What Economists Can Learn from the Mariel Boatlift, Part Two: Answering Questions about Our Research

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Views from the Center | Migration, Immigration, Refugees, labor mobility

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Last week I blogged about a research discovery. An influential study had found that a 1980 wave of Cuban refugees into Miami, known as the Mariel Boatlift, had caused the wages of workers there to fall dramatically. In a new paper co-released by CGD and the National Bureau of Economic Research, my co-author and I revealed that large shifts in the racial composition of the underlying survey data could explain most or all of the same fall in wages. In other words, we found that that the evidence was insufficient to prove that the influx of Cubans caused the fall in wages of Miami workers.

The author of the previous study, George Borjas, raised two substantive questions about our research, which I answer briefly in this post. (A more technical supplement is here.) The changes in sample composition do coincide with the relevant changes in wages, and our results stand up within precisely the regression framework used by Borjas. This reinforces our paper's main finding: that the Borjas study's finding of large, robust, highly significant effects on low-skill workers' wages was spurious. However, nothing here *disproves* any negative wage effects of the Mariel Boatlift; some of the evidence is compatible with negative effects, but it is also compatible with zero effects.

The fall in wages coincides with the change in survey coverage

Borjas notes that, in the data he uses, the increase in the fraction of black workers for 1979 is not accompanied by a fall in the average of wages earned in that year. This is true, but not relevant. The sample in each year is miniscule: in that year it's just *nine* black workers, and eight non-blacks. Any result will fluctuate unpredictably from year to year. What matters for the regressions in Borjas's paper is that the black fraction of the sample is far higher in the years when the average wage is lower (as shown in the graph below). This has the effect of artificially reducing the average wage post-Boatlift (in blue) relative to pre-Boatlift (in red).

Pre-Boatlift in red; post-Boatlift in blue   Miami only, March CPS
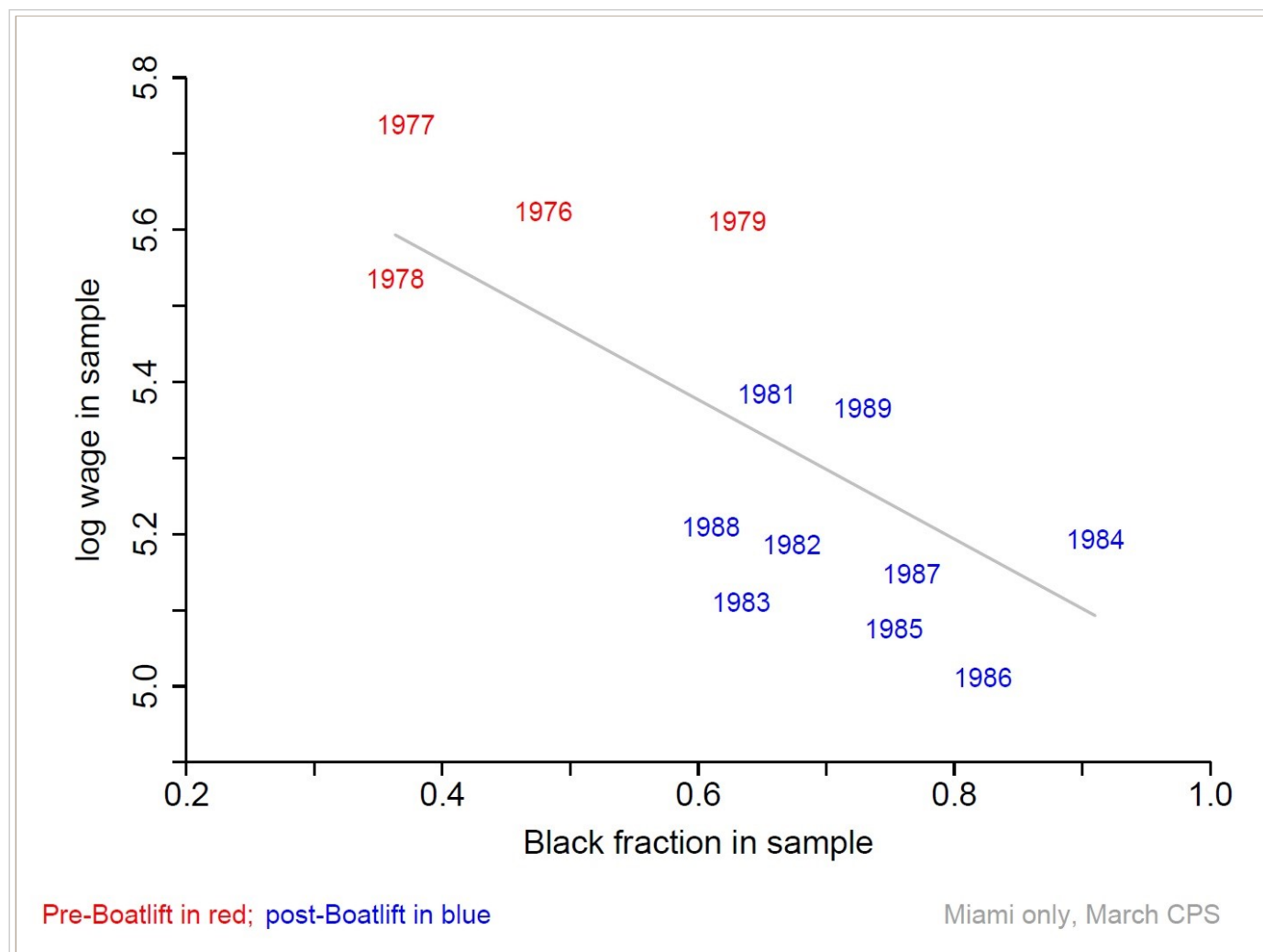
Borjas's argument amounts to claiming that the above, obviously negative relationship is *entirely coincidental*. It has a simpler explanation: adding low-wage blacks to the sample must, arithmetically, create a large spurious treatment effect. (David Roodman makes a related point here.) The only question is whether that spurious effect is large enough to explain the entire effect in the regressions of Borjas's paper. I turn to that now.

Reconciling our results with Borjas's regressions

Our paper already runs regressions to test how much of the wage decline could have been spurious, by controlling for a black indicator variable (Figure 4, page 35 of our paper). Those regressions have a different form than the main regressions in Borjas's paper (his Table 5). In Table 1 below, I reproduce our results in precisely the regression framework used by Borjas.

Miami wage trends for non-Hispanic men with less than high school are compared to trends in two groups of 'control' cities: those preferred in the research by Borjas and those preferred by David Card (whose influential study in 1990 found no negative wage effect of the Cuban immigrants, and was disputed by Borjas). Negative numbers mean that wages for workers with less than high school-level education were lower in Miami after the Boatlift, relative to unaffected 'control' cities. The first column is an exact replication of Table 5 in the Borjas paper, for one extract of the Current Population Survey (CPS) data: the March CPS.

TABLE 1: MARCH CPS, REANALYSIS OF BORJAS TABLE 5, WORKERS WITH LESS THAN HIGH SCHOOL

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Add indicator for black			
	Borjas replication		Nationwide		By city		By city-less than HS	
Control cities:	Card	Borjas	Card	Borjas	Card	Borjas	Card	Borjas
1981–1983	−0.204***	−0.290***	−0.121	−0.194***	−0.0964	−0.174**	0.000854	−0.0785
	(0.0758)	(0.0734)	(0.0776)	(0.0698)	(0.0815)	(0.0722)	(0.0620)	(0.0605)
1984–1986	−0.368***	−0.454***	−0.202***	−0.301***	−0.137	−0.227***	0.109*	−0.00131
	(0.0601)	(0.0587)	(0.0721)	(0.0527)	(0.0828)	(0.0580)	(0.0627)	(0.0591)
1987–1989	−0.329***	−0.303***	−0.202**	−0.237***	−0.135	−0.149**	−0.0247	−0.0492
	(0.0810)	(0.0725)	(0.0928)	(0.0708)	(0.0886)	(0.0658)	(0.0977)	(0.0705)
1990–1992	−0.0259	−0.0561	0.0938	0.0248	0.105	0.0375	0.220	0.121
	(0.0724)	(0.123)	(0.0859)	(0.117)	(0.0895)	(0.112)	(0.134)	(0.103)
N	75	75	75	75	75	75	75	75

Unit of observation is city-period. Dependent variable is log real wage. Robust standard errors in parentheses: $* \; p < 0.10$, $** \; p < 0.05$, $*** \; p < 0.01$. Sample is working non-Hispanic males age 25–59 with less than high school. 'HS' is high school.

In our paper, we claimed that the negative effects of columns 1 and 2 could be explained by a jump in the share of black respondents in the March CPS coinciding with the Boatlift, but unrelated to it. Columns 3 and 4 use wages from which the effect of a black indicator has been removed, in the same way as in the blog by Borjas, exactly replicating those results. About one third of the treatment effect disappears—but, as Borjas noted, not the entire effect. However, unlike the analysis in our original paper, but following Borjas' blog post, columns 3 and 4 impose the strong assumption that the wage gap between blacks and non-blacks is identical in every city, at all levels of education. This is unlikely given that 63 percent of the sample is black in Miami, but under 10 percent in the Borjas control cities. In our paper we show that the difference between black and non-black wages is indeed different in Miami than in other cities. And, as shown in our original blog post, the shift in composition of the sample *only* happened in Miami.

Columns 5 and 6 therefore remove the black effect from wages allowing the black effect to differ between cities, but not between education levels. More than half the treatment effect is gone now, and the treatment effects are statistically insignificant relative to the Card control cities. Since the large shift in the black fraction of Miami's sample occurs only for workers with less than high school, in columns 7 and 8 of the table, we remove the black effect specifically for workers with less than high school. There is no wage effect relative to the Card control cities. We cannot reject an effect relative to the Borjas control cities, of −5 to −8 percent. But amid the statistical noise, we cannot reject a zero effect either. This result corresponds closely to Figure 4d (page 35 again) in our paper; the only material difference is that we did not control for workers' age in that figure.

As we discuss at length in our paper, there's an important caveat to the above findings. It is hypothetically possible that the entire effect of the Mariel Boatlift fell upon black men with less than high school education, not on Hispanics and not on whites or Asians at that education level, or any workers who finished high school. This is unlikely, but cannot be definitively ruled out. If that is the case, columns 7 and 8 in the above tables control away the effect of interest. By allowing the black indicator to take a Miami-specific value for black workers with less than high school, columns 7 and 8 are effectively measuring the effect of the Boatlift on non-Hispanic whites and Asians. If the effect on blacks in that skill group was far greater, that differential effect would be missed by those regressions.

In Table 2, below, I repeat the analysis using the Outgoing Rotating Group (ORG) data, a larger and thus more reliable CPS extract that represents the same population. The treatment effect disappears even when allowing the black indicator to vary only by city—not by education level. Thus even without allowing a different black effect by education level, the negative treatment effect is only present in one (smaller) dataset but not another; and in the dataset where the effect appears, it is present relative to Borjas's chosen control cities, not Card's. This and several other pieces of evidence are discussed in the supplement to this post.

TABLE 2: CPS-ORG, REANALYSIS OF BORJAS TABLE 5, WORKERS WITH LESS THAN HIGH SCHOOL

Control cities:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
					Add indicator for black			
	Borjas replication		Nationwide		By city		By city-less than HS	
	Card	Borjas	Card	Borjas	Card	Borjas	Card	Borjas
1981–1983	−0.0753***	−0.140***	−0.0466	−0.104**	0.00490	−0.0561	0.0361	−0.0252
	(0.0262)	(0.0494)	(0.0288)	(0.0461)	(0.0339)	(0.0402)	(0.0341)	(0.0440)
1984–1986	−0.0690	−0.116*	−0.0240	−0.0792	−0.00149	−0.0533	0.0162	−0.0331
	(0.0568)	(0.0655)	(0.0666)	(0.0595)	(0.0630)	(0.0474)	(0.0514)	(0.0473)
1987–1989	−0.106***	−0.175***	−0.0744*	−0.137**	−0.0460	−0.101	0.0511	0.0152
	(0.0365)	(0.0639)	(0.0393)	(0.0635)	(0.0419)	(0.0666)	(0.0530)	(0.0739)
1990–1992	0.0191	−0.0700	0.0694	−0.0412	0.105*	−0.00450	0.162**	0.0394
	(0.0408)	(0.0620)	(0.0442)	(0.0699)	(0.0552)	(0.0756)	(0.0611)	(0.0780)
N	75	75	75	75	75	75	75	75

Unit of observation is city-period. Dependent variable is log real wage. Robust standard errors in parentheses: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Sample is working non-Hispanic males age 25–59 with less than high school. 'HS' is high school.

It is impossible to settle the issue by studying the wages of non-blacks separately. There are only about *six* such workers per year in the Miami data post-1980, and just 3.5 non-black workers per year after 1984. The CPS is not designed for such a microscopic slice of the sample to represent any population. It is also impossible to rely on results for blacks separately. Our paper discusses why: Very large increases in survey coverage of low-skill black workers would tend to bring in relatively low-wage workers, and a large wave of Haitian immigrants in 1980 did bring many non-Hispanic blacks with very low wages into this population. (Beyond this, a colleague's suggestion led us to recently discover *direct* evidence of such negative selection, shown in the supplement. The Boatlift appears to 'cause' a fall in years of education among Miami workers with less than high school. That can't be a real effect, since the sample is age 25 and up, but must reflect greater survey coverage of people with relatively low education and earnings.)

We can learn little more from this episode

The evidence from the Mariel Boatlift does not offer a compelling reason to revise the consensus among research economists that the effect of immigration on native wages is small, even for native workers without a high school degree. This is fundamentally different from proving that the Mariel Boatlift did *not* reduce wages for that group: no such proof exists. In one dataset but not another, and for some control cities but not others, there is evidence that is compatible with a wage effect of −2 to −8 percent at some points in time. But even there, the evidence is also compatible with a wage effect of zero or above.

This certainly does not disprove any wage effect, but it does imply, as we concluded in our original paper, that the Borjas study's finding of large, robust, highly significant effects on low-skill wages was spurious. Our contribution has been to show that slicing the available data too finely can introduce severe bias by creating unrepresentative

subsamples. The surviving data from the Mariel Boatlift have now been pushed as far as they will go, and they have little more to tell.

I originally blogged to accessibly summarize research that we had spent several months carrying out. We are grateful for the substantive comments and questions our work raised, and will continue this debate carefully and slowly, in the dispassionate academic manner that it deserves.

The code to replicate this analysis is here. *I thank three anonymous referees for helpful suggestions, and George Borjas for making his regression code publicly available.*

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