Downsizing: An Examination of the Consequences of Mass Layoffs

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The recession of the early 1990s brought Adownsizing@ to the vocabulary and policy discussions, but economists have had a shortage of evidence about its effects on the fundamentals of the firms. Recent papers have made event studies of the stock price and have examined the pay of top executives (Hallock ,1998; Farber and Hallock, 1999) but these have not been tied down to fundamental measures of whether the firm is more profitable afterward. As the U.S. economy slows, further news items about mass layoffs have ensured that this question remains relevant.

There have been careful studies of the fates of the workers involved (Farber, 1997), but the fates of the firms have been studied less. Apart from case studies of particular companies, we don=t

know, in general, the effects layoffs have on the financial health of a company. Are the few studies of Adumbsizing@ representative?

This paper examines mass layoffs made by U.S. companies in the 1990s as a group to determine commonalities. By examining large layoffs at large companies, this study will draw conclusions not from isolated incidents but from the general population. By matching layoff announcements to financial records from Compustat on publicly traded firms, this study is able to answer questions about the effects of layoffs upon observed measures of the firms= performance. By examining earnings, sales, and profitability of these layoff firms, and comparing them with similar firms that did not undergo such restructuring, a broader understanding of the role of these layoffs is achieved.

Controlling for the trend of past profitability reveals a positive marginal impact of layoffs on company profitability that strengthens over two years and persists up to four years. While profitability does fall at a typical company that has recently undergone layoffs, this does not mean that mass layoffs cause low profits. Prior declines in profitability are themselves good predictors of future layoffs. This explains the otherwise-puzzling regularity of

increasing stock prices with layoff announcements during profit declines: layoffs are forecasting future increases.

Section II describes the selection of these layoff companies: how the sample was constructed, the layoffs that are in it, the dating of the layoffs, and the construction of a control group. Section III discusses the time path of profitability after layoffs, to demonstrate that any reasonable set of controls indicates a significantly positive impact of layoffs on profitability. Section IV assesses the predictability of the layoff decision to show that prior profitability has some predictive ability on layoffs. Section V concludes.

Selection of companies

Since a dataset combining mass layoff events with financial information did not previously exist, the first item of research is to gather together these separate items. As typical of event studies, companies that had large layoffs in the period 1990-95 are selected based on articles in the *Wall Street Journal*. These companies are matched with financial data describing their profitability and financial status.

Construction of dataset

The *Wall Street Journal Index,* published at the end of each year, was used to gather articles under the keyword Alayoffs@ in the years 1990 to 1995. Although previous studies worried that some layoffs may not be publicized, the Worker Adjustment and Retraining Notification (WARN) Act, passed by Congress in 1988, requires employers to give notice to employees and local governments at least 60 days before plant closings or mass layoffs.

There are of course problems with using the *Wall Street Journal Index* to identify the sample. The coverage of WSJ articles is biased. While it may be presumed that the population of large layoffs at large companies is covered, smaller layoffs may be unevenly covered (Thompson, Olsen, and Dietrich 1987). This method of selecting the (non-random) sample has offsetting advantages: it follows the methodology of previous studies (e.g., Hallock, 1998; Chatrath, Ramchander, and Song, 1995; Madura, Akhigbe, and Bartunek, 1995; Caves and Krepps, 1993; and Worrell, Davidson, and Sharma, 1991), and other data sources suffer even more serious flaws. The BLS began collecting data on Mass Layoffs to restart the series in 1995, but has no information for the 1990-94 period. The BLS Displaced Workers Survey has good data about the affected workers, but scant information about the companies they left. Some states have begun

to make available their unemployment insurance records, but these are not available nationally. Finally, other sources such as the Census of Manufacturers cover only a fraction of U.S. companies. The data collected here are the best available to answer this question.

To be included the layoffs had to meet certain criteria. Layoffs were included if made by a publicly traded U.S. firm. Layoffs affecting fewer than 100 positions were not included. Not only are these not mass layoffs, but newspaper coverage of smaller companies is likely to be much less complete. The median employment size of firms that laid off was 17,000, so layoffs of fewer than 100 employees would be only a tiny fraction of the workforce. Temporary layoffs were not included. Although the BLS Survey of Displaced Workers indicates the large role of temporary layoffs in the labor market experiences of many workers, they are fundamentally different from the permanent layoffs studied here (and most newspaper articles on temporary layoffs only cover the ABig Three@ auto makers anyway).

The company names were matched to their CUSIP using *Compact Disclosure* and the Compustat manual. Data on the companies were obtained from Compustat for the years 1980-1995.

Description of layoffs in sample

For 320 companies, there were 651 announcements of layoffs of 1,700,000 positions in 1990-95. The average announcement was of 2626 positions, although the median was only 900. Although the majority of companies laid off in only one year, some made multiple announcements.

It is useful to understand the dating of the layoffs here, so of the 307 firms with complete financial data:

Table 1. Dating of Mass Layoffs in Sample

	Number of Firms Beginning Layoff	U.S. Unemployment Rate
1990	42	5.62
1991	71	6.85
1992	58	7.49
1993	37	6.91

1994	44	6.10
1995	55	5.60

Source: Column 1, author=s calculations; Column 2 Citibase, U.S. Civilian Unemployment Rate.

There is some correlation of more layoffs with the recession, since more firms began layoffs in 1991. However as the economy improved the firms did not stop making layoffs. Of course, as mentioned before, these numbers may be as representative of editorial standards as of real conditions, so macroeconomic conclusions must be tentative. However, this pattern matches Farber's findings (1997) of increased displacement rates for white-collar workers after the recession (assuming that white-collar workers are more likely to be laid off).

Control group

A control group of companies that did not lay off in 1990-95 allows more accurate comparisons to be drawn about what characteristics influence firms' layoff decisions. In this study I use 4-digit SIC codes for industries. I want to establish a comparable

control group which is better defined than simply the industry average, which may mislead since the average listed firm in many industries is smaller than the giants that dominate this particular sample. Finding such a group is not straightforward because these layoff firms have been through two separate selections: whether or not to layoff workers and whether or not this layoff will be reported and so entered into this sample. To minimize the effects of this later selection, which reflects editorial decisions not economic conditions, I assume that this selection is influenced by the size of the firm. Of course other criteria enter, such as whether the firm is a national player or only regional as well as random selections due to interest or journalistic fashion, but I have data on the sizes of the firms. I select firms from the Compustat panel which are of roughly comparable size and operate in the same industries. I measure size by employment in 1989 and cut firms with fewer than 5000 employees. These 475 companies are large enough that layoffs would have been reported, and so form an appropriate control against the 307 firms which had layoffs. Looking at these two groups of firms in the following table, we see that the mean values (as well as medians and 25^{th} and 75^{th} percentiles) of the financial statistics are quite similar in every case, especially given the large standard deviations. This

sample will form the control in subsequent analyses and regressions. Data are provided for a wide range of financial variables beyond those presented here, including overhead costs (selling, general, and administrative costs), assets and liabilities by term, capital, inventories, and others. There are also information about industry levels of each of these, although the quality varies: the industry employment data are unusable while the industry sales and earnings are fine.

Note that the data coverage changes slightly for each financial statistic. The later analysis uses all available data at each step, but will not explicitly account for the entry or exit decisions. Analyses of the continuing sub-sample yield similar results, so any bias in reporting missing values is not overwhelming.

Comparing distribution of mass layoffs to other data sources

It may be of interest to determine the distribution of these mass layoffs by large companies over the industries, to better understand how the sample looks like the broader population. As noted above, the BLS has resumed collection of data on mass layoffs, which they define as more than 50 initial unemployment claims by employees of a single firm within a state. The distribution of this paper's sample of mass layoffs by large companies would be expected to differ from the BLS distribution for a variety of reasons. The BLS sample includes temporary layoffs and layoffs by relatively small or privately held companies. This paper=s sample will differ from the BLS also since the count of workers affected is taken from company announcements, which as noted include vacancies unfilled and early retirements. Finally, the BLS data are not collected for the time period of this sample, so the earliest BLS mass layoff data is 1995. Thus, differences in the sensitivity of mass layoffs to the business cycle will also cloud the comparisons.

The BLS also collects data on displaced workers. But where the time period matches more closely, the definition of displaced is even farther from this sample's definition. Displaced workers are those with more than three years of tenure who lose a job because of

plant closing, insufficient work, or job/shift abolished. No distinction is made about whether the displaced worker was part of a mass layoff or a small layoff. While the previous figures, requiring at least 50 workers, made some control for layoffs by the smallest firms, these figures cannot. However, the time period is closer, referring to 1991-1993.

The BLS publishes data both on the number of companies and the number of employees affected by mass layoffs, and these are compared to the number of large companies and the announced number of positions at large companies affected in Table 3. The first panel compares the fraction of companies in this sample and the BLS Mass Layoff sample. The second panel compares the fraction of positions in this sample with the fraction of workers in the Mass Layoff sample and the Displaced Worker sample.

Most of the divergences are plausibly explained by the reasons above: firm size and temporary layoffs. For example, the construction industry, which represents 27.8% of the BLS firms making layoffs, 20.8% of the workers being laid off, and 11% of the displaced workers, is not one percent of this sample because construction layoffs are often temporary (this explains much of the difference between the BLS mass layoff and displaced workers) and the firms are generally small. Two industries are correspondingly

over-represented in this sample. Transport and Communications is 22.6% of the companies in this sample and 30.1% of the workers but just under 10% of any of the BLS figures. These seem plausibly explained by the large size of the typical telecommunications or utility company. Finance, Insurance, and Real Estate (FIRE) is 11.9% of the companies in this sample but 1.1% of the companies in the BLS sample, indicating that this sample over-represents them. However, while workers in FIRE make up 7.3% of this sample's layoffs but just 0.7% of BLS mass layoffs, they are 11.9% of displaced workers, indicating that the mismatch is not as bad. These divergences seem plausibly explained by the size of the firms.

Profits after a layoffs

A panel regression model, positing a linear, steady effect among firms that have recently made layoffs, finds a positive impact. Profitability is measured as the ratio of earnings (before interest and taxes, EBIT) to sales. Other measures of profitability such as earnings as a fraction of assets (ROA) or equity (ROE) give similar results, so this paper focuses on the single primary measure.

The profitability of the panel of firms is estimated in a panoply of specifications. Each firm's profitability may be first

represented as a function of its own past values and dummies for layoffs. Four lags are used, as judged by the Akaike Information Criterion. Contemporaneous values of all of the explanatory variables including layoffs are omitted to minimize endogeneity problems, particularly since layoffs are often accompanied by large charges against earnings. The specifications are reported both with and without industry fixed effects: the left columns of Table 4 and 5 have no fixed effects and the right columns include these. There may be some concern that the fixed effects can not be consistently estimated (Holtz-Eakin, Newey, and Rosen 1988). There are 2588 observations in the unbalanced panel representing up to 413 firms in the years 1988-1995.

The layoff coefficients are not necessarily revealing the structural effect of mass layoffs. Of course layoff decisions are influenced by prior experiences and are chosen by the individual firms. However the magnitude of the coefficients is still of interest as we speculate about how to move to models that would reveal the structural impact. The reduced form coefficients reported here can still tell us about the size within which the total effects can occur. This strategy is similar to the work of Smart & Waldfogel (1994) and others examining the effects of mergers on firm performance. The

coefficients show the range of actual outcomes and demonstrate that this estimated range is little changed across various specifications that include other confounding variables.

The first specification is parsimonious and includes only lagged profitability and the layoff dummies. The results are shown in the top panel of Table 4. The numbers at the top of the panel are p-values for the test of whether the variables being controlled for are significant. Evidently the lagged profitability measures are quite significant since their p-values are essentially zero, and the data strongly support industry fixed effects.

A mass layoff has an insignificant effect after just one year but then a significantly positive effect in the second and third years. The sum of the coefficients is also significantly positive, demonstrating that the layoffs are associated with substantial improvements of 2.23 percentage points in profitability (using the model with fixed effects). The insignificant coefficient after one year shows the tremendous variation in the short-term, until the positive effects come through. The short-term negative effects at some companies may either be a residual of the calamity that precipitated the layoffs, or it may represent variations in managerial quality so some firms see positive results sooner than others.

Adding further controls for the industry's profitability reflects the hypothesis that the industry may not only affect the level but also the cyclicality of firm profits. Perhaps layoffs simply occur as a signal of a trough, so the positive effect is spurious and only part of a broad cyclical recovery. However, adding industry profitability, again with or without industry fixed effects, leaves the coefficients on the layoff dummies essentially unchanged, which is the second panel of Table 4. Again, layoffs are insignificantly negative after one year but then have significantly positive effects in two and three years.

Further measures of the level of sales and earnings can be added to control for firm size and other effects of scale. These estimates are the top panel of Table 5. The p-values for the controls indicate that industry profitability and firm sales may not be significant in these specifications. The size and significance patterns of the coefficients are unchanged, however. Layoffs have an insignificantly negative short-run impact but are then followed by substantial improvements in profitability. The total effect, estimated by the sum of the layoff coefficients in the fixed effect models, is virtually unchanged at just over two percentage points.

It may still be the case that these layoffs are merely signals of other financial distress, possibly predictable from the past. One method to control for this is to add the firm's stock return to the regressors. If stock returns forecast future conditions then this will mitigate the measured effects of the layoffs. Also, employment is added since we might have worried that, while some firms announce large layoffs, others may surreptitiously shed workers. Again, however, the positive effects remain, as shown in the bottom panel of Table 5.

The time pattern of profitability after a layoff is unchanged despite all these controls. While the stock return is significant in predicting profitability, it does not change the estimated signs of the layoff coefficients. After one year the typical firm has lowered profitability by 0.46 percentage points below what would be expected (although with so much variation that this is not statistically significant). Thereafter however, the typical firm sees increased profitability after these mass layoffs. The sums of the coefficients are still significantly positive, although the total impact is lowered to just under two percentage points.

The p-values for the various controls demonstrate the surprising insignificance of other employment controls beyond the simple mass layoff dummy. This is likely due to the size of the firms considered. While a layoff of several thousand workers is a substantial event in signaling the intentions of mangers and the general outlook for the firm's future, as well as having a significant psychological impact on the remaining employees, it is generally only a small fraction of total

employment. The average layoff of 2600 workers is only 6% of the average firm's workforce. The median layoff is 5% of the median firm's employment. Since the layoffs are usually spread over several years, employment still grows after a layoff as other positions at other units are added.

If layoffs were easily predictable from past history then they would be considered to be merely a signal rather than a cause. Therefore the next section will investigate the degree to which layoffs are predictable. It may be of interest to estimate a VAR-type model to separate the effects of foreseen or unforeseen layoffs. However the short time series of available data and the difficulty of including limited-dependent variables in the VAR makes this impractical.

Since it is difficult to imagine valid instruments for layoffs we must be satisfied with laying out the basic facts. In common with the results from the literature examining the effects of mergers and acquisitions (such as Healy, Palepu, and Rubak 1990; Ravenscraft & Scherer 1987), these estimates can establish the basic stylized facts that any theory of layoffs must explain, as well as providing bounds on the size of the possible effects. While no definitive separation of

effect from correlation can be made, the robust estimates here presented make a beginning. Some theories, that layoffs have negative effects on profitability despite a positive stock-price boost, are clearly rejected. The net effect of layoffs varies between 2.25 and 1.94 percentage points, but it is positive in every specification examined.

Predicting layoffs

To what extent do prior conditions predict subsequent layoffs? If layoffs are often caused by a fall in demand (whether due to changes in taste or technological change) then sales may decline prior to layoffs. If falls in profitability generally precede layoffs then perhaps they show that the company had become uncompetitive relative to other firms. Rising overhead costs could signal a company grown fat with too many layers of management. On the other hand, if these financial variables do not in general precede layoffs then perhaps the layoffs signal private information held by the managers of dwindling health or an anticipation of declining demand.

I use the panel data to form a regression with the dependent variable as whether or not each company chose to lay off workers in each year (1990-95). On the right hand side I put sales, earnings, and profitability, each with four years of lags, but no contemporaneous values.

I estimate a random effects probit model, which specifies that:

and , where e_{it} is distributed normally, as is standard. The feature which distinguishes this panel model from a cross-sectional probit is the assumption $e_{it} = v_i + u_{it}$, so that the error in any period is partly due to a persistent firm-specific difference (v_i) and partly due to idiosyncratic noise (u_{it}). With this specification the ordinary assumptions about the error structure of the model must be modified, since e_{it} is no longer distributed independently across time

periods: errors for the same company will have a non-zero correlation, ,

for any *t* and *s* not equal. Lacking prior beliefs otherwise, I assume that this factor, , is common across all the firms. As with all random effects models, we must also assume that the firm-specific effect is independent of the explanatory variables on the right-hand side.

The first columns of Table 6 show the probit estimates for the unbalanced panel model. Each column represents a different specification: with levels of sales, earnings, and profitability; adding overhead costs; or adding excess stock returns. I would expect that the

levels of earnings and sales would be negatively related to the probability of layoff, if layoffs are undertaken by companies in financial distress. But the level of earnings is unexpectedly significantly positive at a one-year lag. The coefficient on the level of sales, however, is significantly negative at one lag. The rate of profitability correlates significantly with layoffs negatively at a one-year lag, which is in more accordance with prior notions that layoffs are made by relatively less profitable firms.

Other specifications are tried. Overhead costs, measured by selling, general, and administrative expenses, may control for whether the layoffs are undertaken in order to Atrim the fat@ from companies that have accumulated excess costs. While not themselves significant at any lag, adding overhead costs brings the positive coefficient on the two-year lag of sales to significance while leaving unchanged the significance of the other coefficients. One hypothesis, which has been discussed in the media, is that a firm will lay off workers when its stock is under-performing the market. This can be tested by including a

measure of excess returns, simply , as an explanatory variable, where is the return on the company's stock (not accounting for dividends) and is the market return (measured as S&P 500). The third column shows that this has no significant effect on the probability of layoff, and little effect on the other coefficients estimated. Other explanatory variables are tried (not reported), including employment, R&D expenditure, assets and liabilities including changes in the term structure of these, capital, employment, inventories, and the variances of the financial data. None had important or significant effects. So it seems that the model of layoffs predicted by sales, earnings, and profitability explains the data best.

To relax some of the restrictions that the random effects model imposes, such as the restriction that the random effects are uncorrelated with the right-hand side variables, we can also estimate a fixed effects Aconditional logit@ model. However this fixed effects model must impose restrictions on the cross correlations that _=0.5 while the multinomial probit allows more variation. Each model has

its own strengths, and so estimating both allows us to determine if the results depend crucially upon these assumptions.

Unlike fixed effects models of continuous data, however, the qualitative response dependent variable does not allow consistent estimation of each fixed effect, only the slope coefficients, and so the marginal effects cannot be computed. We can still make qualitative comparisons of the signs of the coefficients estimated by the logit with the signs from the random effects probit model, but other comparisons are not possible. As before, we assume:

and , but *eit* is now assumed to have a logistic distribution. Table 6 shows the results for the logit fixed effects model, where it ratifies the probit model in the three-variable case: at one lag, earnings are positively correlated while sales and profitability are negatively correlated. With the addition of overhead costs (which are, again, all insignificant), the only change is that the other

coefficients become somewhat less significant. Adding excess returns moves the coefficient on earnings out of significance (just as in the random effects model), while moving the positive coefficient on the four-year lag of profitability into significance. Evidently, then, the conclusions of the random effects model are not fragile with respect to the assumption of orthogonality with the regressors.

As the tabulation of predictions against actual layoffs shows, however, this explanation is not very successful. The bottom of Table 6 reports the actual and predicted layoffs: there were 468 company-years of layoffs but the probit only predicts 23, of which 15 are false. The logit predicts just 18 with 9 false. While the examined variables are statistically significant, most of the variation in the data remains unexplained. This may be seen as a defect of the model or it may be seen as a statement that most of the decision to make a mass layoff remains random (to the researcher). Layoffs are not mechanical responses to profitability declines or to any other financial statistic examined. This should encourage our tentative identification of the coefficients earlier found, showing that profitability rises after a layoff, as reflecting causation. If layoffs are not well predicted, given

prior information about the time-series of various financial data, but in fact aid in predicting later profitability, then they at least Granger-cause profitability increases.

Conclusion

This paper has described some of the main facts about firms that made mass layoffs and provided the basic framework into which any model of layoffs must fit. Firms making mass layoffs saw a typical improvement in profitability of about two percentage points in the years after this mass layoff. This increment in profitability is robust to various specifications that control for prior trends in profitability, earnings, sales, stock returns, and employment.

By studying each firm=s decisions on whether or not to make layoffs and estimating both a random effects probit model and a fixed effects logit model, this paper can identify the quantitatively important influences: earnings, sales, and profitability. Other financial variables were examined, to see if there was a significant correlation with a company=s choice of whether or not to make layoffs, but there were few.

These limited-dependent models also show that most of the variation in a firm's decision to make layoffs remains unexplained. While the factors such as prior profitability have some marginal value in predicting the layoff decision, much of the variation remains unexplained. This should give confidence that the previous models finding increases in profitability after a layoff are indeed showing some causal effect from those layoffs rather than a spurious connection with previous events. Many firms see falls in profitability but not all of them make layoffs. The ones that made the layoff decision see increases in profitability after the mass layoffs.

Appendix 1: Worker Adjustment and Retraining Notification (WARN) Act

The WARN Act covers all companies employing more than 100 full time employees (or more than 100 employees together working more than 4000 hours per week), and defines:

- plant closing as the permanent or temporary shutdown of a single site of operation, or even facilities or operating units within a site, affecting more than 50 employees during any 30-day period;
- mass layoff as a reduction in force that results in employment loss for more than 33% or 50 employees;
- employment loss as termination of employment, layoff for longer than six months, or a reduction in hours of more than 50% for each month in a six month period. Exemptions are made if the employees are offered transfers to other parts of the company.

Notification is not required if the closing or layoff is the result of Abusiness circumstances that were not reasonably foreseeable,@ due to natural disasters, or strikes or lockouts.

Companies that do not notify employees in accordance with the act are liable to each employee for back pay and benefits for the time (up to 60 days) when they should have provided notice, as well as $500 per day to the local government.

The act became law without the President=s signature on August 4, 1988, and notification was required as of six months after that date.

Source: *United States Statutes at Large* 1988.

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