

PEOPLE AND MACHINES: A LOOK AT THE EVOLVING RELATIONSHIP BETWEEN CAPITAL AND SKILL IN MANUFACTURING, 1860–1930, USING IMMIGRATION SHOCKS

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Abstract—This paper estimates the elasticity of substitution between capital and skill in manufacturing using immigration-induced variation in skill mix across U.S. counties between 1860 and 1930. We find that capital initially complemented both high- and low-skill labor (determined by literacy) and, unlike today, was more complementary with low-skill labor. Around 1890, capital increased its relative complementarity with high-skill labor. Simulations calibrated to our estimates imply the level of capital-skill complementarity after 1890 allowed the manufacturing sector to absorb the large wave of Eastern and Southern European immigrants with only a modest decline in less-skilled relative wages. This would not have been possible under the older production technology.

I. Introduction

PERSISTENTLY rising inequality in the United States is once again raising concerns that technological change is outpacing many workers' ability to adapt to it (Acemoglu & Restrepo, 2017; Brynjolfsson & McAfee, 2011). These concerns echo with stunning similarity those of earlier times of disruption, like the Great Depression (Jerome, 1934; Keynes, 2008) and industrialization (Marx, 1932).¹ Indeed, the conventional view is that the sorts of changes now leading to greater inequality have been ongoing since at least the early twentieth century (Goldin & Katz, 1998). In this view, capital's greater complementarity with the most skilled workers, combined with the falling relative cost of capital (which embodies much of technological change), have pushed up relative demand for high-skill labor (see also Krusell et al., 2000).²

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¹ See “The Future of Jobs: The Onrushing Wave,” *Economist* (January 2014), for a recent view or Mokyr, Vickers, and Ziebarth (2015) for a historical perspective on “technological anxiety” and economists' views about the relation (race) between technology and workers. Chui, Manyika, and Miremadi (2016) give a business perspective on the opportunities for automation and challenges arising from technological advances.

² The reason inequality in the United States has not always been on an upward trajectory, despite this, is that at some times in U.S. history, this trend has been offset by rising education levels (Goldin & Katz, 2008). A more nuanced view is that technological change is polarizing rather than

A body of indirect or correlational evidence, however, suggests that in the nineteenth century, capital in manufacturing instead may have relatively complemented the least-skilled workers (Atack, Bateman, & Margo, 2004; James & Skinner, 1985). In this view, early factory production was both more low-skill intensive and capital intensive than the artisanal production it pushed out (Goldin & Sokoloff, 1984).³ Even these changes may not have been entirely “deskilling,” though, as artisans may have been less skilled (lower paid) than the “new” workers needed to support mass production, such as managers and other white-collar workers (see Chandler, 1977; Katz & Margo, 2014).⁴ Later, however, new machines enabling continuous mass production in some industries were associated with rising skill intensity (Goldin & Katz, 1998).

Our project has two aims. The first is to credibly estimate the change in the relationship between capital and skill between the nineteenth and twentieth centuries for the entire manufacturing sector in a way that allows quantitative comparisons.⁵ To achieve this, we use a common skill metric, data sources, and identification strategy over the period 1860 to 1930. Our identification exploits the impact that immigration had on different U.S. counties' skill mix, following similar strategies used in research on the labor market impact of immigration (Card, 2001; Cortés, 2008; Tabellini, 2017) and on capital adjustments in modern manufacturing (Lewis, 2011) and early twentieth-century agriculture (Lafortune, Tessada, & Gonzalez-Velosa, 2015).⁶ Explained in greater detail below, it applies a shift-share style instrumental variable (IV) strategy that apportions national arrivals, by skill and origin, to their ethnic enclaves in a base year. Our paper is the first to bring a credible estimation approach to estimating how capital changed its relationship with skills from

purely inequality increasing (Acemoglu & Autor, 2011; Autor, Levy, & Murnane, 2003; Goos & Manning, 2007; Gray, 2013; Katz & Margo, 2014). See Lindert and Williamson (2016) for a more complete historical overview of inequality and economic forces in the United States.

³ For example, Brown and Philips (1986) describe technologies developed in the canning industry to replace skilled cannery workers with machines maintained by low-skill workers as a way to undercut artisans' high wages and bargaining power.

⁴ Case studies describe nineteenth-century advances that created jobs for both low- and high-skill workers (Chin, Juhn, & Thompson, 2006; Meyer, 2005). However, Katz and Margo (2014) show that more capital-intensive factories in 1880 had an overall higher low-skill labor share (table 1.3, panel B), consistent with deskilling.

⁵ The impact of technical change outside manufacturing may have been different (Katz & Margo, 2014), but technical change in manufacturing was an important driver of broader changes in skill demand in the period we examine.

⁶ Goldin (1994) was the first to use this approach in historical data and Griliches (1969) to use regional skill mix differences to identify capital-skill complementarity.

1860 to 1930 for manufacturing as a whole in quantitative terms. Credible causal inference is key here: if satisfied, our estimates can be interpreted not just as the sign of the complementarity between capital and skills (which is what the existing literature has delivered), but rather, under the assumptions of a model described below, as production function parameters that say exactly how much a unit of capital affects the productivity of high- and low-skill workers and how this changes over time. Our estimates can, in particular, be used in simulations.

The use of immigration variation is not incidental. Our project's second aim is to gain insight into how much relative wages in the manufacturing sector would have needed to change to adapt to the influx of immigration during this period. In theory, the impact of immigration-driven skill mix changes on relative wages can be muted when capital complements skills compared to when it does not (Lewis, 2013).⁷ Whether this makes any difference at realistic parameter values, however, has not been evaluated. Our estimates will be used to determine if they do.

We measure skill with literacy, observed at the county level in Censuses of Population microdata (Ruggles et al., 2010) and tabulations. Its key advantage is that it is a prelabor market skill metric (the only one widely available prior to 1940); the only other option in this era is to use occupation-based definitions more likely to reflect demand conditions. Furthermore, immigration, especially in the early twentieth century, had a substantial impact on literacy rates (see appendix figure B.2), so understanding how nonlabor inputs respond to literacy is relevant to the labor market impact of immigration. The downside of using literacy, a binary skill metric, is that it does not allow us to test for more subtle interactions between capital and skill, of the sort that would lead to "polarization" (Gray, 2013; Katz & Margo, 2014). But polarization in this era is largely driven by the simultaneous creation of white-collar jobs in manufacturing needed to coordinate production on a large scale (Chandler, 1977, though, importantly, this may have also been driven by other forces, like the falling price of power and better rail networks, and not just capital deepening), which provides two other reasons that we use literacy. First, literacy rates are, throughout our sample period, monotonic across a three-skill-group occupational ranking that separates out white-collar work.⁸ Second, white-collar work in manufacturing was limited, especially in the early part of our sample period.⁹ However, it remains true that our approach may not capture what is going on at the very high-skill end of the labor market. As a check, we simulate out how much the changing occupational mix of literate and illiterate

workers affects our estimates, but we leave to future research a strategy to separately identify the impact of an area's supply of potential white-collar workers.

Our outcomes come from industry-city (or industry-county) tabulations of the Censuses of Manufacturing from 1860 to 1930, which we digitized. We estimate the aggregate response of capital intensity to a change in the literacy rate, which we allow to differ in the first and second half of this period. We also estimate responses within detailed industries, which enables us to both assess how much the aggregate results are confounded by shifts in industry mix and explore industry heterogeneity in our results.¹⁰

This brings us to the main findings. Our estimates suggest that (predicted) immigration had a significant impact on literacy rates in local labor markets. Although this first result is very basic, it is also important. Without it—if, as it has been suggested, U.S. labor markets were highly geographically integrated by intercity migration (as suggested by Rosenbloom, 2002)—our approach would not be feasible. Furthermore, aggregate capital intensity responded differently in the nineteenth and twentieth centuries to similar skill mix shocks. Between 1860 and 1880, capital's response was consistent with it being a q-complement of both high- and low-skill labor, and, unlike today, the complementarity was stronger with low-skill labor, largely supporting the "deskilling" view of nineteenth-century manufacturing. This response changed dramatically after about 1890, when capital became relatively more complementary with high-skill and a q-substitute for low-skill labor (like today).

An a priori plausible alternative interpretation is that the change in complementarity is an artifact of literate workers being in more middle-skill jobs in the early period and more high-skill jobs in the late period in the face of a constant polarizing effect of capital on skill demand. In simulations (in appendix A.1), however, we are unable to account for our findings by occupational shifts.

We also find that shifts in industry mix had a minimal role in mediating the response of capital to skill mix and, therefore, that immigration induced large within-industry changes in skill ratios.¹¹ Despite this conclusion, simulations of a parametric production function calibrated to our estimates suggest that the manufacturing sector could have absorbed the massive wave of immigration in the early twentieth century with comparatively modest changes in relative wages. Specifically, our simulations investigate the impact of the flow of illiterate immigrants after 1897. Although this flow raised the relative supply of illiterate labor by 35%, our parameter estimates suggest at most, a 7% decline in illiterate relative

⁷ Similarly, the relative wage impacts of skill mix shocks may be muted during periods when modes of production of substantially different factor intensities overlap (Beaudry, Doms, & Lewis, 2010; Caselli & Coleman, 2006) like artisan and factory production.

⁸ See appendix figure A.1, which uses occupational ranks from Katz and Margo (2014), where we show the fraction of literate workers in each of the skill groups over the period we study.

⁹ It did not exceed 10% of manufacturing jobs until 1910 (Katz & Margo, 2014).

¹⁰ We also assess how much shifts in industry mix absorb skill shocks. Although theory suggests these adjustments can be significant (Leamer, 1995), estimates using recent data are small in magnitude. However, there is evidence that shifts in industry mix were important in the early twentieth century in the farm sector (Lafortune et al., 2015).

¹¹ This finding reinforces the idea that the larger response of industry mix to immigration in the agriculture sector during this same period (as documented in Lafortune et al., 2015) may be due to the lack of specificity of capital in agriculture.

wages was needed to absorb them into manufacturing, implying an effective elasticity of substitution of around 5, similar to what Goldin and Katz (1998) estimate between high school graduates and dropouts prewar. Illiterate workers' relative productivity was supported by a slower increase in capital intensity. In contrast, under nineteenth-century technology parameters, which lacked the same capital substitution possibilities, absorbing the same immigration would have pushed down the illiterate workers' relative wages by as much as 35%. In short, our results indicate that production technology was central in the U.S. economy's ability to absorb this wave of immigrants.

II. Theoretical Framework

Our work starts from a simple framework that considers a single (aggregate) production function with three production factors: capital (K), high-skill labor (H) and low-skill labor (L), a common formulation in both the immigration and the technology adoption literature (see, for example, Lewis, 2011, 2013). So let $Y = g(H, L, K)$, where Y is aggregate output, which represents the sum of all firms in a city c .¹² We assume the production function is constant returns to scale and satisfies standard quasi-concavity constraints ($g_j < 0$ and $g_{jj} < 0 \forall j \in \{H, L, K\}$).¹³ Throughout, we also assume that the capital is supplied elastically.¹⁴ Wage levels are determined at the local level.

Under these assumptions, the capital stock adjusts to maintain equality between its marginal product and the cost of capital, which implies that in equilibrium, $d \ln(\frac{\partial Y}{\partial K}) = 0$. Under constant returns to scale, this translates into

$$d \ln K = \frac{L \frac{\partial^2 Y}{\partial K \partial L}}{H \frac{\partial^2 Y}{\partial K \partial H} + L \frac{\partial^2 Y}{\partial K \partial L}} d \ln L + \frac{H \frac{\partial^2 Y}{\partial K \partial H}}{H \frac{\partial^2 Y}{\partial K \partial H} + L \frac{\partial^2 Y}{\partial K \partial L}} d \ln H. \quad (1)$$

Subtracting $d \ln L$ from both sides of this, we derive the following expression, which describes the impact of a change in the endowment of high-to-low-skill workers on the capital-to-low-skill labor ratio, $d \ln(K/L) = \kappa d \ln(H/L)$, where

$$\kappa = \frac{H \frac{\partial^2 Y}{\partial K \partial H}}{H \frac{\partial^2 Y}{\partial K \partial H} + L \frac{\partial^2 Y}{\partial K \partial L}}.^{15}$$

The denominator of κ is positive if the production function displays decreasing returns to capital, which was assumed.

¹² Individual labor markets may differ in overall TFP, say, $Y_c = A_c \times g(H, L, K)$, where A_c is TFP, but otherwise have identical production functions.

¹³ Decreasing returns to capital appear to have been the norm in this era from estimates provided by James (1983) and Cain and Paterson (1986).

¹⁴ Evidence on regional interest rate differences (see Davis, 1965, 1995) appears to be driven by differences in risk levels (Bodenhorn, 1995), not inelastic capital supply, and are roughly constant over the period under study (Bodenhorn & Rockoff, 1992).

¹⁵ The total derivative $d \ln(\frac{\partial Y}{\partial K}) = d \ln g_K$ can be written out as $\frac{H g_{KH}}{g_K} d \ln H + \frac{L g_{KL}}{g_K} d \ln L + \frac{K g_{KK}}{g_K} d \ln K$. Set this equal to 0 and solve for $d \ln K = -\frac{H g_{KH}}{K g_{KK}} d \ln H - \frac{L g_{KL}}{K g_{KK}} d \ln L$. By homogeneity, $-K g_{KK} = H g_{KH} + L g_{KL}$, which when substituted in produces expression (1). Also, as it is assumed that $g_{KK} < 0$, the denominator is positive.

Therefore, the sign of the numerator indicates input complementarity with high-skill labor: capital and high-skill labor are q-complements if $\frac{\partial^2 Y}{\partial K \partial H} > 0$ and q-substitutes if $\frac{\partial^2 Y}{\partial K \partial H} < 0$. One can also subtract $d \ln H$ from both sides of equation (1) to derive a symmetric expression for the complementarity between capital and low-skill labor from the response of the capital-to-high-skill labor ratio to changes in the relative endowment of high-skill workers. The problem with this approach is that it is not robust to mismeasurement of who is high and low skill, a serious concern in the economic Census data we use (which contains only crude categories of "skill"). If our empirical definition of L on the left-hand side includes some high-skill workers, what we would get instead is a weighted average of the complementarity between capital and high-skill labor and capital and low-skill labor. Worse, in the earliest census data we have, we can observe only the total workforce, $N = L + H$. Defining high-skill share $\phi_H = H/N$, the best we can observe in these years is

$$d \ln(K/N) = (-\phi_H(1 - \kappa) + (1 - \phi_H)\kappa) d \ln(H/L) = (\kappa - \phi_H) d \ln(H/L). \quad (2)$$

Note that the relationship between K/N and H/L is not dispositive on its own, of the level of complementarity between capital and either type of labor. However, comparing it with ϕ_H indicates whether capital and high-skill labor are complementary or substitutes and the relative degree of that relationship compared to that of low-skill workers.

We can also obtain similar information by evaluating the response of the capital-output ratio, given by

$$d \ln(K/Y) = (s_L \kappa - s_H(1 - \kappa)) d \ln(H/L), \quad (3)$$

where $s_H = H \frac{\partial Y}{\partial H} / Y$ is high-skill labor's output share and $s_L = L \frac{\partial Y}{\partial L} / Y$ is the low-skill's share. If capital is particularly complementary to low-skill labor, we would thus anticipate a negative response of the capital-output ratio to an increase in the skill ratio.

By combining the two equations, we can estimate more precisely, since

$$\begin{aligned} \ln(K/N) + \phi_H \ln(H/L) &= \kappa(\ln(H/L)), \\ \ln(K/Y) + s_H \ln(H/L) &= \kappa((s_L + s_H) \ln(H/L)), \end{aligned}$$

that is, we estimate these equations jointly and impose that κ is the same in both.

κ measures absolute q-complementarity between capital and skills. To interpret it, recall that $\frac{\partial \ln(K/Y)}{\partial \ln(H/L)} > 0$ defines what is often called capital-skill complementarity (Goldin & Katz, 1998; Krusell et al., 2000), a condition under which capital proportionately raises the marginal product of high- more than low-skill labor. From above, the two are related by $-s_H + (s_L + s_H)\kappa > 0$, or $\kappa > \frac{s_H}{s_L + s_H}$. Thus, this joint estimation of κ allows us to draw simple conclusions about the relationship between inputs:

- If $\kappa < 0$, then capital and skills are q-substitutes and capital and low-skill workers are q-complements.
- If $\frac{s_H}{s_L + s_H} > \kappa > 0$, then both types of labor are q-complements with capital, but capital is more complementary to low-skill labor than high-skill labor.
- If $1 > \kappa > \frac{s_H}{s_L + s_H}$, then both types of labor are q-complements with capital, but capital is more complementary to high-skill labor than low-skill labor.
- If $\kappa > 1$, then capital and high-skill labor are q-complements and capital and low-skill labor are q-substitutes.

The response of capital affects the response of relative wages to skill mix changes, as

$$\frac{d \ln(W_H/W_L)}{d \ln(H/L)} = \frac{\partial \ln(W_H/W_L)}{\partial \ln(H/L)} + \frac{\partial \ln(W_H/W_L)}{\partial \ln K} \frac{\partial \ln K}{\partial \ln(H/L)}, \quad (4)$$

where $\frac{\partial \ln(W_H/W_L)}{\partial \ln(H/L)}$ represents the short-run (capital fixed) relative wage adjustment to a change in relative skill supply, which is negative. Equation (4) says that the long-run relative wage impacts of a change in skill ratios (say, induced by immigration) may be smaller or larger than this depending on the relative complementarity of capital with skill. If capital complements high- relative to low-skill labor, so that $\frac{\partial \ln(W_H/W_L)}{\partial \ln K} > 0$ and $\frac{\partial \ln K}{\partial \ln(H/L)} > 0$, then the long-run wage response is diminished by capital adjustment.¹⁶ Relative wage impacts are larger than this when capital is skill neutral.

A. Alternative Models

Apparent shifts in capital intensity are potentially confounded by endogenous shifts in the industry mix (so-called Rybczynski effects). With one historical exception (Lafortune et al., 2015), these have generally been found to be small in response to immigration-induced skill mix shocks (Card & Lewis, 2007; Gonzales & Ortega, 2011; Lewis, 2003). The primary way in which we will address this is with industry controls, a key motivation for our data collection.

Analogously, as Beaudry and Green (2003) suggest, if there are two modes of production of different factor intensities, then the economy can respond to changes in relative factor supplies by choosing a different mode of production rather than just moving along the same isoquant. In this era, this change might be represented by a shift between “artisanal” and “factory” production, where the latter is thought to be more capital intensive. Researchers typically proxy for factor production with plant size (Kim, 2007); therefore, we will also study plant size as an outcome.

¹⁶ While for this to be true, it is necessary that capital be not just a relative but an absolute complement of skill, in this three-factor setup, capital is always an absolute q-complement of skill ($\partial^2 Y / \partial K \partial H > 0$) whenever it is a relative q-complement of skill (that is, whenever $\frac{\partial \ln(W_H/W_L)}{\partial \ln K} > 0$). As $H \frac{\partial^2 Y}{\partial K \partial H} + L \frac{\partial^2 Y}{\partial K \partial L} = -K \frac{\partial^2 Y}{\partial K^2} > 0$, the larger cross-derivative must be positive.

III. Empirical Methodology and Data Description

A. Baseline Equation

Following the results from our model, we begin by estimating,

$$\begin{aligned} \ln \left(\frac{K}{J} \right)_{ct} &= \gamma_{\text{early}} \ln \left(\frac{H}{L} \right)_{ct} \times 1(t \in \{\text{early}\}) \\ &\quad + \gamma_{\text{late}} \ln \left(\frac{H}{L} \right)_{ct} \times 1(t \in \{\text{late}\}) \\ &\quad + v_c \times 1(t \in \{\text{early}\}) + \delta_c \\ &\quad \times 1(t \in \{\text{late}\}) + \eta_t + \epsilon_{ct}, \end{aligned} \quad (5)$$

for $J = N, Y$, where $(K/J)_{ct}$ corresponds to either the capital per worker (K/N) or the capital-output ratio (K/Y) in county c at time t , $(H/L)_{ct}$ is the high-to-low-skill labor ratio in the county c at time t , $1(t \in \{\text{early}\})$ and $1(t \in \{\text{late}\})$ are, respectively, indicators for the first and second half of our sample period (defined below), and v_c and δ_c represent county and η_t time effects, respectively. Standard errors are calculated to be robust to arbitrary error correlation within county, and regressions are unweighted. We construct the skill ratio using all individuals age 15 and above.¹⁷

To estimate how the production function changes over time, we divide our sample into two periods, “early” and “late,” and allow γ to switch between the two, because we cannot feasibly estimate separately by decade.¹⁸ As historical analyses by Chandler (1977) and Jerome (1934) argue that the Second Industrial Revolution transformed the productive process of manufacturing, we look for a change around 1880 to 1890, during which some of the elements of the Second Industrial Revolution took place.

The interpretation of the coefficient γ depends on the outcome. In equation (3), for example, where $\ln(K/Y)$ is the outcome, γ captures the complementarity between capital and skill relative to capital and low skill: γ will be positive if capital complements high relative to low-skill labor ($\gamma > 0$ implies that $\partial \ln(W_H/W_L) / \partial K > 0$).

Equation (5) is motivated in part by Goldin and Katz’s (1998) argument that capital skill complementarity arises across sectors (in their model, across the combination of a machine- and goods-producing sectors): it examines how manufacturing’s overall capital intensity adjusts to the changes in local skill mix. Estimates of equation (5) may alternatively be viewed as suffering from aggregation bias: shifts in output mix toward industries that use a different factor mix could confound the results. This is why we also collected industry-county data, which allow us to estimate

¹⁷ Restricting the sample to only men does not affect our results; excluding blacks weakens the first stage but has little impact on the reduced form. Results are available on request.

¹⁸ Even when we divide the sample into twenty-year intervals, the results are very noisy. For K/Y , they appear to indicate a clear break between 1890 to 1900, just as we find using our “early” and “late” separation.

responses within industry:

$$\begin{aligned} \ln\left(\frac{K}{J}\right)_{cit} &= \gamma_{\text{early}} \ln\left(\frac{H}{L}\right)_{ct} \times 1(t \in \{\text{early}\}) \\ &+ \gamma_{\text{late}} \ln\left(\frac{H}{L}\right)_{ct} \times 1(t \in \{\text{late}\}) \\ &+ \nu_c \times 1(t \in \{\text{early}\}) \\ &+ \delta_c \times 1(t \in \{\text{late}\}) + \eta_t + \mu_{it} + \epsilon_{cit}. \end{aligned} \quad (6)$$

In this specification, the outcome, $(K/J)_{cit}$, also varies at the industry, i , level, and the specification includes controls for unrestricted trends in aggregate capital intensity by industry, μ_{it} .¹⁹ This regression is also unweighted, and standard errors will again be clustered at the county level.²⁰ Estimates of equations (6) and (5) will differ only if industry mix shifts systematically with skill mix. We also test this directly by using it as an outcome variable—the share of labor—in industries that use some factors more intensively.²¹

B. Identification Strategy

Although our estimation equation and model are tightly linked, in practice identification is an issue: workers' location decisions are likely at least influenced by where their skills are most highly compensated. Depending on how our outcomes are correlated with unobserved determinants of relative skill demand, we could be over- or underestimating the relationships of interest. On this point, it is important to note that manufacturing is only one sector in the broad economy—a minority of employment—so local demand shocks outside manufacturing could be an important source of endogeneity.²² It is thus difficult to sign the bias a priori. OLS estimates might also be attenuated by estimation error in the skill ratios.²³

To solve these problems, we attempt to identify local relative supply shocks using a combination of immigration-driven shocks for the foreign born and aggregate level skill endowment variation for the native born. Specifically, the instrument is given by

$$\ln(\widehat{H/L})_{ct} = \ln\left(\frac{\sum_k \left(\frac{N_{ck0}^{\text{native}}}{N_{k0}^{\text{native}}}\right) H_{kt}^{\text{native}} + \sum_j \left(\frac{N_{cj0}^{\text{imm}}}{N_{j0}^{\text{imm}}}\right) H_{jt}^{\text{imm}}}{\sum_k \left(\frac{N_{ck0}^{\text{native}}}{N_{k0}^{\text{native}}}\right) L_{kt}^{\text{native}} + \sum_j \left(\frac{N_{cj0}^{\text{imm}}}{N_{j0}^{\text{imm}}}\right) L_{jt}^{\text{imm}}}\right), \quad (7)$$

where N_{kt}^{native} is the total number of native-born individuals born in state k living in the United States at time t , N_{ckt}^{native} is the total number of native-born individuals living in county c and born in state k at time t , N_{jt}^{imm} is the total number of foreign-born individuals born in country j living in the United States at time t , and N_{cjt}^{imm} is the total number of foreign-born individuals born in country j living in county c at time t . H and L denote high- and low-skill workers, respectively.

Our instrument thus apportions both high- and low-skill individuals to a county c according to the share of individuals born in country j , for the foreign-born, or born in state k , for the native-born living in county c in some base year 0. As immigrants' origin mix shifted substantially in the late nineteenth century, we will use two base years: 1850 for 1860 to 1880 and 1880 for 1890 to 1930. While we could use a rolling basis approach, we elected to stick to two years since these correspond to the only two years in which 100% counts can be employed. Similarly, we use all natives, regardless of their skill level, to compute baseline shares for native workers.

Thus, our instrument represents a predicted skill ratio based on the interaction of initial conditions and national changes in the skill and country composition of workers. Given it is structured like the actual skill ratio, a first-stage coefficient of 1 means that predicted immigration-driven changes in skill mix have a one-for-one impact on the actual skill ratio; coefficients different from 1 imply that the actual skill mix is offset by either native migratory response or other offsetting demographic changes (e.g., if trends in native-born literacy differed in high- and low-immigration markets).

This instrument will be valid unless workers thought about the future evolution of the manufacturing sector selecting their location and later flows of workers changed in response to those shocks differentially by skill. Note that it is not invalidated just by immigrants of a given skill level coming to the United States when the demand for their skill is highest, since we include time fixed effects. It is also not invalidated just because a location was permanently attractive to immigrants, since we include county fixed effects. One threat would be if local demand shocks were highly correlated over time. We know of no evidence of this from the era studied, but modern evidence suggests local demand shocks do not persist much beyond a decade (Blanchard & Katz, 1992), which is the minimum lag we use. Another threat would be if information on local demand shocks was relayed through migrants to the origin and influenced aggregate flows of that group. To be materially important, such a threat would require a significant concentration of individuals of a given country of origin in one single location, which is largely not the case.

¹⁹ In theory, it is possible to also control for industry $\times$ county effects, but since the right-hand side does not vary at this level, in practice it does not affect the estimates to do so.

²⁰ Results are very similar if we weight them by population.

²¹ We also used a different strategy where we ran our city-industry outcomes against fixed effects by industry, including industry $\times$ year and industry $\times$ area levels and used the averaged city residuals as the outcome, with similar results.

²² Manufacturing ranges from roughly one-quarter to one-third of employment in identified cities over the years in our sample (authors' calculations using Ruggles et al., 2010).

²³ We can get some sense of the magnitude of this by using tabulated data on literacy rates by area (Minnesota Population Center, 2011), which are available for some of the years in our sample. The comparison between our estimated literacy rates and the tabulated ones, conditional on the full set of fixed effects, suggests that OLS estimates might be 10% to 15% attenuated due to measurement error.

One potential exception is New York City, which was the top destination for many immigrant groups, but our results are not sensitive to dropping either or both of the two largest destinations, New York and Chicago.²⁴

Our strategy would also be invalidated if immigrants chose cities where they anticipated that their skills were going to become more valuable in the future (say, Germans settling in place with a future high relative skill demand). This concern is again attenuated by modern evidence that demand shocks are not forecastable over long horizons, but also by using the stock of all immigrants (not only the ones of a given skill level) to apportion both high- and low-skill workers. A final concern would be if illiterate immigrants also arrived with less cash and this influenced the local capital markets directly. However, statistics we digitized from government reports from this era (U.S. Bureau of Immigration, 1932) show that the cash carried by immigrants averaged 100 times less than capital per worker. This information and other evidence that there was well-functioning national capital market in this era make us believe that this issue is of little concern.

While our IV estimates may still not be unbiased, they are likely to be at least less biased than OLS and, if anything, in the same direction. Thus, a comparison with OLS may still allow us to evaluate in what direction a better instrument would lead us.

C. Data and Descriptive Statistics

We tabulate the number of high- and low-skill individuals in a locality using IPUMS data (Ruggles et al., 2010) from 1860 to 1930 (except in 1890, which are estimated from 100% tabulations in U.S. Department of Interior, U.S. Census Office, 1897; see section C.5 in the appendix). As we explained in section I, we use literacy as the best available proxy for prelabor market skill. All of the inputs into our instrument, equation (7), are similarly constructed by aggregating IPUMS data, in this case including 1850 as a base year. The 1890 data were constructed from tabulations, in some cases by interpolating between 1880 and 1900 (see appendix C).

Our outcome variables focus on the adjustment mechanisms in the manufacturing sector over this period. Our estimation framework calls for data at the level of the labor market $\times$ industry. We obtained these data from published Manufacturing Census tabulations, which we entered. Conveniently for our analysis, manufacturing censuses occurred roughly concurrently with the Census of Population over this entire period. They are available in published tabulations, which we have digitalized.²⁵

While we obtained measures for a variety of outcomes, here we focus on capital, labor, and output, the ingredients of

our theoretical framework.²⁶ Value of products and costs are available for the full period, which allows us to define value-added as our measure of output (Y). We use the total number of workers as our measure of labor (N). Value of capital, our key variable, is available from only 1860 to 1920, and we impute 1930 values from the relationship with horsepower in 1910–1920 ($\ln(\widehat{K}) = 0.77839346\ln(\text{Horsepower})$). We also look at a measure closer to “machinery” capital. We first use horsepower directly. This variable is available for 1910 to 1930. Before 1910, we impute horsepower from machinery capital for 1890 and 1900, which is separately reported in these two years. (The estimated relationship from state-level tabulations is $\widehat{\text{Horsepower}} = 0.004 \times M\&E\text{Capital}$.) Before 1890, neither horsepower nor machinery is tabulated, but in the Atack and Bateman (1999) microdata sample, we find evidence that very few firms had positive horsepower in 1860. We thus replace our measure of horsepower with 0.1 for 1860 for all industries and counties.²⁷ Finally, capital utilization may also respond to skill ratios, so we also examine expenditure on fuel and rent of power as a proxy for this while again using 0.1 for all pre-1890 data.

We use IPUMS data to estimate $\phi_H = 0.85$, the fraction of workers in manufacturing who were literate, and this plus our manufacturing tabulations to estimate $s_L = 0.0787$ and $s_H = 0.5085$ illiterate and literate workers’ share of production costs, implying a cutoff κ for capital-skill complementarity of 0.866 ($= \frac{0.5085}{0.5085+0.0787}$). Further details on how this was calculated and how we harmonize geography and industries over time are described in the appendix. Descriptive statistics are shown in appendix table C.8.

IV. Results

A. First Stage

Appendix table B.3 shows the first-stage regressions estimated in the industry $\times$ county level data, combining all years of data.²⁸ To account for both the fact that there are multiple “copies” of a county within a year and for the fact that the errors are likely autocorrelated over time, we cluster standard errors by county. The results suggest that a change of 1% in our predicted skill ratio translates to about a 0.70% change in the actual skill ratio of a county if we split the sample at 1880 and about 0.95% if we split it at 1890. Neither is sensitive to industry controls. The fact that it is less than 1 could be consistent with an endogenous location choice by natives and immigrants, some deterioration of the settlement patterns from historical patterns, or both. That it is larger than 0 is inconsistent with the view that labor markets were perfectly integrated, à la Rosenbloom (2002). Our instrument

²⁴ They are also robust to allocating individuals using the “leave-out” mean for the aggregate flows.

²⁵ See appendix C for an exact description of all tables we entered for this project.

²⁶ Some may worry about the data quality for 1860, but results in our second panel are robust to the exclusion of the first year.

²⁷ Including this information does little to alter the results of our late period but does allow us to estimate a parameter for the early period, which is why we chose to make this assumption.

²⁸ We followed Wooldridge (1997) to instrument for the interaction terms.

TABLE 1.—ESTIMATION WITH AGGREGATE DATA

	OLS		IV	
	K/N (1)	K/Y (2)	K/N (3)	K/Y (4)
A. Early Period Is 1860–1880				
$\ln(H/L) \times \text{Early}$	0.094** (0.045)	0.021 (0.050)	0.389** (0.184)	−0.135 (0.184)
$\ln(H/L) \times \text{Late}$	0.034 (0.061)	−0.150 (0.203)	1.192*** (0.462)	0.992** (0.434)
R^2	0.907	0.935	0.891	0.922
RootMSE	0.799	0.721	0.718	0.655
B. Early Period Is 1860–1890				
$\ln(H/L) \times \text{Early}$	0.082** (0.039)	0.014 (0.043)	0.062 (0.194)	−0.321* (0.164)
$\ln(H/L) \times \text{Late}$	−0.084 (0.143)	−0.385 (0.334)	1.327** (0.541)	0.931** (0.469)
R^2	0.908	0.937	0.898	0.927
RootMSE	0.803	0.718	0.696	0.634

All outcomes in logs. All regressions include area $\times$ period, and year effects and are unweighted. Right-hand-side variable is $\ln(\text{literate/not literate})$ in the age 15+ population (except 1890, which uses published tabulations of the age 10+ population of the area). Standard errors robust to arbitrary error correlation within area shown in parentheses. $N = 994$. Significant at *10%, **5%, and ***1%. K includes capital imputed for 1930 from horsepower. See the data appendix.

follows the evidence in Lafortune and Tessada (2013) showing the ethnic network's critical role in the determination of the first location of immigrants arriving in the United States.

Our estimation equation interacts the skill ratio with period indicators for early or late periods. Following Wooldridge (1997), in IV, it is efficient to interact the predicted values from this joint first stage ($\hat{X}$), rather than the instrument itself, with our period dummies. We do this and obtain a very strong first stage for each subperiod of analysis (as can also be seen in the appendix).

B. Responses of Capital

Having shown that our instrument is capable of generating significant variation in the endogenous variable, we now turn to explore how capital intensity responded to the change in the skill ratio generated by immigration. Table 1 shows results at the aggregate level, using only variation across areas. Columns 1 and 3 examine capital per worker and columns 2 and 4, capital per dollar of output. The first two columns present the OLS, while the last two show the IV estimates. OLS seems to show limited responses of capital ratios to change in skill ratios: there is a positive and significant correlation between capital intensity and skill ratio in the early period. The IV estimates, however, suggest that capital per worker positively responded to an immigration-induced increase in skill ratios in both early and late periods, although the early effect is significant only in panel A. The impact on capital-output ratios is negative and significant for the early period when using 1890 as the last year of the early period, and positive and significant for the late period with both cutoffs. From these aggregate results, we would thus conclude that capital-skill complementarity strengthened over the period and that capital and low-skill workers were q-complements in the early part of our sample.

A concern with the results in table 1 is that they are potentially driven by shifts in industry mix. For example, less-skilled workers may attract less capital-intensive industries (Goldin & Katz, 1998; James & Skinner, 1985), altering aggregate capital ratios. One way to address this is to ask how much the factor intensity of the typical industry responded to a change in skill ratios. To do so, we divided industries into quartiles on, alternatively, their national capital or skill intensity in the first year it was measurable in the data (1860 for K/N and 1890 for H/L).²⁹ We then treat the share of employment in each quartile as a separate outcome in a city $\times$ year regression.

Results are in table 2. The response of employment shares in quartiles 1 to 3 of capital per worker are shown in columns 1 to 3, respectively, and in quartiles 1 to 3 of literate or illiterate in columns 4 to 6. (In each case, the fourth quartile, which is not separately shown, is the negative of the sum of the first three quartiles.) The table shows no strong evidence that the aggregate skill ratio significantly influences industry composition. The strongest finding is that industries in the second quartile of H/L grew more with the local H/L ratio in the early period, at the expense of all other quartiles (mainly the third). Note that this means, if anything, that industry mix was shifting slightly in the opposite direction of our instrument, supporting its validity as a labor supply shock measure.

The capital intensity of the average industry does not shift significantly with our instrument. Summing up by multiplying the responses by the factor-ratio in each quartile (bottom row), we find that, if anything, shifts in industry mix work against finding an aggregate impact on capital intensity in the direction we expect. From 1860 to 1880, the shifts in industry mix imply an increase in capital intensity of 22% per unit increase in skill ratio. For the later period, there is an implied decrease in capital intensity of 3% from 1900 to 1930 and 16% decrease from 1890 to 1930.

Furthermore, we can directly examine the extent to which there are within-industry responses to aggregate skill mix changes, using our new data on production techniques detailed by area and industry. Table 3 shows IV estimates of the relationship between skill mix and capital measures at the industry $\times$ area level.³⁰ The first three columns of each panel present results for capital-worker ratios, and the last three columns present results for the capital-output ratios. For each outcome and period, we successively increase the number of additional fixed effects, starting from none, to fixed effects for industries and finally, for industry $\times$ year.

Table 3 shows a negative relationship in the early period between skill ratio and capital per worker for the later cutoff and capital per output for both cutoffs. In the later period, we find consistently that the skill ratio is associated with more capital per worker and per output within industry. Using our framework, this would imply that in the early period,

²⁹ We also tried using the average value for available all years, with similar results.

³⁰ OLS results (not shown) show a positive association between both measures of capital intensity of a similar magnitude in both periods.

TABLE 2.—IMPACT ON FACTOR INTENSITY OF INDUSTRY COMPOSITION, INSTRUMENTAL VARIABLES

Quartile:	Ranked by Their K/N			Ranked by Their H/L		
	1	2	3	1	2	3
A. Early Period Is 1860–1880						
$\ln(H/L) \times \text{Early}$	−0.063 (0.059)	−0.086 (0.063)	0.026 (0.080)	0.019 (0.049)	0.106** (0.051)	−0.121** (0.053)
$\ln(H/L) \times \text{Late}$	0.126 (0.152)	−0.060 (0.145)	0.012 (0.162)	−0.081 (0.189)	0.178 (0.153)	0.083 (0.113)
Reduced form F -statistic (early)	0.501	3.128	0.058	0.098	2.335	9.937
Reduced form F -statistic (late)	0.544	0.165	0.005	0.116	1.102	0.422
B. Early Period Is 1860–1890						
$\ln(H/L) \times \text{Early}$	−0.008 (0.075)	−0.015 (0.046)	0.066 (0.081)	0.001 (0.061)	0.169*** (0.063)	−0.053 (0.046)
$\ln(H/L) \times \text{Late}$	0.067 (0.133)	−0.031 (0.150)	−0.047 (0.145)	−0.039 (0.168)	0.114 (0.123)	0.025 (0.094)
Reduced form F -statistic (early)	0.003	0.030	0.258	0.021	4.876	0.805
Reduced form F -statistic (late)	0.175	0.028	0.066	0.036	0.613	0.046
Average K/N or H/L	335.059	620.050	955.658	5.627	5.749	5.917

Outcome is share of low-skill workers employed in specified quartile of the distribution of industries. Top quartile, not shown, is minus the sum of coefficients on other quartiles. All regressions include area $\times$ period and year effects and are unweighted. Right-hand-side variable is $\ln(\text{iterate/not iterate})$ in the age 15+ population except 1890, which uses published tabulations of the age 10+ population of the area. Standard errors robust to arbitrary error correlation within area shown in parentheses. $N = 888$. Significant at *10%, **5%, and ***1%.

TABLE 3.—MANUFACTURING OUTCOMES, INSTRUMENTAL VARIABLE ESTIMATES

	Capital/Worker			Capital/Output		
	(1)	(2)	(3)	(4)	(5)	(6)
A. Early Period Is 1860–1880						
$\ln(H/L) \times \text{Early}$	0.072 (0.120)	0.040 (0.136)	0.080 (0.129)	−0.180 (0.245)	−0.203 (0.263)	−0.170 (0.248)
$\ln(H/L) \times \text{Late}$	0.747 (0.589)	0.216 (0.315)	0.121 (0.323)	0.516 (0.424)	0.213 (0.277)	0.146 (0.302)
R^2	0.239	0.519	0.563	0.056	0.235	0.297
RootMSE 0.894	0.711	0.677	0.752	0.677	0.649	
Reduced form F -statistic (early)	0.094	0.099	0.013	1.155	1.154	0.779
Reduced form F -statistic (late)	9.957	1.811	1.191	3.344	0.601	0.426
B. Early Period Is 1860–1890						
$\ln(H/L) \times \text{Early}$	−0.325** (0.162)	−0.216 (0.139)	−0.156 (0.126)	−0.479** (0.191)	−0.397** (0.181)	−0.345** (0.169)
$\ln(H/L) \times \text{Late}$	0.701** (0.340)	0.275 (0.260)	0.216 (0.260)	0.814** (0.352)	0.574* (0.297)	0.493* (0.291)
R^2	0.240	0.513	0.559	0.025	0.210	0.277
RootMSE	0.893	0.715	0.680	0.764	0.688	0.658
Reduced form F -statistic (early)	3.682	0.755	0.082	4.637	2.411	2.003
Reduced form F -statistic (late)	8.404	2.530	1.916	8.570	4.703	3.731
Industry FE	No	Yes	Yes	No	Yes	Yes
Industry $\times$ Year FE	No	No	Yes	No	No	Yes

All outcomes in logs. All regressions include area $\times$ period and year effects. Right-hand-side variable is $\ln(\text{iterate/not iterate})$ in the age 15+ population (except 1890, which uses published tabulations of the age 10+ population of the area). Standard errors robust to arbitrary error correlation within area shown in parentheses. Sample is restricted to industry-years where at least two cities in that year reported a given industry. $N = 37,312$. Significant at *10%, **5%, and ***1%. K includes capital imputed for 1930 from horsepower. See the data appendix.

capital was relatively more complementary with low- than with high-skill workers, but its relative complementarity with low-skill labor fell (and its relative complementarity with high-skill labor increased) as time passed. As we will show, the magnitudes of the positive coefficient in the late period may even suggest that capital and low-skill workers became q-substitutes.

OLS estimates at both the aggregate and levels at best reveal this much more weakly. A plausible contributor to bias in this context is measurement error attenuation since we are using a crude self-reported measure of skill conditional on a large number of fixed effects. However, there are other sources of bias. A key unobservable might be the local outside (nonmanufacturing) option of low-skill workers. For instance, to take a Goldin and Sokoloff (1984) type of story,

certain areas may have very productive agricultural land. In such regions, low-skill workers might be drawn to the area but not to manufacturing, which could reduce the adoption of capital- and low-skill-intensive production techniques.

The results are consistent in both timing and nature with what some historians have previously argued: that in the nineteenth century, capital was a relative substitute of high-skill labor and became a relative complement only sometime later in the nineteenth or early twentieth century. In our analysis so far, however, we have imposed that all industries transformed at the same time. To investigate heterogeneity, in appendix table B.5, we add three-way interactions of skill mix, period, and industry characteristics. Somewhat surprisingly, we find no evidence that capital-skill complementarity arose differentially in industries that Chandler (1977) identified as

amenable to mass production and which Goldin and Katz (1998) show exhibit greater capital and skill intensity.³¹ If anything, we find that industries that initially tended to have larger establishments exhibited less change in the direction of capital-skill complementarity.

However, directly examining the response of plant size to skill does provide some support for capital-skill complementarity and mass production being related. Recall that the skill-induced changes in capital intensity we observe in each period may at least be partly mediated through shifts between modes of production (artisanal $\rightarrow$ factory; factory $\rightarrow$ continuous production); for example, early factories were thought to be low skill and capital intensive relative to the alternative, artisanal production. A commonly used proxy for “factory” production is plant employment (Atack et al., 2004). So putting $\ln(\text{employment})$ on the left-hand side, IV estimates in appendix table B.6 reveal that, indeed, the relationship between skills and plant size was initially negative and became dramatically more positive in the later period. This information may redeem, to some extent, the view that the transformation of the production function enabled production on a larger scale, even if it is not specific to the industries that Chandler studied.³²

Our measure of capital includes land and buildings, which may be an imperfect proxy for machinery, on which we would prefer to focus. We thus turn to our two alternative measures of capital: horsepower (which in some years is predicted from the value of machinery and equipment) and fuel expenditures and rent of power. These two measures are available only from 1890 on, but we (very crudely) extrapolated their value to be near 0 only for 1860 as well. However, since our instrument requires at least two years within each subperiod, this implies that we cannot estimate the effect of a change in the skill ratio when the early period includes only 1860 to 1880. Thus, table 4 includes only one panel. The format of the table mirrors that of the previous one except that for each outcome, we now have two different measures. Columns 1 to 3 and 7 to 9 measure capital from fuel expenditure and the others from horsepower and its proxy. An important difference between this table and previous ones is that here, the early period is essentially an 1890 cross-section, limiting our capacity to make comparisons. This difference may explain why in this table, we do not estimate a significant negative coefficient as we found previously for the early period. However, for both measures, we see strong and positive effects of the skill ratio on capital per worker in the late period, suggesting that the exogenous arrival of more high-skill workers increased the use of these measures of capital. The results for capital-output ratios are also positive and significant. The results for fuel expenditures remain significant after controlling for the

full set of fixed effects. If we use our theoretical framework, we would again draw a similar conclusion as before: fuel expenditures and horsepower became more complementary to high-skill workers around the turn of the twentieth century.

We now use both equations simultaneously to directly estimate the structural parameter κ and report the results in table 5. The first three columns use our regressions of the value of capital, the next three use our fuel expenditures, and the last three, our horsepower measure. For each of these outcomes, we also explore the impact of controlling for fixed effects.

We do gain statistical power by imposing some structure on our estimates. We can now argue that capital increased the marginal productivity of low-skill labor in the early period when using the value of capital or fuel expenditures as our measure of K . Our measure of κ is statistically larger than 0 in all cases for capital and horsepower, and it is also less than 1 except in one case. It is also smaller than 0.866 (the cutoff value) for capital and horsepower, although not statistically different. This information would suggest that for the early period, capital was q-complements of both types of labor, but capital was slightly more complementary to low-skill than to high-skill labor. For 1890 to 1930, we estimate $\kappa > 1$, implying that capital would have, if anything, lowered the marginal product of low-skill labor. When we define our late period to include only 1900 to 1930, we find even larger values for κ , suggesting that capital became a q-substitute to low-skill labor around 1900. The difference between the early and late estimates for capital is not statistically significant in the top panel but always is in the bottom. For fuel expenditures and horsepower, the estimates are always significantly different one from another.

We have explored how sensitive our results are to our assumptions and have found little reason to believe that the results we present would look different if we had used the estimates of parameters for any other years.³³ In particular, given that s_H and ϕ_H were actually larger in the later period, the results we present are potentially an understatement in terms of the change we measure in κ .

Two main criticisms could be made about our results. The first is that immigration in itself was changing over this period, and maybe the changing relationship we are capturing is simply due to the fact that capital was more complementary to the first wave of European migrants to America than the second. However, we do not find any evidence that counties that had a high share of the “old” immigrants versus the “new” ones saw less of a change in κ . If anything, they may have observed a larger one, making that hypothesis less credible.³⁴ Second, one may think that if there was polarization in the economy over this period, our changing relationship could be due to the fact that within literate and illiterate groups, the type of workers was changing and that each type had a different level of complementarity with capital. Appendix A.1 provides what would be our regression estimate under the

³¹ In our implementation, Chandler’s industries are butter and cheese, flour, sugar, all bottling, syrups, cigarettes, paints, oils, gas/petroleum, glass, and iron. Goldin and Katz (1998) identify a similar list.

³² We also explored if responses differed according to county size (see, for example, Hannon, 1982), but do not find evidence of heterogeneity along this dimension (results are available on request).

³³ Results are not presented but available on request.

³⁴ See appendix table B.7 for results.

TABLE 4.—ALTERNATIVE CAPITAL MEASURES, INSTRUMENTAL VARIABLE ESTIMATES
EARLY PERIOD IS 1860–1890

	Capital/Worker						Capital/Output					
	Fuel Expenditures			Horsepower			Fuel Expenditures			Horsepower		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
$\ln(H/L) \times \text{Early}$	0.101 (0.471)	0.341 (0.479)	0.252 (0.505)	−0.157 (0.682)	0.079 (0.620)	0.039 (0.587)	−0.349 (0.436)	−0.153 (0.443)	−0.246 (0.474)	−0.650 (0.703)	−0.423 (0.643)	−0.461 (0.605)
$\ln(H/L) \times \text{Late}$	1.293*** (0.469)	0.654** (0.289)	0.585** (0.281)	1.391*** (0.538)	0.606 (0.414)	0.380 (0.378)	1.420*** (0.492)	0.949*** (0.351)	0.875*** (0.339)	1.502*** (0.546)	0.892** (0.444)	0.658 (0.403)
R^2	0.882	0.910	0.924	0.734	0.808	0.845	0.868	0.890	0.906	0.689	0.753	0.798
RootMSE	1.333	1.162	1.068	1.321	1.122	1.008	1.308	1.191	1.103	1.300	1.158	1.049
Reduced form F -statistic												
Early sample	0.667	0.031	0.035	0.165	0.545	0.246	1.453	0.706	0.900	0.034	0.004	0.034
Late sample	20.133	9.125	8.206	8.813	1.338	0.813	24.915	16.892	15.183	8.621	2.261	1.705
Industry FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry $\times$ Year FE	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes

All outcomes in logs. All regressions include area $\times$ period and year effects and are unweighted. Right-hand-side variable is $\ln(\text{literate/not literate})$ in the age 15+ population (except 1890, which uses published tabulations of the age 10+ population of the area). Standard errors robust to arbitrary error correlation within area shown in parentheses. Sample is restricted to industry-years where at least two cities in that year reported a given industry. $N = 26,300$ for fuel expenditures and $N = 27,258$ for horsepower. Significant at *10%, **5%, and ***1%. “Horsepower” includes imputed measures for 1880 and 1890 from machinery and equipment. See the data appendix.

TABLE 5.—STRUCTURAL ESTIMATE OF RELATIVE CAPITAL-SKILL COMPLEMENTARITY

	Capital			Fuel Expenditures			Horsepower		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Early Period Is 1860–1880									
$\kappa \times \text{Early}$	0.836*** (0.182)	0.802*** (0.205)	0.847*** (0.193)						
$\kappa \times \text{Late}$	1.642*** (0.619)	1.115*** (0.350)	1.016*** (0.366)						
B. Early Period Is 1860–1890									
$\kappa \times \text{Early}$	0.411** (0.186)	0.527*** (0.164)	0.595*** (0.148)	0.784 (0.526)	1.048* (0.535)	0.942* (0.570)	0.461 (0.804)	0.735 (0.730)	0.690 (0.689)
$\kappa \times \text{Late}$	1.738*** (0.404)	1.317*** (0.318)	1.237*** (0.314)	2.442*** (0.561)	1.760*** (0.363)	1.676*** (0.350)	2.547*** (0.637)	1.697*** (0.500)	1.428*** (0.455)
Industry FE	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Industry $\times$ Year FE	No	No	Yes	No	No	Yes	No	No	Yes

All regressions include area $\times$ period and year effects and are unweighted. κ corresponds to the ratio of the cross-partial derivative of the production function with respect to capital and illiterate labor over the sum of the cross-partial derivatives with respect to capital and each type of labor. Standard errors robust to arbitrary error correlation within area shown in parentheses. Sample is restricted to industry-years where at least two cities in that year reported a given industry. $N = 37,312$. Significant at *10%, **5%, and ***1%. K includes capital imputed for 1930 from horsepower and “Horsepower” includes imputed values for 1880 to 1890 from machinery and equipment. See the data appendix.

assumption that there are actually three skill groups instead of two, as in the main text. Using the types of occupations used in Katz and Margo’s (2014) classification (white-collar, artisan, and low-skill workers), we then look at how these evolved in response to changes in the literacy rate. We find no evidence that the shifting composition of literate and illiterate workers between these three groups of occupations by itself would have changed the estimated κ between periods in a way that is consistent with what we estimated. This evidence strongly suggests that while polarization may have been happening during our period, it cannot be leading us to spuriously find evidence of a changing relationship between skills and capital.

V. Parametric Specifications, Calibration, and Simulation

Having estimated that the relationship between capital and skill significantly changed over our period of study and being limited by data to study the wage effects of immigration directly, we now employ a parametric specification to explore

how this changing relationship may have affected how the U.S. economy was able to absorb changes in skill mix generated by migration.

A. Setup

To simulate the wage and capital accumulation impacts of immigration, we turn to a parametric form for our single-good model of production in section II. Capital-skill complementarity is generally modeled using a nested CES structure, which can group together either capital and high-skill labor (Goldin & Katz, 1998; Krusell et al., 2000) or capital and low-skill labor (Autor et al., 2003; Lewis, 2011) in the inner nest. As in Lewis (2013), we run simulations using the function

$$Y = A \left(\alpha(\beta K^\theta + (1 - \beta)L^\theta)^{\rho/\theta} + (1 - \alpha)H^\rho \right)^{1/\rho}, \quad (8)$$

where $\theta > \rho$ implies relative capital-skill complementarity.

Under this parameterization, we show in appendix A.2 that

$$\kappa = \frac{(1 - \rho)s_H(1 - s_H)}{(1 - \rho)s_H(1 - s_L - s_H) + (1 - \theta)s_L} \quad (9)$$

and

$$\frac{d \ln(W_H/W_L)}{d \ln(H/L)} = (\rho - 1) + \frac{(\theta - \rho)(1 - s_L - s_H)\kappa}{1 - s_H}. \quad (10)$$

When capital is more complementary to skills, the second term of equation (10) is positive. Thus, as in section II, the magnitude of the relative wage response to changes in skill mix is smaller than predicted by the short-run inverse elasticity of substitution ($\rho - 1 < 0$). In the appendix, we derive similar expressions for Goldin and Katz's (1998) nesting, which has capital and high-skill labor in the inner nest but is otherwise similar.

B. Parameter Values

Equation (10) shows the parameters needed to simulate the wage response. We have estimates of s_L and s_H described earlier (in section IIIC), as well as κ for different periods of our data, but we do not have parameter estimates of ρ or θ . Obtaining estimates of ρ is especially problematic due to a lack of disaggregated wage data, which means we do not have sound, direct estimates of equation (10). To deal with this, we assumed different values of the parameter ρ , where $(1 - \rho)^{-1}$ represents the short-run elasticity of substitution between high- and low-skill labor.³⁵ We then set θ to be consistent with our estimates of κ , subject to equation (9) and assumed values of capital and high-skill labor's share.

Appendix table A.2 maps out the parameter estimates and relative wage impact of a 1 unit change in $\ln(H/L)$ implied by various assumed parameter estimates. The top panel assumes, as Goldin and Katz (1998) did, that the outer nest is Cobb-Douglas ($\rho = 0$). As a benchmark, we start by assuming that capital is not more or less complementary to skill (i.e., is "skill neutral") by setting $\theta = \rho = 0$, as shown in row 1. This simulation implies that relative wages fall one-for-one as skill ratios rise. (More generally, the relative wage impact of a 1 unit increase in $\ln(H/L)$ is given by $\rho - 1$ in this "skill-neutral" case $\theta = \rho$ (see equation [10]).

Row 2 of that table calculates parameters consistent with our estimate of κ for the early period of 0.7 (roughly, the average of our within-industry estimates for 1860 to 1880 and 1860 to 1890 as the early period in table 5). This implies $\theta = -0.85$, which leads to wage impacts that are larger in magnitude than the capital-neutral benchmark in row 1, as capital adjustments magnify the relative productivity impact of changes in skill supply.

In contrast, as noted in section II, if $\kappa > \frac{s_H}{s_L + s_H} \approx 0.866$ —so that capital and skill are relative complements, as in our later period—then the relative wage impacts are smaller than the benchmark case. Picking a conservative estimate of $\kappa \approx 1.1$ for the later period from table 5 implies a large, positive estimate of $\theta = 0.77$ (row 3). Even with this conservative estimate, the impact of the change in skill ratio on the

relative wage is deeply attenuated by the response of capital, with magnitudes of about one-third of the "skill-neutral" benchmark case shown in the table's first row. Interestingly, in modern data and using a similar approach, Lewis (2011) estimates a κ only slightly more positive (albeit for different skill categories, high school dropouts and completers) than the one we obtain for the late period, consistent with Goldin and Katz's (1998) argument that modern capital-skill complementarity is a continuation of a similar relationship between labor and nonlabor inputs in this earlier era. Rows 4 and 5 show that larger estimates of κ would imply smaller (in the extreme, 0, using Autor et al., 2003) wage impacts.

How sensitive are these relationships to different parameter choices? The pattern of relative magnitudes is not sensitive to the choice of our least well-justified parameter ρ , the one that governs the elasticity of substitution between skill types. For example, the bottom panel shows the same set of simulations, with instead ρ set at 0.33, which is roughly what you would need to get to the consensus value for the elasticity of substitution between college and noncollege labor in the modern U.S. labor market (Hamermesh, 1993). The absolute wage impacts are smaller in this panel (by design of the larger elasticity), but the proportional difference across rows varies in nearly the same way as the upper panel (e.g., the estimates in row 8 are about one-third of those in row 6). The estimated wage impact in the later period would also be even smaller if, realistically, the capital or skill share were even larger in the later period.³⁶

Interestingly, the estimates in the lower panel of appendix table A.2 are also roughly in line with the reduced-form elasticity of substitution between artisans and laborers implied by estimates in Goldin (1994), whose estimates come from the middle of our period of study.³⁷ Given the large differences in methodology, perhaps not too much should be made of this result; nevertheless, because of this similarity, we use the estimates in the lower panel to simulate the impact of counterfactual immigration flows in the next section.

C. Simulating the Impact of Immigration

The 1 unit increase in $\ln(H/L)$ used in appendix table A.2 simulations may not be typical of the impact of immigration. Therefore, we turn to simulations based on the experience of the U.S. economy with immigration during the period of our estimates. Table 6 shows estimates of the impact of immigration on wage ratios in manufacturing under various counterfactual immigration scenarios, using the estimated capital responses from the period under study to generate the parameter values, under the continuing assumptions that

³⁶ Additional discussion, including an example of impacts under different share parameters, appears in appendix B.

³⁷ Using Goldin's (1994) estimates of the impact of immigration on the wages of artisans and laborers, one can derive that the reduced-form elasticity of relative wages with respect to changes in skill mix is in the range of -0.66 to -0.22 , which is consistent with the wage impacts in rows 6 to 8 of the lower panel of appendix table A.2. In appendix A, we present how to obtain and convert her estimates.

³⁵ For verification, we compare our simulations to estimates in Goldin (1994).

TABLE 6.—IMPACT OF COUNTERFACTUAL IMMIGRATION FLOWS ON HIGH-SKILL RELATIVE WAGE

Year	Counterfactual Scenario	Literate/Not Literate Ratio			% Impact on High-Skill Relative Wage (4)	Table A.2 Wage Elasticity Used (5)
		Actual (1)	Counterfactual (2)	Gap (in ln) (3)		
A. Using 1860–1880 Estimated Production Function						
1880	No net immigration, 1860–1880	5.06	4.67	−0.08	8.15%	Row 7, column 5
B. Using 1890–1930 Estimated Aggregate Production Function						
1900	Literacy test imposed in 1897	7.94	8.25	0.04	−0.75%	Row 8, column 5
1910	Literacy test imposed in 1897	10.17	12.72	0.22	−4.31%	Row 8, column 5
1920	Literacy test imposed in 1897	13.58	19.18	0.35	−6.65%	Row 8, column 5
1930	Literacy test imposed in 1897	21.03	29.76	0.35	−6.69%	Row 8, column 5
C. Using 1860–1880 Estimated Production Function						
1900	Literacy test imposed in 1897	7.94	8.25	0.04	−3.88%	Row 7, column 5
1910	Literacy test imposed in 1897	10.17	12.72	0.22	−22.43%	Row 7, column 5
1920	Literacy test imposed in 1897	13.58	19.18	0.35	−34.62%	Row 7, column 5
1930	Literacy test imposed in 1897	21.03	29.76	0.35	−34.84%	Row 7, column 5

Data source for skill ratios: U.S. Census of Population (Ruggles et al., 2008). Literacy rates computed for all those (both men and women) who were at least age 15. Counterfactual in panel A constructed by adding together natives present in 1880 with immigrants present in 1860. In panels B and C, counterfactuals were constructed by dropping illiterate immigrants who reported a year of immigration after 1897 from the sample. The numbers in column 4 were computed by multiplying the wage elasticity in the row of table A.2 designated in column 5 of this table by the counterfactual change in skill ratios shown in column 3 of this table.

$\rho = 0.33$, $s_L = 0.08$, and $s_H = 0.51$. Panel A of table 6 simulates the impact of net immigration between 1860 and 1880 using the production function we estimated for that period. Comparing the actual and counterfactual ratios of literate to nonliterate population, columns 1 and 2 reveal that absent of net immigration in this period, skill ratios would actually have been about 8% lower.³⁸ During this era (at least nationally), immigrants had higher literacy rates than natives. According to the parameterization in appendix table A.2, row 7, column 5, removing immigrants who came between 1860 and 1880 would have raised high-skill relative wages by about 8%, which is equivalent to saying that net immigration during that era raised low-skill relative wages by roughly 8%. Capital intensity was also rising during this era, and our complementarity estimates suggest this also would have raised low-skill relative wages. Thus, both immigration and technological change during this era likely had the effect of compressing the wage distribution of natives.

The remaining rows of table 6 examine what would have happened if the literacy test Congress passed in 1897 had become law.³⁹ This is done under two different scenarios: first, using the production function we estimated for the late period in the aggregate (panel B); and second, using the production function we estimated for the early period (panel C). Panel C therefore asks what the impact of this low-skill wave would have been had production technology not changed.

To implement this simulation, we drop from the Census of Population sample (Ruggles et al., 2010) any illiterate immigrants who arrived after 1897 and compute the counterfactual skill ratios. Column 2 of table 6 shows that this exercise raises

skill ratios over time, by 1920 substantially, about 35%. To do the middle panel simulations, we take the wage elasticity in row 8 of appendix table A.2. Column 4 shows that the literacy test might have lowered high-skill relative wages by 7%; put differently, the illiterate arrivals who stayed in the United States after 1897 appear to have lowered low-skill relative wages by 7%. This wage impact is quite modest, given the magnitude of arrivals over this period and the related outcry, and the sensitivity analysis in the previous section suggests the wage impacts could have been smaller than this.⁴⁰ However, even these alternatives are modest compared to what the relative wage impact would have been had the production technology in use in the early twentieth century remained the same as it had been in our early period. Using that wage elasticity, illiterate relative wages might have declined 35%. Thus, the new role of capital in production—the ability to heavily substitute away from it at a fixed rental rate—may have played an important role in the absorption of large waves of immigrants at the turn of the twentieth century.

VI. Conclusion

In this paper, we find that immigration between 1860 and 1930 altered skill ratios in the local labor markets of urban counties. Manufacturing capital intensity responded to these changes in skill ratios differently in the nineteenth and twentieth centuries.⁴¹ Specifically, the estimates imply that manufacturing capital relatively substituted for high-skill labor in nineteenth-century manufacturing. This result appears to have dramatically changed around the turn of the century when low-skill workers became substitutes for capital and ushered in the level of capital-skill complementarity

³⁸ This calculation is made by imposing that the same number of literate and illiterate immigrants present in the United States in 1860 would have been present in 1880, and the native skill mix would have remained as actually observed.

³⁹ Goldin (1994) investigates the history of attempts to pass immigration restrictions in the United States. According to her research, 1897 was the first credible attempt to impose a literacy test. In that year, a bill made it through Congress but was vetoed by President Cleveland.

⁴⁰ This analysis also leaves out that most native-born workers, who were literate, would have had higher, not lower, wages as a result of this inflow, according to our simulations and to what Tabellini (2017) finds.

⁴¹ Responses are within industry: we find little support for the idea that shifts in industry mix helped absorb skill mix changes during this era.

we see now. We do not find that the change in the relationship between capital and skills was specific to the kinds of industries that Chandler (1977) identified as amenable to mass production, though the increased association between skill and capital intensity occurs along with a dramatically increased association between skill and plant size.

Fitting our estimates to a parametric production function, we simulated the potential wage impact that immigration had on U.S. local economies at the time. These simulations suggest the importance of the production technology in allowing the U.S. economy to absorb the wave of low-skill European migrants in the early twentieth century. We estimate that the production technology in use at that time allowed a sufficient rate of substitution away from capital (at a fixed rental rate) in response to the low-skill labor shock to employ these immigrants with only a modest decline in low-skill relative wages. Under the older production technology in which capital complemented low-skill labor, this substitution would not have been possible. This historical context thus reveals that the way in which nonlabor inputs adjust to labor mix shocks can play a critical role in the economy's ability to adapt to immigration. Research in other contexts may be warranted.

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