

Lecture Note #6: Immigration, Trade, Technology and Prices

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1 IMMIGRATION, TRADE, TECHNOLOGY: IMPLICATIONS FOR GOODS AND FACTOR PRICES

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 effect do immigrants have on natives (use of public resources, wage structure changes, contributions to aggregate or per-capita GDP)?
4. What effect do immigrants have on the wage structure?

The last question, which we will focus on, is actually just a subset of the third, but it has produced a rich, active and important literature that integrates labor demand analysis, trade theory, price and wage analyses and models of endogenous technology adoption. In fact, this is probably the only literature that we will study this spring that combines rich general equilibrium theory with detailed micro-estimation. This is about as good as labor gets!

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 famous 1990 study uses the ‘natural experiment’ of 125,000 Cuban immigrants arriving in Miami between May to September 1980 to study the labor market impacts of immigrant flows. This singular event increased the size of the Miami labor force by 7 percent in a remarkably brief period of time. Card’s idea was to use 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, one would expect that 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 are 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 on two fronts. 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 may have

been *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 these intended immigrants were 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. (Notice that between 1993 and 1995, the black unemployment rate rose by 3.6 percentage points in Miami and fell by 2.7 percentage points in comparison cities).

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.

It is also worth emphasizing that the Mariel results are not an outlier in this literature. Most ‘area studies’ papers have found zero or minimal effects of immigration shocks on native wages (see for example Card’s very careful 2001 piece in the *JOLE*).

The counterpoint to area studies are analyses that take 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. Four recent studies on immigration and the labor market present compelling analyses of immigration’s impacts viewed through the lens of models of varying degrees of sophistication.

3 FRIEDBERG, 2001, *QJE* [FOR SELF-STUDY]

Immigration increased Israel’s population by 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 dis-employment 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 (2001, *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 dis-employment 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, in Section VII, estimate a three-level structural CES model that specifically calculates substitution elasticities across education and experience groups. I will not spend class-time on this model because you have seen many flavors of it already. But it is useful to work through as an exercise. The big payoff to this structural work is found in 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 it will be

readily interpretable to aficionados of Card and Lemieux (*QJE*, 2001).

- 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.
- How do differing assumptions on the elasticity of the supply of capital impact these estimates?

5 HOW DO LOCAL LABOR MARKETS ADAPT TO IMMIGRATION? LEWIS 2005, 2008.

The Borjas 2003 paper may help to answer the question of whether or not immigration supplies affect wages. The answer appears to be that they do—at the national labor market level. But this finding does not resolve the puzzle of why most analyses find no local labor market wage impacts. [But see Aydemir and Borjas 2005 “Attenuation Bias in Measuring the Wage Impact of Immigration” on your syllabus and on Stellar for a possible explanation.] Recent papers by Ethan Lewis and Patricia Cortes propose a set of explanations based on trade theory that are both original and compelling.

5.1 LEWIS 2005: “HOW DO LOCAL LABOR MARKETS IN THE U.S. ADAPT TO IMMIGRATION?”

This paper puts to good use a very simple observation from trade theory. One explanation commonly offered for why in the immigration literature local skill shocks don’t appear to affect local wages is that markets are integrated. One form this could take is, as discussed above, is a ‘skating rink mobility’ model. But this explanation appears strongly at odds with the facts.

An alternative explanation is that market integration occurs through trade among local markets. To see this, return to the simple case of a large number of integrated economies producing two goods, High Tech and Low Tech, using H and L labor where High Tech is more intensive in H than is Low Tech. Each economy operates in the cone of diversification and there is full factor price equalization. Since each local economy is a price taker for goods, the wages of H and L are pinned down. How does a local economy respond to an influx of L labor?

Since the supply shock induces no wage change, there will be no industry-level changes in factor demands. That is, sectors will *not* become less skill intensive because there is no wage incentive to do so. Instead, in response to the supply shock, low-skilled sectors will expand to absorb low-skilled labor. Hence, this class of model makes a strong prediction: if wages don't respond to changes in local supplies, then supply shocks will be accommodated by sectoral expansion and contraction rather than changes in skill-intensity within sector.

This is the implication that Lewis takes to the data. The basic technique here is to 'instrument' for immigration shocks by projecting decadal immigration *flows* onto historical immigration *stocks* by metropolitan area in the 1970s. Thus, cities that are intensive in Mexicans in the 1970s are predicted to get large influxes in the 1980s when Mexicans enter the U.S. in large numbers. The conceptual mechanism underlying this instrument is that immigrants tend to follow the pathways set down by predecessors. The exclusion restriction is that past stocks are otherwise uncorrelated with future changes in wages/employment/prices except through their effect on immigration flows. See Table 4 for evidence on the first stage.

The findings of the Lewis study appear quite clear cut:

1. There is no significant impact of immigration shocks on the wages of *native* dropouts relative to the wages of all natives in cities receiving relatively more immigrants. (Note that *non-native* wages and *non-dropout* wages are not examined.)
2. Almost all of the absorption of low-skilled labor takes place *within* industries.

Both results are visible in the two panels of Figure 1. These results to rule out the plausibility of Heckscher-Ohlin explanations frequently offered to reconcile Card-style 'area studies' results and Borjas-style 'general equilibrium' results. The micro-support for the underlying mechanism is not present. [Note that Guy Michael's 2008 *ReStat* paper on market integration and skill prices in the U.S., which exploits the experiment offered by the construction of the U.S. interstate highway system, also does not find Heckscher-Ohlin (HO) type industry size adjustments in response to market integration, though he *does* find wage effects.]

Thus, the Lewis paper appears to deepen the puzzle. If there are no local wage impacts and no industry mix adjustments (of economically significant magnitude) in response to immigrant

supply shocks, how does the market adjust to supply shocks? Should we throw out supply and demand entirely?

5.2 LEWIS 2008: “IMMIGRATION, SKILL MIX, AND CAPITAL-SKILL COMPLEMENTARITY”

This recent paper nicely integrates material from trade theory, production theory and the ‘area studies’ literature to assess how immigration affects the operation of labor and factor markets. The novel idea underlying the paper is to broaden the analysis of immigration effects to encompass margins of market adjustment to immigration that are implied, but not tested, by analyses that focus exclusively on wages and employment. In particular, Lewis proposes to examine the effect of immigration on the following outcomes simultaneously:

1. Skill intensity of employment
2. Capital intensity of employment
3. Wage levels
4. Wage inequality

The motivating models used in the paper are simple and familiar. Lewis contrasts two alternatives that are commonplace in the literature but have subtle differences for the outcomes above.

5.2.1 A MODEL WITH CAPITAL-SKILL COMPLEMENTARITY

Consider the following aggregate production function

$$Y = A(K_M^\rho + L^\rho)^{\frac{\alpha}{\rho}} H^{1-\alpha},$$

where K_M is high tech machinery (robots, computers), L and H are low and high-skill labor, A is a TFP shifter, $\sigma = 1/(1 - \rho)$ is the elasticity of substitution between K_M and L , and α is the share of the low-skill aggregate in production. If $\rho > \alpha$, this is consistent with capital-skill

complementarity. Specifically:

$$d \ln K_M = \frac{1 - \alpha}{(\rho - \alpha)\omega + (1 - \rho)} d \ln H - \frac{(\rho - \alpha)(1 - \omega)}{(\rho - \alpha)\omega + (1 - \rho)} d \ln L,$$

where $\omega \equiv rK_M/\alpha Y$ and r is the capital rental rate. Notice that K_M is unambiguously increasing in H and is *decreasing* in L iff ρ is larger than α . Intuitively, a rise in L has two countervailing effects on the return to capital. On the one hand, it raises the return to capital due to q-complementarity between L and K_M . On the other, it lowers the return to capital through dilution of H . The second effect dominates if the elasticity of substitution between L and K_M is high (ρ large) or the share of H in output is small (α large). In these cases, a rise in L substantially dilutes the ratio of H to the L/K_M aggregate, and thus depresses the return to (equivalently, reduces the demand for) capital.

5.2.2 A MODEL WITHOUT CAPITAL-SKILL COMPLEMENTARITY

Let the aggregate production function be:

$$Y = [K_M^\nu + f(L, H)^\nu]^{1/\nu},$$

where $f(\cdot)$ is the sub-production function that includes only labor (typically, another CES function). In this production function, there is no capital-skill complementarity; the elasticity of substitution between K_M and the labor aggregate is $\sigma_{Kf} = 1/(1 - \nu)$, and this does not differ between H and L .

Now consider an increase in the *relative* supply of L over H , *holding total labor supply constant* ($H' + L' = H + L$, $H'/L' < H/L$). Assuming we are in a setting where the marginal product of H exceeds that of L (i.e., the skilled-wage premium is positive), then

$$\left. \frac{\partial f(\cdot)}{\partial L/H} \right|_{L+H=c} < 0,$$

(here c is a constant). This case implies that

$$\left. \frac{\partial K_M}{\partial L/H} \right|_{L+H=c} < 0,$$

iff $\nu < 1$ (so $\sigma_{KF} < \infty$). Thus, it would appear that we cannot distinguish capital-skill complementarity from capital-skill neutrality by simply studying the effect of skill supply shocks on the demand for capital.

5.2.3 SO WHAT'S THE DIFFERENCE?

These two models have slightly different implications for the capital-output ratio and for wages.

Under the K-S neutral model, the capital/output ratio is independent of relative skill supplies:

$$\frac{rK_M}{Y} = r^{\nu/(\nu-1)}.$$

The intuition for this result is straightforward: K-S neutrality implies that so long as the rate of return to capital is fixed (equivalently, capital supply is perfectly elastic), changes in skill mix cannot affect capital's share.

By contrast, in the K-S complementary model:

$$\frac{\partial \ln(K_m/Y)}{\partial \ln(L/H)} = -\frac{\rho(1-\alpha)(1-\omega)}{(\rho-\alpha)\omega + (1-\rho)},$$

the output share of capital varies with relative skill supplies. In particular, if $\rho > \alpha$ (the capital dilution effect dominates the H dilution effect), then the righthand side of the above equation is unambiguously negative, and so the capital output ratio *falls* when there is an outward shift in relative supply of L versus H . Moreover, this equation says that capital input falls by proportionately more than output when L/H rises (so $d \ln(K_m) < d \ln Y$). This is an important prediction to be tested.

An equally interesting set of implications follow for wages. In the K-S complementary model, we have:

$$\begin{aligned} \frac{\partial \ln W_L}{\partial \ln(L/H)} &= -\frac{(1-\alpha)(1-\rho)}{(1-\rho) + (\rho-\alpha)\omega}, \\ \frac{\partial \ln(W_L/W_H)}{\partial \ln(L/H)} &= -\frac{(1-\alpha\omega)(1-\rho)}{(1-\rho) + (\rho-\alpha)\omega}. \end{aligned}$$

Note that when $\rho = 0$ (so $\sigma = 1$),

$$\frac{\partial \ln(W_L/W_H)}{\partial \ln(L/H)} = \frac{1-\alpha\omega}{1-\alpha\omega} = 1$$

Thus, the wage implications of skill shocks depend not only on the elasticity of substitution between L and H (equal to $1/(1 - \rho)$), but also on the degree of capital skill-complementarity. If there is K-S complementarity and capital is not fixed, then a regression of changes in the wage ratio on changes in H/L does not identify the elasticity of substitution between H and L .

So, summing up:

1. The more substitutable K_M is for L (that is, the larger is ρ), the less responsive is the low-skill wage or wage inequality to changes in relative supplies of L . This is because the availability of a close substitute substantially pins down the wage of L . This is easy to see in the above equations. If $\rho = 1$, so K_M and L are perfect substitutes, then there is no effect of changes in L/H on W_L or W_L/W_H .
2. Thus, *if there is K-S complementarity*, area studies analyses that essentially regress wage changes on changes in skill mix across markets *will not identify the elasticity of substitution between H and L* —even if the skill shocks used for identification are strictly exogenous. This is because the response of wages to skill shocks is a function of both σ_{HL} (in $f(\cdot)$) and σ_{Kf} . *By contrast, in the K-S neutrality case, $\frac{\partial \ln(W_L/W_H)}{\partial \ln(L/H)}$, depends only on the elasticity of substitution $\sigma = 1/(1 - \rho)$.*
3. Under the K-S model, capital’s share of output varies with relative skill supply. A positive shock to L/H lowers the capital-output ratio. Output also declines, but less than does capital input, so the capital-output ratio falls.

$$\frac{\partial \ln(K_m/Y)}{\partial \ln(L/H)} = -\frac{\rho(1 - \alpha)(1 - \omega)}{(\rho - \alpha)\omega + (1 - \rho)}.$$

By contrast, without K-S complementarity, the capital output ratio is always equal to:

$$\begin{aligned} \frac{rK_M}{Y} &= r^{\nu/(\nu-1)}. \\ \frac{K_M}{Y} &= r^{1/(\nu-1)}, \end{aligned}$$

which does not depend upon L/H . Hence, capital per worker and output per worker should decline equally in the absence of K-S complementarity.

4. Finally observe that under the K-S complementarity case, it's possible that there will be an important discrepancy between area-studies and aggregate relationships between changes in skill mix and changes in relative wages. Imagine that K_M is supplied elastically at the area level but inelastically at the national level (in the short run). In that case:

$$d \ln (W_L/W_H)|_{K_M=c'} = -d \ln (L/H) + \rho\omega d \ln L,$$

which can be rewritten in more familiar terms as $\partial \ln (W_L/W_H)|_{K_M=c'} = -\frac{1}{\sigma} \partial \ln (L/H)$. Thus, the aggregate impact of an increase in L/H will be larger at the national than local level. This difference is a function of the aggregate versus local elasticity of capital supply. It is not due to integration among markets (which would be the standard explanation).

5.2.4 RESULTS [SEE SLIDES]

A particularly critical result is found in Table 8. Capital per worker falls almost twice as much as output per worker in response to a rise in the L/H ratio. This is consistent with K-S complementarity. In the absence of K-S complementarity, capital's share would be fixed, meaning that capital per worker and output per worker would fall proportionately.

5.2.5 CONCLUSIONS

The Lewis paper is original and important. It provides the *only* quasi-experimental evidence of that I know of establishing complementarity between skill input and demand for capital (high tech or otherwise) This is an important contribution on its own, though not the one that Lewis most focuses on. Equally important is the insight that capital-skill complementarity moderates the effect of an increase in low-skill labor supply on wages and wage inequality *at the local level*. If capital is elastically supplied and low-skilled workers and capital are close substitutes, the fixed return to capital moderates the wage impact of the skill shock.

6 CORTES (2008): THE EFFECT OF LOW-SKILLED IMMIGRATION ON US PRICES: EVIDENCE FROM CPI DATA

Cortes also offers an original and insightful analysis of how markets respond to immigration. A major innovation here is to study goods and labor markets simultaneously. The research

question: Do low-skill immigration shocks affect the prices of low-skill intensive goods in major metropolitan areas? Answering this question requires Cortes to overcome three conceptual hurdles:

1. Developing price data that can be used for outcomes.
2. Developing an appropriate conceptual model where local labor market shocks have the potential to affect local prices. Clearly, the standard H-O models and the Beaudry-Green model imply no *local* price effects.
3. Reconciling any price effects found with the *absence* of wage effects found in the rest of the literature.

Cortes' solution to (1) is to gain access to the microdata collected by the Consumer Price Index to develop city level price indices.

The solution to (2) is the model sketched below.

The solution to (3) is to challenge the findings on wage effects of the rest of the literature. Lewis' 2008 paper does this as well, though interestingly, that was not true in the 2005 version of the paper (which assumed no wage effects).

6.1 MODEL

You know these tools well now so I won't be exhaustive. This model is worth studying, however, because Cortes estimates it (quasi-)structurally (perhaps it's better to say 'parametrically'); thus the theory is closer to the estimation here than in most papers we have considered (it is closest in spirit of the papers we've read is Card and Lemieux 2001).

Here are the key assumptions:

1. There are two goods, one that is H intensive and one that is L intensive (the important thing is that one is more L intensive than the others). Call these goods T and NT for trade and non-traded.
2. The H good is traded; its price is set nationally.

3. The L good is non-traded (think of it as in-person services); its price is set locally.
4. The two goods are imperfect substitutes in consumption (otherwise the price of the traded good would pin down the price of the non-traded good).
5. Immigrants supply only L labor. Some natives supply H labor and some supply L labor. See Table 2 for evidence that immigrants are concentrated in low-skill, plausibly non-traded services.
6. Crucially natives and immigrants I are imperfect substitutes in the production of L labor. Table 2 demonstrates that Native and Immigrant high-school dropouts are *not* clustered in the same industries. Immigrants appear to be very low-skill production and service jobs where English language skills are plausibly not very important. Native HSDs are more likely to be in higher skill production sectors. For example, 60 percent of HSDs in the labor force in 2000 were natives, *yet less than a third of HSDs in gardening and housekeeping are natives.*

Let's formalize a bit:

- Production of T : $T = f(H_T) = H_T$
- Production of NT : $NT = H_{NT}^\alpha \left[(\beta L_{NT}^\rho + (1 - \beta) I_{NT}^\rho)^{1/\rho} \right]^{1-\alpha}$
- Notice:
 1. The elasticity of substitution between H labor and the I/L aggregate is 1.
 2. The elasticity of substitution between I and L labor is $1/(1 - \rho)$
 3. It is not assumed that I and L have the same efficiencies. Thus, it is plausible that in addition to being imperfect substitutes, I and L would receive different wages even if relative supply of the two unity.
- Consumer preferences over goods for a representative consumer are: $U(T, NT) = T^\gamma (NT)^{1-\gamma}$. Thus, T and NT goods have elasticity of substitution of 1.

Let's think about market clearing in this model—specifically, how an influx of immigrants affect prices and quantities.

- First, since the price of T is pinned down by the national market (and assuming each local market is small) an immigration shock does not affect the price of T and therefore does not affect w_H in nominal terms.
- Consider what happens to NT . All immigrants must be absorbed into the NT sector (or more generally, the NT sector expands). This raises output of the NT good, which lowers its price in the *local* market (recall imperfectly substitutability).
- This lowers the value marginal product of I and L labor. Both wages will fall but I will fall more than L (since I is a perfect substitute for I and an imperfect substitute for L).
- Question: What if I were used in both sectors but still used more intensively in the non-traded sector? Would there still be local effects on I and L wages? Explain.

Cortes solves the model for its four unknowns (P_{NT}, H_{NT}, w_L, w_I) by solving for the value marginal products of H, I and L labor, the competitive price of NT as a function of factors costs (and hence labor supply to each sector). This is good stuff.

The empirical implementation follows the model to build from primitives to an effective labor supply equation:

1. Initially, Cortes estimates the effect of immigration shocks on *native* low-skilled wages (using the standard immigration instrument). Why natives not immigrants? The hypothesis is that immigrant wages are systematically mis-measured—many are undocumented and/or work in the informal sector. Only a positively selected subset of wages will be observed in Census data. Native wages probably don't suffer from this issue. See Table 8 for estimates. These estimates produce an elasticity of substitution of approximately $\hat{\sigma} = 1.3$. If correct, this suggests that immigrants and native HSDs are far from perfect substitutes. That's important because most of the literature on immigrants effects on native wages has assumed a close degree of substitutability, suggesting a large wage effect.

2. Cortes next estimates the relative efficiency term (β) by using the identity supplied by her model:

$$\left(\frac{(1 - \beta) \bar{I}^\rho}{\beta \bar{L}^\rho + (1 - \beta) \bar{I}^\rho} \right) = \frac{w_I \bar{I}}{w_L \bar{L} + w_I \bar{I}},$$

where the right hand-term is the immigrant relative wage-bill share and \bar{I} and \bar{L} are immigrant and native supplies. Notice that estimation of this equation requires $\hat{\rho} = 1 - 1/\hat{\sigma}$. Cortes estimates β using non-linear least squares to obtain $\hat{\beta} = 0.590$. Thus natives are about 1.5 times ($0.6/0.4$) as physically productive as immigrants.

3. Cortes can now construct the low/high skill relative labor supply term suggested by the model:

$$\hat{S} = \left(\frac{(\beta \bar{L}^\rho + (1 - \beta) \bar{I}^\rho)}{\bar{H}} \right),$$

which is the key endogenous variable in the goods pricing equation.

4. Tables 6 and 7 present evidence on the effects of this supply variable (instrumented using immigration flows) on prices. There do appear to be negative price effects on goods produced by highly immigrant intensive industries.
5. Table 6 tests whether this result is concentrated exclusively in non-traded goods, as the model implies. The results here are so good they could make all but a hardened (i.e., tenured) labor economist cry. The effect on traded goods is not significantly different from zero ($p \geq 0.80$). [Notice that you need to add the two main coefficients together to get the implied effect on traded goods.]

Now, we are left with two unfinished pieces of business. First, the model implies that the price effects work through wages—but where are these wage effects? Second, if there are price and wage effects, then we should be able to make welfare calculations.

Using an identity from her model linking the cross-wage elasticity to the own-wage elasticity and the wage-bill shares, Cortes estimates an own-wage elasticity of 1.25, so a 10 percent increase in labor supply reduces own-wages by 8 percent. She can't check this number directly (assuming immigrant wages are mis-measured). But she can look at the wages of closely competing groups. See Tables 9 and 10. The effect of immigration shocks on Hispanics with

low English proficiency are quite compelling. They are only half the size of the estimated own-wage effect, but then this is not the ‘own’ group. This is great evidence.

Purchasing power effects (‘welfare effects’ under stronger assumptions): Purchasing power effects work through three channels: wage effects and price effects. For low-skill natives, wage effects will be welfare reducing and price effects welfare increasing. For high-skill natives, price effects will be welfare increasing and wage effects zero (under the free trade assumption) or positive (assuming local q-complementarities). The size of these effects also depends on the share of non-traded goods in consumption. As Cortes calculates using the CEX (Consumer Expenditure Survey), high-skill natives consume more immigration-intensive services than do low-skill natives. Hence, high-skill natives also benefit more than low-skill from the price effect. See Table 10 for estimates, which suggest (assuming q-complementarity) that in the 25 largest cities, immigration in the 1990s reduced high school dropout real purchasing power by 2.65 percent (-2.93 through wages, $+0.27$ through prices) and raised college grad purchasing power by 0.71 percent ($+0.34$ through prices, $+0.37$ through wages).

6.2 CONCLUSION ON CORTES

This paper is a major advance for the immigration literature, both conceptually and empirically. It has the potential to reconcile an important and heretofore puzzling set of facts, and to provide economic implications for welfare.

The paper also embodies several virtues for which labor economists should strive: important questions, informed by theory, analyzed using appropriate data with a valid source of variation, and applying theory to get as much ‘juice’ out of the analysis as possible. In the hands of a pure program-evaluation economist, this project would have been just a set of ‘impact estimates’ for how immigration shocks affect prices on average. Cortes takes this paper much further to get wage effects (observed and implied), elasticities of substitution, a potential resolution of the immigration-wage puzzle, and an intriguing set of welfare calculations.

7 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. The literature on the economics of immigration has made factual, conceptual, and methodological progress. This is good for economic understanding, good for policy, and good for graduate education...