Earnings Losses of Displaced Workers Revisited*

June 2007

Kenneth A. Couch
University of Connecticut
Department of Economics
341 Mansfield Rd. U-1063
Storrs, CT 06269

and

Dana W. Placzek
State of Connecticut
Department of Labor
Office of Research
200 Folly Brook Blvd.
Wethersfield, CT 06109

*We thank Doreen LeBel, Jonathan Hand, Garrett McDonald, and Liam McGucken at the Connecticut Department of Labor for assistance in assembling data for this project. Staff resources and other support for the project were made available by Roger Therrien, Director of Research of the Connecticut Department of Labor.

Abstract

Longitudinal administrative data on employees and firms in Connecticut from 1993 through 2004 are used to calculate earnings losses of workers affected by mass layoffs. Estimated earnings reductions are more than 30% at the time of job loss. Six years later, they range from 12 to 15%. These estimates are similar to those commonly obtained using panel data. Thus, they correct the misperception that administrative data result in larger estimated earnings losses. Matching estimators yield the smallest calculated losses and indicate that as much as 20% of other estimates may be due to systematic differences in those affected by mass layoffs relative to continuously employed workers.

I. Introduction

Recent literature reviews conclude that job displacement results in sustained earnings losses (Fallick 1996 and Kletzer 1998). Estimates of the size of those losses vary with the type of data used and the industry within which displacement occurs (Carrington and Zaman 1994). The largest estimated losses were obtained using administrative data from Pennsylvania during the 1970s and 80s (Jacobson, Lalonde, and Sullivan 1993a). The data covered a period of high unemployment in a heavily industrialized state characterized by disproportionate job losses in manufacturing. Thus, the ability to generalize those results to more favorable economic times and states with a greater reliance on service sector employment has been questioned. To address these issues, administrative data are used to similarly examine the impact of job displacement on earnings losses of workers in the state of Connecticut from 1993 to 2004.

Beyond the relatively large estimates of earnings losses, the work of Jacobson, LaLonde, and Sullivan (JLS) also provided important methodological advancement by adapting analytical techniques developed for program evaluation (Heckman and Hotz 1989) to the context of job displacement. The same techniques as those used in the original JLS publication are initially employed here. Then, the methods are extended to include matching estimators developed since their publication (Dehejia and Wahba 1999 and 2002, Dehejia 2005, Heckman, Ichimura, and Todd 1997 and 1998, Heckman, Ichimura, Smith, and Todd 1998, and Smith and Todd 2004). Calculations of Average Treatment on the Treated and the Differenced Average Treatment on the Treated using nearest neighbor matching are adapted to the context of this study.

The results presented here for Connecticut differ from those found in JLS using data for Pennsylvania. In the period immediately following job loss, regardless of the technique employed, earnings reductions for workers displaced through mass layoff range from 32 to 33%. JLS reported immediate losses of more than 40%. Six years later using the same estimators as JLS, earnings reductions in Connecticut range from 13 to 15%. They report sustained losses of 25%. The smaller long-term impacts in Connecticut demonstrate that under more ordinary economic times, estimated losses from administrative data lie within the range observed using panel surveys. This finding resolves a longstanding conflict among the results of high quality panel studies which have used differing data sources to study earnings reductions following job loss.[1]

Using two different matching estimators, earnings losses six years after mass layoff are 12%. The structural estimators used in the original JLS study which employ a comparison group of all continuously employed workers result in estimated losses as large as 15%. While these differences are not large, they do suggest that as much as 20% of the estimated long-term earnings losses traditionally associated with mass layoff may be due to systematic differences in those who lose jobs relative to the typical person contained in continuously employed comparison groups.[2]

II. Prior Literature

The empirical literature on the earnings impact of job displacement is well established.[3] While there is consensus that displacement leads to sustained earnings

---

[1] Kornfield and Bloom (1999) similarly conclude that UI data and surveys yield similar program impacts in the evaluation of manpower training programs.
[2] This result is consistent with the work of Gibbons and Katz (1991), Lengermann and Vilhuber (2002), and Abowd, McKinney and Vilhuber (2005).
[3] For a full discussion see the literature reviews by Fallick (1996) and Kletzer (1998).

losses, the magnitude of those estimates vary systematically with the data source largely due to the availability of a comparison group for use in the analysis (Madden 1998).

The well-known Displaced Workers Supplements (DWS) to the Current Population Survey are retrospective and only ask respondents about pre-displacement employment and subsequent work histories if individuals report losing a job due to plant closure or layoff. Thus, the data do not contain a readily available comparison group. Estimates using it misstate losses by the amount of earnings growth the displaced workers would have experienced had they remained employed (Kletzer 1998). Estimates based on the DWS not employing a comparison group commonly report earnings losses around 12 or 13% the year of the survey (Carrington and Zaman 1994, Farber 1997, Kletzer 1998).[4] Madden (1998, p.101) reports larger losses using the DWS employing a comparison group of respondents matched across rotation groups in the CPS.

Longitudinal data more readily address the need for a comparison group; workers who meet definitions of being at risk of displacement can be selected and followed over time; and as some of them lose their jobs, their experiences can be contrasted with workers who remained employed. One well-known longitudinal survey, the Panel Study of Income Dynamics, has regularly been used (Ruhm 1991 and Stevens 1997) in the literature.[5] Using these data Ruhm (1991) finds that earnings declined by 16 percent in the initial year following a job loss and remained at 14 percent four years later. Stevens

[4] When estimates are calculated using the Connecticut data without the comparison group no earnings losses are observed for the mass layoff sample at the fourth year after separation.
[5] Similarly, NLSY data have been used to examine displacement among younger workers (Fairlie and Kletzer 2003). HRS data have been used in studies of older workers (Chan and Stevens 1999). Experiences of workers in other countries have also been examined using panel data such as the GSOEP (Couch 2001).

(1997) reports a drop in earnings at the time of displacement of 30 percent but by the sixth year the deficit is less than 10 percent.

The only published study of earnings losses following job displacement using administrative data that appear to be representative of most workers in a state is the research of Jacobson, LaLonde, and Sullivan (1993a) on Pennsylvania.[6] They consider the impact of large-scale layoffs on long tenure workers. The administrative data used in their analysis are drawn from wage records states keep for the purpose of calculating unemployment insurance (UI) benefits if a worker loses employment. They match these individual-level records to data from the Quarterly Census of Employment and Wages (QCEW), which are initially collected for the purpose of calculating employer UI tax liability and are enhanced for use in producing statistical reports. One advantage of these administrative records is that the wage data are from firm payrolls so they are more reliable than survey measures requiring individual recall.

Jacobson, LaLonde, and Sullivan (1993a) report earnings losses of more than 40 percent the year of displacement. Six years after the original job is lost, earnings are found to still be 25 percent below their pre-displacement level. These calculations apply to those workers who are observed re-employed.[7]

The Jacobson, LaLonde, and Sullivan study is important. It introduced the use of program evaluation techniques into the job displacement literature. More importantly it

---

[6] Schoeni and Dardia (2003) examine displaced workers in defense related industries in California and draw similar conclusions to the JLS study. Lengermann and Vilhuber (2002) consider the pattern of individual departures from firms that subsequently have mass layoffs using data for Maryland; however, they do not consider the pattern of losses following displacement. Abowd, McKinney, and Vilhuber (2005) also consider the relationship between worker attributes, mass layoffs, and firm closure but do not consider patterns of earnings loss afterwards. Stevens, Crosslin, and Lane (1994) examine trade sensitive employment sectors in three states. They consider a representative sample of UI claimants in their analysis.

[7] JLS have gone on to study the impact of retraining on long-tenure workers who lose their jobs in the state of Washington (Jacobson, LaLonde, and Sullivan 2005a, 2005b). They compare individuals who filed unemployment claims and were retrained in community colleges to those who were not.

used actual payroll data and finds the largest estimates of earnings losses associated with displacement in the literature. While the authors assert their findings are due to the superior quality of the data used in the study, reasonable doubts about the scale of the estimates exist because of location and timing.

The late 1970s and early 1980s were characterized by changes in industrial structure in the United States due to mine exhaustion, high energy prices, and resulting import penetration. Pennsylvania was heavily impacted by this restructuring. Moreover, workers in the study were continuously employed through 1979 and experienced job separations beginning in 1980. The period from January to July of 1980 was the first of the twin recessions, the second occurring from July 1981 to November 1982. Thus, the first three years of the period during which workers' earnings could potentially recover were characterized by recession.

Statistics regarding the Pennsylvania economy during the JLS study confirm that the displacements came at a difficult time. The average rate of unemployment in Pennsylvania in the time period covered by their sample was 8.3%. In the years when displacements would occur and workers would be searching for new jobs, the average unemployment rate was 9.4 and, in many months, exceeded 12%.[8] Thus, the magnitudes of their estimated earnings losses as well as weakness in the pattern of recovery could have been driven by the unusually poor economic conditions at that time.

It is also worth noting that the relative increase in employment in the service sector throughout the United States was accelerated by the restructuring that occurred in the 1980s. Prior studies have found that earnings losses in services are much smaller than

[8] These numbers are calculated using seasonally adjusted monthly rates from 1974 through 1985 available at www.paworkstats.state.pa.us .

in manufacturing (Carrington and Zaman 1994). Thus, the relative composition of employment may also have a large influence on estimated earnings losses. This implies that a more contemporary examination of similar data could result in smaller estimates.

To answer the question of whether the results from JLS for Pennsylvania are an artifact of unique circumstances at that time, a similar analysis is conducted here for the State of Connecticut. In conducting the study, every effort was made to construct a data set similar to that used by JLS and to use the same estimation methodologies.[9] The data used in the study are more recent, covering the period from 1993 through 2004. Workers in the sample are screened to be continuously employed for the first six years of the sample and can be separated from employment beginning in 1999.

The 2001 recession occurred from March to November. Thus, the first two recovery years for the earnings of job separators are prior to the peak of the 1990s business cycle. Despite the 2001 recession, comparisons of conditions across the two states at important points in the analysis show that Connecticut had a more robust economy than Pennsylvania did at the time of the JLS study.

Compared to the average rate of unemployment in Pennsylvania from 1974 to 1985 (8.3), Connecticut's (4.5) was about half that level in the period from 1993 to 2004. In the six years before job separations are examined, the average unemployment rate in PA was 7.2 versus 5.1 in CT. In the important period where job separations are examined, the unemployment rate in CT averaged 3.8 percent compared with 9.4 in PA. Peak unemployment during the period where workers would be recovering from job loss was 12.9 percent in PA versus 5.7 in CT. The economic conditions at any important

[9] The paper drew both from JLS (1993) as well as their monograph JLS (1993b) in producing a data set with a similar structure.

juncture of the sample one might care to examine were more favorable in CT than PA. For those who have questioned whether the results of JLS are simply due to the difficult economic conditions at the time, the relatively favorable conditions in CT during the period of this study provide the type of variation one would like to see to critically examine their results.

II. Estimation Methodology

Estimates of earnings loss are calculated using techniques described in Jacobson, LaLonde and Sullivan (1993a) along with matching methods. The estimators used in the JLS study are well-known by researchers in fields that employ program evaluation methods. The first estimation equation makes use of the longitudinal administrative panel data in an individual fixed-effects model as follows.

$$Y_{it} = \alpha_i + \gamma_t + X_{it}\beta + \sum_{k \geq -20} D_{it}^k \delta_k + \varepsilon_{it} \text{ (1.)}$$

Y_{it} equals earnings of worker i at time t and D_{is} is a dummy variable indicating if a worker is displaced at date s. Here, the parameters, α_i, represent the individual fixed-effects. The γ_t variables represent a set of quarterly dummy variables. X is a matrix of demographic and firm characteristics. k indexes a set of dummy variables, D, that begin 20 quarters prior to separation. The parameters δ_k capture the impact of displacement before, during, and after the event. ε_{it} is a stochastic error term.

The second model adds an individual time trend to equation (1.). The resulting equation is often referred to as a random growth model. The estimation equation is

$$Y_{it} = \alpha_i + \gamma_t + w_i t + X_{it}\beta + \sum_{k \geq -20} D_{it}^k \delta_k + \varepsilon_{it} \text{ (2.).}$$

The parameters, w_i , capture individual specific time trends in earnings. When estimated without the individual and firm characteristics (X), the individual parameters, δ_k , from equations (1.) and (2.) are plotted to show the path of earnings differences over time. In the text, equation (1.) is called a fixed-effects estimator and (2.) a time trend estimator.

For each model, JLS provide an alternative formulation in which a spline which captures the earnings impact of job loss is estimated relative to the individual period categorical variables. The elements of the spline are interacted with analysis variables primarily to demonstrate how the earnings of specific demographic or industry groups track relative to others. For the three years prior to job loss, they estimate a separate slope parameter; for all of the quarters after separation, a parameter which captures the average loss of earnings; and from the 7th quarter through the end of the sample, a separate slope.[10] They refer to these as dip, drop, and recovery parameters. Similar estimates are conducted here. The results are described in the text and tabled estimates are contained in an unpublished Appendix.

The methods used by JLS make functional form assumptions to control for individual heterogeneity across the separators and those who are continuously employed. Madden (1998) explains that if the continuously employed control group is selected from relatively more advantaged workers, estimators such as (1.) and (2.) will overstate the earnings losses associated with job displacement. Lengermann and Vilhuber (2002) along with Abowd, McKinney, and Vilhuber (2005) provide convincing evidence that workers who experience mass layoff are relatively disadvantaged.

An alternative method to control for individual heterogeneity is to choose a person who resembles each specific separator from the continuously employed workers and to use those pairs to calculate the earnings losses. The idea of matching directly on observable characteristics has been previously explored in the program evaluation

[10] The reformulated equations are presented on page 695 of JLS (1993a).

literature (LaLonde 1987 and Fraker and Maynard 1988). The problem that often arises is that in grouping the potential comparisons based on observable characteristics of separators, finding exact matches to serve as comparisons may not be possible.[11]

Rosenbaum and Rubin (1983) reduce the dimensionality of this problem by suggesting that the matches be based upon the predicted probability of the outcome studied. An additional advantage of this approach is that individuals who differ in some areas still may be close matches based on their propensity to experience an event.

More formally, Rosenbaum and Rubin (1983) define the propensity score, $p(\mathrm{x})$, as the ex ante probability of experiencing a later event given prior characteristics.

$$p(\mathrm{x}) = \Pr\{D=1|\mathrm{x}\} = \Pr\{D|\mathrm{x}\}.$$

Individuals who experience the event being studied are matched to others who do not based on these scores, which in practice are simply the predicted values from a probit or logit. The effect of the event on the people who experience it is calculated through direct comparisons. Because this calculation considers only the group impacted, it is referred to as Average Treatment on the Treated (ATT) and is calculated as

$$\tau = E\{\, E\{Y_{1i}|D_i = 1, p(x_i)\} - E\{(Y_{oi}|D_i = 0, p(x_i))\}|\, D_i = 1\}. \quad (4.)$$

The calculation takes place over the support of the distribution for those who experience the event. This is reflected in the last condition in (4.). Discussions of the calculation of ATT can be found in numerous papers that have extended this technique to an economic context (Dehejia and Wahba 1999 and 2002, Dehejia 2005, Heckman, Ichimura, and Todd 1997 and 1998, Heckman, Ichimura, Smith, and Todd 1998, and Smith and Todd 2004, Lechner 1999 and 2002 and Sianesi 2004).

[11] Using dichotomous variables, the number of categories into which potential comparisons will be sorted with n variables is 2^n.

If the propensity score matching is successful, then observable characteristics among groups with similar predicted probabilities should be equal across those who actually experience the later event and those who do not. This is referred to as the balancing property, $(D \perp X \mid p(x))$. Because observations of the outcome of interest are commonly included in the estimation of the propensity scores, satisfying the balancing property effectively equalizes observed characteristics including earnings prior to occurrence of the event being studied. Conceptually, this is similar to a fixed-effect model in removing level differences across impacted individuals and comparisons.

A series of recent papers (Heckman, Ichimura, and Todd 1997 and 1998, Heckman, Ichimura, Smith, and Todd 1998, and Smith and Todd 2004) have explored the extent of estimator bias in matching methods. A key conclusion is that the majority of the bias in program impacts arises when outcome variables are drawn from different data sources and the comparison group is constructed of individuals from different labor markets than those in the impacted group (Heckman, Ichimura, and Todd 1997, p.612). When these problems are present, they find that using a difference in differences version of the calculation of the ATT reduces estimation bias.

A differenced estimator is employed here by matching individuals based on propensity scores, calculating differences between outcome (earnings) values in each period relative to the first quarter of the sample and then taking the difference between observations for matched pairs of individuals. Choosing t' as a period prior to s (time of separation), this estimator can be written as

$$\Delta = E\{ E\{Y_{1it}|D_i = 1, p(x_i)\} - E\{Y_{1it'}|D_i = 1, p(x_i)\} -$$

$$E\{(Y_{oit} |D_i = 0, p(x_i))\} - E\{(Y_{oit'} |D_i = 0, p(x_i))\} \mid D_i = 1\}. \quad (5.)$$

The Δ denotes the differenced average treatment on the treated or DATT. Conceptually, the differencing helps eliminate any systematic bias remaining across those who change jobs and their continuously employed matches once level differences are removed through matching. Thus, it is conceptually similar though not identical to the time trend estimator originally employed by JLS.

III. Data

The data used in this study are drawn from state administrative files from CT. The UI wage file contains a code identifying employers of each individual.[12] Those codes are used to match the wage data to firm information from the QCEW. Further, the wage files contain social security numbers which are used to link them to CT Department of Motor Vehicle records in order to obtain demographic information. The resulting file contains information on 63 percent of all workers in CT. More information on this matching process is available in Appendix Sections A through E. Detailed analyses reveal that the data are highly representative of workers in the state. A brief discussion of the demographic matching and quality of the analysis file is provided here.

In July of 2002, the Connecticut DMV began requiring that social security numbers be obtained and/or verified for license applications and renewals. Normally, licenses expire on a six-year clock. One would expect that a process of systematic checking would result in a fairly random selection of license holders since most obtain them near their 16th birthday. Further, if workers are proportionately distributed among license holders, matches to the wage file should yield a representative sample of workers.

[12] Wages are converted to real 2000 values using the CPI-U. Because the data were top coded at $100,000 1987 dollars in the original JLS study (1993b, p. 57, footnote 2), after adjusting for inflation and rounding up to the nearest $5,000, they are similarly top coded here at the censoring value of $155,000. Removing the top code results in earnings losses that are typically 4 to 6 percent larger than reported in the paper although this result and associated volatility are due to a relatively small number of observations.

For this analysis, a file containing social security numbers for 70.1 percent of licenses is matched to the UI wage records. The only workers systematically excluded are those who commute to Connecticut for work; thus, the matches and the study are representative of the resident worker population.[13] In 2004:1, the matching resulted in coverage of 63 percent of all wage records. This is a match rate of 90 percent.

The original JLS study required that individuals report some positive wages each year.[14] Thus, whether matches are made at the beginning or end of the sample period, the same group of individuals will be selected. If the screening criteria that a person reports positive wages in 1993:1 and that they have some positive earnings every year are applied to the CT data, 1,009,876 individuals pass through these filters. Matching them to DMV files yields 615,973 persons or coverage of 60.99%. Again, given the expectation of proportional matching to the 70.1% DMV file, the match rate is 87%. The match rates across the DMV records and the UI wage file for CT compare well with information provided in Lengermann and Vilhuber (2002, pp. 5-6) who report that the match rate between the Maryland UI wage file and social security records is 89%.[15]

In addition to the high match rates, additional tabulations show a close correspondence between the distributions of wages for matched records relative to the

[13] The JLS data would have had matches for these individuals. Data from the 2000 Census indicate this is 3.5% of workers in Connecticut. The matches for both Connecticut and Pennsylvania also exclude workers who commute to work elsewhere. Again, this is 3.5% of Connecticut workers based on Census data.

[14] When workers do not meet the earnings criteria, they are dropped from the study. 2,751 observations are lost from the mass layoff sample because of a lapse of a complete year in reporting earnings following job separation. This represents an attrition rate of 14.8 percent. Estimates which include those observations as zeros result in earnings losses as much as 18 percentage points larger than those reported in the text. The largest estimated impact is an earnings loss of 33 percent six years after separation in the mass layoff sample. JLS report that they similarly lose 25 percent of their sample observations for this reason (p. 689) and that including them results in estimated losses which are 15 percentage points larger than the 25 percent sustained loss after six years found in their primary analysis. It is likely that the attrition rates are somewhat understated here since demographic information is obtained at the end of the study period.

[15] Similar information on the match rate for the Pennsylvania UI data to social security files is not presented in their 1993b monograph of JLS.

entire UI file. This is true even though commuters who work in the state that cannot be matched are known to have higher earnings. For example, the difference in median quarterly earnings for matched individuals and the entire wage file in 1993:1 is only $265. The average difference is $390. Examining the group of individuals who meet the criteria to be included in this study and for whom matches are obtained, median quarterly earnings are $359 lower and average earnings are $405 less than in the entire UI file.[16]

In examining distributions of sectoral employment in 1993:1 for the entire wage file relative to all individuals for whom DMV information could be obtained, they are quite similar. Differences in the percentage distribution of employment across two digit sectors is less than 2-tenths of a percentage point for 14 of the 21 industry groups examined. The largest deviation occurs for manufacturing where there is a 6 percent understatement of employment in that sector.[17] When matched observations are screened to select those meeting key sample selection criteria of this study, again, 14 of the 21 sectors have employment distributions that differ by 2-tenths of a percentage point or less. The understatement of manufacturing employment is 7 percent.

Just as matches to social security records do not give researchers perfect information, neither does matching to DMV records. Nonetheless, examination of the distribution of wages and employment among matched individuals and the entire UI file for CT does not reveal differences that indicate troubling levels of selection in the resulting samples. Combined with the primary study criterion that individuals report some earnings each year of the analysis, the use of DMV records and the UI wage file

[16] Additional information on percentiles of these distributions are contained in Appendix Section E.
[17] Percent distributions of employment by sector are contained in Appendix Section E.

provide a valid basis to study the topic of interest in this paper. Researchers may wish to explore this combination in other contexts.

Employment separations in the study are identified by tracking changes in employer codes. The validity of the coding over time is important. Internal files from the Connecticut Department of Labor regarding the reason for the change in coding were used to determine whether the lapsing of a particular employer identification number was due to plant closure or not. The Connecticut Department of Labor keeps information on these changes in order to correctly calculate UI risk ratings.[18]

In the simplest case, an individual has a sole employer in a particular quarter and is observed working for a different firm the following period. This is considered a job separation in this study if it occurs after 1998. If the individual's change in employment occurred within a year (before or after) of a drop in the firm's employment to 30 percent or more below its maximum level prior to 1999, it is considered a displacement due to mass layoff.[19] When calculating these employment changes, the figures are volatile for small employers. For this reason, those working for employers with less than 50 employees are removed from the sample.

There are individuals in the sample with multiple employers and the changes between them do not always progress in a smooth manner. If there is a transition from one employer to another, the change is assumed to occur in the last quarter in which the

[18] The calculations involved in assembling these data are very complex. The stream of programs used in creating the data and conducting the estimations are available from the journal or the authors. They can be rerun by request to the Office of Research at the Connecticut Department of Labor. The data are confidential and can not be directly accessed by any individual not employed by the Connecticut Department of Labor. However, individual arrangements for collaborative research can be made with the Director, Office of Research, as was done for this analysis.

[19] Consistent with Stevens, Crosslin, and Lane (1994), varying the rule for determining the event of mass layoff was found to have a large impact on estimated losses. For example, using a rule that mass layoff occurs when the drop in employment is 30 percent or more below the 1998 average results in sustained losses 10 percentage points larger than those reported here.

code of the prior job is observed. The separation is coded at that time and the determination is made of whether this change was associated with a mass layoff event. Appendix Sections A through E contain more information on data construction.

IV. Descriptive Information and Parameter Estimates

A table with descriptive information on the sample in 1998, the last year before job separations, is in Appendix Section F. 95,126 individuals meet screening criteria for the sample. Of those, 60,670 (64%) were continuously employed. 34,456 (36%) separated from jobs after 1998. 15,855 (17%) were displaced in mass layoffs.

The average continuously employed worker earned $14,577 per quarter in 1998; the average separator not in the mass layoff sample earned $13,174; and the average separator in the mass layoff sample earned $13,228. Within the group of separators, women are earning far less than men ($11,166 versus $15,314 respectively). Manufacturing workers have the highest earnings ($13,756) among separators.[20]

When the date of birth screening is combined with the requirement that sample members work six continuous years, the analysis file in 1998 represents prime age workers. The 10th percentile of the age distribution is 31 and the 90th is 48.

Figure 1 provides a graphical presentation of the estimated parameters for the fixed-effects and time trend models estimated for separators who are not part of the mass layoff sample. The figure contains similar patterns to those found in JLS. Using either estimator, there is little difference between the regression-adjusted earnings of the continuously employed and those who later separate prior to job loss. In the quarter after the job separation, there is a sharp drop in earnings. The estimates of earnings reductions

[20] A figure that shows the path of earnings for separators relative to the continuously employed identical to figure 1 from JLS is available in Appendix Section F.

for separators using the fixed-effects and time trend estimators the first quarter following displacement are $4,185 (32%) and $4,361 (33%) respectively. In the sixth year following separation, substantial recovery occurs and the estimated quarterly impacts average $1204 (9%) and $887 (7%) respectively.[21] The one substantive difference found here relative to the equivalent analysis in JLS is that in their samples, earnings of separators had recovered fully to their prior level within six years.[22]

Figure 2 contains an equivalent analysis for the sample of workers in the mass layoff sample. Prior to job loss, relative earnings are observed trending downwards with both estimators although they do not exhibit the sharp dip reported by JLS.[23] In the final period prior to job loss, the workers experiencing mass layoff, on average, receive about $950 in their final pay. This result is due to the receipt of what appear to be sizeable severance payments for a small portion of the sample.[24]

In the period immediately following job loss, using the fixed-effects and time trend models, the estimated reductions in earnings are $4,254 (32%), and $4,341 (33%) respectively. Six years later, the average quarterly earnings losses for that year are $1,699 (13%) and $1,923 (15%) respectively.[25] The initial losses reported here for the mass layoff sample are similar to those of other separators; however, their earnings recover more slowly. It is important to note that the earnings losses of the mass layoff

---

[21] The earnings of the group of separators in 1998:4 averaged $13,174. This figure is used in calculating the percentage losses.
[22] This analysis is contained in Figure 3 of JLS on page 698.
[23] Three working papers using administrative data to track earnings of workers who experience mass layoff also do not find a substantial dip in earnings in pre-displacement periods (Lengermann and Vilhuber (2002), Shoeni and Dardia (2003), and Hildreth, Von Wachter, and Handwerker (2007). Hildreth et al. additionally find upward spikes in earnings in the year prior to separation as reported here. Results from Lengermann and Vilhuber also indicate that it is unlikely that the larger size of the spike observed here relative to JLS is due to the impact of the WARN Act.
[24] 2,532 workers in the mass layoff sample have increases in earnings the last period prior to separation of more than $5,000. 187 workers have increases of more than $50,000.
[25] The earnings of the group of displaced workers in 1998:4 averaged $13,228. This amount was used in calculating the percentage earnings losses in the text.

sample using administrative data for Connecticut are similar in magnitude to the range of estimates obtained by other researchers using panel data such as the PSID.

Beginning with Gibbons and Katz (1991) and recently in Lengermann and Vilhuber (2002) and Abowd, McKinney, and Vilhuber (2005), economists have argued that those who separate from firms may differ systematically from continuously employed workers. Thus, calculations presented thus far may misstate earnings losses for those who actually experience a mass layoff.

Using propensity score methods to match those who experience mass layoff to continuously employed workers who have identical probabilities of displacement, differences in earnings paths of these individual pairs are taken in each pre- and post-displacement period and averaged in order to provide calculations of the impact over time. Additionally, those same pairs of matched individuals have differences taken in each period of their earnings relative to the beginning of the sample, and the difference across pairs in each period is then calculated and averaged as an alternative estimator. The results of these calculations of ATT and DATT are presented in Figure 3 for the mass layoff sample.[26]

The propensity score estimates for the mass layoff sample are similar to those obtained using the estimators from JLS. Using nearest neighbor matching, the estimated earnings loss (ATT) the quarter after the job loss is $4,221 (32%) and $4,237 (32%) with the difference in differences estimator (DATT). Six years later, the average quarterly

[26] The models used to estimate the propensity score models and other relevant information is contained in Appendix Section I along with a similar graph for Separators. The propensity score models were calculated and the balancing properties were checked using programs written by Becker and Ichino (2002). The ATT and DATT estimates were calculated using programs written by the authors. There were never less than 6 exact matches for each separator using 8 digits of precision for the predicted probabilities. In many cases, there were thousands of exact matches. Random draws were made from these available matches to use in the calculations.

earnings losses using the ATT and DATT estimators are $1,555 (12%) and $1,560(12%) respectively.[27]

The estimators from JLS that use a structural approach to estimate the earnings losses associated with mass layoff show reductions six years later ranging from 13 to 15%. The matching estimators each yield an estimated reduction in earnings six years later of 12%. While these differences are not large, they indicate that using a comparison group of all continuously employed workers may result in overstating the impact of mass layoff on the typical person who actually experiences it by as much as 20 percent.

Estimation results for the mass layoff sample using equation (2.) and including other available variables were also calculated.[28] For each variable available in the analysis, a spline was formed as described in the methods section. The spline is estimated first for specific groups of individual or industrial characteristics and then controlling for all of the interactions simultaneously. The results of those estimates are used to calculate the magnitude of earnings losses the fifth year after job loss. The table containing those results is in Appendix Section G. Ordering of the estimated impacts is similar across the two sets of estimates.

Notable patterns include larger earnings losses for workers from Business and Professional Services than from Manufacturing five years after job loss. Workers who separate from jobs in Education and Health Care have the smallest earnings losses five years following job loss. The largest earnings reductions are found among older individuals (born in the 1950s). Their estimated losses are more than three times those of

[27] The similarity of the estimated impacts using the these two estimators is likely due to observation drawn from Heckman, Ichimura, and Todd (1997) that studies like this that use common data for everyone in the study drawn from a relatively small labor market are likely to have less estimation bias and presumably a smaller range of estimates holding other factors equal.
[28] These estimates are similar in form to those contained in JLS Table 2.

the youngest generational cohort in the study (born in the 1970s). Employees in the largest firms also have smaller losses than other workers who experience mass layoff.

Past research (Carrington and Zaman 1994 and Neal 1997) has also investigated the importance of being re-employed in the same specific industry code or in the broader industry group versus transitioning to another sector in terms of influence on the earnings loss associated with displacement. An additional table in Appendix Section H contains estimates of the earnings losses of workers re-employed in the same six-digit NAICS, re-employed outside the identical NAICS code but within the broad industry aggregate, and for those who move across major industry groupings.

Both in manufacturing and non-manufacturing, those re-employed in a firm with the same NAICS code have the smallest earnings losses; those who are re-employed in a firm in the same sector have larger losses; and those who move outside the sector have the largest losses. The primary difference across industry groupings is that earnings losses for manufacturing workers are larger on average when compared to all non-manufacturing workers, and the pattern of earnings losses across the three transitions considered is more severe.[29]

The relative difference in the importance of being re-employed in the same NAICS or the same sector of the economy for manufacturing workers relative to those in non-manufacturing sectors suggests that specific skills may be more important in determining their earnings. If transitioning to a firm with a different NAICS code but in the same super sector of the economy is thought of as distance from the original

[29] This finding is similar to that contained in the Appendix table in Section G which contains calculations of earnings the fifth year after mass layoff by industry group. When only the characteristic of experiencing a mass layoff in manufacturing is considered, earnings losses of those workers are larger than most other categories of employment.

occupation, manufacturing workers appear to lose more as they transition further away from their original occupations. This is consistent with specific skills being a more important determinant of their earnings.

V. Conclusion

The research presented in this paper indicates that it is likely that the estimates of earnings losses associated with job displacement contained in the JLS study are large relative to the rest of the empirical literature due to the particularly severe economic circumstances that existed in Pennsylvania in the time period examined rather than because of properties unique to administrative data. Using similar data from Connecticut during a period of moderate economic conditions estimated earnings losses the quarter following job loss using the same techniques as JLS range from 32 to 33 percent for workers who experience mass layoff and by the same amount for other job separators. Similarly, six years after the typical worker separates from an employer in this study when it is not due to mass layoff, they continue to experience an earnings deficit of 7 to 9 percent. When an individual separates due to mass layoff, the earnings losses are sustained six years later at 13 to 15 percent. These estimates are similar in magnitude to those reported by Ruhm (1991) and Stevens (1997) using the PSID as the basis for their research. Thus, the larger sustained losses reported by JLS appear to be more unique to circumstances in Pennsylvania at the time than to administrative data in general.

Economists have also wondered whether those who experience job loss, even in mass layoffs, are systematically different than those who remain continuously employed. Matching estimators that calculate the difference in earnings across pairs (ATT) as well as the difference in differences (DATT) are employed to investigate this issue. Using each of those estimators, similar earnings losses are found at the time of mass layoff (33 percent), and six years later (12 percent). The largest long-term loss based on comparisons with all continuously employed workers was 15 percent. While this difference is not large, it does indicate that as much as 20 percent of standard estimates of earnings losses due to job displacement could be due to sample selection.

Only a few papers exist which have used administrative data to study worker experiences after job loss. Nonetheless, the estimates contained in them differ in a manner that appears to be systematic. Studies using data from periods with difficult economic conditions (Jacobson, LaLonde, and Sullivan 1993a) or using demographic information only on individuals who filed unemployment claims (Jacobson, LaLonde, and Sullivan 2005a and 2005b) would be expected to yield larger estimated earnings losses. Studies, such as this one, using data that is more representative of the typical worker who experiences a mass layoff in ordinary times are likely to find moderated impacts. To date, this is the pattern observed in the available evidence.

References

Abowd, John M., McKinney, Kevin L., and Lars Vilhuber. 2005. "The Link Between Human Capital, Mass Layoffs, and Firm Deaths." In T. Dunne, J.B. Jensen, and M.J. Roberts, Eds. **Producer Dynamics: New Evidence from Micro Data**, forthcoming, University of Chicago Press.

Becker, Sascha O. and Andrea Ichino. 2002. "Estimation of Average Treatment Effects Based on Propensity Scores." *Stata Journal*, 2(4): 358-377.

Carrington, William J. and Asad Zaman. 1994. "Interindustry Variation in the Costs of Job Displacement." *Journal of Labor Economics*, 12(2): 243-275.

Chan, Sewin and Ann Huff Stevens. 1999. "Employment and Retirement Following a Late Career Job Loss." *American Economic Review Papers and Proceedings*, 89(2): 211-216.

Couch, Kenneth A. 2001. "Earnings Losses and Hours Reductions of Displaced Workers in Germany." *Industrial and Labor Relations Review*, 54(3): 559-572.

Dehejia, Rajeev H. and Sadek Wahba. 1999. "Causal Effects in Non-Experimental Studies: Reevaluating the Evaluation of Training Programs." *Journal of the American Statistical Association*, 94(488): 1053-1062.

Dehejia, Rajeev H. and Sadek Wahba. 2002. "Propensity Score-Matching Methods for Non-Experimental Causal Studies." *Review of Economics and Statistics*, 84(1): 151-161.

Dehejia, Rajeev H. 2005. "Practical Propensity Score Matching: A Response to Smith and Todd." *Journal of Econometrics*, 125: 355-364.

Fairlie, Robert W. and Lori G. Kletzer. 2003. "The Long-Term Costs of Job Displacement Among Young Workers." *Industrial and Labor Relations Review*, 56(4): 682-698.

Fallick, Bruce C. 1996. "A Review of the Recent Empirical Literature on Displaced Workers." *Industrial and Labor Relations Review*, 50(1) October: 5-16.

Farber, Henry S. 1997. "The Changing Face of Job Loss in the United States, 1981-1995." *Brookings Papers on Economic Activity: Microeconomics*.

Fraker, Thomas and Rebecca Maynard. 1987. "The Adequacy of Comparison Group Designs for Evaluations of Employment-Related Programs." *Journal of Human Resources*, 22(2): 194-227.

Gibbons, Robert and Lawrence F. Katz. 1991. "Layoffs and Lemons." *Journal of Labor Economics*, 9(4): 351-380.

Heckman, James J. and V. Joseph Hotz. 1989. "Choosing Among Alternative Non-Experimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training." *Journal of the American Statistical Association*, 84(408): 862-974.

Heckman, James J., Ichimura, Hidehiko, and Petra Todd. 1997. "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme." *Review of Economic Studies*, 64(4): 605-654.

Heckman, James J., Ichimura, Hidehiko, and Petra Todd. 1998. "Matching as an Econometric Evaluation Estimator." *The Review of Economic Studies*, 65(2): 261-294.

Heckman, James J., Ichimura, Hidehiko, Smith, Jeffrey, and Petra Todd. 1998. "Characterizing Selection Bias Using Experimental Data." *Econometrica*, 66(5): 1017-1098.

Hildreth, Andrew K.G., Von Wachter, Till M., and Elizabeth Weber Handwerker. 2005. "Estimating the 'True Cost' of Job Loss: Evidence Using Matched Data from California: 1991-2000." University of California, Berkeley. Mimeograph.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993a. "Earnings Losses of Displaced Workers." *American Economic Review*, 83(4) September: 685-709.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1993b. **The Costs of Worker Dislocation.** W.E. Upjohn Institute for Employment Research, Kalamazoo, Michigan.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1995a. "Estimating the Returns to Community College Schooling for Displaced Workers." *Journal of Econometrics*, 125: 271-304.

Jacobson, Louis S., Robert J. LaLonde, and Daniel G. Sullivan. 1995b. "The Impact of Community College Retraining on Older Displaced Workers: Should We Teach Old Dogs New Tricks?" Industrial and Labor Relations Review, 58(3): 398-415.

Kletzer, Lori G. 1998. "Job Displacement." *The Journal of Economic Perspectives*, 12(1) Winter: 115-136.

Kornfield, Robert and Howard S. Bloom. 1999. "Measuring Program Impacts on Earnings and Employment: Do Unemployment Insurance Wage Reports from

Employers Agree with Surveys of Individuals?" *Journal of Labor Economics*, 17(1): 168-197.

LaLonde, Robert J. 1986. "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *American Economic Review*, 76(4): 604-620.

Lechner, Michael. 1999. "Earnings and Employment Effects of Continuous Off-the-Job Training in East Germany After Unification." *Journal of Business and Economic Statistics*, 17(1): 74-90.

Lechner, Michael. 2002. "Some Practical Issues in the Evaluation of Heterogeneous Labour Market Programmes by Matching Methods." *Journal of the Royal Statistical Society: Series A (Statistics in Society)*, 165(1): 59-82.

Lengermann, Paul A. and Lars Vilhuber. 2002. "Abandoning the Sinking Ship: The Composition of Worker Flows Prior to Displacement." Longitudinal Employer-Household Dynamics Technical Paper No. TP-2002-11.

Madden, Janice F. 1998. "The Distribution of Economic Losses Among Displaced Workers: Measurement Methods Matter." *Journal of Human Resources*: 93-107.

Neal, Derek. 1995. "Industry-Specific Human Capital: Evidence from Displaced Workers." *Journal of Labor Economics*, 13(4): 653-677.

Rosenbaum, P. and D. Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika*, 70: 41-45.

Ruhm, Christopher J. 1991. "Are Workers Permanently Scarred by Job Displacements?" *American Economic Review*, 81(1): 319-324.

Schoeni, Robert and Michael Dardia. 2003. "Estimates of Earnings Losses of Displaced Workers Using California Administrative Data." University of Michigan, Population Studies Center, Report No. 03-543.

Sianesi, Barbara. 2004. "An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s." *Review of Economics and Statistics*, 86(1): 133-155.

Smith, Jeffrey and Petra Todd. 2005. "Does Matching Overcome LaLonde's Critique of Non-Experimental Estimators?" *Journal of Econometrics*, 125(1-2): 305-353.

Stevens, Ann Huff. 1997. "Persistent Effects of Job Displacement: The Importance of Multiple Job Losses." *Journal of Labor Economics*, 13(1): 165-188.

Stevens, David W., Crosslin, Robert L., and Julia Lane. 1994. "The Measurement and Interpretation of Employment Displacement." *Applied Economics*, 26: 603-608.

Figure 1: Earnings Losses for Separators in Non-Mass Layoff Sample

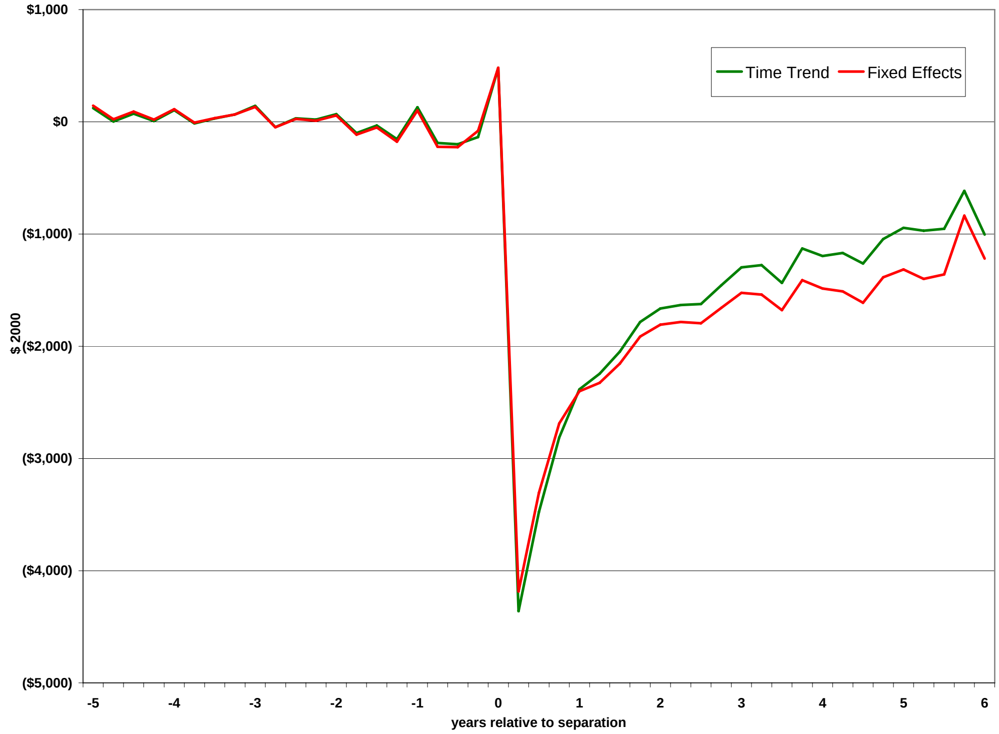

Figure 2: Earnings Losses Mass Layoff Sample

Figure 3: Earnings Losses of Mass Layoff Sample Using Matching Estimators