Inside the Firm: How Employee Knowledge and Ownership Shape Corporate Decisions

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Abstract

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A large share of a firm's day to day operations is carried out by rank and file employees, yet little is known about how their information and incentives shape important corporate outcomes. In the first chapter, I assemble a new dataset on Employee Stock Purchase Plan activity for a set of public firms and ask whether non executives provide useful signals in mergers and acquisitions. I find that acquirer abnormal returns and combined acquirer target synergies at announcement both increase with the target firm's non executive ESPP purchase ratio, suggesting that employee trading behavior contains information about the quality of the deal.

The second chapter examines whether employee ownership helps protect firms from data breaches. Employees can observe and monitor one another in ways managers cannot, and ownership strengthens this incentive. Using the share of active ESOP participants as a measure of monitoring strength, I find that firms with higher active ratios are less likely to experience data breaches, although this effect weakens in larger firms. I also show that firms increase their active ratio by about three percent after their first breach, and by six to ten percent following breaches with impacts at least as large as the twenty fifth percentile in their industry. Overall, the findings highlight the role of non executive employees in shaping acquisition outcomes and helping firms manage operational risks.

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Paper 1

University of Washington

Abstract

The role of target firm non-executive employees' information in M&As

Mahtab Karimi

In this work, I explore whether a firm looking for a takeover target can use a target firm's non-executive information as a signal of the potential worthiness of the acquisition. Specifically, I use the stock purchased by rank-and-file employees in the Employee Stock Purchase Plan (ESPP) of the target firm as the signal. Utilizing a novel dataset of all public target firms' ESPP purchases since 2000, I find that the acquirer's abnormal return at merger and acquisition (M&A) announcements increase with the target firm's non-executive ESPP purchase ratio. Furthermore, acquisition synergies, measured as the acquirer-target combined cumulative abnormal returns at M&A announcements, also increase with the target's non-executive ESPP purchase ratio. However, I do not find a significant effect of the target's ESPP purchase ratio on the deal premium. Overall, my findings suggest that valuable information held by non-executives of a target firm, prior to the M&A announcement, to some extent serves as a credible signal for acquisition outcomes.

1.1 Introduction

Does the valuable information held by non-executive employees of a target firm signal the target's synergy potential in mergers and acquisitions? The current literature of corporate finance primarily focuses on insiders and shareholders as the main sources of information and the decision-making forces in firms. Intuitively and naturally, non-executive employees, as rational agents who have spent considerable time working within a company, should also possess valuable knowledge about the firm, different from the information set available to insiders. This source of information about the firm's internal operations could be a complementary signal for investors to ease the information asymmetry involved in their investment decisions.

Employee Stock Purchase Plans (ESPP) offer a framework to investigate the influence of non-executive employees on firms' investment decisions. An ESPP is a stock-based compensation plan offered by some companies to their employees. The plan lets employees set aside a percentage of their salary to buy the company's shares at a lower price. The key point is unlike stock options or employee stock ownership plans (ESOPs), where the company determines the amount of stock allocated to each employee; in ESPPs, the company only sets up the plan and its broad terms. The extent of participation in the plan is entirely up to the employees' discretion. Therefore, the decision of employees to participate in ESPPs, made at their discretion, could effectively reflect their knowledge about the company. Babenko and Sen (2016) utilized this approach and showed that the ESPP participation ratio predicts the abnormal return over a 12-month horizon, implying that these employees possess value-relevant information about their firms.

The trading activities of corporate insiders, as the literature already knows, can be a signal of various potential firm-level events and the outcome of M&As. Whether non-executive employees information can be used similarly to predict the success of future M&As, as insider information is used, is not clear ex-ante. Therefore, I utilize the stock purchases made by non-executive employees through employee stock purchase plans as an indicator of their knowledge about the company and investigate whether the degree of employee purchases in ESPPs at target firms can help mitigate the 'lemons problem' in the M&A market.

Suppose non-executive employees of the target have information useful for an acquirer to determine the target firm's potential to generate acquisition benefits, whether or not the employees are aware of a potential acquisition. Babenko and Sen (2016) suggest that employees have valuable information about earnings breaks, sales growth, and R&D outcomes.

This means that, after controlling for insider purchases, management information, and other corporate events, a higher ESPP purchase ratio rate correlates with several positive outcomes in the coming year. These include a lower likelihood of a break in earnings, higher sales growth, an increase in the number of filed patents, and a greater number of citations on those patents in the following year. In such a scenario, if ESPP purchase ratio is used to infer the potential gains from an acquisition, an acquirer taking over a target with higher ESPP purchase is likely to experience greater gains from the acquisition.

Thus, the observable ESPP purchase ratio of target employees before M&As helps acquirers make more efficient acquisitions. This "signaling" or "informativeness" hypothesis bears three sets of testable predictions relating to acquirer returns, acquisition synergies, and takeover premiums. First, The bidders of firms with a higher ESPP purchase ratio experience a larger announcement abnormal returns. Second, The purchase ratio is positively correlated with the deal's synergy. Third, if an acquirer uses ESPP purchase ratio of the target to predict acquisition gains and synergies and is willing to pay a larger premium to a target with a higher ESPP, then the takeover premium that the acquirer pays will increase in the target purchase ratio.

I test the three hypotheses using 1,076 acquisitions that occurred among U.S. public target firms from 2000 to the end of 2022, focusing on target ESPP purchases reported within a one-year period prior to the original announcement of the acquisition. For each acquisition, I calculate two measures of the target ESPP purchase ratio: one as the ESPP purchase value relative to the target firm's market value, and the other as the ESPP purchase value per employee of the target. This information is retrieved from the closest 10-K form filed before the deal announcement.

First, I find that the acquirer's abnormal return at the M&A announcement increases with the ratio of target firm ESPP purchase relative to the target market value. Second, acquisition synergies, measured as the acquirer-target combined cumulative abnormal returns at the M&A announcement, increase with target ESPP purchase relative to its market value. Third, I find that both the offer price paid by the acquirer relative to the target's stock price and the target's abnormal return at the deal announcement are positively correlated with target ESPP purchase relative to its market value; however, this result is not significant. Overall, the findings indicate that the acquirer enjoys higher returns and synergies from acquiring a target with higher ESPP purchases, although it is not clear if the acquirer pays a higher takeover premium to such targets.

My research contributes to the literature in the following ways. First and foremost, it adds to the expanding body of research on the advantages of stock-based compensation for companies, as seen in works like Chang et al. (2015), Hoberg and Phillips (2010), Kim and Ouimet (2014). My approach proposes a new perspective on ESPPs, potentially increasing interest among insiders and shareholders in implementing these plans. Secondly, ESPPs haven't been as widely studied as ESOPs and stock options, likely due to limited data. My study addresses this gap by gathering data on ESPP participation, a novel dataset, and explores the role of these plans. Thus, this work contributes to the growing research on ESPPs, following studies like Babenko and Sen (2016), Ouimet and Tate (2020), Babenko and Sen (2014).

Third, my research contributes to the growing literature investigating lower-rank employees' value-relevant information. It complements the studies of Babenko and Sen (2016), Greene et al. (2020), and Huang et al. (2020) by showing that non-executive employees have valuable information about M&A benefits. Fourth, I add to the literature providing evidence that outsiders exploit insider trading as a signal to reduce adverse selection-driven problems before making an investment(e.g., Suk and Wang (2021), McNichols and Stubben (2015), Even-Tov et al. (2022), Hoberg and Phillips (2010), Cunningham et al. (2021), Badertscher et al. (2011)). I depart from this literature by highlighting that non-executive employees' private information is as important as executives' trading signals to interpret a firm's internal conditions.

The rest of the paper proceeds as follows. Section 1.2 reviews the related literature and develops testable hypotheses. Section 1.3 provides background and details about ESPP plans and their relation to insider trading. Section 1.4 describes the sample selection procedure and variable measurements. Section 1.5 discusses empirical analyses and results. Section 1.6 covers robustness tests, Section 1.7 investigate two extensions and Section 1.8 concludes the paper.

1.2 Literature review and hypothesis development

According to Greene et al. (2020), the extant literature categorizes the relationship between takeovers and non-financial stakeholders into: 1) the effects of mergers on the target's labor productivity, employees' stock options, worker retention, and employment growth (Li (2013), Babenko et al. (2021), Ouimet and Zarutskie (2020)); 2) the effects of merging firms' labor similarity on merger outcome (Lee et al. (2018), Duchin et al. (2021));

3) the relationship between labor market laws and the merger outcome (John et al. (2015), Chen et al. (2021)); 4) the relationship between labor unions and merger outcomes (Tian and Wang (2015), Lie and Que (2019)); 5) the effects of takeovers on firms' R&D activities (Li and Wang (2020), Seru (2014)); 6) the role of pension plans in merger outcomes(Rauh (2006), Cocco and Volpin (2013)); 7) the role of employees' ownership in merger outcome through the firm's governance (Pagano and Volpin (2005), Masulis et al. (2020)).

The studies mentioned above overlook the informative role of lower rank employees in corporate takeovers; however, links 6 and 7 are closer to the central theme of my research. Employees' ownership in the firm provides workers with cash flow and voting rights. Pagano and Volpin (2005) demonstrate that to deter hostile takeovers, a manager who owns a small equity stake uses two ways to ally employees with him: he offers them high wages or membership in an employee stock ownership plan. Doing so aligns the incentives of employees and the manager, suggesting that the firm's workers act against raiders and defend an incumbent CEO. Masulis et al. (2020) empirically test the findings of Pagano and Volpin (2005) and provide evidence consistent with its model. The study shows that employee voting rights derived from membership in ESOP plans allow managers to pursue value-destroying acquisitions.

Rauh (2006) empirically shows managers offer employees ownership in DC plans to encourage workers to vote for them in proxy contests; employees are interested in such plans due to longer job retention. Cocco and Volpin (2013) find that the DB pension plans can be a source of uncertainty about a given firm's liabilities, leading to an increase in asymmetric information. As a result, in the case of attempting a takeover, DB-sponsoring firms are more likely to use cash as a payment method. Moreover, these firms are less likely to raise equity through seasoned equity offerings.

It is worth noting that the studies mentioned above treat the employee ownership plans-mostly ESOP- as tools for CEOs to achieve their goals. My study, however, considers these plans as mediums for non-executive workers to respond to the firm's events and operations. Further, the structure of ESPP plans allows workers to have an active role in corporate decisions, which is less available in pension plans. Based on Babenko and Sen (2016), lower-rank employees have information about earnings breaks, the firm's sales growth, and R&D outcomes(number of patents). Similarly, after studying reviews from current employees, Green et al. (2019) show that changes in employer ratings are associated with sales growth and earnings announcement, supporting the wisdom of employees.

The studies mentioned above collectively suggest that treating the non-executive employees' ESPP purchase ratio as a signal of the firm's fundamentals is reasonable. Moreover, this signal contains information that is also important for an acquirer in assessing a potential deal's performance. Therefore, a bidder might exploit this signal as a source to reduce adverse selection problems and gather more information on the target's internal operations. (Indeed, this signal is credible whether or not the employees are aware of an acquisition.) As a result, the non-executives purchase ratio helps the acquirer to have a better valuation of the deal and make a more efficient acquisition. More precisely, the acquirer of a target with a higher purchase ratio rate experiences a larger announcement abnormal return. Also, Such a deal generates a higher synergy. Moreover, due to the less severe information asymmetry and uncertainty about the target, the target firms with a higher purchase ratio are perceived as better candidates to generate higher profitability; therefore, such firms experience a greater announcement abnormal return.

Suk and Wang (2021), the closest work in the literature to my work, focuses on the target firm's insider trading as a signal of the firm's potential synergy; in contrast, I study the signal in non-executive employees' information. Although these two signals are similar in providing information about the firm's internal operations, they could still be quite different in some aspects worth mentioning here.

First, Babenko and Sen (2016) don't find a correlation between the ESPP purchase ratio of the employees and future announcements of open market share repurchases, declarations of dividend increases by the board, and announcements of seasoned equity offerings. So they suggest that employees are unlikely to get their information from management. Given this evidence, the employees' purchase ratio offers an extra and separate source of relevant information for acquirers. Second, in spite of insider trading, lower-level employees are subject to fewer regulations and restrictions in informed transactions, which might make their signals more informative. Third, the majority of firms offering ESPP plans are in the tech industry; these firms closely guard their information to protect their market power(Chondrakis et al. (2021)), so lower-level employees' signals might ease some of the challenges technology acquirers usually face. All in all, these points highlight the advantages of non-executive employees' signals in announcing an M&A.

Moreover, acquisitions are risky decisions; the adverse selection problem associated with the target threatens the bidder's deal performance severely, forcing a given acquirer to be involved in an information-seeking process about the target. A branch of the literature provides evidence showing this link. McNichols and Stubben (2015) find that higher-quality accounting information leads to better bidding decisions in acquisitions, meaning that acquirers capture a greater portion of acquisition gains by paying less for target firms. Raman et al. (2013) conclude that targets' earnings quality affects bidders' takeover decisions, particularly in cases of large asymmetric information between targets and bidders. Even-Tov et al. (2022) find that purchasing representations and warranties insurance reduce target valuation uncertainty and mitigate information asymmetry in acquisitions.

Likewise, Moeller et al. (2004) provide evidence that managers of large firms pay more for acquisitions. Moreover, they show that synergy returns are significantly higher for acquisitions by small firms, meaning that large acquirers reveal more adverse information than small acquirers. Morellec and Zhdanov (2005) and Moeller et al. (2007) show that abnormal acquirer return is negatively related to information asymmetry. Cai et al. (2016) provide evidence that the presence of a common auditor leads to a higher quality M&A deal. Additionally, the paper finds that this effect is stronger when the firms face a higher uncertainty before acquisition.

To summarize, M&As are risky investment decisions in an environment with high levels of asymmetric information. Therefore, acquirers, to decrease this risk, would like to be involved in an information-seeking process about the target. Target insiders' trading could be a signal to address the 'lemon problem'; however, non-executives' purchases in ESPP plans have been shown to bear value-relevant information about the firm's operations, different from insiders' information. Therefore, if employees' purchases in the ESPP plan are used to reveal information about the acquisition's profit or the potential synergy of the deal, then:

- $H1$: The bidders of firms with a higher ESPP purchase measure experience a greater announcement abnormal return.
- $H2$: Deals where targets have higher levels of ESPP purchases experience greater synergy.
- $H3$: Targets with higher levels of ESPP purchases experience greater deal premium.

Before delving into the technical analysis, I will provide background information on ESPP plans in the next section.

1.3 Background and empirical considerations

1.3.1 Background of ESPPs

Employee stock purchase plan or ESPP is a stock-based compensation plan offered by some companies to their employees. The plan lets employees set aside a percentage of their salary to buy the company's shares at a lower price.

For example, company A decides to set up an ESPP. After announcing the plan to employees, interested eligible employees would enroll in the plan, deciding how much of their salary they want to use for the stock purchase; they can't exceed a certain amount set by the company. This enrolment usually happens before what's called an "offering period." The offering period is when employees contribute money to the plan, and within this time frame, there might be multiple "purchase periods." These are specific dates when the company takes the withheld money and uses it to buy shares for the employees.

Referring back to the example, Company A may have an offering period that runs from January 1 to December 31, 2021. Within this year, there could be four purchase periods: January to March, March to June, June to September, and September to December. Each purchase period has a purchase day at its end: the end of March, June, September, and December. So, four times in that year, the company would use the employees' saved money to buy shares at a discounted rate.

An important part of the ESPP plan is the purchase price. Typically, this price is set at 15%(discount rate) of the lower value between the company's stock price on the first day of the offering period and the purchase day. While the discount rate can vary across companies, it generally ranges between 5 to 15 percent. On the purchase day, the company determines the buying price for the employees. They then divide each employee's contribution by this price and allocate the corresponding number of shares to the employee's account. In this study, I refer to the total dollar value of all employees' contributions at a firm over one year as 'ESPP purchase.'

There are several technical details about ESPPs; I will cover the most important ones here:

- Qualified under IRS Section 423: Plans can be either qualified or unqualified under IRS Section 423. If they are qualified, they will receive tax treatment advantages in return for adhering to the restrictions imposed by this section. Details on tax treatment are provided in the appendix.

- A committee of board members is in charge of adopting the plan, suspending it, or making changes to its terms.

- If a plan is qualified under IRS Section 423, no employee can have a total purchase value exceeding \$25,000 in a year. Some companies also have restrictions on the purchase level for each purchase period.

- The committee in charge can determine a minimum and maximum limit for annual purchases by employees. This threshold is usually specified as a percentage of their salary. For example, no employee can have a total purchase exceeding 10% and less than 1% of their salary.

- The stock purchase price in the plan is usually a function of the discount and one of the following: the minimum stock price on the first day of the offering period and the purchase date, the minimum stock price on the first and last day of the purchase period, or the stock price on the purchase date. If the purchase price is determined by the minimum of the first day of the offering period and the purchase date, then the ESPP plan has a look-back feature.

- Some plans have a required holding period, meaning that employees must hold the stocks for a specific length of time after purchase before selling them.

- All eligible employees, including executives, can participate in the plan unless otherwise specified by the committee in charge. Even in an IRS Section 423 qualified plan, if not specified, executives can purchase stock under the plan.

Companies report the number of shares purchased under the plan or the total dollar value of employee purchases under the plan in their annual 10-K forms. To gain insight, I collect data on all S&P 500 and S&P 400 Mid-cap firms offering ESPPs within the time horizon of 2015 to 2021 where firms report ESPP-relevant information in their 10-K forms. I also extract the general terms of the plan from proxy forms.

The collected data consists of 1,650 ESPP firm-year observations covering 268 unique firms. As shown in Table 1.1, in this sample, 98% of firms are qualified under IRS section 423 ($Tax423$). On average, employees pay 87% of the market price to buy the company's stock ($Price\,after\,discount$). Additionally, 70% of the firms have a look-back feature (*Lookback*) with a purchase period of 5.5 months (*Purchase period*).

Furthermore, the average dollar value of the annual purchase limit per employee is \$25,826 (*Annual limit*), and the maximum contribution of each employee is 13.16% of their salary (*Compensation limit*). Most firms offering the plan do not provide information on the holding period or whether officers are excluded from participation in the plan. However, among those that do, they require a 7.5-day holding period (*Holding period*), and 36% explicitly exclude officers from participation in the plan. On average, employees have bought 936,000 shares of their company's stock under ESPP, with a mean value of $48 million.

Table 1.1:
descriptive statistics for the ESPP plans of S&P 500, S&P 400 Midcap
This sample consists of 1,650 firm-year observations and 268 unique firms. The table presents descriptive statistics for the sample of S&P 500, S&P 400 Midcap for which I was able to obtain data and have an employee stock purchase plan. ***Tax423*** is equal to 1 if the plan is tax-qualified by the IRS. ***Price after discount*** is the percentage discount at which employees can buy stock. ***Lookback*** is equal to 1 if the price at which employees can buy the stock is estimated based on the lower of prices at the purchase date and a function of the price on the dates before the purchase date. ***Purchase period*** is the period over which payroll deductions are made for the purchase of shares through ESPP. ***Adoption year*** is the year in which a firm first adopts the plan. ***Annual limit*** equals the maximum value of total purchases an employee can make in the plan over a year. ***Compensation limit*** is the maximum percentage of compensation the employees are allowed to contribute to the plan. ***Holding period*** is the minimum number of days the employee is required to hold the stock. ***Exclude officer*** is equal to 1 if officers are not allowed to contribute to the plan. ***Number of ESPP shares purchased*** is the number of shares employees purchase under the plan during a fiscal year. ***Value of ESPP purchases*** is the total dollar amount of shares purchased by employees during the fiscal year.

Variable	***Obs***	***Mean***	***Std. Dev.***	***Min***	***Max***
Tax423(indicator)	1407	0.98	0.13	0	1
Price after discount (%)	1582	0.87	0.07	0	0.95
Lookback(indicator)	1515	0.7	0.46	0	1
Purchase period (months)	1294	5.42	3.45	0.5	27
Adoption year	1411			1972	2021
Annual limit($)	1414	25,826.43		10,000	250,000
Compensation limit (maximum, % salary)	1249	13.16	4.97	5	50
Holding period(days)	192	7.55	16.92	0	90
Exclude officer(indicator)	154	0.36	0.48	0	1
Number of ESPP shares purchased (in thousands)	1555	936			86,000
Value of ESPP purchases (thousands of $)	1171	47,726		62	4,386,000

1.3.2 ESPP purchase as a signal

In this section, I investigate whether employees' ESPP purchases can serve as a signal distinct from insider trading. My focus is to examine the correlation between ESPP purchases and insider trading as sources of information about the firm. Prior literature has established that insider trading can serve as a signal for outsiders in making risky decisions. It is possible that the valuable information held by non-executive employees is influenced by insiders' actions. Furthermore, since executives can also participate in ESPP plans, this issue needs further investigation. Clearly, a high correlation between insider trading and ESPP purchases would indicate that ESPP purchases by all employees do not contain new information distinct from insiders' information about the firm's operations.

First, before examining the correlations, I demonstrate that, even though insiders can participate in the plan, ESPP purchases can still be a new and distinct source of information for outsiders. The image below is a part of the proxy form of NetApp Inc. at the end of the fiscal year 2018. The table shows the number of shares purchased under the plan during fiscal 2018, together with the value of those shares as of the date of purchase.

AMENDED PLAN BENEFITS
Employee Stock Purchase Plan

Name and Position	Number of Purchased Shares	Dollar Value of Purchased Shares ($)(1)
George Kurian *Chief Executive Officer and President*	—	—
Ronald J. Pasek *Executive Vice President and Chief Financial Officer*	1,349	$37,153
Henri Richard *Executive Vice President, Worldwide Field and Customer Operations*	997	$19,123
Matthew K. Fawcett *Senior Vice President, General Counsel and Secretary*	997	$27,582
Joel Reich *Executive Vice President and General Manager, NetApp Storage Systems and Software business unit*	998	$26,207
All current executive officers as a group (4 persons)	4,341	$110,064
All employees, including current officers who are not executive officers, as a group (7,420 persons)	3,832,844	$101,147,992

Figure 1.1: Insiders and other employees ESPP purchase

As the Figure 1.1 shows, even with insiders purchase, a significant portion of ESPP purchases belongs to other employees of the firm. Unfortunately, information like this is rarely disclosed in company filings and is not accessible for a wide range of companies. To take a closer look, I run the following panel regression on the sample of S&P 500 and S&P 400 Midcap firms collected in the previous section,:

$$\log(ESPP/emp)_{it} = \alpha + \beta_1 * \ \log(\text{insider sale})\ _{it} + X_{it} + f_i + \gamma_t + \epsilon_{it} \tag{1.1}$$

Where $\log(ESPP/emp)_{it}$ equals the logarithm of ESPP purchase (in dollars) per employee[1] for firm i in year t. $log(insider\ sale)_{it}$ shows the log of the total number of shares sold by insiders normalized by the total number of shares outstanding in a year. X_{it} represents the vector of following control variables: $log(wage/emp)$ showing the logarithm of annual wage per employee, Q, $leverage$ and $log(assets)$. fi and γ_t show the firm and year fixed effects, respectively. A significant and large value for the coefficient β_1 suggests that ESPP purchases and insider trading are closely correlated.

[1] The total number of employees of the firm, not just the participants in the plan.

Table 1.2 provides the summary statistics used to estimate Equation 1.1. The ESPP purchase value is the amount reported by the firm at the end of the fiscal year. Insiders' sold shares represent the cumulative number of shares sold by insiders during the same year. The ESPP purchase value is normalized by the total number of employees working in the company, and the insiders' sold shares are normalized by the company's total shares outstanding. Based on the table, in this sample of 1,491 firm-year observations, employees make an average salary of $77,000 annually and pay about $2,400 to their company's ESPP plan to buy its stock with a discount.

Table 1.2:
descriptive statistics of ESPP firms and their fundamentals used in regression 1.1.
ESPP per employee equals the total dollar value of ESPP purchase over a year normalized by the total number of a firm's employee population. ***Insider sold shares/total shares outstanding*** shows the total number of shares sold by insiders every year normalized by the total number of shares outstanding for a firm. ***Q*** is the sum of the equity market value and the debt book value normalized by the assets' book value. ***Wage/emp*** shows the annual wage per employee in thousand dollars estimated based on staff expense and salary in Compustat. If missing, it is replaced by the industry's annual average. ***Leverage*** equals the sum of total long-term debt and debt in current liabilities, normalized by the book assets.***Assets*** is equal to the total assets in million dollars. The sample covers S&P 500 and S&P 400 midcap firms offering ESPP and reporting the level of the contributions in 10-k forms from 2015 to 2021.

Variable	***Obs***	***Mean***	***Std. Dev.***	***Min***	***Max***
ESPP per employee($)	1,491	2,422.14		20.74	13,721.24
Insider sold shares/total shares outstanding	1,472	0	0.01	0	0.28
Q	1,491	2.94	2.22	0.98	13.39
Wage/emp (thousand $)	1,430	76.91	39.47	15.07	446.33
Leverage	1,484	0.28	0.20	0	1.11
Assets (million $)	1,491	25,189.21		323.89	273,869

Table 1.3 presents the results of Equation 1.1. Based on the regression results, employees decrease their ESPP purchases when insiders sell their stock, which is intuitively correct. However, the coefficient on $log(insider\ sold\ shares/total\ shares\ outstanding)$ remains non-significant and small in columns 1 to 5. Consequently, within the scope of this data, there is no correlation between insiders' sales and employees' ESPP purchase. This observation aligns well with my research goal, as it increases the likelihood that employees' signals become a more significant source of information for investors.

Furthermore, in this sample of 224 firms, the standard deviation of ESPP purchase ratio between firms is 1.43, while for a random firm over an average of 6.6 years, it is 0.29. Consequently, in this dataset, the majority of the variation in the ratio is driven by differences between firms rather than changes in the ESPP purchase level within a single firm over time. This difference between firms is specifically useful in studying targets of M&A deals, as it helps in distinguishing firms from each other cross-sectionally. It is worth noting that these results remain zero and non-significant when using insiders' purchased shares instead of sold shares, as well as when using the dollar value of shares instead of the number of shares. Additionally, the results do not change when using the one-period lag of insiders' trading as the explanatory variable.

Table 1.3: ESPP shares purchase and insiders trading.
This table presents the results from the regression of ESPP purchase value per employee (***log(ESPP/emp)***) on normalized total number of shares sold by insiders (***log(insider sold shares/total shares outstanding)***). The control variables include the followings: $\boldsymbol{log(wage/emp)_{it}}$ showing the logarithm of annual wage per employee estimated based on staff expense and salary in Compustat. If missing, it is replaced by the industry's annual average. $\boldsymbol{Q_{it}}$ is the sum of the equity market value and the debt book value normalized by the assets' book value. $\boldsymbol{Leverage_{it}}$ equals the sum of total long-term debt and debt in current liabilities, normalized by the book assets; the variable's cube root is used in this regression. $\boldsymbol{log(assets)_{it}}$ is equal to the logarithm of total assets. Standard errors, listed in parentheses, are clustered at the firm level. The sample covers S&P 500 and S&P 400 Midcap firms offering ESPP and providing information on the plan over 2015-2021.

Variables	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
log(insider sold shares/ total shares outstanding)	-0.00	-0.02*	-0.01	-0.02	-0.02
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
log (Q)		0.23***	0.21***	0.24***	0.25***
		(0.05)	(0.04)	(0.05)	(0.06)
Leverage		-0.26*	-0.22*	-0.21	-0.23*
		(0.13)	(0.12)	(0.13)	(0.13)
log (assets)		-0.01	-0.02	-0.03	-0.02
		(0.06)	(0.06)	(0.06)	(0.06)
log (wage/emp)		0.44**			0.41***
		(0.17)			(0.15)
Constant	6.84***	1.94	6.88***	6.71***	2.46
	(0.06)	(1.78)	(0.55)	(0.56)	(1.70)
Fiscal year dummy	Y	Y	Y	Y	Y
Industry × year dummy	N	N	N	Y	Y
Firm dummy	Y	Y	Y	Y	Y
Observations	1,442	1,376	1,435	1,435	1,376
R-squared	0.13	0.17	0.16	0.38	0.38
Number of firms	224	215	224	224	215

Having gained insight into ESPP purchase patterns and their relation to insider trading for a sample of the market's major firms, I can now proceed to examine the primary question of my research. This question focuses on the role of ESPP purchases in all M&A deals involving an ESPP target from 2000 to 2022.

1.4 Variable measurements and sample

To investigate whether ESPP purchases by non-executive employees at target firms can serve as a signal for M&A deal outcomes, I begin this section by defining the key variables of my research. Additionally, I explain the methodology for identifying all target firms offering

ESPPs and constructing the sample.

1.4.1 Variable measurements

Non-executive purchase

I construct two non-executive ESPP purchase variables: one relative to the target's market value and one relative to the target's employee population.[2] A key point here is that for the ESPP purchase to serve as a signal, the market should know about it before the deal announcement. Therefore, I focus on the most recent data with a filing date before the deal's original announcement.

For example, imagine target firm A has a fiscal year-end on December 31st and reports its associated 10-K by February 20th each year. If the company makes a merger announcement on March 30th, 2010, I will use the ESPP purchase reported in the 10-K form filed on February 20th, 2010, which contains the data for the fiscal year of 2009. If the same company makes the announcement on January 30th, 2010, before the 10-K filing date, I will use the data from the 10-K filed on February 20th, 2009, showing the purchase level for the fiscal year 2008.

In my final sample of 1,076 deals, the mean difference between the end of the fiscal year and the deal's announcement date is 248 days, with a minimum of 35 and a maximum of 544 days. The similar difference between the deal's announcement date and the 10-K filing date equals 173 days, with a minimum of 0 and a maximum of 365 days.

Using the number of shares purchased by non-executives in an ESPP plan, I make my first measure as the following:

$$ESPP/MV_{i,t} = \frac{ESPP\,purchase_{i,(closest\,10-K\,filed\,before\,t)}}{Market\,value_{i,(closest\,10-K\,filed\,before\,t)}} \tag{1.2}$$

ESPP purchase is the dollar value of shares purchaed by target firm i's employees under the ESPP plan, and subscript t is the M&A original announcement date from SDC. I also define another measure based on the number of employees of the firm:

$$ESPP/Emp_{i,t} = \frac{ESPP\,purchase_{i,(closest\,10-K\,filed\,before\,t)}}{Employee\,population_{i,(closest\,10-K\,filed\,before\,t)}} \tag{1.3}$$

[2] Not ESPP participants population, because that data is not revealed.

Clearly, the best measure here would be the exact number of plan participants, but since that data is not revealed in 10-K forms, I use the total population of employees from Compustat instead.

Acquirer abnormal returns

I measure the acquiring firm's cumulative abnormal returns (CAR) around the acquisition announcement date. The acquirer cumulative abnormal return is measured over the event window of three days (-1, +1) around the acquisition announcement, in which the expected returns are obtained from a market model (Acq_CAR_m) or a four-factor CAPM model (Acq_CAR_ff), where I obtain the models' parameter estimates with an estimation window ranging from 300 days to 60 days prior to the M&A original announcement date (i.e., -300, -60).

Target abnormal returns

I measure the target firm's cumulative abnormal returns (CAR) around the acquisition announcement date in two ways: first, the target cumulative abnormal return is measured over the event window of three days (-1, +1) around the acquisition announcement, in which the expected returns are obtained from a market model (Tgt_CAR) or a four-factor CAPM model (Tgt_CAR_ff), where I obtain the models' parameter estimates from an estimation window ranging from 300 days to 60 days prior to the M&A original announcement date (i.e., -300, -60). Secondly, I use a longer window for the target CAR to account for the possibility of information leakage. Therefore, the second set of target CARs is measured over the event window (-20, +1) around the acquisition announcement, using two models: a market model (Tgt_CAR2) and a four-factor CAPM model (Tgt_CAR2_ff), with parameter estimates derived from an estimation window of (-300, -60).

Combined abnormal returns

I use the combined CAR to measure acquisition synergy. Following Suk and Wang (2021), to combine acquirer CAR and target CAR, I use two approaches: firstly, I calculate the weighted linear combination of the acquirer's three-day (-1, +1) CAR and the target's three-day (-1, +1) CAR, in which the weights are the relative market values of the acquirer and target 60 days prior to the acquisition announcement. Similar to the acquirer and target CAR explained above, the expected returns are measured from a market-model (Com_CAR_m) or the four-factor CAPM model (Com_CAR_ff) using the parameters of the model estimated over day -300 to day -60 before the M&A announcement date.

In the second approach, I estimate the combined CAR similar to the first approach with the difference that target CAR over (-20,+1) around the original announcement date is used in the linear combination. Therefore, I use (Tgt_CAR2) instead of target CAR over (-1,+1) with a market model estimaton, and use (Tgt_CAR2_ff) instead of Target CAR over (-1,+1) with a four-factor CAPM model estimation.

In the second approach, I estimate the combined CAR similarly to the first approach, but with the difference that the target CAR over the period (-20,+1) around the original announcement date is used in the linear combination. Therefore, I use (Tgt_CAR2) instead of the target CAR over (-1,+1) with a market model estimation, and (Tgt_CAR2_ff) replaces the target CAR over (-1, +1) with a four-factor CAPM model estimation.

Takeover premiums

Following previous studies, I employ three measures as the takeover premium: $OfferP4W$, $OfferP1W$, and $OfferP1D$, which are defined as the acquirer's offer price over the target firm's closing stock price four weeks, one week, and one day prior to the acquisition announcement date, respectively. These measures are defined as follows:

$$\text{OfferP4W} = \frac{(\text{ Offer price } - \text{ Target Closing Stock Price 4 weeks before announcement day })}{\text{Target Closing Stock Price 4 weeks before announcement day}} \tag{1.4}$$

$$\text{OfferP1W} = \frac{(\text{ Offer price } - \text{ Target Closing Stock Price 1 week before announcement day })}{\text{Target Closing Stock Price 1 week before announcement day}} \tag{1.5}$$

$$\text{OfferP1D} = \frac{(\text{ Offer price } - \text{ Target Closing Stock Price 1 day before announcement day })}{\text{Target Closing Stock Price 1 day before announcement day}} \tag{1.6}$$

1.4.2 Sample

The sample for this study is constructed in the following steps: I search EDGAR filings for terms related to ESPP[3] to identify companies that have adopted the plan. Next, I match these companies with the public targets of US M&A deals covered in SDC, filtered for mergers, acquisitions by stock, and acquisitions by asset, from the year 2000 until 2022. I

[3] "Employee Stock Purchase Plan," "ESPP," "Employees' Stock Purchase Plan"

only keep the transactions where ESPP-related terms appeared in the targets' filings within a year prior to the deal announcement. This gives me a sample of 1,632 public target firms over the time horizon of 2000 to 2022.

After that, I go through each of the matched target filings and collect all the information related to ESPP. Since companies usually mention the data for three consecutive years in one 10-K filing, this process gives me a sample of more than 3,000 ESPP-related observations, each including the number of shares purchased or the dollar value of the purchases.

However, the point is that all observations are not useful for my goal. Sometimes a plan has already been suspended years ago, but its information is still mentioned in the current 10-K filing. Sometimes companies have a very late filing; for example, the date of the report is two years prior to the filing date. Given these considerations, I only focus on the deals where the ESPP target information's report date is within one year before the filing date. This process gives me a final sample of 1,076 ESPP public target firms from 2000 to 2022.

The timeline for my sample collection process is shown in Figure 1.2 below. Table 1.4, Panel A provides details on the process. Panel B of Table 1.4 displays the distribution of the M&A sample by the announcement year. It seems that bidding on ESPP firms as targets was prevalent from 2004 to 2008, after which it declined. However, recently, there has been an increasing trend.

Regarding other variables, the deal characteristics come from SDC, while the data on target and acquirer fundamentals are from Compustat and Thomson. The data required to estimate cumulative abnormal returns are obtained from CRSP. The appendix provides details on all variables and their definitions.

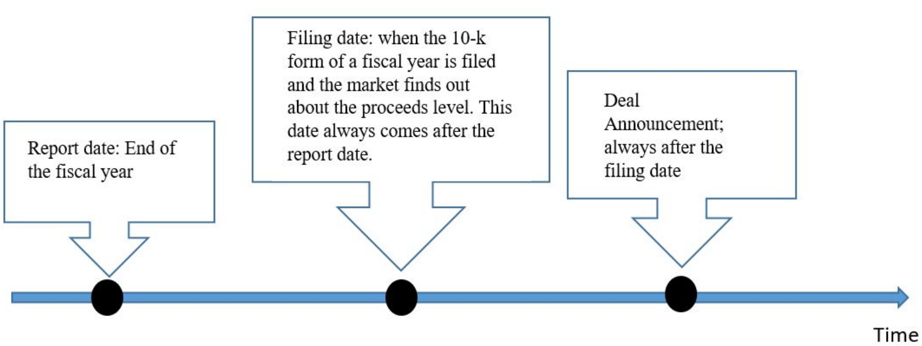

Figure 1.2: Sample collection timeline

Table 1.4: Sample construction and time trend

Panel A: Sample Construction	
Number of US M&A transactions (2000-2022) from SDC, where ESPP-related terms appeared in targets' EDGAR filings within a year prior to the deal announcement.	1,632
Number of transactions that reveal information about the target's ESPP purchase in the filing	1,137
Number of transactions where the ESPP target information's report date is within one year before the filing date (plans suspended in previous years but mentioned in the form are excluded in this step)	1,076

Panel B: M&A sample distribution by the deal announcement year

Announcement year	Number of observations	% of Sample
2000	2	0.19
2001	52	4.83
2002	48	4.46
2003	84	7.81
2004	57	5.30
2005	87	8.09
2006	82	7.62
2007	77	7.16
2008	52	4.83
2009	38	3.53
2010	59	5.48
2011	53	4.93
2012	25	2.32
2013	23	2.14
2014	29	2.70
2015	57	5.30
2016	43	4
2017	35	3.25
2018	34	3.16
2019	31	2.88
2020	33	3.07
2021	34	3.16
2022	41	3.81
Total	1076	100.0

1.4.3 Descriptive statistics

Table 1.5 presents summary statistics for the final M&A sample. On average, target employees have paid \$4.8 million to buy their company's stock with a discount in the year before the deal announcement. This amounts to \$2,000 per employee in a target firm. The average three-day (-1, +1) acquirer CAR is -1% for both measures, based on the market and four-factor models. The average of the acquirer's and target's three-day combined CARs, for all models and both short and long target estimation windows, is 2%. The average excess offer price over target market price (OfferP1D, OfferP1W, and OfferP4W) ranges from 38% to 45%, and the average target cumulative abnormal returns are 25% and 26% for the three-day (-1, +1) and 22-day (-20, +1) windows, respectively.

Suk and Wang (2021), the most recent and closely related paper to my work, studies a broad sample of all public M&A deals from 1987 to 2016. Therefore, I use it as a reference to gain intuition by comparing my statistics with theirs. Upon comparison, it turns out that ESPP target firms have a sales growth of 8%, which is lower than the 15% reported by Suk and Wang (2021). Additionally, ESPP target firms have an institutional ownership ratio of 62%, compared to 25% in the reference. Furthermore, the target run-up, calculated over the (-400, -40) days before the deal announcement, is 4% in my sample, whereas the reference reports a run-up of 12%. This deference suggests that with more concentrated ownership, information leakage is less likely, resulting in a lower run-up. Another point worth mentioning is that, on average, 24% of the transaction is paid in stock in the category of deal characteristics, whereas in the reference study, this value is 54%.

Table 1.6 reports the correlation coefficients between target non-executive ESPP purchase measures and acquisition outcome variables such as acquirer returns, acquisition synergies, and takeover premiums. $ESPP/MV$ has a positive but not significant correlation with deal outcomes, providing preliminary evidence of a positive association between target firm non-executive ESPP purchases and acquisition performance. However, the correlations for $ESPP/Emp$ do not show a similar pattern and require further investigation.

Table 1.5:
Summary statistics
This table shows descriptive statistics for the variables used in this study. All variables are defined in the appendix. All continuous variables are winsorized at the 1% and 99% levels.

Descriptive statistics *Variables*	*N*	*Mean*	*Median*	*Std. Dev*
Main Variables				
ESPP (thousand $)	941	4,780	842	
ESPP/Emp ($)	918	2,122.54	945.43	
ESPP /MV	917	0.00	0.00	0.01
Acq_CAR_m	528	-0.01	-0.00	0.07
Acq_CAR_ff	523	-0.01	-0.00	0.07
Com_CAR_m	415	0.02	0.01	0.06
Com_CAR_ff	411	0.02	0.01	0.06
Com_CAR2	415	0.02	0.01	0.07
Com_CAR2_ff	411	0.02	0.01	0.07
Tgt_CAR	893	0.25	0.19	0.29
Tgt_CAR_ff	895	0.25	0.19	0.29
Tgt_CAR2	958	0.26	0.22	0.31

Tgt_CAR2_ff	960	0.26	0.22	0.31
Target Characteristics				
Tgt_MV (million \$)	917	2,570	391	
Tgt_Size	940	5.88	5.81	1.81
Tgt_MTB	917	3.80	2.28	5.56
Tgt_ROA	939	-0.10	0.01	0.31
Tgt_Lev	940	0.16	0.09	0.19
Tgt_EP	917	-0.18	0.01	0.84
Tgt_Tang	935	0.15	0.09	0.18
Tgt_Liq	874	0.32	0.31	0.27
Tgt_SGrow	866	0.08	0.07	0.47
Tgt_Inst	913	0.62	0.67	0.31
Tgt_Run up	849	0.04	0.03	0.22
Acquirer Characteristics				
Acq_Size	629	8.76	8.38	3.68
Acq_MTB	577	4.83	2.78	13.07
Acq_ROA	628	0.01	0.04	0.19
Acq_Lev	628	0.19	0.15	0.17
Acq_FCF	571	0.03	0.06	0.14
Deal Characteristics				
OfferP1D	880	0.38	0.29	0.65
OfferP1W	879	0.40	0.31	0.66
OfferP4W	878	0.45	0.33	0.84
Diff_Ind(indicator)	1,005	0.51	1.00	0.50
Tender	1,005	0.16	0.00	0.37
Multi_Bidder	1,005	0.06	0.00	0.24
Rel_Size	458	0.35	0.16	0.47
Pct_Stock	856	0.24	0.00	0.39
Deal status (indicator)	1,011	0.80	1.00	0.40

Table 1.6:
Pairwise correlations of $ESPP/MV$, $ESPP/Emp$, and deal measures. Star shows the 1% significance level. All variables are defined in the appendix.

Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
(1) *ESPP/MV*	1.00														
(2) *ESPP/Emp*		1.00													
(3) *Acq_CAR_m*	0.01	-0.03	1.00												
(4) *Acq_CAR_ff*	0.01	-0.04	0.98*	1.00											
(5) *Com_CAR_m*	0.00	-0.05	0.74*	0.71*	1.00										
(6) *Com_CAR_ff*	0.01	-0.05	0.73*	0.74*	0.98*	1.00									
(7) *Com_CAR2*	0.01	-0.04	0.73*	0.70*	0.95*	0.93*	1.00								
(8) *Com_CAR2_ff*	0.02	-0.03	0.73*	0.73*	0.92*	0.94*	0.98*	1.00							
(9) *Tgt_CAR_m*	0.02	0.06	0.06	0.05	0.22*	0.22*	0.20*	0.19*	1.00						
(10) *Tgt_CAR_ff*	0.02	0.06	0.05	0.06	0.21*	0.22*	0.19*	0.19*	1.00*	1.00					
(11) *Tgt_CAR2*	0.04	0.07	0.10	0.10	0.19*	0.19*	0.25*	0.24*	0.90*	0.90*	1.00				
(12) *Tgt_CAR2_ff*	0.04	0.07	0.10	0.10	0.19*	0.20*	0.25*	0.26*	0.89*	0.90*	0.99*	1.00			
(13) *OfferP4W*	0.00	0.07	-0.08	-0.07	-0.03	-0.03	0.00	0.01	0.42*	0.42*	0.46*	0.45*	1.00		
(14) *OfferP1W*	0.00	0.07	-0.10	-0.10	-0.04	-0.03	-0.01	-0.01	0.58*	0.58*	0.55*	0.55*	0.87*	1.00	
(15) *OfferP1D*	0.01	0.06	-0.12*	-0.11	-0.05	-0.05	-0.04	-0.04	0.57*	0.57*	0.50*	0.50*	0.83*	0.97*	1.00

1.5 Baseline analysis

1.5.1 Acquirer abnormal returns

Intuitively, non-executive employees typically do not have detailed information about the acquiring company. Consequently, they primarily base their ESPP purchase decisions on the characteristics of their own firm. Therefore, initially, I only focus on the target firm variables that prior studies suggest acquirer investors might use to evaluate the deal. I implement the following event-day regression to test whether a firm that acquires targets with higher non-executive ESPP purchase has larger abnormal returns at the acquisition announcement:

$$\begin{aligned} Acq\_CAR_j = \alpha + \beta_1(Target\,ESPP\,purchase)_j + \\ \beta_2 * Target\ Control_j + Year_j + Industry + \epsilon_j \end{aligned} \tag{1.7}$$

Subscript j refers to a deal announcement.

Acq_CAR is computed in two ways (Acq_CAR_m or Acq_CAR_ff), as explained in Section 1.4.

($Target\,ESPP\,purchase$) equals one of the two ESPP purchase ratios: $log(1+ESPP/MV)$, which is the transformation of $ESPP/MV$ due to its concentration around zero, or $log(1+ESPP/Emp)$, which is the transformation of $ESPP/Emp$ to account for the small values and because percentage changes make more sense for a dollar variable. If target ESPP purchase ratios forecast the acquirer's M&A announcement abnormal return, the estimated coefficient parameter β_1 is expected to be positive.

As in prior studies, target characteristics ($Target\,Controls$) include firm size (Tgt_Size), market-to-book ratio (Tgt_MTB), leverage (Tgt_Lev), ROA (Tgt_ROA), past market returns ($Tgt\_Run\,up$), and percentage of institutional ownership (Tgt_Inst). Target characteristics are measured at the end of the closest fiscal year prior to the acquisition announcement.

In particular, I control for the target firm's market returns ($Tgt\_Run\,up$) over the period of (-400, -40) days before the acquisition announcement, to account for the momentum effect. This interval is broad enough to allow both the target's non-executive employees and the acquirer investors to use the run-up to make their investment decisions.

Table 1.7 presents the regression 1.7 results. The first two columns show that the coefficient on $log(1+ESPP/MV)$ is 0.345 (sd = 0.695) and the coefficient on $log(1+ESPP/Emp)$ is -0.002 (sd = 0.003) when the market model is used in computing the acquirer three-day

CAR (i.e., Acq_CAR1_m). After adding the run-up variable in columns 3 and 4, the ESPP purchase variables' sign stays the same and remains insignificant. Analogously, after using the four-factor model to compute the acquirer three-day CAR in columns 5 to 8, the sign of the important coefficients remains the same, and they remain insignificant.

Table 1.7:
Target non-executive ESPP purchases and acquirer abnormal returns at the acquisition announcement.
This table presents the results from the regression of acquirer announcement abnormal returns (Acq_CAR_m, Acq_CAR_ff) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	A_CAR_m				***Acq_CAR_ff***			
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
log(1+ESPP/MV)	0.345		0.481		0.444		0.552	
	(0.695)		(0.607)		(0.734)		(0.673)	
log(1+ESPP/Emp)		-0.002		-0.001		-0.002		-0.001
		(0.003)		(0.003)		(0.003)		(0.003)
Tgt_ROA	-0.022	-0.026	-0.025	-0.029	-0.021	-0.026	-0.025	-0.029
	(0.022)	(0.021)	(0.026)	(0.025)	(0.024)	(0.022)	(0.029)	(0.027)
Tgt_Size	0.002	0.002	0.004	0.003	0.002	0.002	0.003	0.003
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Tgt_Inst	0.011	0.011	0.014	0.013	0.011	0.012	0.014	0.015
	(0.017)	(0.017)	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)	(0.017)
Tgt_MTB	-0.000	-0.000	0.000	0.000	-0.000	-0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Tgt_Lev	-0.019	-0.020	-0.024	-0.024	-0.006	-0.006	-0.010	-0.008
	(0.028)	(0.028)	(0.033)	(0.033)	(0.028)	(0.028)	(0.034)	(0.033)
Tgt_Run up			0.071***	0.072***			0.062***	0.063***
			(0.025)	(0.026)			(0.021)	(0.022)
Constant	-0.159**	-0.149**	-0.083	-0.074	-0.151**	-0.139*	-0.073	-0.062
	(0.070)	(0.072)	(0.064)	(0.066)	(0.077)	(0.078)	(0.071)	(0.072)
Observations	438	438	393	393	434	434	389	389
R-squared	0.134	0.134	0.181	0.180	0.137	0.137	0.177	0.175
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Tgt_industry FE	YES	YES	YES	YES	YES	YES	YES	YES

In sum, the results in Table 1.7 needs further investigation to evaluate whether the non-executive ESPP purchase relative to the firm's market value could be a signal for predicting the acquirer's abnormal return. One point worth noting is that in most companies, insiders can also buy shares in ESPP plans. Due to the restrictions applied to the terms of the plan, this value does not account for the larger portion of the ESPP purchase, but it is not insignificant either. Insiders can decide to contribute to the ESPP using the same logic they use to buy company stock at some point before an M&A deal, whether they are aware of an upcoming deal or not.

Although at the beginning of this work, I showed that for a sample of large firms there is no significant correlation between insider trading and ESPP purchases, this might not be the case here for two reasons: First, these targets are not large firms; large firms, due to their broader set of operations, could provide information sources for employees that are different from insiders' sources. Second, employees' investment behavior before a deal announcement could differ from other times. For example, employees might be more sensitive due to rumors or other possible signs within the firm that make them more attuned to insiders' behavior. I believe these reasons are sufficient to include target insider trades before the deal announcement in the specification to isolate the part of ESPP purchases that purely belongs to non-executives.

However, the problem is that I have access to insider data only for after 2012, which covers only 35% of my ESPP target deals. Instead, I decide to add two sets of variables: acquirer characteristics and deal characteristics, which, based on previous literature, are correlated with deal outcomes and insider trades. Therefore, I implement the folowing regression:

$$\begin{aligned} Acq\_CAR_j = \alpha + \beta_1 (Target\ ESPP\ purchase)_j + \beta_2 * Target\ Control_j + \\ \beta_3 * Acquirer\ Control_j + \beta_4 * Deal\ Control_j + Year_j + Industry + \epsilon_j \end{aligned} \tag{1.8}$$

Subscript j refers to a deal announcement.
Acq_CAR is computed in two ways (Acq_CAR_m or Acq_CAR_ff), as explained in Section 1.4.
$(TargetESPPpurchase)$ equals one of the two ESPP purchase measures: $log(1+ESPP/MV)$ or $log(1 + ESPP/Emp)$. If target non-executive ESPP purchase predicts the acquirer's M&A announcement abnormal return, the estimated coefficient parameter β_1 is expected to be positive.

In addition to target variables mentioned in Table 1.7, following the conventional M&A studies, I include firm size (Acq_Size), market-to book ratio (Acq_MTB), leverage (Acq_Lev), ROA (Acq_ROA), and free cash flow (Acq_FCF) as acquirer characteristics and as deal controls, I add a tender offer indicator ($Tender$), a multiple bidder indicator ($Multi\_Bidder$), the percentage of stock payment (Pct_Stock), an indicator of different industries ($Diff\_Ind$), and the relative size of deal value to acquirer market value (Rel_Size).

Table 1.8 presents regression 1.8 results. The first two columns show that the coefficient on $log(1+ESPP/MV)$ is 1.548 (sd = 0.793) and the coefficient on $log(1+ESPP/Emp)$ is 0 when the market model is used in computing the acquirer three-day CAR (i.e., Acq_CAR1_m).

The rightmost two models show that the coefficient on $log(1 + ESPP/MV)$ is 1.755 (sd = 0.829) and the coefficient on $log(1 + ESPP/Emp)$ is 0.001 (sd=0.004). The coefficients on $log(1+ESPP/MV)$ is significant and positive in both models, 1% increase in $1+ESPP/MV$ is associated with 1.5% (1.7%) increase in acquirer cumulative abnormal return holding constant all other variables in the model. In sum, consistent with my hypothesis, these results suggest that the value of target non-executive ESPP purchases relative to the market value of the target can act as a signal to investors of the acquiring firm.

Table 1.8:
Target, acquirer, deal characteristics and acquirer abnormal returns at the acquisition announcement.
This table presents the results from the regression of acquirer announcement abnormal returns (Acq_CAR_m, Acq_CAR_ff) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	Acq_CAR_m		***Acq_CAR_ff***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
log(1+ESPP/MV)	1.548*		1.755**	
	(0.793)		(0.829)	
log(1+ESPP/Emp)		0.000		0.001
		(0.004)		(0.004)
Acq_Size	0.000	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Acq_MTB	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Acq_Lev	0.025	0.022	0.034	0.031
	(0.035)	(0.033)	(0.037)	(0.035)
Acq_FCF	0.026	0.035	-0.015	-0.004
	(0.071)	(0.072)	(0.073)	(0.074)
Acq_ROA	0.038	0.028	0.047	0.035
	(0.035)	(0.040)	(0.034)	(0.039)
Tgt_ROA	-0.021	-0.036	-0.013	-0.030
	(0.023)	(0.030)	(0.024)	(0.032)
Tgt_Size	0.004	0.003	0.006	0.004
	(0.006)	(0.006)	(0.006)	(0.006)
Tgt_Inst	0.002	-0.001	-0.006	-0.006
	(0.020)	(0.019)	(0.020)	(0.020)
Tgt_MTB	0.001	0.001	0.001	0.001
	(0.001)	(0.001)	(0.001)	(0.001)
Tgt_Lev	-0.032	-0.023	-0.035	-0.024
	(0.051)	(0.051)	(0.049)	(0.050)
Tgt_Run up	0.029	0.038	0.033	0.043*
	(0.021)	(0.024)	(0.020)	(0.023)
Tender	-0.004	-0.004	-0.004	-0.004
	(0.009)	(0.009)	(0.009)	(0.009)
Multi_Bidder	0.027	0.028	0.019	0.019
	(0.025)	(0.026)	(0.024)	(0.025)
Rel_Size	0.002	0.002	0.000	0.001
	(0.004)	(0.004)	(0.004)	(0.004)
Diff_Ind	0.005	0.006	0.007	0.008
	(0.010)	(0.010)	(0.010)	(0.010)

Pct_Stock	-0.063***	-0.063***	-0.067***	-0.067***
	(0.019)	(0.019)	(0.019)	(0.019)
Constant	-0.044	-0.021	-0.056	-0.031
	(0.064)	(0.065)	(0.071)	(0.071)
Observations	265	265	263	263
R-squared	0.343	0.323	0.353	0.330
Year FE	YES	YES	YES	YES
Acq_industry FE	YES	YES	YES	YES

1.5.2 Acquisition synergies

I next test whether the target non-executive ESPP purchases before acquisition predict the synergies created by the acquisition deal. As a synergy measure, I use the combined abnormal return to the acquirer and target upon acquisition announcement, which is computed in four ways (Com_CAR_m, Com_CAR_ff, Com_CAR2, and Com_CAR2_ff), as described in Section 1.4. To test this, I estimate equation 1.8 after replacing Acq_CAR with Com_CAR. If target non-executive ESPP purchases signal the target's synergy potential in an acquisition, the estimated β_1 is expected to be positive.

Regression results are reported in Table 1.9. $log(1 + ESPP/MV)$ is positively and significantly associated with all the combined CARs around the announcement date. However, $log(1 + ESPP/Emp)$ shows no effect. Specifically, Models 1 and 3 show the results when three-day acquirer CAR and three-day target CAR are combined. When the market model(column 1) is used, the coefficient on $log(1+ESPP/MV)$ equals 1.276 (sd=0.689) and when the four-factor model(column 3) is used, the coefficient equals 1.488 (sd=0.702). 1% increase in $1 + ESPP/MV$ is associated with 1.28% and 1.5% increase in combined CAR for market and four-factor model, respectively. Models 5 and 7 display the results when three-day acquirer CAR and 22-day target CAR are combined. Model 5 shows that the coefficient on $log(1+ESPP/MV)$ is 1.302 (sd=0.674) and Model 7 shows that it equals 1.545 (sd=0.716). This suggests that 1% increase in $1 + ESPP/MV$ enhances the 3-22-day combined CAR for the market model and the four-factor model by 1.3% and 1.5% respectively. Overall, consistent with the synergy signaling hypothesis, the results in Table 1.9 suggest that target non-executive ESPP purchase signals the acquirer-target combined cumulative abnormal returns at the M&A announcement.

Table 1.9:
Target non-executive ESPP purchase and combined abnormal returns at the acquisition announcement.
This table presents the results from the regression of combined abnormal returns (Com_CAR_m, Com_CAR_ff, Com_CAR2, and Com_CAR2_ff) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	Com_CAR_m		***Com_CAR_ff***		***Com_CAR2***		***Com_CAR2_ff***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
log(1+ESPP/MV)	1.276*		1.488**		1.302*		1.545**	
	(0.689)		(0.702)		(0.674)		(0.716)	
log(1+ESPP/Emp)		-0.000		0.000		-0.000		0.000
		(0.004)		(0.004)		(0.004)		(0.004)
Acq_Size	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Acq_MTB	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001	-0.001
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Acq_Lev	0.024	0.022	0.034	0.032	0.009	0.007	0.019	0.017
	(0.034)	(0.033)	(0.036)	(0.035)	(0.036)	(0.035)	(0.038)	(0.037)
Acq_FCF	0.050	0.058	0.014	0.024	0.020	0.028	-0.016	-0.007
	(0.076)	(0.076)	(0.078)	(0.079)	(0.097)	(0.096)	(0.100)	(0.100)
Acq_ROA	0.015	0.007	0.018	0.009	0.071	0.063	0.083	0.074
	(0.047)	(0.052)	(0.045)	(0.050)	(0.078)	(0.081)	(0.079)	(0.082)
Tgt_ROA	-0.026	-0.039	-0.019	-0.033	-0.028	-0.040	-0.024	-0.039
	(0.022)	(0.025)	(0.023)	(0.026)	(0.025)	(0.027)	(0.024)	(0.027)
Tgt_Size	-0.000	-0.001	0.002	0.000	-0.003	-0.004	-0.001	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)	(0.007)	(0.007)
Tgt_Inst	0.007	0.003	-0.001	-0.001	0.013	0.010	0.002	0.002
	(0.021)	(0.020)	(0.020)	(0.021)	(0.022)	(0.022)	(0.021)	(0.022)
Tgt_MTB	0.000	0.000	0.000	0.000	0.000	0.000	0.000	0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Tgt_Lev	-0.013	-0.004	-0.018	-0.007	-0.004	0.005	-0.009	0.002
	(0.049)	(0.049)	(0.047)	(0.047)	(0.050)	(0.051)	(0.049)	(0.049)
Tgt_Run up	-0.008	-0.001	-0.002	0.007	0.047*	0.055*	0.052*	0.061**
	(0.022)	(0.024)	(0.022)	(0.025)	(0.028)	(0.030)	(0.028)	(0.030)
Tender	-0.006	-0.007	-0.007	-0.006	-0.006	-0.007	-0.007	-0.006
	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.009)	(0.010)	(0.010)
Multi_Bidder	0.007	0.008	-0.001	0.000	0.005	0.006	-0.000	0.001
	(0.025)	(0.026)	(0.025)	(0.026)	(0.024)	(0.026)	(0.024)	(0.026)
Rel_Size	0.015***	0.015***	0.013***	0.013***	0.019***	0.019***	0.018***	0.018***
	(0.004)	(0.004)	(0.004)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Diff_Ind	-0.001	0.000	0.002	0.003	0.001	0.001	0.004	0.005
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)
Pct_Stock	-0.063***	-0.063***	-0.068***	-0.068***	-0.071***	-0.071***	-0.075***	-0.074***
	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Constant	0.036	0.058	0.021	0.044	0.057	0.080	0.042	0.064
	(0.061)	(0.066)	(0.069)	(0.074)	(0.065)	(0.068)	(0.071)	(0.075)
Observations	264	264	262	262	264	264	262	262
R-squared	0.389	0.374	0.379	0.360	0.438	0.426	0.433	0.417
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Acq_industry FE	YES	YES	YES	YES	YES	YES	YES	YES

1.5.3 Offer premium and target return

Offer premium

The signaling view suggests that targets with higher non-executive ESPP purchase ratios receive larger premiums, as acquirers are more willing to pay a higher offer price for these targets. To test this, I regress each of the three offer premium measures ($OfferP4W$, $OfferP1W$, and $OfferP1D$) on the pre-M&A target ESPP purchase and control variables.

$$\begin{aligned} Premium_j = \alpha + \beta_1(Target\ ESPP\ purchase)_j + \beta_2 * Target\ Control_j + \\ \beta_3 * Acquirer\ Control_j + \beta_4 * Deal\ Control_j + Year_j + Industry + \epsilon_j \end{aligned} \tag{1.9}$$

Subscript j refers to a deal announcement.
The dependent variable, Premium, represents $OfferP1D$, $OfferP1W$, or $OfferP4W$, and the variable of interest is $Target\ ESPP\ purchase$ measure, which in the regression has one of two functional forms: $log(1 + ESPP/MV)$ or $log(1 + ESPP/Emp)$. If my hypothesis holds, β_1 is expected to be positive.

The control set for the acquirer's characteristics includes acquirer size (Acq_Size), market-to-book ratio (Acq_MTB), leverage (Acq_Lev), free cash flows (Acq_FCF), and returns on assets (Acq_ROA). I also control for target characteristics, such as firm size (Tgt_Size), market-to-book ratio (Tgt_MTB), leverage (Tgt_Lev), returns on assets (Tgt_ROA), institutional ownership ratios (Tgt_Inst) and Target Run_up (Tgt_Runup). I also add tangibility of assets (Tgt_Tang) and liquidity (Tgt_Liq) as target characteristics that might affect the premium. Babenko and Sen (2016) show that employees have information about earnings breaks and firm sales growth. Therefore, I control for target earning-to-price ratio (Tgt_EP) and sales growth (Tgt_SGrow), as these variables could also be determinants in a deal's premium.

Table 1.10 presents the results from estimating Equation 1.9. Based on the table, all three offer price-based premium measures ($OfferP1D$, $OfferP1W$, and $OfferP4W$) are positively, but not significantly, related to $log(1 + ESPP/MV)$, with coefficients of 1.18 (sd=4.32), 2.56(sd= 4.6), and 1.28 (sd=6.83), respectively. My other ESPP purchase measure, $log(1+ESPP/MV)$, shows no effect. Since the coefficients on the variables of interest are not significant, I can't reject the null hypothesis that they equal zero. However, due to the positive sign on target ESPP purchase relative to target market value, it is possible that acquirers are willing to offer higher premiums when target non-executives purchase more of their own company stock at a discount, which improves the acquirers' confidence in taking over the target.

The results on control variables are mixed in terms of consistency with prior literature and intuition. The coefficient on target MTB (Tgt_MTB) is significantly negative (columns 1-4), which is consistent with the idea that firms with high market-to-book ratios may be overvalued, leading to a lower premium paid in a transaction for such firms. The coefficient on acquirer MTB (Acq_MTB) is also significantly negative (columns 1-6), which aligns with the idea that firms where management has performed well will have higher market-to-book ratios, reflecting more efficient use of firm resources and thus reducing the premium an acquirer would pay to manage the firm differently. However, the coefficient on leverage for both acquirer and target (Acq_Lev, Tgt_Lev) does not match the expectation that acquirers with more debt are more likely to pay lower premiums and targets with higher leverage are more willing to sell, resulting in a lower premium.

Table 1.10:
Target non-executive ESPP purchase and and the premium of offer price.
This table presents the results from the regression of combined abnormal returns ($OfferP1D$, $OfferP1W$, and $OfferP4W$) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	OfferP1D		***OfferP1W***		***OfferP4W***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
log(1+ESPP/MV)	1.182		2.561		1.277	
	(4.320)		(4.607)		(6.838)	
log(1+ESPP/Emp)		-0.001		-0.001		-0.001
		(0.021)		(0.022)		(0.030)
Acq_Size	0.000	0.000	-0.004	-0.004	0.000	0.000
	(0.005)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)
Acq_MTB	-0.001**	-0.001**	-0.002***	-0.002***	-0.002***	-0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Acq_Lev	0.258*	0.256*	0.368**	0.365**	0.397**	0.395**
	(0.141)	(0.141)	(0.149)	(0.149)	(0.170)	(0.169)
Acq_FCF	-0.101	-0.103	-0.233	-0.237	-0.161	-0.162
	(0.354)	(0.361)	(0.374)	(0.384)	(0.436)	(0.435)
Acq_ROA	0.065	0.062	0.231	0.226	0.153	0.149
	(0.195)	(0.199)	(0.230)	(0.238)	(0.236)	(0.236)
Tgt_ROA	-0.342	-0.331	-0.458**	-0.432**	-0.395	-0.383
	(0.209)	(0.214)	(0.220)	(0.217)	(0.313)	(0.288)
Tgt_Size	0.019	0.019	0.019	0.018	0.038	0.038
	(0.023)	(0.022)	(0.027)	(0.026)	(0.032)	(0.030)
Tgt_Inst	-0.164	-0.165	-0.139	-0.142	-0.193	-0.194
	(0.102)	(0.103)	(0.116)	(0.116)	(0.126)	(0.128)
Tgt_MTB	-0.005*	-0.005*	-0.006**	-0.006*	-0.005	-0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.004)
Tgt_Lev	0.361**	0.363**	0.449***	0.455**	0.352*	0.354*
	(0.163)	(0.164)	(0.171)	(0.178)	(0.203)	(0.212)

Tgt_Run up	-0.179	-0.175*	0.087	0.094	0.669***	0.673***
	(0.109)	(0.103)	(0.119)	(0.120)	(0.156)	(0.149)
Tgt_EP	0.281	0.254	0.417**	0.359**	0.360	0.331
	(0.180)	(0.162)	(0.200)	(0.165)	(0.331)	(0.275)
Tgt_SGrow	-0.179***	-0.182***	-0.162**	-0.168**	-0.237***	-0.240***
	(0.057)	(0.056)	(0.071)	(0.072)	(0.056)	(0.055)
Tgt_Tang	-0.308	-0.318	-0.280	-0.299	-0.342	-0.354
	(0.195)	(0.199)	(0.223)	(0.230)	(0.265)	(0.270)
Tgt_Liq	0.026	0.025	0.065	0.060	-0.017	-0.017
	(0.110)	(0.131)	(0.116)	(0.138)	(0.142)	(0.164)
Tender	0.121	0.121	0.098	0.099	0.143	0.144
	(0.079)	(0.080)	(0.076)	(0.078)	(0.087)	(0.088)
Multi_Bidder	0.271*	0.271*	0.150	0.148	0.347	0.346
	(0.154)	(0.153)	(0.153)	(0.151)	(0.212)	(0.213)
Rel_Size	-0.030	-0.030	-0.035	-0.035	-0.043*	-0.042*
	(0.020)	(0.021)	(0.022)	(0.022)	(0.024)	(0.024)
Diff_Ind	-0.096*	-0.096*	-0.075	-0.076	-0.098	-0.098
	(0.055)	(0.056)	(0.060)	(0.061)	(0.074)	(0.073)
Pct_Stock	-0.066	-0.067	-0.146*	-0.148**	-0.108	-0.109
	(0.074)	(0.073)	(0.075)	(0.075)	(0.085)	(0.086)
Constant	-0.052	-0.041	0.211	0.225	-0.163	-0.148
	(0.224)	(0.272)	(0.303)	(0.337)	(0.388)	(0.441)
Observations	256	256	256	256	256	256
R-squared	0.377	0.377	0.422	0.421	0.484	0.484
Year FE	YES	YES	YES	YES	YES	YES
Acq_industry FE	YES	YES	YES	YES	YES	YES

Target's abnormal returns

In this section, I examine the implication of target non-executive ESPP purchase prior to M&A announcements for the target firm's abnormal returns at the deal announcements. To do so, I replicate Equation 1.9 after replacing the offer premiums with target cumulative abnormal returns(Tgt_CAR_m, Tgt_CAR_ff, Tgt_CAR2, and Tgt_CAR2_ff defined in Section 1.4 and Appendix). If the market rewards the target with high ESPP purchases, β_1 should be positive. I control for the same sets of acquirer characteristics, target characteristics, and deal characteristics as in the offer premium analysis, except that now I control for target industry fixed effects instead of acquirer industry fixed effects.

Table 1.11 presents the regression results. For the target three-day (-1,+1) CAR, the coefficient on $log(1+ESPP/MV)$ is 4.85 (sd=4.1) when the market model is used in computing CAR and 4.53 (sd=4.02) when four-factor is used (columns 1 and 3). Meanwhile, the coefficient on the other measure, $log(1+ESPP/Emp)$, equals 0.01 (sd=0.02) for both models (columns 2 and 4). In the case of using the target 22-day (-20,+1) market-model CAR (Tgt_CAR2) and four-factor model CAR (Tgt_CAR_ff), the coefficient on $log(1 + ESPP/MV)$ equals 4.7 with a s.d. of about 4.1 in both cases of columns 5 and 7. In this case, the coefficient of $log(1 + ESPP/Emp)$ equals 0.01 (sd=0.02) for both models (columns 6 and 8).

None of these relationships are statistically significant; therefore, I cannot reject the null hypothesis. However, the positive correlations on $log(1 + ESPP/MV)$ could suggest that investors perceive target non-executive ESPP purchases as a positive signal regarding the target's profitability in the acquisition.

Table 1.11:
Target non-executive ESPP purchase and and the target abnormal return at the deal announcement.
This table presents the results from the regression of combined abnormal returns (Tgt_CAR_m, Tgt_CAR_ff, Tgt_CAR2, and Tgt_CAR2_ff) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	Tgt_CAR_m		***Tgt_CAR_ff***		***Tgt_CAR2***		***Tgt_CAR2_ff***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
log(1+ESPP/MV)	4.847		4.538		4.764		4.711	
	(4.096)		(4.024)		(4.264)		(4.107)	
log(1+ESPP/Emp)		0.009		0.008		0.014		0.011
		(0.020)		(0.020)		(0.021)		(0.021)
Acq_Size	-0.005	-0.004	-0.004	-0.004	-0.003	-0.003	-0.003	-0.003
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)
Acq_MTB	-0.004	-0.004	-0.004	-0.004	-0.001*	-0.001*	-0.001	-0.001
	(0.005)	(0.005)	(0.004)	(0.004)	(0.001)	(0.001)	(0.001)	(0.001)
Acq_Lev	0.239*	0.228*	0.242**	0.231*	0.280**	0.277**	0.306***	0.302**
	(0.122)	(0.121)	(0.119)	(0.118)	(0.122)	(0.121)	(0.117)	(0.117)
Acq_FCF	0.049	0.032	0.025	0.009	-0.048	-0.064	-0.030	-0.041
	(0.304)	(0.310)	(0.299)	(0.305)	(0.321)	(0.324)	(0.314)	(0.318)
Acq_ROA	0.131	0.132	0.120	0.120	0.082	0.084	0.101	0.100
	(0.198)	(0.203)	(0.200)	(0.204)	(0.201)	(0.207)	(0.198)	(0.204)
Tgt_ROA	-0.545***	-0.482**	-0.539***	-0.480**	-0.672***	-0.618***	-0.648***	-0.598***
	(0.198)	(0.209)	(0.194)	(0.205)	(0.189)	(0.197)	(0.180)	(0.189)
Tgt_Size	0.001	-0.004	0.002	-0.003	0.006	0.001	0.004	-0.001
	(0.024)	(0.024)	(0.024)	(0.024)	(0.022)	(0.022)	(0.022)	(0.021)
Tgt_Inst	0.010	0.003	-0.002	-0.008	-0.018	-0.029	-0.033	-0.042
	(0.092)	(0.091)	(0.090)	(0.089)	(0.094)	(0.093)	(0.091)	(0.090)
Tgt_MTB	-0.005*	-0.005*	-0.005*	-0.005*	-0.005*	-0.005*	-0.004	-0.005
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Tgt_Lev	0.315**	0.338**	0.321**	0.343**	0.291**	0.314**	0.284**	0.305**
	(0.139)	(0.146)	(0.136)	(0.143)	(0.133)	(0.141)	(0.130)	(0.137)
Tgt_Runup	-0.214**	-0.192**	-0.199**	-0.179**	0.326***	0.348***	0.305***	0.325***
	(0.087)	(0.086)	(0.086)	(0.085)	(0.096)	(0.093)	(0.094)	(0.091)
Tgt_EP	0.439***	0.320**	0.432***	0.321**	0.564***	0.460***	0.527***	0.424***
	(0.167)	(0.156)	(0.164)	(0.153)	(0.160)	(0.148)	(0.151)	(0.141)
Tgt_SGrow	-0.138**	-0.155**	-0.134**	-0.150**	-0.153**	-0.170**	-0.147**	-0.162**
	(0.062)	(0.060)	(0.061)	(0.059)	(0.071)	(0.067)	(0.072)	(0.068)
Tgt_Tang	-0.293	-0.280	-0.289	-0.278	-0.213	-0.204	-0.186	-0.182
	(0.219)	(0.219)	(0.218)	(0.217)	(0.199)	(0.197)	(0.198)	(0.196)

Tgt_Liq	0.060	0.031	0.053	0.026	0.055	0.017	0.066	0.036
	(0.102)	(0.124)	(0.101)	(0.123)	(0.103)	(0.130)	(0.103)	(0.129)
Tender	0.150*	0.152*	0.150*	0.152*	0.171**	0.171**	0.166**	0.166**
	(0.076)	(0.078)	(0.076)	(0.077)	(0.075)	(0.076)	(0.075)	(0.075)
Multi_Bidder	-0.115	-0.111	-0.115	-0.111	-0.124	-0.128	-0.116	-0.119
	(0.105)	(0.107)	(0.105)	(0.106)	(0.093)	(0.096)	(0.094)	(0.097)
Rel_Size	-0.052***	-0.051***	-0.053***	-0.051***	-0.059***	-0.059***	-0.058***	-0.057***
	(0.018)	(0.018)	(0.018)	(0.018)	(0.017)	(0.018)	(0.017)	(0.017)
Diff_Ind	-0.072	-0.074	-0.071	-0.072	-0.045	-0.047	-0.038	-0.040
	(0.054)	(0.054)	(0.054)	(0.054)	(0.050)	(0.051)	(0.050)	(0.052)
Pct_Stock	-0.049	-0.053	-0.053	-0.057	-0.067	-0.067	-0.064	-0.065
	(0.055)	(0.056)	(0.054)	(0.055)	(0.054)	(0.053)	(0.053)	(0.052)
Constant	0.073	0.108	0.073	0.107	-0.039	-0.126	0.034	-0.031
	(0.213)	(0.212)	(0.213)	(0.212)	(0.292)	(0.299)	(0.274)	(0.282)
Observations	243	243	243	243	262	262	262	262
R-squared	0.423	0.417	0.422	0.417	0.483	0.479	0.473	0.469
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Tgt_industry FE	YES	YES	YES	YES	YES	YES	YES	YES

1.6 Robustness check

Although in the previous section I did not find a significant correlation between the deal premium and the ESPP purchase ratio, it is possible that the significant relationships between the acquirer's cumulative abnormal return and the ESPP ratio, and between the deal's synergy and the ESPP ratio, could be affected by the deal's premium. Naturally, investors may use the deal's premium to assess the deal's quality, so it makes sense to include the deal's premium in my specifications.

Therefore, in this section, I repeat Equation 1.8 but include the deal's premium one week before the deal announcement ($OfferP1W$) in the specification. I select this premium from the three measures ($OfferP1D$, $OfferP1W$ and $OfferP4W$) because, based on Table 1.10, it has the highest correlation with $log(1+ESPP/MV)$. All the other control variables are exactly the same as the control variables used in Tables 1.8 and 1.9.

Tables 1.12 and 1.13 present the results of Equation 1.8 on the set of target, acquirer, and deal characteristics ($Controls$) and deal premium. As shown in the tables, the coefficient on $log(1+ESPP/MV)$ remains significant and positive, similar to the results in Tables 1.8 and 1.9.

Table 1.12:
Deal premium and acquirer abnormal returns at the acquisition announcement.
This table presents the results from the regression of acquirer announcement abnormal returns (Acq_CAR_m, Acq_CAR_ff) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	A_CAR_m		***Acq_CAR_ff***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**
log(1+ESPP/MV)	1.616**		1.849**	
	(0.745)		(0.765)	
log(1+ESPP/Emp)		0.001		0.001
		(0.004)		(0.004)
OfferP1W	-0.009	-0.009	-0.010	-0.010
	(0.007)	(0.008)	(0.007)	(0.008)
Constant	-0.037	-0.016	-0.050	-0.027
	(0.068)	(0.068)	(0.075)	(0.074)
Observations	260	260	258	258
R-squared	0.348	0.326	0.359	0.334
Controls	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Acq_industry FE	YES	YES	YES	YES

Table 1.13:
Deal premium and combined abnormal returns at the acquisition announcement.
This table presents the results from the regression of combined abnormal returns (Com_CAR_m, Com_CAR_ff, Com_CAR2, and Com_CAR2_ff) on target non-executive ESPP purchase ratios ($ESPP/MV$ or $ESPP/Emp$). Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	Com_CAR_m		***Com_CAR_ff***		***Com_CAR2***		***Com_CAR2_ff***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
log(1+ESPP/MV)	1.340*		1.576**		1.353**		1.614**	
	(0.680)		(0.683)		(0.670)		(0.702)	
log(1+ESPP/Emp)		0.000		0.000		-0.000		0.000
		(0.004)		(0.004)		(0.004)		(0.004)
OfferP1W	0.002	0.002	0.001	0.001	0.004	0.004	0.001	0.002
	(0.008)	(0.008)	(0.008)	(0.008)	(0.009)	(0.009)	(0.009)	(0.009)
Constant	0.031	0.053	0.016	0.040	0.051	0.076	0.036	0.061
	(0.061)	(0.067)	(0.069)	(0.074)	(0.065)	(0.069)	(0.072)	(0.076)
Observations	259	259	257	257	259	259	257	257
R-squared	0.385	0.369	0.375	0.354	0.436	0.423	0.432	0.414
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Acq_industry FE	YES	YES	YES	YES	YES	YES	YES	YES

My findings up to this point show that the market uses the ESPP purchase ratio of the target as a signal, but the acquirer itself does not. A reason for this could be that the acquirer assesses the deal and the price it offers through more prominent sources it has access to, rather than relying on the non-executive employees of the firm. However, acquirer shareholders in the market use non-executive employee purchases as a signal because they do not have access to the same sources as the acquiring firm. In other words, the ESPP purchase signal can help mitigate the information asymmetry between the acquirer's shareholders and the acquirer's management when making a major investment such as acquiring another firm.

1.7 Extensions

1.7.1 Instrumental variables approach

In this section, I implement a common practice in empirical work to mitigate omitted variable bias. Some may argue that any association between target ESPP purchase and abnormal returns at M&A announcements could be driven by investors' or employees' reactions to unexpected events. My current investigation does not account for such cases. Therefore, to address this potential endogeneity, I use the non-executive ESPP purchase from one period prior to the current one as an instrument for the current ESPP purchase measure.

To be specific, In the first stage, I obtain instrumented values of target ESPP purchase measures ($log(1 + ESPP/MV)$ and $log(1 + ESPP/Emp)$) by estimating the following regression using the instrument and the controls used in the baseline analysis:

$$\begin{aligned}(Target\ ESPP\ purchase)_{i,(closest 10-K\ filing\ before\ t)} = \alpha + \\ \beta_1 * (Target\ ESPP\ purchase)_{i,(preceding\ 10-K\ filing)} + \\ \beta_2 * (Control\ variables)_{i,(preceding\ 10K\ filing)} + \epsilon_i\end{aligned} \tag{1.10}$$

Where $Target\,ESPP\,purchase$ equals one of the two functions $log(1 + ESPP/MV)$ or $log(1 + ESPP/Emp)$. i represents the target firm i for the deal announced at time t.

The first two columns of Table 1.14 report the results from the first stage regressions where the control variables are the same as those in Equation 1.9. The coefficient estimate on $log(1 + ESPP/MV)_{t-1}$
(or $log(1 + ESPP/Emp)_{t-1}$)indicates that the relation between the instrument and $log(1 + ESPP/MV)$
(or $log(1+ESPP/Emp)$) as a measure of ($TargetESPPpurchase$) is significant and positive. In the second stage, I replicate the previous regression models with the instrumented values

obtained from Equation 1.10.

The last six columns of Table 1.14 provide the results from the second-stage regressions. The non-significant and non-stable results in columns 3 to 8 could arise from two sources: a small sample size or an exclusion restriction in the IV that does not hold. Specifically, considering that the results in Tables 1.8 and 1.9 show significant results for the same model in columns 3 to 6, this suggests that the preceding target ESPP purchase is not a credible IV for the ESPP purchase level just before the deal announcement.

Table 1.14:
Instrumental variables approach.
This table presents the first and second-stage regression results. In the first stage regression, the instrument, $log(1+ESPP/MV)_{(preceding\ 10-K\ report)}$ (or $log(1+ESPP/Emp)_{(preceding\ 10-K\ report)}$) is the target ESPP purchase measure that precedes the closest target ESPP measure just before the deal announcement. In the second stage, estimated values for ($Target\ ESPP\ measure$) from the first stage are used in the regression as a measure for target non-executive ESPP purchase. Standard errors are clustered within the acquiring firm. Standard deviations are in parentheses. *, **, *** indicate the significance of parameter estimates at the 1%, 5%, and 10% levels, respectively.

	1st stage regression		***2nd stage regression***					
	$log(1+\frac{ESPP}{MV})$	$log(1+\frac{ESPP}{Emp})$	***Acq_CAR_ff***		***Com_CAR_ff***		***OfferP4W***	
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**	**(8)**
$log(1+\frac{ESPP}{MV})_{t-1}$	0.69***							
	(0.12)							
$log(1+\frac{ESPP}{Emp})_{t-1}$		0.69***						
		(0.13)						
$\widehat{log(1+\frac{ESPP}{MV})}$			0.25		-1.24		-6.90	
			(1.99)		(1.94)		(13.77)	
$\widehat{log(1+\frac{ESPP}{Emp})}$				-0.01		-0.01		-0.05
				(0.01)		(0.01)		(0.04)
Acq_Size	0.00	0.02	0.00	0.00	0.00	0.00	0.00	0.01
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.01)	(0.01)
Acq_MTB	0.00	-0.00	0.00	0.00	-0.00	0.00	-0.00**	-0.00**
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Acq_Lev	-0.00	-0.43	0.02	0.02	0.04	0.03	0.40**	0.38**
	(0.00)	(0.43)	(0.04)	(0.04)	(0.04)	(0.04)	(0.17)	(0.17)
Acq_FCF	0.00	0.68	0.08	0.09	0.12	0.12	0.23	0.25
	(0.01)	(0.73)	(0.10)	(0.09)	(0.09)	(0.09)	(0.43)	(0.44)
Acq_ROA	0.00	-0.55	0.01	0.01	-0.02	-0.03	0.00	-0.05
	(0.00)	(0.43)	(0.04)	(0.04)	(0.04)	(0.04)	(0.26)	(0.27)
Tgt_ROA	-0.00	-0.05	-0.05	-0.05*	-0.06*	-0.06*	-0.27	-0.32
	(0.00)	(0.54)	(0.03)	(0.03)	(0.03)	(0.03)	(0.38)	(0.39)
Tgt_Size	0.00	0.13*	0.00	0.00	-0.00	-0.00	0.05	0.07*
	(0.00)	(0.07)	(0.01)	(0.01)	(0.01)	(0.01)	(0.03)	(0.04)
Tgt_Inst	-0.00	-0.31	-0.01	-0.01	-0.02	-0.00	-0.36**	-0.31**
	(0.00)	(0.33)	(0.03)	(0.03)	(0.03)	(0.03)	(0.15)	(0.14)
Tgt_MTB	-0.00	0.02*	0.00	0.00	0.00	0.00	0.00	0.00
	(0.00)	(0.01)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Tgt_Lev	0.00	-0.56	0.00	-0.00	0.03	0.02	0.15	0.08
	(0.00)	(0.39)	(0.05)	(0.05)	(0.05)	(0.05)	(0.20)	(0.21)
Tgt_Run up	0.00	0.30	0.04	0.04	-0.00	-0.00	0.63***	0.61***
	(0.00)	(0.34)	(0.03)	(0.03)	(0.03)	(0.03)	(0.19)	(0.18)

Tgt_EP	-0.00	-0.34					-0.05	-0.01
	(0.01)	(0.52)					(0.52)	(0.46)
Tgt_SGrow	-0.00	0.35*					-0.15	-0.11
	(0.00)	(0.18)					(0.10)	(0.09)
Tgt_Tang	-0.01*	-0.15					-0.02	-0.07
	(0.00)	(0.47)					(0.29)	(0.27)
Tgt_Liq	-0.00	1.04**					0.05	0.16
	(0.00)	(0.42)					(0.17)	(0.18)
Tender	0.00	-0.14	-0.00	-0.00	-0.01	-0.01	0.09	0.08
	(0.00)	(0.14)	(0.01)	(0.01)	(0.01)	(0.01)	(0.10)	(0.09)
Multi_Bidder	0.00	1.05	0.02	0.02	-0.02	-0.01	0.35	0.35*
	(0.00)	(0.69)	(0.03)	(0.03)	(0.04)	(0.04)	(0.23)	(0.19)
Rel_Size	0.00	0.05	0.00	0.01	0.01***	0.02***	-0.05**	-0.04*
	(0.00)	(0.04)	(0.01)	(0.01)	(0.00)	(0.01)	(0.02)	(0.03)
Diff_Ind	-0.00	-0.11	0.02*	0.02*	0.01	0.00	-0.15**	-0.14**
	(0.00)	(0.16)	(0.01)	(0.01)	(0.01)	(0.01)	(0.07)	(0.07)
Pct_Stock	-0.00	-0.26	-0.07***	-0.08***	-0.06***	-0.07***	-0.08	-0.09
	(0.00)	(0.18)	(0.02)	(0.02)	(0.02)	(0.02)	(0.09)	(0.09)
Constant	0.00	2.04***	-0.07	-0.03	-0.01	0.03	-0.17	0.02
	(0.00)	(0.73)	(0.08)	(0.08)	(0.07)	(0.07)	(0.34)	(0.33)
Observations	217	217	193	193	193	193	212	212
R-squared	0.58	0.84	0.42	0.43	0.43	0.44	0.47	0.47
Year FE	YES	YES	YES	YES	YES	YES	YES	YES
Acq_industry FE	YES	YES	YES	YES	YES	YES	YES	YES

1.7.2 Probabilities of merger completion

Does the probability of an announced merger being completed increase with target ESPP purchase measures? If target non-executive ESPP purchase signal the potential worthiness of acquiring the target, the acquirer should be more willing to complete the announced deal. I examine this prediction using a probit model, where the dependent variable, equals one if the announced acquisition is reported completed in SDC and zero if not. Based on Table 1.5 about 80% of the transactions in my sample has been completed.

I start by conducting a t-test with unequal variances to study the difference between $ESPP/MV$ (or $ESPP/Emp$) across complete and non-complete deals. The values for $ESPP/MV$ are very small and not significant, so I do not mention them here. However, in a sample of 753 complete deals and 165 non-complete deals, on average, each employee of the target firm in a complete deal has a purchase level of \$35.4 (t stat=0.13) higher than the average ESPP purchase in the non-complete deals category. The results of the probit model for $log(1 + ESPP/MV)$ are negative and not significant. Similarly, the results for $log(ESPP/Emp)$ are positive but not significant. Therefore, I do not expand further on these findings.

1.8 Conclusion

My study extends the literature on the informational role of non-executives prior to mergers and acquisitions (M&As). Using target employees' ESPP purchases as a proxy for non-executives' price-relevant information about their own firm, I show that the ESPP purchase ratio signals the target's synergy potential in M&As. My analysis reveals that the target's ESPP purchase ratio before the M&A announcement is informative of acquirer returns and acquisition synergies. These findings imply that the target firm's non-executive ESPP purchase ratio can improve the efficiency of the M&A market. However, I do not find a significant effect of the ESPP purchase ratio on the target's premium in the deal. Therefore, the ESPP purchase signal can help mitigate the information asymmetry between the acquirer's shareholders and the acquirer's management when making a major investment such as acquiring another firm.

It is worth mentioning that the major drawback of my study is the small sample size, which is due to private acquirers. About half of my ESPP target firms are acquired by private firms, for which data is not available. For future research, it may be worth exploring whether private firms are significantly more interested in acquiring ESPP firms and, if so, why.

1.9 Appendix.A Variable definition

Table 1.15: Variables Definition

VARIABLES	Definition
ESPP	The total dollar value of employees' contributions to the ESPP plan. In other words, this value equals the employees' purchase of the company's stock in the ESPP plan.
ESPP/MV	Total dollar value of ESPP purchase over a fiscal year divided by the market value of the firm at the end of the fiscal year
ESPP/Emp	Total dollar value of ESPP purchase over a fiscal year divided by total population of the firm employees
log(1+ESPP/MV)	logarithm of 1+ESPP/MV
log(1+ESPP/Emp)	logarithm of 1+ESPP/Emp
Acq_CAR_m	Acquirer cumulative abnormal returns, measured over $(-1, +1)$ around the acquisition announcement, in which the expected return is obtained from a market model with the CRSP value-weighted index return as the market return.The parameters of the model are estimated over the period day -300 to day -60.
Acq_CAR_ff	Acquirer cumulative abnormal returns, measured over $(-1, +1)$ around the acquisition announcement, in which the expected return is obtained from a four-factor CAPM with the CRSP value-weighted index return as the market factor.The parameters of the model are estimated over the period day -300 to day -60.
Com_CAR_m	Acquirer and target value-weighted average cumulative abnormal returns, measured over 3 days (-1, +1) around the acquisition announcement for the acquirer and over 3 days (-1, +1) around the acquisition announcement for the target. The expected return is obtained from a market model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day -300 to day -60.
Com_CAR_ff	Acquirer and target value-weighted average cumulative abnormal returns, measured over 3 days (-1, +1) around the acquisition announcement for the acquirer and over 3 days (-1, +1) around the acquisition announcement for the target. The expected return is obtained from a four-factor CAPM model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day -300 to day -60.
Com_CAR2	Acquirer and target value-weighted average cumulative abnormal returns, measured over 3 days (-1, +1) around the acquisition announcement for the acquirer and over 3 days (-20, +1) around the acquisition announcement for the target. The expected return is obtained from a market model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day -300 to day -60.
Com_CAR2_ff	Acquirer and target value-weighted average cumulative abnormal returns, measured over 3 days (-1, +1) around the acquisition announcement for the acquirer and over 3 days (-20, +1) around the acquisition announcement for the target. The expected return is obtained from a four-factor CAPM model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day -300 to day -60.

Tgt_CAR_m	Target cumulative abnormal returns, measured over (−1, +1) around the acquisition announcement, in which the expected return is obtained from a market model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day −300 to day −60
Tgt_CAR_ff	Target cumulative abnormal returns, measured over (−1, +1) around the acquisition announcement, in which the expected return is obtained from a four-factor CAPM model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day −300 to day −60
Tgt_CAR2	Target cumulative abnormal returns, measured over (−1, +20) around the acquisition announcement, in which the expected return is obtained from a market model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day −300 to day −60
Tgt_CAR2_ff	Target cumulative abnormal returns, measured over (−1, +1) around the acquisition announcement, in which the expected return is obtained from a four-factor CAPM model with the CRSP value-weighted index return as the market return. The parameters of the model are estimated over the period day −300 to day −60
Tgt_Size	Target size. Measured as the natural logarithm of target's total assets at the fiscal year-end before the acquisition announcement.
Tgt_MTB	Target's pre-acquisition market to book ratio. Measured as the ratio of target's market value to the book value of equity at the fiscal year-end before the acquisition announcement.
Tgt_ROA	Target's return on assets for the year ended before the announcement year, measured as income before extraordinary items, scaled by total assets.
Tgt_Lev	Target's pre-acquisition leverage. Measured as the sum of long-term debt and short-term debt, deflated by total assets at the fiscal year-end before the acquisition announcement.
Tgt_EP	Target's earnings to price ratio at the fiscal year-end before the acquisition announcement.
Tgt_Tang	Target's ratio of net property, plant and equipment over total assets at the fiscal year-end before the acquisition announcement.
Tgt_Liq	Target's ratio of net liquid assets (total current assets −currentliabilities) to total assets at the fiscal year-end before the acquisition announcement.
Tgt_SGrow	Target's sales growth ratio. Measured as the natural logarithm of target's total sales at the fiscal year-end before the acquisition announcement over the previous year's total sales.
Tgt_Inst	The ratio of target shares held by institutional investors at the fiscal year-end before the acquisition announcement.
Tgt_Run up	Target's market returns over the period of (−400,−40) days before the acquisition announcement. The event window for Run-up equals (−40, −2) days before the announcement.
Acq_Size	Acquirer size. Measured as the natural logarithm of acquirer's total assets at the fiscal year-end before the acquisition announcement.
Acq_MTB	Acquirer's pre-acquisition market to book ratio. Measured as the ratio of acquirer's market value to the book value of equity at the fiscal year-end before the acquisition announcement.

Acq_ROA	Acquirer's return on assets for the year ended before the announcement year, measured as income before extraordinary items, scaled by total assets.
Acq_Lev	Acquirer's pre-acquisition leverage. Measured as the sum of long-term debt and short-term debt, deflated by total assets at the fiscal year-end before the acquisition announcement.
Acq_FCF	Acquirer's pre-acquisition free cash flow. Measured as operating income before depreciation minus interest expense minus income taxes minus capital expenditure, deflated by total assets at the fiscal year end before the acquisition announcement.
OfferP1D	The ratio of excess offer price to target stock price 1 day prior to the M&A announcement date ((Offer price - Target Closing Price 1day)/Target Closing Price 1day).
OfferP1W	The ratio of excess offer price to target stock price 1 week prior to the M&A announcement date ((Offer price - Target Closing Price 1wk)/Target Closing Price 1wk).
OfferP4W	The ratio of excess offer price to target stock price 4 weeks prior to the M&A announcement date ((Offer price - Target Closing Price 4wk)/Target Closing Price 4wk).
Diff_Ind	A dummy variable that is 1 if the acquirer and the target have different two-digit SIC codes and 0 otherwise.
Tender	A dummy variable that is 1 if the deal is classified as a tender offer by SDC and 0 otherwise.
Multi_Bidder	A dummy variable that is 1 if the number of bidders reported by SDC is more than one and 0 otherwise.
REL_Size	Relative deal size. Measured as the ratio of the transaction value to the market value of the acquirer 60 days before the acquisition announcement.
Pct_Stock	Percentage of stock offered as payment medium by the acquirer.

1.10 Appendix.B Tax Treatment in ESPP plans

In this section, I describe how individuals and corporations are taxed in an employee stock purchase plan. This information is primarily based on Engelhardt and Madrian (2004).

1.10.1 Tax treatment in ESPP for individuals

At first, Figure 1.3 presents the definition of qualified versus non-qualified share dispositions in an ESPP plan. Then, I provide details on the tax treatment in ESPP plans.

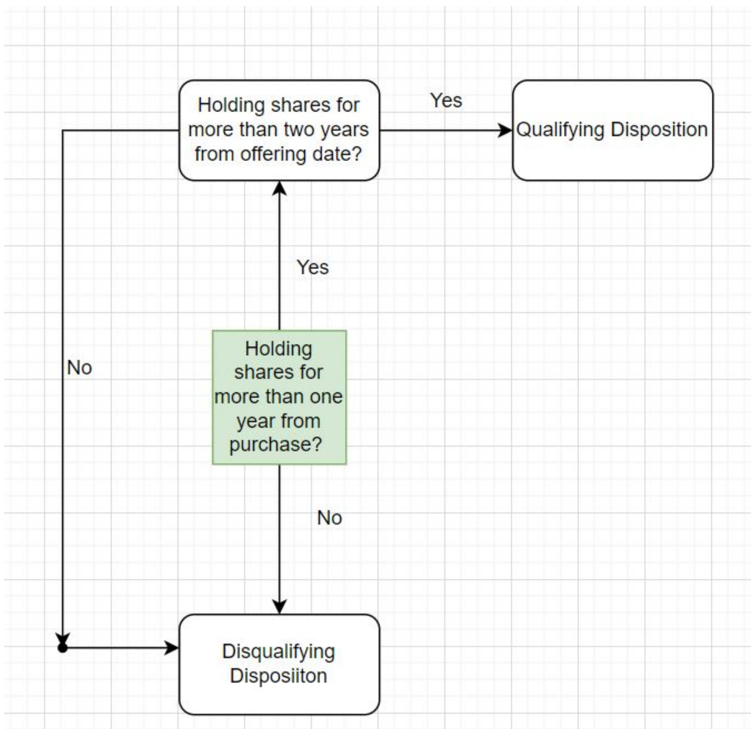

Figure 1.3: Qualified vs non-qualified disposition

- P_f : Fair market value on the first day of the offering period.
- P_l : Fair market value on the last day of the offering period.
- δ : Discount under the plan.
- P_e : Purchase(exercise) price.
- C : Employee's contribution rate out of after-tax income.
- y : Gross earnings.

- N : Number of shares purchased.
- P_q : Disposition price.

Most firms have a plan with a look-back feature, and these plans are technically more complex; therefore, I focus on them here.
The purchase price in a plan with a look-back feature will be:[4]

$$P_e = (1 - \delta)min(P_f, P_l)$$

Then, the number of purchased shares equals:

$$N = \frac{cy}{P_e} = \frac{cy}{(1-\delta)min(P_f, P_l)}$$

And gain from sale will be:

$$Gain = (P_q - P_e)N$$

On a per-share basis, this value can be decomposed into two parts: first, an ordinary income, and second, a capital gain part.

1.10.2 Qualifying disposition in an IRS 423 qualified plan

In a qualifying disposition, the ordinary income equals the minimum value between the discount at the beginning of the offering period and the gain from the sale. In other words:

$$OrdinaryInc = min(\delta P_f, P_q - P_e) = min(\delta P_f, P_q - (1-\delta)min(P_f, P_l))$$

Clearly, the rest of the gain equals capital gain:

$$P_q - P_e - OrdinaryInc = P_q - (1-\delta)min(P_f, P_l) - min(\delta P_f, P_q - (1-\delta)min(P_f, P_l))$$

[4] This price in plans without such a feature will be: $P_e = (1 - \delta)P_l$

1.10.3 Disqualifying disposition in an IRS 423 qualified plan or disposition in a non-qualified plan

In a disqualifying disposition or when the plan is not tax-qualified, the ordinary income is estimated relative to the share price on the last day of the offering. In other words:

$$OrdinaryInc = P_l - P_e$$

This income is treated as cash compensation and taxed in the calendar year the disposition happens.
Therefore, the capital gain equals:

$$P_q - P_e - OrdinaryInc = P_q - P_l$$

1.10.4 An example

- P_f : \$20.
- P_l : \$25.
- δ : %15.
- P_e : Purchase(exercise) price.
- N : 300.
- P_q : \$50.

Additionally, I am assuming that income tax rate and capital gain tax equal %24 and %15, respectively. Based on the previous section, the purchase price equals:

$$P_e = (1 - 0.15)min(20, 25) = \$17$$

- Qualifying disposition:
  $OrdinaryInc = min(\delta P_f, P_q - P_e) = min(\delta P_f, P_q - (1 - \delta)min(P_f, P_l)) = min(0.15 * 20, 50 - 17) = \$3$ per share, and total income=$\$3 * 300 = \$900$.

  Long-term capital gain=$P_q - P_e - OrdinaryInc = 50 - 17 - 3 = \$30$ per share, and total capital gain=$\$30 * 300 = \$9000$.

Therefore, ordinary income tax owned equals $\$900 * 0.24 = \$216$, and capital gain tax equals $\$9000 * 0.15 = \$1350$.

The total tax owed by this employee will be $\$216 + \$1350 = \$1566$.

- Disqualifying disposition or disposition in a non-qualified plan:
  $OrdinaryInc = P_l - P_e = \$25 - \$17 = \$8$ per share, and total income=$\$8 * 300 = \$2400$.

  Long-term capital gain=$P_q - P_e - OrdinaryInc = 50 - 25 = \$25$ per share, and total capital gain=$\$25 * 300 = \$7500$.

  Therefore, ordinary income tax owned equals $\$2400 * 0.24 = \$576$, and capital gain tax equals $\$7500 * 0.15 = \$1125$.

  The total tax owed by this employee will be $\$576 + \$1125 = \$1701$.

1.10.5 Tax treatment in ESPP for corporations

A firm offering a qualified ESPP does not receive any tax deduction in a qualifying disposition. However, under a disqualifying disposition or a non-qualified plan, a firm treats $P_l - P_e$ as cash compensation and pays payroll tax on it in addition to deducting it(cash compensation plus payroll tax) when calculating corporate tax.

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2
Paper 2

University of Washington

Abstract

From Shares to Shields: The Role of Employee Ownership in Mitigating Data Breach Risks

Employees can monitor their peers, a comparative advantage that managers and executives do not have. This monitoring incentive can be strengthened by offering employees company stock, making them partial owners of the firm. This encourages employees to be more careful with both their own actions and those of their peers, which can be particularly helpful in situations where a small mistake by one individual could lead to significant losses for the entire firm, such as a data breach incident. To examine whether mutual monitoring helps prevent data breaches, I focus on firms with employee stock ownership plans (ESOPs). I use the ratio of active ESOP participants to the firm's total employee population, referred to as the active ratio, as a measure of monitoring intensity. I find that a higher active ratio is associated with a lower likelihood of data breaches. However, this protective effect diminishes with firm size, and executive equity have an opposite effect. Next, I study how a firm's active ratio changes following its first data breach and find a 3 percent increase. Using a staggered difference in differences structure, I also find that following the first breach with impacts at least as large as the twenty fifth percentile of all breaches, ESOP firms increase their active ratio by 6 to 10 percent.

" ... No junior employee at Microsoft or Intel can improve the value of her heavyweight employer to such a degree that it will make it worthwhile for her to work harder once stock options are offered. Nevertheless, given the sensitivity of the "knowledge industry" to leakage of its intellectual property, all employees can add much to the company's value by standing on guard against such loss. ... Stock options privatize the firm's monitoring task into the hands of its employees." Hannes, 2006

2.1 Introduction

Companies often state in their 10-K and proxy statements that they provide broad stock-based compensation to enhance productivity. An increase in employee ownership, along with the alignment of employees' interests with those of shareholders, is believed to improve employee performance and overall company productivity. Nevertheless, the economic literature presents mixed results regarding the relationship between productivity and stock-based compensation. This raises another question: what if companies use this type of compensation as a risk management tool rather than as a direct means of increasing productivity? Put differently, what if firms implement stock-based compensation to internalize the negative externalities associated with employee actions, ultimately leading to a lower likelihood of risky outcomes that can be caused by employees actions?

To answer this general question, I focus on one of the key benefits of stock-based compensation: mutual monitoring. This concept is based on the idea that employees have a comparative advantage in monitoring their peers and other employees—an advantage that managers and executives typically lack. By offering employees company stock and making them partial owners, firms encourage employees to act more responsibly, both in their own actions and in overseeing those of their peers. I refer to this mechanism as the mutual monitoring channel throughout this work. Mutual monitoring can be especially valuable in situations where a mistake by one person could lead to significant losses for the entire firm, such as data breaches. In this paper, I examine whether mutual monitoring can help protect firms against risky and costly outcomes like data breaches. More specifically, I ask whether companies offer employee stock-based compensation to encourage employees to monitor one another and help protect the firm from these costly and risky events.

Data breaches are costly, and damage firms' reputations. IBM's 2024 annual report on data breaches identifies three primary root causes: malicious or criminal attacks account for

55%, IT failures for 23%, and human error for 22%. The 22% attributed to human error directly results from individual mistakes, while the other two categories could be indirectly related to human errors and the actions of system users within the firm. In a survey[1] 71% of business leaders indicated that they think next cybersecurity breach will come from the inside.

Given these facts, firms seem to recognize the significant role employees play in these incidents. Ownership in the company can transfer some of the costs of data breaches to employees. If employees who are owners of the company are more careful with their own actions and those of their peers to avoid harming the firm, then there is a strong possibility that companies leverage stock-based compensation as a defense against cybersecurity issues arising from their own employees' actions.

Given the anecdotal consensus that "the company can't even determine who the unauthorized party that breached its network was,"[2] this channel could be even more important. within this framework, employees are likely to work more carefully because they are owners or because their colleagues are monitoring them. Moreover, Kamiya et al.,2021 reports that the unconditional probability of a data breach incident in a given year within the sample of all public firms from 2005 to 2017 is 0.047%, which can result in an average loss of $1.25 billion for shareholders. Therefore, it is reasonable to consider a data breach incident as a tail event—an event with a low probability but high potential damage.

So far, I have introduced data breaches as the risky outcome and presented evidence showing that employees play a significant role in their occurrence, as well as firms' awareness of this fact. Now, I outline the approach I will use to estimate mutual monitoring in this study. To define a mutual monitoring measure, I focus on Employee Stock Ownership Plans (ESOPs). These are retirement stock-based benefit plans offered to employees by their company. The main variable or my mutual monitoring measure is called active ratio and is defined as the ratio of active participants—employees who are hired, actively work for the company, and have an ESOP—to total employees. Therefore, in this work, I use the active ratio and mutual monitoring measure interchangeably.

ESOPs have three characteristics that make them plausible within the structure of my argument. First, compensation is broad-based and offered to employees at various levels, not

[1] https://www.prnewswire.com/news-releases/the-threat-from-within-71-of-business-leaders-surveyed-think-next-cybersecurity-breach-will-come-from-the-inside-301733317.html

[2] https://www.myinjuryattorney.com/consumer-privacy-data-breach-lawyers/if-your-information-has-been-compromised-in-a-data-breach/

exclusively to executives. Second, compensation is determined by the company and given to the employees; employees themselves do not have a discretion in their ownership. Third, employees do not have the option to trade their stock or time the exercise of their options. In other words, employees do not have access to their stock before retirement or leaving the company.

I now explain why ESOP holders have a stake in mitigating the risk of a data breach: Kamiya et al., 2021 reports that firms experience a 0.8% decrease in CAR during the three-day window surrounding data breach announcements. In my sample, the average market value of ESOP assets per participant equals $130,000, translating to an average loss of approximately $1,000 per participant. Moreover, in the case of a tail event, when people try to avoid bad outcomes, they tend to focus on the worst-case scenarios. As a result, employees may be particularly concerned about outcomes that are worse than average. In a subsample of incidents where the breach is followed by an FBI investigation, litigation, or lawsuits, the change in CAR can reach -8%, translating to an average loss of $10,000 per employee. In the worst-case scenario, post-attack costs can become so significant that the company is forced to file for bankruptcy.

In summary, data breaches are events that, when they occur, can cause significant harm to a company and its shareholders. Employees play a role in these incidents. ESOPs activate mutual monitoring among employee owners and help reduce the associated negative externalities. Therefore, I ask three questions. First, does a higher active ESOP employee ratio reduce the likelihood of a data breach? Second, do companies that experience a data breach take steps to increase their active ratio in order to improve protection? Finally, if firms understand that data breaches can result from employees' actions and that offering stock can help internalize the negative externalities of those actions, how do peer firms adjust their own ESOP ownership after a breach in a similar firm that could pose a threat to them as well?

I use two datasets in this study: data breach reports collected by the Privacy Rights Clearinghouse (PRC) from 2005 to 2023, and information on ESOP plans extracted from IRS Form 5500 for the period 2010 to 2023. I begin by using the EDGAR API [3] provided to identify all public and private firms in the data breach incident dataset that have more than 100 employees. I then use the ESOP plan information to map ESOP firms to the public firms identified in the PRC database.

[3] Electronic Data Gathering, Analysis, and Retrieval, It's the filing system used by the U.S. Securities and Exchange Commission (SEC) for companies and individuals to submit documents required by federal securities laws.

To address the first question, My hypothesis is that the higher the active ratio of ESOP employees in a firm, the more employee monitors the firm has, the lower the probability of a data breach. The results of the probit model align with this prediction. After controlling for executive compensation, employee wages, and the value of employees' ESOP ownership, I find that a higher active ratio is associated with a lower probability of a data breach. More specifically, I find that a one–standard deviation increase in the active ratio is associated with a 1% decrease in the probability of a data breach. However, firm size can weaken this protective role of ESOP employees, as larger firms often have more external relationships—such as with contractors or vendors—and larger internal teams, making employee monitoring more difficult.

I also find industry-level heterogeneity in the correlation between the active ratio—defined as the number of employee ESOP owners divided by the total number of employees—and the probability of a data breach. In particular, the finance; transportation and communication; and electric, gas, and sanitary industries exhibit strong negative correlations, whereas the service industry shows a positive correlation.

To examine whether data-breach target firms increase their active participant ratio after experiencing their first breach, as I hypothesize, I use a synthetic control model and a propensity score matching approach. In my sample, most firms do not experience a data breach, resulting in a small number of breached firms and a much larger pool of non-breached firms. This setup is well suited for a synthetic control model, as it allows me to construct a synthetic unit for each breached firm using the untreated firms as potential donors.

For the propensity score approach, I match each firm-year of a breached firm with a firm-year of a non-breached firm. I then apply a staggered difference-in-differences design to study how the active ratio changes after a breach. Using a TWFE estimator, I find that firms increase their active ratio by about 3% after the first breach, and this change is driven by an increase in the number of ESOP participants rather than by a reduction in the firm's labor force. I also find that firms reduce executives' bonuses by roughly 1%. In contrast, the synthetic control approach does not yield meaningful results. This lack of significance may stem from the issue that the donor pool used to construct synthetic controls may be contaminated: if firms in the donor pool have already begun adjusting their ESOP ownership despite not having experienced a breach themselves, the synthetic counterfactual will no longer be valid.

Using the fact that the results from the synthetic control analysis may be influenced by firms' efforts to protect themselves from a possible future breach,[4] and drawing on anecdotal and prior evidence showing that a data breach can reduce the cumulative abnormal returns of firms within the same 4-digit SIC category, I assume that if firms anticipate a breach, they look to incidents occurring in peer firms, meaning those within the same 4-digit SIC. Based on this assumption, I examine how non-breached firms respond to four events in their industry: the first breach, the first breach whose impact is at least at the 25th percentile of all breach impacts, the second breach, and the largest breach in terms of the number of affected individuals. I find that firms do not react to the second breach or to the largest breach in their industry; however, they do react to the first breach whose impact exceeds the 25th percentile of the distribution.

I find that in the three years after the first breach with at least 1,800 impacted individuals (the 25th percentile of the distribution of breach impacts), ESOP firms increase their active ratio by approximately 6% to 10%. This effect is estimated using the staggered difference-in-differences estimators of Sun and Abraham (2021) and Callaway and Sant'Anna (2021), which address several problems inherent in the TWFE estimator—problems highlighted by Goodman-Bacon (2021), such as the fact that TWFE effectively estimates a weighted average of all possible treatment–control comparisons, including those in which some "controls" are already treated. For inference and implementation, I use the estimator from Callaway and Sant'Anna (2021), which computes treatment effects as weighted averages of differences between treated and appropriate comparison groups (either never-treated or not-yet-treated). Because this estimator produces wider confidence intervals, I focus on these estimates throughout the paper in order to present the most conservative results. I also examine the heterogeneous responses of firms experiencing their first breach in different years and find that some industries decrease executives' salaries and non-equity incentive compensation.

In summary, and in a broader context, my findings show that a higher active ratio reduces the risk of a data breach through the mutual monitoring generated by broad-based stock compensation. Moreover, firms deliberately increase their active ratio following the first noticeable data breach in their industry, with the aim of reducing the likelihood of future breaches. It's important to emphasize that my research primarily investigates whether companies use employee stock-based compensation as a defense against data breaches, similar to vaccination, rather than medicine or insurance. In other words, I am not suggesting that

[4] One point worth mentioning is that I do not impose any industry restrictions when constructing the synthetic control. Since this section focuses on 4-digit SIC industries, a firm included in the donor pool for firm i may have already adjusted its active ratio in response to an earlier breach within its 4-digit SIC. Therefore, the donor firm, logically, would no longer serve as a valid untreated comparison unit for firm i.

companies utilize broad-based employee stock compensation to address damages or offset probable costs associated with data breaches. This work empirically and directly study the relationship between mutual monitoring and broad-based stock compensation.

2.1.1 Contribution to the Literature

In the prior literature, mutual monitoring has been examined in several strands. A closely related strand is Freeman et al.,2008 , who provide survey-based evidence that employees monitor their peers in firms that use group incentive plans. In addition, Core and Guay,2001 argue that direct monitoring becomes more difficult as firm size increases, leading larger firms to rely more heavily on stock options as an indirect monitoring mechanism. My work empirically and directly studies the relationship between mutual monitoring and broad-based stock compensation.

In more detail, the extant literature categorizes the relationship between broad-based employee equity plans and firms into three primary areas: the effects of broad-based plans on performance and retention (e.g., Hall and Murphy, 2003, Oyer, 2004, Oyer and Schaefer, 2005, Hochberg and Lindsey, 2010, Kim and Ouimet, 2014, and Aldatmaz et al., 2018); the effects of broad-based plans on firms' governance (e.g., Pagano and Volpin, 2005, Call et al., 2016, Masulis et al., 2020, and Wu et al., 2023); and the effects of the plans on other operational aspects, such as innovation and ESG ratings (e.g., Babenko and Tserlukevich, 2009, Babenko et al., 2011, Chang et al., 2015, Babenko and Sen, 2016, and Kong et al., 2023). I go over each category in the following paragraphs.

The effects of broad based plans on performance and retention. Hall and Murphy, 2003 primarily examine executive stock options but also discuss broad-based options. They suggest that companies offer stock options because the perceived cost is significantly lower than the actual economic cost, due to accounting practices. Oyer, 2004 suggests that firms offer stock-based compensation to index employees' compensation to the value of their outside employment opportunities, which leads to higher retention. Oyer and Schaefer, 2005 calibrate results of a standard moral hazard model and find that stock options granted to middle managers are positively correlated with retention, but they reject an incentive-based explanation for offering the stock. Hochberg and Lindsey, 2010 use the incentive-based compensation of firms that are geographically close but not in the same industry as a determinant of offering broad-based plans. They conclude that offering broad-based options leads to a significant increase in return on assets. Kim and Ouimet, 2014 specifically focus on ESOP plans. They show that adopting an ESOP where employees have high bargaining power leads to higher wages, which they interpret as a sign of improved performance following the adoption of the ESOP. One specific difference between my work and this study is that I am focusing on the variation within ESOP firms, rather than between ESOP and non-ESOP firms, as this work does. In my study, the main variable is a continuous variable, whereas in this work, the main variable is an indicator of whether ESOP plans are offered or not. In a different study, Aldatmaz et al., 2018 find that employee turnover decreases in the years following a large broad-based employee stock option grant in a firm.

The effects of broad based plans on governance. Pagano and Volpin, 2005 demonstrate that a manager with a small equity stake can deter hostile takeovers by using two methods to align employees with them: offering high wages or providing membership in an employee stock ownership plan. This approach aligns the incentives of both employees and the manager, suggesting that the firm's workers will act against raiders and support the incumbent CEO. Masulis et al., 2020 show that employee voting rights derived from membership in ESOP plans allow managers to pursue value-destroying acquisitions. Call et al., 2016 show that companies tend to grant more options during periods of misreporting compared to non-violating firms and their own non-violation periods. They suggest that companies prone to financial misreporting may use the strategic issuance of stock options to non-executive staff as a tactic to buy silence.

The effects of the plans on other operational aspects, such as innovation and ESG ratings. Babenko and Tserlukevich, 2009 study the tax benefits of broad-based stock options for firms. Babenko et al., 2011 show that the cash flow generated by exercising stock options leads to an increase in firm investment. Chang et al., 2015 demonstrate that broad-based employee stock compensation can encourage greater innovation among employees. Babenko and Sen, 2016 find that employees' contributions to ESPP plans[5] are associated with an increase in the number of patents filed by the company. Kong et al., 2023 show that offering ESOPs to non-executive employees leads to increased ESG ratings.

One difference between my work and the other works mentioned above is that I focus on a non-value measure of ESOPs and show that it remains effective even after controlling for the dollar value of ownership. Additionally, my work contributes to the existing literature on the consequences of data breaches for companies. The closest paper to my work in this field is Kamiya et al., 2021, which identifies factors influencing data breach likelihood and explores their impact on shareholders, management, and peer firms. However, my research differs in its focus. Rather than directly examining the consequences of data breaches, I explore how

[5] Employee Stock Purchase Plan

these events can serve as catalysts for mutual monitoring within ESOP firms. In essence, data breaches are treated as shocks that activate a specific mechanism, mutual monitoring, in these firms. The direct implications of data breaches are a secondary consideration in my analysis.

The extant literature on the effects of data breaches can be categorized into: the effects of data breaches on shareholder wealth (e.g., Acquisti et al., 2006, Gatzlaff and McCullough, 2010, Kamiya et al., 2021); the effects of data breaches on other financial outcomes, such as innovation (Ettredge et al., 2018, Huang and Wang, 2021, SUN, 2021, Wang et al., 2023, He et al., 2020); the effects of data breaches on management and governance (Banker and Feng, 2019, Zhang et al., 2024, Kamiya et al., 2021, Hsu and Wang, 2014, Lending et al., 2018,Hartmann and Carmenate, 2021, Ashraf, 2022); and the effects of data breaches on a firm's labor force (Akey et al., 2021, Bana et al., 2022). I go over each category in the following paragraphs.

The effects of data breaches on shareholder wealth. There are several papers in information science and technology science investigating this question. Acquisti et al., 2006 find that there is a negative impact of privacy breaches on a company's market value on the announcement day of the breach. They also provide evidence suggesting that the negative impact increases with the number of individuals affected by the breach. Similarly, Gatzlaff and McCullough, 2010, and Kamiya et al., 2021 demonstrate that data breaches have a negative and statistically significant impact on shareholder wealth.

The effects of data breaches on other financial aspects, such as innovation. Ettredge et al., 2018 show that firms mentioning the existence of trade secrets have a significantly higher probability of being breached compared to firms that do not. Huang and Wang, 2021 indicate that companies experiencing a reported data breach are subject to higher loan spreads and are more frequently required to offer collateral. SUN, 2021 show a positive link between being a target in M&A deals and the adoption of data breach disclosure laws at the state level. Wang et al., 2023 find that there is a positive link between the adoption of new IT technology and the risk of data breaches. He et al., 2020 demonstrate that there is a 10 percent reduction in R&D activities in the year subsequent to a data breach. This decrease is particularly pronounced in firms where R&D is not a central aspect of their business model. The research also highlights a reduction in the number of patents filed two years after the breach and an increase in cash reserves the following year.

The effects of data breaches on management and governance. He et al., 2020 find that breaches caused by system deficiencies increase the chance of a CIO's turnover by 72 percent. Zhang et al., 2024 find that companies reduce overall CEO pay following multiple data breaches, specifically in the non-cash incentive portion of the CEO's compensation. Conversely, the paper shows that companies increase the total compensation for non-CEO executives after repeated data breaches, with this increase primarily focused on the non-cash incentive component. Kamiya et al., 2021 provide evidence that a CEO's bonus and option awards decrease following a data breach. Hsu and Wang, 2014 show that board size, average age/tenure, and age heterogeneity could reduce the likelihood of security breaches, while the proportion of independent directors and tenure heterogeneity could increase it. Lending et al., 2018 show that socially responsible companies with smaller boards and greater financial expertise are less likely to be breached. Ashraf, 2022 finds that data breaches experienced by a firm in the same industry one quarter before leads to a reduction in material weaknesses for non-breached firms in the current period.

The effects of data breaches on a firm's labor force. Akey et al., 2021 show that salaries increase after a firm suffers a data breach. Bana et al., 2022 demonstrate that, following a data breach, companies tend to hire more cybersecurity and public relations professionals.

Specifically regarding industry-wide shocks, Kamiya et al., 2021 demonstrates that the cumulative abnormal returns (CAR) of industry peers are impacted as a result of an incident. Ashraf, 2022 finds that data breaches experienced by a firm in the same industry with a similar product market one quarter prior lead to a reduction in future material weaknesses for non-breached firms in the current period. Different from previous studies, my work is the first to comprehensively investigate the effects of industry data breach incidents on ESOP participation, rank-and-file employees, and executives.

2.2 Institutional Details

2.2.1 Employee stock ownership plan (ESOP)

An employee stock ownership plan (ESOP)[6] is a retirement program that primarily invests in employer stock. After the passage of the Employee Retirement Income Security Act of 1974 (ERISA), this type of plan became widely accepted. The plan is designed to provide employees with income at retirement or termination while offering tax advantages to the company. The key feature of an ESOP is its stock bonus component. This means

[6] This section is based on information from Chapter 8 of IRS documents.

contributions are typically made in company stock, and benefits from an ESOP are usually distributed in whole shares of employer stock, but fractional shares can be paid in cash. Employees' ESOP ownership amount is determined by the company as a function of factors like tenure, and wage. Because an ESOP is a defined contribution plan, it is subject to all of the rules and regulations under Code 401(a). However, because an ESOP must, by definition, be a stock bonus plan, a plan that fails to qualify as an ESOP under the code, may still be qualified as a stock bonus plan.

ESOPs are designed so that employees receive more benefits the longer they work for the company. In other words, employees acquire portions of their shares over time, which are known as vested shares, representing the portion of the ESOP that employees own. The contribution of these shares must be structured to become fully vested by the time of retirement. If an employee leaves the company earlier, they will only own the vested portion of their ESOP account.

An ESOP usually takes two forms: non-leveraged and leveraged. In a non-leveraged ESOP, the company contributes stock or cash to the ESOP each year, which then holds the stock. The stock is allocated to participants' accounts based on the formula outlined in the plan. In a leveraged ESOP, a bank loans money to the ESOP through a promissory note, which is guaranteed by the company. The ESOP uses the loan proceeds to buy stock from the company or existing shareholders. The company makes contributions to the ESOP trust to repay the loan, and the ESOP trustees repay the loan according to the schedule. Finally, participants receive stock or cash when they retire or leave the company.

An ESOP functions through a trust fund, where companies either issue new shares, borrow funds to buy shares, or contribute cash to purchase shares. An ESOP trustee, an impartial third party, holds legal ownership of company stock and must act in the best interests of plan participants. Trustees, who can be either independent or company insiders, are selected by the board of directors and are responsible for protecting participants and improving the ESOP.

Employer contributions to an ESOP are generally deductible by the employer if they do not exceed 25% of the participants' aggregate compensation. Dividends paid on the ESOP's employer securities are considered earnings on account balances, and therefore do not count towards the annual addition limitation and can be deductible if paid out to the plan participant.

Employees do not pay tax at the time of contributions into the ESOP. They are taxed at the time of distributions, and the rates they are taxed on is favorable to the participant. The ESOP distributions can be rolled into an Individual Retirement Account(IRA) or other retirement plans accumulating gains over time taxed as capital gains later. ESOP distributions are taxed as ordinary income, but if employees receive a lump-sum distribution in the form of stock, they will generally pay ordinary income tax on the value of the employer's contributions to the plan, plus capital gains tax on the appreciation in stock value when the stock is sold.

In this study, I use data on ESOP firms obtained from IRS Form 5500, which employers maintaining a pension or welfare benefit plan under ERISA are required to file. I define an ESOP as any form with items '2O' or '2P' marked in the filling.[7] Additionally, I classify a plan as an ESOP if its name includes the terms 'Employee Stock Ownership Plan' or 'ESOP'. This analysis focuses on the stock bonus feature of ESOPs, rather than their qualification under section 401(a). Consequently, I do not examine the specific qualifications of the plans under the Code.

2.2.2 Security breach notification laws

Security breach notification laws are a set of regulations that oblige companies to take a specific set of actions in the case of a data breach incident. In the United States, security breach notification laws were first enacted in California in 2003, with other states enacting similar laws thereafter. Table 2.1 shows when each state implemented these laws. The first part of each regulation provides the definition of a security breach incident. For example, California defines a security breach as "an unauthorized acquisition of computerized data that compromises the security, confidentiality, or integrity of personal information." Definitions in other states are similar, though with some variations.

Generally, the law specifies the conditions under which an entity must notify victims, attorneys general, consumer agencies, or all consumers nationwide. It provides a detailed definition of personal information and includes guidelines on the specifics of the notification, such as the details to be included in the notice and the types of services, like credit monitoring, that the company should offer to victims.

In this work, the data on data breaches comes from privacyrights.org. This dataset covers breaches reported to attorneys general and the Department of Health and Human

[7] According to IRS regulations, '2O' indicates an ESOP other than a leveraged ESOP, while '2P' refers to leveraged ESOPs.

Services from January 1, 2005, to September 28, 2023, and includes 35,167 incident reports. For my analysis, I treat all incidents in the dataset as data breach incidents, regardless of their classification under legal definitions of a security breach. For example, a physical theft of printed names and Social Security numbers of a group of consumers is included in my sample as a data breach incident. However, it is unclear whether such an incident would be classified as a security breach according to the provided definition. Therefore, I include all reported incidents in my analysis.

2.3 Data

This research uses two primary data sources—security-breach data and ESOP data—which are discussed in the next two sections. In addition, the study incorporates several other datasets: Compustat for firm fundamentals, CRSP for stock-market variables, BoardEx for board information, ExecuComp and the EDGAR database for executive compensation, and LSEG (formerly Refinitiv/Thomson Reuters) for institutional ownership, for which I also provide more details in the next section. A full description of all variables and their sources is presented in the Appendix.

2.3.1 Institutional ownership data

Institutional investors are large entities—such as hedge funds and pension funds—that invest substantial capital on behalf of clients. In the United States, since 1978, institutional investment managers exercising investment discretion over accounts totaling at least $100 million have been required to file Form 13F with the SEC each quarter, within 45 days of the quarter's end, pursuant to Section 13(f) of the Securities Exchange Act. These filings, accessible through the SEC's EDGAR system (which provides an API), identify the institutional investor (the filer) and list the issuers whose securities they report holding.

I initially retrieved 13F data via the SEC's EDGAR API. However, machine-readable XML holdings tables were only mandated after June 2013, leading to incomplete coverage before that date. Although I collected records dating back to 2000 and constructed ownership ratios, many early-period observations are missing or zero, as expected due to this reporting change.

Therefore, I rely on the LSEG (formerly Refinitiv/Thomson Reuters) institutional holdings data available through WRDS. These quarterly data extend back to 1980 and provide broader historical coverage suitable for my analysis. Because the LSEG share-count field is often missing or inconsistently scaled, I use shares outstanding from Compustat Quarterly and align them by CUSIP and report date to compute institutional ownership ratios.

I use a ±90-day window around each report date and compute a time-weighted average of the Compustat share counts within this interval to estimate the shares outstanding for each institutional ownership observation, assigning greater weight to values closer to the report date. I apply this procedure to all LSEG observations—not only those with missing values—to ensure consistency. I rely on issuer-reported Compustat share counts for this construction, as they are generally more reliable than figures implied by institutional filings.

After estimating the quarterly institutional ownership ratio—defined as the total number of shares held by institutional investors reported by LSEG divided by total shares outstanding—I map these data back to the annual Compustat dataset. Specifically, for each firm-year, I assign the institutional ownership ratio from the quarter whose report date is closest to the fiscal year-end in Compustat.

2.3.2 Data breach incidents

The data on data breaches comes from privacyrights.org, covering breaches reported to attorneys general and the Department of Health and Human Services from January 1, 2005, to September 28, 2023.[8] This dataset includes 35,167 incident reports. My goal is to identify all public firms that experienced a data breach, as well as all private firms and their corresponding SIC codes.

The main identifier for each incident in this dataset is 'Name of Entity'. I drop all observations that include the words 'university' or 'school' in the name of the entity. This helps to delete incidents that happened in educational centers. Next, I drop all observations where all of the variables related to the date of the incident are missing.

Next, using the EDGAR API, I try to match each entity name in the dataset to its CIK, SIC, and other important identifiers. I write my queries in three steps. First, I use the full name of the entity. Second, I remove phrases such as 'Co', 'Corporation', and 'LLC' from the end of the name. Third, I remove the last word of the entity's name. I am doing this to account for the various ways the name of an entity could have been mentioned. The result of this process includes all possible matches for a firm that has experienced a data

[8] This is the time of the report; there are incidents that occurred before 2005 but were reported later.

breach to any name in the SEC dataset. For example, a company like ‘Apple,’ identified as a data breach company, will be matched to both ‘Apple’ and ‘Applebees’ within the SEC database. This shows that the result needs further cleaning. Nevertheless, the API result is an improvement over the raw data breach reports, as it provides the CIK and additional characteristics for a subset of the firms experiencing data breaches.

Any entity name that is not matched to SEC data falls into one of two categories: it is either not a publicly traded company or the way its name appears in the original data breach dataset is not standard. To clean up this part of 4,500 entity names(not reports), I carefully examine each entity name, removing any nonprofit, government agency, local business, or company with fewer than 100 employees based on data from Pitchbook.com and other available sources. If I can’t find information about the number of employees for an entity, I remove it from the dataset.[9]

After examining the 4,500 names of entities to identify public firms and incorporating results from the EDGAR API, I end up with 530 public firms that have had at least one incident report since 2005. From the set of private firms, I keep only those with a maximum impact reported greater than 500. This results in a sample of 1,805 private firms for which I manually find the SIC code. Figure 2.1 illustrates the number of public and private entities in the top ten industries, ranked by the affected entities, within each 2-digit SIC code from 2005 to 2023. As the graph shows, Health Services and Business Services have the highest levels of data breach incidents, with 15% and 9% of all incidents occurring in those industries, respectively.

Furthermore, for my analysis, I need to make sure that all incidents assigned to each firm are distinct. In the older versions of the dataset, the dataset had repeated reports of the same event that were not easily identifiable, and I used ML models to find the distinct reports. However, in the newer versions, based on the documentation of the dataset using a “set of ML algorithms and the knowledge of experts in the field,” the dataset identifies likely repeated-report clusters for each incident. I pick one incident from each cluster based on the combination of the report time and the number of impacts.

[9] The number of employees is used here only as a criterion to exclude small corporations and is not related to my research question.

2.3.3 IRS Form 5500 and ESOP data

According to the IRS, employers that maintain a plan and administrators of pension or welfare benefit plans covered by ERISA must file Form 5500 to report information on the plan's qualification, financial condition, investments, and overall operations.

Because the IRS filings contain only an EIN and do not indicate whether a firm is public or private at the time of filing, it is important to clarify how I identify public firms in this study. I consider a firm to be public if it has a CIK in the EDGAR database, a GVKEY in Compustat, and a PERMCO in CRSP, along with an EIN in the IRS 5500 forms, with valid linkages across these identifiers in each fiscal year. I further restrict the sample to firms with more than $1 million in total ESOP assets and at least three years of data. This approach may understate the actual number of public ESOP firms, but it ensures a comprehensive coverage of all variables required for the analysis.

Public ESOPs represent roughly 6% of all ESOPs, consistent with statistics reported by organizations such as the NCEO (National Center for Employee Ownership). Because the IRS forms available at the time of research include only a limited number of post-2021 filings, my final sample of public ESOP firms contains 3,521 firm-year observations covering 358 firms from 2004 to 2021. As Figure 2.12 shows, Depository Institutions and Electric, Gas, and Sanitary Services have the highest number of firms offering ESOP. Because it may seem unusual for utilities to have a high rate of ESOP adoption, I list all firms in this group from my sample in Appendix Table 2.29.

2.3.4 Final sample

I identify the fiscal year associated with each distinct incident and match with the firm data across all datasets. I do not impose any condition on the number of incidents per year, so it does not matter how many incidents are assigned to the same year. I keep firms with at least three years of observations and ESOP assets of at least one million dollars. My final dataset has 3,521 observations and consists of 69 public ESOP firms that experienced a breach and 289 public ESOP firms that did not. The breached-firm sample includes 885 firm-year observations, while the non-breached-firm sample includes 2,636 observations, with 140 firm-year pairs that had at least one breach.

2.4 Results

2.4.1 Likelihood of experiencing a data breach incident

To begin, it is important to note that employee ownership through ESOPs is determined by the firm. ESOP shares are allocated to employees in the form of company stock and vest over time. However, employees cannot access, diversify, or sell these shares before reaching age 59.5. If an employee leaves the firm before that age, they receive only the portion of shares that has vested and been added to their account, and they may transfer those shares to another Individual Retirement Arrangement (IRA) after paying a penalty.

I hypothesize that firms with higher levels of employee ownership will experience increased peer monitoring or self-control, which should lead to a lower likelihood of data breaches. Unlike prior literature, which uses dollar value measures, I focus on the number of employees classified as owners or active participants of an ESOP. By definition, an active participant is an employee who works for the company and has contributions made on their behalf to the ESOP plan. Therefore, the primary proxy for employee ownership is the ratio of active participants in the ESOP plan to the total number of plan employees. I call this variable active ratio and it is intended to capture the effect that each rank-and-file employee, as an owner, can have as a monitor; the higher the number of active participants relative to the total number of employees, the greater the monitoring. To provide intuition for why this might be a useful measure to protect the firm from a data breach, I look at the data on ESOP ownership and on breached versus non-breached firms in my sample.

First, I examine the trend in the standard ESOP ownership measure used in the literature: the total value of ESOP assets divided by the total number of employees. As Figure 2.3 shows, the median breached firm has an overall higher ESOP ownership than the median non-breached firm. However, if I decompose this ratio as

$$\frac{\$ESOP}{\text{total employees}} = \frac{\text{Active ESOP participants}}{\text{total employees}} \times \frac{\$ESOP}{\text{Active ESOP participants}}$$
$$= \text{Active ratio} \times \text{plan value per participant}$$

and plot the trend for median of each component in Figure 2.4, I find that non-breached firms have a higher active ratio but a lower level of ESOP ownership per participant. Overall, the dominant component for non-breached firms is the active ratio, whereas the dominant component for breached firms is ESOP ownership per participant.

Then I will have comparisons between different variables of breached and non-breached firms. I classify the variables into three categories, and to present them more clearly, I normalize them using Z-scores so I can plot them on the same graph. The first category follows Kamiya et al. (2021)' variables and includes firm fundamentals. The second category consists of variables related to executive compensation, firm governance, and their components. The third category captures characteristics of rank-and-file employees, including ESOP participation and employee wages.

To obtain a true comparison between breached and non-breached firms in the firm level, I calculate the median value of each variable for each firm across all years it appears in the dataset. This approach yields one aggregate observation per firm, which I then use to compare the variables between the two groups of breached and non-breached firms. All continuous variables are winsorized at the 1st and 99th percentiles.

According to Figure 2.5, firms that experience a successful data breach incident are larger, have higher future growth opportunities(higher Q) and higher ROA, and also less risky stocks(lower stock return volatility). In addition, ESOP firms with a breach incident tend to operate in industries with higher Herfindahl indices, which indicates lower levels of market competition. One important observation is that breached and non-breached firms are not significantly different in wage levels. Wage is measured using staff expenses from Compustat, and missing observations are replaced using the median staff expense for the firm's four-digit SIC code in that year.[10]

In Figure 2.6, which focuses on governance variables, breached firms have higher total executive compensation but allocate a smaller portion of this compensation to salary. CEOs and CFOs in breached firms also hold higher levels of equity relative to non-breached firms, with a larger share of their compensation granted as equity. Institutional block-holder ownership is not significantly different between breached and non-breached firms, although breached firms have a higher number of board committees. In Figure 2.7, breached firms have a higher ESOP participant population, larger ESOP assets, and greater ESOP ownership per employee. However, the ratio of ESOP participants relative to total employees (the active ratio) is lower for breached firms.

Finally, Figure 2.8 compares three indicator variables that cannot be visualized using boxplots in the previous graphs. Breached firms are more likely to be in the Fortune 500 and are less financially constrained. Financial constraints are defined following Whited and

[10]Staff expenses include: incentive compensation, other benefit plans, payroll taxes, pension costs, profit sharing (when included in staff expense), and salaries and wages.

Wu 2006, using the WW Index constructed from cash flow, dividends, long-term debt, total assets, industry sales growth, and firm sales growth; higher values indicate more constrained firms. The presence of a board-level risk committee is not significantly different between breached and non-breached firms. A board is considered to have a risk committee if the term "risk" is included in the committee name. Since the Z-scores in the plots do not reflect the magnitude of each variable for each firm, I also report the differences between medians in Table 2.2. In sum, it seems that firms hit by a data breach are larger, stand out more, and have less volatile stock prices.

Table 2.3 presents the correlation between the main variable of this study, the active ratio, and a group of other important variables. Figure 2.13 shows the distribution of this variable in the sample. Except for 130 observations with a ratio close to one, the remaining values are spread out relatively evenly. Figure 2.10 presents the distribution of industry of firm-years with an incident. As the figure shows, among ESOP firms, the highest number of data breach incidents occurs in the finance industry, followed by manufacturing and wholesale trade. Additionally, the overall trend of incidents over time is increasing. A key point of this table for my analysis is that it provides evidence that data breach incidents in this sample are not restricted to specific industry-year shocks. Instead, each industry can experience incidents in different years, and various industries experience incidents in each year. Figure 2.11 presents the distribution of targeted firm-year observations across each state. This table is important for my analysis because it demonstrates that data breach incidents in this sample are not restricted to specific state-year shocks. Although I can see that the number of reports increases after the adoption of data breach disclosure laws, which is expected.

Because the number of breached events in ESOPs is much smaller than the number of non-breached events, and most of the variation in the active ratio comes from between-firm rather than within-firm variation (std: 0.27 vs. 0.09), I focus my analysis at the industry level in this part. Then, to directly investigate the role of ESOP ownership in the likelihood of becoming a target of a data breach, I estimate the following probit model:

$$\begin{aligned} P(\text{Data breach in ESOP firm } i \text{ in year } t=1) = \alpha + \beta_1 \left(\frac{\text{Active participants}}{\text{total employees}}\right)_{i,t-1} + \\ + \beta_2 \log\left(\frac{\text{ESOP}}{\text{active participants}}\right)_{i,t-1} + X_{i,t-1} + \\ + Industry\ FE + Year\ FE + \epsilon_{i,t} \end{aligned} \tag{2.1}$$

Based on my hypothesis, the higher the active employees ratio, the better the monitoring or self controlling, and the lower the likelihood of a data breach. In other words, I expect to see a negative sign for β_1 in Equation 2.1. I also add the average ESOP assets each employee has. This is supposed to capture the strength of employee ownership; ideally, with no free riding, the higher the employees' ownership, the greater their incentive to monitor their peers, and the lower the probability of a breach. In other words, I expect to see a negative sign for β_2 as well. Other control variables, shown by $X_{i,t-1}$ are variables from Table 2.2. I also add industry and year fixed effects. All variables, except Tobin's q which is estimated for two years prior,[11] are measured for one year prior to the breach; if a breach happens in year t, control variables from year $t-1$ are used on the right side of Equation 2.1.

Table 2.4 reports results of estimating Equation 2.1. I start by regressing the breach indicator on the active ratio alone; the coefficient is zero. In Regression (2), I add industry fixed effects, and the coefficient becomes negative. In the next column, I add the value of employee ownership and find that the active ratio is negative but the ownership value is positive. In Column (4), I add firm size, and the effect of ESOP ownership value is fully absorbed by firm size. In Columns (5), (6), and (7), I add sets of firm fundamentals and governance measures, and the results show that firm size is strongly and positively correlated with the likelihood of a data breach, while none of the other variables such as the board's risk committee or the firm's R&D activities are significant. I expect to see a negative coefficient for the board's risk committee and a negative coefficient for R&D, since firms with more R&D activity should be more careful about protecting their information after controlling for firm size and characteristics such as being in the Fortune 500, which may make a firm more visible and therefore more likely to be targeted.

In Table 2.5, while I keep all control variables from the previous table, I add a set of variables related to executive and employee compensation. In Columns (2) and (3), I add executive equity compensation and wages and do not find any new effects. In Column (4), I add a variable called the employee governance measure. This variable is meant to account for the strength of rank-and-file employees' voting power in the company's decision-making process. Since I do not observe the number of vested shares in each ESOP, I measure this variable by dividing the value of the ESOP assets (the value of the vested shares held in the trust) by the market value of the firm. Adding this variable slightly improves the coefficient of active ratio.

I also add the state unemployment rate, Column (5), which controls for local labor

[11] Because it is highly correlated with past stock performance.

market conditions and may be related to firms' decisions to offer broad-based stock compensation, since firms tend to offer more stock compensation when employees have better outside options. I also control for high/low tax year indicators in Column(6). Based on prior literature, firms time stock compensation to maximize the value of the tax deduction and reduce their overall tax liability. Moreover, based on Column (7), because executive equity is correlated with firm size, dropping firm size from the specification makes this coefficient positive.

In sum, aside from firm size, the only variable that is strongly correlated with the active ratio is institutional block ownership. In a similar specification for all public firms, Kamiya et al. (2021) find that institutional ownership is higher in non-breached firms, but after controlling for similar variables, the coefficient becomes close to zero and is slightly positive in some cases. My sample is a subset of those public firms, and this pattern may indicate that in ESOP firms, monitoring by institutional owners reduces the likelihood of a data breach.

So far, I have assumed that there is no heterogeneity in the relationship between the active ratio and the probability of a data breach within industries. In the next table, I investigate heterogeneity based on different scenarios. As Table 2.6 shows, the first scenario, presented in Column (1), is that employee monitoring could differ based on the value of their ownership; the higher the ownership value, the more incentive employees have to act as monitors. In Column (1), the coefficient on the interaction term is zero. However, the interaction term absorbs the positive correlation in the active ratio, and I observe a negative coefficient on the active ratio that aligns with my hypothesis.

Columns (2), (3), and (4) investigate the correlation between the active ratio and the likelihood of a data breach depending on executive equity compensation and firm size. Based on the results, the higher the active ratio, the lower the probability of a data breach. However, this effect is weaker in larger firms and in firms with higher levels of executive equity compensation. In Model (6), I study both interaction terms together and find that the offsetting effect from firm size is much larger than the offsetting effect from executive equity.

Column (5) shows that in larger firms across all industries, executive equity acts as a factor reducing the likelihood of a data breach. In Column (7), I include all interaction terms between the active ratio and executive equity together, and the results indicate that in larger firms, higher executive equity compensation further reduces the likelihood of a data breach. In addition, the offsetting effect against the preventive impact of the active ratio is stronger in larger firms.

All in all, Table 2.6 suggests that there is a negative correlation between the active ratio and the probability of a data breach across all industries; however, this effect is weaker in firms with larger firm size. On the other hand, an increase in firm size appears to strengthen executives' incentives to reduce the probability of a breach in order to protect the value of their stock. One other point worth mentioning from Table 2.6 is that firms with a risk committee on their board have a lower likelihood of a data breach, which is reasonable.

To interpret the probit results, I take Column (7) of Table 2.6 as the preferred model and examine the marginal effect of the active ratio. It shows that a one–standard deviation increase in the active ratio, holding everything else constant, is associated with a 1% decrease in the probability of a data breach.

Throughout this work, whenever I have discussed monitoring, I have referred to self-control and monitoring together, simply because my data do not allow me to distinguish between them. In other words, I cannot tell whether the negative correlation arises because employees are watching each other or because they are simply more careful with their own actions. In this section, I use my results so far to argue that this effect is more related to monitoring and less related to self-control.

First, the fact that I still observe a negative effect of the active ratio after adding interaction terms provides general evidence in favor of a monitoring mechanism. If the entire effect were due to self-control, then it would not depend on any outside factor, and the coefficient should not change after conditioning on the employees' environment. A person who is careful with their own actions, will be always more careful, it should not change based on the firm size. Employees might claim they are more careful with their actions in larger firms because those firms hold more data and the potential damage would be greater. However, if that were the case, the interaction term should be negative rather than positive.

Second, the interaction term between the active ratio and the value of the ESOP is not significant. This suggests that employees' behavior does not vary with the value of their ownership, which is inconsistent with a self-control mechanism.

Third, one possible interpretation of the interaction between executive equity and the active ratio is that, because executives own a much larger share of the firm, higher executive equity could weaken the effect of the active ratio: employees might free-ride and assume that executives will take the lead in preventing incidents. If this channel were true, I should have observed meaningful coefficients for wage or ESOP value, but I do not.

Fourth, given the points above, the positive coefficient on the interaction between firm size and the active ratio can be interpreted as a decline in the quality of employee monitoring in larger firms. As firms grow, teams become larger and more dispersed, making employee monitoring less effective.

I end this section by examining the response of the active ratio in each industry on the probability of a data breach. To do so, I run the following regression:

$$P(\text{Data breach in ESOP firm } i \text{ in year } t=1) = \alpha + \beta_{Ind}\,(Active\ ratio \times Inustry) + X_{i,t-1} + \\ + Industry\ FE + Year\ FE + \epsilon_{i,t} \tag{2.2}$$

This regression fits a different line for each industry, and the results are shown in Table 2.7. Column (1) is the base model and shows that the correlation between the active ratio and the likelihood of a data breach is negative in Finance, Wholesale, and Electric, Gas, and Sanitary Services. One point worth mentioning is that these industries have a larger number of breach observations, and the positive coefficient for the service industry could simply reflect its small sample size, since I am fitting a separate line in each industry. In Columns (2) and (3), my goal is to study how the interaction between executive equity and the active ratio varies across industries. Based on Column (2), this offsetting effect exists in most industries, whereas Column (3) shows that there is no meaningful effect of executive equity alone on the likelihood of a data breach across industries.

Overall, the results in this section suggest that a higher ratio of ESOP participants is associated with a lower likelihood of a data breach, at least in Finance, Wholesale, and General Utilities, due to the peer-monitoring that employee–owners can provide. Moreover, two factors can weaken this monitoring effect: executive equity and firm size. I could not perform a firm-level analysis(firm fixed effects) because in a probit model many observations are dropped due to perfect prediction. In addition, because I have many zeros relative to ones, running a linear probability model also leads to coefficients of zero for all variables. In the next section, I shift to a firm-level investigation of what happens after a firm experiences its first data breach incident.

2.4.2 Effects of a data breach on ESOP participation

In this section, I examine what happens to a firm's ESOP participation following its first data breach incident. Based on my hypothesis, if firms use ESOPs as a way to impose peer monitoring after a data breach, the number of active participants relative to the firm's employee population in the plan should increase compared to the period before the breach.

To establish a causal interpretation, I need to compare the observed change in a firm's active ratio with the change that would have occurred had the firm not experienced its first breach. This counterfactual is not observable in practice. Therefore, to approximate it and support a causal inference, I rely on two approaches: (1) a synthetic control methods and (2) a matching method.

Synthetic control model

The synthetic control method constructs a data-driven comparison unit that approximates how a treated firm would have evolved in the absence of the treatment. By assigning optimal weights to untreated firms, the method generates a synthetic counterpart that closely matches the treated firm's pre-treatment trajectory and predictor values. The post-treatment gap between the treated firm and its synthetic version then reflects the treatment effect.

Generally, the synthetic control is constructed by choosing a set of non-negative weights that sum to one and minimize the distance between the treated firm and a weighted combination of control firms(or donor firms) in the pre-treatment period. These weights are selected to match both the pre-treatment outcome path and relevant predictors, ensuring that the synthetic unit provides a credible approximation of the untreated counterfactual. The estimator relies on the idea that, when a suitable convex combination of control firms reproduces the treated firm's pre-treatment characteristics, the weighted control group can be a benchmark for isolating the effect of the treatment.

Specifically, the method searches over weights that are non-negative and sum to one, and chooses the vector that minimizes a weighted quadratic loss function comparing pre-treatment outcomes and predictors of the treated unit to those of the synthetic control. The optimization typically proceeds in two nested steps: first determining the optimal predictor weights that balance the importance of different predictors (covariates), and then selecting the donor weights conditional on those predictor weights.

Formally, a synthetic control can be represented by a $J \times 1$ vector of weights for J firms of the donor pool, $\mathbf{W} = (w_2, \ldots, w_{J+1})'$. Given a set of weights, $\mathbf{W}$, the synthetic control estimators of Y_{1t}^N and τ_{1t} are, respectively:

$$\widehat{Y}_{1t}^N = \sum_{j=2}^{J+1} w_j Y_{jt}, \tag{2.3}$$

and

$$\widehat{\tau}_{1t} = Y_{1t} - \widehat{Y}_{1t}^N. \tag{2.4}$$

where Y_{1t}^N represents the value of the outcome variable that would have been observed for the affected unit in the absence of the intervention, and Y_{1t} denotes the observed value of the outcome variable for the treated unit.

Following Abadie and Gardeazabal, 2003 and Abadie et al.,2010, the weights $w_2, \ldots, w_{J+1}$ are chosen so that the resulting synthetic control best reproduces the pre-intervention values of the predictors of the outcome variable for the treated unit.

In other words, weights minimize

$$\|X_1 - X_0 W\| = \left(\sum_{h=1}^{k} v_h \left(X_{h1} - w_2 X_{h2} - \cdots - w_{J+1} X_{h,J+1} \right)^2 \right)^{1/2} \tag{2.5}$$

$$\text{subject to } w_j \geq 0 \text{ for all } j = 2, \ldots, J+1, \quad \sum_{j=2}^{J+1} w_j = 1.$$

Then, the estimated treatment effect for the treated unit at time $t = T_0 + 1, \ldots, T$ is

$$\widehat{\tau}_{1t} = Y_{1t} - \sum_{j=2}^{J+1} w_j^* Y_{jt}. \tag{2.6}$$

The positive constants $v_1, \ldots, v_k$ in 2.5 reflect the relative importance of the synthetic control reproducing the values of each of the k predictors for the treated unit, $X_{11}, \cdots, X_{k1}$.

The choice of the weight vector $V = (v_1, \ldots, v_k)$ depends on how important each predictor $X_{11}, \ldots, X_{k1}$ is for forecasting the counterfactual outcome Y_{1t}^N. Each v_h is intended to represent the contribution of matching X_{h1} to accurately predict Y_{1t}^N in the post-intervention period. Because Y_{1t}^N is not observed for post-treatment, it is not possible to directly assess how well each predictor explains post-intervention outcomes. However, Y_{1t}^N is observed in the pre-intervention period, which allows to evaluate how informative the predictors $X_{1j}, \ldots, X_{kj}$ are. In practice, V is chosen so that the resulting weights $W(V)$ minimize the mean squared prediction error (MSPE) of the synthetic control relative to Y_{1t}^N in the pre-intervention period.

To design my model, I follow Abadie, 2021. Because my dataset is an unbalanced panel and consists of 18 years of annual observations—which is not a long period for this class of models—I carefully examine the data to identify the most suitable subsets for constructing a reliable synthetic control. It is typically recommended to have at least five pre-treatment periods and four to five post-treatment periods for the treated unit. A key requirement is that the treated unit must have no missing observations for the outcome variable during the pre-treatment period.

As Table 2.8 shows, because the number of observations in 2004 and 2005 is very small, many treated firms do not have data for those years, so I drop those years from the pre-treatment period for all firms. I also cannot construct synthetic units for firms treated before 2011, since their pre-treatment period would be too short—fewer than five years. Moreover, because those firms are treated during the treatment windows of firms treated after them, they cannot serve as donors. I also cannot construct synthetic units for firms treated in 2019, 2020, or 2021, as they do not have enough post-treatment periods, although they can still be included in the donor pool for firms treated earlier. Overall, the most suitable sample of treated firms for which I can construct synthetic control units consists of those treated between 2011 and 2018.

I conduct a more thorough investigation of the data to identify the treated units suitable for my synthetic control model. Among the two firms treated in 2011, one begins reporting only in 2010 and the other has no observations for 2008, so neither can have a synthetic unit constructed for them. A similar issue arises for two firms treated in 2012. For the firms treated in 2013, if I focus on the period from 2008 to 2018 and drop one treated firm due to missing data between 2008 and 2013, I can construct synthetic controls for the remaining two.

It is also worth noting that the donor pool for these cases consists of all non-treated firms with available data on the active ratio during that window, as well as any treated firms whose treatment occurs after 2018. Following the same logic, the best coverage for firms treated in 2014 is 2009–2019. For firms treated in 2015, the appropriate window is 2010–2021; for those treated in 2016, it is 2008–2021; for those treated in 2017, it is 2010–2021; and for those treated in 2018, the best coverage is 2007–2021. This process gives me a sample of 24 firms for which I can construct a synthetic unit.

In the next part, I need to choose my predictors, or the X vectors, as described above. In datasets with long pre-treatment periods, it is common to divide the pre-treatment years into training and validation subsets and select the predictor set that minimizes the mean squared prediction error (MSPE) in both periods. Because my dataset does not have long pre-intervention windows, I instead try different sets of predictors that are strongly correlated with the active ratio and not correlated with the breach shock. I then select the predictor set that yields the lowest prediction error.

Table 2.9 presents a sample of ten predictor subsets for firms, along with the mean of their associated mean squared prediction error (MSPE) and their signal-to-noise ratio. An "x" in a column indicates that the corresponding variable is included in that predictor set; thus, all "x" entries in a given row represent the full set of predictors used in that specification. A lower MSPE indicates a better pre-intervention fit. The signal-to-noise ratio is calculated as the mean absolute gap between the synthetic unit and the treated firm divided by the MSPE; for a good pre-intervention fit, a higher signal-to-noise ratio may indicate a stronger estimated effect of the shock.

Table 2.9 reports the mean squared prediction error and the signal-to-noise ratio across the 24 treated firms in the sample. Based on Table 2.9, adding the first lag of the active ratio as a predictor reduces the MSPE by half. Further adding four additional lags of the active ratio, firm size, the institutional block ownership ratio, ESOP value divided by total employees, the industry Herfindahl index, and both the industry Tobin's Q and its lag yields an MSPE of 0.06 and a relatively high signal-to-noise ratio. I tested several additional combinations not shown in the table, and this specification produced the best combination of MSPE and signal to noise ratio. Therefore, I use this predictor set for the synthetic control model for all treatment years.

After selecting the predictor set, I construct synthetic units for each of the 24 firms. Figure 2.14 shows the path of each synthetic unit compared to the path of the treated unit.

As the graphs show, CIK 4457, 62709, 73721, 318673 and 899051 provide relatively good visual fits in the pre-intervention period.

In the next step, I need to assess whether the estimated treatment effect for each treated firm is distinguishable from patterns that could arise by chance. To do this, I conduct a placebo test by reassigning the treatment to each donor firm one at a time and estimating its corresponding synthetic control using the same predictors and time windows. This process is repeated for every donor firm in the donor pool of each treated firm. Since donor firms did not experience a breach shock, the estimated post-intervention gaps for these placebo units should be small and should not exhibit the sharp divergence observed for the true treated firms. Comparing a treated firm's gap to the distribution of placebo gaps provides both a visual and quantitative benchmark for evaluating whether the observed effect is unusually large relative to what we would expect from untreated firms.

In the next step, I use the distribution of placebo effects to construct a placebo-based p-value for each treated firm. After estimating the synthetic control for all donor firms, I compute the post-treatment gap for each donor firm. The placebo-based p-value is then defined as the proportion of placebo units whose estimated effects are at least as large in absolute value as the effect estimated for the treated firm. Formally, for a treated unit i, the p-value is

$$p_i = \frac{1}{J+1} \sum_{j=1}^{J+1} \mathbf{1}(|\widehat{\tau}_j| \geq |\widehat{\tau}_i|) \, ,$$

where $\widehat{\tau}_i$ denotes the estimated treatment effect for the treated firm, $\widehat{\tau}_j$ denotes the placebo effects, and J is the number of donor firms. A small value of p_i indicates that the observed impact for the treated unit is unusually large relative to those generated by untreated firms, providing evidence that the effect is unlikely to be driven by noise.

Figure 2.15 presents the results of the placebo test. Each gray background line shows the gap in the active ratio between a donor firm and its corresponding synthetic unit. The bold black line shows the same gap for the treated firm, with the treatment year indicated by the vertical line. The p-value for most firms is quite large, indicating no evidence of a causal effect. CIK 732712 has a p-value of 0.042, which could suggest that this firm's active ratio increases by about 0.4- value of the gap after the shock as shown in the graph- following its first breach. CIKs 732717 and 912595 also have relatively small p-values; however, their trends appear to begin before the treatment year.

Overall, the synthetic control results do not provide evidence of an increase in a firm's active ratio after its first breach. From a technical point of view, the synthetic control approach does not produce meaningful estimates in this setting because the treated and control firms exhibit substantial differences in their pre-treatment trajectories, and firms may adjust their ESOP ownership in anticipation of breaches in peer firms. This pre-treatment contamination and lack of path convergence violate the key assumptions required for synthetic control. In the next section, I conduct a similar analysis using propensity score matching.

Matching

In the sample, 69 ESOP firms have experienced a data breach incident at least once. For each treatment firm, I use propensity score matching to identify a control firm that has not experienced a data breach. The propensity score is calculated using the logit regression of data breach (an indicator that takes the value of one if a firm experiences an incident in a given year, and zero otherwise) on firm size, stock performance, stock return volatility and leverage. I also require that the both treated and matched firm come from the same fiscal year.

The control group is chosen based on the closest propensity score without replacement from the set of all firms that have never experienced a data breach between 2004 and 2021. The perfectly matched control group for my sample would be a firm from the same industry, with each control firm chosen only once. However, this will lead to a small set of matched firms. Instead, I do the matching without replacement for firm-year pairs with no industry criteria and will control for industry fixed effects later in regressions.

Based on these matching criteria, my model identifies matches for 53 data breach firms. Table 2.10 presents descriptive statistics for the matched sample. For most of the variables, I find no significant difference between firms with an incident and their matching non-breached counterparts, suggesting that the matching approach identifies control firms that are very similar to the treatment firms. To create the final difference in difference sample, I include all years of data for the treatment firms, as well as all years of data for any firm that is chosen as a control firm at least once in the matching process. To ensure a reasonable number of observations after the first breach, This results in a sample of 1,204 ESOP firm-year observations from 2004 to 2021.

Since the units do not get treated all at the same time, I need to define a group unit based on the treatment timing. In my work, one group consists of all firms that have been targeted by a data breach incident for the first time in the same year, with the year ranging from 2004 to 2021, or firms that have never been targeted. With this definition, the structure of this experiment is summarized as follows:

- **Treatment**: the first data breach incident in a firm
- **Treatment group**: All firms targeted by a data breach for the first time in the same year(2004-2021)
- **Control group**: The closest propensity score without replacement from the set of all firms without a data breach between 2004-2021.

I estimate the following difference-in-difference model:

$$Active\ ratio_{itg} = \alpha + \sum_{e=-15}^{-2} \delta_e \cdot D^e_{g,t} + \sum_{e=0}^{13} \beta_e \cdot D^e_{g,t} + \alpha_t + \alpha_i + \epsilon_{it} \tag{2.7}$$

where $Active\,ratio_{itg}$ equals the number of active employees in the ESOP plan relative to the total number of employees for firm i in fiscal year t which belongs to treatment unit g (i.e., all firms belonging to industries experiencing their first data breach in year g or never, each firm i is assigned to only one g).

$D^e_{i,t} = \mathbf{1}\{t - G_i = e\}$ is an indicator for unit i being e periods away from initial treatment at time t. α_t is fiscal year fixed effect, α_i represents the firm fixed effect, and $\epsilon_{i,t}$ is error term. Since each firm belongs to only one treatment group, I do not include any treatment group fixed effects here, as they would be redundant after including firm fixed effects. The parameter of interest in equation 2.8 is β, which shows the change in the active ratio following the first noticeable data breach in the industry relative to the change in the control group firms over the same time period. I expect to see a positive sign for the β in periods closer to the treatment time.

The key identification assumption in a difference-in-differences model is common counterfactual trends between the treatment and control groups; i.e. in the absence of the incident the treatment and control groups would have experienced the same changes in outcomes. Therefore, δe that is zero in the periods before the treatment indicates that the change in the active ratio for the treatment and control groups would follow the same trend when there is no incident. A significant β_e indicates a causal effect of the incident on the active ratio of firms.

The parameter of interest in Equation (2) is β, which represents the difference between the change in the treatment group and the change in the control group relative to the pre-treatment period. If my hypothesis holds, I should observe a positive value for β, indicating that firms experiencing a data breach will have a greater change in their active ratio compared to matched firms with no incident in the sample. Figure 2.16 illustrates the dynamics of the treatment effect on the active ratio following a firm's first breach in the sample. The figure shows a 3% increase within three years after the breach. I then examine changes in the components of the active ratio—specifically the ESOP employee population and the firm's total employee population—as well as related variables such as wage and the value of ESOP ownership.

According to Table 2.11, the increase in the active ratio is driven primarily by growth in the population of active ESOP owners rather than by a reduction in the total employee population. In Table 2.12, I examine changes in the components of executive compensation and find that firms reduce executives' bonuses by about 1% following the first breach.

All in all, the evidence in this section shows—using a matching framework—an increase in the active ratio after the first data breach among treated firms. In the next section, I conduct an after-shock analysis at the industry level.

2.4.3 Peer firms' behavior following industry breach incidents

"The best way to get management excited about a disaster plan is to burn down the building across the street." Dan Erwin, Security Officer, Dow Chemical Co

In this section, I examine how the active ratio and other employee-related behaviors change when a peer firm experiences a data breach. Kamiya et al., 2021 show that a data breach in one firm decreases the cumulative abnormal return of the portfolio of firms within the same four-digit SIC by –0.37% to –0.92%. Given these observations, firms have an incentive to respond to a data breach in their industry and take actions that reduce the likelihood of such an event when they feel the threat.

When a firm decides to respond to an external data breach incident, it should not matter whether the breach occurred in a public or a private firm. Therefore, all public and private firms with reported incidents are included in the sample. To ensure that the breach is noticeable, I focus on incidents where the number of affected individuals exceeds 500; in most cases, this is also the minimum threshold for mandatory disclosure under state laws. Table 2.13 shows the distribution of incidents across all public and private firms and the ESOP firms. The first row shows that, in the sample of all incidents, 75% of them have more than 1,780 impacts. The second row shows that, in the subsample of ESOP firms in this study, the first incident in 75% of industries has 2,950 impacts. The third row shows the impacts for the second incident.

Given this setup, I construct the sample of public ESOP firms used to study their reaction to an external incident by removing all firms that ever experienced a breach from the same sample used in the previous sections. Because I need observations both before and after the shock for each firm, I drop the data from 2004 and 2005. This gives me a sample of 2,610 firm-year that never experienced a data breach from 2006 to 2021.

The first industry incident I examine is the earliest data breach (with at least 500 affected individuals), shown in the second row of Table 2.13. Table 2.14 shows the detailed distribution of the sample's observations, along with their SIC industry and the number of impacts. In summary, the structure of this experiment is as follows:

- Experiment: How does the active ratio change after the first data breach in the firm's 4-digit SIC industry?
- **Treatment**: The earliest breach in each 4-digit SIC code- public or private firm- with at least 500 impacts.
- **Treatment group**: All firms that belong to industries experiencing their first noticeable data breach between 2006 and 2021 and that have never experienced a breach themselves.
- **Control group**: Observations from industries with no noticeable data breach incident.

To find the average treatment effect, I estimate the following staggered difference-in-differences regression used in the previous section:

$$Active\ ratio_{itg} = \alpha + \sum_{e=-15}^{-2} \delta_e \cdot D^e_{g,t} + \sum_{e=0}^{13} \beta_e \cdot D^e_{g,t} + \alpha_t + \alpha_i + \epsilon_{it} \tag{2.8}$$

where $Active\ ratio_{itg}$ equals the number of active employees in the ESOP plan relative to the total number of employees for firm i in fiscal year t which belongs to treatment unit g (i.e., all firms belonging to industries experiencing their first data breach in year g or never, each firm i is assigned to only one g).

$D_{i,t}^{e} = \mathbf{1}\{t - G_i = e\}$ is an indicator for unit i being e periods away from initial treatment at time t. α_t is fiscal year fixed effect, α_i represents the firm fixed effect, and $\epsilon_{i,t}$ is error term.The parameter of interest in equation 2.8 is β, which shows the change in the active ratio following the first noticeable data breach in the industry relative to the change in the control group firms over the same time period. I expect to see a positive sign for the β in periods closer to the treatment time.

The key identification assumption in a difference-in-differences model is common counterfactual trends between the treatment and control groups; i.e. in the absence of the incident the treatment and control groups would have experienced the same changes in outcomes. Therefore, δe that is zero in the periods before the treatment indicates that the change in the active ratio for the treatment and control groups would follow the same trend when there is no incident.

A significant β_e indicates a causal effect of the incident on the active ratio of firms. I use three estimators from Sun and Abraham, 2021, and Callaway and Sant'Anna, 2021 to estimate the coefficients in the staggered difference-in-differences setting. Sun and Abraham, 2021 adjust the problems that arise in the TWFE estimator, where the treatment effect can be contaminated by comparisons between treated units and already-treated units rather than treated units and valid controls. Their approach keeps the TWFE structure but corrects it by re-weighting and removing the bad comparisons. In contrast, Callaway and Sant'Anna, 2021 estimate cohort-specific treatment effects using only not-yet-treated or never-treated units as controls and then stack these effects to construct an average treatment effect.

Figure 2.17 shows the dynamics of the active ratio after the first industry breach. Figure 2.18 shows the dynamics of the active ratio after the first industry breach with at least 1,800 impacts, which corresponds to the first quartile of impacts across all firms. Firms appear to increase their active ratio by about 10% within three years after the first breach with at least 1,800 impacts in their four-digit industry. Figure 2.19 shows firms' reaction after the second breach in their industry (all breaches have more than 500 impacts), and Figure 2.20 shows firms' reaction after the largest data breach in their industry.

Based on the third and fourth figures(2.19 and 2.20), firms do not adjust their active ratio after either the second breach or the largest breach. Based on the first two figures(2.17

and 2.18), firms take action before the second breach—specifically when their industry experiences a breach with at least 1,800 impacts (25% of all impacts)—which is very similar to the event of the first incident. This is consistent with Table 2.14, which shows that the first breach with more than 500 impacts already has more than 1,800 impacts.

Table 2.15 shows the magnitude of the average treatment effect estimated by the CS estimator with never-treated controls. Since the two CS estimators produce very similar results, I only report this one. The table presents the ATE for four incidents: the first breach above the first quantile of impacts (1,800), the first breach overall, the second breach, and the breach with the greatest number of impacts in the industry. One point worth mentioning is that the ATE estimated after the first breach is about one third of the value for the 1,800-impact threshold and is very close to the TWFE estimates.

Based on Table 2.14, second row, most of the first incidents already have more than 1,800 impacts. Therefore, using either the first breach or the first breach with more than 1,800 impacts does not meaningfully change the treatment group for most industries. However, because the estimated effect is roughly three times as large, it indicates that this threshold does shift the treatment timing for some firms and their industries to years that widen the gap from the control group after treatment. This explains the slightly larger gap between the control and treatment groups in the year before treatment for the "after 1,800 impacts" specification relative to the specification based only on the first breach.

One other point worth mentioning is that, based on Table 2.14, a large share of the firms are in the finance industry and had their first incident in 2008. In this part, I exclude this group from the sample and investigate the firm's reaction after the first incident and after the first incident with at least 1,800 impacts. This also helps explain the difference in the treatment effects between the two incidents. Figures 2.22 and 2.21 show the ATT for all groups except the 2008 shock. Table 2.16 shows the dynamic average treatment effects for the group that received its shock in 2008. Based on this table, the ATT increases by about 6% to 10% within one to three years after the shock. Based on the figures, the ATT becomes close to zero after excluding the 2008 group; however, for the first-incident group with at least 1800 impacts, the ATT remains close to 10% even after excluding the 2008 shock group.

All in all, it seems that firms increase their active ratio by 6–10% after the first breach with impacts at least as large as the first quantile of the overall impact distribution. Two points are worth mentioning: first, this relies on the implicit assumption that firms have some sense of the distribution of breach impacts. This may be reasonable because the quantile threshold is derived from the distribution of incidents among all public and private firms, and far more breach information is available when private-firm incidents are included than when looking only at public firms. Also, firms know exactly how many customers they have and how much data they store. Given basic market shares, they can roughly infer how much data other firms hold as well. Since the number of impacted individuals in a breach depends mostly on data volume and customer count, it is not unreasonable that Firms have a sense of the distribution of impacts.

Second, the fact that firms that received their shock in 2008 show an increase in their active ratio may reflect the effect of the financial crisis rather than the breach itself, since both occurred at the same time. To examine this further, in the remainder of this section I analyze the reaction of other firm-level variables to the first breach with 1,800 impacts in their industry.

First, Table 2.17 shows the change in the active ratio across treatment groups. The treatment groups for which no effect is estimated are those dropped due to small sample sizes. As the table shows—and as discussed earlier—the 2008 group experiences an increase in its active ratio relative to the not-yet-treated groups. However, because this group has only one pre-treatment period, it is difficult to make a reliable judgment about the absence of pre-treatment trends for this group. The 2012 group also shows an increase, although it clearly exhibits a pre-treatment trend. The same situation applies to the 2019 group.

Next, I examine the changes in the components of the active ratio: the population of ESOP-owner employees and the total population of employees. Tables 2.20 and 2.21 show that G2008 experiences an increase in its population of active ESOP employees. G2012 shows a similar increase, and the same pattern appears for G2018. G2019 shows a decline in active ESOP employees, but the rate of decline is smaller than the decrease in its total employee population.

In Tables 2.18 and 2.19, I examine the change in ESOP value per participant and value of total assets. Most treatment groups experience a decrease in value, which could be due to the financial crisis for G2008 and the negative reaction to the breach news for the other groups.

In Table 2.22, I examine the change in wages for all employees. G2019 shows an increase in employees' wages after the breach, even though the value of ESOP ownership decreases. G2008 displays a similar pattern: an increase in wages alongside a decrease in the value of

ESOP shares.

In Tables 2.23, 2.24, 2.25, and 2.26, I examine changes in the components of executive compensation—equity, salary, bonus, and non-equity incentive compensation. G2016 increases equity compensation, and it does not exhibit a pre-treatment trend for a meaningful period. G2018 also shows an increase of about 9% in the three years following the shock. One notable point is that for G2008, there is no change in executives' equity compensation. According to Table 2.24, G2012, G2015, and G2016 all reduce executives' salaries. G2012 and G2015 show an increase in bonus compensation (Table 2.25). Finally, G2016 increases non-equity incentive compensation, while G2019 decreases it.

Overall, it appears that firms increase their active ratio after the first noticeable breach in their industry. They also adjust wages and various components of executives' compensation. The direction and magnitude of these adjustments are heterogeneous across industry groups. One important point is that the results above are estimated using the CS[12] estimator, which produces wider confidence intervals than a traditional TWFE setup in this sample. Therefore, the significance levels discussed should be interpreted as a worst-case scenario. I also find that firms do not wait for a second incident to adjust their active ratio. Nor do they necessarily respond solely to the size of the incident; rather, their response depends on a combination of the timing and the magnitude—particularly the first incident, which is as large as the 25th percentile of all incidents in the distribution.

2.5 Robustness check

In Table 2.27, I repeat the estimations from the first part and include lags of the active ratio as additional control variables. As the results show, the coefficient on the active ratio is not affected by these controls.

Next, I investigate the effects of ESOP ownership on the probability of a data breach from both an extensive and intensive margin perspective. The intensive margin refers to the effect of the value of ESOP assets per employee on the data breach incident, while the extensive margin refers to the effect of the active ratio (ESOP employees per employee) on the incident. This idea is driven by the following equality:

$$\frac{ESOP}{employees} = \frac{Active}{employees} \times \frac{ESOP}{Active}$$

[12]Callaway & Sant'Anna

where ESOP represents the total assets of a firm's ESOP plan and employees refer to the employee population.

In Table 2.28, I divide the data into three quantiles and estimate a quantile regression model to analyze the relationships across those quantiles. In Regression (1), I sort the data based on $(ESOP)/emp$ each year and then examine the effect of the active ratio on the probability of an incident. If the role of active participants is completely driven by the ESOP value, then the higher quantile of $log(ESOP)/emp$ should have a lower probability of a data breach relative to the lower quantile, and the active ratio should not have any effect. However, Regression (1) demonstrates the opposite. In other words, among firms with similar levels of $log(ESOP)/emp$, a higher active ratio can effectively protect the firm from a data breach.

In Regression (2), I sort the data based on the active ratio in each year and examine the effects of $log(ESOP)/emp$ on the probability of a breach. First, I observe a negative coefficient for the highest quantile of the active ratio relative to the lowest. Second, I do not see any effect of $log(ESOP)/emp$ on the likelihood of an incident. The results of columns 1 and 2 prove that the effects in the prior tables are not driven by size or ESOP value. In other words, two firms with similar levels of $log(ESOP)/emp$ can have different exposures to an incident based on their active ratio. This table provides insight into why the distribution of ESOP assets within the firm matters, showing that it is more than just an average value.

2.6 Conclusion

Employees can monitor their peers in ways that managers and executives cannot. This monitoring incentive can be strengthened by offering employees company stock, making them partial owners of the firm. This encourages employees to be more careful with both their own actions and those of their peers, which can be particularly helpful in situations where a small mistake by one individual could lead to significant losses for the entire firm, such as a data breach incident.

To examine whether mutual monitoring helps prevent data breaches, I focus on firms with employee stock ownership plans (ESOPs). I use the ratio of active ESOP participants to the firm's total employee population, referred to as the active ratio, as a measure of monitoring intensity. I find that a higher active ratio is associated with a lower likelihood of data breaches. However, this protective effect diminishes with firm size, and executive equity have an opposite effect.

Next, I study how a firm's active ratio changes following its first data breach and find a 3 percent increase. Using a staggered difference in differences structure, I also find that following the first breach with impacts at least as large as the twenty fifth percentile of all breaches, ESOP firms increase their active ratio by 6 to 10 percent.

In sum, these findings suggest that firms maintain a higher active ratio in response to data breach shocks within their environment, which is negatively correlated with the likelihood of future data breach threats.

Tables

Table 2.1: Timing of the first implementation of security breach laws in the U.S.
The table shows the year in which each state first enacted a security breach law. The group on the right represents the states where ESOP firms with an incident in my sample are located.
Source: perkinscoie.com

ESOP firms with a breach			
State	**Adoption year**	**State**	**Adoption year**
AR	2005	AL	2018
AZ	2006	AK	2009
CA	2003	CO	2006
CT	2006	DE	2005
FL	2014	DC	2007
GA	2005	HI	2007
IA	2008	ID	2006
IL	2006	IA	2008
IN	2006	KS	2007
KY	2014	LA	2006
MA	2007	ME	2006
MD	2008	MS	2011
MI	2007	MT	2006
MN	2006	NE	2006
MO	2009	NH	2007
NC	2005	NM	2017
NJ	2006	ND	2005
NV	2005	OK	2008
NY	2005	OR	2007
OH	2006	PA	2006
RI	2023	PR	2006
SD	2018	SC	2009
TN	2005	VT	2012
TX	2009	WV	2008
UT	2007	WY	2007
VA	2019		
WA	2005		
WI	2006		

2.7 Likelihood of experiencing a data breach incident

Table 2.2: Summary statistics.
The table shows summary statistics for a sample of 69 firms that experienced a data breach incident and 289 ESOP firms that never experience a data breach over the period 2004 to 2021. The appendix provides detailed descriptions of the construction of the variables. ***,** and * denote the Mann–Whitney U test for the difference in median at the 1%, 5%, and 10% levels, respectively.

Variables	***Breach***	***Non-breach***	***B-NB***
	Median	*Median*	
employee population(000)	37.20	2.88	34.32***
Total assets (billions)	32.78	3.63	29.14***
Stock return volatility	0.02	0.02	-0.0***
Tobin's Q	1.40	1.16	0.24***
Return on assets (ROA)	0.04	0.02	0.02***
Lagged Tobin's Q	1.39	1.17	0.22**
Stock performance	0.01	-0.03	0.04**
Industry Tobin's Q	1.36	1.25	0.11*
Leverage	0.86	0.76	0.11*
Industry Herfindahl index	0.03	0.03	0.0*
Wage ($000)	79.91	72.12	7.79
R&D / assets	0.00	0.00	0.0
CAPX / assets	0.02	0.02	0.01
Sales growth	1.03	1.04	-0.01
Asset intangibility	0.87	0.90	-0.03
Average total executive pay ($million)	5.56	1.43	4.12***
CFO salary ($million)	0.65	0.40	0.25***
CEO salary ($million)	1.15	0.75	0.4***
Salary / total executive pay	0.16	0.34	-0.19***
Stock awards / total executive pay	0.37	0.20	0.17***
Non-equity incentives / total pay	0.20	0.17	0.03***
Number of board committees	4.00	4.00	0.0*
Bonus / total executive pay	0.00	0.00	0.0*
Options / total executive pay	0.10	0.02	0.08
Institutional blockholder ownership (%)	0.18	0.18	0.0
ESOP participants (000)	15.18	1.64	13.54***
Total ESOP assets(billions)	2.77	0.11	2.66***
ESOP assets per employee ($000)	143.68	86.53	57.15***
Active ratio	0.51	0.72	-0.21**
ESOP ownership (% of firm equity)	0.10	0.08	0.02*

Table 2.3: Correlation of important variables used in the analysis. The active ratio is defined as the ratio of plan active participants to the total number of employees in the ESOP firm. Star shows the significance level of 5% and less. The appendix provides detailed descriptions of the construction of the variables.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)
(1) Active ratio	1.00												
(2) Log(ESOP/ participants)	-0.18*	1.00											
(3) Executives equity compensation	-0.28*	0.47*	1.00										
(4) log_wage	0.09*	0.25*	0.17*	1.00									
(5) log(total assets)	-0.09*	0.43*	0.60*	0.24*	1.00								
(6) q_prev_year	-0.29*	0.27*	0.29*	-0.07*	0.03	1.00							
(7) Stock performance	-0.03	0.03	-0.04*	-0.03	-0.06*	-0.01	1.00						
(8) Stock return volatility	0.08*	-0.25*	-0.17*	-0.06*	-0.25*	-0.18*	0.13*	1.00					
(9) Fiancial constraint	0.03	-0.19*	-0.34*	-0.15*	-0.59*	-0.05*	0.05*	0.18*	1.00				
(10) Contains risk	0.26*	-0.05*	-0.02	0.19*	0.26*	-0.22*	-0.06*	0.01	-0.14*	1.00			
(11) R&D/assets	-0.19*	0.25*	0.21*	0.07*	0.01	0.34*	0.04*	-0.02	0.08*	-0.19*	1.00		
(12) Number of board committees	0.00	0.23*	0.29*	0.11*	0.50*	0.01	-0.04*	-0.14*	-0.26*	0.29*	0.04*	1.00	
(13) Instituitonal block ownership	-0.08*	0.15*	0.18*	0.12*	0.01	0.05*	-0.04*	-0.03*	-0.08*	-0.01	0.02	0.07*	1.00

Table 2.4: Role of ESOP holdings in the likelihood of a data breach incident.
The table presents estimates of probit regressions in which the dependent variable is an indicator that takes the value one if a firm experiences a data breach in a given year, and zero otherwise. The sample consists of 885 firm-year observations that experienced a data breach in the following fiscal year (69 distinct ESOP firms) and the remaining firm-year observations that did not experience an incident during the period from 2004 to 2021. All explanatory variables are measured one year before the attack except for Tobin's q, which is measured two years before the attack. The appendix provides detailed descriptions of the construction of the variables. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level.***,** and * denote significance at the 1%, 5%, and 10% levels, respectively. Normalized values of the active ratio are used in this table.

	Dependent variable = Data breach incident (indicator)						
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Active_ratio	0.00	-0.21***	-0.19**	-0.14*	-0.10	-0.10	-0.08
	(0.06)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
log(ESOP $/ active participants)			0.23**	-0.03	-0.05	-0.05	0.01
			(0.09)	(0.11)	(0.10)	(0.10)	(0.10)
log(total assets)				0.57***	0.68***	0.64***	0.57***
				(0.10)	(0.12)	(0.15)	(0.15)
Q previous year					-0.10	-0.10	-0.11
					(0.08)	(0.09)	(0.08)
ROA					0.13	0.11	0.11
					(0.10)	(0.11)	(0.11)
Sales growth					0.02	0.03	0.03
					(0.06)	(0.06)	(0.06)
Stock performance					-0.00	-0.00	-0.00
					(0.07)	(0.07)	(0.07)
Leverage					0.06	0.05	0.06
					(0.06)	(0.06)	(0.06)
Financially constraint (indicator)					0.73*	0.68	0.48
					(0.43)	(0.43)	(0.47)
Stock return volatility					-0.17	-0.17	-0.14
					(0.18)	(0.18)	(0.17)
CAPX/assets					0.22	0.22	0.21
					(0.16)	(0.17)	(0.17)
Asset intangibility					0.27	0.25	0.24
					(0.21)	(0.21)	(0.22)
log(age)					-0.04	-0.02	-0.04
					(0.08)	(0.08)	(0.09)

R&D/assets					-0.11	-0.11	-0.13
					(0.11)	(0.11)	(0.12)
Zero R&D indicator					0.29	0.32	0.31
					(0.20)	(0.20)	(0.21)
Fortune 500 (indicator)						0.23	0.26
						(0.22)	(0.23)
Risk committee (indicator)						-0.29	-0.29
						(0.20)	(0.20)
Number of board committees						-0.02	0.02
						(0.07)	(0.07)
Institutional block ownership							-0.20**
							(0.08)
Constant	-1.43***	-1.50***	-1.86***	-2.15***	-2.20***	-2.33***	-2.30***
	(0.20)	(0.34)	(0.37)	(0.36)	(0.38)	(0.43)	(0.43)
Observations	3,140	2,584	2,583	2,583	2,513	2,513	2,513
Industry FE	N	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Pseudo R-squared	0.0512	0.204	0.215	0.273	0.302	0.306	0.311

Table 2.5: Role of employees and executives related variables in the likelihood of a data breach incident.
The table presents estimates of probit regressions in which the dependent variable is an indicator that takes the value one if a firm experiences a data breach in a given year, and zero otherwise. The sample consists of 885 firm-year observations that experienced a data breach in the following fiscal year (69 distinct ESOP firms) and the remaining firm-year observations that did not experience an incident during the period from 2004 to 2021. All explanatory variables are measured one year before the attack except for Tobin's q, which is measured two years before the attack. All control variables from Table 2.4 are included in estimation but not present in the table. The appendix provides detailed descriptions of the construction of the variables. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level.***,** and * denote significance at the 1%, 5%, and 10% levels, respectively. Normalized values of the active ratio are used in this table.

	Dependent variable = Data breach incident (indicator)						
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Active_ratio	-0.08	-0.08	-0.08	-0.15*	-0.16*	-0.16*	-0.14
	(0.08)	(0.08)	(0.08)	(0.09)	(0.09)	(0.09)	(0.09)
log(ESOP $/ active participants)	0.01	0.00	0.02	-0.06	-0.07	-0.07	0.08
	(0.10)	(0.10)	(0.10)	(0.11)	(0.12)	(0.12)	(0.12)
log(total assets)	0.57***	0.53***	0.51***	0.56***	0.59***	0.58***	
	(0.15)	(0.16)	(0.16)	(0.17)	(0.18)	(0.18)	
Executive equity		0.06	0.06	0.05	0.04	0.03	0.18*
		(0.10)	(0.10)	(0.10)	(0.11)	(0.11)	(0.10)
log(wage)			0.22	0.21	0.17	0.17	0.20
			(0.18)	(0.18)	(0.18)	(0.18)	(0.18)
Employees governance				0.16*	0.19*	0.19*	0.12
				(0.09)	(0.10)	(0.10)	(0.10)
Risk committee (indicator)	-0.29	-0.28	-0.30	-0.30	-0.33	-0.33	-0.23
	(0.20)	(0.20)	(0.20)	(0.20)	(0.21)	(0.20)	(0.20)
Number of board committees	0.02	0.02	0.01	0.01	0.03	0.04	0.09
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
Institutional block ownership	-0.20**	-0.20**	-0.22***	-0.24***	-0.21***	-0.21***	-0.27***
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)
log(State unemployment rate)					-0.06	-0.05	-0.06
					(0.12)	(0.12)	(0.12)
High tax						-0.06	-0.07
						(0.07)	(0.07)
Constant	-2.30***	-2.31***	-2.34***	-2.38***	-2.30***	-2.33***	-2.42***
	(0.43)	(0.44)	(0.43)	(0.43)	(0.44)	(0.45)	(0.48)

Observations	2,513	2,513	2,509	2,509	2,378	2,370	2,370
Industry FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y
Pseudo R-squared	0.311	0.312	0.314	0.317	0.303	0.304	0.291

Table 2.6: Role of executive equity compensation, firm size, and the active ratio in the likelihood of a data breach incident. The table presents estimates of probit regressions in which the dependent variable is an indicator that takes the value one if a firm experiences a data breach in a given year, and zero otherwise. The sample consists of 885 firm-year observations that experienced a data breach in the following fiscal year (69 distinct ESOP firms) and the remaining firm-year observations that did not experience an incident during the period from 2004 to 2021. All explanatory variables are measured one year before the attack except for Tobin's q, which is measured two years before the attack. All control variables from Table 2.4 are included in estimation but not present in the table. The appendix provides detailed descriptions of the construction of the variables. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level.***,** and * denote significance at the 1%, 5%, and 10% levels, respectively. Normalized values of the active ratio are used in this table.

	Dependent variable = Data breach incident (indicator)						
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**	**(7)**
Active ratio	-0.18**	-0.22***	-0.21**	-0.30***	-0.15*	-0.30***	-0.31***
	(0.08)	(0.09)	(0.08)	(0.10)	(0.09)	(0.10)	(0.10)
Log(ESOP $/ active participants)	-0.06	-0.06	0.09	-0.05	-0.08	-0.05	-0.07
	(0.12)	(0.12)	(0.11)	(0.12)	(0.12)	(0.12)	(0.12)
Active ratio × Log(ESOP $/ active participants)	0.05						
	(0.06)						
log(total assets)	0.58***	0.57***		0.62***	0.67***	0.61***	0.68***
	(0.18)	(0.19)		(0.19)	(0.17)	(0.19)	(0.18)
Executive equity	0.03	0.05	0.19*	-0.02	0.18	-0.01	0.15
	(0.11)	(0.11)	(0.10)	(0.11)	(0.12)	(0.11)	(0.12)
Employees governance	0.19*	0.17*	0.10	0.17	0.20**	0.16	0.17*
	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)	(0.10)
Risk committee (indicator)	-0.34*	-0.36*	-0.27	-0.44**	-0.42**	-0.43**	-0.51**
	(0.20)	(0.21)	(0.21)	(0.21)	(0.21)	(0.22)	(0.22)
Institutional block ownership	-0.22***	-0.23***	-0.29***	-0.24***	-0.25***	-0.24***	-0.28***
	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.08)	(0.09)
Active ratio × Executive equity		0.16**	0.17**			0.05	0.07
		(0.07)	(0.07)			(0.09)	(0.09)
Active ratio × log(total assets)				0.23***		0.20**	0.18*
				(0.08)		(0.10)	(0.10)
log(total assets) × Executive equity					-0.20*		-0.19**
					(0.11)		(0.10)
Constant	-2.31***	-2.22***	-2.30***	-2.24***	-2.30***	-2.22***	-2.19***
	(0.45)	(0.45)	(0.48)	(0.46)	(0.46)	(0.46)	(0.47)
Observations	2,370	2,370	2,370	2,370	2,370	2,370	2,370

Industry FE	Y	Y	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y	Y	Y
Control variables	Y	Y	Y	Y	Y	Y	Y
Pseudo R-squared	0.304	0.308	0.296	0.312	0.308	0.312	0.316

Table 2.7: heterogeneity across industries.
The table presents estimates of probit regressions in which the dependent variable is an indicator that takes the value one if a firm experiences a data breach in a given year, and zero otherwise. The sample consists of 885 firm-year observations that experienced a data breach in the following fiscal year (69 distinct ESOP firms) and the remaining firm-year observations that did not experience an incident during the period from 2004 to 2021. All explanatory variables are measured one year before the attack except for Tobin's q, which is measured two years before the attack. All control variables from Table 2.4 are included in estimation but not present in the table. The appendix provides detailed descriptions of the construction of the variables. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level.***,** and * denote significance at the 1%, 5%, and 10% levels, respectively. Normalized values of the active ratio are used in this table.

	Dependent variable = Data breach incident (indicator)		
VARIABLES	**(1)**	**(2)**	**(3)**
Active ratio × manufactuirng	-0.24	-0.24	-0.22
	(0.16)	(0.20)	(0.15)
Active ratio × Transporation & communication	0.22	0.93***	0.14
	(0.22)	(0.34)	(0.16)
Active ratio × Wholesale & retail	-2.09***	-2.19***	-1.82***
	(0.63)	(0.67)	(0.62)
Active ratio × Finance	-0.32***	-0.22**	-0.31***
	(0.10)	(0.09)	(0.10)
Active ratio × Service industry	0.37**	0.34	0.36**
	(0.19)	(0.21)	(0.18)
Active ratio × electric, gas, sanitory	-0.49***	-0.65**	-0.43***
	(0.14)	(0.31)	(0.14)
Active ratio× log(assets)	0.18***		0.15**
	(0.07)		(0.07)
Executive equity	0.05	0.07	-0.16
	(0.10)	(0.10)	(0.70)
Executive equity × Manufacturing			0.16
			(0.71)
Executive equity × Transportation			-0.38
			(0.75)
Executive equity × Wholesale & retail			-0.09
			(0.71)
Executive equity × Finance			0.38
			(0.71)
Executive equity × Service industry			0.22
			(0.70)
Executive equity × electric, gas, sanitary			-0.26
			(0.76)
log(total assets)	0.62***	0.58***	0.59***
	(0.13)	(0.13)	(0.13)

log(ESOP $/participants)	-0.12	-0.14	-0.15
	(0.10)	(0.10)	(0.10)
Active ratio × Executive equity × Manufacturing		0.17	
		(0.18)	
Active ratio×Executive equity* Transportation		-0.42	
		(0.29)	
Active ratio×Executive equity× Wholesale & retail		0.19*	
		(0.10)	
Active ratio×Executive equity× Finance		0.22**	
		(0.10)	
Active ratio×Executive equity× Service industry		0.10	
		(0.14)	
Active ratio×Executive equity× electric, gas, sanitary		0.45	
		(0.42)	
Constant	-2.84***	-2.79***	-2.54***
	(0.53)	(0.49)	(0.71)
Observations	2,879	2,879	2,879
Industry FE	Y	Y	Y
Year FE	Y	Y	Y
Control variables	Y	Y	Y
Pseudo R-squared	0.289	0.291	0.294

2.7.1 Effects of a data breach on ESOP participation

Table 2.8: Distribution of firms by year
How to read the table:
For example, in 2008, the dataset contains 217 observations in total. Among them, 179 correspond to firms that are never treated. In the same year, 41 observations belong to firms receiving their first breach shock, representing 3 unique firms.

Fiscal year	**Number of observations**	**Number of observations for each treatment group**	**Number of CIKs with a first breach in that year**	**Numebr of observations for firms with no breach (294 unique frims)**
2004	10			6
2005	10			6
2006	176	16	1	141
2007	223	32	2	184
2008	217	41	3	179
2009	225	31	2	182
2010	232	39	3	184
2011	221	27	2	169
2012	219	18	2	165
2013	218	37	3	164
2014	208	107	8	153
2015	207	64	5	154
2016	212	139	11	155
2017	229	72	5	170
2018	230	49	4	170
2019	235	48	4	177
2020	230	51	4	174
2021	219	52	5	165
Total	3,521	823		2,698

Table 2.9: A sample of different sets of predictors with Mean Squared Prediction Error (MSPE) and signal to noise ratio across 24 treated firms.
An "x" indicates that a variable belongs to a given set of predictors; each row represents one specific set.

L1_ active_ ratio	**L2_ active_ ratio**	**L3_ active_ ratio**	**L4_ active_ ratio**	**Q_ prev year**	**ESOP $ per emp**	**Inst block ownership**	**Industry Herfindahl Index**	**log (total assets)**	**ind tobin Q**	**RD/ assets**	**MSPE**	**Signal/ Noise**
x	x	x	x	x	x	x	x	x			0.06	2.96
				x	x	x	x	x			0.17	1.24
				x	x	x	x	x	x		0.17	1.15
				x	x	x	x	x	x	x	0.17	1.1
x											0.07	2.38
x	x										0.06	2.75
x	x	x									0.06	3.02
x	x	x	x								0.05	2.64
x	x	x	x					x			0.06	2.72
x	x			x	x	x	x	x	x		0.07	2.1

Table 2.10: Summary statistics of treatment and control observations. The table presents summary statistics for a sample of 53 firms that experienced their first breach between 2004 and 2021, along with the firms matched to them using propensity score matching. The appendix provides detailed descriptions of the construction of the variables. , and denote that median differences in firm and industry characteristics between attacked and nonattacked firms are significant at the 1%, 5%, and 10% levels, respectively.

Variable	*Breach (53 firms)*	*Non-breach*	*B−NB*
	Median	*Median*	
Stock performance	0.02	-0.02	0.04**
Industry Tobin's Q	1.52	1.24	0.27*
Tobin's Q	1.53	1.30	0.23*
Sales growth	1.04	1.03	0.01
Industry Herfindahl index	0.04	0.03	0.01
Return on assets (ROA)	0.05	0.03	0.02
CAPX / assets	0.03	0.02	0.01
R&D / assets	0.00	0.00	0.0
Total assets	2.85	2.61	0.24
Stock return volatility	0.01	0.02	-0.0
Asset intangibility	0.86	0.86	0.01
Leverage	0.81	0.85	-0.04

Table 2.11: Effect of data breach incidents on employee related variables.
This table presents results of Equation 2.8. The sample results from a propensity score matching process without replacement. The propensity score is calculated using the logit regression of data breach incident (an indicator that takes the value one if a firm experiences a data breach involving financial information loss, and zero otherwise) on firm size, stock performance, stock return volatility, and leverage. I require both treated and matching firms to be in the same same fiscal year. The sample consists of 1,204 firm-year observations (53 treated firms that experience a cyberattack). The appendix provides detailed descriptions of the construction of the variables. **Active ratio** equals the number of active ESOP participants relative to firm employee population. **log(esop/participants)** equals logarithm of ESOP assets per participant. In column (2), **log(emp)** equals logarithm of the firm employees population. In column (3), **log(active)** equals the logarithm of the ESOP active participants. In column (4), **log(wage)** equals the logarithm of wage per employee. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level., and denote significance at the 1%, 5%, and 10% levels, respectively.

time	**Active ratio**	**log_esop/ person**	**log_wage**	**log_employee population**	**log_active**
-16	-0.045	-2.232***	0.181***	0.336***	0.152
	(0.029)	(0.347)	(0.054)	(0.065)	(0.144)
-15	0.194***	-1.097***	-0.031	-0.065	0.178
	(0.032)	(0.200)	(0.045)	(0.053)	(0.125)
-14	0.033	0.129	0.026	-0.132	-0.217
	(0.052)	(0.469)	(0.044)	(0.126)	(0.174)
-13	-0.004	-0.063	0.033	-0.152**	-0.213
	(0.049)	(0.287)	(0.045)	(0.064)	(0.143)
-12	0.007	0.297	0.076	-0.132*	-0.182
	(0.062)	(0.188)	(0.063)	(0.067)	(0.189)
-11	0.008	0.321*	0.108**	-0.145*	-0.210
	(0.063)	(0.170)	(0.054)	(0.073)	(0.168)
-10	0.058	0.164	-0.026	-0.121	-0.096
	(0.044)	(0.148)	(0.110)	(0.075)	(0.109)
-9	0.075*	0.144	0.220***	-0.136**	-0.049
	(0.040)	(0.128)	(0.076)	(0.060)	(0.105)
-8	0.073*	0.019	0.164***	-0.126**	-0.059
	(0.038)	(0.113)	(0.062)	(0.055)	(0.104)
-7	0.060	0.114	-0.123	-0.116**	-0.085
	(0.037)	(0.095)	(0.172)	(0.051)	(0.103)
-6	0.046	0.090	0.008	-0.064	-0.063
	(0.034)	(0.092)	(0.051)	(0.042)	(0.092)
-5	0.049	0.069	0.066*	-0.092**	-0.070
	(0.034)	(0.098)	(0.036)	(0.038)	(0.088)
-4	0.029	0.026	0.037	-0.019	-0.036
	(0.029)	(0.066)	(0.027)	(0.036)	(0.076)
-3	0.016	0.103	0.038	-0.022	-0.046
	(0.021)	(0.078)	(0.025)	(0.027)	(0.069)
-2	0.017	-0.031	0.038*	-0.014	0.018
	(0.014)	(0.039)	(0.020)	(0.019)	(0.032)
0	0.031***	-0.060*	0.043	-0.005	0.048***
Treatment time	(0.008)	(0.033)	(0.027)	(0.014)	(0.017)
1	0.031***	-0.034	0.047*	-0.020	0.031
	(0.011)	(0.035)	(0.026)	(0.023)	(0.027)
2	0.030**	-0.080	0.010	0.005	0.066
	(0.014)	(0.052)	(0.029)	(0.029)	(0.041)
3	0.034	-0.034	0.059	-0.019	0.019
	(0.022)	(0.080)	(0.070)	(0.045)	(0.074)
4	0.012	-0.068	0.053	0.001	0.001
	(0.022)	(0.090)	(0.074)	(0.052)	(0.079)
5	0.002	-0.193*	0.037	0.009	-0.001
	(0.025)	(0.114)	(0.080)	(0.053)	(0.088)

6	-0.013	-0.100	0.003	-0.011	-0.063
	(0.032)	(0.126)	(0.082)	(0.046)	(0.098)
7	-0.019	0.151	-0.011	-0.015	-0.076
	(0.033)	(0.137)	(0.095)	(0.066)	(0.132)
8	0.034	0.218	-0.134***	-0.061	0.061
	(0.035)	(0.138)	(0.049)	(0.052)	(0.086)
9	0.090*	0.319**	-0.148**	-0.081	0.154
	(0.050)	(0.152)	(0.074)	(0.058)	(0.122)
10	0.053	0.295**	-0.105	-0.124*	0.031
	(0.034)	(0.125)	(0.072)	(0.072)	(0.096)
11	-0.025	0.535***	-0.091	-0.140	-0.132
	(0.040)	(0.152)	(0.107)	(0.090)	(0.106)
12	0.162**	0.128	-0.127	-0.289***	0.183
	(0.073)	(0.139)	(0.096)	(0.088)	(0.181)
13	0.094	0.208	-0.052	-0.307***	0.027
	(0.065)	(0.188)	(0.132)	(0.098)	(0.169)
14	0.039	0.229	-0.095	0.043	0.333*
	(0.050)	(0.259)	(0.266)	(0.165)	(0.171)

Table 2.12: Effect of data breach on executive compensation.
This table presents results of Equation 2.8. The sample results from a propensity score matching process without replacement. The propensity score is calculated using the logit regression of data breach (an indicator that takes the value one if a firm experiences a data breach involving financial information loss, and zero otherwise) on firm size, stock performance, stock return volatility, and leverage. I require both treated and matching firms to be in the same same fiscal year.The sample consists of 1,204 firm-year observations (53 treated firms that experience a cyberattack).**Executive equity** equals the mean of the equity portion (stock + options) relative to total compensation across all executives. **Executive salary** equals the mean of salary relative to total compensation across all executives. **Executive bonus** equals the mean of bonus relative to total compensation across all executives. **Executive non-equity incentive** equals the mean of non-equity incentives relative to total compensation across all executives. **CEO equity** equals the equity portion (stock + options) relative to total compensation for CEO. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level., and denote significance at the 1%, 5%, and 10% levels, respectively.

	Equity	**salary**	**Bonus**	**non-equity-incentive**
-16	-0.372***	0.354***	0.260***	-0.301***
	(0.029)	(0.027)	(0.031)	(0.069)
-15	-0.028	0.091***	0.150***	-0.213***
	(0.029)	(0.016)	(0.018)	(0.041)
-14	-0.115***	0.036	0.156***	-0.145***
	(0.041)	(0.036)	(0.020)	(0.044)
-13	-0.062	0.007	0.045	-0.036
	(0.051)	(0.029)	(0.038)	(0.055)
-12	-0.104**	0.028	0.049*	-0.020
	(0.046)	(0.048)	(0.028)	(0.042)
-11	-0.110*	0.077	-0.014	0.055
	(0.064)	(0.067)	(0.016)	(0.057)
-10	-0.042	0.010	0.022	0.019
	(0.051)	(0.044)	(0.020)	(0.041)
-9	-0.032	0.007	-0.004	0.017
	(0.043)	(0.033)	(0.013)	(0.033)
-8	-0.050	0.015	-0.013	0.033
	(0.034)	(0.025)	(0.012)	(0.028)
-7	-0.018	-0.021	-0.014	0.020
	(0.037)	(0.027)	(0.010)	(0.029)
-6	-0.044	0.017	0.007	0.003
	(0.036)	(0.024)	(0.020)	(0.032)
-5	-0.039	0.002	-0.000	0.026
	(0.028)	(0.016)	(0.010)	(0.022)
-4	-0.038*	0.006	-0.004	0.030
	(0.021)	(0.012)	(0.008)	(0.022)
-3	-0.038*	0.013	-0.006	0.014
	(0.020)	(0.009)	(0.007)	(0.020)
-2	-0.030*	0.006	-0.003	0.010
	(0.017)	(0.009)	(0.005)	(0.017)
0	-0.022	0.001	-0.012**	0.021*
Treatment year	(0.016)	(0.007)	(0.006)	(0.013)
1	0.020	-0.002	-0.014**	-0.013
	(0.014)	(0.008)	(0.006)	(0.016)
2	0.017	0.004	-0.009	-0.018
	(0.019)	(0.011)	(0.008)	(0.020)
3	0.013	0.001	-0.008	-0.010
	(0.025)	(0.012)	(0.011)	(0.021)
4	-0.002	0.004	-0.028*	0.006
	(0.022)	(0.014)	(0.014)	(0.018)
5	-0.021	0.010	-0.023**	0.030
	(0.031)	(0.016)	(0.011)	(0.023)

6	0.006	0.015	-0.037***	-0.005
	(0.035)	(0.018)	(0.012)	(0.031)
7	-0.033	0.026	-0.039***	0.030
	(0.029)	(0.020)	(0.013)	(0.024)
8	-0.032	0.032	-0.052***	0.024
	(0.046)	(0.024)	(0.014)	(0.050)
9	-0.010	0.018	-0.054***	0.025
	(0.044)	(0.028)	(0.017)	(0.042)
10	0.011	0.017	-0.071***	0.011
	(0.039)	(0.030)	(0.019)	(0.039)
11	-0.012	0.018	-0.073***	0.019
	(0.040)	(0.032)	(0.020)	(0.044)
12	0.027	0.015	-0.103***	0.040
	(0.056)	(0.029)	(0.024)	(0.072)
13	-0.001	0.013	-0.058*	0.007
	(0.087)	(0.029)	(0.032)	(0.134)
14	-0.021	-0.029	-0.092	0.109
	(0.139)	(0.027)	(0.060)	(0.180)

2.7.2 Data breach incidents as industry-wide shocks

Table 2.13: Distribution of data breach incidents based on the number of impacts
The first row shows the distribution for all public and private incidents in the sample. The second row shows the distribution of the first incident in the group of ESOP firms in this study. The third row shows the distribution of the second incident in the group of ESOP firms in this work.

	mean	min	25%	50%	75%	max
Impacts, all incidents, private and public firms	3.75e+06	500.0	1780	5448	30758	2.00e+09
Impacts, first incident, ESOP sample	1.8e+05	504	2950	7000	7000	4.90e+06
Impacts, second incident, ESOP sample	1.27e+05	511	3950	36311	43750	1.90e+06

Table 2.14: Distribution of the sample's observations and their industries based on the shock time. The shock group equals the year an industry experiences its first breach with more than 500 impacts. Impacts is the number of impacts of that incident. Observations show the number of observations in the sample for each SIC that experienced its breach incident in the shock-group year.

Shock: the first shock greater than 25%(1800)					**Shock: the first breach**			
sic	**maximpacted**	**shock**	**N**		**sic**	**maximpacted**	**shock**	**N**
3561	2000	2006	24		3561	2000	2006	24
2834	34000		40		2834	17000		40
6211	32204	2007	14		5812	1006		16
6331	3500		64		6331	3500	2007	64
7374	2300000		15		7370	1468		17
3841	6900	2008	41		7374	2300000		15
6020	7000		607		3841	6900	2008	41
6141	9475		6		6020	7000		607
6311	1200000	2010	19		2844	840		16
8062	15500		6		6141	9475	2010	6
5912	2900		5		6311	1200000		19
6199	3790		5		8062	15500		6
6411	3994	2011	6		4210	1089		4
7370	1900000		17		5912	2900		5
7372	32008		28		6199	1116		5
7373	4900000		17		6411	1631	2011	6
4931	2500000		66		7372	8795		28
7311	56000	2012	16		7373	4900000		17
8711	17933		6		8731	600		3
4813	150000	2013	4		4931	2500000		66
3562	4983		5		7311	56000	2012	16
4512	7000		6		7389	600		11
5110	1160000	2014	5		8711	17933		6
7389	9800		11		4813	150000	2013	4
8731	9386		3		4924	876		19
1389	10844		4		3562	4983		5

4911	5412	2015	84	4512	7000		6
5812	5278		16	5110	1160000	2014	5
2842	4171		31	5411	504		3
2844	41485		16	1389	10844	2015	4
3510	11363		16	4911	5412		84
3674	7395	2016	22	2836	1639		10
3721	96000		16	2842	4171		31
5411	13283		3	3510	11363		16
2631	7569		20	3674	1006	2016	22
2836	2657	2017	10	3721	35681		16
3663	2521		9	6282	1222		16
3621	2423		34	2631	7569		20
3711	19320		29	3663	2521		9
3826	2900	2018	3	3679	955	2017	23
4210	2395		4	3711	1118		29
6282	3070		16	5122	737		24
6035	8450	2019	66	2911	1287		34
1311	45529		38	3621	2423	2018	34
1600	23751		21	3826	2900		3
2511	2062		5	3411	1353		16
2860	12233		16	3728	1496	2019	31
2911	5743		34	6035	2950		66
3312	4322	2020	37	1311	11014		38
3670	5364		9	1600	23751		21
4011	5983		38	2511	2062		5
4924	54534		19	2860	12233		16
5080	15390		15	3312	4322		37
9997	26356		16	3670	5364	2020	9
2810	8841	2021	27	4011	5983		38
7359	6686		11	5080	15390		15
				6036	517		34
				9997	10275		16
				2810	8841		27
				2851	1350	2021	32
				7359	6686		11

Table 2.15: Dynamic average treatment effects of active ratio; CS estimator with never-treated controls. The first column shows the results after the first breach with 1,800 impacts. The second column shows the results after the first breach. The third column shows the results after the second breach. The fourth column shows the results after the incident with the maximum number of impacts in the four-digit SIC. * shows confidence intervals that do not include zero.

	The first breach with 1800 incidents	**The first breach**	**The second**	**Maximum impacts**
		0.014 (0.031)	-0.012 (0.021)	0.002 (0.022)
ATT(-13)	-0.059	-0.073	0.017	-0.041

	(0.040)	(0.037)	(0.022)	(0.029)
ATT(-12)	-0.003	0.016	0.008	-0.011
	(0.026)	(0.018)	(0.011)	(0.019)
ATT(-11)	0.010	0.008	-0.017	0.002
	(0.013)	(0.011)	(0.043)	(0.011)
ATT(-10)	0.002	0.008	0.006	0.013
	(0.019)	(0.010)	(0.027)	(0.011)
ATT(-9)	-0.016	-0.013	-0.005	0.003
	(0.027)	(0.026)	(0.012)	(0.013)
ATT(-8)	0.007	-0.001	0.020	0.004
	(0.022)	(0.019)	(0.010)	(0.012)
ATT(-7)	0.005	-0.007	0.008	0.009
	(0.018)	(0.009)	(0.023)	(0.016)
ATT(-6)	0.005	0.009	-0.025	-0.009
	(0.015)	(0.012)	(0.037)	(0.013)
ATT(-5)	-0.021	-0.023	0.014	0.002
	(0.015)	(0.014)	(0.017)	(0.015)
ATT(-4)	0.021	-0.019	0.023	-0.008
	(0.023)	(0.025)	(0.012)	(0.020)
ATT(-3)	0.041	0.011	0.020	0.006
	(0.023)	(0.019)	(0.013)	(0.022)
ATT(-2)	0.002	0.011	-0.021	-0.014
	(0.013)	(0.013)	(0.013)	(0.010)
ATT(-1)	-0.023	-0.017	0.002	0.012
	(0.012)	(0.010)	(0.016)	(0.017)
ATT(0)	0.011	-0.006	0.001	0.013
	(0.011)	(0.013)	(0.016)	(0.014)
ATT(1)	0.061 **	0.020	0.020	0.002
	(0.021)	(0.019)	(0.021)	(0.021)
ATT(2)	0.090 **	0.034	-0.008	0.022
	(0.031)	(0.026)	(0.034)	(0.027)
ATT(3)	0.112 ***	0.057	-0.033	0.020
	(0.029)	(0.026)	(0.045)	(0.031)
ATT(4)	0.089 **	0.030	-0.078	0.085
	(0.033)	(0.021)	(0.052)	(0.067)
ATT(5)	0.090 **	0.036	-0.099	-0.033

	(0.031)	(0.021)	(0.057)	(0.048)
ATT(6)	0.056	-0.016	-0.094	
	(0.058)	(0.028)	(0.063)	
ATT(7)	0.018	-0.018	-0.098	
	(0.063)	(0.059)	(0.059)	
ATT(8)	0.002	-0.040	-0.062	
	(0.060)	(0.057)	(0.069)	
ATT(9)	0.017	-0.039	-0.082	
	(0.068)	(0.061)	(0.076)	
ATT(10)	0.034	-0.015	0.046	
	(0.075)	(0.071)	(0.097)	
ATT(11)	0.019	-0.033	0.068	
	(0.072)	(0.071)	(0.115)	
ATT(12)	0.053	0.001	0.007	
	(0.081)	(0.091)	(0.094)	
ATT(13)	0.038	-0.017	0.098	
	(0.079)	(0.082)	(0.234)	

Table 2.16: Dynamics of the ATT for the group that received their first breach in 2008; this group is the same as the group with impacts greater than 1,800, since their incident has 7,000 impacts.

	The first breach with 1800 incidents	**The first breach**
Dynamic ATT for firms that received their shock in 2008		
ATT(2008,2007)	-0.000	0.001
	(0.015)	(0.017)
ATT(2008,2008)	0.030	0.029
	(0.015)	(0.015)
ATT(2008,2009)	0.066 **	0.065
	(0.023)	(0.024)
ATT(2008,2010)	0.066	0.064
	(0.031)	(0.032)
ATT(2008,2011)	0.104***	0.102
	(0.024)	(0.025)
ATT(2008,2012)	0.069	0.067
	(0.026)	(0.026)
ATT(2008,2013)	0.073***	0.070
	(0.023)	(0.024)
ATT(2008,2014)	0.011	0.008
	(0.024)	(0.026)
ATT(2008,2015)	0.011	0.011
	(0.068)	(0.071)
ATT(2008,2016)	-0.025	-0.022
	(0.066)	(0.066)
ATT(2008,2017)	-0.009	-0.025
	(0.067)	(0.071)
ATT(2008,2018)	0.014	0.004
	(0.076)	(0.080)
ATT(2008,2019)	0.015	-0.003
	(0.069)	(0.078)
ATT(2008,2020)	0.053	0.028
	(0.079)	(0.086)
ATT(2008,2021)	0.038	-0.017
	(0.079)	(0.085)

Table 2.17: Estimated ATT effects of active ratio across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.000	0.066***	0.009	-0.025	0.008	-0.021**	-0.041
	(0.018)	(0.016)	(0.026)	(0.065)	(0.030)	(0.009)	(0.036)
2008	0.030*	0.027**	-0.019	0.002	-0.033	0.053***	-0.000
	(0.015)	(0.013)	(0.013)	(0.017)	(0.046)	(0.010)	(0.021)
2009	0.066***	0.019***	-0.019	-0.011	0.033**	0.001	0.008
	(0.023)	(0.005)	(0.022)	(0.014)	(0.014)	(0.006)	(0.007)
2010	0.066**	0.003	0.014	0.007	0.028	-0.084***	0.012
	(0.034)	(0.005)	(0.021)	(0.008)	(0.025)	(0.008)	(0.020)
2011	0.104***	-0.137***	-0.013	-0.045	0.006	0.080***	0.001
	(0.025)	(0.006)	(0.017)	(0.040)	(0.010)	(0.006)	(0.019)
2012	0.069***	0.026***	-0.006	-0.010	0.070	-0.056***	-0.010
	(0.026)	(0.008)	(0.010)	(0.012)	(0.046)	(0.017)	(0.059)
2013	0.073***	0.004	-0.011	0.080	-0.049***	0.067***	0.009
	(0.023)	(0.028)	(0.027)	(0.073)	(0.016)	(0.011)	(0.033)
2014	0.011	0.068***	-0.006	-0.020	0.013	0.019***	-0.022
	(0.025)	(0.026)	(0.013)	(0.023)	(0.021)	(0.004)	(0.016)
2015	0.011	0.060**	-0.003	-0.018	0.051	-0.052***	-0.017
	(0.065)	(0.028)	(0.008)	(0.012)	(0.050)	(0.013)	(0.025)
2016	-0.025	0.022	0.148	-0.024	-0.008	0.005	0.060
	(0.063)	(0.038)	(0.109)	(0.043)	(0.033)	(0.013)	(0.057)
2017	-0.009	0.010	0.163	0.023	-0.068	0.124***	0.021
	(0.068)	(0.045)	(0.101)	(0.070)	(0.067)	(0.018)	(0.017)
2018	0.014	-0.024	0.193	0.013	0.001	0.008	-0.014
	(0.074)	(0.051)	(0.125)	(0.072)	(0.011)	(0.008)	(0.009)
2019	0.015	-0.010	0.217	0.062	0.092	-0.033***	-0.025
	(0.071)	(0.049)	(0.133)	(0.065)	(0.057)	(0.005)	(0.016)
2020	0.053	0.017	0.172	0.052	0.126	0.012**	0.007
	(0.080)	(0.091)	(0.109)	(0.067)	(0.089)	(0.005)	(0.024)
2021	0.038	-0.012	0.240	0.063	0.123	0.124***	0.019
	(0.081)	(0.089)	(0.204)	(0.065)	(0.115)	(0.012)	(0.034)

Table 2.18: Estimated ATT effects of ESOP dollar value per participant across treatment groups, using the not-yet-treated estimator.
Each column represents a treatment group and its corresponding treatment year, while the “time” column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.364***	-0.046	0.168	0.157	0.180*	-0.323***	0.293**
	(0.122)	(0.105)	(0.102)	(0.190)	(0.099)	(0.039)	(0.122)
2008	0.361**	0.112	0.128	0.072	0.031	-1.437***	0.238
	(0.148)	(0.287)	(0.261)	(0.216)	(0.267)	(0.075)	(0.305)
2009	-0.255	-0.119	-0.148	-0.131	-0.109	-0.391***	-0.330
	(0.201)	(0.239)	(0.246)	(0.179)	(0.093)	(0.052)	(0.257)
2010	-0.186	0.129	0.186	0.109	-0.153	-0.402***	0.125
	(0.190)	(0.177)	(0.173)	(0.145)	(0.131)	(0.050)	(0.188)
2011	-0.332*	0.191***	0.076	-0.004	-0.098	-0.514***	-0.062
	(0.197)	(0.027)	(0.057)	(0.063)	(0.094)	(0.039)	(0.039)
2012	-0.309	0.010	0.025	-0.000	-0.158	0.666***	-0.121**
	(0.189)	(0.021)	(0.040)	(0.082)	(0.199)	(0.050)	(0.055)
2013	-0.292*	0.100**	0.135	-0.216***	-0.025	0.855***	0.014
	(0.171)	(0.044)	(0.095)	(0.083)	(0.167)	(0.041)	(0.043)
2014	-0.315*	-0.078*	-0.042	0.034	-0.120	0.238***	0.021
	(0.190)	(0.044)	(0.063)	(0.063)	(0.104)	(0.038)	(0.061)
2015	-0.156	-0.031	0.022	0.082	0.116	-0.049	-0.070
	(0.195)	(0.059)	(0.052)	(0.054)	(0.101)	(0.051)	(0.062)
2016	0.025	-0.185	-0.076	-0.104	-0.065	0.226***	-0.202
	(0.202)	(0.236)	(0.228)	(0.173)	(0.094)	(0.034)	(0.244)
2017	-0.105	-0.068	0.135	-0.002	0.083	-0.078***	-0.071
	(0.212)	(0.237)	(0.327)	(0.349)	(0.097)	(0.028)	(0.058)
2018	-0.205	0.315***	0.512	-0.137	-0.023	-0.003	0.200
	(0.225)	(0.093)	(0.396)	(0.144)	(0.087)	(0.024)	(0.255)
2019	-0.318	0.179	0.322	-0.108	-0.289**	0.005	-0.246
	(0.227)	(0.275)	(0.445)	(0.285)	(0.117)	(0.017)	(0.268)
2020	-0.708***	-0.142	0.116	-0.147	-0.480**	-0.299***	0.042
	(0.220)	(0.150)	(0.657)	(0.313)	(0.245)	(0.079)	(0.049)
2021	-0.535**	-0.171	-0.025	-0.214	-0.282***	-0.068***	0.206**
	(0.234)	(0.135)	(0.520)	(0.276)	(0.099)	(0.025)	(0.102)

Table 2.19: Estimated ATT effects of log(ESOP assets) across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	**G_2008**	**G_2012**	**G_2015**	**G_2016**	**G_2018**	**G_2019**	**G_2020**
2007	-0.366***	0.110	0.240**	0.267	0.225**	-0.393***	0.195*
	(0.136)	(0.090)	(0.107)	(0.492)	(0.100)	(0.049)	(0.118)
2008	0.442***	0.165	0.175	0.170	-0.119	-1.340***	0.229
	(0.127)	(0.265)	(0.244)	(0.182)	(0.135)	(0.067)	(0.295)
2009	-0.065	-0.107	-0.153	-0.141	-0.111	-0.317***	-0.291
	(0.206)	(0.220)	(0.222)	(0.172)	(0.112)	(0.051)	(0.295)
2010	0.029	0.160	0.165	0.137	-0.092	-0.525***	0.129
	(0.169)	(0.178)	(0.174)	(0.141)	(0.126)	(0.042)	(0.217)
2011	-0.061	-0.040*	0.002	-0.017	-0.062	-0.445***	-0.029
	(0.193)	(0.023)	(0.033)	(0.036)	(0.076)	(0.046)	(0.045)
2012	-0.051	0.085***	-0.019	-0.037	-0.170	0.536***	-0.079*
	(0.195)	(0.019)	(0.024)	(0.084)	(0.143)	(0.043)	(0.042)
2013	-0.023	0.108***	-0.088**	0.060	-0.080	0.892***	-0.026
	(0.176)	(0.027)	(0.035)	(0.120)	(0.155)	(0.030)	(0.040)
2014	-0.065	0.105***	-0.032	0.033	-0.040	0.207***	-0.055
	(0.184)	(0.037)	(0.025)	(0.028)	(0.098)	(0.036)	(0.061)
2015	-0.099	0.128**	-0.013	0.015	0.100**	-0.155***	-0.114***
	(0.175)	(0.051)	(0.019)	(0.026)	(0.048)	(0.019)	(0.044)
2016	0.080	-0.040	-0.165	-0.194	-0.053	0.277***	-0.162
	(0.189)	(0.176)	(0.175)	(0.178)	(0.044)	(0.024)	(0.249)
2017	-0.036	0.029	-0.137	0.055	0.081**	-0.022	-0.079
	(0.189)	(0.167)	(0.162)	(0.163)	(0.038)	(0.029)	(0.056)
2018	-0.136	0.254***	0.062	-0.058	0.047	0.003	0.144
	(0.215)	(0.072)	(0.043)	(0.205)	(0.100)	(0.017)	(0.284)
2019	-0.217	0.149	-0.071	0.150	0.014	-0.068***	-0.310
	(0.217)	(0.220)	(0.208)	(0.241)	(0.106)	(0.011)	(0.296)
2020	-0.585***	-0.235	-0.349	0.008	-0.011	-0.351***	-0.049
	(0.206)	(0.178)	(0.384)	(0.326)	(0.292)	(0.075)	(0.047)
2021	-0.481**	-0.245	-0.401	-0.021	0.155	-0.161***	0.005
	(0.208)	(0.160)	(0.374)	(0.307)	(0.160)	(0.048)	(0.054)

Table 2.20: Estimated ATT effects of log(employee population) across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.016	0.027	0.017	0.029	0.018	-0.051***	0.042
	(0.027)	(0.017)	(0.017)	(0.026)	(0.027)	(0.014)	(0.083)
2008	0.014	-0.010	0.094**	0.080**	-0.121	0.000	0.033
	(0.032)	(0.039)	(0.045)	(0.040)	(0.189)	(0.030)	(0.062)
2009	0.080*	-0.028**	0.095***	0.027	-0.056	0.084***	0.014
	(0.041)	(0.014)	(0.017)	(0.038)	(0.065)	(0.014)	(0.021)
2010	0.086**	0.021**	0.012	0.018	-0.001	-0.022	-0.009
	(0.042)	(0.010)	(0.027)	(0.031)	(0.033)	(0.014)	(0.024)
2011	0.078	0.037**	0.057	0.112	0.011	-0.054***	0.009
	(0.048)	(0.014)	(0.040)	(0.101)	(0.023)	(0.014)	(0.031)
2012	0.092*	0.001	0.052	-0.006	-0.137**	-0.054**	0.061
	(0.053)	(0.012)	(0.032)	(0.020)	(0.054)	(0.027)	(0.059)
2013	0.101	-0.000	-0.027	0.040	0.009	-0.050***	-0.019
	(0.061)	(0.041)	(0.041)	(0.036)	(0.025)	(0.013)	(0.042)
2014	0.164**	0.009	-0.042**	0.038	0.053***	-0.076***	-0.017
	(0.068)	(0.040)	(0.017)	(0.030)	(0.018)	(0.008)	(0.024)
2015	0.107	0.021	-0.022	0.006	-0.027	-0.006	0.002
	(0.077)	(0.044)	(0.023)	(0.011)	(0.096)	(0.020)	(0.014)
2016	0.147*	0.073	-0.894***	-0.018	0.007	0.043***	-0.051
	(0.087)	(0.061)	(0.063)	(0.019)	(0.043)	(0.014)	(0.031)
2017	0.143*	0.049	-1.109***	-0.020	0.089	-0.118***	-0.019
	(0.085)	(0.066)	(0.092)	(0.037)	(0.121)	(0.032)	(0.027)
2018	0.113	-0.029	-1.371***	0.020	0.070***	0.014	0.022**
	(0.087)	(0.074)	(0.090)	(0.056)	(0.014)	(0.014)	(0.011)
2019	0.128	-0.033	-1.365***	0.020	0.143***	-0.032***	0.030
	(0.090)	(0.075)	(0.104)	(0.057)	(0.020)	(0.010)	(0.042)
2020	0.142	-0.093	-1.282***	-0.016	0.243	-0.099***	-0.074*
	(0.113)	(0.127)	(0.139)	(0.058)	(0.207)	(0.012)	(0.043)
2021	0.068	0.003	-1.365***	-0.008	0.167	-0.314***	-0.137*
	(0.117)	(0.122)	(0.170)	(0.070)	(0.307)	(0.020)	(0.072)

Table 2.21: Estimated ATT effects of log(active ESOP employees) across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.002	0.156***	0.073	0.110	0.045	-0.070***	-0.099
	(0.038)	(0.034)	(0.065)	(0.172)	(0.046)	(0.024)	(0.061)
2008	0.081**	0.053	0.048	0.098*	-0.150	0.097***	-0.009
	(0.034)	(0.042)	(0.043)	(0.053)	(0.238)	(0.035)	(0.056)
2009	0.191***	0.012	-0.005	-0.010	-0.002	0.075***	0.039
	(0.046)	(0.019)	(0.104)	(0.081)	(0.069)	(0.018)	(0.029)
2010	0.214***	0.031***	-0.021	0.028*	0.061*	-0.123***	0.005
	(0.053)	(0.008)	(0.038)	(0.015)	(0.034)	(0.010)	(0.023)
2011	0.270***	-0.230***	-0.074*	-0.013	0.036	0.070***	0.033
	(0.049)	(0.014)	(0.039)	(0.038)	(0.034)	(0.007)	(0.029)
2012	0.258***	0.074***	-0.044	-0.036*	-0.013	-0.131***	0.042
	(0.052)	(0.015)	(0.058)	(0.021)	(0.062)	(0.013)	(0.044)
2013	0.270***	0.008	-0.223*	0.276	-0.055	0.037	-0.039
	(0.060)	(0.050)	(0.128)	(0.220)	(0.038)	(0.027)	(0.076)
2014	0.250***	0.183***	0.011	-0.001	0.080***	-0.032**	-0.076**
	(0.064)	(0.048)	(0.064)	(0.071)	(0.022)	(0.013)	(0.034)
2015	0.057	0.159***	-0.035	-0.067	-0.016	-0.107**	-0.043
	(0.230)	(0.060)	(0.047)	(0.052)	(0.099)	(0.054)	(0.071)
2016	0.054	0.145**	-0.089	-0.090	0.012	0.051**	0.040
	(0.235)	(0.061)	(0.102)	(0.110)	(0.067)	(0.022)	(0.088)
2017	0.070	0.097	-0.272	0.057	-0.002	0.057***	-0.008
	(0.245)	(0.076)	(0.361)	(0.147)	(0.088)	(0.012)	(0.044)
2018	0.069	-0.062	-0.450	0.079	0.071***	0.006	-0.057
	(0.251)	(0.071)	(0.683)	(0.175)	(0.021)	(0.009)	(0.045)
2019	0.101	-0.030	-0.393	0.258	0.303***	-0.073***	-0.065
	(0.256)	(0.077)	(0.606)	(0.238)	(0.086)	(0.020)	(0.048)
2020	0.123	-0.094	-0.465	0.155	0.469***	-0.052**	-0.091*
	(0.271)	(0.075)	(0.644)	(0.225)	(0.055)	(0.027)	(0.055)
2021	0.054	-0.075	-0.376	0.194	0.437***	-0.093**	-0.202*
	(0.280)	(0.077)	(0.268)	(0.211)	(0.086)	(0.044)	(0.112)

Table 2.22: Estimated ATT effects of log(wage) across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.093***	-0.027	-0.106*	0.031	-0.041	-0.036	0.066
	(0.031)	(0.058)	(0.058)	(0.068)	(0.046)	(0.022)	(0.121)
2008	0.130***	0.122***	-0.021	0.020	-0.017	0.095***	0.114***
	(0.031)	(0.024)	(0.034)	(0.065)	(0.088)	(0.022)	(0.042)
2009	0.144***	0.053*	0.011	-0.049	-0.028	0.024	-0.008
	(0.041)	(0.030)	(0.123)	(0.039)	(0.055)	(0.020)	(0.051)
2010	0.120***	-0.149***	-0.013	0.011	0.026	-0.041*	-0.037
	(0.044)	(0.027)	(0.067)	(0.030)	(0.041)	(0.022)	(0.090)
2011	0.080	0.110***	-0.194***	-0.056	-0.027	-0.084***	0.037
	(0.057)	(0.042)	(0.042)	(0.050)	(0.058)	(0.022)	(0.062)
2012	0.134**	-0.015	0.008	-0.005	0.030	-0.020	-0.026
	(0.053)	(0.020)	(0.056)	(0.037)	(0.034)	(0.026)	(0.069)
2013	0.141***	-0.064***	-0.101***	-0.004	0.013	0.088***	0.038
	(0.051)	(0.020)	(0.032)	(0.038)	(0.034)	(0.013)	(0.106)
2014	0.060	0.012	0.015	-0.002	-0.050	0.039	-0.006
	(0.063)	(0.024)	(0.023)	(0.027)	(0.049)	(0.025)	(0.076)
2015	0.166***	0.012	-0.035	-0.028	-0.023	0.005	-0.060
	(0.056)	(0.036)	(0.045)	(0.041)	(0.054)	(0.018)	(0.058)
2016	0.106	-0.115*	0.371	-0.027	-0.006	-0.053***	0.071
	(0.065)	(0.060)	(0.366)	(0.034)	(0.032)	(0.016)	(0.054)
2017	0.078	-0.078	0.513	0.016	0.002	0.074**	-0.042
	(0.066)	(0.067)	(0.428)	(0.067)	(0.076)	(0.036)	(0.053)
2018	0.110*	-0.112*	0.574	0.040	0.008	0.006	-0.042
	(0.064)	(0.067)	(0.475)	(0.065)	(0.019)	(0.014)	(0.034)
2019	0.145*	-0.167**	0.534	0.030	0.045	0.109***	-0.075**
	(0.077)	(0.077)	(0.474)	(0.079)	(0.029)	(0.011)	(0.034)
2020	0.140*	-0.357***	0.458	-0.017	-0.133	0.051***	0.071
	(0.076)	(0.091)	(0.456)	(0.044)	(0.101)	(0.020)	(0.056)
2021	0.206***	-0.330***	0.541	0.046	-0.088	0.254***	0.047
	(0.080)	(0.095)	(0.503)	(0.093)	(0.082)	(0.022)	(0.112)

Table 2.23: Estimated ATT effects of executives' equity compensation across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	**G_2008**	**G_2012**	**G_2015**	**G_2016**	**G_2018**	**G_2019**	**G_2020**
2007	-0.011	0.025	0.045	0.051	0.011	0.068**	-0.006
	(0.059)	(0.016)	(0.034)	(0.051)	(0.037)	(0.027)	(0.046)
2008	-0.022	0.457***	-0.012	-0.080**	-0.018	0.061**	-0.031
	(0.050)	(0.024)	(0.127)	(0.035)	(0.042)	(0.028)	(0.083)
2009	-0.039	-0.297***	-0.037	0.028	0.093	-0.034	-0.015
	(0.041)	(0.024)	(0.141)	(0.057)	(0.091)	(0.025)	(0.091)
2010	-0.060	-0.253***	-0.096***	0.064*	0.027	-0.052*	0.104
	(0.043)	(0.018)	(0.032)	(0.033)	(0.055)	(0.030)	(0.113)
2011	-0.055	0.040	-0.039	-0.031	-0.058	-0.057***	-0.069
	(0.055)	(0.027)	(0.030)	(0.053)	(0.037)	(0.020)	(0.055)
2012	-0.024	-0.071***	-0.023*	0.036	-0.041	-0.078***	0.044
	(0.059)	(0.012)	(0.013)	(0.072)	(0.034)	(0.012)	(0.034)
2013	-0.068	-0.079***	0.047**	-0.001	0.096	0.042	-0.077*
	(0.073)	(0.021)	(0.018)	(0.052)	(0.079)	(0.032)	(0.044)
2014	-0.086	-0.074***	0.030	-0.021	-0.162*	0.185***	0.069
	(0.053)	(0.017)	(0.062)	(0.023)	(0.097)	(0.033)	(0.046)
2015	-0.088	-0.115***	-0.007	0.009	-0.011	-0.050***	0.025
	(0.055)	(0.015)	(0.097)	(0.022)	(0.027)	(0.010)	(0.030)
2016	-0.048	-0.062*	0.014	0.057*	0.028	-0.088***	-0.040
	(0.054)	(0.033)	(0.031)	(0.032)	(0.088)	(0.016)	(0.056)
2017	-0.114**	-0.078***	-0.075	-0.044	-0.006	-0.060***	-0.025
	(0.051)	(0.021)	(0.050)	(0.044)	(0.119)	(0.018)	(0.055)
2018	-0.102*	-0.105***	-0.017	-0.012	-0.012	-0.033	-0.037
	(0.060)	(0.019)	(0.069)	(0.032)	(0.027)	(0.022)	(0.037)
2019	-0.105**	-0.022	0.054	-0.032	0.001	0.085**	0.044
	(0.054)	(0.017)	(0.048)	(0.026)	(0.037)	(0.034)	(0.049)
2020	-0.065	-0.019	0.071	0.040	0.053	0.116***	-0.024
	(0.064)	(0.020)	(0.077)	(0.037)	(0.036)	(0.024)	(0.076)
2021	-0.090	0.363***	0.046	0.031	0.095**	0.092***	-0.065
	(0.063)	(0.021)	(0.043)	(0.050)	(0.045)	(0.028)	(0.042)

Table 2.24: Estimated ATT effects of executives' salary across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.014	-0.048**	-0.014	-0.020	-0.068**	-0.070***	-0.000
	(0.042)	(0.024)	(0.028)	(0.022)	(0.035)	(0.026)	(0.030)
2008	0.068	0.015	-0.008	0.030	-0.025	0.118***	0.001
	(0.044)	(0.019)	(0.026)	(0.030)	(0.087)	(0.030)	(0.033)
2009	0.074	-0.041	-0.009	-0.016	0.118	-0.125***	0.115
	(0.066)	(0.027)	(0.043)	(0.053)	(0.103)	(0.044)	(0.116)
2010	0.118	0.089***	0.024	0.002	-0.019	0.235***	-0.157
	(0.073)	(0.019)	(0.021)	(0.046)	(0.063)	(0.033)	(0.124)
2011	0.089	-0.010	0.035	-0.005	0.018	0.023	0.045
	(0.067)	(0.015)	(0.022)	(0.023)	(0.047)	(0.022)	(0.032)
2012	0.027	0.009	0.039***	0.012	0.033	0.073**	-0.003
	(0.070)	(0.006)	(0.014)	(0.026)	(0.070)	(0.030)	(0.010)
2013	0.113**	-0.006	-0.017*	0.013	-0.097	-0.062	0.035*
	(0.053)	(0.008)	(0.009)	(0.026)	(0.118)	(0.043)	(0.018)
2014	0.041	0.078***	0.008	0.006	0.135	-0.363***	-0.032*
	(0.056)	(0.008)	(0.016)	(0.011)	(0.164)	(0.054)	(0.018)
2015	0.074	-0.025**	-0.052*	0.018*	-0.036	0.184***	0.030
	(0.056)	(0.010)	(0.030)	(0.010)	(0.041)	(0.016)	(0.022)
2016	0.082	-0.026**	0.002	-0.021	0.007	0.062***	0.029
	(0.064)	(0.013)	(0.022)	(0.015)	(0.047)	(0.015)	(0.031)
2017	0.086	-0.028**	0.015	-0.011	-0.015	0.065***	-0.055
	(0.055)	(0.012)	(0.014)	(0.023)	(0.045)	(0.014)	(0.035)
2018	0.073	0.098***	-0.014	-0.006	-0.006	-0.059***	0.018
	(0.051)	(0.010)	(0.037)	(0.027)	(0.027)	(0.015)	(0.024)
2019	0.064	-0.054***	-0.075***	-0.030*	0.042	-0.068***	0.032
	(0.053)	(0.012)	(0.018)	(0.017)	(0.055)	(0.025)	(0.037)
2020	0.075	0.043**	-0.034	-0.034*	-0.011	-0.075***	-0.003
	(0.054)	(0.018)	(0.041)	(0.018)	(0.033)	(0.029)	(0.029)
2021	0.084	-0.072***	-0.031***	-0.025	0.049	-0.003	-0.022
	(0.061)	(0.016)	(0.009)	(0.020)	(0.044)	(0.019)	(0.025)

Table 2.25: Estimated ATT effects of executives' bonus across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	0.019 (0.029)	0.003 (0.007)	0.008 (0.009)	-0.007 (0.013)	0.023 (0.022)	0.028* (0.016)	0.012 (0.015)
2008	0.009 (0.038)	0.002 (0.009)	0.002 (0.010)	0.001 (0.009)	0.039 (0.045)	-0.121*** (0.017)	-0.019 (0.022)
2009	0.036 (0.042)	0.009* (0.005)	0.010** (0.005)	-0.000 (0.011)	-0.051** (0.020)	0.023*** (0.006)	0.006 (0.007)
2010	0.022 (0.048)	-0.013** (0.006)	-0.010 (0.007)	-0.011* (0.006)	0.003 (0.015)	0.009 (0.009)	0.001 (0.010)
2011	-0.006 (0.033)	0.003 (0.006)	0.004 (0.006)	0.010 (0.011)	0.018 (0.026)	-0.007 (0.015)	0.009 (0.008)
2012	-0.006 (0.041)	-0.002 (0.004)	0.028 (0.019)	-0.021** (0.010)	-0.012 (0.050)	-0.002 (0.030)	0.002 (0.007)
2013	0.001 (0.030)	-0.006 (0.012)	-0.038 (0.027)	0.002 (0.014)	0.029 (0.044)	-0.016 (0.027)	-0.013 (0.020)
2014	-0.049** (0.025)	0.099*** (0.004)	0.007 (0.012)	0.010 (0.013)	-0.052 (0.066)	0.100*** (0.032)	0.004 (0.018)
2015	-0.032 (0.027)	0.015** (0.007)	0.009* (0.005)	0.015** (0.007)	0.067 (0.066)	-0.130*** (0.037)	0.002 (0.009)
2016	-0.027 (0.028)	0.044*** (0.015)	0.031*** (0.011)	-0.056 (0.062)	-0.030 (0.028)	0.020** (0.009)	-0.010 (0.014)
2017	-0.010 (0.029)	0.004 (0.011)	0.004 (0.007)	-0.066 (0.064)	0.021 (0.021)	-0.007 (0.007)	-0.001 (0.011)
2018	-0.026 (0.035)	0.007 (0.006)	0.008 (0.009)	-0.069 (0.066)	0.015 (0.026)	-0.015 (0.016)	0.008 (0.017)
2019	-0.025 (0.036)	0.070*** (0.010)	0.025 (0.023)	-0.074 (0.064)	-0.045 (0.039)	0.003 (0.016)	-0.001 (0.025)
2020	-0.049 (0.032)	-0.029*** (0.006)	0.074 (0.073)	-0.075 (0.085)	-0.014 (0.025)	-0.001 (0.022)	-0.027 (0.027)
2021	-0.030 (0.043)	-0.033*** (0.010)	-0.019 (0.026)	-0.089 (0.076)	-0.047 (0.043)	0.006 (0.017)	0.001 (0.022)

Table 2.26: Estimated ATT effects of executives' non-equity incentive compensation across treatment groups, using the not-yet-treated estimator. Each column represents a treatment group and its corresponding treatment year, while the "time" column reports the calendar year. Shaded cells indicate periods occurring after treatment for each group.

time	G_2008	G_2012	G_2015	G_2016	G_2018	G_2019	G_2020
2007	-0.047	0.023	-0.048	0.021	-0.068**	0.033*	0.010
	(0.045)	(0.025)	(0.042)	(0.033)	(0.028)	(0.019)	(0.059)
2008	0.050*	-0.291***	0.006	0.069***	0.063	0.021	0.095
	(0.029)	(0.019)	(0.078)	(0.026)	(0.055)	(0.020)	(0.063)
2009	-0.002	0.335***	0.058	-0.074*	-0.103	-0.046	-0.139
	(0.025)	(0.027)	(0.093)	(0.044)	(0.078)	(0.029)	(0.085)
2010	-0.041	-0.033	0.064**	-0.010	-0.059**	-0.033**	0.003
	(0.038)	(0.020)	(0.031)	(0.032)	(0.023)	(0.015)	(0.032)
2011	0.019	0.020	-0.006	0.020	0.063***	0.028**	0.059*
	(0.036)	(0.014)	(0.020)	(0.026)	(0.024)	(0.014)	(0.036)
2012	0.011	-0.001	-0.073***	-0.018	0.019	-0.007	-0.013
	(0.040)	(0.010)	(0.022)	(0.046)	(0.052)	(0.010)	(0.030)
2013	0.042	0.005	-0.027	-0.012	0.031	0.022**	0.029
	(0.047)	(0.014)	(0.031)	(0.030)	(0.026)	(0.010)	(0.019)
2014	0.092*	-0.005	-0.028	-0.014	0.064*	0.056***	-0.031**
	(0.049)	(0.014)	(0.045)	(0.017)	(0.034)	(0.017)	(0.015)
2015	0.109**	0.023	0.041	-0.035	-0.025	-0.002	-0.055
	(0.047)	(0.015)	(0.050)	(0.034)	(0.039)	(0.021)	(0.034)
2016	0.074*	-0.035	0.004	0.041	-0.012	0.039*	0.012
	(0.040)	(0.035)	(0.075)	(0.058)	(0.148)	(0.021)	(0.047)
2017	0.107**	-0.012	0.125	0.128***	-0.017	0.006	0.087
	(0.045)	(0.019)	(0.114)	(0.044)	(0.158)	(0.024)	(0.059)
2018	0.103**	-0.004	0.036	0.077	0.038	-0.012	0.016
	(0.048)	(0.021)	(0.064)	(0.067)	(0.049)	(0.016)	(0.036)
2019	0.095**	0.014	0.063	0.136***	0.041	0.032	-0.090***
	(0.043)	(0.012)	(0.071)	(0.032)	(0.061)	(0.024)	(0.028)
2020	0.058	-0.002	-0.070**	0.070	-0.049	-0.107***	-0.016
	(0.056)	(0.031)	(0.033)	(0.047)	(0.068)	(0.038)	(0.054)
2021	0.116*	-0.114***	0.058	0.094	-0.095*	-0.104***	0.092***
	(0.067)	(0.025)	(0.087)	(0.065)	(0.057)	(0.029)	(0.031)

2.7.3 Robustness check

Table 2.27: Robustness check- Sample: all reports of data breach.
The table presents estimates of probit regressions in which the dependent variable is an indicator that takes the value one if a firm experiences a data breach incident in a given year, and zero otherwise. The sample consists of 197 firm-year observations that reported a data breach incident in the following fiscal year and the remaining observations that did not experience a data breach incident over the period 2005 to 2023. All explanatory variables are measured one year before the attack except for Tobin's q, which is measured two years before the attack. All control variables from Table **??** are included in estimation but not present in the table. The appendix provides detailed descriptions of the construction of the variables. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level.***,** and * denote significance at the 1%, 5%, and 10% levels, respectively. Normalized values of the active ratio are used in this table.

	Dependent variable = Data breach incident (indicator)				
VARIABLES	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**
Active ratio	-0.18*	-0.20**	-0.28***	-0.24**	-0.27**
	(0.09)	(0.10)	(0.10)	(0.11)	(0.11)
Log(ESOP $/ active participants)	-0.04				
	(0.12)				
Log(ESOP $/ active participants) × Active ratio	0.04				
	(0.06)				
log(total assets)	0.55***	0.54***	0.59***	0.53**	0.45**
	(0.20)	(0.20)	(0.21)	(0.22)	(0.23)
Executive equity	0.03	0.05			
	(0.12)	(0.12)			
L1.Active ratio	0.68	0.57	0.46	0.52	1.20
	(0.88)	(0.89)	(0.92)	(0.99)	(1.23)
Active ratio* Executive equity		0.11			
		(0.07)			
Active ratio * log(total assets)			0.21**	0.22**	0.18**
			(0.08)	(0.09)	(0.09)
L2.Active ratio				0.79	1.03
				(0.95)	(1.18)
L3.Active ratio					-0.21
					(0.74)
Constant	-2.56***	-2.48***	-2.49***	-2.27***	-2.28***
	(0.56)	(0.55)	(0.55)	(0.56)	(0.61)
Observations	2,157	2,157	2,157	1,740	1,476
Industry FE	Y	Y	Y	Y	Y
Year FE	Y	Y	Y	Y	Y

Control variables	Y	Y	Y	Y	Y
Pseudo R-squared	0.298	0.300	0.305	0.301	0.289

Table 2.28: This table presents estimates from probit regressions for two subsets of the data. The dependent variable is an indicator equal to one if a firm experiences a data breach in a given year, and zero otherwise. In Column 1, data is sorted based on log(ESOP/employees). In Column 2, sorting is based on active ratio. All control variables are included in estimation but not present in the table. The appendix provides detailed descriptions of the construction of the variables. Standard errors reported in parentheses are adjusted for heteroskedasticity and clustering at the firm level., and denote significance at the 1%, 5%, and 10% levels, respectively. Normalized values of the active ratio are used in this table

	Dependent variable = Data breach incident (indicator)	
	Sorted on (ESOP/emp)	**Sorted on active ratio**
VARIABLES	**(1)**	**(2)**
Q2-Q1	0.45*	-0.26
	(0.25)	(0.20)
Q3-Q1	0.39	-1.01***
	(0.34)	(0.29)
Active ratio	-0.40***	
	(0.11)	
log(ESOP/emp)		-0.05
		(0.12)
Constant	-2.51***	-1.90***
	(0.49)	(0.51)
Observations	2,370	2,370
Independent variables	Y	Y
Industry FE	Y	Y
Year FE	Y	Y
Pseudo R-squared	0.316	0.318

Figures

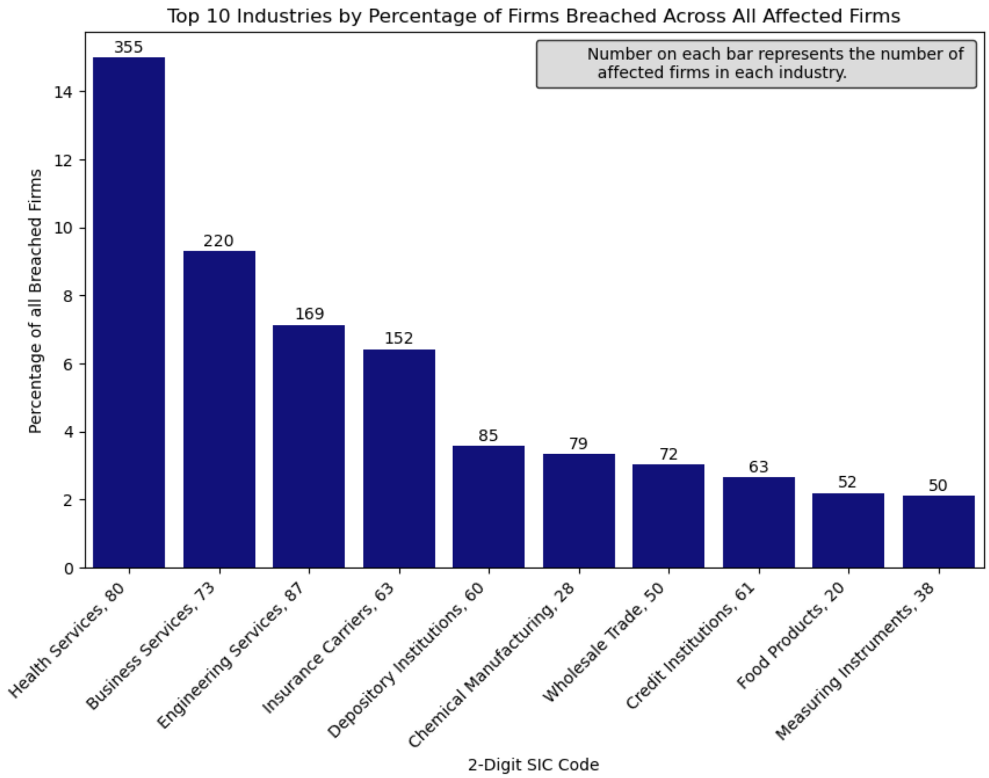

Figure 2.1: Timeline trends for the median per-employee ESOP ownership value in breached and non-breached firms.

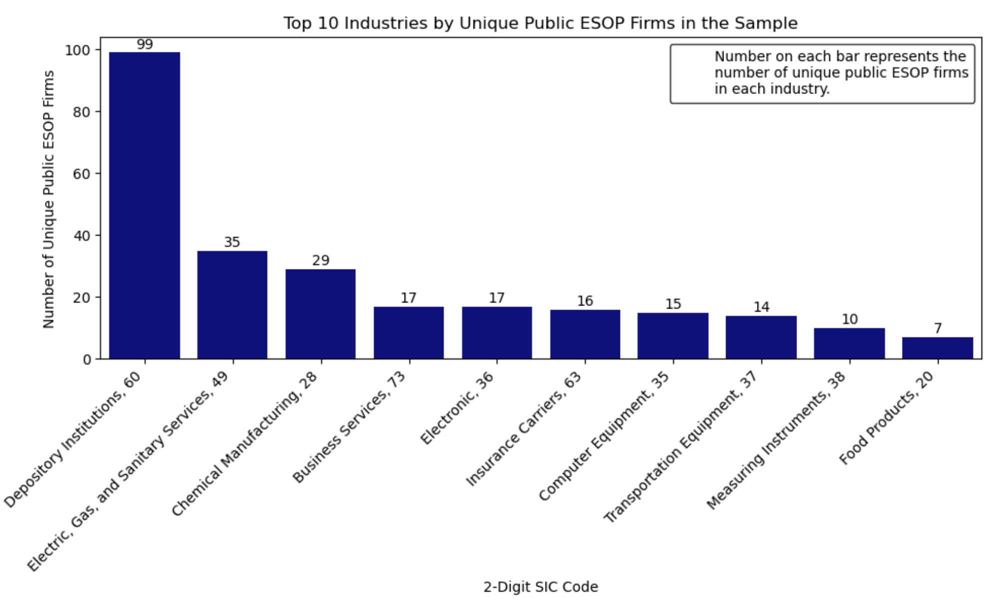

Figure 2.2: Percentage of Public ESOP firms in each industry in the sample.

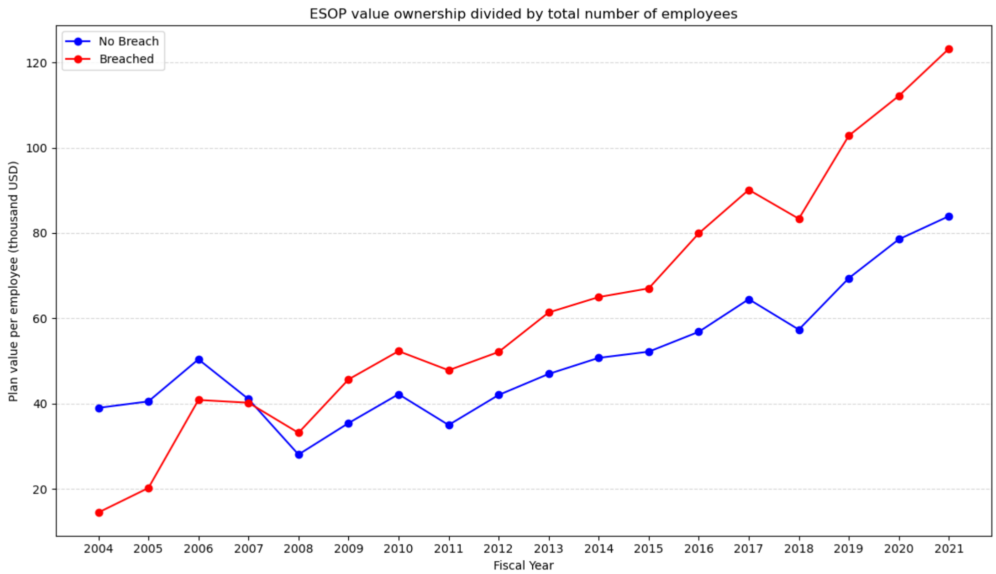

Figure 2.3: Timeline trends for the median per-employee ESOP ownership value in breached and non-breached firms.

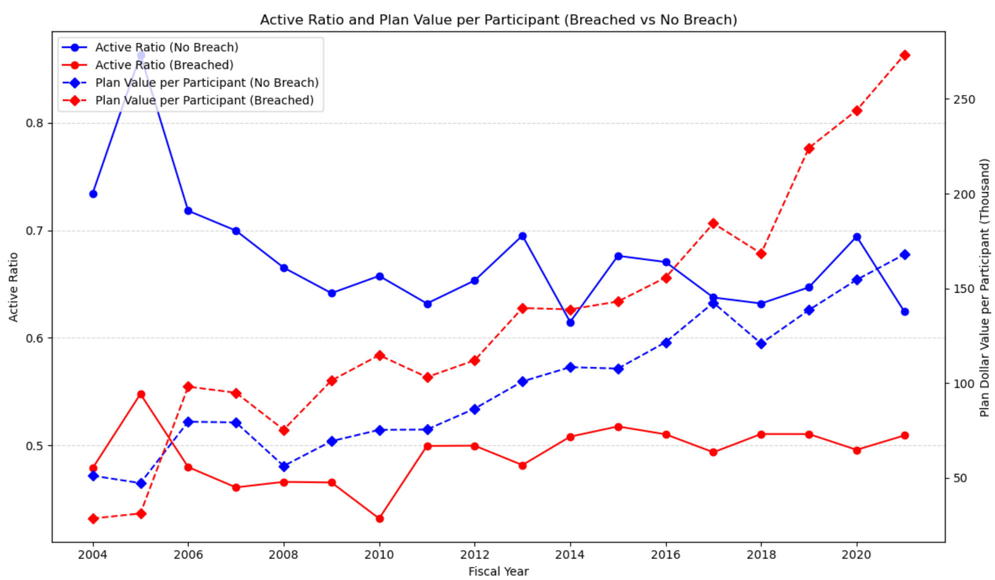

Figure 2.4: Timeline trends for the median active ratio and per-participant ESOP ownership value in breached and non-breached firms.

0.9

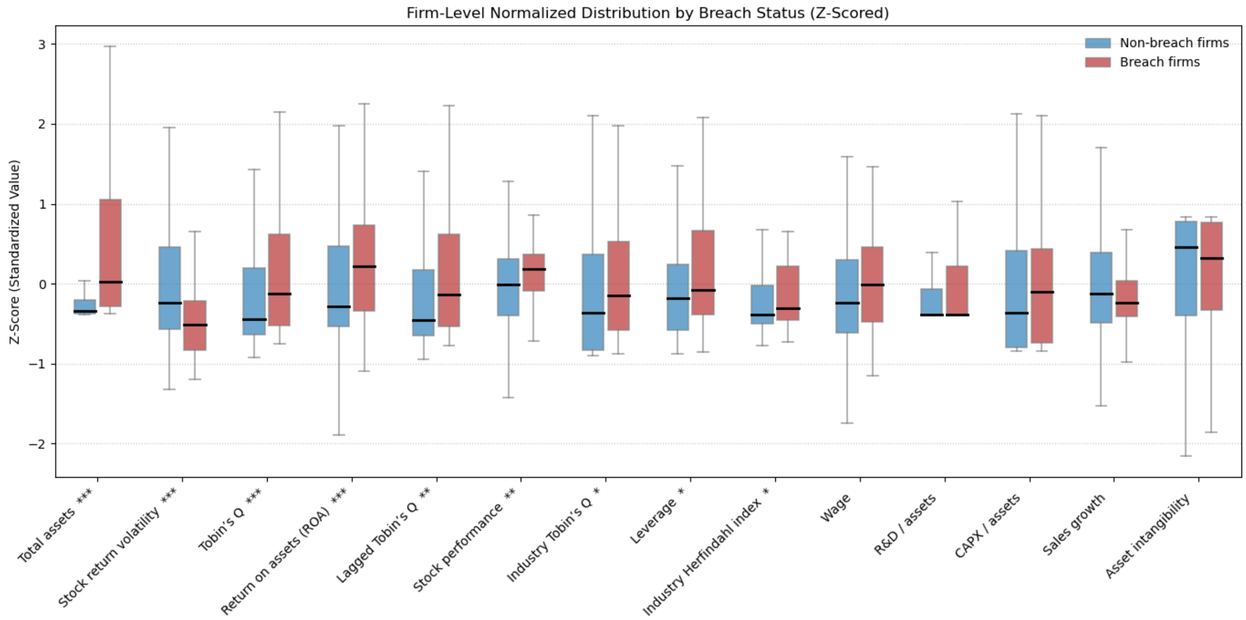

Figure 2.5: Fundamentals; , and denote significance at the 1%, 5%, and 10% levels, respectively.

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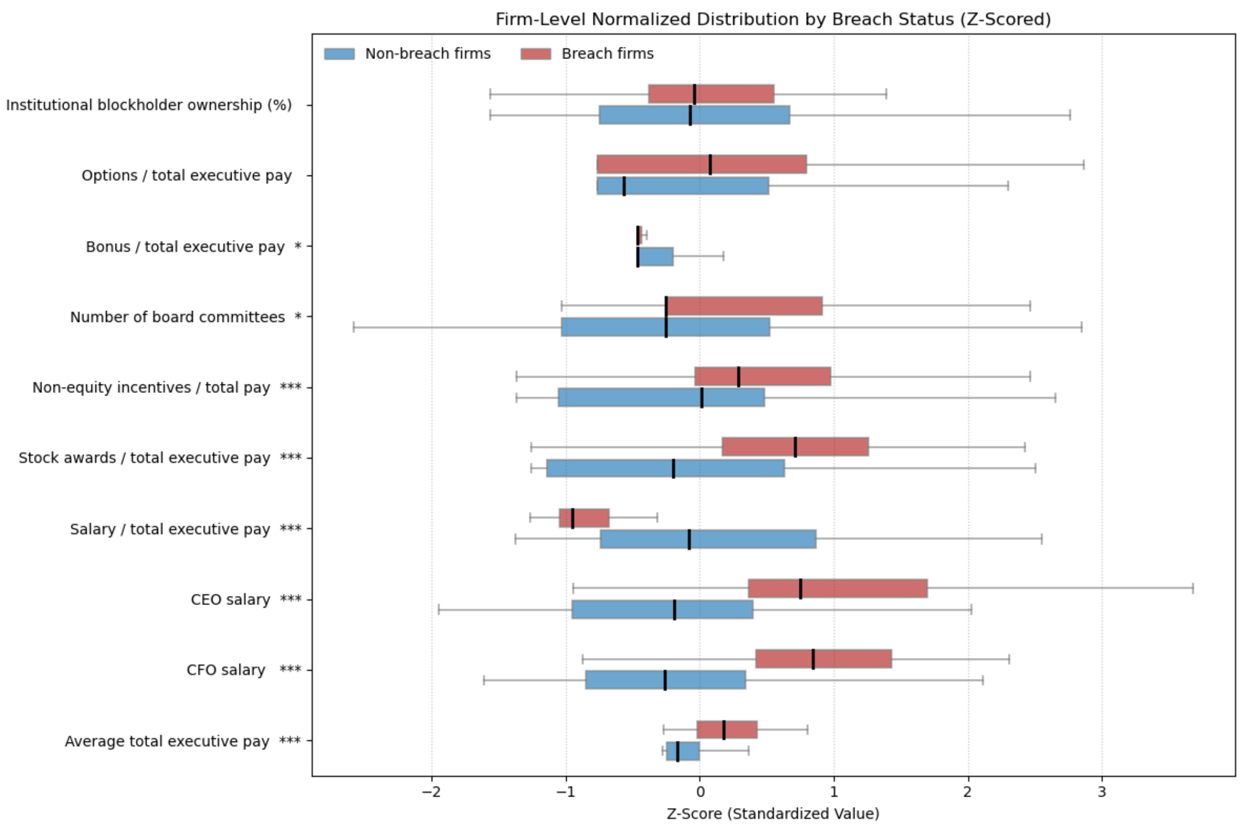


Figure 2.6: Governance; , and denote significance at the 1%, 5%, and 10% levels, respectively.

Figure 2.7: Summary statistics, Panels (a)–(b).

0.9

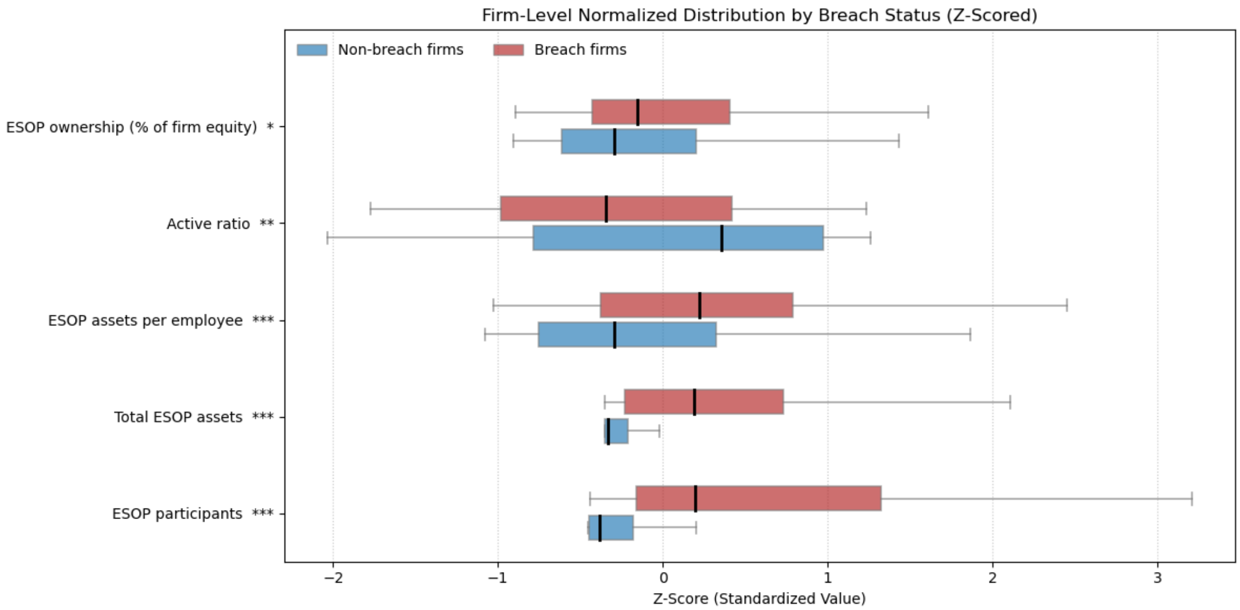


Figure 2.7: ESOP variables; , and denote significance at the 1%, 5%, and 10% levels, respectively.

0.9

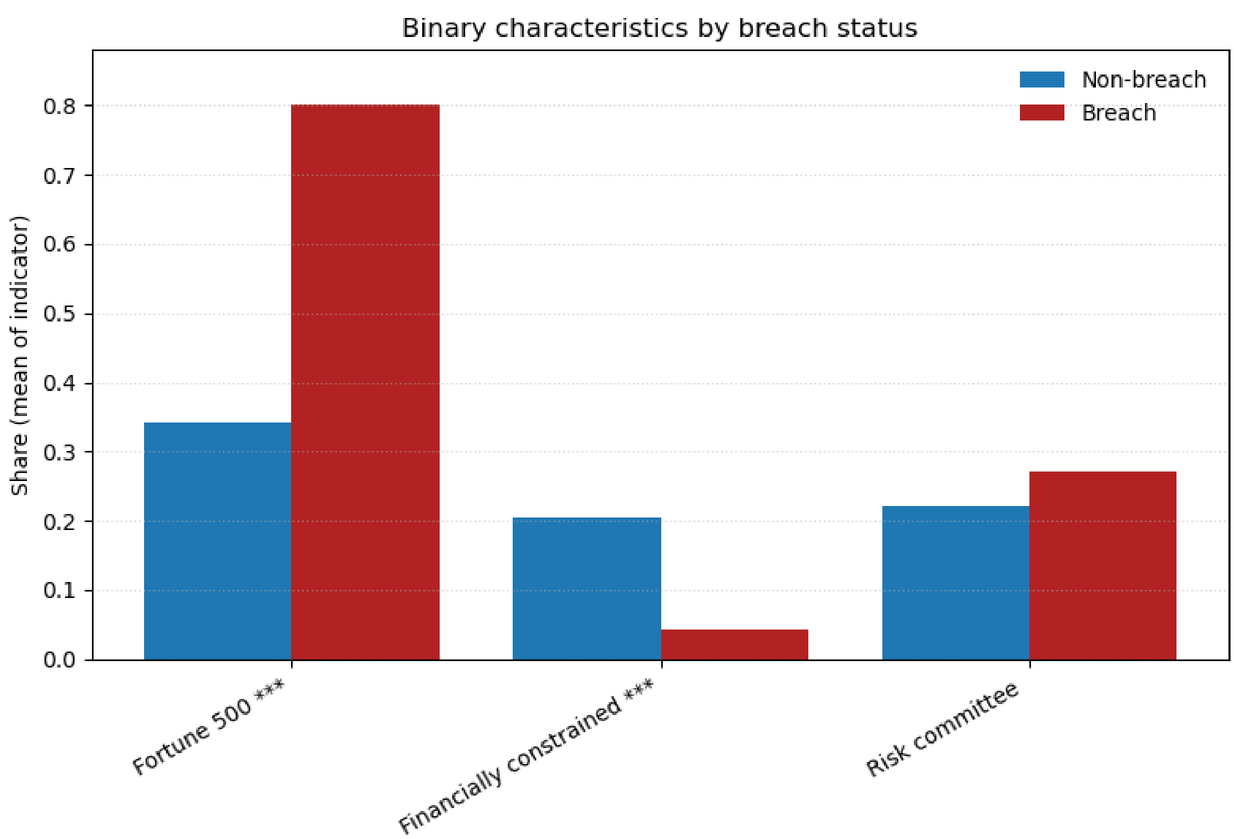


Figure 2.8: Indicator variables; , and denote significance at the 1%, 5%, and 10% levels, respectively.

Figure 2.9: Summary statistics (continued), Panels (c)–(d).

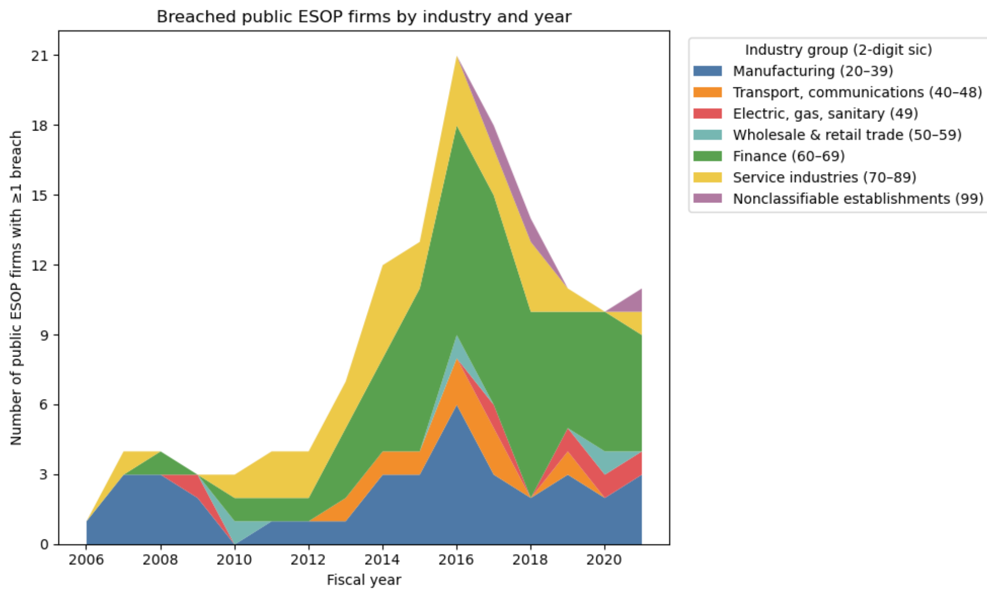


Figure 2.10: Number of public ESOP firms that experienced at least one breach in a year, by 2-digit industry

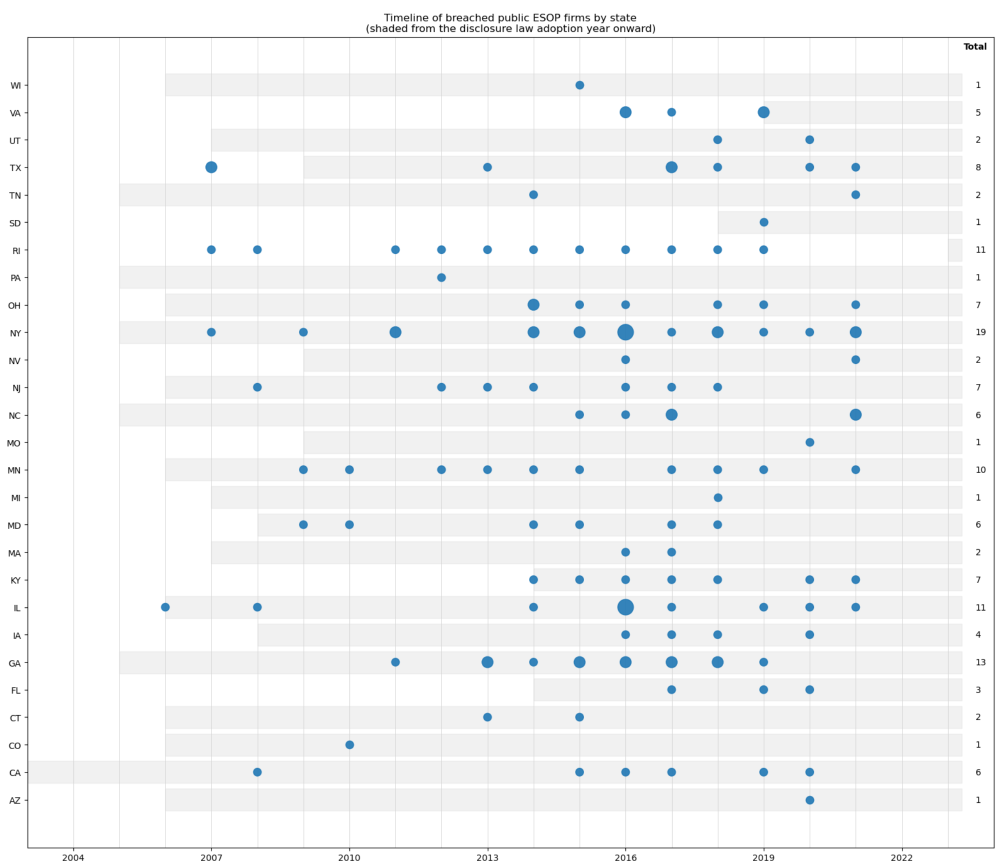


Figure 2.11: Distribution of public ESOP firms with breaches across states over time; the shaded area indicates the years following disclosure law adoption.

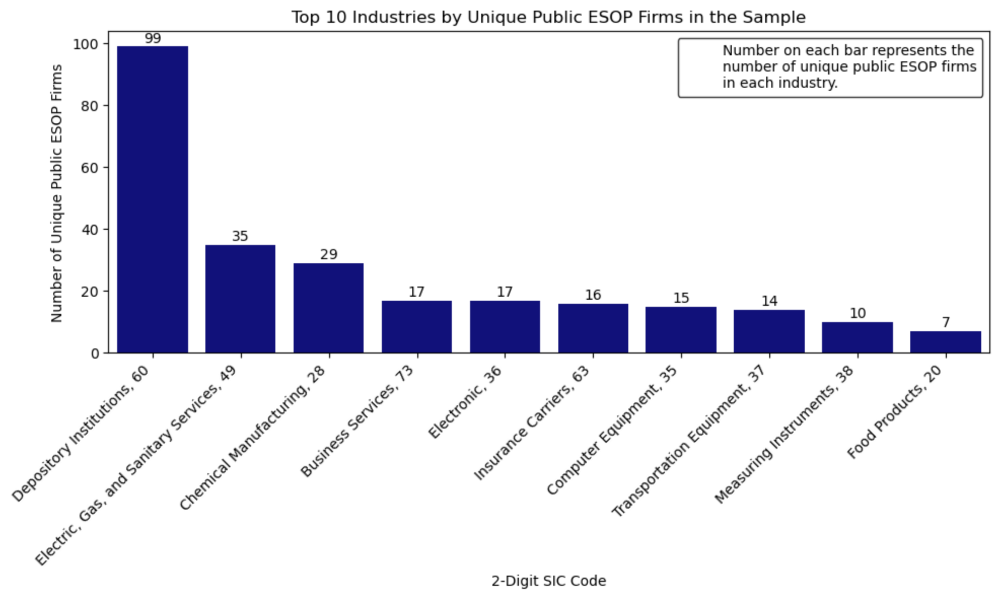


Figure 2.12: Percentage of Public ESOP firms in each industry

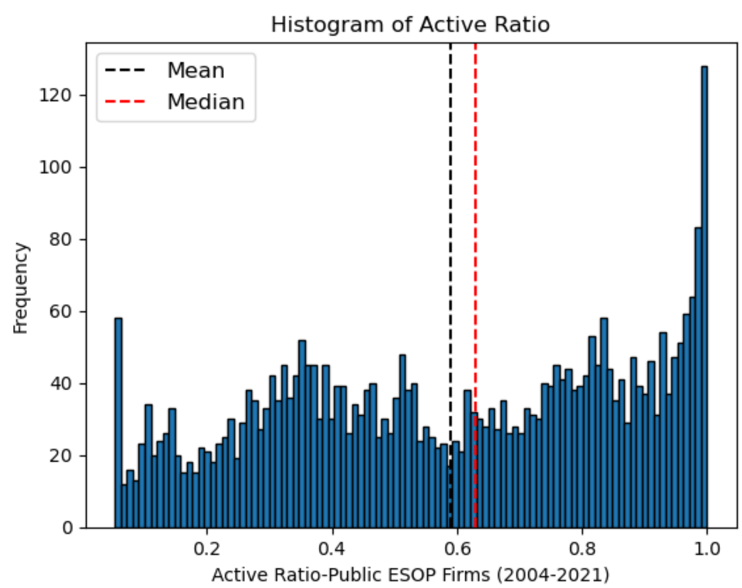


Figure 2.13: Distribution of active ratio

2.7.4 Effects of data berach incidents on firms

Figure 2.14: Synthetic control estimates for the 24 treated firms
The number on each graph indicates the firm's CIK. The dashed line shows the path of the synthetic unit. The vertical line shows the treatment time.

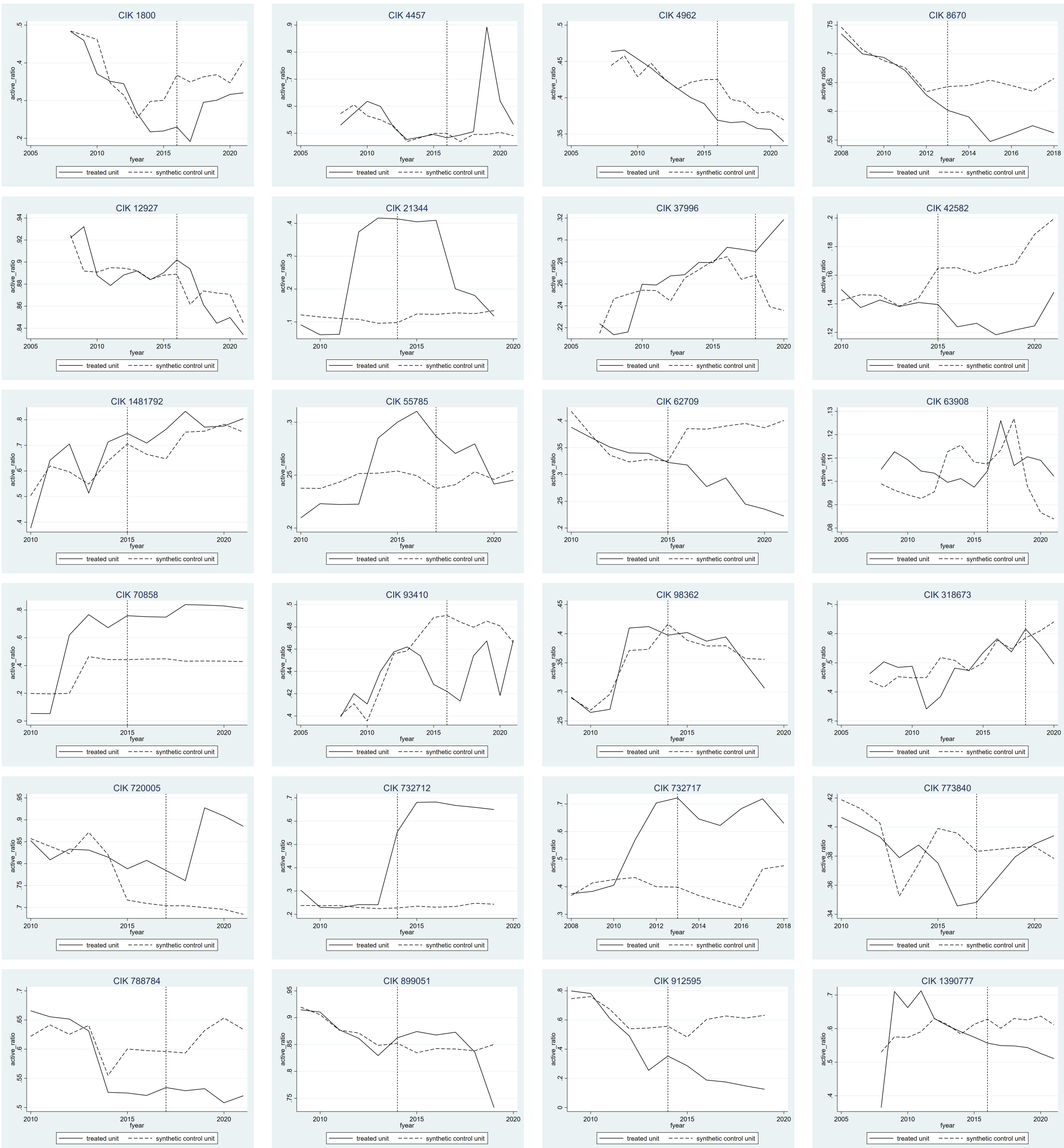
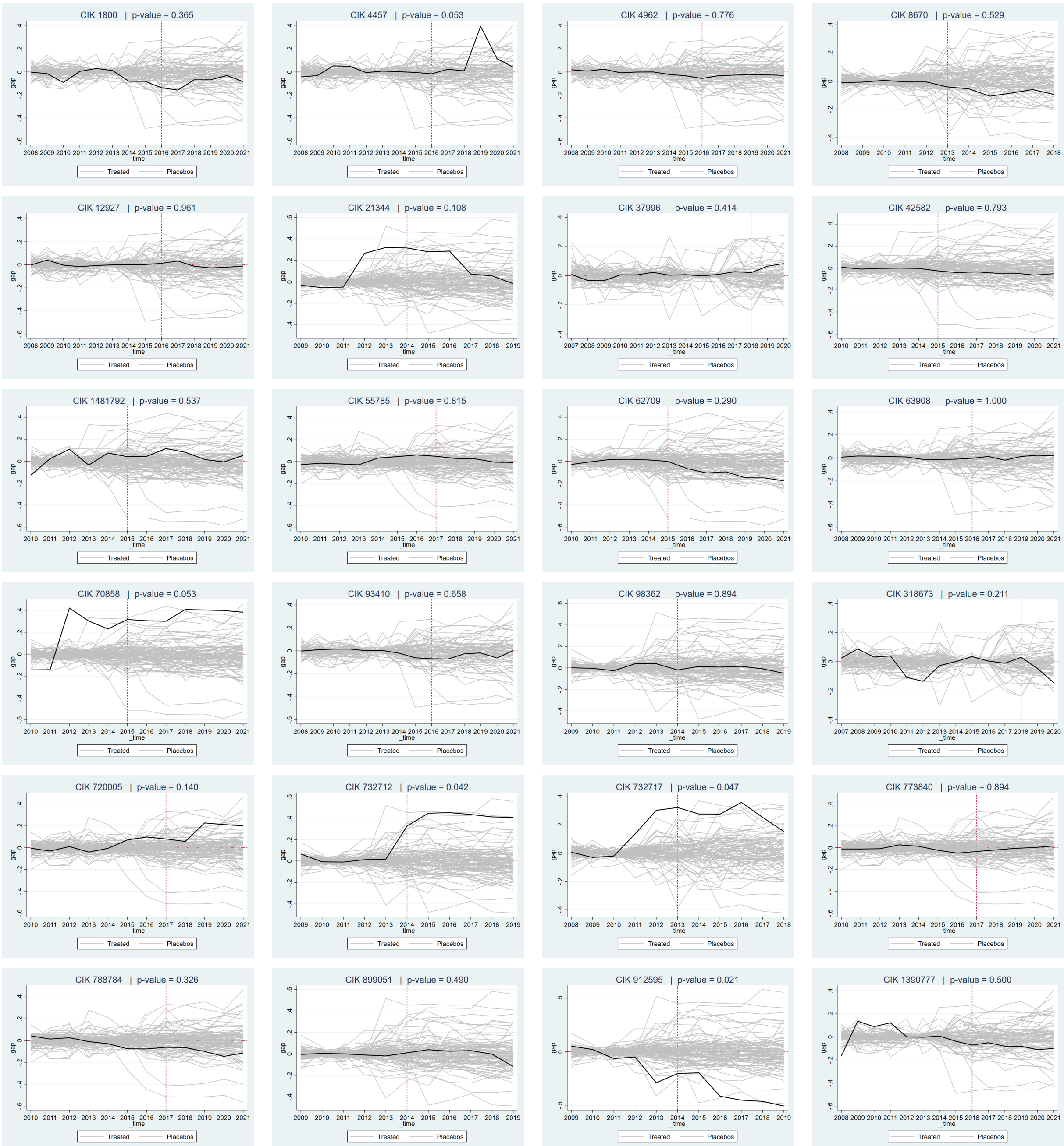


Figure 2.15: Placebo Test Results (24 Firms)
The gray background lines show the gap between each donor firm's synthetic unit and its original data in the placebo test. The bold black line shows the gap between the synthetic unit and the observed data for the treated firm in each year. The CIK and p-value for each experiment are displayed in the graph, and the vertical line marks the treatment year.



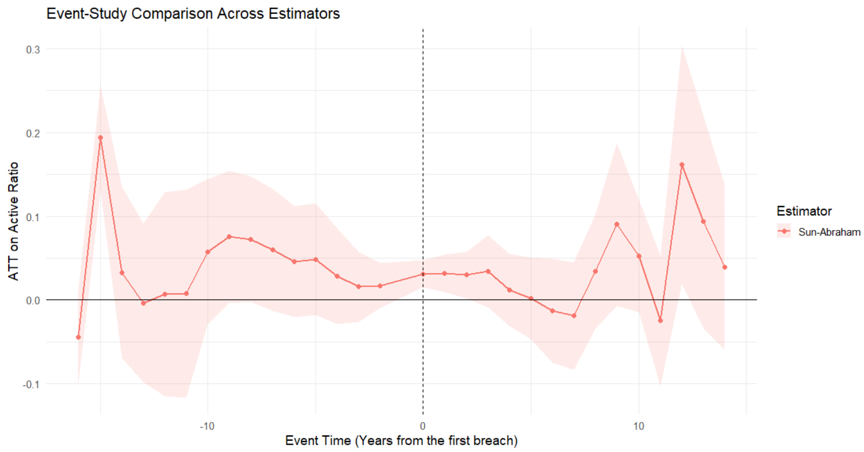


Figure 2.16: ATT of the active ratio after the first breach in a firm

2.7.5 Peer firms' behavior following industry breach incidents

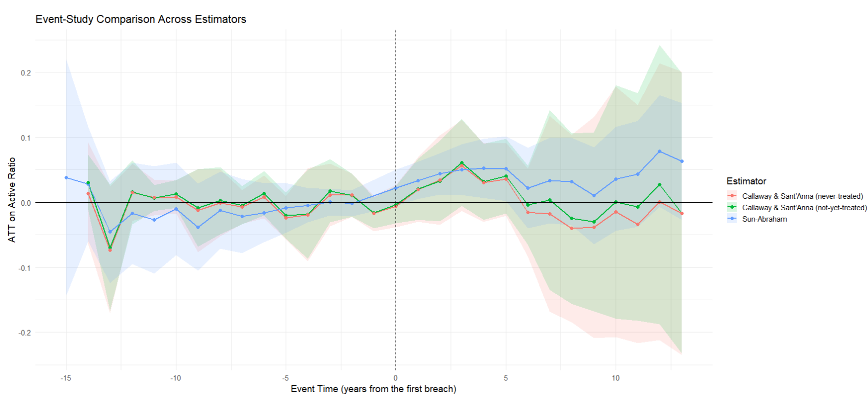


Figure 2.17: ATT of active ratio after the first industry breach

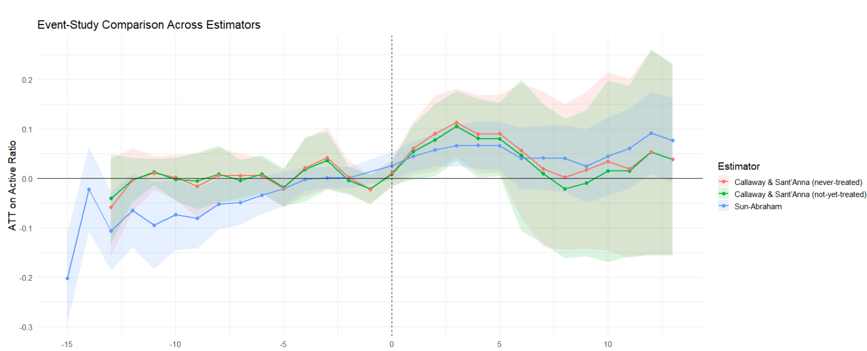


Figure 2.18: ATT of active ratio after the first industry breach with 25% of impacts

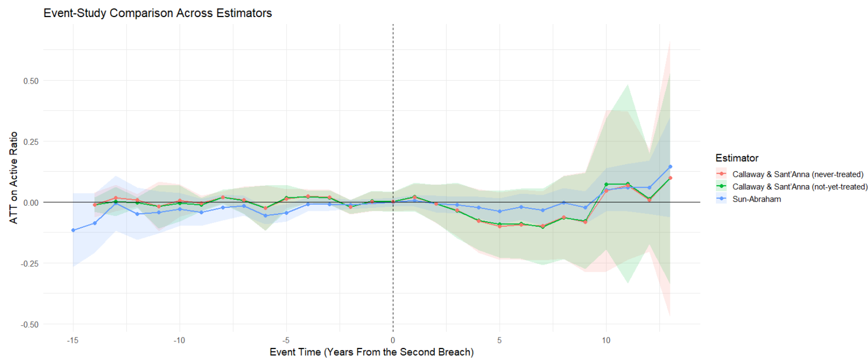


Figure 2.19: ATT of active ratio after the second breach of the industry

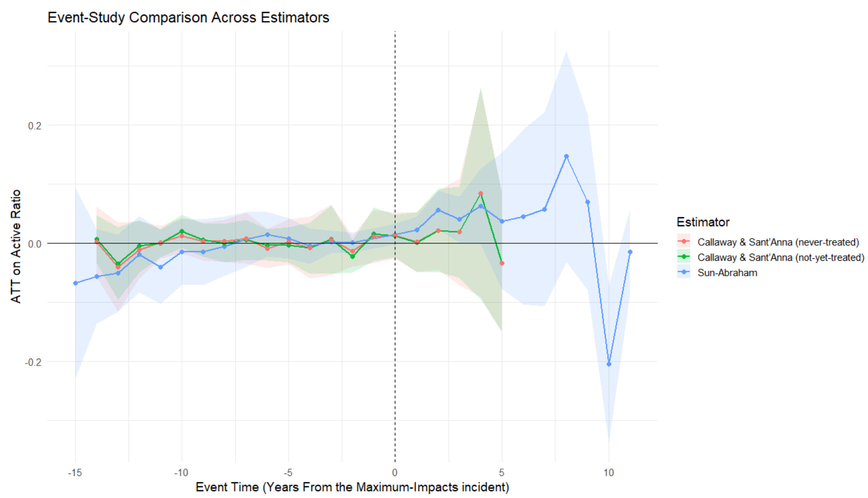


Figure 2.20: ATT of active ratio after the breach with the max impacts in the industry

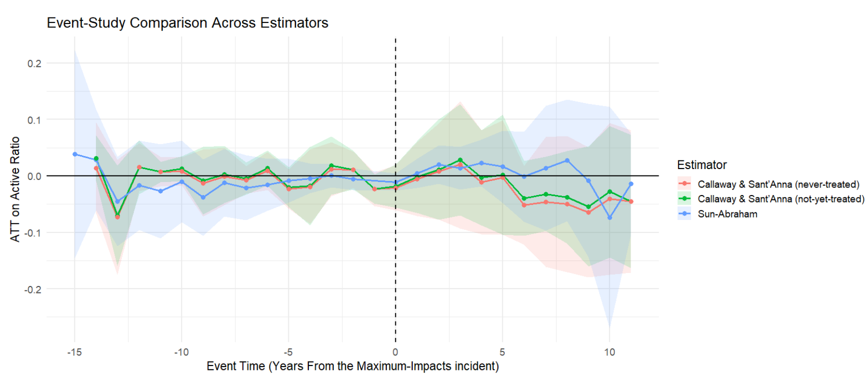


Figure 2.21: ATT of active ratio after the first breach when 2008 group is excluded.

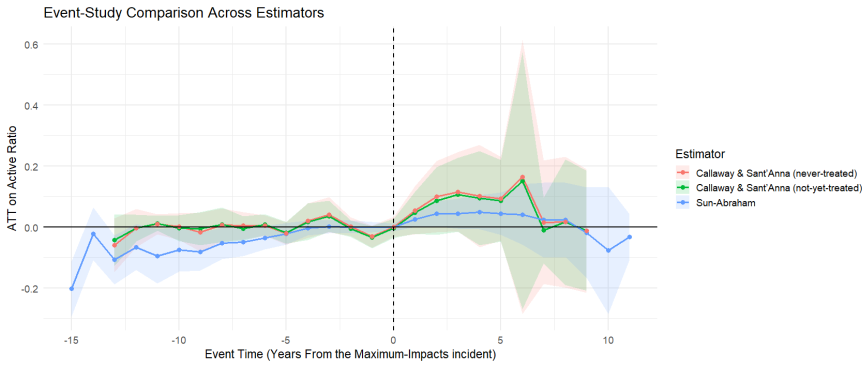


Figure 2.22: ATT of active ratio after the first breach with 1800 impacts when 2008 group is excluded.

Appendix A.

Table 2.29

List of public ESOP firms with sic=49 in the sample	
PINNACLE WEST CAPITAL CORPORATION	FIRSTENERGY CORP.
CONSTELLATION ENERGY GROUP, INC.	OGE ENERGY CORP.
CHESAPEAKE UTILITIES CORPORATION	ONEOK, INC.
DOMINION RESOURCES, INC.	OTTER TAIL CORPORATION
ENERGEN CORPORATION	PG&E CORPORATION
ATMOS ENERGY CORPORATION	SEMPRA ENERGY
NEXTERA ENERGY, INC.	EXELON CORPORATION
HAWAIIAN ELECTRIC INDUSTRIES, INC.	PEPCO HOLDINGS, INC.
CENTERPOINT ENERGY, INC.	PUBLIC SERVICE ENTERPRISE GROUP INCORPORATED
GREAT PLAINS ENERGY INCORPORATED	QUESTAR CORPORATION
EVERGY, INC.	TECO ENERGY, INC.
ENTERGY CORPORATION	AMEREN CORPORATION
ALLETE, INC.	AVISTA CORPORATION
NATIONAL FUEL GAS COMPANY	THE WILLIAMS COMPANIES, INC.
NISOURCE INC	WEC ENERGY GROUP, INC.
XCEL ENERGY INC.	ENERGY WEST, INC.
SPECTRA ENERGY CORP	WASTE MANAGEMENT, INC.
THE AES CORPORATION	

Table 2.30: Detailed descriptions of all the variables used in the tables.

Variable	Description	Source
Active participants/total employees	Active participants in the plan/ employees population	IRS Form 5 Compustat
Assets in master trust	Assets in master trust (may include employer securities) (maseoy)	IRS Form 5
Asset intangibility	1 –totalproperty, plant, and equipment (ppent)/ total assets (at)	Compustat and 10-Q f
All executives salary	Total dollar amount of salary paid to all executives of a firm during a fiscal year	ExecuCom DEF 14A
All executives total pay	Executives total pay including salary, option awards, stock units, bonus, etc.(tdc1)	ExecuCom DEF 14A
CAPX/assets	Capital expenditures (capx)/total assets (at)	Compustat and 10-Q f
CEO salary	Total dollar amount of salary paid to the CEO of a firm during a fiscal year	ExecuCom DEF 14A
CEO total pay	CEO total pay including salary, option awards, stock units, bonus, etc.(tdc1)	ExecuCom DEF 14A
CFO salary	Total dollar amount of salary paid to the CFO of a firm during a fiscal year	ExecuCom DEF 14A
CFO total pay	CFO total pay including salary, option awards, stock units, bonus, etc.(tdc1)	ExecuCom DEF 14A
ESOP assets per participant	Total ESOP assets/ active participants	IRS Form 5
Employer securities in plan	Employer securities in plan (seceoy)	IRS Form 5
Executive equity based compensation option awards for all executives	Sum of the grant-date value of stock awards and Execucomp	

Financial constraint (indicator)	The WW index derived by White and Wu (2006) based on an Euler equation approach from a structural model of investment. The WW index is a linear combination of six factors according to the following formula: WW = −0.091CF−0.062DIVPOS+0.021TLTD −0.044LNTA+0.102ISG−0.035SG, where CF is the ratio of cash flow to total assets; DIVPOS is a dummy variable that takes the value of one if the firm pays cash dividends; TLTD is the ratio of the long-term debt to total assets; LNTA is the natural log of total assets; ISG is the firm's three-digit industry sales growth; and SG is firm sales growth. Firms with a higher value of the WW index are more constrained. I rank firms based on the WW index and group the top (bottom) tertile into constrained (unconstrained) portfolios.	Compustat and 10-Q f
Financial industry (indicator)	One for industries with SIC codes of 6000 and above and less than 7000, and zero otherwise	Compustat
Fortune 500 membership (indicator)	One if a firm is included in the list of Fortune 500 companies in a given year, and zero otherwise	Fortune.co 50pros.com Kaggle.com
Industry's Herfindahl index	index computed as the sum of squared market shares of firms' sales at the two-digit SIC industry level	Compustat
Industry's Tobin's q	Median Tobin's q of all firms in the same two-digit SIC code industries in a given year.	Compustat
Institutional block ownership	Max(0,Number of shares held by institutional shareholders that own more than 5% of a firm's equity scaled by the total number of shares outstanding)	Thompson
Leverage	long term debt(dltt)+ debt in current liabeities(dlc)/ stock holder equity(seq)	Compustat and 10-Q f
Max of ESOP assets per participant	Min of ESOP assets per participant+ assets in master trust	IRS Form 5
Min of ESOP assets per participant	Max(Employer securities in plan, 51% * total plan assets)	IRS Form 5
mean(executive equity/total pay)	Average equity portion of compensation across all executives	Execucomp
Number of board committees	Number of board committees in a given fiscal year	Boardex
R&D/assets	Max (0, R&D expenditures (xrd))/total assets (at)	Compustat and 10-Q f
Risk committee (indicator)	One if the name of a firm's board committee includes "risk," and zero otherwise	Boardex
ROA	Net income (ni)/total assets (at)	Compustat and 10-Q f

Sales growth	$\text{Sales}_t / sales_{t-1}$	Compustat and 10-Q f
Service industries	One for industries with SIC codes of 7000 and above and less than 9000, and zero otherwise	
Similarity measure	Result of the S-BERT model, which quantifies the similarity between the descriptions of two data breach incidents.	
Stock performance	Buy-and-hold return for the year net of the CRSP value-weighted index return	CRSP
Stock return volatility	Standard deviation of a firm's daily stock returns during a fiscal year	CRSP
Taxable income	(Federal tax + foreign tax)/top marginal corporate tax rate) - change in tax loss carry forward	Compustat and 10-Q f
Tobin's q	(Total assets (at) – common/ordinary equity (ceq) + market value of equity (prcc_f × csho))/ total assets (at)	Compustat and 10-Q f
Total ESOP assets	Total plan assets (taseoy)	IRS Form 5
Transportation and communications	One for industries with SIC codes of 4000 and above and less than 4900, and zero otherwise	
Wage	Staff expense as reported in Compustat is divided by the total employee population. This item includes all elements of employee compensation, including salary, pension, etc. If it is missing, it is replaced by the median wage per employee in the corresponding 2-digit SIC code over the same fiscal year.	Compustat
Wholesale trade and retail trade (indicator)	One for industries with SIC codes of 5000 and above and less than 6000, and zero otherwise	
Zero institutional ownership indicator	An indicator with value one if a firm has no fillings in Thompson 13F, and zero otherwise.	
Zero R&D indicator	An indicator with value one if a missing R&D expense is replaced by zero, and zero otherwise	

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