IMF Working Paper

Strategic Corporate Layoffs

by Ruchir Agarwal and Julian Kolev

INTERNATIONAL MONETARY FUND

IMF Working Paper

Middle East and Central Asia Department

Strategic Corporate Layoffs

Prepared by Ruchir Agarwal and Julian Kolev

Authorized for distribution by Annalisa Fedelino

December 2016

IMF Working Papers **describe research in progress by the author(s) and are published to elicit comments and to encourage debate.** The views expressed in IMF Working Papers are those of the author(s) and do not necessarily represent the views of the IMF, its Executive Board, or IMF management.

Abstract

Firms in the S&P 500 often announce layoffs within days of one another, despite the fact that the average S&P 500 constituent announces layoffs once every 5 years. By contrast, similar-sized privately-held firms do not behave in this way. This paper provides empirical evidence that such clustering behavior is largely due to CEOs managing their reputation in financial markets. To interpret these results we develop a theoretical framework in which managers delay layoffs during good economic states to avoid damaging the markets perception of their ability. The model predicts clustering in the timing of layoff announcements, and illustrates a mechanism through which the cyclicality of firms layoff policies is amplified. Our findings suggest that reputation management is an important driver of layoff policies both at daily frequencies and over the business cycle, and can have significant macroeconomic consequences.

Strategic Corporate Layoffs[*]

Ruchir Agarwal   Julian Kolev

IMF   Harvard University

Abstract

Firms in the S&P 500 often announce layoffs within days of one another, despite the fact that the average S&P 500 constituent announces layoffs once every 5 years. By contrast, similar-sized privately-held firms do not behave in this way. This paper provides evidence that such clustering behavior is largely due to CEOs managing their reputation in financial markets. To interpret these results we develop a theoretical framework in which managers delay layoffs during good economic states to avoid damaging the markets perception of their ability. The model predicts clustering in the timing of layoff announcements, and illustrates a mechanism through which the cyclicality of firms layoff policies is amplified. The models predictions are tested using two novel datasets of layoff announcements and actual mass layoffs. We compare the layoff behavior of publicly-listed and privately-held firms to estimate the impact of reputation-based incentives on cyclicality of layoffs. Compared to observably similar matched private firms, public firms are 2.5-3.5 percentage points more likely to conduct mass layoffs in a recession month, indicating a doubling of layoff sensitivity to recessions. In addition, we find that the firms that cluster layoff announcements at high frequencies are also the ones that are more likely to engage in mass layoffs during recessions. These effects are not driven by leverage, lifecycle differences, or the criteria used to match public and private firms. Our findings suggest that reputation management is an important driver of layoff policies both at daily frequencies and over the business cycle, and can have significant macroeconomic consequences.

1 Introduction

Voluntary disclosures of bad news by firms are often immediately followed by similar disclosures by other firms. Clustering of bad news is observed in the release of negative earnings announcements, write-downs, or layoff announcements.[1] This paper focuses on layoffs and investigates the mechanisms that influence managers' decisions to cluster their layoff announcements. It also studies the aggregate implications of such decisions, which is relevant for welfare since the timing and quantity of layoffs is tightly linked to unemployment dynamics.

As a case study of clustering in layoff announcements, we consider the behavior of the top three US firms in the banking industry (Bank of America, J.P. Morgan and Citigroup) and the automobile manufacturing industry (G.M., Ford and Chrysler) around the 2001 recession. Figure 1 represents the timelines of layoff announcements for each of these firms from 2000 to 2003. Layoff announcements tend to be clustered within industry: in many cases we observe announcements within the same week. We also observe clustering of announcements across industries.[2] To further investigate the degree of clustering, we apply standard temporal clustering tests to our full sample from 1970 to 2010, and find statistically significant evidence for excess clustering in the time series of layoff announcements. Further, we show that such clustering behavior is observed only in publicly-traded firms ("public" firms), and not in comparable privately-held firms ("private" firms). Motivated by these facts, the central question in this paper is: why do public firms engage in clustering of layoffs, while private firms do not; and what are the aggregate implications of such behavior?

We interpret the observed degree of clustering and the differences between public and private firms through a model based on asymmetric information between managers of firms and the financial market. The central mechanism of the model is as follows. The market perceives a layoff announcement as a negative signal about the manager's ability. When aggregate business conditions are adverse, such as during recessions or industry-wide downturns, the market will attribute greater blame to external factors than to managerial ability. This generates incentives for managers

---

[1] Acharya, DeMarzo, and Kremer (2011) present a model based on asymmetric information that predicts such behavior. Tse and Tucker (2010) provide evidence of clustering of bad news in the form of earnings warnings. Our paper is the first to document herding in layoff announcements.

[2] In the Fall of 2002, Chrysler and Ford announced layoffs on the same day. Similarly, in the Fall of 2003, J.P. Morgan and G.M. announced their layoffs in the same week, and days later Bank of America announced a layoff.

to time their layoff announcements to occur during downturns, thereby minimizing the blame for the bad news. This key idea of our paper is a counterpart to the early paper by Gibbons and Katz (1991), who provide evidence that workers laid off on a discretionary basis are viewed less favorably by the market than are those losing jobs in plant closings. We invoke the same Gibbons-Katz mechanism to illustrate how the cyclicality of layoffs is linked to the lower reputation penalty that managers face in recessions.

The model has two main cross-sectional predictions. First, if managers care more about their reputation (relative to the cash flows of the firm), then they are more likely to engage in layoffs during adverse states. Second, the model also predicts that firms with managers who don't have a long-track record are more likely to engage in layoffs during adverse states. This is because the market's perception of their ability is more sensitive to new information. These predictions of our model apply to both business cycle and daily frequencies. The business cycle frequency results predict differential layoff strategies in recessions, while the daily frequency results are associated with differential propensity in clustering of layoff announcements. We test both mechanisms in turn using two novel datasets.

The first dataset consists of layoff announcements by the largest publicly-listed firms (Fortune 500 constituents) and largest privately-held firms (Forbes 100 constituents), collected from daily issues of the Wall Street Journal between 1970 and 2010. The second dataset, which we are the first researchers to access, consists of confidential microdata on actual mass layoffs from the Bureau of Labor Statistics under their Mass Layoff Statistics program. Since this program collects data from unemployment insurance (UI) claims, it allows us to observe the timing and the exact number of displacements arising from mass layoffs.[3]

To estimate the impact of reputation management on layoff propensity at the business cycle frequency, we focus on differences between public and private firm behavior. This analysis is motivated by certain fundamental differences between public and private firms, which make the public firm managers relatively more likely to manage their reputation in financial markets. First, since public firms sell shares to outside investors who are not involved in managing the firm, there exists separation between ownership and control. Therefore, since the managers of public firms do not completely internalize the costs of adopting inefficient policies, they are more likely to prioritize

---

[3]Because it looks at actual displacements, the BLS data is not subject to reporting bias.

2

reputation management over maximizing firm value. Second, owners of public firms typically have shorter investment horizons: the increased liquidity of public markets makes it easy for shareholders to sell their stock at the first sign of trouble rather than actively monitoring management. This leads to relatively myopic behavior among investors of public firms, which weakens incentives for effective corporate governance (Amar (1993)) and generates incentives for managers to engage in myopic reputation management (Stein (1989)). Third, managers of public firms are subject to takeover threats, which depend on the stock price of targeted firms. This can lead to managerial myopia in public firms in order to actively manage current stock prices (Stein (1988) and Edmans, Goldstein, and Jiang (2011)). Because reputation management focuses on boosting perceptions of short-term performance at the expense of long-run value, myopic managers are more likely to engage in the strategic behavior predicted by our model. Taken together these differences imply that if reputation management drives layoff behavior over the business cycle, the effect should show up as differences in behavior of comparable public and private firms. We emphasize reputation management in *financial markets*, because the managers of both public and private firms are likely to have similar motivations for reputation management among other constituents.[4]

Using a pairwise matching estimator based on size and three-digit industry, we find that the layoff propensity of public firms is twice as sensitive to recessions, relative to their matched private counterparts.[5] In a range of robustness tests, we show that these differences are not driven by public-private differences in lifecycle effects, leverage, workforce size, or on the criteria we use to match public and private firms. Within our sample of public firms, we find that firms predicted to be more strategic by our theoretical analysis, are also the ones more likely to engage in mass layoffs during recessions. Our results therefore suggest that reputation management is an important driver of the observed differences in the cyclicality of layoffs between public and private firms.

Next, we test our model at the daily frequency, and find further support for its predictions. We show that a large firm announcement (i.e. the 20 largest firms based on past year's revenue) is associated with future layoffs by other Fortune 500 firms, but not with past layoffs.[6] We find

---

[4]Notable recent work that explores public-private differences are Asker, Farre-Mensa, and Ljungqvist (2011) and Davis, Haltiwanger, and Jarmin (2006)

[5]That is, in recession months, the propensity to layoff for public firms increases by roughly 5 percentage points, whereas that of private firms increases by roughly 2.5 percentage points.

[6]Our results are related to the work of Gabaix (2011), who examines the role of large firms in explaining aggregate fluctuations.

that this effect is twice as strong if the large firm is in the same industry as the follower firm. For our sample of privately held firms we find no such clustering behavior either before or after the large-firm layoff announcement. Moreover, when we compare the characteristics of firms that lay off in the five days after a large-firm announcement ("followers") to those that lay off in the five days before a large-firm announcement ("counterfactual followers"), we find that follower firms have a greater likelihood to be managed by short-tenured CEOs (i.e. with a tenure between 0 and 4 years) and to place greater reliance on equity-linked compensation for their CEOs. Consistent with our theoretical framework, these results suggest that reputation management is an important driver for the timing of layoff announcements at high frequencies.

Lastly, we establish a connection between high-frequency clustering and layoff behavior at business cycle frequencies. We find that the follower firms are roughly 3 percentage points more likely to engage in a mass layoff during a recession month, compared to counterfactual follower firms. This link between the daily frequency reputation management and the cyclicality of layoffs over the business cycle provides significant evidence that reputation concerns are an important driver of firms' layoff policies.

To rule out the role of alternate theories in driving our results, we conduct a series of additional tests. The key alternate explanations we consider are: common shocks, learning from other managers, compassionate CEOs, and market inattention. While each of these mechanisms may contribute to some of the patterns we observe in layoff behavior, no combination of these effects can explain the full range of our results. Taken together, the findings of this paper suggest that managerial behavior not only has costs for the individual firm, but also has significant aggregate implications at business cycle frequencies.

The remainder of the paper is structured as follows: Section 2 presents statistical tests for detecting excess clustering in layoff announcements. Section 3 presents the theoretical model. In Section 4 we describe the construction of our two main datasets. Section 5 presents the empirical methodology and business-cycle frequency tests of the model, while Section 6 presents daily-frequency tests. In Section 7 we link the daily-frequency results to the business-cycle frequency results. Section 8 discusses alternate explanations of layoff behavior, and Section 9 concludes.

2 Statistical Evidence of Clustering

As illustrated in the case study (Figure 1), we observe that layoff announcements are often clustered within days of each other. The size of these clusters ranges from within a day to over two weeks. Though this is suggestive evidence of excess clustering, it is not clear whether this observation represents a general trend which applies to other periods and other firms in the economy. In this section, we take a systematic approach to identify and characterize the nature of excess clustering in firms' layoff announcements.

Our approach uses a measure called the scan statistic, which is used to detect unusual clusters in a sequence of events that occur over time. The approach is known as "moving window analysis" in the engineering literature (see Glaz, Naus, and Wallenstein (2001)). To see how it works, consider N events that occur on an unit interval. First, consider the number of occurrences in each window of size w. Then consider the maximum of these over all windows of size w in the unit interval. Under the null of a uniform distribution, the distribution of this maximum can be calculated. For a confidence level, e.g. $\alpha = 0.05$, we can then construct a critical value, c_α, such that $\Pr[S_w > c_\alpha] = \alpha$. Here S_w denotes the largest number of events to be found in any subinterval of $[0,1]$ of length w, and is called the scan statistic. If the maximum observed local statistic, S_w, is larger than or equal to c_α, then we should reject the null hypothesis and infer existence of a local region with a statistically significant cluster.

The distribution of scan statistic described above is a function of two parameters: the size of the subwindow, w (relative to the size of the entire interval); and the number of events, N, which occur in the entire interval $[0,1]$. We denote the p-value of this test as $\Pr[k; N, w]$. This p-value should be interpreted as follows: *under the null of N events independently drawn from the uniform distribution on $[0,1]$, $\Pr[k; N, w]$ is the probability that we observe k or more events in any subwindow of size w.*[7]

The unit of time for our tests is business days. We conduct our tests for two different interval sizes: 60 business days (approximately one quarter) and 20 business days (approximately one

---

[7]Under the null hypothesis, the probability of observing a scan statistic, S_w, greater than k, can be characterized as a function of the two parameters:
$$\Pr[S_w \geq k] \equiv \Pr[k; N, w]$$
Exact estimates of this common probability exists for certain cases, and researchers have to rely on approximations for the other cases. See chapter 10 of Glaz, Naus, and Wallenstein (2001).

month). Our sample period begins in 1970 and ends in 2010, and we run separate tests for every non-overlapping window in this period of 41 years. Since the test has low power when N is small, we exclude months in which we observe fewer than 5 layoff announcements.[8] Also, we run the tests for two categories: all industries combined; and the manufacturing industry. This allows us to assess whether there is excess clustering at both the aggregate and the industry level.

[INSERT FIGURE 2 HERE]

Figure 2 plots the results of our analysis for each non-overlapping interval of 60 business-days. To facilitate viewing, we present the negative log of the p-value as our y-axis variable. Therefore, a higher value suggests that we can reject the null with greater confidence. Though this sequence of individual tests is suggestive of several episodes of clustering, we would like to combine the results from these different independent observations into a single statistical test. For this, we rely on Fisher's method to combine the p-values from our tests into a single statistic, using the formula $X^2 = -2 \sum_{i=1}^{k} \log{(p_i)}$, where p_i is the p-value for the i-th independent test. This 'combined p-value' is reported in the last column of Appendix Table 1. The main conclusion of this analysis is that we can reject the null of no excess clustering for subwindows of 5 or more days using one-month intervals, and for subwindows as small as three days using quarterly intervals. Having established the existence of excess clustering, we proceed to offer a potential explanation for this phenomenon in Section 3, where we present our theoretical model.

3 Model

In this section, we present a reputation-based model of management layoff decisions, focusing on the tradeoff between firm profits and the perception of managerial talent. A similar model was presented in Rajan (1994), which studies the clustering of credit policies by banks. We focus on a three-period version of the model in the main text, incorporating fully rational Bayesian expectations and solving for the set of trembling-hand perfect equilibria. We discuss the implications of relaxing these assumptions and expand to multiple periods in the theoretical appendix.

---

[8]Our results are do not change when we include all the months in our analysis.

3.1 Setup

Our model starts at date 0, and ends at date 2. There is no discounting between periods. There are two types of agents in this model: firm managers and the market.[9] Managers care about the profits of their firm, and about their reputation with the market. The market takes no direct action in this model, but simply observes the actions of managers, and updates its priors according to Bayes' Rule.

Firm managers are the primary decision-making agents in this model. Each manager i is associated with a firm which, at date 0, begins a new project and hires one unit of labor to engage in production. There is continuum of manager types, which differ along the dimension of managerial talent, denoted by η_i. The only restriction on the distribution of talent, described by density $f(\eta_i)$, is that its support be within the unit interval, [0,1]. For convenience, we define the mean and variance of the distribution of talent to be μ and σ_η^2, respectively.

After the project is undertaken, the aggregate economic state is realized at date 1.[10] The aggregate state is denoted by $s \in S = \{N, A\}$: it can be adverse (A) with probability π, or normal (N) with probability $1 - \pi$.

The probability that a project succeeds depends on both the talent of the manager and the aggregate state, and is given by:

$$\theta_s^i = \eta_i \lambda_s \tag{1}$$

such that $\lambda_N = 1$, and $\lambda_A = 1 - \delta$. In the adverse state the probability of project success is $\eta_i(1-\delta)$, and in normal states it is η_i. There is symmetric uncertainty about the aggregate state for both managers and the market throughout all time periods.[11]

The manager privately observes the outcome of his firm's project at date 1. If the project was successful, there is no decision to make: the project continues into date 2, where it generates

---

[9]There are many possible interpretations for the role of the market. One possibility is the population of equity market investors - this interpretation links our model to concerns about stock price responses to layoffs. Another option is the demand side of the market for managerial talent - this interpretation is more in line with the literature on career concerns (Holmström (1999)). We do not pin down a specific interpretation in order to allow for the broadest possible application of the model.

[10]We can interpret the aggregate economic state as either an economy-wide indicator, or a measure of the health of a particular sector.

[11]The results of this model do not rely on the particular functional form assumed here for the probability of success. The key comparative statics are identical if instead of a multiplicative function we assume an additive function: $\theta_s^i = \alpha \eta_i + (1 - \alpha) \lambda_s$, for $0 < \alpha < 1$.

earnings of $1, and then ends. If the project is not successful, the manager has to decide whether to terminate or continue the project. Termination involves firing the labor force hired at time 1, and is therefore fully observable. The firm's date 2 earnings are zero if it terminates an unsuccessful project at date 1. We label this approach as the "*terminate*" policy.

Instead of termination, the firm can hide the unsuccessful outcome of the project from the market by not laying off the labor force assigned to the project. If the manager adopts such a policy, he must pay the worker for one more period even though the worker will not be productive. We denote this cost as C. Relative to the *terminate* policy, this decision delays the end of the project by one period. We therefore label this approach as the "*delay*" policy.

We assume that adopting the *delay* policy is costly relative to the decision to *terminate*. This is given by *Assumption 1:* $C > 0$. This assumption implies that in a first-best world, the *delay* policy should not be adopted.

Despite its inefficiency, managers have an incentive to adopt the *delay* policy because it is better for their reputation to hide an unsuccessful project outcome. In setting up the maximization problem for managers, let $D \in \{0, 1\}$ represent whether or not a given manager adopts the *delay* policy when his firm's project fails. We can then describe managers' preferences by the following utility function:

$$\max_D U_i = -DC + \gamma E_{mkt}\left[\eta_i | \hat{D}, \text{Layoffs}\right] \tag{2}$$

such that γ is the utility weight the management places on his reputation in the eyes of the market, and $\hat{D}$ is the conjecture of the manager's strategy that the market uses to interpret the observation of layoffs or no layoffs.

3.2 Reputation and Updating Rules

The market's updating rule depends on 2 factors: a) its conjecture about the manager's strategy in addressing a failed project; and b) whether or not it observes layoffs. We begin our equilibrium analysis by focusing on four primary cases:

	No Layoffs	Layoffs
Market conjecture: 'Terminate'	$E_{mkt}\left[\eta_i | \hat{D} = 0, \text{Layoffs} = 0\right]$	$E_{mkt}\left[\eta_i | \hat{D} = 0, \text{Layoffs} = 1\right]$
Market conjecture: 'Delay'	$E_{mkt}\left[\eta_i | \hat{D} = 1, \text{Layoffs} = 0\right]$	$E_{mkt}\left[\eta_i | \hat{D} = 1, \text{Layoffs} = 1\right]$

When the market conjectures that the firm will adopt the *Terminate* policy

If the market believes the firm is going to adopt the *terminate* policy, i.e. $\hat{D} = 0$, then the firm will lay off the project's workers upon failure, and not lay off workers when the project succeeds. Therefore, not observing layoffs implies that the firm's project has succeeded. This makes the updating rule straightforward: the observation of layoffs or no layoffs is perfectly correlated with the outcome of the project.

Using Bayes Rule we can calculate the resulting posteriors as:

$$
\begin{aligned}
E_{mkt}\left[\eta_i | \hat{D} = 0, \text{Layoffs} = 0\right] &= E_{mkt}\left[\eta_i | \text{Project Succeeds}\right] = \mu + \frac{\sigma_\eta^2}{\mu} \\
E_{mkt}\left[\eta_i | \hat{D} = 0, \text{Layoffs} = 1\right] &= E_{mkt}\left[\eta_i | \text{Project Fails}\right] = \mu - \frac{\sigma_\eta^2 \left(1 - \pi\delta\right)}{1 - \mu\left(1 - \pi\delta\right)}
\end{aligned}
\tag{3}
$$

(Proof in Appendix A.1)

To interpret these results, note that the market's prior about managerial talent is given by μ. When the market observes no layoffs, they update their beliefs about managerial talent positively, which is reflected by the positive additive term in the first equation. Analogously, when the market observes a layoff, then they update their beliefs negatively, which is reflected by the negative additive term in the second equation. The weight of the additive terms depend positively on σ_η^2 since it measures how noisy was the market's prior was at the start of the period. The second equation also depends negatively on the probability of being in an adverse aggregate state (π). Therefore, when π is high, the reputation penalty of laying off is lower.

When the market conjectures that the firm will adopt the *Delay* policy

We get an analogous updating rule for the case in which the market believes the firm is going to adopt the *delay* policy, i.e. $\hat{D} = 1$. Under standard equilibrium assumptions, the outcome of layoffs under this policy will never occur, and the Bayesian posterior would not be uniquely determined. We therefore introduce trembles and focus on trembling-hand-perfection as our equilibrium concept. Because successful projects continue automatically, they do not require any action from the manager and are not susceptible to trembles. By contrast, the decision to adopt the *delay* policy requires a direct action by the manager, who could tremble and choose to terminate the project instead. Therefore, whenever the firm engages in layoffs the market knows the project must have failed, even though this outcome will (almost) never be observed in equilibrium. The updating rule in this

situation can be calculated as follows:

$$E_{mkt}\left[\eta_i|\hat{D}=1,\text{Layoffs}=1\right] = E_{mkt}\left[\eta_i|\text{Project Fails}\right] = \mu - \frac{\sigma_\eta^2\left(1-\pi\delta\right)}{1-\mu\left(1-\pi\delta\right)} \quad (4)$$

Once again when the market observes a layoff they update their beliefs about managerial talent negatively. By contrast, when the market observes no layoffs, they do not know whether the project failed or not. This is because the firm is expected to adopt the *delay* policy of no layoffs irrespective of project outcomes. Therefore, when the market observes no layoffs, they get no new information about the firm, and the updating rule is simply:

$$E_{mkt}\left[\eta_i|\hat{D}=1,\text{Layoffs}=0\right] = E_0\left[\eta_i\right] = \mu \quad (5)$$

(Proof in Appendix A.2)

3.3 Equilibrium Selection

In equilibrium the market conjecture about the manager's policy must be correct, and hence $\hat{D}=D_i$.

To support the equilibrium where the manager always adopts the *terminate* policy, the following incentive compatibility condition must hold:

$$\gamma E_{mkt}\left[\eta_i|\hat{D}=0,\text{Layoffs}=1\right] \geq -C + \gamma E_{mkt}\left[\eta_i|\hat{D}=0,\text{Layoffs}=0\right] \quad (6)$$

By contrast, to support the equilibrium where the manager always adopts the *delay* policy, the IC constraint is:

$$\gamma E_{mkt}\left[\eta_i|\hat{D}=1,\text{Layoffs}=1\right] \leq -C + \gamma E_{mkt}\left[\eta_i|\hat{D}=1,\text{Layoffs}=0\right] \quad (7)$$

Using the Bayesian analysis in the previous section, the above constraints, respectively, simplify

to:

$$C \geq \gamma\sigma_\eta^2 \left[\frac{1}{\mu} + \frac{1 - \pi\delta}{1 - \mu\left(1 - \pi\delta\right)} \right] \tag{8}$$

and

$$C \leq \gamma\sigma_\eta^2 \left[\frac{1 - \pi\delta}{1 - \mu\left(1 - \pi\delta\right)} \right] \tag{9}$$

From the above constraints, it is clear that for sufficiently high values of γ and σ_η^2, managers will choose to always adopt the *delay* policy. At the same time, for sufficiently low values of these variables, managers will always choose to adopt the *terminate* policy. Having already characterized these equilibria, we now move to consider the intermediate set of parameter values, which support neither pure-strategy equilibrium.

For intermediate values of γ and σ_η^2, the equilibrium reputation penalty under the *terminate* policy is so large that managers prefer the *delay* policy, and the equilibrium reputation penalty under the delay policy is so small that they prefer the *terminate* policy. This means that for these parameter values, there is no equilibrium in pure strategies. We therefore proceed to analyze a mixed-strategy equilibrium, where managers randomize between adopting the *terminate* and *delay* policies.

As in the previous section, we begin our characterization of the mixed-strategy equilibrium by focusing on the market's posterior following an observation of either layoffs or no layoffs. In this case, instead of a binary conjecture about the policy of the manager, we move to a continuous conjecture $\hat{D} \in (0, 1)$ which corresponds to the probability with which the market expects the manager to choose the *delay* policy, conditional on project failure. In such a setting, we calculate the market's posteriors as:

$$E_{mkt}\left[\eta_i | \hat{D}, \text{Layoffs} = 0 \right] = \mu + \frac{(1 - \hat{D})(1 - \pi\delta)\sigma_\eta^2}{\hat{D} + (1 - \hat{D})(1 - \pi\delta)\mu} \tag{10}$$

and

$$E_{mkt}\left[\eta_i | \hat{D}, \text{Layoffs} = 1 \right] = \mu - \frac{\sigma_\eta^2 (1 - \pi\delta)}{1 - \mu(1 - \pi\delta)}$$

(Proof in Appendix A.3)

Note that the above posteriors match up with pure-strategy beliefs when we take the limits as

$\hat{D} \to 0$ and $\hat{D} \to 1$ for the cases of the *terminate* and *delay* policies, respectively.

To complete the characterization of the mixed-strategy equilibrium, we need the manager to be indifferent between the two strategies available to him. Using the same IC constraint framework as in the pure-strategy case, we need:

$$\gamma E_{mkt} \left[\eta_i | \hat{D}, \text{Layoffs} = 1 \right] = -C + \gamma E_{mkt} \left[\eta_i | \hat{D}, \text{Layoffs} = 0 \right] \tag{11}$$

Using the posterior market beliefs for the mixed-strategy case, the above constraint simplifies to:

$$C = \gamma \sigma_\eta^2 \left[\frac{(1 - \hat{D})(1 - \pi \delta)}{\hat{D} + (1 - \hat{D})(1 - \pi \delta)\mu} + \frac{1 - \pi \delta}{1 - \mu(1 - \pi \delta)} \right] \tag{12}$$

The equation above allows us to solve for the manager's randomization probabilities in the mixed-strategy equilibrium. Note that because the RHS is monotonically decreasing in $\hat{D}$, there is a unique set of mixing probabilities that supports equilibrium play for any given set of parameter values. Further, because the limits of the expression match up to the pure-strategy equilibria described in the previous section, there is a continuous progression from always choosing the *terminate* policy, through a mix of both options, and finally to always choosing the *delay* policy, as the product of γ and σ_η^2 increases from zero.

3.4 Equilibrium Implications

The results of the previous section describe conditions under which the firm will undertake the *delay* policy, despite the inefficient reduction in earnings that result from it. To gain an insight into these central results, Figure 3 plots the equilibrium policies of managers based on their values of γ (degree of reputational concerns) and σ_η^2 (variance of market's prior about firm). Managers who adopt the *delay* policy will lie above and to the right of mixed-strategy region. In this region, managers and their firms will delay project termination and avoid layoffs, despite their project failing.

[INSERT FIGURE 3 HERE]

Based on this analysis, we can conclude that the firm has an incentive to undertake the *delay* policy when:

- the manager places a high weight on reputation (as measured by γ)

- layoffs are particularly informative about the manager's ability, due to significant uncertainty in the market's prior beliefs (i.e. a high value of σ_η^2).

The above implications have direct links to observable variables in empirical corporate finance. A high value of γ is likely to be associated with firms that incentivize their management with high-powered, market-based compensation packages. As for the informativeness of layoffs, a high value of σ_η^2 (a sufficient statistic for the signal-to-noise ratio in our model) is likely to be associated with firms that have a new management, precisely because the market will have less information about them, and any action taken by them will be relatively more informative.

An interesting implication of the model is the effect of changing beliefs about the aggregate state S, where the expectation of an adverse state is measured by π. Figure 4 plots the same boundaries as Figure 3, and adds another set of boundaries to demonstrate the effect of the market's perception about aggregate state becoming pessimistic. Assuming that this perception is justified, there will be a direct effect of fewer successful projects in an adverse economic environment. Because of this, the market is less likely to attribute the negative signal of a layoff to the manager's level of talent, and consequently the reputational concern associated with layoffs diminishes. This is illustrated by the rightward shift of the boundaries in Figure 4. As a result, the parameter region for which firms will adopt the *delay* policy shrinks. More firms now will choose to announce layoffs if their project fails.

[INSERT FIGURE 4 HERE]

In addition to the direct effect of adverse economic conditions leading to higher rates of project failure, our model also predicts a shift-in-equilibrium effect: conditional on project failure, a larger fraction of managers will choose to terminate their projects and engage in layoffs during these economic downturns.[12] In the pure-strategy regions of the parameter space, there is no shift in equilibrium because managers are effectively at a corner solution. Those who strictly prefer the *delay* policy will continue to have a layoff rate of precisely zero, while those who strictly prefer

---

[12] In the multi-period model discussed in the appendix, we show that firms with failed projects are likely to continue them until the next economic downturn, effectively saving their layoffs until they can implement them without suffering the normal reputational penalty.

the *terminate* policy will have a layoff rate equal to their project failure rate: $1 - \eta(1 - \pi\delta)$, or $1 - \mu(1 - \pi\delta)$ on average. The interesting case is that of managers in the mixed-strategy region. We describe their layoff rate in the following proposition:[13]

PROPOSITION 1. For managers in the mixed-strategy region of the parameter space, an increase in the expected probability of a downturn leads to a strictly higher rate of layoffs. Specifically:

$$\frac{\partial \Pr[\text{Layoffs} \mid \pi, \gamma, \sigma_\eta^2; C, \delta]}{\partial \pi} = \frac{\delta\gamma\sigma_\eta^2}{C\left[1 - \mu(1 - \pi\delta)\right]^2} > 0$$

PROOF. See appendix A.4. ∎

From this result, it follows that managers who care most about reputation (i.e. have a high value of γ) and who have a short track record (i.e. a high value of σ_η^2) are most likely to be affected by the adverse shift in market's perception of the aggregate state. These insights are summarized in the following two corollaries:

COROLLARY 2. If the market's belief about a firm's management is less precise, then the manager is more likely to announce layoffs in downturns. That is, $\frac{\partial^2 \Pr[\text{Layoff} \mid \pi, \gamma, \sigma_\eta^2; C, \delta]}{\partial\pi\partial\sigma_\eta^2} \geq 0$.

COROLLARY 3. If the manager's utility function puts more weight on his reputation, then the manager is more likely to announce layoffs in downturns. That is, $\frac{\partial^2 \Pr[\text{Layoff} \mid \pi, \gamma, \sigma_\eta^2; C, \delta]}{\partial\pi\partial\gamma} \geq 0$.

3.5 Extending the Model: Impact of a Large Firm

In this section we extend the model presented above by introducing a large firm into the economy. The notion of large here is that the firm's performance contains information about the aggregate state of the industry. By contrast, the performance of small firms is heavily influenced by conditions in their local market, so the ability to obtain information about the aggregate state of the industry from the performance of a small firm is assumed to be negligible. As a result, the large firm's layoffs decision will influence the other firms in the industry through the information it provides about the aggregate state about the industry. As shown in Propositions 1 and 2, the

---

[13]While Proposition 1 and the two corollaries below and the rest of the analysis focus on changes in the probability of experiencing an adverse aggregate state, similar results obtain when considering an increase in the severity of the adverse state, represented by the magnitude of δ.

market's beliefs about the aggregate state have a strong influence on firms' layoff decisions. With the addition of a large firm, the model therefore generates strategic interaction between firms.

In formulating this extension of the model, we move from a general-purpose metric of beliefs about the aggregate state, π, to a firm-specific metric represented by π_i. Here it's more appropriate to interpret π_i as the probability of an adverse economic state in the industry or local market of small firm i. Specifically, it combines the outcome of the aggregate state s_{agg} with firm-specific conditions described by ε_i:

$$\pi_i = f(1 \cdot (s_{agg} = A) + \varepsilon_i)$$

and we restrict $f(\cdot)$ to be a monotonically increasing function with a support equal to the interval $[0,1]$.[14] For simplicity, we assume that the project outcome for the large firm is directly dependent on s_{agg} as before, with the prior probability of an adverse aggregate state measured by π_0. For the small firm, an adverse aggregate state increases the chances that the firm-specific economic state will also be adverse, but does not predict this perfectly. We maintain the same framework of observability as before, where prior distributions are common knowledge, but only the layoff decision is observed by the market.

The market updates the smaller firms' reputation in two steps. First, it updates its prior on the realization of s_{agg}, using the large firm's layoff decision. This, in turn, leads to an updated belief about π_i for the small firm, in turn impacting the reputational penalty the small firm would face if it announced layoffs of its own.

Using Bayes' Rule, the process of updating expectations about s_{agg} using the layoff decision of the large firm is straightforward. Letting π_0 be the prior expectation of the adverse state, the posterior expectation conditional on observing layoffs by the large firm, $\pi_1 \equiv \Pr[s_{agg} = A|\text{Large Firm Layoff} = 1]$ is given by:

$$\pi_1 = \pi_0 \left(\frac{1 - \eta(1 - \delta)}{1 - \eta(1 - \pi_0\delta)} \right) > \pi_0 \tag{13}$$

Note that the above updating rule is true for all possible strategies employed by the large firm, as long as we allow for trembles in the case where the large firm would like to always choose the

---

[14]While we do not specify a functional form, the most widely-used options include the logistic function and the probit, or normal quantile function.

delay policy. Moreover, while the market does not know the value of η for the large firm, taking expectations over any prior distribution leads to the same conclusion: $\pi_1 > \pi_0$. Thus, whenever the market observes layoffs by the large firm, its posterior beliefs imply that there is a higher chance that the aggregate state is adverse. This, in turn, increases the likelihood that the firm-specific economic state s_i will be adverse for the small firm. As a result, layoffs by the large firm lead to an increase in π_i as the market prepares to observe the action of the small firm.

Combining this result with the analysis in Figures 3 and 4, it follows that following a layoff by the large firm, the small firm will be more likely to choose the *terminate* policy. Intuitively, the layoff by the large firm means the market will be more willing to attribute poor performance to an adverse state rather than a lack of managerial talent, making further layoffs more likely. In effect, our model predicts a clustering or "safety in numbers" effect, where some firms will strategically announce layoffs close to the announcements of other firms, in groups in order to minimize the reputational costs they incur. In particular, we expect that firms whose characteristics normally push them toward the *delay* policy will be followers in such situations, announcing layoffs in the wake of firms whose characteristics push them toward the *terminate* policy. The following proposition summarizes this insight:

PROPOSITION 4. Firms tend to cluster layoff announcements after layoffs by a large firm. That is,
$$\frac{\Delta \Pr\left[\text{Layoffs}_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2\, C, \delta\right]}{\Delta(\text{Large Firm Layoff})} \geq 0.$$

PROOF. See appendix A.5. ■

The mechanism described above can also occur in response to economic news that signals a deterioration of firm performance. Consequently, we expect that adverse aggregate news, correlated with real firm performance, will also trigger clustering of layoff announcements. This gives us the following corollary:

COROLLARY 5. Firms cluster layoff announcements after negative macroeconomic news. The strength of the effect depends on the predictive power of the negative news with respect to firm performance. Specifically,

$$\frac{\Delta \Pr\left[\text{Layoffs}_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2\, C, \delta\right]}{\Delta\,(\text{Adverse Macro Event})} \geq 0$$

and

$$\partial \left[\frac{\Delta \Pr\left[\text{Layoffs}_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2\, C, \delta\right]}{\Delta\left(\text{Adverse Macro Event}\right)} \right] \Big/ \partial \left[\frac{\Delta pi_i}{\Delta\left(\text{Adverse Macro Event}\right)} \right] \geq 0$$

PROOF. See appendix A.6. ∎

The key message of this analysis is that after a large firm layoff (or release of negative macro-economics news), perceived probability of industry downturn (π_i) increases. This in turn leads to an increase in the layoff propensity of other firms. Since other firms' layoff propensity increases simultaneously, they all tend to lay off at the same time, leading to clustering. Therefore clustering in this model is not driven by the desire of firms to lay off close to other firms. Instead, it is driven by an aggregate shock (layoff of a large firm in the same industry, or other types of common bad news).

Extending the analysis further, we turn to the types of firms which are more likely to cluster their layoffs in response to shocks such as layoffs by large firms and adverse macroeconomic shocks. Similar to the results of proposition 1 and 2, we find with strong reputation-based incentives and shorter track records are most likely to engage in layoff clustering. The following two corollaries summarizes these results:

COROLLARY 6. If there is significant uncertainty about the manager's talent, then he is more likely to cluster layoff announcements after layoffs by a large firm: $\frac{\partial}{\partial \sigma_\eta^2} \left[\frac{\Delta \Pr\left[\text{Layoffs}_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2\, C, \delta\right]}{\Delta(\text{Large Firm Layoff})} \right] \geq 0.$

PROOF. See appendix A.6. ∎

COROLLARY 7. If the manager's utility function puts more weight on his reputation then he is more likely to cluster layoff announcements after leader layoff. That is $\frac{\partial}{\partial \gamma} \left[\frac{\Delta \Pr\left[\text{Layoffs}_i = 1 \mid \pi_i, \gamma, \sigma_\eta^2\, C, \delta\right]}{\Delta(\text{Large Firm Layoff})} \right] \geq 0.$

PROOF. See appendix A.6. ∎

The propositions and corollaries in Section 3 summarize the testable predictions of the model. We now move to Section 4, which discusses how we link the parameters of the model to measurable attributes of firms and managers. With respect to the empirical tests, proposition 2 and its associated corollaries deal with leader-follower behavior, which we test using high-frequency data over short time horizons. By contrast, we test propositions 1 and its corollaries using lower-frequency data over the course of the business cycle.

3.6 Mapping the Model to the Data

The model presented in this section is a static three-period model. Therefore, in order to test the model's predictions we need to specify the appropriate time horizon. In principle, the time horizon depends on the persistence of beliefs about the economic state, and the corresponding persistence of reduced reputational costs to layoffs. Thus, to guide our empirical tests we choose the appropriate time horizon for our tests based on the frequency at which market's belief about the aggregate state of the economy changes. As summarized in the propositions above, the key comparative statics involve change in manager's behavior after the release of *adverse aggregate* news. Guided by this principle, our empirical tests are conducted at two frequencies: business cycle frequencies and daily frequencies. The timescale of business cycles is a natural candidate because market participants are much more pessimistic about the aggregate states during recessions relative to booms. Similarly, testing the predictions at daily frequencies is informative, since release of unexpected bad news by a leading firm in an industry often drastically changes market's beliefs about the state of the industry in a matter of hours. The next section describes how we construct our datasets, and then turns to the empirical strategies and results for both the business cycle frequency tests (Section 5), and the daily frequency tests (Section 6).

4 Data Construction

4.1 Constructing Dataset of Layoff Announcements and Firm Characteristics

The data for this study are based on two sets of firms: large publicly-listed firms, and large privately-held firms. The publicly-listed firms are the population of firms in the annual Fortune 500 from 1970 to 2010. Analogously, the privately-held firms are the population of firms in the Forbes annual list of largest 100 privately held firms ("Forbes 100") from 1985 to 2010. To minimize selection bias, we restrict the sample in any given year to the subset of firms that are contemporaneously constituents of the Fortune 500 or Forbes 100 in that year.[15] Over the relevant range of years, we track 1013 different publicly-listed firms and 436 privately-held firms at an annual level. With this framework, we track announcements of layoffs by these firms in the Wall Street

---

[15]However, conducting our empirical analyses on the entire sample of firms that were *ever* in the Fortune 500 or Forbes 100 does not alter our key results.

Journal, the definitive source of news for large US-based firms. For the publicly-held firms data from 1970 to 2006 comes from Kevin Hallock.[16] Using the same methodology as Hallock (2009), we extended this dataset to 2010[17], and independently constructed a dataset of layoff announcements for the private firms from 1985 to 2010 (see data appendix for more details).

From the Wall Street Journal announcement dataset, we focus not only on the number of layoffs by a particular firm in a given year, but also track the total number of workers laid off. We then match our firms to four of COMPUSTAT's datasets: Prices, Dividends, and Earnings; Fundamentals Annual, Fundamentals Quarterly, and ExecuComp. From the Prices, Earnings, and Dividends dataset, we obtain a firm's NAICS code, as well as information on its annual earnings and its equity: shares outstanding, market and book values, and dividends. From Fundamentals Annual, we obtain firm employment numbers and information from balance sheets and income statements: measures of debt, revenues, income, and capital expenditures. From Fundamentals Quarterly, we obtain date of earnings announcements, which serves as an important control variable in some of our empirical tests.

The data from ExecuComp is limited by the fact that it starts in 1992; however, it provides valuable information on the tenure and compensation of the CEOs of firms in our sample. We supplement this dataset with information from the Forbes CEO Compensation list of the largest 500 firms from 1970 to 1991. This allows us to construct measures of CEO tenure over several decades for a large subset of firms. This is critical for some of our empirical tests involving CEO tenure, as it allows us to include firm-fixed effects to examine within-firm variation.

In addition to these firm-specific measures, we also obtain sector-level data from the BLS Current Employment Statistics National Survey covering employment levels and number of hours worked, and measures of value-added from the National Income and Product Accounts of BEA, decomposed by NAICS major industry groups. We also obtain daily stock market returns from the CRSP database for the entire sample period, 1970 to 2010.[18]

---

[16] For related research using this data see Billger and Hallock (2005) and Farber and Hallock (2009). Also, Hallock (2009) provides an interesting discussion of other aspects of this dataset which we do not explore.

[17] There are several approaches to conducting searches on historical news database. In consultation with Kevin Hallock we narrowed the search critieria to three different methods. Using the three criteria we re-constructed the dataset for the publicly-held firms for three random years in the period 1970 and 2006. To ensure consistency we settled on the search criteria that yielded the maximum amount of overlap between the two datasets.

[18] The data appendix goes into more detail about our methodology and procedures for constructing and merging the different datasets.

With these data, we first construct a range of standard control variables in order to cover a wide range of standard predictors of firm behavior. Specifically, the following variables are constructed based on firms' annual earnings reports covering the year prior to the layoff announcements being analyzed. We begin with the standard measures investors use to categorize companies into groups: firm size and value vs. growth. For the former, we include both the traditional market capitalization measure, as well as a measure of total firm value which combines equity market capitalization with the firm's long-term debt obligations. For the latter, we use both the ratio of equity book value to equity market value, as well as the earnings to price ratio for the firm's stock. In addition to these, we include a measure of financial leverage, equal to the ratio of the value of long-term debt obligations to the sum those obligations and the firm's equity. We also construct a measure of firm maturity as measured by years since initial public offering (IPO) date.

To test the propositions outlined in the theory section we construct two different datasets. In the first dataset each firm is tracked annually (Annual dataset), and in the second each firm is tracked every business day (Business Day dataset). Out of 5569 layoff announcements we only find two to be announced in the Weekend edition of the Wall Street Journal. Consequently, the Business Day Histories is at the business day level rather than the calendar day level.[19]

4.2 Confidential Microdata from the Mass Layoff Statistics Program of BLS

The Mass Layoff Statistics program (MLS) of the Bureau of Labor Statistics (BLS) is a Federal-State cooperative statistical effort which uses a standardized, automated approach to identify, describe, and track the effects of major job cutbacks, using data from each State's unemployment insurance database. Establishments which have at least 50 initial claims for unemployment insurance (UI) filed against them during a consecutive 5-week period are contacted by State agencies to determine whether the claimants are facing separations of at least 31 days duration, and, if so, information is obtained on the total number of separations, the reasons for these separations, and recall expectations. Establishments are identified according to industry classification and location, and unemployment insurance claimants are identified by demographic characteristics including age, race, sex, ethnic group, and place of residence. The data is collected at a monthly frequency starting

---

[19]These two layoff announcements were recoded as occuring on the following Monday. Our results are identical when we drop these two observations.

in April of 1995. We end our sample in December 2010.

According the MLS definitions, a mass layoff occurs when at least 50 initial claims are filed against an establishment during a consecutive 5-week period. An extended mass layoff occurs when at least 50 initial claims are filed against an establishment during a consecutive 5-week period and at least 50 workers have been separated from jobs for more than 30 days. Since extended mass layoffs are a better measure of layoffs that lead to more permanent job dislocation (greater than 30 days), we focus on this measure in our analysis.

Our focus on the subset of establishments employing 50 or more workers means that, according to the 2003 data, 4.6 percent of all covered employers and 56.7 percent of covered employment are in the program's scope.[20] This measure has been quite stable over time: more than two decades ago, 5 percent of employers and 61 percent of total employment were reported in establishments with 50 or more workers (Brown (2004)).

The Bureau of Labor Statistics keeps the identity of companies that engage in mass layoffs confidential. Under the auspices of the onsite researcher program of BLS, we were able to access the confidential microdata, which allowed us to extend our empirical analysis to actual mass layoffs, in addition to the layoff announcement observations based on the Wall Street Journal data. Five state employment offices, however, rejected our proposal to access the confidential data citing state legislation that disallows them to share the identity of establishments even for research purposes. Nevertheless, the researchers at the BLS estimate that the confidential data that was accessible to us covered more than 85% of all the mass layoff events they track. Having access to actual mass layoffs data allows us to examine the degree to which strategic behavior by firms can lead to actual changes in the labor market outcomes.

5 Business Cycle Frequency Tests

One of the main predictions of the model presented in Section 3 is that firms with managers who care most strongly about reputation are more likely to engage in layoffs during downturns (proposition 2 and 3). In an ideal experiment, we would estimate the magnitude of this effect using two identical firms, such that the manager of one firm has incentives to manage reputation while

---

[20]The large difference in percentages reflects the strongly right-skewed distribution of employer size, where a relatively small fraction of establishments provide a majority of jobs.

the other does not. In the absence of such an experiment, we exploit differences in the incentives faced by publicly-listed firms ("public" firms) and privately-held firms ("private" firms). Public firms differ from private firms along three major margins, all of which make their managers more likely to manage their reputation in financial markets, relative to a similar private firm. First, since public firms sell shares to outside investors who are not involved in managing the firm, there exists separation between ownership and control. This may lead to agency problems if managers' interests diverge from those of their investors (Jensen and Meckling (1976)). Second, owners of public firms typically have shorter horizons, since liquidity makes it easy for shareholders to sell their stock at the first sign of trouble rather than actively monitoring management. This relative myopic behavior of investors, weakens incentives for effective corporate governance (Amar (1993)), and generates incentives for managers to be myopic in their reputation management (Stein (1989)). Third, managers of public firms are subject to takeover threats, which are, in part, dependent on the stock price of targeted firms. This can lead to managerial myopia in public firms in order to actively manage current stock prices (Stein (1988) and Edmans, Goldstein, and Jiang (2011)).[21]

If reputation management drives layoff behavior over the business cycle, the first-order effect should show up when comparing differences in behavior of *similar* public and private firms. Here we emphasize reputation management in *financial markets*, since the managers of public and private firms are likely to have similar motivations for reputation management among other constituents. The next section describes the empirical strategy and data samples we use for our tests.

5.1. Comparing Public-Private Firms: Empirical Strategy

The analysis of this section is based on the confidential microdata collected at a monthly frequency from April 1995 to December 2010 by the Mass Layoff Statistics program of the Bureau of Labor Statistics. The dataset includes firms that ever engaged in a layoff during the sample period. Using this data we create three different samples for our study.

5.1.1 Full Sample. — The construction of our full sample for this portion of our analysis begins with all public firms that are Fortune 500 constituents between 1985 and 2010; and all

---

[21] Private firms, in contrast, are often owner-managed and even when not, are both illiquid and typically have highly concentrated ownership, which encourages their owners to monitor management more closely. Indeed, evidence from the Federal Reserve's 2003 Survey of Small Business Finances (SSBF) shows that 94.1% of the larger private firms in the survey have fewer than ten shareholders (most have fewer than three), and 83.2% are managed by the controlling shareholder. As a result, agency problems are likely to be greater among public firms than among private ones.

private firms that are Forbes 100 constituents between 1985 and 2010. We then match these firms to the microdata we accessed from the MLS database, resulting in a total of 478 public firms, and 135 private firms tracked over the period covered by MLS, namely, 1995-2010[22]. We call this sample the "full sample." Table 1 reports the characteristics of both the private and public firms in this sample. Over the 1995-2010 time period, the public firms tend to be larger than the private firms in terms of both revenue and number of employees. Also, the baseline layoff propensity of public firms is about 1.8 percentage points grater than the private firms, although we find no difference in the number of workers laid off by both these firms in a given mass layoff event. In an ideal world, we would like to compare the investment behavior of two otherwise identical firms that differ only in their listing status. To get closer to this ideal we need to find pairs of public and private firms that are observably similar to each other. One convenient way to do this is through matching, which is what we turn to next.

[INSERT TABLE 1 HERE]

5.1.2 Matching Sample. — Since size is an important observable difference between the public and private firms in our sample, we match on size (revenue) in addition to matching on industry. This procedure closely follows the methodology of Asker, Farre-Mensa, and Ljungqvist (2011), who conduct a similar matching between public and private firms to investigate differences in investment sensitivities. Matching on size implies that our matched sample consists of the bottom half of public firms in the Fortune 500, which correspond to the size of all private firms in the Forbes 100. (see Table 1 for a comparison).

In the language of the matching literature surveyed in Imbens and Wooldridge (2009), we use a nearest-neighbor match adapted to a panel setting. Starting in fiscal year 1985, for each public firm, we find the private firm that is closest in size and that operates in the same three-digit NAICS industry, requiring that the ratio of their total revenue (TR) is less than 2 (i.e., $\max\left(TR_{public}, TR_{private}\right) / \min\left(TR_{public}, TR_{private}\right) < 2$). If no match is found, we discard that observation and look for a new match for that firm in the following year. Once a match is formed,

---

[22]The number of firms we track for this analysis is reduced by two factors: First, five states did not allow us to access their mass layoffs information. Second, not all firms in our broad sample engaged in layoffs between 1995 and 2010. This sample differs from our other results in that it considers non-contemporaneous constituents. The vast majority of our results are unchanged when restrict the sample to contemporaneous constituents of the two lists between 1995 to 2010, although this substantially reduces the sample size.

it is kept in subsequent years to ensure the panel structure of the data remains intact. If a matching firm exits the panel, a new match is spliced in. Because we match with replacement, to maximize the match rate and match quality, the matched sample contains 206 public firms and 74 private firms. Our results are not sensitive to matching without replacement, although this substantially reduces the sample size. The standard errors are appropriately clustered to account for the resampling of private firms. The middle three columns in Table 1 compare the characteristics of the matched sample, and allows us to assess how good this match is. Since we match on size as measured by revenue, it is not surprising to find no statistical difference in the average revenue of public and private firms in our matched subsample. We find almost no difference in the average number of employees between public and private firms, and no difference in average layoff propensity or severity.

5.1.3 Leverage Buyout Sample. — Next, we create an alternate subsample based on leverage buyout (LBO) attempts. From the full sample we only keep private firms that were once public and went private through a LBO after 1985. We obtain this data from the Forbes annual survey of largest private firms in the US. As for the public firms, we track firms after 1985, and only include the public firms that were targeted by an unsuccessful LBO attempt.[23]

In Table 1 we report the observed differences between public and private firms in this subsample. We again find no significant differences in revenue, number of employees, layoff propensity, and the share of employees laid off between public firms (resulting from unsuccessful LBO attempts) and private firms (resulting from successful LBO attempts). We do find a difference in the average number of workers laid off, but the difference between medians is much smaller.

5.2 Comparing Public-Private Firms: Results

The main results of the analysis are reported in Table 2. We estimate the same set of two regressions using the three different samples described above. In each set the first regression has an indicator for mass layoff as the dependent variable, while the second regression uses the share of employees laid off (conditional on a layoff). All regressions include controls for the log of the previous

---

[23]If the withdrawal of the LBO action is random, differences between successful LBOs and withdrawn LBOs will allow us to identify the differences in layoff propensity of public firms relative to private firms. However, the withdrawal of LBOs are not always random. We run several tests to examine the observable differences between the two sets of firms. We find no systematic differences in firm characteristics. Still, unobservable differences may exist, which we are unable to control for.

year's revenues. Additionally, the regressions with layoff indicator as the dependent variable controls for the previous year's employee size. We are unable to control for other firm characteristics since the Forbes dataset only reports these two variables for private firms.

5.2.1 Full Sample Results. —   In the first set of specifications, (1) and (2), we estimate the regressions on the full sample, with no firm fixed effects or time fixed effects, but with 4-digit industry fixed effects. In addition we control for seasonality by including calendar month fixed effects, and for the overall time trend using a quadratic function. Using the BLS microdata for the period 1995 to 2010 at a monthly frequency, we find that private firms are roughly 2.01 percentage points more likely to layoff in a recession month. Compared to them, the public firms' propensity to lay off workers in recession months is 2.47 percentage points *greater*, indicating that the layoffs of public firms are more than twice as sensitive to recessions as those of private firms. The relatively modest response of private firms to recessions also shows up in the share of employees they lay off in recession years. Conditional on a layoff, private firms exhibit no difference in the share of employees they layoff in recession months and non-recession months. Contrastingly, the share of workers laid off by public firms goes up by 0.28 percentage points (conditional on a layoff) in a recession month when compared to the share laid off by private firms.

In the next set of specifications, (3) and (4), we conduct the same analysis but with firm fixed effects instead of industry fixed effects, and with month fixed effects instead of the controls for seasonality and time trends. We are still able to identify the impact of the private firm indicator, since this regression takes advantage of public-to-private and private-to-public transitions. During our sample years of 1995-2010, we have 38 such transitions. The recession indicator in these regressions is not identified due to presence of month fixed effects. Under these specifications we find very similar results:a firm is more likely to announce layoffs in a recession if it is public. Similarly, when compared to private firms, public firms lay off more workers in recession years, although this coefficient is imprecisely estimated.

[INSERT TABLE 2 HERE]

5.2.2 Matched Sample Results. —   Our results using the matching estimator are reported in columns (5)-(8). These four specifications are a counterpart to the first four specifications discussed above. In addition to using our smaller matched sample, the key difference is in the

control structure: we include a matched-pair fixed effect instead of the industry- and firm-level fixed effects in the previous specifications. Therefore, the identification in these regressions is based off within-pair variation, where each pair consists of one public and one private firm within the same subindustry matched on size. The matching estimator results are in line with the results above: the layoff propensity of public firms is more than twice as sensitive to recessions as that of public firms, and the same is true for the share of employees laid off conditional on a layoff. Specifically, the propensity of private firms to engage in a mass layoff increases by 2.4 percentage points in recession months, while public firms experience an increase that is 3.2 percentage points *greater* than the effect we see in private firms. We also find that public firms are 0.64 percentage points less likely to lay off workers outside recession months when compared to private firms, although this result is not precisely estimated. We find similar results for the fraction of employees laid off conditional on a layoff. Outside recession months public firms lay off a relatively smaller fraction of employees compared to private firms, whereas the opposite holds in recession months. These coefficients, however, are not precisely estimated and cannot be interpreted as definitive results.

The general message from the matched sample results is that the public firms are more cyclical in their layoff policies compared to their matched private counterparts. These results suggest that public firms may be carrying excess capacity and waiting for longer periods between layoffs when compared to similar private firms. To investigate this further, we use the same matching methodology to estimate both the total number of workers laid off and the median duration between layoffs over the course of a full business cycle. We present these results in Table 3. In examining the total number of workers laid off, we use a peak-to-peak identification, starting from October of 2000 and ending in July of 2007. We report our findings in column (1): public firms tend to lay off approximately 30% more workers over the course of the entire business cycle, though this result is not precisely estimated. When we consider the median duration between layoffs, we use a slightly different time period. We begin our sample in April of 2002, approximately six months after the trough of the 2001 recession, and end in December of 2009. The motivation for this is to observe firm behavior in the period after they are most likely to have adjusted their labor force to their desired optimal level: most layoffs occur between the start and the trough of a recession, and the firms in our sample would have had ample opportunity to adjust their labor force. Using within-pair variation, we find evidence for a difference in layoff timing between public and private firms, and

report these results in column (2) of Table 3. We find that the average duration between layoffs for a sample of firms that engage in layoffs during the 2002-10 period is 7.76 months. Using within-pair variation, we find that the duration between layoffs for public firms is roughly 0.65 months greater than their matched private counterpart. While this effect is not precisely estimated, it is consistent with the view that public firms may wait longer to announce layoffs.

[INSERT TABLE 3 HERE]

5.2.3 Leveraged Buyout Targets Sample Results. — The last set of results are estimated using a fixed effects specification with the firms in our LBO sample, and we report these results in Table 4. We find that after controlling for 4-digit industry category, log of revenues and log of employees, the layoff propensity of public firms increases by 6.04 percentage points in recession months when compared to private firms in the sample. Outside of recession months, however, we find that public firms are slightly less likely to engage in layoffs. Examining the share of workers laid off, we once again we find that public firms are likely to lay off a larger fraction of their workforce in a recession month when compared to a private firm, but the effect is not statistically significant. Overall, these results are consistent with those from our other samples: public firms are much more cyclical when compared to private firms.

[INSERT TABLE 4 HERE]

5.3 Possible Alternate Explanations for the Difference in Public vs. Private Layoff Behavior

This section explores the plausibility of explanations other than reputation management for the difference in layoff propensity between public and private firms. The key concern is that when we compare public and private firms, there are unobservable differences between unrelated to reputation management which may be driving the results presented above. Among the set of possible sources of unobservable heterogeneity between public and private firms, financial leverage and lifecycle effects are central and may directly alter the layoff behavior of firms independent of any reputation-management behavior of managers. Private firms tend to have greater degree of financial leverage compared to public firms, and also tend to be younger than public firms. Since

we do not observe these characteristics for our sample of private firms we cannot control for them in our regressions. In order to assess the importance of these characteristics we instead compare our sample of private firms to the most levered public firms, and to the youngest public firms. If these characteristics are drivers of layoff policies, we would expect that in recessions, the layoff behavior of high-leverage public firms and young public firms will be quite similar to that of private firms. We investigate these alternate hypotheses in Table 5.

The dependent variable in all the regressions in Table 5 is the layoff indicator. Column 1 restricts the sample of public firms to 'young' firms (those whose time-since-IPO in their first year in our panel is less than the median time-since-IPO of all public firms in a given calendar year), while column 2 restricts the sample of public firms to 'old' firms. Similarly, Column 3 restricts the sample of public firms to 'high leverage' firms (those whose leverage ratio exceeds the median leverage ratio of all public firms in a given calendar year), while column 4 restricts the sample of public firms to 'low leverage' firms. In the last specification, we restrict the sample to public firms only, and estimate the interactions between the recession indicator and each of the two characteristics above: the log of years since IPO, and the leverage ratio. Each regression includes month fixed effects, the log of previous year's employees and its interaction with recession indicator, and the log of the previous year's revenue and its interaction with the recession indicator.

We find that the younger public firms are much more likely to lay off in a recession month compared to older public firms (specification (5)). We also find no significant effect of leverage on the sensitivity of layoff propensity to recessions. These results are consistent with specifications (1)-(4): the difference between public and private firms is strongest when comparing against young public firms, and is relatively consistent when comparing against high- and low-leverage public firms. The key implication of the results in this table is that the observed differences in layoff propensity in recessions between public and private firms are not being driven by unobservable differences in either leverage or lifecycle effects.

Though these results rule out two key possible alternate explanations, there may be other forms of unobserved variation between public and private firms. In order to investigate this further, we refined our matching criteria to match on size and 4-digit subindustry (instead of 3-digit subindustry). This reduces our sample size by approximately 50% , but we have enough observations to conduct similar analysis as reported in Table 2. The results of this analysis is reported in Appendix

Table 2. These results based on the four digit industry level replicate what we find in Table 2, and the magnitudes of the coefficients in this table line up with the analysis using a matching criteria based on the three digit industry level. Therefore, our results is robust to changes in the matching criteria we use. Moreover, using a more stringent matching criteria (i.e. at the four digit industry level) allows us to mitigate unobservable differences between public and private firms.

[INSERT TABLE 5 HERE]

5.4 Variation within Public Firms

So far, our analysis has been based on comparing public and private firms. In this section we look for differences in layoff behavior within our sample of public firms. In seeking to identify managers that are more likely to engage in reputation management and time layoffs strategically, we rely on the following two measures. First, we use an indicator variable called short-tenured CEO, which takes a value of 1 if the CEO's tenure with the firm is four years or less.[24] Second, we measure a firm's past equity-linked compensation share as the share of total CEO compensation that comes from equity-linked instruments over the past 5 years. We are interested in the impact of these variables on actual layoff behavior over the business cycle, and we report the results in Table 6. We use the same set of firm-level controls as in Table 5, and also include month fixed effects and firm fixed effects. The sample includes all contemporaneous constituents of the Fortune 500 that ever engaged in a layoff. In specification (1) we find that short-tenured CEOs are roughly 1.44 percentage points more likely to engage in a layoff in a recession month, relative to firms with longer-tenured CEOs.[25] Similarly, in specification (2), we find that a one-standard-deviation increase in the share of equity-linked compensation (controlling for the level of total compensation) is associated with an increase of roughly 0.7 percentage points in layoff propensity during a recession

---

[24] Some CEOs may choose to lay off workers immediately after they are hired, so as to start with a 'clean sheet.' Therefore, we also run the same test with a separate indicator variable for CEOs with a tenure between 0 and 1 year. Our results remain unchanged.

[25] One might expect that firms which bring in new CEOs are also aiming to maintain (or even reduce) their labor force, rather than looking to expand aggressively. If true, this could lead to the result that firms with younger CEOs also engage in relatively more layoffs during recessions: firms looking to expand aggressively can respond by cutting back on hiring rather than announcing layoffs. To evaluate this alternative interpretations, we examine the relationship between new CEOs and employment growth at their firms. We find no effect, indicating that our layoff results are not coming from reduced hiring by new CEOs.

month.[26]

In specification (3) we restrict the sample to all firms that have option share above the median level for a given year. Similarly, in specification (4) we restrict the sample to all firms that have CEO tenure below the median level for a given year. We estimate the same regressions as in specifications (1) and (2), and find that our results get stronger: both short-tenured CEOs and equity-linked compensation are linked to a greater incidence of layoffs during recessions. These results suggest that within the set of public firms, the CEOs who are most likely to care strongly about reputation are also the ones who are most likely to engage in layoffs during recessions. This suggests that reputation management by CEOs plays a significant role in determining the cyclicality of their firms' layoff polices.

[INSERT TABLE 6 HERE]

6 Daily Frequency Tests

In this section we turn to the daily frequency tests of our model's predictions. First we establish that the reputation penalty is lower if a firm announces a layoff right after other large firms in the economy announce layoffs (Section 6.1). We evaluate the strength of the response to this reduction in reputation penalty when we test Proposition 4, which predicts that firms will cluster layoffs after layoff announcements by large firms. Finally, we test the differential sensitivity results in Corollaries 6 and 7, which predict that strategically-motivated managers (those with high γ and σ_η^2) will be more likely to cluster their layoff announcements.

6.1 Is the Reputation Penalty of Layoff Announcements Lower after Layoffs by Other Firms?

Though the manager may be managing his reputation with several constituents–the stock market, the board of directors, employees of the firm– the analysis in this section focuses on financial market reputation, which we can test this using daily stock returns. This focus on stock-market-based measures is driven by ease of testability, and should not be seen as a narrow interpretation of

---

[26]The equity-linked compensation findings are consistent with results in the earnings management literature with respect to CEO incentives (see Bergstresser and Philippon (2006))

the reputation management mechanism in our model. Several studies have documented a negative stock market reaction to layoff announcements (Farber and Hallock (2009), Hallock (2009)). In this section we are interested in whether this negative penalty is lower if a firm announces a layoff within days of a layoff announcement by a large firm. Our empirical strategy follows the conventional event-study approach.

Using a sample of the Fortune 500 constituents from 1970 to 2010, we calculate cumulative excess returns using return data from the Center for Research in Security Prices (CRSP) at the University of Chicago. The excess return is the part of the movement in the stock return of a company that is not correlated with overall market movement in stock returns and presumably reflects unexpected firm-specific factors. To do this we run a first-stage regression where the daily stock return for company i on day t, denoted by R_{it}, is regressed on the value-weighted return of the market, R_{mt}:

$$R_{it} = \alpha_i + \beta_i R_{mt} + \eta_{it}$$

Next, for days around the event, the daily abnormal (or excess) returns is calculated as follows:

$$ER_{it} = R_{it} - \left(\hat{\alpha}_i + \hat{\beta}_i R_{mt} \right)$$

where $\hat{\alpha}_i$ and $\hat{\beta}_i$ are estimates of the previous regression. The first stage regression is run for a period in the past, which in this study ranged for a period of one year ending 30 days before the event. We rely on the average cumulative excess return over a five-day window–two days before, the day of, and two days after the event for each of the 41 years from 1970 to 2010. Changes to this window length has no material effect on the results. The results are listed in Table 7 Panel A. Specification (1) restricts the sample to all layoff announcements that occur within the three days following a layoff by the largest 20 public firms in the economy as measured by the previous year's revenue. Specification (2) considers the complementary case, in which the sample includes layoff announcements that occur on all other days.

We find that the cumulative excess return around a layoff announcement that occurs within the three days following a top 20 firm announcement is -0.38 percentage points. On the other hand, if a layoff occurs on any other day, the cumulative excess return is more than twice as large: -0.87

percentage points. This suggests that in the context of financial markets, the reputation penalty of layoffs is lower immediately after negative signals about the state of the economy.

In Panel B of Table 7 we further examine the nature of stock market penalty of layoff announcements. Our model predicts a mitigated reputation penalty for the first-mover when their layoff is followed by layoffs by others in the industry. This is because the initial negative penalty should partially be undone when the market realizes other firms in the industry also are laying off. We test this in column (1) of Panel B. For a top-20 firm that laid off within the last three days, we find a three-day cumulative abnormal return of 0.33 percentage points when other firms in the industry engage in a layoff. Consistent with the results of Panel A, this result also confirms the mitigated penalty prediction of our model.

In column (2) we estimate the stock market reaction of other firms in the industry when a top-20 firm (as measured by previous year's revenue) engages in a layoff. The model predicts a negative effect since a layoff by a large firm changes market perception about industry condition. We find a small negative but insignificant effect (-0.03 percentage points). This result indicates that the change in perceptions regarding industry conditions is largely offset by a countervailing effect, likely stemming from the dynamics of product market competition. Specifically, when one firm in an industry does poorly, other firms may benefit due to reduced competition in the product market. The results suggest that this second effect is offsetting the primary effect predicted by our model (i.e. adversely changing the belief about industry condition).

Lastly, in appendix table A4 we find that the first-mover penalty to persist even after one month. We refer you to table A4 for further discussion of this result. In the next section, we proceed to test whether firms respond to these incentives by timing their layoff announcements to occur immediately after a layoff announcement by top-20 firms.

6.2 Do Firms Announce Layoffs after other Large Firm Layoff Announcements?

6.2.1 Empirical Strategy. — To assess whether firms engage in a 'leader-follower' behavior with respect to layoff announcements, our estimation strategy relies on a dynamic regression model with lagged and future effects. For a firm i at time t, the regression specification is

$$Layoff_{it} = \alpha_i + \sum_{j=-p}^{p} \beta_j MacroEvent_{g,t-j} + X'_{it}\phi + Y'_t\omega + \varepsilon_{it} \qquad (14)$$

The central variables are $Layoff_{it}$, an indicator variable which takes a value of one when firm i announces layoff on business day t, and $MacroEvent_{g,t-j}$, which is an indicator variable that takes a value of one if there was a negative macroeconomic news released on date $t-j$ relevant for a firm in industry g. We also include firm-level controls, denoted by X'_{it} to control for firm-level heterogeneity. We begin our analysis by focusing on the layoff announcements of public firms. For public firms, these controls include total revenue, number of employees, years since IPO, book-to-market ratio, earnings-price ratio, leverage ratio, and days since last earnings announcement.[27] For private firms, we include the two control variables that we are able to observe: total revenues and number of employees. In addition to concerns about firm heterogeneity, we also need to account for the possibility that firms may be more likely to announce layoffs on certain days of the week (e.g. Friday) or in certain months. To address these concerns, the vector Y_t includes year fixed effects, month fixed effects, and day-of-week fixed effects.

We consider three different types of macro events: layoff by a top 20 firm (as measured by previous year's revenue), layoff by a firm in the top 20 which shares the same 1-digit NAICS code as firm i, and, as a placebo test, unexpected negative news announcements that are not directly linked to economic performance. We conduct this analysis separately for public firms and private firms. In the first measure, which we label "Leader Layoffs," we restrict our group of large firms to the largest 20 firms as measured by previous year's revenue.[28] This ensures that the layoff announcements of these firms correspond closely with the notion of 'negative macroeconomic news' in our model. This measure takes the form of an indicator variable equal to one on the business day the WSJ reports any such layoff announcement. Our second measure, which we label "Industry Leader Layoffs," is similar in structure. It takes a value of one whenever the first measure takes a value of one *and* the large firm is in the same 1-digit NAICS industry as firm i. The final measure,

---

[27] Controlling for earnings announcement date is important since firms may be clustering layoff announcements around earnings announcement date, and we may observe clustering merely because different firms have earnings announcement dates close to each other.

[28] Press and analyst coverage of publicly-listed firms is highly skewed, with the largest firms receiving an inordinately high degree of coverage compared to slightly less large firms (Fang and Peress (2009)). Our results are almost identical when we use a threshold of 5, 10, 25 or 30 for classification of large firms instead of the 20 largest firms measured by previous year's revenue.

which we label "News Media Events", is based on the 'biggest news stories' as measured by press coverage. Using a survey of major news events from USA today (2007), we construct a list of events for the period 1970 to 2010. Events are selected to be negative in nature (e.g. the Sept. 11 attacks), and to occur over a span of a day or less (i.e. news events such as the Afghanistan invasion of 2001 are excluded). As above, this measure is an indicator variable that takes the value of one on days when these events occur.[29]

Our model predicts a strong asymmetry in layoff behavior before and after a large-firm layoff announcement. This is in sharp contrast to the common shock hypothesis, which predicts layoff announcements by smaller firms both before and after the leader layoff. The event study framework also enables us to tackle the issue of reverse causality: a potential concern is that smaller firms may drive large firm layoff announcements. Therefore, the coefficient on future events will enable us to establish whether this mechanism is at play. We report results for a lag length of $p = 5$, but note that our results are almost identical when we choose $p = 10$ or $p = 15$. To ensure comparability across all the regressions the sample of public firms is restricted to all contemporaneous constituents of the annual Fortune 500 list excluding the top 20 firms. Similarly, our sample of private firms include all contemporaneous constituents of the annual Forbes 100 list.

6.2.2 Results: High Frequency Announcement Behavior of Firms. — Figure 5 plots the sequence of β_j estimates from the event study specifications (14), along with point-wise 95% confidence intervals using standard errors clustered at the firm level. Panels A and B report the response to all "Leader Layoffs" by public and private firms, respectively. In Panel A, we see that the sequence of β_j estimates is roughly flat and close to zero before the leader layoff announcement ($j < 0$), and then jumps discretely at $t = 0$, and thereafter decreases gradually over the next 5 business days. Thus, a large firm layoff announcement is associated with future layoffs by other large public (Fortune 500) firms, but not with past layoffs. The magnitudes of these β_j should be compared to the unconditional average daily layoff announcement propensity of 0.0008. In Panel B, by contrast, we find no similar response to "Leader Layoffs" by private firms. Specifically, we find no evidence of clustering either before or after a top 20 layoff announcement. This is consistent with

---

[29]The purpose of considering these three different measures of macro events is to evaluate the predictions of the model in Proposition 4 and Corollary 5. As we will discuss in the alternate hypotheses section below, juxtaposing the results of these three regressions will allow us to exclude several alternate hypotheses that are otherwise consistent with some of our leader-follower results. Future terms are included in the dynamic regression model for falsification purposes. Most importantly, it allows us to tackle one of the main alternate hypotheses of common shocks.

our business cycle frequency results, in which the public firms exhibited much greater propensity to engage in actual layoffs in recession months compared to the private firms.

In Panel C, we return to public firms and investigate the response to "Industry Leader Layoffs." We again find the pattern of no effect prior to the event date, followed by a strong jump and gradual decline at $t = 0$. Notably, the magnitude of the within-industry response of public firms to a layoff by a large firm is almost twice that of the economy-wide response described in Panel A. In Panel D, we turn our focus to the response of public firms to "News Media Events." In contrast to events based on large-firm layoffs, a large negative media event does not predict future or past layoff announcements by the Fortune 500 firms. This suggests that firms are not attempting to time layoff announcements during periods when investors may be distracted by non-economic events.

Taken together, these results suggest that the Fortune 500 firms are more likely to time their layoff announcements in the days immediately after negative economic news is released, such as the aftermath of a layoff announcement by a very large firm. The strength of the 'follower' behavior is stronger when the negative news is more related to the firm's own productivity; and the effect is absent after large negative news that are non-economic in nature. Therefore, these results are more in line with the leader-follower mechanism being driven by an informational channel, rather than a 'hiding behind the headlines' channel. Moreover, the asymmetric response of the firms before and after the large layoff announcements offer evidence against the 'common shock' hypothesis, which would predict a more symmetric response in layoff announcement responses of the Fortune 500 firms.

[INSERT FIGURE 5 HERE]

6.3 Which Type of Firms Layoff after a Large Firm Layoff Announcement?

6.3.1 Empirical Strategy. —   To identify the characteristics of firms that announce layoffs immediately after a large firm layoff announcement, we construct an annual dataset in which the unit of observation is at the firm level for every year. With this framework, we aim to test the model's predictions on the type of firms which are most likely to behave strategically. Specifically, corollaries 6 and 7 predict that managers for whom the market lacks strong priors, and who care most about reputation, will be most likely to engage in strategic layoff timing. These results rely

on public firms since we do not observe clustering behavior for private firms. The basic regression specification is:

$$y_{it} = \alpha_i + Z'_{it}\phi + V'_t\omega + \varepsilon_{it} \tag{15}$$
$$z_{it} = \alpha_i + Z'_{it}\phi + V'_t\omega + \varepsilon_{it}$$

In these regressions, y_{it} is an indicator variable which takes a value of one when a firm is a 'follower' firm and zero otherwise; analogously, z_{it} is an indicator variable which takes a value of one when a firm is a 'counterfactual follower' firm and zero otherwise. A follower firm is defined as any firm that announces a layoff in a 5-day window following a large layoff (including the day of the large layoff). Similarly, a counterfactual follower firm is a firm that announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first set of regressions (specification (1) and (2)) is based on all firms in the dataset. This effectively treats all other Fortune 500 constituents as the control group. The second pair of regressions (specification (3) and (4)) restricts the sample to all firms that announce a layoff in year t. In effect, the control group in these specifications is the set of firms which also announced layoffs, but were outside the ten-day window which identifies firms as followers or counterfactual followers. In the last column (specification (5)), we restrict the sample to all firms that announce a layoff within 5 days (before or after) a large firm layoff announcement. In this last specification, the control group is simply the counterfactual followers: firms that announced layoffs in the five days *prior* to a large-firm layoff.

In all five specifications, the vectors Z_{it} and V_t include the same control variables and explanatory variables used in Table 6 and Figure 5: total revenue, number of employees, years since IPO, book-to-market ratio, earnings-price ratio, leverage ratio, and days since last earnings announcement. We also include a full set of time-based (annual) fixed effects.

In a closely-related test, we examine whether clustering at these short horizons is driven by information. Specifically, we test whether firms change their clustering behavior after they start being covered by financial analysts. We estimate the same set of five specifications as described above, and include an indicator variable for analyst coverage as an explanatory variable. Using I/B/E/S sell-side analyst recommendations for U.S. stocks from 1993 to 2010, we construct an aggregate analyst coverage indicator variable. I/B/E/S codes recommendations from 1 (strong

buy) to 5 (sell). We first restrict our sample to all firms that appear at least once in the I/B/E/S database. Next, we create an indicator variable, 'Past 3 years coverage,' which takes a value of 1 if an analyst covered by the I/B/E/S dataset made a recommendation in the previous three years. We report the results on determinants of follower behavior in Tables 8 and 9, with the former focusing on compensation structure and CEO tenure, while the latter focuses on analyst coverage.

6.3.2 Results. —   The key independent variables in Table 8 are the same ones we used in the business cycle frequency results of public firms in Table 6: an indicator variable for short-tenured CEO and the average of the share of CEO compensation that derives from equity-linked compensation over the past 5 years.

The results in this panel suggest that firms with a higher degree of equity-linked compensation are more likely to be followers; by contrast, this variable has no predictive power for counterfactual followers. Similarly, firms with short-tenured CEOs are more likely to be followers, while the same quality has a weak negative effect on the likelihood of being a counterfactual follower. These results are robust to the estimation using the full sample (specifications (1) and (2)), or the more restricted samples in specifications (3)-(5), although the coefficients are less precisely estimated due to reduced sample size.[30] Overall, we find that the same characteristics which predict layoff cyclicality on a business-cycle level also predict strategic behavior over shorter horizons: firms with short-tenured CEOs and with significant equity-linked compensation are much more likely to act as follower firms, but not as counterfactual followers. This suggests an important role of reputation management in driving the high degree of observed 'leader-follower' behavior in layoff announcements.

[INSERT TABLE 8 HERE]

To further explore the role of asymmetric information and reputation management, Table 9 reports our results from the analyst coverage regressions. The specifications here are identical to those in Table 8, except here the key explanatory variable is the indicator variable of analyst coverage. The results are broadly similar: when a firm is being covered by analysts, it is much less likely to announce layoffs after a large firm layoff. At the same time, the analyst indicator has no predictive power for counterfactual follower firms. We interpret these results as supporting the

---

[30]To ensure that the short-tenured CEO result is not being driven by the desire of newly-appointed CEOs to start with a 'clean sheet' we run the same regressions with a separate indicator variable for CEOs with tenure between 0 and 1 year. The results of this analysis are almost identical to those discussed here.

idea of asymmetric-information based factors driving the leader-follower behavior.

[INSERT TABLE 9 HERE]

7 Linking High Frequency Results to Business Cycle Frequency Results

The high frequency results in the previous section offer strong evidence for strategic behavior, but on their own, they do not indicate the existence of significant welfare-relevant effects. In order to assess this, we investigate the impact of being a *follower* firm on changes in layoff propensity over the business cycle. Being a high-frequency follower in layoff announcements is, in principle, a better measure of active reputation management than other measures we have used (such as short-tenured CEO and equity-linked compensation). This is because we can rely on direct observations of reputation management behavior rather than predictions of such behavior. The results in this section seek to establish a connection between our high frequency results and the business-cycle-frequency results described in Section 5.

7.1 Empirical Strategy. — For this analysis, we first identify firms that have been 'follower' firms in the past five years, using the same methodology as in the previous section. This is our basis for our measure 'Past 5 year follower,' which takes the value of one for a specific firm in a given calendar month if, at any point over the prior five years, that firm has engaged in a layoff announcement within the five days following a layoff announcement by a large (i.e. top-20) firm. Analogously, we create a measure of 'Past 5 year counterfactual followers,' which takes a value of one for a specific firm in a given calendar month if, at any point over the prior five years, that firm has engaged in a layoff announcement within the five days prior to (and not after) a layoff announcement by a large firm.

7.2 Results. — The results of this analysis are reported in Table 10. We find that outside of recession months the propensity of follower and counterfactual follower firms are statistically indistinguishable. However, within recession months, a firm that has been a follower firm in the previous five years is roughly 3.14 percentage points more likely to engage in a mass layoff. By contrast, we find the impact of recessions on layoff propensity is not statistically different from zero

for counterfactual follower firms. These results suggest that the firms we identify as 'strategic' over the very short horizons of our daily frequency data are also the firms that are more likely to have cyclical layoff policies over the course of the business cycle.

[INSERT TABLE 10 HERE]

8 Alternate Explanations of Layoff Behavior

The results in Sections 5 and 6 are consistent with the predictions of the model of strategic corporate layoffs; however, several alternative theories also predict the temporal clustering of both mass layoffs and announcements of mass layoffs. In this section, we discuss whether these alternate theories are partially or fully consistent with the broad set of results presented so far. The leading alternatives theories focus on common shocks, compassionate CEOs, management learning from other firms, and market inattention. Sections 8.1-8.4 discuss these alternative mechanisms and explore whether their implications match the empirical results. Notably, we do not seek to reject these alternate explanations; rather, we argue that none of them can explain the full range of results in the previous sections. We see this as strong support for the conclusion that reputation management in the context of financial markets plays a significant role in determining the layoff behavior of large firms.

8.1 Common Shocks

The first alternative explanation for the patterns in Sections 5 and 6 is rooted in common shocks. If an aggregate shock hits a large subset of firms simultaneously, this will lead them to announce layoffs within a short period of time. A simple model with such common shocks will generate temporal clustering of layoff announcements, and also that of actual layoffs. A more sophisticated model of common shocks may generate excess sensitivity of firms with certain characteristics to these common shocks. If these firm characteristics are correlated with the structure of executive compensation or CEO tenure, we would not only observe temporal clustering of layoffs, but also that firms with greater equity-linked compensation or short-tenured CEOs are more sensitive to the common shocks.

While common shocks are certainly part of the story, the results in Figure 5 provide suggestive evidence against the common shock theory at a daily frequency. If either the crude or the more-sophisticated version of the common shock theory were true, we should observe responses in layoff announcements both before and after layoff announcements by large firms, both in the aggregate and within industries. However, the results demonstrate a stark asymmetry in the dynamic response of layoff announcements of Fortune 500 firms. In the business days leading up to a large firm announcement, the response of layoff announcements of the other firms is flat and close to zero. By contrast, it jumps up on the day of the large firm announcement, and gradually returns to zero in the next 4-5 business days. Additionally, in the results of Table 7 we find that firms that announce layoffs within the five days following a large firm announcement are more likely to have greater equity-linked compensation and short-tenured CEOs. Conversely, no such association was found among the firms that lay off in the five days prior to a large firm's layoff announcement.

Over the longer horizons explored in our business cycle frequency data, our matching estimator results (based on size and four-digit industry) suggest a differential sensitivity of public firms' layoff behavior in response to recessions, compared to the behavior of matched private firms. Neither version of the common shock theory offers a clear prediction on the differential sensitivity of public and private firms, indicating that common shocks alone cannot explain the full range of our results.

8.2 Compassionate CEOs

Another mechanism that may generate the observed layoff behavior is that CEOs are compassionate and care about their labor force. This interpretation is compatible with the analysis in our model, but shifts the context of reputation from financial markets to the firm's employees. The degree of reputation concerns would therefore reflect factors such as altruistic motives or the CEO having strong ties to the labor force. Such motivations may lead the CEO to be biased against engaging in layoffs, and delay their layoffs until absolutely necessary. Consequently, CEOs with such characteristics will appear to pursue a cyclical layoff policy, or announce layoff announcements after other large firms have announced a layoff. Moreover, it is quite reasonable to expect that there may be strong correlations between a CEO's level of compassion for his employees, and his tenure and compensation structure.

Testing the general validity of this mechanism is difficult because the level of compassion of

a CEO may manifest itself in a number of different ways. We propose the following narrow test to identify the strength of such a mechanism in explaining our observed empirical patterns. If the CEO has spent many years at the firm (greater than 10 or 15 years) before being appointed its CEO ("home-grown CEOs"), then he is more likely to be compassionate. Conversely, CEOs that are externally-recruited or did not spend many years at the firm before being appointed to head it are less likely to be compassionate towards their labor force. We therefore evaluate the viability of this mechanism by testing its ability to explain the pattern of strategic firms having a greater sensitivity of layoff propensity to recessions.

We report the results of these tests in Table A5. The results suggest that home-grown CEOs who have been with the firm for many years are less likely to announce layoffs in general (although the point-estimates are imprecisely estimated). However, we find no effect of being home-grown on the cyclicality of layoff announcements. Therefore, we conclude that though compassionate CEOs may alter the firm's baseline layoff propensity, we find no evidence for this mechanism affecting layoff behavior differently over the business cycle. Thus, we cannot appeal to this mechanism in explaining the cyclical layoff behavior of the strategic firms in our analysis.

8.3 Market Inattention

Another alternate mechanism that can lead to 'leader-follower' behavior and potentially cyclical layoff policies is that firms are relying on market inattention to hide behind bad news. There are two versions of this alternate theory. The first relies on the market's underreaction to information caused by limited attention of market participants (Dellavigna and Pollet (2009)). In this version, there are certain days, e.g. Fridays, on which the market participants pay less attention to news, and therefore firms choose to release negative information on such days so as to reduce the adverse reputational effect. Thus, we would expect to observe firms clustering their layoff announcements around certain dates, but this would not be driven by interactions between firms. Rather, it would be the direct result of firms responding to common external drivers of market inattention.

The second version of the market inattention theory is that firms are following large firm layoff announcements because it allows their news article to be pushed to the back pages of the newspapers (or analogously gain less prominence in televisions news or other media). If market participants have some information processing cost, they are less likely to chance upon this negative

41

news, allowing the firms to release negative news in a relatively 'concealed' manner.

The high-frequency event study results presented in Figure 5 already controls for day of week and calendar month to control for predictable market inattention. In addition, the regressions also control for whether the daily observation occurs within a week (before or after) the firm's scheduled earnings announcement date. Correspondingly, the leader-follower behavior observed in Panels A and C of Figure 5 stems from mechanisms other than those suggested by the first version the market inattention theory. As for the second version, we refer to the results of Panel D. If firms were trying to hide behind other negative news, we should find the same mechanism to hold after days of major negative non-economic news (e.g. Hurricane Katrina). The results presented in the last panel, however, illustrate that there is no systematic change in layoff announcement behavior either before or after such major negative non-economic news. In addition, we separately conduct an analysis of the page of the Wall Street Journal on which each layoff announcement was originally reported. We find no significant difference in the placement of layoff announcement coverage on days of layoff announcements by large firms, when compared to other days. While it is possible that market inattention may play a role in determining the timing of layoff announcements, we are unable to find any evidence for this in our analysis, and cannot appeal to this mechanism to explain our results at either the daily or the business cycle frequency.

8.4 Learning from Other Managers

A final alternate mechanism that may lead to 'leader-follower' behavior is that managers are uncertain about the state of the aggregate economy, and they are waiting to receive a signal from the actions of the largest firms in the economy. By virtue of being larger, the managers of the largest firms may have better information about the aggregate state. Consequently, managers learn about the aggregate state from the performance of large firms, and respond to layoff announcements by large firms with layoffs of their own. This mechanism would also predict that short-tenured CEOs would be more likely to react to the announcements of large firms, as they are more likely to be inexperienced, and thus more reliant on learning from other managers.

Despite its intuitive appeal, this mechanism cannot account for the differences we observe between public and private firm layoff behavior. If the 'learning-from-others' theory is the primary driver of layoff policy, we should observe little difference between public and private firms, partic-

ularly when matching on size and industry. We would expect all firms within an industry to face similar market conditions regardless of their ownership status, so the optimal response to learning about changes in market conditions should be identical across the two groups.

Moreover, the stock penalty results we find in Table 7 fit squarely with our model, but cannot be explained by this alternate mechanism. In particular, the reputation-based mechanism has strong predictions about the cross-effects of one firm's action on the reputation penalty of other firms in the same industry (e.g. mitigated reputation penalty for layoffs followed by a large-firm layoff). Our empirical findings confirm these predictions. By contrast, the learning-based mechanism has limited predictions with respect to such cross-effects. In particular, mechanisms based on learning should be largely predictable, and therefore subsequent layoffs should have no observable (abnormal) cross effects on stock prices. In sum, to explain the full range of our empirical findings we need to appeal to reputation-based mechanism such as the one described by our model.

8.5 Alternate Mechanisms: Taking Stock

The key conclusion of this section is *not* that the alternate mechanisms discussed here play *no* role in the high-frequency clustering of layoff announcements or the cyclicality of layoffs at the business-cycle level. Instead, we conclude that the main results of this paper cannot be fully explained solely by these alternate mechanisms. Instead, we argue that reputation concerns in the context of financial markets represent the most salient explanation for the patterns we observe.

9 Conclusion

In this paper we document that there is excess clustering of layoff announcements at weekly and daily horizons. We interpret this clustering of announcements within a theoretical framework in which managers delay layoffs during good economic states to avoid damaging the market's perception of their ability. We test the implications of our model at both daily and business-cycle frequencies using two novel datasets. Using a pairwise matching estimator based on size and three-digit industry, we find that the layoff propensity of public firms is twice as sensitive to recessions, relative to their matched private counterparts.

In a range of robustness tests we show that these differences are not being driven by public-

private differences in lifecycle effects, leverage, workforce size, or on our matching criteria. Within our sample of public firms, we find that firms predicted to be more strategic by our theoretical analysis, are also the ones more likely to engage in mass layoffs during recessions. Our results therefore suggest that reputation management is an important driver of the observed differences in the cyclicality of layoffs between public and private firms.

At the daily frequency, we also find significant support for our model. We show that a large firm announcement (i.e. the 20 largest firms based on past year's revenue) is associated with future layoffs by other Fortune 500 firms, but not with past layoffs. We find that this effect is twice as strong if the large firm is in the same industry as the follower firm. For our sample of privately held firms we find no such clustering behavior either before or after the large-firm layoff announcement. Moreover, when we compare the characteristics of firms that lay off in the five days after a large-firm announcement ("followers") to those that lay off in the five days before a large-firm announcement ("counterfactual followers"), we find that follower firms have a greater likelihood to be managed by short-tenured CEOs (i.e. with a tenure between 0 and 4 years) and to place greater reliance on equity-linked compensation for their CEOs. Consistent with our theoretical framework, these results suggest that reputation management is an important driver for the timing of layoff announcements at high frequencies.

Lastly, we establish a connection between high-frequency clustering and layoff behavior at business cycle frequencies. We find that the follower firms are roughly three percentage points more likely to engage in a mass layoff during a recession month, compared to counterfactual follower firms. This link between the daily frequency reputation management and the cyclicality of layoffs over the business cycle provides significant evidence that reputation concerns are an important driver of firms' layoff policies. Taken together, the findings of this paper suggest that managerial behavior not only has costs for the individual firm, but also has significant aggregate implications at the business cycle frequencies.

Taken together, the findings of this paper indicate that reputation management in financial markets may strongly impact the real decisions of firms, particularly with respect to labor decisions. At the firm level, this mechanism can lead to significant deviations from optimal policy by delaying the termination of unprofitable projects. In addition, this mechanism also impacts industry dynamics by influencing the timing of layoffs by other firms in the same industry.

This paper suggests that understanding how distortions in managerial behavior impact industry dynamics can lead to a number of implications and directions for future research. First, to understand industry dynamics one should place added emphasis on shocks to large players such as Walmart, Ford, or Nokia. For example, in our analysis, layoffs by one of these firms may trigger clustering of layoffs by other firms in the economy. Second, this paper highlights a novel mechanism through which corporate governance can not only influence the financial decisions, but also the real decisions of firms and industries. Third, our research suggests that the timing of other forms of corporate disclosure, such as dividend cuts and writedowns, may also be subject to clustering, and have implications for determining firm value and performance. Future papers can take advantage of the empirical methodologies used here to understand the mechanisms driving disclosure dynamics.

References

ACHARYA, V. V., P. M. DEMARZO, AND I. KREMER (2011): "Endogenous Information Flows and the Clustering of Announcements," *American Economic Review*, forthcoming.

AMAR, B. (1993): "The hidden costs of stock market liquidity," *Journal of Financial Economics*, 34(1), 31 – 51.

ASKER, J. W., J. FARRE-MENSA, AND A. LJUNGQVIST (2011): "Comparing the Investment Behavior of Public and Private Firms," *SSRN eLibrary*, forthcoming.

BERGSTRESSER, D., AND T. PHILIPPON (2006): "CEO incentives and earnings management," *Journal of Financial Economics*, 80(3), 511–529.

BILLGER, S. M., AND K. F. HALLOCK (2005): "Mass Layoffs and CEO Turnover," *Industrial Relations, Vol. 44, No. 3, pp. 463-489, July 2005.*

BROWN, S. (2004): "Mass Layoff Statistics Data in the United States and Domestic and Overseas Relocation," *Bureau of Labor Statistics papers.*

DAVIS, S. J., J. HALTIWANGER, AND R. JARMIN (2006): "Volatility and Dispersion in Business Growth Rates: Publicly Traded versus Privately Held Firms," *NBER Macroeconomics Annual*, 21, pp. 107–179.

DELLAVIGNA, S., AND J. M. POLLET (2009): "Investor Inattention and Friday Earnings Announcements," *Journal of Finance*, 64(2), 709–749.

EDMANS, A., I. GOLDSTEIN, AND W. JIANG (2011): "The Real Effects of Financial Markets: The Impact of Prices on Takeovers," *Journal of Finance.*

FANG, L., AND J. PERESS (2009): "Media Coverage and the Cross-section of Stock Returns," *The Journal of Finance*, 64(5), 2023–2052.

FARBER, H. S., AND K. F. HALLOCK (2009): "The changing relationship between job loss announcements and stock prices: 1970 to 1999," *Labour Economics*, 16(1), 1 – 11.

GABAIX, X. (2011): "The Granular Origins of Aggregate Fluctuations," *Econometrica*, 79(3), 733–772.

GIBBONS, R., AND L. F. KATZ (1991): "Layoffs and Lemons," *Journal of Labor Economics*, 9(4), 351–80.

GLAZ, J., J. NAUS, AND S. WALLENSTEIN (2001): *Scan statistics*, Springer series in statistics. Springer.

HALLOCK, K. F. (2009): "Job Loss and the Fraying of the Implicit Employment Contract," *Journal of Economic Perspectives*, 23(4), 69–93.

HOLMSTRÖM, B. (1999): "Managerial Incentive Problems: A Dynamic Perspective," *Review of Economic Studies*, 66(1), 169–182.

IMBENS, G. W., AND J. M. WOOLDRIDGE (2009): "Recent Developments in the Econometrics of Program Evaluation," *Journal of Economic Literature*, 47(1), 5–86.

JENSEN, M. C., AND W. H. MECKLING (1976): "Theory of the firm: Managerial behavior, agency costs and ownership structure," *Journal of Financial Economics*, 3(4), 305 – 360.

RAJAN, R. G. (1994): "Why Bank Credit Policies Fluctuate: A Theory and Some Evidence," *The Quarterly Journal of Economics*, 109(2), 399–441.

STEIN, J. C. (1988): "Takeover Threats and Managerial Myopia," *Journal of Political Economy*, 96(1), 61–80.

——— (1989): "Efficient Capital Markets, Inefficient Firms: A Model of Myopic Corporate Behavior," *The Quarterly Journal of Economics*, 104(4), 655–69.

TSE, S., AND J. TUCKER (2010): "Within-industry timing of earnings warnings: do managers herd?," *Review of Accounting Studies*, 15, 879–914.

Appendix

Appendix A.1: Updating rule for the *terminate* policy

Given the market's conjecture that the firm has adopted the *efficient* policy, $\hat{D} = 0$, the conditional distribution of η given that the project failed is given by Bayes' Rule:

$$
\begin{aligned}
f\left(\eta|\text{Proj. Fails}\right) &= \frac{f\left(\eta\right)\Pr\left(\text{Proj. Fails}|\eta\right)}{\int_{\hat{\eta}} f\left(\hat{\eta}\right)\Pr\left(\text{Proj. Fails}|\hat{\eta}\right) d\hat{\eta}} \\
&= \frac{f\left(\eta\right)\left[\pi\left(1 - \eta(1-\delta)\right) + (1-\pi)\left(1-\eta\right)\right]}{\pi\left(1 - \mu(1-\delta)\right) + (1-\pi)\left(1-\mu\right)} \\
&= \frac{f\left(\eta\right)\left[1 - \eta(1-\pi\delta)\right]}{1 - \mu(1-\pi\delta)}
\end{aligned}
$$

Therefore, we can calculate the conditional expectation of η as:

$$
\begin{aligned}
E_{mkt}\left[\eta|\text{Proj. Fails}\right] &= \int_{\eta} \eta f\left(\eta|\text{Proj. Fails}\right) d\eta \\
&= \frac{1}{1 - \mu(1-\pi\delta)} \int_{\eta} \eta\left[1 - \eta(1-\pi\delta)\right] f\left(\eta\right) d\eta \\
&= \frac{\mu - E\left[\eta^2\right](1-\pi\delta)}{1 - \mu(1-\pi\delta)} \\
&= \frac{\mu - (\mu^2 + \sigma_\eta^2)(1-\pi\delta)}{1 - \mu(1-\pi\delta)} \\
&= \mu - \frac{\sigma_\eta^2(1-\pi\delta)}{1 - \mu(1-\pi\delta)}
\end{aligned}
$$

Similarly, the conditional distribution of η given that the project succeeded is given by the Bayes Rule:

$$
\begin{aligned}
f\left(\eta|\text{Proj. Succeeds }\right) &= \frac{f\left(\eta\right)\Pr\left(\text{Proj. Succeeds}|\eta\right)}{\int_{\hat{\eta}} f\left(\hat{\eta}\right)\Pr\left(\text{Proj. Succeeds}|\hat{\eta}\right) d\hat{\eta}} \\
&= \frac{f\left(\eta\right)\left[\pi\eta(1-\delta) + (1-\pi)\eta\right]}{\pi\mu(1-\delta) + (1-\pi)\mu} \\
&= \frac{f\left(\eta\right)\left[\eta(1-\pi\delta)\right]}{\mu(1-\pi\delta)} \\
&= f\left(\eta\right)\frac{\eta}{\mu}
\end{aligned}
$$

Therefore, conditional expectation is

$$
\begin{aligned}
E_{mkt}\left[\eta|\text{Proj. Succeeds}\right] &= \int_{\eta} \eta.f\left(\eta|\text{Proj. Succeeds}\right) d\eta \\
&= \int_{\eta} \frac{\eta^2}{\mu} f\left(\eta\right) d\eta \\
&= \frac{E\left[\eta^2\right]}{\mu} \\
&= \frac{\sigma_{\eta}^2 + \mu^2}{\mu} \\
&= \mu + \frac{\sigma_{\eta}^2}{\mu}
\end{aligned}
$$

Appendix A.2: Updating rule for the *delay* policy

Given the market's conjecture that the firm has adopted the defensive policy, $\hat{D} = 1$, the outcome of observing layoffs would be off the equilibrium path and Bayes' Rule would not apply. However, as specified in the main text, introducing trembles to the model allows for a positive probability of observing layoffs. Further, because the manager only has a decision point when the project fails, the observation of layoffs in this setting is equivalent to observing project failure. Thus, the application of Bayes' Rule is identical to the case of the *terminate* policy:

$$
\begin{aligned}
f\left(\eta|\text{Proj. Fails}\right) &= \frac{f\left(\eta\right)\Pr\left(\text{Proj. Fails}|\eta\right)}{\int_{\hat{\eta}} f\left(\hat{\eta}\right)\Pr\left(\text{Proj. Fails}|\hat{\eta}\right) d\hat{\eta}} \\
&= \frac{f\left(\eta\right)\left[1 - \eta(1 - \pi\delta)\right]}{1 - \mu(1 - \pi\delta)}
\end{aligned}
$$

Again, we calculate the conditional expectation of η as:

$$
\begin{aligned}
E_{mkt}\left[\eta|\text{Proj. Fails}\right] &= \int_{\eta} \eta f\left(\eta|\text{Proj. Fails}\right) d\eta \\
&= \mu - \frac{\sigma_{\eta}^2(1 - \pi\delta)}{1 - \mu(1 - \pi\delta)}
\end{aligned}
$$

For the case of no layoffs, because the market expects firms to never announce a layoff when $\hat{D} = 1$, the observation of no layoffs should not lead to any updating. This is exactly what we find:

$$f\left(\eta|\text{No Layoffs}, \hat{D}=1\right) \quad = \quad \frac{f\left(\eta\right)\Pr\left(\text{No Layoffs}|\eta\right)}{\int_{\hat{\eta}} f\left(\hat{\eta}\right)\Pr\left(\text{No Layoffs}|\hat{\eta}\right)d\hat{\eta}}$$

$$= \quad \frac{f\left(\eta\right)\left[\pi\left(\eta(1-\delta)+(1-\eta(1-\delta))\right)+(1-\pi)\left(\eta+(1-\eta)\right)\right]}{\pi\left(\mu(1-\delta)+(1-\mu(1-\delta))\right)+(1-\pi)\left(\mu+(1-\mu)\right)}$$

$$= \quad f\left(\eta\right)$$

By inspection, the posterior distribution is identical to the prior, so the market's expectation of talent remains at μ.

Appendix A.3: Updating rule for a mixed-strategy policy

For the case where the market observes layoffs, the signal is once again equivalent to observing project failure. Thus, the calculation proceeds as in the previous sections, and yields the posterior expectation:

$$E_{mkt}\left[\eta|\text{Proj. Fails}\right] \quad = \quad \int_{\eta} \eta f\left(\eta|\text{Proj. Fails}\right)d\eta$$

$$= \quad \mu - \frac{\sigma_{\eta}^{2}(1-\pi\delta)}{1-\mu(1-\pi\delta)}$$

For the case of no layoffs, the market expects firms to announce "no layoffs" with probability $\hat{D}$ conditional on project failure, and with probability 1 conditional on project success. The updating rule proceeds as follows:

$$f\left(\eta|\text{No Layoffs}, \hat{D}\right) \quad = \quad \frac{f\left(\eta\right)\Pr\left(\text{No Layoffs}|\eta, \hat{D}\right)}{\int_{\hat{\eta}} f\left(\hat{\eta}\right)\Pr\left(\text{No Layoffs}|\hat{\eta}, \hat{D}\right)d\hat{\eta}}$$

$$= \quad \frac{f\left(\eta\right)\left[\pi\left(\eta(1-\delta)+\hat{D}(1-\eta(1-\delta))\right)+(1-\pi)\left(\eta+\hat{D}\left(1-\eta\right)\right)\right]}{\pi\left(\mu(1-\delta)+\hat{D}(1-\mu(1-\delta))\right)+(1-\pi)\left(\mu+\hat{D}\left(1-\mu\right)\right)}$$

$$= \quad \frac{f\left(\eta\right)\left[\eta(1-\pi\delta)+\hat{D}(1-\eta(1-\pi\delta))\right]}{\mu(1-\pi\delta)+\hat{D}(1-\mu(1-\pi\delta))}$$

Therefore, conditional expectation is

$$
\begin{aligned}
E_{mkt}\left[\eta|\text{No Layoffs, }\hat{D}\right] &= \int_{\eta} \eta f\left(\eta|\text{No Layoffs, }\hat{D}\right) d\eta \\
&= \frac{1}{\mu(1-\pi\delta) + \hat{D}(1-\mu(1-\pi\delta))} \int_{\eta} \eta\left[\eta(1-\pi\delta) + \hat{D}(1-\eta(1-\pi\delta))\right] f(\eta)\, d\eta \\
&= \frac{E\left[\eta^2\right](1-\pi\delta) + \hat{D}(\mu - E\left[\eta^2\right](1-\pi\delta))}{\mu(1-\pi\delta) + \hat{D}(1-\mu(1-\pi\delta))} \\
&= \frac{(\mu^2+\sigma_\eta^2)(1-\pi\delta) + \hat{D}(\mu - (\mu^2+\sigma_\eta^2)(1-\pi\delta))}{\mu(1-\pi\delta) + \hat{D}(1-\mu(1-\pi\delta))} \\
&= \mu + \frac{\sigma_\eta^2(1-\pi\delta) + \hat{D}(-\sigma_\eta^2)(1-\pi\delta)}{\mu(1-\pi\delta) + \hat{D}(1-\mu(1-\pi\delta))} \\
&= \mu + \frac{(1-\hat{D})(1-\pi\delta)\sigma_\eta^2}{\hat{D} + (1-\hat{D})(1-\pi\delta)\mu}
\end{aligned}
$$

Appendix A.4: Proof for proposition 1

Taking the total derivative of equation 12 with respect to both $\hat{D}$ and π, we find:

$$
\frac{\partial\hat{D}}{\partial\pi}\left[(1-\mu(1-\pi\delta))\frac{C}{\gamma\sigma_e ta^2}\right] = -\delta\left[\frac{1}{[1-\mu(1-\pi\delta)]^2} - \mu(1-\hat{D})\frac{C}{\gamma\sigma_e ta^2}\right]
$$

Next, we know that the probability of observing a layoff is $(1-\hat{D})(1-\eta(1-\pi\delta))$, or $(1-\hat{D})(1-\mu(1-\pi\delta))$ in expectation. Taking the total derivative with respect to π yields:

$$
\frac{\partial\Pr[\text{Layoffs}]}{\partial\pi} = -\frac{\partial\hat{D}}{\partial\pi}(1-\mu(1-\pi\delta)) + (1-\hat{D})\mu\delta
$$

Combining the two results above and simplifying, we find:

$$
\frac{\partial\Pr[\text{Layoffs}]}{\partial\pi} = \frac{\delta\gamma\sigma_\eta^2}{C\left[1-\mu(1-\pi\delta)\right]^2} > 0
$$

Appendix A.5: Proof of proposition 2

We can rewrite $\Delta\Pr[\text{Layoffs}]/\Delta(Large\ Firm\ Layoff)$ as

$$\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(Large\ Firm\ Layoff\right)} = \frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(\pi\right)} \frac{\Delta\pi}{\Delta\left(Large\ Firm\ Layoff\right)}$$

From Proposition 1 we know $\frac{\Delta \Pr[\text{Layoffs}]}{\Delta(\pi)} > 0$. Also from equation (13) we know that $\Delta\pi/\Delta\left(Large\ Firm\ Layoff\right) > 0$. These two inequalities imply

$$\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(Large\ Firm\ Layoff\right)} > 0.$$

Appendix A.6: Proof of corollaries 3-5.

Following from appendix A.5, we can write the following derivative as:

$$\partial\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(Macro\ Event\right)}\right] \Big/ \partial\left[\frac{\Delta\pi}{\Delta\left(Macro\ Event\right)}\right] = \frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(\pi\right)}$$

From appendix 4, we know the right hand side is positive. Therefore,

$$\partial\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(Macro\ Event\right)}\right] \Big/ \partial\left[\frac{\Delta\pi}{\Delta\left(Macro\ Event\right)}\right] > 0$$

Similarly from appendix 4, we know $\partial\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta(\pi)}\right]\Big/\partial\sigma_\eta^2 > 0$ and $\partial\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta(\pi)}\right]\Big/\partial\gamma > 0$. Therefore we get:

$$\frac{\partial}{\partial\sigma_\eta^2}\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(Large\ Firm\ Layoff\right)}\right] = \frac{\Delta\pi}{\Delta\left(Large\ Firm\ Layoff\right)}\partial\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(pi\right)}\right]\Big/\partial\sigma_\eta^2 > 0$$

$$\frac{\partial}{\partial\gamma}\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(Large\ Firm\ Layoff\right)}\right] = \frac{\Delta\pi}{\Delta\left(Large\ Firm\ Layoff\right)}\partial\left[\frac{\Delta \Pr[\text{Layoffs}]}{\Delta\left(\pi\right)}\right]\Big/\partial\gamma > 0$$

Appendix A.7: Dynamic Version of the Model with Exogenous Updating Rule

In this appendix we present a simple dynamic extension of our model based on McCall's (1970) model of intertemporal job search. At date 0 there is a continuum of managers who evaluate their investment opportunities and hire a worker to engage in production. Thereafter in each period t, the project has a positive chance of failure, which depends positively on both the manager's quality

given by η_i, and the time-varying aggregate state, given by $\lambda_{s,t}$. The states can be normal (N) or adverse (A), such that such that $\lambda_{N,t} = 1$, and $\lambda_{A,t} = \delta$. After observing the realization of the project on each date, each firm has to decide whether to continue or terminate production. Given the parameters of this model, it will always be optimal for firms to continue if their projects do not fail. Therefore, the key decision has to be made after the project fails.

The main cost of announcing a layoff is that the market's belief about the manager type is adversely affected when they observe a layoff. This is what we call *reputation cost*. In each period nature draws a reputation cost $z(\pi)$ for the manager. For simplicity, we take this reputation cost to be exogenously given. The market's belief that the aggregate is in a normal state at time t is given by π_t. The key assumption about the reputation cost is that $z'(\pi) > 0$, i.e. when the market thinks the aggregate state is more likely to be normal, then their posterior beliefs about manager talent will be more pessimistic after they observe a layoff. In each period there is a random draw of π, which maps 1-to-1 into a reputation cost. Let the distribution of reputation cost be given by $F(Z) = \Pr[z \le Z]$, with $F(0) = 0$, $F(B) = 1$ for $B < \infty$. The manager has the option of not engaging in a layoff, in which case he pays c in this period and waits until next period for another draw of reputation cost from F. The per-period cost c is the net loss the firm bears every period by keeping the worker at a failed project for an additional period.

Let y_t be the manager's payoff in period t. To be consistent with the pre-existing search models it is convenient to characterize the payoff as $y_t = -\gamma z(\pi)$ if the manager with a failed project decides to layoff when the reputation cost is $z(\pi)$, and $y_t = -c$ when then manager decides to delay layoff. Here γ again measures the degree of reputational concern a manager has. The managers devise a strategy to maximize $E \sum_{t=0}^{\infty} \beta^t y_t$, such that $0 < \beta < 1$ is the discount factor.

Let $v(z(\pi))$ be the expected value of $\sum_{t=0}^{\infty} \beta^t y_t$ for an optimally-behaving manager who faces a reputation cost of $z(\pi)$, and is deciding whether to layoff or not. In this model we assume no recall. The value function $v(z(\pi))$ satisfies the Bellman equation

$$v(z(\pi)) = \max\left\{ -\frac{\gamma z(\pi)}{1 - \beta}, -c + \beta \int v(z(\pi')) \, dF(\pi') \right\}$$

There exists a threshold reputation $z(\bar{\pi})$, such that if the manager is facing a reputation cost $z(\pi) \le z(\bar{\pi})$, he should layoff, and delay otherwise. Solving for the threshold reputation, we can

characterize his strategy as

$$\gamma z\left(\bar{\pi}\right) - c = \frac{\beta\gamma}{1-\beta} \int_0^{\bar{\pi}} \left[z\left(\pi\right) - z\left(\bar{\pi}\right)\right] dF\left(\pi'\right)$$

Further rewriting and by applying integration by parts we can characterize the reputation cost threshold as

$$\gamma z\left(\bar{\pi}\right) - c = \beta\left(\gamma E\left[z\left(\pi\right)\right] - c\right) + \beta\gamma \int_{\bar{\pi}}^{\infty} F\left(\pi'\right) d\pi'$$

Thus, in this economy, managers whose project fails, will delay announcing layoffs until he faces a sufficiently high market-wide belief of being in an adverse aggregate state (i.e. a low value of π). This effectively generates periods of no or little layoffs, and large number of layoffs when market's belief about the aggregate state is adverse with high likelihood. In this model the firms need not all layoff in the same period. Their decision rule will depend on their degree of reputational concerns, γ. For a large class of functional forms for $z\left(.\right)$ it can be shown that the threshold $\bar{\pi}$ is a decreasing function of γ. This suggests that when managerial reputational concerns rises (i.e. high γ), their threshold for waiting becomes more restrictive (i.e. lower $\bar{\pi}$), as these managers are waiting to engage in layoffs in periods when the market's belief puts a very high probability on the aggregate state being adverse. As an example consider the case in which F is a uniform distribution with support $[0, B]$. Additionally assume that $z\left(x\right) = \phi x$. Under this example, we can characterize the threshold as

$$\bar{\pi} = \frac{\beta\left\{B + \phi E\left[\pi\right]\right\}}{\phi + \beta} + \frac{\left(1 - \beta\right)c}{\gamma\left(\phi + \beta\right)}$$

It is clear that $\partial\bar{\pi}/\partial\gamma < 0$, $\partial\bar{\pi}/\partial c > 0$, and $\partial\bar{\pi}/\partial B > 0$, while the effect of ϕ is ambiguous. This suggests that a greater reputational concern makes the threshold more restrictive, while a larger cost c (which is a per-period loss made by the firm because of following the inefficient policy) and a higher variance of beliefs as measured by B, leads to a less restrictive threshold.

Figure 1: Timeline of layoff announcements by top 3 firms in the banking and automotive industry between 2001 and 2003. A dot represents a layoff announcement on a given day. To highlight clustering of announcements we include (orange) boxes with width 30 days whenever multiple announcements occur within a period of 30 days. The longer boxes represent clustering across industries, while shorter boxes represent clustering within industries.
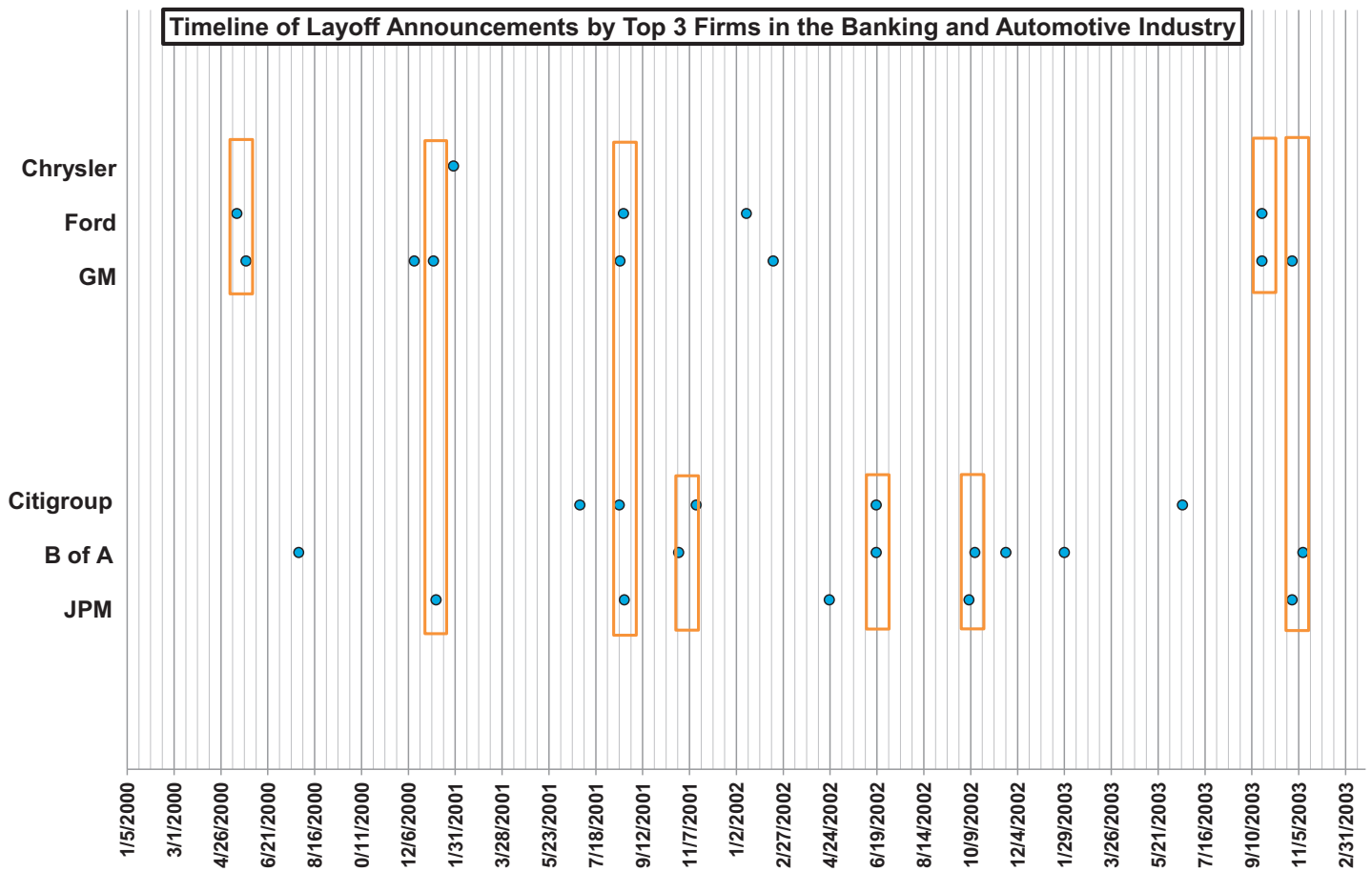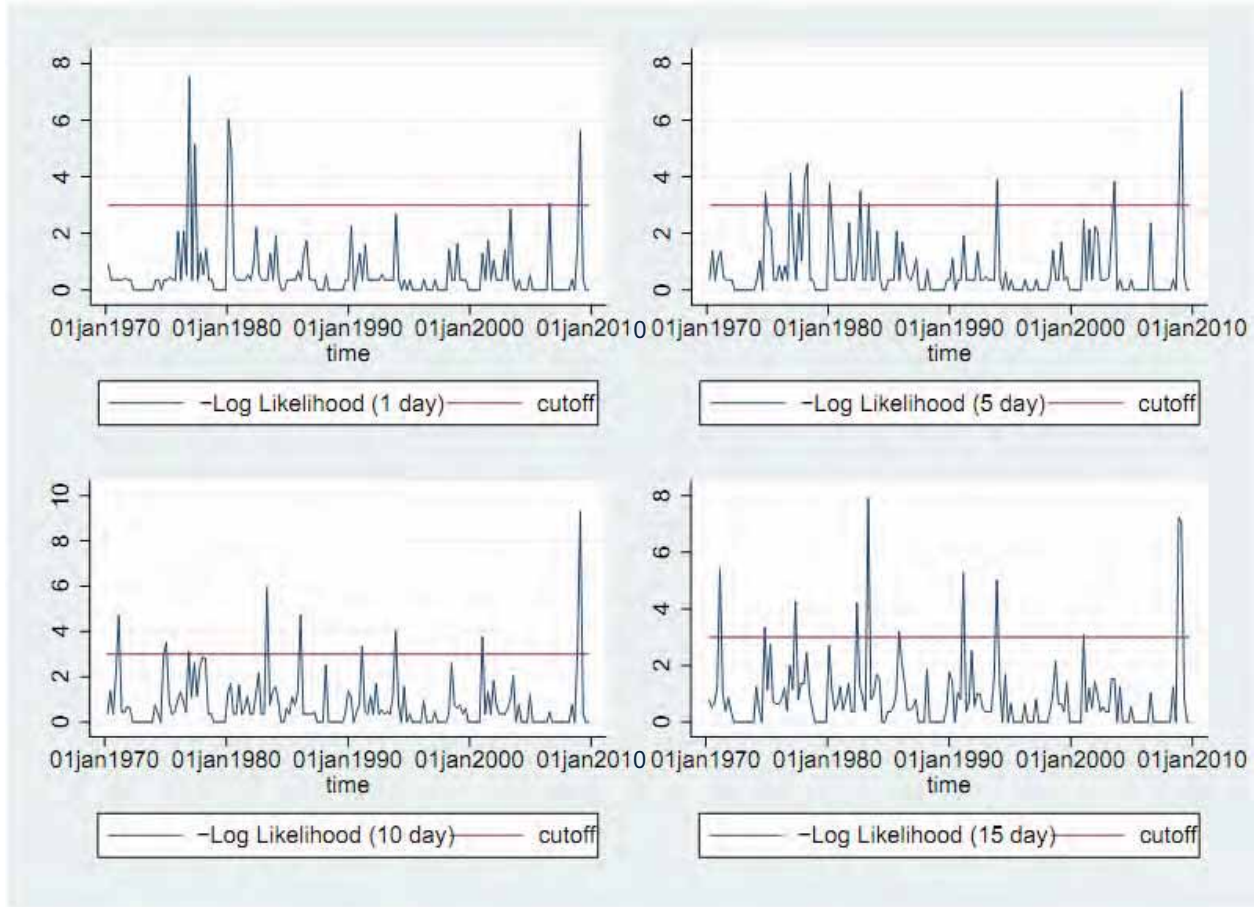
Figure 2: This graph reports the Log p-values (multiplied by negative unity) of the sequence of tests for each non-overlapping 60 (business) day window to identify clustering at 1, 5, 10 or 15 day horizons when the null is that there is layoff announcements are distributed uniformly over each 60 day window. The horizontal red line corresponds to a significance level of 0.05. Correspondingly, whenever p-values exceed the red line, we can reject the null hypothesis at the 5% level.
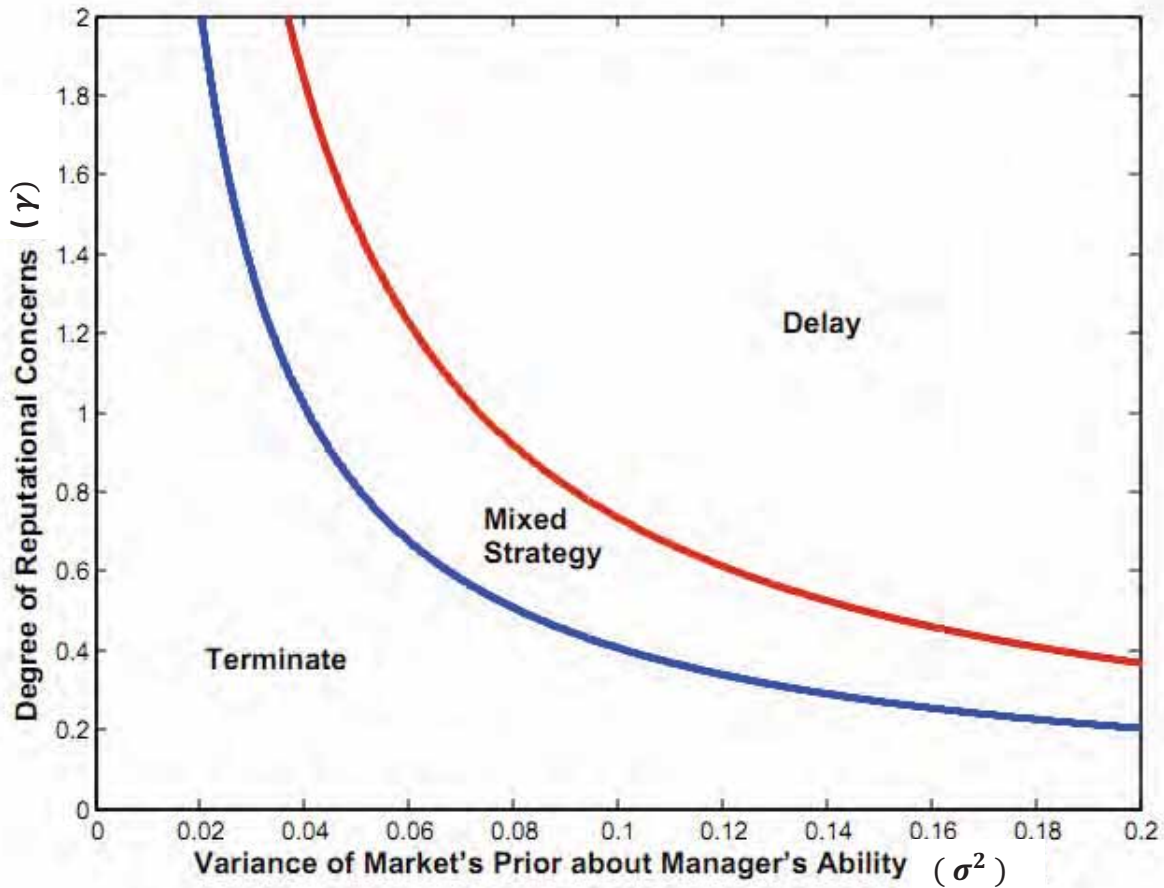
Figure 3: *Parameter Space and Equilibrium Layoff Policy.* The region on the right of the solid lines represents the parameter space in which the equilibrium layoff policy is to delay. Analogously, the equilibrium layoff policy in the region on the left is to terminate. The region between the two lines represents the parameter space in which the equilibrium policy is a mixed strategy, which randomizes between terminate and delay.
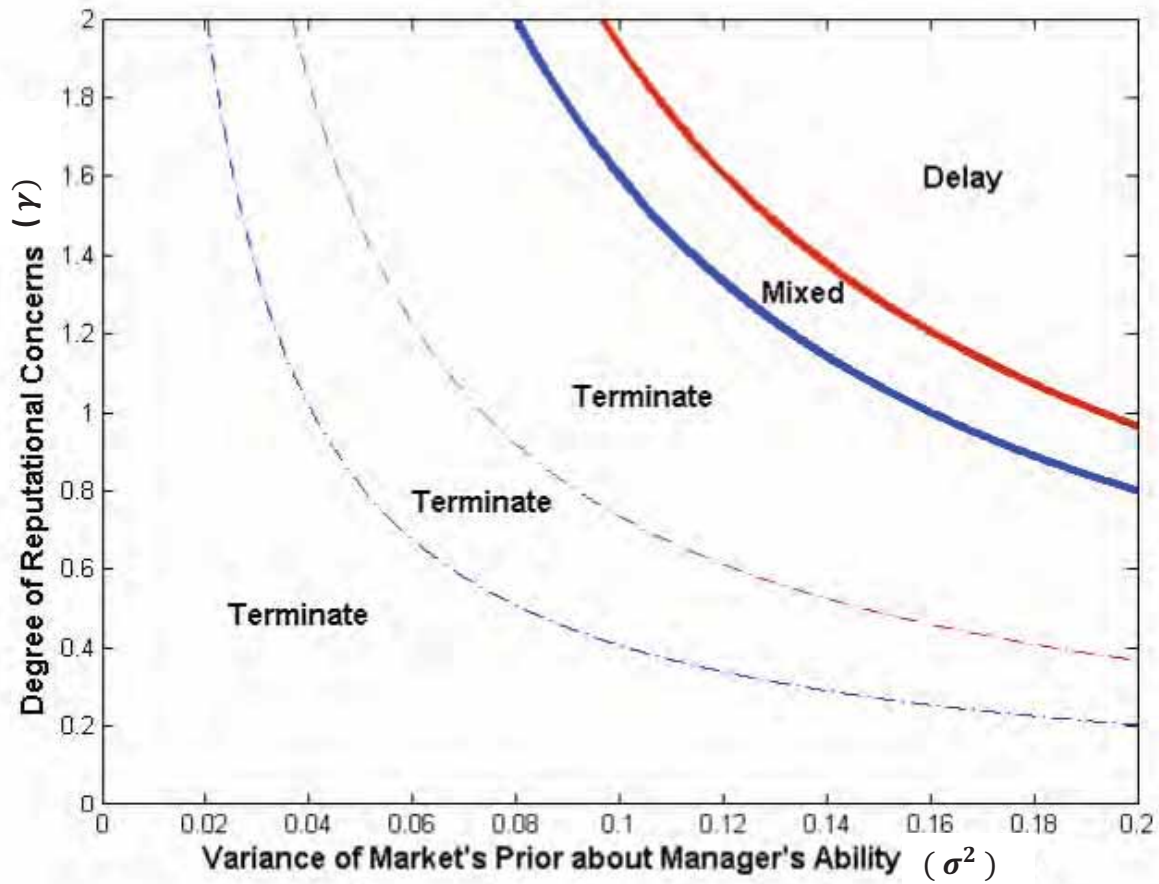
Figure 4: *Comparative statics – when belief about aggregate state worsens.*
The dotted lines represent the mapping of different regions of the parameter
space to different equilibrium layoff policy before we take comparative statics.
When belief about aggregate state worsens, the lines separating the regions
shift rightward, and are then represented by the solid lines.

Figure 5: These graphs report the response of layoff propensities to "Leader Layoffs" - layoff announcements by the largest 20 firms in the economy, based on previous year's revenue. We plot the coefficients of dynamic regressions which predict daily layoff propensities over the 11-day event-time periods surrounding large-firm layoff announcements. In all cases, we include firm-level controls, and fixed effects at the annual, monthly, and day-of-week levels. We also include point-wise 95% confidence intervals, based on standard errors clustered at the firm level. In Panel A, we examine the response of layoff propensity for the public Fortune-500 firms in our dataset. We juxtapose this with the results in Panel B, where we plot the layoff propensity response for Forbes-100 firms. In Panel C, we replicate the results of Panel A but restrict our coefficient analysis to firms which are in the same 1-digit NAICS industry as the leader firm for each "Leader Layoff" event. In Panel D, we plot the response of all public Fortune-500 firms to "News Media Events" - the biggest negative news stories based on press coverage from1970 to 2010.
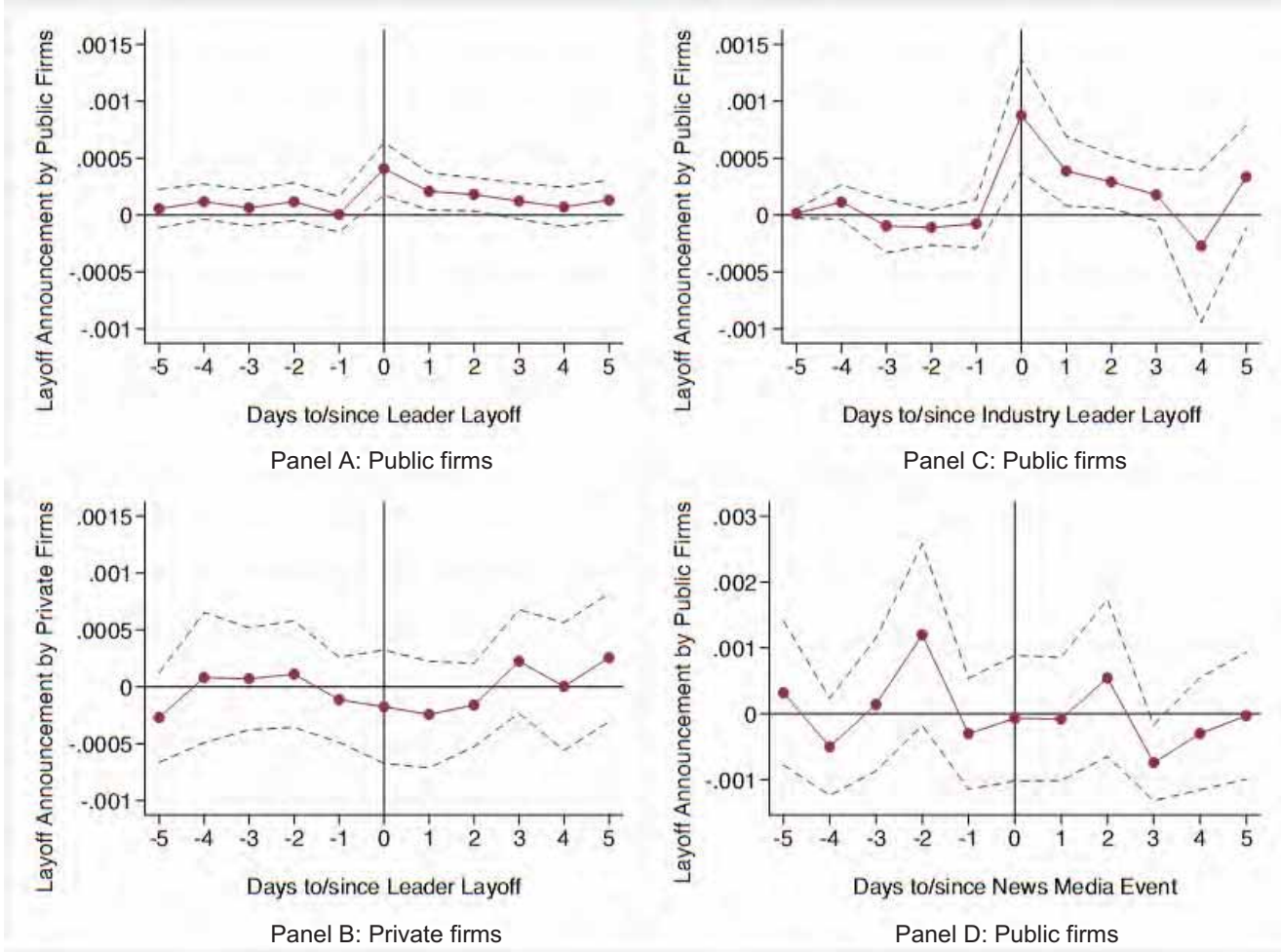
TABLE 1: Public - Private Firm Characteristics

This table presents descriptive statistics for the full samples of public and private firms, for a size-and-industry matched sample, and for a sample of public and private firms that were successfully or unsuccessfully targeted by leveraged buyouts (LBO), over a period from 1995 and 2010. The table reports mean, median, and standard deviation of the key variables used in our empirical analysis that compares public-listed and privately-held firms. We also report pairwise differences in means, with *** and ** indicating a difference that is significant in a t-test at the 1%,and 5% level, respectively.

		FULL SAMPLE			MATCHED SAMPLE			BUYOUT SAMPLE		
		Public Firms	Private Firms	Difference in Means	Public Firms	Private Firms	Difference in Means	Public Firms	Private Firms	Difference in Means
Revenue (in 2005 USD)										
	Mean	15143.29	8067.53	7064.76***	7162.07	6992.29	169.78	9218.31	8609.35	608.97
	Median	6035.37	4406.83		5075.34	4903.13		4871.72	5258.40	
	St. Dev	30540.05	10817.58		5122.21	4951.14		14895.95	6995.246	
Employees										
	Mean	46803.01	36624.15	10178.86***	29341.66	29614.29	-272.63	43101.70	46120.20	-3018.50
	Median	21586	17649		19117	15000		21000	28000	
	St. Dev	98809.90	57237.13		20212.44	26967.78		68204.04	44141.21	
Layoff Propensity										
	Mean	0.0721	0.0541	0.0180***	0.0584	0.0558	0.0027	0.0689	0.0698	-0.0009
	St. Dev	0.2587	0.2263		0.2345	0.2295		0.2533	0.2550	
Number Laid off										
	Mean	337.74	357.88	-20.14	281.59	248.82	32.77	284.32	409.57	-125.25**
	Median	174	200		160	150		166	191	
	St. Dev	953.98	490.70		521.45	418.56		446.33	615.03	
Share Laid Off										
	Mean	0.0134	0.0165	-0.0031	0.0138	0.0169	-0.0031	0.0134	0.0168	-0.0034
	Median	0.0035	0.0060		0.0047	0.0056		0.0043	0.0056	
	St. Dev	0.0819	0.0356		0.0132	0.0442		0.0686	0.0280	
No. of Firms		478	135		206	74		42	23	
No. of Observations		70665	10632		10470	10470		5502	1803	

Table 2: Public - Private Comparison over the Business Cycle

This table analyzes differences in actual layoff behavior over the business cycle between public and private firms. The unit of observation is a the firm level tracked montly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). In this table we report four sets of regressions, each with two different dependent variables. The first dependent variable is layoff indicator, which takes a value of one if the firm engages in a mass layoff in a given month. The second dependent variable is the number of workers laid off as a share of previous year's employees. The first two set of regressions are estimated over the entire sample, which include firms that were in the Fortune 500 (public firms) or Forbes 100 (private firms) between 1985 and 2010. The only difference between the first and second set of regressions is that the first set includes 4-digit industry fixed effects instead of firm fixed effects, and a quadratic time trend and controls for seasonality (calendar-month fixed effects) instead of month fixed effects. On the other hand the second set includes firm fixed effects and month fixed effects, instead of industry fixed effects or controls for time trend. In the second set of regressions we are able to identify the effect of the private firm indicator variable since we have 38 cases of firms that transitioned from private-to-public or vice-versa. In the last two set of regressions, (5)-(8), we restrict the sample to matched public-private pairs (see section 5.1.2 for the methodology) based on size (revenue) and 3-digit NAICS industry. These specifications rely on comparing each public firm to its matched private counterpart since we include matched-pair fixed effects. All regressions include controls for previous year's log revenue and its interaction with the recession dummy, and previous year's number of employees and its interaction with the recession dummy (except when the dependent variable is share laid off). Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in the specifications that include month fixed effects. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Full Sample		Full Sample		Matched Sample		Matched Sample	
Dependent Variable	Layoff Indicator (1)	Share Laid Off (2)	Layoff Indicator (3)	Share Laid Off (4)	Layoff Indicator (5)	Share Laid Off (6)	Layoff Indicator (7)	Share Laid Off (8)
Public Indicator	0.0051	0.0020	-0.0029	0.0029	-0.0064	-0.0069***	-0.0044	-0.0060
	(0.0085)	(0.0013)	(0.0127)	(0.0017)	(0.0090)	(0.0025)	(0.0063)	(0.0037)
Recession Indicator	0.0201**	-0.0011			0.0240**	0.0008		
	(0.0087)	(0.0015)			(0.0161)	(0.0014)		
Public x Recession	0.0247***	0.0028*	0.0259***	0.0025	0.0320**	0.0040*	0.0307**	0.0012
	(0.0097)	(0.0015)	(0.0090)	(0.0018)	(0.0136)	(0.0022)	(0.0142)	(0.0028)
Mean	0.0696	0.0137	0.0696	0.0137	0.0569	0.0155	0.0569	0.0155
Std. Dev	0.2544	0.0785	0.2544	0.0785	0.2318	0.0267	0.2318	0.0267
Industry Fixed Effect (4-digit NAICS)	✓	✓						
Quadratic Time Trend & seasonality controls	✓	✓			✓	✓		
Month Fixed Effects			✓	✓			✓	✓
Firm Fixed Effects			✓	✓				
Matched-Pair Fixed Effects					✓	✓	✓	✓
Log Employees and Recession Interaction	✓		✓		✓		✓	
Log Revenue and Recession Interaction	✓	✓	✓	✓	✓	✓	✓	✓
Observations	81414	5587	81414	5587	20592	1167	20592	1167

Table 3: Public - Private Comparison over the Business Cycle (part II)

This table reports characteristics of the matched sample and examines differences in public and private firm behavior over the business cycle with respect to their layoff policies. The first specification (1) computes the total number of workers laid off by a firm over an entire business cycle (peak-to-peak from 6 months prior to the peak of the 2001 recession (October 2000) to 6 months prior to the peak of the 2008 recession (July 2007)). Correspondingly each firm has one observation in this sample. We control for matched pair fixed effects, and correspondingly the coefficient on the public indicator variable relies on a comparison of a public firm with its matched private counterpart. In specification (2) using the same data structure we study the median distance mass layoffs for each firm over an entire business cycle. For this specification we rely on a trough-to-trough period, since the period right after a recession is a more natural starting point for this analysis. Once again each firm has one observation in the sample. We again control for matched pair fixed effects so as to rely on within pair variation. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Matched Sample	
Dependent Variable	workers laid off over business cycle (1)	Median Months Since Last Layoff (2)
Public Indicator	0.3039 (0.4251)	0.0656* (0.0363)
Mean	2.5745	7.76
Std. Dev	3.1077	6.69
Matched-Pair Fixed Effects	✓	✓
Observations	236	260

Table 4: Public - Private Comparison over the Business Cycle (part III)

This table analyzes differences in actual layoff behavior over the business cycle between public and private firms. The unit of observation is a the firm level tracked montly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). The set of regressions is estimated on a subsample of firms that were targets of a leveraged buyout (LBO). Among the targets the public firms are those for whom the LBO offer was withdrawn, and the private firms are those for whom the buyout offer was successful. The regressions include month fixed effects, and controls for previous year's log revenue and its interaction with the recession dummy, and previous year's number of employees and its interaction with the recession dummy (except when the dependent variable is share laid off). Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	LBO Targets	
Dependent Variable	Layoff Indicator (1)	Share Laid Off (2)
Public Indicator	-0.0079	-0.0088
	(0.0261)	(0.0272)
Public x Recession	0.0604**	0.0181
	(0.0258)	(0.0176)
Mean	0.0692	0.0142
Std. Dev	0.2550	0.0610
Industry Fixed Effect (4-digit NAICS)	✓	✓
Month Fixed Effects	✓	✓
Log Employees and Recession Interaction	✓	
Log Revenue and Recession Interaction	✓	✓
Observations	7116	470

Table 5: Assessing Alternate Explanations for Public - Private Differences in Cyclicality of Mass Layoffs

This table explores the plausibility of alternate explanations for the difference in layoff propensity between public and private firms. The dependent variable in all the regressions is the layoff indicator. Column 1 restricts the sample of public firms to 'young' firms (those whose time-since-IPO in their first year in our panel exceeds the median time-since-IPO of all public firms in the same calendar year), while column 2 restricts the sample of public firms to 'old' firms. Similarly, Column 3 restricts the sample of public firms to 'high leverage' firms (those whose leverage ratio exceeds the median leverage ratio of all public firms in the same calendar year), while column 4 restricts the sample of public firms to 'low leverage' firms. In the last specification, we restrict the sample to public firms only, and estimate the interaction between the recession indicator and, both, log of years since IPO and leverage ratio. Each regression includes month fixed effects, log of previous year's employees and its interaction with recession indicator, and log of the previous year's revenue and its interaction with the recession indicator. The first four specifications include industry fixed effects, whereas the last one includes firm fixed effects. Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Young Firms	Old Firms	High Leverage Firms	Low Leverage Firms	Public Only
Dependent Variable	Layoff Indicator	Layoff Indicator	Layoff Indicator	Layoff Indicator	Layoff Indicator
	(1)	(2)	(3)	(4)	(5)
Public Indicator	0.0095	0.0055	0.0009	0.0071	
	(0.0154)	(0.0231)	(0.0139)	(0.0196)	
Public x Recession	0.0478***	0.0133	0.0187*	0.0250**	
	(0.0127)	(0.0099)	(0.0108)	(0.0108)	
Leverage Ratio					0.0721***
					(0.0137)
Log(1+Years since IPO)					0.0005
					(0.0136)
Recession X Lev. Ratio					0.0259
					(0.0190)
Recession X Log Age					-0.0341***
					(0.0116)
Mean	0.0567	0.0785	0.0602	0.0759	0.0718
Std. Dev	0.2314	0.2689	0.2379	0.2639	0.2582
Firm Fixed Effects					✓
Industry Fixed Effect (4-digit NAICS)	✓	✓	✓	✓	
Month Fixed Effect	✓	✓	✓	✓	✓
Log Emp. and Interaction with Rec. Indicator	✓	✓	✓	✓	✓
Log Rev. and Interaction with Rec. Indicator	✓	✓	✓	✓	✓
Observations	44805	47019	45078	46746	69621

Table 6: Actual Layoff Propensity over the Business Cycle (Public Firms), 1995-2010

This table exploits within industry variation to analyze the impact of the equity-linked executive compensation and short tenure of CEO on layoff propensity over the business cycle. The unit of observation is at the firm level tracked monthly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). The sample for the first two specifications includes all contemporaneous constitutents of Fortune 500 that ever announced a layoff. In specification (3) we restrict the sample to all firms that are above the median with respect to option share of compensation in the previous year. Similarly, in specification (4) we restrict the sample to all firms above the median with respect to CEO tenure in the previous year. The dependent variable is an indicator variable, which takes a value of one when a firm announces a layoff in a given month, and zero otherwise. In each regression specification the main explanatory variables are the recession indicator, key variable, and the interaction of the key variable with the recession indiciator. The recession indicator takes a value of one in months classified as recession months by the NBER Business Cycle Dating Committee. The first key variable (specification (1) and (3)) is a measure of short-tenured CEO, which is an indicator variable that takes a value of one if the tenure of a given CEO at the firm is between 0 and 4 years. The second key variable (specification (2) and (4)) is a measure of equity-linked compensation, which is the Black-Scholes past five year average of the value of stock-option grants a CEO receives as a share of the average total compensation. All the specifications include month fixed effects, industry fixed effects, and firm-level controls and the interaction of each of these controls with the recession indicator. The firm level controls include Earnings-Price ratio, Book-to-Market Ratio, Market Capitalization, Leverage Ratio, Years since IPO, which are all obtained from COMPUSTAT. Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample	Full Sample		High Option Share Firms	Short-tenured CEO Firms
Dependent Variable	Layoff Indicator (1)	Layoff Indicator (2)	Layoff Indicator (3)	Layoff Indicator (4)
KV = Short CEO Tenure Indicator	0.0032 (0.0050)		0.0025 (0.0062)	
KV = Avg. Equity-Linked Compensation Share		0.0072 (0.0071)		0.0164 (0.0091)
Key Variable (KV) x Recession	0.0144* (0.0079)	0.0261** (0.0126)	0.0255** (0.0123)	0.0352** (0.0168)
Mean	0.0787	0.0790	0.0812	0.0831
Std. Dev	0.2693	0.2697	0.2732	0.2761
Industry Fixed Effects	✓	✓	✓	✓
Month Fixed Effects	✓	✓	✓	✓
Firm Controls and Recession Interaction	✓	✓	✓	✓
Observations	28746	27882	16818	13410

Table 7: Stock Penalty of Layoffs: Cumulative Abnormal Returns (from day -2 to day +2)

This table reports the cumalative abnormal returns (CAR) around a layoff announcement. The sample includes daily observations from 1970 to 2010. In Panel A, the baseline CAR is reported in specification (1) and (3) against the constant term of the regression. The main coefficient of interest is associated with the indicator variable reported in the first line, which takes a value of one if the firm under observation announces a layoff within three days after a layoff announcement by any of the largest 20 firms in the economy (as measured by previous year's revenue). In specifications (3) and (4) we also include an indicator variable for whether there was a layoff announcement by the same set of largest 20 firms within the three days following the layoff announcement by the firm under observation. Specification (2) and (4) also includes year fixed effects and industry fixed effects. Inclusion of these fixed effects implies that we cannot estimate the constant term in these specifications. In Panel B, we estimate the cross-effects of a layoff announcement on other firms. Column (1) reports the CAR of a top-20 firm that laid off within the last three days when another firm in the industry engages in a layoff. This is effectively the reverse effect on the industry leader when other firms follow up with a layoff. Column (2) reports the CAR of other firms in the industry when a top-20 firm lays off. Heteroskedasticity-consistent standard errors clustered at the industry level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Panel A

Dependent Variable	Cumulative Abnormal Return	Cumulative Abnormal Return	Cumulative Abnormal Return	Cumulative Abnormal Return
	(1)	(2)	(3)	(4)
Layoff by Top 20 Firm in Previous 3 days = 1	0.0049***	0.0052***	0.0042**	0.0044**
	(.0017)	(.0019)	(.0020)	(.0021)
Layoff by Top 20 Firm in Next 3 days = 1			-0.0025	-0.0027
			(.0029)	(.0029)
Constant (Baseline CAR)	-0.0087***		-0.0081***	
	(0.0013)		(0.0016)	
Year Fixed Effect		✓		✓
Industry Fixed Effect		✓		✓
Observations	4796	4796	4796	4796

Panel B

Dependent Variable	CAR of Industry Leader that Laid off Within Last 3 Days when Follower Firms Lay off	CAR of Other Firms in Industry when Industry Leader Lay off
	(1)	(2)
Constant (CAR)	0.0033***	-0.0003
	(.0011)	(.0007)
Observations	212	8286

Table 8: Follower Chracteristics, 1970-2010

In this table we report our results about the characteristics of public firms that layoff before and after the largest 20 firms in the economy as mesured by previous year's revenue. Correspondingly the sample is restricted to the Fortune 500 constituents that are not the largest 20 firms as measured by revenue. The unit of observation is at the firm level observed over every business day between 1970 and 2010. There are two indicator variables that serve as dependent variables in the analysis. The follower indicator takes a value of one if in a given year the firm announces the layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator takes a value of one if in a given year the firm announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first pair of regressions ((1) and (2)) are estimated using the entire sample (i.e. all Fortune 500 firms not in the top 20). The second pair of regressions ((3) and (4)) are estimated using only the sample of firms that laid off in a given year. By restricting the sample to this pool of firms, we are effectively relying on variation within firms that have relatively high propensity to layoff. In the last specification (5), we restrict the sample even further and include only firms that laid off within 5 days before or after a large firm announcement. Since in this case the two dependent variables are perfectly negatively correlated, we just need to estimate one regressions (instead of the pair). All the specifications include year fixed effects and firm level controls. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Follower Indicator (1)	Couterfactual Follower Indicator (2)	Follower Indicator (3)	Couterfactual Follower Indicator (4)	Follower Indicator (5)
Sample	All Firms	All Firms	All Firms that Laid Off in Given Year	All Firms that Laid Off in Given Year	All Firms that Laid off within 5 days before/after large firm layoff
Avg. Equity-linked Compensation Share	0.0741***	0.0186	0.1850	-0.1086	0.4689*
	(0.0186)	(0.0165)	(0.1546)	(0.1415)	(0.2833)
Total Compensation	-0.0014***	0.0004	-0.0043	0.0110***	-0.0073*
	(0.0004)	(0.0011)	(0.0029)	(0.0025)	(0.0038)
Ceo Tenure = 0 - 4 years	0.0229***	-0.0042	0.1458**	-0.0945*	0.2534***
	(0.0075)	(0.0056)	(0.0583)	(0.0496)	(0.0928)
Mean	0.0292	0.01763	0.2764	0.1645	0.6238
Std. Dev	0.1685	0.1316	0.4482	0.3715	0.4869
Year Fixed Effects	✓	✓	✓	✓	✓
Firm Level Controls	✓	✓	✓	✓	✓
Observations	2151	2151	228	228	101

Table 9: Follower Chracteristics, 1970-2010 (part II)

In this table we conduct the same analysis as in Table 8, except now our key dependent variable is a measure of analyst coverage. The sample is still restricted to the Fortune 500 constituents that are not the largest 20 firms as measured by revenue. The unit of observation is at the firm level observed over every business day between 1970 and 2010. There are two indicator variables that serve as dependent variables in the analysis. The follower indicator takes a value of one if in a given year the firm announces the layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator takes a value of one if in a given year the firm announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first pair of regressions ((1) and (2)) are estimated using the entire sample (i.e. all Fortune 500 firms not in the top 20). The second pair of regressions ((3) and (4)) are estimated using only the sample of firms that laid off in a given year. By restricting the sample to this pool of firms, we are effectively relying on variation within firms that have relatively high propensity to layoff. In the last specification (5), we restrict the sample even further and include only firms that laid off within 5 days before or after a large firm announcement. Since in this case the two dependent variables are perfectly negatively correlated, we just need to estimate one regressions (instead of the pair). All the specifications include year fixed effects and firm level controls. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Follower Indicator (1)	Couterfactual Follower Indicator (2)	Follower Indicator (3)	Couterfactual Follower Indicator (4)	Follower Indicator (5)
Sample	All Firms	All Firms	All Firms that Laid Off in Given Year	All Firms that Laid Off in Given Year	All Firms that Laid off within 5 days before/after large firm layoff
Analyst Coverage in Past 3 years	-0.0254***	-0.0068	-0.2061**	-0.0476	-0.1222
	(0.0093)	(0.0060)	(0.0898)	(0.0777)	(0.1935)
Mean	0.0361	0.0246	0.2464	0.1836	0.5940
Std. Dev	0.1864	0.1550	0.4439	0.3874	0.4916
Firm Fixed Effects	✓	✓	✓	✓	
Year Fixed Effects	✓	✓	✓	✓	✓
Firm Level Controls	✓	✓	✓	✓	✓
Observations	7185	7185	964	964	436

Table 10: Linking Daily Frequency Layoff Behavior to Business Cycle Outcomes, 1995-2010

This table examines whether firms that engage in high frequency clustering of layoff announcements also are the ones that are more likely to layoff in recessions. The unit of observation is at the firm level tracked monthly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). The dependent variable is an indicator variable, which takes a value of one when a firm announces a layoff in a given month, and zero otherwise. In each regression specification the main explanatory variable is the interaction of the key variable with the recession indiciator. The recession indicator takes a value of one in months classified as recession months by the NBER Business Cycle Dating Committee. The first key variable (specification (1)) is a follower indicator, which takes a value of one if in the past five years a given firm has announced a layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator (specification (2)) takes a value of one if in the past five years a given firm has announced a layoff in a 5-day window prior a large firm layoff excluding the day of the large layoff). All the specifications include month fixed effects, firm fixed effects, and firm level controls and the interaction of each of these controls with the recession indicator. The firm level controls include Earnings-Price ratio, Book-to-Market Ratio, Market Capitalization, Leverage Ratio, Years since IPO, which are all obtained from COMPUSTAT. Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified in these specifications. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Sample		
Dependent Variable	Layoff Indicator (1)	Layoff Indicator (2)
Past 5 year Follower Indicator	0.0135*** (0.0035)	
Past 5 year Counterfactual Follower Indicator		0.0120*** (0.0036)
Recession × Past 5 year Follower Indicator	0.0314*** (0.0085)	
Recession × Past 5 year Counterfactual Follower Indicator		0.0041 (0.0095)
Mean	0.0722	0.0722
Std. Dev	0.2587	0.2587
Firm Fixed Effects	✓	✓
Month Fixed Effects	✓	✓
Firm Controls and Recession Interaction	✓	✓
Observations	70170	70170

Table A1: Fisher's Method for Combining Results from Independent Scan Statistics

This table reports our results for Fisher's Method, which is a method to combind results from the independent Scan Statistics we compute for each non-overlapping interval of 20 or 60 business-days between 1970 and 2010. The top half of the table conducts this analysis for all layoffs in our sample, while the bottom half restricts the sample to layoffs in the 3 digit NAICS industries. The null hypothesis is to assume that there is uniform layoff propensity for each 20 or 60 day window. The subwindow lists the length of window under consideration for excess clustering. The test statistic is a combined p-value of all individual tests, and is distributed with a chi-squared distribution. The degrees of freedom is simply twice the number of individual tests, which is given in the third column. The last column lists the 'combined p-value' from Fisher's method. If this p-value is below 0.05 then we can reject the null of no excess clustering for the given subwindow.

Window (days)	Sub-window (days)	# of months	Test Statistic (Fisher's Mehtod)	Degrees of Freedom	p-value (Fisher's Mehtod)
All Firms					
20	1	325	507.99	650	1.000
20	3	325	626.82	650	0.736
20	5	325	708.67	650	0.055
20	10	325	781.41	650	0.0003
60	1	109	202.26	218	0.771
60	3	109	255.70	218	0.041
60	5	109	254.52	218	0.045
60	10	109	268.86	218	0.011
60	15	109	288.03	218	0.001
3-digit Industry Firms					
20	1	252	374.27	504	1.000
20	3	252	437.20	504	0.986
20	5	252	551.12	504	0.072
20	10	252	631.32	504	0.0001
60	1	83	181.26	166	0.198
60	3	83	192.81	166	0.076
60	5	83	198.53	166	0.043
60	10	83	213.82	166	0.007
60	15	83	212.48	166	0.009

Table A2: Public - Private Comparison over the Business Cycle (Robustness Tests)

This table is a robustness test for results presented in Table 2, in which the matching algorithm relies on 4-digit industry instead of 3-digit industry. The table exploits within-firm or within-industry variation to analyze differences in actual layoff behavior over the business cycle between public and private firms. The unit of observation is a the firm level tracked montly between April 1995 and December 2010. In this table, a layoff constitutes a mass layoff as defined by the Bureau of Labor Statistics under its Mass Layoff Statistics Program (see section 4.2 for further discussion). In this table we have two different dependent variables. The first dependent variable is layoff indicator, which takes a value of one if the firm engages in a mass layoff in a given month. The second dependent variable is the number of workers laid off as a share of previous year's employees. The sample includes matched public-private pairs (see section 5.1.2 for the methodology) based on size (revenue) and 4-digit NAICS industry. These specifications rely on comparing each public firm to its matched private counterpart since we include matched-pair fixed effects. All regressions include controls for previous year's log revenue and its interaction with the recession dummy, and previous year's number of employees and its interaction with the recession dummy (except when the dependent variable is share laid off). Since the month fixed effects absorb all variation over time, the effect of a recession indicator is not separately identified. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

	Sample	Matched Sample	
Dependent Variable		Layoff Indicator (1)	Share Laid Off (2)
Public Indicator		-0.0194** (0.0090)	-0.0117*** (0.0041)
Recession Indicator			
Public x Recession		0.0312* (0.0185)	0.0115** (0.0048)
Mean		0.0569	0.0155
Std. Dev		0.2318	0.0267
Quadratic Time Trend & seasonality controls			
Month Fixed Effects		✓	✓
Matched-Pair Fixed Effects		✓	✓
Log Employees and Recession Interaction		✓	
Log Revenue and Recession Interaction		✓	✓
Observations		10920	656

Table A3: Follower Chracteristics, 1970-2010 (Robustness Tests)

This table has the same results as in Table 8, but with firm fixed effects. In this table we report our results about the characteristics of public firms that layoff before and after the largest 20 firms in the economy as mesured by previous year's revenue. Correspondingly the sample is restricted to the Fortune 500 constituents that are not the largest 20 firms as measured by revenue. The unit of observation is at the firm level observed over every business day between 1970 and 2010. There are two indicator variables that serve as dependent variables in the analysis. The follower indicator takes a value of one if in a given year the firm announces the layoff in a 5-day window following a large firm layoff (including the day of the large layoff). Similarly, the counterfactual follower indicator takes a value of one if in a given year the firm announces a layoff in a 5-day window prior to a large layoff (excluding the day of the large layoff). The first pair of regressions ((1) and (2)) are estimated using the entire sample (i.e. all Fortune 500 firms not in the top 20). The second pair of regressions ((3) and (4)) are estimated using only the sample of firms that laid off in a given year. By restricting the sample to this pool of firms, we are effectively relying on variation within firms that have relatively high propensity to layoff. In the last specification (5), we restrict the sample even further and include only firms that laid off within 5 days before or after a large firm announcement. Since in this case the two dependent variables are perfectly negatively correlated, we just need to estimate one regressions (instead of the pair). All the specifications include firm fixed effects, year fixed effects and firm level controls. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Follower Indicator (1)	Couterfactual Follower Indicator (2)	Follower Indicator (3)	Couterfactual Follower Indicator (4)	Follower Indicator (5)
Sample	All Firms	All Firms	All Firms that Laid Off in Given Year	All Firms that Laid Off in Given Year	All Firms that Laid off within 5 days before/after large firm layoff
Avg. Equity-linked Compensation Share	0.1296***	0.0117	1.2822***	-0.7038**	4.1088***
	(0.0356)	(0.0282)	(0.3119)	(0.2738)	(0.4515)
Total Compensation	-0.0013***	0.0009	0.0006	0.0105**	-0.0343*
	(0.0004)	(0.0014)	(0.0047)	(0.0046)	(0.0179)
Ceo Tenure = 0 - 4 years	0.0178	-0.0062	0.3022*	-0.1993	0.9573***
	(0.0122)	(0.0091)	(0.1832)	(0.1538)	(0.2286)
Mean	0.0292	0.01763	0.2764	0.1645	0.6238
Std. Dev	0.1685	0.1316	0.4482	0.3715	0.4869
Firm Fixed Effects	✓	✓	✓	✓	
Year Fixed Effects	✓	✓	✓	✓	✓
Firm Level Controls	✓	✓	✓	✓	✓
Observations	2151	2151	228	228	101

Table A4: Stock Penalty of Layoffs: One-Month Cumulative Abnormal Returns

This table reports the one-month cumalative abnormal returns (CAR) after a layoff announcement. The sample includes daily observations from 1970 to 2010. In Column (1) the sample includes all firms that announce layoffs and are not classified as industry leaders (based on prior-year revenue). This column estimates the cumulative 1-month return of being a follower layoff firm (i.e. layoff within 1-week after a leader layoff) versus being a counterfactual-follower layoff firm (i.e. layoff within 1-week before a leader layoff). In column (2) the sample includes all firms that are industry leaders (i.e. firms that are classified as industry leaders based on prior-year revenue). This column estimates the cumulative 1-month return after a leader firm layoff for firms that have follower firms layoff within the next week versus those who do not using an indicator variable. Heteroskedasticity-consistent standard errors clustered at the industry level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Cumulative One Month Return of Firms that Lay off	Cumulative One Month Return of Firms that Lay off
Sample	Firms that Announce Layoffs (Excluding Industry Leaders)	Industry Leader Firms
	(1)	(2)
Laid off within 1-week **after** Leader Layoff = 1	0.0039	
	(.0088)	
Laid off within 1-week **before** Leader Layoff =1	-0.0168	
	(.0113)	
Industry leader with Follower Layoffs		0.0229***
		(.0068)
Constant (Baseline CAR)	0.0040	0.0023
	(.0029)	(.0022)
Observations	3125	1882

Table A5: Compassionate CEOs and Layoff Announcements over the Business Cycle, 1970-2010

This table analyzes the impact of compassionate CEOs on layoff announcement propensity over the business cycle. The unit of observation is at the firm level tracked yearly between 1970 and 2010. The sample includes all contemporaneous constitutents of Fortune 500 that ever announced a layoff. The dependent variable is an indicator variable, which takes a value of one when a firm announces a layoff in a given year, and zero otherwise. Our first measure of compassionate CEO is an indicator variable which takes a value of one, if the tenure of a given CEO at the firm before he was appointed CEO is greater than 5 years. This is used in specification (1) and (2). For specification (3) and (4) we change the cutoff from 5 years to greater than 15 years. In specifications (1) and (3) all firms in the sample are used for estimation, whereas in (2) and (4) we restrict the sample to firms that had no externally-appointed CEOs in the given year (i.e. only "home-grown CEOs). All the specifications include year-fixed effects, firm-fixed effects, and firm-level controls and the interaction of each of these controls with the recession indicator. The firm level controls include Earnings-Price ratio, Book-to-Market Ratio, Market Capitalization, Leverage Ratio, Years since IPO, which are all obtained from COMPUSTAT. Note that since we include year-fixed effects, the reccesion indicator cannot be spearately identified in these regressions. Heteroskedasticity-consistent standard errors clustered at the firm level are shown underneath the coefficient estimates in all columns. We use ***, **, and * to denote significance at the 1%, 5%, and 10% level (two-sided), respectively.

Dependent Variable	Layoff Indicator (1)	Layoff Indicator (2)	Layoff Indicator (3)	Layoff Indicator (4)
Sample	All Firms	Only Firms with "home-grown" CEOs	All Firms	Only Firms with "home-grown" CEOs
Recession x Years at Firm before CEO > 5 years	-0.0160	0.0058		
	(0.0372)	(0.0435)		
Years at Firm before CEO > 5 years	-0.0011	-0.0194		
	(0.0554)	(0.0588)		
Recession x Years at Firm before CEO > 15 years			-0.0129	0.0048
			(0.0413)	(0.0437)
Years at Firm before CEO > 15 years			-0.1005*	-0.1240**
			(0.0530)	(0.0573)
Mean	0.1438	0.1438	0.1438	0.1438
Standard Deviation	.3509	.3509	.3509	.3509
Firm Fixed Effects	✓	✓	✓	✓
Year Fixed Effects	✓	✓	✓	✓
Firm Level Controls and Recession Interactions	✓	✓	✓	✓
Observations	2295	1694	2295	1694