Earnings Losses of Displaced Workers

By LOUIS S. JACOBSON, ROBERT J. LALONDE, AND DANIEL G. SULLIVAN*

We exploit administrative data combining workers' earnings histories with information about their firms to estimate the magnitude and temporal pattern of displaced workers' earnings losses. We find that high-tenure workers separating from distressed firms suffer long-term losses averaging 25 percent per year. In addition, we find that displaced workers' losses: (i) begin mounting before their separations, (ii) depend only slightly on their age and sex, (iii) depend more on local labor-market conditions and their former industries, (iv) are not, however, limited to those in a few sectors, and (v) are large even for those who find new jobs in similar firms. (*JEL* J65)

Concerns about the plight of experienced workers losing jobs due to trade liberalization, increased environmental protection, or technological change have been an important part of recent public-policy debate.[1] Both researchers and policymakers recognize that these displaced workers experience costly spells of unemployment and short-term earnings declines. However, there is less agreement about these workers' long-term earnings losses, losses that may greatly exceed those suffered during their initial spells of unemployment. In this article, we use a new data set, derived from administrative records of the state of Pennsylvania, to assess the magnitude and temporal pattern of these long-term losses. In addition to better estimating the average loss, we show how workers' losses vary according to their demographic characteristics, the industry and size of their former employers, the conditions of their local labor market when they are displaced, and whether they find new employment in their former industry.

Theory suggests several reasons why displaced workers might experience earnings

*Jacobson: Westat Incorporated, Rockville, MD 20850, and The W. E. Upjohn Institute for Employment Research, Kalamazoo, MI 49007; LaLonde: Graduate School of Business, University of Chicago, Chicago, IL 60637, and the National Bureau of Economic Research; Sullivan: Federal Reserve Bank of Chicago, Chicago IL, 60604. We thank Joseph Altonji, David Card, Robert Gibbons, John Ham, Daniel Hamermesh, Bruce Meyer, Canice Prendergast, Christopher Ruhm, Jeffrey Smith, Robert Topel, and seminar participants at the University of Pittsburgh, the W. E. Upjohn Institute for Employment Research, the University of Chicago, the Univeristy of Illinois, The Federal Reserve Bank of Chicago, Princeton University, the University of Iowa, the National Bureau of Economic Research, the Board of Governors, Texas A&M University, and Johns Hopkins University for helpful comments and Carolyn Thies and Melissa Barnhart for extremely helpful research assistance. We also thank the Pennsylvania Department of Labor and Industry for providing the data used in this study and technical assistance. This research was funded by the Allegheny County Department of Planning, the Graduate School of Business at the University of Chicago, the W. E. Upjohn Institute for Employment Research, and the Industrial Relations Section at Princeton University.

[1]For instance, on several occasions during the recent recession, Congress and the Bush administration debated what additional assistance to provide to workers who had exhausted their regular unemployment insurance benefits. Concern about workers' jobs losses also arose in the Congressional debate over whether the Bush administration should have "fast-track" authority when negotiating a free-trade agreement with Mexico and in discussions about how much protection to accord the Spotted Owls in northwestern U.S. forests.

losses beyond a period of unemployment following their job losses. First, workers possessing skills that were especially suited to their old positons are likely to be less productive, at least initially, in their subsequent jobs. Such a fit between workers' skills and the requirements of their old jobs could have resulted from on-the-job investment in firm-specific human capital or from costly search resulting in particularly good matches with their old firms.[2] Second, workers losing jobs that paid wage premiums are likely to earn less if their subsequent jobs pay standard wages. Such wage premiums could have arisen because of direct or threat effects of unions or because such premiums directly raised workers' productivity on their old jobs.[3] Finally, displaced workers' long-term earnings will be lower if, on their previous jobs, they had accepted wages below their level of productivity in return for higher earnings later in their careers. Workers might have accepted such "tilted" tenure profiles in order to enhance their employers' incentives to invest in their human capital (see e.g., Edward P. Lazear, 1981).

Our study focuses on high-tenure workers because they are the ones most likely to have accumulated substantial amounts of firm-specific human or "match" capital prior to their job losses. Likewise, because wage premiums and deferred compensation are likely to decrease turnover, high-tenure workers are more likely than others to experience losses for these reasons as well. However, beyond suggesting which workers are likely to lose the most, the theories mentioned above do not provide much guidance to policymakers and others as to the magnitude and persistence of displaced workers' earnings losses. To answer such questions requires empirical work like that presented in this article. Indeed, research on the pattern of displaced workers' losses may shed some light on the importance of those theories more generally.

Many studies, including several using the Displaced Worker Survey (DWS) supplements to the Current Population Survey (CPS) (U.S. Bureau of the Census, 1988), find that workers characterizing themselves as displaced frequently report significantly lower earnings on their new jobs.[4] However, the DWS has several shortcomings that make it difficult to use in assessing the magnitude and temporal pattern of displaced workers' earnings losses. These shortcomings include the lack of a comparison group of nondisplaced workers, the lack of extensive predisplacement earnings data, and a documented tendency for workers not to report more remote instances of displacement (see e.g., Robert H. Topel, 1990). Recently, Christopher J. Ruhm (1991) avoided these problems by using the Panel Study of Income Dynamics (PSID) to document significant long-term effects of displacement. We go beyond his work by employing a more comprehensive statistical methodology and by documenting how the estimated earnings losses vary over time and among workers. This added detail should aid assessments of the varied theoretical explanations for the earnings losses discussed above.

To develop this detailed picture, we created an unusual longitudinal data set by merging administrative records covering 52 quarters of workers' earnings histories with information about their firms. The resulting data set contains quarterly earnings histories for a large number of high-tenure displaced and nondisplaced workers, including those who remained employed at displaced workers' former firms. As we explain below, these data offer several advantages over those used in other studies.

[2]For example, on the former possibility see Gary S. Becker (1975), and on the latter possibility see Boyan Jovanovic (1979).

[3]For example, on the former possibility see H. Gregg Lewis (1986), and on the latter possibility see Joseph E. Stiglitz (1974).

[4]For the original analysis of the 1984 DWS, see Paul Flaim and Ellen Seghal (1985). Daniel S. Hamermesh (1989) summarizes a dozen displaced-workers studies. More recent studies include John T. Addison and Pedro Portugal (1989), Lori Gladstein Kletzer (1989), Topel (1990), and Paul Swaim and Michael Podgursky (1991).

Like previous researchers, we find that high-tenure workers incur large losses when they separate from distressed firms. However, we also find a consistent temporal pattern to these losses in which displaced workers' relative earnings begin to decline substantially three years prior to their separations, drop sharply when they actually leave their jobs, and then rise rapidly during the six quarters immediately following their separations. After that point, however, these workers' earnings recover very slowly, so that five years after separating from their former firms, their losses still amount to 25 percent of their predisplacement earnings. This finding of large losses holds for virtually every group of workers that we examine, including males and females, as well as younger and older workers. The losses exhibit greater variability among workers displaced from different industries and sizes of firms and amidst different local labor-market conditions. Nevertheless, our basic finding holds for workers formerly employed across a broad spectrum of industries and labor-market conditions. Further, even those who find work in the same industries as their old jobs experience large earnings losses.

The remainder of this article proceeds as follows. In Section I we describe our longitudinal data and comment on some of their strengths and shortcomings. In Section II we discuss the statistical issues involved in estimating the earnings losses incurred by displaced workers. In Section III we present estimates of the earnings losses for high-tenure workers who separated from their firms during the early and mid-1980's. Some concluding remarks follow in Section IV.

I. The Pennsylvania Data

The statistical framework developed in this article applies generally to the problem of estimating earnings losses for displaced workers. However, our empirical work assesses the magnitude and pattern of those losses only for workers displaced in Pennsylvania during the early and mid-1980's. We have limited our analysis to these workers in order to take advantage of a rich set of administrative data on Pennsylvanian workers and their firms. By combining quarterly earnings histories for a 5-percent sample of the state's workers with their firms' employment data, we have created a data set that contains workers' quarterly earnings extending from 1974 through 1986 as well as information about their firms, including employment levels and growth, geographic location, and "four-digit SIC" industry.[5] By observing changes in the sources of earnings we are able to date with relative accuracy the quarter in which some workers separate from their employers, as well as to identify other workers who remain continuously employed by a single firm.

These administrative data have several advantages over data used in other studies. First, we have a large sample of nondisplaced workers. This allows us to borrow statistical techniques from the program-evaluation literature in order to obtain more reliable estimates of the cost of displacement, including the cost related to the earnings growth that workers would have received in the absence of job loss. Second, we are able to track workers' quarterly earnings over a relatively long period of time. This allows us to distinguish short-term from long-term losses and also to be more confident that our results are free of statistical biases.[6] Third, we have data on a much larger number of workers than are followed in the PSID or the National Longitudinal Surveys (NLS). This allows us to provide useful results for relatively narrowly defined groups of workers. Finally, we have information on employment changes in workers' firms. This allows us to identify workers who separated from distressed firms. Such workers are likely to have been displaced, rather than to have quit or been dismissed for cause.

[5]For details on how we constructed our data see the Appendix.

[6]One interpretation of the exchange between LaLonde (1986) and James J. Heckman and V. Joseph Hotz (1989) is that reliable nonexperimental estimation of program impacts requires data on workers a substantial amount of time prior to their participation.

Our Pennsylvania data set also allows us to avoid two problems inherent in the use of the standard survey-based data sets. First, earnings data in the CPS and PSID are reported by workers with significant error, while our data are based on firms' reports that are used to calculate tax liabilities and are presumably virtually free of measurement error (see Greg J. Duncan and Daniel H. Hill, 1985; John Bound and Alan B. Krueger, 1991). Second, workers in the DWS are less likely to report instances of job loss, the longer that the displacement occurred prior to the interview date. If, as seems likely, the less severe setbacks are the ones that are not reported, it becomes difficult to use these data to determine the rate of recovery from job loss. By contrast, our administrative data allow us to identify all separations experienced by workers.

Of course, there are also some disadvantages associated with the use of our data set. Most obviously, we have data only on Pennsylvanian workers. Although we cannot be sure that our findings for these workers reflect the experiences of displaced workers generally, it is worth noting that Pennsylvania is a large state with a diverse industrial base. Further, during the 1980's—the period covered by this study—the economic performance of the eastern half of the state, which shared in the growth experienced by the other middle-Atlantic states and New England, was considerably better than that of the western half, which experienced double-digit unemployment rates (see Jacobson, 1988). This variation allows us to determine how losses depend on local labor-market conditions and, by extension, the importance of our restriction to Pennsylvanian workers.

Another disadvantage of our data is that demographic information on workers is limited to their sex and date of birth. By comparison, data sets such as the DWS or the PSID include a wider array of characteristics, among them workers' educational attainments, their occupations, and their marital and union statuses. The statistical techniques that we employ below account for unobserved heterogeneity in ways that ensure that our lack of such information does not lead to any biases in our estimates of average losses. However, lack of data does limit the extent to which we can learn how earnings losses vary among different demographic groups. Similarly, lack of data prevents us from decomposing earnings losses into effects due to lower wages and reduced hours. However, even given our data limitations, we are able to provide a substantially more complete assessment of the determinants of long-term earnings losses than has previously been possible.

Another possible shortcoming of our data is that they do not explicitly identify whether workers' separations resulted from quits, discharges for cause, or displacements.[7] In order to minimize these ambiguities, we constructed a "mass-layoff" sample that includes separators whose firms' employment in the year following their departure was 30-percent or more below their maximum levels during the late 1970's.[8] This definition encompasses firms that closed around the time of workers' separations, as well as others that had large employment declines. Although some employees from the mass-layoff sample may have quit their jobs or been discharged for cause, the vast majority probably separated involuntarily from their firm for economic reasons.

Finally, our data's most important disadvantage is that it is impossible to distinguish

[7]In related research Jacobson (1991) found that, between 1977 and 1987, the rate of separations for workers from Allegheny County (Pittsburgh) was 80 percent for workers with less than one year of tenure, 43 percent for workers with one year of tenure, 24 percent for workers with two or three years of tenure, and 13 percent for workers with four or more years of tenure. For those with four or more years of tenure, one-half of the separations were estimated to be retirements, and one-third were estimated to be displacements. Thus the quit rate for that group would be about 2 percent per year.

[8]This categorization of workers is less sensible for those from small firms. Accordingly we further restricted our sample to those whose firms had at least 50 employees in 1979. We have experimented with other similar definitions of "mass-layoff" and obtained results similar to those presented here.

between workers who leave the Pennsylvanian wage and salary work force and workers who remain unemployed for long periods of time. In our administrative data base, both groups of workers have zero earnings. For the unemployed, those earnings are their actual earnings; but, for workers who moved out of the state, became self-employed, or were working under a different social-security number, zero clearly understates their actual earnings.[9] Therefore, to avoid overstating workers' earnings losses we have eliminated from our sample the approximately 25 percent of high-tenure separators who subsequently never have positive earnings in our data. Because some of those workers probably were unemployed, we believe that this decision biases downward our displacement-cost estimates. Without this sample restriction, our estimates of the losses would be approximately 15 percentage points larger. Alternatively, even with this sample restriction, we might overstate losses if the most resilient workers are significantly more likely to move out of the state. However, we discount this latter possibility because, before their separations, the excluded workers had similar characteristics to the rest of the sample. Moreover, in results not reported in this article, we find that displaced workers who move within Pennsylvania actually experience somewhat larger than average losses.

To construct the sample of workers analyzed in this article, first we identified those workers who had six or more years of tenure by the beginning of 1980. Second, we restricted our sample to workers for whom we had information on their age and sex and, to avoid complications associated with early retirement, to workers born between 1930 and 1959. Finally, to reduce biases due to sample attrition, we required that every worker receive some wage or salary earnings during each calendar year. This restriction ensures that the losses we observe result from wage and hours changes instead of differing rates of nonemployment or missing earnings data. As we have noted, its potential drawback is that the excluded separators may suffer systematically larger or smaller losses.[10]

As shown by panel A of Table 1, the separators' median 1979 age was 37, only one year less than the median age of the nonseparators. In addition, 80 percent of both groups were between the ages of 27 and 47. Further, this characterization of separators' ages holds for several groups in our sample, namely, male and female workers, manufacturing and nonmanufacturing workers, workers from eastern and western Pennsylvania, and the mass-layoff and non-mass-layoff subsamples.

The earnings figures in panel B of Table 1 indicate that the median separator earned $22,904 (1987 dollars) in 1979. With the exception of the females in the sample, the other separator groups received approximately the same earnings. Despite being approximately the same age, the separators earned approximately $2,000, or 9-percent less than the median nondisplaced worker, suggesting that the latter were more highly skilled. This fact underscores the potential importance of accounting for individual-specific heterogeneity when estimating earnings losses due to worker displacement.

Relatively simple earnings comparisons suggest that displaced Pennsylvanian workers experienced substantial long-term earnings losses. For example, as shown by Figure 1, the earnings of workers who sepa-

[9]Frederick J. Tannery (1991) used United States Social Security Administration data to study the rates at which workers left the Pennsylvania wage and salary work force between 1979 and 1987. Although his sample is not restricted to high-tenure workers, he found that, among those who left the Pennsylvanian wage and salary labor force for reasons other than retirement, 60 percent had earnings outside the state. However, among those who left the state by 1987, more than one-half had 1979 earnings of less than $3,000, and less than 8 percent had earnings greater than $20,000.

[10]Such potential sample selection problems are not unique to studies using administrative data. For example, in the 1984 DWS, wage data were unavailable for the approximately 40 percent of the sample who were not employed at the survey date (see Flaim and Seghal, 1985).

TABLE 1—SAMPLE CHARACTERISTICS

Workers	Observations	Mean	Standard deviation	Median	10th percentile	90th percentile
A. *Age in 1979:*						
Separators:						
All	9,507	37.0	7.4	37	27	47
Males	7,092	36.9	7.2	37	27	47
Females	2,415	37.3	7.8	38	27	48
Nonmanufacturing	2,870	36.9	7.3	37	27	47
Manufacturing	6,637	37.1	7.4	37	27	47
Western Pennsylvania	3,804	36.8	7.4	37	27	47
Eastern Pennsylvania	5,703	37.1	7.3	37	27	47
Non-mass layoffs	3,072	36.9	7.3	37	27	47
Mass layoffs	6,435	37.1	7.4	37	27	47
Stayers	13,704	37.7	7.0	38	28	47
B. *1979 Earnings:*						
Separators:						
All	9,507	\$24,196	\$12,287	\$22,904	\$11,525	\$36,798
Males	7,092	27,363	12,161	25,942	16,326	38,557
Females	2,415	14,897	6,641	14,275	7,595	22,928
Nonmanufacturing	2,870	24,648	15,547	22,363	10,029	39,358
Manufacturing	6,637	24,001	10,566	23,096	12,070	35,963
Western Pennsylvania	3,804	25,147	12,449	24,292	12,359	37,561
Eastern Pennsylvania	5,703	23,561	12,138	22,176	11,005	36,140
Non-mass layoffs	3,072	23,640	14,415	21,665	10,585	36,726
Mass layoffs	6,435	24,461	11,120	23,593	12,037	36,805
Stayers	13,704	26,322	12,980	24,867	13,644	38,880

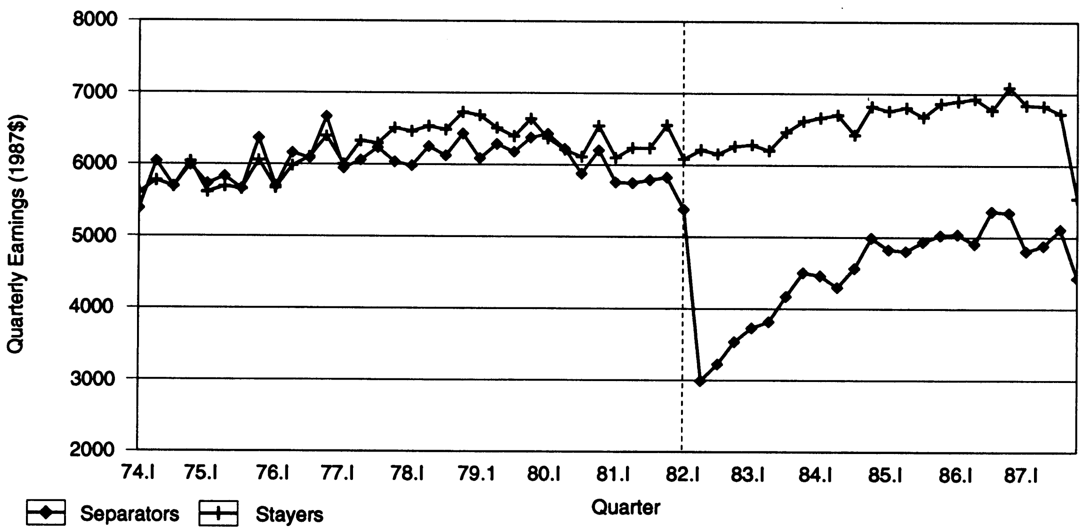

FIGURE 1. QUARTERLY EARNINGS (1987 DOLLARS) OF HIGH-ATTACHMENT WORKERS SEPARATING IN QUARTER 1982:1 AND WORKERS STAYING THROUGH QUARTER 1986:4

rated from their firms during the first quarter of 1982 fell sharply relative to the earnings of workers who remained with their firms through the end of 1986. Even four years after separation, their earnings remained nearly $2,000 per quarter less than their nondisplaced counterparts.

There are at least two ways to interpret the differences between stayers' and separators' earnings patterns. One interpretation is that because the earnings of displaced and nondisplaced workers were nearly the same during the mid-1970's, their earnings-related characteristics also must have been similar and, absent some adverse event, their earnings would have remained similar for the rest of the sample period.[11] Accordingly, the earnings differences between the two groups that emerge in the late 1970's and persist for the rest of the sample period should be interpreted as losses due to displacement or, more precisely, as losses due to the events that led to workers' displacements. Alternatively, the divergence between the two groups' earnings starting in the late 1970's might indicate that separators had more slowly growing earnings before their displacement and would have continued to have had slow earnings growth even without being displaced. Under this interpretation, some or all of the postdisplacement earnings gap between separators and stayers would have existed even if there had been no separation. We argue below that the first interpretation is the appropriate one.

II. Statistical Models of Earnings Losses

In this section, we develop a statistical framework for summarizing the evidence on the magnitude and the temporal and cross-sectional patterns of displaced workers' earnings losses. We begin by specifying more precisely our definition of displaced workers' earnings losses. Next we describe our statistical model. Finally, we discuss the circumstances that may lead to biases in our estimates.

A. *Definition of Earnings Losses*

Many displaced-worker studies measure losses as the difference between workers' earnings in some postdisplacement period and their earnings in a period shortly before separation. There are three reasons why this measure may not capture the full effects of structural or public-policy changes on workers' earnings. First, this measure does not control for macroeconomic factors that cause changes in workers' earnings regardless of whether they are displaced. Second, this measure does not account for the earnings growth that would have occurred in the absence of job loss; in the long term, workers' earnings may return to their predisplacement levels, but not to the levels expected prior to their job losses. Finally, firms' declining fortunes may adversely affect workers' earnings several years prior to their job losses. Therefore, to capture the full effect of the events that lead to workers' displacements, it is important to calculate their earnings declines relative to a point several years prior to their separations.

In this study, we define displaced workers' earnings losses to be the difference between their actual and expected earnings had the events that led to their job losses not occurred. To make this definition more precise, we let y_{it} denote the earnings of worker i at date t and let $D_{i,s}=1$ if worker i was displaced at date s (and $D_{i,s}=0$ otherwise). Our definition of earnings loss is the change in expected earnings if, several periods prior to date s, it was revealed that the worker would be displaced at date s rather than being able to keep his or her job indefinitely. More formally, our definition of the loss is

$$(1)\quad E\left(y_{it}|D_{i,s}=1,\mathrm{I}_{i,s-p}\right) - E\left(y_{it}|D_{i,\nu}=0 \text{ for all } \nu,\mathrm{I}_{i,s-p}\right)$$

[11]As Table 1 suggests, the near coincidence of stayers' and separators' mid-1970's earnings levels in Figure 1 is atypical.

where $I_{i,s-p}$ is the information available at date $s-p$, and p is sufficiently large that the events that eventually lead to displacement would have not begun by date $s-p$. This definition of workers' earnings losses allows the events that lead to workers' displacements to affect earnings prior to separation. In addition, our definition compares displacement at date s to an alternative that rules out displacement at date s and at any time in the future.[12] This choice ensures that we compare different cohorts of job losers' earnings to a common standard and simplifies the interpretation of several of our empirical results.[13]

The magnitude and interpretation of workers' earnings losses also depend on the variables in the information set $I_{i,s-p}$. To the extent that we can, we want to control for the standard demographic variables that influence earnings. In addition, our data set allows us to condition on displaced workers' former industries and even on their former firms. However, the danger in using a measure that conditions on workers' industries or firms is that even the workers who retain their jobs in industries or firms that permanently lay off other workers may themselves experience some earnings losses. Thus, if we conditioned on these variables, we might obtain relatively small displacement-effects estimates simply because workers who lost their jobs were hurt only a little more than those who kept their jobs. For example, if policy changes cause both displaced and nondisplaced workers' earnings to decline, an earnings-loss measure that controls for workers' industries or firms does not capture the full impact of those changes. Instead it captures only the effects specifically associated with workers' job losses.

Another way to make this point is to note that, in order to understand the importance of workers' attachments to particular firms, we must observe variation in outcomes for similar workers in different firms. This is impossible if we assume that workers are similar only when they work for the same firm. Therefore, we define displaced workers' earnings losses by conditioning only on general characteristics that would, at date $s-p$, be expected to affect earnings at date t. Nevertheless, we also report estimates that condition explicitly on workers' firms, because the difference between the two estimates provides an indirect estimate of the magnitude of losses imposed by structural changes on workers who retain their jobs.

B. *The Statistical Model*

To estimate the earnings losses corresponding to our definition we specify a statistical model to represent workers' earnings histories and identify the displacement effect with a subset of the model's parameters. Our specification is intended to exploit two of the principal strengths of our data set—that it covers a long period of time and that it contains data on many individuals—so as to obtain a very detailed picture of the pattern of earnings losses across both time and workers.

In order to allow our estimates to vary across both time and worker characteristics, we pool information for workers displaced between 1980 and 1986. A convenient way to do this is to introduce a series of dummy variables for the number of quarters before or after workers' separations. Accordingly, we let $D_{it}^{k}=1$ if, in period t, worker i had been displaced k quarters earlier (or, if k is negative, worker i was displaced $-k$ quarters later).[14] By restricting attention to these dummy variables, we formalize the idea that a worker displaced in 1982 was in much the

[12] Because our data end after 1986, we have no way of knowing whether some nondisplaced workers were displaced in 1987 or beyond. Therefore, our alternative rules out displacement at date s, and at any time through 1986.

[13] In Ruhm (1991), on the other hand, displacement at a given date is compared to an average that includes workers displaced at other dates.

[14] Alternatively, $D_{it}^{k}=1$ if worker i was displaced in quarter $t-k$.

same position in 1985 as a worker displaced in 1981 was in 1984.

Our first statistical specification assumes that workers' earnings at a given date depend on displacement through the set of previously defined dummy variables and on some controls for fixed and time-varying characteristics:

$$(2)\quad y_{it} = \alpha_i + \gamma_t + \mathbf{x}_{it}\boldsymbol{\beta} + \sum_{k \geq -m} D_{it}^k \delta_k + \varepsilon_{it}.$$

In (2), the dummy variables, D_{it}^k, $k = -m, -(m-1), \ldots, 0, 1, 2, \ldots,$ jointly represent the event of displacement. In particular, δ_k is the effect of displacement on a worker's earnings k quarters following its occurrence.[15] In the empirical work that follows, we allow displacement to affect earnings up to 20 quarters prior to separation.[16] The vector $\mathbf{x}_{it}$ consists of the observed, time-varying characteristics of the worker, which in this article are limited to the interactions among sex, age, and age squared. The γ_t's are the coefficients of a set of dummy variables for each quarter in the sample period that capture the general time pattern of earnings in the economy. The "fixed effect," α_i, summarizes the impact of permanent differences among workers in observed and unobserved characteristics. Finally, the error term, ε_{it}, is assumed to have constant variance and to be uncorrelated across individuals and time.

We estimate the parameters of (2), including the fixed effects, by least squares. Thus, no matter how workers' permanent characteristics are related to their displacement status, our estimates of the displacement effects are unbiased. This estimation approach generalizes the "difference-in-differences" technique, which uses a comparison group to estimate the earnings changes that would have occurred in the absence of displacement, by accounting for the effects of time-varying variables and by allowing the effects of displacement to vary by the number of quarters relative to separation.

As we have noted, many studies have taken the simple change in earnings between a postdisplacement period and some base period as an estimate of displaced workers' losses. In terms of model (2), this is adequate only if the γ_t's are constant over time, $\boldsymbol{\beta}$ is zero, and the base period is sufficiently far before separation. Ruhm's (1991) estimates are not constrained in these ways but differ from ours in the treatment of unobserved heterogeneity.[17] Specifically, he estimates cross-sectional regression models for postdisplacement earnings in which workers' predisplacement earnings are used as a control variable.[18] Our approach also uses predisplacement earnings to control for unobserved heterogeneity, but it does so in the fashion model (2) implies is optimal.

As the discussion surrounding Figure 1 indicated, one potential problem with specification (2) is that it does not allow for the possibility that workers might have different trend rates of earnings growth and that firms might be more likely to lay off workers with more slowly growing earnings. This practice would cause us to overstate the effects of displacement. Accordingly, our second specification takes this possibility into account

[15]Our statistical model is similar to those used to evaluate the earnings impact of public-sector training programs (see Orley Ashenfelter, 1978; Heckman and Richard Robb, 1985; LaLonde, 1986).

[16]To identify the parameters of (2) we must observe the earnings of at least some displaced workers more than m quarters prior to their displacement. The choice of $m = 20$ presents us with no problems of identification, because there are six years of predisplacement data for even our first cohort of displaced workers who separated from their firms in the first quarter of 1980.

[17]Ruhm's (1991) estimates that include earnings one year prior to displacement as a control variable will be biased according to (2) unless $\delta_{-4}, \ldots, \delta_{-1}$ are all zero. Analogous reasoning leads him to refer to such estimates as lower bounds.

[18]In one of Ruhm's (1991) specifications, it is the predisplacement earnings of workers who will be displaced in the future that provide the control for unobserved heterogeneity.

by adding to (2) a set of "worker-specific time trends," $\omega_i t$:[19]

$$(3) \qquad y_{it} = \alpha_i + \omega_i t + \gamma_t + \mathbf{x}_{it}\boldsymbol{\beta} + \sum_{k \geq -m} D_{it}^k \delta_k + \varepsilon_{it}.$$

Again, we estimate (3), including the fixed effects and the worker-specific time trends, by least squares, thereby allowing for arbitrary permanent heterogeneity between displaced and nondisplaced workers in both levels and trends of their unobserved characteristics.[20]

Because we effectively identify the influence of macroeconomic factors, γ_t, and of age and sex, $\mathbf{x}_{it}\boldsymbol{\beta}$, on earnings by using data on all workers who did not separate from their firms between 1980 and 1986, our framework essentially compares changes in displaced workers' earnings to those of the typical nondisplaced worker. As we noted above, it is also informative to compare displaced workers' earnings growth to that of nondisplaced workers from the same firms. Of course, we can only make this comparison for workers who were displaced from firms that continued to be in existence throughout the sample period. To compute this alternative estimator, first we subtracted from the quarterly earnings of each displaced worker the mean quarterly earnings of nondisplaced workers in their former firms. Then we estimated, by least squares, a model with individual-specific fixed effects and the full set of displacement indicators, $D_{it}^k, k = -m, -(m-1), \ldots, 0, 1, 2, \ldots$. Such estimates follow from a specification that interacts the quarter dummies with the set of dummy variables denoting workers' 1979 firms. More formally, if y_{ijt} denotes the earnings of worker i in 1979 at firm j in quarter t, then

$$(4) \quad y_{ijt} = \alpha_{ij} + \gamma_{jt} + \sum_{k \geq -m} D_{it}^k \delta_k + \varepsilon_{ijt}.$$

If workers who retain their jobs in firms that lay off other workers also suffer earnings losses, then the γ_{jt}'s for those workers' firms will decline around the time that those layoffs occur, and as a result, the estimates of the δ_k's based on (4) will be closer to zero. Moreover, the differences between displacement estimates obtained from (4) and (2) serve to gauge the size of losses suffered by workers who do not lose their jobs.

The foregoing models describe the temporal pattern of displaced workers' earnings losses in a very flexible manner. In principle, they can be easily modified to summarize how this pattern varies among different groups of workers. The least restrictive modification interacts each displacement dummy variable, D_{it}^k, with variables indicating workers' gender, age, industry, or region. The problem with this approach is that it leads to a very large number of parameters. For example, because there are 48 pre- and postdisplacement time periods observed in the data, to characterize the earnings losses across 12 industires in the most flexible manner requires nearly 600 displacement parameters. Fortunately, after examining such estimates it became apparent that differences among groups in the time pattern of earnings losses occurred mainly along just three dimensions: the rate at which earnings "dip" in the period before separation, the size of the "drop" that occurs at the time of separation, and the rate of "recovery" in the period following separation.

We use the fact that differences in the losses among groups can be summarized by three magnitudes to construct a more parsimonious representation of losses across time

[19]In the program-evaluation literature this specification has fit the earnings data of program and nonprogram participants more successfully than the simpler fixed-effects specification (see Ashenfelter and David Card, 1985; Heckman and Hotz, 1989).

[20]Computationally this is accomplished by a generalization of the "deviations from worker-specific mean" technique, in which we replace the time dummies, the x's, and the displacement dummies by deviations from person-specific time trends in these variables and then estimate the resulting model by least squares.

and workers. Specifically we define:

$F_{it}^1 = t-(s-13)$, if worker i is displaced at time s and $s-12 \leq t \leq s$, and $F_{it}^1 = 0$ otherwise;
$F_{it}^2 = 1$, if worker t is displaced at time s and $t \geq s+1$, and $F_{it}^2 = 0$ otherwise;
$F_{it}^3 = t-(s+6)$, if worker i is displaced at time s and $t \geq s+7$, and $F_{it}^3 = 0$ otherwise.

Then, if $\mathbf{c}_i$ is a vector of characteristics of individual i, our parsimonious model takes the form

$$(5) \qquad y_{it} = \alpha_i + \gamma_t + \mathbf{x}_{it}\boldsymbol{\beta} + \sum_{k \geq -m} D_{it}^k \delta_k + F_{it}^1 \mathbf{c}_i \boldsymbol{\varphi}_1 + F_{it}^2 \mathbf{c}_i \boldsymbol{\varphi}_2 + F_{it}^3 \mathbf{c}_i \boldsymbol{\varphi}_3 + \varepsilon_{it}$$

where $\boldsymbol{\varphi}_1$, $\boldsymbol{\varphi}_2$, and $\boldsymbol{\varphi}_3$, are parameter vectors giving the effect of workers' characeristics on the dip, the drop, and the recovery, respectively. To implement our parsimonious representation we include the full set of displacement dummies but only allow for interactions between worker characteristics and the three variables F_{it}^1, F_{it}^2, and F_{it}^3. Specification (5) forces the gap between the estimated losses of two workers (i) to be zero in the period more than three years prior to separation, (ii) to grow or decline linearly during the period from three years before separation until the quarter of separation, (iii) to be constant during the period from one to six quarters after displacement, and (iv) to grow or decline linearly from its value six quarters after separation until the end of the sample period. Accordingly, the losses k quarters after separation for a worker with characteristics $\mathbf{c}_i$, take the following form:

δ_k	if $k \leq -13$
$\delta_k + \mathbf{c}_i \boldsymbol{\varphi}_1(k+13)$	if $-12 \leq k \leq 0$
$\delta_k + \mathbf{c}_i \boldsymbol{\varphi}_2$	if $1 \leq k \leq 6$
$\delta_k + \mathbf{c}_i \boldsymbol{\varphi}_2 + \mathbf{c}_i \boldsymbol{\varphi}_3(k-6)$	if $k \geq 7$.

In cases in which worker characteristics are categorical variables, we can write specification (5) as

$$(5') \qquad y_{it} = \alpha_i + \gamma_t + \mathbf{x}_{it}\boldsymbol{\beta} + \sum_{k \geq -m} D_{it}^k \delta_k + \sum_j E_{it}^j \left(F_{it}^1 \varphi_{1j} + F_{it}^2 \varphi_{2j} + F_{it}^3 \varphi_{3j} \right) + \varepsilon_{it}$$

where E_{it}^j is an indicator variable for whether worker i is a member of group j and φ_{1j}, φ_{2j}, and φ_{3j} give the relative size of the dip, drop, and recovery for group j. If the second sum in (5′) extends over all possible levels of a categorical variable, then the model will not be of full rank. However, instead of dropping the first dummy variable, which would be equivalent to setting $\varphi_{\ell 1} = 0$ for $\ell = 1,2,3$, we impose the restrictions that $\sum \varphi_{\ell j} f_j = 0$ for $\ell = 1,2,3$, where f_j is the fraction of all displaced workers who are in group j. Alternatively, when worker characteristics are continuous rather than categorical variables, we subtract the variables' means over all displaced workers from their levels before forming the interaction terms in (5). The advantage of these parameterizations is that the average loss for all displaced workers for the kth quarter after separation simply equals δ_k. Moreover, φ_{1j}, φ_{2j}, and φ_{3j} express the difference between the jth group's dip, drop, and recovery and those of the average displaced worker.

Below, we implement versions of (5) that simultaneously include interactions for workers' gender, age, industry, firm size, and local labor-market conditions. Such estimates show how earnings losses depend on these factors, controlling for other factors that affect the pattern of losses. For example, we present estimates of how the temporal pattern of earnings losses differs between men and women, after controlling for any differences in their ages, industries, firm sizes, and local labor-market conditions.

C. *Potential Biases*

The foregoing statistical framework addresses several sources of bias that have plagued many previous studies. In particular, it is worth noting that no biases arise in the least-squares estimation of (2) if firms choose whom to layoff partly on the basis of the permanent characteristics embodied in workers' fixed effects. Similarly, least-squares estimation of (3) is unbiased even if firms tend to lay off workers partially on the basis of worker-specific fixed effects or time trends. However, our estimates are biased if firms selectively lay off employees whose performance was unusually poor in the quarters around the time of separation. In terms of our models, such behavior could be modeled by assuming that firms selectively lay off workers for whom the error term associated with the layoff date, ε_{is}, is low.

The importance of any resulting biases depends critically on the time-series properties of the error terms. For example, when those errors are independent across time as we assumed above, such behavior biases only estimates of δ_0, the displacement effect associated with workers' dates of separation. Unfortunately, when the errors are correlated over time, estimates of other displacement coefficients are likely to be biased. The program-evaluation literature sometimes accounts for this source of bias by explicitly modeling the selection process and simultaneously estimating its parameters along with those of the earnings equation (see e.g., Ashenfelter and Card, 1985; Card and Sullivan, 1988).

We have chosen not to estimate such a model because, for most commonly adopted specifications, doing so would have little or no impact on our estimates of long-run displacement effects. For example, if we assume that the error process for each individual is stationary, the spurious effects of displacement are symmetric about the date of separation.[21] We show below that the estimated effects of displacement are close to zero for periods more than three years before separation. Therefore, because the spurious and true effects of displacement are of the same sign, it follows that during this period both are close to zero and thus that during the period more than three years after separation the spurious effects of displacement are zero. Alternatively, given the assumptioin of stationarity, the evidence on earnings losses before separation shows that by three years after separation the error will have completely "regressed to the mean," implying little or no bias in estimates of long-term displacement effects.

The above argument fails when the error process is nonstationary. In this case, when firms discharge recent poor performers, the mean of ε_{it} conditional on displacement will not generally regress back to zero. Consequently, even our long-term loss estimates may be biased. However, we can substantially lessen the importance of this selectivity bias by restricting our analysis to workers who separate from firms that close all or a large part of their operations. Such workers are unlikely to have left their jobs as a result of their own poor performance. Therefore, in the empirical work that follows we give greater weight to the estimated earnings losses of workers in our mass-layoff sample.

III. Empirical Findings

The model developed in the previous section defines displaced workers' earnings losses as the difference between their quarterly earnings and their expected earnings had they remained with their former employer. We report estimates of that difference below for each quarter beginning with the 20th quarter prior to their separations and ending with the 27th quarter after their separations. To facilitate the exposition, we plot these estimated effects against the number of quarters before or after workers' separations.

A. *Earnings Losses and Mass Layoffs*

As shown by Figure 2, we find that high-tenure, prime-age workers endure substantial and persistent earnings losses when they

[21] See Heckman and Robb (1985) for a similar argument supporting the use of a symmetric difference-in-differences estimator in the estimation of earnings impacts of training programs.

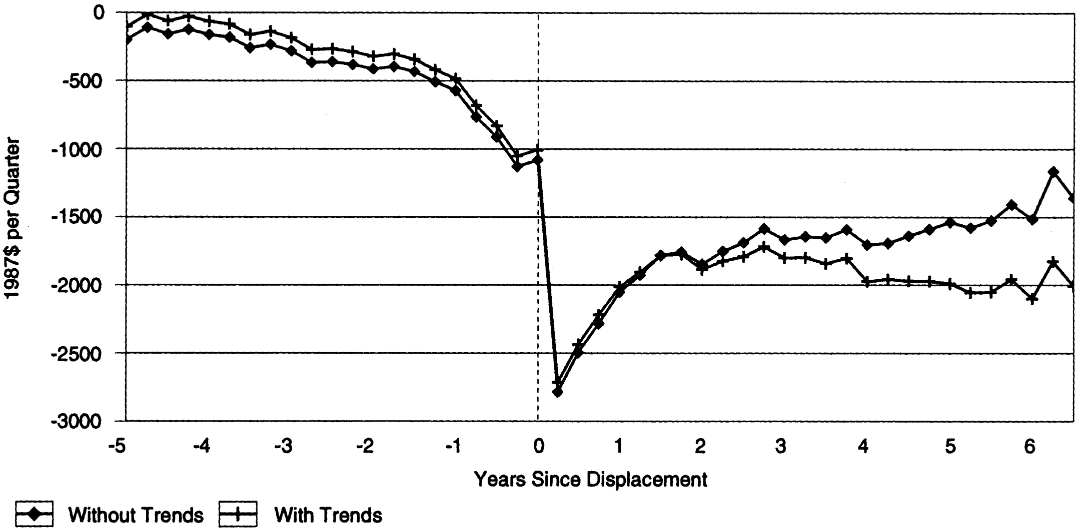

FIGURE 2. EARNINGS LOSSES FOR SEPARATORS IN MASS-LAYOFF SAMPLE

are displaced during or following mass layoffs. Even six years after their separations, their quarterly earnings remain $1,600 below their expected levels.[22] This loss represents 25 percent of their predisplacement earnings. Moreover, because the estimated loss is even larger when we control for worker-specific time trends, these estimates do not result from employers systematically displacing workers with more slowly growing earnings. Further, because the estimated losses do not decline significantly after the third year following their separations, there is little evidence that displaced workers' earnings will ever return to their expected levels.[23]

We also find evidence that the events that lead to workers' separations cause their earnings to depart from their expected levels even before they leave their firms.[24] As shown by Figure 2, these workers' quarterly earnings begin to diverge meaningfully from their expected levels approximately three years prior to separtion. That divergence accelerates during the quarters immediately prior to separation, so that by the quarter prior to displacement, these workers' earnings are approximately $1,000 below their expected levels. Although we cannot determine from our data whether these preseparation declines result from cuts in real wages or weekly hours, in other work we find that the incidence of temporary layoffs increased

[22]Although not shown, the quarterly employment rates of the displaced workers in our sample depart only slightly from their expected levels, except for the year following separation. This behavior for displaced workers' employment rates is not surprising, because our sample excludes workers with extremely long spells without wage and salary earnings. Thus, the substantial earnings losses observed in Figure 2 are largely due to lower earnings for those who work, rather than an increase in the number of workers without quarterly earnings.

[23]Because our sample is large, the estimated standard errors are relatively small. For example, between the fifth year prior to workers' separations and the second quarter after their job losses the standard errors associated with the displacement effects average $30 per quarter. After that quarter, the standard errors increase, so that by the 20th quarter following the separations the standard errors are approximately $60.

[24]Ruhm (1991 p. 322; using the Panel Study of Income Dynamics), David Blanchflower (1991 p. 489; using data from Great Britain), and Sara Di la Rica (1992; using the DWS) each report that displaced workers' earnings declined prior to separation.

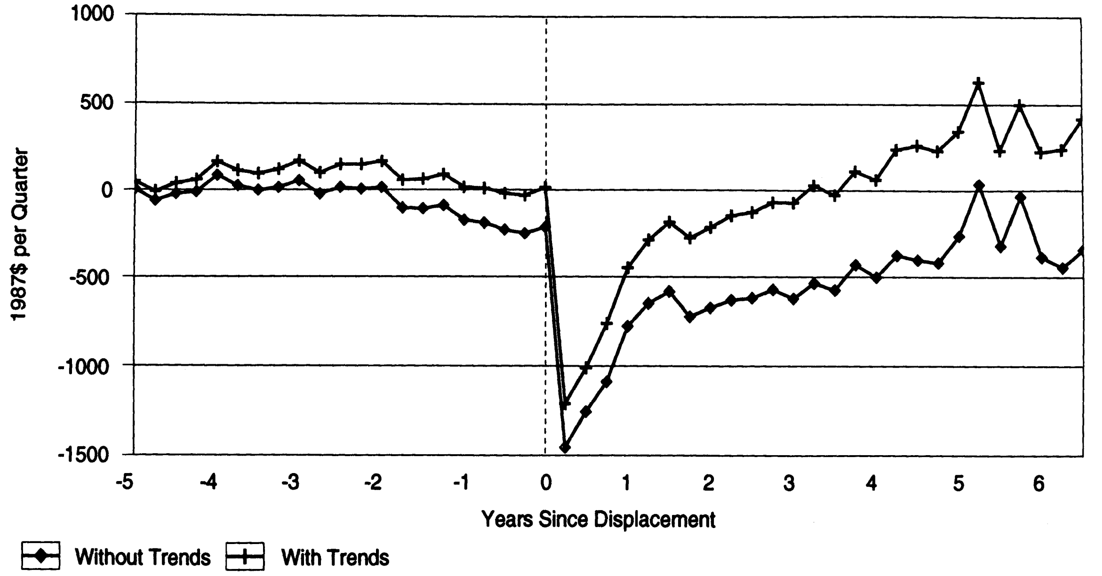

FIGURE 3. EARNINGS LOSSES FOR SEPARATORS IN NON-MASS-LAYOFF SAMPLE

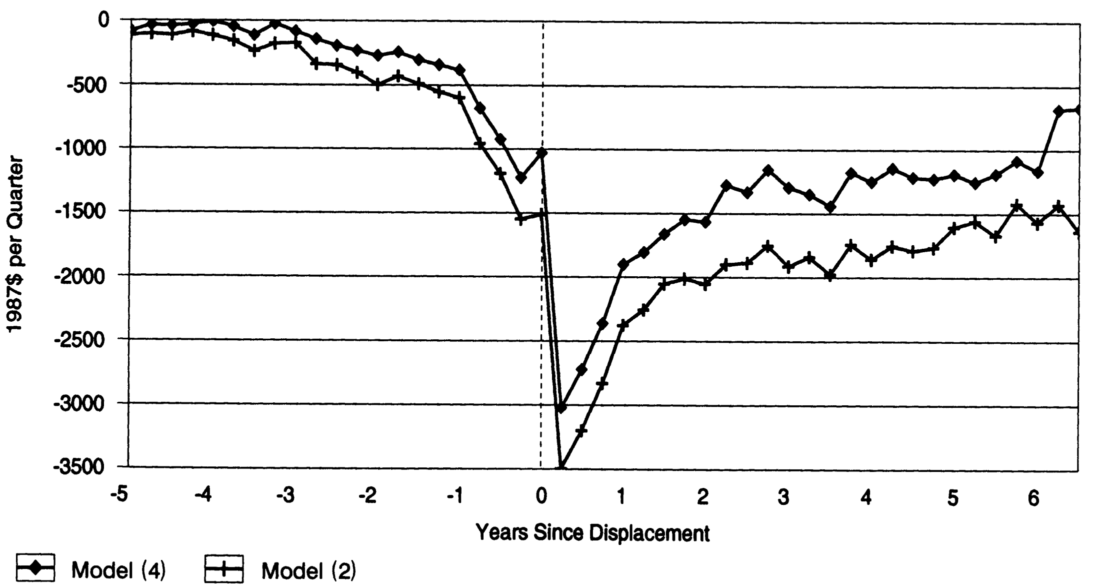

FIGURE 4. SENSITIVITY OF EARNINGS-LOSS ESTIMATES FOR MASS-LAYOFF SAMPLE TO DIFFERENT COMPARISON GROUPS

for these workers before their final separations (see Jacobson et al., 1993).

Our confidence in these results—that earnings losses are large and long-term and that they appear even before workers permanently lose their jobs—is enhanced because we do not find substantial estimated losses during the period 4–5 years before separation. Many forms of model misspecification would generate estimated "displacement effects" arbitrarily long before job loss. Instead, we find that estimated losses are small for time periods more than three years before separation. To explore this issue further, we relaxed the assumption of no displacement effects more than five years before separation by setting m in equation (2) equal to values of up to 10 years. In no case did we observe evidence of a meaningful displacement effect more than three years before workers' actual separations.

A different earnings-loss pattern emerges for workers from the non-mass-layoff sample. First, as shown by Figure 3, depending on which model we used to estimate the losses, this group's earnings fully recover 3–5 years following their separations. Second, prior to separations, their earnings depart only slightly from their expected levels, and following separation they drop by only one-half as much as workers in the mass-layoff sample. This pattern of earnings losses for the non-mass-layoff sample is not surprising, because this sample probably includes larger fractions of workers who quit their jobs or who had fewer firm-specific skills. Indeed, this pattern of losses for workers who are likely to adjust easily to separation enhances our confidence in our previous result that workers displaced during mass layoffs experience large earnings losses. The comparative ease of adjustment of workers in the non-mass-layoff sample demonstrates that there is nothing in our specification that necessarily generates large loss estimates.

The foregoing findings demonstrate that when estimating the effects of displacement it is important to have long time-series on workers' earnings histories as well as information about their firms. Studies that use data lacking these features, such as the DWS, have likely underestimated the earnings losses associated with worker displacement. For example, as shown by Figure 2, displaced workers' earnings are abnormally low in the year prior to separation. As a result, if we had only one year of preseparation earnings data, our earnings-loss estimates would have been nearly 50-percent smaller than the estimate based on workers' long-term earnings histories. Likewise, we might have underestimated workers' earnings losses if we had to rely on displaced workers' assessments of their firms' economic well-being rather than the firms' administrative records. As indicated by Figure 3, if workers who separated from "normal" firms report that they were laid off from distressed firms, researchers would understate the long-term losses associated with displacement.

B. *Sensitivity of Losses to Comparison Group*

In the foregoing analysis, high-tenure workers who remained with their firms for the entire sample period identified the influence of macroeconomic factors, γ_t, and of age and sex, $\mathbf{x}_{it}\boldsymbol{\beta}$, on earnings. As we previously observed, it is also of interest to compare displaced workers' earnings to those of nondisplaced workers in the same firm. The estimated earnings losses based on this alternative estimator should be smaller as long as nondisplaced workers in distressed firms have earnings that grow more slowly than those of other nondisplaced workers. Such a finding would suggest that nondisplaced workers' earnings are adversely affected by the events that lead to mass layoffs in their industry or firm.

As shown by Figure 4, when we use the nondisplaced workers in displaced workers' former firms to identify the influence of macroeconomic factors, the estimated earnings losses are smaller by about 20 percent. For example, five years after separation, displaced workers' quarterly earnings are $1,200 below (compared to $1,500 below) their expected levels when we use all nondisplaced workers to identify the influ-

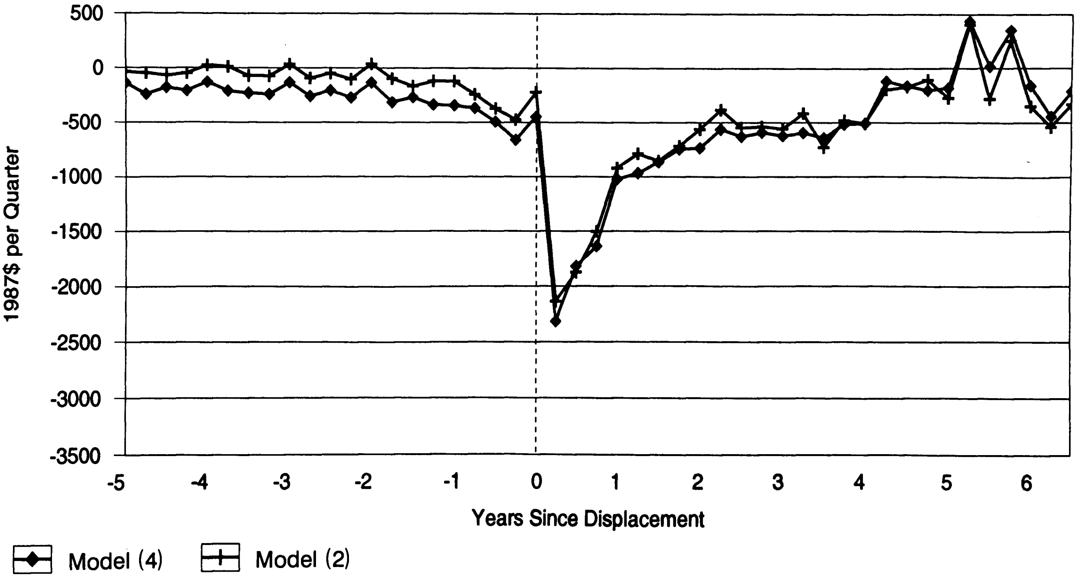

FIGURE 5. SENSITIVITY OF EARNINGS-LOSS ESTIMATES FOR NON-MASS-LAYOFF SAMPLE TO DIFFERENT COMPARISON GROUPS

ence of macroeconomic factors.[25] The gap between these two sets of estimates indicates that employees who remain employed during mass layoffs experience only modest declines in earnings relative to other nondisplaced workers.

It is also apparent from Figure 4 that, because the gap between the two sets of estimates becomes large only after separation, nondisplaced workers in distressed firms fall behind other nondisplaced workers only after their firms lay off large numbers of workers. Before the mass layoffs, the displaced workers' earnings fall substantially relative to either comparison group of nondisplaced workers. This implies that when firms seem likely to reduce their workforces dramatically, it is probably apparent which employees are most likely to be permanently laid off, namely, those who have experienced temporary layoffs in the past. This result suggests that stayers in distressed firms may not accept significant cuts in their own earnings because they do not consider themselves at risk for job loss.

Turning to the non-mass-layoff sample, we find that our earnings-loss estimates do not depend on the comparison group. As shown in Figure 5, the estimated earnings losses are the same whether or not we condition on a displaced worker's firm. This finding is not surprising, for when few employees separate from their firms it is unlikely that those separations would be associated with earnings losses for those who remain employed at the firm.

C. *Earnings Losses by Worker Group*

The findings reported above indicate that, on average, workers separating from firms during mass layoffs experience large long-term earnings losses. To determine how the pattern of these losses varies by worker characteristics, we use our mass-layoff sample to estimate several versions of model (5). That model allows workers' earnings-loss patterns to differ from the average pattern shown in Figure 2 in (i) their rates of

[25]The two sets of estimates in Figure 4 are based on the fixed-effects estimator described in equation (4). The sample of displaced workers used in Figure 4 differs from that used in Figure 2, because there is no corresponding comparison group for workers displaced during plant closings. Accordingly, in Figure 4 we use only workers displaced during mass layoffs when the firm continued its operations.

earnings decline during the 12 quarters prior to their job losses (their dip), (ii) their average quarterly earnings loss during the first six postseparation quarters (their drop), and (iii) their rate of earnings recovery after the sixth quarter following their separtions (their recovery).[26] In Table 2, we report estimates of these differences corresponding to differences in sex, decade of birth, industry, firm size, and local labor-market conditions. In addition, as a summary measure of groups' long-term losses, we report estimates of their losses during the fifth year following displacement. The set of columns on the left, labeled "without other controls," contains estimates of model (5) in which only one group of interactions is included in the model, while the set of columns on the right, labeled "with other controls," contains estimates of model (5) in which all interactions are included simultaneously.

To interpret the estimates in Table 2, consider the estimated differences between the patterns of men's and women's losses when no other interactions are included in the model. The dip coefficients reveal that men's and women's predisplacement earnings losses were, respectively, \$10.8 per quarter more and \$36.7 per quarter less than the average rate of decline depicted in Figure 2 (of approximately \$83.3 per quarter). The difference between these two figures indicates that, during the preseparation period, men's earnings declined by \$47.5 per quarter more than did earnings of their female counterparts. Thus, by the quarter of separation, the gap between men's and women's earnings losses was \$618 (or 47.5 multiplied by 13, the value of the dip time trend on the date of separation). Immediately after separation, the drop coefficients indicate that men's and women's quarterly earnings losses were, respectively, \$217 more and \$738 less than the average loss depicted in Figure 2 (of approximately \$2,219). Thus, we estimate that men's short-term earnings losses were \$955 per quarter more than women's losses. However, after this initial postdisplacement period, men's earnings rebound somewhat relative to women's earnings. The difference between their recovery coefficients indicates that their earnings rise \$28.5 (6.5 − 22.0) per quarter faster than women's earnings. Nevertheless, during the fifth year after displacement, their earnings losses exceed women's by \$2,398 (−545 − 1,853). Given the average level of losses, this estimate implies that during the fifth year after displacement men's losses amounted to \$7,143, and women's losses amounted to \$4,744.[27]

The difference observed above between men's and women's losses nearly disappears when we hold constant the distribution of workers' ages, industries, and firm sizes as well as local labor-market conditions at the time of their displacements. As shown by the second group of columns in Table 2, the difference between men's and women's rates of preseparation declines falls to only \$15 per quarter, and the difference between their initial postdisplacement losses falls to \$453 per quarter. The latter figure suggests that the women in our sample had somewhat fewer firm-specific skills or were less likely to have been receiving wage premiums on their old jobs. Finally, holding constant other factors, in the period more than six quarters after separation, women are estimated to recover \$20 per quarter more slowly than men. This result suggests that, other factors held constant, women are less likely to acquire new skills after their job losses.

What is most notable about the results for workers from different birth cohorts is

[26]The overall loss estimates obtained by estimating (5) for various sets of worker characteristics are quite similar to those plotted in Figure 2 and are therefore not shown.

[27]The estimates in the columns labeled "fifth year loss dif" are equal to four times the drop estimate plus 50 times the recovery estimate. The coefficient on the drop estimate is four because the drop estimate applies to each of the four quarters in the fifth year after displacement. The coefficient on the recovery coefficient is 50 because that is the sum of the values (11, 12, 13, and 14) that the recovery time trend takes on during the fifth year after displacement. The estimates labeled "fifth year loss" are equal to those labeled "fifth year loss dif" plus the average loss during the fifth year after displacement, which in terms of model (5) is $\delta_{17}+\delta_{18}+\delta_{19}+\delta_{20}$.

TABLE 2—LOSSES BY WORKER CHARACTERISTICS

		Without other controls[a]					With other controls[b]				
Group	Number	Dip[c]	Drop[d]	Recovery[e]	Fifth-year loss dif	Fifth-year loss	Dip	Drop	Recovery	Fifth-year loss dif	Fifth-year loss
Overall	6,435						−83.3	−2,179	15.4	—	−6,611
							(2.2)	(16)	(4.4)		(150)
Sex:											
Male	4,972	−10.8	−217	6.5	−545	−7,143	−3.4	−103	4.7	−177	−6,788
		(0.7)	(7)	(0.9)	(40)	(132)	(0.7)	(7)	(0.9)	(43)	(157)
Female	1,463	36.7	738	−22.0	1,853	−4,744	11.6	350	−16.0	602	−6,009
		(2.2)	(24)	(3.0)	(136)	(184)	(2.3)	(25)	(3.2)	(145)	(207)
Decade of birth:											
1930's	2,599	−0.0	116	−10.9	−79	−6,677	−0.3	55	−10.1	−284	−6,896
		(1.4)	(16)	(2.0)	(92)	(159)	(1.4)	(16)	(2.1)	(94)	(182)
1940's	2,584	7.2	3	4.6	241	−6,356	3.6	−28	5.6	171	−6,440
		(1.4)	(15)	(2.0)	(87)	(151)	(1.4)	(15)	(2.0)	(88)	(172)
1950's	1,252	−14.9	−247	13.1	−333	−6,932	−6.9	−58	9.4	238	−6,374
		(2.4)	(25)	(3.2)	(144)	(188)	(2.4)	(25)	(3.2)	(145)	(203)
Industry:											
Mining and construction	247	1.3	−497	7.5	−1,616	−8,435	9.5	−387	−0.1	−1,549	−8,160
		(5.6)	(58)	(7.6)	(332)	(352)	(5.8)	(59)	(7.8)	(339)	(369)
Nondurable manufacturing	1,206	26.5	624	−14.6	1,766	−5,052	18.3	338	−7.7	967	−5,644
		(2.3)	(25)	(3.3)	(144)	(188)	(2.6)	(28)	(3.7)	(160)	(224)
Primary metals	1,354	−121.2	−1,991	54.1	−5,256	−12,074	−104.5	−1,476	40.5	−3,878	−10,489
		(2.2)	(24)	(3.6)	(157)	(210)	(2.7)	(30)	(4.4)	(191)	(241)
Fabricated metals	436	21.0	611	−11.2	1,882	−4,936	15.9	488	−9.8	1,465	−5,146
		(4.2)	(44)	(6.4)	(274)	(301)	(4.2)	(45)	(6.5)	(279)	(312)
Nonelectrical machinery	632	47.9	1,005	−36.9	2,174	−4,644	35	797	−27.4	1,817	−4,794
		(3.4)	(38)	(5.8)	(249)	(284)	(3.5)	(39)	(5.9)	(257)	(306)
Electrical machinery	421	43.2	288	7.0	1,500	−5,318	49.5	494	−2.7	1,842	−4,769
		(4.2)	(46)	(6.1)	(270)	(300)	(4.3)	(47)	(6.4)	(282)	(322)
Transportation equipment	419	25.0	422	−27.5	310	−6,508	14.1	215	−15.5	85	−6,526
		(4.3)	(46)	(6.2)	(264)	(291)	(4.4)	(48)	(6.6)	(282)	(324)
Other durable manufacturing	441	25.6	525	3.0	2,248	−4,570	18.9	338	9.1	1,807	−4,804
		(4.2)	(43)	(5.5)	(237)	(262)	(4.2)	(43)	(5.7)	(242)	(282)
Transportation, communication, and public utilities	348	6.6	150	−63.5	−2,573	−9,392	5.5	66	−63.6	−2,916	−9,527
		(4.7)	(49)	(7.0)	(295)	(321)	(4.8)	(50)	(7.1)	(301)	(333)
Wholesale and retail trade	545	18.7	198	2.0	891	−5,927	20.0	126	4.8	745	−5,866
		(3.7)	(38)	(4.8)	(207)	(235)	(3.8)	(38)	(4.9)	(211)	(251)
Finance, insurance, and real estate	183	127.7	1,312	14.3	5,963	−855	115.7	947	24.3	5,004	−1,608
		(6.6)	(70)	(8.2)	(352)	(369)	(6.7)	(72)	(8.3)	(358)	(387)
Professional, business, and entertainment services	203	82.0	1,158	−18.2	3,725	−3,093	93.1	1,270	−26.2	3,769	−2,843
		(6.3)	(63)	(8.4)	(360)	(378)	(6.4)	(64)	(8.7)	(369)	(394)
Firm size:											
50–500	1,704	7.9	351	0.6	1,434	−5,403	−16.1	−37	13.0	501	−6,110
		(1.9)	(20)	(2.6)	(113)	(163)	(2.1)	(22)	(2.9)	(124)	(193)
501–2,000	1,497	33.5	501	−14.1	1,298	−5,540	13.9	214	−4.7	625	−5,986
		(2.0)	(22)	(2.9)	(127)	(176)	(2.2)	(23)	(3.1)	(135)	(246)
2,001–5,000	1,381	40.9	720	−32.3	1,267	−5,570	27.2	480	−23.8	730	−5,881
		(2.2)	(23)	(3.1)	(134)	(179)	(2.3)	(24)	(3.5)	(149)	(203)
Greater than 5,000	1,853	−64.8	−1,265	34.9	−3,312	−10.150	−16.7	−497	9.6	−1,510	−8,121
		(1.8)	(19)	(2.9)	(125)	(190)	(2.3)	(25)	(3.6)	(154)	(224)

TABLE 2—*Continued.*

		Without other controls[a]					With other controls[b]				
Group	Number	Dip[c]	Drop[d]	Recovery[e]	Fifth-year loss dif	Fifth-year loss	Dip	Drop	Recovery	Fifth-year loss dif	Fifth-year loss
Local labor market:											
Employment trend		129.2 (7.5)	2,391 (84)	−73.2 (10.9)	5,903 (485)		38.8 (7.9)	743 (87)	18.1 (11.7)	2,069 (520)	
Employment deviation		79.9 (6.4)	900 (64)	−50.3 (9.3)	1,082 (401)		51.8 (6.4)	376 (65)	−40.9 (9.4)	−540 (408)	
Unemployment rate		41.5 (8.9)	213 (97)	−29.7 (14.3)	−635 (631)		1.2 (9.0)	−555 (98)	1.3 (14.6)	−2,153 (643)	

Notes: "Dip," "drop," "recovery," and "fifth year loss diff" columns give groups' deviations from the mean of the variable for all displaced workers given in the first row of the table. See text for full explanations of entries. Numbers in parentheses are standard errors.

[a]Estimates derived from models that include interactions with exactly one of sex, birth cohort, industry, firm size, or local labor-market interactions.

[b]Model includes all interactions with sex, birth cohort, industry, firm size, and local labor market.

[c]Coefficient on predisplacement time trend.

[d]Coefficient on dummy for first six quarters after displacement.

[e]Coefficient on postseparation time trend.

that the differences between these groups' patterns of losses are generally very small. Younger workers have a somewhat greater rate of decline in the period before separation, probably reflecting a greater vulnerability to temporary layoffs due to lower levels of seniority, but the difference is barely statistically significant. Younger workers also have a larger drop in earnings in the period after displacement, but when other controls are included, the gap in quarterly losses between workers born in the 1930's and those born in the 1950's is estimated to be only $113. The larger initial losses suffered by younger workers are quickly canceled by their faster rate of recovery. During the period more than six quarters after separation, the youngest group of workers have earnings that recover $19.4 per quarter faster than those of the oldest workers. As a result, in the fifth year after separation, the oldest workers lose $521 more than the youngest workers. The finding of more slowly growing earnings of older workers is consistent with their facing a shorter time horizon and thus being less likely to acquire new skills.

Our results on how losses vary with the characteristics of displaced workers' former firms indicate that workers' earnings losses are substantial across a broad range of industries and firm sizes. The pattern of losses for workers displaced from industries as diverse as nondurable manufacturing, motor vehicles, and wholesale and retail trade closely resembles the pattern depicted in Figure 2. Likewise, the pattern of losses for workers displaced from smaller firms, with between 50 and 500 employees, is similar to the pattern for workers displaced from larger firms, with between 2,000 and 5,000 employees.

Although workers experience large losses regardless of their industry or firm size, these characteristics are nevertheless important determinants of the magnitude of their losses. The differences in the magnitude of losses across both industries and firm sizes suggest that the loss of rents, including union premiums, may contribute to workers' earnings losses. Losses were especially large, both prior to and after displacement, among workers separating from the heavily unionized mining and construction, primary metals, and transportation, communications, and public-utility sectors. Consistent with this evidence on the potential loss of rents, we also find much larger losses among workers displaced from very large firms. By contrast, losses were relatively small among workers displaced from the largely nonunion business and professional service and financial sectors.

To assess the importance of labor-market conditions on workers' losses, we included in $\mathbf{c}_i$ in equation (5) variables that summarize both their locales' long-term economic conditions and business-cycle conditions at the time of their job losses.[28] We summarize the locale's long-term labor-market conditions by its trend in nonagricultural employment. We summarize the effect of the locale's cyclical conditions on the date of workers' separations with two variables: (i) the locale's unemployment rate and (ii) the deviation of the locale's employment level from its trend.[29] Because the coefficients of these labor-market variables are difficult to interpret, Table 2 presents estimates of differences between earnings losses for those displaced amidst what were approximately the best and worst conditions observed in the data. The range of quarterly employment growth rates among Pennsylvania locales was approximately 0.01, corresponding to the difference between these rates in its strongest labor market, Lancaster, and its weakest labor market, Johnstown. The range of unemployment rates and employment deviations are both approximately 0.1, corresponding to a 10-percentage-point difference in these variables between the peak and the trough of the business cycle.

Our findings show that workers' losses increase when the workers are displaced in regions that have depressed rates of employment growth. In the quarter prior to their separations, workers displaced in the weakest labor markets have losses that are more than \$500 (13×38.8) larger than those experienced by workers displaced in the strongest markets. The gap between workers' losses widens to approximately \$750 per quarter during the first year following their separations and is still \$500 per quarter five years after their separations. This long-term differential between the losses suffered in the strongest and weakest labor markets corresponds to about one-third of the average loss depicted in Figure 2.

The figures in Table 2 also indicate that cyclical conditions at the time of workers' job losses have substantial and long-lasting effects on their earnings. By themselves, neither unemployment rates nor employment deviations adequately capture the impact of a locale's cyclical conditions on the pattern of workers' losses. The results indicate that differences in locales' unemployment rates correspond to differences in postseparation earnings declines but are not correlated with the rate of predisplacement earnings decline or the rate of postdisplacement earnings recovery. The impact of cyclical conditions on these terms is better captured by variation in local employment deviations at the time of job loss. Together, the two measures indicate that locales' cyclical conditions affect the magnitude of both workers' pre- and postdisplacement losses. Moreover, severe cyclical conditions have an enduring impact on workers' earnings. The figures in Table 2 indicate that workers displaced during particularly adverse cyclical conditions have losses after five years that are nearly \$1,500 larger than those experienced by workers displaced during the best cyclical conditions.

Like industry and firm size, local labor-market conditions are important determinants of earnings losses. It is important to recognize, however, that even workers displaced in strong labor markets experience large losses. Our figures indicate that workers displaced in the best of circumstances have losses that are at most only one-third less severe than the average losses depicted in Figure 2.[30] This finding suggests that some valuable attribute of the employment relationship itself is lost when high-tenure workers are displaced.

[28]For details on the local labor-market variables see the Appendix.

[29]As we noted in Section II-B, to construct the variables entering $\mathbf{c}_i$, we subtracted these three continuous variables' means, taken over all displaced workers, from their levels for each worker.

[30]We have obtained similar estimates of the importance of local labor markets in models that include regional dummies in the list of variables in $\mathbf{c}_i$.

D. *Losses and Sector of New Jobs*

To explore further this possibility we examined the relationship between workers' losses and the industrial sector of their new jobs.[31] If the skills required on two jobs are more similar when the jobs are in the same industry and if the loss of specialized skills is an important determinant of workers' losses, displaced workers returning to the same industry should experience smaller earnings declines than those whose new jobs lie outside their old industry. Accordingly, we examined the earnings losses of workers whose new jobs were (i) in the same four-digit SIC industry as their old job, (ii) in the same sector (manufacturing or nonmanufacturing) but in a different four-digit industry, or (iii) in a different sector.

Manufacturing workers' earnings losses depend crucially on whether they obtain new jobs in the manufacturing sector. As shown by Table 3, the losses of those who leave the manufacturing sector equal 38 percent of their predisplacement earnings.[32] However, for those who found new jobs in the manufacturing sector it does not appear to matter whether they found a job in their old four-digit industry. As shown in panel A, 24 quarters after their separations workers' losses were 20 percent of predisplacement earnings if they found new jobs in the same four-digit industry, compared with 18 percent if they found new manufacturing jobs in different four-digit industries.

TABLE 3—EARNINGS LOSSES BY SECTOR OF NEW JOB: DEVIATION BETWEEN ACTUAL AND EXPECTED QUARTERLY EARNINGS

	New job in same sector		
Quarters since separation	Same four-digit SIC	Different four-digit SIC	New job in other sector
A. Displaced Manufacturing Workers:			
−8	−$379	−$117	−$237
	(82)	(67)	(73)
	[−7]	[−2]	[−4]
12	−1,044	−1,117	−2,616
	(82)	(67)	(73)
	[−19]	[−21]	[−44]
24	−1,103	−958	−2,221
	(197)	(137)	(150)
	[−20]	[−18]	[−38]
B. Displaced Nonmanufacturing Workers:			
−8	−229	−26	−151
	(132)	(128)	(231)
	[−4]	[0]	[−3]
12	−1,129	−1,305	−1,498
	(132)	(128)	(231)
	[−18]	[−23]	[−26]
24	−1,103	−1,276	−1,949
	(315)	(241)	(476)
	[−18]	[−22]	[−33]

Notes: Numbers in parentheses are standard errors. Numbers in square brackets express the estimated losses as a percentage of predisplacement earnings.

The findings for displaced nonmanufacturing workers are similar to those for their manufacturing counterparts. The long-term earnings losses for those who find new jobs in the same four-digit industry amount to 18 percent. That percentage rises to 22 percent when their new jobs are in different four-digit industries but still in the same sector. Finally, those losses are larger for those who found new jobs in the manufacturing sector, though the standard error associated with that estimate is relatively large, as few displaced nonmanufacturing workers found jobs in manufacturing. Nevertheless, the findings for both displaced manufacturing and nonmanufacturing workers indicate that a substantial portion of their earnings losses result from the loss of some highly firm-specific component of earnings. Even those

[31] In keeping with this study's focus on displacement's long-term impact, we want to assess the relationship between earnings losses and the industry of workers' new jobs several years following separation. For workers displaced in 1985 and 1986 such an assessment is impossible because we have only a few quarters of postseparation data. Accordingly, we examined the relationship between earnings losses and new job's industry for workers displaced from distressed firms between 1980 and 1983. The new job's industry was the workers' primary employer in 1986, which was 3–6 years following displacement.

[32] This finding showing greater losses when displaced workers switch sectors does not result because workers with jobs in the nonmanufacturing sector have been displaced for a shorter period of time. The mean quarter of separation for those who switch sectors is the same as for those who remain in the manufacturing sector.

who found new employment in the same industry experienced large and persistent losses.

IV. Conclusion

Consistent with previous research using other data sets, we have shown that high-tenure workers experience substantial earnings losses when they leave their jobs. In addition, we have shown that, for workers displaced from distressed firms, these losses (i) are long-term, with little evidence of substantial recovery after the third year; (ii) arise even prior to workers' separations; (iii) vary modestly with local labor-market conditions, industry, and firm size; (iv) do not depend very much on workers' gender and age; and (v) are substantial even for those who find new jobs in similar firms.

The significance of these results is heightened by the large number of workers adversely affected by structural change during the first half of the 1980's. Because we employed a 5-percent sample of Pennsylvania workers, we estimate that approximately 135,000 of that state's high-tenure workers were permanently laid off from distressed firms. Further, because approximately 5 percent of U.S. workers are employed in Pennsylvania, if the rest of the nation experienced similar rates of displacement, our results represent the experiences of 2.6 million workers nationwide. This estimate agrees closely with the number of high-tenure prime-age displaced workers reported by the DWS during approximately the same period.[33]

Our findings also bear on the importance of several alternative theories of why job losses should be costly. First, losses are larger in settings where unions or rent-sharing are likely to be prevalent. Second, long-term losses depend modestly on business-cycle conditions at the time of workers' job losses. This result is consistent with implicit-contracting models of wage determination (see Paul Beaudry and John DiNardo, 1991). Third, the relatively slow rates of earnings recovery after workers secure new jobs suggests that wage gains generated from idiosyncratic job-matching must accrue slowly over time. Finally, our results indicate that there is something intrinsic to the employment relationship itself that is lost when workers are displaced. If it is workers' skills that are lost, these skills must be firm-specific, as opposed to industry-specific. Alternatively, such earnings losses may result from the workings of internal labor markets. Although we cannot assess the relative importance of these two alternatives, our finding that losses are large for almost every group we studied suggests that wage premiums—whether due to firm-specific skills or to internal labor markets—must be commonplace.

Appendix

A. *Constructing the Data*

We constructed our longitudinal data from a 5-percent sample of workers included in Pennsylvania Unemployment Insurance (UI) tax reports and the state ES202 data on firms' employment. The firms' UI tax records report the quarterly wage and salary earnings for each employee. Because the state requires accurate and timely information to calculate unemployment-insurance taxes and workers' benefits, it cross-checks these earnings records against earlier reports and federal corporate tax returns. In these data, employers report their employees' total earnings; unlike Social Security earnings data, these data are not top-coded. For a portion of the sample, the UI tax records also identify workers' sex, age, and race, data that the state obtained from the Social Security Administration in 1976. Unfortunately, even for workers who were in the Pennsylvania labor force in 1976, this information is sometimes missing.

[33]To obtain the latter number, we applied the same tenure and age restrictions used in our study to the 1984 and 1986 DWS's. We counted persons who were displaced from full-time private nonagricultural jobs, but we excluded persons who reported that they had been self-employed or were displaced for unspecified reasons. Like our sample, the resulting DWS sample covered a seven-year period, but it began in 1979 instead of 1980.

The ES202 reports provide the information about firms' employment that the Bureau of Labor Statistics uses to compile its reports on employment and earnings. A key element of our analysis is that we use the information on the sources of workers' earnings to track accurately their separations from individual firms. Thus, it is important to account for cases in which a firm's employer identification number (EIN) changes from one period to the next, creating the appearance of a closing followed by an opening of a new firm. Fortunately, the Pennsylvania ES202 data include files detailing EIN changes which we were able to use to construct a consistent set of corrected EIN's. In several years, well over 5 percent of total employment was affected by EIN changes. Indeed, had we not eliminated bogus changes, such changes would be the primary source of movement of workers between employers with the same four-digit SIC but different EIN's. In cases of mergers and divestitures that occurred during the sample period, we treated the separate parts as a single firm, even in years when they were legally distinct.

Finally, we created our longitudinal file by merging UI tax reports and ES202 records with the same corrected EIN. Both sets of data also include a plant identification number; but the coding schemes differ between the two files, so that we were unable to obtain an employment figure for each worker's establishment. The resulting file contains workers' quarterly earnings from 1974 to 1987 and, for each calendar year, their principal employers' EIN, four-digit SIC industry, location, and average employment during the last, current, and following years. To keep the data-processing to a manageable level, we only attached data on the employment of the firm from which the worker received the greatest amount of earnings during the year.

B. *Dating Workers' Separations*

We used two pieces of information from our Pennsylvania data to determine which workers separated from their firms and when those separations occurred. First, a change from one year to the next in the EIN of a worker's principal employer was taken to indicate his separation from his incumbent firm. Second, data on the percentage of total quarterly earnings received from the year's principal employer were used to date the quarter of that separation. In particular, we attempted to determine the last quarter that the employee received earnings from the old principal employer. When this quarter was clearly in the last year when the old employer was still the principal employer, the quarter of separation was the last quarter of positive earnings from that employer. For example, if the worker has earnings from the old employer in the third but not the fourth quarter of the last year when the old employer was the principal employer, we declare the separation to have occurred in the third quarter of that year. However, when the worker derived 100 percent of his fourth-quarter earnings from the old principal employer, the separation date was taken to be the quarter of the following year in which the employee last received earnings from sources other than the new principal employer. For example, if the worker receives all of his earnings from the new employer in the second but not the first quarter of the first year the new employer is the principal employer, we declare the separation to have occurred in the first quarter of that year.

In most instances the foregoing procedure precisely dates the separation. There appear to be two exceptions: first, when the worker has another wage or salary job besides the job with the incumbent firm; second, when the incumbent firm grants the employee severance pay after displacement. Both of those exceptions may cause us to date the separation after it actually occurred. Thus, our dating procedure may cause displaced workers' earnings to appear falsely to decline during the quarters prior to displacement. However our finding reported in the text, showing that predisplacement earnings losses are small in the non-mass-layoff sample, would imply that if there was a problem with our dating of separations, it would only occur when workers separate from distressed firms. We know of no reason why that should be the case in these data.

C. *Sample Restrictions*

As we noted in the text, we have chosen to focus on high-tenure workers and have restricted our sample in other ways in order to avoid the difficulties associated with early retirement and lack of attachment to Pennsylvania's wage and salary work force. The high-tenure workers we examine are those that were hired by their firms prior to 1974 and who remained with those firms (i.e., did not experience a change of EIN) at least through the end of 1979. Further, we limited our sample to those workers for which the sex and age variables were present, though we did not require them to have data on race, a variable that was more frequently missing. In order to make our construction of the mass-layoff sample more reasonable we eliminated workers whose 1979 firms had fewer than 50 employees.

To reduce the problems associated with early retirement, our sample includes only workers born between 1930 and 1959. As a result, in 1979, workers in our sample were at most 50 years of age and thus very unlikely to retire following their separations.

Finally, to avoid the difficulties associated with persons who seem to disappear from our data set, we required displaced workers to have positive wage or salary earnings in each calendar year between 1974 and 1986. This restriction eliminated approximately 38 percent of our sample of high-tenure prime-age separators. A majority of the eliminated workers (70 percent) never had any positive reported earnings following their job losses. Prior to their displacements, this group earned $250 more per quarter and were one and one-half years older than workers in our sample. These persons were also modestly more likely to be female and to have been displaced from the service sector.

D. *Local Labor-Market Conditions*

We obtained information on local employment and unemployment rates for 1976 through 1987 from various issues of the U.S. Department of Labor's publication *Employment and Earnings*. To compute a locale's trend level of employment growth we regressed its log nonagricultural employment on a time trend and on a vector of seasonal dummy variables. The coefficients on the trend variable represented a locale's long-term employment conditions. We controlled for a locale's business-cycle conditions by using its unemployment rate and the deviation of employment from trend. We constructed separate series for 12 of the state's labor markets: Allentown, Altoona, Erie, Harrisburg, Johnstown, Lancaster, Philadelphia, Pittsburgh, Reading, Williamsport, York, and Scranton-Wilkes Barre. We assigned the series for the whole state to workers from firms not in one of these markets.

REFERENCES

Addison, John T. and Portugal, Pedro, "Job Displacement, Relative Wage Changes, and Duration of Unemployment," *Journal of Labor Economics*, July 1989, *7*, 281–302.

Ashenfelter, Orley, "Estimating the Effect of Training Programs on Earnings," *Review of Economics and Statistics*, February 1978, *60*, 47–57.

______ **and Card, David,** "Using the Longitudinal Structure of Earnings to Estimate the Effect of Training Programs," *Review of Economics and Statistics*, November 1985, *67*, 648–60.

Beaudry, Paul and DiNardo, John, "The Effect of Implicit Contracts on the Movement of Wages over the Business Cycle: Evidence from Micro Data," *Journal of Political Economy*, August 1991, *99*, 665–88.

Becker, Gary S., *Human Capital*, 2nd Ed., New York: National Bureau of Economic Research, 1975.

Blanchflower, David, "Fear, Unemployment, and Pay Flexibility," *Economic Journal*, May 1991, *101*, 483–96.

Bound, John and Krueger, Alan B., "The Extent of Measurement Error in Longitudinal Earnings Data: Do Two Wrongs Make a Right?" *Journal of Labor Economics*, January 1991, *9*, 1–24.

Card, David and Sullivan, Daniel, "Estimating the Effect of Subsidized Training Programs on Movements In and Out of Employment," *Econometrica*, May 1988, *56*, 497–530.

Di la Rica, Sara, "Displaced Workers in Mass Layoffs: Pre-Displacement Earnings Losses and Unions Effect," Industrial Relations Section Working Paper No. 303, Princeton University, 1992.

Duncan, Greg J. and Hill, Daniel H., "An Investigation of the Extent and Consequences of Measurement Error in Labor-Economics Data on Earnings," *Journal of Labor Economics*, October 1985, *3*, 508–32.

Flaim, Paul and Seghal, Ellen, "Displaced Workers of 1979–83: How Well Have They Fared?" Bulletin No. 2240, U.S. Department of Labor, Bureau of Labor Statistics, Washington, DC, 1985.

Hamermesh, Daniel S., "What Do We Know About Worker Displacement in the U.S.?" *Industrial Relations*, Winter 1989, *28*, 51–9.

Heckman, James J. and Hotz, V. Joseph, "Choosing Among Alternative Nonexperimental Methods for Estimating the Impact of Social Programs: The Case of Manpower Training," *Journal of the American Statistical Association*, December 1989, *84*, 862–74.

______ and Robb, Richard, "Alternative Methods for Evaluating the Impact of Interventions," in James J. Heckman and Burton Singer, eds., *Longitudinal Analysis of the Labor Market Data*, Cambridge: Cambridge University Press, 1985, pp. 156–245.

Jacobson, Louis S., "Structural Change in the Pennsylvania Economy," mimeo., W. E. Upjohn Institute for Employment Research, Kalamazoo, MI, 1988.

______, "The Dynamics of the Pittsburgh Labor Market," mimeo., W. E. Upjohn Institute for Employment Research, Kalamazoo, MI, 1991.

Jacobson, Louis S., LaLonde, Robert J. and Sullivan, Daniel G., "The Costs of Worker Dislocation," W. E. Upjohn Institute for Employment Research Monograph Series, 1993 (forthcoming).

Jovanovic, Boyan, "Job Matching and the Theory of Turnover," *Journal of Political Economy*, October 1979, *87*, 972–90.

Kletzer, Lori Gladstein, "Returns to Seniority After a Permanent Job Loss," *American Economic Review*, June 1989, *79*, 536–43.

LaLonde, Robert J., "Evaluating the Econometric Evaluations of Training Programs with Experimental Data," *American Economic Review*, September 1986, *76*, 604–20.

Lazear, Edward P., "Agency, Earnings Profiles, Productivity, and Hours Restrictions," *American Economic Review*, September 1981, *71*, 606–20.

Lewis, H. Gregg, *Union Relative Wage Effects: A Survey*, Chicago: University of Chicago Press, 1986.

Ruhm, Christopher J., "Are Workers Permanently Scarred by Job Displacements?" *American Economic Review*, March 1991, *81*, 319–23.

Stiglitz, Joseph E., "Alternative Theories of Wage Determination and Unemployment in LDCs: The Labor Turnover Model," *Quarterly Journal of Economics*, May 1974, *88*, 194–227.

Swaim, Paul and Podgursky, Michael, "The Distribution of Economic Losses Among Displaced Workers: A Replication," *Journal of Human Resources*, Fall 1991, *26*, 742–55.

Tannery, Frederick J., "Labor Market Adjustments to Structural Change: Comparisons Between Allegheny County and the Rest of Pennsylvania, 1979–87," mimeo, Economic Policy Institute, University of Pittsburgh, 1991.

Topel, Robert, "Specific Capital and Unemployment: Measuring the Costs and Consequences of Worker Displacement," *Carnegie-Rochester Series on Public Policy*, Autumn 1990, *33*, 181–214.

U.S. Bureau of the Census, *Current Population Survey, January 1986: Displaced Workers*, Ann Arbor, Michigan: Inter-university Consortium for Political and Social Research, 1988.

U.S. Department of Labor, *Employment and Earnings*, Washington, DC; U.S. Government Printing Office, various issues.