The Effects of Mergers on Workers' Earnings and Employment[1]

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Abstract

This paper makes use of linked employer-employee data from the U.S. Census Bureau's Longitudinal Employer-Household Dynamics (LEHD) Program and matches it to data on business acquisitions from the Federal Trade Commission to examine labor market outcomes of employees at firms undergoing mergers. Earnings and employment can be observed over time for workers at both the acquired firm and the acquiring firm. The findings suggest that while wages tend to be about the same or higher for workers at these restructuring firms, turnover is significantly higher, and the costs of job-loss are large and long lasting.

Keywords: Merger, acquisition, labor market, predecessor/successor, takeover.

1 Introduction

The costs of industrial reorganizations to experienced workers have been a concern in the debate over whether takeover activity should be restricted. However, these costs are tough to quantify, and the debate instead tends to focus on the implications of restructuring on productivity, and whether the gains for the firms in question come from tax avoidance and imperfect information or actual efficiency/productivity enhancements (Jensen 1988). The major concern, of course, is that such firm restructuring often goes hand-in-hand with significant worker turnover, and the long term earnings losses experienced by displaced workers has been well-documented in the labor economics literature. However, the displaced worker literature has typically focused on mass-layoff events which tend to be related to firm deaths, not mergers and acquisitions. Moreover, it is an open question as to what the effects on earnings are for workers caught in an industrial reorganization who do not lose their jobs. Similar to the methods used in recent displaced worker literature to identify mass layoffs, this paper uses linked employer-employee administrative data to help identify mergers and acquisitions and examine their long-term effects on earnings of workers at these firms.

The primary obstacles to the analysis of the impact of mergers on labor have been the lack of extensive, longitudinal data on employees and firms as well as the difficulty in identifying acquisitions in such data. This paper combines data from the Longitudinal Employer-Household Dynamics (LEHD) program of the U.S. Census Bureau with data from the Federal Trade Commission (FTC) of the Department of Commerce to overcome these issues. Workers at both the acquired and acquiring firms are observed over time and compared to workers at firms that do not experience a major restructuring in the same time period. The findings suggest that the wages of workers at restructuring firms are actually a little higher than their counterparts, but the turnover is significantly higher starting slightly before the reference period and persisting for a long time afterwards. The paper proceeds as follows: section 2 gives a little background to the discussion, section 3 describes the various data used in the analysis and how it was assembled, section 4 explains the methods of analyzing the data, section 5 reports the results, and section 6 offers some conclusions.

2 Background

Much of the study of the consequences of mergers and acquisitions focuses largely on productivity questions. In his paper, "Takeovers: Their Causes and Consequences," Michael Jensen (1988) summarizes many of the results in this literature on the value of the firm, shareholder behavior, and managerial incentives. He acknowledges that these "corporate control transactions and the restructurings that often accompany them are frequently wrenching events in the lives of those linked to the involved organizations," but most of the analysis is directed at efficiency and productivity in the market. Because of the lack of good data on the workers at restructuring firms, the focus turns to concerns that the gains from acquisitions are illusory and based largely on tax incentives or short-term benefits. Jensen argues that the literature shows acquisitions and even the threat of takeover have real, positive benefits for the value of the firm and place heavy pressure on managers to maintain efficiency. However, he also contends that this same pressure is an incentive to form special interest groups supporting governmental restrictions on takeover activity.

Charles Brown and James Medoff (1987) use Michigan ES-202 data compiled by the Michigan Employment Security Commission to analyze mergers and acquisitions and their impact on labor. These data are quarterly data at the firm level, and they contain a field for identifying a predecessor or a successor firm in a given quarter. Brown and Medoff use this predecessor/successor information to identify acquisitions, but must use intuitive rules on overall firm employment counts to decide whether the workforce of the predecessor was acquired by the successor, in which case the event is deemed a merger. They find small negative changes in the average wage and slight increases in overall employment after a merger. However, because they only have firm-level employment and payroll, they cannot observe the compositional changes that may be driving the wage results.

Jagadeesh Gokhale, Erica Groshen, and David Neumark (1995) use smaller but more detailed survey data to explore how hostile takeovers affect implicit contracts, such as job security and steeper wage profiles, despite having little impact on current wages. Their data comes from the Community Salary Survey collected by the Federal Reserve Bank of Cleveland. The data covers select employers in the cities of Cleveland, Cincinnati, and Pittsburgh between 1980 and 1991. They identify mergers by linking employers who report ownership changes to hostile tender offers published by W.J. Grimms and Co.'s *Mergestat Review* and the *Wall Street Journal Index*. They end up with a small set of eight hostile takeovers, but they have longitudinal information on compensation for workers in a large number of occupations at these firms. The results show that wage differentials increase after hostile takeovers. Moreover, they find that job security and returns to seniority decrease for the more senior workers.

Clearly, there is much more to be learned about the effects of mergers and acquisitions (not just hostile) on labor market outcomes for a more representative sample of the U.S. and for the entire workforce at these firms. Since much of the literature on this topic suffers from data limitations, this paper turns to the recent displaced worker literature for guidance on how to approach this problem with large, linked employer-employee data. Louis Jacobson, Robert LaLonde, and Daniel Sullivan (1993) use Pennsylvania wage record data to identify mass layoffs and examine their effects on the long term earnings of workers. By observing large clusters of workers all separating from a firm in one quarter, they deduce that the worker-firm separations were not voluntary quits, and can then analyze the affected workers' earnings in a large window around the event. Because of the size of the sample they are able to study, they can estimate large regression equations with individual earnings components and a series of dummies for the quarter relative to a layoff. They find that the average worker caught in a mass layoff begins losing earnings a few quarters before the layoff, then takes a large hit at the time of the layoff, followed by some recovery but never achieving previous earnings levels.

Other papers since then have used similar data to advance the study of mass layoffs. Robert Schoeni and Michael Dardia (2000) use California administrative data, controlling for possible ownership changes when identifying mass layoffs and looking in more detail at the distribution of earnings losses. Paul Lengermann and Lars Vilhuber (2001) use the same LEHD administrative data that this paper used to look at the distribution of human capital among the job leavers in the time period leading up to the mass layoff. This paper will attempt to use some similar techniques from this branch of displaced worker literature to analyze labor market outcomes for workers caught in the middle of restructuring firms. There are certainly some similar questions to be asked since many of these restructurings go hand-in-hand with large clusters of job separations. On the other hand, there are also some new questions in that it is interesting to ask what happens to the workers who keep their jobs and if outcomes differ based on whether the worker started out at the acquired or the acquiring firm.

3 Data

3.1 General Overview

This paper makes use of administrative, linked employee-employer data put together by the LEHD program at the U.S. Census Bureau and combines it with public-use data on mergers and acquisitions provided by the FTC, and labor force data collected by the Census Bureau's Survey of Income and Program Participation (SIPP). The LEHD data provide the basis for the analysis of workers' labor market outcomes over several years, and the worker flows observed in these data are used to construct a set of candidate firm-pairs for possible merger/acquisition events. These candidate pairs are then matched to the FTC data to identify a set of clear-cut business acquisitions. Finally, responses from the SIPP on the reasons for job loss are used in the model estimation for multiply-imputing the missing data on the nature of worker-firm separations.

The LEHD program matches household and business data together using state level unemployment insurance (UI) wage record data to create a comprehensive and unique resource for data analysis (Abowd et al., 2000). Every employer covered by the UI program reports earnings for each employee receiving positive earnings during the quarter (accounting for approximately 98% of employment in each state). The UI account numbers from these data are then matched to the business data collected by the Quarterly Census of Employment and Wages (QCEW) program of the Bureau of Labor Statistics. The QCEW data provides information on industry, employment, and payroll for every establishment on the 12th of each month, as well as providing the establishments' employer identification numbers (EIN). Moreover, the micro-level data collected by the Census Bureau provides data on the workers, such as date of birth, race, and gender. Together, these data provide detailed information at the quarterly level on employment and earnings histories for every worker-firm pair.

The strengths of these data are that they are extensive and current offering an enormous sample size with rich variation. For most states the data series begins in the early 1990's and are updated on a quarterly basis (six months after the transaction date). As of the beginning of 2006, forty one states have partnered with the LEHD program, creating a longitudinal data set covering about 85% of US employment. This particular analysis uses data from 31 states accounting for approximately 69% of US employment and contains all the data for these states from the beginning of the LEHD sample through the year 2004.

There are also a number of drawbacks, which are extensively documented in Abowd et al 2000. The major weakness of using such a data set to examine labor market outcomes is that we do not know exactly why a worker leaves the sample (death, moves out of state, quits, etc) or why a worker appears at a different employer from one quarter to the next (quit and found new job, laid-off and found new job, same job but firm underwent some kind of administrative change). For firms, it is not clear when a firm ID appears/disappears from the sample whether the firm truly was born/died or whether there was some change in ownership, reporting, or coding.

The first external data set used to overcome some of these problems was the set of Early Termination Notices available on the FTC's website. The Hart-Scott-Rodino Antitrust Improvements Act was instituted in 1976 in order to allow the federal government to review mergers and acquisitions meeting certain criteria primarily regarding size of the companies involved and size of the transaction. As part of the act, the firms seeking permission to merge must wait thirty days before completing the transaction unless they file for and are granted "early termination" of this waiting period by the government. For the firms allowed to circumvent the waiting period, the FTC publishes the names of the acquiring and acquired companies and the date of early termination. The notices are available publicly on the FTC website covering acquisition activity from 1998 to the present.

The second external data set, used to gain some insight into the nature of worker/firm separations, was the 1996 SIPP panel. One of the labor force participation questions asked in this panel was for the reason an individual ceased working for their previous employer. There are 15 possible answers offered ranging from reasons such as retirement, health, or child care to layoff, quits, or new job opportunities. The individuals are interviewed in 12 waves spanning 4 years. With four rotation groups, the overall data begins in December 1995 and ends in February 2000, giving excellent overlap with the LEHD data used in this analysis. The raw internal SIPP files available to the LEHD program also contain business name information along with industry. Abowd and Stinson (2006) had great success using these variables for a probabilistic match to the Census Bureau's Business Register to obtain the EIN which can be used in linking back to the administrative data.

3.2 Method of Identifying Firm Restructurings

Even though the LEHD data does not formally identify business restructurings, a great deal can be learned about such events by observing the flows of workers between firms. Benedetto et al. (2006) describe how the LEHD program flags large flows of workers between firms and offers a glimpse into how much can be learned about how modern firms organize themselves by examining the nature of these movements. A flow-based link between hypothetical firms A and B in quarter, t, is formed by finding all work patterns in the UI work histories that look like:

	$t-1$	t	$t+1$
Worker 1 Firm A	1	1	0
Worker 1 Firm B	0	1	1
Worker 2 Firm A	1	1	0
Worker 2 Firm B	0	0	1

If there are five or more such transitions, then Firm A is flagged as a potential predecessor of Firm B in the flow-based links. This cutoff is somewhat arbitrary, but it should eliminate most coincidental links, and still offers a very large set of potentially related firms.

If all the transitions for a given link look like Worker 2, then the assumption is that the workers were continuously employed and the change took place at the "quarter boundary." Otherwise, the transition is said to be "within quarter." The timing of the link may be an important clue into the nature of the relationship. A large cluster of workers all suddenly disappearing from one firms records and appearing in another firms records essentially overnight (as in the case of the quarter boundary links) is certainly a strong indication that this is not just coincidence. On the one hand, this would seem to suggest a simple record keeping change by the UI collection agencies. On the other hand, there is probably a large incentive to make acquisitions and ownership changes official at the quarter boundary for ease of paperwork.

Benedetto et al. (2006) also categorized these links based on the relative size of the transitioning cluster to the predecessor and successor firms. Not surprisingly, clusters that were small percentages of the firms' employment dominated the links, but an interesting spike was found in the data for clusters greater than 80% of either the predecessor's prior employment or the successor's subsequent employment. A reasonable conclusion from this results is that most true ownership changes (as opposed to coincidental flows of workers between the same two firms) usually involve substantial percentages of at least one of the firms involved. While the links between firms created by this method offer strong evidence of firm restructurings, what exactly the nature of the relationship is between the linked firms remains an open question.

In order to make the jump from strong evidence to almost certain merger/acquisition, the set of candidate, flow-based links was matched with the set of Early Termination notices. This match is relatively difficult because the Early Termination notices have only business names to identify the firms involved; however, the list of potential candidates has been severely reduced with the identification of the flow-based, predecessor/successor links. Generally, such a name matching exercise would be exceedingly difficult, but considering each record contains a pair of names (the acquired firm from the Early Termination notices compared to the origin firm in the worker-flow links and the acquiring firm from the Early Termination notices compared to the destination firm in the worker-flow links) and also the approximate time of the transaction (the date on the Early Termination notice compared to the quarter of the worker-flow), the probability of finding a decent set of matches was large. Moreover, the LEHD data offers two distinct sources for business name information (the QCEW and the Business Register), thus quadrupling the probability of finding a name match.

Minimum distance matching techniques were used with very high reservation scores to insure that only the most convincing matches were used; after all, far more importance was placed on finding high quality matches than on finding a match for a large percentage of Early Termination notices. In the end 157 links were identified from the years 1998 through 2000, accounting for 1,597,529 jobs (worker-firm matches). Table 1 shows how this set of matches compares to the overall set of flow-based, predecessor/successor links using categories defined with the 80% cutoff mentioned above and the timing of the link. The most striking difference, not surprisingly, is that the matched links had transitioning clusters accounting for at least 80% of one of the firms' employment much more frequently (58%) than the overall set (12%). Moreover, of those 58%, the vast majority involved more than 80% of the predecessor's employment but less than 80% of the successor's employment. Again, this makes perfect sense since one usually thinks of an acquisition as an already large company absorbing all of another company of comparable or lesser size. The timing of the link offers less, it seems, in distinguishing acquisitions from the rest of the flow-based links. However, for the subset of links where the size of the transition is less than 80% of both the predecessor and successor, quarter boundary links are significantly more common in the matched set than in the overall set. Perhaps this implies that when the size of the link may not be significant enough to convince us that the flow is more than mere coincidence, the timing of the link can suggest a real event if the entire flow happens at the quarter boundary.

3.3 Multiple Imputation of Missing Data: Reason for Separation

The final major obstacle to this analysis is the unknown reason for a separation of a worker from the employer. The most obvious problem with not knowing this information is that the effects of job loss on earnings should be allowed to differ between voluntary and involuntary separations. On the other hand, even if the reason is known, the difference between a quit and a layoff may not be all that striking, especially in a framework such as this where a worker might quit his/her job in anticipation of layoffs due to a major firm restructuring on the horizon. In such a case, the "voluntary" separation may resemble more closely an involuntary separation since the choice was made due to firm-level events outside of the worker's control. As a result, this analysis does not distinguish between a quit and a layoff in estimating hazard rates for job loss around the time of the merger/acquisition event. Nevertheless, it is still important to distinguish between job loss due to firm-level events and separation due to shocks in the personal life of the worker (e.g. health issues, death, child care problems, or even retirement). For these reasons, an effort was made to fill in this missing information in an unintrusive way.

Since the late 1970's, the theory and techniques for multiple imputation in order to fill missing data have been developed and refined (Rubin 1996). These methods offer an analytically useful set of completed data that allows the analyst to measure the noise introduced through imputation and properly take that into account in estimating statistics and their measures of uncertainty. Adapting Rubin's notation to this missing data problem, the data can be expressed as (Y, X) where Y is a variable with some missing values (in this case the reason for separation) and X is a set of complete covariates (ie no missing values). Y can be expressed as (Y_{obs}, Y_{mis}) where Y_{obs} represents the observed values of Y and Y_{mis} represents the missing values of Y. The inclusion indicator, I, is a structure equivalent in size to Y with elements equal to 1 where Y is non-missing and 0 otherwise. The database can then be expressed by the joint distribution, $p(X, Y, I, \theta)$, where θ are unknown parameters. In this case, the missing data mechanism is said to be missing at random if

$$p(I|Y, X) = p(I|Y_{obs}, X) \tag{1}$$

which is certainly a realistic assumption in this situation since being sampled by the SIPP should be entirely unrelated to reason for job loss or even if job loss occurs. Draws are taken from the posterior predictive distribution

$$p(\widetilde{Y}|Y_{obs}, X) = \int p(\widetilde{Y}|X, \theta)p(\theta|Y_{obs}, X)d\theta \tag{2}$$

to produce L multiply-imputed completed data files (Y^{ℓ}, X) where $Y^{\ell} = (Y_{obs}, \widetilde{Y}^{\ell})$ for $\ell = 1, ..., L$. The resulting L data files are individually referred to as implicates.

One of the huge advantages of this data completion method is the ease with which statistical inference can be performed on the completed data. For a given estimand Q, the analyst calculates the estimator, q, and its variance estimator, u, on implicate, ℓ, exactly as it would be done on a complete data set. Doing this for every implicate gives $q^{(\ell)}$ and $u^{(\ell)}$ for $\ell = 1, ..., L$. From these, the following can be calculated:

$$\overline{q}_L = \sum_{\ell=1}^{L} q^{(\ell)}/L \tag{3}$$

$$b_L = \sum_{\ell=1}^{L} (q^{(\ell)} - \overline{q}_L)^2/(L-1) \tag{4}$$

$$\overline{u}_L = \sum_{\ell=1}^{L} u^{(\ell)}/L \tag{5}$$

$$T_L = (1 + 1/L)b_L + \overline{u}_L \tag{6}$$

$$\nu_L = (L-1)(1 + \overline{u}_L/((1 + 1/L)b_l)^2 \tag{7}$$

Using $\overline{q}_L$ as the estimator for Q, and T_L as the estimate of the variance of $\overline{q}_L$, inferences can then be based on a t-distribution with degrees of freedom, ν_L. (Rubin 1987).

The merged SIPP-LEHD data described earlier offers a large amount of useful information as a basis for estimating the joint distribution of the "reason for separation" variable with administrative variables. When the SIPP data was matched to the set of UI wage record histories, 13,245 records were found where the person and EIN matched and the date of separation in the SIPP was within one quarter of the observed separation in the administrative data. Using only the variables in the LEHD data available to both the matches (now with non-missing reason for separation) and the non-matches (the records to receive multiple imputations of reason for separation), a large number of stratification variables were used to break down the data into detailed sub-domains. In other words, workers with similar demographic characteristics and similar work histories (e.g. tenure and length of unemployment after separation) were grouped together. Next, the Bayes' bootstrap described by Rubin (1981) was used to sample from the posterior predictive distribution in each of these sub-domains and produce ten (in the notation above, $L = 10$) draws of the imputed reason for work.

Besides the nice features of this imputation for statistical analysis, there are strong reasons for optimism that the multiple imputation, Bayes' bootstrap method can provide quality imputes of this variable. Table 2 shows how the reason for separation variable (grouped into three categories: [1] quit and stay in the labor force, [2] lay-off, fire, or discharge, [3] exit the labor force) relates to some of the covariates in the LEHD data that are available for everyone in the sample. While there are not significant differences between the results for quits and layoffs, there are large differences for those leaving the labor force, which is the group that this paper wants to separate out anyway. The people exiting the labor force are more frequently females most likely leaving work for child care reasons. Moreover, the average age of exiters is significantly higher indicating that many from this group are retiring. The most striking difference, however, is that the number of quarters without a job after separation is dramatically higher for those exiting the labor force. For these reasons, it seems reasonable to think the imputation can do a good job of distinguishing labor force exits from quits and layoffs in the sample. The results of the imputation are summarized in the last three rows of Table 3b. The fact that labor force exits are relatively less frequent in the treatment group is encouraging since turnover is much higher for these firms, and one would not expect workers to grow older faster, have more children, or get sick at a significantly greater rate just because their employers undergo corporate restructurings.

3.4 Selecting the Population to Analyze

This analysis uses a five year window around the quarter of restructuring event to examine its effects on earnings and employment. The firms identified to have acquired another firm or to have been acquired by another firm form the basis of the treatment group. Every employee observed at these firms inside the half of the five year window leading up to the event is included in the treatment group. This group can be divided into workers who originated at the acquired firm (Type A) and those who originated at the acquiring firm (Type B). The analysis compares the outcomes of these two types of workers to a set of controls who are workers at firms that do not undergo a merger/acquisition in a given period of time.

With the enormous size of the LEHD data, one of the toughest challenges is reducing the control data to a size that makes estimation less computationally burdensome. Given that multiple imputation is already being used to address the missing data on the reason for job loss, independent random samples were drawn to form the control groups for each of the ten implicates to reduce the impact of a single draw on the estimates. All firm-year-quarters belonging to firms that were not identified to have undergone a restructuring were treated as potential controls for this analysis. Of course, not every possible merger/acquisition was identified in forming the treatment group, so it remains possible for one of the firms from this control set to in fact be involved in a restructuring. However, given the relatively small set of restructurings in comparison to the universe of firm-year-quarters, this probability is very small and will be ignored.

For each of the ten implicates, a random sample of firm-year-quarters was drawn from the overall distribution of controls weighted so as to mimic the features of the firm-year-quarters in the treatment group. The year-quarters drawn in the control sample mark the timing of the hypothetical restructuring around which a similar five year window will be examined. A set of stratification variables including state, SIC division, a seven category firm size class variable, year, and quarter were used to form the weights. Once these firm-year-quarters were selected, the set of workers forming the control group was assembled in the same fashion as the treatment group. The results of this method can be seen by comparing the control and treatment columns of Tables 3a and 3b.

4 Empirical Model

4.1 General Strategy

The overall approach was to analyze the various labor market outcomes of interest sequentially. First, a wage regression was estimated for workers in the sample just using quarters in which they had positive full quarter earnings at a treatment or control firm. From this regression, wage profiles of employees can be compared between Type A, Type B, and control workers. This regression can also provide estimates for an individual earnings component to be used in later models. Next, a logit regression was used to compare the probabilities of job loss for workers of the three types. Finally, the earnings losses were estimated for those who did lose their jobs with a regression similar to those found in the displaced worker literature. In the end, the pieces can be put together to tell a fairly detailed story of what happens to employees caught in the midst of major firm reorganizations.

4.2 Earnings Regression

The first model estimated was an earnings regression restricted to observations where workers were observed with full quarter earnings to simulate a wage rate. The dependent variable, w_{ijt}, is the log of the full quarter earnings. A worker is said to have full quarter earnings in period, t, at firm, j, if he/she has positive wages at firm j in periods $t-1$, t, and $t+1$. The natural assumption is that this wage record pattern implies continuous employment during quarter, t; therefore, the earnings in that quarter can be thought of as a quarterly wage rate. This wage rate is regressed on a set of time varying person-firm characteristics, X_{ijt}, an individual component, θ_i, a set of dummies referring to any existing separation in the near future, and dummies to identify the merger effects for workers at both the acquiring and acquired firms.

$$w_{ijt} = X_{ijt}\beta + DI'_{iJ(i,t)}\alpha + DJA'_{J(i,t)}\gamma^A + DJB'_{J(i,t)}\gamma^B + \theta_i + \varepsilon_{ijt} \tag{8}$$

$$DI'_{ij}\alpha = \sum_{0\le\tau\le M_s} DI^{\tau}_{ij}\alpha_\tau \quad where \quad DI^{\tau}_{i} = \begin{cases} 1 & \text{if worker i separates from firm j in period } t+\tau \\ 0 & \text{otherwise} \end{cases}$$

$$DJA'_{ij}\gamma^A = \sum_{-M_r\le\tau\le M_r} DJA^{\tau}_{ij}\gamma^A_\tau \quad where \quad DJA^{\tau}_{ij} = \begin{cases} 1 & \text{if firm j is acquired in period } t+\tau \\ 0 & \text{otherwise} \end{cases}$$

$$DJB'_{ij}\gamma^B = \sum_{-M_r\le\tau\le M_r} DJB^{\tau}_{ij}\gamma^B_\tau \quad where \quad DJB^{\tau}_{ij} = \begin{cases} 1 & \text{if firm j acquires another firm in period } t+\tau \\ 0 & \text{otherwise} \end{cases}$$

$J(h,s)$ identifies firm of worker h at time s.

The model is generalized from the OLS case by allowing ε_{ijt} to be an AR(1) process for every individual (ie $\varepsilon_{ijt} = \rho\varepsilon_{ijt-1} + v_{ijt}$ where v_{ijt} is white noise, $E(\varepsilon_{ijt}\varepsilon_{iks}) = 0$ for all s and all $k \neq j$, and $E(\varepsilon_{ijt}\varepsilon_{hjs}) = 0$ for all s and all $h \neq i$). The time-varying person-firm variables include an estimate of the firm wage component, year dummies, age, and observed tenure over the course of the sample. The firm wage component was separately estimated on the full sample using techniques pioneered by Abowd, Kramarz, and Margolis (1999) and later applied to the LEHD data by Abowd, Haltiwanger, Lane, and Sandusky (2001). The separately measured firm wage component was used because there is not enough variation in this sample to jointly estimate individual and firm wage components, but the previously estimated firm effect should offer a good measure to control for high wage firms in the regression. The tenure variable is potentially limited by the lower bound of the dates in the sample; however, any unobserved initial tenure should be soaked into the individual wage component, θ_i.

The results of this regression should offer some more insight into the wage questions explored by Brown and Medoff (1987). While they concluded wages decreased only slightly at merged firms, they were only able to observe a firm-level average quarterly earnings and acknowledged there could be unobserved compositional effects biasing the results. With this more detailed sample of data, these compositional changes can be controlled for, and the question of what happens to wages of workers from the acquiring and acquired firms around the time of a merger can be answered with more certainty.

4.3 Logistic Regression on Quits and Layoffs

The second piece of the puzzle is the question of how these restructurings affect the probability of job loss. The logit model was used to regress whether a worker separated from his/her employer in a given period ($m_{ijt} = 1$ if worker i separates from firm j in quarter t and $m_{ijt} = 0$ otherwise) on a set of time-varying worker-firm characteristics, X^{ℓ}_{ijt}, and the same merger-effect dummies from the initial earnings regression.

$$\begin{aligned} &\text{let } \widetilde{m}_{ijt} = X^{\ell}_{ijt}\beta^{\ell} + DJA'_{J(i,t)}\varphi^A + DJB'_{J(i,t)}\varphi^B \\ &\Pr(\text{worker } i \text{ quits or is laid-off from firm } j \text{ at time } t) = \tfrac{1}{1+e^{-\widetilde{m}_{ijt}}} \end{aligned} \quad (9)$$

The worker-firm characteristics include all the characteristics from the previous regression as well as sex and race dummies and the estimates of θ_i fully interacted with dummies for type A and B workers.

Even though previous literature has found small changes in wages and employment at restructuring firms, that is more a consequence of the typical productivity gains from corporate takeovers, and does not reflect the cost of potentially higher turnover rates. Certainly, the gains from more efficient management must be weighed against the costs of job loss, especially if the job losers during mergers face long term earnings losses similar to those caught in mass layoffs. Once again, the detail of the data allows for distinguishing the risks of job loss between workers starting at the acquired firms and those originally employed at the acquiring firm. Intuition suggests that the workers from the acquiring firms would be better matches with the organizational structure of the new merged firm and, in turn, face lower risk of job loss than the acquired workers.

4.4 Examining Earnings Losses of Quits and Layoffs

Finally, the consequences of job loss for workers at the restructuring firms were examined to see if the results from the displaced worker literature apply to the job losers in this sample. Another earnings regression was run, but this time the log of total earnings, y_{ijt}, was regressed on a set of time-varying worker-firm characteristics, X^q_{ijt}, and a set of indicators for future job loss. The model can be written as:

$$y_{ijt} = X^q_{ijt}\beta^q + DS'_{iJ(i,t)}\delta + \varepsilon_{ijt} \tag{10}$$

$$DS'_{ij}\delta = \sum_{-M_r \le \tau \le M_r} DS^\tau_{ij}\delta_\tau \quad where \quad DS^\tau_i = \begin{cases} 1 & \text{if worker i loses job at firm j in period } t+\tau \\ 0 & \text{otherwise} \end{cases}$$

$J(h, s)$ identifies firm of worker h at time s.

The major challenge with this regression was how to treat quarters of zero earnings. Clearly, some employees who lose their job will experience some quarters of no employment after the job loss. Ideally, those quarters would influence the parameter estimates properly showing the cost of job loss on future earnings. However, in a log earnings regression those observations would be dropped. Moreover, some of the zero earnings observed in this data will in fact be workers who obtained jobs in states outside of this sample. As a result, this regression was run several times with different strategies on handling the zero earnings observations and different sample restrictions in an attempt to get an upper and lower bound on the potential earnings losses faced by job losers in this sample.

In the first regression, zero earnings observations were recoded to $1 prior to taking the log, following the strategy of Kenneth Couch and Dean Lillard (1998) in their paper, "Sample Selection Rules and the Intergenerational Correlation of Earnings." As with the previous regressions, all workers at the sampled firms during the time of the event (or hypothetical event in the case of the control set) with at least three full quarters of earnings at some point in the first half of the five year window around the event were kept. The outcome of this regression can be thought of as a lower bound since clearly some of the job losers would have gotten jobs in states outside of the current LEHD sample and show up in this regression as false zeros. On the other hand, this sample does account for most of the US labor force, so it is reasonable to think that there are not too many false zeros. In an attempt to get some idea of the impact of this problem, another regression was run restricting the analysis to 15 of the original 31 states from this sample, accounting for approximately 37% of US employment. The total earnings measure used for this second regression was recalculated by summing up earnings only over these same 15 states. Presumably this regression should overstate earnings losses of job losers even more than the first regression, but how much more might give some insight into the size of this bias.

A third regression was run using the same sample as the first regression, but all observations with zero earnings were dropped from the data matrix. The resulting parameter estimates should provide an upper bound to the earnings losses experienced by the job losers in this sample, since obviously some of the zeros reflect true unemployment spells. Moreover, the log transform in this regression is more natural since there is no spike in the data at zero earnings, and no need for any recoding.

Finally, one last regression was run using a similar sample selection rule to the one used by Jacobson, LaLonde, and Sullivan (1993). In this regression, only individuals with earnings in at least one quarter of every year of the sample were kept. Zero earnings quarters were again recoded to \$1 before the log transformation. This strategy should prevent many of the false zeros from entering the regression; therefore, it should provide a more conservative estimate of earnings losses than the first regression. Nevertheless, it is still possible that some of the zero earnings quarters could still be false for very mobile workers or workers residing near state boundaries, so it is not clear on which side of the truth this estimate should lie.

5 Results

To examine the results of the wage and earnings regressions, the expected values of the dependent variable with and without the treatment effect were compared. For example, if $\ln(z)$ is the dependent variable, and it is regressed on a set of variables, W, and a treatment indicator, d, then one can calculate the expected value of $\ln(z)$ given W for either value of the treatment dummy as:

$$E(\ln(z)|W,d) = \widehat{\vartheta}W + \widehat{\kappa}d \tag{11}$$

where $\widehat{\vartheta}$ and $\widehat{\kappa}$ are the estimated regression coefficients. Transforming these expected values back to their natural scale and taking the ratio gives the following:

$$\frac{\exp(E(\ln(z)|W,d=1))}{\exp(E(\ln(z)|W,d=0))} = \exp(\widehat{\kappa}) \tag{12}$$

The interpretation of this ratio is that the expected value of z with the treatment in its natural scale is $\exp(\widehat{\kappa})$ times greater than the expected value of z without the treatment in its natural scale.

Applying this strategy to the first wage regression gives a time-series of these ratios, $\exp(\gamma_\tau^{type})$, where the treatment is to be at a firm that underwent a merger τ periods ago. Figure 1 plots $\exp(\gamma_\tau^{type})$ for $type = A$ (workers at the acquired firm) and for $type = B$ (workers at the acquiring firm), and τ ranges from 10 quarters before the reference period to 10 quarters after the reference period along the x-axis. Wages are highest for workers who started at the acquiring firm (even higher than the controls), but the wage gap closes slightly over time presumably as the workers from the acquired firm that are the best matches for the new management survive and the lower quality matches lose their jobs. The full set of parameter estimates and their measures of uncertainty can be observed in tables 4a and 4b.

Figure 2 plots the odds ratio calculated from coefficients on the treatment dummies in the job-loss, logistic regression. Turnover is generally higher for both groups at the restructuring firms, but significantly higher for those who started at the acquired firm. The hazard rate for workers who started at the acquired firm peaks right at the time of the restructuring, while the ratio for those who started at the acquiring firm peaks a few quarters after the restructuring. This suggests that the worker-firm match quality declines for some of the incumbents after the acquisition. So while the results follow the intuition that the workers at the acquired firm face the highest risk of turnover, it is interesting to see that even those from the acquiring firm face a huge risk after the predecessor's workforce has been assimilated. Moreover, given that the spike in the risk of job loss to workers at the acquiring firm coincides with the wages from the first regression evening out, one can deduce that the job losers at the acquiring firm after the restructuring event tend to be relatively high wage workers such as managers. It seems that the new, merged firm begins to take on a brand new identity from the top down, and while this new identity on average favors the incumbent staff, the adaptation substantially reduces job security for them as well. Tables 5a and 5b give the coefficient estimates and their levels of significance.

Using the ratios of transformed expected earnings, $\exp(\delta_\tau)$, calculated from the results of the final earnings regressions, figure 3 verifies that the earnings losses for job losers (as they have been defined in this analysis) is similar to the losses found in the displaced worker literature. For all four regressions, there is some earnings loss prior to job loss followed by a severe drop in earnings directly after the separation. The different strategies for handling zero earnings quarters do, however, result in vastly different estimates of the size of the earnings drop immediately after separation and of the speed and extent of earnings recovery. Not surprisingly, when the zeros are dropped from the regression, the earnings hit at the time of the separation is not nearly as large as it is in the other three regressions. The first two regressions also show nearly the same results with the 15 state curve only slightly lower than the 31 state curve, implying that the false zero problem is not very large. When the sample was restricted to those with some earnings in every year, the earnings drop at the time of the separation is essentially just as large as the lower bound, but the recovery afterwards is much steeper. This curve is more in line with previous estimates from the displaced worker literature, although all four curves essentially have a similar pattern with varying magnitudes. Tables 6-9 give the coefficient estimates and their significance for the four earnings regressions.

6 Conclusion

The results of this analysis give a fairly complete story of labor market outcomes for the average worker at a firm involved in a major corporate acquisition where at least part of the workforce of the acquired firm is merged with the acquiring firm. Wages are similar at the acquired firm to those at non-restructuring firms, and they are significantly higher at the acquiring firm. Despite good wages, however, acquisitions also imply significantly higher risk of job loss especially for workers starting out at the acquired firm. Even the workers at the acquiring firm, who might think they have more job security, experience higher risk of job loss once the acquired firms workforce has been assimilated. Not surprisingly, the costs to overall earnings for these workers who lose their jobs follows a similar pattern to what has been consistently shown in the displaced worker literature. Their earnings dip before separation, plummet immediately after separation, and only partially recover from the main earnings hit in the first couple of years after separation.

There is still much that can be done with this data set in the study of mergers and their effect on labor. Certainly it would be interesting and feasible to expand this research in much the same way that Dardia and Schoeni (2000) and Lengermann and Vilhuber (2001) expanded on the displaced worker literature. For instance, looking at the distribution of wages and earnings around the time of these restructurings, as well as the distribution of human capital for the various types of workers who stay and leave from these firms would be a natural progression. Moreover, many of the techniques used in this paper to build the data set could be expanded on. The matching techniques used to link on the Early Termination notices could be improved with the availability of high quality probabilistic matching software. The weights used to draw the control set could be constructed with more detail on geographical location and finer industry information. The imputation of "reason for job separation" might be improved by using information on firm growth rates as those have been shown to be highly correlated with turnover (Davis et al., 2006). Also, since LEHD is rapidly expanding, more states should soon be able to be incorporated into the analysis resulting in better match rates to the Early Termination notices and more accurate measures of total earnings.

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Table 1

Size of Transition Relative to Predecessor	Size of Transition Relative to Successor	All Links: Percentage of Column	All Links: Percentage of Cell That Are Quarter Boundary Links	Matched Links: Percentage of Column	Matched Links: Percentage of Cell That Are Quarter Boundary Links
Less Than 80% of Predecessor's Employment Moves To Successor	Less Than 80% of Successor's Employment Comes From Predecessor	88%	9%	42%	33%
	More Than 80% of Successor's Employment Comes From Predecessor	4%	33%	6%	25%
More Than 80% of Predecessor's Employment Moves To Successor	Less Than 80% of Successor's Employment Comes From Predecessor	4%	47%	41%	54%
	More Than 80% of Successor's Employment Comes From Predecessor	4%	63%	10%	60%

Table 2

	Training Data for Multiple Imputation of "Reason for Separation"		
Reason for Separation	Variable	Mean	Standard Deviation
Layoff	Male	0.55	0.50
	Age	35.18	12.81
	Length of Unemployment	3.47	7.83
Quit	Male	0.47	0.50
	Age	30.46	11.86
	Length of Unemployment	3.72	8.41
Exit Labor Force	Male	0.31	0.46
	Age	41.78	16.44
	Length of Unemployment	10.10	12.15

Table 3a

Firm-Year-Quarter level data		Control Group Percent	Control Group Between-Implicate Standard Deviation	Control Group Overall Standard Deviation	Treatment Group Percent	Treatment Group Between-Implicate Standard Deviation	Treatment Group Overall Standard Deviation	Matched SIPP-LEHD sample Percent	Matched SIPP-LEHD sample Between-Implicate Standard Deviation	Matched SIPP-LEHD sample Overall Standard Deviation
Year	1995							**0.29**	0.000	0.046
	1996							**25.87**	0.000	0.381
	1997							**25.47**	0.000	0.379
	1998	**32.27**	32.467	7.341	**30.57**	0.000	2.604	**23.94**	0.000	0.371
	1999	**21.15**	4.734	4.397	**21.02**	0.000	2.303	**23.62**	0.000	0.369
	2000	**46.59**	24.994	6.953	**48.41**	0.000	2.825	**0.82**	0.000	0.078
Size Class	[0,5)							**5.60**	0.000	0.206
	[5,20)	**14.00**	10.833	4.685	**13.06**	0.000	1.904	**16.27**	0.000	0.330
	[20,50)	**15.98**	5.785	4.207	**17.20**	0.000	2.133	**13.70**	0.000	0.308
	[50,100)	**12.74**	8.643	4.340	**13.38**	0.000	1.924	**11.10**	0.000	0.281
	[100,250)	**22.98**	25.830	6.568	**22.93**	0.000	2.376	**13.59**	0.000	0.307
	[250,500)	**7.18**	3.367	3.053	**7.32**	0.000	1.473	**9.50**	0.000	0.263
	>500	**27.13**	18.235	6.059	**26.11**	0.000	2.483	**30.24**	0.000	0.411
SIC division	A							**5.80**	0.000	0.203
	B	**1.17**	0.361	1.174	**1.59**	0.000	0.708	**0.44**	0.000	0.057
	C	**8.05**	9.856	4.130	**8.28**	0.000	1.558	**4.95**	0.000	0.189
	D	**13.84**	6.922	4.206	**11.15**	0.000	1.779	**10.49**	0.000	0.266
	E	**9.36**	9.577	4.202	**9.24**	0.000	1.637	**4.21**	0.000	0.174
	F	**16.18**	20.185	5.783	**16.24**	0.000	2.085	**4.39**	0.000	0.178
	G	**13.47**	5.826	4.025	**12.74**	0.000	1.885	**29.87**	0.000	0.398
	H	**8.87**	14.732	4.795	**10.19**	0.000	1.710	**4.11**	0.000	0.173
	I	**29.05**	24.442	6.641	**30.57**	0.000	2.604	**34.17**	0.000	0.412
	J							**1.56**	0.000	0.108

Table 3b

Worker-Firm level data		Control Group Percent	Control Group Between-Implicate Standard Deviation	Control Group Overall Standard Deviation	Treatment Group Percent	Treatment Group Between-Implicate Standard Deviation	Treatment Group Overall Standard Deviation	Matched SIPP-LEHD sample Percent	Matched SIPP-LEHD sample Between-Implicate Standard Deviation	Matched SIPP-LEHD sample Overall Standard Deviation
Gender	F	**49.03**	26.023	5.351	**48.79**	0.000	0.040	**53.66**	0.000	0.433
	M	**50.97**	26.023	5.351	**51.21**	0.000	0.040	**46.34**	0.000	0.433
Race	White	**60.19**	25.278	5.274	**63.88**	0.000	0.038	**77.77**	0.000	0.361
	Black	**16.96**	8.553	3.068	**14.77**	0.000	0.028	**8.64**	0.000	0.244
	Hispanic	**12.38**	15.044	4.069	**10.57**	0.000	0.024	**8.24**	0.000	0.239
	Other	**10.48**	4.613	2.253	**10.78**	0.000	0.025	**5.36**	0.000	0.196
Age	<18	**4.30**	2.679	1.717	**4.39**	0.000	0.016	**0.68**	0.000	0.071
Category	[18,25)	**25.55**	13.807	3.898	**24.22**	0.000	0.034	**25.65**	0.000	0.379
	[25,35)	**26.23**	1.407	1.247	**26.19**	0.000	0.035	**30.32**	0.000	0.399
	[35,45)	**21.66**	4.998	2.346	**22.12**	0.000	0.033	**20.58**	0.000	0.351
	[45,55)	**14.38**	3.274	1.899	**14.31**	0.000	0.028	**13.36**	0.000	0.296
	[55,65)	**6.00**	0.547	0.777	**6.57**	0.000	0.020	**6.35**	0.000	0.212
	>65	**1.87**	0.048	0.231	**2.21**	0.000	0.012	**3.07**	0.000	0.150
Reason for	1. Layoff	**11.96**	0.835	0.960	**14.50**	0.002	0.058	**16.53**	0.000	0.323
Separation	2. Quit	**56.84**	8.363	3.035	**57.28**	0.002	0.058	**73.20**	0.000	0.385
	3. Exit Labor Force	**31.19**	13.198	3.811	**28.22**	0.001	0.044	**10.27**	0.000	0.264

Table 4a: Wage Regression

Time-varying Worker-Firm Characteristics		Indicators for Worker-Firm Separations	
Variable Description	Parameter Estimate	Variable Description	Parameter Estimate
Age	**0.117**	1 Quarter Prior to Separation	**-0.0772**
	0.0211**		0.0227**
0.1*(Age squared)	**-0.109**	2 Quarters Prior to Separation	**-0.00975**
	0.0366**		0.0235
0.01*(Age cubed)	**0.00131**	3 Quarters Prior to Separation	**0.0202**
	0.00293		0.0242
Firm Wage Component	**0.384**	4 Quarters Prior to Separation	**0.0531**
	0.0229**		0.0242**
Tenure	**0.0271**	5 Quarters Prior to Separation	**0.0462**
	0.00438**		0.0258
0.1*(Tenure squared)	**-0.0716**	6 Quarters Prior to Separation	**0.0565**
	0.00951**		0.0242**
0.01*(Tenure cubed)	**0.00759**	7 Quarters Prior to Separation	**0.0662**
	0.00192**		0.0237**
1996 Indicator	**0.0656**	8 Quarters Prior to Separation	**0.0757**
	0.0386*		0.027**
1997 Indicator	**0.0511**	9 Quarters Prior to Separation	**0.0772**
	0.0315*		0.0296**
1998 Indicator	**0.0438**	10 Quarters Prior to Separation	**0.0751**
	0.0232**		0.0249**
1999 Indicator	**-0.00693**	11 Quarters Prior to Separation	**0.0728**
	0.00727		0.0298**
2001 Indicator	**-0.00822**	12 Quarters Prior to Separation	**0.104**
	0.0167		0.0266**
2002 Indicator	**-0.00376**	13 Quarters Prior to Separation	**0.0787**
	0.0106		0.0355**
Individual Wage Components		14 Quarters Prior to Separation	**0.0708**
Average	5.69		0.0298**
Standard Deviation	1.03	15 Quarters Prior to Separation	**0.0639**
			0.0254**
AR(1) coefficient	0.313	16 Quarters Prior to Separation	**0.0848**
			0.0435**
		17 Quarters Prior to Separation	**0.0538**
			0.0388
		18 Quarters Prior to Separation	**0.0564**
			0.0542

Table 4b: Wage Regression

Indicators for Quarter Relative to Restructuring Event		
Relative Quarter	Type A (initially at acquired firm)	Type B (initially at acquiring firm)
-9	**-0.0147**	**-0.000503**
	0.0219	0.0254
-8	**-0.00947**	**0.147**
	0.0205	0.0233**
-7	**-0.0347**	**0.0789**
	0.0204	0.0231**
-6	**-0.00188**	**0.145**
	0.0206	0.0232**
-5	**-0.0184**	**0.0635**
	0.0211	0.0277**
-4	**-0.00242**	**0.154**
	0.0205	0.0271**
-3	**-0.0412**	**0.13**
	0.0201**	0.0246**
-2	**0.0373**	**0.179**
	0.0193**	0.0231**
-1	**0.0132**	**0.0763**
	0.0179	0.0214**
0	**0.0455**	**0.19**
	0.0181**	0.0205**
1	**-0.0149**	**0.144**
	0.0162	0.0192**
2	**0.0133**	**0.122**
	0.0158	0.0199**
3	**-0.00854**	**0.0797**
	0.0166	0.0234**
4	**0.0623**	**0.142**
	0.0154**	0.0227**
5	**0.0484**	**0.0805**
	0.0154**	0.0193**
6	**0.0183**	**0.0979**
	0.0138	0.0142**
7	**0.0257**	**0.0118**
	0.0105**	0.0084
8	**-0.00549**	**0.12**
	0.00836	0.00455**

Table 5a: Job-loss Logistic Regression

Time-Varying Worker-Firm Characteristics in Job Loss Regression			
Variable Description	Parameter Estimate	Variable Description	Parameter Estimate
Intercept	**-0.211**	Person Wage Component (Theta)	**-0.00113**
	0.118**		0.0105
Age	**-0.0726**	Theta*(Indicator for Acquired Firm Before Acquisition)	**0.000544**
	0.0124**		0.0147
0.1*(Age Sqaured)	**0.125**	Theta*(Indicator for Acquired Firm After Acquisition)	**0.00215**
	0.0286**		0.0225
0.01*(Age Cubed)	**-0.00725**	Theta*(Indicator for Acquiring Firm Before Acquisition)	**-0.00412**
	0.00205**		0.0206
Black	**0.0933**	Theta*(Indicator for Acquiring Firm After Acquisition)	**0.00356**
	0.0573*		0.015
Hispanic	**-0.0533**	1995 Indicator	**-1.3**
	0.0554		0.811*
Initial Observed Tenure	**-0.597**	1996 Indicator	**-1**
	0.118**		0.499**
Log(Average Earnings)	**-0.223**	1997 Indicator	**-0.527**
	0.0389**		0.207**
Male	**0.163**	1998 Indicator	**0.382**
	0.0497**		0.115**
Firm Wage Component	**-0.299**	1999 Indicator	**0.0927**
	0.163**		0.112
Tenure	**0.472**	2001 Indicator	**-0.206**
	0.0403**		0.184
0.1*(Tenure Squared)	**-2.1**	2002 Indicator	**-0.295**
	0.229**		0.442
0.01*(Tenure Cubed)	**0.271**	2003 Indicator	**-0.654**
	0.0378**		0.276**

Table 5b: Job-loss Logistic Regression

Indicators for Quarter Relative to Restructuring Event		
Relative Quarter	Type A (initially at acquired firm)	Type B (initially at acquiring firm)
-10	**-2.5**	**-1.6**
	0.264**	0.299**
-9	**-2.7**	**-2.5**
	0.209**	0.289**
-8	**-3**	**-2.5**
	0.245**	0.304**
-7	**-3**	**-2.2**
	0.227**	0.273**
-6	**-3**	**-2.2**
	0.2**	0.268**
-5	**1.1**	**-0.244**
	0.169**	0.215
-4	**0.921**	**-0.096**
	0.174**	0.22
-3	**0.965**	**-0.276**
	0.164**	0.209
-2	**1.1**	**-0.0847**
	0.144**	0.201
-1	**1.3**	**0.0352**
	0.167**	0.205
0	**2.4**	**0.124**
	0.172**	0.14
1	**2**	**0.164**
	0.189**	0.145
2	**1.2**	**1.7**
	0.204**	0.148**
3	**1.3**	**0.232**
	0.207**	0.202
4	**0.801**	**0.725**
	0.205**	0.22**
5	**0.593**	**0.728**
	0.228**	0.259**
6	**0.335**	**0.1**
	0.252	0.32
7	**0.455**	**0.158**
	0.386	0.441
8	**1.2**	**0.12**
	0.314**	0.459
9	**0.75**	**-0.124**
	0.443*	0.456
10	**-15**	**-18**
	32	79

Table 6: Full sample with zeros recoded to $1 (upper bound of earnings losses)

Time-Varying Worker-Firm Characteristics		Indicators for Quarter Relative to Job Loss	
Variable Description	Parameter Estimate	Relative Quarter	Parameter Estimate
Intercept	**3.8**	-10	**0.216**
	0.0253**		0.0616**
Age	**0.185**	-9	**-0.00588**
	0.0166**		0.06
0.1*(Age Sqaured)	**-0.247**	-8	**-0.0903**
	0.0388**		0.0459**
0.01*(Age Cubed)	**0.00461**	-7	**-0.0842**
	0.00293*		0.0384**
Black	**-0.196**	-6	**-0.0213**
	0.0319**		0.0401
Hispanic	**-0.12**	-5	**0.0718**
	0.0253**		0.0366**
Male	**0.327**	-4	**0.104**
	0.0195**		0.0379**
Firm Wage Component	**1.2**	-3	**0.0356**
	0.129**		0.0393
Person Wage Component	**0.24**	-2	**-0.0046**
	0.0441**		0.0409
1995 Indicator	**-0.398**	-1	**-0.0482**
	0.104**		0.0375
1996 Indicator	**-0.564**	0	**-0.0886**
	0.173**		0.0472**
1997 Indicator	**-0.22**	1	**-2**
	0.0333**		0.177**
1998 Indicator	**-0.0803**	2	**-1.9**
	0.0195**		0.193**
1999 Indicator	**0.0251**	3	**-2.1**
	0.068		0.186**
2001 Indicator	**-0.06**	4	**-2**
	0.033		0.19**
2002 Indicator	**-0.25**	5	**-1.9**
	0.0588**		0.261**
2003 Indicator	**-0.787**	6	**-1.8**
	0.176**		0.319**
		7	**-1.9**
			0.311**
		8	**-1.9**
			0.306**
		9	**-1.8**
			0.227**
		10	**-1.8**
			0.224**

Table 7: 15 state sample with zeros recoded to $1

Time-Varying Worker-Firm Characteristics		Indicators for Quarter Relative to Job Loss	
Variable Description	Parameter Estimate	Relative Quarter	Parameter Estimate
Intercept	**4.6**	-10	**0.0012**
	0.0173**		0.026
Age	**0.155**	-9	**0.00169**
	0.00966**		0.0243
0.1*(Age Sqaured)	**-0.225**	-8	**0.00735**
	0.0224**		0.0234
0.01*(Age Cubed)	**0.0081**	-7	**-0.00994**
	0.0016**		0.0237
Black	**-0.201**	-6	**-0.032**
	0.0249**		0.0219
Hispanic	**-0.138**	-5	**-0.0457**
	0.0173**		0.0209**
Male	**0.266**	-4	**-0.00296**
	0.0168**		0.0191
Firm Wage Component	**1.2**	-3	**-0.0685**
	0.0593**		0.0181**
Person Wage Component	**0.225**	-2	**-0.0715**
	0.0364**		0.0188**
1995 Indicator	**-0.356**	-1	**-0.0966**
	0.0494**		0.0178**
1996 Indicator	**-0.287**	0	**-0.276**
	0.0237**		0.0281**
1997 Indicator	**-0.217**	1	**-0.265**
	0.0117**		0.025**
1998 Indicator	**-0.13**	2	**-0.312**
	0.00525**		0.0245**
1999 Indicator	**-0.0669**	3	**-0.224**
	0.00531**		0.026**
2001 Indicator	**0.0122**	4	**-0.229**
	0.0146		0.0231**
2002 Indicator	**0.0155**	5	**-0.192**
	0.0184		0.0149**
2003 Indicator	**0.024**	6	**-0.181**
	0.0357		0.0287**
		7	**-0.166**
			0.0302**
		8	**-0.152**
			0.0276**
		9	**-0.128**
			0.036**
		10	**-0.106**
			0.0377**

Table 8: Zeros dropped (lower bound of earnings losses)

Time-Varying Worker-Firm Characteristics		Indicators for Quarter Relative to Job Loss	
Variable Description	Parameter Estimate	Relative Quarter	Parameter Estimate
Intercept	**4.1**	-10	**0.134**
	0.0277**		0.0586**
Age	**0.173**	-9	**-0.0995**
	0.0215**		0.0705
0.1*(Age Sqaured)	**-0.235**	-8	**-0.106**
	0.0482**		0.053**
0.01*(Age Cubed)	**0.00513**	-7	**-0.147**
	0.00326*		0.0641**
Black	**-0.149**	-6	**-0.1**
	0.038**		0.113
Hispanic	**-0.109**	-5	**0.0278**
	0.0277**		0.0548
Male	**0.299**	-4	**0.0729**
	0.0202**		0.056
Firm Wage Component	**1.2**	-3	**-0.0285**
	0.175**		0.064
Person Wage Component	**0.232**	-2	**-0.0453**
	0.0396**		0.0535
1995 Indicator	**-0.525**	-1	**-0.0938**
	0.215**		0.0537
1996 Indicator	**-0.371**	0	**-0.141**
	0.149**		0.054**
1997 Indicator	**-0.268**	1	**-1.9**
	0.0564**		0.189**
1998 Indicator	**-0.0897**	2	**-1.9**
	0.0335**		0.214**
1999 Indicator	**0.0668**	3	**-2.5**
	0.112		0.388**
2001 Indicator	**-0.0747**	4	**-2.3**
	0.0417		0.285**
2002 Indicator	**-0.2**	5	**-2.3**
	0.0384**		0.461**
2003 Indicator	**-0.589**	6	**-2**
	0.196**		0.335**
		7	**-1.9**
			0.262**
		8	**-2**
			0.327**
		9	**-1.9**
			0.292**
		10	**-1.8**
			0.195**

Table 9: JLS restriction (only individuals with positive earnings in the sample every year)

Time-Varying Worker-Firm Characteristics		Indicators for Quarter Relative to Job Loss	
Variable Description	Parameter Estimate	Relative Quarter	Parameter Estimate
Intercept	**2.7**	-10	**0.149**
	0.0223**		0.101
Age	**0.183**	-9	**-0.103**
	0.0167**		0.14
0.1*(Age Sqaured)	**-0.267**	-8	**-0.117**
	0.0369**		0.0826
0.01*(Age Cubed)	**0.0106**	-7	**-0.146**
	0.00244**		0.0722**
Black	**-0.168**	-6	**-0.151**
	0.034**		0.247
Hispanic	**-0.106**	-5	**0.0368**
	0.0223**		0.0395
Male	**0.219**	-4	**0.0609**
	0.0331**		0.0402
Firm Wage Component	**0.916**	-3	**0.0383**
	0.147**		0.0401
Person Wage Component	**0.458**	-2	**-0.0169**
	0.0588**		0.0422
1995 Indicator	**-0.49**	-1	**-0.0338**
	0.251**		0.0343
1996 Indicator	**-0.676**	0	**-0.0936**
	0.354**		0.0475**
1997 Indicator	**-0.336**	1	**-1.6**
	0.0697**		0.276**
1998 Indicator	**-0.166**	2	**-1.3**
	0.0276**		0.267**
1999 Indicator	**-0.131**	3	**-1.1**
	0.0971		0.229**
2001 Indicator	**0.0231**	4	**-0.842**
	0.0198		0.289**
2002 Indicator	**-0.0377**	5	**-0.768**
	0.0394		0.361**
2003 Indicator	**-0.0241**	6	**-0.469**
	0.0859		0.129**
		7	**-0.347**
			0.134**
		8	**-0.698**
			0.31**
		9	**-0.841**
			0.24**
		10	**-1.1**
			0.252**

Figure 1

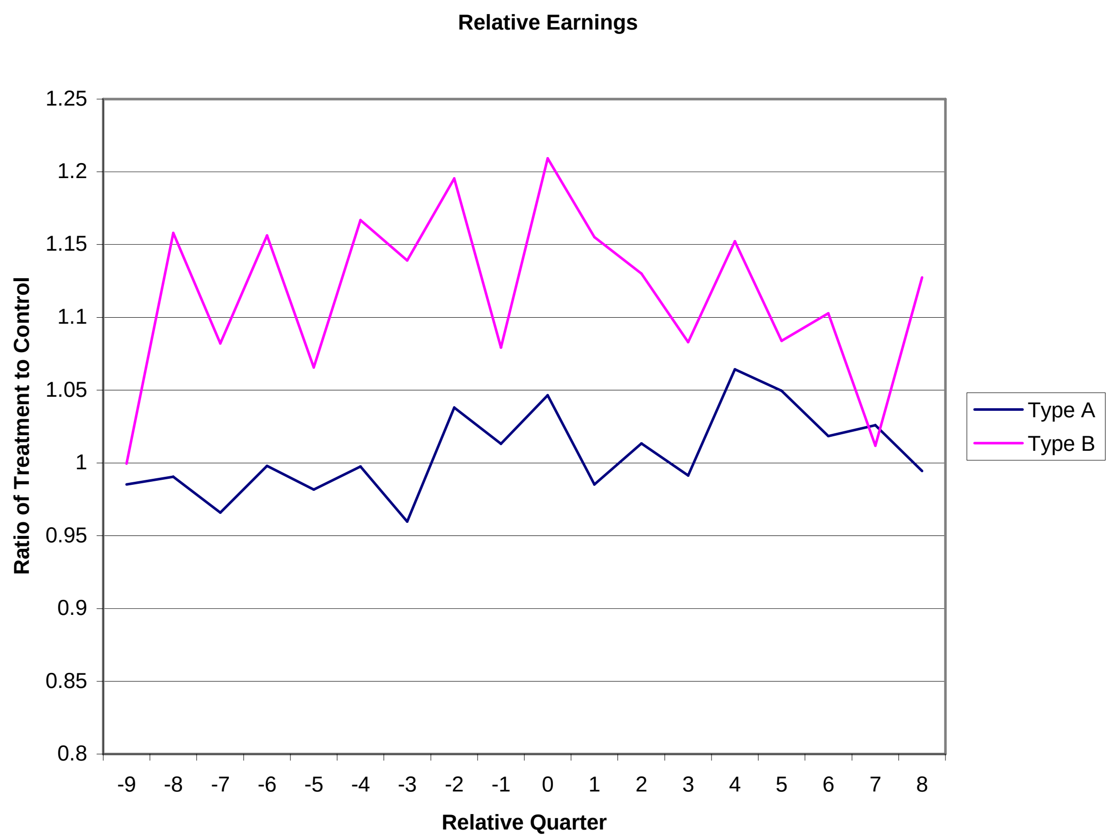

Relative Earnings
Ratio of Treatment to Control
1.25
1.2
1.15
1.1
1.05
1
0.95
0.9
0.85
0.8
-9
-8
-7
-6
-5
-4
-3
-2
-1
0
1
2
3
4
5
6
7
8
Relative Quarter
Type A
Type B

Figure 2

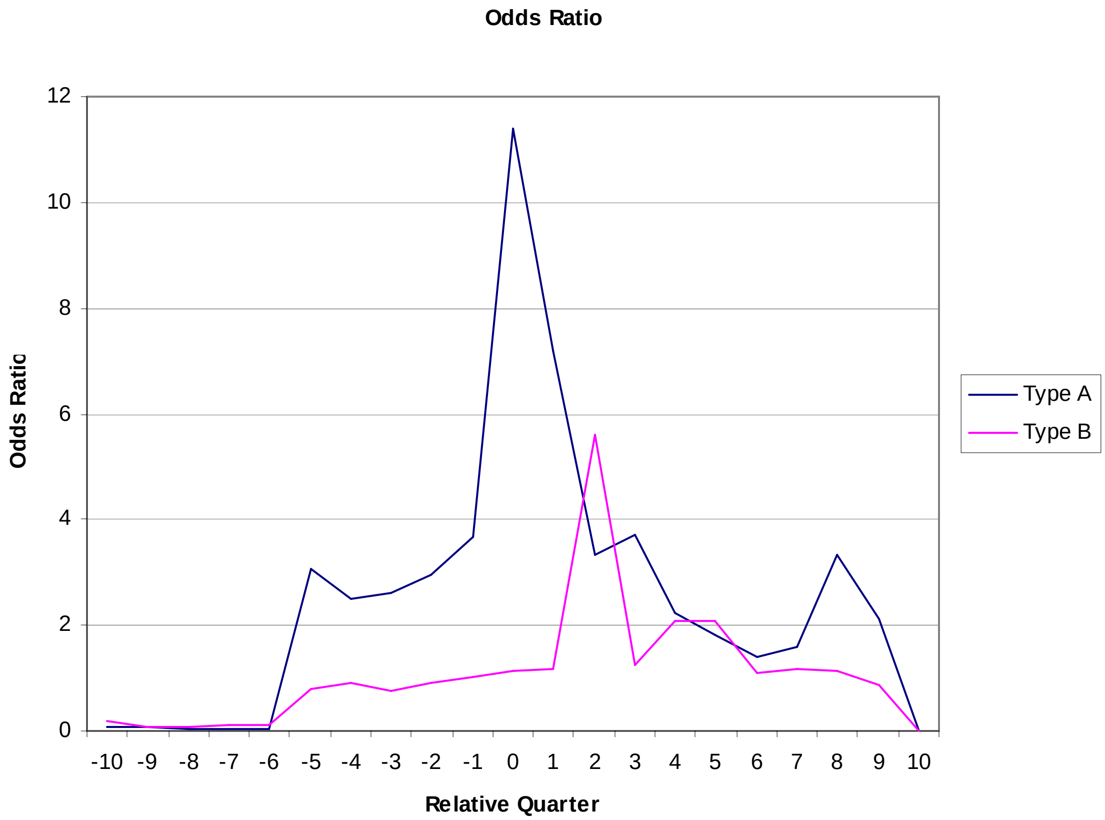
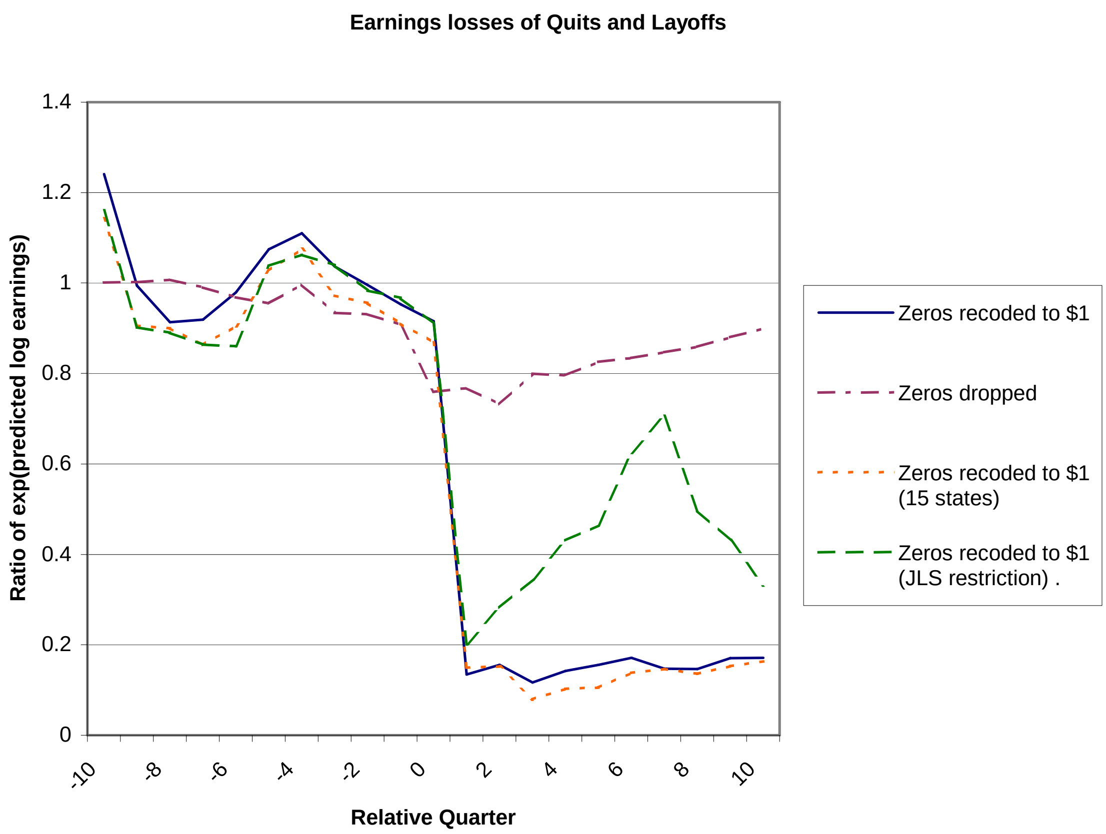

Odds Ratio
12
10
8
6
4
2
0
Odds Ratio
-10 -9 -8 -7 -6 -5 -4 -3 -2 -1 0 1 2 3 4 5 6 7 8 9 10
Relative Quarter
Type A
Type B

Figure 3



Earnings losses of Quits and Layoffs
Ratio of exp(predicted log earnings)
1.4
1.2
1
0.8
0.6
0.4
0.2
0
-10
-8
-6
-4
-2
0
2
4
6
8
10
Relative Quarter
Zeros recoded to $1
Zeros dropped
Zeros recoded to $1 (15 states)
Zeros recoded to $1 (JLS restriction) .