Ashwini Agrawal, Prasanna Tambe

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PRIVATE EQUITY AND WORKERS' CAREER PATHS: THE ROLE OF TECHNOLOGICAL CHANGE [*]

ASHWINI AGRAWAL[†]
LONDON SCHOOL OF ECONOMICS

PRASANNA TAMBE
STERN SCHOOL OF BUSINESS,
NEW YORK UNIVERSITY

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[†] Address for correspondence: London School of Economics, Houghton Street, London WC2A 2AE. E-mail a.agrawal14@lse.ac.uk; Phone Number: +44 020 7955 6875.

PRIVATE EQUITY AND WORKERS' CAREER PATHS: THE ROLE OF TECHNOLOGICAL CHANGE

Abstract

We analyze a new dataset on workers' career paths to examine whether private equity (PE) investments can have positive spillover effects on workers. We study leveraged buyouts in the context of recent information technology (IT) diffusion, and find evidence supporting the argument that many employees of companies acquired by PE investors gain transferable, IT-complementary human capital. Our estimates indicate that these workers experience increases in both long-run employability and wages relative to what they would have realized in the absence of PE investment. The findings underscore PE's role in mitigating the effects of workforce skill obsolescence due to technological change.

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

INTRODUCTION

A private equity acquisition typically serves as an important catalyst for major changes in the trajectory of a firm, but its economic impact on the firm's employees is poorly understood. Many observers have argued that leveraged buyouts (LBOs) cause the employees of an acquired firm to experience significant losses in the form of layoffs, wage reductions, and/or changes in working conditions (Creswell 2012; Parker 2012). This view likely describes the experiences of many workers affected by LBO activity, but may not be a complete description of the effects of PE ownership on employees. In particular, studies that document productivity enhancements at target corporations suggest that many workers may actually benefit from the spillover effects of operational upgrades facilitated by PE ownership (Bernstein et al. 2010; Boucly et al. 2011; Davis et al. 2014).

To date, however, there has been no direct evidence documenting whether private equity investments have spillover effects on the employees of acquired companies, principally because of data limitations. In most commonly available data sets, it is difficult to observe information about PE investments in acquired companies and observe how these investments relate to the long-run career paths of employees. To resolve this issue, we use proprietary data from one of the largest online job search websites in the United States to construct a new panel dataset of individual worker career paths. The data, based on workers' employment histories as reported on their resumes, allow us to track the long-run career paths of individual workers who are employed by PE targets during an acquisition. The employment histories that we study are drawn from an underlying sample of over 20 million workers who post on this jobs board; therefore, the sample includes a sizable fraction of the U.S. workforce and covers a broad swath of workers across many industries and occupations.

We use these data to test the hypothesis that PE investments in an acquired firm's production methods can produce positive spillovers for many of the firm's workers. In the absence of PE investments, many of these workers may be vulnerable to skill depreciation by remaining employed by a firm that uses outdated production methods. We test this hypothesis in the context of a particularly significant class of recent operational improvements—those associated with the rapid diffusion of information technologies during the recent decade. We choose this setting for several reasons: because of its economic significance, because of the rapid skill obsolescence faced by workers due to these changes, and also because a well-established literature provides clear empirical guidance about which types of jobs within the firm should, and should not, be affected by these types of investments (Bresnahan et al., 2002; Bartel et al. 2007, Autor et al. 2003).

We argue that many employees of PE-acquired firms, and in particular, those employees in jobs that have been transformed by computerization, have benefited from exposure to PE-led IT investments. A number of studies find that investments in IT have changed the skill requirements for jobs, and that the new skills are often transferable across employers (Bresnahan et al. 2002; Bartel et al. 2007, Autor and Acemoglu 2010). For example, the introduction of computerized numerical control (CNC) machines in manufacturing has prompted many production-line workers to become proficient in programming and computer-aided design; these skills, over time, have become increasingly valued across a wide variety of industries. We hypothesize that LBO-facilitated IT investments induce workers in many jobs to adjust their mix of skills, and that the acquisition of these skills has persistent effects on workers' labor market outcomes, even after they separate from their employers. Specifically, we hypothesize that LBO workers in jobs where important work activities have been heavily transformed by IT investment are more likely to acquire new skills that positively impact their long-run employability and wages.

The ideal experiment for our exercise is to measure the long-run labor market outcomes of an individual employed by a target firm at the time of an LBO, and compare these outcomes to the counterfactual outcomes that the individual would have realized in the absence of an LBO. If the LBO imparts the worker with IT-complementary human capital, we should observe differences in these two sets of labor market outcomes. Because the counterfactual is unobservable in practice, we use nearest-neighbor matching estimators to identify a worker that most closely resembles the LBO employee prior to an acquisition, and we measure her realized labor market outcomes to approximate the counterfactual path of the LBO worker. The identification assumption is that two workers with matching individual characteristics who simultaneously join two firms with matching employer characteristics, prior to an LBO, would have had otherwise similar career paths had the LBO never taken place. The estimated difference in the subsequent labor market outcomes realized by an LBO individual and a matched control individual can therefore be interpreted as the "treatment effect" of the LBO.

Operationally, we divide our sample of individuals into treatment and control groups based on the following criteria: if a resume indicates that an individual is currently or was previously employed by a firm at the time that it gets acquired in an LBO, we categorize this worker as “treated”; otherwise, the worker is part of the control group. For each worker in the treatment group, we identify workers from the control group (i.e. the “nearest neighbors”) that most closely match the characteristics of the treated worker prior to an LBO. Our choice of matching variables is motivated by both the labor economics and corporate finance literatures, to control for the individual- and firm-level characteristics that relate PE investments to individual labor market outcomes. The detailed nature of our data allows us to control for a number of factors that impact individual labor market outcomes, and we use these data to provide evidence that supports the identification assumption implicit in our matching strategy.

Our first finding is that LBOs, particularly those where IT investment is significant after an acquisition, are positively associated with the long-run employability of many workers in our sample. In both the raw data and in our econometric analysis, we estimate that on an annualized basis, employment spells for a typical worker in our sample are 6 to 9 percentage points longer after an LBO. The effects that we document include time spent at an LBO firm after an acquisition, and are largest for LBOs that occur in years that are often associated with the rapid diffusion of IT across industries

Furthermore, within LBO firms, we observe the largest gains in employability for employees who perform jobs that are transformed by IT. In these jobs, tasks such as analyzing data and information, making decisions and problem solving, and interacting with computers are especially important. The findings support the argument that within occupations, workers at LBO targets must acquire skills to perform their jobs in ways that complement new, IT-based production methods. In contrast, counterfactual workers who perform the same jobs using older production methods do not gain similar skills. Our sample includes LBO workers in a broad set of occupations ranging from marketing to manufacturing (not only IT workers), and we are able to quantify the extent to which different jobs require IT-complementary skills. Therefore, we are able to study the effects of PE investments on workers who perform jobs with the largest scope for change due to IT adoption. Our data suggest that it takes these workers 1.3 years on average to gain the skills that increase their long-run employability. Presumably, this time reflects training and learning-by-doing for LBO employees in jobs that are transformed by computerization.

We also find that LBOs are associated with other differences in workers' long-run career paths. For example, workers who perform IT-complementary tasks experience shorter unemployment spells immediately after separating from an acquired firm. These workers are also more likely to transition to companies that have demand for IT-complementary human capital. These transitions appear to be incentivized by wage concerns; our data indicate that LBO employees who perform jobs that are transformed by IT earn higher long-run wages. Thus overall, these findings further support the view that PE-led IT investments have improved the long-run career prospects for many employees of LBO acquired firms.

We present a number of analyses to discuss alternative explanations for our findings. The variation in LBOs that we exploit for our identification is inherently non-random; therefore, there are a number of endogenous, unobservable variables that could bias our estimates. Our ability to quantify the effects of these variables and thus fully rule out all alternative theories is limited by the data that we can observe. However, we are able to exploit the detailed nature of our data to qualitatively assess the likely direction and magnitude of biases resulting from these factors.

For example, if LBO employees have higher unobservable ability than their matched non-LBO counterparts *prior* to a takeover, then our treatment estimates may reflect differences in worker quality rather than the acquisition of IT-complementary skills. To assess the likelihood of this possibility, we examine the career paths of workers who complete job spells at LBO targets in the years prior to an acquisition; if LBO and non-LBO firms historically produce workers with different levels of ex-post employability, then one might expect to see divergent career paths for workers who exit these firms prior to an acquisition. In contrast to this outcome, however, we see no differences in the long-run careers of these workers; this finding suggests that our results are unlikely to be driven by worker quality differences alone.

We then examine whether our findings are fully explained by the endogenous exit of workers from target firms *after* an LBO. For example, PE-appointed managers may selectively retain high ability workers, who in turn, use their retention to credibly signal their ability to the labor market and thus subsequently realize labor market gains. We argue that this theory is unable to explain our full set of findings, as the career effects that we observe are relevant primarily for workers in jobs most transformed by IT adoption, rather than all workers who are presumably screened and retained by management. Moreover, the individuals that earn higher wages after an LBO are the ones who are employed by companies with high demand for IT;

other companies that hire former employees of LBO targets do not pay these workers systematically higher wages.

We also discuss whether high quality LBO employees search for jobs at higher rates and are thus more likely to enter our resume sample than high quality non-LBO employees. If such differences in sample selection rates are unaccounted for by variables utilized in the matching strategy, then these differences might lead to overestimates of our treatment effect. To evaluate this concern, we examine the observable characteristics of workers who are employed by LBO and non-LBO firms in our sample. The data indicate that the distributions of individual traits are very similar across both groups. Moreover, the distributions of worker characteristics for people who are employed by LBO firms before versus after an acquisition look very similar. Thus, based on observable characteristics, it does not appear that LBOs have a significant impact on the types of workers that appear in our sample and are matched between the treatment and control groups.

The collective set of facts support the hypothesis that PE investments can have spillover effects on workers. The findings contribute to our understanding of the overall economic impact of PE on labor markets. While many observers argue that PE can have deleterious effects on workers through layoffs and plant closures, our paper points out that some of the operational upgrades engendered by PE investment can have positive effects for many employees. This lesson is of particular importance during periods of rapid technological progress. As technological advancements render obsolete the business processes used by many firms, employees at these companies often face poor long-run employment prospects due to skill obsolescence. Our evidence suggests that private equity can serve as a catalyst for introducing production upgrades at many such companies, and help workers slow, if not reverse, the costly depreciation of human capital that stems from technological change.

II. INSTITUTIONAL BACKGROUND AND HYPOTHESIS

Private equity acquisitions are considered by many to have been 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 2000's. 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).

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 causes layoffs that are costly to workers who face non-trivial transaction costs during unemployment (Agrawal and Matsa 2013). The impact of LBOs on workers could be further complicated by the extent to which workers possess low levels of transferable skills, as they may have spent many years investing in firm-specific human capital (Shleifer and Summers 1988).

The debate about the impact of PE on workers is divisive, in part, because there is little empirical evidence on the topic. The most prominent studies in the area, such as Davis et al. (2014), Boucly et al. (2011), and Lichtenberg and Siegel (1990), use net establishment employment data and focus primarily on measuring aggregate changes in employment around LBOs. In this paper, we use a new dataset of matched employer-employee data constructed from resumes to explore whether the operational upgrades associated with PE ownership have spillover effects on the career paths of *individual* workers. We examine this question in the context of the IT-related operational upgrades that were prevalent during the second LBO wave. We build our hypothesis, about the spillover effects of PE-led IT investments, by appealing to various literatures comprised of case studies and large sample analyses.

First, we note that many papers collectively find that operational upgrades at PE-acquired firms are characterized by new work practices. For example, Bloom et al. (2009) and Bernstein and Sheen (2013) provide evidence that PE-managed companies improve management practices after a buyout. Bacon et al. (2004, 2012) and Wright et al. (2012) show that PE managed firms invest in improved human resource management (HRM) practices. 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.

In recent years, many of these operational improvements have been associated with IT investment. Matthews et al. (2009) describe a PE firm's acquisition of a large supermarket chain in 2007. The PE firm implemented 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 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, which have made significant efforts to improve data analytics to help mitigate medical errors and reduce 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."

Second, questions about the effects of PE investments in IT on employees are partly informed by a literature that finds that IT investments often transform occupations by altering the mix of skills they require. Many studies, for example, find that complementarities exist between IT investment and the skills such as data analysis, decision-making, and team-based problem solving (Bresnahan et al. 2002; Autor et al. 2003; Tambe et al. 2012; Hitt and Brynjolfsson 1997). Other tasks, such as process control and diagnostic repair, also appear to be intrinsic to IT–based production systems (Osterman 1994; Huselid 1995; Macduffie 1995; Bartel 2007). To acquire the skills necessary to perform these tasks, workers often require training or learning-by-doing during production (Bartel et al. 2007). Recent work further shows that the skills required by many IT-complementary production processes are task-specific, and thus transferable across employers (Autor and Acemoglu 2010).

In our paper, we tie the lessons learned from these disparate literatures together, and hypothesize that LBO employees in jobs being transformed by PE-led IT acquire new skills that are transferable across firms. Assuming that the rising demand for such skills across the economy is not immediately met by a commensurate, frictionless increase in the supply of these skills (an assumption supported by a number of papers on labor market search frictions and skill-biased technical change (see Acemoglu 2002)), many employees of LBO firms could benefit from IT investment spillover effects even after they leave employers acquired through an LBO.

In particular, we predict that workers who acquire these skills will experience greater levels of employability, become more likely to transition to companies with demand for IT-complementary human capital, and will earn higher wages in the long-run. In the Analysis section, we develop these predictions in more detail.

III. DATA

III.A. Sample Construction

By bringing together three separate data sources, we construct a panel dataset that contains time-varying information about individual workers and their employers. In this section, we outline the data construction process, provide sample descriptive statistics, and discuss important sampling considerations. Because the data construction process itself is extensive, we provide an online appendix to this paper that describes key steps of the data construction in greater detail.[1]

The first source of information comes from an online job website focused on the U.S. labor market. The website serves as a platform for two-sided matching between job seekers and companies; job seekers post their resumes on the website to look for jobs, while employers search through the resumes on the website to identify job candidates.[2] Individual job seekers voluntarily provide information about their backgrounds and employment histories to the website through their resumes and by entering information in various standardized fields.

The website provided us with the most recent information posted by individual job seekers as of 2010. From an individual's resume, we observe the highest level of education attained and the names of past employers. For each position listed on a resume, we also collect the position's job title and description, as well as the position's start date and end date. From the website, we obtain information on the dates when users first post and last update their resumes, their employment status as of the time they last update their resume, and the wages they earned in their most recent job. We also collect user demographic information such as race and gender. There are approximately 23 million workers in our sample, or 13% of the U.S. labor force. The vast majority of users post their resumes during the years 2005 to 2010, with employment histories dating back primarily to the early 1990's.

[1] The Appendix also discusses various issues pertaining to data quality as it relates to the analysis.
[2] The data were obtained through a proprietary agreement with the company; as part of the agreement, the company name cannot be revealed.

We standardize each employment spell listed on a person's resume by classifying the occupation held by the individual in accordance with the U.S. Department of Labor's Standard Occupational Classification (SOC) system. Using information on job title, job description, and worker education, we are able to identify the 6-digit SOC code that most accurately characterizes an individual's job at a particular firm.[3] An observation in our dataset is thus uniquely defined by a specific individual holding a specific 6-digit occupation with a unique start and end date.

We merge this data with a second source of information: the Department of Labor (DOL) and Employment and Training Administration's (ETA) 2012 survey data on occupational requirements. The U.S. DOL/ETA's Occupational Information Network (O*NET) database contains information on the work activities, skills, and tasks required in a given occupation (at the 6-digit SOC level). This information is collected from national surveys of each occupation's worker population (randomly selected from the entire population of establishments in the U.S.) and occupation experts for those occupations where worker sampling is difficult. For example, the O*NET program quantifies the extent to which work activities such as "Analyzing Data or Information" and "Making Decisions and Problem Solving" are important for every SOC code defined by the DOL. The O*NET database has become a major data source for empirical work in labor economics (Jensen and Kletzer (2010), Blinder (2009), and Hallock (2013)). For every 6-digit SOC code in our resume dataset, we merge the corresponding data from the O*NET database on work activities so that we have standardized occupational characteristics for each individual employment spell in the resume sample.

We then merge our linked data of individual employment spells to a third source of information: Capital IQ's database on public and private firm characteristics. For each employer associated with a unique employment spell on an individual's resume, we collect data on the employer's balance sheet and income statements as of the years when an individual is employed by the firm.[4] We also collect data on whether the company was ever acquired in a leveraged buyout during the sample period. Specifically, for each company, we collect information on the size of its assets, physical capital stock (plant, property, and equipment (PPE)), operating earnings, and 4-digit standard industrial classification (SIC) code.

3. See www.bls.gov/soc/major_groups.htm for more detailed information on official SOC group descriptions.
4. 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.

The final, merged dataset consists of detailed occupation data and detailed employer data for each employment spell for all job seekers who use the website. For computational feasibility, we analyze a 10% random sample for this paper. As discussed in the appendix, our results robust to the choice of sample size, as we observe similar findings for random 5%, 10%, and 15% subsamples of the full data.

III.B. Sample Descriptive Statistics

Table 1 presents summary statistics describing the individuals in our sample. For comparison, we also present the corresponding characteristics for workers in the U.S. labor force using data from the 2012 CPS March supplement, BLS statistics, and OES employment surveys. The figures in the table indicate that our data cover a wide spectrum of the U.S. work force, as online job sites such as our data provider are a major job searching channel (Kuhn and Skuterud (2000, 2004)). Not surprisingly, however, there are some important differences between our sample and the overall population. Panel A shows that our sample is approximately 52% female; the U.S. labor force is approximately 47% female. Panel B illustrates 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).[5]

The distribution of employment across industries for our sample is compared to that of the U.S. labor force in Panel C. Industry classifications for the employers in our sample are by SIC 2-digit major group. The span of industries for workers in our sample closely resembles that of the total labor force, as the employers in our sample consist of nearly all public firms as well as many of the larger private firms in the U.S. There is oversampling of the finance and business sectors in our data relative to the U.S. labor force, and there is undersampling of agriculture, construction, and retail trade. Both patterns are to be expected, as the propensity to find employment through online resources is likely to be higher in knowledge-intensive industries such as finance relative to industries such as agriculture. Moreover, industries that are

5. 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.

undersampled in our data tend to consist of smaller, private firms with relatively fewer employees.

The distribution of occupational employment for our sample is compared to that of the U.S. labor force 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. Moreover, the large number of observations across occupations illustrates that we observe job histories for workers across many categories, ranging from lower ranked employees to higher ranked managers. There is some oversampling of management and administrative and clerical positions in our data, and there is undersampling of occupations related to food, construction, installation, and production services.

In Panel E, we report the number of weeks that workers are unemployed (for each of the years when users upload their resumes). The average number of weeks of unemployment across years ranges from 6.2 to 11.1 weeks per year for the sample, and is nearly identical to the time-series distribution of weeks of unemployment for the labor force. In Panel F, we report that the mean and median annual wages earned by users in our sample is $38,000 and $33,000, respectively. These figures are very close to the U.S. labor force mean and median incomes in 2010 of $38,337 and $26,197 (as per the 2011 CPS), respectively.

Overall, Table 1 illustrates that our dataset contains detailed information about the types of job seekers who tend to use online resources to find employment. While the number of such workers in this population is significant and covers a large cross-section of the skill distribution, as evidenced by the broad similarities in worker attributes between the sample and the labor force, there are many workers who are not represented in our data. Therefore, we are able to use our data to assess how private equity impacts many, but not all, workers within a firm.

IV. EMPIRICAL FRAMEWORK

IV.A. Identification Strategy

To test whether private equity investments in IT can have spillover effects on workers, we examine the career paths of individuals in our sample using the nearest neighbor matching estimator developed by Abadie and Imbens (2006). Our hypothesis predicts that LBO-led IT impacts many individual workers at target firms by transforming their work activities and incentivizing them to acquire skills to perform these tasks; in turn, the acquisition of these skills has a positive effect on workers' labor market outcomes such as long-run employability and wages. The ideal experiment for testing our hypothesis would be to measure the long-run labor market outcomes of an individual employed by a target firm at the time of an LBO, and compare these outcomes to the counterfactual outcomes that the individual would have realized in the absence of an LBO. Because the counterfactual is unobservable in practice, we use the matching estimator to identify workers in our sample that closely resemble the LBO employees prior to an acquisition, and measure their labor market outcomes to approximate the counterfactual outcomes of LBO employees.

More specifically, we divide our sample of workers into treatment and control groups based on the following criteria: if a person's resume indicates that the individual is employed by a firm at the time that it gets acquired in an LBO (as per Capital IQ), we categorize this worker as "treated"; otherwise, the worker is part of the control group. In our sample, matched workers may still be currently employed by the same employer at the time of the match (employment status after the LBO event is not used as a matching variable). For each worker in the treatment group, we identify four workers from the control group (i.e. the "nearest neighbors") that most closely match the characteristics of the treated worker prior to an LBO.[6] We match workers using a number of characteristics measured at the beginning of a job spell at a given company (and hence prior to the LBO transaction for treated individuals).

Our choice of covariates is motivated by both the labor economics literature and corporate finance literature, as our experiment requires that we control for the individual and firm level characteristics that relate PE investments to individual labor market outcomes. We match workers based on individual-level characteristics such as education, gender, race, and 2-digit occupation; employer-level characteristics such as 2-digit industry, operating performance, size, and capital intensity; and time characteristics such as the year when a worker starts employment at a given firm. The identification assumption is that two workers with matching

6. 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.

individual characteristics who simultaneously join two firms with matching employer characteristics, prior to an LBO, would have had otherwise similar subsequent career paths had the LBO never taken place.

We look at several labor market outcomes as dependent variables in our analysis. The primary outcome that we examine is a worker's long-run employability, measured by the fraction of time that an individual is employed over her observed career, potentially across several firms. Specifically, for any given worker starting a job at time t, we define the long-run employment duration for the worker at time t to be the ratio of the sum of all K job spell lengths t_i starting at time t until time $t+N$, divided by N, where N is the length of time for which the worker's remaining job history is observed and K is the number of jobs held between time t and time $t+N$. For sample workers who never change jobs after time t and remain employed at a firm for the entire sample period, the end date for their job is replaced by the date when they last uploaded their resume.

Our measure of long-run employment duration is inherently right censored, as we do not observe career paths after the most recent resume update by an individual. However, censoring does not adversely impact our analysis because our measure of employment spells can be compared across workers with different career lengths, as the measure depicts the fraction of time that a given worker is observed in employment relative to the total amount of time observed in the labor force.[7,8]

IV.B. Identification: Strengths and Limitations

Our estimation strategy has several advantages and disadvantages. One advantage of our approach is that the treatment effect estimate is easy to interpret: conditional on the identification assumption, it captures the average causal effect of an LBO on the long-run employability of individuals in our sample who are LBO employees at the time of a PE acquisition. A second advantage of our strategy is that we can ignore unmatched control workers that might otherwise skew our treatment effect estimates—without having to specify a selection model of individual

[7] 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 her 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.

[8] As a robustness check, we also control for the total length of the observed career path in our specifications to ensure that we compare treatment and control workers whom we observe for equal amounts of total time, and we verify that our estimates remain unchanged.

assignment into treatment and control groups (Abadie and Imbens (2006), and Imbens and Wooldridge (2009)).

A third advantage of our empirical context is that our data contain a large sample of non-LBO workers that closely resemble individuals in the treatment group, which supports our identification assumption. Consistent with this assertion, Table 2 describes the characteristics of workers who are assigned to treatment and control groups as per our empirical strategy. As illustrated in the table, 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. To the extent that unobservable worker characteristics are correlated with the many observable characteristics that can be measured in our data, the similarities across workers support the identification assumption.

There are two important limitations to our analysis. The first limitation stems from our sample coverage. Our data covers a large cross-section of the U.S. labor force, but is not representative of the entire population, as there are many workers who are affected by LBOs who do not appear in our sample because they do not use the internet for job search. Thus, we are able to identify the impact of PE spillovers on many, but not all, workers.

A second limitation of our approach is that LBOs are non-random events; the validity of our identification assumption thus depends on our ability to control for the variables that matter for explaining long-run labor market outcomes. There are potentially several unobserved, endogenous factors that could bias our estimates, such as the unobserved abilities of workers across treatment and control groups, the unobservable investment opportunities that drive PE firms to target specific companies and their respective workforces, and the unobservable drivers of job search. Although we cannot precisely estimate the effects of these unobservable factors, we utilize the detailed nature of our data to assess the likely direction and magnitudes of biases that arise as a result of these factors. We discuss this analysis in Section V.C.

IV.C. LBO Characteristics

As described in section 3, we use Capital IQ to identify individuals in our database who have been employed by companies acquired in LBOs during the sample period. Figure 1 depicts the yearly number of target firms, along with the total transaction value of the associated acquisitions, for our sample. Figure 1 also depicts the annual number of LBO targets and total transaction values for all U.S. LBOs (population) for comparison. Our data capture approximately 35% (4,193 out of 12,143) of all deals that take place in the U.S., which amounts to approximately 58% ($261 of $450 billion) of total LBO transaction value during the sample period. Our sample coverage compares favorably to other studies in the PE literature; Davis et al. (2014) for example, are able to identify LBOs in Census establishment data that account for 69% of total LBO transaction value from 2000 to 2005. The firms that are missing from our sample correspond to employers who do not have any employees that use our data provider for job search. These companies are typically smaller firms with relatively fewer employees, which accounts for the discrepancy between the deal number and transaction value population percentages represented by our sample.

Figure 2 illustrates the industry distribution of LBOs in our sample relative to the population. The figure shows that the LBOs analyzed in our sample are highly representative of the entire population of LBOs that have taken place across various industries, ranging from manufacturing to retail trade and service sectors. Taken together with the worker distributions depicted in Tables 1 and 2, the data illustrate that our sample covers a broad swath of worker types across a heterogeneous set of LBOs.

We also describe IT investments made at sample firms following an LBO. We use measures of IT investment based on prior work that utilizes the same underlying data source as this paper (Tambe and Hitt 2012). Specifically, we use our employment history data to measure the number of IT employees in our sample who join firms every year from 1990 to 2010. Job board users specify whether they consider themselves as being employed in sectors such as IT, Sales, Marketing, Manufacturing, etc. on the Web site when they post their resumes. Using job start dates from worker resumes, we add up the number of IT workers who join each firm in a given year. The idea underlying this IT investment measure is that when firms implement IT-enabled work practices, they tend to employ significantly more IT labor to facilitate the integration of new technologies with the existing operations of the firm (Tambe, Hitt, and Brynjolfsson 2012; Tambe and Hitt 2014). IT worker hiring rates are also informative because labor commands a larger fraction of the corporate IT budget than physical capital (Saunders

2010) and likely best reflects broader investments into technology-complementary organizational transformation (Hall 2002).[9,10]

We compare IT investment at buyout targets and non-buyout targets by estimating the following linear regression:

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

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*$_i$, indicator variable for whether firm *i* is ever acquired in an LBO during the sample period, and *LBO Treatment*$_i$ *x Post*$_{it}$, an interaction term in which *Post*$_{it}$ is an indicator for whether firm *i* has been acquired by a PE group by year *t*. We also include controls for year and firm fixed effects in other specifications (and therefore drop *LBO Treatment* from the regression). 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 3. Columns 1 and 2 show coefficient estimates for β_2 ranging between 0.038 and 0.068, which indicate that IT labor flows increase by approximately 3% to 7% following a private equity acquisition. The effects are driven primarily by LBOs starting in the year 2000, and especially starting in the year 2003, rather than the LBOs 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 labor flows around LBOs indicates that PE acquisitions are associated with significant IT investments. These investments likely reflect the changes described in the survey evidence and anecdotes discussed in section 2. The data are

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[9] Tambe and Hitt (2012) assess the sampling properties of these IT investment measures, and show that IT employment-based measures are highly correlated with other IT measures used for earlier sample periods. We do not repeat their analysis in this paper for the sake of brevity, but we point readers to Tambe and Hitt (2012) for further details.

10. Alternative sources of data that have been used to measure IT investment are subject to several known 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 investment, but is subject to significant measurement error, is relevant only for Fortune 1000 firms, and most importantly, is unreliable for this type of measurement after 1994 (Brynjolfsson and Hitt 2003).

consistent with the view that private equity firms capitalized on the diffusion of Internet-enabled technologies following the boom in e-commerce of the late 1990s (Farrell et al. 2005; Jorgenson et al. 2007), creating a setting for many LBO employees to acquire new skills on the job.

V. ANALYSIS

V.A. Private Equity and Long-run Employability

In this section, we present evidence that supports the hypothesis that PE investments during our sample period have a positive, causal effect on the long-run employability of workers in our sample. Figure 3 compares the measures of long-run employment durations for treated versus control workers in the raw data (i.e. without any controls). 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 alone, therefore, support the argument that the average worker from an LBO firm in our sample is subsequently employed for a longer fraction of time during her career after an LBO relative to non-LBO workers in our sample.

Table 4 presents the nearest neighbor matching estimates for various combinations of worker and firm characteristics used to match individuals across the treatment and control samples. Under our identification assumption, the estimates can be interpreted as measures of how a treated worker fares after an LBO compared to how she would have fared had the LBO never taken place. In column 1 of panel A, estimates of long-run employment durations are 8% higher for LBO employees in our sample. Intuitively, this estimate indicates that workers of acquired firms are employed for 8% longer time *each* year as a result of an LBO, across subsequent job spells at the acquired firm and any subsequent employers. When we match along different combinations of firm characteristics to vary the predictors of LBO and non-LBO firm assignment, 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 specifications, the annualized difference in long-run employment duration ranges from 6.0% to 8.2%, mirroring the mean estimates computed from the raw data alone.

The estimates are broadly consistent with the view that PE investments can have positive spillover effects on a worker's long-run employability. To shed light on the mechanism that explains these findings, we next test sharper predictions of the hypothesis that IT investments are a principal driver of these changes. The next section focuses attention on workers who perform tasks that are known to have been transformed by information technologies. If workers performing these tasks acquire transferable skills that complement PE-led IT investments, they should experience changes in their long-run employability.

V.B. Mechanism: IT-Related Production Upgrades

As described in Section 2, a large body of labor economics and productivity research finds that recent advancements in information technologies have changed the skill requirements necessary to complete many jobs. Within our sample, we more closely examine the firms and workers that are likely to have been most affected by PE-led IT investment. For LBOs where IT investments are a major source of operational upgrades, we would expect to see workers benefiting significantly from skill acquisition required by complementary technology investments.

Firm-Level Variation in IT investment. We begin by generating matching estimates, similar to those used in Table 4, on subsamples of workers employed by firms that invest heavily in IT after an LBO. We split our treatment sample of workers into individuals who are employed by firms that experience above versus below mean changes in IT labor hiring rates following an LBO. As column 1 of Table 5 indicates, the mean treatment effect of LBOs on long-run employment durations 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, as the years after 2003 were a period of significant information technology diffusion (as discussed in Section 2). The treatment estimates show a significant increase in long-run employment durations (12.1%) for individuals who are employees of LBO targets during years of rapid IT diffusion.

In column 3, we separately examine workers at firms acquired by PE investors for whom IT investments at target operations appear to be a major source of strategic planning. Some PE firms emphasize operational upgrades 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 higher estimates for long-run employment durations for workers associated with deals managed by PE investors that have a high propensity to invest in IT following an LBO (9.1%). The estimates on employability in panel B, in contrast, for workers associated with PE investors that invest less frequently in IT, are insignificant. Across columns 1-3, the estimates in Panels A and B support the argument that PE-led IT spillover effects are largest for LBO employees who are most exposed to IT developments during the sample period.

Worker-Level Variation in IT Complementarity. To introduce greater precision to this argument, we examine workers who perform activities that have been most transformed by IT investment. IT has required workers in many occupations to acquire new skills. Our hypothesis predicts that when we compare two individuals who, prior to an LBO, perform similar jobs, the individual employed by an LBO firm investing in IT should experience improved employability relative to the individual employed by a non-LBO firm. If IT investments facilitated by PE investors serve to simply substitute for workers at LBO targets, then we would not expect LBO employees to realize any labor market gains as a result of PE ownership.

Columns 4-7 of Table 5 examine the impact of LBOs on the employment durations of workers who perform work activities that prior research has found to be significantly transformed by IT investment. Autor et al. (2003, 2009) and Bartel et al. (2007), for example, find evidence that the computerization of many functions has significantly shifted the demand for tasks such as analyzing data, problem-solving, and making decisions, and has effectively increased the skill requirements for workers performing these tasks. In line with this reasoning, Bresnahan et al. (2002), and Black and Lynch (2001) find that IT has increased the demand for college level education—ostensibly a proxy for the skills needed to perform tasks that are central to IT-based production systems. The impact of LBOs on workers' employment durations should, therefore, be especially pronounced for college-educated workers in jobs where IT-complementary tasks are important.

To test these predictions, we utilize data from the O*Net Survey of Work Activities to identify the extent to which various activities are expected of a worker within a particular occupation (at the 6-digit SOC level). The DOL assigns numerical scores corresponding to the relative importance of each activity 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 activity. See the Appendix for more detail on these measures.

We classify the work activities "Analyzing Data", "Decision making and problem solving", and "Interacting with Computers" as being IT-complementary (Autor et al. (2003, 2009), Bartel et al. (2007), Bresnahan et al. (2002), and Black and Lynch (2001)). Workers in occupations with above-median scores for the importance of these work activities should have the greatest scope for changes in subsequent employment durations because these work activities are particularly impacted by IT improvements.

Consistent with these predictions, columns 4-6 of Table 5 indicate that the treatment effect estimates for employment duration for workers who are required to analyze data, make decisions and solve problems, and interact with computers are economically large and significant (ranging from 0.092 to 0.140). The comparable estimates for workers for whom such activities are of little importance (Panel B) are not statistically significant.[11] Column 7 indicates that the largest estimates of our employability measure are for college-educated workers. These findings support the view that workers in jobs requiring new skills to perform activities transformed by IT show the largest improvements in employability.

Time required to acquire human capital. We use our data to estimate the amount of time that workers require to pick up IT-complementary skills following an LBO. We hypothesize that workers who remain at a target firm for longer durations are better able to acquire human capital, through job training and learning-by-doing, than workers who separate from their employer soon after an acquisition. We identify treated workers who leave their employer during the sample period, and group them into quartiles based on the length of time elapsed between the LBO event date and the date when they leave the firm. The first quartile contains workers who remain at the

11. In addition to distinguishing workers based on sample median task scores, we find that our results are robust to alternative measures of task importance, such as sample mean and top tercile scores. We also examine other types of non-routine tasks that could be classified as complementary to new IT work practices (as per Autor et al. ([2003]), such as "Thinking Creatively", and find similar results that are consistent with our hypothesis.

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.

Table 6 and Figure 4 illustrate that workers who exit the firm in the first two quartiles do not have significantly different employment durations than 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 results suggest that workers in jobs transformed by IT investment require about 1.3 years to gain the skills necessary to realize improvements in employability.

Unemployment Duration after an LBO. To describe the temporal dynamics of the increased employability realized by LBO workers, we examine the length of unemployment spells immediately experienced by those workers who separate from their employer following an LBO. Specifically, 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 job title listed on the worker's resume, where observed. For each separated worker in the treatment 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 the treated worker, and we estimate the mean difference in unemployment durations for the workers across the treatment and control groups.

Table 7 presents the treatment estimates for various combinations of worker and firm characteristics used to match individuals across both samples. Panel A contains results for the full treatment sample, while Panel B contains results for the treatment sample split by quartiles of time spent at the target firm following an LBO (as per Table 6). Column 1 of Panel A 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 matched control worker. Columns 2–5 indicate similar estimates, illustrating the robustness of the effect to the choice of matching specification. For workers who leave acquired firms prior to an LBO, the lengths of subsequent unemployment durations are slightly longer than matched control sample workers (column 6). This result shows that workers from target firms do not historically experience shorter unemployment durations after exit.

Panel B dissects the results in Panel A by time quartile of exit after an LBO. The first three columns show statistically insignificant (or at most, weakly significant) effects of the LBO treatment on unemployment durations for workers who exit LBO firms within 1.3 years. Column 4, in contrast, shows that workers who stay longest at the LBO firm have the shortest subsequent unemployment durations. Although subsequent unemployment spells (not just the immediate spell after an LBO) also factor into the long-run employment duration measures studied in Table 3, the results in Table 7 illustrate that the increases in long-run employability that result from LBOs are not simply artifacts of longer spells within LBO firms.

Employer Transitions. Our hypothesis predicts that LBO workers who acquire IT-complementary skills should be more likely to transition to companies that have demand for these skills. To characterize the nature of employer transitions in the data, we estimate the following logit specification:

$$IT\ Employer_{ijkt} = \beta_1(LBO_{ijt}) + \beta_2(Worker\ Characteristics_{ijt}) + \beta_3(Firm\ Characteristics_{kt}) + \varepsilon_{ijkt},$$

where the subscripts *ijkt* denote person *i* moving from firm *j* to firm *k* after being employed by firm *j* at time *t*. The dependent variable is an indicator for whether firm *k*'s IT labor hiring rate in year *t* is above the 2-digit industry median firm's hiring rate in year *t*; 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 key explanatory variable is LBO_{ijt}, an indicator variable for whether firm *j* is acquired in an LBO while person *i* is an employee. We include a number of control variables describing worker characteristics: binary indicators for race, gender, education, 2-digit occupation, and the year that the worker joins the firm. We also include firm characteristics such as the return on assets, firm size (log assets), capital intensity. Standard errors are clustered at the industry and year level, to account for correlations in the unobserved worker-level attributes that influence employment transitions to a given industry in a particular year.

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 (Tambe and Hitt 2012). If workers acquire transferable skills that complement new information technologies following an LBO, then these workers should exhibit a higher likelihood of transition to firms that have demand for their skills.

The coefficient estimates for our logit specification, reported 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 report results for the full sample. In both specifications, the coefficients are positive and statistically significant, which is consistent with the argument 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, the estimates in column 1 suggest that the odds of a 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 a similar individual employed at a non-acquired firm. The estimates in column 2 suggest 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 activities such as "Decision making and problem solving" ("Analyzing Data and Information") have above-industry median levels of importance as per U.S. DOL classifications. Workers who perform activities that are central to IT-enabled production should be more likely to acquire skills that enable them to transition to firms that have demand for IT-complementary skills. Consistent with this conjecture, estimates of the likelihood that employees transition to firms with demand for IT-complementary skills, reported in columns 3 through 6, are higher for employees who make decisions, solve problems, and process information at LBO targets, relative to similar workers at non-acquired firms (the estimated odds increase from 23% to 119% across all columns).

Long-run wages. Our PE investment spillover hypothesis predicts that LBO workers who perform jobs that have been transformed by PE-led IT during the sample period will acquire transferable human capital. An implication of this hypothesis is that as IT diffuses throughout the economy, the acquisition of this human capital should be associated with a positive effect on wages.

We test this implication by examining long-run wages reported by individuals in our sample. We compare the wages most recently earned by employees of LBO firms with the wages reported by employees of non-LBO firms, by running the following regression:

$$Log(Wage_{it})= \beta_1(LBO\ Treatment_{it}) + \beta_2(LBO\ Treatment_{it}*IT\ Task_{it}) + \beta_3(IT\ Task_{it}) + \nu_i + \omega_t + \varepsilon_{it},$$

where $Wage_{it}$ corresponds to the most recent annual wage earned by person *i* in year *t*, *LBO treatment* is an indicator for whether person *i* has been employed by an LBO target at any time up till time *t*, and $IT\ Task_{it}$ is the O*NET score of the work activity importance associated with the occupation held by the worker. We also include variables describing person *i* at time *t*: binary indicators for race, gender, education, and 2-digit industry of employment. The time characteristics include indicators of the year for which the wage of person *i* corresponds.[12]

The interaction term in the specification can be interpreted as the impact of LBOs on the wages of workers who perform jobs that have been transformed by IT. Table 9 reports results across specifications based on the tasks analyzed in Table 5 (each panel corresponds to a specific task, such as "Analyzing Data and Information"). The column 1 wage estimates in each panel are larger for workers in the sample who perform jobs with higher scores for IT-complementarity, consistent with aforementioned papers on skill-biased technical change. The wage estimates in column 2 are higher still for those workers who perform these tasks and had worked previously at an LBO firm during the sample period. Across all panels, the estimates suggest a wage premium for these workers that ranges from 2.5-3.5% for each additional unit of IT-complementary activity score. The difference in the LBO coefficient estimate between columns 1 and 2 suggests that the link between LBOs and worker wages is heterogeneous across LBO employees. In our sample, it appears that the only LBO workers who realize a wage premium are the ones in jobs that are being transformed by IT.

When we split our treatment sample into workers who transition to firms with high vs. low levels of IT stock, we see some evidence for further disparity of wage premia across workers. The wage premia earned by LBO workers who perform tasks transformed by IT is realized mainly by individuals who work for companies that have high levels of IT stock; the coefficient for the interaction term is positive and significant in column 3, but not in column 4 for Panels A and B. Companies with low levels of IT stock, which presumably have lower demand for IT-complementary work activities within jobs, do not offer a wage premium to similar workers. These figures suggest that workers who acquire IT-complementary human capital, and who are employed by companies that have demand for these skills, earn higher wages.

Collectively, the estimates support the hypothesis that LBOs have a positive impact on the long-run earnings of workers who acquire the skills necessary to be productive in IT-enabled

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[12] We do not use a matching framework to analyze wages because we only observe one wage per person in our sample and thus cannot use prior wage data to match workers. To compensate for this limitation, we use a regression framework, as this enables us to incorporate commonly used controls for prior wages.

work contexts. LBOs do not appear to impact the wages of workers who do not perform such jobs in our sample. The data are consistent with the view that LBOs, during the recent decade of IT adoption, are associated with wage premiums for those workers whose jobs have been most transformed by IT-enabled work practices at PE-owned firms.

V.C. Alternative Explanations and Endogeneity Analysis

In this section, we discuss alternative explanations for the findings. The variation in LBOs that we exploit for identification is inherently non-random, so our empirical analysis is subject to some of the same endogeneity concerns that are endemic to the PE literature. We cannot precisely estimate the effects of unobservable variables that might be missing from our econometric specifications. Therefore, it is difficult for us to test and completely rule out all alternative theories. However, we are able to exploit the detailed nature of our data to examine related corollaries of these theories, and thus qualitatively assess the likely direction and magnitude of biases resulting from various unobservable factors.

1. Endogenous, ex-ante differences between LBO and non-LBO workers

One alternative explanation for our findings is that LBO and non-LBO firms historically employ different types of employees. These differences may result from the ex-ante sorting process by which firms and workers match *prior* to an LBO. For example, PE firms may target companies with higher-than-average quality workforces in order to generate high investment returns. If the matching variables do not sufficiently capture worker quality, these quality differences could explain the observed differences in worker labor market outcomes.

To consider this possibility, we repeat our analysis in Panel A of Table 4 and empirically examine the career paths of workers who complete employment spells at LBO targets *prior* to a PE acquisition and examine their labor market outcomes relative to matched control sample workers. If LBO and non-LBO firms historically produce workers with different levels of ex-post employability—even in the absence of an LBO— then one might expect to see divergent career paths for workers who exit these firms prior to an acquisition.

In contrast to this outcome, however, panel B of Table 4 illustrates that there are no significant differences in our estimates for workers who leave LBO firms prior to an acquisition. Across all specifications, the treatment estimates for this sample are economically small and statistically insignificant. The findings suggest that the observed differences in long-run employability of 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.

2. Endogenous, ex-post differences between LBO and non-LBO workers

Another alternative explanation for the findings centers around the endogenous exit of workers from LBO and non-LBO companies, *after* the LBO takes place. For example, PE-appointed managers may selectively retain workers of high ability, who in turn credibly signal their ability to subsequent employers by remaining at the LBO target for long durations. Alternatively, 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 unlikely to fully account for our findings, for several reasons. First, if PE firms simply screen workers for ability, then presumably all retained LBO employees should realize higher wages, irrespective of their task and employer characteristics. In our data however, the only individuals that earn higher wages after an LBO are the ones who perform tasks that are transformed by IT and who are employed by companies with high demand for IT-skills. Additionally, the evidence we document is driven by LBOs that have high levels of IT investment following an acquisition—not LBOs with little IT expenditure. Second, if mass layoffs cause the average quality of workers exiting LBO firms to be of higher quality than non-LBO workers, then we should likely observe positive treatment estimates for workers who separate from their employers soon after an acquisition. The findings in Table 6 and Figure 4, however, reject this hypothesis.

3. Sample Selection

We also consider whether high quality LBO employees search for jobs at higher rates and are thus more likely to enter our resume sample than high quality non-LBO employees. If such differences in sample selection rates are unaccounted for by variables utilized in the matching strategy, then these differences might lead to overestimates of our treatment effect. To evaluate this concern, we examine the observable characteristics of workers who are employed by LBO and non-LBO firms in our sample.

Table 2 shows that the distributions of observable characteristics of workers in our treatment and control samples are nearly identical. Moreover, Table 2 also shows that these distributions mirror that of workers who complete employment spells at LBO firms *prior* to an acquisition. The similarities in the genders, races, educations, and occupations of individuals across these groups suggest that the LBO event per se does not differentially impact the types of workers that we observe in the treatment and control samples. To the extent that unobservable worker quality is correlated with these observable characteristics, it appears unlikely that worker quality would be systematically different across these groups of individuals.

To further explore selection bias, we utilize our data on the employment status of job seekers at the time that they last update their resume. If LBOs cause workers to utilize the job search site at rates that depend on their employment status (and thus implicitly, their ability) after an acquisition, then our treatment estimates might be biased. We assess this possibility by performing our matching strategy in Table 4 with added controls for employment status as of the last resume update. Even after adding this control, however, we find that our treatment estimates remain the same.

VI. CONCLUSION

We construct and analyze a novel employer-employee matched dataset to empirically measure the impact of PE investment spillovers on workers, in the context of the rapid IT advancements that characterized much of the last decade. Following an LBO during the sample period, we estimate that workers from target firms who acquire IT-complementary skills realize increases in long-run employability and wages relative to what they would have realized in the absence of PE investments. The findings suggest that private equity may benefit the employment prospects of workers who are otherwise limited by their exposure to outdated production methods. The results inform our understanding of the impact of PE on workers, by showing that some workers benefit from PE investments in ways that are typically ignored in policy debates.

Our work points to other avenues for potential research. In particular, we believe that it is important to assess the extent to which private equity, more broadly, plays a role in influencing the diffusion of new production technologies across firms. It is unknown whether private equity serves as a catalyst for target firms to be leaders or laggards in technological adoption within a given industry. Additionally, it would be interesting to understand the value implications of such changes, to help further resolve debate surrounding the broader impact of private equity in the economy.

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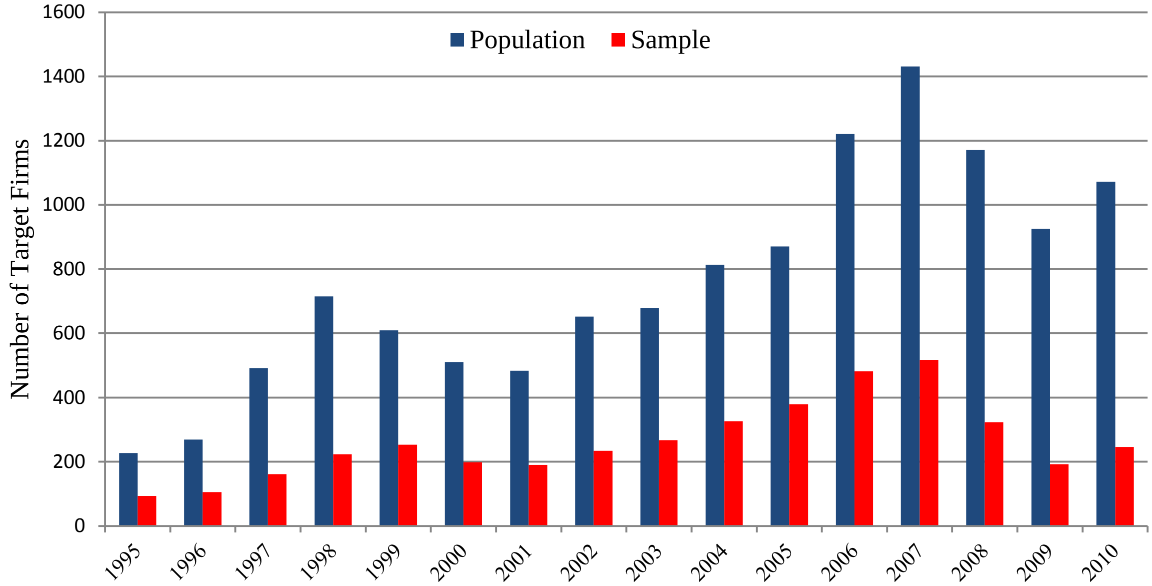
FIGURE 1
POPULATION AND SAMPLE DISTRIBUTIONS OF LEVERAGED BUYOUTS
The top bar chart depicts the number of target firms that appear as employers in our sample, compared to the population of target firms. The vertical axis depicts the number of target firms. The bottom bar chart depicts the total transaction value of the deals involving sample employers, compared to the population. The vertical axis depicts total transaction value, in $MM. The horizontal axis in both charts depicts the transaction years.



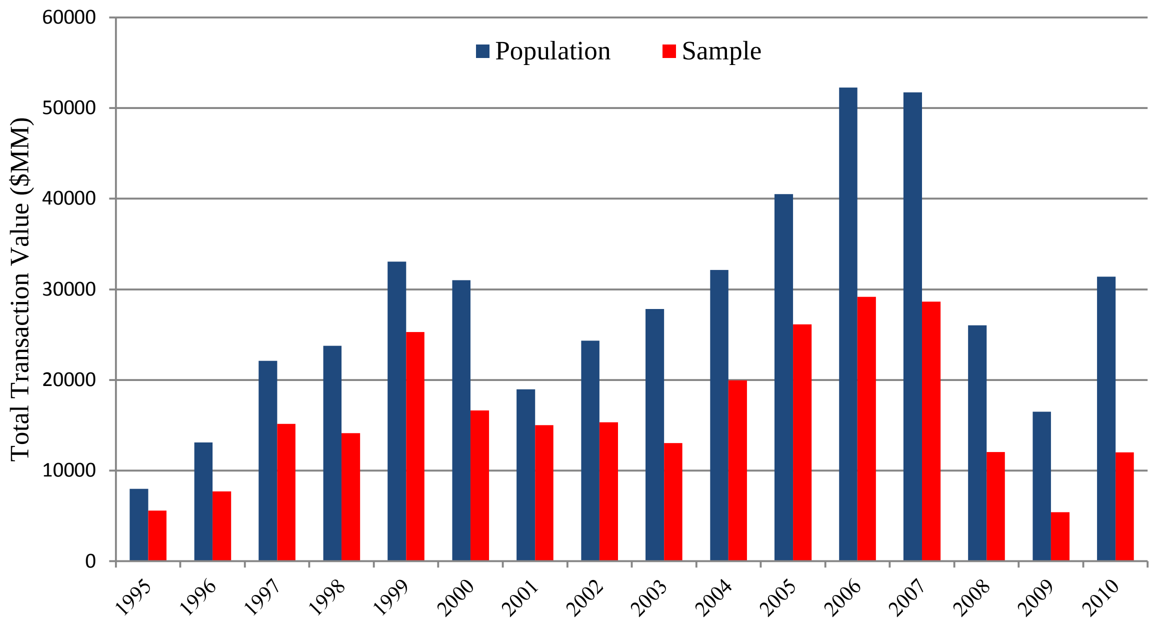


FIGURE 2
POPULATION AND SAMPLE DISTRIBUTION OF LEVERAGED BUYOUTS ACROSS INDUSTRIES

This histogram depicts the frequency of leveraged buyouts across 2-digit SIC major industry groups for the entire population and for our resume sample. Population data comes from Capital IQ. The horizontal axis depicts the industry major group name, the vertical axis depicts the percentage of the total represented by a particular industry (by number of deals). For each industry group, the column on the left corresponds to the population, while the column on the right corresponds to the resume sample.

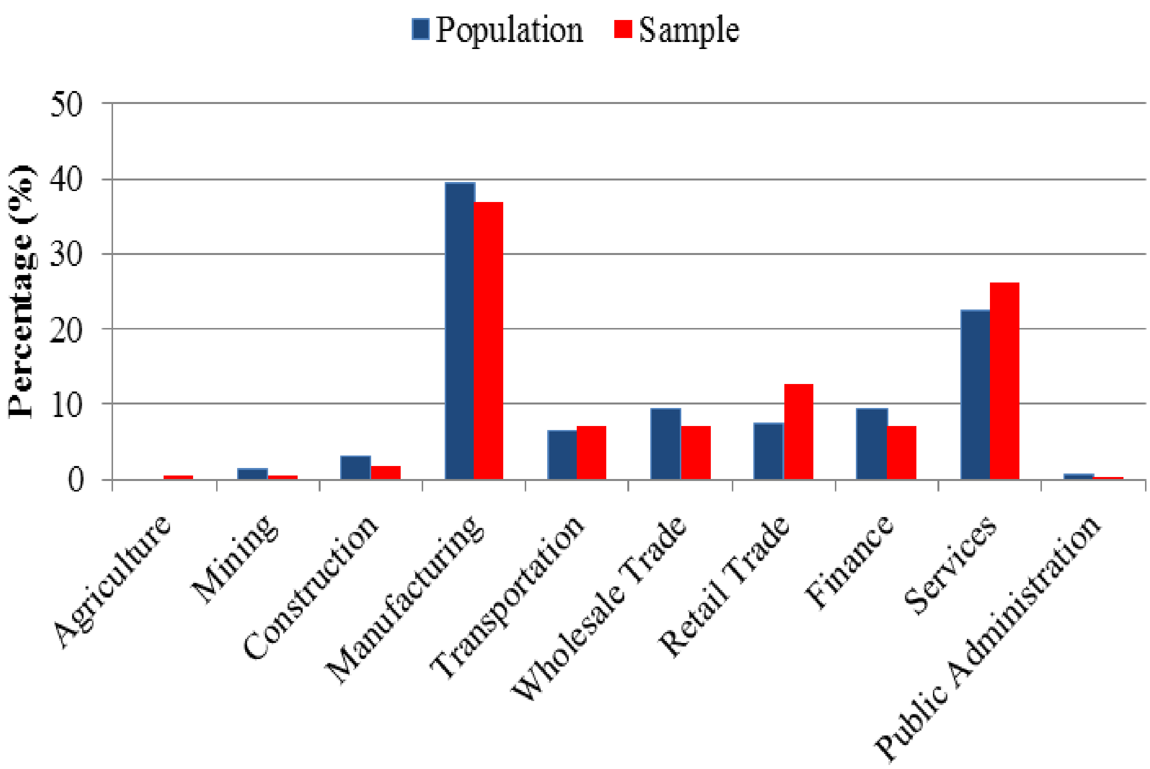
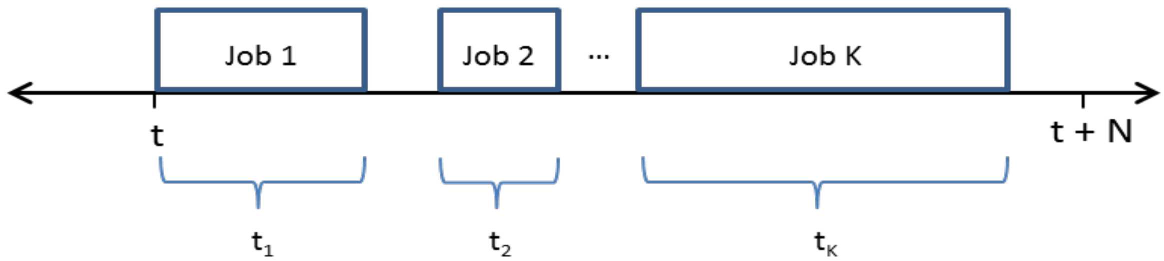


FIGURE 3
LONG-RUN EMPLOYMENT DURATION CONSTRUCTION AND COMPARISON

This figure depicts the construction of our measure of long-run employment duration and presents descriptive statistics summarizing our measure for workers in the treatment and control samples. For any given worker starting a job at time *t*, we define the long-run employment duration for the worker to be the ratio of the sum of all *K* job spell lengths t_i starting at time *t* until time *t*+*N*, divided by *N*, where *N* is the length of time for which the worker's remaining job history is observed and *K* is the number of jobs held between time *t* and time *t*+*N*.



$$\text{Employment Duration} = \frac{\sum_{i=1}^{K} t_i}{N}$$

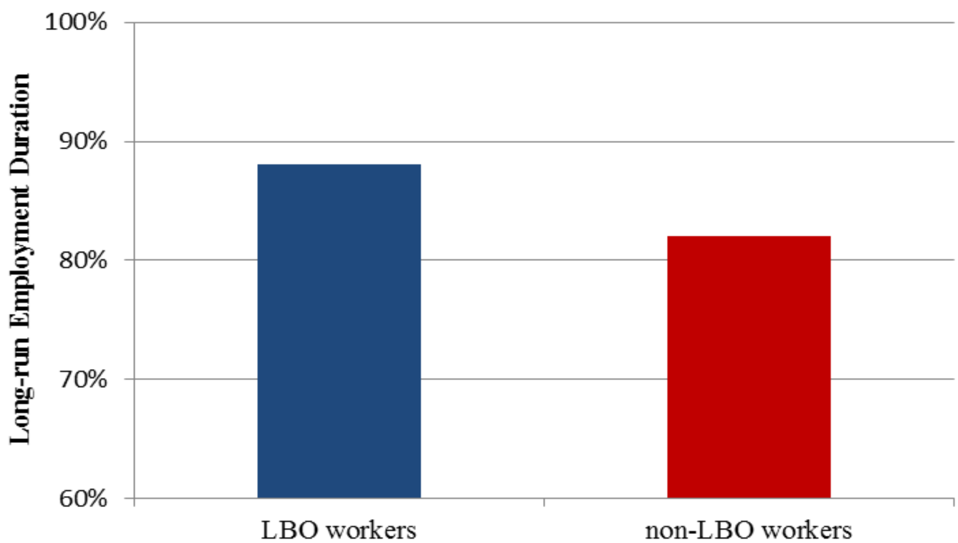


FIGURE 4
DIFFERENCES IN LONG-RUN EMPLOYMENT DURATION FOR LBO VERSUS NON-LBO WORKERS

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.

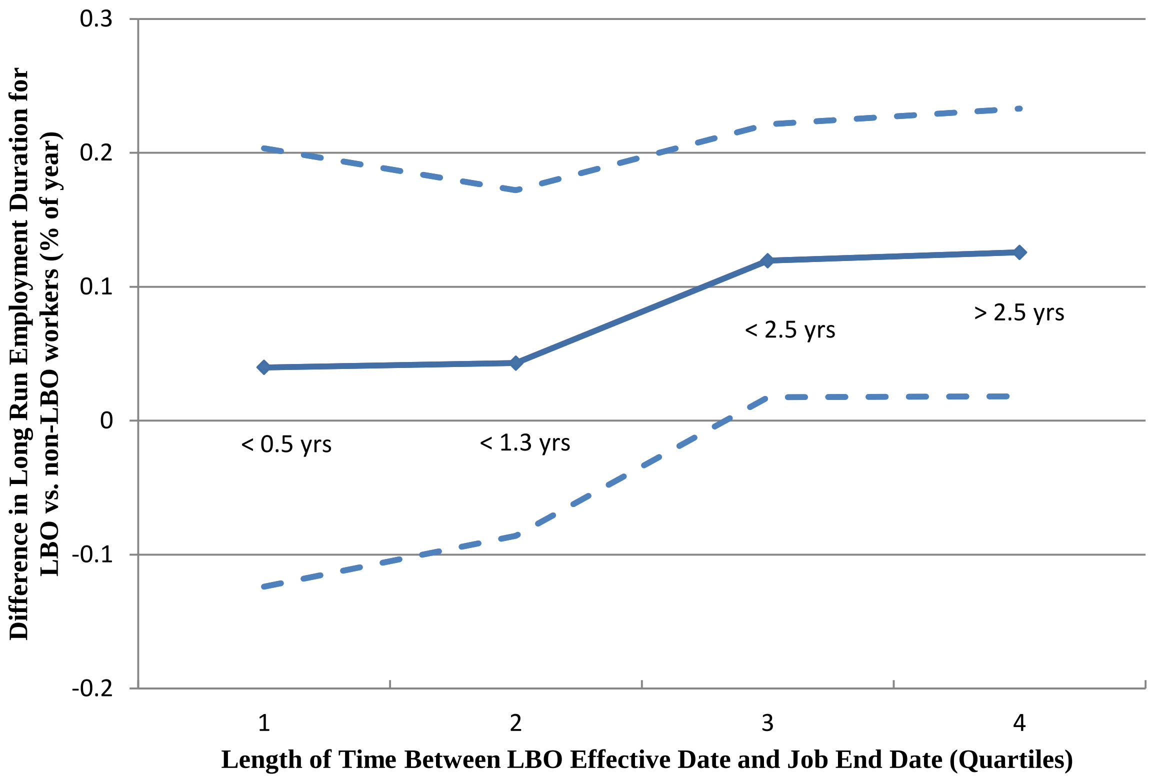


TABLE 1

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. Unemployment Durations are average number of weeks per year that workers are not working, while annual wages are self-reported earnings for the most recent job held by an individual. Total is the number of individuals in the sample.

Category	% Sample	% Labor Force
Panel A: Gender		
Female	52	47
Male	48	53
Panel B: Education		
4-year college	33	21
High School	27	27
2-year	20	19
Graduate degree	10	8
Vocational	9	10
Doctorate	1	2
Panel C: Industry		
Agriculture	0.3	1.6
Mining	0.8	0.5
Construction	2.7	5.7
Manufacturing	18.1	15.8
Transportation	7.6	5.8
Wholesale Trade	5.4	6.0
Retail Trade	17.8	20.0
Finance	15.3	6.4
Services	31.4	32.3
Public Administration	0.7	6.0

Category	% Sample	% Labor Force
Panel D: Occupation		
Management	15.8	4.9
Business	6.1	4.9
Computer	5.2	2.7
Engineering	1.6	1.8
Life Sciences	1.3	0.8
Social Services	1.4	1.4
Legal	1.0	0.8
Education	3.8	6.4
Arts	1.7	1.3
Healthcare	2.3	5.9
Health Support	2.1	3.0
Protective Service	1.3	2.5
Food	3.2	8.9
Maintenance	0.7	3.3
Personal Care	1.3	2.9
Sales	12.6	10.6
Administrative	28.4	16.4
Construction	1.9	3.8
Installation	1.2	3.9
Production	3.0	6.6

	Sample	Labor Force
Panel E: Unemployment Durations		
2005	6.16	6.15
2006	6.42	5.98
2007	6.67	6.27
2008	7.97	8.55
2009	9.59	11.18
2010	11.14	10.66
Panel F: Annual Wages		
Mean	$38,000	$38,337
Median	$33,000	$26,197
Total	202,114	

TABLE 2

TREATMENT VERSUS CONTROL SAMPLE CHARACTERISTICS

This table presents summary statistics describing individuals in the treatment, control, and prior-LBO sample. The treatment sample consists of individuals who have been employed by a company at the time that it gets acquired in a leveraged buyout. The control sample consists of all other workers in the sample. The prior-LBO sample consists of individuals who have been employed by an LBO target but separate from their employer prior to the leveraged buyout. % Treatment , % Control, % Prior-LBO refer to the percentage of individuals in the treatment, control, and prior-LBO 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.

Category	% Treatment	% Control	% Prior-LBO	Category	% Treatment	% Control	% Prior-LBO
Panel A: Gender				*Panel D: Occupation*			
Female	51	52	52	Management	19.2	15.7	16.3
Male	49	48	48	Business	6.3	6.1	4.8
				Computer	6.4	5.1	5.2
Panel B: Education				Engineering	1.9	1.6	1.2
4-year college	34	33	33	Life Sciences	1.0	1.3	1.0
High School	30	27	30	Social Services	0.9	1.4	0.8
2-year	20	20	22	Legal	0.7	1.0	0.6
Graduate degree	10	9	8	Education	2.5	3.9	2.3
Vocational	5	9	7	Arts	1.7	1.7	1.42
Doctorate	1	1	0	Healthcare	1.5	2.4	1.05
				Health Support	1.5	2.1	1.6
Panel C: Industry				Protective Service	1.4	1.3	1.0
Agriculture	0.3	0.3	0.3	Food	3.7	3.2	4.3
Mining	0.4	0.8	0.6	Maintenance	0.6	0.7	0.7
Construction	1.8	2.8	1.8	Personal Care	1.2	1.3	1.2
Manufacturing	21.4	18.0	18.9	Sales	14.6	12.6	13.5
Transportation	7.4	7.6	7.4	Administrative	24.9	28.5	32.9
Wholesale Trade	6.4	5.3	6.1	Construction	1.7	2.0	1.8
Retail Trade	22.9	17.6	24.5	Installation	1.3	1.1	1.2
Finance	10.7	15.4	12.0	Production	3.1	3.0	3.2
Services	28.2	31.5	27.4				
Public Administration	0.5	0.7	0.5	No. of observations	5,680	196,434	4,846

TABLE 3

LEVERAGED BUYOUTS AND INFORMATION TECHNOLOGY INVESTMENT

This table reports OLS regression estimates describing IT investment around LBO events. The dependent variable is the natural log of the quantity of incoming IT workers at a firm in a given year. The explanatory 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.

Panel B: IT Hiring	*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 4

IMPACT OF LEVERAGED BUYOUTS ON WORKER LONG-RUN EMPLOYABILITY

This table reports the matching estimates of the differences in long-run employment durations (annualized) for treated LBO workers and matched non-LBO control workers. Panel A presents estimates of the treatment effect for LBO workers employed by an acquired firm at the time of the LBO transaction (treatment sample A). Panel B presents estimates of the treatment effect for workers who have employment spells at LBO target firms prior to an acquisition (treatment sample B). The control sample for both panels consists of matched non-LBO workers. *LBO* is defined as a binary indicator for whether the individual is in the treatment or control group. 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 by an acquired firm during an 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 an 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 5

IMPACT OF LEVERAGED BUYOUTS ON WORKER EMPLOYABILITY BY CROSS-SECTIONAL EXPOSURE TO INFORMATION TECHNOLOGY

This table reports the matching estimates of the differences in long-run employment durations (annualized) for treated LBO workers and matched non-LBO control workers, across samples of workers facing differing exposures to IT transformation. Panel A presents estimates of the treatment effect for LBO workers employed by an acquired firm at the time of the LBO transaction (treatment sample A). Panel B presents estimates of the treatment effect for workers who have employment spells at LBO target firms prior to an acquisition (treatment sample B). The control sample for both panels consists of matched non-LBO workers. *LBO* is defined as a binary indicator for whether the individual is in the treatment or control group. 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. Task categories for columns 6–8 correspond to U.S. Dept. of Labor survey scores for work activities that have been significantly transformed by IT (across 6-digit SOC codes). *IT-complementary tasks* refer to workers with above (A) versus below (B) median scores for *Analyzing Data and Information, Making Decisions and Problem Solving,* and *Interacting with Computers.* Standard errors are reported in italics. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Firm:			*IT Complementary Tasks:*			*Skills:*
	IT worker hiring rates	Buyout occurs after 2003	PE firms with IT focus	Analyzing Data and Information	Decision making & problem solving	Interacting with Computers	College education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Strong exposure to IT							
LBO	0.172***	0.121***	0.091***	0.140***	0.117***	0.092***	0.120***
	0.052	*0.046*	*0.034*	*0.048*	*0.041*	*0.039*	*0.038*
No. of obs	19,018	19,108	19,193	19,059	19,101	19,105	19,135
Panel B: Weak exposure to IT							
LBO	0.075	0.048	-0.045	0.015	0.017	0.036	0.062
	0.049	*0.038*	*0.069*	*0.039*	*0.044*	*0.047*	*0.061*
No. of obs	19,070	19,041	18,954	19,116	19,074	19,070	19,014

TABLE 6

IMPACT OF LEVERAGED BUYOUTS ON EMPLOYABILITY BY TIME-SERIES EXPOSURE TO IT

This table reports the matching estimates of the differences in long-run employment durations (annualized) for treated LBO workers who separate from their employer after an LBO and matched non-LBO control workers. The treatment sample is split into quartiles, based on the length of time that a treated worker is employed at an acquired firm following an LBO. The control sample consists of matched non-LBO workers. *LBO* is defined as a binary indicator for whether the individual is in the treatment or control group. 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 SPELL LENGTH

This table reports the mean differences in the duration of unemployment (in years) for spells that take place immediately after an individual holds a job at a specific company, for treated workers exiting LBO firms after an acquisition, and matched non-LBO workers. Panel A contains results for the full treatment sample; Panel B contains results for the full sample split across quartiles of time spent at an acquired firm following an LBO. *LBO* is defined as a binary indicator for whether an individual is in the treatment or control group. *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. 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. In Panel A, the specifications also include the following characteristics where indicated: the length of an individual's unemployment spell immediately prior to the matched position, firm assets, return on assets, and capital intensity. In Panel B, the full specification is analyzed. 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.

Panel A: Full Sample	(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

Panel B: Split Sample	*Time quartile:*			
	(1)	(2)	(3)	(4)
LBO	-0.052	-0.193*	-0.138	-0.374**
	0.087	*0.107*	*0.177*	*0.168*
No. of obs.	12,383	12,400	12,383	12,391
Time between LBO and exit	0–6 months	6–16 months	16–30 months	>30 months

TABLE 8

IMPACT OF LEVERAGED BUYOUTS ON WORKER TRANSITIONS ACROSS COMPANIES

This table reports logit estimates of the impact of LBOs on workers' employment transitions across companies. 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 is part of the treatment or control group. 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 IT-transformed 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***
	0.081	0.372	0.109	0.598	0.109	0.843
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

TABLE 9

IMPACT OF LEVERAGED BUYOUTS ON LONG-RUN WAGES

This table reports regression estimates of describing the relationship between LBOs, IT-complementary tasks, and workers' long-run wages. Worker wages are self-reported earnings for the most recent job listed on an individual's resume. IT Task is the U.S. DOL O*NET occupational score for IT complementary tasks performed by the individual: *Analyzing Data and Information* (Panel A), *Making Decisions and Problem Solving* (Panel B), *Interacting with Computers* (Panel C). *LBO* is defined as a binary indicator of whether an individual ever worked for a firm that gets acquired in an LBO, and *LBO×IT* is an interaction term of LBO and the IT task score. Control variables in all columns include the following characteristics: indicator variables for individual race, gender, education, and industry (2-digit SIC code), year fixed effects for the last year of the job for which the earnings correspond, and the log of the years of labor market experience up until the wage reporting year. Treatment Sample denotes the types of firms for which wages are included in the specification for treated individuals: All, High, and Low IT Firms, where High and Low refers to above and below treatment sample median IT employment levels as a fraction of total firm employment. Number of obs. denotes number of observations in each column. Standard errors are reported in italics. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)
	Panel A:Analyzing Data and Information			
IT Task × LBO		0.025**	0.130**	0.045
		0.011	*0.055*	*0.028*
IT Task	0.161***	0.160***	0.159***	0.159***
	0.002	*0.002*	*0.002*	*0.002*
LBO	0.080***	-0.002	-0.381**	-0.060
	0.010	*0.036*	*0.189*	*0.093*
	Panel B: Making Decisions and Problem Solving			
IT Task × LBO		0.034***	0.114**	0.051
		0.012	*0.052*	*0.032*
IT Task	0.185***	0.184***	0.184***	0.184***
	0.003	*0.003*	*0.003*	*0.003*
LBO	0.074***	-0.065	-0.392	-0.125
	0.010	*0.049*	*0.257*	*0.126*
	Panel C: Computer Interaction			
IT Task × LBO		0.026**	-0.005	0.044
		0.012	*0.047*	*0.038*
IT Task	0.085***	0.083***	0.083***	0.083***
	0.002	*0.002*	*0.002*	*0.002*
LBO	0.089***	0.003	0.057	-0.050
	0.002	*0.040*	*0.164*	*0.123*
Treatment Sample	All	All	High IT Firms	Low IT Firms
Number of Obs.	39,489	39,489	38,098	38,214