

Lecture Note: The Economics of Immigration

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Table removed due to copyright considerations. Please see the following:

Friedberg, Rachel, and Jennifer Hunt. Tables 1, 2, Figure 1. In "The Impact of Immigrants on Host Country Wages, Employment, and Growth." *Journal of Economic Perspectives* 9, no. 2 (1995).

Borjas, George. Table 1. In "The Economics of Immigration." *Journal of Economic Literature* 32, no. 4 (1994): 1668.

Card, David. Tables 1, 2, 6, and 7. In "The Impact of the Mariel Boatlift on the Miami Labor Market." *Industrial and Labor Relations Review* 43, no. 2 (1990).

Card, David, and O. Ashenfelter. *Handbook of Labor Economics* 3C. Amsterdam: Elsevier, 1999: 1297, 1329.

Card, David, and John DiNardo. Figures 1 and 2. In "Do Immigrant Inflows Lead to Native Outflows?" *American Economic Review* 90, no. 2 (2000).

Friedberg. "The Impact of Mass Migration." Figure 1, Table 2, 3, 5. *QJE* (2001):1377, etc.

Borjas, George. Figures 1, 2, Tables 2-5, 9. In "The Labor Demand Curve is Downward Sloping?" *Quarterly Journal of Economics* 116, no. 4 (2003).

1 THE ECONOMICS OF IMMIGRATION

A large and contentious literature studies immigration and the labor market. There are at least four central questions in this literature:

1. Who decides to immigrate, and how do they decide where to go? This is the topic of the Borjas 1987 *AER* article.
2. How do immigrants fare in the labor market relative to natives?
3. What affect do immigrants have on natives (use of public resources, wage structure changes, contributions to aggregate or per-capita GDP)?
4. What affect do immigrants have on the wage structure?

The last question is actually just a subset of the third, but it's the most contentious of the literature. We'll focus on this question for this lecture. But first some background.

See Table 1 of Friedberg and Hunt (1995, *Journal of Economic Perspectives*). Immigration is a major feature of Western economies. Immigrants range from a low of 0.7 percent in Finland to a high of 28.4 percent in Luxembourg. The U.S. in 1991 was 7.9 percent, which puts it above the median but below the mean.

Many countries have also seen large increases in immigration in recent decades, and much more can be expected. Figure I from Friedberg and Hunt (1995) shows the incredibly sharp changes in flows in the United States. Starting in the 1980s, immigrant flows to the U.S. rose dramatically. See Table 1 of Borjas *JEL* 1994. Although the share of U.S. population that is foreign born was much lower in 1990 than in 1890, immigration flow as a percentage of population growth (births + immigrants - deaths - emigrants) in 1990 was at its highest level in 1990 since 1911. Immigration is understandably a subject of public debate and economic analysis.

Adding to the U.S. controversy, recent wave of immigrants are distinctly different from the large waves of European immigrants at the turn of the century. As Table 2 of Friedberg and Hunt shows, there has been a sharp increase in immigration from less-educated countries (Mexico, and South America) but also Asia, which is relatively highly educated. The education

gap between Mexican and South American immigrants and average U.S. citizens is probably fairly large, even relative to the European immigration wave of a century ago. (Europeans were not especially educated, but nor were Americans at that time.) In fact, a striking statistic is that is not given in any of these articles is that a very large share of the less educated U.S. work force is not native born. In 2000, 36 percent of U.S. high school dropouts who were participating in the labor market were foreign born (my calculations from the 2000 CPS). If the sample were restricted to high immigration states like New York, Texas, California and Florida, these shares would be far higher.

It's thus no surprise that economists have focused analysis on the question of how immigrant flows affect native wages. The difficulty in analyzing these effects is that this is a general equilibrium question. How might flows of immigrants into a given area affect wages of natives? Depends upon:

- Size of immigrants flows
- Substitutability between natives and immigrants
- Relative abundance of natives in different skill, education, occupation and/or experience groups
- Integration of the 'host' labor market with other markets. In the extreme case, perfect integration with other labor or product markets can mean that there are no local effects of local immigration since these effects are entirely mediated through general equilibrium impacts on the larger market (this follows from trade theory; if economies are perfectly integrated, then local quantities are unrelated to local prices – the law of one world price for all factors will prevail).

So, this is not a standard 'program evaluation' problem. Moreover, the endogeneity problems typically associated with program evaluation are at a greater level of aggregation here. "The locational choices of immigrants and natives presumably depend on expected labor market opportunities. Immigrants tend to move to cities where the growth in demand for labor can accommodate their supply. Even if a new immigrants cluster in only a few cities (as they do

in the United States), inter-city migration of natives will tend to offset the adverse effects of immigration.” (Card, 1990, *ILRR*). Hence, simply correlating immigrant densities with native wages is unlikely to be informative about causal parameters.

2 CARD 1990, *ILRR*

Card’s influential 1990 study uses the ‘natural experiment’ (a term that has since gone out of vogue) of the arrival of 125,000 Cuban immigrants in Miami between May to September 1980 to study the labor market impacts of immigrant flows. This increased the size of the Miami labor force by 7% in a remarkably brief period of time. Card’s idea is a simple difference-in-difference comparison of wages and unemployment rates of ethnic groups between Miami and four other high immigration cities: Atlanta, Houston, Los Angeles, and Tampa-St. Petersburg. (In fact, there are no regression equations in the paper, and few actual regression coefficients.)

- Table 1 shows that, based on education and occupation, Cubans are more likely to compete with Hispanics and blacks than whites.
- Table 2 shows that Mariel Cubans are much less educated and earn substantially less (34 log points) than other Cubans. Note that there are only 50 Cubans in this special 1985 CPS supplement.
- Tables 3 - 5 show simple means.
- Table 6 shows raw and regression adjusted comparisons of wages, employment/population, and unemployment among all and low-educated blacks in Miami relative to the 4 other chosen cities. There is evidence that black wages, emp/pop, and unemployment in Miami all take a turn for the worse relative to blacks in other cities. But, this does not start until 1982 and has reversed by 1985. So, this makes it doubtful that it can be attributed to Mariel.
- Two general problems here: 1) there is almost no ‘pre’ period to get a baseline prior to Mariel. This is because the CPS MORG does not become available until 1979; 2) the U.S. experiences a severe recession from 1980 to 1983, and this makes all wage and employment

numbers highly variable. Hence, we don't have a good sense of what magnitude of causal effect we *could* reliably detect.

- Table 7 examines the effects of the Mariels on wages of Cubans. The real wage of Miami Cubans falls by 9 log points between 1979 and 1985. But, regression adjustment predicts a fall of 6 log points simply due to composition. So, the remaining 3 points could be a price effect, but it is small.
- The final column of Table 7 compares Miami Mariels to Cubans in the rest of the U.S. The wage gap between Miami Cubans and other Cubans does increase by 14 log points. Regression adjusted, this is still 9 log points. But the big jump is from 1984 to 1985! Ignoring 1985, any other difference between Miami and rest-of-U.S. Cubans appears to be due to compositional changes; Mariel Cubans were less educated and so earned lower wages. Adjusting for these differences, the gap is quite stable from 1979 to 1984.

2.1 INTERPRETATIONS

Card's results were quite striking and unexpected – certainly to the black Miami residents who rioted in 1980, in part because of grievances over labor market competition from Cubans. Card's paper moved the literature. It shifted priors on the effect of immigration on labor markets and it helped to popularize the use of natural experiments and diff-in-diff techniques.

Why did immigration *not* have an effect in the Mariel 'experiment' – or at least not a detectable one?

1. One possibility is a reduction in native inflows to Miami. From 1970 to 1980, the Miami population grew at 2.5% per year while the rest of Florida grew at 3.9%. After April 1, 1980, the growth rate in Miami slowed to 1.4% per year, while the rest of the state declined to only 3.4%. Notably, Miami still received a disproportionate share of new immigrants in this time. Thus, it appears that natives and older immigrants were *deterred* from migrating to Miami. So, this is a type of general equilibrium response that would mute the local impact of native inflows. It implies that a national impact may have occurred, but would have been undetectable.

2. Another explanation is that Miami was extremely well set up to absorb Cuban immigrants. There are numerous Cuban employment and social networks in Miami; the industry/occupation structure has high demand for their skills (endogenous to the fact that so many are already there); and the city has been doing this kind of thing for decades.
3. A third possibility is that this is not a high-powered test. As noted above, we don't really have a good sense of how much these variables (employment, unemployment wage levels) bounce around from year to year, especially in small samples, so we may have less ability to detect small effects than we might assume. Moreover, the diff-in-diff assumption – i.e., that the other four cities would have behaved similarly to Miami but for the Mariel boat lift – is not infallible. What if those other cities experienced other shocks? Or, if there is enough normal city-level variation in these outcomes that we cannot be comfortable making the 'but for' assumption.

Some evidence on this point is provided in Angrist and Krueger's (excellent) 1999 Handbook of Labor Economics chapter. They consider the 'Mariel Boat Lift that Didn't Happen.' In the summer of 1994, tens of thousands of Cubans boarded boats destined for Miami for a 2nd boatlift. Wishing to avoid the political fallout, the Clinton administration interceded and had these intended immigrants diverted to Guantanamo Bay. Angrist and Krueger perform an 'event study' like Card's study using the non-event to evaluate the diff-in-diff counterfactual: absent the influx of immigrants, these cities would have otherwise moved similarly. The disturbing answer is that the data come perilously close to showing that the 'Mariel Boat Lift that Didn't Happen' raised the black unemployment rate in Miami by 6.3 percentage points ($t = 1.70$). See A&K Figure I, Table 4, and Table 7.

4. A final possibility is that Miami cannot realistically be treated as an autarkic labor market. Labor or product market integration between Miami and the rest of the U.S. may dissipate any local effects through general equilibrium adjustments. In fully integrated economies, local factor supply changes have no effect on local goods prices – rather, the law of one price prevails for each good. In this case, there is no strong presumption that the Mariel boat lift should affect the Miami labor market *more than* it affects the four comparison

cities; there is no ‘control’ city that can be used for the diff-in-diff.

This final criticism – that the Mariel experiment is unsuitable for learning about the effects of immigration on the labor market – sparked a lively debate between proponents and critics of ‘area studies’ analyses. The Card paper takes the ‘area studies’ approach. But many economists, most vocally Borjas, have argued that general equilibrium spill-overs make these results meaningless. One of the arguments levied against the area studies analyses is that natives will out-migrate in response to immigration, meaning that the *net* effect of immigration on factor supplies could be zero (even ignoring product market integration). Card refers to this idea sardonically as the ‘skating rink mobility model,’ whereby each immigrant who skates into an area knocks one native off the ice.

Card acknowledges this possibility in his *ILRR* paper. But his 2001 *AER P&P* paper with John DiNardo takes a direct whack at this argument. Figures 1 and 2 of that paper show that, to a first approximation, a 1 immigrant increase in the low-skill population in an MSA (Metropolitan Statistical Area) increases the total low-skill population by about 1 over the 1980-1990 period. This suggests that area studies analyses are not completely (or even mostly) ‘undone’ by out-migration. This does not exclude GE effects through product market integration, however.

The counterpoint to area studies are analyses that attempt to a more explicitly GE approach. One well know example is Borjas, Freeman and Katz (1997) in *Brookings*. But these studies are not so much empirical analyses as simulations: given a production function and an estimate of the elasticity of substitution between immigrants and natives, what does an increase in immigration of size X imply for native wages? With some justification, Angrist calls this approach ‘feeding the numbers to the theory.’ Can we do better?

Yes. The two recent studies on immigration and the labor market in the *QJE*, one by Friedberg in 2001 and the second by Borjas in 2003, both present compelling GE ‘experiments.’ Surprisingly, they reach quite different conclusions.

Immigration increased Israel's population 12 percent between 1990 and 1994 after emigration restrictions were lifted in the Soviet Union. (See Figure I.) This seems another great possibility for an event study. Unlike the Mariel case, this influx was *large* relative to the *national* labor market. So, there could easily be a measurable GE effect. But we still have the problem of 'no control group.'

Idea: use occupations as unit of analysis. Did the occupations that Russian Jews differentially entered experience differential declines in wages? The assumption needed to make this work is that there is limited occupational mobility so that workers don't simply enter a new occupation when wages fall.

Friedberg takes three approaches to estimating this relationship:

1. Estimating cross-section correlations between immigrant occupational penetration and wage levels. This is not likely to be satisfactory since immigrants might differentially enter low-wage occupations.
 2. Estimate 'change' regressions: $\Delta \text{Wage}_j = \alpha + \beta \Delta \text{Immigrants}_j + \varepsilon_j$, where j indexes occupations. This seems much more appealing, especially over a short time interval. But occupational choice could be endogenous.
 3. To purge endogeneity, use self-reported occupation in the Soviet Union as an instrument for occupation in Israel. This seems like a reasonable idea.
- See Table II. The first two approaches yield large, negative associations between immigrant occupational entry, r , and wages. The first (cross-sectional) estimate is not too convincing, but the latter seems plausible enough.
 - The 1st column shows the 1st stage relationship between Russian occupation and Israeli occupation. It's not especially strong. The coefficient of 0.24 ($t = 2.8$) shows a surprisingly weak relationship between source and host country occupation (note that if this relationship were perfect, the coefficient would be 1.0).

- More puzzlingly, the reduced form relationship is *positive*. That is, entry into Israel of immigrants who were employed in a given occupation while in Russia is associated with *rising* wages in the Israeli occupation. This would almost seem to suggest endogenous self-selection of Russians to Israel based on trends in occupational wages within Israel. Or it's possible that wages in these occupations were already rising. It would be nice to control for pre-period trends (or 'difference' these out).
- Given this positive reduced form, the IV estimates are also positive, albeit insignificant.
- Table III confirms these findings using individual level data.
- Table IV presents some robustness checks.
- Table V checks for native disemployment effects by occupation. These go the same way as the wage estimates: negative for OLS, positive for IV.

Hence, the conclusion is that large immigrant flows do not harm either wages or employment of natives. I find this set of results intriguing but quite puzzling. We would normally expect the opposite: immigrants would tend to endogenously select into occupations with growing wages and employment. This simultaneity would attenuate the estimated negative impact of immigration on native wages or employment. By this logic, the IV estimates should be more *negative* than OLS estimates. What is the source of endogeneity that causes OLS estimates to be more *negative* than IV estimates?

4 BORJAS, 2003, *QJE*

The Friedberg potentially addresses the general equilibrium concern posed by 'area studies' papers. In theory, it also addresses the endogeneity concern: forward looking immigrants enter labor markets where their skills are in demand, and hence migration decisions are endogenous. On the other hand, this study uses kind of a strange instrument. Occupations are not labor markets, and depending upon elasticities of substitution/complementarity, it's not entirely clear how flows into one occupation should affect wages in another. With only two occupations, this is easy. With numerous occupations, it is not.

It might be nice to have a GE ‘experiment’ where we do not have to instrument labor force categories (such as occupation) because these categories are arguably exogenous. For example, immigrants cannot readily change their education and potential experience (i.e., age minus education minus 6) in response to labor market conditions. So perhaps the education and experience of immigrants can be viewed as exogenous. But that brings us back to the ‘feeding numbers to the theory’ problem. With a small number of education categories, we’re not going to get any meaningful identification from exploiting changes in immigrant supplies by education. Which brings us to Borjas 2003.

Borjas makes use of the observation that workers of the same education but of different age or experience are unlikely to be perfect substitutes. For example, Welch (1979, *JPE*) and Card and Lemieux (1999, *QJE*) both present evidence that within an education group, young workers are closer substitutes for one another than are young and old workers. Hence, if it were the case that the immigrant flow is not only concentrated among certain education groups (high school dropouts in recent years), but also concentrated among experience groups within education groups, we would potentially have many more quasi-independent data points with which to achieve identification. Based on this idea, Borjas conducts an analysis of the impact of immigration on native earnings in cells defined by decade (1960 - 2000), education (4 groups), and 5-year-potential-experience groups. Hence, there are $5 \times 4 \times 8 = 160$ cells.

- Figure I of Borjas tells an important part of the story:
 1. During the 1980 - 2000 period, immigrants became an increasingly significant share of U.S. labor supply.
 2. The growth was most concentrated among high school dropouts. By the year 2000, 50 percent of young high school dropouts were foreign born.
 3. Immigrants tend to be younger than other workers, and this is particularly true for recent and less educated immigrants. (Note that experience is calculated as potential rather than actual experience.)
- Borjas’ basic immigrant labor supply measure is:

$$p_{ijt} = \frac{M_{ijt}}{M_{ijt} + N_{ijt}},$$

where M refers to weeks of labor supply by immigrant workers, N is weeks of labor supply by native workers, and i, j, t index education group, experience, and time.

- Table II makes the case that experience groups *are* imperfect substitutes. Within education groups, immigrants and natives have far more similar occupational distributions if they are at the same experience level than if they are at different experience levels. (This similarity index is like a correlation coefficient.)
- Figure II lends support to the basic story of a negative sloping relationship between immigrant supply and native wages. This figure summarizes the identifying variation of the entire paper, and it strengthens the argument considerably.
- The basic regression model is as follows

$$y_{ijt} = \theta p_{ijt} + s_i + x_j + \pi_t + (s_i \times x_j) + (s_i \times \pi_t) + (x_j \times \pi_t) + \varepsilon_{ijt}.$$

This model contains main effects for schooling, experience, and time, plus a full set 2nd order interactions for schooling by experience, schooling by time, and experience by time. The $(s_i \times x_j)$ interactions control for differences in the experience profile by schooling group; the $(s_i \times \pi_t)$ terms control for changes in the return to schooling; and the $(x_j \times \pi_t)$ terms control for changes in the overall return to experience. Hence, what is left for identification is immigration and wage changes within education-by-experience cells over time. You can think of this as a triple-difference strategy.

- Table III has initial results. These coefficients are difficult to interpret b/c they don't have a natural scale as parameterized. What they give is

$$\frac{\partial \ln w_{ijt}}{\partial (M_{ijt}/(M_{ijt} + N_{ijt}))} = \hat{\theta}$$

So, define

$$m_{ijt} = M_{ijt}/N_{ijt},$$

which is the ratio of immigrants to natives in a cell. Then, $\partial \ln w_{ijt}/\partial m_{ijt}$ is the percentage changes in wages associated with a percentage point change in the immigrant/native ratio.

Some algebra:

$$\begin{aligned}
\frac{\partial \ln w_{ijt}}{\partial m_{ijt}} &= \frac{\partial \ln w_{ijt}}{\partial (M_{ijt}/(M_{ijt} + N_{ijt}))} \cdot \frac{\partial (M_{ijt}/(M_{ijt} + N_{ijt}))}{\partial (M_{ijt}/N_{ijt})} \\
&= \frac{\partial (N_{ijt}M_{ijt}/(M_{ijt} + N_{ijt}))}{\partial M_{ijt}} = \hat{\theta} \left(\frac{N_{ijt}}{M_{ijt} + N_{ijt}} \right)^2 \\
&= \hat{\theta} \left(\frac{N_{ijt} + M_{ijt}}{N_{ijt}} \right)^{-2} = \hat{\theta} \cdot (1 + m_{ijt})^{-2} = \frac{\hat{\theta}}{(1 + m_{ijt})^2},
\end{aligned}$$

which is a convoluted way to get a readable point estimate. But that's how the paper is written.

- Page 1349 says that $1/(1 + m_{ijt})^2 \approx 0.7$. So the Table III coefficients imply that a 10 percent increase in immigrant labor supply reduces native weekly earnings by 4.0 log points (-0.572×0.7), reduces the fraction of time worked by 3.7 log points, and reduces total native earnings by 6.4 log points.
- Notice also that the effect on log annual earnings is the effect on log weekly earnings plus the effect on log weeks. Thus, a 10 percent increase in immigrant labor supply must native reduce weeks worked by $6.4 - 4.0 = 2.4$ log points. This is a large disemployment effect.
- This result also suggests that immigrant labor supply could also potentially be endogenous. Footnote 8 discusses this concern and gives results when immigrant LF participation is instrumented by immigrant population shares in the relevant education/experience groups. The point estimates given corroborate the main results. This is an important finding and probably deserves more than a footnote.
- Footnote 9 documents that results are roughly similar (though much less precise) when estimated separately for each pair of decades. This also deserves a table.
- Table IV shows that these results hold within education groups – though not for college grads.

4.1 LINK TO ‘AREA STUDIES?’

- A good labor economist takes the literature seriously enough to want to reconcile prior findings with her own. In this spirit, Borjas applies his education/experience technique to a state level wage analysis in Table V. This is the ‘area studies’ approach where the state is the assumed closed-market geographic area. To the degree that there is interstate arbitrage of labor market impacts through labor mobility or trade, these estimates should be smaller in absolute magnitude than those in Table III.
- And they are only $1/3^{rd}$ of the size! This is a very nice result. Given the presence of substantial measurement error in the state \times education \times experience \times time cells, however, it would have been valuable if Borjas had instrumented labor supply with populations shares as above. As estimated, we cannot tell if the ‘area studies’ estimates are much smaller because of arbitrage or attenuation.
- A second notable pattern: the point estimates get larger in absolute magnitude when fixed effects are added for experience \times education \times time (2-way interactions). This indicates that the identification is coming from the within education-experience over-time component of the data. So, it’s specifically by honing in on the imperfect substitutability of experience cohorts within education group that this paper obtains such striking findings.

4.2 SUMMING UP

- The remaining sections of the paper deal first deal with the issue of measuring potential experience among immigrants and then estimate a more ‘structural’ model that specifically calculates substitution elasticities across education and experience groups. The big payoff to this work is Table IX, which contains the implied effects of immigration on native wages. I won’t go through the model in this case due to time, but I will cover a closely related model by Card and Lemieux (*QJE*, 2001) in the spring.
- The implied effects on native wages are quite large in this table: -0.089 log points for high school dropouts, -0.049 for college grads, and -0.032 overall (Q: Is everyone worse off?). It would have been nice to separate these effects further into the 1980s and 1990s,

since it was during the 1980s that wages fell particularly rapidly for less-skilled workers, whereas it appears that high skill immigration was most pronounced in the 1990s.

5 CONCLUSION

The analysis of the impact of immigration on wages of natives illustrates how a healthy labor literature evolves. Over the last 15 years, there has been a steady accumulation of good work on this topic. The findings have been highly contentious. But the debate has been solidly grounded in theory and methodology, and not ideology (for the most part). And the scrutiny each paper has received has advanced the literature. Like the literatures on ‘specific capital’ and biased-technical change (which we’ll read in the spring), the economics of immigration literature has made factual, conceptual, and methodological progress on its topic. This is good for economic understanding, policy, and graduate education...