



POLITICAL ECONOMY  
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Important Wage Effects?  
A Review of the U.S. Evidence**

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**Do Surges in Less-Skilled Immigration Have Important Wage Effects?  
A Review of the U.S. Evidence**

This paper reviews a small part of a vast professional literature on the labor market effects of new immigrants. It focuses on recent studies that have employed econometric techniques to estimate wage effects of less-skilled immigrants during the two great American immigration surges (roughly 1870-1914 and 1980 to the present). This literature is fairly consistent in finding that large long-term immigrant surges have at least small negative wage effects for less-advantaged members of the labor force, and that these are likely to be largest for earlier cohorts of foreign-born workers and less-educated African-Americans in major immigrant-receiving regions. While this is consistent with the simple textbook prediction in a largely deregulated labor market, we might have expected more robust negative effects. The explanation may be that these effects are inherently difficult to isolate, especially given the quality of the data - a large share of less-skilled new immigrants are undocumented workers who are employed by individuals or small family businesses under-the-table and are either not counted or counted poorly. The paper concludes that, while all consumers and many employers (both as households and as firms) have undoubtedly benefited substantially from the surge in undocumented low-skilled workers since the early 1980s, there are also some losers, and there is consequently a need for policy interventions designed to ensure that socially acceptable wage levels, employment opportunities, and working conditions are maintained for our least advantaged workers, native- and foreign-born alike.

Over the last three decades the U.S. has experienced a second great surge in immigration, comparable in many respects to the massive increase in foreign born workers in the Age of Mass Migration – the decades around the start of the last century. Between 1970 and 2005 the foreign-born share of the U.S. labor force increased from 5.3% to 14.7% (Ottaviano and Peri, 2006, p. 1). In the last five years alone (2000-05) some 4.1 million new immigrants entered the American labor market and an estimated 1.4 to 2.7 million of these newcomers were undocumented. According to a recent study, new immigrants accounted for 86 percent of the net increase in total U.S. employment (Sum et al., 2006, p. 1).

Until recently, this dramatic demographic transformation of the labor force was largely embraced and, indeed, facilitated by public policy, since it has been widely accepted that among the winners were employers, consumers, many native-born workers (whose jobs are complementary) and the new immigrants themselves. In 2000, the *New York Times* ran a front page article with the title “I.N.S. Is Looking the Other Way As Illegal Immigrants Fill Jobs.” As the agency’s Associate Commissioner for Policy and Planning explained, “It is just the market at work, drawing people to jobs, and the I.N.S has chosen to concentrate its actions on aliens who are a danger to the community.”<sup>1</sup> The following year, the *Times* ran another story under the heading “Meatpackers’ Profits Hinge on Pool of Immigrant Labor,” which noted that “cracking down on illegal workers could disrupt an industry.”<sup>2</sup>

In the meantime, the labor market has performed terribly for the bottom half of the skill distribution. For male workers, between 1979 and 2004 the median wage for those with less than a high school degree fell from \$517 to \$402 (adjusted for inflation); the median real wage also declined for high school graduates, from \$637 to \$592; and even workers with some college saw hardly any improvement, rising from \$658 to just \$672 (Blank, 2006, table 1).<sup>3</sup>

In particular, it is poor labor market outcomes for *native-born* workers that can be expected to focus public attention on possible downward wage and employment effects of immigrants. Between 1990 and 2004, real annual wages of U.S. natives with less than a high school education fell by 11.5%; high school graduates saw gains of just 6.5% over these 15 years; and those with some college gained only slightly more, 8.5% (Ottaviano and Peri, 2006, figure 4). On the employment side, 1.7 million *fewer* young (16-34) native-born men were employed in 2005 than five years earlier, compared to 1.9 million *more* male immigrant workers (Sum et al., 2006, p. 1).

So it should not be surprising that anti-immigrant sentiment has been rising in recent years. While there are many other sources of anger and resentment (fiscal costs, violation of national immigration and local zoning laws, racism, etc.), low wages and worsening employment conditions have certainly played a central role. It is widely accepted that the recent surge in low-skill immigration has produced a growing pool of workers, even outside the major gateway cities, with very low reservation wages (the wage at which

workers will agree to offer their services) and no bargaining power. While offering a windfall for employers and consumers, many believe that the newcomers reduce the wages and job opportunities of both native-born workers and earlier cohorts of immigrants.

Policy makers have responded in the last couple of years with a dramatic increase in enforcement efforts against undocumented workers. Over 1,000 Immigration agents participated in raids on six Swift & Co. plants in December 2006, arresting almost 1300 of Swift's 15,000 workers.<sup>4</sup> If the evidence from earlier raids is any indication, in the short run both wages and the share of native-born workers employed in the plants will rise substantially. *The Wall Street Journal* reports that raids a few months earlier on Crider Inc., a poultry processing company in Stillmore Georgia, resulted in the loss of "75% of its mostly Hispanic 900-member work force... for local African-Americans, the dramatic appearance of federal agents presented an unexpected opportunity. Crider suddenly raised pay at the plant... For the first time in years, local officials say, Crider aggressively sought workers from the area's state-funded employment office." Indeed, it turns out that prior to the late 1990s's most of Crider's workforce was black: "With the arrival of so many immigrants willing to toil for rock-bottom wages on brutal round-the-clock shifts, the number of black workers at Crider declined steadily to 14% in early 2006 from as high as 70% a decade ago, the company says. Wages stagnated at about \$6 an hour..."<sup>5</sup>

Are news reports of large local labor market impacts of these recent dramatic law enforcement operations consistent with the findings of research on the effects of surges in low-skilled immigration? Does the statistical evidence support public concerns that the wages and employment opportunities of many native-born workers are harmed by labor market competition with immigrants from impoverished foreign countries? Or are these new foreign-born workers just filling job slots that would otherwise have migrated abroad anyway, with the result that there is little or no clear negative impacts on wages and employment?

This paper surveys recent research that addresses these questions. The literature on the incorporation and effects of immigrants in U.S. labor markets is enormous, so in addition to focusing on recent studies I narrow the focus to those that explore the employment and earnings impacts of less-skilled immigrants in the United States primarily with statistical analysis (econometric studies). Even for this seemingly narrow scope, the

coverage of studies is far from comprehensive. The aim is to provide a reasonably balanced picture of what the recent statistical research has produced. This essay specifically does not cover the huge qualitative case study literature (though it may be at least as informative).

## **1. Theory and Method**

In simple demand and supply terms, a persistent and substantial increase in the flow of less-skilled workers from very low income regions to high income regions can be described as an outward shift in the supply curve and a movement down the employer's demand curve for this type of labor, resulting in lower wages for less-skilled workers in receiving labor markets.<sup>6</sup> This may raise the demand for higher skilled workers (if output increases and more highly skilled workers are complementary) or reduce the demand for them (if employers take advantage of cheaper labor by shifting away from production technologies that are complementary with skilled labor). In either case, an increase in supply of less-skilled workers in a labor market not marked by labor shortage can be expected to lower the wages of workers to the extent that they are closely substitutable in the workplace. Again, it is conceivable that low-skill immigration to an area could trigger economic development, ultimately leading to an increase in demand for less-skilled workers and higher wages. But this would require that additional flows of low-skill immigrants would not offset any such induced demand.

So for most economists there is a strong theoretical "prior" that a large and persistent supply of less-skilled workers (especially those with low reservation wages and whose bargaining power is undermined by their illegal status) into labor markets not characterized by labor shortages will reduce local wage levels of substitute workers. The empirical question is whether, in a large and dynamic economy such as the U.S., even the recent immigration surge can be shown with the available data to have unambiguous and robust wage and employment effects.

Until recently, the consensus among researchers was that there has been little if any observable impact of immigration on wages using standard data and methods. Indeed, even George Borjas, among the most prominent researchers who have presumed substantial negative labor market effects at the bottom of the labor market, has termed these results an "unresolved puzzle" (Borjas 1994a, 1994b). In their survey of this literature, Friedberg and

Hunt (1999) confirm their earlier (1995) conclusion: “Given the widespread nature of the popular view that immigration has large adverse effects on the economic outcomes of the native-born population of the United States, there is surprisingly little evidence to support this... Most research finds that a 10 percent increase in the fraction of immigrants in the population reduces the wages of even the least skilled native-born workers by at most 1 percent... Evidence of immigrants reducing employment or labor-force participation rates or increasing the unemployment rate is even harder to find” (1999, p. 358).

Many researchers who find this a “puzzle” attribute it to data availability and methodological difficulties. Most new immigrants locate in one of a small number of large cities. In the U.S., these would include Los Angeles, San Francisco, New York, Chicago, and Miami. A natural approach to testing labor market effects with aggregate statistics is through “spatial correlations”: associating the share of new immigrants in an area with levels of native-born wages across cities or regions; or better yet, correlating the *change* in immigrant share against the change in wages. But the problem here is that workers feeling the competition from new immigrants may relocate, producing their own “supply shocks” on their destination communities. In this case the wage effects may be transferred from the local to the larger regional or national level, a process known as “factor price equalization” – in this case, between native- and foreign-born workers at a particular skill level. There is also the problem of controlling for local labor demand and for longer run effects of demand, trade patterns, and capital mobility, which may to some degree be responses to the initial immigrant supply shock (lower wages produce an increase in investment and output growth, increasing the demand for both low and high skill native workers). With all of this challenging the researcher, this is an area of research particularly susceptible to the heavy hand of theoretical (and ideological) priors: a somewhat jaundiced reviewer might suspect that researchers tend to find what they want to find.<sup>7</sup>

## **2. Evidence from the ‘Age of Mass Migration’**

For obvious reasons, most empirical work on the labor market effects of large-scale immigration has focused on the current (post-1980) surge, but several studies have addressed America’s earlier “age of mass migration” (1890-1914). The debate over what

the evidence in each of these periods of mass immigration can tell us about labor market effects is similarly contentious.

Two highly influential studies of the turn-of-the-century wage effects appeared in the 1990s, one by Claudia Goldin (1994) and the other by Timothy Hatton and Jeffrey Williamson (1998). Both concluded that there was fairly clear evidence that mass immigration had had substantial downward wage effects. Goldin found that “the impact of immigrants on the wages of laborers are generally negative and often substantial... in general, a one percentage point increase in the population share that was foreign born decreased wages by about 1 to 1.5 percent... In men’s clothing, which contained a large proportion of immigrants, wages were distinctly depressed in cities having an increase in the percentage of their population that was foreign born from 1899 to 1909.” At least as persuasive is the evidence from the polling booth. Goldin found that the lower the occupational wage increases in cities from 1907 to 1915, the more likely House Representatives were to vote to override President Wilson’s veto of proposed immigration restriction legislation (involving a literacy test). On the other hand, the higher the foreign-born share of the local population, the less likely representatives were to vote to override the veto (pp. 22-24). This suggests a sharp divide between native-born constituents concerned about labor market competition and foreign-born voters concerned about the native backlash against immigrants. There was at least a widely accepted popular perception of negative wage effects.

Summarizing their earlier work, Hatton and Williamson found that, for both the U.S. (immigration) and Britain (emigration) in the 1870-1910 period, taking into account capital mobility “reduces dramatically the effect of migration on real wages.” Still, their estimates of the wage effects from migration are substantial: “In the absence of immigration, the U.S. real wage would have been about 9 percent higher (in an economy with much less capital), and in the absence of emigration, the British real wage would have been almost 7 percent lower than it actually was” (2006, p. 6). They note that Boyer et al. (1994) found similar for Ireland as a result of the post-1851 emigration – without the mass out-migration there would have been “a 6 percent fall in both rural and urban real wages” (p. 6).

The Hatton-Williamson (2006, p. 5) results are generated by “a multi-sector competitive general equilibrium open economy model based on three factors (labor, capital, and land).” Given the inherent problems with data quality and the extreme simplicity of the model, the confidence with which they present their results seems rather breathtaking: how sure can we really be that the U.S. real wage would have been 9 percent higher, rather than, say, 8 or 10 percent higher, in the absence of immigration? New immigrants were such an important part of the American economy during this period that it seems hard to even imagine what it would have been like without any immigrants, much less to estimate and then assert such precise wage effects.

Carter and Sutch (1999, 2006) have challenged the Hatton and Williamson conclusions, in some case by using the same data. As they put it, “The state-level data on immigration and native migrant flows do not support the conclusion that immigration during this period reduced the wages of residents” (2006, p. 12). But this critique, it turns out, rests on whether “wage reduction” means 1) lower absolute levels of real wages, or 2) lower wage levels than would otherwise have prevailed. Carter and Sutch agree that in all likelihood, mass immigration reduced *wage growth*. They write that “The estimates reported by Goldin and Hatton and Williamson of a wage setback for resident workers are only valid if we interpret them to suggest that resident wages would have risen even faster without immigration” (p. 17-18). But it seems reasonable to call a slowdown in wage growth attributable to a surge in the supply of immigrant labor a negative wage effect (or “a wage setback”) for native-born workers.

The conclusions by Goldin and Hatton/Williamson that new immigrants had downward wage effects on native-born wages during the Age of Mass Migration, even when interpreted as manifested in slower rates of wage growth, are viewed by Carter and Sutch to be “problematic” for two reasons. The first is that slower wage growth does not, in their view, provide a sound basis for explaining political opposition to immigration during the period. This may or may not be the case, but it is not clear why this should make the statistical findings problematic. The second reason is that the results reflect a static analysis that fails to recognize that the slower wage growth caused by the surge in low skill immigration “accelerated the rate of economic growth” (p. 18). If new immigrants promoted economic growth, labor demand may have increased as a result,

leading in the longer run to rising wages even for the less-skilled. Indeed, Ottaviano and Peri (2006) show how such a dynamic approach applied to the post-1980 surge in U.S. immigration produces much more positive estimates of the overall labor market effects of immigrants, reflecting less negative wage effects for the least skilled (see below).

In assessing the Carter and Sutch (1999, 2006) proposition that a properly dynamic perspective might actually show positive immigrant effects of the first great immigration surge, even on less skilled native wages, the question is whether these less skilled immigrants really triggered significant productivity growth. Unfortunately, Carter and Sutch fail to address the possibility that a surplus pool of less-skilled workers may have done just the reverse, encouraging employers to adopt “low-road” labor intensive methods of production which delays the introduction of skill-complementary (usually labor-saving) higher productivity technologies. There is, in fact, some evidence for this low-road effect, at least for recent years. Ethan Lewis (2004) found that for manufacturing plants in the 1980s and 1990s, “the adoption of advanced technologies by individual plants is significantly slowed by the presence of a greater relative supply of unskilled labor in the local labor market” (Card and Lewis, 2005, p. 25).

### **3. Evidence from the 2<sup>nd</sup> Great Surge: the Post-1980 U.S. Experience**

As noted above, the recent surge in low-skill immigration has coincided with a collapse in the wages of native-born dropouts. From 1990 to 2004, these workers saw a decline in the level of real wages of almost 12 percent, and a decline of more than 24 percent relative to the average wage (Ottaviano and Peri, 2006, figure 4 and p. 32). It is hard to imagine, at least for economists, that these two phenomena are unrelated. Yet, also noted above, the professional consensus was that the aggregate statistical evidence failed to show strong support for this straightforward supply-side crowding story (Friedberg and Hunt, 1995; Smith and Edmonston, 1997; Borjas, 1999). For example, David Card’s (1990) study of the Mariel boat lift appeared to show that the sudden large influx of Cuban refugees to Miami had no significant local wage effects. Other, more conventional “spatial correlation” studies across metropolitan areas seemed to confirm this result.

George Borjas has been outspoken about what he thinks of this spatial correlation research. As he put it in his 1999 book, *Heaven's Gate*, "Put bluntly, the spatial correlations are completely uninformative." The main problem is that native workers "vote with their feet," dispersing local wage effects to a larger geographical area (1999, p. 73). But it turns out that the evidence for substantial native outflows in response to immigrant competition is at best mixed. Borjas admits that the literature shows a "confusing set of results" (Borjas, 1999, fn 25). Borjas' own evidence in *Heaven's Gate* (1999) is remarkably unconvincing. One figure actually shows a *positive* association between the change in immigration and the change in native population across states for 1970-90. Even his preferred figure, which makes the changes relative to the 1960-70 trends, appears to indicate that what he terms a "clearly negative" relationship is largely the result of a single outlier (California).

But even if these correlations were stronger, Card counters that it is not total population, but the size of a specific skill group in the labor force that ought to be examined. When this is done there is no evidence of offsetting native outflows; more low skilled immigrant arrivals just tends to produce a higher share of low skill workers in the local labor market (Card, 2005, p. 9). It should be noted that many of these are his own studies (Card and DiNardo, 2000; Card, 2001; Card, 2005).

Although there remain mechanisms that will dilute a substantial immigrant wage effect (local demand, capital mobility, intercity trade), this evidence pointing to little native migration response to immigrant inflows would seem to put the spatial correlations approach back in play. In an important contribution, Card uses 1990 Census data for 175 cities to explore the effects of new immigrants (1985-1990) on 1989 wages for 6 broad occupations. Card concludes that "immigrant inflows over the late 1980s reduced the relative wages of laborers and less-skilled service workers in high immigrant cities by no more than 3%. The effects in other cities, and for other occupation groups that were less affected by new immigrant arrivals, were probably much smaller" (2001, p. 57). In a more recent study with Census data for 2000 using 325 metropolitan areas, Card finds no relationship between the fraction of immigrant high school dropouts (less than a high school degree) in the local labor market and the relative wages of native dropouts. He does find a small but significant negative effect on native employment rates.

What could explain these weak results? Card concludes, like Borjas a decade earlier, that it's a "puzzle" (Card and Lewis, 2005, p. 26). For whatever reason, firms seem to adjust to large low-skill supply shocks mainly on the quantity side. They do so, according to Card and Lewis "by changes in skill intensity within narrow industries" (p. 26).

For Borjas, spatial correlation studies are inherently flawed. In the flexible U.S. economy, he seems to argue that empirical strategies should never assume isolated local labor markets, whatever the evidence on native migration flows. In any case, in the early 1990s Borjas turned to "factor proportions" time series analysis to explore immigrant wage effects in work co-authored with Richard Freeman and Larry Katz (1992, 1997). These studies begin with a time series estimation of the effects of changes in the relative supplies of college graduates and high school dropouts to their relative wages. An increase in the relative size of the supply of "dropouts" is found, as expected, to lower their wage relative to college graduates. The next step is to calculate the contribution of immigrants to the rise in the relative supply of dropouts. To the extent that immigrants increased the size his low skill pool, they can be assigned responsibility for part of the their decline in relative earnings. In this way, for 1980-88, Borjas et al. conclude that one quarter of the decline in relative dropout wages was due to immigrants (1992); for 1980-95, this relative wage effect if found to increase to half the overall relative wage decline (1997).

The problem with this empirical approach is that, since their data does not distinguish native- from foreign-born workers, the impacts of supply shifts on relative wages is base on the aggregate relationship, which requires them to assume that the native/foreign composition of dropouts does not matter. But as Friedberg and Hunt (1999, p. 355) point out, this tends to overstate the impact of immigration: "there is good reason to believe... that the impact of immigrant dropouts on natives to be lower than the impact of native dropouts on natives: immigrants and native dropouts are unlikely to be good substitutes for each other because even among high school dropouts immigrants have much less education." It could be added that there are many other, perhaps more important, reasons to believe there is limited substitutability between native and foreign born dropouts, including differences in experience (see Borjas, 2003) and other sorts of

skills (language, to begin with) as well as the fact that wage differences may reflect the fact that native- and foreign-born workers are concentration in different locations, occupations, and industries.

While recognizing the limits of the factor proportions approach, Borjas (1999) finds it far better than the spatial correlations approach. He gives two reasons. First, “the cross-city comparison of native employment opportunities has failed to reveal with any degree of precision the impact that immigration has on the wage structure;” and second, “*any* interpretation of statistical correlations... requires a story” and the factor proportions “story” should be favored since it is the standard textbook supply and demand account (Borjas, 1999, p. 84, emphasis in the original). This seems to presume that the simple supply-demand story *must* be correct and the role of empirical research is simply to verify it. It is hard not to agree with Card (2005, p. 25) that “Underlying this argument is the belief that labor market competition posed by immigration *has* to affect native opportunities, so if we don’t find an impact, the research design must be flawed.”

In an important new paper, Borjas (2003) acknowledged that “the factor proportions approach is ultimately unsatisfactory,” but continues to maintain that “geographic dispersion in native employment opportunities is not an effective way for measuring the economic impact of immigration” (p. 6). Like Card’s recent work, in this new work he turns to an analysis of “skill groups,” but unlike Card he does so at the national level. Workers are aggregated into education-experience groups for each census year from 1960 to 2000 and immigrant wage effects are estimated across these 160 observations. The key assumption is that foreign and native born workers are reasonably close substitutes for one another *within* each of these skill groups. While he makes a convincing case that workers are *more alike* when work experience is used to define the skill groups, this is far from establishing that there is perfect, or even reasonably close, substitutability between native- and foreign-born workers within in each his education-experience groups.

Borjas concludes that his results are consistent with those “suggested by the simplest textbook model of a competitive labor market” (p. 36). Specifically, he finds that between 1980 and 2000 “this immigrant influx reduced the wage of the average native worker by 3.2 percent... with the wage falling by 8.9 percent for high school dropouts,

4.9 percent for college graduates, 2.6 percent for high school graduates, and barely changing for workers with some college” (p. 36).

Although broadly following Borjas’ approach, Ottaviano and Peri (2006, p. 3) point out that Borjas’ time series approach provides only “the partial effect of immigration on wages (as it omits all cross-interactions with other types of workers and with capital) and as such is uninformative on the overall effect of immigrants.” The demand for native- and foreign-born workers may be different within each of these education-experience groups. Like Borjas (2003), Ottaviano and Peri (2006) analyze education-experience groups at the national level, but they do not assume that foreign and native-born workers within each of the “skill” groups are perfect substitutes. They adopt a general equilibrium framework in which a production function “describes how these different types of workers interact with each other and with physical capital to produce output. Then, one can derive the demand for each type of labor, which depends on productivity and employment of the other labor types as well as on physical capital.” (p. 3).

These differences in modeling produce dramatically different empirical results. Ottaviano and Peri (2006, p. 4) conclude that 1) “the group of least educated U.S.-born workers suffers a significantly smaller wage loss than previously calculated” (e.g. by Borjas); and 2) that “all other groups of U.S.-born workers (with at least an high school degree) who account for 90% of the U.S.-born labor force in 2004, gained from immigration;” and 3) “the group whose wage was most negatively affected by immigration is, in our analysis, the group of previous immigrants.”

Interestingly, Manacorda, Manning and Wadsworth (2006) employ a similar methodology for the U.K. and find similar results: negative sizable wage effects are found for earlier cohorts of male immigrants, but not for native-born male workers. The authors conclude that “the impact of increased immigration on native wages is muted by the low substitutability between immigrants and natives” (p. 3).

It should be underlined that, like Ottaviano/Peri, even Card (2001) has found negative wage effects for *some* workers in at least *some* cities. For example, he concludes that “The results imply that immigrant inflows over the 1980s reduced wages and employment rates of low-skilled natives in traditional gateway cities like Miami and Los Angeles by 1-3 percentage points” (p. 22).

New York City is a traditional immigrant gateway and in tests across jobs (detailed occupation-industry cells), I also found some evidence that the share of recent immigrants in jobs had negative wage effects (Howell and Mueller, 2000). While Borjas insists on national level time series analysis and Card (and many others) have focused on cross-metropolitan (or state) tests, Howell and Mueller (2000) look for immigrant wage effects on white, black and Hispanic native-born male workers and on new immigrants themselves across detailed jobs in a single large metropolitan area.<sup>8</sup> Admittedly, the native-flows problem may apply here as well: recent immigrants will tend to be attracted to the highest paying jobs and faced with intensifying employment and wage competition, native workers may respond by moving to other jobs and/or locations that are more sheltered from immigrant competition. As in the cross-metropolitan tests, native mobility will tend to bias downward the measured wage effects of the immigrant ‘supply-shock.’ For this reason, a finding of substantial negative effects would be particularly notable.

We found that between 1980 and 1990 foreign-born workers increased from about 30 percent to almost 50 percent of all workers in the secondary (lowest quality) job segment. Employment growth for all three native-born groups – white, black and Hispanic – in both secondary and subordinate primary (middle quality) jobs was inversely related to the growth of recent immigrant employment. In contrast, among independent primary jobs, both native-born black and Hispanic employment growth was fastest in the jobs also experiencing rapid growth of recent immigrants. Controlling for mean education, time worked and job quality measures, the recent immigrant share has a significant negative impact on 1979 mean earnings in most of the tests. The ‘changes on changes’ tests for 1979–89 show negative effects of the change in immigrant share on the change in mean earnings for all three native-born groups, although the findings are strongest for white and black workers in the independent primary and secondary segments – the best and worst jobs, which are located mainly in the service sector. In contrast, the negative effect of the growth in recent immigrant share on native-born Hispanic workers is concentrated in the blue-collar goods-producing jobs – the subordinate-primary segment.

Many other studies could be cited. Given space constraints, I will briefly mention just a few. Several studies published in the late 1990s also found some evidence of negative wage effects using the 1980 and 1990 Public Use Microdata Samples of the Census. For Los Angeles, Catanzarite (1998) found that “native workers suffer significant immigrant-related pay penalties, larger for blacks and Latinos than whites” (p. 147). In the Russell Sage volume *Help or Hindrance?*, both Cordelia Reimers (1998) and Kristin F. Butcher (1998) find that, across metropolitan areas, greater immigration was associated with lower African American earnings in the 1980s, particularly among the least skilled. And finally, much more recent work by Andrew Sum and his colleagues (Sum et al., 2006) finds that across states the probability of employment for youth was significantly negatively related to the size of the immigrant inflow into the state’s labor market. In the first test, immigrant flows between 2000 and 2003 had the largest negative impact on the probability of employment for 16-24 year old black dropouts. The second test, with more observations, also found strong negative effects from 2000-2004 immigrant inflows on 16-20 year old employment rates in 2004, particularly for black men (p. 5).

#### **4. Conclusions**

This paper has surveyed a small part of a vast professional literature on the labor market effects new immigrants, focusing on recent studies that have employed various econometric techniques to estimate the wage effects of less-skilled immigrants during the two great American immigration surges (1870-1914 and 1980 to the present). To say that this is a highly contentious literature would be an understatement. But while the interchanges have been heated and even hostile, a birds-eye view suggests that there is a rough consensus, not dissimilar to that found by several influential surveys published in the 1990s (Smith and Edmonston, 1997; Friedberg and Hunt, 1995, 1999). Based on the econometric studies surveyed here, it seems safe to say that there is considerable evidence that large surges of new low skill immigrants have found at least small negative wage effects for less advantaged members of the labor force: less-educated blacks, native-born Hispanics, and immigrants themselves, particularly in the major immigrant-receiving metropolitan areas.

Given that high unemployment and jobless rates for less skilled native-born workers persist in the main immigration gateway cities even in the best of times (the late 1990s, for example), the main question for researchers might be why there isn't a *more* consistent finding of substantial wage and employment effects. On opposite ends of this debate, both Borjas (2003) and Card (2005) seem still puzzled by the weakness of most findings. Given the strength of his own factor proportions (1997) and time series results (2003), Borjas asks, but has no answer, for "why the spatial correlation approach fails to find these effects" (p. 36).<sup>9</sup> On the other side, failing to find either wage flexibility or industry mix flexibility in response to large supply shocks uncompensated for by native outflows, Card and Lewis (2005) are forced to conclude that employers must be adjusting skill intensity in the workplace with a "remarkable flexibility" (p. 26).

The reality may be that there are sizable but hard-to-measure wage and employment effects because of the very nature of the problem – a large share of less-skilled new immigrants are undocumented workers who are self-employed or are employed by individuals or small family businesses. For many years, public policy has more than tolerated the employment of undocumented immigrants fearful of both employers and public authorities. In this setting, it may be too much to ask of the aggregate data to tease out precise and robust employment and wage effects. But we should also recognize that there may be equally important effects on the quality of jobs and the employment relationship. As Sum et al. put it, "The growing inflow of illegal-immigrant workers has contributed to a fundamental breakdown in the nation's labor laws and labor standards as the sheer volume of illegal hiring activity overwhelms what has amounted to meager enforcement levels..." (p. 10).

Although the strength of our desire for social protection waxes and wanes over the course of time, the history of wage labor over the last two centuries is replete with evidence that increasing new supplies of less-skilled labor is harmful to current workers. In Volume I of *Capital*, Marx documented the role played by women and children in undermining male worker bargaining power in the early stages of the industrial revolution. The recent dependence of many employers on undocumented new immigrants while at the same time advocating for an even more de-regulated labor market suggests that a similar role is played today by immigrant workers.

On balance, the findings of recent econometric research seem consistent with the anecdotal evidence of rising wages and increasing job opportunities for local African-American workers in the recent enforcement episode in Stillmore, Georgia: the post-1980 surge in less skilled and heavily undocumented immigrants in largely unregulated labor markets contributes to downward wage and employment rates at the bottom of the labor market. While many consumers and employers (individuals and firms) have certainly benefited substantially from the surge in undocumented low-skilled workers since the mid-1980s, these research findings suggest a need for policy interventions that ensure socially acceptable wage levels, employment opportunities, and working conditions for our least advantaged workers, both native- and foreign born.

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<sup>1</sup> Louis Uchitelle, New York Times, March 9, 2001 (p. A1)

<sup>2</sup> David Barboza, New York Times, December 21, 2001 (p. A26)

<sup>3</sup> Rebecca Blank, “A Review of the Labor /Market Discussion in the 2006 Economic Report of the President,” *Journal of Economic Literature*, Sept. 2006 (669-673)

<sup>4</sup> Philip Brasher, “ID theft probe began in Marshalltown,” The Des Moines Register, Dec. 13, 2006.

<sup>5</sup> Evan Perez and Corey Dade, “An Immigration Raid Aids Blacks – For a Time,” *The Wall Street Journal*, p. 1.

<sup>6</sup> The assumption here is that we do not live in a world characterized by what economists call “factor price equalization,” in which unhindered trade ensures that all factors (including low skill workers) are everywhere paid the same (have the same price). This has the interesting consequence that there is no economic incentive for labor migration in the first place (see Friedberg and Hunt, 1999, p. 344).

<sup>7</sup> On the unfortunate role that theoretical priors appear to play in other areas of recent empirical research, see Freeman (2005).

<sup>8</sup> There are several advantages to this ‘jobs’ approach. First, if wage-setting is best understood as a response to supply, demand and institutional forces at the jobs level in a particular sector, using jobs as the unit of analysis (for example, laborers in construction firms, or cashiers in retail outlets) should be preferable education or education-experience groups).

<sup>9</sup> Borjas seems to concede here that the evidence does not show large native outflows in response to immigrant inflows.