Broad-based Employee Stock Ownership: Motives and Outcomes

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Abstract

Firms initiating broad-based employee share ownership plans often claim ESOPs increase productivity by improving employee incentives. Do they? The answer depends. Small ESOPs comprising less than 5% of shares, granted by firms with moderate employee size, increase the economic pie, benefitting both employees and shareholders. The effects are much weaker when there are too many employees to mitigate free-riding. Although some large ESOPs increase productivity and employee compensation, the average impacts are small, because they are often implemented for non-incentive purposes, such as conserving cash by substituting wages with employee shares or forming a worker-management alliance to thwart takeover bids.

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A number of studies argue broad-based employee stock ownership (BESO) can increase productivity by improving worker incentives, team efforts, and co-monitoring among workers.[1] Consistent with this argument, earlier event studies show positive stock price reactions to the announcement of adopting employee stock ownership plans (ESOPs), except when they appear as a means to entrench management.[2] However, the literature on BESO has important voids. It does not offer evidence on how it affects employees – the agents BESO plans target – nor does it provide comprehensive evidence on how BESO plans in the U.S. affect productivity.

Therefore, we ask: Are employees better off as a result of BESO plans? Is their sole purpose to improve employee incentives? Maybe some plans are motivated to conserve cash by issuing stocks to employees in return for lower wages, or to help thwart hostile takeover bids. Even where the intent is to improve worker incentives, it is not obvious the stock price alignment scheme will work because of the potential free-rider problem. When there are many employees, individual workers may feel they have little impact on the stock price and may not exert extra effort. Finally, if firms overcome the free-rider problem and improve worker productivity, how is the surplus divided between employees and employers? Do employees benefit at all?

To answer these questions, we investigate how adopting a BESO plan affects employee compensation, shareholder value, the level of employment, and total factor productivity, as well as possible motives behind their adoption. For identification

[1] See for example, Jones and Kato (1995), Blasi, Conte, and Kruse (1996), Freeman, Kruse, and Blasi (2010) about employee share ownership, and Kandel and Lazear (1992) about profit sharing.

[2] When ESOPs appear to be anti-takeover devices, previous studies shows the market reaction is either neutral or negative. See Gordon and Pound (1990), Chang and Mayers (1992), Chaplinsky and Niehaus (1994), Beatty (1995), and Cramton, Mehran, and Tracy (2007).

purposes, we choose a specific type of BESO plans – ESOPs, which provide a clear, sharp difference in BESO before and after the initiation.

Our questions illustrate the various issues concerning ESOPs are intertwined and complex. To simplify analysis, we separate ESOPs into four types: small and large ESOPs by "numerous" and "not-so-numerous" employee firms. Numerous-employee firms are those in the top quartile in total number of employees among our sample of publicly listed firms with ESOPs.[3] These firms are highly susceptible to free-rider problems. Small ESOPs are defined as those never controlling more than 5% of the firm's outstanding stocks. If ESOPs are implemented to conserve cash or prevent takeover, firms are likely to implement larger ESOPs, because small ESOPs are not very useful for those purposes. We conjecture small ESOPs are less confounded by non-incentive purposes and provide a cleaner sample to study the incentive effects.

We obtain wage and employment data from the U.S. Bureau of Census, which provides micro-data on employee payroll at the establishment (workplace) level. We begin by examining changes in cash wages associated with ESOP adoptions by panel regressions using a control group of non-ESOP establishments. Cash wages include all forms of taxable ordinary income, such as regular paychecks, bonuses, and commissions, but do not include the value of ESOP shares granted. Hence, our cash wage estimates underestimate changes in total compensation.

We find small ESOPs adopted by not-so-numerous-employee firms are associated with improved productivity. On average, cash wages increase by 20% and industry-adjusted Tobin's Q by 21% following these ESOP adoptions. The two main claimholders

[3] We cannot disclose the exact number of employees at the top quartile due to the Census confidentiality policy. We obtained permission to disclose the mean of the firms that fall between the 65th and 85th percentile, which provides a similar estimate to the sample-wide third quartile. This value is 15,500 employees.

to firm surplus are better off, implying higher productivity. In addition, these ESOP adoptions are followed by increases in employment and in the number of establishments. To provide more direct evidence, we limit our sample to manufacturing firms and follow Schoar (2002) in estimating total factor productivity. Consistent with the wage and Q results, we find a significant increase in TFP at not-so-numerous-employee firms after the adoption of a small ESOP.

If these ESOPs lead to productivity gains, the gains will be shared between employees and shareholders according to their relative bargaining power. We measure employee bargaining power by one minus the Herfindahl index of employer concentration within each industry and geographic location of work place. We find that at not-so-numerous-employee firms adopting small ESOPs, wage gains are greater and shareholder gains are smaller, the greater the employee bargaining power. These results are not driven by pre-existing conditions or anticipated changes in worker bargaining power at the time of adopting ESOPs. We also examine timing-varying variables, such as pre-ESOP growth opportunities or pre-ESOP firm performance, which may be related to future performance and affect the decision to adopt an ESOP, or the choice between a small or large ESOP. Our results are robust to these selection issues.

When not-so-numerous-employee firms adopt large ESOPs which control more than 5% of the firm's outstanding shares at any point in time (ESOPg5) they show no significant effects on cash wages, shareholder value, or employment. However, cash wages do not include the value of ESOP shares. The average market value of shares granted through ESOPg5 in our sample is $26,796 (in 2006 dollars) per employee, equal to 5.06% of annual wages if the shares were allocated equally over 10 years. When this

value is taken into account, large ESOPs also seem to increase total employee compensation. Our total factor productivity estimates also indicate an increase in productivity. However, the overall evidence suggests large ESOPs at not-so-numerous-employee firms are associated with smaller productivity gains than small ESOPs.

The smaller gains can be explained by heterogeneity in motives behind initiating large ESOPs. Some large ESOPs seem to be implemented to conserve cash by substituting cash wages with ESOP shares. Employees cannot sell ESOP shares until they leave the company or are close to retirement age, preventing adequate diversification. They will value ESOP shares less than the market value; hence, issuing shares through ESOPs is more costly than issuing shares in the open market unless the issuer is cash constrained and has limited access to external financing. Our data show wage gains following ESOPg5 adoptions are significantly and negatively related to financial constraints at the time of the ESOP adoption.

This finding on cash-conservation motivated ESOPs adds to the empirical debate on whether cash-constrained firms are more likely to have broad-based stock option (BBSO) plans. Core and Guay (2001) argue they do, whereas Ittner, Lambert, and Larcker (2003) argue they do not, and Oyer and Schaefer (2005) find mixed evidence. ESOPs are similar to BBSO plans in that they both grant equity stakes to employees on a broad base. Our evidence supports the argument that for cash-constrained firms, cash conservation is an important motive in issuing equity instruments to employees.

Another motive unrelated to improving employee incentives is the worker-management alliance suggested by Pagano and Volpin (2005): Management bribes employees with higher wages to garner worker support to thwart hostile takeover bids.

Some states have business combination statutes (BCS) that impose a temporary moratorium on takeover bids if a sufficiently large block of shareholders unaffiliated with management, such as a large ESOP, vote against the bids. If a firm's state of incorporation has such a BCS, the firm may adopt a large ESOP as an anti-takeover device and increase wages to garner worker support. We find higher wages following the adoption of such ESOPs by numerous-employee firms. The unearned wages impose costs on shareholders, a previously undocumented cost of using ESOPs for managerial entrenchment. This result complements the previously documented cost of reducing the probability of takeovers via ESOPs (Chaplinsky and Neihaus, 1994; and Rauh, 2006).

Numerous-employee firms' adoption of ESOPs has a neutral effect on cash wages and shareholder value.[4] We attribute the neutral effect to the free-rider problem and to the initiation of large ESOPs for anti-takeover purposes. However, changes in both cash wages and factor productivity are significantly and positively related to the ESOP size. Larger ESOPs seem to help mitigate free-rider effects at larger firms.

In addition to these findings, this paper provides a new way to measure the relative bargaining power between employers and employees. This measure may help study the role of bargaining power in determining various issues concerning implicit and explicit contracts between suppliers of labor and capital, which are of considerable interest in labor economics.

The rest of the paper is organized as follows. Section I develops hypotheses and outlines test designs, followed by data description in Section II. Employee compensation

[4] The difference between numerous and not-so-numerous-employee firms is consistent with Hochberg and Lindsey (2010), who find the positive relation between broad-based stock option plans and performance is confined to small employee firms.

is analyzed in Section III. Section IV examines the interactive effects of worker bargaining power and addresses selection issues. Evidence on firm valuation, factor productivity, and employment is presented in Section V. Section VI examines non-incentive motivated ESOPs. Section VII summarizes.

I. Hypotheses Development and Test Outlines

The initial focus in this section is whether ESOPs help improve productivity by enhancing worker incentives. Then we examine other motives for large ESOPs: conserving cash and forming a worker-management alliance.

A. Institutional Details

ESOPs are a type of tax-qualified pension plan (see Beatty, 1995). To qualify for tax benefits, the plan must allocate shares in a broad-based manner. Usually, all full-time employees must be included in the plan. To prevent discriminatory share allocation, shares must be allocated equally to all employees or based on relative compensation, seniority, or some combination thereof. ESOPs are not bonus plans because they are deferred compensation allocating control rights and cash flow rights to employees, creating a new class of stakeholders, owner-employees. In public corporations, ESOPs must allow employees to vote their allocated shares as would any other holders of the security.[5] This right to vote applies only to allocated shares or shares that have been distributed to individual employee accounts. Shares bought for the ESOP but unallocated are open to multiple voting options. The only specific rule regarding voting of unallocated shares is that the voting rights must be specified in the original ESOP trust

[5] Employees must be allowed to vote if an ESOP is created with voting shares; if the shares are non-voting, employees have no voting rights.

agreement.[6] ESOP shares are typically held by employees for an extended duration because employees cannot sell the shares until they leave the firm, although some exceptions are made to allow employees to diversify their holdings as they near retirement age.[7]

B. Productivity Gains

Requiring employees to hold ESOP shares for an extended period can help align employee incentives with shareholder value; however, achieving an incentive effect is complicated by a free-rider problem. The employee may feel she has little impact on the stock price and, therefore, be unwilling to alter her behavior in tasks requiring additional effort or sacrifice. This free-rider effect, often referred to as the 1/N effect, intensifies as the number of employees, N, increases.

Kandel and Lazear (1992) argue the free-rider problem can be overcome through peer pressure. Workers covered by an ESOP can profit as a whole if everyone agrees to work harder; that is, solve the free-rider problem through collective collaborative activities such as peer pressure and co-monitoring. If employees as a group can observe one another's work and influence behavior through peer pressure, cooperation becomes more likely. Thus, an ESOP implemented in a corporate culture promoting group cooperation and co-monitoring may have a substantially positive impact on productivity.

Survey evidence confirms that ESOPs encourage co-monitoring. Freeman et al. (2010) survey over 40,000 employees from 14 companies with employee ownership plans. The survey asks employees how they would respond if they observed a co-worker

[6] The most common practices of voting unallocated shares are: 1) unallocated shares are voted in the same proportion as workers voted their allocated shares, or 2) the ESOP trustee (appointed by management) votes on workers' behalf.
[7] ESOPs can help retain employees until the shares are fully allocated because employees have to stay in the company to receive forthcoming share grants. Once all shares are allocated and vested, the incentive may work in reverse, because employees can resign from the firm to sell their ESOP shares for diversification purposes.

underperforming. They are given three options: 1) talk directly to the employee; 2) speak to a supervisor; or 3) do nothing. An anti-shirking index based on the employee answers reveals that those with company stock are much more likely to actively respond to shirking by a co-worker.

Aligning employee incentives with share price through ESOPs also can mitigate inefficiencies in monitoring. Milgrom (1988) shows workers over-invest in tasks that are easily observed by their bosses. Thus, even if workers do not shirk, their effort may be sub-optimally allocated across tasks. By aligning team incentives and the focus of co-monitoring with shareholder value, ESOPs can also reduce inefficient allocation of employee effort.

B.1. Related Evidence

Using a Cobb-Douglas production function, Jones and Kato (1995) estimate changes in productivity at Japanese firms following the adoption of an ESOP. They find an ESOP adoption leads to a 4-5% increase in productivity, starting about three years after the adoption. However, the typical Japanese ESOP in their sample is allocated 1% or less of outstanding shares, much smaller than a typical ESOP observed in the U.S.[8]

B.2. Hypotheses and Test Outlines

We hypothesize that adopting ESOPs can lead to productivity gains. The gains are more likely to materialize at firms with a modest number of employees. When the number of employees is numerous, free-riding is likely to overwhelm the incentive

[8]The Jones and Kato (1995) finding is in sharp contrast to Faleye, Mehrotra, and Morck (2006), who examine only large employee share ownership blocks. Based on cross-sectional correlations, Faleye, Mehrota, and Morck (2006) draw opposite inferences: When employees control 5% or more of voting stocks, firms have lower Tobin's Q, invest less, grow more slowly, and create fewer new jobs.

effects, unless the number of ESOP shares granted to employees is sufficiently large to offset the free-rider effect.

Prediction 1: If ESOPs are adopted to improve incentives, worker productivity increases when the number of employees is not-so-numerous. The incentive effects will not work with numerous employees, unless the size of ESOP is sufficiently large.

If productivity increases following some ESOP adoptions, how are the gains shared between employees and shareholders? Akerlof and Yellen (1990) argue that workers will demand a "fair" wage. When ESOPs induce workers to be more productive, workers' perception of a fair wage will increase to reflect a fair share of the firm's surplus. If wages fall short, they will be more inclined to shirk or quit. Hildreth and Oswald (1997) find wages increase following shocks to firm-level productivity, suggesting employees capture a share of productivity gains.

We hypothesize that the share of productivity gains captured by employees depends on worker bargaining power (WBP). When workers have few alternative employment opportunities, the threat to shirk or quit matters less, giving employers greater bargaining power (Bhaskar, Manning, and To, 2002; Manning, 2003), which, in turn, helps shareholders retain more of the surplus. To test this prediction, we develop a Herfindahl index of employer concentration within each industry and location of workplace, which is the inverse of WBP.

Prediction 2: If ESOPs increase productivity, wages (shareholder value) will increase more (less) at establishments where WBP is greater.

To estimate how productivity changes following ESOP adoptions, we first estimate how wages and firm valuation change relative to a control group of firms that

are not identified as having ESOPs. Since employees and shareholders are the two main claimants of firm surplus, we infer productivity gains or losses based on their combined changes. This approach allows us the full use of our sample. It also helps identify whether gains to employees or to shareholders arise at the expense of the other claimant. We also provide direct estimates of productivity gains by estimating total factor productivity (TFP). TFP estimates are presented last because the estimation is possible only for firms in manufacturing industries, which drastically reduces the sample size.

Most estimation is performed separately for small and large ESOPs, using 5% as the demarcation point. We choose 5% because it is the threshold for various disclosure requirements, presumably because it signifies an important source of control rights. Proxy statements detail the size only when an ESOP has more than 5% of the firm's outstanding shares, so we are able to use a continuous measure of ESOP size only for large ESOPs.

C. Cash Conservation

Firms may seek to conserve cash by offering stocks to employees in exchange for cuts in wages. Chaplinksy, Niehaus, and Van de Gucht (1998) document that 56% of their sample firms undergoing employee buyouts (EBO) report wage reductions and/or terminate defined benefit plans to reduce the cash burden of labor costs in exchange for issuing stocks to their employees.[9] In a similar vein, Core and Guay (2001) find that broad-based stock option (BBSO) plans are more common at firms with large financing

[9] These findings also raise the possibility that some cash-constrained firms adopting ESOPs may terminate a defined benefit (DB) plan. However, terminating DB plans in exchange for ESOP adoptions appears to be specific to EBOs. Kruse (1995) examines Department of Labor (DOL) Form 5500 data on all US pension plans and reports, "…only 3.4% of the growth in ESOP participants occurred in companies terminating DB plans, while 75.1% of this growth occurred in companies adopting or maintaining DB plans."(pg. 225) Likewise, Blasi and Kruse (1991) examine 1,000 US public companies with at least 4% employee ownership and identify that in only 41 (4.1%) of these ESOPs was a DB plan terminated around the time of their adoptions. Furthermore, Rauh (2006) notes, "While ESOPs have similar disclosure requirements as pension plans, historically they have not been implemented as substitutes for retirement benefits provided by companies." (p. 4)

needs and high costs for external financing. However, Ittner, Lambert and Larcker (2003) find the opposite; using a different sample, they find more cash-constrained firms are less likely to adopt BBSO plans. Oyer and Schaefer (2005) examine this issue based on cash flows and investment outlays and find mixed evidence.

C.1. Hypothesis and Test Outlines

Underlying the empirical debate is a theoretical issue: Is substituting cash wages with equity-based compensation an efficient cash conservation strategy for a publicly traded firm? Because of the sales restrictions, ESOP shares impose on employees risk that can be diversified by other investors; hence, risk-averse employees will value ESOP shares below market price. Thus, issuing shares to employees to conserve cash is more costly than raising cash through share issuance to the public. Conserving cash through ESOPs, however, can be an attractive option when firms are cash constrained with limited access to external financing.

Prediction 3: ESOPs adopted by cash-constrained firms are more likely to lead to lower cash wages than non-constrained firms.

D. Worker-Management Alliance

Pagano and Volpin (2005) develop a theoretical model showing employee control rights through share ownership can give rise to a worker-management alliance, whereby management bribes employees with above-market wages to garner their support in voting against a hostile takeover bid.[10] Shareholders pay for this alliance through (1) higher wages and (2) a lower probability of receiving a takeover premium. Chaplinsky and

[10] See Atanassov and Kim (2009) for evidence in support of the worker-management hypothesis in the context of corporate restructuring around the world.

Neihaus (1994) and Rauh (2006) show employee share ownership significantly reduces the probability of a takeover.

D.1. Hypothesis and Test Outline

Our test focuses on the first prediction: higher wages. To distinguish wage increases due to the alliance from those due to higher productivity, we estimate the incremental difference in wages following adoptions of large ESOPs by firms incorporated in states with business combination statutes (BCS) that require the bidder to wait three to five years before pursuing the takeover if a sufficient block of investors unaffiliated with management vote against a tender offer. This type of BCS makes ESOPs, especially large ESOPs, an effective anti-takeover device because courts have established holders of ESOP shares as "unaffiliated" investors.

Prediction 4: Wages will increase following large ESOPs adopted by firms incorporated in states with BCS that allows a block of investors unaffiliated with management to thwart hostile bids.

E. Employee Ownership through 401K Plans

Another important form of employee ownership is 401K plans (see Rauh, 2006). Ownership through a 401K may also help align employee incentives with shareholder value. However, there are important differences between 401K plans and ESOPs. With 401Ks the alignment effect is weaker because employees are free to sell their shares, whereas with ESOPs, employees are restricted from selling shares. Furthermore, ESOPs have to be given to all employees subject to few exceptions. Participation in a 401K plan is optional for employees, and when firms offer a "match" in the form of company stock, the employee can sell the shares and invest elsewhere for diversification. As such,

employee share ownership through 401Ks is unlikely to be as broad-based as an ESOP plan and less likely to induce productivity-improving peer pressure and co-monitoring.

Furthermore, sample properties of employee ownership through 401K plans make it difficult to test our alignment hypothesis. With ESOPs, we have no employee share ownership and then observe a sharp jump—a true before and after. With 401K plans, within-firm variation in employee ownership over time is more gradual, which weakens the power of the test, making it difficult to reject the null even when the employee ownership via 401K matters. Nevertheless, we re-estimate our wage regressions while controlling for employee share ownership through 401K plans.[11] We find no significant relation between share ownership via 401K and wages. The results concerning ESOP variables are robust.

II. Data Construction and Summary Statistics

Our data on ESOPs cover US public firms from 1982 through 2001. The presence of an ESOP and the first year of ESOP implementation are identified by a two-step Factiva search. In the first step, we search using the terms "ESOP" and "employee stock ownership plan." This compiles an initial list of all firms with ESOPs and the first year the ESOP is mentioned. This process yields 739 unique public firms with ESOPs over the sample period. Of these firms, we drop 35 firms with total assets less than $10 million (in 2006 dollars).

In the second step, we conduct additional Factiva searches using each identified firm's name and "employee stock" to obtain further information on its ESOP. We find more articles because of the less restrictive search terms; we carefully read the full text of

[11] We thank Josh Rauh for sharing his ownership data that includes 401K ownership. The results are reported in the internet appendix.

all articles about the firms to ascertain whether an ESOP was indeed implemented and when it was initiated. We occasionally find firms with ESOPs established prior to the year the media first mentions them having ESOPs. We exclude these cases to avoid bias toward finding a positive correlation between ESOP adoptions and firm performance. For example, a firm may not attract media attention at the time of an ESOP adoption because it is too small. As the firm grows over time, media attention increases, leading to the publication of an article about an ESOP adopted in earlier years. Had the firm failed to grow, the lack of media attention would have continued and we would not have identified the ESOP. These additional steps yield 410 unique firms for which we can accurately identify the year of ESOP initiation.

Table I lists the number of ESOP initiations and firm-year observations with ESOPs during 1982 through 2001. The second column counts the number of ESOP initiations by year. It shows fewer ESOP adoptions during recessions, a pattern consistent with fewer firms issuing stocks when the stock market gets cold. The last column counts the total ESOP firm-year observations for each year, including those from ESOPs established in previous years.[12] The total sample contains 4,594 ESOP firm-year observations. Regression estimates are based on smaller sample sizes, because some observations cannot be matched to the Census database.[13]

The size of ESOPs is determined by reading annual proxy statements. In most cases, ESOP share ownership is reported only if the plan has more than 5% of the firm's common equity. We assume the ESOP controls less than 5% of the firm's outstanding

[12] If a firm goes bankrupt or is dropped from Compustat for a year or more, we assume the ESOP was terminated unless other information is present.

[13] The exact sample matched to the Census database for every year during our sample period cannot be disclosed because of the Census confidentiality policy.

shares if the proxy statement does not report the ESOP size. The median ESOP controls 6.70% of shares outstanding. (The mean is not available because the size of small ESOPs with less than 5% is not reported.)

Our ESOP database is matched to the Longitudinal Business Database (LBD) maintained by the Census. The LBD is a panel dataset tracking all business establishments with at least one employee or positive payroll from 1975 to the present. The database is formed by linking years of the Business Register (formally, the Standard Statistical Establishment List). The Business Register is a Census Bureau construct based primarily on information from the Internal Revenue Service.[14] The LBD links the establishments contained in the Business Register over time and can be matched to Compustat using a bridge file provided by the Census.

The Census data contains information on the number of employees working for an establishment and total annual establishment payroll, which includes all forms of compensation taxed as ordinary income. We measure wages per employee as the ratio of annual payroll (normalized to 2006 dollars in thousands) to the number of employees. Since this measure does not include the value of ESOP shares given to employees, we sometimes refer to it as "cash" wages. We exclude 1988 wage data in all wage analyses because 1988 wages in the LBD have unique sampling properties relative to other years.

This Census data is an improvement in several ways over the wage and employment data in Compustat. For one, the Census data is available at the establishment level, which allows us to identify changes at each facility instead of relying on firm-level aggregate data. Second, we are able to identify the state of location for each establishment, making it possible to control for geography-dependent wage factors.

[14] See Jarmin and Miranda (2002) for more information.

Finally, the majority of firms in Compustat do not report employee compensation, and the reported wage data is unreliable, because personnel information is subject to looser reporting and auditing requirements than financial variables.

For the analyses of wages and total factor productivity, matching is done at the establishment level. For shareholder value, matching is done at the firm level. For each ESOP establishment or firm, we identify the three nearest-neighbor matches based on size, industry, and year. For establishments, we measure size as the count of total employees; for firms, as total assets. When we have more than three exact matches for an establishment with a small number of employees (e.g., fast food restaurants), we match with the most similar total payroll. Industry is defined by 4-digit SIC for establishments, and 3-digit SIC for firms because a firm represents a grouping of individual establishments, which might be operating in different industries. The sample of possible matches is drawn from firms (or establishments owned by such firms) in Compustat that are not identified as having an ESOP through our two-step search process. For establishments, matches are establishments owned by these non-ESOP public firms. Because we identify the match from the same pool of establishments (firms), in some cases a control establishment (firm) is matched to multiple ESOP establishments (firms). We adjust for the bias due to the presence of repeated observations by clustering standard errors.

While we take careful steps to identify all firms with ESOPs, we are unlikely to catch each and every ESOP at all firms in Compustat. As such, there may be some cases where firms do have ESOPs and yet are included in our control sample. This creates a

bias against finding differences between ESOP and non-ESOP firms, weakening the power of our tests.

Firm valuation is proxied by industry-adjusted Tobin's Q. Q is measured as fiscal year-end market value of equity plus market value of preferred stock plus total liabilities divided by the book value of total assets. Following Bebchuk and Cohen (2005), we measure industry-adjusted Q by subtracting the median Q matched by industry and year.

ESOP firms are included in our sample for the five years before and the ten years after the ESOP initiation. We begin five years prior, to capture the most current information, and extend to ten years afterward, because ESOP shares must be granted to individual employee accounts within ten years. Observations after ten years are excluded to reduce the impact of changes unrelated to the ESOP occurring well after the initiation. We also exclude observations after an ESOP termination to ensure that our baseline is not picking up post-termination effects.[15] The matched groups are kept for the five years before and up to ten years after the match.

Table II provides summary statistics, comparing pre-ESOP, post-ESOP, and control firms. Panel A compares firm-level variables; Panel B compares employment and wages. The first column details firms that later initiate an ESOP, over the pre-ESOP period of up to five years prior. The second column describes firms over the post-ESOP period of up to ten years. The third column summarizes the set of matched firms over the

[15] There are 56 ESOP terminations (138 plant-year observations) in our ESOP database. Terminating an ESOP is a complex legal procedure. The firm must be able to legally justify why the ESOP was value-increasing for the firm in the past but is now value-decreasing, otherwise, it is open to lawsuits from ESOP holders and shareholders. Thus, it is more common to "freeze-out" an ESOP. Usually a freeze-out is not announced officially, so is difficult to identify. In our sample, firms that elect to freeze-out their ESOP are recorded as having an ESOP, which is literally true because the ESOP still exists. Some firms also have rolled up their ESOP into a 401K plan, which may be recorded in our database as an ESOP; hence, it is not completely off-base because they still represent employee stock ownership.

up-to-16-year period from five years before to ten years after the match. Statistical differences are noted in the table.

Panel A indicates ESOP firms are more profitable and larger than matched firms, with the pre-ESOP sample showing higher profitability and more capital expenditures. Financial leverage increases following ESOP initiations because they are often debt financed. Industry-adjusted Q shows no significant difference across the three samples. Panel B shows wages increase after an ESOP adoption, while the average number of employees per establishment declines. Wages at matched firms' establishments are between ESOP firms' pre- and post-ESOP levels. Matched firms show more employees per establishment than ESOP firms.[16]

Table III compares large ESOPs, ESOPg5, with the average ESOP. Of the 410 ESOPs in Table I, 225 are ESOPg5. The median and the mean ownership of these large ESOPs is 12.20% and 18.35% of shares outstanding. In Panel A, firms adopting a large plan show a significant drop in profitability after the plan, although before the plan their profitability is not statistically different from the average. These firms also show significantly lower industry-adjusted Q than the average, although the drop in Q following the ESOP adoption is not significant. Assets increase following ESOPs, probably because the post-ESOP firms are older. There is little difference in capital expenditures.

Panel B shows significant differences in both employment and wages between firms adopting large ESOPs and the average ESOP firm. Large ESOP firms have fewer employees per establishment before and after the ESOP adoption and show a greater drop

[16] Some of the variables in Table II show large differences between the matched entities, because the reported numbers cover many years preceding and following the year of match and, over time, firms and establishments diverge in characteristics. At the year of the match ESOP and control firms show only modest differences.

in employees following the ESOP. Wages increase following ESOP adoptions for both groups, but large ESOP firms show a larger increase.

It is difficult to draw inferences from these comparisons because the differences could be attributed to a host of factors that are not controlled for—e.g., entry and exit of firms and establishments, firm and establishment characteristics, macro trends, and so on. What stands out from these comparisons are the higher wages following ESOP adoptions.

III. Employee Compensation

In this section we estimate the relation between changes in employee cash wages and the adoption of an ESOP. We also provide an estimate of the value of ESOP shares granted to employees, which is not included in the wage estimates.

A. Univariate Analysis

We begin by examining how cash wages change around the ESOP initiation over a five-year window, -2 to +2 years, surrounding the year of ESOP initiation (year 0), separately for establishments with small ESOPs, large ESOPs, and the control group. We do not consider years beyond the five-year window because the high rate of establishment entry and exit over time leads to significant changes to the sample. This is not a concern in the next section where regressions control for establishment fixed effects.

Since wages are affected by location- and industry-specific factors, we estimate unexplained wages by residuals of regression (1).

$$Wages_{it} = \alpha_0 + \beta_1 S\text{-}Y\ mean\ wages_{it} + \beta_2 I\text{-}Y\ mean\ wages_{it} + \mu_{it} \quad (1)$$

Subscripts *i* and *t* indicate establishment i and year t, and wages are the log of the average wage per employee because wages are highly skewed. S-Y mean wages is the log mean wage per employee in the state of location of the establishment in the same year. I-Y

mean wages is the log mean wage per employee matched to the establishment's industry and by year. Both exclude the establishment itself in calculating the mean.

The estimates, reported in Table IV, Panel A, show different wage patterns between small and large ESOPs. The small ESOP sample shows significant increases in unexplained wages following the ESOP adoption, whereas the large ESOP sample shows no clear trend. The unexplained wages for the control group also show no noticeable pattern.

To examine how the results vary across not-so-numerous and numerous-employee firms, Panels B and C repeat the estimation separately for each group. The separation is based on the total number of employees in the year of ESOP adoption as reported by Compustat, which includes all employees, including those located overseas. When this number is missing, we rely on the Census data and use the sum of employees at all US establishments owned by the firm, which excludes workers located overseas. Firms with numerous employees are defined as those in the top quartile of firms ranked by the number of employees; the remaining firms are defined as not-so-numerous-employee firms. Because numerous-employee firms tend to have more establishments, this separation provides a roughly equal number of establishments between the two groups. The ranking in the number of employees is based on the entire sample of ESOP firm-year observations. We cannot disclose the number of employees for the 75^{th} percentile firm because of the Census confidentiality policy. We can report, however, the mean number of employees belonging to the firms falling between the 65^{th} and 85^{th} percentile is 15,500.

The small ESOP sample shows a more pronounced and stable increase in unexplained wages for not-so-numerous-employee firms (Panel B) than for the total

sample (Panel A).[17] The large-ESOP sample continues to show no clear trend. In contrast, the numerous-employee sample (Panel C) shows no clear wage pattern following small ESOPs, but an increasing trend in unexplained wages, albeit modest, following large ESOPs.

B. Multivariate Analyses

In this section, we control for additional establishment and firm characteristics with the following baseline regression at the establishment level:

$$Wages_{it} = \eta_t + \theta_i + \alpha_0 + \alpha\, ESOP_{it} + \beta\, Z_{it} + \mu_{it} \quad (2)$$

The regression estimates the mean change in cash wages following ESOP adoptions. $ESOP_{it}$ includes ESOP and ESOPg5 indicators: *ESOP* is equal to one if the firm has an ESOP; and *ESOPg5* is equal to one if, at its maximum, an ESOP controls more than 5% of outstanding shares. Year- and establishment fixed effects, η_t *and* θ_i, control for economy-wide effects on wages and time-invariant establishment characteristics.

The set of time varying control variables, Z_{it}, include state-year mean wages to control for changing local conditions affecting wages over time (Bertrand and Mullainathan, 2003). We also control for industry-year mean wages. To control for changes over time in establishment and firm characteristics, we include establishment age and total sales at the firm level. Brown and Medoff (2003) find wages increase as firms get older, and Oi and Idson (1999), among others, document a positive relation between wages and firm size. We also control for financial leverage because some ESOPs are debt financed. Berk, Stanton, and Zechner (2010) argue that higher leverage imposes greater risk on employees, resulting in higher wages. In addition, we include the capital-to-labor

[17] The magnitudes of unexplained wages are smaller than those in Panel A, which includes large firms, because smaller firms tend to have lower wages.

ratio (fixed assets/number of employees) to control for the relative importance of labor to a firm's output; tangibility (fixed assets/total assets) for operating risk; and R&D and advertising expenditures (R&D/Sales and Advertising/Sales) for growth opportunities and risk taking. Because R&D and advertising expenditures are often missing, we avoid reducing the sample size by including dummy variables for missing observations. Table AI in the Appendix describes all variables.

Wages at establishments owned by the same firm may be correlated, understating the standard errors. So we cluster standard errors at the firm-level. In addition, we exclude transition periods, the year of the announcement of ESOP adoption and the year after, because ESOP implementation takes time.

B.1. Basic Results on Cash Wages

Table V reports estimation results. The first column shows wages are highly correlated with the concurrent average wages in the same state and the same industry. The coefficient on *ESOP* is insignificant, because treating all ESOPs the same masks important heterogeneity across ESOPs and firms of different employee size. In the second column, we add an indicator for ESOPg5 to separate ESOPs into small and large. The coefficients on the indicators hint at differences between small and large ESOPs, but neither is significant.

A clear pattern emerges when we separate the sample into not-so-numerous and numerous-employee firms to control for differences in the free-rider problem. Columns (3) and (4) reveal sharp differences between the two subsamples. The coefficient on *ESOP* in Column (3) indicates a significant wage increase following the adoption of a small ESOP by not-so-numerous-employee firms. The magnitude of the coefficient

implies a 20% average wage increase. This positive relation between ESOP adoptions and wages applies only to small ESOPs established by not-so-numerous-employee firms. The cumulative coefficient on *ESOPg5* in Column (3) is insignificant (0.20 – 0.29 = -0.09), indicating no significant changes in wages following large ESOP adoptions at not-so-numerous-employee firms.

For numerous-employee firms, neither ESOP indicator variable is significant, consistent with our prediction that free-rider problems will overwhelm the incentive effects. These results illustrate how the incentive effect interacts with the free-riding effect: small ESOPs adopted to improve employee incentives are followed by higher cash wages when free-riding can be managed. When employees are too numerous to mitigate free-riding, incentives through share price alignment do not work as well.

To check the sensitivity of these results to separating firms by the top quartile in the number of employees, we re-estimate the regressions using the number (or the log) of employees at the year of the ESOP initiation (or the match year for control firms). The results (available in the internet appendix) are robust to the alternative specifications.

Some numerous-employee firms may attempt to overcome the free-riding effect by endowing their ESOPs with more shares. Any such effects captured by *ESOPg5* coefficient will be noisy, because the coefficient will reflect the average impact of a pool of moderately large and very large ESOPs as well as the presence of large ESOPs motivated for non-incentive purposes. To account for different sizes within ESOPg5, we use a new variable, *Maxesop*, which is equal to the maximum fractional share ownership attained by an ESOPg5, and zero if a firm has no ESOP or a small ESOP. This continuous variable allows us to use the cross-sectional variation in ESOP size within the

ESOPg5 sample, which can increase the power to detect the incentive effects. However, it can also exacerbate the confounding effects of cash conservation and worker-management-alliance motivated ESOPs, which are also likely to be larger.

Column (5) shows a positive and significant coefficient on *Maxesop*, indicating larger ESOPs at numerous-employee firms are associated with higher wages. However, the economic magnitude implied by the coefficient is rather modest; for example, if an ESOP controls 20% of outstanding shares, the coefficient implies it will be accompanied with a 2.6% (0.20 x 0.13) higher wage than a small ESOP.

We repeat the same estimation for not-so-numerous-employee firms in Column (6). The coefficient on *Maxesop* is significant, but in the opposite direction.[18] This significant negative coefficient is consistent with the significant negative coefficient for ESOPg5 in Column (3). It is also consistent with our finding (reported later in Section VI.A) that some large ESOPs are motivated to conserve cash by cash-constrained firms and lead to lower cash wages. There are more cash-constrained firms among not-so-numerous-employee firms.

B.2. Value of Shares Granted through Large ESOPs

Looking at cash wage changes alone understates total compensation changes because the value of ESOP shares is unaccounted for. The underestimation is greater for larger ESOPs. The average size of shares granted to employees through ESOPg5 is

[18] To provide further evidence on how larger ESOPs at numerous employee firms are related to wage changes, we create dummy variables for ESOPs controlling more than 20%, 30%, and 50% of outstanding shares. We include these dummy variables one at a time to a regression with ESOP, ESOPg5, and the same set of control variables. Each of these dummy variables is significantly associated with greater wage gains (relative to ESOPg5) at numerous-employee firms. When we repeat the same exercise for not-so-numerous-employee firms, none of the very large ESOP dummies shows a significant coefficient.

18.35%.[19] The average firm with an ESOPg5 has a market capitalization of $3.5 billion (in 2006 dollars) and 23,959 employees. Thus, the average ESOPg5 has a total value of $642 million, which translates into $26,796 per employee. Given that the average annual wage for workers at these ESOPg5 firms is $52,981, the value of the ESOP shares allocated would be equal to 5.06% (10.12%) of annual wages if the shares were allocated equally over ten (five) years. Although employees may value ESOP shares less than cash wages because of the sales restriction, this is of substantial economic magnitude. Taken together, large ESOPs are also followed by an increase in total employee compensation.

B.3. Summary

The evidence of higher cash wages following ESOP adoptions is most significant and substantial when small ESOPs are adopted by not-so-numerous-employee firms. This is consistent with—though no causality is yet established—the hypothesis that when the number of employees is of manageable size to control free-riding effects, small ESOPs improve incentives and workers share in the productivity gains. Large ESOPs also seem to be associated with higher employee compensation, if the value of ESOP shares granted is accounted for. However, the evidence on large ESOPs is considerably weaker.

For firms with numerous employees, cash wage gains are mostly insignificant, consistent with ineffective incentives due to free-rider problems. However, when these firms adopt very large ESOPs, we find significant but economically modest cash wage gains, suggesting very large ESOPs help alleviate the free-riding effect.

IV. Worker Bargaining Power and Causality

[19] We do not have the equivalent statistic for small ESOPs because, as mentioned earlier, proxy statements do not provide the percentage held in ESOP accounts if it is below 5%.

Because we control for establishment fixed effects, our main concerns in establishing causality are time-varying omitted variables that are correlated with future performance and either (1) the decision to adopt an ESOP or (2) the decision to adopt a small ESOP versus a large ESOP. In this section, we first provide further corroborating evidence in support of the productivity hypothesis. Then, we build on the results to present evidence suggesting the relation is causal – that establishing small ESOPs by not-so-numerous-employee firms lead to higher cash wages. We also address selection issues concerning the choice of small ESOP vs. large ESOPs, and estimation issues due to serial correlation in the dependent variables.

To provide a closer link between the wage gains and productivity, we test the second prediction of the productivity hypothesis: ESOPs increase productivity, employees share the gains, and the fraction of gains accruing to employees depends on the relative bargaining power between employees and employers. While worker bargaining power (WBP) is likely to affect wages at all firms, the effect of WBP should be stronger at firms that recently experienced a productivity shock. The fair wage hypothesis of Akerlof and Yellen (1990) suggests employees' belief of a fair wage is formed relative to other groups. In our case, shareholders are the benchmark group. If ESOPs increase productivity and shareholders gain, employees will demand their fair share. Such demands are more likely to be met when employees' external employment opportunities increase, giving them greater bargaining power. At control firms, we may also observe a positive relation between wages and increases in WBP; however, without a recent productivity shock, there may be little-to-no new surplus to be shared. Therefore, we hypothesize 1) implementation of small ESOPs by not-so-numerous-employee firms

leads to productivity gains greater than the average productivity shock at non-ESOP firms and 2) increases in WBP enable employees to take a greater share of productivity gains.

A. Worker Bargaining Power (WBP)

To construct a measure of WBP, we assume (1) labor markets are geographically constrained due to pecuniary and non-pecuniary costs of moving and (2) workers prefer to remain employed in the same industry due to industry-specific human capital. These assumptions allow us to measure WBP within an industry-location pair. We classify industry by 3-digit SIC and geographic location by Metropolitan Statistical Area (MSA).[20] Then, we calculate a Herfindahl index of employer concentration for each industry-MSA pair. To construct the index, we sum the total number of workers at all establishments linked to a firm for each industry-MSA pair.[21] The firm's share of employees is the number of its employees divided by the total employees in that industry-MSA pair. The Herfindahl index is then calculated as the sum of the squares of the employee share of all firms in that industry-MSA pair. This measure is re-estimated every year. Since the index measures employer bargaining power, WBP is one minus the Herfindahl index. For establishments located in non-MSA areas, we are unable to calculate the index and use the sample median value to avoid changing the sample.[22]

B. Interactive Effects of Worker Bargaining Power and ESOPs

[20] An MSA is defined by the US Census as a geographic area that consists of a core urban area (which can be as small as 10,000 people) and all surrounding but integrated counties.

[21] We start by using all establishments included in the SSEL available through the US Census. The SSEL identifies the total count of employees at all public and private US business establishments. It also includes information on the establishment industry (SIC code) and location (state and county Federal Information Processing Standard (FIPS) codes). Using the state and county FIPS codes, we map each establishment located in an MSA to its appropriate MSA. The link between FIPS and MSA is provided by the US Census for 1981, 1983, 1990, 1993, and 1999 at: http://www.census.gov/population/www/metroareas/pastmetro.html. We use the most recent link for each match to allow for changes to MSA definitions over time.

[22] The Census disclosure process prohibits the release of too "similar" samples from which one can identify a small set of firms by using the difference. Dropping observations without MSA values would create a different but similar sample and lead to disclosure problems. As an alternative to replacing missing observations with the sample median, we also assign zero worker mobility when establishments are located in non-MSA areas, which tend to be rural areas with few alternative employers. The results are robust and available in the internet appendix.

We re-estimate the baseline regression while adding WBP and its interaction with the ESOP variables. We estimate the regression only for the not-so-numerous-employee firms, because these firms are where we observe statistically and economically significant wage gains. Table VI, Column (1) reports the estimation results which show standalone WBP is insignificant. The coefficient on WBP reflects effects of worker bargaining power on non-ESOP firms. While greater worker bargaining power should lead to higher wages, we expect modest effects for non-ESOP firms because, on average, non-ESOP firms are less likely to have recently experienced a productivity shock. Furthermore, changes in worker bargaining power are not random; all else equal, when firms make location decisions for their establishments, they choose locations where wages are low and expected to remain low. New entrants will increase WBP, but new entrants should be more likely in areas with low expected wage increases.[23] Thus, the observed relation between wage changes and changes in WBP will depend on the opposing pressures of greater employment opportunities for local workers versus the underlying trend of decreasing wages that attracted the new entrants to the location.

If small ESOPs at not-so-numerous-employee firms indeed lead to higher productivity and WBP impacts the sharing of the new surplus, then we should observe a more positive relation between WBP and wage changes at such ESOP firms. This is what we find. The interaction of WBP and ESOP shows a positive and significant coefficient, indicating that wages at small ESOP firms increase as WBP increases. The interaction of WBP with ESOPg5 shows a negative but insignificant coefficient. Column (2) shows the

[23] Our identification does not require that changes in WBP are exogenous. We only require that the unobserved location-industry characteristics influencing both new establishment location decisions and future wages affect ESOP firms and non-ESOP firms equally. For example, wages for workers in an industry-location pair may be expected to remain low for the foreseeable horizon because of limited local employment options following the bankruptcy of a major local employer. Such situations will attract new entries, increasing WBP, while at the same time lowering wage pressure at all local firms operating in the same industry – regardless of whether they have ESOPs.

cumulative interactive effects of large ESOPs are positive but not significantly different from zero, consistent with our earlier findings of no significant wage gains for this group of ESOP firms.

Our measure of WBP assumes workers are immobile across industries. However, inter-industry immobility varies across industries. For example, airline pilots' skills are less transferrable to other industries than accountants'. Thus, we predict that the positive interactive effects of WBP and ESOP on wages will be stronger when workers are less mobile across industries. We use two proxies for inter-industry immobility: (1) a measure based on the DOL occupational data and (2) industry mean wages. The first is similar to the proxy used in Donangelo (2012).[24] Because this measure is time invariant due to data limitations, we also use industry mean wages as a proxy for inter-industry immobility. Workers in higher wage industries are likely to be less mobile to other industries, as they tend to be more skilled with more industry-specific human capital. This measure, constructed with our Census wage data, is time-varying and is available for all years in our sample period. We use industry mean wages at the year of the ESOP initiation for ESOP firms, and at the year of the match for control firms. Year 0 industry wages are winsorized at 1%.

Columns (3) and (4) add these measures of industry immobility, *Ind_immobility* and *Indywages*, respectively, as standalone variables and as interaction terms with WBP and ESOP variables. Estimation is based on not-so-numerous-employee firms.

[24] Our measure follows the approach in Donangelo (2012) with one exception. We calculate industry worker mobility as an employee-weighted average of the occupations represented in an industry, as opposed to payroll-weighting in Donangelo (2012), because the DOL database does not provide payroll data prior to 1997. This is a time invariant measure because of data unavailability for many years in our sample and because our estimates of industry mobility tend to be stationary over time. The Bureau of Labor Statistics, Occupational Employment Statistics, www.bls.gov/oes/, was first conducted in 1988 for only a subset of industries. More industries were added over time and in 1999 there was a change in the definition of occupations, making estimates of industry mobility based on the survey data pre- and post-1998 incomparable. Thus, we use the average of annual worker mobility estimates based on available data from 1988 to 1998.

Coefficients on both triple interaction terms are positive and significant. (To conserve space, standalone industry immobility variables and their interactions with WBP or ESOP variables are not reported.)[25] Regardless of which measure of inter-industry immobility is used, the interactive effects of WBP and small ESOPs are stronger when workers are less mobile to other industries. This evidence further buttresses our argument that small ESOPs at not-so-numerous-employee firms increase productivity and WBP increases employees' share of the gains.

C. Causal Interpretations

While we argue the WBP-related evidence further corroborates our causal interpretation, there are alternative selection stories. For example, establishing ESOPs in high WBP areas might be part of attempts by the firm to retain workers in a very competitive labor market, leading to the appearance of higher wages following ESOP adoptions. In other words, our WBP results could be picking up firms reacting to pre-existing conditions or to anticipated increases in WBP. To control for pre-existing conditions, we add WBP at the year of ESOP adoption for ESOP firms or the year of the match for control firms, *WBP_y0*, and its interactions with the ESOP variables to the regression in Column (1). The interaction term, *WBP_y0*ESOP*, will pick up cross-sectional variation in the wage changes around an ESOP adoption that depends on the degree of pre-existing WBP.[26]

The estimation result is reported in Column (5). The coefficients on *WBP_y0* and its interaction with ESOP variables are insignificant. Because we control for interactions

[25] The full regression is available in the internet appendix.

[26] Control establishments may appear more than once at different points of time, providing some within-establishment variation in WBP_y0 for the set of control groups. Otherwise, the coefficient on WBP_y0 would not be identified because of establishment fixed effects.

with *WBP_y0*, the coefficient on *WBP*ESOP* will capture only the effect of changes in bargaining power occurring after an ESOP adoption. The coefficient on *WBP*ESOP* remains positive and significant, implying wages increase more at establishments with ESOPs when WBP increases after the ESOP initiation.[27] This result is not affected by an establishment's endogenous decision to move because the Census assigns a new identification number to any establishment changing locations.[28]

Although we can rule out pre-existing WBP conditions as an explanation, it is possible to anticipate future increases in WBP at the time of the ESOP adoption. To address this issue, we re-estimate the regression while excluding observations from the year of ESOP adoption to year +5 for both the control and the ESOP group. This sample construction addresses the anticipation issue because employers are unlikely to anticipate changes in worker bargaining power five years out in deciding ESOP adoptions. Excluding the first five years of observations greatly reduces the sample size. To compensate the loss of power due to smaller sample size, we estimate the regression on the low inter-industry mobility subsample. This sample contains observations with above the median industry mean wage at the year of the ESOP initiation (for ESOP firms) or the year of the match (for non-ESOP firms). We control for WBP at year +5, *WBP_y5*, and its interactions with the ESOP variables. Column (6) reports the re-estimation results. The coefficient on *WBP*ESOP* remains positive and significant. The positive interactive

[27] For reasons explained earlier, we use the sample median for establishments located in non-MSA areas to avoid changing the sample. The result is robust to assigning zero bargaining power instead of the median and available in the internet appendix.

[28] When a firm moves an establishment, the establishment will be assigned a new identifier after the move, which means the post-move establishment will have no pre-move observations. Thus, although moves by firms will affect the local WBP, such moves will have no direct effect on the coefficient of *WBP*ESOP*.

effects of WBP and ESOP adoptions on wages are not driven by the anticipation of greater WBP.

D. Other Selection Issues

The evidence in the previous section counters alternative selection stories based on the endogenous timing of the ESOP adoption. In this section, we address the endogenous choice between a small ESOP and a large ESOP.

The wage gains associated with small ESOPs established by not-so-numerous-employee firms could be due to differences in growth opportunities. Consider a firm with good growth opportunities. It may prefer a small ESOP to a large ESOP. As this firm grows, worker wages also grow more than control firms. Our regressions control for R&D and advertising expenditures, which are related to growth opportunities. Nevertheless, we re-estimate wage regressions for not-so-numerous-employee firms with additional proxies for growth: sales change, industry sales change, asset utilization change, and Q. Sales and asset utilization changes are measured over the past two years, and Q is contemporaneous. The results are reported in Table AII in the Appendix, which shows virtually unchanged coefficients on ESOP variables. The coefficient on Q is significant and positive, implying firms with higher growth opportunities have higher wages. However, it does not change the coefficients on ESOP variables. To further check the robustness, we add Q or industry sales changes at the time of the ESOP initiation and interact it with ESOP variables. (For matched firms, they are measured at the year of the match). The results (available in the internet appendix) are robust.

Another selection story is that firms with superior past performance may prefer small ESOPs to large ESOPs. If the superior performance continues after ESOP

adoptions, it will give the appearance of small ESOPs leading to higher wages, shareholder value, and productivity. We proxy for pre-ESOP performance by the pre-ESOP run-up in stock price and calculate raw and market-adjusted returns over one, two, and three years preceding the ESOP adoptions. This is done separately for small ESOPs, ESOPg5s, and small ESOPs at not-so-numerous-employee firms. The results (available in the internet appendix) show similar patterns of pre-ESOP run-up in prices for all three groups. We also estimate the correlations for each of the six measures of run-up in prices with changes in cash wages and in industry-adjusted Q. All correlations are estimated separately for small ESOPs, large ESOPs, and small ESOPs at not-so-numerous-employee firms. The estimated correlations (available in the internet appendix) are mostly insignificant.

E. Estimation Issues

Bertrand, Duflo, and Mullainathan (2004) point out standard errors from difference-in-differences tests can be biased in the presence of serial correlation in the dependent variable and suggest alternative estimation procedures. We follow one of their suggested procedures to re-estimate the relation for not-so-numerous-employee firms. The results are reported in Table AIII in the Appendix with a description of the estimation procedure in the table legend. *ESOP* continues to show positive and significant coefficients; however, *ESOPg5* shows an insignificant coefficient, implying large ESOPs are also associated with significant wage gains.[29]

V. Firm Valuation, Productivity Gains, and Employment

[29] A likely explanation for the insignificant coefficient on *ESOPg5* is the limited power in this type of estimation procedure, which is also noted by Bertrand, Duflo and Mullainathan (2004).

Our analyses of wages and the value of ESOP shares granted suggest that ESOPs often lead to higher employee compensation. The productivity hypothesis predicts that both employees and shareholders will gain following an ESOP. But employees' gains may come at the expense of shareholders. For example, worker control rights bestowed by ESOPs may enable employees to extract unearned wages. Furthermore, higher employee compensation does not necessarily imply all employees are better off. If ESOPs lead to lower employment, the higher compensation does not benefit non-surviving employees. In this section, we address these issues by estimating changes in shareholder value, total factor productivity, and employment.

A. Shareholder Value

To estimate changes in shareholder value associated with ESOP adoptions, we estimate the following regression for industry-adjusted Q:

$$Ind\text{-}Adj\ Q_{it} = \eta_t + \theta_i + \alpha_0 + \alpha\, ESOP_{it} + \beta\, Z_{it} + \mu_{it} \quad (3)$$

Subscript i now indicates firm i, and θ_i is firm fixed effects. Z_{it} includes the log of total assets and the log of sales (both normalized in 2006 dollars), R&D expenses/sales, RD_missing indicator, capital expenditures normalized by total assets, Sigma (a measure of idiosyncratic risk) and Sigma dummy.[30] All independent variables are lagged by one year.

To compare Q estimates with wage estimates, we estimate regression (3) with the same six combinations of ESOP variables and employment size as in Table V. The results are reported in Table VII. No estimates indicate wage gains come at the expense of shareholders. Columns (1) and (2) show shareholders gain from the implementation of small ESOPs; large ESOPs show neutral effects. Columns (3) and (4) are estimates based

[30] These are standard controls for Q regressions, e.g., Himmelberg, Hubbard, and Palia (1999) and Kim and Lu (2011).

on subsamples of not-so-numerous- and numerous-employee firms, separated by the same criterion as in wage regressions. The results look remarkably similar to the wage estimates. The not-so-numerous-employee subsample shows a positive and significant coefficient on *ESOP*. The point estimate implies firms adopting small ESOPs realize about a 21% increase in shareholder value relative to the sample mean. For this group of firms, both workers and shareholders gain from small ESOP adoptions. As in wage estimates, *ESOPg5* shows a negative and significant coefficient and its cumulative coefficient is insignificant. For this group of firms, large ESOPs have neutral effects on both cash wages and shareholder value. The numerous-employee subsample in Column (4) shows insignificant coefficients for both ESOP variables, implying no significant gains to shareholders. In the last two columns, we replace ESOPg5 with Maxesop. For the most part, they are similar to their counterparts in wage regressions.

How do changes in worker bargaining power affect stockholders' share of productivity gains? Since the wage estimates indicate increases in WBP increase workers' share of productivity gains, we expect higher WBP will have a negative impact on shareholder value. To test this conjecture, we add WBP and its interactions with the ESOP variables to the baseline industry-adjusted Q regression. As in Table VI, estimations are based on not-so-numerous-employee firms because only this group of firms shows consistently significant and substantial gains. Table VIII, Column (1) shows the interactive effects of WBP and small ESOPs on shareholder value is negative, implying that the greater WBP, the smaller the gains to shareholders. The greater wage gains associated with higher WBP at not-so-numerous-employee firms seem to come

from shareholders. These results are based on equally-weighted measures of WBP.[31] Columns (3) and (4) repeat the estimations using payroll-weighted WBP. The results are robust.

These results on Q, together with the wage results, imply small ESOPs by not-so-numerous-employee firms lead to productivity gains, because employees and shareholders are the two main claimants of productivity surplus. Given the gains are most reliably significant for both claimants only at not-so-numerous-employee firms, we conclude that a moderate number of employees allows improved team incentives and group efforts to overcome the free-rider problem. Point estimates imply shareholder value increases by about 21% and cash wages increase by about 20%. These estimates suggest substantial productivity gains. Moreover, our worker bargaining power analyses show that when WBP increases, workers enjoy a greater share of the productivity gains, leaving a smaller share to stockholders.

B. Factor Productivity Gains

The evidence of worker productivity gains through cash wage and Q estimates are indirect. In this section, we provide more direct evidence by measuring total factor productivity (TFP) changes associated with ESOPs. We estimate TFP at the establishment level, following the estimation approach in Schoar (2002). The factor productivity estimation is possible only for firms in manufacturing industries, resulting in a drastic reduction in the sample size. Establishment-year observations are reduced to fewer than 7% of those used for wage estimates, with the bulk of unique firms coming from the control group matched at the establishment level. The inferences we can draw from TFP estimates are limited to a small set of manufacturing firms.

[31] Since Q is measured at the firm level, we aggregate the establishment-level WBP measures to the firm level.

Our TFP estimation relies on two Census databases: the census of manufacturers (CMF) and the annual survey of manufacturers (ASM). The CMF covers all manufacturing establishments, but is available only every five years in years ending with 2 and 7. For years between these Census years, we rely on the ASM, which is a survey of a representative sample of manufacturing establishments with limited coverage.

We measure TFP by estimating Cobb-Douglas production functions separately for each 4-digit SIC industry group, and for each year. Individual establishments are indexed by *i*, industries by *j*, and years by *t*. The TFP measure for each individual establishment is the estimated residual from the following regression:

$$\text{Value Added}_{ijt} = \alpha_0 + \alpha_1 \text{labor}_{ijt} + \alpha_2 \text{capital}_{ijt} + \varepsilon_{ijt} \quad (4)$$

Value Added, labor, and capital are all log transformed. Value Added is calculated as output minus inputs. Output, capital, and inputs are all adjusted for inflation using the 4-digit SIC code price deflators for output, capital, and materials from the Bartelsman and Gray database.[32] Labor is measured as the total number of production-worker-hours plus the total number of salaried-worker-hours. Because only the total count of salaried workers are available, we assume each salaried worker works 40 hours a week for 50 weeks a year with two weeks' vacation.

To reduce the impact of potential accounting manipulation on book values, capital stock is calculated using the perpetual inventory method in Schoar (2002). Capital stock is defined as:

$$\text{Capital}_{ijt+1} = \text{TAB}_{ijt} * (1\text{-dep}_{jt}) + \text{RNM}_{ijt} + \text{RUM}_{ijt} \quad (5)$$

TAB is the reported total assets at the beginning of period reported either in 1978 or in the first year the establishment appears in the data. RNM is the value of new machinery

[32] The Bartelsman and Gray database is available at: http://www.nber.org/nberces/.

added to capital stock in year t and RUM is the value of used machinery added to capital stock in year t. All values are adjusted for inflation using 4-digit SIC code price deflators for capital. Average industry depreciation rates are obtained from the Bureau of Economic Analysis (BEA).[33]

To relate TFP to ESOP variables, we use decile in the distribution of TFP as the dependent variable. Deciles are formed using the residuals from regression (4) for each observation's industry and year, with 1 the most negative. The decile approach reduces the importance of outliers, which could be byproducts of the "functional form and estimation issues usually associated with computing TFP" (Bertrand and Mullainathan, 2003, p. 1068).

Table IX reports the estimation results using the same set of controls used in the wage regressions, while excluding state-year and industry-year mean wages. Columns (1) and (2) show the results for all manufacturing establishments satisfying data requirements. Coefficients on both ESOP variables are significant, implying the adoption of a small or a large ESOP improves the relative rankings in productivity. The positive and significant coefficient on ESOP is consistent with the combined evidence concerning wages and Q. (The coefficients on ESOP for the full sample are insignificant for wages but positive and significant for Q.)

The positive and significant coefficient on ESOPg5 in the full and numerous-employee sample is, perhaps, a bit surprising. The coefficient on ESOPg5 is not positive and significant in any of the wage or Q regressions. However, this result is consistent with the positive coefficient on Maxesop (the continuous measure of ESOPg5) that we

[33] BEA estimates are available at: http://www.bea.gov/National/FA2004/Tablecandtext.pdf.

observe in the wage regression on numerous-employee firms. One possible explanation for the stronger impact of large ESOPs at numerous-employee firms on TFP, as compared to wages, is that our wage estimate does not include the value of the ESOP shares granted to employees. This underestimation in the wage results will be most substantial following large ESOPs.

In column (5) we include Maxesop and $Maxesop^2$ in place of ESOPg5 and re-estimate the regression for numerous-employee firms. We find TFP is increasing with the size of large ESOPs at a decreasing rate.[34] We repeat the same exercise for not-so-numerous-employee firms in Column (6). The size of ESOPg5 does not matter for these firms, confirming the large ESOP size effect is unique to numerous-employee firms. At a numerous-employee firm a larger ESOP seems to help alleviate free-riding effects.

For robustness, we follow Maksimovic, Phillips, and Yang (2013) and re-estimate TFP by industry group while including firm fixed effects in the TFP equation (4). The results are reported in the Appendix Table AIV. Although the signs of the coefficients on ESOP variables for the full sample remain the same, they are no longer significant. However, our hypotheses are specific to the two subsamples, which are tested by regressions in Columns (3) through (6). There the results are remarkably consistent with those in Table IX, albeit at lower significance levels.

In sum, TFP estimates paint a more favorable picture for large ESOPs. This is not surprising because unlike cash wage estimates, TFP estimates are devoid of the underestimation due to the omission ESOP shares granted. The evidence also suggests that very large ESOPs help moderate free-rider problems inherent in numerous-employee

[34]The regression estimates suggest an inverse U-shaped relation between TFP and Maxesop, with the peak estimated at a 50% ESOP ownership. However, given the presence of few very large ESOPs, the relation is effectively one of declining marginal gains.

firms. However, the results are specific to firms in manufacturing industries. Because of the small number of numerous-employee firms with larger ESOPs in this sample, we are reluctant to conclude that numerous-employee firms in general can overcome free-riding effects by making the size of ESOPs larger.

C. Changes in Employment

Productivity gains may take the form of higher efficiency requiring fewer employees. Higher compensation applies only to surviving employees and does not necessarily imply all employees are better off. To examine employment changes associated with ESOPs, we relate log (1 + employment) at the firm level to ESOP and ESOPg5 with the same set of control variables used in the firm level regressions. Because the Census does not cover foreign establishments, we estimate changes in US employment only. The control group is the matched firms.

Table X reports the estimation results. The first two columns are based on the full sample, which show no significant changes in employment. An interesting pattern emerges, however, when we divide the sample into not-so-numerous and numerous-employee firms in the last two columns. The coefficients on ESOP variables indicate that the adoption of small ESOPs is followed by significant increases in employment at not-so-numerous-employee firms, and decreases at numerous-employee firms.

We also examine employment changes at the establishment level by regressing log (1 + employment) at a given establishment on ESOP and ESOPg5 with the same set of control variables as in the wage regressions, but we now replace the state-year and industry-year average wages by state-year and industry-year average employment. Estimation results (available in the internet appendix) show no significant relation

between ESOP adoptions and the level of employment at the establishment level. This is true even when we divide the sample into numerous- and not-so-numerous-employee firms. The establishment-level evidence, together with the evidence at the firm level, indicates the increase in employment at not-so-numerous-employee firms is achieved by opening or acquiring new establishments. When we regress the log of the count of establishments on the ESOP variables, we find the number of establishments increases following small ESOP adoptions by not-so-numerous-employee firms and decreases following ESOP adoptions by numerous-employee firms. These findings suggest the productivity gains from adopting small ESOPs by not-so-numerous-employee firms lead to greater investment and employment.

VI. ESOPs for Non-Incentive Purposes

Some firms may adopt ESOPs for reasons unrelated to worker incentives. They may issue stocks in the form of ESOPs in exchange for cash wages, or establish ESOPs as a means to help thwart a potential future hostile takeover bid. In this section, we investigate the empirical validity of these alternative motives.

A. Cash Conservation

If an ESOP is established to conserve cash by substituting cash wages with company stocks, the ESOP adoption will lead to wage cuts. Such ESOPs are likely to be large in size and observed at cash-constrained firms. We measure cash constraints by *CashConst_y0,* the Kaplan and Zingales (KZ) index at the time of the ESOP adoption for ESOP firms or at the time of the match for control firms. [35] We add *CashConst_y0* and its

[35] We calculate the KZ index following the methodology in Lamont, Polk and Saa-Requejo (2001) based on results in Kaplan and Zingales (1997).

interaction with ESOPg5 to the wage and Q regressions. The control variables are the same, but their coefficients are not reported.

For this analysis, we do not separate firms based on employee size because the cash conservation motive is unrelated to the free-rider problem. However, consistent with Hadlock and Pierce (2010), who find smaller firms are more cash constrained, we observe more cash-constrained firms among not-so-numerous-employee firms.

Table XI, Column (1) presents the wage estimation result. It shows a negative and significant coefficient on the *CashConst_y0*, indicating that when a firm is more cash constrained its wages are lower.[36] The interaction term, *CashConst_y0 *ESOPg5*, is negative and significant. Post-ESOPg5 wage gains are lower, the more cash constrained an ESOPg5-initiating firm is at the time of the initiation. The coefficients on *CashConst_y0* and the interaction term indicate one standard deviation increase in the *CashConst_y0* is associated with a 9.4% wage decline. This cash conservation motive applies primarily to large ESOPs. We find an insignificant coefficient on the interaction term *CashConst_y0*ESOP* when we add it to the regression.[37] The lower average wage gains following ESOPg5, documented in previous sections, are driven, at least in part, by the substitution of cash wages with ESOP shares by cash-constrained firms.

The results (available in the internet appendix) are robust to restricting the sample to not-so-numerous-employee firms, or to using firm age as a proxy for cash-constrained firms (Hadlock and Pierce, 2010).[38]

[36] The coefficient on *CashConst_y0* is time-invariant for each ESOP firm. However, there is within-firm variation for some control firms which match to more than one ESOP firm at different points of time.
[37] Results are available in the internet appendix.
[38] We do not include the size component of the age-size index of Hadlock and Pierce (2010) because of the high correlation between the size as measured by sales or total assets and the number of employees, which is used to separate the sample according to the susceptibility to free-rider problems.

How do cash-conservation motivated ESOPs affect shareholder value? Column (4) in Table XI presents Q regression results containing the cash constraint variables. It shows a positive and significant coefficient on the interaction of CashConst_y0 and ESOPg5.[39] Although the positive coefficient implies the market reacts more positively to large ESOPs adopted by more cash-constrained firms, this Q result is not robust to using firm age as an alternative proxy for cash constraints.[40]

B. Worker-Management Alliance

Another causal scenario unrelated to employee incentives is the worker-management alliance hypothesis. ESOPs are effective anti-takeover devices when a firm's state of incorporation has a business combination statute (BCS) requiring the bidder to wait three to five years before pursuing the takeover if a sufficient block of investors unaffiliated with management, such as an ESOP, votes against a tender offer. Such BCS, denoted as BCS_Blk, provide an incentive for management to strengthen their alliance with employee-shareholders by raising wages.[41] BCS_Blk is distinct from BCS_Board, which requires board approval for any takeover.[42] In states with BCS_Board, an ESOP is less relevant in deciding a takeover bid outcome—hence, there is less incentive to bribe workers.

BCS were enacted at the state level in a staggered fashion during our sample period. In 1985 New York was the first state to pass it. Romano (1987) and Bertrand and Mullainathan (2003) argue that the enactment of BCS is exogenous to most firms

[39] When we interact CashConst_y0 with ESOP, we find an insignificant coefficient on the interaction term. Results are available in the internet appendix.

[40] Results are available in the internet appendix.

[41] DE, GA, IL, IA, KS, MA, OK, OR, PA, SD, and TX passed BCS_Blk. See Investor Responsibility Research Center (1998).

[42] AZ, CT, ID, IN, KY, ME, MD, MI, MN, NE, NV, NJ, NY, OH, RI, SC, TN, VA, WA, WI and WY passed BCS_Board . WY first passed a BCS_Blk type law in 1989, and then passed a BCS_Board type law in 1990 superseding the earlier law.

incorporated in the affected states. Using a difference-in-differences like approach, Bertrand and Mullainathan (2003) document significant wage increases following the enactment of BCS, which they attribute to management's pursuit of quiet lives after BCS relieves them of hostile takeover threats.

We begin with an estimation of the BCS effect on wages. We add to the baseline wage regression the BCS_Board and BCS_Blk indicators in Column (2) of Table XI. The indicators turn on for all establishments belonging to a firm incorporated in a state with BCS_Board or BCS_Blk in effect. We limit the sample to numerous-employee firms to minimize confounding effects of sample firms initiating ESOPs to conserve cash, a motive more prevalent among not-so-numerous employee firms. The estimation result shows a positive and significant coefficient on the BCS_Board indicator and an insignificant coefficient on BCS_Blk. The wage increases documented in Bertrand and Mullainathan (2003) seem to be concentrated in BCS_Board states. The BCS_Board makes it easier to thwart a hostile takeover bid, making it more conducive for management to enjoy quiet life.

The worker-management alliance hypothesis requires a more refined test, as it predicts greater wage gains only at establishments of ESOP firms incorporated in states with BCS_Blk. In Column (3) BCS_Blk is interacted with the ESOP dummy and Maxesop. The estimation result shows a positive and significant coefficient on the interaction of BCS_Blk and Maxesop. When a firm is subject to BCS_Blk and has a large ESOP, wages increase with the ESOP size, because the number of voting rights matters in takeover battles. The coefficient on *BCS_Blk*Maxesop* implies the average ESOPg5, which controls 18.35% of outstanding shares in our sample, will increase wages by 3.67%

(0.1835 x 0.2) if BCS_Blk applies. These wage gains are unearned, representing a direct cost to shareholders of using ESOPs to form a worker-management alliance. This result is specific to large ESOPs; the interaction of BCS_Blk and ESOP is insignificant. The anti-takeover motive applies only to large ESOPs.

To investigate how these wage effects are related to shareholder value, we estimate two Q regressions that parallel the wage regressions in Columns (2) and (3). We add the same BCS indicators and interact BCS_Blk with ESOP and Maxesop. The Q regression estimates are reported in Columns (5) and (6). Neither version of BCS shows a significant effect on Q. The variable of main interest here is the interaction of BCS_Blk and Maxesop in Column (6), which shows a negative coefficient with *p*-value = 0.11. While it is of marginal significance, the negative coefficient suggests that the unearned wages hurt shareholders.

Our analyses so far are conducted sequentially and separately on three different hypotheses. To check the robustness, we re-estimate both the wage and industry-adjusted Q regressions while including all three factors simultaneously—the cash constraint index, the BCS variables, and WBP. The re-estimation is performed for all firms, not-so-numerous and numerous- employee firms. The results (available in the internet appendix) are robust.

VII. Summary

This paper investigates whether adopting a broad-based employee stock ownership plan enhances productivity by improving worker incentives and co-monitoring. That is, does employee capitalism work? If so, how are the gains divided between shareholders and employees?

We find the answers depend on employment size and ESOP size. Employment size is important because incentivizing employees with share price is susceptible to free-rider problems. The size of ESOP matters because some large ESOPs are implemented by cash-constrained firms with limited access to external financing. These firms seek to conserve cash by substituting wages with employee shares, leading to lower wages post ESOP. Other large ESOPs are established by managers to form an alliance with workers to thwart hostile takeover bids. These non-incentive motivated large ESOPs are unlikely to enhance productivity. As such, the observed average impact of large ESOPs is confounded, making it difficult to identify the benefits of incentivizing employees.

Our investigation focuses on four different types of ESOPs: Small ESOPs and large ESOPs adopted by numerous- and not-so-numerous-employee firms. The evidence is most striking for small ESOPs adopted by not-so-numerous-employee firms – they are less likely to suffer from free-rider problems or be confounded by non-incentive motives. Consistent with the improved team incentives and co-monitoring hypothesis, small ESOPs at these firms are followed by significant increases in productivity. The productivity gains are shared by employees and shareholders according to their relative bargaining power. The average wages, level of employment, and shareholder value increase. These small ESOPs are win-win schemes.

When not-so-numerous-employee firms adopt large ESOPs, the overall evidence is considerably weaker. Benefits to employees are largely limited to the value of ESOP shares granted, which because of sales restrictions, are valued less by employees than the market. Factor productivity also increases, but shareholders, on average, do not benefit from the productivity gains. These weaker average effects reflect the confounding effects

associated with non-incentive motivated large ESOPs. ESOPs established to conserve cash should reduce cash wages. ESOPs implemented for managerial entrenchment should hurt shareholder value.

There is little evidence ESOPs established by numerous-employee firms increase cash wages or shareholder value, although some larger ESOPs are followed by modest increases in cash wages. For numerous-employee firms, the free-rider problem seems to be a dominant factor. One exception is larger ESOPs implemented by manufacturing firms, which are followed by higher total factor productivity. However, we are reluctant to conclude that numerous-employee firms can overcome free-riding effects by offering very large ESOPs because of the limited and unique sample upon which the evidence is based on.

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Table I: Employee Stock Ownership Plan (ESOP) Initiations and Firm-year Observations by Year.

A count of initiations and firm-year observations for the ESOP sample identified by our two step search procedure and screens, 1982 through 2001.

Fiscal Year	ESOP Initiations	Count of ESOP firm-year observations
1982	2	6
1983	5	13
1984	8	22
1985	13	38
1986	14	50
1987	24	72
1988	36	105
1989	82	189
1990	53	247
1991	16	262
1992	22	275
1993	10	314
1994	24	332
1995	15	349
1996	26	388
1997	18	396
1998	16	393
1999	17	396
2000	7	381
2001	2	362
Total	410	4,594

Table II, Panel A: Summary Statistics for ESOP Firms and Matched Firms.

Accounting variables are from Compustat. All variables are winsorized at 1%. Assets and sales are normalized to $2006. Q is fiscal year-end market value of equity plus market value of preferred stock plus total liabilities divided by total assets. Industry-adjusted Q is obtained by subtracting the median Q matched by industry (3-digit SIC code) and year. Means are reported with standard deviations in brackets. Pre-ESOP Firms include firm-year observations before the ESOP is adopted for firms that eventually adopt an ESOP. Post-ESOP Firms include all firm-year observations for firms with ESOPs. Matched firms are a group of control firms, which are not identified as having an ESOP, matched to the ESOP firms. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	Pre-ESOP Firms	Post-ESOP Firms	Matched Firms	Difference between Pre-ESOP and Post-ESOP	Difference between Pre-ESOP and Matched Firms	Difference between Post-ESOP and Matched Firms
Operating Income/Assets	0.129 [0.094]	0.117 [0.091]	0.110 [0.108]	***	***	***
Leverage	0.169 [0.157]	0.209 [0.172]	0.194 [0.183]	***	***	***
Assets (millions)	5,377.72 [11,953.69]	7,175.55 [13,545.77]	4,394.99 [11,655.32]	***	***	***
Sales (millions)	3,124.70 [6,233.29]	4,452.52 [7,728.11]	2,468.98 [5,616.81]	***	***	***
Capex/assets	0.070 [0.054]	0.063 [0.048]	0.063 [0.051]	***	***	insignificant
Industry Adjusted Q	0.082 [0.561]	0.098 [0.699]	0.103 [0.745]	insignificant	insignificant	insignificant
N	1480	1884	8265			

Table II, Panel B: Summary Statistics for Establishments of ESOP Firms and Matched Firms.

All variables are winsorized at 1%. Annual payroll and wages per employee are normalized to $2006. Means are reported with standard deviations in brackets. Pre-ESOP Firms include firm-year observations in the sample years before the ESOP is adopted for firms that eventually adopt an ESOP. Post-ESOP Firms include all firm-year observations for firms with ESOPs. Matched establishments are a group of control establishments, which are not identified as having an ESOP, matched to the ESOP establishments. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	Pre-ESOP Firms	Post-ESOP Firms	Matched Firms	Difference between Pre-ESOP and post-ESOP	Difference between Pre-ESOP and Matched Firms	Difference between Post-ESOP and Matched Firms
Annual payroll (thousands)	2,490.32 [6,807.09]	2,479.67 [6,783.98]	3,130.58 [8,886.49]	insignificant	***	***
Number of Employees	58.41 [136.42]	52.42 [130.07]	65.58 [160.55]	***	***	***
Wages per employee (thousands)	40.52 [30.79]	51.89 [42.11]	46.51 [27.11]	***	***	***
N	206,433	364,820	657,700			

Table III, Panel A: Summary Statistics for ESOP Firms, Pre- and Post-ESOP.

Accounting variables are from Compustat. All variables are winsorized at 1%. Assets and sales are normalized to $2006. Q is fiscal year-end market value of equity plus market value of preferred stock plus total liabilities divided by total assets. Industry-adjusted Q is obtained by subtracting the median Q matched by industry (3-digit SIC code) and year. Means are reported with standard deviations in brackets. Pre-ESOP Firms include firm-year observations before the ESOP is adopted for firms that eventually adopt an ESOP. Post-ESOP Firms include all firm-year observations for firms with ESOPs. Firms with ESOPg5 include all firm-year observations for firms with ESOPs controlling 5% or more of the firm's outstanding common stock at its maximum point. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	Pre-ESOP Firms	Post-ESOP Firms	Pre-ESOPg5 firms	Post-ESOPg5 firms	Difference between Pre-ESOP and Pre-ESOPg5	Difference between Post ESOP and Post-ESOPg5	Difference between Pre- and Post ESOPg5
Operating Income/Assets	0.129 [0.094]	0.117 [0.091]	0.126 [0.078]	0.110 [0.082]	insignificant	**	***
Leverage	0.169 [0.157]	0.209 [0.172]	0.178 [0.159]	0.217 [0.173]	insignificant	insignificant	***
Assets (millions)	5,377.72 [11,953.69]	7,175.55 [13,545.77]	4,394.99 [11,655.32]	6,418.75 [13,039.51]	***	insignificant	***
Sales (millions)	3,124.70 [6,233.29]	4,452.52 [7,728.11]	2,468.98 [5,616.81]	4,255.22 [7,610.72]	insignificant	insignificant	***
Capex/assets	0.070 [0.054]	0.063 [0.048]	0.066 [0.049]	0.062 [0.048]	insignificant	insignificant	*
Industry-Adjusted Q	0.082 [0.561]	0.098 [0.699]	-0.028 [0.389]	-0.029 [0.489]	***	***	insignificant
N	1480	1884	657	1136			

Table III, Panel B: Summary Statistics for Establishments of ESOP Firms, Pre- and Post ESOP.

All variables are winsorized at 1%. Annual payroll and wages per employee are normalized to $2006. Means are reported with standard deviations in brackets. Pre-ESOP Firms include firm-year observations in the sample years before the ESOP is adopted for firms that eventually adopt an ESOP. Post-ESOP Firms include all firm-year observations for firms with ESOPs. Firms with ESOPg5 include all establishment-year observations for firms with large ESOPs controlling 5% or more of the firm's outstanding common stock at its maximum point. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	Pre-ESOP Firms	Post-ESOP Firms	Pre-ESOPg5 Firms	Post-ESOPg5 Firms	Difference between Pre-ESOP and Pre-ESOPg5	Difference between Post ESOP and Post-ESOPg5	Difference between Pre- and Post ESOPg5
Annual Payroll (thousands)	2,490.32 [6,807.09]	2,479.67 [6,783.98]	2,346.84 [6566.68]	2,220.18 [6,407.07]	***	***	***
Number of Employees	58.41 [136.42]	52.42 [130.07]	57.02 [137.19]	48.05 [126.05]	***	***	***
Wages per Employee (thousands)	40.52 [30.79]	51.89 [42.11]	38.41 [31.59]	52.98 [45.66]	***	***	***
N	206,433	364,820	92,834	232,664			

Table IV: Unexplained Wages per Employee around ESOP Initiations.

Average unexplained wages for the sample of firms with small ESOPs never controlling more than 5% of the firm's outstanding stock, large ESOPs controlling 5% or more of the firm's outstanding common stock at its maximum point, and matched control firms. Unexplained wages are reported by relative year to the year of ESOP initiation (year 0) or to the year of match (year 0). Means are reported with standard deviations in parentheses. Unexplained wage is the residual from the following regression: log wages per employee = $a_0 + a_1$ S-Y mean wages + a_2 I-Y mean wages + ε. S-Y mean wage is the log mean wage per employee in the state of location of the establishment and matched by year. I-Y mean wage is the mean log wage per employee matched to the establishment's industry and by year. Both state-year and industry-year mean wages exclude the establishment itself in calculating the mean. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

Panel A: All firms

Relative Year	Small ESOPs	Large ESOPs	Matched Firms
-2	0.049 (0.365)	-0.055 **(**0.444**)**	0.017 (0.420)
-1	-0.026 **(**0.495**)**	-0.113 **(**0.395**)**	-0.002 (0.290)
0	0.054 (0.479)	-0.070 **(**0.383**)**	0.017 (0.477)
1	0.098 (0.579)	-0.048 **(**0.415**)**	0.015 (0.450)
2	0.060 (0.526)	-0.067 **(**0.448**)**	-0.001 (0.510)
diff year -2 vs 1	(+) ***	insignificant	insignificant
diff year -2 vs 2	(+) *	(-) **	(-) ***
diff year -1 vs 1	(+) ***	(+) ***	(+) ***
diff year -1 vs 2	(+) ***	(+) ***	insignificant

Panel B: Not-so-numerous-employee firms

Relative Year	Small ESOPs	Large ESOPs	Matched Firms
-2	-0.072 (0.379)	-0.090 **(**0.433**)**	-0.024 (0.419)
-1	-0.100 **(**0.370**)**	-0.125 **(**0.442**)**	-0.026 (0.310)
0	0.003 (0.327)	-0.116 **(**0.396**)**	-0.034 (0.450)
1	0.052 (0.405)	-0.095 **(**0.406**)**	-0.014 (0.447)
2	0.054 (0.427)	-0.128 **(**0.472**)**	-0.026 (0.518)
diff year -2 vs 1	(+) ***	insignificant	(+) ***
diff year -2 vs 2	(+) ***	(-) **	insignificant
diff year -1 vs 1	(+) ***	(+) ***	(+) ***
diff year -1 vs 2	(+) ***	insignificant	insignificant

Panel C: Numerous-employee firms

Relative Year	Small ESOPs	Large ESOPs	Matched Firms
-2	0.091 (0.350)	-0.018 **(**0.453**)**	0.062 (0.417)
-1	0.000 **(**0.531**)**	-0.101 **(**0.340**)**	0.023 (0.263)
0	0.062 (0.522)	-0.024 **(**0.364**)**	0.079 (0.501)
1	0.113 (0.624)	0.002 **(**0.419**)**	0.057 (0.450)
2	0.063 (0.556)	-0.005 **(**0.412**)**	0.037 (0.494)
diff year -2 vs 1	(+) ***	(+) **	insignificant
diff year -2 vs 2	(-) ***	insignificant	(-) ***
diff year -1 vs 1	(+) ***	(+) ***	(+) ***
diff year -1 vs 2	(+) ***	(+) ***	(+) ***

Table V: Wage Changes following ESOP Initiations.

The dependent variable is log wages per employee. The set of observations includes a sample of ESOP firms and a sample of matched control firms, as described in the text. Columns 1 and 2 include all observations. Columns 3 and 6 include observations at not-so-numerous-employee firms. Columns 4 and 5 include observations at numerous-employee firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. Maxesop is equal to the maximum fractional share ownership attained by an ESOPg5, if an ESOPg5 exists, and zero otherwise. See Table AI for a description of variables. All regressions include establishment- and year fixed effects. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
Sample	All	All	Not-numerous	Numerous	Numerous	Not-numerous
	0.01	0.06	0.20***	0.01	0.01	0.06**
ESOP	(0.02)	(0.04)	(0.07)	(0.04)	(0.03)	(0.03)
		-0.08	-0.29***	0.02		
ESOPg5		(0.05)	(0.08)	(0.05)		
					0.13**	-0.23***
Maxesop					(0.07)	(0.08)
	0.67***	0.68***	0.50***	0.70***	0.70***	0.50***
S-Y mean wages	(0.08)	(0.08)	(0.12)	(0.06)	(0.06)	(0.12)
	0.29***	0.29***	0.29**	0.27***	0.28**	0.29***
I-Y mean wages	(0.07)	(0.07)	(0.13)	(0.04)	(0.05)	(0.13)
	-0.02	-0.02	-0.04	0.01	0.01	-0.04
Establishment age	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.03)
	0.01	0.02	0.03	-0.00	-0.00	0.02
Sales	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)
	-0.04	-0.03	-0.01	-0.10**	-0.10**	-0.03
Leverage	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
	0.32*	0.31*	0.29*	0.40	0.38	0.28*
Ad	(0.18)	(0.17)	(0.15)	(0.72)	(0.71)	(0.15)
	0.03*	0.03*	0.02	0.05***	0.05***	0.02
Ad_missing	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
	0.00	0.00	0.00	-1.04	-1.00	0.00
R&D	(0.00)	(0.00)	(0.00)	(0.87)	(0.88)	(0.00)
	-0.02	-0.02	-0.10	0.03	0.03	-0.10
R&D_missing	(0.04)	(0.03)	(0.06)	(0.03)	(0.03)	(0.07)
	0.06	0.07	0.05	0.10	0.10	0.05
Tangibility	(0.08)	(0.08)	(0.12)	(0.12)	(0.11)	(0.12)
	0.00	0.00	0.00	0.00	0.00	0.00
Cap_labor	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
N	1,045,138	1,045,138	512,216	532,922	532,922	512,216
Adjusted R^2	0.74	0.74	0.70	0.77	0.77	0.70

Table VI: Wage Changes following ESOP Initiation—Interactive Effects of Worker Bargaining Power (WBP).

The dependent variable is log wages per employee. The set of observations includes a sample of ESOP firms and a sample of matched control firms at not-so-numerous-employee firms. Column 6 excludes observations during the first 5 years after an ESOP initiation (for ESOP firms) or after the year of match (for control firms) and observations where the industry year mean wage at year 0 is below the sample median. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. The following variables are included in the relevant regressions but not reported to conserve space: Ind_immobility (column 3), Ind_immobility*WBP (column 3), Ind_immobility*ESOP (column 3), Ind_immobility*ESOPg5 (column 3), indywages_y0 (column 4), indywages_y0*WBP (column 4), indywages_y0*ESOP (column 4), indywages_y0*ESOPg5 (column 4), WBP_y0 (column 5) WBP_y5 (column 6). The following control variables are included in all regressions: state-year mean wages, industry-year mean wages, establishment age, sales, leverage, Ad, Ad_missing, R&D, R&D_missing, tangibility, cap_labor, and establishment- and year fixed effects. See Table AI for a description of variables. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
	-0.07	0.20***	0.00	1.82*	0.01	-0.30
ESOP	(0.15)	(0.07)	(0.11)	(1.10)	(0.06)	(0.24)
	-0.25**	-0.50***	0.01	-0.95*	-0.15	-0.03
ESOPg5	(0.12)	(0.20)	(0.12)	(0.57)	(0.10)	(0.24)
	-0.46	-0.44	-0.03		-0.48	-1.57*
WBP	(0.32)	(0.31)	(0.10)		(0.33)	(0.87)
	0.33**		0.02		0.40*	1.37*
WBP*ESOP	(0.15)		(0.13)		(0.23)	(0.78)
	-0.06	0.24	-0.03		-0.06	0.23**
WBP*ESOPg5	(0.10)	(0.21)	(0.15)		(0.10)	(0.12)
Ind_immobility*W			0.23**			
BP* ESOP			(0.12)			
Ind_immobility*W			-0.02			
BP* ESOPg5			(0.14)			
Indywages_y0 *				0.62*		
WBP*ESOP				(0.37)		
Indywages_y0 *				-0.09		
WBP * ESOPg5				(0.17)		
					-0.17	
WBP _y0*ESOP					(0.21)	
					-0.13	
WBP _y0*ESOPg5					(0.14)	
						-0.88
WBP _y5*ESOP						(0.66)
						-0.45
WBP _y5*ESOPg5						(0.32)
N	512216	512216	512216	512216	512216	112768
Adjusted R^2	0.70	0.70	0.70	0.70	0.70	0.66

Table VII: Changes in Industry-adjusted Q following ESOP Initiations.

The dependent variable is industry-adjusted Q, winsorized at 1%. Q is fiscal year-end market value of equity plus market value of preferred stock plus total liabilities divided by total assets. Industry-adjusted Q is obtained by subtracting the median Q matched by industry (3-digit SIC code) and year. The set of observations includes a sample of ESOP firms and a sample of matched control firms, as described in the text. Columns 1 and 2 include all observations. Columns 3 and 6 include not-so-numerous-employee firms. Columns 4 and 5 include numerous-employee firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP which controls at least 5% of the firm's outstanding common stock at any given time. Maxesop is equal to the maximum fractional share ownership attained by an ESOPg5, if an ESOPg5 exists, and zero otherwise. All regressions include firm- and year fixed effects. See Table AI for a description of variables. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
Firm employment sample	All	All	Not-so-numerous	Numerous	Numerous	Not-so-numerous
ESOP	0.13***	0.23***	0.21**	0.12	0.09	0.13**
	(0.05)	(0.08)	(0.10)	(0.15)	(0.10)	(0.06)
ESOPg5		-0.17*	-0.20*	0.02		
		(0.09)	(0.11)	(0.17)		
Maxesop					0.39	-0.31*
					(0.41)	(0.17)
Sales	0.02	0.02	0.02	0.27	0.27	0.02
	(0.06)	(0.06)	(0.06)	(0.22)	(0.22)	(0.06)
Assets	-0.12**	-0.12*	-0.12**	-0.26	-0.27*	-0.12*
	(0.05)	(0.06)	(0.05)	(0.16)	(0.16)	(0.05)
R&D	0.07***	0.07***	0.07***	9.45	9.73	0.07***
	(0.03)	(0.03)	(0.03)	(7.00)	(7.05)	(0.03)
R&D_missing	0.04	0.04	0.05	0.06	0.06	0.05
	(0.05)	(0.05)	(0.05)	(0.14)	(0.14)	(0.05)
Capex	1.30***	1.33***	1.46***	-0.40	-0.47	1.44***
	(0.24)	(0.24)	(0.25)	(1.39)	(1.36)	(0.25)
Sigma	-0.04	-0.05	0.05	-1.11	-1.08	0.08
	(0.25)	(0.25)	(0.26)	(0.89)	(0.89)	(0.26)
Sigma_missing	-0.13	-0.13	-0.11	-0.12	-0.12	-0.10
	(0.09)	(0.09)	(0.11)	(0.10)	(0.10)	(0.11)
N	5014	5014	4074	940	940	4047
Adjusted R^2	0.56	0.56	0.57	0.59	0.59	0.57

Table VIII: Changes in Industry-adjusted Q following ESOP Initiation—Interactive Effects of Worker Bargaining Power (WBP).

The dependent variable is industry-adjusted Q, winsorized at 1%. Q is fiscal year-end market value of equity plus market value of preferred stock plus total liabilities divided by total assets. Industry-adjusted Q is obtained by subtracting the median Q matched by industry (3-digit SIC) and year. The set of observations includes a sample of not-so-numerous ESOP firms and a sample of matched control firms, as described in the text. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP which controls at least 5% of the firm's outstanding common stock at any given time. The following control variables are included in the regressions: assets, sales, R&D, R&D_missing, capex, Sigma, Sigma dummy, and firm- and year fixed effects. See Table AI for a description of variables. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4
ESOP	0.88***	0.21**	0.99**	0.21**
	(0.34)	(0.10)	(0.48)	(0.10)
ESOPg5	-0.98**	-0.34	-1.09**	-0.33
	(0.50)	(0.38)	(0.56)	(0.35)
Equal weighted WBP	0.00	-0.05		
	(0.15)	(0.16)		
Equal weighted WBP*ESOP	-0.82*			
	(0.43)			
Equal weighted WBP*ESOPg5	0.96	0.18		
	(0.60)	(0.43)		
Payroll weighted WBP			-0.15	-0.22
			(0.20)	(0.20)
Payroll weighted WBP*ESOP			-0.91*	
			(0.53)	
Payroll weighted WBP*ESOPg5			1.03*	0.16
			(0.63)	(0.39)
N	4074	4074	4074	4074
Adjusted R^2	0.57	0.57	0.57	0.57

Table IX: Total Factor Productivity (TFP) Changes following ESOP Initiations.

The dependent variable is TFP. The sample covers only establishments in manufacturing industries. Columns 1 and 2 include all observations. Column 3 and 6 include not-so-numerous-employee firms. Columns 4 and 5 include numerous-employee firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. See Table AI for a description of variables. All regressions include establishment- and year fixed effects. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
Firm employment sample	All	All	Not-so-numerous	Numerous	Numerous	Not-so-numerous
ESOP	0.80*** (0.24)	0.34* (0.20)	0.90*** (0.25)	0.20 (0.19)	0.47 (0.30)	0.73*** (0.23)
ESOPg5		0.70*** (0.28)	-0.17 (0.29)	0.87*** (0.26)		
Maxesop					4.81** (1.97)	1.16 (2.29)
$Maxesop^2$					-4.79*** (1.97)	-2.05 (2.17)
Sales	-0.08 (0.06)	-0.07 (0.06)	0.09 (0.08)	-0.07 (0.15)	-0.08 (0.15)	0.09 (0.08)
Leverage	-0.09 (0.19)	-0.11 (0.19)	-0.09 (0.21)	-0.04 (0.40)	-0.01 (0.41)	-0.07 (0.21)
Ad	1.94 (2.58)	1.79 (2.49)	0.41 (2.30)	1.37 (5.44)	1.77 (5.83)	0.46 (2.30)
Ad_missing	0.31 (0.26)	0.38 (0.27)	0.15 (0.40)	0.45 (0.30)	0.36 (0.29)	0.16 (0.40)
R&D	-0.12*** (0.02)	-0.12*** (0.02)	-0.11*** (0.01)	-19.06*** (6.03)	-18.85*** (6.05)	-0.10*** (0.01)
R&D_missing	-0.13 (0.22)	-0.12 (0.22)	-0.02 (0.22)	-0.65 (0.56)	-0.66 (0.56)	0.02 (0.22)
Tangibility	-0.29 (0.53)	-0.40 (0.50)	-0.67 (0.49)	0.13 (0.91)	0.34 (1.00)	-0.68 (0.49)
Cap_labor	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Establishment age	0.39* (0.21)	0.38* (0.20)	0.32 (0.27)	0.55* (0.29)	0.56* (0.30)	0.31 (0.28)
N	68671	68671	30431	38240	38240	30431
Adjusted R^2	0.47	0.47	0.50	0.47	0.47	0.50

Table X: Employment Changes following ESOP Initiations.

The dependent variable is log employee per firm. The set of observations includes a sample of ESOP firms and a sample of matched control firms, as described in the text. Columns 1 and 2 include all observations. Column 3 includes not-so-numerous-employee firms. Column 4 includes numerous- employee firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. See Table AI for a description of variables. Firm- and year fixed effects are included. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4
Firm employment sample	All	All	Not-so-numerous	Numerous
	0.02	0.06	0.21***	-0.47**
ESOP	(0.06)	(0.07)	(0.07)	(0.20)
		-0.06	-0.11	-0.00
ESOPg5		(0.07)	(0.09)	(0.09)
	0.19**	0.19**	0.18*	0.54*
Sales	(0.10)	(0.10)	(0.10)	(0.29)
	0.38***	0.38***	0.37***	0.15
Assets	(0.08)	(0.08)	(0.08)	(0.31)
	0.00	0.00	0.00	-6.75**
R&D	(0.01)	(0.01)	(0.01)	(3.43)
	-0.07	-0.07	-0.08	-0.00
R&D_missing	(0.09)	(0.09)	(0.11)	(0.10)
	0.43	0.44	0.38	1.51
Capex	(0.53)	(0.53)	(0.53)	(1.90)
	-0.01	-0.01	-0.03	0.25
Sigma	(0.25)	(0.25)	(0.24)	(1.33)
	-0.20	-0.20	-0.21	-0.05
Sigma_missing	(0.14)	(0.14)	(0.17)	(0.20)
N	5014	5014	4074	940
Adjusted R-squared	0.93	0.92	0.9	0.54

Table XI: Wage and Industry-adjusted Q Changes following ESOP Initiations—Interactive Effects of Cash Constraints and Business Combination Statutes (BCS).

The dependent variable in columns 1-3 is log wages per employee. The dependent variable in columns 4-6 is industry-adjusted Q, winsorized at 1%. The set of observations includes a sample of ESOP firms and a sample of matched control firms, as described in the text. Columns 1 and 4 include all observations. Columns 2, 3, 5 and 6 include observations at numerous-employee firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. The following control variables are included in the regressions in columns 1-3: state-year mean wages, industry-year mean wages, establishment age, sales, leverage, Ad, Ad_missing, R&D, R&D_missing, tangibility, cap_labor, and establishment- and year fixed effects. The following control variables are included in the regressions in columns 4-6: assets, sales, R&D, R&D_missing, capex, Sigma, Sigma dummy, and firm- and year fixed effects. See Table AI for a description of variables. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
Firm employment sample	All	Numerous	Numerous	All	Numerous	Numerous
	0.06	0.02	0.03	0.23***	0.12	0.03
ESOP	(0.04)	(0.04)	(0.03)	(0.08)	(0.15)	(0.12)
	-0.12**	0.02		-0.14	0.01	
ESOPg5	(0.06)	(0.05)		(0.09)	(0.17)	
	-0.00*			0.00		
CashConst_y0	(0.00)			(0.00)		
	-0.03***			0.01*		
CashConst_y0 *ESOPg5	(0.01)			(0.00)		
		-0.00	-0.00		0.03	0.04
BCS_Blk		(0.02)	(0.03)		(0.10)	(0.12)
		0.05**	0.05**		0.10	0.05
BCS_Board		(0.02)	(0.02)		(0.08)	(0.06)
			-0.04			0.01**
Maxesop			(0.10)			(0.00)
			-0.02			0.08
BCS_Blk *ESOP			(0.05)			(0.19)
			0.20*			-0.01
BCS_Blk *Maxesop			(0.12)			(0.00)
N	1045138	532922	532922	5014	940	940
Adjusted R^2	0.74	0.77	0.77	0.56	0.58	0.59

Appendix:

AI: Variable Definitions.

Variable Name	Definition
S-Y mean wages	Log mean wage per employee in the state of location of the establishment and matched by year.
I-Y mean wages	Log mean wage per employee matched to the establishment's industry and by year.
Establishment age	Log of the number of years since an establishment is first observed in the SSEL.
Sales	Log of sales in millions of 2006 dollars.
Leverage	Total debt divided by total assets.
Ad	Advertising expenditures/sales. It is 0 if advertising expenditures are missing but sales are reported.
Ad_missing	A dummy variable equal to 1 if the firm does not report advertising expenditures but reports sales.
R&D	R&D expenditures/sales. It is 0 if R&D expenditures are missing but sales are reported.
R&D_missing	A dummy variable equal to 1 if the firm does not report R&D expenditures but reports sales.
Tangibility	PP&E divided by total assets.
Cap_labor	PP&E (in millions) divided by number of employees (in thousands).
CashConst_y0	Cash constraint index at the year of ESOP initiation (for ESOP firms) or at the year of match (for control firms).
Maxesop	Equal to the maximum fractional share ownership attained by an ESOPg5, if an ESOPg5 exists, and zero otherwise.
BCS_Blk	Equal to 1 if the firm's state of incorporation has a business combination statute (BCS) that requires the bidder must wait three to five years before pursuing the takeover if a block of investors unaffiliated with management, such as an ESOP, vote against a tender offer.
BCS_Board	Equal to 1 if the firm's state of incorporation has a business combination statute (BCS) that requires board approval for any takeover.
Sigma	The standard deviation of residuals from a CAPM model estimated over the previous fiscal year.
Sigma_missing	Dummy variable equal to 1 if the data to estimate Sigma is available, and zero otherwise.
Ind_immobility	Inter-industry immobility measure using DOL occupational data (Donangelo, 2012).
Indywages_y0	Industry-year mean wages at the year of ESOP initiation (for ESOP firms) or at the year of match (for control firms.), winsorized at 1%.
Sales change	2-year historic geometric average of annual sales growth, measured at the firm level.
Asset utilization change	2-year historic geometric average of annual growth in the sales to book value of assets ratio, measured at the firm level.
Industry sale change	2-year historic geometric average of annual industry sales growth, measured at the 3-digit SIC.
Capex	Capital expenditures divided by total assets.

AII: Wage Changes following ESOP Initiations with Additional Controls for Growth Opportunities.

The dependent variable is log wages per employee. The observations include establishments belonging to not-so-numerous-employee firms with ESOPs and their matched control firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. Missing observations for sales change, asset utilization change, industry sales change, and Q are replaced with the sample median to avoid changing samples. See Table AI for a description of control variables. All regressions include establishment- and year fixed effects. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4
ESOP	0.20***	0.20***	0.20***	0.20***
	(0.07)	(0.07)	(0.07)	(0.07)
ESOPg5	-0.29***	-0.29***	-0.29***	-0.29***
	(0.08)	(0.08)	(0.08)	(0.08)
S-Y mean wages	0.50***	0.50***	0.50***	0.50***
	(0.12)	(0.12)	(0.12)	(0.12)
I-Y mean wages	0.29***	0.29**	0.29**	0.29**
	(0.13)	(0.13)	(0.13)	(0.12)
Establishment age	-0.04*	-0.04	-0.04	-0.04
	(0.03)	(0.03)	(0.03)	(0.03)
Sales	0.03*	0.03*	0.03*	0.03*
	(0.01)	(0.01)	(0.01)	(0.01)
Leverage	-0.01	-0.01	-0.01	-0.01
	(0.04)	(0.04)	(0.04)	(0.04)
Ad	0.28*	0.30*	0.28*	0.27*
	(0.15)	(0.16)	(0.15)	(0.15)
Ad_missing	0.02	0.02	0.03	0.02
	(0.02)	(0.02)	(0.02)	(0.02)
R&D	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
R&D_missing	-0.10	-0.10	-0.10	-0.10
	(0.06)	(0.06)	(0.06)	(0.06)
Tangibility	0.05	0.05	0.05	0.07
	(0.12)	(0.12)	(0.12)	(0.12)
Cap_labor	0.00	0.00	0.00	0.00
	(0.00)	(0.00)	(0.00)	(0.00)
Sales change	-0.00			
	(0.00)			
Asset utilization change		0.00		
		(0.00)		
Industry sale change			0.00	
			(0.00)	
Q				0.03**
				(0.01)
N	512216	512216	512216	512216
Adjusted R^2	0.74	0.7	0.7	0.7

AIII: Difference-in-Differences Control Tests.

In this table we follow an alternative estimation procedure suggested by Bertrand, Dufflo, and Mullainathan (2004). In step 1, we estimate the following regression:

$$Wages_{it} = \eta_t + \theta_i + \alpha_0 + \beta Z_{it} + \mu_{it}.$$

Subscripts *i* and *t* indicate firm i and year t, and η_t *and* θ_i are year- and firm fixed effects. Each observation reflects a weighted mean of all establishment observations for that firm-year. We use three different weighting schemes: employee-weighted in columns (1) and (2); equal-weighted in columns (3) and (4); and payroll-weighted in columns (5) and (6). Z_{it} is a set of control variables, which include S-Y mean wages, I-Y mean wages, Establishment age, Sales, Leverage, Ad, Ad_missing, R&D, R&D_missing, Tangibility, and Cap_labor. See Table AI for a description of variables. This first step regression is estimated on the full sample of both ESOP and control firms for all not-so-numerous-employee firms.

In step 2, we use the estimated residuals from step 1, but retain these values for only ESOP firms, thus, our sample in step 2 is limited to firms which either currently have an ESOP or will adopt an ESOP in the future. In addition, we use only one observation for each firm for the pre-ESOP period and one observation for each firm for the post-ESOP period, where each observation reflects a weighted mean of all establishment observations for that firm-year. We then regress the residuals on the ESOP dummy variables. ESOP is a dummy variable, which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable, which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. Coefficients are reported with standard errors in parentheses. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
ESOP	0.04***	0.04***	0.05***	0.04***	0.06***	0.05**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)
ESOPg5		-0.00		0.01		0.01
		(0.01)		(0.01)		(0.02)
N	321	321	321	321	321	321
R^2	0.08	0.08	0.08	0.08	0.04	0.04

AIV: TFP Changes following ESOP Initiations—Alternate Measure of TFP.

In this table we report results with an alternate approach to estimating total factor productivity (TFP), the dependent variable. As in Maksimovic, Phillips, and Yang (2013), we account for differences in firm-level productivity by including firm fixed effects in the TFP equation:

$$\text{Value Added}_{ijkt} = \alpha_0 + \alpha_1 \text{labor}_{ijkt} + \alpha_2 \text{capital}_{ijkt} + k + \varepsilon_{ijt}.$$

Individual establishments are indexed by i, industries by j, firms by k, and years by t. Value Added, labor, and capital are log transformed and measured as described in the text. In order to estimate the firm fixed effects, we run the above regression using the past five years of data to predict TFP for the current year. For each industry-year, TFP is standardized by subtracting the industry-year mean TFP and dividing by the industry-year standard deviation of TFP. Columns 1 and 2 include all observations. Columns 3 and 6 include not-so-numerous-employee firms. Columns 4 and 5 include numerous-employee firms. ESOP is a dummy variable which takes the value of 1 if the firm has an ESOP. ESOPg5 is a dummy variable which takes a value of 1 if the firm has an ESOP that controls at least 5% of the firm's outstanding common stock at any given time. See Table AI for a description of variables. All regressions include establishment- and year fixed effects. Coefficients are reported with standard errors in parentheses. All standard errors are clustered at the firm level. "*", "**", and "***" indicate statistical significance at 10%, 5%, and 1%, respectively.

	1	2	3	4	5	6
Firm employment sample	All	All	Not-so-numerous	Numerous	Numerous	Not-so-numerous
ESOP	0.07	0.01	0.18*	-0.02	-0.02	0.14*
	(0.05)	(0.07)	(0.10)	(0.07)	(0.07)	(0.09)
ESOPg5		0.10	-0.09	0.15*		
		(0.08)	(0.11)	(0.08)		
Maxesop					1.88***	-0.20
					(0.63)	(0.87)
$Maxesop^2$					-2.00***	-0.15
					(0.62)	(0.83)
Sales	0.03	0.03	0.06**	0.05	0.05	0.06*
	(0.02)	(0.02)	(0.03)	(0.05)	(0.05)	(0.03)
Leverage	-0.08	-0.09	-0.11	-0.11	-0.13	-0.10
	(0.07)	(0.07)	(0.09)	(0.11)	(0.11)	(0.09)
Ad	0.47	0.45	0.38	0.46	0.57	0.39
	(0.84)	(0.84)	(0.63)	(1.98)	(1.99)	(0.63)
Ad_missing	0.07	0.08*	0.19*	0.10	0.08	0.19*
	(0.05)	(0.05)	(0.11)	(0.06)	(0.07)	(0.11)
R&D	0.00	-0.08***	-0.08***	-4.63***	-4.49***	-0.08***
	(0.05)	(0.00)	(0.00)	(1.69)	(1.62)	(0.00)
R&D_missing	0.00	0.00	0.02	-0.08	-0.08	0.03
	(0.05)	(0.05)	(0.07)	(0.10)	(0.10)	(0.07)
Tangibility	-0.07	-0.08	-0.05	-0.20	-0.20	-0.05
	(0.15)	(0.15)	(0.18)	(0.25)	(0.25)	(0.18)
Cap_labor	0.00***	0.00***	0.00***	0.00*	0.00*	0.00***
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Establishment age	0.21**	0.21**	0.03	0.39***	0.39	0.03
	(0.11)	(0.11)	(0.18)	(0.10)	(0.10)***	(0.19)
N	68671	68671	30431	38240	38240	30431
Adjusted R^2	0.42	0.42	0.41	0.44	0.44	0.41