pre-treatment period would presumably bias the results *against* finding any wage impact.

I re-estimate the regression models using the calendar years 1979 to 1980 as the pre-treatment period. Table 2 reports the resulting regression coefficients. It is evident that excluding the pre-1979 data from the analysis does not change the conclusion that Miami's relative wage fell substantially after 1980. In fact, there is little change in many of the regression coefficients in Table 2 from those reported in the analogous table 5 in Borjas (2017). Consider, for example, the estimated wage impact relative to all other cities in the 1984 to 1986 period. In Borjas (2017), the estimated effect was −0.393 (with a standard error of 0.022). The coefficient falls slightly to −0.385 (0.029) when I exclude all pre-1979 March CPS observations.

In sum, a substantial decline is evident in the post-Mariel wage of Miami's low-skill workers even when the regression model is estimated using a time frame that *entirely* avoids the potential problems created by the sizable increase in the black share between calendar years 1978 and 1979.

The Race-Adjusted Wage

Finally, I can document the role of the change in the racial mix of the low-skill workforce in Miami by examining trends in the *race-adjusted* average wage. These results, however, are sensitive to how a researcher chooses to adjust the wage data for changes in the racial composition of the sample.

Borjas (2017) examined the trend in the age-adjusted wage. I now present a parallel analysis that examines the trend in the age- *and* race-adjusted

Table 2. Difference-in-Differences Impact of the *Marielitos* on the Age-Adjusted Wage of High School Dropouts (Using 1979–1980 Calendar Years as Pre-Treatment Period)

	Card placebo	*Employment placebo*	*Low-skill placebo*	*Synthetic control*	*All cities*
A. March CPS					
1981–1983	−0.186	−0.220	−0.178	−0.138	−0.183
	(0.096)	(0.069)	(0.089)	(0.047)	(0.078)
1984–1986	−0.344	−0.384	−0.388	−0.345	−0.385
	(0.086)	(0.049)	(0.064)	(0.046)	(0.029)
1987–1989	−0.304	−0.233	−0.224	−0.180	−0.278
	(0.099)	(0.066)	(0.070)	(0.029)	(0.061)
1990–1992	−0.002	0.019	0.041	−0.101	0.005
	(0.094)	(0.116	(0.074)	(0.153)	(0.036)
B. CPS-ORG					
1981–1983	−0.084	−0.129	−0.089	−0.121	−0.099
	(0.025)	(0.047)	(0.023)	(0.025)	(0.024)
1984–1986	−0.078	−0.105	−0.087	−0.119	−0.104
	(0.057)	(0.064)	(0.054)	(0.056)	(0.051)
1987–1989	−0.116	−0.163	−0.111	−0.114	−0.159
	(0.036)	(0.062)	(0.036)	(0.025)	(0.031)
1990–1992	0.010	−0.058	0.026	−0.028	−0.025
	(0.040)	(0.060)	(0.037)	(0.017)	(0.032)

Notes: Robust standard errors in parentheses. Data consist of annual observations for each city between 1979 and 1992. All regressions include vectors of city and year fixed effects. Table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. All regressions are weighted by the number of observations used to calculate the dependent variable. The number of observations of the synthetic control is a weighted average of the sample size in the actual cities that make up the synthetic city.

wage. I initially estimate the following regression model separately in each cross section of the March CPS or CPS-ORG files:

$$\text{(1a)} \qquad \log w_{it} = \alpha_t + \beta_t A_{it} + \gamma_t R_{it} + \lambda_t C_{it} + \varepsilon_{it},$$

where R_{it} is an indicator variable telling us if the worker is African American. The age- and race-adjusted wage is given by the residual of this regression, and I use this residual to calculate the mean age- and race-adjusted wage in each city in each year for the subsample of high school dropouts. The specification in Equation (1a) restricts the racial wage gap to be the same across all cities and all types of workers at a given point in time, but allows the national racial wage gap to vary over time. This type of race adjustment is, by far, the most straightforward (and parsimonious) way to fix concerns about sampling.

Table 3, panel A reports the regression coefficients that show how the age- and race-adjusted wage in Miami diverged from that of alternative control groups during the 1980s. Many of the regression coefficients—which measure the impact of Mariel after accounting for the changing racial composition of the workforce in Miami and elsewhere—still show a significant wage drop in Miami relative to any control group one cares to examine. For example, a comparison of Miami and the synthetic control shows

Table 3. Wage Impact of *Marielitos* Using Alternative Measures of Race-Adjusted Wage

	March CPS			CPS-ORG		
	Card placebo	*Synthetic control*	*All cities*	*Card placebo*	*Synthetic control*	*All cities*
A. No interaction model						
1981–1983	−0.121	−0.183	−0.117	−0.047	−0.123	−0.052
	(0.078)	(0.053)	(0.063)	(0.029)	(0.015)	(0.010)
1984–1986	−0.202	−0.290	−0.251	−0.024	−0.115	−0.045
	(0.072)	(0.059)	(0.041)	(0.067)	(0.055)	(0.055)
1987–1989	−0.202	−0.190	−0.193	−0.074	−0.114	−0.114
	(0.093)	(0.039)	(0.065)	(0.039)	(0.023)	(0.021)
1990–1992	0.094	−0.112	0.062	0.069	−0.056	0.021
	(0.086)	(0.133)	(0.044)	(0.044)	(0.024)	(0.048)
B. Two-way interaction model						
1981–1983	−0.184	−0.256	−0.187	−0.050	−0.127	−0.052
	(0.070)	(0.057)	(0.055)	(0.026)	(0.023)	(0.008)
1984–1986	−0.231	−0.339	−0.273	−0.074	−0.152	−0.081
	(0.065)	(0.055)	(0.036)	(0.053)	(0.033)	(0.042)
1987–1989	−0.274	−0.281	−0.243	−0.102	−0.133	−0.124
	(0.073)	(0.058)	(0.044)	(0.039)	(0.030)	(0.025)
1990–1992	−0.014	−0.201	−0.025	0.011	−0.083	0.020
	(0.071)	(0.158)	(0.045)	(0.043)	(0.028)	(0.043)
C. Full interaction model						
1981–1983	−0.011	0.018	−0.013	−0.031	−0.023	−0.022
	(0.016)	(0.028)	(0.010)	(0.011)	(0.021)	(0.010)
1984–1986	0.000	0.011	−0.003	−0.049	−0.037	−0.033
	(0.015)	(0.008)	(0.007)	(0.015)	(0.021)	(0.010)
1987–1989	−0.004	−0.017	−0.017	−0.043	−0.033	−0.033
	(0.023)	(0.013)	(0.013)	(0.013)	(0.021)	(0.007)
1990–1992	0.003	0.008	0.005	−0.037	−0.028	−0.023
	(0.013)	(0.012)	(0.010)	(0.014)	(0.023)	(0.011)

Notes: Robust standard errors in parentheses. Data consist of annual observations for each city between 1977 and 1992 (1980 excluded). All regressions include vectors of city and year fixed effects. Table reports the interaction coefficients between a dummy variable indicating if the metropolitan area is Miami and the timing of the post-Mariel period. All regressions are weighted by the number of observations used to calculate the dependent variable. The number of observations of the synthetic control is a weighted average of the sample size in the actual cities that make up the synthetic city.

that the Miami wage in 1984 to 1986 had fallen by 29.0% in the March CPS (and by 11.5% in the CPS-ORG) relative to its pre-Mariel level.

However, the assumption that the racial wage gap is constant across all cities and skill groups at a point in time is probably too restrictive. Consider instead the following generalization of the regression model:

$$\text{(1b)} \qquad \log w_{it} = \alpha_t + \lambda_t C_{it} + \varphi S_{it} + \beta_t A_{it} + \gamma_t R_{it} + \mathrm{k}_{1t}(C_{it} \times R_{it}) + \mathrm{k}_{2t}(S_{it} \times R_{it}) + \mathrm{k}_{3t}(A_{it} \times R_{it}) + \varepsilon_{it}$$

where S_{it} is a vector of fixed effects indicating the worker's education group. I again use the residual from this regression to calculate the mean age- and race-adjusted wage in each city in each year for high school dropouts.

The specification in Equation (1b) includes all two-way interactions (city $\times$ race, education $\times$ race, and age $\times$ race) to calculate the race-adjusted wage. Because the regression model is estimated separately in each cross section, it allows for the average racial wage gap for each city, or for each education group, or for each age group, to change over time. *The crucial restriction of the two-way interaction model is that it does not allow the racial wage gap for high school dropouts in a particular city (such as Miami) to change over time.*

As Table 3, panel B shows, this alternative approach to calculating the race-adjusted wage again leads to a much larger wage drop in post-Mariel Miami than in comparison cities, and the estimated wage effects are significant and often larger than the estimates resulting from the no-interaction model in Equation (1a). For example, the March CPS reveals that the relative wage drop in Miami in 1984 to 1986 (relative to the synthetic control) is 34% in the two-way interaction model in Equation (1b), but only 29% in the no-interaction model.

It is easy to show, however, that relaxing the assumption that the racial wage gap for high school dropouts in a given city is constant over time can change the results dramatically. Consider the full interaction specification given by:

$$\text{(1c)} \qquad \log w_{it} = \alpha_t + \lambda_t C_{it} + \varphi S_{it} + \beta_t A_{it} + \gamma_t R_{it} + k_{1t}(C_{it} \times S_{it} \times A_{it} \times R_{it}) + \varepsilon_{it}$$

where the vector $(C_{it} \times S_{it} \times A_{it} \times R_{it})$ denotes all possible two-, three-, and four-way interactions among all variables—the city fixed effects, the education fixed effects, the age fixed effects, and race. Because the micro-level regression is estimated separately in each CPS cross section, the model is also fully interactive with respect to time. The age- and race-adjusted wage calculated from the residuals to this regression allows for the racial wage gap to vary across age-education-city cells at a point in time as well as within each cell over time.

Table 3, panel C reports the evidence using the age- and race-adjusted wage calculated from the full interaction model. The magnitude of the estimated wage impacts on the Miami labor market is far smaller than in the comparable regressions in the two other panels of the table. This outcome should not be surprising because a fully interactive model nets out much of the wage variation that exists across the various markets. The typical March CPS cross section in the 1980s, for instance, had 8,865 observations. The fully interactive model adds 2,816 regressors to the regression (or 44 metro areas $\times$ 4 education groups $\times$ 8 age groups $\times$ 2 race groups). Given the relatively small sample sizes available in the March CPS cross sections and the large number of regressors, it also should not be surprising that this kitchen-sink approach reduces the precision of the estimates sufficiently so that the null hypothesis of "no Mariel effect" can no longer be rejected. Note, however, that many of the coefficients remain negative and significant in the ORG, so that a suggestion remains that low-skill workers in post-

Mariel Miami experienced a steeper wage drop than did comparable workers in other cities.

But how *should* one calculate the race-adjusted wage? It is far from clear that the residuals from the full interaction model can be used to estimate the "true" impact of the Mariel supply shock. The variation in the racial wage gap for a particular education group across cities and over time might have arisen partly *because* of immigration. After all, a supply shock might affect the wage and employment of low-skill black and white workers differently.[7] Substantial differences exist in the jobs the two groups hold, in the occupations they enter, and in the industries that employ them. The residual from a regression that nets out this differential impact removes much of the effect that immigration might have had on the local labor market—effectively throwing the baby out with the bathwater.

Note, however, that the Mariel supply shock is not the only factor that potentially changed the racial wage gap in Miami relative to other cities. Other shocks may have affected the relative demand for low-skill workers, and particularly the relative demand for black workers, differentially across cities (Bound and Holzer 2000). For example, the explosive growth of the illicit narcotics trade in Miami or the economic and political response to the racial riots that ravaged the city within a month after the Mariel refugees began to arrive may have led to a different trend in the racial wage gap. These other shocks suggest that we might indeed want to allow for the relative wage of low-skill blacks in Miami to change over time (relative to other cities). But the use of a full interaction model, which incorporates the role of these extraneous and contemporaneous events, would throw out the potential impact of Mariel at the same time.

Using a trivial numerical example, Table 4 illustrates the problem introduced by implementing the full interaction model. Suppose there are only two cities, Miami and New York. New York did not receive any immigrants, but Miami did. The table shows the average log wage of black and white low-skill workers in the two cities before and after the supply shock. Panel A gives the unadjusted wage data (i.e., the data that would be reported in the CPS). By construction, immigration had a far larger impact on black workers in Miami, reducing their log wage from 0.80 to 0.45, while the wage of white workers fell by only 5% (with the log wage falling from 1.00 to 0.95). Assuming that half of the workforce in both cities is black, and that the

[7]Very few studies estimate the potentially different impact of immigration on both black and white workers. Borjas, Grogger, and Hanson (2010) modeled the source of the various effects but did not separately estimate the impact of immigration on low-skill blacks and whites. The wage elasticity is essentially the same for the average worker in the two race groups, but immigration has a much greater adverse effect on black employment. Smith (2012, table 8) found that low-skill immigration leads to a much greater reduction in annual hours worked for black young men than for white young men. Finally, the early work of Altonji and Card (1991, tables 7.8 and 7.9, column (4)) did estimate the elasticity separately for the two low-skill groups. The negative wage effect is almost twice as large for blacks as it is for whites.

Table 4. Numerical Example of Bias from Alternative Calculations of the Race-Adjusted Wage

Log wage of low-skill workers:	*Before*		*After*		
	Whites	*Blacks*	*Whites*	*Blacks*	*% drop in average wage*
A. Actual data					
Miami	1.00	0.80	0.95	0.45	20.0
New York	1.10	0.90	1.10	0.90	0.0
B. Race-adjusted log wage, using race-year interaction					
Miami	1.00	1.00	0.95	0.65	20.0
New York	1.10	1.10	1.10	1.10	0.0
C. Race-adjusted log wage, using city-race-year interaction					
Miami	1.00	1.00	0.95	0.95	5.0
New York	1.10	1.10	1.10	1.10	0.0

Notes: The calculation of average wage assumes that 50% of the low-skill workforce is black in both Miami and New York, and both before and after the supply shock.

black share in each city is constant over time, the average wage in Miami fell from 0.9 to 0.7, or a wage impact of about 20%.

Panel B of the table reports the race-adjusted wage in each city, with the race-adjusted wage computed in a way that captures the essence of the two-way interaction model in Equation (1b). Suppose that Miami is a small city relative to New York, so that the national racial wage gap in the post-migration period is 0.2 log points. The two-way interaction model would then use this national racial wage gap to adjust the log wage of Miami's black workers upward by 0.2 log points. The exercise then implies that the average race-adjusted wage in Miami fell from 1.00 to 0.80, again implying a 20% wage impact.

Finally, panel C calculates the race-adjusted wage suggested by the full interaction model. This regression would "see" a 0.2 log point racial wage gap among low-skill workers in Miami prior to the supply shock and would use that information to conclude that the race-adjusted wage of a black worker should be 1.0. After the supply shock, the regression model would "see" a 0.5 log point racial wage gap and would use *that* information to conclude that the race-adjusted wage of a black worker should be 0.95. The average race-adjusted log wage in Miami fell from 1.00 to 0.95, or a 5% drop. In fact, the average wage in Miami fell by 20%. Ignoring that the supply shock might itself have increased the racial wage gap can greatly underestimate the wage impact of immigration.

In short, the empirical exercise used to compute the race-adjusted wage should not follow blindly from a kitchen-sink approach to regressions. Careful thought must be given to *why* racial wage differences arise within skill groups and across cities, how the time trend of those racial differences might be affected by immigration, and exactly which value of the racial "wage penalty" should be used to calculate the race-adjusted wage. To the extent that immigration may have contributed to racial wage differences, it is incorrect to calculate a race-adjusted wage that nets out that potential impact.

Conclusion

The timing of the increase in the black share of the low-skill workforce in Miami sampled by the March CPS (which happened between calendar years 1978 and 1979) does not coincide with the substantial decline in the low-skill wage after Mariel (which happened between calendar years 1981 and 1983). As a result, the Clemens–Hunt claim that changes in the racial composition of Miami's workforce created a spurious correlation that explains this wage drop is demonstrably false. Further, adjusting for the racial composition of the workforce in either the March CPS or the CPS-ORG files does not change the key insight of the Mariel reappraisal in Borjas (2017) as long as one allows for the possibility that the supply shock might have affected Miami's black and white workers differently. The relative wage of low-skill workers in Miami fell significantly in the period after Mariel, reaching a nadir around the mid-1980s and recovering fully by 1990.

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