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1. INTRODUCTION

Concern over the plight of experienced workers losing jobs due to trade liberalization, increased environmental protection, or technological change has been an important part of recent public policy debate.[1] These displaced workers are widely recognized to experience costly spells of unemployment and short-term earnings declines. However, less is known about the long-term earnings losses imposed on these workers, losses that may greatly exceed those suffered in the form of unemployment. In this paper, we use a new data set, derived from the administrative records of the state of Pennsylvania, to assess the magnitude and temporal pattern of long-term earnings losses experienced by high-tenure displaced workers. In addition to better estimating the average loss, we show how losses vary among workers 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 losses beyond a period of unemployment following their job losses. First, workers possessing skills that were especially suited to their old positions are likely to be less productive, at least initially, in their subsequent work. 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 a job that paid a wage premium 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

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.

2. For example, on the former possibility see Becker (1975) and on the latter possibility see Jovanovic (1979).

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 a "tilted" tenure profile in order to enhance their employers' incentives to invest in their human capital.[4]

Our study focuses on high-tenure workers because they are the ones that are most likely to have accumulated a substantial amount 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 policy makers and others as to the magnitude and persistence of displaced workers' earnings declines. To answer such questions requires empirical work like that presented in this paper. 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 using the Displaced Worker Survey[5] (DWS) supplements to the Current Population Survey (CPS), find that workers characterizing themselves as displaced frequently report lower earnings on their new jobs.[6] However, there are several shortcomings associated with the DWS that make it difficult to use to assess the magnitude and temporal pattern of displaced workers' earnings losses. These shortcomings include the lack of a comparison group of nondisplaced workers and of sufficient pre-displacement earnings data, and a documented tendency for workers not to report more remote instances of displacement.[7] Recently, Ruhm (1991) avoided these problems, by using the

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3. For example, on the former possibility see Lewis (1986) and on the latter possibility see Stiglitz (1974).

4. See, for example, Lazear (1981).

5. See Flaim and Seghal (1985).

6. Hamermesh (1989) summarizes a dozen of these studies. More recent studies include Addison and Portugal (1989), Kletzer (1989), Topel (1990), Swaim and Podgursky (1991).

7. See, for example, Topel (1990).

Panel Study of Income Dynamics (PSID). His results are broadly in agreement with ours reported below. 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.

We develop this detailed picture by exploiting the features of an unusual longitudinal data set that we have created by merging administrative records covering 13 years of workers' quarterly earnings histories with information about their firms. As we explain below, these data which comprise a long panel of quarterly earnings histories for a large number of high-tenure displaced and nondisplaced workers, including workers who remained employed at displaced workers' former firms, offer several advantages over the data used in other studies.

As previous studies have found, we find that high-tenure workers separating from distressed firms incur large losses when they separate from their firms. However, in addition, we find a consistent temporal pattern to these losses in which displaced workers' earnings decline substantially even prior to their separations, drop sharply at the time of separation, and then rise rapidly during the six quarters immediately following their separations. But after that point these workers' earnings recover very slowly, so that five years after separating from their former firms, their losses are still equal to 25 percent of their pre-displacement earnings. This finding of large losses holds for virtually every group of workers that we examine. Male and female workers, as well as younger and older workers suffer similarly large losses. There is more diversity among the losses experienced by workers in different industries and sizes of firms, and those displaced amidst different demand conditions. However, our basic finding holds for workers formerly employed across a broad spectrum of industries and labor market conditions. Further, even those workers finding work in the same industries as their old jobs experience large earnings losses.

The remainder of this paper proceeds as follows: In section 2 we describe our longitudinal data and comment on some of their strengths and shortcomings. In section 3 we discuss the statistical issues involved in estimating the earnings losses incurred by displaced workers. In section 4 we present estimates of the earnings losses for high-tenure workers who separated from their firms during the early and mid-1980s. Some concluding remarks follow in section 5.

2. The Pennsylvania Data

The statistical framework developed in this paper 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 1980s. 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' ES202 data, we have created[8] 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 "4-digit SIC" industry. In addition, for most workers who were in the labor force in 1976, we have information on sex and year of birth.[9] 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 sets used in other studies. First, we have a large sample of non-displaced 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 due to lost earnings growth that would have occurred 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.[10] Third, we

8. For details on how we constructed our data see the appendix.

9. Our empirical analysis is limited to workers for whom we do know age and sex.

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 separate from distressed firms. Such workers are likely to have been displaced rather than to have quit or been dismissed for cause.

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,[11] while our data are based on firms' reports that are used to calculate tax liabilities and are presumably virtually free of measurement error. 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 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 1980s – 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.[12] 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.

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10. One interpretation of the exchange between LaLonde (1986) and Heckman and Hotz (1989) is that reliable non-experimental estimation of program impacts requires data on workers a substantial amount of time prior to their participation.

11. See Bound and Krueger (1991) and Duncan and Hill (1985).

12. See Jacobson (1988).

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 they do not explicitly identify whether workers separations resulted from quits, discharges for cause or displacements.[13] In order to minimize these ambiguities, we used the information about changes in firms' workforces to split our sample of separators into two groups. Specifically, 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 1970s.[14] This definition encompasses firms that closed around the time of workers' separations as well as others that had large employment declines. The "non-mass-layoff" sample includes all other separators. Although some employees from that 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.[15]

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13. 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 1 year of tenure, 43 percent for workers with one year of tenure, 24 percent for workers with two to 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, he estimated that one-half were retirements and one-third were displacements. Thus the quit rate for that group would be about 2 percent per year.

14. 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.

15. We have experimented with other similar definitions of "mass-layoff" and found results similar to those presented below.

Finally, the most important disadvantage of our data is that it is impossible to distinguish between workers who for some reason leave the Pennsylvanian wage and salary work force and workers who remain unemployed for long periods of time. In both cases, we simply observe zero earnings in our administrative data base. For the unemployed, those earnings are their actual earnings. But, attributing zero earnings to workers who moved out of the state, became self-employed, or were working under a different Social Security number would clearly lead us to overstate the losses associated with displacement. Thus we have chosen to eliminate from our sample the approximately 25 percent of high-tenure separators who subsequently appear to never have positive earnings in Pennsylvania. Because some of those workers probably were actually unemployed, we believe that this decision biases downward our displacement cost estimates. Thus it is worth noting that without this sample restriction our estimates of the losses would be approximately 15 percentage points larger. Alternatively, it might be argued that workers willing to move out of the state might be more resilient than most and that consequently, even with our sample restriction, we might overstate losses. However, before their separations, the workers we excluded had similar characteristics to the rest of the sample. Moreover, in results not reported in this paper, we find that displaced workers who move within Pennsylvania actually experience somewhat larger than average losses.[16]

We constructed the sample of workers analyzed in this paper by identifying those workers who were employed at the same firm since at least 1974 until at least the end of 1979. Thus even those who separated from their firms between 1980 and 1986 had 6 or more years of tenure. We also restricted our sample to workers for whom we had information on age and sex and, in order to avoid complications associated with early retirement, we eliminated workers born before 1930 as well as a very few workers who were born after 1959. In addition, in order to deal with the possibility of workers disappearing from our sample, we

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16. Tannery (1991) studied the rates that workers left the Pennsylvania wage and salary workforce between 1979 and 1987. Although his sample is not restricted to high-tenure workers, he found that among those who left for reasons other than retirement 60 percent had left the state. Among those who left the state by 1987, over one-half had 1979 earnings of less than $3,000 and less than 8 percent had earnings greater than $20,000.

required that every worker have received 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. Its potential drawback, as we have noted, is that separators who we eliminate may systematically suffer larger or smaller losses.[17]

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 about $2,000 or 9 percent less than the median worker in our sample of nondisplaced workers. Accordingly, we conclude that the nondisplaced workers were more 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 separated 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. Moreover, four years after separation their earnings still were nearly $2,000 per quarter less than their nondisplaced counterparts.

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

There are at least two ways to interpret the differences in stayers' and separators' earnings patterns. One interpretation is that because the earnings of displaced and nondisplaced workers were nearly the same during the mid-1970s,[18] their earnings-related characteristics also must have been similar and, absent some event, their earnings would have remained similar for the rest of the sample period. Accordingly, the earnings differences between the two groups that emerge in the late 1970s 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 1970s 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 post-displacement 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.

3. Statistical Models of Earnings Losses

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

3.A. Definition of Earnings Losses

Many displaced worker studies measure workers' earnings losses as the difference between their earnings in some post-displacement period and their earnings in a period shortly before separation. There are three reasons why this measure may not capture the full effects of struc-

18. The near coincidence of stayers' and separators' earnings levels is, as Table 1 shows, atypical; usually a "difference-in-difference" type estimator that estimates the effect of displacement as the increase in the gap between the two lines would be appropriate.

tural or public policy changes on workers' earnings. First, this measure does not control for macroeconomic factors that may have caused changes in workers' earnings regardless of whether they were displaced. Second, this measure does not take into account that, in the absence of job loss, workers' relative earnings are likely to rise with age and years on the job. Therefore, in the long-term, workers' earnings may return to their pre-displacement levels, but not to the levels they could have expected prior to their job losses. Finally, a firm's declining fortunes may begin to adversely affect its 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 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 earnings and their expected earnings had the events that lead 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 $D_{is} = 1$ if worker i was displaced at date s (and $D_{is} = 0$ otherwise). Our definition of earnings loss is the change in expected earnings if, p 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

$$(1) \qquad E(y_{it} \mid D_{is} = 1, I_{is\text{-}p}) - E(y_{it} \mid D_{i\nu} = 0 \text{ for all } \nu, I_{is\text{-}p}),$$

where $I_{is\text{-}p}$ is the information available at date s-p, and p is sufficiently large that the events that eventually lead to displacement have not begun. 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.[19] This choice ensures that we compare job losers' earnings at different dates to a common standard and simplifies the interpretation of several of our empirical results.[20]

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19. 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.

The magnitude and interpretation of workers' earnings losses also depend on the variables in the information set $I_{is\text{-}p}$. 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 very specific factors such as a worker's industry or firm is that even the workers who are fortunate enough to retain their jobs in industries or firms that permanently lay off other workers may themselves experience some earnings losses. For example, suppose the apparel industry was adversely affected by reduced import barriers and the effects of this reduction were also felt by workers who kept their jobs. If we conditioned on industry, we would obtain relatively small estimates of the displacement effects because workers who lost their jobs were hurt only a little more than those who kept their jobs. More generally, if nondisplaced workers' earnings also decline in response to policy changes, an earnings loss measure that controls for workers' industries or firms would not capture the full impact of the events that led to workers' displacements. Instead it would capture 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 variation is impossible to observe if we assume workers are similar only when they work for the same firm. Therefore, we prefer to 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.

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

3.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 a worker's separation. 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).[21] By restricting attention to these dummy variables, we formalize the idea that a worker displaced in 1982 was in much the same position in 1985 as a worker displaced in 1981 was in 1984.

Our first statistical specification assumes that a worker's 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:

$$(2) \qquad y_{it} = \alpha_i + \gamma_t + x_{it}\beta + \sum_{k \geq -m} D_{it}^{k}\delta_k + \varepsilon_{it}$$

In (2), the dummy variables, D_{it}^{k}, $k = -m, -(m-1), \ldots, 0, 1, 2, \ldots$ together represent the event of displacement. In particular, δ_k is the effect of displacement on a worker's earnings k quarters following its occurrence.[22] Thus, displacement is allowed to affect earnings up to m quarters before separation actually occurs where in what follows m is equal to 20 quarters or five

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

22. Our statistical model is similar to those used to evaluate the earnings impact of public sector training programs. See Ashenfelter (1978), Heckman and Robb (1985), and LaLonde (1986).

years.[23] The vector x_{it} consists of the observed, time varying characteristics of the worker, which in this paper are limited to the interactions among sex, age, and age squared. The γ_t's are the coefficients on a set of dummy variables for each quarter in the sample period that capture the general time pattern of earnings in the economy. The impact of permanent differences across workers in observed and unobserved characteristics is summarized by the "fixed effect" α_i. 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 α_i's, by least squares. Thus our estimates of the displacement effects are unbiased no matter how workers' permanent characteristics are related to their displacement status. Estimation of (2) generalizes the "difference-in-differences" technique of using 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 post-displacement 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, β is zero, and the base period is sufficiently far before separation. Ruhm's (1991) estimates are not constrained in these ways,[24] but differ from ours in the treatment of unobserved heterogeneity. Specifically, he estimates cross-sectional regression models for post-displacement earnings in which workers' pre-displacement earnings are used as a control variable.[25] Our technique also can be viewed as a method of using pre-displacement earnings to control for unobserved

23. 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, for even our first cohort of displaced workers who separated from their firms in the first quarter of 1980 have 6 years of pre-displacement data.

24. 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.

25. In one specification, it is the pre-displacement earnings of workers who have not yet been displaced that provide the control for unobserved heterogeneity.

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 layoff workers with more slowly growing earnings. This practice would cause our estimates of the δ_k's in (2) to overstate the effects of displacement. Accordingly, our second specification takes this possibility into account by adding to (2) a set of worker-specific time trends, $\omega_i t$:[26]

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

Again, we estimate (3), including the α_i's and the ω_i's, by least squares[27] thereby allowing for arbitrary permanent heterogeneity between displaced and non-displaced workers in both levels and trends of their unobserved characteristics.

Because, the influence of macroeconomic factors, γ_t, and of age and sex, $x_{it}\beta$ on earnings are effectively identified by data on all workers who did not separate from their firms between 1980 and 1986, our framework can be viewed as comparing 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 those of nondisplaced workers in their same firms. Of course, we only can make this comparison for workers who were displaced from firms that continued in existence throughout the sample period. We computed this alternative estimator by first subtracting for each quarter the mean earnings of nondisplaced workers in the displaced workers' former firms from their own earnings and then estimating, by least squares, a model with individual specific fixed effects and the full set of displacement

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26. 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 Card (1985), and Heckman and Hotz (1990).

27. 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.

indicators, D_{it}^k, $k = -m, -(m-1),..., 0, 1, 2,...$. Such estimates follow from a specification in which the quarter dummies are interacted with the set of dummy variables denoting workers' 1979 firm. More formally, if y_{ijt} denotes the earnings of worker i in 1979 firm j in quarter t, then

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

If workers who retain their jobs in firms that layoff other workers also suffer earnings losses, then the γ_{jt}'s for those workers' firms will decline around the time that layoffs occur and the estimates of the δ_k's will be closer to zero. The differences between displacement estimates obtained from (4) and (2) thus 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 across different groups of workers. The least restrictive modification involves interacting 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 post-displacement time periods observed in the data, to characterize the earnings losses across 12 industries in the most flexible manner requires nearly 600 displacement parameters. Fortunately, in 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 and workers. Specifically we define

$F^1_{it} = t - (s - 13)$, if worker i is displaced at time s and $s\text{-}12 \leq t \leq s$ and $F^1_{it} = 0$ otherwise,

$F^2_{it} = 1$, if worker i is displaced at time s and $t \geq s+1$ and $F^2_{it} = 0$ otherwise,

$F^3_{it} = t - (s+6)$, if worker i is displaced at time s and $t \geq s+7$ and $F^3_{it} = 0$ otherwise.

Then, if w_i is a vector of characteristics of individual i, our parsimonious model takes the form,

$$(5) \qquad y_{it} = \alpha_i + \gamma_t + x_{it}\beta + \sum_{k \geq -m} D^k_{it}\delta_k + F^1_{it}w_i\varphi_1 + F^2_{it}w_i\varphi_2 + F^3_{it}w_i\varphi_3 + \varepsilon_{it},$$

where φ_1, φ_2, and φ_3, are parameter vectors giving, respectively, the effect of workers' characteristics on the dip, the drop, and the recovery. Operationally, we achieve our parsimonious representation by including the full set of displacement dummies but only allowing for interactions between worker characteristics and the three variables F^1_{it}, F^2_{it}, and F^3_{it}. Specification (5) forces the gap between the estimated losses of two workers to (i) be zero in the period more than three years prior to separation, (ii) grow or decline linearly from zero to some amount in the period from three years before separation until the quarter of separation, (iii) be constant in the period from one to six quarters after displacement, and (iv) 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 w_i, take the form

δ_k, if $k \leq -13$
$\delta_k + w_i\varphi_1(k+13)$, if $-12 \leq k \leq 0$
$\delta_k + w_i\varphi_2$, if $1 \leq k \leq 6$
$\delta_k + w_i\varphi_2 + w_i\,\varphi_3(k-6)$, if $k \geq 7$.

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

$$(5') \qquad y_{it} = \alpha_i + \gamma_t + x_{it}\beta + \sum_{k \geq -m} D^k_{it}\delta_k + \sum_j E^j_{it}\,(F^1_{it}\varphi_{1j} + F^2_{it}\varphi_{2j} + F^3_{it}\varphi_{3j}) + \varepsilon_{it},$$

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 workers in group j. If the second sum above extends over all possible levels of a categorical variable, then (5′) will not be of full rank. However, instead of dropping the first dummy variable, which would be equivalent to setting $\varphi_{l1} = 0$ for $l = 1,2,3$, we impose the restrictions that $\sum \varphi_{lj} f_j = 0$ for $l = 1,2,3$, where f_j is the fraction of displaced workers in category j. Alternatively, when worker characteristics are continuous variables, we subtract the variables' means over all displaced workers from their levels before forming the interaction variables. The advantage of these parameterizations is that the average loss for all displaced workers for the k'th quarter after separation simply equals δ_k. Moreover, φ_{1j}, φ_{2j}, and φ_{3j} express the difference between the j'th groups "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.

3.C. Potential Biases

As we have noted, 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 a worker's fixed effect, α_i. Similarly, least squares estimation of (3) is unbiased even if firms tend to layoff workers partially on the basis of the values of their fixed effects or permanent trend rate of earnings growth, ω_i. However, our estimates may be subject to bias if firms selectively layoff 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 layoff 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 – as we assumed above – those errors are independent across time, such behavior biases only δ_0, the displacement effect associated with workers' date of separation. Unfortunately, when the errors are correlated over time, estimates of other δ_k coefficients are likely to be biased. In the program evaluation literature, this source of bias sometimes has been accounted for by explicitly modeling the selection process and simultaneously estimating its parameters along with those of the earnings equation.[28]

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. First, if we assume that the error process for each individual is stationary, the spurious effects of displacement are symmetric about the date of separation.[29] Second, 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 both are close to zero during this period and thus that the spurious effects of displacement are zero during periods more than three years after separation. Alternatively, given the assumption 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 long-term displacement effect estimates.

The above argument fails when the error process is nonstationary. In this case, when firms discharge recent poor performers there is no reason to expect the mean of ε_{it} conditional on displacement to regress back to zero. Consequently, even our long-term earnings loss estimates may be subject to bias. However, we can substantially diminish the importance of this source of 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 be leaving their jobs as a result of their own poor performance. Therefore, in the empirical work that follows we

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28. See, for example, Ashenfelter and Card (1985) and Card and Sullivan (1988).

29. See Heckman and Robb (1985) for a similar argument advocating the use of a symmetric difference in differences estimator in the estimation of earnings impacts of training programs.

give greater weight to the estimated earnings losses of workers in our mass-layoff sample.

4. 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 and ending with the 27th quarter after their separations. To facilitate the exposition, we plot these estimated differences against the number of quarters before or after workers' separations.

4.A. Earnings Losses and Mass Layoffs

As shown in Figure 2, we find that high-tenure, prime-age workers endure substantial and persistent earnings declines when they are displaced during or following mass layoffs. Even six years after their separations, their quarterly earnings remain $1,600 below their expected levels.[30] This loss represents 25 percent of workers' pre-displacement earnings. Moreover, because the estimated loss is even larger when we control for individual-specific rates of earnings growth, this loss does 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.[31]

We also find evidence that the events that lead to workers' separations cause their earnings to

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30. 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.

31. 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 their separations, the standard errors are $60.

depart from their expected levels even before they leave their firms.[32] As shown in Figure 2, these workers' quarterly earnings begin to diverge meaningfully from their expected levels approximately three years prior to separation. That divergence accelerated 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 pre-separation declines result from cuts in real wages or weekly hours, in other work we find that the incidence of temporary layoffs increased for these workers before their final separations.[33]

Our confidence in these results – that earnings losses are large, long-term, and appear even before workers permanently lose their jobs – is enhanced because we find no substantial estimated losses during the period four to five 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* 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 pattern of earnings losses emerges 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 recover three to five years following separation. Second, prior to separation, 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, considering that this sample probably includes larger fractions of workers who quit their jobs or who had fewer firm-specific

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

33. See Jacobson, LaLonde, and Sullivan (1993).

skills. In addition, this pattern of losses for workers expected 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 pre-separation 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 may 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, we would understate the long-term losses associated with displacement.

4.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, $x_{it}\beta$, on earnings. As observed in the previous section, 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 non-displaced 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 non-displaced workers to identify the influence of macroeconomic factors.[34] 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, non-displaced workers in distressed firms fall behind other non-displaced 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 dramatically reduce their workforces, it is probably apparent which employees, namely those who have experienced temporary layoffs in the past, are most likely to be permanently laid off. This result suggests that stayers in distressed firms do not accept significant cuts in their own earnings because they don't 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.

4.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

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34. The two sets of estimates in Figure 4 are based on the fixed effects estimator described in (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 where the firm continued its operations.

these losses vary by worker characteristics, we use our mass-layoff sample to estimate several versions of model (5). In that model workers' earnings loss patterns are allowed to differ from the average pattern shown in Figure 2 [35] in (i) their rates of earnings decline during the 12 quarters prior to their job losses (their "dip"), (ii) their average quarterly earnings loss during the first six post-separation quarters (their "drop"), and (iii) their rate of earnings recovery after the 6th quarter following their separations (their "recovery"). 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 see how 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 pre-displacement earnings declined by 10.8 dollars per quarter more and women's earnings declined by \$36.7 per quarter less than the average rate of decline depicted in Figure 2 (approximately \$83.3 per quarter). Accordingly, in the period prior to separation men's earnings declined by \$47.5 (-\$10.8 - \$36.7) per quarter more than their female counterparts. Thus, by the quarter of separation, the gap between men's and women's earnings losses is estimated to be \$618 (47.5 multiplied by 13, the value of the dip time trend on the date of separation). During the six quarters immediately after separation, the "drop" coefficients indicate that men's and women's quarterly earnings losses are, respectively, \$217 more than and \$738 less than the average loss depicted in Figure 2 (approximately \$2,219). Thus, the short-term earnings losses for men are estimated to be \$955 per quarter more than those of women. After this initial post-displacement period, how-

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35. 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.

ever, men's earnings rebound somewhat as the "recovery" coefficients indicate that their earnings rise by \$28.5 (6.5 - -22.0) per quarter more than women's earnings. During the fifth year after displacement, their earnings losses exceed women's by \$2398 (-545 - 1853). Given the average level of losses, this implies an estimate for losses in the fifth year after displacement of \$7,143 for men and \$4,744 for women[36].

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 pre-separation declines falls to only \$15 per quarter and the difference between their short-term losses falls to \$453 per quarter. The latter figure suggests that the women in our sample had 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 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 that the differences between these groups' pattern 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

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[36]. 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, 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 $\delta_{17}+\delta_{18}+\delta_{19}+\delta_{20}$.

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 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 no matter 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 financial, and business and professional service sectors.

To assess the importance of labor market conditions on workers' losses, we included in w_i in (5) variables that summarize both their locales' long-term economic conditions and business cycle conditions at the time of their job losses.[37] To summarize the effect of the long-term

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[37] For details on the local labor market variables see the appendix.

conditions, we used the locales' trend in nonagricultural employment. To summarize the effect of the cyclical conditions on the date of workers' separations, we used: (i) the locale's unemployment rate and (ii) the deviation of employment from its trend.[38] To facilitate the interpretation of these estimates, the figures in Table 2 present the differences in earnings losses corresponding to approximately the range of conditions observed in the data. In Pennsylvania, the range in quarterly employment growth rates across locales is 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 from their trends 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 they are displaced in depressed regions as measured by the trend rate 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 multiplied by 38.8) larger than those experienced by workers displaced in the strongest markets. The gap between workers' losses widened to approximately $750 per quarter during the first year following their separations and was still $500 per quarter during the fifth year following their separations. This long-term differential between the strongest and weakest labor markets corresponds to about 1/3 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 can have substantial and long-lasting effects on their earnings. By themselves, neither local unemployment rates nor deviations of employment from trend 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 post-separation earnings declines, but are not correlated with the rate of pre-displacement earnings decline or the

38. As we noted in Section 3.B, to construct the variables entering w_i, we subtracted these three continuous-variables' means, taken over all displaced workers, from their levels for each worker.

rate of post-displacement earnings recovery. The impact of cyclical conditions on these terms is better captured by movements in locales' employment levels. This result suggests that this measure is a better indicator of shifts in firms' demand for labor. Together, the two measures indicate that locales' cyclical conditions affect the magnitude of both workers' pre- and post-displacement 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 5 years that are nearly $1,500 larger than those experienced by workers displaced during the best conditions.

Like industry and firm size, local labor market conditions are important determinants of earnings losses. But, it is important to recognize that even workers displaced in strong labor markets experience large losses. Our figures indicate that even 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.[39] This result underscores the point that local labor market conditions are only of modest importance when accounting for the magnitude of displaced workers' earnings losses. We view this finding as evidence that some valuable attribute of the employment relationship is generally lost when high-tenure workers are displaced.

4.D. Losses and Sector of New Jobs

To further explore this possibility we examined the relationship between workers' losses and the industrial sector of their new jobs.[40] Specifically, we examined the earnings losses among workers whose new jobs were (i) in the same 4-digit SIC industry as their old job, (ii) in the same sector (manufacturing vs. nonmanufacturing) but in a different 4-digit industry,

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39. We have obtained similar estimates of the importance of local labor markets in models that include region dummies in the list of variables in w_i.

40. 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 post-separation 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.

or (iii) in a different sector. By characterizing displaced workers' new jobs in this fashion, we have implicitly assumed that the skills required on new jobs in the same 4-digit SIC industry are similar to those required on workers' old jobs. Therefore, if loss of specialized skills is a large 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.

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 displaced workers who leave the manufacturing sector are equal to 38 percent of their pre-displacement earnings.[41] 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 4-digit industry. As shown in Panel A, 24 quarters after their separations workers' losses were 20 percent of pre-displacement earnings if they found new jobs in the same 4-digit industry compared with 18 percent if they found new manufacturing jobs in different 4-digit industries.

The findings for displaced nonmanufacturing workers, although less conclusive, are similar to those for their manufacturing counterparts. When displaced nonmanufacturing workers find new jobs in the same 4-digit industry their long-term earnings losses amount to 18 percent. That percentage rises to 22 percent when their new jobs are in a different 4-digit industry, but still in the same sector. Finally, those losses are larger for those who find 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 firm-specific component of earnings. Even those who found re-employment in the same industry, and thus presumably in jobs requiring similar skills, experienced large and persistent losses.

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41. 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.

5. Conclusion

As we have shown in this paper, high-tenure workers experience substantial earnings losses when they leave their jobs. Of course, several other studies have found short-term losses of similar magnitude using other data sets. But we also find that for workers displaced from distressed firms that these losses are (i) long-term, with little evidence of substantial recovery after the third year; (ii) arise even prior to workers' separations; (iii) vary modestly by 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 1980s. Because we employed a five percent sample of Pennsylvania's 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 seems reasonable as the DWS reports the same number of high-tenure prime-age workers were displaced during approximately the same period.[42]

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.[43] Third, the relatively slow rates of earnings recovery after workers secure new jobs suggests that wage gains generated from idiosyncratic job matching must

42. We calculated this figure by applying the same tenure and age restrictions used in our study to the 1984 and 1986 Displaced Workers Surveys (DWS). 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, except it began in 1979 instead of 1980.

43. See Beaudry and DiNardo (1991) for a similar result.

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- as opposed to industry-specific. Alternatively, such earnings losses may result from the workings of internal labor markets. In either case, because losses are almost always large, wage premiums due to firm specific skills or to internal labor markets must be a commonly occurring component of workers' earnings. These conclusions are, of course, only tentative, and reflect considerations beyond the scope of this paper. Their proper consideration is left for future research.

Appendix

A. Constructing the Data

We constructed our longitudinal data base from Pennsylvania Unemployment Insurance (UI) tax reports and ES202 data. The firms' UI tax records report the quarterly wage and salary earnings for each employee.[44] 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 topcoded. For a subset 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, these data are sometimes missing.[45]

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 using the information on the sources of workers' earnings to accurately track their separations from individual firms. Thus, it is important to account for cases where

44. We obtained this data for a 5 percent sample of workers based on the last two digits of their Social Security numbers.

45. The Social Security Administration was apparently unable to provide this information for all workers.

firms' EINs change 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 that we were able to use to construct a consistent set of corrected EINs. In several years, well over 5% 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 4-digit SIC but different EINs. In cases of mergers and divestitures that occurred during the sample period, we treated the separate parts as a single firm even in years in which they were legally distinct.

Finally, we created our longitudinal file by merging UI tax reports and ES202 records with the same corrected EIN[46]. This allowed us to construct a file that contains for each worker information on their quarterly earnings from 1974 to 1987 and for each calendar year their principal[47] employers' EIN, 4 digit SIC industry, location, and average employment during the last, current, and following years.

B. Dating Workers' Separations

Our Pennsylvania data contain two pieces of information that we used 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 workers' principle 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 was 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 in the last year in which the old employer was still the principal employer, the quarter of separation was the last quarter of positive earnings from that employer.[48] However, when the worker derived 100 percent of

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46. Both sets of data also include a plant identification number. Unfortunately, the coding schemes differ between the two files and we were unable to obtain a figure for employment at a worker's establishment.

47. 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.

his fourth quarter earnings from the old principal employer, the separation date was taken to be the last quarter of the following year in which the employee received earnings from sources other than the new principal employer.[49]

In most instances the foregoing procedure precisely dates the separation. But there appear to be two exceptions: First, when the employee has another wage or salary job besides the job with the incumbent; second, when the incumbent grants the employee severance pay after displacement. Both of those exceptions may cause us to date the separation after it actually occurred. As a result of our dating procedure, displaced workers' earnings may falsely appear to decline slightly during the quarters prior to displacement. However our finding reported in the text, showing that pre-displacement 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 workforce. The group of high tenure workers who we examine are those that were hired by their firms prior to 1974 and who remained with those firms (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.

To reduce the problems associated with early retirement, our sample includes only workers

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48. For example, if the worker has earnings from the old employer in the third but not the fourth quarter of the last year the old employer was the principal employer, we declare the separation to have occurred in the third quarter of that year.

49. 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.

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 appear 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. One concern about these persons was that their characteristics might differ substantially from workers who remained in Pennsylvania's wage and salary work force. 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 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.

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Table 1: Sample Characteristics

Workers	Observations	Mean	Standard Deviation	Median	10th Percentile	90th Percentile
Panel A — 1979 Age						
Separators						
All	9507	37.0	7.4	37	27	47
Males	7092	36.9	7.2	37	27	47
Females	2415	37.3	7.8	38	27	48
Nonmanufacturing	2870	36.9	7.3	37	27	47
Manufacturing	6637	37.1	7.4	37	27	47
Western Pennsylvania	3804	36.8	7.4	37	27	47
Eastern Pennsylavnia	5703	37.1	7.3	37	27	47
Nonmass Layoffs	3072	36.9	7.3	37	27	47
Mass Layoffs	6435	37.1	7.4	37	27	47
Stayers	13704	37.7	7.0	38	28	47
Panel 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 Pennsylavnia	5,703	23,561	12,138	22,176	11,005	36,140
Nonmass 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

Table 2: Losses by Worker Characteristics[a]

Group	Number	Without Other Controls[b] dip[d]	drop[e]	recovery[f]	fifth year loss dif	fifth year loss	With Other Controls[c] dip	drop	recovery	fifth year loss dif	fifth year loss
Overall	6,435						-83.3 (2.2)	-2179 (16)	15.4 (4.4)	—	-6,611 (150)
Sex											
Male	4972	-10.8 (0.7)	-217 (7)	6.5 (0.9)	-545 (40)	-7,143 (132)	-3.4 (0.7)	-103 (7)	4.7 (0.9)	-177 (43)	-6,788 (157)
Female	1463	36.7 (2.2)	738 (24)	-22.0 (3.0)	1,853 (136)	-4,744 (184)	11.6 (2.3)	350 (25)	-16.0 (3.2)	602 (145)	-6,009 (207)
Decade of Birth											
1930's	2599	-0.0 (1.4)	116 (16)	-10.9 (2.0)	-79 (92)	-6,677 (159)	-0.3 (1.4)	55 (16)	-10.1 (2.1)	-284 (94)	-6,896 (182)
1940's	2584	7.2 (1.4)	3 (15)	4.6 (2.0)	241 (87)	-6,356 (151)	3.6 (1.4)	-28 (15)	5.6 (2.0)	171 (88)	-6,440 (172)
1950's	1252	-14.9 (2.4)	-247 (25)	13.1 (3.2)	-333 (144)	-6,932 (188)	-6.9 (2.4)	-58 (25)	9.4 (3.2)	238 (145)	-6,374 (203)
Industry											
Mining & Construction	247	1.3 (5.64)	-497 (58)	7.5 (7.6)	-1,616 (332)	-8,435 (352)	9.5 (5.8)	-387 (59)	-0.1 (7.8)	-1,549 (339)	-8,160 (369)
Nondurable Manufacturing	1,206	26.5 (2.3)	624 (25)	-14.6 (3.3)	1,766 (144)	-5,052 (188)	18.3 (2.6)	338 (28)	-7.7 (3.7)	967 (160)	-5,644 (224)
Primary Metals	1,354	-121.2 (2.2)	-1,991 (24)	54.1 (3.6)	-5,256 (157)	-12,074 (210)	-104.5 (2.7)	-1,476 (30)	40.5 (4.4)	-3,878 (191)	-10,489 (241)
Fabricated Metals	436	21.0 (4.2)	611 (44)	-11.2 (6.4)	1,882 (274)	-4,936 (301)	15.9 (4.2)	488 (45)	-9.8 (6.5)	1,465 (279)	-5,146 (312)
Non-electrical Machinery	632	47.9 (3.4)	1,005 (38)	-36.9 (5.8)	2,174 (249)	-4,644 (284)	35 (3.5)	797 (39)	-27.4 (5.9)	1,817 (257)	-4,794 (306)
Electrical Machinery	421	43.2 (4.2)	288 (46)	7.0 (6.1)	1,500 (270)	-5,318 (300)	49.5 (4.3)	494 (47)	-2.7 (6.4)	1,842 (282)	-4,769 (322)
Transportation Equipment	419	25.0 (4.3)	422 (46)	-27.5 (6.2)	310 (264)	-6,508 (291)	14.1 (4.4)	215 (48)	-15.5 (6.6)	85 (282)	-6,526 (324)
Other Durable Manufacturing	441	25.6 (4.2)	525 (43)	3.0 (5.5)	2,248 (237)	-4,570 (262)	18.9 (4.2)	338 (43)	9.1 (5.7)	1,807 (242)	-4,804 (282)

Table 2: Losses by Worker Characteristics[a]

Group	Number	Without Other Controls[b]					With Other Controls[c]				
		dip[d]	drop[e]	recovery[f]	fifth year loss dif	fifth year loss	dip	drop	recovery	fifth year loss dif	fifth year loss
Transportation, Commumication, and Public Utilities	348	6.6 (4.7)	150 (49)	-63.5 (7.0)	-2,573 (295)	-9,392 (321)	5.5 (4.8)	66 (50)	-63.6 (7.1)	-2,916 (301)	-9,527 (333)
Wholesale and Retail Trade	545	18.7 (3.7)	198 (38)	2.0 (4.8)	891 (207)	-5,927 (235)	20.0 (3.8)	126 (38)	4.8 (4.9)	745 (211)	-5,866 (251)
Finance, Insurance, and Real Estate	183	127.7 (6.6)	1,312 (70)	14.3 (8.2)	5,963 (352)	-855 (369)	115.7 (6.7)	947 (72)	24.3 (8.3)	5,004 (358)	-1,608 (387)
Professional, Business and Entertainment Services	203	82.0 (6.3)	1,158 (63)	-18.2 (8.4)	3,725 (360)	-3,093 (378)	93.1 (6.4)	1,270 (64)	-26.2 (8.7)	3,769 (369)	-2,843 (394)
Firm Size											
50-500	1704	7.9 (1.9)	351 (20)	0.6 (2.6)	1,434 (113)	-5,403 (163)	-16.1 (2.1)	-37 (22)	13.0 (2.9)	501 (124)	-6,110 (193)
501-2,000	1497	33.5 (2.0)	501 (22)	-14.1 (2.9)	1,298 (127)	-5,540 (176)	13.9 (2.2)	214 (23)	-4.7 (3.1)	625 (135)	-5,986 (246)
2,001 - 5,000	1381	40.9 (2.2)	720 (23)	-32.3 (3.1)	1,267 (134)	-5,570 (179)	27.2 (2.3)	480 (24)	-23.8 (3.5)	730 (149)	-5,881 (203)
Greater than 5,000	1853	-64.8 (1.8)	-1,265 (19)	34.9 (2.9)	-3,312 (125)	-10,150 (190)	-16.7 (2.3)	-497 (25)	9.6 (3.6)	-1,510 (154)	-8,121 (224)
Local Labor Market											
Employment Trend		12.9 (7.5)	2,391 (84)	-73.2 (10.9)	5903 (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)	1082 (401)		517.7 (64.4)	3762 (645)	-40.9 (94.1)	-540 (408)	
Unemployment Rate		41.5 (8.9)	213 (97)	-29.7 (14.3)	-635 (631)		11.9 (90.0)	-5,545 (976)	13.1 (145.9)	-2,153 (643)	

a. "dip", "drop", "recovery", and "fifth year loss dif" 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 explanation of entries. Numbers in parentheses are standard errors.

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

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

d. Coefficient on pre-displacemnt time trend.

e. Coefficient on dummy for first six quarters after displacement.

f. Coefficient on post separation time trend.

Table 3: Earnings Losses by Sector of New Job

Deviation between actual and expected quarterly earnings[a]

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

a. Number in parenthese are standard errors. Numbers in square brackets express the estimated losses as a percentage of pre-displacement earnings.

Figure 1: Quarterly Earnings (1987$) of High-Attachment Workers Separating in Quarter 82.I and Workers Staying Through Quarter 86.IV

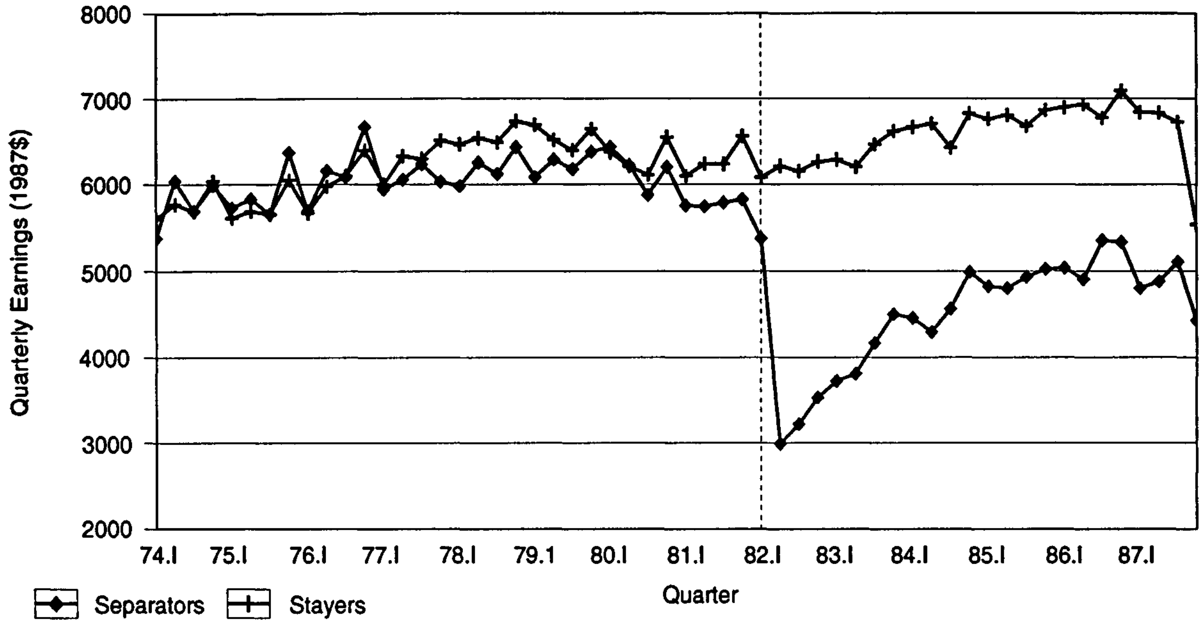

Figure 2: Earnings Losses for Separators in Mass Layoff Sample

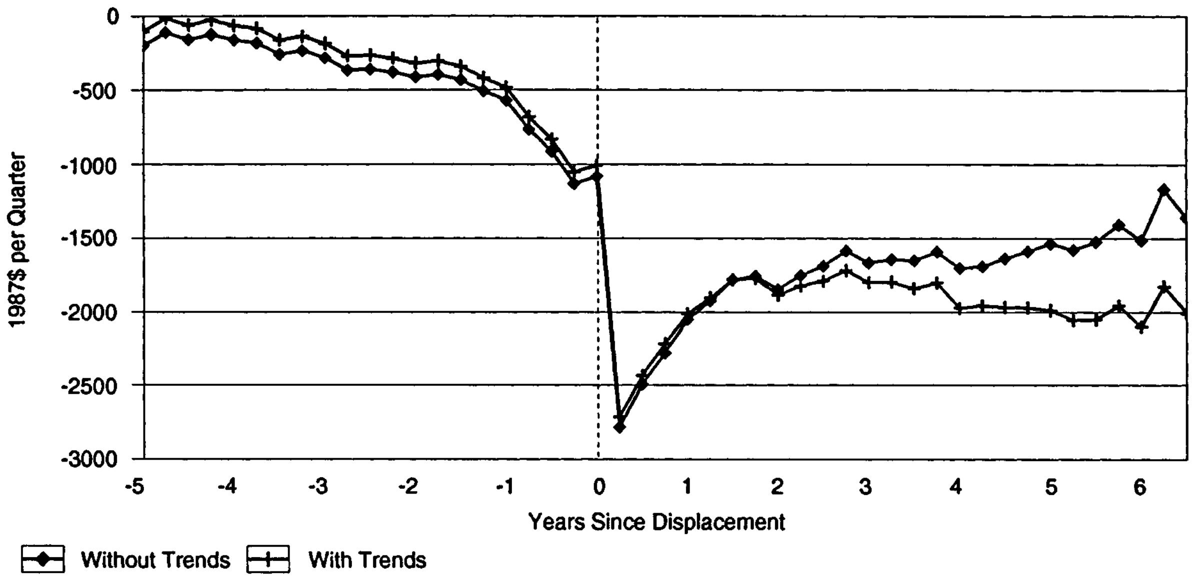

Figure 3: Earnings Losses for Separators in Non-Mass Layoff Sample

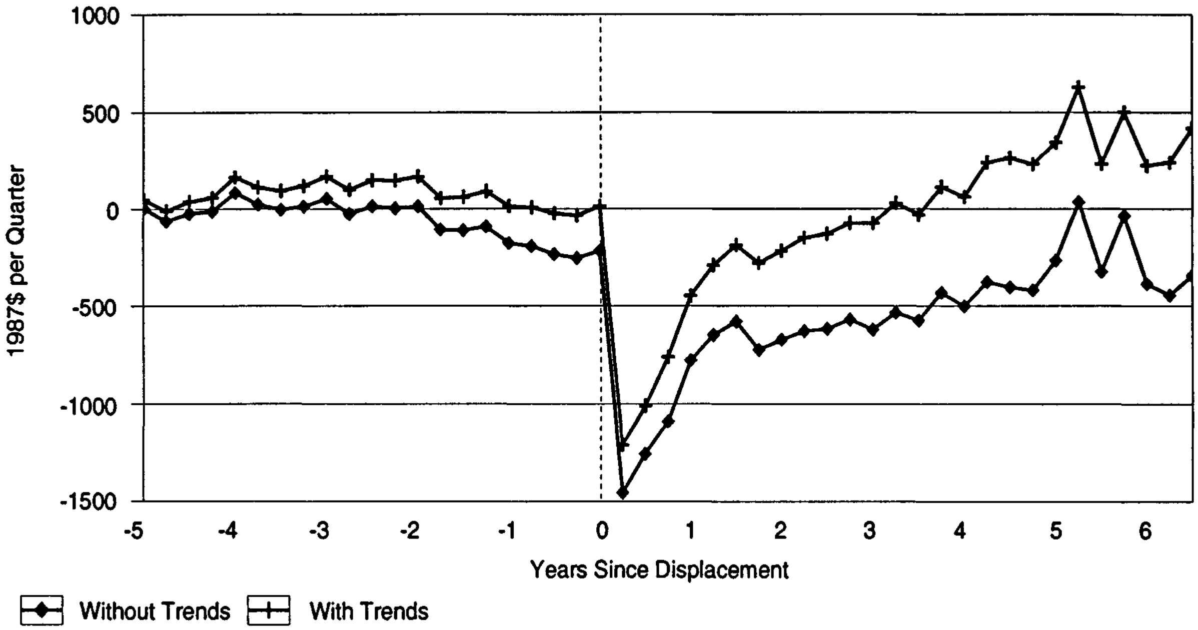

Figure 4: Sensitivity of Earnings Loss Estimates for Mass Layoff Sample to Different Comparison Groups

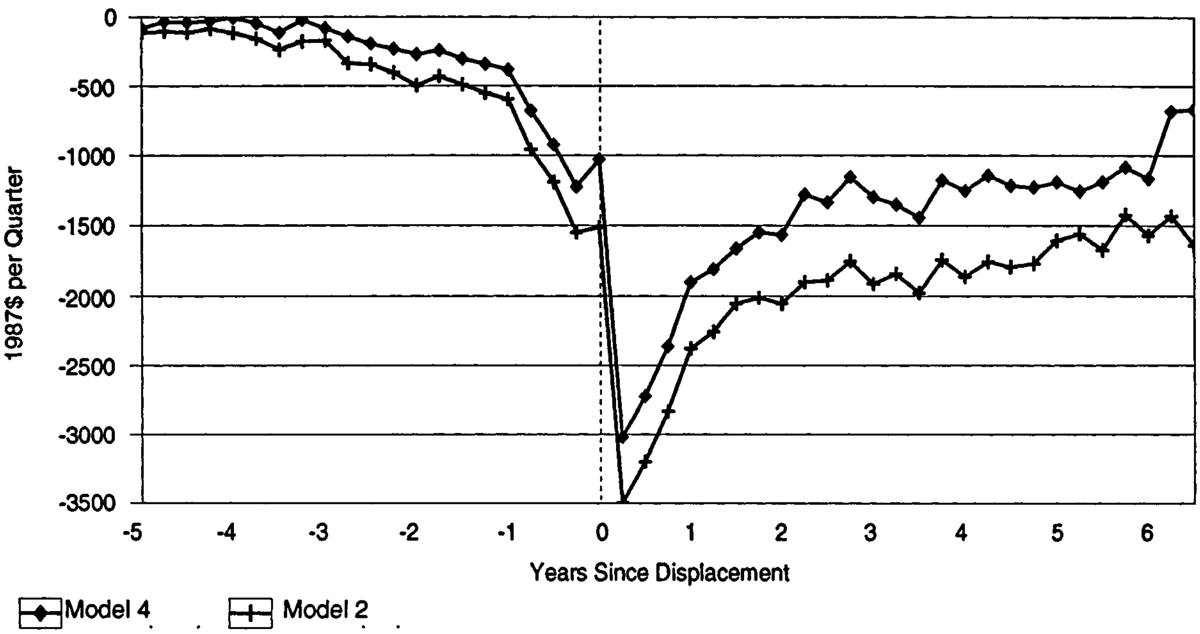

Figure 5: Sensitivity of Earnings Loss Estimates for Mass Layoff Sample to Different Comparison Groups