IZA/CEPR 11TH EUROPEAN SUMMER SYMPOSIUM IN LABOUR ECONOMICS

Supported and Hosted by the Institute for the Study of Labor (IZA)

Buch, Ammersee

17-19 September 2009

Long-Term Earnings Losses Due to Mass Layoffs During the 1982 Recession: An Analysis Using U.S. Administrative Data from 1974 to 2004

*Till von Wachter, Jae Song and Joyce Manchester

Columbia University, NBER, and CEPR

Social Security Administration

Congressional Budget Office

Preliminary – Please do not cite without permission.

The findings and views expressed here are those of the authors and should not be interpreted as those of the Social Security Administration or the Congressional Budget Office.

This paper uses longitudinal data from Social Security records covering up to 30 years of earnings to present the first national estimates of the long-term cost of job displacements during the 1982 recession. We use a new longitudinal data set containing firm size to isolate workers who separate from their stable job during a sudden mass-layoff. When we compare the workers displaced from their jobs to similar non-displaced workers, we find large immediate losses in annual earnings of 30%. After 15 to 20 years, these losses are still 20% and thus represent a significant setback in workers' life-time resources. Our estimates are robust to alternative specifications including industry-year or firm-year effects, also hold for workers with weak prior job attachment, and are strong and long-lasting for all age- and industry-groups we study. They are still large and permanent, albeit somewhat smaller, for workers displaced at the peak of the late 1980s recovery. Our estimates confirm the larger range of estimates from previous studies based on single U.S. states and selected samples of workers.

Job displacements through layoffs or plant closures are a common feature of the U.S. labor market. Conservative estimates suggest each year about 10% of workers leave their job involuntarily (e.g., Farber 2003), although the actual fraction may be much higher, in particular during recessions (e.g., Hildreth, von Wachter and Weber 2005, Farber 2007). Studies based on large administrative earnings data sets from several U.S. states suggest earnings losses after job displacements may be large and long lasting (e.g., Jacobson, Lalonde, and Sullivan 1993 [JLS], Schoeni and Dardia 2002, Couch 2006, Kodrzycki 2007).[1]

A difficulty with the existing large estimates of the cost of job loss is that they are based on administrative data covering only certain time periods and regions. This reduces the external validity of these studies for the cost of job loss for the U.S. labor market as a whole. In fact, work based on the Displaced Worker Survey (DWS) covering the entire U.S. labor market suggests the short-run cost of job loss in terms of reduced earnings is comparatively small. At present, there are no representative estimates of the long-term effects of job loss on earnings for the United States.[2]

In addition, the nature of state-specific data prevents fully including workers with zero reported earnings, since those workers may have moved out of state in response to a job loss. Thus, existing studies based on administrative data are based on selected samples. In addition, an important criticism is that often the focus is on high-seniority workers laid off in manufacturing, further reducing external validity. Moreover, both survey and administrative data sources are typically too short to allow an evaluation of the truly long-term consequences of job loss.

We exploit access to a new data source that contains longitudinal information on earnings and employment histories for a large random sample of the U.S. population spanning over 30 years. We use this data to evaluate the long-term effect of a job displacement on the development of earnings and employment for up to 20 years after a job loss during the early 1980s. In addition to allowing us to provide estimates covering almost the entire U.S. labor market, a particular advantage of our data is that we do not lose workers

[1] In addition, it has been shown that job losses reduce job stability, consumption and access to health insurance (Gruber 1997, Browning and Crossley 2001, Olson 1992).

[2] An exception is Ruhm (1991) who reports persistent earnings losses lasting up to five years after a job loss based on the PSID.

as they move between states. Thus, we are able to estimate the cost of job loss on a representative sample of workers.

As is typically the case with administrative data sources, it is difficult to clearly establish the cause of job separations with the information in our data. Thus, to isolate job displacements, we have merged the worker data with longitudinal information on the size of workers' employers created for the purpose of this project. Using the merged data, we identify job displacements as an instance when a worker permanently leaves his stable job in the course of a large and sudden reduction in the work force of his employer. To make the measures of mass layoff we construct meaningful, we follow JLS and restrict ourselves to firms that had initially at least 50 employees.

We strive to maintain as much comparability with previous studies as possible and use the study by JLS as a benchmark. Where possible, we follow JLS closely in the way we restrict our sample and date worker separations. Thus, we analyze separations occurring during mass layoffs from 1980 to 1986 for workers approximately in middle age. Similarly, as in JLS we choose workers not switching employers during this period as our control group. We analyze the effect for non-mass layoff separators separately. We also follow the literature in dealing with non-random selection of displaced workers by including worker fixed effects (e.g., Ruhm 1991, Jacobson, Lalonde, and Sullivan 1993). However, besides our more general sample, we depart from JLS in two ways. First, to maintain reasonable data processing, we focus on men and only briefly discuss how women compare. Second, we report results for broader definitions of 'stable' jobs that include workers with shorter tenure at the time of job loss than is typically analyzed in the literature.

We find job displacements during the early 1980s led to large and persistent earnings losses that last over 20 years. Those workers in stable employment from 1974 to 1979 who left their jobs in a mass-layoff during 1980 to 1986 had – compared to workers with no permanent job separation during that period – initial earnings losses of up to 30%. These losses decline to 20% after ten years but don't recover much more even 20 years after a job loss. Such estimates confirm the larger estimates from the previous literature based on more limited data sets. These findings suggest that earnings losses extend well beyond the five-year period typically studied and last up to 15-20 years after a displacement. We also find that ignoring workers with zero earnings has biased the results of previous studies downward.

Earnings losses are also substantial and last up to 20 years for displaced workers with pre-separation tenure of only three instead of six years.

The rest of the paper proceeds as follows. Section 2 presents the worker and firm data and discusses sample characteristics. Section 3 presents our main results on the long-term earnings trends of workers displaced during a mass-layoff (MLF), non-MLF separators, and non-separators. Section 4 summarizes the statistical analysis of earnings losses at job separation; there we compare the earnings trends of job separators and stable workers while controlling for permanent heterogeneity in earnings potential using worker fixed effects. Section 5 examines earnings losses by age at displacement, briefly compares earnings losses for women, and contrasts earnings losses following displacement during the peak years of the economic recovery of the late 190s. Throughout the discussion, we highlight the role of zero earnings and of weaker restrictions on pre-separation job tenure. The last section summarizes and presents directions of future research on this project.

The sample of workers used for our analysis of the long-term consequences of job separations is based on three data sets. The three sources are the 2004 Continuous Work History Sample (CWHS) active file, a 1% extract from the Master Earnings File (MEF), and a 1% extract from the Longitudinal Employee-Employer Data (LEED). The 2004 CWHS gives us the baseline sample universe and basic demographic information for 1% of individuals covered by Social Security from 1957 to 2004. In a first step, we merge this baseline sample with information on workers' total uncapped annual earnings for each job held in a given year beginning in 1978 through 2004 obtained from the MEF. Besides annual earnings and an identification number for each employer (EIN), the MEF also contains information on industry for each job. In a second step, we complement this data on uncapped earnings with information on annual earnings for each job from 1974 to 1977 from the LEED. See the Data Appendix for more detailed information.

Our sample was explicitly chosen to be comparable to the seminal work of Jacobson, Lalonde, and Sullivan (1993) (henceforth JLS), who studied the effect of job loss during the early 1980s in Pennsylvania on workers in stable employment from 1974 to 1979. JLS have become the benchmark in the literature that examines the effect of job loss using

administrative data, and we will compare our estimates explicitly to theirs below. To be comparable to JLS, however, we have to ensure that information from the LEED is comparable to that of the MEF. This is achieved in two steps. First, earnings in the LEED are capped at the Social Security taxable maximum. We follow a simple imputation procedure described in Kopczuk, Saez, and Song (2007) to make the earnings levels from the LEED comparable to those in the MEF.

Second, coverage of Social Security was extended in the late 1970s and early 1980s to encompass public administration and other sectors. To maintain consistency of our sample over time, we exclude job separations from public administration and several social services (such as health and legal services) from our sample. Following Kopczuk, Saez, and Song (2007) we also exclude job separations from agriculture. To avoid censoring of our earnings observations, these sectors remain as sources of post-separation employment. Excluding those sectors also helps to avoid changes in employer identification numbers (EIN) occurring due to administrative reasons. The exclusions also help to smooth the incidence of job separation considerably in the period from 1980 to 1987, when public administration was gradually added to the sample, and in 1978, the year we change data sources for uncapped earnings.

From this sample, we extract two groups of male workers with high attachment to their employer. First, our main sample contains workers in stable employment from 1974 to 1979. Second, we also keep a sample of workers in stable employment from 1977 to 1979. A criticism of JLS has been that by focusing on workers with six or more years of tenure at job loss, they isolate those workers bound to experience the largest losses. Thus, the comparison of the effect of job separation for workers with high job tenure (six years) with workers with shorter job tenure (three years) is of particular interest.

In our final restriction we follow JLS and require that workers are born in or after 1930. This implies that the average age at the time of job separation is roughly 40 years, and that the majority of the sample is in their prime working years during the follow-up period. Thereby, we avoid having to explicitly model retirement for our immediate follow-up. To avoid an increasing fraction of workers dropping out of the labor force over time, we restrict workers in our sample to be no older than 55. Clearly, our data series is so long that we cannot avoid the fact that an increasing fraction of workers drops out of the labor force. This will be apparent in some of the results we discuss below.] In the future, we will be able

to directly examine the receipt of public pension and disability insurance using administrative information on benefit receipt from the Master Beneficiary Record (MBR).

A crucial step in our analysis is the dating of job separations. We have experimented with alternative definitions of separation from the stable job held in the late 1970s. The most straightforward definition is simply a change in EIN from one year to the next. However, there are many cases in which longer employment spells at the same EIN are interrupted for a single year. This occurs either due to a transition to non-employment – such as in the case of temporary layoffs – or because a worker receives more earnings from another employer. Since we are interested in permanent separations from the long-term job, an alternative definition is to consider the first separation that is permanent. After some experimentation we have settled on defining job separations as occurring when the first separation is also permanent. This is the cleanest definition, and avoids possible intermediate cases, such as when a worker leaves the firm and then receives severance pay a year later. The last two definitions yield almost identical results; the first definition also yields similar results (see von Wachter, Song, and Manchester 2007).

A key innovation of this study is to use firm-level employment data covering any employer operating in the U.S. from 1978 to 2004 that reports earnings covered by Social Security. For each employer in each calendar year, we have obtained the total number of workers with positive earnings at that employer in the given year.[3] In the case of multiple employers in a single year, we only count a worker at the employer at which the worker received the highest earnings. This avoids double counting of workers switching employers within a year or workers holding multiple jobs.[4]

We define mass layoffs as instances where the employment of a firm drops by at least 30%.[5] Since such a measure makes less sense for smaller firms with high variance of employment, we follow JLS by restricting ourselves to firms that had at least 50 employees

[3] Note that the unit of analysis is the EIN, which may contain multiple establishments.
[4] Given substantial worker mobility within a year, there is some inherent ambiguity in measuring firm size. Figures reported by the Census Bureau often refer to firm size at a given calendar date. This double counts workers with multiple jobs, but does not count each job for workers switching jobs. We have experimented with various ways in defining a worker-firm pair for generating estimates of firm size. This is discussed in more detailed in the Appendix of our companion paper.
[5] At present, this measure does not include plant closings because of a non-negligible number of reappearing EINs. However, few EINs disappear suddenly, but instead have large employment declines prior to closing.

in 1979. Since our employment measures are based on annual earnings, if workers receive earnings for part of the year at the old employer, even a sudden drop in firm size may not appear immediately as a drop in employment. Thus, we consider changes in firm size over one and over two years. To make sure we capture a permanent decline and not temporary fluctuations in a firm's work force, we require firms' employment to have a minimum amount of stability before and after a sudden drop in employment.[6]

The resulting incidence of mass layoffs at the firm level and job destruction at the worker level suggests a large spike of layoffs in the early 1980s. This is the subject of our companion paper exploring the effect of the incidence of layoff on the labor market as a whole (von Wachter, Song, and Manchester 2008). In von Wachter, Song, and Manchester (2007) we have shown that the transition rate to non-employment was especially high in the early 1980s, consistent with a high rate of layoffs. Here, we analyze the effect on earnings of permanently separating from a stable job during a mass-layoff.

We call a worker displaced if he permanently leaves his stable job in the year the firm experiences a sudden drop in employment. The majority of the workers leaving the firm during a mass-layoff should in fact quit their jobs involuntarily, especially in the course of a strong recession. However, some workers may have left the firm voluntarily as well. If voluntary movers change to higher paying jobs, this may lead us to understate the effect of layoffs. On the other hand, an important concern is that employers may selectively lay off their least productive workers. Again, this may be less of a concern during large layoffs, since employers may not be able to be as selective regarding which workers to lay off. This is true especially during the 1980s, when most firms – whether unionized or not – were bound by seniority rules (Abraham and Medoff 1984). We address the concern of selective displacement directly in our regression analysis.

Table 1 displays basic characteristics of our sample by mobility status during the years 1980-1985. We show sample statistics for workers not separating during that period, those permanently leaving their long-term employer during a mass-layoff, as well as non-

[6] Some of the permanent declines in employment we observe may be due to takeovers. To assess this possibility, we constructed the complete matrix of worker flows between firms, and flagged employment declines in which a high fraction of workers moved between just two EINs. Our results are robust to this restriction.

mass layoff separators. The entries in the table confirm that there are important differences in characteristics of workers permanently leaving their long-term employer in 1980-1985 relative to other workers not experiencing a permanent separation during that period. Job separators – whether with six or with three years of job tenure – are on average younger and have 15-20% lower annual earnings in 1979. Table 1 also shows the industry distribution for job separators and non-separators. Job displacements are more likely in construction and manufacturing and less likely in transportation. On the other hand, non-MLF separations are more likely in trade and finance, insurance and real estate (FIRE). Interestingly, for workers with at least three years of tenure, manufacturing and construction do not emerge as high-layoff industries. Given these significant pre-separation differences, it is important to control for the potential of selective job displacement when comparing the earnings developments of job separators with a control group, something we will address in our regression analysis.

We begin with a descriptive analysis of earnings of workers displaced in 1981. This should capture layoffs during the first year of the 1980-1982 double-dip recession. Given our definition of job separation, 1980 is the last year these workers receive the majority of earnings from their current employer. For example, if a worker left their employer in, say, March of 1980 and had no other job that year, we would assign 1981 as date of separation. If he had earnings from another employer in 1980 that exceeded the earnings in that year from his long-term job, we would assign 1980 as separation date. Alternatively, many workers may leave their long-term employer during 1980 but still have the majority of earnings from that employer during that year. In that case, we assign 1981 as displacement date.

Trends in total annual average earnings for workers separating from their long-term employer during a mass-layoff in 1980 appear in Figure 1. Figure 1A includes workers with zero earnings. To be consistent with the restrictions imposed by JLS and others working with state-level administrative data, we include only workers with positive annual earnings in Figure 1B. Several features of the data stand out. First, one can see a rise and decline in earnings prior to 1981 in tune with the business cycle. Second, the figures show a decline in earnings from 1979 to 1980, followed by another drop in 1981. The fact that earnings drop gradually is a by-product of our definition of job separations. Third, one can see some recovery through the year 2000. However, in the long run a clear gap in earnings relative to

the pre-separation level remains. Note that to avoid counting transitions into retirement as earnings loss, we have set earnings to missing for workers who are older than 55. This may lead us to undercount earnings losses if job separation leads to earlier retirement in the long run.[7]

To put these earnings losses in perspective, Figure 1 also displays the development of annual earnings for workers not separating during 1980. It is not a priori clear which group of workers yields the appropriate counterfactual earnings trend. Each different control group will yield another answer to the question of the cost of job loss. The control group we use here is relatively unrestricted, since we do not impose any further restriction on employment or earnings. Generally, the more stable is the job attachment of the control group, the higher are the predicted earnings losses of displaced workers.

Three results are apparent from the consideration of non-separators. First, as noted in Table 1, non-separators' earnings are higher prior to separation. Second, had separators not left their jobs, their earnings would have remained stable throughout the early 1980s and trended slightly upwards. Third, it is apparent from the figure including zeros that even 15-20 years after a job separation affected workers have somewhat lower annual earnings than workers who stayed with their employer in the early 1980s. These results are independent of which control group is chosen as counterfactual (see von Wachter, Song, and Manchester 2007). Once we restrict the sample to workers with positive earnings, we see that non-MLF separators have somewhat higher annual earnings on average after 10 years.

Our results also suggest persistent losses in earnings for workers who lost their job during a mass-layoff even 20 years later. The loss is especially noticeable when we include workers with zero earnings. Restricting the sample to workers with positive earnings shows that MLF separators almost have returned to their pre-layoff earnings level by 2000. (see Appendix Figure 1A ??gone??).[8]

In contrast to the permanent decline in earnings experienced by displaced workers, workers separating from their long-term job but not during mass layoffs do not experience long-run losses in earnings. In fact, after about five to eight years they have caught up with

[7] Note that there is little difference in the effects whether we measure MLF over one or two years. The latter series is smoother, since the 2-year measure captures a larger number of layoffs.

[8] The fraction of workers with positive earnings among non-separators exhibits a monotonic downward trend. For separators, there is a large decline in the fraction employed at job separation (over 15 percentage points). From there, employment declines, albeit at a slower rate than for non-separators (note that as for earnings, non-employment at age 60 or above is not included in the figures). Still true??

their previous level and with the control group. Non-MLF separators even appear to have higher earnings than the control group in the long run when we include workers with zero earnings. However, some of these workers may experience a transition to disability or out of employment covered by Social Security that is unrelated to layoff. Thus, we interpret the results for non-MLF separators with caution.

These descriptive results can be compared to those in JLS' Figure 1, which shows quarterly earnings losses for all workers separating from their long-term job in the first quarter of 1982 (a more detailed comparison follows below). The initial loss is about $3,000 in 1987 prices; inflating this to 2000 prices at a factor of 1.52 and converting to total annual earnings, this would suggest annual earnings losses of about $18,000.[9] The loss relative to the pre-recession peak in earnings suggested by Figure 1B is about $10,000 when we exclude zero earnings as in JLS. In Figure 1A, which includes zero annual earnings and is discussed next, the loss is closer to $18,000. Both numbers have some validity as a point of comparison, since quarterly earnings in JLS can be zero (though annual earnings are restricted to be positive). Thus, as perhaps expected, the earnings loss at job displacement on a strictly comparable basis is smaller in the U.S. as a whole than in Pennsylvania, where steel and other heavy industry was particularly hard hit during the 1982 recession. However, our findings point to earnings losses at job displacements that are similar in overall magnitude and duration as found in JLS for Pennsylvania and by others for Massachusetts, California, and Connecticut in the early 1990s.[10]

An important constraint of analyses based on state-level administrative data such as JLS for Pennsylvania is that nothing is known about workers moving out of state. To counter this problem, JLS require minimal positive earning each year in the Pennsylvania labor market of all workers, whether separators or not. By ignoring workers with zero earnings, this may lead to an underestimation of the cost of job loss. Moreover, displaced workers employed after job separation may be a positively selected sample with regard to earnings potential, further biasing the results towards finding smaller earnings losses. On the

[9] With 2000 as a base, the 1987 CPI was 65.95.
[10] See Schoeni and Dardia (2003), Couch (2006), Kodrzycki (2007), and Hildreth, von Wachter, and Handwerker (2008).

other hand, if the most successful workers move out of state, excluding these workers may lead to an overestimate of the cost of job displacement.

Since we do have information on workers' earnings no matter what state they reside in, we do not need to impose such a condition. To see the impact of relaxing this restriction, Figure 1A examines the trends in average annual earnings at the main job for separators but *including* wage observations with zero earnings.[11] The patterns in Figure 1A are overall quite similar to those in Figure 1B. Thus, the main results in JLS and other state-specific analyses are upheld in our more general sample.

However, some expected differences arise when zero earnings are included that should be addressed in a more complete analysis of the role of participation. First, the earnings loss is larger than in Figure 1B. [??This is not surprising, since Appendix Figure 1 still there?? indicates that an important fraction of displaced workers stop working after leaving their stable job.] Second, the pattern of recovery after separation is somewhat weaker. However, the earnings gap relative to the control group in the long run appears similar,[?? in part due to a faster rise in non-employment among the latter??]. Third, the biggest apparent difference is for non-MLF separators; once we include zeros, we see larger and more persistent losses. As suggested above, this may partly be because some of these workers may not be laid off, but experience a voluntary transition out of the covered labor force. However, some of these workers may indeed lose their attachment to the labor force because of the separation.[12]

Another concern with the analysis of administrative data of JLS and others is that they impose six years of pre-separation job tenure to isolate workers in stable employment. This may be an overly restrictive sample, in particular since earnings and job losses have been shown to increase with job tenure (e.g., Kletzer 1989).[13] This implies that workers leaving their employer after a shorter employment spell may fare better in terms of post-

[11] Note that zero annual earnings here means no earnings from employment covered by Social Security. Thus, it may be that workers with zero earnings are self-employed, or work for some levels of government (though the majority of public employment is included in Social Security by the mid-1980s).

[12] Note that the recovery itself is unlikely to be driven by selective changes in participation, [??since Appendix Figure 1A suggests that employment for non-MLF separators stays relatively constant at the lower level after separation??].

[13] It has long been speculated that part of the persistent earnings losses after a job separation may be due to a loss in job-specific investments that workers and firms incur during a longer employment spell. Since by definition these investments are not productive in the outside market, workers' earnings decline at a job loss.

separation earnings. Another reason to contrast the earnings losses at job separation is that job separations at three years of tenure are more common than at six years.

We replicated our main results for workers in stable employment for three years, from 1977 to 1979. The resulting numbers are shown in Figure 2A (including zero earnings) and Figure 2B (positive earnings). Overall, the figures show quite similar patterns as for workers with six years of job tenure in 1979. As expected, independently of separation status mean earnings of low-tenured workers in the late 1970s is lower (see also Table 1). Similarly, earnings declines at displacement are somewhat smaller and earnings appear to recover somewhat faster. However, the difference is one of degree rather than in kind. The magnitude of the losses will be easier to compare in our regression analysis. Nevertheless, it is clear from these figures that the losses in annual earnings at displacement are large even for a broader group of workers with lower pre-displacement job attachment.

As documented in Table 1 and apparent from Figures 1 and 2, systematic differences in average earnings and age exist between workers who separate from their long-term employment in the early 1980s and workers who keep their jobs. It is also apparent that strong cyclical swings and trends in earnings may confound the effects of job loss. To get a complete picture of the long-term earnings losses of job separators, we need to make a comparison to a control group but at the same time explicitly account for possible systematic differences among workers in a regression framework.

We will estimate various specifications of the following distributed lag model

$$y_{it} = \alpha_i + \gamma_t + \beta X_{it} + \sum_{k \geq -m,} \delta_k D_{it}^k + u_{it} \qquad (1)$$

where the outcome variable y_{it} represents a measure of annual earnings, the year dummies γ_t are identified by the presence of workers not separating from their job (the control group), and the error u_{it} represents truly random components affecting the outcome. The coefficients δ_k on the dummies indicating the k-th period before, during, or after job separation (D_{it}^k) measure the time path of earnings changes of job separators before and after a displacement relative to the baseline and the control group. The ability to estimate the

dynamic effect of job separation is of particular interest since it will allow us to obtain summary measures of the overall lifetime cost of job separation.

The displacement effect is identified by the inclusion of workers staying at their employers throughout the period under study (the control group). To interpret the estimated effects δ_k as the causal impact of job separation on earnings, however, we have to assume that conditional on worker fixed effects and included observable baseline characteristics, displaced workers are observationally equal to those workers in the control group. This is the strategy chosen by most classic studies of the effect of job loss (e.g., Ruhm 1990, Jacobson, Lalonde, and Sullivan 1993). If workers are on average remunerated according to their productivity, then the long-run average of earnings should be a good index of their overall earnings potential. In this case, comparing a job separator and a non-separator with similar worker fixed effects yields a valid estimate of the effect of job loss.

Given the large change in earnings for job separators, this approach is most persuasive in the presence of a long window of observation prior to the job separation. Similarly, it is most appropriate for mature workers whose earnings represent their productivity. For example, in the case of younger workers, wages often do not yet reflect their long-term earnings potential and fixed effect strategies are not viable (e.g., von Wachter and Bender 2006). Since our observation window covers a long time period prior to job separation and the average age of workers in our sample is close to 40, we believe our fixed effect estimation strategy will uncover estimates that yield good first approximations of the causal effect of job separations on earnings.

A potential concern with estimates obtained from the model in equation (1) is that it may attribute negative trends in earnings in industries experiencing high rates of layoff to the event of job displacement. To make sure our estimates do not simply reflect differential industry trends we augmented model (1) by interacting the year effects with the industry in which workers held a job in 1979. If j denotes the 2-digit industry in 1979, then we estimate

$$y_{ijt} = \alpha_i + \gamma_{tj} + \beta X_{it} + \sum_{k \geq -m,} \delta_k D_{it}^k + u_{ijt} \qquad (2)$$

This effectively uses as a counterfactual earnings measure in the absence of displacement the evolution of earnings of workers who are not displaced belonging to the same 1979 industry. (Below, we will also estimate a fully interacted model where we allow the effect of displacement itself to differ by broad industry). We will also present estimates where we use

workers employed at the same employer in 1979 who were not displaced as a control; there, j represents the identity of the employer in 1979, and model (2) includes employer-year fixed effects. This can account for the fact that firms engaging in mass layoffs may have been in economic difficulties before the layoff; we would not want to attribute such a trend to the effect of displacement. A drawback of this estimate is that it effectively subtracts any effect of the mass-layoff on workers staying at the firm ('stayers') from the effect on displaced workers. We return to this point below when we discuss an alternative strategy to estimate the effects of mass-layoff within an intent-to-treat framework.

In our sensitivity analysis, we also estimate a range of other variations on the basic regression model. Among others, we present estimates for groups of workers with different degrees of labor force attachment. While the high tenured workers we focus on in our main analysis constitute a small fraction of the overall labor market, these estimates are based on broad groups of workers more representative of the U.S. labor market as a whole. A related concern is that the industry and age decomposition of the U.S. labor market has changed since the early 1980s. To show directly that our estimates are not driven by particular industries or age-groups, below we analyze these groups separately. To summarize, we also present estimates based on samples that have been re-weighted to reflect the characteristics of the labor market in 2003.

To display our results, we graph the coefficients δ_k capturing the effect of separation on annual earnings before and after the year of employment at the lost job (i.e., k=0 during the last year we record the old employer to be the main employer). We follow JLS in pooling all job separations from 1980 to 1986 to maximize our sample size. All figures show the results from our preferred specification, which includes year effects (and thereby effectively introduces the control group); a fourth-order polynomial in current age (and thus accounts for differential age of the treatment and control groups); and worker fixed effects eliminating permanent differences in earnings potential between separators and non-separators. A detailed discussion of the role of each of these specification choices can be found in von Wachter, Song, and Manchester (2007).

The results for losses in annual earnings among displaced workers in covered employment are shown in Figure 3. The figure strongly confirms that job separation during

the early 1980s recession and its immediate aftermath had very long-lasting effects on annual earnings, and significantly reduced workers' lifetime earnings. For workers displaced at mass layoffs during 1980-1986, the losses are about $10,000 ten years after job loss when we look at workers with positive earnings, or about 21% relative to average 1979 earnings shown in Table 1. Twenty years after job separations, earnings remain about $10,000 below the baseline – maintaining a long run loss of about 21%. We find very large persistent earnings losses following job separation that decline ever so gradually and never recover fully.

Controlling for year fixed effects alone increases the persistence of earnings losses compared to average losses shown in Figure 1. This result implies that part of the apparent recovery in average earnings observed in Figure 1 is due to cyclical recovery in the mid-1980s.

If we include observations with zero earnings in the analysis, the losses become even larger and initially more persistent (Figure 3). The immediate losses are $16,000 vs. $13,000, and the 10 year loss stands at $13,000 vs. $10,000 if we include zero earnings instead of excluding it. Those losses persist even after 20 years.

The magnitude of these estimates is on the same order as what Jacobson, Lalonde, and Sullivan (1993) found for workers laid off in Pennsylvania during the same time period. JLS report earnings losses up to six years after job loss. We show losses up to 20 years. The loss JLS estimate for the sixth year, rescaled to represent annual earnings and converted to 2000 dollars using the CPI, is $9,000 (the specification comparable to ours is in their Figure 4, model 2). This is of similar order of magnitude of the ten year loss we report, ranging between $10,000 and $13,000, depending on whether zero earnings are excluded or not (as we discussed above, both estimates bear some validity when compared to the quarterly estimates in JLS restricted to workers with positive annual earnings).

The long-run results we report suggest that the medium-run effects reported by JLS and others persist into the future, with lingering effects 15-20 years after a job displacement.

Last, a note of caution is appropriate. The sample used in this paper does not contain the same range of industries as JLS does, and so far we have only considered men, whereas they consider both men and women. [??omit: Similarly, the final JLS estimate includes worker-specific earnings trends, which will be included in the next version of this paper.]

The annual losses we find accumulate to represent a substantial loss in life-time earnings. This is shown in Table 2, where we present the implications for loss in the present-discounted value of earnings losses assuming that losses are zero after 20 years for different interest rates. This can be thought of the earnings loss for a worker retiring twenty years after a job displacement. The substantial losses range from $110,000 to $140,000. Had a worker earned about $50,000 each year in the absence of the loss for twenty years, at an 8% real interest rate his remaining PDV for twenty years would have been about $530,000. A loss of $110,000 would thus represent a decline in life-time earnings of about 20%.

The table has two additional messages. First, the number of high-tenured workers potentially affected by these large earnings losses is substantial. Second, the number of workers affected by displacement during mass layoffs quickly grows if we consider workers with shorter tenure durations or even weaker employment histories prior to job loss. Thus, the number of potentially affected workers rises from about one million to three million if we require six consecutive years of positive earnings at any employer instead of six years of tenure at the same employer. As further discussed in the next section, the remaining columns of Table 2 reveal that albeit estimated earnings losses for these added groups of workers is lower, it is still substantial.

Results from the regression analysis also reveal that relative to the control group of workers not separating between 1980 and 1986, earnings losses among the sample that includes shorter tenure durations at job loss are of the same order of magnitude and persistence as for our main estimates. This is shown in Figure 5. There is no indication in the figure that the cost of job loss is concentrated among displaced workers with six or more years of tenure. In fact, relative to the control group of workers with three or more years of tenure, the losses even appear to last longer than 15 years beyond the layoff. Similarly, if we include zero earnings, we find that workers displaced during a mass-layoff still fare significantly worse than non-MLF separators with at least three years of job tenure in 1979 (not shown).

Figure 5 also shows estimates for two additional groups of workers with even weaker restrictions on their prior labor force participation. In our broadest sample, we only require that workers have a significant attachment to the labor force, represented by three years of

consecutive positive earnings in 1979. Table 3 shows that these groups represent a much broader fraction of all individuals in our sample with positive earnings in 1979. While this may represent an advantage over the more narrowly defined high-tenure samples, it is important to realize that these groups are more likely to change jobs voluntarily. Since voluntary movers typically gain from job mobility (e.g., Topel and Ward 1992), this introduces measurement error in our displacement indicator that is likely to bias our estimates of the effect of layoff on earnings towards zero. Figure 5 shows that the broader groups of workers indeed experience somewhat lower earnings losses than the high-tenure groups. While this is expected given that higher job duration prior to displacement is typically associated with larger earnings losses, part of the difference might also be due to measurement error. In either case, the estimated reduction in earnings is still substantial, persistent, and large after 15-20 years even for these broad groups of workers.

To estimate our augmented model (2), we first included year-effects for the 2-digit industry of a worker's main employer in 1979. If we compare the change in displaced workers' earnings with the evolution of earnings of non-displaced workers in the same 2-digit industry, Figure 4 shows that earnings losses are somewhat lower, albeit still significantly negative, long-lasting, and substantial. The same applies when we include firm-specific year effects, although now the reduction is somewhat larger, representing roughly 25-30% of the original effect obtained from model (1). Thus, even within 2-digit industries, all workers in firms experiencing mass-layoff tend to fare somewhat worse. Note that these estimates control for both a pre-existing trend at the firm level as well as the effect of mass layoffs on workers staying at the firm. Whether this is desirable depends on our notion of the appropriate counterfactual. Clearly, from the point of view of the single worker, had he stayed at the firm instead of being laid off, he would have suffered the effect of the mass-layoff for those staying. On the other hand, to evaluate the overall cost of layoffs, we want to compare the outcomes of both displaced and non-displaced workers at the affected firms with the outcome of workers at comparable but stable employers. This will be discussed below.

To directly address the concern that changes in the industry and age distribution in the U.S. labor market would affect the relevance of our findings, we also estimated model (1) for various samples that have been reweighted to reflect the characteristics of our sample in later years. Figure 6 shows the estimates for weights that are chosen to reflect the industry-age distribution for high-tenured workers satisfying our main sample restrictions in the years 1986 and 2003. Panel A of Appendix Table 1 shows characteristics of the original samples in 1979 and 2003, as well as of the reweighted sample.[14] The findings, shown in Figure 6, indicate that the reweighting makes little difference. This foreshadows the results in Section 5 demonstrating that the effects of displacement are pervasive and negative for all broad age- and industry-groups we study. Thus, our findings are neither driven by any particular group of workers, nor would changes in the industry or age distribution imply radically different qualitative results. This is *not* to say that the effect of layoff would not be different in, say, 2003, only that we would not predict it to be different based on our data.

The analysis thus far applies to men who were displaced during the recession of the early 1980s. In this section, we explore how the effects of mass layoffs differ based on the age of workers when the layoffs occur, effects on earnings of women who are displaced during mass layoffs in the early 1980s, and effects on men displaced during the peak of the economic recovery of the late 1980s.

We expect that older workers might suffer larger losses in earnings following mass layoffs than younger workers. Older workers are likely to have more job-specific and firm-specific human capital and thus cannot replace their pre-layoff earnings as easily. Our regression analysis supports our expectation, as shown in Figure 7. Including workers with zero earnings, workers who were displaced at ages 50-55 see an initial fall of about $23,000 in earnings, compared to a loss of about $19,000 for workers ages 40-50 at displacement (Figure 7A). Workers in their 20s and 30s at displacement suffer a loss in earnings of about $16,000. Recovery in earnings following the initial loss is very slow for all age groups. Perhaps surprisingly, even the earnings of younger workers who were displaced at ages 20-40

[14] Panel B shows similar numbers for characteristics of a sample with only some work restriction in 2003. The resulting estimates based on the reweighted sample are very similar to what is shown in Figure 6.

do not recover fully. Earnings losses of about $12,000 persist 20 years after the mass layoffs. The time horizon is shorter for the older workers because we drop workers above age 55, but again the recovery in earnings is slow.

Excluding workers with zero earnings yields smaller initial losses in earnings and a less clear-cut age pattern (Figure 7B). Workers displaced at ages 20-55 experience losses of about $12,000 to $14,000 initially. The differences between workers in their 20s, 30s, 40s, and early 50s are small. However, recovery is more pronounced for workers in their 20s and 30s than for workers in their 40s.

Women show smaller dollar losses in earnings following mass layoffs in the early 1980s, but larger percentage losses (Figure 8). Looking at women with positive earnings shows initial losses of about $8000, or about 32% of their average earnings of $25,000 in 1979. Comparable results for men showed losses of $13,000, or about 27%. After 10 years, the earnings loss for women is about $6000 and becomes about $5000 after 20 years. The long-term loss of 20% is similar to the percentage loss for men. Including women with zero earnings yields initial losses of about $10,000, or about 40%. Men showed losses of about $16,000, or about 33%. After 10 years, the earnings loss for women is about $7000 and becomes about $4000 after 20 years. The long-term loss of 16% for women is somewhat smaller than men's loss of 25%.

To examine whether effects of job loss might differ if the layoffs occur at the peak of the late 1980s recovery, we compare earnings losses following job loss in 1987-1989 to our main results for the 1980-86 period (see Figure 9). Including workers with zero earnings who had been in a stable job for six years, the initial loss in earnings following job loss in the 1987-89 period is a bit smaller, but the persistence of the loss is roughly similar (Figure 9A). The initial earnings loss for the later period is $12,000, again expressed in 2000 dollars, compared with $16,000 for the early 1980s recession period. After 10 years, losses for the later period are about $8000 compared with about $13,000 for the earlier period. Patterns for men in stable jobs for three years in the two periods show similar results (Figure 9B).

Every year a large fraction of workers lose their job in the U.S. labor market. Yet, despite a long literature attempting to quantify the costs of job loss, no representative and comprehensive estimate of the long-term consequences of job displacement exists. This is

particularly worrisome because existing estimates based on limited samples or time periods suggest that the costs could be large and persistent at least for some groups of workers. However, existing estimates suffer from important limitations since they are often based on single U.S. states, have no information on earnings of workers moving between states, and can follow workers only for a limited amount of time.

In this paper, we show that the results based on more limited samples are upheld in a representative and large sample of the U.S. work force covering over 25 years. We merge longitudinal administrative earnings data with data on employment of firms to study the long-term effects of job separation on earnings for male high-seniority workers displaced from their jobs during the 1982 recession. In doing so, we follow the literature in isolating displaced workers as those permanently leaving their stable job when their employer experiences a sudden large drop in employment.

Our sample allows us to follow workers who left a stable job for over 20 years after job loss, and compare their earnings developments to the outcomes of workers of similar age and earnings potential who did not separate from their employers. To control for the potential of negative selection of job separators, the long time period of our data allows us to control for worker fixed effects.

Our preliminary results suggest that displacements in mass layoffs occurring in the early 1980s led to large and persistent earnings losses that last over 20 years. Those workers in stable employment from 1974 to 1979 who were displaced from 1980 to 1986 – compared to workers with no permanent job separation during that period – had initial earnings losses of up to 33%. These losses decline to 21-27% after ten years and remain roughly similar 20 years after a job loss. We also show that even for workers with lower pre-separation tenure, losses are still substantial and last up to 20 years.

This has potentially important implications for the costs of economic adjustment in the U.S. economy. In particular, while the ability to fire 'at will' may benefit adjustment in the labor market as a whole, the costs in terms of lost productivity and earnings of individual workers may be much higher than typical replacement rates of unemployment insurance or other programs designed to smooth temporary earnings fluctuations.

We will work on several extensions of our analysis. On a substantial level, we will analyze how the speed of recovery depends on the number of workers affected. We will also engage in further important sensitivity checks. First, we will make sure that the losses we

find are not driven by a correlation of the incidence of job separations with average earnings in affected firms or industries. Second, our long time period allows us to follow JLS and control for potential differences in individual long-term earnings trends of separators and non-separators. Last, our large sample size allows us to analyze the effects of job separation by age, gender, race, region, industry, and socio-economic status.

Aaronson, Daniel and Daniel Sullivan (1998). 'The decline of job security in the 1990s: Displacement, anxiety, and their effect on wages.' *Federal Reserve Bank of Chicago Economic Perspectives*, Vol.22 No. 1.

Abowd, John, Robert Creecy, and Francis Kramarz (2002). 'Computing Person and Firm Effects Using Linked Longitudinal Employer-Employee Data.' Cornell, mimeo.

Abraham, Katharine G. and James L. Medoff (1984). 'Length of Service and Layoffs in Union and Nonunion Work Groups.' *Industrial Labor Relations Review*, Vol.38 No.1 pp. 87-97.

Anderson, Patricia and Bruce Meyer (1994). 'The Extent and Consequences of Job Turnover.' Brookings Papers on Economic Activity 1994:1.

Bound, John and Alan B. Krueger (1991). The Extent of Measurement Error in Longitudinal Earnings Data: Do Two Wrongs Make a Right?' *Journal of Labor Economics*, Vol. 9, No. 1 pp. 1-24

Browning, Martin and T.F. Crossley (2001). 'Unemployment Insurance Levels and Consumption Changes.' *Journal of Public Economics*. 80(1):1-23

Chan, Sewin and Ann Huff Stevens (2001) "Job Loss and Employment Patterns of Older Workers," *Journal of Labor Economics* 19, 484-521.

Couch, Kenneth (2006). 'The Earnings Impact of Job Displacement Measured with Longitudinally Matched Individual and Firm Data.' Presented at 2006 SOLE Meetings.

Elsby, Michael, Ryan Michels, and Gary Solon. (2007). 'The Ins and Outs of Cyclical Unemployment.' NBER Working Paper No. 12853.

Farber, Henry (2003). 'Job Loss in the United States, 1981-2001.' IRS Working Paper No. 471 Princeton University.

Farber, Henry (2007). 'Job Loss and the Decline in Job Security in the United States.' Industrial Relations Section Working Paper 520.

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

Gottschalk, Peter and Robert Moffitt (1994). 'Growth in Earnings Instability in the U.S. Labor Market.' *Brookings Papers on Economic Activity*.

Gottschalk, Peter and Robert Moffitt (2002). 'Trends in the Transitory Variance of Earnings in the U.S.' *The Economic Journal*, Vol. 112 (478), C68-C73.

Gruber, Jonathan (1997). 'The consumption smoothing benefit of unemployment insurance.' *American Economic Review* 87 (1): 192–205.

Gruber, Jonathan, and Brigitte C. Madrian (2002) "Health Insurance, Labor Supply, and Job Mobility: A Critical Review of the Literature," NBER Working Paper No. 8817.

Hildreth, Andrew, Till von Wachter, and Elizabeth Weber (2005). 'Estimating the 'True' Cost of Job Loss: Evidence Using Matched Data from California 1991-2000.' Mimeo, Columbia University.

Jacobson, Louis, Robert LaLonde and Daniel Sullivan (1993). 'Earnings Losses of Displaced Workers.' *American Economic Review* 83(4): 685-709.

Kletzer, Lori (1989). 'Returns to Seniority after a Permanent Job Loss.' *American Economic Review* 79: 536-43.

Kodrzycki, Yolanda K. (2007). 'Using Unexpected Recalls to Examine the Long-Term Earnings Effects of Job Displacement.' Federal Reserve Bank Working Paper W07-2.

Kopczuk, Wojciech, Emmanuel Saez, and Jae Song (2007). 'Uncovering the American Dream: Inequality and Mobility in Social Security Earnings Data since 1937.' NBER Working Paper No. 13345.

Krashinsky, Harry (2002). 'Evidence on Adverse Selection and Establishment Size In the Labor Market.' *Industrial and Labor Relations Review* 56(1): 84-96.

Neal, Derek (1995). 'Industry-Specific Human Capital: Evidence From Displaced Workers,' *Journal of Labor Economics*, October, vol. 13: 653-77.

Olson, Craig (1992). 'The Impact of Permanent Job Loss on Health Insurance Benefits.' Industrial Relations Section Working Paper No. 305. Princeton University.

Ruhm, Christopher (1991). 'Are Workers Permanently Scarred by Job Displacements?' *American Economic Review* 81: 319-23.

Schoeni, Robert and Michael Dardia (2003). "Estimates of Earnings Losses of Displaced Workers Using California Administrative Data." PSC Research Report No. 03-543.

Shimer, Robert (2005). 'The Cyclicality of Hires, Separations, and Job-to-Job Transitions.' *Federal Reserve Bank of St. Louis Review*, 87(4): 493-507.

Stevens, Ann Huff (1997). 'Persistent Effects of Job Displacement: The Importance of Multiple Job Losses.' *Journal of Labor Economics*, Vol. 15, No. 1, Part 1, pp. 165-188

Stevens, Ann Huff (2001). 'Changes in Earnings Instability and Job Loss *Industrial and Labor Relations Review*, Vol. 55, No. 1, 60-78.

Topel, Robert and Michael Ward (1992). 'Job Mobility and the Careers of Young Men.' *Quarterly Journal of Economics* 107(2): 439-479.

von Wachter, Till and Stefan Bender (2006) 'At the Right Place at the Wrong Time: The Role of Firms and Luck in Young Workers' Careers.' *American Economic Review.*

von Wachter, Till, Jae Song, and Joyce Manchester (2007). `Long-Term Earnings Losses due to Job Separation During the 1982 Recession: An Analysis Using Longitudinal Administrative Data from 1974 to 2004.' Mimeo, Social Security Administration.

von Wachter, Till, Jae Song, and Joyce Manchester (2008). `The Role of Mass Layoffs in Explaining Trends in Instability and Inequality in the Labor Market.' Mimeo, Social Security Administration.

Weber-Handwerker, Elizabeth (2006). 'The Social-Security Notch, Mortality, and Cohort Variability: A Cautionary Tale.' Mimeo. University of California Berkeley.

The sample of workers used for our analysis of the long-term consequences of job separations is based on three data sets. The three sources are the 2004 Continuous Work History Sample (CWHS) active file, a 1% extract from the Master Earnings File (MEF), and a 1% extract from the Longitudinal Employee-Employer Data (LEED).

SSA 1% samples are selected by a "stratified cluster design" based on certain serial digits of the Social Security Number (SSN). They are generally considered to be random samples and contain a large number of observations that represent the general population. Individuals are followed through their lives, thereby giving us longitudinal data.

The 2004 CWHS is a 1% sample that gives us the baseline sample universe and the matching longitudinal earnings and demographic information. It contains information on Social Security covered ("capped") earnings from 1951 through 2004, uncapped total earnings from 1978 through 2004, and basic demographic characteristics of persons who have any report of covered earnings in 1951 through 2004 as well as those who have any report of uncovered earnings in 1978 through 2004.

The MEF contains information on a worker's total uncapped annual earnings for each job held in a given year beginning in 1978 through 2004 and an identification number for each employer (EIN). It also has information on industry for each job.

The LEED is a longitudinal employee-employer file that starts in 1957. The sampling approach is the same as in the 1% CWHS, but individual earnings are reported at the employer level. A record exists for each employer that employed a worker in the sample in each year. The dataset includes basic demographic characteristics as well as compensation information subject to top-coding at the employer-employee record level up to 1978 but with no top-coding after 1978. The dataset also contains information about the employer such as geographic information and a three-digit industry code. The LEED also includes imputed wages above the taxable maximum from 1957 to 1977. The imputation procedure is based on the quarter in which a person reached the taxable maximum and is known as Method II. The idea is to use earnings for quarters when they are observed to impute earnings in quarters that are not observed because the annual taxable maximum has been reached. When the taxable maximum is reached in the first quarter, imputations rely on a Pareto interpolation.

	19084	7579	3696	4826	26893	15409	6602	8450
	37.7	36.5	37.3	37.1	35.9	33.4	34.6	34.5
	0.02	0.02	0.04	0.03	0.02	0.02	0.04	0.04
	0.03	0.04	0.05	0.04	0.04	0.04	0.07	0.06
	0.56	0.50	0.60	0.62	0.55	0.47	0.55	0.57
	0.18	0.12	0.09	0.10	0.16	0.10	0.08	0.08
	0.10	0.18	0.09	0.10	0.12	0.21	0.12	0.13
	0.07	0.10	0.06	0.05	0.07	0.10	0.06	0.06
	0.03	0.04	0.07	0.06	0.03	0.06	0.07	0.06
	55.658	48.737	47.870	47.845	52.238	42.057	42.993	43.108
	56.074	49.888	48.855	48.779	52.742	43.453	44.214	44.257
	51.240	43.734	44.283	44.729	48.353	38.050	40.041	40.471
	51.645	44.727	44.989	45.440	48.831	39.111	41.001	41.470

Notes:1% Sample Social Security Administrative Data. Earnings are in 1000s of dollars, deflated by 2000 CPI .

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	0.15	4,826	965,200	-$163,897	-$158.2	-$127,988	-$123.5
	0.17	8,450	1,690,000	-$141,190	-$238.6	-$110,818	-$187.3
	0.19	14,041	2,808,200	-$106,672	-$299.6	-$82,035	-$230.4
	0.20	18,321	3,664,200	-$102,897	-$377.0	-$79,632	-$291.8
	0.15	4,826	965,200	-$131,661	-$127.1	-$103,976	-$100.4
	0.17	8,450	1,690,000	-$110,259	-$186.3	-$87,843	-$148.5
	0.19	14,041	2,808,200	-$83,269	-$233.8	-$65,027	-$182.6
	0.20	18,321	3,664,200	-$79,099	-$289.8	-$62,170	-$227.8

Notes: Earnings figures are in 2000 Dollars. Discounted values are calculated at five years before displacement. Earnings losses are assumed to have returned to zero 20 years after displacement. Calculations based on 0.5% sample, such that column 3 is 200 times column 2. The total is the product of column 5 (7) is the product of columns 3 and 4 (6).

A. Effect of Alternative Restrictions							
	193524	*161209*	*114673*	*93002*	*74232*	*50752*	*31489*
		0.83	0.59	0.48	0.38	0.26	0.16
B. Drop Firm Size Restriction							
				127743	*98875*	*64837*	*37670*
				0.66	0.51	0.34	0.19
C. Drop Industry Restriction							
				111665	*88348*	*55698*	*34023*
				0.58	0.46	0.29	0.18

Notes: Calculations from Social Security Data

	0.02	0.04	0.56	0.15	0.04	0.08	0.08	0.04
	0.29	0.24	0.21	0.12	0.24	0.19	0.16	0.33
	0.22	0.18	0.17	0.09	0.16	0.12	0.11	0.23
	59.3	57.6	52.9	57.4	54.0	49.5	60.1	51.9
	-0.8	5.7	0.2	3.8	-0.5	-1.9	7.8	-5.6
	-23.9	-14.8	-15.8	-20.9	-17.9	-19.4	-5.8	-24.6
	-16.7	-10.4	-14.1	-18.2	-19.8	-15.7	-10.8	-22.6
	-18.9	-4.0	-11.6	-15.2	-17.4	-11.1	-12.5	-19.2
	-13.8	-5.2	-10.5	-11.8	-16.2	-7.0	-22.5	-19.2

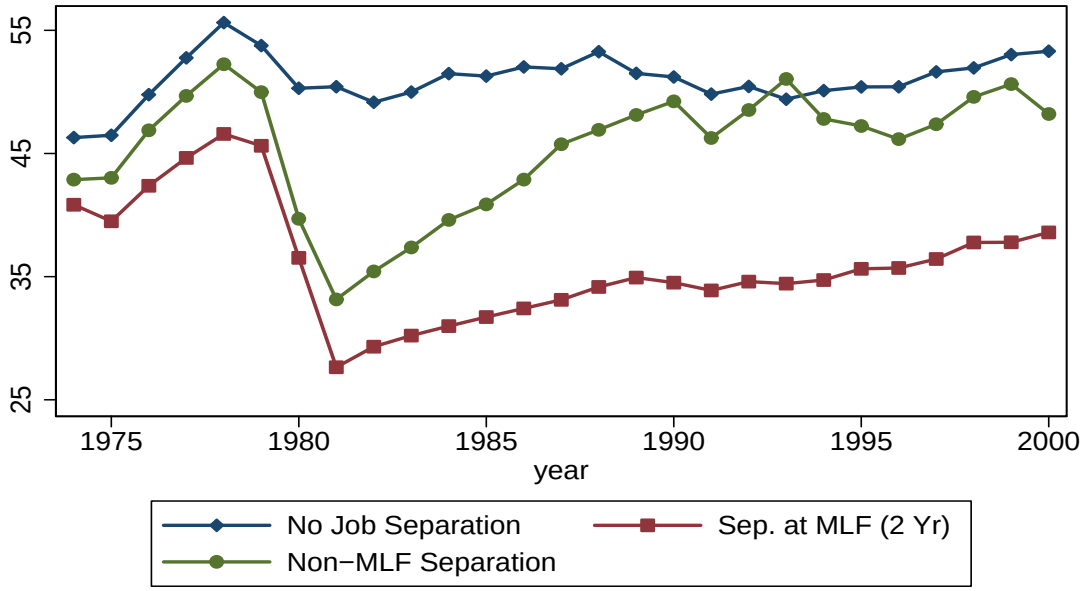
Notes: Industry refers to industry of main employer in 1979. Earnings in 2000 Prices. Industry layoff-rate not including non-mass layoff separators excludes non-mass layoff separators from denominator. It represents the fraction in the actual estimation sample that is used to estimate the effect of displacement on earnings. The industry layoff-rate for all workers includes non-mass layoff separators in the denominator.

	38.3	40.7	*2.3*	40.8	*-0.1*
	2.1	1.0	*-1.1*	0.9	*0.0*
	3.6	6.9	*3.3*	7.0	*-0.1*
	55.6	37.8	*-17.8*	37.5	*0.4*
	15.5	16.7	*1.2*	16.2	*0.5*
	4.4	8.0	*3.6*	7.9	*0.0*
	7.6	12.4	*4.8*	12.7	*-0.3*
	7.7	7.9	*0.1*	7.9	*0.0*

	38.3	34.8	*-3.6*	37.0	*-2.2*
	2.1	0.8	*-1.3*	0.8	*0.0*
	3.6	12.8	*9.2*	13.7	*-0.9*
	55.6	21.1	*-34.5*	22.7	*-1.5*
	15.5	9.1	*-6.3*	9.5	*-0.4*
	4.4	8.1	*3.7*	8.9	*-0.8*
	7.6	20.3	*12.8*	17.8	*2.5*
	7.7	8.0	*0.3*	8.2	*-0.1*

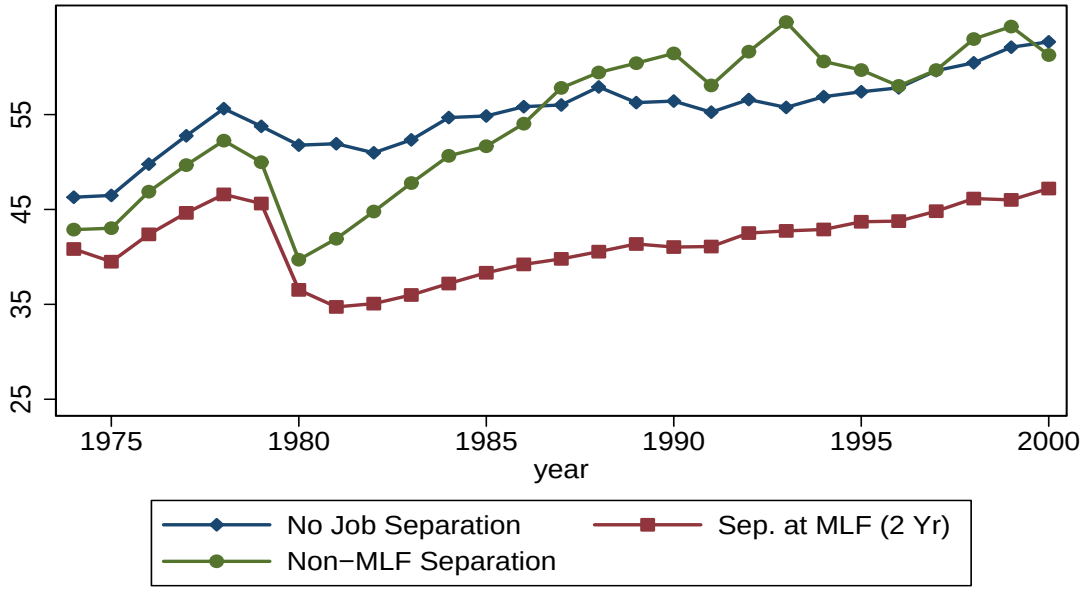
Notes: Tabulation from SSA Data. Weights calculated by a probit regression. High-tenured sample refers to workers at same job from 1974-1979 or 1998-2003. Sample with some work restriction refers to workers with three consecutive years of employment in 2003.

Figure 1A: Annual Earnings for Workers Separating and Not Separating in 1981
Earnings at All Jobs, Including Zeros, Men in Stable Job 1974−1979 (in $1000)



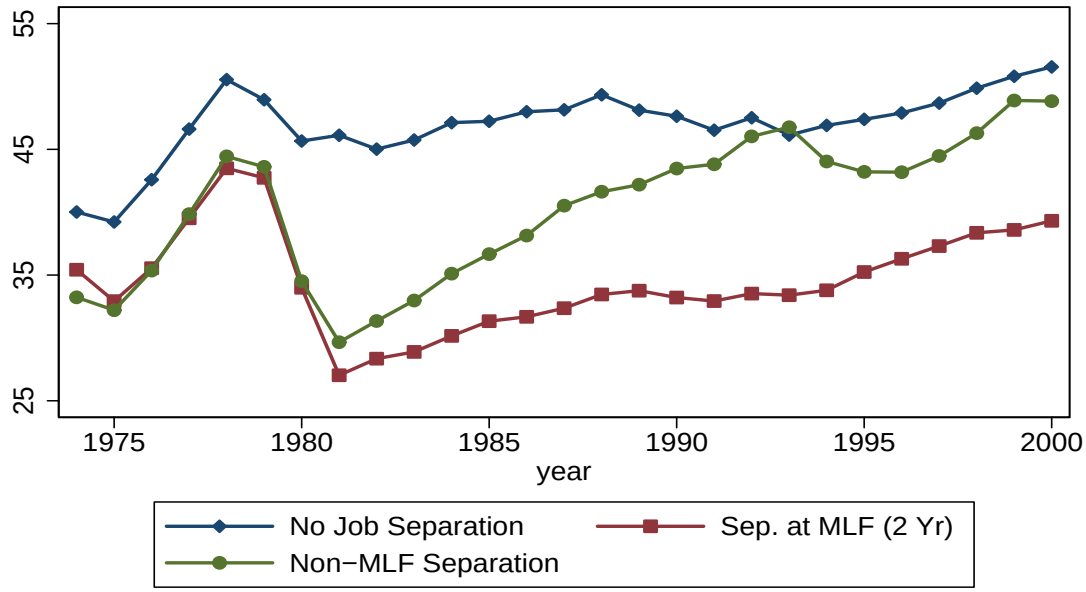
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 1B: Annual Earnings for Workers Separating and Not Separating in 1981
Earnings at All Jobs, Excluding Zeros, Men in Stable Job 1974−1979 (in $1000)



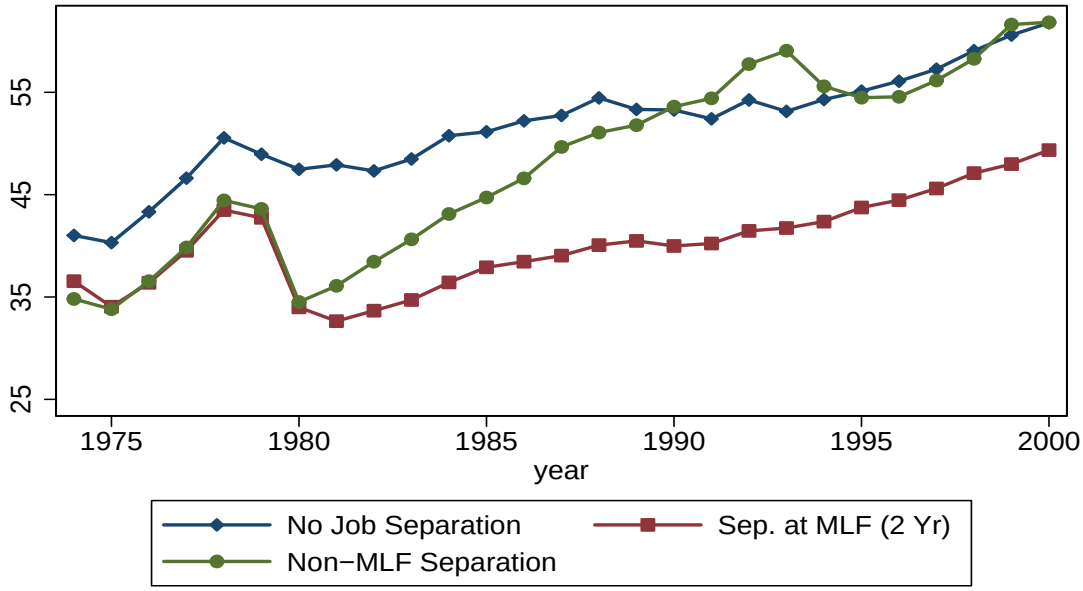
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 2A: Annual Earnings for Workers Separating and Not Separating in 1981
Earnings at All Jobs, Including Zeros, Men in Stable Job 1977−1979 (in $1000)



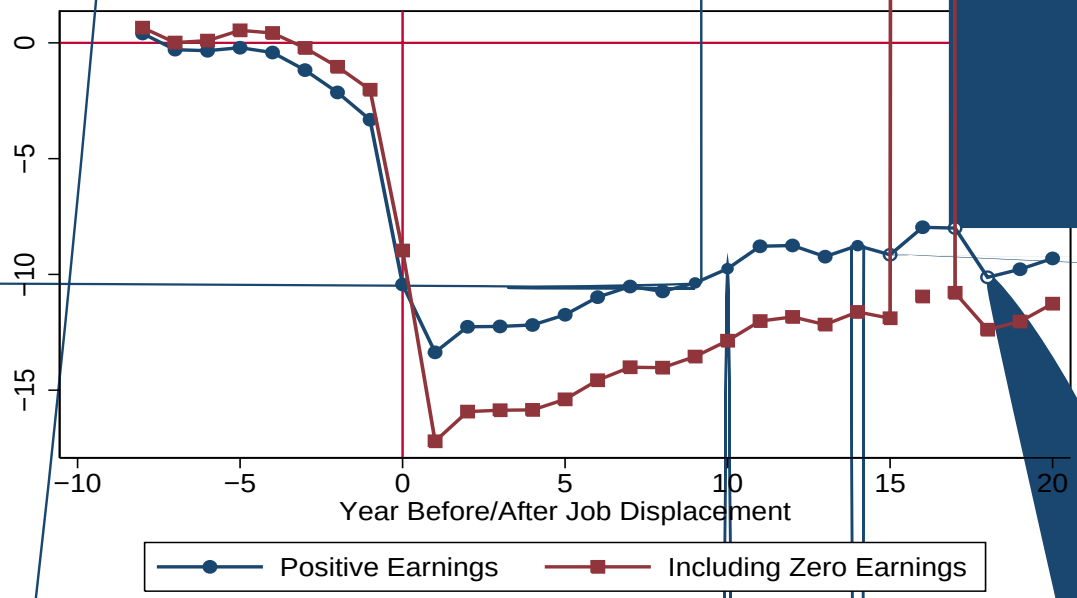
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 2B: Annual Earnings for Workers Separating and Not Separating in 1981
Earnings at All Jobs, Excluding Zeros, Men in Stable Job 1977−1979 (in $1000)



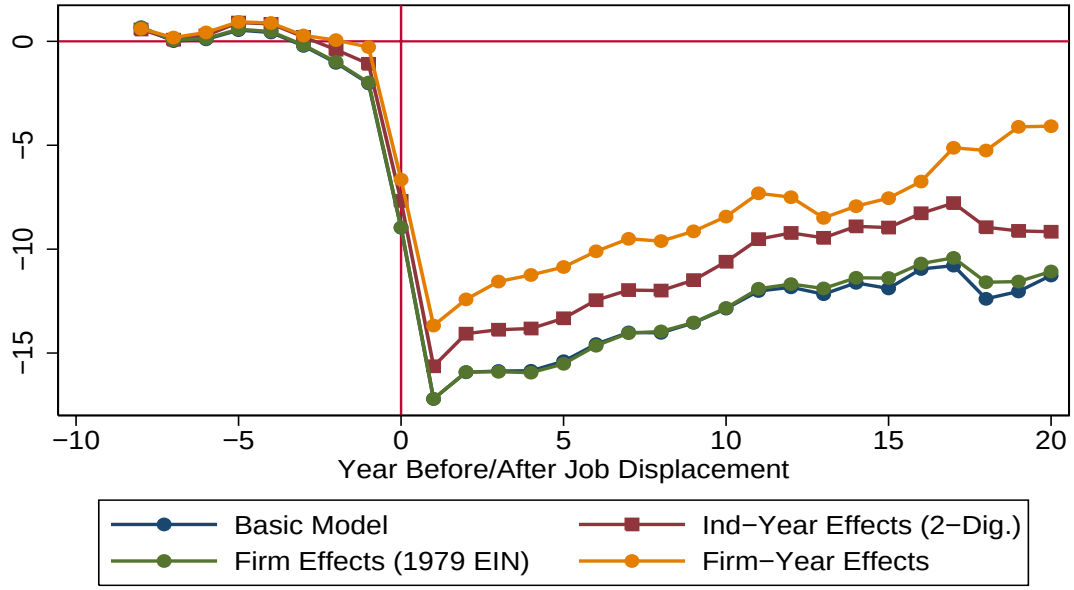
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 3: Earnings Losses at Job Separation 1980−1986 vs. Non−Separators
Earnings at All Jobs Including Zeros, Men in Stable Job 1974−1979 (in $1000)



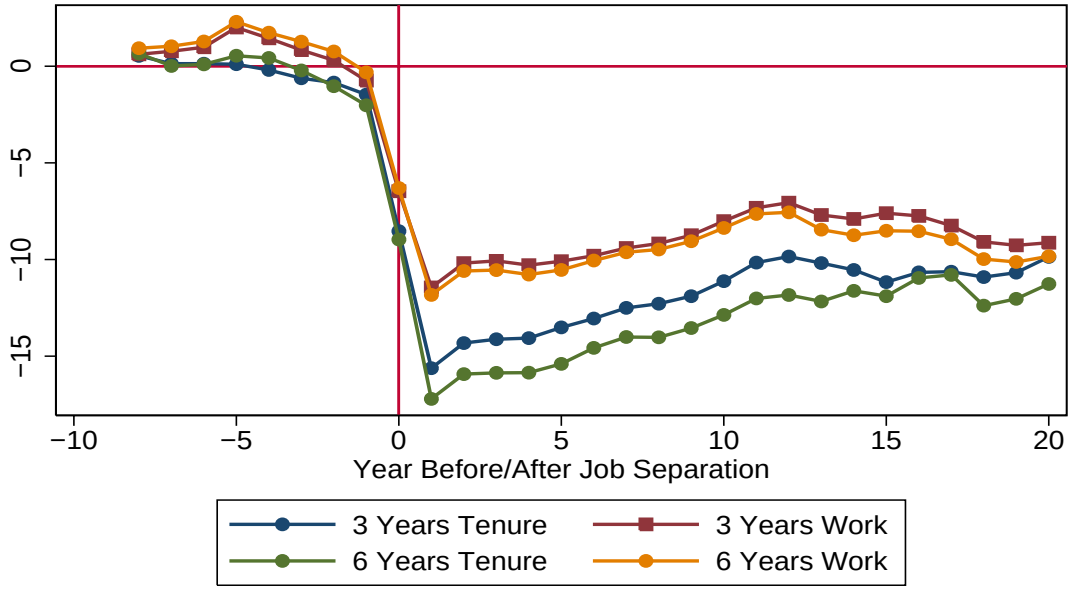
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 4: Earnings Losses at Job Separation 1980−1986 vs. Non−Separators
Earnings Including Zeros, Men in Stable Job 1974−1979 (in $1000)



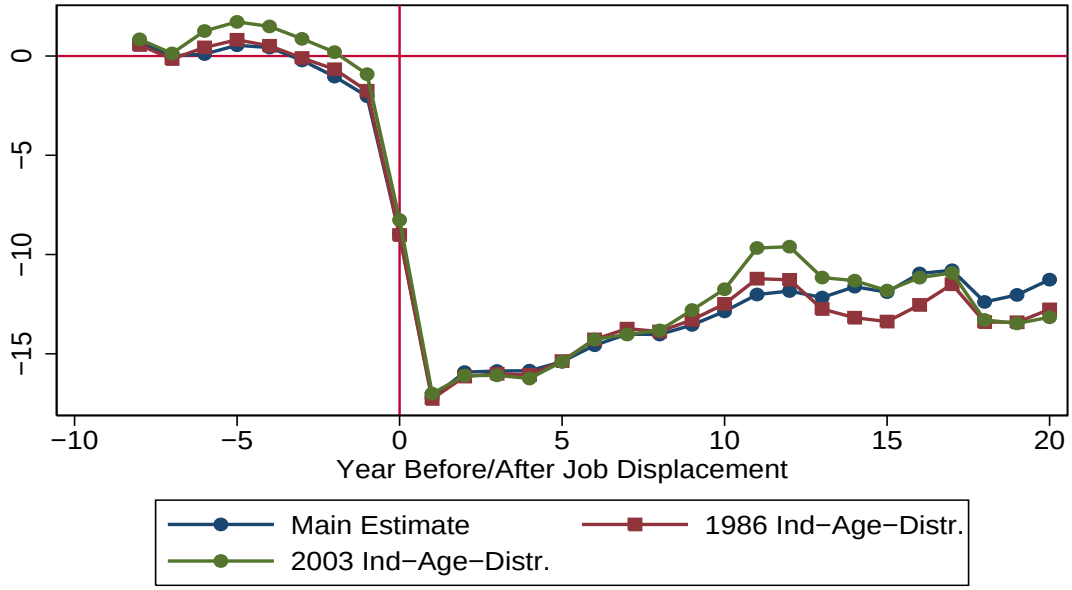
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 5: Earnings Losses at Job Separation 1980−1986 vs. Non−Separators
Earnings Including Zeros (in $1000), Men, Various Work Histories in 1979



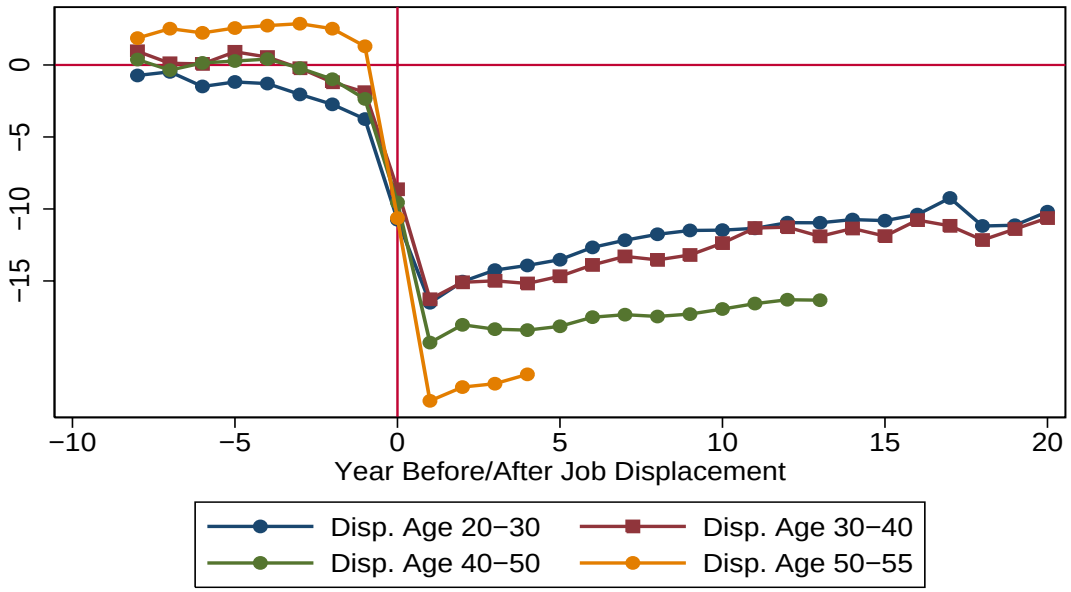
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 6: Earnings Losses at Job Separation − Original and Reweighted Earnings All Jobs Without Zeros, Men in Stable Job 6 Years (in $1000)



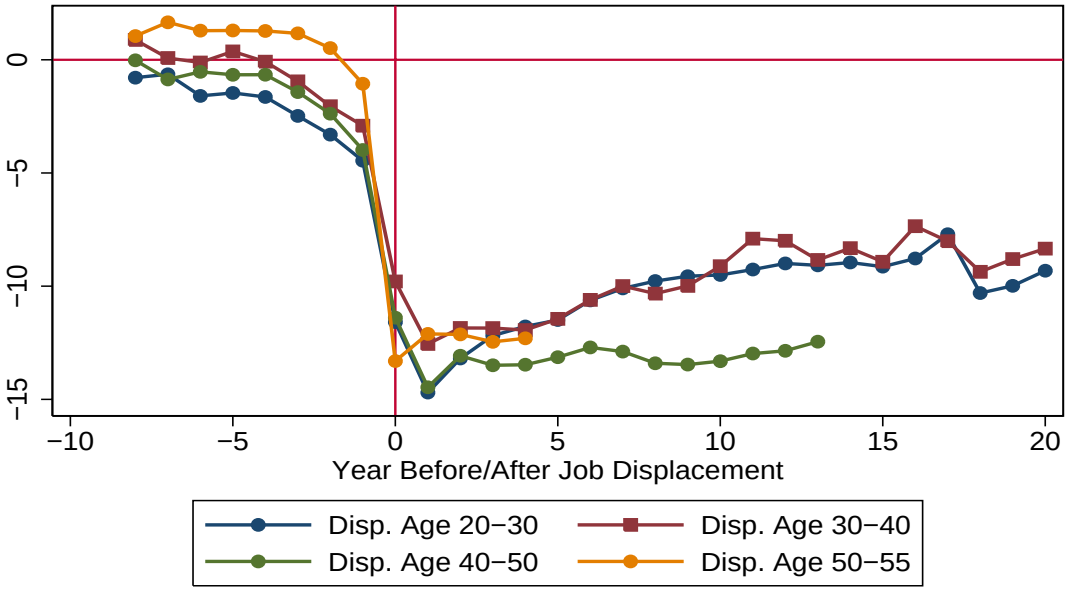
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 7A: Earnings Losses at Job Separation By Age at Displacement
Earnings All Jobs Including Zeros, Men in Stable Job 1974−1979 (in $1000)



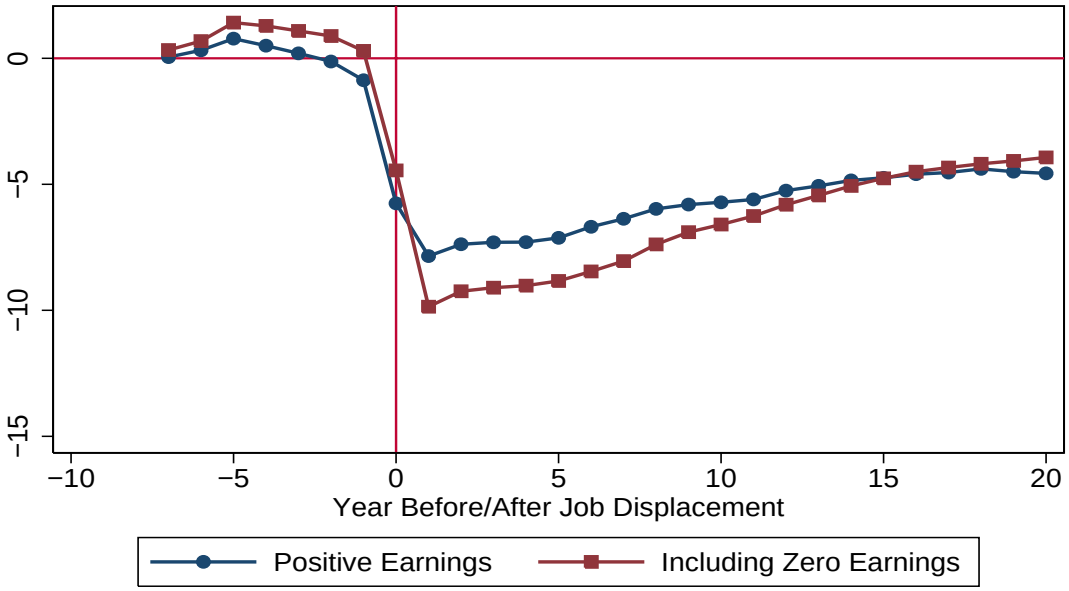
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 7B: Earnings Losses at Job Separation By Age at Displacement
Earnings All Jobs Without Zeros, Men in Stable Job 1974−1979 (in $1000)



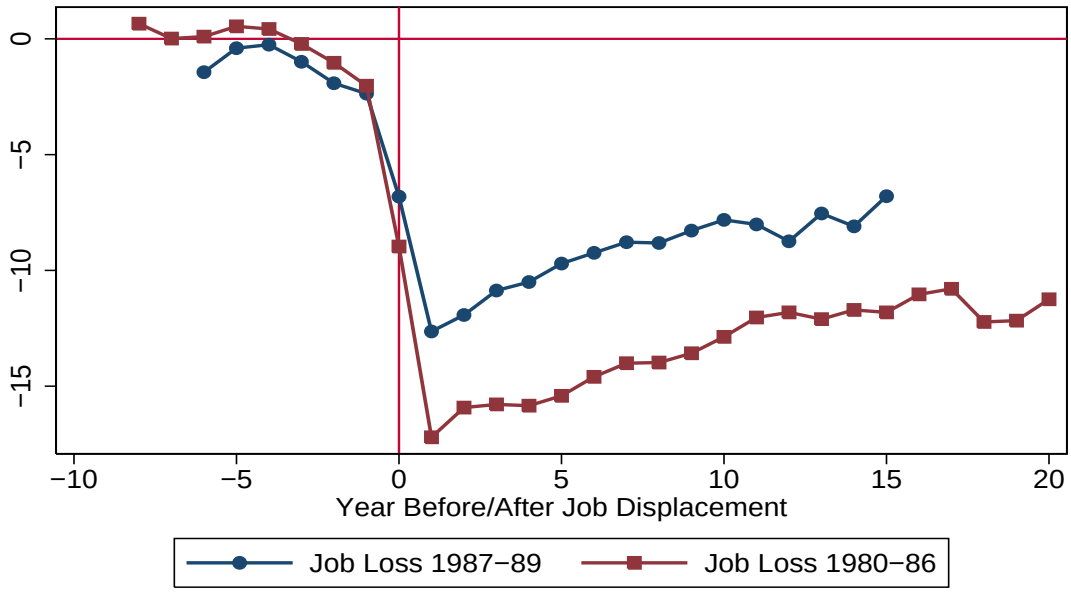
Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

Figure 8: Earnings Losses at Job Separation 1980−1986 vs. Non−Separators
Earnings All Jobs Including Zeros, Women in Stable Job 1974−1979 (in $1000)

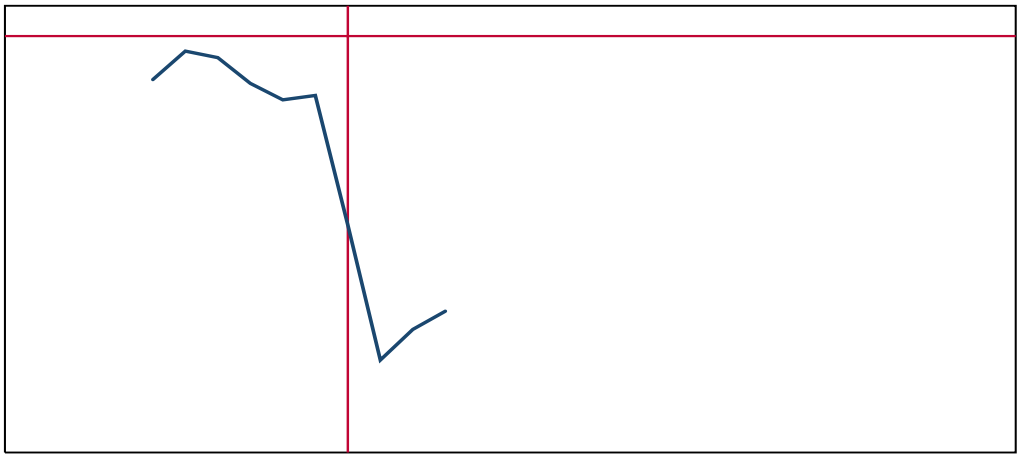


Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

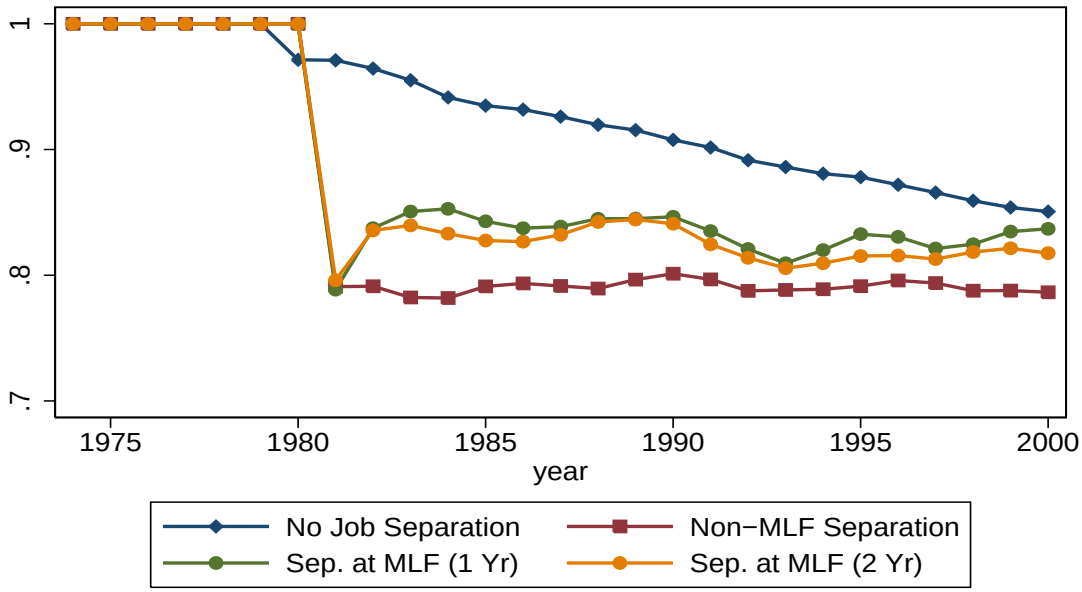
Figure 9A: Earnings Losses at Job Separation in Different Periods
Earnings All Jobs Including Zeros, Men in Stable Job 6 Years (in $1000)



Source: 1% Files of Social Security administrative data (see text). Earnings in 2000 Dollars.

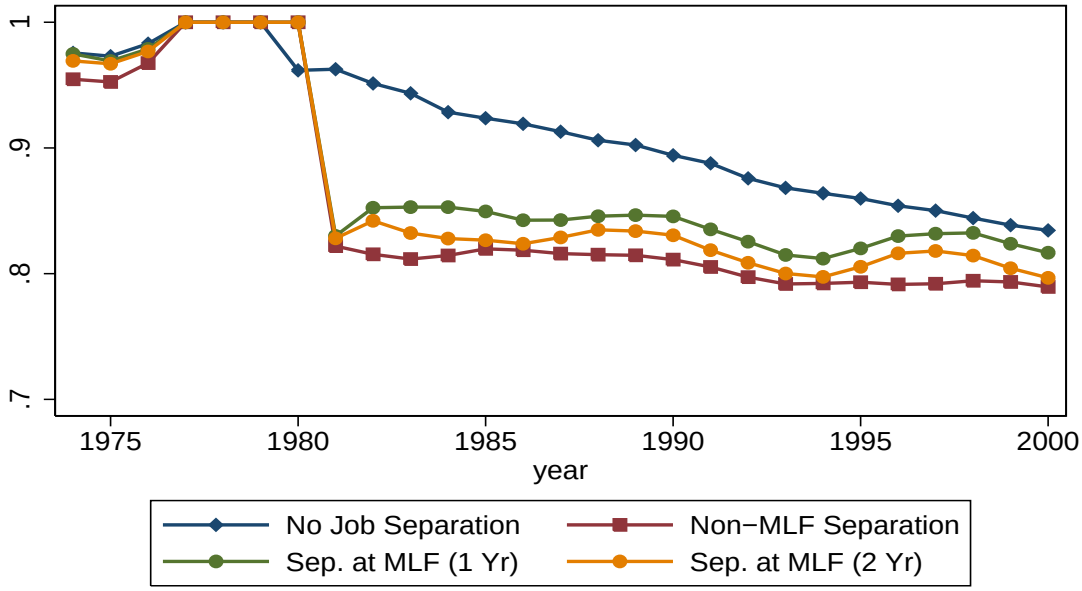


Appendix Figure 1A: Fraction Any Positive Earnings Among Workers Separating and Workers Not Separating in 1981−83, Men in Stable Job 1974−1979



Source: 1% Files of Social Security administrative data (see text).

Appendix Figure 1B: Fraction Any Positive Earnings Among Workers Separating and Workers Not Separating in 1981−83, Men in Stable Job 1977−1979



Source: 1% Files of Social Security administrative data (see text).