PRIVATE EQUITY, TECHNOLOGICAL INVESTMENT, AND LABOR OUTCOMES [*] [†]

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Abstract

This paper uses proprietary data on the employment histories of a large fraction of the U.S. labor force to assess how private equity acquisitions impact the long-run labor market outcomes of workers. In contrast to commonly held views, we find that employees working at a target firm at the time of an acquisition subsequently realize longer employment durations over their careers relative to comparable workers at non-acquired firms. We provide evidence that the mechanism for these outcomes is employees' acquisition of human capital facilitated by firm investment in information technologies (IT) and complementary work practices. The effects are especially pronounced for workers who perform tasks that are central to IT-enabled production processes, for workers who are able to quickly acquire new skills, and for workers who remain at an acquired firm for longer durations before exit. We also find that employees of acquired firms are more likely to eventually transition to companies that have demand for IT-related human capital. The findings suggest that the recent wave of private equity acquisitions has imparted workers with transferable skills that complement modern technological advances.

JEL Codes: G30, G31, J24, M51, M54.

INTRODUCTION

Private equity (PE) transactions represent dramatic shifts in corporate operations that are typically associated with gains for investors (Kaplan 1989a,b; Lichtenberg and Siegel 1990; Kaplan and Schoar 2005; Acharya et al. 2013). There is divisive debate in both the academic and public domain, however, about whether these gains are realized at the expense of workers.[1] Many people believe that PE transactions impose large costs on workers because employees may find it difficult to respond to disruptions in their career paths. For instance, one oft-cited cost that workers may experience is the loss of firm-specific human capital. Some scholars suggest that PE transactions represent breaches of implicit contracts previously written between employees and managers in order to incentivize employee investment in firm-specific skill development (Shleifer and Summers 1988). As a result of a PE buyout, employees who invested in firm-specific capital acquisition might suddenly face a labor market in which their skills are of limited value, diminishing their long-run employment prospects.

There is no empirical evidence, to date, that illustrates the effects of PE transactions on individual workers' long-run labor market outcomes. Identifying the impact of private equity on workers is difficult for two reasons. The first is data limitations: there is no comprehensive dataset that contains detailed information on both firm characteristics (such as ownership structure) and worker characteristics (such as long-run labor outcomes).[2] A second problem is that even if such data were available, there is a challenging identification problem: it is difficult to estimate the causal impact of PE on worker outcomes and distinguish this effect from other important factors, such as the endogenous sorting of workers with different abilities into heterogeneous firms.

To overcome these difficulties, we use proprietary data from one of the largest online job search companies in the United States to construct a novel employer-employee matched panel dataset that contains detailed information on the career paths of U.S. workers, with in-depth

1.This debate reached a crescendo during the 2012 Unites States presidential campaign, as many observers across the political spectrum fiercely criticized the impact of PE companies, such as Mitt Romney's Bain Capital, on workers (Creswell 2012; Parker 2012; Davis et al. 2011).

2. Datasets such as Compustat contain detailed information about firm financial characteristics but lack information about the workers employed by these companies. Conversely, datasets such as the Current Population Survey and the Census contain information on workers, without corresponding information about employers.

information on their employers over time. The data, based primarily on workers' employment histories as reported on their resumes, allow us to track the long-run labor outcomes of workers who are employed by specific firms during the sample period, including firms that are acquired by private equity investors. We examine how workers fare following leveraged buyouts (LBO's) and compare their labor market outcomes with those of similar workers at comparable firms that do not get acquired.

Our principal finding is that employees of firms acquired in LBO's subsequently realize *longer* employment durations than similarly matched workers at comparable firms. In contrast to the commonly held view that PE buyouts adversely affect workers' long-run employability, the data indicate that long-run employment durations for workers at acquired firms increase by 5 to 9%. While employees may in fact incur large costs as a result of a PE transaction, the data suggest that there are other economic forces that impact their long-run labor outcomes.

To explain these findings, we propose and find empirical support for the hypothesis that over the last decade, private equity activity has impacted workers' long-run career paths by imparting them with transferable skills that complement modern technological advances. When PE investors acquire firms in leveraged buyouts, they upgrade target operations by investing in new information technologies and complementary work practices – implementing arguably the most salient technological developments of the last 30 years (Bresnahan et al. 2002). Employees who work within the resulting new production systems acquire human capital through training and on-the-job learning. These new skills enable workers to perform certain tasks with greater efficiency, and are valuable because they provide benefits to workers *even after* they leave their employers.

Our explanation for the findings builds on a large body of evidence—both case study and large sample—that the most recent wave of leveraged buyouts (LBO's) is characterized by technological upgrades at target companies (Kaplan and Strömberg 2009; Amess et al. 2007; Bloom et al. 2009; Bacon et al. 2004, 2012). These improvements typically reflect investment in information technologies (IT) and complementary investments in employee training and work reorganization that facilitate employee skill acquisition (Bresnahan et al. 2002; Bartel et al. 1998). We find evidence consistent with this literature by showing that firms in our sample hire

significantly more IT labor—an informative measure of IT investment developed in recent work—following an LBO. Previous studies have shown that when firms upgrade their production processes and install modern work practices, they employ significantly more IT labor (Tambe, Hitt, and Brynjolfsson 2012). Our labor-based measures of IT investment are useful partly because labor commands a larger fraction of the IT budget than IT capital stock (Saunders 2010); these data are also more representative of IT investment patterns after 2000 than other datasets used in previous studies (Tambe and Hitt 2012).

We then document several additional empirical findings that further support our hypothesis. We show that the longer employment durations realized by workers of acquired firms are especially pronounced in firms that invest most heavily in modern technologies following an acquisition. Moreover, within acquired firms, the observed effects on employment are driven by employees who perform tasks that are complementary to IT-enabled production methods. For example, the effects are largest for workers in occupations in which tasks such as problem solving, decision making, and analyzing data play an important role (Autor et al. 2003; Bartel et al. 2007). The effects are also more pronounced for employees with a college education and for employees who can be trained quickly, precisely the types of workers that prior studies have found to be complementary to IT investments (Bresnahan et al. 2002). We also observe that the length of job tenure at the acquired firm matters for subsequent labor market outcomes: employees who remain at an acquired firm for over 1.3 years, and hence are more likely to acquire skills through training and learning-by-doing, have longer subsequent job tenures than employees who leave the firm soon after an acquisition (who, implicitly, are not employed for a long enough time after an LBO to acquire any new human capital).

We also present evidence that illustrates the evolution of worker career paths following an LBO. Following a PE acquisition, workers who exit an acquired firm experience shorter unemployment durations than comparable workers at similar firms; the data suggest that these workers are able to more quickly find new jobs. They are also more likely to transition to firms that have higher demand for the skills required to perform IT-intensive production tasks. Collectively, the findings are consistent with the view that PE investment imparts workers with transferable human capital that complements modern technologies.

We evaluate several competing explanations for the observed link between PE ownership and employee labor outcomes. We show that the most salient explanation for our empirical findings is that workers acquire new skills following an LBO. For example, we find that the long-run labor outcomes of workers at LBO targets do not appear to be driven by innate differences in the (unobserved) abilities of workers who sort into firms that are eventually acquired versus firms that are not acquired. We examine the career paths of workers employed at LBO targets in the years *prior* to an acquisition and find no discernible differences in their labor outcomes relative to similarly matched workers at comparable firms. These findings suggest that LBO targets do not attract inherently different workers whose long-run labor market outcomes simply reflect ex-ante differences in skill endowments.

We also present evidence that the observed impact of PE investments on employees is driven by new skill acquisition, rather than PE firms' ability to retain higher quality workers (who could, in turn, use their retention by the LBO target to signal their high quality to future employers [Waldman 1984; Gibbons and Katz 1991]). For example, the patterns that we observe for employment durations and job transitions are principally relevant for workers in jobs most affected by the adoption of IT-enabled production methods, rather than all workers retained by LBO targets following an acquisition, who are also likely to be screened by PE management. We further assess this alternative explanation and other sources of potential bias in greater detail in the remainder of the paper.

The primary contribution of this paper is to show how individual employees' labor market outcomes are shaped by PE investments. The impact of PE on workers has been a controversial issue in recent years, mainly because there has been no evidence, to date, of how employees fare in the long-run following an LBO. [3] Ours is the first paper to show that private equity investment is valuable partly because it facilitates workers' acquisition of human capital. By implication, our results also suggest that private equity financing can slow or reverse the depreciation of skills for workers at firms that underinvest in modern technologies. A second, more general, contribution of this paper is to illustrate how individual labor market outcomes can

3. Related work has primarily studied aggregate employment changes within PE-acquired firms and industries (Bernstein et al. 2010; Davis et al. 2011; Boucly et al. 2011). In contrast to these studies, we use novel data to illustrate how LBO's impact individual workers' long-run career paths (both within and outside of acquired firms).

be explained by previous employer characteristics. Most models in labor economics typically abstract away from the importance of past employer characteristics in explaining workers' labor market outcomes (Card 2011), while most models in financial economics ignore the impact of corporate policies on employees. Recent debate over labor outcomes such as aggregate unemployment and the potential "skills gap" in the U.S. suggests that private sector investment may play a greater role than previously considered in explaining human capital stock.

The remainder of this paper is as follows. Section II presents the institutional background and theoretical framework used to guide the empirical analysis. Section III describes the data. Section IV presents the analysis. Section V concludes.

II. INSTITUTIONAL BACKGROUND AND THEORETICAL FRAMEWORK

Private equity acquisitions have long been considered one of the most salient drivers of significant corporate change in the U.S. during the last 30 years. Kaplan and Strömberg (2009) estimate that the enterprise value involved in private equity transactions from 1970 to 2007 totaled more than $3.5 trillion dollars (in 2007 dollars). They note that this value was realized primarily during two major waves of private equity activity in the U.S. – the first one taking place in the 1980's, and the second one taking place in the last decade. Interestingly, Kaplan and Strömberg (2009) further document that approximately 40% of this value was realized between the years of 2005 and 2007, a period of both unprecedented private equity activity, and perhaps not coincidentally, significant information technology investment (Farrell et al. 2005; Jorgenson et al. 2007; Tambe and Hitt 2012).[4]

Many studies have found that PE activity leads to gains for private equity investors on average (Kaplan 1989a,b; Lichtenberg and Siegel 1990; Kaplan and Schoar 2005; Acharya et al. 2013). Debate about the impact of PE on workers, however, remains contentious. Academics, policy makers, and union leaders have voiced strong opinions about the perceived effects of private equity on workers, with some arguing that PE buyouts represent adverse shocks to workers' long-run career trajectories. For example, unanticipated shocks to career paths through layoffs could be costly to workers who face non-trivial transaction costs during unemployment

4. See Altman (2007) for related discussion of debt market activity during this time period.

(Agrawal and Matsa 2013). One type of transaction cost that is often discussed specifically in the context of PE is the loss of firm-specific human capital. Shleifer and Summers (1988) claim that buyouts allow PE investors to breach implicit contracts written between managers and workers that were originally intended to incentivize employee investment in firm-specific capital acquisition. One implication of this hypothesis is that buyouts might hurt workers' long-run employability if their firm-specific human capital is rendered obsolete and they lack general skills that they could have previously acquired elsewhere.

The debate about the impact of PE on workers is divisive mainly because there is little empirical evidence to support or refute many of the claims that are made about how PE investments impact workers. In this paper, we argue that a principal mechanism through which private equity impacts workers' long-run labor outcomes is new skill acquisition following operational changes implemented after a buyout. There is widespread evidence—both in the form of case studies and large sample analysis—that investments in new information technologies and complementary work practices are a critical component of the operational improvements that PE investors implement at the firms that they acquire.

In one case study, Matthews et al. (2009) describe a PE firm's acquisition of a large supermarket chain in 2007. The PE firm decided to implement a new data processing system to assist the company with inventory management and product pricing. Investing in the new system required "months of work and hundreds of man-hours to design and implement." Though the investment was costly, the firm was able to realize significantly higher profits by implementing new, data-driven pricing techniques. In another example, Becker et al. (2011) explains that the health care industry has been a frequent target of PE firms, who have made significant efforts to install new information technologies to improve data analytics, which in turn help mitigate medical errors and patient readmission rates at health care facilities. Finally, Greeley (2012) describes operational improvements at the Heat Transfer Products Group following a buyout by Monomoy capital. He writes: "[The partners] visited hundreds of plants. The better-performing companies consistently used some form of the Toyota Production System. The partners began to realize that the traditional private equity approach to operations—putting a former CEO on a

company's board—wouldn't work for some of their purchases. "You could have the best CEO in the world," Hillenbrand says, "but in a manufacturing company profits are made on the floor."

In addition to these case studies, large sample studies illustrate that production upgrades at PE-acquired firms are often characterized by new information technologies and complementary work practices. For example, Bloom et al. (2009) provide survey evidence that PE-managed firms are especially strong at using modern manufacturing processes and complementary work practices. Bacon et al. (2004, 2012) and Wright et al. (2012) show that PE managed firms invest in improved human resource management (HRM) practices that are often employed simultaneously with IT upgrades. Amess et al. (2007) show increased levels of discretionary decision making for skilled employees after a buyout, consistent with theories of skill biased technological change following PE acquisitions.[5]

Prior work provides further evidence that the adoption of IT-enabled work practices may be associated with higher performance, in part, because they facilitate human capital acquisition by the firm's employees, given the emphasis on process control and diagnostic problem solving intrinsic to new production systems (Osterman 1994; Huselid 1995; Macduffie 1995). This literature argues that complementarities exist between investment in IT capital and organizational work practices such as decentralized decision making, team-based problem solving, and skills training (Bartel et al. 1998; Bresnahan et al. 2002; Paravisini and Schoar 2013).

The impact of recent LBO's on workers' subsequent career outcomes could therefore be explained by PE firms' investments in information technologies. Employees may acquire human capital through a combination of job training (Becker 1962) and learning-by-doing (Arrow 1962; Lucas 1988; Yang and Borland 1991). Consistent with this conjecture, Bruining (2005) finds greater employee involvement and training following a buyout. Hitt and Brynjolfsson (1997) conclude that IT investment is complementary to increased training levels, particularly for educated workers who can more easily acquire new skills. Greeley (2012), in his case study of HTPG, describes how Monomoy Capital introduced experts to train assembly floor workers in kaizen practices immediately after the acquisition. In the remainder of this paper, we empirically

5. See Gaudalupe et al. (2012) for related analysis of acquisitions and IT investment.

assess whether and how the skills that workers acquire as a result of PE investment in new technologies impact their long-run labor outcomes.

III. DATA

III.A. Labor Market Outcomes

We construct a new panel dataset that contains detailed information on the individual career paths of a large fraction of the U.S. labor force. Our dataset also contains detailed information on the (time-varying) characteristics of firms that employ these workers. In contrast to employer-employee matched datasets commonly used in other studies (most notably the Longitudinal Employer-Household Dynamics [LEHD] data compiled by the U.S. Census Bureau and administrative datasets available for Scandinavian countries), we have detailed information on the specific nature of the occupations and job titles held by employees, and we can track worker movements both *within* and *across* firms in great depth (the LEHD data do not contain data on workers' occupations or job titles).[6]

The data were obtained through a proprietary agreement with one of the largest online job search boards focused on the U.S. labor market. Job seekers come to the Web site to post their resumes and look for jobs, while employers search through the resumes to identify suitable candidates for their job openings. The population of job seekers that utilize this Web site includes both employed and unemployed workers. The raw data used to create our dataset come from employment history information extracted from resumes that were posted by job seekers on the Web site from 2005 to 2010. Like the sample resume depicted in Figure 1, typical resumes posted to the Web site contain information about individual educational backgrounds, such as highest level of education attainment. We also observe individual employment spells across specific employers. For each employment spell, we observe the job title(s) held by the individual and the start and end dates for each job title (and hence the employment spell itself). We also collect data that users enter into the Web site, but do not appear on their resumes. For example,

6. Our data on worker occupations and tasks performed within firms allow us to more precisely measure the impact of PE investment on workers, as we can compare workers within narrowly defined occupation and task categories. See Olsson and Tag (2012) for related analysis using administrative data for Swedish workers.

individuals identify their race, gender, the date and their current employment status when they last post their resume.

We parse and assemble this information into a panel dataset that describes individual worker backgrounds, education, and employment characteristics. Figure 2 contains an extract of our dataset. The job title and description for a given position enable us to identify a worker's occupation as per the U.S. Department of Labor's (DOL) Standard Occupational Classification (SOC) system. For example, a worker with a job title assigned to the 6-digit SOC code of 11-2022 (major group: Management Occupation, minor group: Sales Manager) would be mapped to tasks including the "planning and distribution of sales across territories…", as the official DOL description for a Sales Manager is

> "Plan, direct, or coordinate the actual distribution or movement of a product or service to the customer. Coordinate sales distribution by establishing sales territories, quotas, and goals and establish training programs for sales representatives. Analyze sales statistics gathered by staff to determine sales potential and inventory requirements and monitor the preferences of customers."[7]

We link our panel dataset of worker characteristics to data on employer characteristics. We standardize the free-text employer name that appears on worker resumes and use fuzzy text-matching algorithms to link employer names to company identifiers in Capital IQ.[8] Capital IQ contains detailed, time-varying information about firms, such as annual balance sheet and income statement data. The data available in Capital IQ correspond to both public and private firms. Capital IQ also contains detailed information about whether a company is acquired in a leveraged buyout during the sample period. For each company that appears in our linked database, we identify whether it is successfully acquired in a merger or acquisition that Capital IQ classifies as a leveraged buyout.

Table 1 presents summary statistics describing the background characteristics of the individuals in our sample. For comparison, we also describe the corresponding characteristics of workers in the U.S. labor force (using data from the 2012 CPS March supplement, BLS statistics, and OES employment data). Panel A shows that our sample is approximately 52% female; the

7. See www.bls.gov/soc/major_groups.htm for more detailed information on official SOC group descriptions.
8. Capital IQ maintains name history files that are used to ensure that a given company with multiple name changes in the resume database is correctly linked to the same firm identifier in Capital IQ.

U.S. labor force is approximately 51% female. Panel B shows that our sample has a similar distribution of education levels across workers, except for those with a college degree, who are overrepresented in our sample. The difference in college degree attainment likely reflects the fact that college-educated workers are more likely to use Internet job resources than are individuals without a high school education (i.e., the remaining workers in the CPS sample).[9]

The distribution of employment across industries for our sample relative to the U.S. labor force is depicted in Panel C. Industry classifications are by SIC 2-digit major group. The distribution of industries for our sample closely resembles that of the total labor force. There is oversampling of the finance and business sectors in our data relative to the U.S. labor force, while there is undersampling of agriculture, construction, and retail trade. Both patterns are to be expected, however, as the propensity to find employment through online resources is likely to be higher in skill-intensive industries such as finance relative to industries such as agriculture.

The distribution of occupational employment for our sample relative to the U.S. labor force is depicted in Panel D. Occupational statistics for the U.S. labor force are obtained from the DOL's 2012 Occupational Employment Statistics (OES) program. To compare the resume-based sample with the OES sample, we map the occupational subcategories in the data to the major occupational headings as per the DOL's SOC system (2-digit level). Panel D shows that the distribution of occupations in the sample is similar to that of the U.S. labor force. There is oversampling of management and administrative and clerical positions in our data, while there is undersampling of occupations related to food, construction, installation, and production services. These patterns also reflect heterogeneity in the propensity to find employment through online resources.

III.B. IT Flows

We use these data to construct a measure of corporate IT investment that has been used in the prior literature: annual inflows of IT employees at a given firm. This measure, first developed by Tambe and Hitt (2012), enables the characterization of a firm's IT investment for a large

9. Panel C excludes workers who have either less than high school educational attainment or unspecified educational attainments; we exclude this group from the current analysis because many of these workers may have incorrectly specified their education levels on the website.

cross-section of firms, over a time-period during which no other relevant data are available. Alternative sources of data that have been used to measure IT investment are poorly suited for our purposes due to several limitations. For example, early studies on IT usage by firms used survey data on IT employment and IT capital captured by trade publications (Lichtenberg 1995, Brynjolfsson and Hitt 1996), but these data are normally available only for a small sample of firms over a few years. A more recent wave of studies uses panel data on IT capital stock acquired from the Harte-Hanks Computer Intelligence Technology Database (CITDB) (Bresnahan et al 2002; Brynjolfsson and Hitt 2003; Forman 2005). This database provides information on computer hardware investment, but is subject to significant measurement error, inconsistent reporting after 1994, and is relevant only for Fortune 1000 firms (Brynjolfsson and Hitt 2003).

Because firm-level data on IT investment are sparse, particularly for the last 20 years, studies measuring IT usage since the mid 1990's have been scarce. Recent studies have developed their own IT investment measures based on IT employment. Tambe and Hitt (2012) show that these firm-level IT employment measures cover a larger cross-section of firms than the CITDB IT capital stock data in the time periods during which both sets of measures are available, but also have the benefit that they are consistently available from the mid 1980's through 2006. For our purposes, these IT labor data provide a significant advantage given the timing of the more recent LBO wave—these are some of the first data to track IT spending after 2000. Moreover, IT hiring rates are a particularly interesting measure of IT investment because labor commands a larger fraction of the IT budget than IT capital stock (Saunders 2010) and may be a better representation of firms' investment into complementary organizational transformation (Hall 2002). Tambe and Hitt (2012) provide a detailed discussion comparing measures of IT investment based on IT labor data with other known data sources on IT investment, describe the construction of these measures, report firm-level correlations with other measures of firm-level IT investment (such as the Harte-Hanks CITDB capital stock data), and analyze the error characteristics of these measures.

We use these data—extended through 2010—to capture changes in IT investment following LBO's. To test whether the LBO's impacting the workers in our sample are associated

with greater IT investment, we aggregate our panel dataset of individual worker-firm-start year observations to the firm-year level. That is, we compute the total number of IT employees who join each firm, in each year during the sample period. Prior studies have shown that when firms upgrade their production processes and implement IT-enabled work practices, they tend to employ significantly more IT labor and that the flow of these workers into firms is a principal mechanism for the diffusion of modern, IT-enabled work practices (Tambe, Hitt, and Brynjolfsson 2012; Tambe and Hitt 2013). IT workers facilitate the integration of new technologies with the existing operations of the firm, consistent with the case study evidence of Matthews et al. (2009). A systematic increase in the hiring rates of IT employees is therefore a strong indicator of an increase in IT investment at a given firm.[10]

III.C. Sampling issues

There are several advantages and caveats to consider with these data. The primary advantages of the data are that we are able to assess the impact of private equity on the labor market outcomes for workers across a broad set of industries and occupations. We are also able to precisely evaluate the theoretical channels through which PE impacts workers by using in-depth information on the tasks that individual workers perform for their employers. The main caveats to the data are that we have labor market information primarily for workers after 1995, and are thus unable to assess the impact of LBO's which took place during the first PE wave of the 1980's; we are able to describe the impact of LBO's in the second, most recent wave of PE in the 2000's (see Kaplan and Strömberg 2009 for related discussion). A second limitation of our data is that we rely on information posted to an online job search site and thus examine workers who have chosen to remain in the labor force. We cannot assess the impact of private equity acquisitions on workers who would have dropped out of the labor work force irrespective of whether or not their employers were acquired in LBO's.[11] We can, however, use this data to estimate the impact of private equity on the labor outcomes of workers who choose to remain in

10. It is worth nothing that this *flow*-based measure of IT investment is actually an understated proxy for the IT changes that a firm makes, as the firm may improve its technological capabilities with lower rates of hiring if that is all that is required to reach a critical *stock* of complementary IT employment.
11. We empirically assess the importance of workers who would have remained in the labor force in the absence of an LBO, but choose to drop out of the labor force following an LBO, in Section IV.C.

the labor force following an acquisition. This population is potentially of more interest, as much of the debate surrounding PE has been centered on the impact of LBO's on workers' long-run employment prospects.

IV. ANALYSIS

IV.A. PE and Labor Market Outcomes

1. Empirical Framework

To assess the impact of PE investment on individual workers' long-run labor outcomes, we begin by summarizing the raw data to describe the average differences in employment durations and lengths of unemployment spells across workers. We then use more rigorous statistical methods to estimate the link between PE activity and individual labor outcomes. Specifically, we utilize the nearest neighbor-matching procedure developed by Abadie and Imbens (2006). This nonparametric estimation tool allows us to identify the mean effect of LBO's on workers' long-run employment durations and unemployment spells without requiring potentially arbitrary assumptions about the functional form describing the empirical relationship between firm characteristics and workers' career paths.

We construct our treatment sample by first identifying all workers who are employed at a given company at the time that the company is acquired by a PE investor in a leveraged buyout. For each job record in our dataset, we can identify the start and end dates for a particular job title held by an individual worker at a given company. We use Capital IQ to identify whether the company is acquired in a leveraged buyout at any time between the start and end dates of the job held by the individual.[12] We create the treatment sample by aggregating all such instances: there are 5,680 such workers in our sample.[13]

To construct our control sample, we draw from our remaining pool of workers who do not experience an LBO during their careers. This control sample is chosen to approximate the unobserved counterfactual of what would happen to treated workers in the absence of the LBO.

12. We also include any workers who remain at the firm for the entire sample period and do not have a job end date, though the number of such individuals is negligible, as almost all workers impacted by LBO's in our sample hold job titles at other firms at some point in time after an acquisition.
13. We examine a 10% random subsample of the total data available in the job search database for computational feasibility; the results are nearly identical for smaller subsets of data, such as random 5% samples.

There are 196,434 such workers in this group. By construction, we are able to assess the labor outcomes of workers who remain in the labor force following an LBO. Our sampling strategy does *not* allow us to measure the impact of LBO's on workers who choose to exit the labor force following an LBO. We assess the selection biases that might result from our sampling choices in Section IV.C.

Table 2 presents statistics describing the characteristics of workers in our treatment and control samples. The workers in the treatment and control sample are nearly identical across many dimensions. The distribution of race, gender, and education of workers in the treatment sample mirrors that of the matched control sample. Moreover, the distributions of industry and occupational assignment for individuals in both groups are nearly identical. The similarities across both groups support the argument that workers in both groups are statistically indistinguishable ex-ante, prior to the LBO transaction occurring.

The size of the data and the similarities between the treatment and control groups are thus well suited for estimating the average treatment effect for the treated group (ATT), because we are potentially able to identify many, similar observations from the control group that can be matched to each observation in the treatment group. We are most interested in estimating the average treatment effect for the treated (ATT), rather than the average treatment effect for the entire sample (ATE) or the average treatment effect for the controls (ATC). Estimating the ATE or ATC is potentially problematic both conceptually and empirically because of the non-random nature of LBO's and the differences in the *size* of the control and treatment samples. Because LBO's are not random, it is conceptually more sensible to estimate the impact of LBO's on workers in the treatment sample, rather than attempt to estimate the impact of LBO's on workers for whom private equity is less relevant (see Heckman et al. (1998) for related discussion concerning program evaluation). Moreover, estimating the ATE or ATC empirically is difficult because the differences in treatment and control sample sizes make it difficult to find the requisite observations in the treatment group that could be used as matches for observations in the control group. In turn, the poor matching that results could severely bias the estimated ATE

or ATC (see Abadie et al. (2004), Imbens (2001), and Heckman et al. (1998) for a related discussion).[14]

2. Private Equity and Long-Run Labor Outcomes

Employment Duration. The raw data indicate that employees of firms that get acquired by PE investors are subsequently employed for longer durations than workers employed elsewhere. We measure long-run employment duration by the total fraction of time that a worker is employed over her career, potentially across several firms, to proxy for her employability over time. Specifically, *Post-employment duration*, is defined as the fraction of time spent employed between the start date of a given job title and the end date of the last job held by a given worker.[15,16] The average employee of a target firm in our sample is employed for 88.1% of a given year (standard error 0.20%). In contrast, the average employee of a non-acquired firm is employed for 82.0% of a given year (standard error of 0.03%). The difference of 6% across the two groups is statistically significant at the 1% level. The raw data, thus, appear inconsistent with the commonly held view that LBO's diminish workers' long-run employability.

We examine the raw data more precisely by using the Abadie and Imbens (2006) matching procedure to estimate the mean differences in long-run employment durations between workers in the treatment and control sample. We use the nearest neighbor-matching algorithm to identify four observations from the control sample that most closely match the pre-LBO characteristics of each treated worker, and we estimate the mean difference in employment duration for the workers across the two groups.[17] That is, for each treatment observation, we identify four workers from the control sample whose characteristics are most similar to the

14. The Abadie and Imbens (2006) matching estimator allows us to specifically estimate the ATT, rather than the ATE or ATC. This flexibility, along with the freedom from imposing functional form assumptions, make matching a more desirable estimation technique than regression.
15. For sample workers who never change jobs and remain employed at the firm for the entire sample period, the end date is replaced by the date when they last uploaded their resume.
16. For example, following an LBO in 2002, one treated worker might upload her resume in 2008 (thus providing six years of employment history), whereas another treated worker might upload his resume in 2010 (thus providing eight years of employment history); we compute the average amount of time that each worker is employed per year to facilitate a comparison in mean employment durations.
17. The nn-match procedure developed by Abadie and Imbens allows for "ties;" that is, if multiple control observations are equidistant from a given treatment observation, all observations are used with the appropriate weighting matrix. We choose four matches, following Abadie and Imbens (2006). The results are similar if we vary the number of matches.

treated individual's characteristics, where worker traits are measured at the beginning of a job spell at a given company (and hence *prior* to the LBO transaction for treated individuals).

Across all specifications, workers are matched on individual person and firm characteristics that previous studies have found to have an impact on labor market outcomes: race, gender, education, occupation (at the 2-digit SOC level), starting year of the position held, years of labor market experience up until the starting year, and firm industry (at the 2-digit SIC level). We weight the start year by a factor of 1,000 relative to other covariates, to ensure that we are comparing workers who join firms at the same time, so as to control for differences in accumulated human capital over time.[18] We also match workers based on firm characteristics to control for differences between workers that may result from the endogenous sorting of workers across firms. We match on firm characteristics, such as *Assets* (defined as the book value of firm assets), *Return on assets* (defined as the ratio of operating earnings to assets), *Capital intensity* (defined as the ratio of net plant, property, and equipment to assets), and individual characteristics, such as *Unemployment duration* (defined as the length of an individual's unemployment spell immediately prior to the matched position), across various specifications.

Table 3 presents the ATT estimates for various combinations of worker and firm characteristics used to match individuals across the treatment and control samples. Column 1 of panel A illustrates that the mean annualized employment duration for workers employed by LBO targets is approximately 8% longer than similarly matched workers employed by comparable firms. Intuitively, this estimate means that workers of acquired firms are employed for 8% longer time in *each* year following an LBO, compared to similar workers employed by non-acquired firms. The measured effect captures employment both at the acquired firm and subsequent firms. When we match along various combinations of firm characteristics, such as ROA or capital intensity, to potentially improve the matching precision, the treatment estimate remains economically large and statistically significant (columns 2-4). Similarly, when we match individuals on the length of their most recent unemployment spell (an additional control that might capture unobserved worker ability), the treatment effect is largely the same. Across all

18. The results are not sensitive to this weighting scheme. Estimates based on an equal weighting scheme yield similar results.

specifications, the annualized difference in long-run employment duration ranges between 6.0% and 8.2%, mirroring the mean estimates computed from the raw data alone.

These results are inconsistent with the commonly held view that LBO's adversely affect workers' employability over the long-run, as this view suggests that employees of LBO targets should experience shorter employment durations after an acquisition. Instead, the findings indicate that when a PE firm acquires a target company, the average worker appears to be *more* employable over his subsequent career. While employees may in fact experience costly disruptions to their careers and possibly realize losses in the value of their firm-specific human capital, the results suggest that there are countervailing forces that improve their prospects of finding new employment.

To verify the causal interpretation of the observed link between the LBO event and subsequent employment durations, we assess whether differences in long-run employment outcomes could be instead explained by systematic differences in the types of workers who join LBO targets and workers who join non-acquired firms. For example, if workers who "select into" an eventual LBO target have higher unobserved ability than workers who join firms that are never acquired, then it is possible that subsequent differences in employment duration are driven by differences in unobserved ability, rather than by the LBO.

Panel B of Table 3 presents estimates of the treatment effect for a different treatment sample: workers who join and leave LBO targets, *prior* to the LBO announcement date. If there are systematic differences in worker characteristics that account for the treatment effect presented in panel A, then similar differences in long-run employment durations should be exhibited by workers who leave eventual LBO targets prior to an acquisition. As illustrated across columns 1 through 5 of panel B, however, the treatment estimates for this sample are all economically small and statistically insignificant. The findings suggest that the observed differences in long-run employment durations for workers employed at the time of the LBO (panel A) are not explained by systematic differences in the types of workers who join LBO targets versus non-LBO targets. Instead, the findings support a causal interpretation of the relation between the LBO event and workers' subsequent employment outcomes.

IV.B. Mechanism: IT Investment and Skill Acquisition

1.Private Equity Investment in Information Technology

We offer the following explanation for the surprising findings in Table 3: workers acquire transferable human capital as a result of PE investments in IT-enabled production technologies following an LBO. To support this hypothesis, we provide several pieces of additional evidence. We begin by verifying the link between LBO's and IT investment for the firms in our sample. As discussed in Section II, a number of studies have found evidence that the recent wave of LBO's is characterized by the introduction of IT-enabled work practices at target firms.

To illustrate that the LBO's in our sample are associated with greater IT investment, we utilize a novel measure of corporate IT investment, described in Section III: annual inflows of IT employees at a given firm. We assess the increase in IT flows after LBO events in two ways: first by looking at simple statistics from the raw data, and second by computing more precise estimates that control for related economic factors. First, for each firm acquired in an LBO, we compute the average inflow of IT employees in the years following the LBO and divide this figure by the average inflow of IT employees in the years prior to the LBO. The mean (median) IT flow ratio is 1.4 (1.0). Figure 4 depicts a histogram of these ratios for the LBO targets in our sample. The large right tail of the distribution illustrates that a large fraction of firms in the sample increase IT hiring substantially following an LBO.

Second, to control for various factors that are also likely have an impact on IT hiring rates, we estimate the following linear regression:

$$Log\ IT\ Flow_{it} = \beta_1(LBO\ Treatment_i) + \beta_2(LBO\ Treatment_i\ x\ Post_{it}) + \nu_i + \omega_t + \varepsilon_{it}, \quad (1)$$

where we define $Log\ IT\ Flow_{it}$ as the natural logarithm of the number of IT workers who are hired by firm i in year t. We regress this measure on $LBO\ Treatment_i$ and $LBO\ Treatment_i\ x\ Post_{it}$, an indicator variable for whether firm i is ever acquired in an LBO, and an interaction term with $Post_{it}$ (an indicator for whether the firm has been acquired by a PE group), respectively. We also include controls for year and firm fixed effects in other specifications (and therefore drop *LBO Treatment* as a covariate). The year fixed effects ensure that we control for aggregate changes in IT labor that uniformly impact all firms in the sample, while the firm fixed

effects control for heterogeneous flows in IT labor due to static differences in firm characteristics, such as industry or average size.

The results are presented in Table 4. Columns 1 and 2 show coefficient estimates for β_2 ranging between 0.0383 and 0.0675. Intuitively, these results indicate that IT labor flows increase by approximately 3% to 7% following a private equity acquisition. The effects are driven primarily by LBO's starting in the year 2000, and especially starting in the year 2003, rather than the LBO's that take place in the mid-1990s, as illustrated in columns 3 through 5. For the years 1995 to 2000, the coefficient estimate of the treatment effect is economically small and statistically insignificant (0.0183), whereas IT flows increase substantially afterward; the coefficient estimates for β_2 reach up to 0.103. The timing of the observed changes in IT flows in our data is consistent with the argument that private equity firms contributed to the diffusion of Internet-enabled technologies following the boom in e-commerce of the late 1990s.[19] Overall, the results for our sample confirm the view found in survey evidence and anecdotes discussed earlier: PE ownership leads to increased IT investment at acquired firms.

2. PE-related IT Investment and Labor Outcomes.

Cross-Sectional Results. To provide supporting evidence for the link between long-run employment durations and upgraded information technologies, we exploit several sources of cross-sectional and time-series variation in individual workers' exposure to new production technologies. Specifically, we examine subsequent employment durations for workers at acquired firms that exhibit above-average changes in IT hiring rates, workers at firms acquired after 2003, and workers at firms acquired by PE investors that typically invest heavily in IT after LBO's. We also study subsamples of workers that previous studies have found to be especially impacted by changes in IT: workers who are highly educated and able to acquire new skills, workers who can be trained relatively quickly to perform certain tasks, and workers who perform

19. Due to the measurement difficulties discussed above, the spread of e-ecommerce technologies after the bust has never been empirically documented, but economists have noted a shift in productivity growth from IT-producing to IT-using industries that occurred after the bust and would be consistent with the spread of new IT innovations and IT-enabled business methods from IT-producing industries to other sectors of the economy (Farrell et al. 2005; Jorgenson et al. 2007). Our findings support the argument that private equity firms played a role in this diffusion process.

tasks complementary to IT-enabled work practices. We also estimate the impact of LBO's on workers who, by comparison, are likely to gain relatively little from the introduction of new information technologies.

We begin by repeating the matching estimation method used in Table 3 on subsamples of LBO-acquired firms that experience different levels of IT upgrades. The results are presented in Table 5. Following the regression results in Table 4, which illustrate that LBO's are associated with mean firm-level changes in annual IT labor hiring rates, we split the treatment sample into individuals who are employed at firms that experience above (panel A) versus below (panel B) mean changes in IT labor hiring rates following an LBO. As column 1 indicates, the mean treatment effect of LBO's on long-run employment duration is 17.2% for individuals employed at firms with high "IT labor hiring rates" (Panel A), whereas the treatment effect is statistically insignificant and economically smaller (7.5%) for individuals at firms with low "IT labor hiring rates" (Panel B). In column 2, we separate workers based on whether they were employed by target firms acquired after versus prior to 2003. Panel A shows that workers acquired after 2003, a period of significant information technology diffusion (as discussed in Section 3), exhibit a significant increase in long-run employment duration (12.1%), whereas workers of firms involved in LBO's prior to 2003 do not experience a significant treatment effect.

In column 3, we separately examine workers at firms acquired by PE investors that have a high versus low propensity to introduce new information technologies at target operations over the entire sample period. Some PE firms emphasize operational upgrading as part of their general takeover strategy, while other PE firms specialize in financial engineering and other sources of value creation unrelated to target operations. We characterize differences in investment strategies empirically by computing average IT labor hiring rate changes across target firms for every PE investor in the sample, and split the treatment sample by PE firms that exhibit above versus below sample median changes in IT labor hiring rates following an average acquisition. Panel A shows that workers associated with deals managed by PE investors that have a high propensity to invest in IT following an LBO experience a significant increase in long-run employment durations (9.1%). Panel B, in contrast, shows that workers associated with PE investors that invest less frequently in IT do not experience a significant change in employability. Across

columns 1-3, the differences in economic magnitudes and statistical significance between the estimates in Panels A and B illustrate that the treatment effect is especially pronounced for workers at firms that have greater exposure to information technology following an LBO, consistent with our hypothesis.

We also test how the treatment effect varies across different subsamples of employees. Prior studies find evidence that IT usage is complementary to increased training levels, particularly for more educated workers who can more easily acquire new skills (Bresnahan et al. 2002; Hitt and Brynjolfsson 1997; Bartel et al. 2007). Therefore, if LBO's foster new skill acquisition, the effects of LBO's on subsequent employment durations should be especially pronounced for college-educated workers and workers who can be trained relatively quickly.

Columns 4 and 5 examine how the effect of LBO's on employment duration varies across workers by education level and different training requirements. Panel A (B) presents estimates of the effect of LBO's on employment durations for individuals with (without) a college degree and for individuals in occupations that require less than (more than) three months of training. Data on occupation training requirements at the 6-digit SOC level are obtained from the U.S. Department of Labor surveys; the median amount of on-the-job training required by workers in the treatment sample is three months.[20] The effects of LBO's on employment duration are especially pronounced for college-educated workers and workers who can be trained relatively quickly. The treatment estimates in columns 4 and 5 range between 0.120 and 0.136 (panel A), whereas workers who have less than college attainment or who hold occupations that require more than three months of training do not exhibit any significant differences in subsequent employment durations relative to the control group (panel B).

Columns 6 and 7 of Table 5 examine the impact of LBO's on employment durations for workers who perform tasks complementary to IT-enabled work practices. Prior research shows that IT investment has a significant impact on the demand for certain tasks. Autor et al. (2003, 2009) and Bartel et al. (2007), for example, find evidence that IT investment increases the

20. The three-month window for training requirements is in line with the typical PE fund life of ten years. Most funds require that their investments generate positive cash flows in the latter half of a fund's life, so it is reasonable to expect that any new training required of employees to work with new information technologies should be fast enough for the acquired firm to turn around positive profits (Kaplan and Strömberg 2009).

demand for non-routine tasks such as problem-solving, making decisions, and analyzing data. The impact of LBO's on workers' employment durations should, therefore, be especially pronounced for workers in who perform these tasks, as these workers would be the most exposed to IT-based production improvements.

Moreover, the effects of IT-related investments in training and on-the-job learning on worker skill acquisition should be most apparent for workers directly involved with production. Bresnahan et al. (2002) and Black and Lynch (2001) find that IT investments are typically bundled with new HRM practices such as self-managing teams, quality circles, and the decentralization of decision authority that facilitates skill acquisition in line workers at the expense of those who principally coordinate work activities or guide subordinates. Therefore, workers directly involved with production activities, rather than those who manage them, should be most likely to acquire human capital resulting from the adoption of IT-enabled work practices.

To test these predictions, we collect data from the U.S. DOL's Survey of Work Activities to identify the extent to which various types of tasks (defined by the DOL) are expected of a worker within a particular occupation (at the 6-digit SOC level). The DOL assigns scores corresponding to the relative importance of each task within each occupation. For example, managers who principally coordinate work (SOC codes 11–1010) have high scores for "Guiding, directing, and motivating subordinates," whereas motor vehicle operators (SOC codes 53–3000) have low scores for this task.

We classify the tasks "Decision making and Problem solving" and "Analyzing Data" as complements to IT-enabled work practices, and judge a worker's (lack of) direct involvement with production activities by using "Coordinating work activities" and "Guiding, directing, and motivating subordinates." These classifications are based on previous studies which examine information technology and task complementary, such as Autor et al. (2003, 2009), Bartel (2007), Bresnahan (2002), and Black and Lynch (2001). Workers in occupations with above-median scores for tasks complementary to IT should exhibit the largest changes in subsequent employment durations because these workers are most exposed to IT improvements (Table 5, panel A, columns 6 and 7). Conversely, workers with above-median scores for coordinating

activities and guiding subordinates are less likely to be directly involved in production and should exhibit no significant changes in subsequent employment durations (Table 5, panel A, columns 8 and 9).

Consistent with these predictions, columns 6 and 7 of Table 5 indicate that workers heavily involved in decision making and problem solving appear to differentially benefit from PE investments, as their treatment effect estimates for employment duration are economically large and significant (0.117), whereas workers for whom such tasks are of little importance do not exhibit significantly longer employment durations following an LBO. The effects are similar for workers in occupations that are highly intensive in analyzing data (treatment effect of 0.140); workers in occupations with low levels of data analysis do not experience longer employment durations over their careers. Additionally, panel B (columns 8 and 9) illustrates that workers with above-median scores for hierarchical tasks, such as "Coordinating work activities" and "Guiding, directing, and motivating subordinates," do not realize longer employment durations than workers in the control sample.

Collectively, the findings in Table 5 support the view that PE investments impart workers with transferable skills that facilitate longer employment durations over their careers. The impact of LBO's on subsequent employment is especially pronounced for the types of workers whose tasks are most central to IT-enabled work practices and are therefore the most likely to acquire new human capital. We measure exposure to IT at the firm level, at the worker-skill level, and at the occupation-task level and find that the impact of PE investments is strong for workers categorized across all of these dimensions.

Time-Series Results. We also show that the effects of PE investment on worker employment duration are especially strong for workers who remain at the acquired firm for longer time periods after an LBO. Workers who remain at the acquired firm for longer durations are more likely to acquire human capital through job training and learning-by-doing over time and should therefore experience the largest increases in subsequent employment duration. We separate workers into quartiles based on the length of time elapsed between the LBO event date

and the date when they leave the firm.[21] The first quartile contains workers who remain at the firm for 0 to 0.5 years; the second quartile is for workers who stay for 0.5 to 1.3 years; the third quartile is for workers who stay for 1.3 to 2.5 years; and the fourth quartile is for workers who stay more than 2.5 years at the acquired firm following an LBO.

As illustrated in Table 6 and Figure 5, workers who exit the firm in the first two quartiles do not have significantly different employment durations relative to similar workers who exit comparable firms. The workers who experience significantly longer employment durations over their careers following an LBO are the individuals who remain at the acquired firm for 1.3 years or more: the treatment effect estimate for these workers ranges between 0.119 and 0.126. The data further support the view that PE investment imparts transferable human capital to workers. The treatment effects documented in Tables 3 and 5 appear to be driven by those workers who remain at the firm long enough to acquire new human capital through job training and learning-by-doing.

Unemployment Duration. The results thus far indicate that LBO's impact individual employment durations vis-à-vis human capital acquisition. We now present evidence that illustrates the evolution of worker career paths following an LBO. If workers acquire transferable human capital as a result of PE investment in IT-enabled work practices, then one would to expect to see employees of acquired firms quickly find new jobs immediately after being employed at an LBO target, relative to the average worker employed elsewhere. Estimating the causal impact of LBO's on career paths is complicated because subsequent unemployment spells and firm transitions will be endogenously affected by other LBO-driven outcomes, such as workers' outside opportunities. Nevertheless, we believe it is informative to describe how careers evolve following an LBO to assess whether the data appear consistent with the transferable human capital hypothesis.

We begin by examining the raw data to measure the length of treated workers'

21. Although the choice to leave the acquired firm is endogenously determined by the LBO event itself and could be correlated with factors unrelated to skill acquisition, such as unobserved worker ability, we analyze the data for workers across this time dimension to assess whether exit rates and subsequent employment durations are at least consistent with the view that workers acquire transferable human capital over time.

unemployment spells immediately after leaving a firm acquired in an LBO. We define *Post unemployment duration* as the length of time between the end date of a given job title and the start date of the next, most recent job title listed on the worker's resume. *Post unemployment duration* is thus the length of time that elapses between the date when a treated worker leaves a firm acquired in an LBO and the date when the worker starts working at her next employer. The average length of such spells is approximately 3.3 months (standard error of 0.20 months). In contrast, the average time spent in unemployment following a job held at a non-acquired firm is approximately 4.9 months (standard error of 0.02 months). The difference of 1.6 months between the two groups is statistically significant at the 1% level, and illustrates that the LBO appears to have an immediate impact on workers' ability to find a new job.

To control for confounding factors that may also impact unemployment spells, we again utilize the Abadie and Imbens (2006) nearest-neighbor matching method. For each worker in the treatment sample, we use the algorithm to identify four observations from the control sample that most closely match the pre-LBO characteristics of the treated worker, and we estimate the mean difference in unemployment duration for the workers across the two groups.[22] Table 7 presents the treatment estimates for various combinations of worker and firm characteristics used to match individuals across both samples.

Column 1 contains the baseline specification and shows that the average worker leaving an acquired firm experiences an unemployment spell that is approximately 2.9 months (0.243 years) shorter than the average spell realized by a comparable control worker. Columns 2–5 indicate similar estimates, illustrating the robustness of the effect to the choice of matching specification. We also show that for workers who leave acquired firms prior to an LBO, the length of subsequent unemployment durations are slightly longer than matched control sample workers (column 6). This result contradicts the potential alternative explanation that workers in PE-targeted firms might be systematically different from control sample workers in ways that would otherwise predict shorter unemployment durations for workers who leave LBO targets.

22. The results are also similar if we instead estimate a hazard rate model to explain worker exit from unemployment, where the baseline rate of exit from unemployment is parameterized for workers who leave firms that have not been acquired by a PE firm. We prefer the matching estimator for this analysis because the inclusion of (the many) control sample workers that are very different from the treatment sample workers could create bias in the hazard rate estimates if the functional form of the model is mis-specified.

The findings support the view that workers in PE-acquired firms gain transferable skills as a result of corporate investments in information technology. After gaining these new skills, workers at acquired firms appear more able to quickly find employment at other establishments, relative to similar workers who do not acquire the same skills. The findings are inconsistent with commonly held view that workers impacted by LBO's experience longer unemployment spells than comparable workers because LBO's serve as negative disruptions to their careers.

Job Transitions. We now describe the types of firms that workers transition to, following an LBO. We examine whether workers from LBO targets transition to firms that have demand for IT-complementary skills. To characterize the nature of job transitions, we estimate the following logit specification:

$$Demand_{ijkt} = \beta_1(LBO\ Treatment_{ij}) + \mu_j + \nu_i + \omega_t + \varepsilon_{ijkt}, \quad (2)$$

where the subscripts *ijkt* denote person *j* moving from firm *i* to firm *k* after being employed by firm *i* at time *t*. The dependent variable is an indicator for whether firm *k*'s IT labor hiring rate in year *t* is above the median firm's hiring rate in year *t* (within the same 2-digit SIC industry); a firm's IT labor hiring rate is defined as the ratio of a firm's IT labor inflow to the total number of its employees. The control variables include $LBO\ Treatment_{ij}$, an indicator variable for whether firm *i* is acquired in an LBO while person *j* is an employee, and variables describing person *j*, firm *i*, and time *t* characteristics. The person characteristics include binary indicators for race, gender, education, and 2-digit occupation, the firm characteristics include return on assets, firm size (log assets), capital intensity, and the time characteristics include indicators for the year that person *j* starts working at firm *i*.

We use industry-adjusted IT labor flows to proxy for demand for IT-complementary skills; we assume that a firm that hires above its industry median level of IT labor has greater relative demand for IT complementary human capital than firms with below median levels of IT labor hiring rates. If workers acquire transferable skills that complement new information technologies following an LBO, then these workers should exhibit a higher rate of transition to firms that have demand for their skills.

The coefficient estimates for our logit specification, in odds ratios, are presented in Table

8. The odd-numbered columns contain only the LBO treatment as an explanatory variable, while the even-numbered columns also include the full set of controls. Columns 1 and 2 depict results for the full sample. In both specifications, the coefficients are positive and statistically significant, illustrating that workers of PE-acquired firms are on average much more likely to transition to companies that have above-median industry demand for IT complementary skills. Specifically, without controls, column 1 shows that the odds of transition to a firm with above-median demand for IT-complementary skills are approximately 17.9% higher for an individual employed by an acquired firm than for an individual employed at a non-acquired firm. Column 2 illustrates that the estimated effect is even stronger (57.1%) when we control for worker and firm characteristics. In columns 3 and 4 (5 and 6), we limit the treatment sample to those workers who hold occupations in which tasks such as "Decision making and problem solving" ("Analyzing Data") have above-industry median levels of importance as per U.S. DOL classifications. Workers who perform tasks that are central to IT-enabled work practices should especially be more likely to acquire skills that enable them to transition to firms that have demand for IT-complementary skills. Consistent with this conjecture, columns 3 through 6 indicate that employees of acquired firms who make decisions, solve problems, and process information at LBO targets are more likely to transition to firms with demand for IT-complementary skills, relative to similar workers at non-acquired firms (the estimated odds increase from 23% to 119% across all columns).

IV.C. Identification

The identification assumption that is central to a causal interpretation of the findings is that PE investments are independent of unobservable factors that impact workers' labor market outcomes. Although this assumption is fundamentally untestable, we conduct several analyses to empirically evaluate the extent to which confounding factors might bias our estimates. Section IV.A.1, for example, illustrates that our findings are unlikely to be explained by the endogenous sorting of workers of heterogeneous ability *into* LBO versus non-LBO acquired firms. In this section, we evaluate other competing explanations for our findings and show that our results are best explained by the channel of LBO-related IT upgrades leading to transferable human capital

acquisition.

1. *Heterogeneous Human Capital Acquisition Rates across Firms.*

One alternative explanation for our findings is that firms that are eventually acquired in LBO's impart workers with skill endowments that are systematically different from those of workers who join non-acquired firms, even in the absence of PE ownership. For example, firms that are acquired might have systematically stronger training programs than non-LBO targets. In turn, these accumulated skill differences could explain divergences in long-run labor market outcomes. To assess this explanation, we again examine the career paths of workers employed at LBO targets in the years *prior* to an acquisition and examine their labor outcomes relative to similarly matched workers at comparable firms.

The alternative explanation suggests that workers who leave LBO targets prior to an acquisition should experience significantly longer employment durations over time. This prediction is observationally equivalent to the alternative hypothesis analyzed in Section IV.A.1, which posits that workers who sort into acquired versus non-acquired firms might be systematically different in ways that explain long-run labor market outcomes. In contrast with this hypothesis, however, panel B of Table 4 illustrates that there are no discernible differences in the labor outcomes of workers who leave LBO firms prior to an acquisition, relative to similarly matched workers at comparable firms. These findings suggest that LBO targets do not impart workers with different skills even in the absence of PE ownership.

2. Endogenous Selection of Workers Out of Firms.

Another alternative explanation for the findings is that workers who *exit* LBO firms might be of higher (unobservable) quality than observationally similar workers who exit non-acquired firms. For example, it is possible that following an LBO, PE-appointed managers screen and retain workers of high ability, who in turn credibly signal their higher ability to potential employers after exiting an LBO target. Equivalently, PE firms may shut down various divisions of a newly acquired company, leading to mass layoffs. Workers who leave non-acquired establishments for idiosyncratic reasons could be inferred to be of lower average quality than

workers who are laid off from LBO targets (Waldman 1984; Gibbons and Katz 1991).

This explanation is also unlikely to fully account for our findings, for four reasons. First, the effects that we observe are principally relevant for those workers in jobs most affected by the adoption of IT-enabled production technologies—specifically, workers at acquired firms that invest most heavily in IT and workers whose tasks are most complementary to new work practices. We do not observe similar effects for other workers who are retained by LBO targets following an acquisition; these workers are also likely to be screened by PE management (or released in a mass layoff). Second, the effects are strong for workers who can be trained quickly to acquire new skills; these are the types of workers for whom IT acquisition is likely most relevant but for whom ability signalling is least relevant (as other employers can quickly replace and train new workers in case of a poor match). Conversely, the treatment effects are negligible for workers who require several years of job training; these are the workers for whom skill acquisition is likely limited following an LBO but for whom ability signalling is important (since training is costly). Third, the results in Table 8 illustrate that employees of LBO targets are more likely to transition to firms that also have demand for IT-complementary skills; if workers were revealed to be of high ability simply by staying at a firm acquired in an LBO (irrespective of technical skills), one might expect these workers to successfully move to jobs even at firms that do not have demand for IT-complementary skills per se.

3. Sample Selection Bias.

As discussed previously, our sampling strategy relies on data obtained from workers who search for a job at some point in time after they experience an LBO. As a result, we are able to make statements about the economic outcomes of workers who remain in the labor force following an LBO; we are not able to assess the impact of LBO's on workers who leave the labor force. One potential source of upward bias in our estimates could result from the fact that LBO's might cause some workers who would have otherwise remained in the labor force (in the absence of the LBO), to drop out of the labor force following an acquisition. If these workers drop out of the labor force because their subsequent labor market outcomes would have been sufficiently poor following an LBO, then the causal impact of LBO's on long-run outcomes such as

employment durations would actually be lower than the estimates in Table 3.[23]

We find evidence, however, that suggests our estimates do not appear to suffer significantly from such sampling bias. We estimate the model used in Table 3 on workers who have below-median levels of labor market experience prior to their employment spell at firms acquired in LBO's. This sample of treated workers is least likely to exhibit such upward bias, as workers with comparatively little labor market experience are likely to remain in the labor force following an acquisition. If this sample yields lower treatment estimates than the full sample, then it is possible that the estimates presented in Table 3 are subject to sampling bias. In contrast to this hypothesis, however, the point estimate for this treatment sample is 0.130 (p-value 0.03), which is much larger than the corresponding estimate of 0.070 in column 5 of Table 3. This finding illustrates that our full sample estimates in Table 3 do not appear to be biased upward due to workers exiting the labor force following an LBO.

V. CONCLUSION

We construct and analyze a novel employer-employee matched dataset and find evidence that PE acquisitions have a significant impact on workers' long-run career paths. In contrast to commonly held views, we find that employees of firms acquired in LBO's subsequently realize longer employment durations over their careers, relative to similar workers at comparable firms that do not get acquired. The data are best explained by the hypothesis that employees of LBO targets acquire transferable human capital as a result of PE investments in technology upgrades. The results are strongest for workers who perform tasks that are complementary to IT-based production systems; these workers are also more likely to subsequently transition to firms that have demand for IT-complementary skills.

The findings have specific implications for the current debate about the impact of private equity on workers, and have general implications for understanding how various corporate policies shape workers' long-run labor market experiences. Economic models often abstract from previous employer characteristics in explaining workers' labor market outcomes (Card 2011).

23. One benefit of our sampling strategy is that our estimates capture the causal impact of LBO's on the labor outcomes of workers who would have dropped out of the labor force in the absence of the LBO, but then choose to remain in the labor force following an acquisition.

The findings in this paper, however, show one important channel by which firm characteristics such as financing, ownership structure, and investment policy impact workers' employment durations and job transitions over the long-run. It is likely that other corporate decisions play a critical role in the formation of human capital stock and other features of the labor market. We believe that understanding these issues can play a critical role in helping mitigate current labor market challenges such as the high rates of unemployment and perceived skill shortages among U.S. workers.

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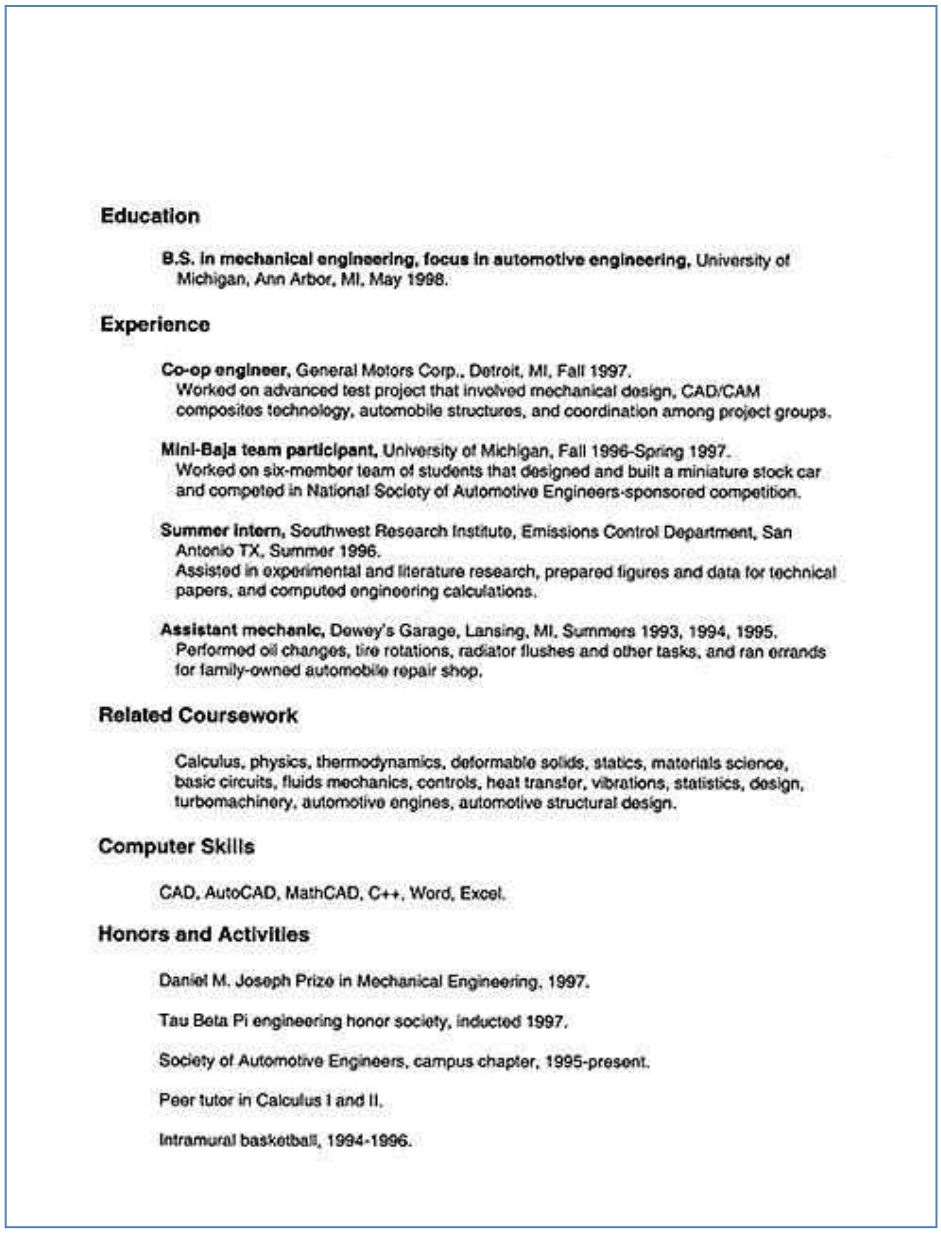
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FIGURE 1
Sample Resume



Education

B.S. in mechanical engineering, focus in automotive engineering, University of Michigan, Ann Arbor, MI, May 1998.

Experience

Co-op engineer, General Motors Corp., Detroit, MI, Fall 1997.
Worked on advanced test project that involved mechanical design, CAD/CAM composites technology, automobile structures, and coordination among project groups.

Mini-Baja team participant, University of Michigan, Fall 1996-Spring 1997.
Worked on six-member team of students that designed and built a miniature stock car and competed in National Society of Automotive Engineers-sponsored competition.

Summer intern, Southwest Research Institute, Emissions Control Department, San Antonio TX, Summer 1996.
Assisted in experimental and literature research, prepared figures and data for technical papers, and computed engineering calculations.

Assistant mechanic, Dewey's Garage, Lansing, MI, Summers 1993, 1994, 1995.
Performed oil changes, tire rotations, radiator flushes and other tasks, and ran errands for family-owned automobile repair shop.

Related Coursework

Calculus, physics, thermodynamics, deformable solids, statics, materials science, basic circuits, fluids mechanics, controls, heat transfer, vibrations, statistics, design, turbomachinery, automotive engines, automotive structural design.

Computer Skills

CAD, AutoCAD, MathCAD, C++, Word, Excel.

Honors and Activities

Daniel M. Joseph Prize in Mechanical Engineering, 1997.

Tau Beta Pi engineering honor society, inducted 1997.

Society of Automotive Engineers, campus chapter, 1995-present.

Peer tutor in Calculus I and II.

Intramural basketball, 1994-1996.

FIGURE 2
Sample Data Extract

Employee work history data				
Employee	Employer name	Job title	Start date	End date
I.T. Worker	Firm Name 3	Project Manager	5-01-2006	present
I.T. Worker	Firm Name 2	Software Engineer	9-01-2003	3-15-2006
I.S. Manager	Firm Name 2	Director of Technology	4-01-2006	present
I.S. Manager	Firm Name 1	MIS Manager	1-01-2006	3-20-2006

FIGURE 3

Distribution of Leveraged Buyouts across Industries

This histogram depicts the frequency of leveraged buyouts across 2-digit SIC major industry groups. The horizontal axis depicts the industry major group name, the vertical axis depicts the fraction of the total sample represented by a particular industry (by number of deals).

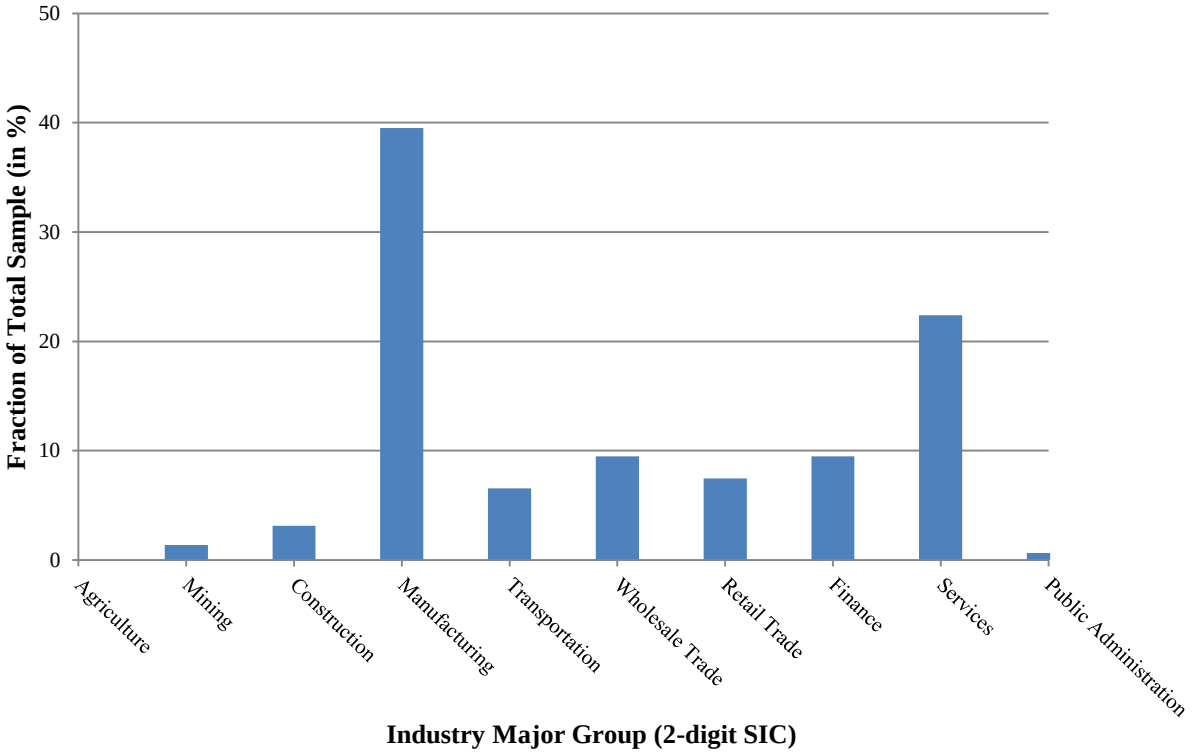


FIGURE 4

Changes in IT Labor Hiring Rates Following Leveraged Buyouts

This histogram depicts changes in the hiring rates of IT employees around leveraged buyouts for sample firms. For each firm, the (mean) *IT_ratio* is defined as the average annual inflow of IT employees across all years after the LBO divided by the average annual inflow of IT employees across all years prior to the LBO. The y-axis depicts the frequency of IT_ratios across all firms in the sample in percentage terms.

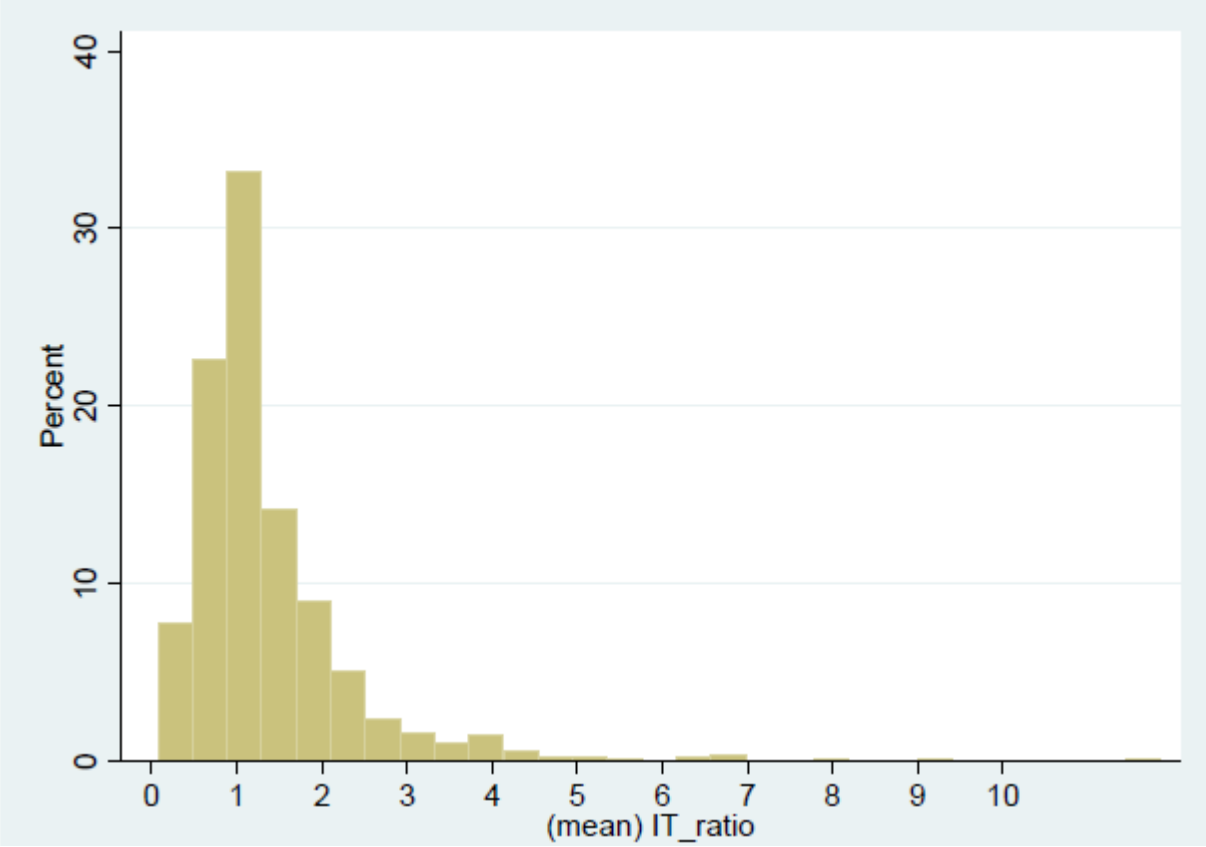


FIGURE 5

Differences in Long-Run Employment Durations for LBO versus non-LBO Workers

This figure depicts the differences in long-run employment durations (annualized) for workers in the treatment sample (LBO workers) versus workers in the matched control sample (non-LBO workers). For all workers in the treatment sample, we compute the distribution of the time elapsed between the LBO effective date and the date of job exit (or last recorded date of employment if still employed at the target firm). Treated workers are then sorted on the quartile of elapsed time to which they belong. The first quartile sample contains workers who remain at the firm for 0 to 0.5 years; the second quartile is for workers who stay for 0.5 to 1.3 years; the third quartile is for workers who stay for 1.3 to 2.5 years; and the fourth quartile is for workers who stay more than 2.5 years at the acquired firm. The solid line represents the matching estimates computed for each quartile sample, and the dashed lines represent the 95% confidence intervals around the estimates.

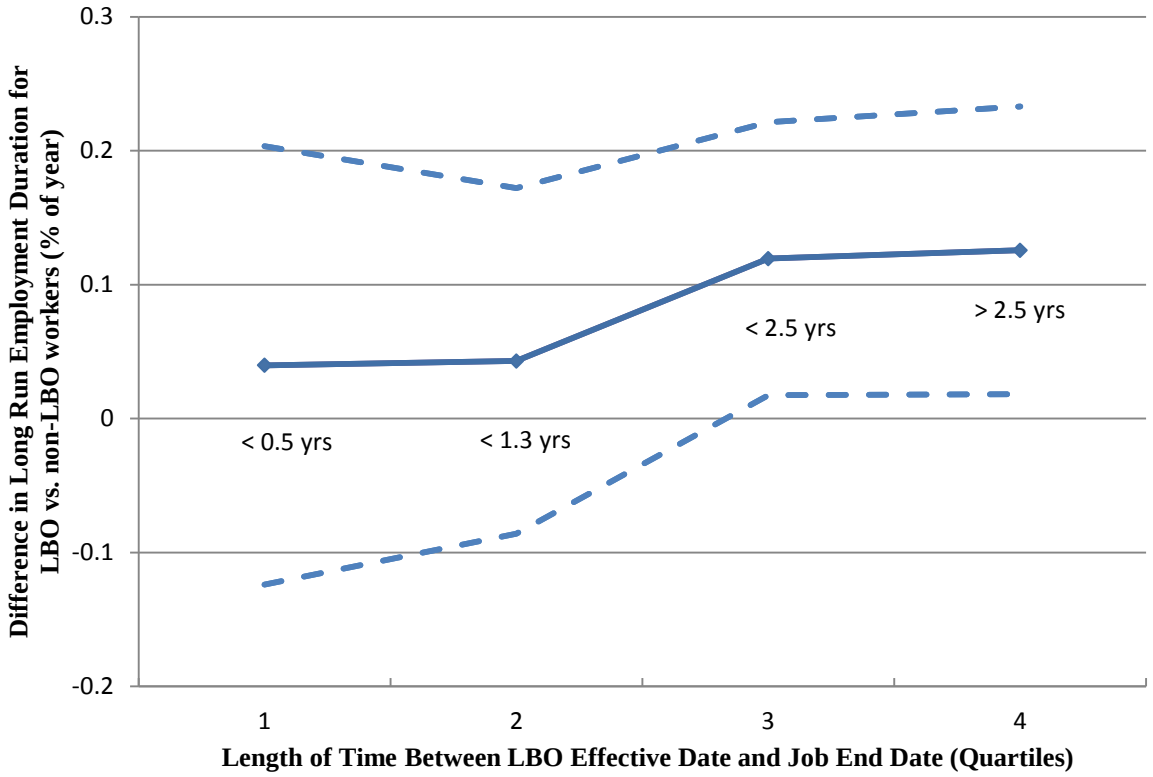


TABLE 1

SAMPLE DESCRIPTIVE STATISTICS

This table presents summary statistics describing the full sample, and for comparison, the characteristics of the U.S. labor force (from the BLS CPS and OES). % Sample and % Labor Force refer to the percentage of individuals in the sample and U.S. labor force, respectively. Industry classifications are based on 2-digit SIC major groups, while Occupation classifications are based on 2-digit SOC major groups. Industry and occupation designations for a sample worker refer to the most recent job title held by the worker for which data is available. Total refers to the total number of observations in the sample for which data is available.

Category	% Sample	% Labor Force	Category	% Sample	% Labor Force
Panel A: Gender			*Panel D: Occupation*		
Female	52	51	Management	15.8	4.9
Male	48	49	Business	6.1	4.9
			Computer	5.2	2.7
Panel B: Education			Engineering	1.6	1.8
4-year college	33	21	Life Sciences	1.3	0.8
High School	27	27	Social Services	1.4	1.4
2-year	20	19	Legal	1.0	0.8
Graduate degree	10	8	Education	3.8	6.4
Vocational	9	10	Arts	1.7	1.3
Doctorate	1	2	Healthcare	2.3	5.9
			Health Support	2.1	3.0
Panel C: Industry			Protective Service	1.3	2.5
Agriculture	0.3	1.6	Food	3.2	8.9
Mining	0.8	0.5	Maintenance	0.7	3.3
Construction	2.7	5.7	Personal Care	1.3	2.9
Manufacturing	18.1	15.8	Sales	12.6	10.6
Transportation	7.6	5.8	Administrative	28.4	16.4
Wholesale Trade	5.4	6.0	Construction	1.9	3.8
Retail Trade	17.8	20.0	Installation	1.2	3.9
Finance	15.3	6.4	Production	3.0	6.6
Services	31.4	32.3			
Public Administration	0.7	6.0	Total	202,114	

TABLE 2

TREATMENT VERSUS CONTROL SAMPLE CHARACTERISTICS

This table presents summary statistics describing workers in the treatment and control sample. The treatment sample consists of workers who are employed at a company at the time that it gets acquired in a leveraged buyout. The control sample consists of all workers who are never employed by a target firm. % Treatment and % Control refer to the percentage of individuals in the treatment and control samples, respectively. Industry classifications are based on 2-digit SIC major groups, while Occupation classifications are based on 2-digit SOC major groups. Industry and occupation designations for a sample worker refer to the most recent job title held by the worker for which data is available. Total refers to the total number of observations in the sample for which data is available.

Category	% Treatment	% Control	Category	% Treatment	% Control
Panel A: Gender			*Panel D: Occupation*		
Female	51	52	Management	19.2	15.7
Male	49	48	Business	6.3	6.1
			Computer	6.4	5.1
Panel B: Education			Engineering	1.9	1.6
4-year college	34	33	Life Sciences	1.0	1.3
High School	30	27	Social Services	0.9	1.4
2-year	20	20	Legal	0.7	1.0
Graduate degree	10	9	Education	2.5	3.9
Vocational	5	9	Arts	1.7	1.7
Doctorate	1	1	Healthcare	1.5	2.4
			Health Support	1.5	2.1
Panel C: Industry			Protective Service	1.4	1.3
Agriculture	0.3	0.3	Food	3.7	3.2
Mining	0.4	0.8	Maintenance	0.6	0.7
Construction	1.8	2.8	Personal Care	1.2	1.3
Manufacturing	21.4	18.0	Sales	14.6	12.6
Transportation	7.4	7.6	Administrative	24.9	28.5
Wholesale Trade	6.4	5.3	Construction	1.7	2.0
Retail Trade	22.9	17.6	Installation	1.3	1.1
Finance	10.7	15.4	Production	3.1	3.0
Services	28.2	31.5			
Public Administration	0.5	0.7	Total	5,680	196,434

TABLE 3

IMPACT OF LEVERAGED BUYOUTS ON LONG-RUN EMPLOYMENT DURATION

This table reports the mean differences in long-run employment durations (annualized) for workers of firms acquired in leveraged buyout (LBO) transactions and similarly matched workers at firms that are not acquired in LBO's. Panel A presents estimates of the treatment effect for workers employed at the acquired firm at the time of the LBO transaction (treatment sample A). Panel B presents estimates of the treatment effect for workers who are employed at the acquired firm but leave prior to the LBO taking place (treatment sample B). The control sample for both panels consists of similarly matched workers who do not work for firms that are acquired in an LBO. *LBO* is defined as a binary indicator for whether the individual works for a firm that gets acquired in an LBO. Across all specifications, workers are matched on individual person and firm characteristics: indicator variables for race, gender, education, occupation (2-digit SOC code), and industry (2-digit SIC code), the starting year of the position held at a given firm, years of labor market experience up until the starting year, and the total number of years of observed employment history. For each treatment observation, four matches from the control sample are identified (with replacement, allowing for ties). Exact matching is imposed on the year in which an individual begins serving a particular job title. Where indicated, additional variables used to match treatment and control observations include firm characteristics, such as *Assets* (defined as the book value of firm assets), *Return on Assets* (defined as the ratio of operating earnings to assets), *Capital Intensity* (defined as the ratio of net plant, property, and equipment to assets), and person characteristics such as *Unemployment Duration* (defined as the length of an individual's unemployment spell immediately prior to a given position). Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
Panel A: Workers who are employed at an acquired firm during the LBO					
LBO	0.080***	0.060**	0.060**	0.066**	0.082**
	0.024	*0.06*	*0.026*	*0.027*	*0.032*
No. of obs.	34,003	28,209	28,204	27,523	19,207
Panel B: Workers who leave an acquired firm prior to the LBO					
LBO	-0.001	0.008	0.007	0.002	0.016
	0.014	*0.015*	*0.015*	*0.015*	*0.019*
No. of obs.	31,686	26,245	26,240	25,602	17,740
Add'l match variables:					
Assets	x		x	x	x
Return on assets		x	x	x	x
Capital intensity				x	x
Unempl. duration					x

TABLE 4

IMPACT OF LEVERAGED BUYOUTS ON INFORMATION TECHNOLOGY (IT) LABOR HIRING RATES

This table reports OLS regression estimates of the impact of LBO's on the annual flow of IT labor into sample firms. The dependent variable is the natural log of the quantity of incoming IT workers at a firm in a given year. The independent variables include a binary indicator (*post-LBO*) for whether the firm has been previously acquired through an LBO in a given year, a binary indicator of whether the firm was ever an LBO target during the sample period (*LBO target*) , and year and firm fixed-effects. Columns 1 through 5 present estimates for different subsets of sample years (the full sample is presented in column 1). Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Sample years:				
	1995–2010	1995–2010	1995–2000	2000–2010	2003–2010
	(1)	(2)	(3)	(4)	(5)
Post-LBO	0.068*	0.038**	0.018	0.062**	0.103**
	0.*035*	0.*016*	*0.053*	*0.020*	*0.026*
LBO target	0.031				
	0.*030*				
Year fixed effects	x	x	x	x	x
Firm fixed effects		x	x	x	x
No. of obs.	143,360	143,360	46,362	107,342	76,718

TABLE 5

IMPACT OF LEVERAGED BUYOUTS ON EMPLOYMENT DURATION BY CROSS-SECTIONAL EXPOSURE TO IT

This table reports the mean differences in long-run employment duration (annualized) for workers of firms acquired in leveraged buyout (LBO) transactions and similarly matched workers at firms that are not acquired in LBO's. *LBO* is defined as a binary indicator for whether the individual works for a firm at the time that it is acquired in an LBO. Across all specifications, workers are matched on individual person and firm characteristics: indicator variables for race, gender, education, occupation (2-digit SOC code), and industry (2-digit SIC code), the starting year of the position held at a given firm, years of labor market experience up until the starting year, the total number of years of observed employment history, the length of an individual's unemployment spell immediately prior to the matched position, and firm assets, return on assets, and capital intensity. For each treatment observation, four matches from the control sample are identified (with replacement, allowing for ties). Exact matching is imposed on the year in which an individual begins serving a particular job title. Panel A (B) presents estimates of the treatment effect for workers who have strong (weak) exposure to IT investment, as per the following variables: *IT worker hiring rates* is defined as an indicator for whether a treated individual is employed at a firm that experiences an above (panel A) versus below (panel B) average industry-adjusted changes in annual IT hiring rates following an LBO. *Buyout occurs after 2003* corresponds to an indicator of whether an LBO takes place after January 1, 2003 (A). *PE firms with IT focus* corresponds to an indicator for whether the PE firm that acquires a target company exhibits above PE-industry median investment in IT. *College education* is an indicator for whether a treated individual has a college degree (A). *Training* is an indicator for whether an individual holds a position that requires less than (A) vs. more than (B) 3 months of training. Task categories for columns 6–9 correspond to U.S. Dept. of Labor survey scores for work activities by 6-digit SOC. *Tasks that are production-oriented* refers to workers with above (A) versus below (B) median scores for *Making Decisions and Problem Solving* or *Analyzing Data*. *Tasks that are not management-oriented* refers to workers with below (A) versus above (B) median scores for *Coordinating Work Activities* or *Guiding, Directing, and Motivating Subordinates*. Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	IT-enabled production intensity:			Skills:		Tasks that are … … production-oriented:		… not management-oriented:	
	IT worker hiring rates	Buyout occurs after 2003	PE firms with IT focus	College education	Training	Decision making & problem solving	Analyzing Data	Coordinating work activities	Guiding, directing, & motivating subordinates
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Strong exposure to IT									
LBO	0.172***	0.121***	0.091***	0.120***	0.136***	0.117***	0.140***	0.175***	0.194***
	0.052	*0.046*	*0.034*	*0.038*	0.040	*0.041*	*0.048*	*0.050*	*0.047*
No. of obs.	19,018	19,108	19,193	19,135	19,132	19,101	19,059	19,065	19,065
Panel B: Weak exposure to IT									
LBO	0.075	0.048	-0.045	0.062	-0.025	0.017	0.015	0.006	-0.028
	0.049	*0.038*	*0.069*	*0.061*	0.043	*0.044*	*0.039*	*0.036*	*0.039*
No. of obs.	19,070	19,041	18,954	19,014	19,043	19,074	19,116	19,110	19,110

TABLE 6

IMPACT OF LBO'S ON EMPLOYMENT DURATION BY TIME-SERIES EXPOSURE TO IT

This table reports the mean differences in long-run employment durations (annualized) for workers of firms acquired in leveraged buyout transactions and similarly matched workers at firms that are not acquired in LBO's. The treatment sample is split into quartiles, based on length of time that a treated worker is employed at an acquired firm following an LBO. The control sample consists of similarly matched workers at firms that are not acquired in an LBO. *LBO* is defined as a binary indicator for whether the individual works for a firm at the time that it is acquired in an LBO. Across all specifications, workers are matched on individual person and firm characteristics: indicator variables for race, gender, education, occupation (2-digit SOC code), and industry (2-digit SIC code), the starting year of the position held at a given firm, years of labor market experience up until the starting year, the total number of years of observed employment history, the length of an individual's unemployment spell immediately prior to the matched position, and firm assets, return on assets, and capital intensity. For each treatment observation, four matches from the control sample are identified (with replacement, allowing for ties). Exact matching is imposed on the year in which an individual begins serving a particular job title. Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Time quartile:	(1)	(2)	(3)	(4)
LBO	0.040	0.043	0.119**	0.126**
	0.084	*0.066*	*0.052*	*0.055*
No. of obs.	19,003	19,021	19,001	19,008
Time between LBO and exit	0–6 months	6–16 months	16–30 months	>30 months

TABLE 7

IMPACT OF LEVERAGED BUYOUTS ON WORKER UNEMPLOYMENT DURATION

This table reports the mean differences in the duration of unemployment (in years) immediately after an individual holds a job title at a specific company, for workers of firms acquired in leveraged buyout (LBO) transactions, and similarly matched workers at firms that are not acquired in LBO's. *LBO* is defined as a binary indicator for whether an individual holds a position at the time when her employer is acquired in an LBO (columns 1–5). *LBO* (*Prior*) is defined as a binary indicator for whether an individual holds a position and leaves her employer before her employer is acquired in an LBO (column 6). Across all specifications, workers are matched on individual person and firm characteristics: indicator variables for race, gender, education, occupation (2-digit SOC code), and industry (2-digit SIC code), the starting year of the position held at a given firm, years of labor market experience up until the starting year, the total number of years of observed employment history, and where indicated: the length of an individual's unemployment spell immediately prior to the matched position, firm assets, return on assets, and capital intensity. For each treatment observation, four matches from the control sample are identified (with replacement, allowing for ties). Exact matching is imposed on the year in which an individual begins serving a particular job title. Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
LBO	-0.243***	-0.209***	-0.205***	-0.201***	-0.201***	
	0.056	*0.056*	*0.056*	*0.056*	*0.058*	
LBO (prior)						0.070
						0.053
No. of obs.	24,120	20,319	20,317	19,820	12,510	11,483
Match variables:						
Assets	x	x	x	x	x	x
Return on assets		x	x	x	x	x
Capital intensity			x		x	x
Prior duration				x	x	x

TABLE 8

IMPACT OF LEVERAGED BUYOUTS ON JOB TRANSITIONS ACROSS FIRMS

This table reports logit estimates of the impact of LBO's on workers' job transitions across firms. The dependent variable is a binary indicator of whether an individual transitions to a firm that exhibits above-median industry (2-digit SIC) IT-worker hiring rates. *LBO* is defined as a binary indicator for whether an individual holds a position at the time when her employer is acquired in an LBO. Where indicated, the other independent variables include controls for person, firm, and time characteristics: indicator variables for race, gender, education, 2-digit SOC occupation, log years of labor market experience, log of firm assets, return on assets, capital intensity, and indicators for the starting year of each person at the source firm. Treatment sample refers to the sample of treatment workers included in each specification. The treatment sample in columns 1 and 2 includes all workers employed by firms at the time of an LBO, the treatment sample in columns 3 and 4 (5 and 6) only includes workers at LBO targets in occupations in which tasks such as "Decision making and problem solving" ("Analyzing Data") have above-median levels of importance as per U.S. DOL classifications. Coefficients are reported as odds ratios. Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Treatment Sample:					
	All workers		Decision making & problem solving		Analyzing Data	
	(1)	(2)	(3)	(4)	(5)	(6)
LBO	1.179**	1.571**	1.234**	1.958**	1.314***	2.191***
	0.077	*0.356*	*0.092*	*0.571*	*0.097*	*0.631*
No. of obs	151,216	8,242	151,001	8,212	151,029	8,216
Controls:						
Experience		x		x		x
Race		x		x		x
Gender		x		x		x
Education		x		x		x
Occupation		x		x		x
Firm Size		x		x		x
Profitability		x		x		x
Capital Intensity		x		x		x
Unemployment		x		x		x