Private Equity, Technological Investment, and Labor Outcomes

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Abstract

This paper uses proprietary data on the employment histories of a large fraction of the U.S. labor force to show that labor market outcomes can be partly explained by previous employers' technology investment decisions. Exploiting leveraged buyouts as shocks to firms' production technologies, we find that workers employed when a private equity acquisition occurs realize longer subsequent employment tenures, reductions in short run unemployment durations, and higher rates of within-occupation mobility over their careers. The effects are especially pronounced for workers in occupations that are complementary to IT-enabled work practices, workers who can be trained to acquire new skills quickly, and for workers who are retained at the acquired firm for longer durations prior to exit. The findings suggest that corporate investment in information technology imparts transferrable, task-specific human capital to workers.

Key words: Corporate Governance, Private Equity, Information Technology, Technological Change, Human Capital

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

1. Introduction

Rapid advancements in information technology (IT) over the past 30 years have significantly impacted the nature of corporate investment. Improvements in IT have substantially altered the operations of the firm in a number of ways—for example, by enabling supply chain efficiencies and facilitating communications among employees. Accompanying these changes has been divisive debate over how firms' IT investments impact employees. Much research indicates that investment in technology leads to aggregate job loss and aggregate job creation. However, we have little understanding of the extent to which IT investment creates new human capital[1], and how this impacts employee outcomes following major changes to firms' information technologies. This question has recently become particularly important in the wake of an apparent technical 'skills gap' that some have argued is responsible for the current employment problems faced by many US workers.

This paper examines the hypothesis that corporate investments in information technology impart transferable, *task-specific* human capital to workers. When firms invest in new technologies, employees who work with these new technologies acquire human capital through training and through on-the-job learning. These new skills enable workers to perform certain tasks with greater efficiency, and are valuable because they provide benefits to workers *even after* they leave the firm. This hypothesis builds upon recent work by Acemoglu and Autor (2011) who argue that task-specific human capital is an important dimension of worker skill and hence a potentially critical determinant of labor market outcomes.

Empirical identification of the impact of corporate investment in information technology on workers' labor market outcomes is difficult for two reasons. The first is data limitations: there

[1] One exception is Tambe and Hitt, who provide evidence for a link between technological investment and human capital acquisition in the IT labor force (forthcoming).

is no comprehensive dataset that contains detailed information on both firm characteristics (such as investments in information technology) and worker characteristics (such as labor market outcomes).[2] A second problem is that even if such data were available, there is a challenging identification problem: it is difficult to estimate the causal impact of firm investment on worker outcomes and to distinguish this effect from other important factors, such as the endogenous selection of workers into firms with different investment patterns.

To address these difficulties, we use proprietary data from one of the largest online job search companies in the U.S. to construct a novel employer-employee matched panel dataset that contains detailed information on the career paths of U.S. workers, with in-depth information on their employers over time. The data allows us to track the long-run labor outcomes of workers who are employed by specific employers during the sample period. We identify shocks to firm information technologies by exploiting variation in private equity activity. A large body of research finds that the most recent wave of leveraged buyouts (LBOs) is characterized by information technology changes at the acquired firm (Kaplan and Stromberg 2009; Amess et. al 2007; Bloom et. al 2009). We examine how workers fare following an LBO, and compare their labor market outcomes to similar workers at comparable firms that do not get acquired in LBOs.

We find that employees retained by firms acquired in LBOs realize greater long-run employment durations in subsequent years than similarly matched workers at comparable firms. Differences in employment durations are especially pronounced for college-educated workers who perform tasks complementary to IT-enabled production methods. We also observe that the length of employment duration at the acquired firm matters for subsequent labor market outcomes: employees retained after an LBO appear to benefit from human capital acquisition

[2] Datasets such as Compustat have detailed information about firm financial characteristics, but lack information about the workers employed by these companies. On the other hand, many datasets such as the Current Population Survey or the Census contain information on workers alone, without corresponding information about employers.

much more than workers who exit the firm soon after acquisition (and thus do not acquire human capital as a result of the PE takeover). The findings are consistent with the view that corporate technological investment imparts transferable, task-specific human capital to workers whose tasks complement new production techniques, and that this human capital can explain long-term labor market outcomes.

The identification assumption that is central to causally interpreting the findings is that PE investment in technology is independent of unobservable factors that impact worker labor market outcomes. Although this assumption is fundamentally untestable, we conduct several analyses to empirically evaluate the extent to which confounding factors may bias our estimates of the impact of PE investment on worker outcomes. Collectively, the results of our analyses indicate that our findings are best explained by the channel of IT upgrades leading to valuable human capital acquisition by employees retained after an LBO.

2. Data

We construct a novel panel dataset that contains detailed information on the individual career paths of a large fraction of the U.S. labor force. Our dataset also contains detailed information on the (time-varying) characteristics of firms that employ these workers. In contrast to employer-employee matched datasets commonly used in other studies (most notably the Longitudinal Employer-Household Dynamics (LEHD) data compiled by the U.S. Census Bureau), we have detailed information on the specific nature of the occupations and job titles held by employees, and we can track worker movements both *within* and *across* firms in great depth (LEHD do not contain data on workers' occupations or job titles).[4]

[4] Other commonly used datasets that have detailed information about individual workers, such as the Current Population Survey (CPS), do not contain information that can be used to identify workers' employers.

We assemble this information into a panel dataset that contains information about individual worker background, education, and employment characteristics. One of the key novelties of our data is that we are able to identify the occupation held by the employee as per the U.S. Department of Labor's (DOL) Standard Occupational Classification (SOC) system.

We link our panel dataset of worker characteristics to data on employer characteristics. We standardize the free-text employer name that appears on worker resumes and use fuzzy text-matching algorithms to link employer names to company identifiers in Capital IQ.[6] Capital IQ contains detailed, time-varying information about firms such as annual balance sheet and income statement data. The data available in Capital IQ corresponds to both public and private firms. Capital IQ also contains detailed information about whether a company gets acquired in a leveraged buyout during the sample period. For each company that appears in our linked database, we identify whether it gets successfully acquired in a merger or acquisition that Capital IQ classifies as a leveraged buyout. Figures 1 through 6 and Table 1 present sample statistics from the data and compare them to other administrative datasets with known sampling properties, but these comparisons are not discussed here due to space limitations.

3. Analysis

3.1 Labor Market Outcomes

3.1.1 Empirical Framework

To measure the impact of PE investment in information technology on workers' labor market outcomes[7], we use the nearest-neighbor matching methods developed by Imbens and Abadie (2006). This non-parametric estimation method allows us to identify the treatment effect

[6] 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.

[7] A longer version of this paper provides evidence that PE acquisition is associated with technological investment (see Table 3). Also, see Bloom et al (2009).

without requiring (potentially arbitrary) assumptions about the functional form describing the relationship between firm characteristics and subsequent worker labor market outcomes. This estimator is especially well suited to our setting because our control sample is significantly larger than our treatment sample; we can accurately estimate the average treatment effect for the treated (ATT) in our sample because we are able to identify many potential observations from the control group that can be matched to each observation in the treatment group.

We estimate the treatment effect using matching methods rather than regression methods because a regression framework produces biased estimates of treatment effects for dependent variables that are censored and have non-normally distributed error terms (as is common for measures of employment and unemployment durations). Moreover, regression methods require functional form assumptions about the empirical relation between firm characteristics and labor market outcomes--there is no prior work that precisely establishes what the correct functional form should be, so whatever form we choose will be somewhat arbitrary. To the extent that the functional form we choose is mis-specified (relative to the true model), it is likely that the resulting estimates of the treatment effect will be biased by specification error. Misspecification is likely to be especially costly in our setting, because the distribution of explanatory covariates in the control sample may be significantly different from the distribution of explanatory covariates in the treatment sample; this difference could potentially exacerbate the estimation bias due to model misspecification (see Imbens and Wooldridge (2009) for further discussion).

We are most interested in estimating the average treatment effect for the treated (ATT), rather than the average treatment effect for the entire sample (ATE) or the average treatment effect for the controls (ATC). Estimating the ATE or ATC is problematic because of differences in the size and characteristics of the control and treatment samples. In order to estimate the ATE or ATC, it would be difficult to find the requisite observations in the treatment sample that could be used as matches for observations in the control sample. In turn, poor matching between the treatment and control sample could severely bias the estimated ATE or ATC[8].

To identify the impact of PE investment on labor market outcomes, we define our treatment sample by identifying workers who have been directly impacted by LBO's during their tenure at a given company. For each job record in our dataset, we can identify the start and end dates for a particular job title held by an individual worker at a given company. We use Capital IQ to identify whether the company gets acquired in a LBO at any time between the start and end dates of the job held by the individual. We construct our treatment sample by aggregating all such instances, and identify the set of workers in our data employed at a company when a leveraged buyout becomes effective. We can identify 5,285 such workers in our sample.[9]

To construct our control sample, we draw from our remaining pool of workers who do not experience an LBO during their careers. There are 875,756 such workers. For each worker in our treatment sample, we use the nearest-neighbor matching algorithm to identify (at least) four matches from our control sample.[10] As a starting point, we match on 2-digit SIC industry, 2-digit SOC occupation, gender, race, level of education, years of prior labor market experience, and the year in which the individual started the specific position held during the LBO transaction. We weight the start year by a factor of 1,000 relative to other covariates, to ensure that we are comparing workers who join a particular position across firms at the same time, so as to control for differences in accumulated human capital over time.[11] Table 2 contains statistics describing the characteristics of workers in our treatment and control sample using this matching scheme.

[8] See Abadie, et al. 2004; Imbens 2001; and Heckman, Ichimura, and Todd (1998) for related discussion.
[9] In this version of the paper, we examine a random subsample of the total data available in the database; the results are similar for smaller subsets of data. The next version of this paper will contain results for a larger sample.
[10] The nn-match procedure developed by Abadie and Imbens allows for "ties"; if multiple control observations are equidistant from a given treatment observation, all observations are used with the appropriate weighting matrix.
[11] The results are not sensitive to this weighting scheme. Estimates using equal weighting yield similar results.

As illustrated across various characteristics, the workers in the treatment and control sample are remarkably similar across many dimensions. The distribution of race, gender, and education of workers in the treatment sample mirrors that of the matched control sample.

3.2.1 Employment Tenure

We utilize the Abadie and Imbens (2006) matching procedure to estimate the mean differences in employment durations for workers who leave companies acquired by PE firms and for workers who leave companies that do not get acquired by PE firms. For each worker in our treatment and control sample, we define *Post Employment Duration* as the length of time (in years) between the start date of a given job title and the end date of the last job held by a given worker. Thus, *Post Employment Duration* is the length of time that elapses between the date that a treated worker joins a firm that gets acquired in an LBO, and the date that the worker ends her last known job spell. This measure is annualized, to allow for comparisons across pairs of workers with different career lengths. For each 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 employment duration for the workers across the two groups. We estimate the ATT by finding control matches for every observation in the treatment sample; we do not identify corresponding treatment matches for every observation in the control sample (which would otherwise allow us to compute the ATC and/or the ATE).

Table 4 presents the ATT estimates for various combinations of worker and firm characteristics used to match individuals across both samples. In Panel A, the treatment effect is shown for workers employed at the acquired firm at the time of the LBO transaction (treatment sample A). Panel B presents estimates of the treatment effect for workers who are employed at the acquired firm but leave prior to the LBO taking place (treatment sample B). The control sample for both panels consists of all workers who never work for firms that get acquired in an LBO. *LBO Treatment* is defined as a binary indicator for whether the individual works for a firm that gets acquired in an LBO. Across all specifications, workers are matched on individual person and firm characteristics: race, gender, education, occupation, starting year of the position held at the time of the treatment, years of labor market experience up until the starting year, total years of observed employment history, and firm industry. Across various specifications, we also match workers based on firm characteristics such as *Assets* (defined as the book value of firm assets), *Return on Assets* (defined as the ratio of operating earnings to assets), *Capital Intensity* (defined as the ratio of net plant, property, and equipment to assets), and individual characteristics such as *Unemployment Duration* (defined as the length of an individual's unemployment spell immediately prior to the matched position).

Column 1 of Panel A illustrates that the mean difference in annualized employment tenure for workers who leave LBO firms is approximately 0.105 years longer than similarly matched workers who leave comparable firms. Adding additional variables to improve the matching precision in columns 2 through 5 does not materially change the treatment estimate. The annualized difference in employment durations ranges between 0.087 years and 0.105 years. The results support the view that PE investment has a significant impact of workers; in the long run, it appears that workers who exit LBO firms appear to have longer employment tenure at subsequent establishments.

We also assess whether differences in employment durations are driven by the LBO event, rather than systematic differences in employment durations for workers who leave firms that become acquired vs. workers who leave other, comparable establishments that do not get acquired. Panel B presents treatment estimates for workers who join and leave firms that eventually get acquired, prior to the LBO actually taking place. As illustrated across columns 1 through 5 of Panel B, the treatment estimates are all economically small and statistically insignificant, indicating that the observed differences in employment durations for workers existing LBO firms is causally linked to the LBO itself.

The findings in Table 4 suggest that PE investment is associated with changes in labor market outcomes. These results, alone, however, do not sufficiently illustrate whether the link between PE activity and worker labor market outcomes is through technology investment imparting skills onto workers. In order to illustrate this link more clearly, we focus on workers in occupations that are known to complement IT enabled work practices, following the extensive literature on productivity and organizational practices. For each worker's job title, we identify the individual's specific Standard Occupational Classification (SOC) code, at the 6-digit level. We then match the occupation to the U.S. Department of Labor's (DOL) Survey of Work Activities to approximate the tasks that are expected of a worker within a particular class of occupation. For each task, the DOL assigns scores corresponding to the amounts of activity required by the task. For example, managers (SOC code 11-1010) have high mean scores for "Guiding, directing, and motivating subordinates" (a work category defined by the DOL), whereas motor vehicle operators (SOC code 53-3000) have low mean scores for this category.

Table 5 presents the estimates of the LBO treatment effect on workers split across various categories. Column 1 illustrates, for example, that workers with educational attainments of four-year college degrees or higher appear to be much more affected by PE investment in IT than workers with lower educational attainment levels. The estimated difference in annualized employment duration for college-educated workers is 0.149 years, while the estimated difference in annualized employment duration for other workers is only 0.055 years. This finding is consistent with Hitt and Brynjolfsson (1997), who present evidence that IT improvements appear to benefit college educated workers relatively more than non-college educated workers. The reason for this difference is that college educated workers presumably have the skills to more quickly adapt to new production technologies, thus facilitating their relatively higher rates of human capital acquisition and benefiting in the labor market accordingly.

Column 2 indicates that workers in occupations that are highly data processing intensive also appear to benefit from private equity investments (treatment effect estimate of 0.149 years), relative to workers in occupations with low levels of data processing needs (treatment estimate of 0.054 years). These occupations complement IT enabled work practices, as improvements in computing power facilitate more efficient task completion for data intensive work. Similarly, column 3 shows that workers involved in tasks that require decision-making and problem-solving appear to differentially benefit from PE investments. These are also the types of workers who benefit from information technologies that improve decision-making efficiency. It is likely that these technologies allow for greater discretion to decisions makers within firms, a finding that is consistent with a large literature on IT work practices (discussed in Section 2).

To further illustrate the link between PE investment and complementary work practices, columns 4 and 5 indicate that the labor market outcomes of workers for whom it is important to coordinate work activities and guide subordinates (essentially managers) benefit relatively less than workers who are more directly involved in the production process and could benefit from greater discretion in day-to-day activities – precisely the types of tasks that benefit from improvements in IT investment. Below-median scores for both work categories are associated with treatment effects of 0.184 and 0.175, respectively.

Finally, we show that the effects of PE investment on worker employment tenure are especially strong for workers who are retained after the LBO event. We separate workers into quartiles based on the length of time elapsed between the LBO event date and the date when they exit the firm. 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. Figure 8 illustrates that workers who exit the firm in the first two quartiles do not have statistically different employment tenures from similar workers who exit comparable firms. The only workers with significantly longer employment tenures are those retained by the acquired firm for 1.3 years or more. The results are consistent with the view that PE investment imparts human capital to workers; the effects of the LBO on worker employment tenure appear only to be present for workers retained at the firm long enough to work with new production processes and employed for a long enough time to acquire human capital.[12]

3.2.2 Occupational Mobility

We also look at the occupational transitions that workers make after leaving firms that have been acquired in LBOs. If PE investments in technology cause workers to acquire transferrable skills, then it is likely that workers who leave firms acquired in LBO's are less likely to have to switch occupations and more able to transition within the new firms that they join, relative to workers who leave companies that have not been acquired by PE firms.

To evaluate these hypotheses empirically, Table 7 presents results for probit specifications of job transition characteristics on the LBO treatment and various measures of individual and firm characteristics. Specifically, we control for the various measures of worker

[12] In a longer version of this paper, we show that these effects are partially driven by shorter unemployment durations after leaving the acquired firm (see Table 6), but these results are omitted here due to length constraints.

and company traits that we use in the matching estimation procedures in the above Sections (Tables 4 through 6). First, we estimate the effect of the LBO treatment on the probability of transitioning across different occupations for workers leaving LBO firms vs. workers who leave non-LBO firms (the dependent variable is a binary indicator for whether a worker is able to remain in the same occupation, defined at the 2-digit SOC level).

Column 1 of Table 7 illustrates that workers who leave LBO firms are more likely to maintain employment in the same occupation held the acquired firm, whereas similarly matched workers leaving comparable firms take up occupations that are different from that of their previous employers. The estimated treatment effect is 0.105, which illustrates a 10% difference in the relative likelihood of maintaining the same occupation as held earlier. Similarly, across all other columns in Table 7, as we add controls for various firm characteristics (to control for the possibility that occupational transitions might be impacted by the type of business that a worker is exiting), we find that the probability of staying within occupation is the same. To show that this effect is driven by the LBO, rather than a firm specific attribute associated with companies acquired in LBO's, we estimate the treatment effect for workers who exit treated firms prior to the LBO, and find no significant differences in occupational transition likelihoods for these workers relative to the control sample. Therefore, the evidence supports the view that LBO's impact the acquisition of task-specific capital and consequently a worker's ability to maintain employment within a given occupation.

3.3 Identification

The identification assumption that is central to a causal interpretation of the findings is that PE investment in technology is independent of unobservable factors that impact worker labor market outcomes. We conduct several analyses to evaluate the extent to which confounding factors may bias our estimates of the impact of PE investment on worker outcomes. Collectively, the results of our analyses indicate that our findings are best explained by the channel of IT upgrades leading to valuable human capital acquisition by employees retained after an LBO.

3.3.1 Sorting of workers into firms.

We show that our findings cannot be explained by unobserved differences in the abilities of workers who sort *into* firms acquired by PE firms and workers who sort *into* firms that do not get acquired by PE firms. Positive assortative matching in the labor market suggests that workers of low ability sort into poorly performing firms, while workers of high ability sort into companies that have strong performance (Becker 1973). This theory has found empirical support in a number of studies in the labor economics literature (Abowd et. al 2009). Since PE firms typically target underperforming companies for leveraged buyouts (Kaplan and Stromberg 2009), it is likely that firms acquired by PE investors employ workers with lower levels of ability than firms that do not get acquired by PE firms. Thus, it is unlikely that the relatively longer employment tenures of workers who leave LBO targets can be explained by superior ability. Rather, to the extent that there is positive assortative matching between workers of heterogeneous ability and poorly performing firms that receive PE investment, our estimates actually understate the impact of PE investment on worker labor market outcomes.

The results in Panel B of Table 4 also illustrate that our findings cannot be explained by the effects of workers sorting into LBO vs. non-LBO targets based on unobservable ability. If LBO targets attract higher quality workers, then differences in labor market outcomes should be observed for all workers who join an LBO target. However, the results in Panel B indicate that workers who join and leave eventual LBO targets prior to an acquisition actually have labor market outcomes that are statistically indistinguishable from the outcomes realized by individuals employed elsewhere. The data appear to contradict the view that LBO targets systematically employ workers of higher ability than other firms.

Moreover, the results depicted in Figure 8 illustrate that the effects of PE investment on workers are heterogeneous across workers who have varying employment tenures after an LBO takes place. If workers of acquired firms are of higher average ability than workers employed elsewhere, then significant differences in long run employment spells should be observed for all workers who leave the LBO target, not just for those individuals who remain at the firm for more than 1.3 years. These findings indicate that differences in labor market outcomes for employees who leave acquired firms and individuals employed elsewhere cannot be explained by the endogenous sorting of workers into firms based on unobservable ability.

3.3.2 Sorting of workers out of firms

We also show that our findings cannot be explained by unobserved differences in the abilities of workers who sort *out of* firms acquired by PE firms. An alternative explanation of the findings is that PE firms may close various divisions of a newly acquired company, leading to mass layoffs of workers with high ability who do not fit into the operational plans of the company's new owners. Other workers who leave establishments for idiosyncratic reasons could be of lower average quality as such separations reveal low ability that has been directly observed by managers. Thus, potential employers can surmise that workers displaced by PE acquisitions are of higher ability than individuals who leave firms that do not experience mass layoffs.

This hypothesis is unlikely to explain our findings. First, if workers who leave firms are displaced by mass layoffs and have higher ability than observably similar workers who separate from comparable firms, one would expect to find significant differences in the labor market outcomes for workers who leave immediately after the LBO takes place. The findings in Figure

8, however, are inconsistent with this theory. Workers who separate from an acquired firm within the first 1.3 years after the PE firm takes control have long run employment tenures that are no different than the tenures of comparable workers who separate from other companies. These workers are especially among the most likely to be displaced through mass layoff, as empirically approximately half of the separations at PE acquisitions take place within two years of the buyout (Lerner et al, 2011). Moreover, PE firms typically seek to reap returns from their investments within 5-10 years (Kaplan and Stromberg 2009), so it is likely that mass layoffs take place soon, rather than many years later, after a company gets acquired.

Second, it is not obvious that the abilities of workers who leave an acquired firm are superior to the abilities of individuals employed elsewhere. One of the ostensible efficiencies that PE firms introduce to the firms they acquire is the ability to identify workers of low ability and dismiss them from the target firm. Under this hypothesis, one would expect that the labor outcomes of employees that leave LBO targets would reflect inferior ability and be worse than the outcomes for other workers. Our findings to the contrary suggest that the true impact of PE investment on labor outcomes is larger than what we are able to estimate in this paper.

4. Conclusion

We find that following an acquisition, PE firms invest heavily in IT use. Retained employees who perform tasks complementary to these new IT-based production practices experience improved labor outcomes in the form of longer employment durations and improved within-occupation mobility. There is much debate over the impact of corporate policies on the subsequent labor market outcomes of workers. These findings suggest that at least in the current period characterized by rapid technological change, firms play a role in human capital acquisition, and thus, the labor outcomes of the firm's workers in both the short and long run.

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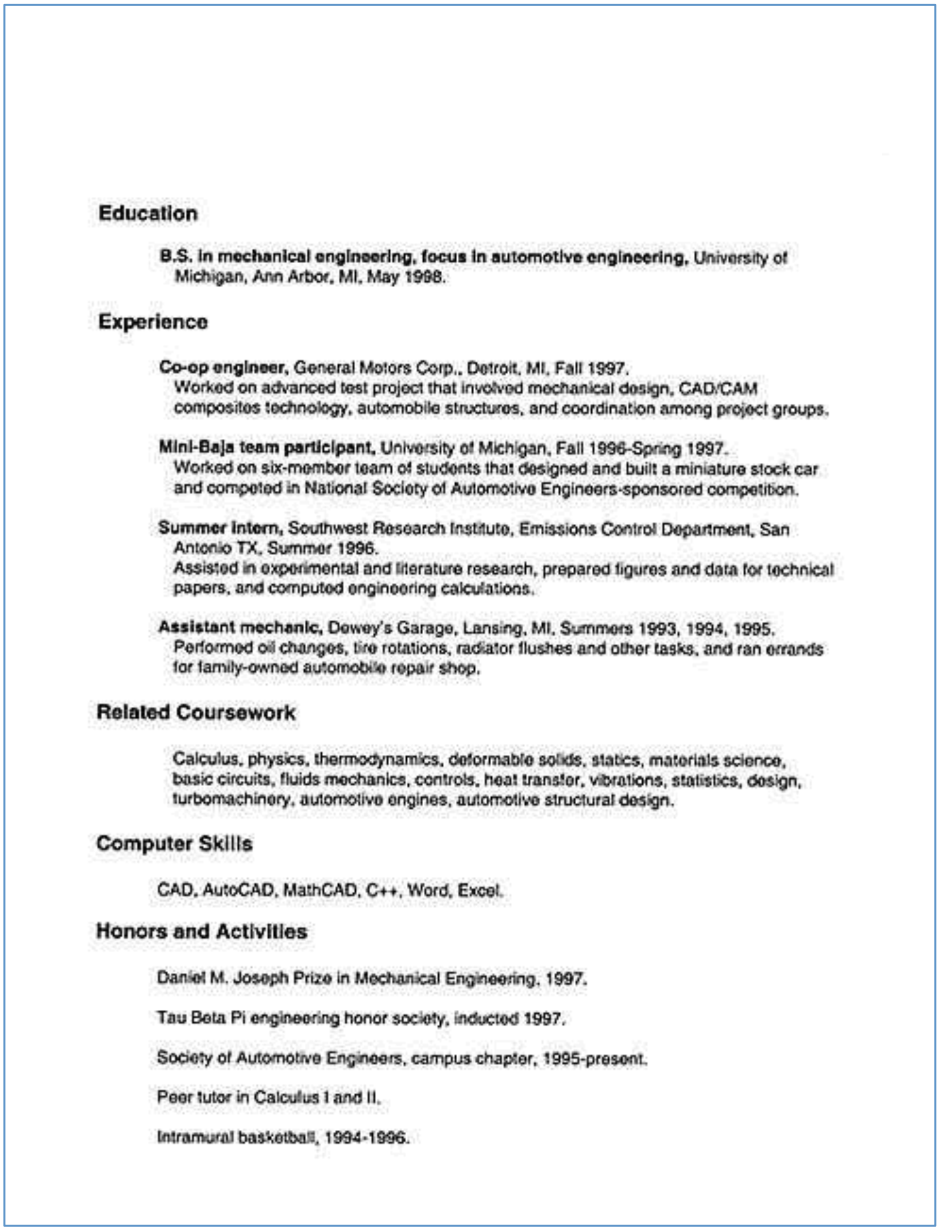
Figure 1: Sample Resume



Education

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

Experience

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

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

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

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

Related Coursework

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

Computer Skills

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

Honors and Activities

Daniel M. Joseph Prize in Mechanical Engineering, 1997.

Tau Beta Pi engineering honor society, inducted 1997.

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

Peer tutor in Calculus I and II.

Intramural basketball, 1994-1996.

Figure 2: Sample Data Tables

Employee Data					
Employee	Education	Experience	Manager	Wage	Wage Type
I.T. Worker	4 Years College	60 months	N	60,000	Yearly
I.S. Manager	4 Years College	120 months	Y	100,000	Yearly

Employee Work History Data				
Employee	Employer Name	Job Title	Start Date	End Date
I.T. Worker	Firm Name 3	Project Manager	5-01-2006	Present
I.T. Worker	Firm Name 2	Software Engineer	9-01-2003	3-15-2006
I.S. Manager	Firm Name 2	Director of Technology	4-01-2006	Present
I.S. Manager	Firm Name 1	MIS Manager	1-01-2006	3-20-2006

Figure 3. Distribution of Education Levels for Sample Dataset vs. U.S. Population

This figure depicts the distribution of education levels across workers in our sample dataset, relative to the distribution of education levels across workers in the U.S. population. Estimates for the U.S. population are computed using data from the Current Population Survey (CPS).

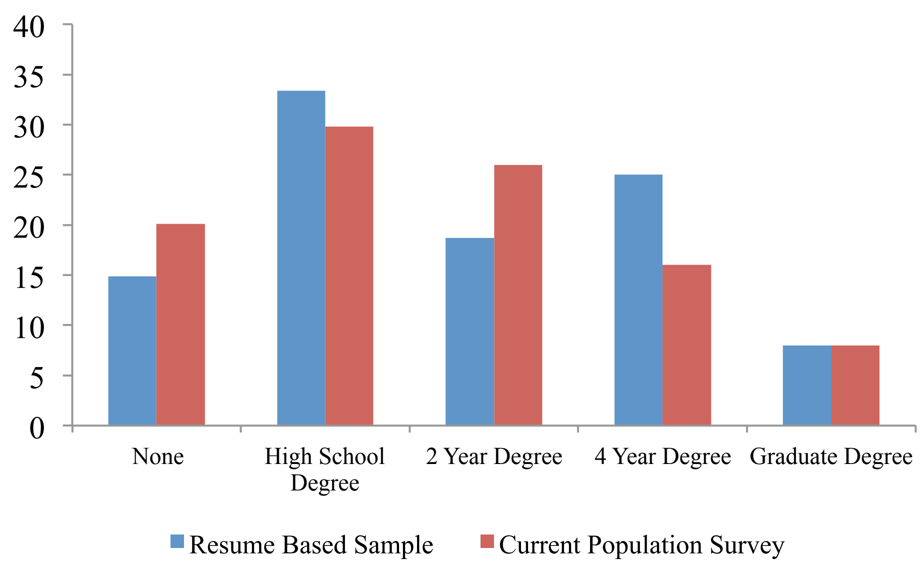


Figure 4. Distribution of Occupations for Sample Dataset vs. U.S. Labor Force

This figure depicts the distribution of occupations across workers in our sample dataset, relative to the distribution of occupations across workers in the U.S. labor force. Estimates for the U.S. labor force are computed using Occupational Employment Statistics from the Bureau of Labor Statistics (BLS).

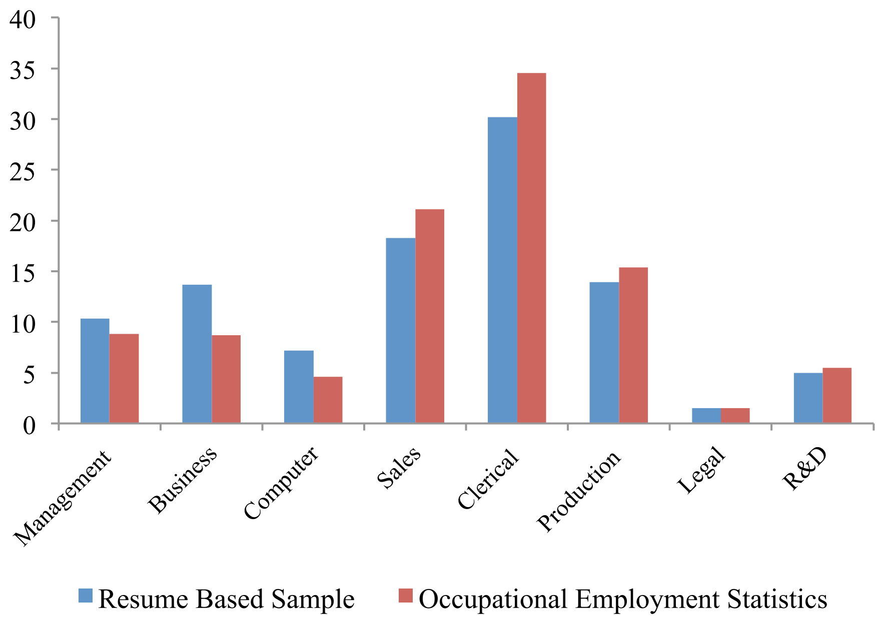


Figure 5. Distribution of Education for IT workers in Sample vs. U.S. Labor Force

This figure depicts the distribution of education levels across workers in our sample dataset, relative to the distribution of education across workers in the U.S. population. Estimates for the U.S. population are computed using data from the Current Population Survey (CPS).

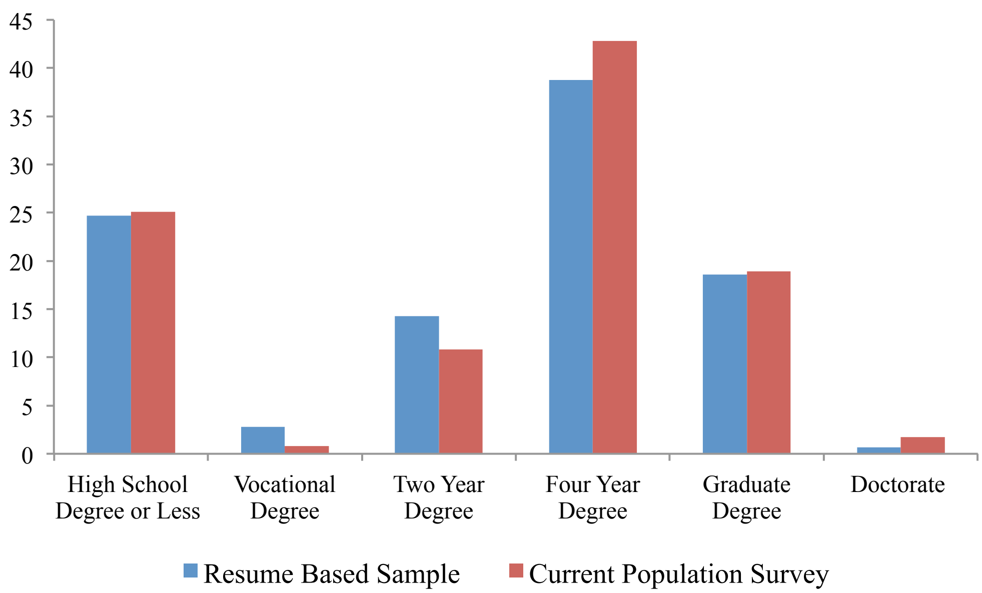
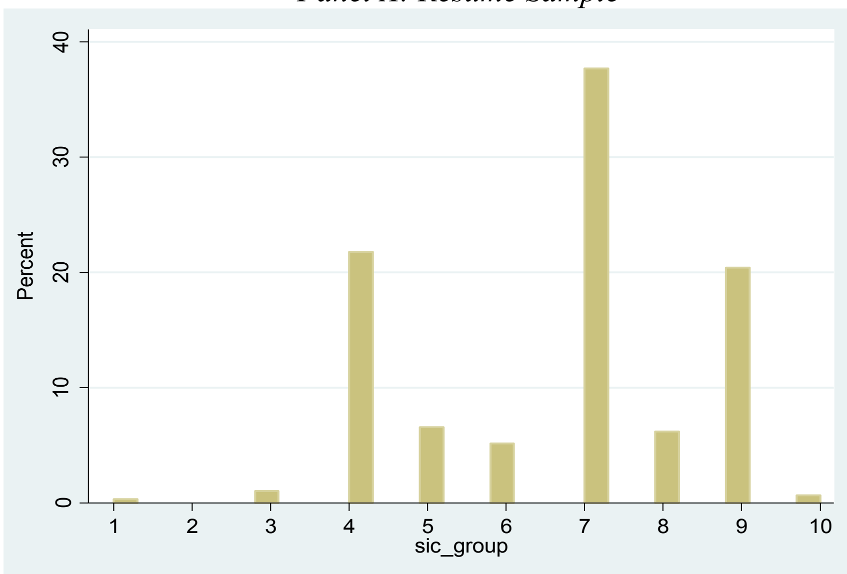


Figure 6: Distribution of Industry Employment

This pair of histograms depicts the frequency of employment by 2-digit SIC industry code for workers in the sample and the U.S. labor force (as calculated from the 2012 CPS March supplement). Group 1 corresponds to SIC major group A, Group 2 corresponds to SIC major group B, etc.

Panel A: Resume Sample



Panel B: CPS sample

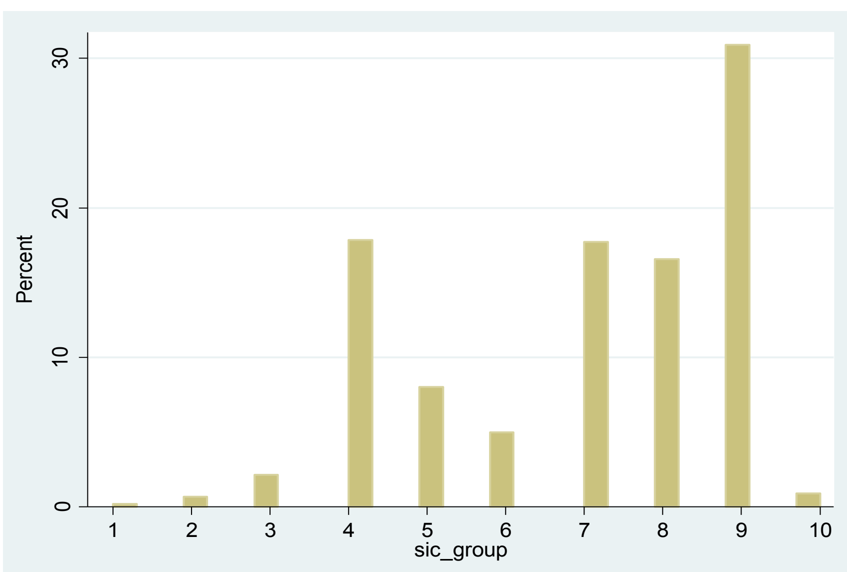


Figure 7: Distribution of Sample LBO's across Industries

This histogram depicts the frequency of sample LBO events by major industry group. (Group 1 corresponds to SIC major group A, Group 2 corresponds to SIC major group B, etc.).

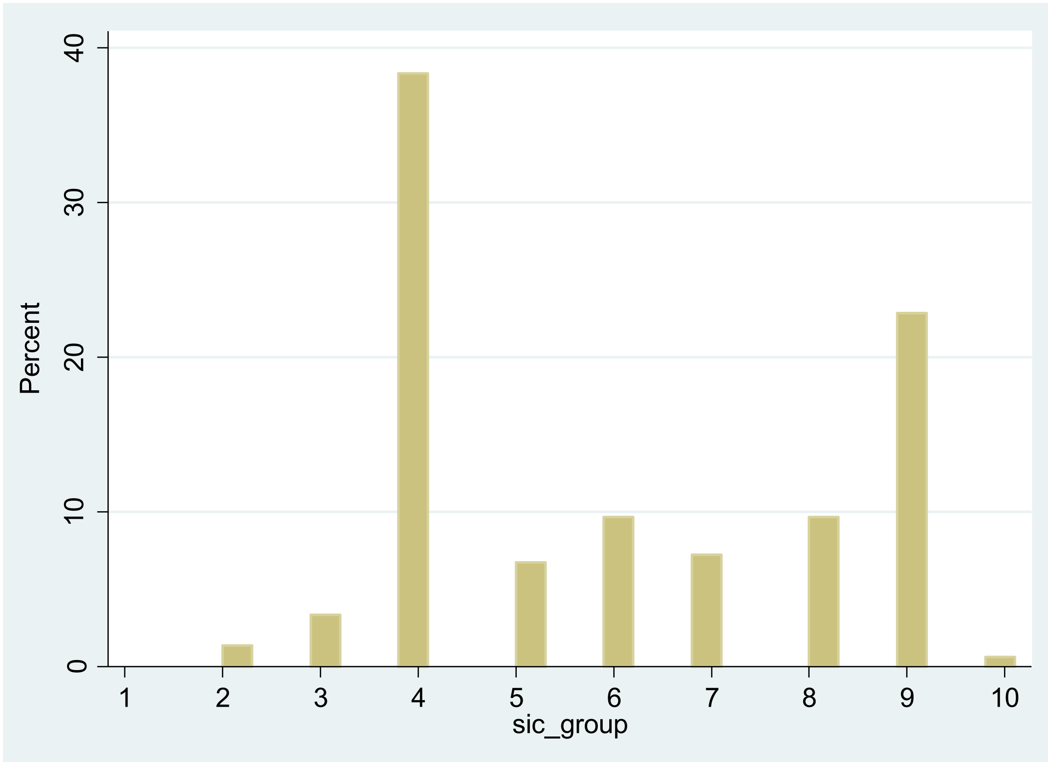


Figure 8. Differences in Long Run Employment Durations for LBO vs. non-LBO workers
This figure depicts the differences in long run employment durations (annualized) for workers in the treatment sample (LBO workers) vs. workers in the matched control sample (non-LBO workers). For all workers in the treatment sample, we compute the distribution of the time elapsed between the LBO effective date and the date of job exit. 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 for1.3 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.

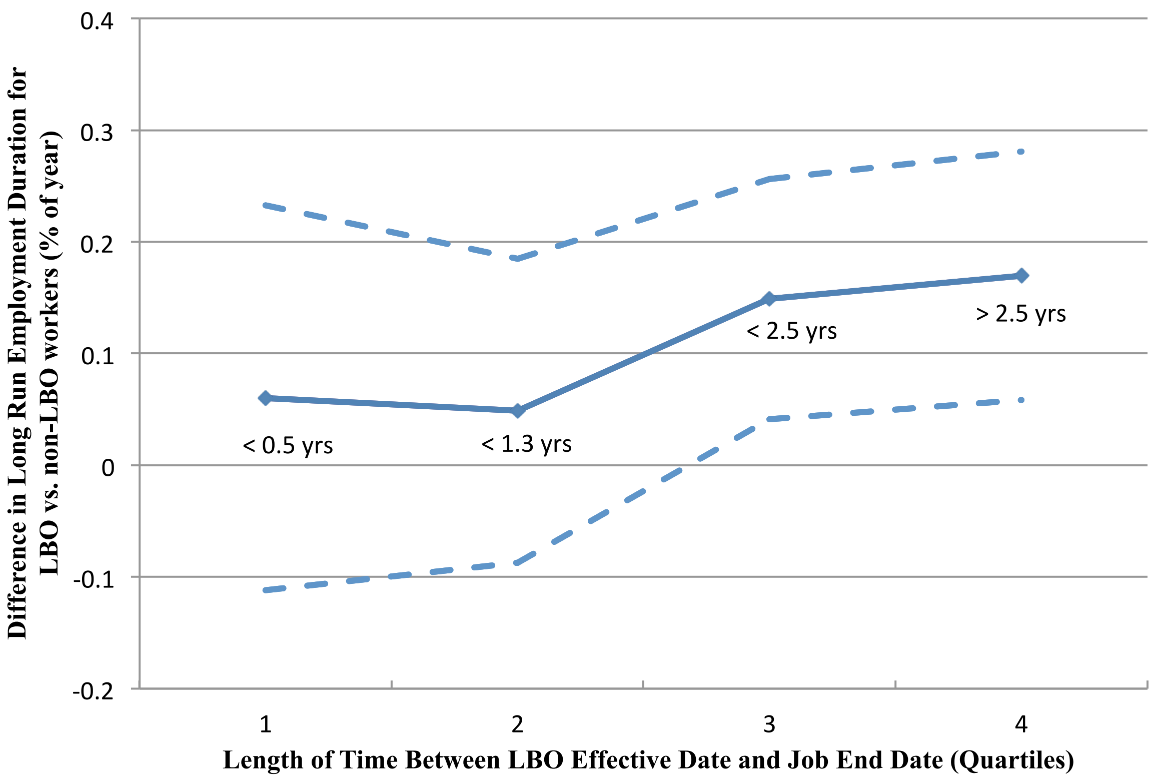


Table 1. Sample Descriptive Statistics

This table presents summary statistics describing the characteristics of the sample dataset, and for comparison, the characteristics of the U.S. labor force (taken from the 2012 March CPS Supplement). Number (% Sample) refers to the number (percentage) of individuals in the sample dataset for which data is available. % CPS Sample refers to the percentage of individuals in the U.S. workforce as of 2012 with the following attributes (estimates are weighted by individual supplement weights).

	Number	% Sample	% CPS Sample
Panel A: Gender			
Female	67,405	52%	47%
Male	62,252	48%	53%
Panel B: Education			
4 year college	42,256	33%	21%
High School	34,657	27%	27%
2 year	24,815	20%	19%
Graduate Degree	12,475	10%	8%
Vocational	11,112	9%	10%
Doctorate	1,254	1%	2%
Total	202,114		

Table 2. Treatment vs. Control Characteristics

This table presents summary statistics describing the characteristics of the workers who leave firms that are acquired in LBO's vs. workers who do not leave firms acquired in LBO's. % Sample refers to the percentage of individuals in each group for which data is available.

	% Treatment Sample	% Control Sample
Panel A: Gender		
Female	51%	52%
Male	49%	48%
Panel B: Education		
4 year college	34%	33%
High School	30%	27%
2 year	20%	20%
Graduate Degree	10%	9%
Vocational	5%	9%
Doctorate	1%	1%

Table 3: Impact of Leveraged Buyouts on IT flows

This table reports estimates of the impact of LBO's on the annual flow of IT labor into the firm using data on IT labor investment at the firm level on sample firms from 1995 through 2010. The dependent variable in both columns is the log of the quantity of incoming IT workers at the firm in a given year. Column (1) includes controls for whether or not the firm was ever an LBO target during our sample years and a dummy variable (*post-LBO*) indicating whether the firm has already been acquired through an LBO, as well as year fixed-effects. Column (2) includes both year and firm fixed-effects. Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	Log(IT flows)	Log(IT flows)	Log(IT flows)	Log(IT flows)	Log(IT flows)
Years	1995-2010	1995-2010	1995-2000	2000-2010	2003-2010
Post-LBO	.0675*	.0383**	.0183	.0622**	.103**
	.0353	*.0162*	*(.0534)*	*(.0197)*	*(.0257)*
LBO target y/n	.0314				
	.0295				
Controls	Year	Year Firm Effects	Year Firm Effects	Year Firm Effects	Year Firm Effects
Observations	143,360	143,360	46,362	107,342	76,718

Table 4. Impact of Leveraged Buyouts on Worker Employment Duration (Long Run)

This table reports the mean differences in long run employment durations (annualized) for workers of firms acquired in leveraged buyout (LBO) transactions and similarly matched workers at firms that are not acquired in LBOs. Panel A presents estimates of the treatment effect for workers employed at the acquired firm at the time of the LBO transaction (treatment sample). Panel B presents estimates of the treatment effect for workers who are employed at the acquired firm but leave prior to the LBO taking place (treatment sample). The control sample for both panels consists of all workers who never work for firms that get acquired in an LBO. *LBO Treatment* is defined as a binary indicator for whether the individual works for a firm that gets acquired in an LBO. Across all specifications, workers are matched on individual person and firm characteristics: race, gender, education, occupation, starting year of the position held at the time of the treatment, years of labor market experience up until the starting year, total years of observed employment history, and firm industry. 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 *Unemployment Duration* (defined as the length of an individual's unemployment spell immediately prior to the matched position). Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)
Panel A: Workers who are employed at acquired firm at the time of the LBO transaction					
LBO Treatment	0.105***	0.094***	0.087***	0.087***	0.093***
	0.034	*0.028*	*0.028*	*0.028*	*0.026*
No. of obs.	19,296	27,640	28,321	28,326	34,110
Panel B: Workers who leave acquired firm prior to LBO transaction					
LBO Treatment	0.016	0.000	0.002	0.002	-0.007
	0.019	*0.015*	*0.014*	*0.014*	*0.014*
No. of obs.	17,789	25,653	26,296	26,301	31,723
Match Variables:					
Assets	x		x	x	x
Return on Assets		x	x	x	x
Capital Intensity				x	x
Unemployment Duration					x

Table 5. Impact of Leveraged Buyouts on Employment Duration – By Skill and Occupation

This table reports the mean differences in long run employment durations (annualized) for workers of firms acquired in leveraged buyout (LBO) transactions and similarly matched workers at firms that are not acquired in LBOs. Panel A presents estimates of the treatment effect for workers employed at the acquired firm at the time of the LBO transaction (treatment sample). Panel B presents estimates of the treatment effect for workers who are employed at the acquired firm but leave prior to the LBO taking place (treatment sample). The control sample for both panels consists of all workers who never work for firms that get acquired in an LBO. *LBO Treatment* is defined as a binary indicator for whether the individual works for a firm that gets acquired in an LBO. Across all specifications, workers are matched on individual person and firm characteristics: race, gender, education, occupation, starting year of the position held at the time of the treatment, years of labor market experience up until the starting year, total years of observed employment history, and firm industry. 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. 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 *Unemployment Duration* (defined as the length of an individual's unemployment spell immediately prior to the matched 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)
	College education	Processing Information	Making Decisions/ Problem Solving	Coordinating Work Activities	Guiding, directing, and motivating subordinates
Panel A: Workers in occupations above the median					
LBO Treatment	.149***	.149**	.120**	.035	.037
	.040	*.053*	*.043*	*.039*	*.040*
No. of obs.	12,672	19,149	19,193	19,198	19,205
Panel B: Workers in occupations below the median					
LBO Treatment	.055	.054	.056	.184**	.175**
	.062	*.039*	*.046*	*.051*	*.051*
No. of obs.	6,624	19,214	19,170	19,165	19,158

Table 6. Impact of Leveraged Buyouts on Worker Unemployment Duration (Short Run)

This table reports the mean differences in unemployment durations immediately after an individual holds a job title at a specific company, for workers of firms acquired in leveraged buyout (LBO) transactions and similarly matched workers at firms that are not acquired in LBOs. *LBO Treatment* is defined as a binary indicator for whether an individual holds a position at the time when her employer gets acquired in an LBO. Across all specifications, workers are matched on individual and firm characteristics: individual race, gender, education, occupation at the time of the LBO, starting year of the position held at the time of the LBO transaction, years of labor market experience up until the starting year, and firm industry. 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 *Return on Assets* (defined as the ratio of operating earnings to assets), *Assets* (defined as the book value of firm assets), *Capital Intensity* (defined as the ratio of net plant, property, and equipment to assets), and *Prior Duration* (defined as the length of an individual's unemployment spell immediately prior to the matched position). *LBO Status* denotes whether the LBO transaction was effective or cancelled. Standard errors are reported in italics underneath the coefficient estimates. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
LBO Treatment	-0.194***	-0.193***	-0.185***	-0.198***	-0.171***	
	0.048	*0.048*	*0.048*	*0.058*	*0.056*	
LBO Treatment (Prior)						0.074*
						0.040
No. of obs.	24,006	24,003	23,387	15,371	14,968	13,804
Match Variables:						
Assets	x	x	x	x	x	x
Return on Assets		x	x	x	x	x
Capital Intensity			x		x	x
Prior Duration				x	x	x

Table 7. Impact of Leveraged Buyout on Worker Occupational Mobility

This table reports probit estimates of the impact of LBO's on worker occupational mobility. The dependent variable is a binary indicator of whether a worker maintains the same occupation (measured at the 2-digit Standard Occupational Classification (SOC) level)) when transitioning across employers. *LBO Treatment* is defined as a binary indicator for whether the individual transitions from a firm after the firm gets acquired in an LBO. *LBO Treatment (Prior)* is defined as a binary indicator for whether the individual works for a firm and then leaves the firm prior to a LBO. The control sample across all specifications consists of all workers who never work for firms that get acquired in an LBO. All specifications include controls for individual person and firm characteristics: indicator variables for race, gender, education, occupation, years of labor market experience, and indicators for firm industry (2-digit SIC). Where indicated, additional variables used in various specifications include *Assets* (defined as the book value of firm assets), *Return on Assets* (defined as the ratio of operating earnings to assets), and *Capital Intensity* (defined as the ratio of net plant, property, and equipment to assets). 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)
LBO Treatment	0.105**	0.254**	0.254**	0.254**	
	0.053	*0.109*	*0.109*	*0.109*	
LBO Treatment (Prior)					0.036
					0.063
No. of obs.	58,126	13,943	13,940	13,603	7,873
Additional Covariates:					
Assets		x	x	x	x
Return on Assets			x	x	x
Capital Intensity				x	x