Essays on Employer Credit Screening, Manufacturing Skill Gaps, and the Relationship between Skill Demands and Capital Intensity

by

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Abstract

This thesis consists of three essays that explore hiring criteria, skill demands and labor market outcomes. The first essay investigates the use of worker credit status as a hiring screen. The practice has sparked debate, with opponents asserting that it amounts to discrimination and proponents maintaining that it is an important tool for employers to assure the quality of new employees. This study uses a unique identification strategy along with credit proxy variables in a national dataset to test whether credit status reveals information about an employee's character that is predictive of employee productivity. The study finds that the character-related portion of credit status is not a significant predictor of worker productivity.

The second essay addresses the question of whether U.S. manufacturers face a skill gap in hiring production workers. This study explores the issue by presenting and analyzing results from an original, nationally representative survey of U.S. manufacturing establishments that directly measures concrete employer skill demands and hiring experiences. The results indicate that demand for higher-level skills is generally modest, and that three quarters of manufacturing establishments do not show signs of hiring difficulties. Among the remainder, demands for higher-level math and reading skills are significant predictors of long-term vacancies, but, contrary to some theories of technical change, demands for computer skills and other critical-thinking/problem-solving skills are not. In terms of mechanisms, factors that complicate the interaction of supply and demand and that are associated with communication/coordination failures appear to play an important role.

The third essay combines data from an original manufacturing skill survey with industry-level data on capital and equipment to explore the connection between capital intensity and current skill demands. The results indicate that contemporaneous capital intensity does predict higher-level skill demands, but the effect is driven by higher-level reading skills rather than the math and computer skills that dominate the current debate. With regard to historical patterns, the study finds that the relationship between historical capital intensity and current skill demands changes over time, with increasing capital investment per worker showing opposite effects in the 1990s and 2000s.

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Essay 1: Is Credit Status a Good Signal of Productivity?

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Introduction

With the recent economic downturn and deterioration in household finances, the issue of the use of personal credit status as a criterion in the hiring process has drawn increased scrutiny (Hughes 2012, Murray 2010, Martin 2010). Evidence from employer surveys indicates that about half of employers use credit reports as screens for at least some of their hires (SHRM 2012, SHRM 2010a, b). In 2010, the Department of Labor won a court case against Bank of America over the improper use of credit checks as an employee-screening device (Traub 2013). Ten states have passed laws limiting the practice, and over the past few years legislation has been introduced to ban credit screening at the federal level in both the House of Representatives and the Senate (Dwoskin 2013, NCSL 2012, Deschenaux 2011). Employers and their advocates maintain that credit checks are a useful tool in judging the quality of some potential hires. They cite the importance of credit checks in assessing employee character and minimizing fraud (SHRM 2010a, b). Opponents of the practice, by contrast, see the use of credit reports as a backdoor form of discrimination. They maintain that personal credit status is not related to job performance and that the practice is likely to disproportionately harm minority workers due to the lower incomes and greater incidence of poor credit in minority populations (EEOC 2010, Fremstad 2010, Fellowes 2006).[1]

This topic is also of interest due to the growing use of Big Data in hiring and employment practices. In many ways, the management and use of databases containing personal credit files over the past few decades foreshadows the rapidly expanding use of a broader range of personal

[1]Title VII of the Civil Rights Act of 1964 bans employment practices or policies that have disparate impacts on members of protected groups.

data that is occurring today. The same issues about the validity of making hiring decisions based on observed correlations arise with Big Data as with the case of credit checks.

Despite the prevalence of the practice, little is known about the relationship between credit status and productivity. The literature on the relationship between credit status and labor market outcomes is thin. The industrial psychology literature focuses on the correlation between credit status, worker characteristics and employment outcomes such as performance evaluations (Bernerth et al. 2012a, 2012b; Palmer and Koppes 2003, 2004). However, these studies are typically focused on single organizations and do not include causal research designs. The statistical discrimination literature in economics has tested whether increased information from employer screening mechanisms leads to an improvement in the labor market outcomes of stigmatized groups (Autor and Scarborough 2008). For example, does the increased availability of criminal records improve labor market outcomes for minority job applicants who do not have criminal records (Holzer et al. 2006, Finlay 2008, Stoll and Bushway 2008)? However, this literature has generally not tested whether the underlying characteristic—credit status in the case of this paper—is in fact predictive of labor market productivity.

The principal challenges in tackling the topic of the impact of credit status on labor market productivity are data availability and causation. Datasets that combine detailed individual credit reports with economic and demographic variables are hard to come by. Credit reports are proprietary, and even were they available, they lack the longitudinal labor market data that researchers often rely on to disentangle difficult economic questions. Leaving aside the challenges of measuring credit status, it might seem that we could simply compare over time the labor market outcomes of individuals with good and bad credit to determine whether bad credit harms productivity. However, simple correlations in national datasets cannot tell us whether factors relating to an employee's credit status are causing bad labor market outcomes, or economic shocks related to bad labor market outcomes are causing the observed credit status. Furthermore, as will be discussed below, there are multiple potential motivations for employer use of credit checks; the validity of these different motivations cannot be simultaneously tested with a single identification strategy. In this paper I focus on testing a key belief that employers frequently cite in support of credit checks: the idea that credit status reveals productivity-relevant information about an employee's character. I specifically explore the following question: would it help employers up front in the hiring process to know about any hidden character traits associated with bad credit? The literature shows that hidden measures of ability such as an individual's Armed Forces Qualifying Test (AFQT) score are highly predictive of future productivity (Farber and Gibbons 1996, Altonji and Pierret 2001). Is knowledge about the character-related elements of bad credit status similarly predictive?

To address the data challenge, I rely on proxy variables for credit quality contained in the National Longitudinal Survey of Youth from 1979 (NLSY79). The NLSY79 contains a rich set of credit-relevant variables, including data on net worth, rejections of credit applications, and credit card debts, among other indicators. Because employers examine the contents of credit reports rather than credit scores (see below for discussion), and because these proxy items reflect the components of credit reports, this dataset allows us to make valid assessments of credit quality.

To rigorously test whether employer beliefs about credit status and character are justified, I employ a unique two-part identification strategy. First, because we cannot disentangle causal effects when we observe current credit status, I isolate the causal impact of the character effect by using the *future* value of an individual's credit status as a measure of any time-invariant, person-specific ("character-related") aspects of bad credit status. I then employ a firm-learning model to test whether character-related credit status has any predictive value for an individual's productivity in the labor market. The results of this analysis indicate that the portion of credit status that is related to an employee's time-invariant character does not have a significant relationship with the employee's productivity as the employer learns about the employee's unobserved characteristics. This outcome contrasts sharply with the predictive power of AFQT scores. Although there are other potential rationales for the use of credit checks, these results call character-related rationales into question.

Credit History Background

Any individual who accesses credit—including credit cards, car loans, and mortgages—will generally have a record of credit-related activity at a firm called a credit reporting agency (or credit bureau). There are three major credit reporting agencies in America: Equifax, Experian, and TransUnion. The records these firms keep are generally referred to as credit reports or credit histories. To indicate the general quality of an individual's credit report, I will use the term "credit status." It is important to make a distinction between credit reports/histories and credit scores. A credit score is a single number that is designed to represent the level of credit risk a particular individual represents to a lender. Although the credit score is based on the contents of an individual's credit report, it is the result of an algorithm that is specific to consumer lending. Employers access credit reports, but they do not typically look at credit scores (EEOC 2010).

Although the format of credit reports varies by credit bureau, all of the various reports contain the following components (Avery et al. 2010):

- Personal information (address, social security number, etc.)
- Summary of credit accounts and payment history (including late payments, etc.)

- Information on non-credit related bills in collection (utility bills, medical bills, etc.)
- Inquiries (requests from companies to see the credit report)
- Monetary-related public records (judgments, liens, bankruptcies, etc.)

The Fair Credit Reporting Act (FCRA), which was passed by Congress in 1980, gives employers the right to request a job applicant's credit report after obtaining the individual's written permission.[2] Although this theoretically gives the applicant a measure of control, the employee has very little bargaining power as the employer is free to make signed permission a condition of application. I will refer to the act of an employer accessing a job applicant's credit report as a "credit check."

Theory and Evidence: Employer Beliefs about Credit Status

To explore the connection between credit status and worker productivity, it is necessary to be more specific about the various scenarios that lead to bad credit and the employer beliefs associated with these scenarios. Bad credit can result either from choices that an individual makes or from events that are beyond the individual's control. An employer, in turn, could care about an employee's credit status for several reasons. On the one hand, the employer could review a credit report in order to try to learn about an enduring characteristic of the employee, such as a personality trait or a fixed non-cognitive skill. The idea in this case is that the personal attributes that lead to bad credit will also lead to low productivity on the job. This unproductive behavior could simply be subpar performance, or it could include fraud or negligence. I will heuristically refer to this employer belief as the "character" screen.[3]

Employers could also care about credit status because they believe that a difficult personal financial situation will prevent the employee from doing a good job, regardless of

[2]See 15 U.S.C. § 1681b.

[3] The character screen can be seen as an aggregate form of the personality-based hiring measures that are the focus of the industrial psychology selection literature (Bernerth 2012, Oppler 2008).

character traits. An employee who must spend time dealing with creditors may be distracted on the job, even if the ultimate cause of the credit problems was beyond the employee's control. I will heuristically refer to this employer belief as the "distraction" screen.

Having established a typology of employer beliefs about credit status, we can now evaluate what employers actually say about their reasons for using credit status as a hiring screen. Interestingly, in surveys and in legislative testimony, employers and their advocates tend to point to character-based reasons for performing credit checks. For example, in testimony before the Equal Employment Opportunity Commission (EEOC), a representative of the U.S. Chamber of Commerce stated: "Employers are much less likely to be concerned with a debt that arose as a result of a medical issue, a period of unemployment or a divorce. On the other hand, some types of debt might raise red flags more quickly such as gambling debt." The Chamber spokesman later asserts: "And I think the employers I've spoken with realize that most people out there don't have perfect credit . . . . And in my experience they're looking to find out something more than that. Is this just a rough patch someone went through, a medical issue, a divorce, or; is there something more that calls into question this potential employee's ability to represent our company with integrity?" (EEOC 2010, testimony of Michael Eastman). From a distraction point of view, the source of the debt is not important. By contrast, the Chamber's defense of employers' ability to conduct credit checks seems to rely on the argument that certain types of debt—such as gambling debt—imply something about a job applicant's judgment or character that other types of debt do not.

The Society of Human Resource Management (SHRM) is the principal employer association that both collects data on this issue and testifies on behalf of employer interests at hearings regarding employee credit checks. In a survey released in July 2012, SHRM asked employers about their primary reason for using credit reports as a screening device (SHRM 2012). Two of the top three reasons employers gave—specifically, preventing theft and assessing trustworthiness—involve the relation of credit checks to some aspect of employee character.

The SHRM survey also provides information on how employers use credit checks. Among employers responding to the survey, 64 percent stated that in certain circumstances they allow job candidates to explain the negative factors on their credit reports. Because the origin of the credit problems is not relevant to the distraction screen, this behavior primarily makes sense if employers are interested in determining whether issues of judgment or character were involved in the generation of bad credit results.

Taken as a whole, the evidence indicates that the character screen provides an important rationale for employer use of credit checks. The distraction screen may or may not be important in practice, but the public statements of employers and their advocates point to the centrality of character-based arguments for the reliance on credit status as a screening device. In this paper I will focus on testing these character-related claims.

Identification Strategy Part 1: Isolating Character-Related Credit Status

Now that we have established the importance of the character-related portion of credit status as a potential explanatory variable for labor market outcomes, the question is how to determine whether such character-related factors are predictive of employee productivity. To do so, we must first isolate this character-based component and establish causality. To the extent that we observe a correlation between an individual's bad credit and a bad labor market outcome, we cannot be sure what this correlation implies. The correlation could stem from a distraction effect, from the impact of bad character/judgment on labor market performance, or from bad luck. Indeed, the bad labor market outcome could cause the bad credit, as when an employer cuts wages due to macroeconomic conditions and the worker's credit deteriorates. We do not escape these problems even if we estimate the impact of credit status from a prior period on current period labor market outcomes because employer wage reductions or other exogenous sources of bad credit could have occurred in periods prior to the measurement of credit status. Only a strategy that both isolates the portion of credit status related to character and establishes causality can assist with an evaluation of the validity of the character screen.

To gain traction on this problem, we can develop a model that decomposes the elements of an individual's credit status. Let r_i be a measure of individual *i's* credit status at time *t,* and let b_i be the portion of an individual's credit status that reflects transient economic circumstances. Let p_i be the portion of an individual's credit status that reflects permanent character or judgment. Think of *b* and *p* as indices. Thus we have:

(1) $$r_i = b_i + p_i$$

Note that p_i does not have a time subscript. The idea is that character is a permanent attribute that persists beyond the fluctuations of the transient economic factors that otherwise affect credit status.[4] Also note that as long as both *b* and *p* are present, we can't disentangle the effect of the character-related portion of credit status by observing *r*. Furthermore, if the individual has poor credit ($r > z$, where *z* is a critical value defining bad credit) and experiences certain transient economic shocks ($b > 0$), we cannot establish the direction of causality within a given time period.[5] Suppose, though, that we identify a sample S of individuals who have credit problems at a future point in time. Let the time period during which we will measure labor

[4] For credit status to have predictive value, as opposed to being an endogenous reflection of current economic circumstances, there must be a time-invariant component of credit status that "stays with the person." In other words, if poor credit status reveals that a person has bad judgment in a way that will affect future work performance, then this bad judgment must continue over time: it must be a persistent characteristic of this individual. If it is not, then knowing about an individual's credit status today would not reveal anything about that individual's judgment or reliability tomorrow.

[5] Even if the credit problems are observed in a time period prior to the labor market outcomes, we cannot know that these problems are not the outcome of earlier labor market processes.

market outcomes (the "base" period) stretch from time *t-k* to time *t*. Let the future time period when we will measure credit status (the "future reference" period) be noted as time *t+y*. For the individuals who will one day have credit problems, $r_{i,t+y} > z f \quad i \in S$. If character-related factors are important causes of credit problems, then on average we would expect $p > 0$ across the sample of individuals with credit problems. However, while p is persistent, the transient economic factors vary over time. Although at time $t+y$ it is the case that $b_{i,t+y} > 0$, during the base period these future contemporaneous economic conditions do not yet exist. Thus we can eliminate the effect of the future contemporaneous economic factors by evaluating future credit status in the base period: $b_{i,t+y} = 0\big|_{time \leq t}$. As a result, when evaluated in the base period, the variable indicating poor credit status at time $t+y$ will be an indicator for the permanent character-related component of credit status $(r_{i,t+y} = p_i\big|_{time \leq t})$. Because the value of p is fixed over time, it will have a causal interpretation in a regression model. The use of future credit status thus isolates the character-related portion of credit status and identifies a causal effect.[6]

The principal challenge to this strategy is the possibility that $b_{i,t+y}$ is a function of b_i . In other words, if current economic conditions determine future conditions via serial correlation, then the coefficient on future credit status will be biased. I discuss the steps that I take to address this challenge below in the "Threats to Validity" section of the paper. Most importantly, I show that the bias from this correlation is negative, thus making my finding of no significantly negative effect a conservative one.

In terms of mechanics, I refer to the period in which we measure labor market outcomes as the "base period," and the period in which we measure future credit status as the "future

[6] See Altonji (2010) for an example—in a different context—of the use of future values of variables as an identification strategy.

reference period." As described below, the base period in my empirical specifications stretches from 1979 to 1992. This period is followed by a gap to address issues related to serial correlation/reverse causality stemming from the use of future credit status (see discussion below). Finally, there is a future reference period that contains indicators of credit status during the years 1999-2010. I measure credit status using a variety of proxy variables. Some of these variables cover a single year, while others cover a range of years (see data section for more details).

The above-described strategy isolates the character-related component of credit status. However, we still need to take some additional steps to complete the identification strategy. We cannot simply compare the difference in static labor market outcomes—say wage levels as an indicator of marginal productivity—between individuals with and without future bad credit for two reasons. First, there are a number of variables that employers can observe at the point of hiring but that are invisible to researchers. These include the outcome of a personal interview, elements of a candidate's resume, and factors such as school quality. As a result, we cannot perfectly control for the impact of these items on initial wage levels. Second, we are exploring the effect of a hidden character trait that is generally unobservable to employers. We expect employers to learn about this characteristic over time to the extent that it has an impact on work-related productivity. What we want to know is whether up-front knowledge about this characteristic would be helpful to employers in predicting employees' future productivity. Would such knowledge be a good signal of productivity in the same manner as an individual's unobservable AFQT score? We thus need a strategy to identify the effect of firms learning about unobservable characteristics over time.[7] The firm-learning models of Farber and Gibbons (1996;

[7] In principle, one could pursue a propensity score or matching identification strategy in order to compare the levels of outcome variables rather than the growth of these variables over time. However, in practice not only

hereafter FG) and Altonji and Pierret (2001; hereafter AP) provide a means to accomplish this goal.

Identification Strategy Part 2: Developing a Firm-Learning Model for Credit Status

Based on the discussion above, we can now develop a firm-learning model involving credit status. Let a worker's log productivity, y, be a linear function of the transient and permanent components of credit status (b and p), a vector of variables that the employer observes (G), and a vector of variables that the employer does not observe (M), along with a concave polynomial function of experience, H(x):

(2) $$y_{it} = \alpha_1 b_{it} + \alpha_2 p_i + G_{it}\alpha_3 + M_{it}\alpha_4 + H(x_{it})$$

Schooling would be an example of a "G" variable that the employer observes, while an applicant's score on a standardized test would be an example of an "M" variable that the employer does not observe. From this point on I will drop the time and individual subscripts unless they are required to clarify the model.

Employers cannot observe b, p, or M. As a result, they form expectations of these variables conditional on the variables that they do in fact observe:

$$b = E[b \mid G] + u$$

$$p = E[p \mid G] + v$$

$$M = E[M \mid G] + e$$

The final terms in equations 3-5 (u, v, and e) are the errors in employer beliefs regarding the variables that the employer cannot see. Assume that (G,M,b,p) are jointly normally distributed,

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are we are unable to match on key variables witnessed up front by employers, but, even for the variables we possess, the reduction in sample size for longitudinal datasets such as the NLSY79 through such a matching process is prohibitive. Both of these limitations point to the use of employer learning over time as an effective identification strategy.

and that all three error terms are normally distributed. We can now rewrite an employee's log productivity as a function of the variables that the employer sees:

(6) $y = \alpha_1 E[b \mid G] + \alpha_2 E[p \mid G] + G\alpha_3 + E[M \mid G]\alpha_4 + \alpha_1 u + \alpha_2 v + e\alpha_4 + H(x)$

In equation (6), $\alpha_1 u + \alpha_2 v + e\alpha_4$ represents the difference between employer expectations and true productivity, net of the effect of the experience profile. This overall employer error can be decomposed into three components: one resulting from incorrect beliefs about the transient component of credit status $(\alpha_1 u)$, one resulting from erroneous beliefs about the character component ($\alpha_2 v$), and one stemming from all other employer errors regarding the factors affecting employee productivity $(e\alpha_4)$.

Every period, the employer sees a noisy signal of the employee's true productivity. As the employer observes the employee's output, the employer witnesses its own errors as well as the random noise from the signal. Over time the employer is able to distinguish the errors in its beliefs about worker productivity $(\alpha_1 u + \alpha_2 v + e\alpha_4)$ from the random noise associated with the signal. The employer thus places greater weight on observed productivity in the wages it pays, and reduces the error it makes every period by updating its beliefs about worker productivity. We can see that if employees with bad credit status have lower productivity due to bad character/poor judgment, and if employers do not expect this negative effect due to lack of knowledge, then the portion of the error term associated with the character effect ($\alpha_2 v$) will be negative. Thus as the employer incorporates more of this observed effect on productivity into wages over time, the wages of individuals with bad credit due to character issues should fall relative to individuals with good credit. In absolute terms, the wages of all workers are rising due to increased experience (H(x)). As a result, the differential effect of the character portion of credit will show up empirically as relatively slower growth in the wages of "irresponsible" individuals compared to individuals with good credit.[8] The gist of this strategy is thus to compare the wage-experience profiles of individuals with and without the key unobservable characteristic (future bad credit).

It is worth emphasizing that this identification strategy is based on differential wage growth over time, not on initial differences in wage levels. Because we cannot perfectly control for initial wage levels due to variables that are witnessed by employers but not by researchers, and because credit problems are more prevalent among lower wage populations, we generally expect individuals with future credit trouble to have lower starting wages. Also, whatever factors cause the initial difference in wage levels, this difference is unlikely to be attributable to credit-related character as proxied by future credit status because employers only learn about this unobservable characteristic over time. From a policy point of view we are interested in whether analyzing credit status reveals hidden worker characteristics that are relevant to productivity. Initial wage differentials are attributable to factors that are observable to the employer up front. To ensure that observation of credit status is not one of these initial factors, I limit the estimation sample to individuals who do not have signs of bad credit during the base period (see discussion below).

The impact of employer learning on wages in this style of firm-learning model extends beyond individual worker-employer matches. The market learns about an employee's

[8] Measured productivity can change for more than one reason. In addition to firm learning about true productivity, it can also be the case that different individuals learn or acquire skills at different rates, thus generating heterogeneous productivity growth (Kahn and Lange 2013). Although I have motivated the above discussion by focusing on firm learning, the identification strategy and the paper's conclusions apply to the case of heterogeneous productivity growth as well. The key question is whether a measure of credit-related character can predict individuals who either have lower levels of unobserved productivity or who will experience lower productivity growth over time (due to lower learning or skill acquisition).

productivity at the same time as the employer. Thus we can apply this model to a worker's labor market experience at a succession of employers.[9]

To implement this strategy empirically, we can estimate:

$$(7) \qquad w_{it} = \beta_1 C_i + \beta_2 A_i + \beta_3 E_{it} + \beta_4 (C_i * E_{it}) + \beta_5 (A_i * E_{it}) + X_{it}\beta_6 + {}_{it},$$

Where w is log wages, C is an indicator for negative future credit status, A is a measure of the individual's cognitive ability (specifically, the individual's score on the Armed Forces Qualification Test [AFQT]), E is experience, and X is a vector containing other relevant variables, including education, the interaction of education and experience, gender, marital status, race/ethnicity, and initial occupation on entry into the labor force. The key variable of interest is the interaction between future credit status and experience.

As a benchmark, we can compare the behavior of the interaction between future credit and experience with the interaction between AFQT scores and experience. FG and AP show that AFQT scores, as an unobserved measure of ability, gain in predictive power over time as employers learn about worker productivity and adjust the wages of high-scoring individuals upward to reflect their relatively higher productivity. If it is the case that poor credit indicates something about a potential worker's time-invariant character that is harmful to productivity, then the coefficient on the interaction between future credit and experience should be negative, significant, and economically meaningful in magnitude.

[9] See FG and AP for more discussion of "public learning" regarding worker productivity. Although the exact degree of public learning can be debated, the FG and AP models appear to fit empirical labor market data well. In addition, the fact that a worker's wages do not typically fall back to their level at initial transition into the labor market provides additional evidence of some degree of public learning.

Threats to Validity

There are several potential threats to the validity of this empirical approach. First, it might be the case that credit-related character manifests itself through unemployment or time out of the labor force rather than lower productivity. Ultimately, investigating these alternative labor market responses requires a different identification strategy, as they are not revealed via firm learning. To the extent that individuals who spend time unemployed or out of the labor force eventually earn wages again, the strategy in this paper will pick up the differential wage profiles of these individuals. This paper will focus on productivity and wages, while acknowledging that more work remains to be done on other labor market responses.

Another threat is that the current and future contemporaneous economic determinants of credit status $(b_i, b_{i,t+y})$ might be correlated. In this case, even though future economic conditions don't yet exist, future credit status would not provide a clean measure of the character-related component of credit status because future credit status would be a function of current economic determinants of credit status. I minimize this concern by only selecting individuals in the base period who do not have indications of credit problems. By doing so, I am effectively comparing the wage growth of two sets of individuals who do not have obvious signs of credit trouble in the base period. The treatment group will be the set of individuals who will one day develop credit problems despite the fact that they do not show signs of these problems in the current period.[10]

[10] Individuals who have signs of bad credit in the base period as well as future bad credit exhibit lower wage growth than individuals without such signs. However, as noted in the identification strategy discussion, this negative correlation between bad credit and wage growth is not interpretable. We do not know if exogenous economic shocks or character was responsible for the initial credit problems. As a result, due to the persistence of shocks and serial correlation, we cannot assign a character interpretation to this outcome. In order to isolate the character effect, it is necessary to drop individuals who show signs of base-period credit distress.

Even after we have eliminated individuals who have signs of poor credit in the base period, we might still worry that the labor market outcomes in the base period (say, wages) are causing the future credit problems. This would result in correlation due to reverse causality. I take several steps to minimize this problem. First, I leave a gap of 7-18 years between the end of the base period (during which we measure labor market outcomes) and the future reference period (during which we measure future credit status). For example, wages are measured from 1979 to 1992, while future credit is measured from 1999 to 2010, depending on the particular credit proxy. This gap reduces issues of serial correlation and reverse causality. In addition, I eliminate individuals who report serious medical conditions at the end of the base period because such conditions are known to be a cause of credit problems and are likely to persist over time. I also control for marital status/divorce to minimize the effect of another potentially persistent factor.

Taken together, these adjustments should lessen the threat to validity. However, even after taking these steps, it is still the case that some level of serial correlation and potential for reverse causality remains. To deal with this remaining threat, it is helpful to sign the bias and incorporate this directional bias into our interpretation of the results. We can start by noting that the regressor of interest is the interaction between future credit status and experience. Due to the potential endogeneity of actual experience, I follow AP and Lange (2007) in using potential experience in the empirical specifications. Because potential experience is simply a mechanical calculation (age-education-6), the serial-correlation bias in the interaction term results from whatever bias is present in the measure of future credit. We can represent single-period bias from serial correlation in the transient component of credit status in the following manner:

(8) $$b_{i,t+1} = f(b_{it}) = \alpha b_{it} + {}_{t+1},$$

where ε is distributed ~i.i.d.(0, σ). To the extent that economic conditions from recent time periods are more highly correlated with each other than with conditions from the more distant past, we expect that $\alpha < 1$.

Given equation (8), the measure of future credit status evaluated in a prior time period, takes the following form:

(9) $$r_{i,t+y} = p_i + f(b_t)\Big|_{time=t}.$$

For a given gap y between the future reference period (T) and the base year (t), we can use the right-hand side of (8) to expand the expression to:

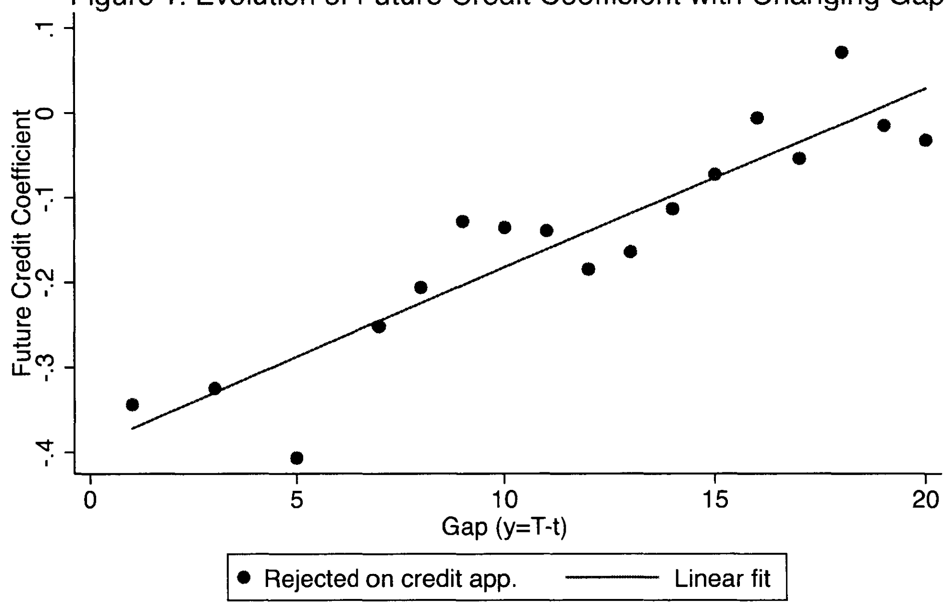
(10) $$r_{i,t+y} = p_i + \alpha^y b_t + \sum_{s=1}^{y} \alpha^{y\ \ s}\ {}_{t+s}\Big|_{time=t},$$

where y=T-t. Although future credit is evaluated in a prior period that predates contemporaneous economic conditions, serial correlation results in persistent bias. The key fact to note about equation (10), however, is that as the gap y gets arbitrarily large, for $\alpha < 1$ we asymptotically recover the unbiased measure of the character-related component of credit status (p). Although in practice the gap does not necessarily become large enough to eliminate bias, we can use the tight relationship with y to sign the bias. As y increases, the bias diminishes. If the net bias is positive, then we would expect coefficients estimated with increasingly larger gaps (bigger y) to become smaller (more negative). If, on the other hand, the net bias is negative, then we would expect coefficients estimated with larger gaps to become larger (less negative).

To test the directionality of the bias, we can take advantage of the fact that the effect of future credit status is identified by differences between individuals (each individual has only one future credit value). Thus we can estimate the future credit status coefficient through repeated cross-sections based on a single base year of data. Each base year will imply a fixed gap between the base period and the future reference period. We can then observe how the coefficient behaves as the gap increases. It is important to note that although this method eliminates the longitudinal panel structure of the data that we will use in the main empirical specifications, it retains the type of serial correlation we are concerned about. Because each individual-year observation contains economic variables from that year as well as a future credit variable, serial correlation between b_t and b_{t+1} remains in the cross-section.

Figure 1 contains the results of this exercise. The coefficient on the future credit status variable from the primary empirical specification (an indicator for rejection on a future credit

Figure 1. Evolution of Future Credit Coefficient with Changing Gap



Source: NLSY 1979-1992, main and supplemental samples.

application; see empirical discussion below) becomes larger (less negative) as the size of the gap between the base period and the future reference period increases. The regression line reveals a tight linear relationship (R-squared=.84) and a correlation between future credit status and the size of the gap that is significant at the 99 percent level. Other measures of future credit status have similar results (not shown).

These outcomes indicate that the net bias stemming from serial correlation due to the use of a future credit variable is negative. Given that we are using potential experience as our experience measure, it follows that the serial correlation bias in the interaction between future credit status and potential experience—the main variable of interest—will reflect the bias in the main future credit variable and will thus be negative. If we find that the actual coefficient on the credit-experience interaction variable is significantly negative, we cannot be certain whether character or serial correlation generated the result. However, if we find an insignificant negative or a positive coefficient, then the effect of the character-related component of credit status on workplace productivity is likely to be negligible since it is counteracting rather than amplifying any existing negative bias.

Data

Following FG and AP, I employ the National Longitudinal Survey of Youth 1979 (NLSY79) to estimate the empirical model. The NLSY79 is a panel survey that follows a nationally representative sample of men and women who were aged 14-22 in 1979. The sample members were surveyed annually from 1979 through 1994. In 1994 the NLSY79 switched to a biennial survey format. There are several advantages to using the NLSY79 for this research. First, it allows us to track individuals over long periods of time, so we can implement the future-credit-status identification strategy described above. Second, because the survey focuses on a sample of young individuals who are moving into the labor force, it is ideal for studying the impact of unobserved characteristics that firms (and the market) learn about over time. Third, the NLSY79 contains data on assets and liabilities that allow us to construct proxies for credit quality. Finally, the fact that the NLSY79 extends back into the pre-Internet era allows us to analyze data from a time period when credit checks were arguably less widespread. Along with other measures, this helps to isolate the firm-learning effect (as distinct from firms' explicit credit evaluations).

The NLSY79 contains three samples. The first is a random sample of 6,111 non-institutionalized men and women; the second is an oversample of 5,295 Hispanics, blacks, and disadvantaged whites; and the third is a military sample. I restrict the analysis to the main sample and the minority/disadvantaged oversample. I eliminate individuals with less than eight years of education, as well as individuals who have reported hourly wages of less than two dollars or more than 100 dollars in constant 1992 dollars (deflated by the Bureau of Economic Analysis PCE deflator). To address reverse causality, I drop survey respondents who have negative net worth any time from 1979 to 1998, as well as individuals with medical conditions as of 1992. I also drop individuals with missing values for wages, education, and relevant measures of credit status. I additionally drop observations before an individual has made his or her first transition into the labor market. After making all of the other adjustments, I use all observations that are not missing the key independent variable for a given specification. Sample size ranges from 966 to 4,364 individuals, depending on the specification. Total observations range from 7,834 to 40,453. The Data Appendix contains more details about the adjustments to the sample as well as summary statistics.

For my dependent variable, I use the log of hourly wages from the respondent's main job (the "CPS" job in NLSY79 parlance). For the main explanatory variable, I employ several measures of credit status. The first is a variable indicating whether the respondent has been rejected on a credit application in the last five years. The second is an indicator variable that takes a value of one if the individual has negative net worth (that is, the individual's debts and other liabilities exceed the individual's assets). For the net worth measure, I do not include the effect of assets and liabilities related to housing because housing asset values are more volatile than other asset values and have more potential to create a situation where the individual has negative net worth that is not reflected in the individual's credit status. In addition to these measures, I also use the following proxies: credit card debt as a percentage of income, net worth as a percentage of assets, negative net worth status exclusive of student debt, and an indicator for whether the individual was ever more than two months late on his or her mortgage payment from 2007 to 2010.

For each of these proxies, the question exists whether the variable is measuring credit quality in a manner similar to employers' use of credit reports. The first thing to note is that, as described above, employers do not evaluate credit *scores*. Rather, employers idiosyncratically examine a multi-page credit report looking at indicators of credit behavior. The credit variables that I employ are designed to proxy for either the outcome or the components of this process. The credit rejection variable is a sign that the individual's overall credit status is not strong enough to meet credit-underwriting standards for additional indebtedness (thus reflecting a negative credit evaluation outcome). The negative net worth indicator variable reflects a situation in which an individual would be unable to meet current debt obligations by liquidating assets. Such a situation is almost certain to be reflected in the items contained in a credit report. Credit-card debt as a percentage of income relies on a popular conception of financial prudence that may be influential with employers. Net worth as a percentage of assets provides a measure of how close to the financial "red line" individuals live, taking into account their levels of wealth. Negative net worth status exclusive of student debt provides a measure that eliminates one scenario in which an individual may have poor credit status that employers nevertheless view favorably. The late mortgage payment variable reflects an inability to meet obligations that would unquestionably be reflected in a credit report.[11] Although none of these measures is complete by itself, taken together they represent a robust approximation of credit quality. Furthermore, because employers engage in idiosyncratic assessment of multi-page credit reports, there is no single measure that could proxy for the entire employer evaluation process. In many ways the varied and disaggregated picture painted by these variables is closer to the reality of the screening process than a single summary variable such as a credit score.

Following AP and Lange (2007), I treat actual experience as endogenous and use a measure of potential experience as an independent variable (age – education – 6). I also divide experience by ten in order to make the resulting coefficients easier to read. I utilize unweighted observations. I use observations from 1979-1992 as the base period, and observations from 1999-2010 as the future reference period. For comparison's sake, I have included the interaction between AFQT and experience, as it is a significant and economically meaningful indicator of firm learning about unobservable productivity. I employ the 1989 standardized AFQT scores, which are adjusted for age group (NLS User Services 1992). I have also included the interaction between education and experience, along with the main effects of the interaction terms. All specifications include a cubic in experience; indicator variables for black, Hispanic, and female status; interactions between experience and black, Hispanic, and female status; year effects; an indicator for urban location; and an indicator for divorce. In addition, following AP, I include

[11] The reason for including a measure of late mortgage payments while excluding housing assets and mortgage liabilities from the net worth calculation is that these mortgage-related items have very different relationships with credit reports. In the case of net worth, the value of housing is volatile and could lead to a situation in which an individual has negative net worth but is able to pay bills and otherwise maintain good credit. By contrast, late mortgage payments represent a concrete violation of financial covenants that will appear in credit reports and will be interpreted by reviewers of these reports as a decline in credit quality.

fixed effects for the first occupation that individuals have when they transition to the labor market (using two-digit 1970 Census occupation codes). The idea with this latter control is that different occupations may have different wage trajectories, and otherwise similar individuals may get "tracked" into different trajectories based on this initial choice of occupation. I cluster standard errors at the individual level.

Results

In all of the empirical specifications, I estimate versions of equation (7) via linear regression. In the first specification, the future credit status variable is an indicator that takes on a value of one if the individual reported that he or she had been rejected on a credit application in the last five years. I pool the responses to this question from the 2004, 2008, and 2010 surveys (the question was not asked in 2006). The key explanatory variable in the first specification is the interaction between the future credit variable (indicator for rejection on a credit application) and experience. Twelve percent of the individuals in this specification have an indication of future bad credit.

Before discussing the detailed results, it's worth presenting a simple visual representation of the relationship between credit status and the wage-experience profile. Figure 2 contains wage-experience profiles for both credit groups based on the NLSY79 data. The credit status variable is the credit rejection measure used in the first empirical specification.

Figure 2. Wage-Experience Profiles, 1979-1992

Source: NLSY 1979-1992, main and supplemental samples.

We can see from Figure 2 that the wages of individuals with future bad credit do not show slower growth than the good credit group as employers learn about any unobservable characteristics of the future bad credit group. This pattern is not what we would expect if character-related credit factors were significant determinants of productivity. However, we cannot draw firm conclusions from this graphical exercise. The profiles in Figure 2 do not control for other determinants of wages. In addition, Figure 2 uses actual experience, which may itself be endogenous. We can now turn to more detailed multivariate estimation, including exogenous potential experience and multiple different measures of credit status, to get a more accurate sense of relative wage-experience profiles.

Main Results

The baseline regression results are contained in Table 1. As the first column of Table 1 shows, both future credit rejections and AFQT scores have significant main effects on the level of initial wages. Future credit rejection is associated with an initial wage level that is 6.8 log points lower (7 percent lower) than those who will not experience this condition, while a one standard deviation increase in AFQT is associated with a 6.5 log-point increase (6.7 percent increase) in initial wage levels. Note that these wage differentials occur before employers learn about unobservable characteristics (such as future credit status and AFQT), so these effects reflect the impact of other correlated variables that employers can witness up front but that are not observable by researchers. As noted above our main interest is with the variables that describe what happens as employers begin to learn about the elements of employee productivity that are related to unobservable characteristics.

Our prior expectation is that the interaction between future bad credit and experience will be negative and significant. This expectation is reinforced by the fact that any bias stemming from serial correlation/reverse causality should be negative. However, in contrast to this expectation, the coefficient on the interaction of future credit rejections and experience is positive, implying that individuals with future credit rejections experience a 6.4 log point increase (6.6 percent increase) in wages over 10 years of experience relative to other individuals. Furthermore, the 95 percent confidence interval rules out values smaller than -0.008 (-0.8 percent), indicating a "precise" zero or positive effect. Thus as employers learn about the unobservable character-related components of credit status—and the associated productivity characteristics that accompany them—they do not find this group of employees relatively less productive than other individuals after adjusting for initial wages. It is helpful to compare this finding with the behavior of a known unobservable predictor of employee productivity, namely the interaction of standardized AFQT scores and experience. In the first specification, this interaction is associated with a significant increase of 4.0 log points in wages when experience is equal to 10 years. In other words, in this sample, and with this specification, unobservable AFQT scores behave as predicted in a firm-learning model, showing a significant association with higher wages. In contrast, unobservable future credit rejections fail to show an equivalent negative and significant impact on wages over time.

One question that might arise about the inclusion of two unobservable characteristics in the regression specifications is whether some type of collinearity might affect the estimates of the future credit variables. The second specification in Table 1 explores this possibility by excluding the AFQT variables. We can see that the magnitudes of the future credit variables remain basically the same. Because the AFQT variable measures a conceptually distinct concept (cognitive ability) that employers learn about separately from credit-related character, and because it provides an instructive contrast with credit status, I include the direct and interacted AFQT variables in all the remaining specifications.

The next specification involves the use of a different measure of future credit status (column three of Table 1). Because the NLSY collects detailed information on assets and liabilities, we can determine whether individuals had positive or negative net worth. Net worth is defined as total assets minus total liabilities, not including housing assets or liabilities (due to volatility). In this specification I use an indicator that takes on a value of one if an individual had negative net worth in 2000, 2004, or 2008 (the relevant variable is not available for 2010).[12]

[12] Net worth and other asset/liability data are measured at the family level for each individual in the NLSY sample.

Table 1. Effect of Character-Related Credit Status on Productivity (Wage Growth), 1979-1992

	Measure of Future Credit Status					
	Credit Reject ('04, '08, '10)	Credit Reject w/o AFQT	Neg. Net Worth ('00, '04, '08)	Neg. Net Worth ('08 control)	Neg. Net Worth Renters	Neg. Net Worth ('00 and '04)
Future credit status variable	-0.068**	-0.082***	-0.086***	-0.120***	-0.117***	-0.125***
	(0.029)	(0.029)	(0.030)	(0.041)	(0.044)	(0.034)
Future credit*experience/10	0.064*	0.067*	-0.026	0.042	0.033	0.025
	(0.037)	(0.038)	(0.039)	(0.054)	(0.062)	(0.045)
Future credit 2008				0.101*		
				(0.056)		
Future credit '08*experience/10				-0.163**		
				(0.074)		
AFQT	0.065***		0.071***	0.081***	0.052**	0.069***
	(0.017)		(0.013)	(0.013)	(0.021)	(0.013)
AFQT*experience/10	0.040**		0.055***	0.050***	0.074***	0.060***
	(0.020)		(0.015)	(0.016)	(0.024)	(0.015)
Education	0.072***	0.085***	0.076***	0.075***	0.081***	0.077***
	(0.008)	(0.007)	(0.006)	(0.006)	(0.011)	(0.006)
Education*experience/10	-0.010	0.006	-0.021***	-0.017**	-0.045***	-0.023***
	(0.009)	(0.008)	(0.007)	(0.008)	(0.013)	(0.007)
Number of individuals	2,061	2,061	3,307	2,919	966	3,204
Number of observations	19,371	19,371	30,023	26,677	7,834	29,205
Adjusted R-sq.	0.343	0.328	0.345	0.351	0.288	0.347

Data: NLSY79. Notes: Standard errors are clustered at the individual level. All models control for year effects, a cubic in experience, urban residence, divorce, and female, black, and Hispanic interactions with experience. Non-fixed effects models control for female, black, and first occupation. Experience is potential experience. Standard errors are in parentheses; *=p<.10, **=p<.05, ***=p<.01.

The results for this specification are somewhat similar to the prior ones, with one key difference. AFQT and future negative net worth have significant main effects that are consistent with the prior results. The AFQT-experience variable remains significant, positive and large in magnitude (5.5 log points). The one difference is that the future credit-experience interaction is now negative, although it is small in magnitude and not significantly different than zero. Nevertheless, this change in sign is worth exploring. It turns out that this negative shift in the earnings-experience profile comes entirely from 2008 data. In the next specification, I include separate variables for negative net worth in 2008, and experience interacted with 2008 negative net worth. The overall negative net worth-experience variable reverts to the previous pattern of positive and insignificant, while the 2008 negative net worth variables display very different behavior. The 2008 negative net worth variable has a positive value that is significant at the ten-percent level, while the 2008 negative net worth-experience interaction is strongly negative and significant at the five-percent level.

Is this result evidence that the character-related component of credit status is associated with lower productivity and wage growth? In making sense of this result, it's important to note that 2008 was a very unusual year in terms of economic shocks. The data based on 2000 and 2004 conform to the earlier pattern from the prior specification. In 2008, it's possible that a strong economic shock amplified the level of correlation between negative economic outcomes in the base period and the future reference period, thus exerting a stronger downward bias than is seen in other years. In other words, similar groups of people might have gotten hit by exogenous economic shocks in both 2008 and the base period for estimation (1979-1992). In the discussion of the economic model above, this would take the form of correlation between contemporaneous economic determinants of credit status (b_i , $b_{i,t+y}$). To explore this possibility, we can begin by noting that the economic shocks of 2008 hit the housing market particularly hard. Despite the fact that our measure of net worth does not include housing assets or liabilities, these shocks were clearly large enough that their impact spread beyond narrow measures of home values and mortgage debts. If these unique shocks, rather than the character-related component of future credit status, are responsible for the 2008 results, then individuals who were less exposed to housing equity shocks should show a different pattern than those who were more exposed.

We can operationalize this approach by estimating the future negative net worth specification for a population of renters (again pooling future data from 2000, 2004, 2008). Specifically, I drop individuals who owned a home in 2004 or 2008. Although the sample size suffers from these exclusions, we can see in column 5 of Table 1 that the results for renters conform to the earlier pattern. The AFQT-experience variable is positive, large, and significant, while the future negative net worth-experience variable is positive and insignificant. We can additionally test this explanation by limiting the future negative net worth variable to data from the 2000 and 2004 surveys, thus excluding the macroeconomic effects of the 2008 meltdown. The results from this specification demonstrate the same pattern in which AFQT-experience is positive and significant, and negative net worth-experience is neither negative nor significant. On this basis, it appears that, in the absence of the effect of severe economic shocks, unobserved future negative net worth is not conveying significant negative character-related productivity information to employers as they learn about employee characteristics over time.

Additional Specifications and Robustness Analysis

To further explore the relationship between the character-related components of credit status and productivity, we can look at the behavior of several other measures of future credit status. One issue that might be raised is whether it is better to have a discrete measure of future bad credit (like rejection on a credit application or negative net worth), or whether a continuous measure would better capture the dynamic of credit quality. In the first specification in Table 2, I have used credit card debt as a percentage of an individual's annual income to construct one continuous measure. The idea is that individuals who carry higher levels of credit card debt relative to their income may be less responsible than otherwise similar individuals who carry less debt. To account for any nonlinearities involving the individuals who have zero credit card debt, I have controlled for zero debt via an indicator variable (Angrist and Pischke 2010). I have estimated results for this credit card specification for both 2004 and 2008. From the first two columns of Table 2, we can see that the interactions of future credit with experience are small and not significantly different from zero in the 2004 and 2008 specifications. Because the credit-card debt variable is stored as a decimal, these results imply that a 100 percentage point increase in credit card debt as a percentage of income is associated with an insignificant 0-1.5% increase in wages over 10 years of experience. The 95 percent confidence interval rules out negative effects larger than -0.002 and -0.008, thus implying a precise zero. By contrast, the interaction of AFQT and experience is positive, significant, and substantially larger in magnitude.

We can see the same pattern in another continuous measure of future credit status. The third column of Table 2 contains a specification that employs an individual's net worth as a percentage of total assets in 2004 (stored as a decimal). The idea is that individuals who live more prudently may accumulate relatively more assets relative to liabilities than otherwise similar individuals. This net worth figure is then normalized by the individual's total asset wealth. Although there are obviously many different interpretations of why this percentage might vary that do not rely on character, the goal of this exercise is to present many different potential measures of credit quality to see if any of them yield results consistent with the prior expectation that bad credit signals a character trait that negatively affects productivity. As we can see from Table 2, the results of the percentage net worth specification are consistent with the results of the other specifications. For this specification, our expectation is that higher levels of percentage net

Table 2. Effect of Character-Related Credit Status on Productivity (Wage Growth), 1979-1992: Additional Specifications

	Measure of Future Credit Status							
	Credit Card Debt % of Inc. ('04)	Credit Card Debt % of Inc. ('08)	Net Worth as % of Assets ('04)	Credit Reject Incl. All Student Loan Holders	Credit Reject (Netting Out Student Loans)	Neg. Net Worth ('04 no student debt)	Late Mort. Pmt. (2007-2010)	Late Mort. Pmt. Expanded Sample
Future credit status variable	-0.001	-0.020*	-0.001***	-0.076***	-0.089***	-0.088	0.022	0.002
	(0.001)	(0.012)	(0.000)	(0.022)	(0.029)	(0.054)	(0.043)	(0.024)
Future credit*experience/10	-0.000	0.015	0.001***	0.020	0.079**	0.030	-0.002	0.003
	(0.001)	(0.012)	(0.000)	(0.031)	(0.037)	(0.070)	(0.045)	(0.029)
AFQT	0.078***	0.080***	0.070***	0.051***	0.065***	0.072***	0.078***	0.055***
	(0.014)	(0.015)	(0.014)	(0.014)	(0.017)	(0.013)	(0.015)	(0.011)
AFQT*experience/10	0.037**	0.039**	0.053***	0.050***	0.039**	0.059***	0.029	0.048***
	(0.018)	(0.018)	(0.018)	(0.017)	(0.0196)	(0.017)	(0.020)	(0.013)
Education	0.079***	0.077***	0.077***	0.077***	0.072***	0.078***	0.070***	0.075***
	(0.007)	(0.007)	(0.007)	(0.006)	(0.007)	(0.007)	(0.007)	(0.005)
Education*experience/10	-0.016*	-0.012	-0.022***	-0.023***	-0.009	-0.024***	-0.006	0.020***
	(0.009)	(0.009)	(0.008)	(0.008)	(0.009)	(0.008)	(0.009)	(0.006)
Number of individuals	2,375	2,393	2,681	3,056	2,159	2,898	2,108	4,364
Number of observations	22,521	22,600	25,046	27,483	20,107	26,563	19,931	40,453
adj. R-sq	0.353	0.350	0.346	0.316	0.337	0.347	0.354	0.320

Data: NLSY79. Notes: Standard errors are clustered at the individual level. All models control for year effects, a cubic in experience, urban residence, divorce, race, gender, and female, black, and Hispanic interactions with experience. Models also include controls for first occupation, except for models with controls for first wage. Credit card models also include zero-debt controls (see text for discussion). Experience is potential experience. Standard errors are in parentheses; *=p<.10, **=p<.05, ***=p<.01.

worth will be associated with a large and significant positive effect on wages (implying that lower levels of percentage net worth have a large and significant negative impact). However, the percentage net worth-experience coefficient is very small (0.001) and precisely estimated. The 95 percent confidence interval includes no effects larger than 0.0008. By contrast, the AFQT-experience coefficient is positive, significant at the 99 percent level, and sizable in magnitude (.053). Overall, the continuous future credit variables do not provide any evidence that the character-related component of future credit status is a meaningful predictor of employee productivity.

An additional issue that might be raised about these results concerns the way in which the sample is trimmed. As discussed above, it is important for this estimation strategy to remove individuals with prior signs of bad credit (negative net worth) in order to minimize serial correlation. However, it might be asked whether all forms of negative net worth should be treated equally. In particular, negative net worth from student loan debt may not imply the same type of serial correlation in credit outcomes as other sources of bad credit status. Twenty-nine percent of those with negative net worth report having had some type of student loan. In general, excluding student loan-holders along with other negative net worth individuals in the main estimation samples is a conservative approach because individuals with student loan debt have significantly faster income growth than other individuals in the NLSY79. However, the dynamics can be complicated, and I explore the effect of alternative approaches.

In the fourth column of Table 2, I present a specification for the credit rejection dependent variable from Table 1 in which I retain all individuals who have negative net worth but who report having had a student loan from 1979 to 1998. As with other specifications, the future credit-experience coefficient is positive and insignificant, while the AFQT-experience interaction is positive, large in magnitude, and highly significant. The drawback of this blanket approach is that many of the individuals retained may have negative net worth from other causes than their student loans. For the next specification, I calculate which individuals had a volume of student debt that was large enough to have reversed their negative net worth status.[13] I retain these individuals in the sample. In this specification (column five of Table 2), the future credit-experience interaction is positive, large in magnitude (0.079), and significant at the 95 percent level. The 95 percent confidence interval rules out any coefficient value smaller than 0.007, again pointing to a precise zero or positive effect.

Another student-debt related concern involves not the composition of the sample but the construction of the future credit variable. Incurring student loan debt may be associated with higher productivity, while irresponsibly running up other debts may indicate the type of poor judgment that could negatively affect worker productivity. Thus including student debt in the calculation of negative net worth could "dilute" a negative character effect. To explore this possibility, I employed a specification that nets out the effect of student loan debt on future negative net worth in 2004.[14] The results in the sixth column of Table 2 demonstrate the familiar pattern. The AFQT-experience coefficient is positive, large, and highly significant (.072), while the negative net worth-experience coefficient is neither negative nor significant (.030).

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[13] There are a number of challenges in working with the data on student loan amounts in the NLSY79. The student loan variables measured debt ever incurred until 1989, at which point they began to measure the annual flow of debt. Furthermore, these variables do not measure the outstanding debt but rather the unamortized original loan amount (the 2004 survey reports on outstanding quantities). Nevertheless, it is possible to construct a noisy measure of the unamortized loan total. It turns out that most individuals with negative net worth maintain this status even after netting out the effect of student debt.

[14] The 2004 NLSY79 permits more accurate measurement of this student loan contribution to negative net worth than prior years as it asks about the amount of currently outstanding student debt (rather than unamortized debt incurred).

Finally, one might also ask how a future credit indicator that is directly tied to the recent housing crisis behaves. In the seventh column of Table 2, I employ a credit quality indicator variable that takes on a value of one if an individual reported having been late on or missed a mortgage payment in the last three years. The question was asked in the 2010 survey, so the years covered are 2007-2010. The late mortgage-experience interaction has a coefficient that is negative, small, and not significantly different than zero (-.002). However, unlike all the other specifications, in this specification the AFQT-experience coefficient is not significant. The coefficient is positive and of reasonable magnitude (.029), and it is more precisely measured than the late mortgage-experience coefficient, but the contrast in predictive value between the AFQT and the future credit interaction variables is less pronounced in this specification. One potential explanation may be that the dynamics associated with housing market shocks are different than those that relate to other time periods and populations that are less exposed to these particular economic shocks. Another explanation may be that the sample that contained non-missing values for this independent variable is not large enough for precise estimation. I explored this possibility by altering the method by which the sample was trimmed. Specifically, I added back in individuals who reported negative net worth anytime from 1979 to 1998. The downside of adding these individuals back is that it increases the potential for reverse causality and negative bias in the future credit-experience variable due to serial correlation of economic outcomes. The benefit is that it will increase the size and variation of the sample. In the original sample six percent of individuals reported being late or missing mortgage payments. In the expanded sample, nine percent report this condition. The additional sample variation should also improve the precision of the AFQT estimates. We can see the results in the final column of Table 2. The AFQT-experience coefficient is positive, large, and highly significant (.048). By contrast, the late mortgage-experience coefficient is very small and not significantly different from zero (.003). The results thus appear to be broadly consistent with the results of the other specifications.

Discussion

In multiple different specifications, measures of future credit status do not convey negative information about the character-related component of employee productivity as firms learn about unobservable employee characteristics over time. Indicators of future bad credit are associated with significantly lower *initial* wages, but this wage differential is the result of factors that are visible to employers at the beginning of the employment relationship. Because we have dropped individuals with signs of prior bad credit, and because most individuals are not subjected to credit checks for a particular job, these initial wage differentials are likely due to heterogeneity that is correlated with future credit status.[15] The comparison with AFQT scores is instructive. AFQT scores are unseen by employers, but they nevertheless are associated with a large and significant effect on initial wage levels due to other correlated factors that are visible to employers up front. The key point, however, is that as employers learn about workers' true productivity (or as workers differentially acquire skills to enhance productivity), AFQT scores become highly predictive of the resulting wage growth. The character-related component of credit status, as measured by future credit indicators, is also correlated with initial level effects, but unlike AFQT scores it does not contain a significant prediction of hidden productivity or productivity growth in the expected direction.[16]

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[15] Only 13 percent of employers in a 2010 SHRM survey used credit checks for all of their hires (SHRMb 2010). Although data are not available, in the pre-Internet 1980s, this figure was almost certainly much lower.

[16] One question that might be raised about the results is what factors might generate a positive relationship between the future bad credit-experience variable and wage growth, as some specifications show magnitudes that are similar to the AFQT-experience variable. The first thing to note is that most of the specifications show point estimates for the credit-experience interaction that are much smaller than the AFQT-experience effects.

There is an alternative interpretation of these results. A critic might say that the above-described methodology does in fact isolate the character effect, but note that a distribution of credit-related character (p) exists. The fact that the empirical specifications show no significant negative effect on the wage-experience profile could be due to either the fact that on average credit-related character is not predictive of worker productivity, or to the fact that the threshold values above which p harms productivity are high, and most individuals who experience (future) bad credit have values of p below this threshold. The first thing to note is that these competing explanations dramatically narrow the scope of the debate on this topic: the character-related portion of credit status is either uninformative about employee productivity or it is only meaningful at extreme values that infrequently occur. Even if the latter were true, from a policy perspective we should be concerned that the likelihood of employer errors in identifying this threshold through the review of credit reports rises as the condition becomes more improbable.

However, the empirical results cast doubt on the idea that a significant negative character effect is being masked by more benign sources of future credit problems. The specifications where I eliminate more "blameless" sources of credit trouble—such as student debt—and thus raise the potential proportion of character-related causes show the same patterns as the other more general specifications. Likewise, the specifications that show a higher proportion of credit

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Although not all of these measures are strictly comparable, as some of the credit measures are binary and some are continuous, the percentage credit card debt, percentage net worth, and late mortgage payment specifications have credit-experience interactions that are very nearly zero. In addition, only the AFQT-experience confidence intervals consistently rule out values of zero. All of the AFQT-experience coefficients but one are positive and significantly different that zero at the 95 percent confidence level or greater. By contrast, only one of the credit-experience confidence intervals excludes zero at the 95 percent level. The specifications that utilize the credit rejection dependent variable are the principal ones that show reasonably large positive magnitudes and significance or borderline significance for the future credit interactions. It is possible that future credit rejections identify some aspect of credit-related character that differs slightly from the aspects identified by other measures such as future negative net worth or future high amounts of credit card debt. This character trait might have a more positive association with wage growth than other credit-related character measures. Regardless, the point remains that all of the specifications point to effects that contradict our prior expectations of significant and large negative effects.

problems due to exogenous macroeconomic factors (i.e., the 2008 results) do not show more positive effects of character on the wage-experience profile. In fact, they show more negative results. Continuous measures such as credit card debt as a percentage of income—commonly thought to indicate "personal responsibility"—also show the same patterns as the baseline specifications.

There are some limitations to the approach adopted in this paper. In order to isolate the character effect via future bad credit, it is necessary to drop individuals with signs of bad credit in the base period. Developing identification strategies to test for character effects in this group is an appropriate task for future research. In addition, the methodology employed in this study does not allow us to evaluate the validity of non-character-related rationales for credit checks such as the distraction screen. Finally, this paper focuses on wages as a sign of worker productivity. Testing for impacts on other labor market outcomes is another promising direction for future studies. Despite these caveats, the identification strategy in this paper allows us to evaluate the causal claims behind an important argument for credit checks.

Conclusion

The practice of screening workers by conducting credit checks is a controversial one, with advocates insisting that it improves the quality of job matches and opponents maintaining that it results in discrimination against disadvantaged groups with poor credit. The rise of similar screening practices that rely on correlations present in Big Data further increases the salience of the issue. The contribution of this paper to the literature is to utilize an economic identification strategy along with credit proxies in a national dataset (the NLSY79) to determine whether credit status contains information about a worker's character that is predictive of worker productivity. The identification strategy consists of the use of a novel mechanism—future credit status—to identify time-invariant character traits, along with a firm-learning model to identify the impact of unobserved variables on productivity growth. While there are other rationales, such as distraction effects, for the use of credit checks, character-related factors are a major rationale offered in support of the practice. The results of this analysis indicate that credit status does not contain a meaningful signal about the unobserved character-related components of employee productivity.

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Data Appendix

The data used in this study come from the 1979-2010 waves of the NLSY79, a nationally representative survey of individuals aged 14-21 in 1979. As described above, the base period data, in which labor market outcomes and contemporary economic variables are measured, are from the 1979-1992 period. The future credit variables come from the 2004-2010 waves (although some of the retrospective questions in the 2004 survey cover the period going back to 1999). The NLSY79 consists of a main sample with 6,111 individuals, a supplemental sample focused on disadvantaged populations with 5,295 individuals, and a military sample. I exclude the military sample, yielding a starting point of 11,406 individuals. After excluding time periods after 1992, there are 159,684 observations. I take a number of further steps to prepare the sample used in the empirical specifications. I exclude those who have made the transition to the labor market before the first observation in 1979 (94 individuals). Following Altonji and Pierret (2001), I drop observations that are missing wage data (for the "CPS" job) or that have values for real wages in 1992 dollars that are less than $2/hour or more than $100/hour (eliminates 322 individuals). I also drop individuals with missing education or who report less than eight years of education (183 individuals). Likewise I drop individuals who did not take the tests comprising the AFQT or who have missing AFQT scores (553 individuals). Finally, to reduce serial correlation I take two further steps. I eliminate individuals who, in the final base period year (1992), report work-limiting health conditions that have persisted since the prior year (456 individuals). I also drop respondents who have reported negative net worth in 1998 or earlier (5,045 individuals). I then use the maximum sample available for the respective dependent variables (i.e., I drop individuals with missing values for the dependent variables). These adjustments leave a sample of 2,061 individuals with 19,371 observations for the credit rejection specification and 3,307 individuals with 30,023 for the negative net worth specification.

Below are summary statistics for individuals in 1992 with and without future bad credit status. The significant differences between these populations are one of the prime motivations for using a longitudinal firm-learning identification strategy rather than a comparison of level effects.

Table A1. Summary Statistics for Main Specifications, 1992 Values

	Credit Reject ('04, '08, '10)		Neg. Net Worth ('00, '04, '08) + '08 Control	
	No	Yes	No	Yes
Hourly wage	12.72	11.17	11.94	9.47
	(7.12)	(9.35)	(7.40)	(6.85)
Education	13.74	12.84	13.38	12.54
	(2.25)	(2.15)	(2.26)	(2.08)
Experience (years)	14.18	12.92	13.73	12.00
	(5.08)	(5.68)	(5.16)	(5.84)
AFQT (standardized)	0.42	-0.14	0.16	-0.34
	(0.84)	(0.85)	(0.97)	(0.99)
Female	0.47	0.47	0.43	0.49
	(0.50)	(0.50)	(0.50)	(0.50)
Black	0.15	0.36	0.24	0.35
	(0.36)	(0.48)	(0.43)	(0.48)
Hispanic	0.15	0.27	0.16	0.27
	(0.36)	(0.45)	(0.37)	(0.44)
Number of individuals	1,814	247	2,630	289
Number of observations	17,140	2,231	24,299	2,378

Source: NLSY79 main and supplemental samples. Standard errors in parentheses.

Essay 2: Skill Demands and Mismatch in U.S. Manufacturing

Evidence and Implications

Co-authored with Paul Osterman

Introduction

Matching skilled workers to appropriate jobs is one of the most critical processes in a modern economy. Firms depend on the timely supply of skills to grow and thrive, and workers depend on access to quality employment opportunities to achieve financial security and professional fulfillment. All parties have a vested stake in how supply and demand for skills equilibrate in the labor market. Over the past few decades, recurring debates have taken place over whether mismatch exists in this process (Cappelli, forthcoming; Kalleberg 2007; Handel 2003; Berg 1970). Researchers have posited that structural factors such as geographic immobility or shifting industry demands have driven a wedgc between supply and demand, thus generating economic inefficiency and, in some cases, structural unemployment. One of the most frequent assertions is that workers lack the skills that employers demand, resulting in an economically damaging skill gap (Carnevale, Smith, and Strohl 2010; Manyika et al. 2011).

These arguments have intensified in the wake of the Great Recession of 2007-2009. While some analysts have concluded that cyclical factors were responsible for the subsequent high unemployment levels (Rothstein 2012, Lazear and Spletzer 2012), others continue to assert the importance of structural factors in generating mismatch (Mulligan 2009, 2011a, 2011b; Charles, Hurst, and Notowidigdo 2013). Although the balance of the evidence tends to point to the centrality of cyclical factors (Neumark and Valletta 2012), a number of researchers who acknowledge the importance of cyclicality nevertheless find non-trivial levels of structural mismatch (Sahin et al. 2012; Estevau and Tsounta 2011; Rothwell 2012; Canon, Chen, and Marifian 2013).

One of the challenges in sorting through these claims is that the debates frequently take place at high levels of abstraction with imprecisely measured proxy variables. Most prior research does not measure either the skills employers demand or the degree to which employers have difficulty in securing these skills (Kalleberg 2007; Handel 2010). Furthermore, much of the existing evidence on this topic takes place at the inter-industry level (Sahin et al. 2012; Canon, Chen, and Marifian 2013). While this approach is informative, it ignores mismatch that may occur within industries, and it obscures the mechanisms that underlie labor market frictions. In particular, mismatch indices typically cannot pinpoint the degree to which a shortfall in particular workforce skills is associated with signs of mismatch. Addressing this gap in the research is important because skill mismatches are a nearly constant source of public debate and carry substantial public policy implications.

We seek to add to knowledge about skill mismatch by examining employer-level data within a broad industry sector in order to gauge the incidence of the problem and to uncover which mechanisms are at work. Specifically, we explore mismatches related to worker skills ("skill gaps") by examining detailed evidence on employer skill demands and hiring experiences in the manufacturing sector. We pose the following questions: What skills do manufacturers demand for production workers? What is the incidence of hiring difficulties (potential skill gaps) among manufacturers? What factors, including skill demands, predict these hiring difficulties? Which stories about skills and skill gaps are the observed patterns consistent with? To address these issues, we designed and administered a nationally representative survey of manufacturing establishments that, to our knowledge, provides data on skills and hiring that are unavailable from any other source.

To preview our results, we find that three quarters of manufacturing establishments do not have persistent problems hiring the skilled production workers they demand. Consistent with this finding, the general pattern of skill demands in the manufacturing sector is one of widespread demand for basic skills but generally modest demands for extended or advanced skills. Among the establishments that do show signs of prolonged hiring difficulties, higher-level demands for reading and math are important factors. Other skill demands, including those for soft skills and various high-performance work characteristics (such as the ability to learn new skills or solve unfamiliar problems), are not related to signs of mismatch. High-tech establishments and computer skill demands are generally associated with lower rather than higher levels of hiring challenges. When we examine the characteristics of the minority of establishments that do show signs of hiring difficulties, we find little support for factors that relate purely to either supply-side factors or internal firm behavior. Rather, we find that factors that place stress on the interaction of supply and demand—such as specialized skill demands, disaggregated firms, and adjustment in the face of employment changes—are more predictive of recruitment challenges. These findings imply that the traditional skill gap formulation may be a misleading way of thinking about labor market challenges. Skills are critical to improving productivity, but rather than solely focusing on the inadequate educational attainment of the American workforce, policymakers would do well to focus on improving the quality of the institutions that improve communication and coordination in the labor market.

The structure of the paper is as follows. In the first section we discuss the debate over labor market mismatch and skill gaps. In the next two sections, we discuss various potential mechanisms behind skill mismatch and outline our empirical strategy. We then describe our survey methodology, followed by a presentation of data on the skills that manufacturers demand and the incidence of hiring difficulties. Next, we explore the characteristics of establishments that experience hiring challenges. We subsequently analyze which skill gap stories are consistent with our data. In the final section we conclude.

The Debate over Skill Gaps and Mismatch

The smooth functioning of the labor market depends on the equilibration of the supply and demand for skills, which in turn depends on matching workers to job openings. To the extent that mismatch or a gap exists between supply and demand, economic growth will suffer and workers with ill-matched skills will experience reduced economic opportunities or unemployment. Unlike mismatch due to cyclical causes, structural mismatch will, by definition, persist even as general economic conditions improve, thus making it an issue that must be dealt with through some combination of structural reforms, policy interventions, and behavioral changes among workers.

Researchers have made a number of attempts to quantify the level of structural mismatch in the economy, often employing quite different approaches to the topic. Mulligan (2009) uses a Cobb-Douglas production function to decompose labor market changes into shifts in productivity and shifts in labor supply. He concludes that negative shifts in labor supply were behind the spiking unemployment in the Great Recession. In subsequent work, he asserts that comparison of the incidence of government benefit programs with unemployment patterns implies that the labor market disincentives associated with government programs are to blame (Mulligan 2011a, 2011b).

A number of researchers have composed mismatch indices to measure whether imbalances exist that imply structural unemployment. These indices typically employ one of two broad methodologies. The first consists of calculating the ratio of unemployment to vacancies and interpreting the variation in this measure across industries as a sign of structural mismatch. This methodology is conceptually similar to a Beveridge-curve approach to mismatch. The second methodology consists of estimating industry demand for various skills, subtracting estimated regional supply of these skills, and labeling the difference a measure of mismatch.

Utilizing the first, Beveridge-style methodology, Sahin et al. (2012) find that labor market mismatch may be responsible for up to a third of the increase in unemployment from the Great Recession.[17] Canon, Chen, and Marifian (2013) calculate several alternative indices of this sort and conclude that mismatch could explain up to 51% of the unemployment increase. Estevau and Tsounta (2011) employ a version of the supply/demand index methodology to find that mismatch may account for 20 to 30 percent of the rise in unemployment. Other researchers using variations of this latter methodology also find a significant role for structural mismatch (Rothwell 2012; Carnevale, Smith, and Strohl 2012; Manyika et al. 2011).

The foregoing research has pushed the debate forward in constructive ways but there remain significant limitations. Mismatch indices rely on highly aggregated data and are unable to identify underlying mechanisms. Industry unemployment/vacancy ratios could diverge due to a skills gap, geographic immobility, or a host of other factors. Even granting the relevance of general mismatch measures that obscure underlying causes, many of the indices are highly sensitive to cyclicality, thus calling into question the structural nature of the results (see Canon, Chen, and Marifian (2013) and Lazear and Spletzer (2012) for indications of cyclicality). Furthermore, as Rothstein (2012) notes, the Beveridge curve does not necessarily measure labor market tightness (and hence mismatch) because changes in firm strategy regarding recruitment intensity and wages can shift the curve independent of structural factors such as matching efficiency or the deficient skills of the workforce.

[17] Sahin et al. (2012) emphasize the fact that they see cyclical factors as the primary force behind the spike in unemployment. Nevertheless, as noted above, their results imply a non-trivial role for structural factors.

Supply/demand indices would seem to pinpoint the source of the mismatch as skill-related, but in fact they suffer from flaws of equal magnitude. Due to data limitations, none of these supply/demand indices directly measure either the demand for or the supply of skills. Rather, they utilize education as a proxy for skill. While understandable given constraints, this assumption is very problematic. On the demand side, changing educational composition does not necessarily measure skill demands. For example, if a college-educated individual takes a job at a coffee shop, it would be misleading to conclude that skill demands for baristas have risen (Harrington and Sum 2010). On the supply side, educational proxies obscure variation in skill levels within educational categories. Such intra-category variation is a large and important source of skill variation (Lemieux 2006), as many employers do not choose between filling a vacancy with a high school graduate versus a college-educated worker but rather choose between workers of different skill levels within a given educational category.

In addition to these methodological concerns, there are some empirical findings that call the structural mismatch results into question. Rothstein (2012) investigates a battery of empirical measures to investigate structural claims. He demonstrates that unemployment levels among industries or groups that are often asserted to be the source of structural mismatch are not unusual once prior trends are taken into account. He also shows that aggregate, industry, and group-specific wages generally do not show the pattern of increases that would be consistent with structurally tight labor markets. Lazear and Spletzer (2012) add additional evidence that cyclical factors are central to explaining the rise in unemployment associated with the recession of 2007-2009. They demonstrate that although Beveridge-style indices based on the unemployment-vacancy ratio do show variation by industry, industry-specific changes in the ratio are proportional before and after the recession, yielding an overall level of industrial mismatch in 2011 that is the same as the level that prevailed before the recession. Other researchers have similarly cast doubt on the importance of structural mismatch in explaining current economic conditions (Valletta and Kuang 2010; Dickens 2011; Daly, Hobijn, and Valletta 2011; Ghayad 2013).

All of this said, even if labor market mismatch is not the primary cause, the possibility that mismatch could account for a quarter to a third of unemployment increases is worthy of attention (Sahin et al. 2012). In addition, although they should be treated with caution, there are innumerable ground-level reports of employers who cannot find the skilled workers they seek (Krouse 2013, Maltby and Needleman 2012, Whoriskey 2012, Woellert 2012). Applied researchers should always seek to explain divergence between micro phenomena and macro results.

One clear way to make progress in the debate over structural mismatch is to generate better data that focus on within-industry variation, that directly measure skill demands and hiring outcomes, and that measure key variables that allow us to test for the mechanism behind any observed mismatch. This paper seeks to achieve these objectives by presenting original survey data on manufacturing skills and skill mismatch. We believe that building up detailed industry-level results is a necessary complement to the more abstract aggregate methodologies. To our knowledge, our survey is the first to directly measure both concrete skill demands and skill mismatch in an industry context.

Skill Gap Mechanisms

To situate our investigation of skill gaps in the manufacturing sector, it is useful to describe more specifically the economic factors and stories that could potentially give rise to such mismatch. Defining a skill gap starts with the observation that the market for skills has both a supply side and a demand side. A skill gap implies that the quantity demanded for a particular skill exceeds the quantity supplied. One of the puzzles that arise from the skill gap claims is that wages have not risen in a manner consistent with excess demand for skills (Osterman and Weaver 2014, Rothstein 2012). To the extent that employers or analysts claim that there is a skill gap, they are asserting that the market is not clearing. There are several potential explanations for this skill gap phenomenon, which we discuss in turn.

No skill gap exists. Employers might complain about skills in order to lobby for increased public training subsidies. In these instances there is no mismatch to explain, with cyclical economic factors accounting for observed labor market outcomes. A variant of this explanation would be that only a small minority of manufacturers experience hiring difficulties, but that these are the loudest and most vocal employers. Such a situation would generate a disconnect between the debate in the popular media and the economic statistics. It is worth noting that this "no skill gap" outcome is consistent with a two different demand stories. On the one hand, skill demands could have risen to high levels, but workers have generally kept pace in educational and skill attainment. On the other hand, it might be the case that skill demands have not risen to very high levels, thus explaining why stagnating educational attainment has not generated spiking wages for skilled production workers (Beaudry, Green, and Sand 2013).

Some manufacturers are not competitive. Another way in which skill gaps could be illusory relates to the competitive position of manufacturers. Although the conventional narrative concerning skill gaps is that supply constraints are holding up employers whose cost structure is otherwise in line with industry standards, it may also be the case that some manufacturers are simply not competitive for a variety of cost, productivity, or quality reasons. From 2000 to 2011, over 66,000 U.S. manufacturing establishments closed, and a third of all manufacturing jobs were lost (Atkinson et al. 2012). It is doubtful that even substantial improvements in America's education system could have reversed these losses. To the extent that a manufacturer can only survive by paying below-market wages, the resulting situation is not a skill gap but rather a painful transition period.

Lack of competitiveness could result from factors that are either external or internal to the establishment. External factors include globalization shocks and the rise of Chinese manufacturing. Internal factors include sub-optimal organization of production, low-productivity utilization of human resources, or poor hiring/recruitment practices.

Temporary adjustment problems exist. If employer demands for a particular skill escalate quickly, there will be an inevitable delay while the supply side of the market responds. For example, if a technology shock, such as fracking, were to lead to a sudden increase in demand for welders in areas where no such welders had previously been employed, it would take time for local educational institutions to change their curricula or for individuals with the requisite skills to move to the area. As long as this mismatch diminishes relatively rapidly, we would generally not think of this situation as a skill gap.

Supply-side frictions might lead to market failure. It might be the case that worker attitudes or low-quality educational institutions seriously hinder the regional production of particular skills. In such a scenario, the wage necessary to recruit workers with the required skills might be extremely high. Given that manufacturers deal in tradable goods whose prices are set in national and international markets, employers might decide to forego these skill inputs rather than incur heavy labor costs. Note that this latter scenario requires an additional assumption about employer training. It must also be the case that employers do not find the return from utilizing these skills to be high enough to justify internal training in these skills. This refusal to conduct internal training could result from the fact that the skills are quite general, leading to fears of poaching from other employers. Alternatively, the cost of producing the desired skills (whether general or specific) could be very high for a single manufacturing establishment. Smaller employers might lack the economies of scale necessary to efficiently conduct training in particular skills.

Increased rate of technical change. Many analysts assert that technical change has led to increased demand for higher-level skills. Although the details of these various skill-biased technical change (SBTC) stories vary, the general idea is that new technology is a complement to the skills necessary to perform complex cognitive tasks and a substitute for more routine skills (Autor, Levy, and Murnane 2003; Brynjolfsson and McAfee 2011). In theory, the overall effect on skilled manufacturing production workers is ambiguous, with some skills being made redundant and others seeing enhanced demand. However, to the extent that there are skill gaps for higher-level skills, these theories imply that technical change should be a strong predictor of these gaps. Note that SBTC by itself does not generate skill mismatch. It must be combined with another market friction or failure that explains why the supply side of the market does not respond. Nevertheless, it is important to include these technical change theories in the analysis because they imply a testable prediction about the positive correlation of technology and skill mismatch.

Problems of communication, coordination, and disaggregation might drive a wedge between supply and demand. In this case, it is not that workers have some behavioral resistance to education, or that the school system is fundamentally flawed, but rather that systematic issues prevent supply and demand from equilibrating. It might be that employers are willing to pay more for a high-productivity set of skills, and local community colleges are willing to train in such skills, but the two sides simply do not communicate about their needs and constraints, perhaps due to an absence of appropriate labor market intermediaries (communication failure). Alternatively, it might be that the payoff to investing in a production system that utilizes higher skills depends on worker investments in human capital, while the payoff to making these human capital investments depends on employer investments. In such a case, neither side will make the investments due to a coordination failure (Cooper and John 1988). To the extent that employers recognize that a higher-productivity equilibrium exists, they might complain about the fact that employees do not make the first move. Note that these scenarios also rely on an assumption that employers cannot unilaterally solve the problem through internal training.

It is important to stress that one of the factors that exacerbates these coordination and communication failures is the presence of many disaggregated establishments. Larger, more vertically integrated manufacturing plants have the potential to internalize more of these externalities. However, over the past few decades the average size of a manufacturing establishment has declined by over 40 percent (Henly and Sanchez 2009). In other research we have found that the disaggregated nature of modern manufacturing has created significant labor market challenges (Weaver and Osterman 2014). Although we do not have good data on corporate training, disaggregation may reduce internal training and increase reliance on external sources of skill training (such as community colleges) that are subject to these challenges. The disaggregation scenario raises the possibility that there will be mixed labor market effects associated with clusters of manufacturing establishments. One the one hand, such clusters may benefit from the agglomeration economies and more sophisticated labor markets often cited in the literature (Porter 2000). On the other hand, some of these clusters may suffer from workforce problems associated with disaggregation.

Empirical Strategy

We seek to answer four questions: 1) what skills do manufacturers demand for production workers? 2) what is the average level of hiring difficulties (potential skill gaps) among manufacturers? 3) what factors, including skill demands, predict these hiring difficulties? and 4) with which stories about skill gaps are the observed patterns consistent?

To answer these questions we conduct empirical analysis of our survey data. The survey was administered to a nationally representative sample of manufacturing establishments in late 2012 and early 2013, and its questions focus on "core" production workers (defined as the workers most central to the production process). The survey asked detailed questions about skill demands, and it operationalized the concept of a skill gap by measuring the number of core production worker vacancies that an employer was unable to fill for a period of three months or more. We discuss the survey methodology and key variables in greater detail below.

To address our first research question, we analyze average employer demands for the detailed skills that we measure in our survey. To address the average level of skill gaps, we measure what proportion of manufacturers show evidence of prolonged hiring difficulties. We explore the third question by conducting a reduced form analysis in which we regress an indicator for skill gaps/hiring difficulties on a range of explanatory variables. In addition to detailed skills, these include controls for supply and demand factors, as well as firm-level characteristics and measures of the regional/institutional environment. We organize these explanatory variables in groups that allow us to distinguish between the various economic stories that could explain the skill gap phenomenon.

Survey Methodology

As part of the Massachusetts Institute of Technology's Production in the Innovation Economy (PIE) project, we developed an original survey instrument that was mailed to 2,700 nationally representative manufacturing establishments with at least 10 employees beginning in October 2012.[18] The PIE survey focused on concrete skill questions, hiring and vacancy patterns, and establishment characteristics. The sample was randomly selected on a stratified basis from Dun & Bradstreet's database to reflect the frequency of different establishment sizes based on 2010 employment data from the Census Bureau's County Business Patterns survey.[19].

Because it would not be informative to ask specific questions about skills or hiring that characterize the entire manufacturing workforce (production employees, managers, clerical workers, etc.), the survey asked respondents to answer questions based on their "core" workers.

[18] For more information on the PIE project and its overall results, see Berger (2013).
[19] The sample was also trimmed to exclude industry codes for the baking, quick printing, and publishing industries We excluded these sectors because we feel that the industry dynamics associated with quick printers (such as FedEx/Kinkos), small bakeries, and newspaper/book publishers differ substantially from other manufacturing establishments.

These were defined as the employees who are most critical to the production process (for other examples of this approach see Ben-Ner and Urtasun 2013 and Osterman 1995). Examples of occupational titles that respondents classified as core workers include manufacturing associates, fabricators, assemblers, production technicians, and process operators. We asked respondents to base their answers on permanent employees, as opposed to temporary staff or independent contractors. On average permanent core employees represented 63 percent of total employment in the survey establishments.

The survey administrators called each firm in the sample to identify the individual who would be the most appropriate respondent. The target respondents were either plant managers or human resources staff with knowledge of operations. As an incentive, and as compensation for response time, a $10 bill was included with each survey packet. Excluding the ineligible establishments, incorrect addresses, and the unusable surveys, the total response rate was 35.7 percent, yielding 903 completed surveys. Responses were submitted from October 2012 to January 2013.[20]

To test data integrity, we conducted a data quality and bias analysis. The results of this exercise, which are contained in Appendix A, indicate that our data are representative of American manufacturing establishments.

Skill Demands

We can now turn to the question of what skills employers actually demand. In the survey we posed a battery of concrete questions about the skills required to perform core production jobs. For example, with respect to reading skills we asked four questions: whether the job

[20] This paper includes additional survey responses that were not available at the time that the MIT PIE report was prepared (Berger 2013; Osterman and Weaver 2014).

requires reading basic instruction manuals, complex technical documents, any document longer than five pages, or articles in trade journals. With regard to computers and technology, we asked whether the job requires skills ranging from internet search capabilities to computer-aided design skills to computer-numerically controlled (CNC) programming.

We categorized skills in two groups: basic and extended. Basic skills involve lower level skill demands in each category, while extended skills involve elevated skill demands. For example, in the math category we defined basic math as addition/subtraction, multiplication/division, and fractions/decimals/percentages. Extended math consists of algebra, geometry, trigonometry, probability, statistics, calculus, and other advanced math demands. Tables 1 and 2 contain the basic and extended skill results. More detailed data on the component skill questions is contained in Appendix B.

Table 1. Basic Skill Demands for Core Production Jobs

	All Establishments
Basic reading (ability to read basic instruction manuals)	75.6%
Basic writing (ability to write short notes, memos, reports less than one page long)	60.5%
Basic math (ability to perform all of math categories below)	74.0%
Addition and subtraction	
Multiplication and division	
Fractions, decimals, or percentages	
Require basic reading, writing, and math	42.4%
Require use of computers several times per week or more frequently	62.3%
Ability to use word processing software or ability to search Internet for information	41.7%

Source: PIE Manufacturing Survey.

Demand for particular basic skills is widespread. Seventy-six percent of establishments require basic reading for their core production positions, while 74 percent require the ability to perform a bundle of basic math skills (addition/subtraction, multiplication/division, and fractions/decimals). Sixty-two percent of establishments require computer usage on at least a weekly basis, and 42% require either basic word processing or the ability to perform Internet searches for information. What is striking, however, is how modest these demands are. Only 42 percent of establishments require all of the basic reading, writing, and math skills.

With regard to extended skills, extended reading and extended computing are the most frequently demanded higher-level skills, with 53 percent and 42 percent of establishments requiring these for core production workers, respectively (Table 2). By contrast, only 22 percent of establishments require higher-level writing. In addition to academic skills, we asked a question about whether the establishment required a unique skill that that other firms in the area do not require. About one quarter of establishments reported such a requirement.

As with basic skill demands, the extended skill demands are notable for their modesty, particularly with regard to math. Only 38 percent of establishments require at least one of the extended math skills. The breakdown in math demands is even more revealing. While just under a third of establishments require algebra, geometry or trigonometry, only seven percent require calculus or other similar advanced mathematics. Thus even among some of the more demanding establishments, the math requirements for core production workers in America are mostly at a level that is attainable by a talented high-school graduate or, without question, a community college graduate.

After reading and math, computer capabilities are the next most commonly required higher-level skills. Twenty-eight percent of establishments require core workers to be able to use computer-aided design or manufacturing (CAD/CAM) software, while a similar percentage require the use of some other type of engineering or manufacturing software. Only 19 percent require computer-programming skills. Overall, 42 percent of establishments require some type of extended computer capability.

Table 2. Extended Skill Demands for Core Production Jobs

	All Establishments
Extended reading	52.6%
Extended writing	22.1%
Extended math (ability to perform any of three math categories below)	38.0%
Algebra, geometry, or trigonometry	31.7%
Probability or statistics	14.0%
Calculus or other advanced mathematics	7.3%
Extended computer	41.9%
Use CAD/CAM	28.4%
Use other engineering or manufacturing software	29.2%
Ability to write computer programs (such as program a CNC machine for a new piece, etc.)	18.6%
Unique skill	25.9%

Source: PIE Manufacturing Survey.

Although the skill debate in America frequently focuses on "hard," STEM-related skills, there is another line of thought that argues that soft skills—such as the ability to work in teams—are increasingly important to modern high-productivity production systems (Osterman 1995; Gale, Wojan, and Olmstead 2002; Heckman and Kautz 2012). In this view, demands have risen for both interpersonal skills and for skills involving worker initiative and problem solving. We asked a variety of questions to gauge the levels of demand in these areas. Table 3 contains the percentages of establishments that reported these various skills were "very important" for core production positions, as well as the percentages that reported either "very important" or "moderately important." The results contain several notable features. First, a large majority of manufacturing establishments—more than eight in ten—place high importance on cooperation with fellow employees. Just under two-thirds of respondents also selected the ability to work in teams as a critical skill for core workers. Two other skills that received high ratings were the ability to assess the quality of output, and the ability to take steps to fix quality problems. Seventy-one and 76 percent of establishments cited these quality-related

Table 3. Percent of Establishments Citing Interpersonal, Problem-Solving, and Other Soft Skills as Very or Moderately Important for Core Jobs

	Very Important	Very or Moderately Important
Cooperation with other employees	81.2%	99.3%
Ability to evaluate quality of output	71.0%	95.8%
Ability to take appropriate action if quality is not acceptable	76.3%	97.7%
Ability to work in teams	64.2%	91.1%
Ability to learn new skills	50.1%	89.3%
Ability to independently organize time or prioritize tasks	45.6%	84.4%
Ability to solve unfamiliar problems	38.8%	83.0%
Ability to critically evaluate different options	35.7%	74.1%
Ability to initiate new tasks without guidance from management	35.2%	80.9%

Source: PIE Manufacturing Survey.

measures as very important. Thus interpersonal skills and quality assessment appear to be critical skills for core workers in production systems around the country.

The next notable feature about the results is the relatively low level of "very important" responses for skills that are often thought to be essential for modern high-tech, high productivity manufacturing. While eight out of ten establishments view solving unfamiliar problems or initiating new tasks without guidance as at least moderately important, less than 40 percent view these skills as very important for core production workers. Only half feel that the ability to learn new tasks is very important. The picture that these results provide of core production systems in U.S. manufacturing is one in which performing high-quality work in a cooperative fashion using existing procedures is important, but exercising creative problem solving or taking initiative is substantially less so.

There are several takeaways from these descriptive survey results on skill demands for core production workers. First, basic academic skills and interpersonal skills are important. Demand for basic levels of math, reading, and computer skills is widespread. Requirements for extended reading and computer abilities, in particular, are common, encompassing more than half of all manufacturing establishments. Cooperation and teamwork are also skills that large numbers of manufacturing establishments place great value on. However, at the same time, a substantial percentage of establishments have relatively low skill demands. Even among the plants requiring higher skill levels, the skill demands appear very attainable, particularly with regard to math. With regard to skills that are thought to be part of high-tech flexible manufacturing systems, emphasis on problem solving, initiative, self-management and other skills appears surprisingly muted.

Hiring and Vacancies: Evidence on Skill Gaps

With regard to the hiring process, PIE Survey data suggest that most employers are able to find the workers they seek in a reasonable time frame (Table 4). The mean establishment in our survey required about six weeks to recruit and hire a core worker, while the median establishment required four weeks. Employers in the survey received an average of 24 applications per open position, and conducted six interviews per open position. The average acceptance rate by successful applicants was 85%.

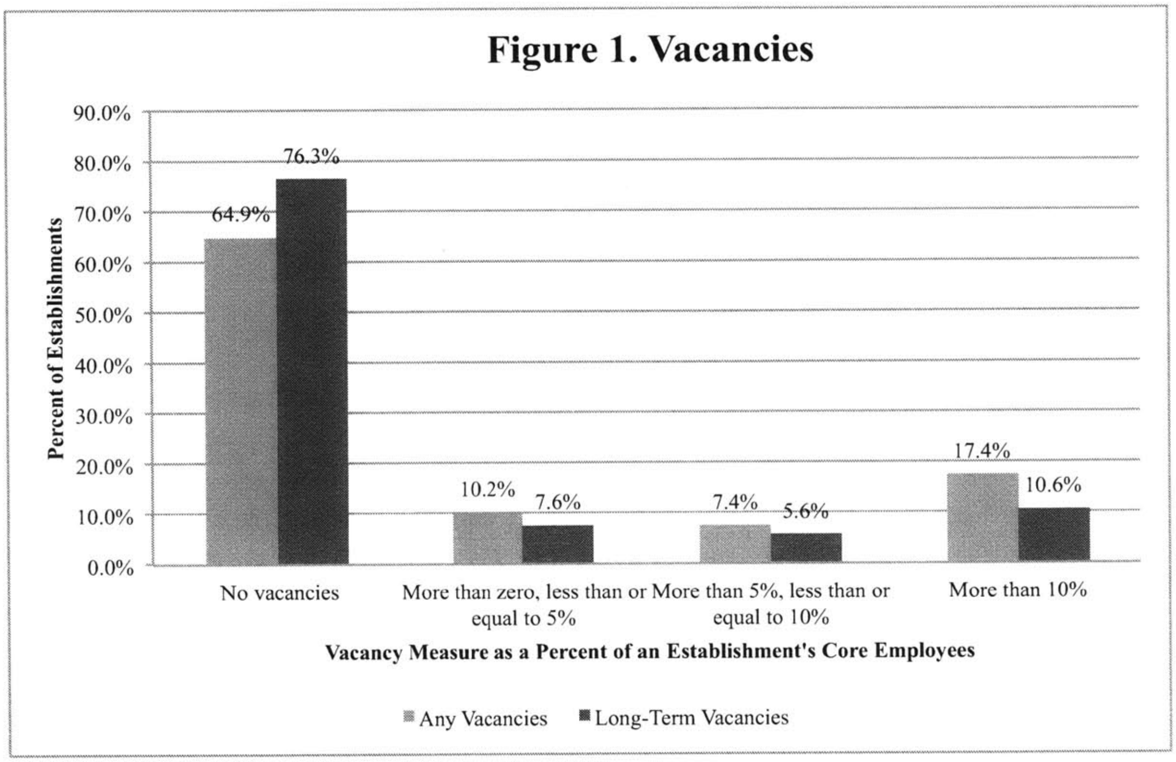
Table 4. Hiring Funnel for Core Workers

	Mean	Median
Weeks required to recruit and hire applicant (start of process to extension of offer)	5.9	4.0
Typical number of applications received per open core position	23.8	10.0
Typical number of interviews conducted per open core position	5.9	5.0
Acceptance rate by applicants who are extended an offer	85.4%	95.0%

Source: PIE Manufacturing Survey.

In order to probe more deeply regarding skill mismatch, we focus on vacancies among core production positions. Some positive level of vacancies is required for the smooth operation of the labor market, and therefore the presence of a vacancy for a given position at a particular point in time is not necessarily a sign of a problematic gap between demand and supply. To address this issue, we asked establishments about the number of current core production vacancies that had persisted for three months or more. We believe that such long-term vacancies are the best concrete measure of potential skill gaps. Even in the case of these extended vacancies, there are factors other than skill mismatch that can explain the existence of such a prolonged job opening. As Peter Cappelli (2012) has noted, in the face of weak product demand, some firms may advertise an open position while waiting for a truly extraordinary candidate to come along. Indeed, Davis, Faberman, and Haltiwanger (2012) have argued that the intensity with which employers searched for workers fell during the Great Recession. Nevertheless, this long-term vacancy measure represents a substantial improvement over the use of undifferentiated vacancies. Long-term vacancies can be viewed as an upper bound on the potential amount of skill mismatch.

Figure 1 contains data on core worker vacancies and long-term core worker vacancies as a percentage of an establishment's core workforce. Nearly two thirds of establishments do not have any core worker vacancies, and 76 percent do not have any long-term vacancies. Just under eight percent of establishments have long-term vacancies that amount to between zero and five percent of the establishment's total permanent core workers. About sixteen percent of establishments experienced long-term vacancies at a level that was greater than five percent of their core workforces.



Source: PIE Manufacturing Survey.

These results contrast greatly with the opinion surveys from non-random samples that have shaped the public debate about manufacturing skill gaps. While the Deloitte/NAM 2011 opinion survey (which has received considerable publicity) finds that 74% of manufacturers suffer from a lack of skilled production workers, the PIE Survey data indicate that at most a quarter of manufacturing establishments show signs of hiring distress with regard to production workers. Similarly, the Deloitte survey reports that the median manufacturer has vacancies equivalent to five percent of its total workforce. Although our survey focuses on core workers and not the entire manufacturing workforce, it is worth noting that our data indicate that the median firm has zero core worker vacancies. Given that core workers are 63 percent of establishment employment, these results call into question both the incidence and severity of manufacturing skill gaps. We believe the PIE Survey data indicate that at most 16-25 percent of manufacturing establishments have signs of hiring distress that could potentially indicate structural mismatch.

Predictors of Hiring Difficulties

We now turn to understanding what factors predict the signs of hiring difficulties that we do observe. Are the long-term vacancies in our data the result of skill demands? If so, which skills? We can also explore whether other establishment-level or regional variables are associated with hiring problems and assess which stories about the origin of skill gaps these results are consistent with.

We utilize two dependent variables in our initial analysis. The first is a binary indicator that equals one if an establishment reports any long-term vacancies among core workers. We employ logistic regression to estimate these specifications (the tables report marginal effects). The second is a continuous measure of long-term vacancies as a percentage of total core workers. We use linear regression to estimate these specifications. These two dependent variables can be thought of as measuring the incidence and severity of hiring difficulties, respectively. After our initial analysis, we focus on the binary indicator for the presence of any long-term vacancies in order to streamline the subsequent analysis. All data in the logit and linear analyses are unweighted. We cluster standard errors by four-digit NAICS industry codes. Descriptive statistics for all key variables are contained in Appendix B.

Our empirical strategy is to first examine the relationship between skill variables and long-term vacancies (Table 5). We then turn to an investigation of different packages of explanatory variables that are associated with different economic explanations for the skill gap phenomenon (Table 6). We should note that our data are cross-sectional and that instrumental variables or other techniques that clearly establish causality are generally unavailable given both the questions that we explore and the nature of our data. We recognize, for example, that just as wage levels could cause hiring problems, employers might respond to hiring problems by adjusting wages. Similar endogeneity arguments can be raised with regard to other variables as well. Our strategy is thus to use a reduced form approach to test for consistency of the cross-sectional data with various economic stories as outlined above. Although we cannot definitively prove that a given mechanism or dynamic is taking place, we can show that the data are not consistent with some mechanisms. Furthermore, we can shift the prior probability associated with the economic stories for which we find positive evidence.

In the first two columns of Table 5 our right-hand side variables consist of binary indicators for whether a plant demands the extended skills discussed above: extended reading, writing, math, and computer demands. We also include demand indicators for soft/critical thinking skills: ability to learn new skills, ability to solve unfamiliar problems, ability to work in teams, and ability to evaluate quality of output. In addition, as our survey was stratified by employment size, we include controls for total establishment employment and employment squared (to capture non-linearities) with all specifications (in Tables 5 and 6).

The results from the first two columns of Table 5 indicate that not all higher-level skills have similar relationships with hiring difficulties. Only extended reading and extended math demands are significant in this regard. Demand for extended reading is associated with a 9.6 percentage point increase in the probability of an establishment experiencing a prolonged vacancy (column (1), $p<0.01$) and a 1.1 percentage point increase in long-term vacancies as a percentage of an establishment's core workforce (column (2), $p<0.05$). Extended math is associated with a 10 percentage-point increase in the probability of having a prolonged vacancy ($p<0.01$) and a 1.7 percentage point increase in the percentage measure ($p<0.05$). Demand for the ability to learn new skills is a marginally significant predictor of long-term vacancies ($p<0.10$), but in general the soft/critical thinking marginal effects are small in magnitude and insignificant. Demands for higher-level computer and writing skills are likewise insignificant. Establishment size has a significant relationship with long-term vacancies, but the sign of the effect depends on the dependent variable. In the binary specification, increasing total employment by 100 employees is associated with a significant 4.5 percentage point increase in the probability of any long-term vacancy, while in the linear model a similar increase is associated with a significant 0.3 percentage point decrease in long-term vacancies as a proportion of the establishment's workforce. Thus larger establishments are more likely to have a long-term vacancy—perhaps because they have more resources to tolerate unfilled positions—while smaller establishments that have vacancies experience higher percentage levels of these hiring difficulties. This latter effect is a somewhat mechanical result of establishment size. To streamline the analysis, we will focus on the binary vacancy dependent variable for the remainder of this paper. This measure is arguably the most general and the most policy relevant.

The last column of Table 5 explores whether the intensity of demand for higher-level skills matters. For the skill categories in which we asked about three or more distinct higher level skills, we construct indices for each skill category that measure how many extended skills of that type an establishment demands. For example, if an establishment demands one extended math skill, the value of the math index would be one, while the index value would equal three if the establishment demands three such skills. The results indicate that greater intensity in math and reading demands is associated with significant increases in long-term vacancies. However, the results for the computer index are surprising. Greater intensity in higher-level computer demands is associated with a significant 9.2 percentage point decrease in the probability of experiencing a long-term vacancy. This result runs counter to the idea that computer technology is raising skill demands for production workers. We will explore the role of technology further below.

Table 5. Long-Term Vacancies and Skill Demands

	(1)	(2)	(3)
	Any Long. Vac.	Long Vac Pct.	Any Long Vac.
Extended reading	0.096***	0.011**	
	(0.031)	(0.005)	
Extended writing	-0.020	-0.002	
	(0.037)	(0.008)	
Extended math	0.100***	0.017**	
	(0.034)	(0.007)	
Extended computer	-0.011	0.007	
	(0.032)	(0.006)	
Ability to learn new skills	0.063*	-0.003	
	(0.035)	(0.005)	
Ability to solve unfamiliar problems	-0.049	0.009	
	(0.039)	(0.006)	
Ability to work in teams	0.000	0.000	
	(0.033)	(0.006)	
Ability to evaluate quality of output	-0.039	-0.001	
	(0.035)	(0.006)	
Demand for any extended skill			
Extended reading index			0.137***
			(0.042)
Extended math index			0.120**
			(0.054)
Extended computer index			-0.092**
			(0.042)
Total estab. employment (100s)	0.045***	-0.003**	0.049***
	(0.014)	(0.001)	(0.015)
Observations	833	832	846
R-sq./Pseudo R-sq.	0.049	0.043	0.055

Source: PIE Manufacturing Survey. Standard errors in parentheses. All specifications also include a quadratic in establishment employment. Specification (1) uses logit analysis and reports marginal effects. Specification (2) employs linear regression. Standard errors are clustered by four-digit NAICS codes.
* p<0.10, ** p<0.05, *** p<0.01

Before moving on to the rest of the analysis, it is worth commenting on the overall explanatory power of the skill variables. Although the marginal effects of math and reading skill demands are highly significant, the skill variables combined explain four to five percent of the total variation in long-term vacancies.[21] Given that these skill measures are the most precise ones currently available for manufacturing establishments, this result implies that it will be important to investigate a range of other predictors of hiring difficulties in future research.

In Table 6 we expand our analysis to include variables associated with various skill gap mechanisms and explanatory stories. We employ the binary long-term vacancy indicator as our dependent variable, and we include the binary skill indicators and employment size controls in all specifications. Our first package of "mechanism" regressors contains external variables associated with the "no skill gap," "not competitive," and "temporary adjustment" explanations discussed above. To test for whether macroeconomic factors are driving hiring patterns, we have included the county unemployment rate from 2011.[22] We have included a variable measuring the difference between an establishment's average wage for core production workers and the county's average manufacturing wage to measure whether non-competitive wages are responsible for prolonged vacancies.[23] We additionally include an indicator for whether a plant reports having more foreign then domestic competition. If globalization pressures were behind

[21] Although the pseudo-R-squared value presented with the logit models cannot be interpreted as the percentage of explained variation, calculation of the R-squared value for linear probability models with the same specifications (not shown) confirms the four to five percent range of explained variation.
[22] Bureau of Labor Statistics (BLS) Local Area Unemployment data.
[23] BLS Quarterly Census of Employment and Wages (QCEW), third quarter 2012.

hiring difficulties, we would expect the marginal effect of this variable to be positive and significant. To explore whether hiring difficulties are an adjustment problem for plants that are growing, we have added a variable measuring the percentage growth in the core production workforce over the past two years.

The results from column (1) of Table 6 do not show a significant relationship between these variables and the probability of experiencing a long-term vacancy. Increased unemployment is associated with a decrease in prolonged vacancies, as would be expected if the economy rather than supply frictions were driving vacancies, but the effect is insignificant. Higher wages relative to the average county manufacturing wage are actually associated with a very small and insignificant positive effect. Both more foreign competition and greater percentage employment growth over the past two years are positively associated with vacancies (as would be predicted by the globalization and temporary adjustment theories), but the effects are very imprecise. By contrast, extended reading and math continue to be large and significant predictors of long-term vacancies. The other skill variables generally remain small and insignificant.

Our second package of mechanism variables contains measures of internal firm practices that may indicate a link between an establishment's human resource strategy and the hiring outcomes it experiences (as opposed to external supply frictions). We include an indicator for whether an establishment reports having a preference for internal hiring to fill vacancies (thus implying an internal labor market (ILM)). As a measure of the plant's approach to quality systems, we incorporate a variable that measures the percentage of a plant's core workforce that participates in a Total Quality Management (TQM) program. To directly measure training, we use a variable that contains the total hours of formal training that an establishment provides to its core workers on an annual basis. If plants have the ability to solve some of their recruiting difficulties through the adoption of "high-road" human resource strategies such as ILMs, TQM systems, and internal training, then we would expect the marginal effects of these variables to be negative. We also include an indicator for union status, as unions have the potential to promote workforce investment and training. Finally, because one of the important factors an employer must deal with in securing a trained workforce is the specificity of the skills it demands, we include a binary indicator for whether a plant demands a unique or specialized skill not demanded by other area firms. By testing whether problems exist with unique skill demands, we are testing whether establishments face hiring challenges as a result of being unable or unwilling to internalize the production of firm-specific skills. Such challenges call our attention to the boundaries of the firm and its relationship with external economic actors.

Table 6. Long-Term Vacancies and Skill Mismatch Mechanisms

	(1) Any LT Vac.	(2) Any LT	(3) Any LT	(4) Any LT	(5) Any LT	(6) Any LT Va
Extended reading	0.091***	0.093***	0.094***	0.120***	0.099***	0.115***
	(0.034)	(0.032)	(0.030)	(0.034)	(0.032)	(0.038)
Extended writing	-0.019	-0.002	-0.025	-0.026	-0.015	0.000
	(0.038)	(0.037)	(0.038)	(0.038)	(0.036)	(0.038)
Extended math	0.094**	0.092**	0.093***	0.104***	0.111***	0.100**
	(0.038)	(0.037)	(0.034)	(0.036)	(0.034)	(0.039)
Extended computer	-0.003	0.007	-0.020	-0.003	-0.018	0.004
	(0.034)	(0.035)	(0.031)	(0.031)	(0.032)	(0.033)
Ability to learn new skills	0.065*	0.056	0.058	0.068*	0.074**	0.053
	(0.035)	(0.038)	(0.036)	(0.035)	(0.035)	(0.037)
Ability to solve unfam. probs.	-0.053	-0.063	-0.046	-0.057	-0.055	-0.085**
	(0.041)	(0.042)	(0.039)	(0.039)	(0.039)	(0.039)
Ability to work in a team	-0.000	0.012	-0.005	0.009	-0.005	0.020
	(0.033)	(0.030)	(0.033)	(0.033)	(0.032)	(0.032)
Ability to eval. quality of output	-0.044	-0.044	-0.035	-0.042	-0.043	-0.052
	(0.035)	(0.034)	(0.033)	(0.034)	(0.033)	(0.033)
Package 1: External/no skill gap						
Unemp. rate	-0.006					-0.005
	(0.007)					(0.007)

Wage differential	0.002					0.002
	(0.002)					(0.002)
More foreign comp.	0.022					-0.002
	(0.040)					(0.041)
Pct. change in core emp. (2 years)	0.031					0.043
	(0.042)					(0.050)
Package 2: Internal HR						
Pref. for internal hiring		-0.036				-0.041
		(0.031)				(0.036)
TQM pct.		-0.001				-0.000
		(0.001)				(0.000)
Union		-0.001				-0.004
		(0.039)				(0.044)
Formal training hrs.		-0.000				-0.000
		(0.000)				(0.000)
Unique skill		0.079**				0.079***
		(0.034)				(0.030)
Package 3: Supply/Geo. frictions						
Total county emp.			-0.000			-0.000
			(0.000)			(0.000)
Comm. coll.--no resources			0.010			0.020
			(0.037)			(0.040)
Comm. coll.--no help			-0.011			-0.026
			(0.036)			(0.043)
Package 4: Technology						
High tech				-0.109**		-0.070
				(0.042)		(0.046)
Above avg. tech.				-0.017		-0.033
				(0.033)		(0.031)
Frequent product innovation				0.020		0.035
				(0.039)		(0.047)
Frequent process innovation				-0.005		-0.006
				(0.039)		(0.047)
Package 5: Disagg./Communication/Coordination						
Industry cluster					0.107***	0.118***
					(0.037)	(0.037)
Work w/ind. cluster					-0.019	-0.001
					(0.046)	(0.053)
Other institutions					-0.011	-0.031
					(0.032)	(0.033)
Observations	*770*	*780*	*823*	*796*	*793*	*678*
Pseudo R-sq.	0.050	0.060	0.058	0.056	0.065	0.106

Source: PIE Manufacturing Survey. Table contains marginal effects from logit analysis. Standard errors in parentheses. All specifications include controls for total establishment employment and a quadratic in establishment employment. Standard errors are clustered by four-digit NAICS codes. * $p<0.10$, ** $p<0.05$, *** $p<0.01$

None of the internal human resource variables show significant effects. However, unique skill requirements are a significant predictor of hiring difficulty. Demanding a unique skill is associated with a 7.9 percentage point increase in the probability of experiencing an extended core worker vacancy. The predictive power of this variable indicates that we should pay attention not only to the general quality of the region's education system but also to the various challenges and externalities associated with obtaining workers with highly specialized skills.

Our third package of mechanism variables includes proxies for supply frictions and geographic constraints. We include total county employment to investigate whether hiring difficulties are the result of the size of the regional labor market (this variable also serves as a rough proxy for geographic mismatch from historical patterns of development). We include two measures relating to community colleges to proxy for the quality of labor supply. The first is an indicator variable that equals one if the establishment reported that its local community college lacked sufficient resources (financial and physical plant) to produce high-quality graduates. Respondents without a local community college were coded as lacking resources. The second is an indicator that equals one if the establishment reported that the community college had not been helpful in meeting its needs. Respondents who had no local community college or no relationship with that college were coded as not helpful.[24] It should be noted that this second

[24] Because fewer respondents answered these specific community college questions that some of the more general skill questions, we have not dropped missing responses for these questions from the analysis in order to preserve sample size. Rather, we have included dummy variables that flag these observations in order to control for any heterogeneity. The community college variables have the same signs and significance levels in specifications that drop the flagged observations.

measure is somewhat ambiguous as it is not clear who is at fault for this unhelpful status. Nevertheless, if the failure of local educational systems to produce skilled production workers were a major driver of hiring difficulties, we would expect a positive correlation between both of the community college variables and long-term vacancies. The results, however, only show weak effects for these various supply measures. Counties with larger employment bases are associated with lower incidence of prolonged vacancies, but the relationship is insignificant. The effect of the no-resources variable is positive but insignificant. The no-help measure is actually associated with lower experience of vacancies, but again is insignificant. Although these three measures clearly provide only partial measures of the quality of regional labor supply, they do not provide significant evidence in support of pure supply-side causes for hiring problems.

Our fourth package of mechanism regressors focuses on technology variables. We include a measure of whether a plant is in a high-tech industry subsector (based on Bureau of Labor Statistics methodology; see Hecker 2005). We additionally include an indicator that equals one if an establishment reported that an industry expert would rate the level of the establishment's technology as above average. We incorporate two variables designed to measure innovation. The first is an indicator that equals one if establishments report that they introduce new or redesigned products with more-than-incremental changes at least every two years or more frequently. The second is a similar variable for process innovation in production methods. The most surprising result from this specification is that high-tech status is associated with a significant 10.9 percentage point reduction in the probability of experiencing a long-term vacancy. The sign on the above-average technology effect is also negative, although imprecisely measured. At first blush, this is a surprising result. Conventional stories about technology generally predict that higher levels of technology will be associated with higher skill demands and consequently greater hiring problems. Two points are worth keeping in mind. First, because in these specifications we have independently controlled for skill demands, it could be that high-tech status is serving as a proxy for more sophisticated or effective human resources or recruitment practices. However, we should note that when we include the high-tech indicator in specifications that do not control for other skill demands (not shown), it consistently shows a negative—albeit insignificant—relationship with long-term vacancies. Above-average technology becomes positive, although very small in magnitude and highly insignificant. These results highlight a second point: the high-tech result is consistent with the fact that computer skill demands were not associated with greater hiring problems. We will discuss this issue further below; at a minimum the fact that these variables are not positive and significant is a sign that the relationship between technology and skill demands may be more complex than is commonly thought. The product and process innovation effects have opposite signs and are not significant predictors of hiring difficulties.

Our fifth package of mechanism regressors explores the disaggregation/communication/coordination failure scenario discussed above. We first include a measure of whether an establishment reports being part of a geographic cluster of firms in the same industry. The expected sign of the relationship between industry cluster and hiring difficulties is ambiguous. On the one hand, clusters may imply denser labor markets with a greater availability of specialized skills, thus lowering hiring problems. On the other hand, cluster status might imply a disaggregated situation where competition for workers is more intense or where firms have greater difficulty communicating or coordinating their needs with other firms and educational institutions. To test whether communication is an issue, we also include an indicator that equals one if an establishment reports that it works with the other firms in its cluster on training, skills or other employment-related issues. To test whether the cluster might suffer from inadequate labor market intermediaries or other problems of disaggregation, we include an indicator that equals one if an establishment reports that institutions other than the community college have been helpful in employment matters. The results show that cluster status is highly predictive of hiring difficulties. Membership in an industry cluster is associated with a 10.7 percentage point increase in the probability of incurring a long-term vacancy ($p<0.01$). The cluster cooperation and other institution effects are both negative—possibly implying that collaboration and other institutions can address some of the challenges of disaggregation—but both are imprecisely measured. Taken together, this package of variables provides an indication that some aspect of clusters may be problematic from a labor market perspective.

In the final specification in Table 6, we combine the skill regressors with all of the various mechanism regressors. Extended reading and math maintain their large and significant associations with hiring difficulties. Interestingly, the demand for the ability to solve unfamiliar problems shows a significant negative effect in this general specification. Demand for this skill is associated with an 8.5 percentage point reduction in the probability of experiencing a long-term vacancy. Among other significant predictors, unique skill maintains its predictive power, implying that manufacturers struggle with the issue of externalizing training for firm-specific skills. In this aggregate specification, high-tech loses its significance (but retains a negative sign and a reasonably large magnitude). Industry cluster retains its large and highly significant association with hiring problems. In many ways this latter result is the most intriguing, as it is one of the largest and most robust effects in our data. The strength of the effect raises the possibility that geographical or industry factors may play a significant role in hiring frictions. Before discussing the overall regression results, we explore potential explanations for this effect.

Cluster Discussion

The cluster result presents a puzzle. One might reasonably suppose that the more specialized workforce and more sophisticated labor market institutions often associated with clusters would mitigate rather than exacerbate hiring problems. Our analysis of sub-national geographic and industry effects necessarily remains tentative due to limited sample size. However, we have conducted some exploratory analyses that point toward interesting areas for future research. At first blush, it might seem that greater levels of hiring problems in clusters simply reflect more intense competition for a limited pool of available workers. One possibility is that clusters are competing for a pool of workers with higher-level general skills (such as math and reading), but that supply does not respond because cluster demand is too small to influence overall production of these general skills. Regardless of the skills involved, if non-cluster firms were bidding wages for similar workers out of reach of manufacturing plants, then we would expect to see more sensitivity to wage differentials in clusters than in non-clusters. However, the results from additional analysis of interaction effects (not shown) contradict these hypotheses. The marginal effects on long-term vacancies of demanding higher-level reading and math skills have roughly the same magnitude and significance among cluster and non-cluster establishments, and wage differentials show no greater predictive power in clusters than in non-clusters.

Another possibility is that clusters imply a greater specialization of skills than non-cluster settings. Such highly specialized skills may be a problem if a plant is unwilling or unable to conduct internal training and if local training providers also refuse to do so (perhaps due to limited economies of scale). The results of our additional analysis likewise cast doubt on this hypothesis. Demand for unique skill is a large and significant predictor of hiring difficulties for non-cluster establishments, but a much smaller and insignificant predictor for cluster establishments. Thus clusters, in accordance with agglomeration theories, would appear to mitigate rather than exacerbate the hiring of highly specialized skills (Porter 2000).

If these factors do not, at least initially, appear to explain the cluster effect, what factors might contribute? One potential explanation relates to adjustment problems and the communication and coordination failures mentioned earlier. When industry sectors and establishments experience growth, it is possible that cluster and non-cluster areas respond differently to this growth. It could be that the disaggregated nature of clusters presents special communication and coordination challenges in the face of changing demand for workers. Note that under this latter version of the disaggregation hypothesis, clusters might present labor market advantages under stable equilibrium conditions, but face unique difficulties under conditions involving economic growth or decline. Indeed, our tentative analysis of interaction effects shows that while growth in the total core workers employed at non-cluster establishments is not predictive of hiring problems, growth in core workers employed in cluster establishments is associated with a much larger and significant increase in long-term vacancies. Ultimately, more analysis and additional data will be required to pin down the mechanisms that are underlying the cluster effect.

Discussion of Long-Term Vacancy Regression Results

Overall, these long-term vacancy results qualify our view of skill mismatch. Even among the minority of manufacturing establishments that do show potential signs of hiring distress, the relationship between skill demands and hiring problems is not simple or clear-cut. While higher-level math demands are predictive of hiring difficulties—consistent with the popular focus on science, technology, engineering, and math (STEM) skills—higher-level computer demands are not. Extended reading skills are surprisingly prominent as predictors of long-term vacancies. Many other skill demands, including those for soft skills and problem-solving/initiative skills, are not associated with hiring difficulties. Overall, skill demands—even very precisely measured skill demands—explain about five percent of the variation in long-term vacancies.

When we examine the mechanisms that might contribute to hiring difficulties, a mixed picture emerges. High-tech plants, often thought to be hampered by inadequate workforce skills, are not associated with significantly greater hiring difficulties (either with or without independent controls for skill demands). Consistent with the computer skill results, high-tech establishments generally experience lower rates of hiring problems, although the significance of this effect varies by specification. Given that high-tech plants do have significantly higher skill demands (results not shown), the long-term vacancy results imply that some element of operations or environment enables high-tech establishments to satisfy their high-skill demands. It is possible that technology simultaneously substitutes for some skill demands in a manner that alleviates hiring difficulties, or it could be that high-tech plants have more effective human resource or recruitment practices, including better relationships with labor market institutions. It is also possible that workers more readily supply their labor to high-tech establishments that they view as having a more promising future than other manufacturing sectors. Exploring these dynamics is a promising area for future research.

Beyond higher-level math and reading demands, the two largest and most consistently robust predictors of hiring difficulties are demand for unique skills and membership in an industry cluster. Both of these factors raise the question of the relationship between a manufacturing establishment and other regional actors, including other firms, educational institutions, and training providers. The positive relationship between unique skill demands and long-term vacancies indicates that there are a number of establishments that are unwilling or unable to solve their skill challenges through internal training, even for skills that are highly specific to a particular plant. As the size of manufacturing plants has declined (Henly and Sanchez 2009)—and with it the economies of scale in internal training—the issue of how manufacturing plants obtain specialized skills from external institutions is worthy of attention.

Further examination of cluster effects indicates that hiring difficulties in clusters may be due to adjustment problems in the face of employment changes rather than heightened competition or specialization of cluster skill demands, although sample size limits our ability to explore sub-national phenomena. We hypothesize that this effect may be related to challenges associated with disaggregated establishments and communication/coordination failures. Overall, the picture that emerges is one in which what matters for the smooth operation of the labor market is the connection between the demand and supply side of the market rather than the unilateral actions of either side. Pure regional/supply-side variables are not predictive, and neither are variables related to purely internal firm practices. Rather, factors that point to the existence of multiple disaggregated firms or that complicate the interaction between a firm and labor supply institutions (such as unique skill demands) emerge as the most significant predictors of hiring challenges.

Conclusion

Assuring the balance of supply and demand for labor in the economy is critical for economic growth as well as economic opportunities for workers. The elevated unemployment rates in the recent recession have heightened debate over whether structural mismatch is driving poor labor market outcomes. One of the most common claims is that employers cannot find the skilled workers they seek due to gaps between skill demands and supplies in the labor market. Unfortunately, the existing literature tests for mismatch by relying on highly aggregated data and noisy proxies that obscure underlying mechanisms as well as the degree to which mismatch takes place within industries. We address these issues by presenting and analyzing results from a survey of U.S. manufacturing establishments. Our survey is the first, to our knowledge, to directly measure concrete employer skill demands and hiring experiences in a nationally representative survey at the industry level.

We measure skill demands by asking detailed questions about the skill requirements for core production jobs. We quantify skill gaps by measuring the number of vacancies among core production workers that have persisted for three months or more. We find that basic skill demands are widespread, but that demands for higher-level skills and skills related to high-performance work systems are surprisingly modest. With regard to skill gaps, three quarters of establishments show no sign of hiring difficulties. We estimate an upper bound on potential skill gaps of 16-25 percent of manufacturing establishments. This finding contrasts sharply with other, non-representative surveys that have reported figures in excess of 60 or 70 percent (Deloitte 2011).

We explore the characteristics and factors associated with the minority of establishments that show signs of hiring difficulties. Higher-level math and reading skills are predictive of long-term vacancies, while computer skills and soft/critical-thinking skills are not. High-tech establishments appear to experience no higher—and possibly lower—hiring difficulties than other types of manufacturers. Unique skill demands and membership in an industry cluster are both highly predictive of long-term vacancies. These results may imply that disaggregated establishments and establishments with specialized skill demands have trouble recruiting workers, not so much from low-quality educational institutions as from the communication, coordination, and economy-of-scale challenges that accompany disaggregation and specialization.

Overall, we find that skill mismatch is a limited phenomenon. To the extent it occurs, our data imply that it is related not to spiking high-tech skill demands but rather to higher-level demands for basic academic skills (reading and math), as well as the mechanics of a disaggregated labor market. These mechanics of disaggregation and specialization may provide a more appropriate framework for thinking about labor market challenges than conventional skill mismatch formulations.

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Appendix A: Survey Data Quality Analysis

To test the quality of the data we took two steps: a bias analysis using data available in the Dunn and Bradstreet universe on respondents and non-respondents to determine what response biases might exist, and, secondly, a comparison of patterns in our survey with those in the Census Bureau's Current Population Survey (CPS) in order to assess external validity.

We conducted the response bias analysis by using a linear probability model to regress an indicator for completing the survey on indicators for the various establishment size categories, indicators for geographic region, and indicators for two-digit SIC codes.[25] The results indicate that the largest size categories of establishments—those with more than 100 employees—were 12-20 percent less likely to respond.[26] We employ establishment size weights in our descriptive statistics to correct for these deviations. We also control for employment size in our regression specifications. Establishments in the South were eight percent less likely to respond than their counterparts in the Northeast. Other geographic differences were insignificant. Out of the 20 two-digit industry SIC codes, five were significantly more likely to respond than the base category of food products, with increased response probabilities in the 8-12 percent range. These sectors were: rubber and miscellaneous plastic; stone, clay, glass, and concrete; fabricated metal; industrial machinery and equipment; and electronic equipment except computers. Many of these are establishments in the size category—less than 100 employees—that was most likely to respond. Our use of establishment size weights and employment controls should mitigate this issue.

To test the external validity and data quality of our survey after the application of the relevant weights, we calculated a number of statistics on the profile of the production workers

[25] We conducted this bias analysis with SIC rather than NAICS codes because only SIC codes were available for the nonrespondents in the sample.
[26] Results are available upon request from the authors.

covered by the survey and compared these with comparable statistics on manufacturing production workers from the 2012 CPS merged outgoing rotation groups (as compiled by the National Bureau of Economic Research). We should note that because the PIE Survey is an establishment survey and the CPS is a household survey, we expect some deviation between the two surveys.[27] The key question is whether the broad data patterns are similar. Table A1 contains the comparison data.

Table A1. Comparison of PIE and CPS Data

	PIE	CPS (2012)
Hourly wage	16.95	16.49*
Union	18.1%	13.7%*
Female	26.7%	26.6%
Age 30 or less	20.6%	21.3%
Age 31-40	27.5%	22.0%*
Age 41-55	35.8%	38.8%*
Age 56 plus	16.1%	17.9%*

Source: PIE Manufacturing Survey and Current Population Survey NBER Merged Outgoing Rotation Group (MORG) data.
Note: We have used employment weights for the PIE survey and individual earnings weights for the CPS.
*=significant differences at 95 percent level or higher.

We can see from examination of Table A1 that although there are a number of statistically significant differences between means in the two surveys, the magnitude of these differences is generally modest. Average wages for production workers are quite close, and both datasets show almost identical percentages of female production workers. The PIE survey has somewhat greater union representation among production workers (18 percent vs. 14 percent). In terms of age structure, the percentages of young (less than 30 years old) and old (56 years old or

[27] We chose to validate our data with the CPS rather than with an establishment survey such as the Bureau of Labor Statistics' Current Employment Statistics (CES) survey because the CES includes working supervisors in its definition of production workers. As a result, wages and some other workforce characteristics are not comparable between the PIE survey and the CES.

older) workers were very similar, while the PIE survey reports higher percentages of young middle-aged workers (aged 31-40) than the CPS (28 percent vs. 22 percent). The CPS reports a correspondingly greater percentage of older middle-aged workers. Overall, these differences are modest and imply that the PIE survey is faithfully capturing data on manufacturing production workers and the establishments at which they are employed.

Appendix B: Survey Data and Descriptive Statistics

Table B1. Skill Question Responses

	Proportion Answering Yes or Selecting Option	SE
Does the job require reading:		
Basic instruction manuals	0.756	0.022
Complex technical documents or manuals	0.386	0.024
Any document that is longer than 5 pages	0.347	0.023
Articles in trade journals, magazines, newspapers	0.106	0.017
Does the job require:		
Preparing bills, invoices, etc.	0.180	0.020
Writing short notes, memos, reports, or requests	0.605	0.024
Writing anything at least one page long or longer	0.218	0.021
Writing anything at least 5 pages long or longer	0.047	0.011
What is the frequency of computer use:		
Everyday	0.503	0.026
A few times per week	0.120	0.016
Less often	0.109	0.015
Never	0.268	0.023
Does this job require (applies only to estab. that do not report "never" requiring computer use):		
Use of word-processing software	0.279	0.022
Use of spreadsheet or database software	0.220	0.029
Computer-aided design (CAD) or computer-aided manufacturing (CAM) skills?	0.284	0.023
Use of other engineering or manufacturing software	0.292	0.023
Ability to write computer programs	0.186	0.020

Performing internet searches and/or using the World Wide Web to gather information or seek solutions	0.330	0.024
Does this job require mathematical operations involving:		
Addition and Subtraction	0.944	0.011
Multiplication and Division	0.856	0.017
Fractions, decimals, or percentages	0.779	0.020
Using algebra, geometry, or trigonometry	0.317	0.024
Probability or statistics	0.140	0.017
Calculus or other advanced mathematics	0.073	0.014
How important is cooperation with other employees:		
Very important	0.812	0.019
Moderately important	0.181	0.019
Not very important	0.005	0.005
Not at all important	0.000	0.000
Not applicable	0.002	0.002
How important is the ability to work in teams:		
Very important	0.642	0.024
Moderately important	0.269	0.022
Not very important	0.076	0.014
Not at all important	0.004	0.002
Not applicable	0.009	0.005
How important is the ability to solve unfamiliar problems:		
Very important	0.388	0.024
Moderately important	0.442	0.024
Not very important	0.142	0.017
Not at all important	0.018	0.008
Not applicable	0.010	0.004
How important is the ability to learn new skills:		
Very important	0.501	0.025
Moderately important	0.392	0.023
Not very important	0.096	0.016
Not at all important	0.010	0.007
Not applicable	0.002	0.002
How important is the ability to initiate new tasks without guidance from management:		
Very important	0.352	0.024
Moderately important	0.456	0.024
Not very important	0.169	0.018
Not at all important	0.015	0.006

Not applicable	0.009	0.003
How important is the ability to independently organize time or prioritize tasks:		
Very important	0.456	0.024
Moderately important	0.388	0.024
Not very important	0.133	0.016
Not at all important	0.016	0.004
Not applicable	0.007	0.003
How important is the ability to critically evaluate different options:		
Very important	0.357	0.024
Moderately important	0.384	0.023
Not very important	0.205	0.019
Not at all important	0.043	0.010
Not applicable	0.012	0.004
How important is the ability to evaluate quality of output:		
Very important	0.710	0.022
Moderately important	0.248	0.021
Not very important	0.034	0.009
Not at all important	0.003	0.002
Not applicable	0.005	0.003
How important is the ability to take appropriate action if quality is not acceptable:		
Very important	0.763	0.021
Moderately important	0.214	0.020
Not very important	0.017	0.005
Not at all important	0.003	0.002
Not applicable	0.003	0.002

Source: PIE Manufacturing Survey. The means use establishment size weights that reflect the survey design. The second column presents the standard error of these weighted survey mean estimates.

Table B2. Descriptive Statistics for Key Variables

	Mean	SE
Long-term vacancy (indicator)	0.239	0.021
Long-term vacancy (percent)	0.037	0.006
Extended reading	0.526	0.025
Extended writing	0.221	0.021
Extended math	0.380	0.024
Extended computer	0.419	0.024
Ability to learn new skills	0.501	0.025

Ability to solve unfamiliar problems	0.388	0.024
Ability to work in teams	0.642	0.024
Ability to evaluate quality of output	0.710	0.022
Total establishment employment (100s)	0.721	0.020
More foreign competition	0.225	0.021
Pct. change in core workers (past 2yrs)	-0.165	0.050
Preference for internal hiring (ILM)	0.620	0.024
Pct. of core workers who participate in TQM	24.707	1.851
Union	0.065	0.009
Hours of formal training/year (core wkrs.)	14.283	1.500
Unique skill	0.259	0.022
Comm. college--no resources	0.242	0.021
Comm. college--no help	0.587	0.024
High tech	0.218	0.020
Above average technology	0.361	0.023
Frequent product innovation	0.591	0.025
Frequent process innovation	0.562	0.025
Industry cluster	0.439	0.025
Do you work with cluster on employment?	0.077	0.012
Are other institutions helpful on emp.?	0.253	0.022
County unemployment rate (BLS LAU 2011)	8.989	0.093
Wage differential (vs. QCEW 3Q 2012)	-10.338	0.570
Total county employment (QCEW 3Q 2012; 100s)	3776.464	305.788

Source: PIE Manufacturing Survey. The means use establishment size weights that reflect the survey design. The second column presents the standard error of these weighted survey mean estimates.

Essay 3: Variation in Manufacturing Skill Demands by Industry-Level Capital Intensity

Introduction

A puzzle exists regarding the level and evolution of skill demands, particularly in manufacturing. On the one hand, skill demands appear to have risen substantially over time. Today's worker is required to have higher levels of basic cognitive skills than the farmers or manual laborers of 100 years ago. Screening based on cognitive abilities is more rigorous, and the importance of physical strength has greatly diminished (Wilk and Cappelli 2003, Wyatt and Hecker 2006). Many researchers argue that technological upgrading increases the demand for skill, thus benefitting skilled workers and leading to a "race" between technology demands and the supply of skilled workers produced by the nation's educational system (Goldin and Katz 2008, Brynjolfsson and McAfee 2011).

On the other hand, when researchers take a detailed look at the relationship between technology and skill, the results are often surprising. Although a correlation exists between capital-intensive industries and higher skill demands, when researchers examine the longitudinal connection between capital intensity and skill demands, the relationship disappears or becomes quite limited (Dunne and Troske 2005; Doms, Dunne, and Troske 1997). Some analysts even find a reversal in skill demands as technology becomes more pervasive (Beaudry, Green, and Sand 2013).

There is a sense in which the debate may have gotten ahead of the phenomenon. Much of the debate relies on crude measures of skill such as educational attainment or the ratio of non-production to production workers. In this paper I seek to add to the literature by examining how skill demands in manufacturing industries vary by capital and equipment intensity. This study relies on data from a unique nationally representative survey that provides the most detailed measures of skill requirements at the plant level currently available. I merge this dataset with the

NBER-CES manufacturing database to incorporate the longitudinal evolution of capital stock at the industry level into the analysis. Because the main data on skill demands are cross-sectional, I do not attempt to resolve debates that depend on longitudinal skill data. Rather, I seek to establish what patterns are evident when skill demands are well measured and which economic theories these patterns are consistent with.

The results indicate that greater contemporaneous capital and equipment intensity is associated with greater demand for higher-level skills. However, contrary to the conventional narrative, I find that this effect is almost entirely driven by demands for higher-level reading as opposed to math or computer skills. In thc historical specifications, I find that the relationship between current skill demands and increases in capital intensity varies over time, with some periods showing negative effects. These findings imply that the relationship between capital and skill is not a simple one, either in content or direction.

Literature

A vast literature has developed on the topic of skills and technology over the past few decades. The general hypothesis, broadly referred to as skill-biased technical change (SBTC), has been that the growth of technology has raised demands for higher-level skills and the workers who possess them (Autor, Katz, and Krueger 1998, Machin and Van Reenen 1998). More recent research has emphasized the possibility that technology may be substituting for routine skills while relative demand increases for non-routine cognitive and manual skills, thus leading to a polarization in labor demand (Autor, Levy, and Murnane 2003; Acemoglu and Autor 2011). Most of this literature concludes that some type of SBTC or polarization process is occurring in which capital investment complements the demand for higher-level cognitive skills.

Despite these results, it is worth exploring whether the relationship between investment in technology and skill demands is in fact a smooth and clear-cut one. Because much of the variation in skill demands and skill proxies occurs within industry, it makes sense to investigate these claims for particular industry sectors and subsectors. In the manufacturing context, researchers who have analyzed skill demands have found a mixed picture. Using data from the Annual Survey of Manufactures (ASM), Berman, Bound, and Griliches (1994) find that increasing use of more skilled non-production labor within manufacturing industries is correlated with investment in computers and R&D expenditures, as well as with changes in equipment investment per worker. However, Doms, Dunne, and Troske (1997) arrive at somewhat different conclusions based on the detailed technology adoption data in the Survey of Manufacturing Technology (SMT). While the authors find a cross-sectional relationship between technology usage and the employment of more skilled workers, in a longitudinal analysis they "find little correlation between skill upgrading and the adoption of new technologies" (p.1). Dunne and Troske (2005) also do not find a relationship between the adoption of new technologies and increasing employer skill demands.

Measurement is an important issue in this debate. Almost all of the existing studies in the SBTC literature use one of three methods to measure skill demand: 1) inferring demand patterns based on observation of supply (proxied by education) and wage differentials, 2) educational attainment of the existing workforce, or 3) changes in occupational composition. While all of these methods provide valuable insights, they each have significant drawbacks that prevent definitive conclusions. The first method is problematic because it does not involve direct measurement of the quantities of interest. The second method does not measure within-educational skill differences, and it is confounded by under- or over-educated workers taking particular jobs. The third method is the strongest, but it captures one particular type of skill demand. If technology leads a firm to increase its non-routine/administrative personnel and reduce its routine/production personnel, then this method will (perhaps accurately) record this development as an increase in skill demands. However, if a firm upgrades the skills of its production personnel—say by requiring more computer programming skills—this methodology will not capture this effect. As will be discussed below, one of the main contributions of this paper is to employ a dataset that measures specific skills at a fine-grained level.

Another issue that surfaces in this debate over the relationship between technology and skill demands concerns whether the relationship varies over time. One possibility is that the impact of technology on skill demands depends on the technology investment cycle. Beaudry, Green, and Sand (2013) develop a model in which skill is used to build a stock of organizational capital, which then serves as a production input and reduces employer skill demands. This dynamic leads to a boom-bust cycle in skill requirements that runs contrary to the conventional view that (modern) technology monotonically leads to greater cognitive skill demands. The authors provide empirical evidence from the Current Population Survey (CPS) that demand for cognitive-skill occupations rose rapidly in the 1990s and subsequently fell following the year 2000.

Another possibility is that different types of technology have inherently different substitution or complementarity effects on skill demands. Some scholars have argued that in the 19th century, technology was skill replacing rather than skill enhancing (Acemoglu 2002; Atack, Bateman, and Margo 2004). By contrast, Katz and Margo (2013) argue that the same polarization patterns that some researchers have found in recent years appear in manufacturing in the late 19th and early 20th centuries. Gray (2011) likewise finds that electrification from 1880 to 1940 produced many skill-biased and polarized occupational patterns. I will explore some of these temporal capital-skill issues below.

Empirical Strategy

In this paper I estimate the relationship between fine-grained skill demands and industry-level capital intensity in the manufacturing sector. I am specifically interested in two questions: 1) how do cross sectional skill demands vary with capital intensity when skills are precisely measured, and 2) do historical changes in capital intensity by industry predict the current level of skill demands? The purpose of this latter question is to determine whether increases in capital intensity uniformly predict increasing skill levels, or whether the predictive relationship varies over time.

Specifically, for firm *i* in industry *j* at time *t* we estimate:

(1.1) $$S_{ijt} = \quad + CapInt_{j,t-k} \;{}_{1} + X_{ijt} \;{}_{2} + \;{}_{ijt}$$

where *S* is a measure of concrete skill demands (see discussion below), *CapInt* contains one or more measures of capital intensity, *X* is a vector of control variables, and *k* indexes the number of years between skill measurement and prior measures of capital intensity. I use two principal measures of capital intensity. For comparability with other studies, I employ the total real capital stock in an industry (defined at the six-digit NAICS level) divided by total employment in the industry. I also use real equipment capital stock divided by the total number of production workers in an industry. The purpose of including this latter measure is that equipment investment may be more closely related to skill demands, and production workers are the target employment category in our survey data.

While capital/technology and skill are clearly endogenous to one another, this reduced form approach remains informative for several reasons. First, it is valuable to know the size and significance of the correlations. Second, it is worth understanding how these correlations vary in the presence of detailed skill measures and over time. Establishing these results provides useful benchmarks for comparison against the (often endogenous) capital-skill results that exist in the literature. Finally, from a policy perspective, the prevailing narrative is that technology and employer investments in technology are outpacing worker skills, thus implying a causal belief. It is worth testing whether empirical correlations are consistent with this story.

To mitigate endogeneity, and to explore changing capital investment patterns over time, I use capital measures from prior time periods to predict skill demands from late 2012 and early 2013. In the contemporaneous specifications, I use capital intensity measures from 2009. In the historical specifications, I use the average level of industry capital intensity in 1989 as a baseline. I then test for the relationship between changes in capital intensity and current skill demands by employing a series of variables measuring the changes in capital intensity during the 1989-99 and 1999-2009 periods.

Data

This study makes use of two datasets: the MIT Production in the Innovation Economy (PIE) Manufacturing Survey and the National Bureau of Economic Research-Center for Economic Studies (NBER-CES) database compiling manufacturing industry data from the Annual Survey of Manufactures and the Census of Manufactures from 1958 to 2009. Industry data from the NBER-CES was matched to the PIE Manufacturing Survey at the six-digit North American Industry Classification System (NAICS) level.

The PIE survey data come from an original survey instrument that was mailed to 2,700 nationally representative manufacturing establishments with at least 10 employees, beginning in October 2012. The PIE survey focused on concrete skill questions, hiring and vacancy patterns, and establishment characteristics. The sample was randomly selected on a stratified basis from Dun & Bradstreet's database to reflect the frequency of different establishment sizes based on 2010 employment data from the Census Bureau's County Business Patterns survey. The target respondents were either plant managers or human resources staff with knowledge of operations. The respondents were asked to answer questions with regard to their core production workers, defined as the workers most central to the production process. In practice, respondents included almost all production workers in this definition.[28] Excluding the ineligible establishments, incorrect addresses, and the unusable surveys, the total response rate was 35.7 percent, yielding 903 completed surveys. Responses were submitted from October 2012 to January 2013. For more details on the survey methodology, as well as validation against existing national data, see Osterman and Weaver (2014) and Weaver and Osterman (2014).

The distinguishing feature of the PIE manufacturing survey was its focus on measurement of concrete skills. Rather than relying on opinion questions or abstract scales, the survey asked a battery of questions about specific skill demands. For example, does this job require multiplication and division? Reading basic instruction manuals? Using a computer to conduct web searches? For each skill category (math, reading, computer, etc.), the survey asked about both basic skills and higher-level skills that we refer to as "extended" skills. In math, requirements for the use of skills such as addition and subtraction were categorized as basic,

[28] Core production workers make up 62 percent of total establishment employment in the PIE Survey versus 70 percent for manufacturing production workers in the Bureau of Labor Statistics' Current Employment Survey (CES). Part of the difference is attributable to the fact that the CES data includes working supervisors in their definition of production worker.

while algebra, geometry, trigonometry, probability, statistics, calculus, and other advanced mathematics were categorized as extended math demands. In reading, requirements for reading complex technical documents/manuals, reading any document longer than five pages, and reading articles in trade journals were considered extended skill demands. For computers, the extended skills were computer-aided design/computer-aided manufacturing (CAD-CAM) software, other engineering or manufacturing software, and any requirement to write computer programs.

As discussed above, I employ the NBER-CES database to calculate capital intensity from 1989 to 2009. I utilize real capital stock per worker and real equipment per production worker as measures of capital intensity. I then match the NBER-CES capital intensity data to the PIE survey establishments at the six-digit NAICS level.

Descriptive Statistics

To provide a sense of the variation in the dataset, I have broken out some of the key variables by industry. Although the NBER-CES dataset is large enough to support very detailed reporting, the PIE survey dataset is not large enough to support representative statistics at the three-digit NAICS level. As a result, I have created five broader industry categories within the manufacturing sector: chemical and plastics (NAICS 325, 326); metal, fabrication, and machinery (NAICS 331, 332, 333), computer and electronics (NAICS 334, 335), transportation (NAICS 336), and all other three-digit NAICS manufacturing codes. Table 1 contains capital

Table 1. Capital Intensity by Industry, 2009 ($100,000s)

	Total Capital per Worker	Total Capital per Production Worker	Equipment per Worker	Equipment per Production Worker
Chemical & Plastics (325,326)	2.060	3.136	1.483	2.247
	(0.167)	(0.277)	(0.134)	(0.217)
Metal, Fabrication, & Machinery (331,332,333)	1.161	1.653	0.854	1.211
	(0.038)	(0.051)	(0.028)	(0.037)
Computer & Electrical (334,335)	1.774	3.492	1.246	2.451
	(0.206)	(0.387)	(0.150)	(0.281)
Transportation (336)	1.806	2.538	1.279	1.767
	(0.221)	(0.271)	(0.174)	(0.211)
All Other	1.966	2.768	1.381	1.950
	(0.171)	(0.268)	(0.126)	(0.197)
Observations	738			

The means in this table use establishment size weights for national representation. The standard errors of these weighted survey mean estimates are in parentheses. Three-digit NAICS codes are listed after industry categories.
Source: NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

statistics based on these categories. Capital intensity varies substantially by industry (Table 1). Capital includes equipment as well as physical plant. The chemical and plastics industry category had $206,000 in capital stock per worker in 2009 (measured in 1997 dollars), while primary metal, fabrication, and machine manufacturers had $116,000. The pattern is fairly similar when we analyze equipment per worker. The results vary, however, when we normalize total capital and equipment by production workers rather than the total workforce. The computer and electrical industry sector has the largest capital and equipment totals per production worker ($349,000 and $245,000, respectively). The metal industries have the lowest per production worker totals. Going forward I use two capital measures: total capital per worker and equipment per production worker. The first provides comparability with the literature, while the second is of interest because equipment may be more tightly tied to skill demands and production workers are the target employment category.

Table 2 contains data on the historical evolution of the two main capital intensity measures by industry. The first column presents the level of capital intensity in 1989. I have chosen 1989 as a baseline because it provides a good starting point from which to examine the changes during the 1990s and 2000s. The 1990s provide a window on increasing computer-related investments, while the 2000s provide a more recent period that is thought to embody patterns of skill-biased technical change. The chemical and plastics industries had the highest base level of capital intensity in 1989, with $113,000 in capital per worker (1997 dollars). At this early stage, the computer industry had the lowest average capital intensity, with $41,800 per worker. The next few columns present the change in real capital intensity over subsequent decades. From 1989-1999, capital intensity in chemical and plastics grew by $43,500 per worker, the fastest of our industry groupings. Transportation and computer and electrical manufacturers also increased capital intensity by more than the rest of the manufacturing sector. By contrast, traditional manufacturers in the metalworking and machinery sector saw an anemic increase during the 1990s of only $5,800 per worker.

In the 2000s, capital investment accelerated on a per worker basis. The patterns also shifted somewhat. The transportation and computer/electrical industries now show the biggest absolute changes, with per-worker increases of $110,500 and $78,700, respectively.

The figures for equipment investment per production worker show largely similar patterns, although this measure yields greater comparative increases for the computer and electrical manufacturing sector. In the 1990s, this sector increased equipment investment per production worker by $35,000, second only to the chemical and plastics sector. By the 2000s, the computer and electrical sector emerged as the source of the greatest absolute increase, with equipment intensity rising by $122,000. Overall, these data provide a picture in which capital intensity has increased substantially over time, with particularly large absolute increases in the 2000s. Over time, the chemical and plastics sector has increased intensity substantially from a large capital base, while intensity in the computer and electronics sector has risen dramatically from a small base, thus demonstrating the greatest percentage growth.

Table 2. Changes in Capital and Equipment Intensity by Industry ($100,000s)

	CI/wkr. 1989	ΔCI/wkr. 1989-99	ΔCI/wkr. 1999-2009	EI/pwkr. 1989	ΔEI/pwkr. 1989-99	ΔEI/pwkr. 1999-2009
Chemical & Plastics (325,326)	1.132	0.435	0.642	1.064	0.647	0.726
	(0.166)	(0.061)	(0.097)	(0.174)	(0.114)	(0.085)
Metal, Fabrication, & Machinery (331,332,333)	0.528	0.0578	0.435	0.457	0.0981	0.523
	(0.017)	(0.009)	(0.017)	(0.014)	(0.009)	(0.019)
Computer & Electrical (334,335)	0.418	0.193	0.787	0.434	0.352	1.220
	(0.030)	(0.017)	(0.187)	(0.034)	(0.037)	(0.269)
Transportation (336)	0.821	0.215	1.105	0.672	0.266	1.142
	(0.155)	(0.064)	(0.295)	(0.131)	(0.069)	(0.311)
All Other	0.684	0.156	0.533	0.569	0.204	0.570
	(0.052)	(0.017)	(0.038)	(0.051)	(0.020)	(0.035)
PIE Survey Observations	717					

Standard errors in parentheses. Three-digit NAICS codes are listed after industry categories. CI/wkr.=total capital stock divided by total employment. EI/pwkr.=equipment stock divided by total production workers. See text for further explanation. Source: NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data, matched to PIE Manufacturing Survey establishments.

Table 3 presents the industry variation in skill demands based on the PIE survey. The first column contains the percentage of establishments in each industry category that demand any extended skill (extended math, reading, writing, or computer skills). In 2012-13, the computer and electrical industries had the highest extended skill demands, with 85 percent of establishments requiring one or more of these skills. The chemical/plastics and the other

Table 3. Higher Level Skill Demands by Industry

	Any Extended Skill	Extended Math	Extended Reading	Extended Computer	Ability to Learn New Skills	Ability to Solve Unfamiliar Problems
Chemical & Plastics (325,326)	0.706	0.257	0.606	0.339	0.505	0.303
	(0.044)	(0.042)	(0.047)	(0.046)	(0.048)	(0.044)
Metal, Fabrication, & Machinery (331,332,333)	0.774	0.493	0.522	0.507	0.489	0.321
	(0.025)	(0.030)	(0.030)	(0.030)	(0.030)	(0.028)
Computer & Electrical (334,335)	0.849	0.321	0.755	0.557	0.519	0.302
	(0.035)	(0.046)	(0.042)	(0.048)	(0.049)	(0.045)
Transportation (336)	0.739	0.232	0.594	0.377	0.536	0.420
	(0.053)	(0.051)	(0.060)	(0.059)	(0.060)	(0.060)
All Other	0.676	0.172	0.466	0.356	0.466	0.314
	(0.027)	(0.021)	(0.028)	(0.027)	(0.028)	(0.026)
Observations	867					

Standard errors in parentheses. Three-digit NAICS codes are listed after industry categories.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

industries had the lowest extended skill demands (71 percent and 68 percent, respectively). When we examine individual extended skills, we see that these skill demands do not always move together. Only 26 percent of chemical/plastics manufacturing establishments demand extended math skills, while 61 percent of these plants demand extended reading. In addition, different industries have different relative skill rankings. Computer and electrical manufacturers unsurprisingly show the highest level of extended computer demands (56 percent), but these factories have lower extended math demands than metal/fabrication/machine manufacturers (32 vs. 49 percent). In addition to the skill types above, we also included data on demands for some of the soft/critical-thinking skills that are often thought to be essential to modern high-tech manufacturing production systems. The last two columns of Table 3 show the demand for these skills by industry. Compared to the extended skill types, these skills show somewhat less variation. Roughly half of manufacturing establishments require their core production workers to have the ability to learn new skills, while for most manufacturing sectors just under a third require the ability to solve unfamiliar problems. Transportation shows the highest level of demand for both these skill categories. Having established these basic patterns, we can now explore whether the variation in capital intensity among manufacturers predicts the level of skill demands in the PIE survey.

Results

Contemporary Specifications

In the first specifications, I investigate whether a significant relationship exists between detailed skill demands in 2012-13 and capital intensity in 2009. All specifications control for employment size (via an indicator for more than 100 total employees) and for whether the establishment is part of a larger firm. In order to compare these results with the literature, I have calculated two additional measures of skill demands. The first is the percentage of core production workers who have an educational attainment level of "some college" or more. The second is the percentage of establishment employees who are not core production workers. This variable primarily measures the percentage of administrative and managerial staff. I estimate these two continuous measures by linear regression. For the detailed skill specifications, the dependent variables are binary indicators for establishment-level demand for particular skills. I estimate these specifications via probit models, with marginal effects presented in the tables. All specifications cluster standard errors at the three-digit NAICS code level.

Table 4 contains the results for specifications with total capital stock per worker as the key explanatory variable. We can see that higher levels of capital stock are associated with significantly greater extended skill demands for specifications with the percent college measure, but significantly lower demands for those with the percent non-production dependent variable.

Column one indicates that an additional $100,000 in capital stock per worker in 2009 is associated with a significant 1.3 percentage point increase in the proportion of a plant's core production workforce that has at least some college education in 2012-2013. By contrast, the same increase in capital investment predicts a nearly one percent decrease in the proportion of a manufacturing establishment's non-production workforce (significant at the 99 percent level). For both of these dependent variables, we have presented specifications with and without industry fixed effects (based on the broad industry categories). The coefficients are largely unchanged. Thus the conventional skill proxies tell a mixed story.

Turning to the detailed measures, an additional $100,000 in capital per worker is associated with a 2.3 percentage point increase in the probability of demanding at least one extended skill. Including industry fixed effects raises this result to a 2.8 percentage point increase (significant at the 95 level). The computer/electrical, metal/fabrication/machine, and transportation industries are all significantly more likely than other manufacturing industries to demand at least one higher-level skill, with computer/electrical plants being 14 percentage points more likely to do so. However, when we examine skill demands at a finer level, the results are surprising. Although the conventional story about technological impact is that it increases demand for technical skills involving math and computers, greater levels of capital stock per worker do not predict significantly greater demand for either of these skills. Instead, the any-extended-skill results are driven entirely by extended reading demands. A $100,000 increase in capital stock per worker is associated with a 4.7 percentage point increase in the likelihood that a manufacturing plant will require higher-level reading skills (significant at the 99 percent level). All of the detailed skill results include industry fixed effects.

Table 4. Detailed Skill Demands and 2009 Capital Intensity

	Pct. Some College or More	Pct. Some College or More	Pct. Non-Prod. Wkrs.	Pct. Non-Prod. Wkrs.	Any Ext. Skill	Any Ext. Skill	Ext. Math	Ext. Reading	Ext. Computer	Ability to Learn New Skills	Ability to Solve Unfamiliar Problems
Capital stock/wkr. (2009)	0.013**	0.013**	-0.008***	-0.008***	0.023*	0.027**	0.009	0.047***	0.013	0.001	-0.007
	(0.005)	(0.005)	(0.003)	(0.003)	(0.012)	(0.013)	(0.010)	(0.017)	(0.009)	(0.008)	(0.007)
Total emp. > 100	-0.026	-0.025	0.017	0.024	0.015	0.018	-0.059**	-0.002	0.039	-0.034	-0.068
	(0.016)	(0.016)	(0.030)	(0.029)	(0.032)	(0.032)	(0.026)	(0.039)	(0.038)	(0.031)	(0.045)
Part of Larger Firm	-0.031	-0.032	-0.009	-0.016	0.025	0.029	-0.064	0.066*	-0.013	-0.002	-0.031
	(0.019)	(0.019)	(0.033)	(0.032)	(0.050)	(0.047)	(0.043)	(0.040)	(0.061)	(0.029)	(0.035)
Chemical & Plastics		-0.015		0.068		-0.011	0.111***	0.068	-0.027	-0.013	-0.036
		(0.019)		(0.048)		(0.093)	(0.042)	(0.110)	(0.081)	(0.036)	(0.037)
Computer & Electrical		0.020		0.109***		0.141***	0.177***	0.247***	0.190***	0.032	-0.011
		(0.031)		(0.026)		(0.028)	(0.043)	(0.049)	(0.037)	(0.066)	(0.025)
Metal, Fab., and Mach.		-0.006		0.021		0.138***	0.381***	0.083*	0.182***	0.044	0.015
		(0.019)		(0.028)		(0.042)	(0.073)	(0.049)	(0.057)	(0.037)	(0.030)
Transportation		-0.047***		-0.066***		0.123***	0.037	0.194***	0.078*	0.081***	0.152***
		(0.016)		(0.022)		(0.027)	(0.038)	(0.042)	(0.040)	(0.025)	(0.027)
N	674	674	729	729	735	735	728	727	723	730	730
Adj. R-sq./Pseudo R-sq.	0.023	0.022	-0.002	0.006	0.009	0.039	0.105	0.049	0.027	0.003	0.014

Standard errors in parentheses. Specifications with Pct. Some College and Pct. Non-Production as dependent variables are estimated by linear regression. Specifications with indicators for other skill demands are estimated by probit models. The figures in the probit models are marginal effects. Std. Errors are clustered at the three-digit NAICS level.
* p<0.10, ** p<0.05, *** p<0.01.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

Table 5. Detailed Skill Demands and 2009 Equipment Intensity per Production Worker

	Pct. Some College or More	Pct. Some College or More	Pct. Non-Prod. Wkrs.	Pct. Non-Prod. Wkrs.	Any Ext. Skill	Any Ext. Skill	Ext. Math	Ext. Reading	Ext. Computer	Ability to Learn New Skills	Ability to Solve Unfamiliar Problems
Equipment stock/prod. wkr. (2009)	0.011**	0.011**	-0.005	-0.006***	0.029**	0.030**	0.008	0.050***	0.015**	0.000	-0.007
	(0.004)	(0.004)	(0.003)	(0.002)	(0.011)	(0.012)	(0.007)	(0.016)	(0.007)	(0.005)	(0.005)
Total emp. > 100	-0.025	-0.024	0.015	0.023	0.018	0.022	-0.059**	0.003	0.039	-0.034	-0.069
	(0.016)	(0.016)	(0.030)	(0.029)	(0.032)	(0.031)	(0.025)	(0.038)	(0.039)	(0.031)	(0.045)
Part of Larger Firm	-0.032	-0.032	-0.010	-0.017	0.019	0.026	-0.065	0.061	-0.016	-0.002	-0.031
	(0.019)	(0.019)	(0.033)	(0.032)	(0.049)	(0.046)	(0.043)	(0.039)	(0.060)	(0.030)	(0.035)
Chemical & Plastics		-0.017		0.069		-0.018	0.109**	0.057	-0.030	-0.013	-0.035
		(0.019)		(0.049)		(0.087)	(0.043)	(0.100)	(0.075)	(0.036)	(0.037)
Computer & Electrical		0.012		0.114***		0.127***	0.171***	0.221***	0.181***	0.031	-0.007
		(0.029)		(0.025)		(0.026)	(0.045)	(0.045)	(0.035)	(0.067)	(0.026)
Metal, Fab., and Mach.		-0.008		0.023		0.135***	0.380***	0.078*	0.183***	0.043	0.016
		(0.019)		(0.028)		(0.041)	(0.072)	(0.046)	(0.056)	(0.037)	(0.030)
Transportation		-0.047***		-0.066***		0.121***	0.038	0.191***	0.079**	0.080***	0.152***
		(0.016)		(0.023)		(0.026)	(0.038)	(0.041)	(0.039)	(0.025)	(0.027)
N	674	674	729	729	735	735	728	727	723	730	730
Adj. R-sq/Pseudo R-sq.	0.025	0.024	-0.003	0.005	0.015	0.043	0.105	0.054	0.029	0.003	0.014

Standard errors in parentheses. Specifications with Pct. Some College and Pct. Non-Production as dependent variables are estimated by linear regression. Specifications with indicators for other skill demands are estimated by probit models. The figures in the probit models are marginal effects. Std. Errors are clustered at the three-digit NAICS level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

Table 5 presents the same specifications but with equipment intensity per production worker in 2009 as the measure of capital intensity. We see a similar pattern of results, with increasing equipment stock per worker predicting an increased percentage of production workers with some college, but a small decrease in the non-core-production workforce percentage. With regard to detailed skills, an additional $100,000 in equipment per worker is associated with a three percentage-point increase in the probability of demanding any extended skill. The one difference in the equipment-intensity specifications is that the marginal effect of increased equipment investment per production worker is now associated with a significant increase in the percentage of establishments demanding advanced computer skills, although the magnitude of the effect is much smaller than the advanced reading effect. A $100,000 increase in equipment investment per production worker is associated with a 1.5 percentage point increase in a plant's probability of requiring extended computer skills. In results not shown, I investigated whether this difference in the significance of computer demands relates to the difference between total capital and equipment, or to the normalization by the total workforce versus production workers. The normalization by the production workforce was the decisive factor. One possible interpretation is that higher overall capital intensity is associated with production systems that require higher-level reading skills, while investments that differentially increase capital intensity for the production workforce are tied to both extended reading and computer skills. The effects of capital intensity on extended math and the learning/problem-solving variables remain small and insignificant.

Historical Specifications

I next investigate whether historical increases in capital intensity uniformly predict greater skill demands. Although it would obviously be ideal to have a panel dataset with detailed skills measured on an annual basis, such data do not exist. We can nevertheless learn something from whether the correlations between historical capital investment and current skill demands have consistent signs and magnitudes. Are greater amounts of capital investment per worker associated with higher skill demands in some cumulative fashion?

Table 6 contains the results of specifications in which I regressed skill demand variables on the level of capital intensity in a base period (1989), changes in capital intensity over the 1990s and 2000s, and the same set of controls as the contemporaneous specifications. The specifications that contain percent college and percent non-production worker as dependent variables utilize linear regression, while the specifications that utilize binary indicators for any extended skill demand are estimated by probit models. I present results with and without industry fixed effects, as both specifications are potentially informative. One the one hand, industry fixed effects help control for differential investment patterns due to globalization or other non-capital factors. On the other hand, capital investment is likely one of the factors that distinguishes industries, and stripping away this variation is not always desirable.

The results show that higher levels of capital investment per worker at the six-digit NAICS level in 1989 are associated with establishments that have lower percentages of non-core-production workers in 2012-13. By contrast, the relationships between these base-year capital intensity levels and the other skill demand measures—percent of production workforce with at least some college and the demand for any extended skill—are insignificantly positive. Net of these initial differences, the first thing to observe is that the sign and magnitude of the capital intensity coefficients change over the decades in non-monotonic patterns that are not consistent with either a simple positive relationship or attenuation over time.

Table 6. Skill Demands and Historical Changes in Capital Intensity

	Pct. Some College or More	Pct. Some College or More	Pct. Non-Prod. Wkrs.	Pct. Non-Prod. Wkrs.	Any Ext. Skill	Any Ext. Skill
Capital intensity 1989	0.034	0.041*	-0.063***	-0.051***	0.002	0.017
	(0.023)	(0.023)	(0.017)	(0.018)	(0.037)	(0.038)
Chg. in cap. int. 1989-99	-0.020	-0.030	0.147***	0.129***	0.169**	0.203**
	(0.044)	(0.047)	(0.029)	(0.039)	(0.082)	(0.093)
Chg. in cap. int. 1999-2009	0.006	0.002	-0.031***	-0.034***	-0.029	-0.047*
	(0.014)	(0.012)	(0.010)	(0.009)	(0.025)	(0.027)
Total emp. > 100	-0.026	-0.024	0.017	0.023	0.014	0.016
	(0.015)	(0.015)	(0.031)	(0.030)	(0.032)	(0.031)
Part of larger firm	-0.029	-0.031	-0.016	-0.020	0.017	0.021
	(0.020)	(0.020)	(0.033)	(0.033)	(0.048)	(0.045)
Chemical & plastics		-0.010		0.053		-0.036
		(0.019)		(0.043)		(0.078)
Computer & electrical		0.041		0.087**		0.139***
		(0.038)		(0.034)		(0.029)
Metal, fab., and mach.		-0.005		0.031		0.149***
		(0.022)		(0.025)		(0.038)
Transportation		-0.040**		-0.069***		0.134***
		(0.017)		(0.022)		(0.024)
N	672	672	726	726	732	732
adj. R-sq	0.025	0.028	0.009	0.012		
pseudo R-sq					0.018	0.054

Standard errors in parentheses. Specifications with Pct. Some College and Pct. Non-Production as dependent variables are estimated by linear regression. Specifications with indicators for other skill demands are estimated by probit models. Figures for probit models are marginal effects. Std. errors are clustered at the three-digit NAICS level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

The 1990s (1989-1999) show a particularly strong positive relationship, with a $100,000 increase in capital stock per worker over the time period implying a 16.9 percentage point increase in the probability of an establishment demanding any extended skill in 2012-13. It is worth noting that the percentage non-core-production worker specifications show a similarly strong positive correlation in the 1990s, while the education specifications do not. However, the relationship between capital investment and skills reverses in the 2000s. From 1999-2009, increases in capital intensity are associated with both a lower percentage of non-core-production workers and a lower probability of demanding any extended skill, although this latter effect is only significant at the 90 percent level (for the specification with industry fixed effects in column six). This reversal in the nature of the relationship between capital and skill is potentially consistent with the model and results in Beaudry, Green, and Sand (2013).

Table 7 repeats this analysis with equipment capital per production worker as the measure of capital intensity. The overall patterns are similar, with the positive equipment-skill relationship in the 1990s again showing up strongly. A $100,000 increase in capital investment from 1989 to 1999 implies an 18 to 20 percentage-point increase in the likelihood of demanding any extended skill in 2012-13. The sign of the relationship between equipment intensity and extended skill demands again turns negative for investment increases in the 2000s, although these effects are not significant. The percentage production worker specifications show strong positive equipment intensity effects in the 1990s and strong negative effects in the 2000s. The percent college specifications show small and insignificant effects for both periods.

Table 7. Skill Demands and Historical Changes in Equipment Intensity (per Production Worker)

	Pct. Some College or More	Pct. Some College or More	Pct. Non-Prod. Wkrs.	Pct. Non-Prod. Wkrs.	Any Ext. Skill	Any Ext. Skill
Equipment intensity 1989	0.048*	0.055**	-0.076***	-0.061**	-0.009	-0.002
	(0.026)	(0.026)	(0.023)	(0.022)	(0.036)	(0.037)
Chg. in equip. intensity 1989-99	-0.022	-0.025	0.106***	0.095***	0.151**	0.189**
	(0.024)	(0.026)	(0.025)	(0.028)	(0.067)	(0.075)
Chg. in equip. intensity 1999-2009	0.007	0.002	-0.029**	-0.036***	-0.010	-0.035
	(0.008)	(0.006)	(0.013)	(0.011)	(0.022)	(0.024)
Total emp. > 100	-0.024	-0.023	0.015	0.022	0.017	0.020
	(0.015)	(0.016)	(0.031)	(0.030)	(0.032)	(0.031)
Part of larger firm	-0.032	-0.035	-0.012	-0.018	0.013	0.019
	(0.020)	(0.020)	(0.033)	(0.033)	(0.049)	(0.045)
Chemical & plastics		-0.015		0.055		-0.045
		(0.019)		(0.043)		(0.077)
Computer & electrical		0.037		0.105***		0.129***
		(0.036)		(0.032)		(0.024)
Metal, fab., and mach.		-0.005		0.032		0.148***
		(0.021)		(0.024)		(0.034)
Transportation		-0.040**		-0.068***		0.131***
		(0.017)		(0.021)		(0.023)
N	672	672	726	726	732	732
adj. R-sq	0.032	0.035	0.007	0.012		
pseudo R-sq					0.022	0.057

Standard errors in parentheses. Specifications with Pct. Some College and Pct. Non-Production as dependent variables are estimated by linear regression. Specifications with indicators for other skill demands are estimated by probit models. Figures for probit models are marginal effects. Std. errors are clustered at the three-digit NAICS level.
* $p<0.10$, ** $p<0.05$, *** $p<0.01$.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

Tables 8 and 9 extend this analysis to specifications that employ measures of detailed skills as dependent variables (all specifications are probit models that include industry fixed effects). In Table 8 we can see that a higher base level of capital intensity in 1989 is associated with a significantly lower probability of an establishment demanding extended computer skills in 2012-13. This result may reflect the effect of mature industries. In the 1990s, increases in capital intensity show strong relationships with demands for extended math, reading, and computer skills. A $100,000 increase in capital intensity from 1989-

1999 is associated with a 14 percentage-point increase in extended math demands and a 33 percentage-point increase in extended computer demands (significant at the 95 and 99 percent levels, respectively). By contrast, the same increase in capital intensity in the 2000s predicts a five percentage-point decrease in math demands and a six percentage-point decrease in computer demands, although only the math result is significant. The reading specification shows a 3.6 percentage-point decrease in response to a $100,000 increase in capital intensity in the 2000s (significant at the 90 percent level). Historical changes in capital intensity appear to be unconnected to employer requirements for the ability to learn new skills or solve unfamiliar problems.

Table 9 utilizes the same methodology with equipment per production worker as the measure of capital intensity. The signs and magnitudes of the equipment intensity marginal effects are similar to the capital intensity results, although a number of effects lose significance. The significant and strong positive association between higher-level skill demands and 1990s investment patterns continues, as does the (insignificantly) negative relationship with investment in the 2000s.

Table 8. Detailed Skill Demands and Historical Changes in Capital Intensity

	Any Extended Skill	Extended Math	Extended Reading	Extended Computer	Ability to Learn New Skills	Ability to Solve Unfamiliar Problems
Capital intensity 1989	0.017	-0.012	0.015	-0.085***	-0.045	0.020
	(0.038)	(0.028)	(0.041)	(0.027)	(0.030)	(0.040)
Chg. in cap. int. 1989-99	0.203**	0.138**	0.290***	0.331***	0.107	-0.012
	(0.093)	(0.067)	(0.074)	(0.075)	(0.070)	(0.075)
Chg. in cap. int. 1999-2009	-0.047*	-0.046**	-0.036*	-0.056	-0.002	-0.042
	(0.027)	(0.020)	(0.020)	(0.041)	(0.038)	(0.028)
Total emp. > 100	0.016	-0.058**	0.002	0.039	-0.037	-0.074
	(0.031)	(0.026)	(0.040)	(0.041)	(0.030)	(0.047)
Part of larger firm	0.021	-0.072*	0.053	-0.017	0.001	-0.031
	(0.045)	(0.042)	(0.037)	(0.062)	(0.028)	(0.035)
Chemical & plastics	-0.036	0.090*	0.039	-0.057	-0.020	-0.040
	(0.078)	(0.053)	(0.090)	(0.060)	(0.038)	(0.046)
Computer & electrical	0.139***	0.171***	0.238***	0.146***	0.006	0.010
	(0.029)	(0.048)	(0.053)	(0.041)	(0.070)	(0.030)
Metal, fab., and mach.	0.149***	0.389***	0.102**	0.210***	0.054	0.015
	(0.038)	(0.069)	(0.041)	(0.051)	(0.036)	(0.031)
Transportation	0.134***	0.037	0.205***	0.086**	0.079***	0.160***
	(0.024)	(0.038)	(0.038)	(0.039)	(0.027)	(0.028)
N	732	725	725	720	727	727
pseudo R-sq	0.054	0.110	0.063	0.049	0.006	0.016

Standard errors in parentheses. Specifications are estimated by probit models, and figures are marginal effects. Std. errors are clustered at the three-digit NAICS level. * p<0.10, ** p<0.05, *** p<0.01.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

Table 9. Detailed Skill Demands and Historical Changes in Equipment Intensity (per Production Worker)

	Any Extended Skill	Extended Math	Extended Reading	Extended Computer	Ability to Learn New Skills	Ability to Solve Unfamiliar Problems
Equipment intensity 1989	-0.002	-0.029	0.002	-0.123***	-0.051	0.003
	(0.037)	(0.029)	(0.047)	(0.043)	(0.046)	(0.055)
Chg. in equip. int. 1989-99	0.189**	0.086	0.251***	0.248***	0.090*	0.029
	(0.075)	(0.052)	(0.073)	(0.055)	(0.051)	(0.053)
Chg. in equip. int. 1999-2009	-0.035	-0.019	-0.023	-0.044	-0.023	-0.039*
	(0.024)	(0.020)	(0.022)	(0.040)	(0.021)	(0.023)
Total emp. > 100	0.020	-0.058**	0.008	0.040	-0.038	-0.074
	(0.031)	(0.026)	(0.039)	(0.042)	(0.030)	(0.048)
Part of larger firm	0.019	-0.069	0.049	-0.012	0.002	-0.034
	(0.045)	(0.044)	(0.037)	(0.064)	(0.029)	(0.036)
Chemical & plastics	-0.045	0.096**	0.025	-0.058	-0.022	-0.046
	(0.077)	(0.042)	(0.086)	(0.059)	(0.042)	(0.048)
Computer & electrical	0.129***	0.166***	0.218***	0.152***	0.024	0.010
	(0.024)	(0.047)	(0.050)	(0.049)	(0.070)	(0.026)
Metal, fab., and mach.	0.148***	0.386***	0.097**	0.207***	0.054	0.020
	(0.034)	(0.068)	(0.038)	(0.047)	(0.035)	(0.030)
Transportation	0.131***	0.035	0.200***	0.085**	0.082***	0.156***
	(0.023)	(0.036)	(0.038)	(0.037)	(0.026)	(0.027)
N	732	725	725	720	727	727
pseudo R-sq	0.057	0.108	0.068	0.050	0.006	0.017

Standard errors in parentheses. Specifications are estimated by probit models, and figures are marginal effects. Std. Errors are clustered at the three-digit NAICS level. * p<0.10, ** p<0.05, *** p<0.01.
Source: PIE Manufacturing Survey and NBER-CES database, based on Annual Survey of Manufactures and Census of Manufactures data.

Conclusion

A pervasive belief exists that increases in technology and corporate investment in technology increase the level of skill demands. Although skill demands are rising over time, the exact nature of the relationship between skill requirements and capital investment in technology remains unclear. Part of the reason for the confusion may lie with the fact that researchers have often had to rely on very indirect proxies for skill requirements. In this paper I seek to add to the literature by linking precisely measured manufacturing skill demands for production workers with contemporary and historical capital intensity data. The findings show that higher levels of contemporaneous capital and equipment intensity are associated with significantly greater demands for higher-level skills. However, these results are entirely driven by greater demands for higher-level reading skills, not by demands for the math and computer skills that are commonly cited in the popular debate. In the historical specifications, I find that the relationship between changes in industry capital intensity and skill demands is not constant over time. Capital intensity increases in the 1990s predict greater skill demands in 2012-13, but similar increases in the 2000s are associated with either lower skill demands or no significant difference in demands.

To benchmark these results against the literature, I also included conventional skill proxies in the analysis: percentage of core production workforce with some college or more and percentage of non-production workers. In general, there was more consistency between the detailed skill measures and the educational attainment measure in the contemporaneous specifications. In the historical specifications, the percentage of non-production workers showed more consistency with the detailed skill demands. It is worth noting that these two proxy measures frequently showed opposite signs. They do not measure the same type of skill dynamic, and they are only consistent with detailed measures of skill demands in some specifications.

I also tested for the relationship between capital intensity and the types of critical-thinking/problem-solving skills that are often thought to be essential for modern advanced manufacturing production systems. In these data, capital and equipment intensity show no relation to demand for the ability to learn new skills or the ability to solve unfamiliar problems.

These findings imply that the relationship between technology investments and skill demands is not a simple one, either with regard to the content or direction of skill demands. They raise the possibility that human capital investments in higher-level skills will not pay off equally in all time periods, and that technological investment has heterogeneous—and time-varying—effects on the demand for skill.

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