Job Displacement and Mortality: An Analysis Using Administrative Data

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Job Displacement and Mortality:

An Analysis using Administrative Data[1]

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Abstract

We use administrative data on the quarterly employment and earnings of Pennsylvanian workers in the 1970s and 1980s matched to Social Security Administration death records covering 1980-2006 to estimate the effects of job displacement on mortality. We find that for high-seniority male workers, mortality rates in the year after displacement are 50-100% higher than would otherwise have been expected. The effect on mortality hazards declines sharply over time, but even 20 years after displacement, we estimate a 10-15% increase in annual death hazards. If such increases were sustained indefinitely, they would imply a loss in life expectancy of 1.0-1.5 years for a worker displaced at age 40. We show these results are not due to selective displacement of less healthy workers or to unstable industries or firms offering less healthy work environments. We also show that workers with larger losses in earnings tend to suffer greater increases in mortality, and that this correlation remains when we examine predicted earnings declines based on losses in industry, firm, or firm-size wage premiums. While these results do not necessarily imply a causal effect of earnings on health, they do suggest that factors associated with displacement's impact on long-run earnings play a role in explaining the effects of displacement on mortality that we find.

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1. Introduction

A growing literature shows that displaced workers – individuals who lose their job as part of plant closings, mass layoffs, and other firm-level employment reductions – tend to experience significant long-term earnings losses as well as decreased job stability, lower employment rates, earlier retirement, lower consumption, and decreased health insurance coverage.[2] In this paper, we provide evidence that displaced workers can also experience higher rates of mortality.

To study the link between displacement and mortality, we use administrative data on earnings and employment histories for male workers from Pennsylvania in the 1970s and 1980s matched to Social Security Administration (SSA) death records covering the entire United States from 1980 to 2006. Following Jacobson, LaLonde, and Sullivan (1993) (hereafter JLS), we identify instances of displacement as those in which high-tenure workers leave firms experiencing large employment declines.[3] We then compare these displaced workers' subsequent mortality rates to those of similar workers who did not suffer job loss.

We find that high-tenure male workers displaced during the early- and mid-1980s experienced a significant increase in mortality. Indeed, our estimates suggest a 50-100% increase of the mortality hazard during the years immediately following job loss. The estimated impact of displacement on annual mortality rates declines substantially over time, but appears to converge to a 10-15% increase in the hazard rate. If these increases last beyond the 25-year window we follow, they would imply a loss in life expectancy of 1.0-1.5 years for workers displaced in middle age. In contrast, we find little effect of job loss on mortality for workers displaced near retirement age.

Firm-level employment declines should be exogenous to individual workers' health developments. Moreover, our results control for the mean and standard deviation of workers' earnings over a period of several years prior to job loss and are robust to the inclusion of industry or firm effects. They should thus be little affected if, for example, firms selectively lay off less-

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[2] See for example Ruhm (1991), Jacobson, LaLonde, and Sullivan (1993), Farber (2003), Chan and Stevens (2001), Stevens (1997), Gruber (1997), and Olson (1992). The Bureau of Labor Statistics defines displaced workers to be individuals who lose their main job because of the operating decisions of their employers, where in the case of multiple jobs 'main job' refers to the job held the longest (see, e.g., Hildreth, von Wachter, and Handwerker, 2008).

[3] We analyze workers with at least six or at least three years of tenure at the time of job displacement.

productive workers and less-productive workers tend to be less healthy, or if unstable industries or firms provide less healthy work environments. In addition, we show that these worker-level results are consistent with a firm-level analysis of the impact of employment declines on mortality that pools displaced workers with those remaining with affected firms. By construction, these "intent-to-treat" estimates are unaffected by the possibility of firms selecting the least healthy workers for layoffs or by misclassification of dying workers as job losers. Thus, our estimates likely identify the causal effect of job loss on mortality.

Our estimates of the short- and long-run effects of displacement on the mortality hazard roughly parallel the short- and long-run effects of displacement on earnings and employment reported in JLS and elsewhere. In the short run, displacement is associated with a sharp drop in mean earnings, increased unemployment, and high earnings instability. Our results are consistent with these effects causing acute stress that substantially raises the mortality hazard in the short term. In the long run, displacement is associated with a substantial drop in mean earnings and modestly higher employment instability and earnings variability. Several economic models of health determination predict that a decline in lifetime resources should raise mortality.[4] Our empirical findings are consistent with a reduction in such resources leading to reduced investments in health or chronic stress that, in turn, lead to a smaller, but longer-term increase in the mortality hazard. Increased earnings instability may also contribute to chronic stress and a long-run increase in mortality.[5]

To gain insight into the relative importance of some of the channels through which job loss could affect the long-run mortality hazard, we compare our estimates of the "reduced form" effect of displacement on mortality to what one would expect on the basis of displacement's long-run effect on the mean and variability of workers' earnings and the correlation of those factors with mortality. In our Pennsylvania data, displacement reduces the mean of long-run earnings by 15-20%.

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[4] For example, a shift in the life-time budget constraint would reduce health investments in a neo-classical model of health; alternatively, it could reduce social status and may raise mortality through social stress (see Deaton 2001 for further discussion of these and other approaches). While some of these factors are likely to operate in the short-run as well, too many factors vary simultaneously to separately assess the effect of any single channel.

[5] Another potential channel to which our data does not speak is the loss in health insurance. Losses in health insurance may be correlated with earnings reductions (Olson 1993), but may have independent effects as well.

Given the correlation of mean earnings with mortality, such an effect can explain an increase in the death rate hazard equal to 50-75% of our estimate of the reduced-form effect. Though displacement does not have a significant long-run effect on employment rates, it does raise the variability of earnings somewhat. Given the significant correlation of earnings variability with mortality in our data, this implies an additional effect of displacement on mortality on the order of 20-25% of what we estimate for the full reduced-form effect of job loss on mortality. Thus, the impact of displacement on the mean and variability of earnings may explain an important fraction of the increase in the long-run mortality hazard that we estimate. Our analysis of groups of workers who by their industry or their employer's characteristics have greater predicted earnings losses confirms that larger earnings reductions at job displacement are associated with greater increases in long-term mortality risk.

Our results are consistent with those of the large literature documenting a strong correlation of socioeconomic status with health.[6] However, our paper is one of the first studies to use United States data to estimate the long-term effect of a plausibly exogenous labor market event on an objective measure of health for a large group of workers. It thereby establishes a much clearer causal link between labor market and health outcomes than most of the previous literature.[7] Our study complements important recent studies based on detailed European administrative data, which find

[6] Typical estimates suggest a strong correlation between income and mortality (e.g., Deaton and Paxson, 1999). However, cross-sectional estimates may not represent causal effects of earnings on mortality due to reverse causality running from health to income, omitted worker characteristics, and measurement error (e.g., Smith, 1999; and Cutler, Deaton, and Lleras-Muney, 2006). An exception is a recent experimental study analyzing lottery winners in Sweden that suggests a significant causal relationship running from income to health (Lindahl, 2005). In addition, a large literature in economics and sociology has shown that unemployment correlates strongly with the incidence of depression, low self-esteem, unhappiness, and even suicide (see papers summarized in Darity and Goldsmith, 1996, and Burgard, Brand, and House, 2005, 2007). Similarly, several authors in sociology, social work, and epidemiology have analyzed the effect of job loss on mental and physical health using survey data such as the Health and Retirement Survey (e.g., Gallo et al., 2006).
[7] Although they provide significant insights, many existing United States studies suffer from mostly data-related shortcomings. Perhaps most importantly, most studies do not have access to exogenous sources of variation in individuals' labor market conditions that allow them to study the causal effect of income or labor market events on health. Typical data sources used also contain only self-reported measures of health, job loss, and income. This leads to measurement error in some of the key right-hand-side variables (e.g., Hildreth, von Wachter, and Handwerker, 2008). In most cases, sample sizes are too small and the follow-up period to short to study the long-run effects of negative labor market shocks on health. Similarly, typical studies can not control for detailed longitudinal career outcomes as we do.

mixed results on the effects of job loss on health.[8] Our paper is the only study to closely replicate and extend the approach used in JLS's well-known analysis of job displacements. In addition to methodological differences, the European studies differ from ours in that they analyze the effect of displacement over shorter horizons. In addition, United States health care and labor market institutions differ substantially from those in Europe, where workers often have access to universal health insurance and where the earnings consequences of job loss typically are less severe than in the United States.[9] Our results do not conflict with those of Ruhm (2000), who finds that aggregate mortality rates tend to fall during recessions. As we discuss more fully in the conclusion, the situation of an individual displaced worker differs qualitatively from that of the average worker during a recession. Briefly, for the average worker, short-term declines in economic activity may increase time available for healthy activities without significantly reducing lifetime resources, However, the high-tenure displaced workers we study suffer significant long-term earnings reductions without benefiting from an offsetting increase in leisure time.

A potential limitation of our data is that the experiences of workers displaced from jobs in Pennsylvania during the early- and mid-1980s may not be fully representative of those of the typical displaced worker. Indeed, given the severity of the early-1980s recession in Pennsylvania, it is quite possible that our results somewhat overstate the average impact of displacement on mortality. However, the qualitative effects of displacement on other aspects of workers' lives have been found to be reasonably robust across time and place,[10] so our results likely give a good indication of the

[8] Rege, Telle, and Votruba (forthcoming) find that workers (men and women) losing their job in a plant downsizing during 1993-98 are more likely to receive disability insurance in 1999 and have a somewhat higher probability of death during 1999-2002. Eliason and Storrie (2007) find male workers losing their jobs in establishment closures in Sweden during 1987-88 experience excess mortality for up to four years after job loss. Martikainen, Maki, and Jantti (2007) find no such effects in Finland. Results are similarly mixed for other measures of health. E.g., Kuhn, Lalive, and Zweimueller (2007) find that job loss reduces mental health of men in Austria, while Browning, Dano, and Heinesen (2006) find no such effects in Denmark.

[9] There is considerable heterogeneity in approaches and results among studies analyzing the effect of job loss on earnings in Europe. For example, the effects of job loss on earnings in Austria are small (Card, Chetty, and Weber, 2006). Earnings losses in Sweden have been found to be more persistent (Eliason and Storrie, 2006). Income losses in Norway fall somewhere in between (Telle, Rege, and Votruba, forthcoming).

[10] Earnings losses of similar duration and magnitude as found by JLS for Pennsylvania have been found in other states in the 1990s (such as California, Connecticut, or Massachusetts [in Schoeni and Dardia (2003), Couch (2006), and Kodrzycki (2007)], respectively) and for the entire United States during the early 1980s (von Wachter, Song, and Manchester, 2007).

direction and at least the rough magnitude of the effects that can be expected for the typical displaced high-tenure worker.

The next section discusses the properties of our data and introduces our econometric framework. Section 3 contains our main results; Section 3.1 presents the average effect of displacement on mortality; Section 3.2 distinguishes between the short- and long-run effects of displacement on mortality and breaks out the effects by current age, age at displacement, and job tenure; Section 3.3 discusses the implied reductions of life-expectancy; and Section 3.4 summarizes our sensitivity analysis. Section 4 discusses our assessment of potential mechanisms through which displacement raises mortality; and section 5 concludes.

2. Empirical Approach: Data and Econometric Framework

This section details the construction of our data set, which merges quarterly wage records derived from the state of Pennsylvania's unemployment insurance (UI) system with death records maintained by the Social Security Administration. It also explains how we identify displaced workers and contrasts their characteristics with workers not affected by displacement. It then describes our basic empirical strategy, which is to compare the mortality experience of workers identified as being displaced, with otherwise similar workers who are not displaced.

2.1 Data Construction and the Characteristics of Displaced Workers

Our data on workers' employment and earnings histories are derived from the unemployment insurance records of the state of Pennsylvania over the period from 1974 to 1991. For a 5% sample of workers who held jobs covered by unemployment insurance, we observe quarterly earnings from each Pennsylvania (PA) employer, as well as the employer's industry.[11] Our data on mortality are derived from a database compiled by the Social Security Administration and cover deaths occurring

[11] Jacobson, Lalonde, and Sullivan (1993) used the same data for the period 1974 to 1986. For a detailed description of the data and its advantages and shortcomings, please see their paper. An 'employer' in our data refers to a firm, which may operate multiple establishments as long as they are in Pennsylvania.

anywhere in the United States between 1974 and 2006. The accuracy of the death information has been found to be good for the sample of mature and older male workers we consider.[12]

We follow JLS in focusing on workers who had very stable employment relationships in the 1970s. Specifically, we analyze data on male workers who had the same principal employer from 1974 to 1979, where the principal employer for a year was the employer from which the worker received the most wage income. We also replicate our results for men with at least three years of job tenure in 1979. In both cases the restriction isolates stable workers separating from what they had reasons to expect to be long-term jobs in the absence of mass layoffs. For these workers, displacement was likely to be unexpected and costly.[13]

A limitation of administrative data is that we do not have a direct measure of whether a particular separation was voluntary or involuntary. As in JLS, we deal with this limitation by defining displaced workers to be those who leave their firms during the 1980-86 period and for whom their former firms' employment in the following year was 30% or more below its peak since 1974.[14] Other workers leaving their firms during this period are not considered displaced, and in most specifications are left in the comparison group. JLS found that such non-mass-layoff job separators did not, on average, experience long-term earnings losses.[15]

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[12] The Social Security Administration's Death Master File (DMF) is described and evaluated in Hill and Rosenwaike (2002). Coverage of the death data is better in the 1990s, for older workers, and for men. Recent work comparing the DMF with complete mortality data from the National Center for Health Statistics suggests coverage for men is between 80-90% before age 65 and above 95% after age 65 (see extensive notes by Elizabeth Weber Handwerker, http://socrates.berkeley.edu/~eweber/DMFnotes.htm). We replicated these tabulations for deaths in Pennsylvania for 1980-2002 and find similar results (in our empirical analysis, we also include deaths occurring in other states).

[13] Our sample is not meant to capture all job losers, but maintains the focus on workers losing stable jobs common in the literature on job displacements (e.g., JLS; Schoeni and Dardia, 2003; Couch, 2006). Another reason to impose a minimal tenure restriction when working with administrative data is to exclude voluntary movers. This is discussed in detail in Hildreth, von Wachter, and Handwerker (2008). Hildreth et al. (2008) and von Wachter, Song, and Manchester (2007) show using administrative data that earnings losses from job displacements increase with job tenure, but that they are still substantial and long lasting at shorter tenure durations.

[14] Hildreth, von Wachter, and Handwerker (2008) explore the issues arising in the measurement of displacement using administrative data in detail and conclude that the results based on JLS's way of identifying mass layoffs are robust to alternative definitions of mass layoffs.

[15] These workers were excluded in JLS because, due to their uncertain layoff status, they may belong in the treatment group, in which case including them in the comparison group would underestimate the effects of displacement. On the other hand, if these workers are of worse underlying health, excluding them would bias our results upwards. Thus, to err on the conservative side, we included them into our main sample as non-displaced workers. We also show results based on the original JLS sample restriction.

Because we use percentage changes in firm employment to identify displaced workers and such changes are not very meaningful for small employers, we further limit our sample to those whose firms employed at least 50 workers in 1979. In addition, we restrict our analysis to male workers. During the period we study, there were relatively few female workers with such stable employment relationships. As a result, sample sizes are too small to derive meaningful results for women. Again following JLS, we restrict some of our analysis to workers born between 1930 and 1959, a group for whom retirement is unlikely before the 1990s.[16] However, for some analyses, which are noted below, we expand the age range to include workers born between 1920 and 1959.

A potential concern with our procedure for identifying displacement is that workers who just happen to die in a year in which their firms substantially reduce employment will appear to be job losers and, thus, displaced workers, even if they would have been able to retain their jobs had they lived. This misclassification of some dying workers as displaced rather than non-displaced workers would tend to bias upward simple estimates of the effect of displacement on mortality. To address this problem, following a suggestion from a referee, we drop from our samples workers who died during the year their firm suffered a mass layoff. Because we find below that the effects of displacement tend to be largest immediately after job loss, this likely leads us to underestimate the average effect of displacement on mortality.

The first three columns of Table 1 show means for a number of worker characteristics for the full sample just described, as well as for displaced and non-displaced workers separately. Both groups of workers were, on average, in their late 30s, with earnings in the middle of the income distribution for the period. Displaced workers were about a half year younger, had earnings about 6% lower, and experienced slightly more quarters without earnings in the 1974-79 base period than non-displaced workers. Displaced workers did, however, have somewhat faster earnings growth during the base period. In addition, during this period, displaced workers were employed by larger firms, and were more likely to work in the steel industry or other durable goods producing

[16] JLS imposed this restriction to focus on workers expected to continue to be employed, since they did not have information on company pension plans and other retirement options, or health and disability insurance typically available to older workers. We do not have such information either.

industries. These patterns suggest it is important to control for potential differences across sectors and firms, but also for pre-job loss differences in career outcomes among movers and stayers.

Despite having relatively similar earnings during the 1974-79 base period, displaced workers had much lower average earnings between 1987 and 1991. In part, however, this difference may reflect some displaced workers leaving the state or taking jobs in sectors not covered by unemployment insurance. Such workers have zero reported earnings, but may, in fact, have income not covered in the Pennsylvania UI system. To mitigate such concerns, the last three columns of Table 1 show results limited to workers who had positive reported earnings in each calendar year from 1980 to 1986, a restriction JLS imposed in their empirical analysis. Differences between displaced and non-displaced workers in the 1974-79 period are little affected by this restriction. However, the earnings differential for the 1987-91 period is narrowed considerably, though it remains quite large.

Figure 1 displays estimates of the percentage difference in annual earnings relative to the base period and to the comparison group of workers remaining at their employer in 1980 to 1986 controlling for year, age, and worker fixed effects. As in the last three columns of Table 1, the model is estimated using a sample that is restricted to workers who had positive earnings every year from 1980 to 1986. In the year immediately after displacement, earnings are over 50 log points below levels expected in the absence of displacement. Losses decline over time, but even 11 years after displacement are approximately 15%. Clearly, displacement is a major economic setback for the affected workers. We have also analyzed in a similar manner the impact of displacement on several other career outcomes, finding that displacement leads to modest long-run increases in earnings variability and the likelihood of changing jobs or industries. The effect of displacement on other outcomes – such as incidence of non-employment, industry mobility, or mobility across counties – is significant in the first 2-4 years after layoffs, but not afterwards.[17]

The last several rows of Table 1 show mortality rates over a number of time periods (deaths can occur anywhere in the United States). As explained above, to avoid misclassifying dying non-separators as displaced, we drop workers dying in the year of displacement. Not surprisingly, rates

[17] See Table 9 of our longer working paper for detailed results (Sullivan and von Wachter, 2007).

for all workers rise over time, from about 4 per 1,000 between 1987 and 1993 to more than 10 per 1,000 between 2000 and 2006. The table also shows that displaced workers experienced higher mortality rates than those who were not displaced. The gap between the groups' mortality rates was especially high in the 1987-93 period shortly after displaced workers lost their jobs. Indeed, during this period, displaced workers were more than 40% more likely to die as non-displaced workers (5.151 per 1,000 versus 3.670 per 1,000). However, even 20 years later, during the 2000-06 period, mortality rates were more than 15% higher for the displaced workers. Of course, these simple comparisons of mortality rates do not control for the systematic differences between displaced and non-displaced workers that are illustrated in the upper portions of the table.[18]

2.2 Main Estimation Strategy

In order to control for differences in other variables that may affect mortality, we employ a standard logistic regression framework. Specifically, we estimate a number of logistic regression models of the form

$$\ln\left(\frac{p_{it}}{1-p_{it}}\right) = x_i\,\beta + \delta D_{it} + \chi_{a(i,t)} + \phi_t \tag{1}$$

where $p_{it} \equiv \Pr\left\{Death_{it} = 1 \mid Death_{it-1} = 0\right\}$ is the hazard of worker i dying in year t given survival through year $t-1$, and D_{it} is a dummy variable equal to one if worker i has been displaced prior to year t and zero otherwise.[19] Thus, the coefficient on the indicator variable for displacement measures the increase in the log odds of death in a given year, holding constant the other variables in the model. Because the probability of death is typically quite small, the increase in the log-odds ratio approximates the percentage increase in the death rate itself. In some models, we also include interactions of the displacement dummy with other variables, which allows the effect of displacement on mortality to vary in a number of important ways.

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[18] Note that we also replicated standard estimates of the age-gradient in mortality for our sample (Appendix, Figure 1, Sullivan and von Wachter, 2007) and found them to be quite similar to typical patterns for representative U.S. samples.
[19] This is a standard logistic regression model, and we obtain our parameter estimates by maximum likelihood. Workers contribute one observation for each year that they are alive during the follow-up period. The risk set evolves over time as workers die. Efron (1988) shows that the logistic model we estimate approximates standard continuous parametric models of the survival hazard.

All the specifications we report below include year dummies (ϕ_t), which among other things may control for variation over time in the completeness of the Social Security Administration's death records. They also include a 4th order polynomial in age ($\chi_{a(i,t)}$). Results are very similar if the age quartic is replaced by an unrestricted set of age dummies, or even a simple linear time trend. None of our results are sensitive to the logistic functional form; they are all evident in straightforward tabulations of average mortality rates and in linear probability models.

The firm-level shocks that lead to employment reductions should be exogenous to workers' individual health problems. However, it is possible that firms faced with the need to reduce employment may tend to lay off their least productive workers, who may in turn be in poor health. To address this potential problem, we consider a number of specifications that control for variables likely to capture productivity differences in the 1974-79 period (x_i). In Section 3.4, we summarize several additional robustness checks confirming that our results are not affected by selective job displacement.

3. Displacement, Mortality, and Life Expectancy

This section presents our basic estimates of the effect of displacement on the mortality hazard. We first show results based on models that assume a constant effect on the hazard. We then show how the effect varies by time since displacement and other variables. Finally, we derive the implications of our estimates for life expectancy and summarize our sensitivity analysis.

3.1. Displacement and the Mortality Hazard

The first column of Table 2 shows estimates of the coefficient on the displacement dummy of model (1) for various sets of control variables (x_i). Models are estimated using the full sample of workers over the entire 1980-2006 period. Controlling only for the mean and standard deviation of earnings during the 1974-79 period as shown in row (A), we estimate that displacement is associated with about a 17% increase in the mortality hazard. The remaining rows probe the robustness of this result. Adding 1-digit industry fixed effects, the growth in earnings and the number of quarters of zero earnings during the base period, and interactions of the career variables with age as shown in rows (B)–(D) has very little effect on the estimate.[20] Row (E) shows the estimate for a linear probability model version of row (B), while row (F) shows results from a linear probability model that includes firm fixed effects. When expressed as a percentage of the baseline hazard, these latter two estimates are modestly higher but in the same ballpark as the estimates from the logit models. Overall, there is no indication that our effects can be explained by firms selectively displacing less productive workers who are also less healthy than their peers. Similarly, it does not appear firms or sectors with high average layoff rates provide less healthy career environments or attract less healthy workers.[21]

The remaining columns of Table 2 show the impact of changing the data set over which model (1) is estimated. In column (2) we continue to use the full set of workers, but restrict the time period over which we track mortality to 1987-2006. Restricting the time period this way lowers the estimates to the 10% to 15% range, which is consistent with the biggest effects being observed immediately after displacement. In column (3), we restrict the set of workers to those who have positive reported earnings in at least three years between 1980 and 1986. This has very little effect on the estimates. However, requiring workers to have earnings in all years, as shown in column (4), lowers the estimates to the 7% to 9% range. This suggests that part of the effect estimated on our more general sample in column (1) is due to workers permanently dropping out of the labor force or leaving PA. However, the majority of the effect is still present for workers with stable attachment to the PA labor force after job loss. Finally, column (5) shows results using the original JLS sample which, in addition to requiring earnings in each year from 1980 to 1986, drops non-mass-layoff separators. These estimates again range from 9% to 11%.[22] Overall, we consider the estimates shown

[20] If we only include year and age effects for our most general sample in column (1), we obtain a displacement effect of 0.227 (0.0354); if we only include log average earnings as an additional control variable, we obtain 0.2005 (0.0355).
[21] The fact that the within-firm estimate of the effect of displacement on death is not smaller suggests that workers remaining at the firm experiencing mass layoff do not have higher mortality that may have arisen, say, due to increased uncertainty. This is consistent with our finding that mortality increases are correlated with large earnings losses of displaced workers.
[22] This suggests that non-mass-layoff separators experience mortality increases as well, something confirmed in Appendix Table 3 of our longer working paper (Sullivan and von Wachter, 2007); this is not surprising because for high-attachment workers in the difficult economic environment in the early to mid-1980s, most job separations tended to lead to non-trivial earnings losses (see also von Wachter, Song, and Manchester, 2007).

in Table 2 as indicating a reasonable degree of robustness to the set of additional control variables and the sample of workers included in the estimation.

Table 3 displays the other coefficients in the models of row (C) of Table 2, which, as we discuss in Section 4, are useful for trying to understand the channels through which displacement affects mortality. The elasticity of the mortality hazard with respect to average quarterly earnings in 1974 to 1979 is about -0.5.[23] The elasticity of mortality with respect to the standard deviation of income is estimated to be around 0.17, indicating that higher income variability tends to increase mortality.[24] Holding constant average earnings and earnings variability, an additional quarter of non-employment in the base period reduces mortality by about 9%, an effect that is, perhaps, consistent with the findings of Ruhm (2000).[25] Conditional on the other variables, the earnings growth trend from 1974 to 1979 has little effect on mortality.

3.2. Mortality Effects by Year Since Layoff, Age, and Job Tenure

The results in Table 2 suggest the immediate impact of displacement on mortality differs from the long-run effect. To explore this pattern further, column 1 in Table 4 breaks up the effect of displacement on death by year since layoff for our most general sample (as in column 1 in Table 2). The first row shows the long-run effect in manufacturing industries. The effect is statistically significantly different from zero and substantial even at 16 or more years after layoff. The last row of the table shows that this effect is not statistically significantly different in non-manufacturing industries.

The remaining rows of column 1 show how the effect differs in the first 15 years after layoff. To obtain the full effect of displacement on mortality at different years since displacement, the

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[23] This estimate is somewhat higher than typical estimates of the correlation of mortality with a single year of income (e.g., Deaton and Paxson, 1999). This is because our data on average earnings over a six-year period do a better job capturing a notion of permanent income and are less affected by measurement error present in self-reported income measures in survey data. This is further discussed in Appendix 1 of our longer working paper.

[24] We use the standard deviation of the log of quarterly earnings to obtain a measure of earnings variability based on percentage changes that is comparable across wage levels.

[25] Note that workers in all of our samples can be on temporary layoff or sick leave from their main employer for as much as three quarters each year in the period from 1974 to 1979.

coefficients on the interactions have to be added to the main effect in the first row.[26] We see large percentage increases immediately after job displacement. The effect remains high for the first five years after job loss and then gradually declines with time since layoff and bottoms out at a long-run average of about 13%. This is shown graphically in Panel A of Figure 2, which plots the point estimates and two-standard-error bands. These estimates suggest strong immediate responses when the impact of a layoff on earnings, employment, job mobility, and other career outcomes is most severe. The effects then stabilize at a permanent difference as workers continue to suffer negative consequences of layoffs in terms of reduced earnings.

As in Tables 2 and 3, column 1 of Table 4 includes workers born after 1930 that we observe up to age 76. To further study the long-run effect of displacement on death, column 2 shows the same estimates when we include workers whom we observe closer to the end of their life (up to age 86).[27] The short- and long-run effects of job loss on the long-term mortality rate are similar but somewhat smaller. This suggests that the proportional effect of displacement on the mortality rate varies by age. Thus, column 3 includes interactions of displacement with age groups. The excluded age group is 56-64. The specification also includes a dummy for whether workers were displaced near retirement age (age 60-69).

We find that workers younger than age 55 suffer significantly higher percentage increases in mortality hazards in response to a displacement than older workers. This difference is particularly strong for workers under 45, but still present for workers age 46 to 54. We also find that workers displaced near retirement age appear to respond significantly less to job loss than workers displaced in middle age. This is perhaps not surprising, since older workers are more likely to have access to Social Security benefits, to company pension plans, or to Medicare. Even for workers not yet at retirement age, access to federal disability insurance increases substantially at age 55, when workers can claim loss of vocational qualifications to qualify for disability insurance (e.g., Chen and Van der Klaauw, 2007; Black, Daniel, and Sanders, 2002). Younger and middle-aged workers do not have

[26] E.g., for a displaced worker 2-3 years after layoff, the effect of displacement on mortality would be 0.131+0.559=0.69.
[27] For men of the birth cohorts in our samples at age 40-50 average life-expectancy is about 70-75 (National Center for Health Statistics, 2006, Table 11).

access to similar mechanisms to smooth long-term earnings losses. Moreover, as further discussed in Section 4, for these workers the reduction in lifetime earnings is larger because earnings losses accrue over a longer period.[28]

As workers age, the total effect of displacement on mortality is determined by the sum of the long-run mortality effect in the first row and the coefficients of the relevant interactions of displacement with year since displacement, current age, and age at layoff; for example, for a worker under 45 and 2-3 years after displacement, the effect of displacement on mortality is 0.795 (=0.133+0.279+0.383). The resulting total effects of displacement on mortality by current age for different ages of separation are shown in Panel B of Figure 2. The entries in the figure are obtained by summing up the relevant coefficients in column 3 of Table 4.[29] In the first years after layoff, workers at all ages experience large increases in the probability of death in the range of 50-100%. The effect declines as workers age and settles at a positive long-run effect 16 years after layoff for all age groups (the effect in the first row of Table 4). Only for workers displaced after age 60 is the long-run effect close to zero.[30]

To assess whether our results are robust to restricting our sample to those with at least six years tenure, columns 4 to 6 of Table 4 replicate the estimates for a sample of workers with at least three years of tenure. Column 4 shows that the results are now larger both in the long and the short run. This is not surprising, since the lower tenure requirement tends to draw younger workers into the sample of displaced workers, and we find younger workers tend to show a larger mortality effect. Once we consider a sample including older workers (column 5) and control for age (column 6), the

[28] Another possibility is that more frail displaced workers die first, reducing the gap in mortality rates between displaced workers and non-displaced workers. Such dynamic selectivity would lead us to understate the effect of displacement on mortality as workers age. However, since the number of deaths is small relative to the overall population at risk of death, the average underlying health of the population at risk is unlikely to be greatly affected by selection. Thus, the effect of dynamic selection is likely to be small in the present case.

[29] We obtain similar patterns with complete interactions of age-at-displacement and years-since-layoff; however, with the increased number of parameters, the estimates turn imprecise.

[30] The figure also shows that for workers age 50 and older at displacement, the effect actually increases somewhat 15 years after job loss, amounting to a U-shape, albeit a weak one. A similar pattern has been observed for the event of losing a spouse that leads to an initial increase in mortality from stress, and a weak long-term rise of the mortality rate to the level of single individuals (e.g., Martikainen and Valkonen, 1996). The pattern is suggestive of an initial response due to acute stress caused by the job loss, followed by a long-term cumulative impact of increased chronic stress due to lower earnings.

results are quite similar across tenure restrictions. Thus, reducing the tenure requirement to at least three years of job tenure at displacement does not substantially affect our results, especially once we account for differences in the effect of job displacement on mortality by age.[31]

3.3 The Reduction in Life-Expectancy due to Job Loss

Since increased mortality affects workers over a long time horizon, our estimates imply substantial reductions in life expectancy. The average loss in life expectancy can be used as a summary measure of the cost of job loss at mass layoff in terms of life-years, much as the present discounted value of lifetime earnings losses provide summary measures in conventional analyses of the cost of job loss. In Table 5, we present a range of estimates of losses in life expectancies for alternative samples and ages.

Since some cohorts are still alive at the end of the sample period, to calculate the total cumulated effect of permanently greater mortality hazards we have to make an assumption about the development of mortality differences among laid off workers and the control group past our observation window.[32] Specifically, we assume that the proportional increase in the odds of death that we estimate for the highest observed age is maintained indefinitely. Given that the typical profile of the increase in the log odds of dying is stable through older age ranges, and is so within the groups of our sample, this is a plausible assumption.[33] All life expectancies are based on our most general sample with no further employment restriction, include older workers, and are calculated for workers with average annual earnings in 1974-79 working in non-manufacturing industries in 1979.

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[31] If we replicate the estimates for workers with 3-5 years of tenure, as expected the point estimates are larger, but also more imprecise because of smaller sample sizes.

[32] Life expectancy can be calculated as the sum of survivor probabilities over the remaining potential age-years of an individual. The difference in life expectancies then is the sum of the differences in survivor probabilities. We experimented with different windows of extrapolation, and found our results are not driven by differences in extrapolated survival probabilities outside our sample period.

[33] Since extrapolation of a quartic polynomial in age can be unstable, for calculating life expectancies we worked with a linear age specification for calculating differences in life expectancy. We find the log-odds ratio is well approximated by a linear function in age. Efron (1988) shows that with a linear age-component, our logistic model would imply a Gompertz-distribution for lifetime in a continuous time setting, a distribution commonly used in the analysis of survival times. We have also experimented with a quartic polynomial, and found it did not significantly alter our results.

Since there are large increases in mortality right after layoff, Table 5 is based on estimates of survival curves that take into account the dynamic response in mortality found in Table 4. Since the effect of displacement status also differs by age, we use our most general specification in Table 4 to calculate life expectancy. The last column of Table 5 shows that losses in life expectancy are larger for workers losing their job in their 30s and 40s. Life expectancy of workers displaced in their 50s still declines, but by less. This difference arises both because younger workers are exposed to higher mortality rates over a longer period of time and because the increase in mortality rates tends to be greater for younger workers.[34] In our longer working paper (Sullivan and von Wachter, 2007), we confirm similar orders of magnitude for samples restricting workers to have some employment in the period of job loss.

Given these substantial declines in life expectancy, an important question is how these losses should be treated with respect to more conventional estimates of the cost of job loss. The typical measure of the cost of displacements is based on the loss in present discounted value of earnings. For the average worker in our high-tenured sample, this amounts to about $200,000 at a real interest rate of four percent. To make the losses in life expectancy comparable, we can monetize them by choosing an estimate of the statistical value of life. At about five million dollars per statistical life, a loss of one and a half years would amount to a monetary loss of $100,000. Although they can be no more than broadly indicative, values of this order of magnitude imply that cost of job loss for displaced worker may be substantially underestimated by traditional summary measures such as the loss in lifetime earnings.[35]

[34] Since life expectancy is the sum of the survivor probabilities over the remaining potential lifetime, summing up reduced survivor probabilities over a longer period reduces predicted life expectancy. The effect of displacement on survivor probabilities varies with age because of the functional form of the logistic function itself (as discussed in Sullivan and von Wachter, 2007, Figure 3), as well as from explicit age-displacement interactions as included in Table 4.

[35] To think about how these two numbers should be treated, consider the typical neoclassical model of health investments in which long-term health is an outcome of optimally chosen inputs given a lifetime budget constraint. In this case, the loss in the present discounted value (PDV) of earnings is a sufficient statistic for the cost of job loss, since the reduction of lifetime would be the optimal response to the reduced resources. However, only a small fraction of health expenditures are out of pocket. Moreover, health is likely to be affected by factors other than consumption or health inputs. For example, it may be affected by losses in social status related to earnings declines at layoff. In that case, the PDV of earnings losses is not a sufficient statistic anymore, and to obtain the total costs of job displacements the monetary value of the direct effect of layoffs on mortality should be added to the PDV of earnings losses. In particular, we should have that the Total cost of job loss = PDV of Earnings Losses + a × Monetary Value of Lost Life, where a is

3.4. Sensitivity Analysis: Pooling Displaced and Non-Displaced Workers

We have shown that our estimates are robust to the inclusion of an extensive set of alternative control variables as well as industry and firm fixed effects. This suggests that any remaining bias from selective displacement or from sorting of workers into firms is likely to be small, especially in the environment of Pennsylvania in the early to mid-1980s, when employment reductions were often severe. Moreover, in this period, most firms in the sectors most prominent in our sample were either unionized or followed seniority rules in dismissals (Abraham and Medoff, 1984). Both of these factors are likely to have reduced employers' ability to selectively displace workers.

To address the question of a remaining bias from selective displacement directly, in this section we present estimates that pool displaced and non-displaced workers. These are based on a specification similar to equation (1), but with the variable of interest defined at the firm level, rather than the worker level. Since the mass-layoff dummy now varies only at the firm level, the estimates compare the change over time in mortality of all workers present at a firm experiencing a mass layoff with that of similar workers at stable firms. By construction, these "intent-to-treat" estimates are not affected by selection at the time of job loss or by misclassification of dying workers as job losers.[36]

The results shown in Table 6 are consistent with the findings of our main analysis. Working at the firm level considerably reduces our degrees of freedom, so to maximize precision, samples include workers born after 1920 and the specification includes fewer interactions. In the first two columns mass layoff is defined as in our main estimates – employment falling 30% below its previous peak. In many cases, however, employment declines are gradual, so firms may be laying off

between zero and one. In the neoclassical model, $a = 0$; in a model of social status, $a = 1$. The right value probably lies in between, but given the limited role of individual health expenditures, it is likely to be closer to one than to zero. This implies that increases in mortality due to layoffs substantially raise the costs of job loss and workers' willingness to pay to avoid displacement.

[36] The estimates may still be affected by sorting of workers into firms prior to layoff. In our main analysis, we showed that the results are unaffected by the inclusion of firm fixed effects, indicating that sorting plays no major role in our sample. Here, we cannot include firm fixed effects without losing precision. Yet, the results are robust to the inclusion of industry effects and of controls for characteristics of employers (average wage and average employment size from 1974 to 1979 of a worker's 1979 employer).

workers before they reach the 30% threshold. Thus, in the third and fourth columns, we present a specification in which mass layoff is defined as a sudden large drop in employment at the firm.[37]

To compare the magnitudes of the estimates in Table 6 to those in earlier tables, columns (2) and (4) show estimates divided by 0.3, which is approximately the effect of mass layoff on job mobility.[38] For either definition of firm-level mass layoff, the rescaled long-run effects of displacement on mortality shown in the bottom of Table 6 are of the same order of magnitude as the estimates shown in column 2 of Table.[39] Table 6 also suggests a large effect of displacement on mortality in the year of layoff. This suggests that dropping separators dying in the year of layoff in our main analysis may lead us to underestimate the full effect of displacement on mortality shown in Table 2. Correspondingly, the first row of Table 6 shows that the average effect of displacement on the mortality hazard, properly rescaled, is now larger.

These findings suggest that our main results are unlikely to be driven by selective displacement of less healthy workers. Two further results contained in our longer working paper indicate that firms with greater flexibility in selecting which workers to lay off did not systematically displace their least healthy workers. First, we show that the effect of displacement on mortality does not decline with the fraction of workers involved in a mass layoff. Second, we also find that other separators (those permanently leaving their long-term employers but not during a mass layoff) do not have higher mortality rates.[40]

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[37] Our results confirm that the timing of the shock at the firm level appears better captured by a sudden drop in employment. In case of more gradual employment reductions it is generally difficult to assign the year of a distinct shock occurring at the firm level. This is less of a problem at the individual level in our main estimates, since an individual's job separation always constitutes a distinct treatment.

[38] If we estimate the same regression models with an individual dummy for the event of job displacement as dependent variable, the coefficient is 0.299 and 0.289 for the gradual and sudden drop definitions of mass layoff, respectively. This number also results from the fraction displaced shown in Appendix Table 1 of our longer working paper.

[39] Note that the identification strategy differs for the two sets of estimates; in Table 6, the effect of the firm level event on separators and non-separators gets *added*, while in the estimates in Table 4 the estimates result from *subtracting* the effect on non-separators from that of separators.

[40] See Appendix Table 3 of Sullivan and von Wachter (2007). In an additional indication that selective displacement of less healthy workers mattered little in a similar context, Eliason and Storrie (2007) show that using detailed information on pre-displacement occupation, health status, and demographics does not affect their estimates of the impact of job loss during establishment closures during 1986-87 on health in Sweden (see the second and third set of estimates in their Table 6).

4. Potential Channels of Mass-Layoff Effect on Mortality

Our estimates of the short- and long-run effects of displacement on the mortality hazard in Figure 2 roughly parallel the short- and long-run effects of displacement on earnings shown in Figure 1 and reported in JLS and elsewhere. In the short run, displacement is associated with a sharp drop in mean earnings, but also increased unemployment, job, region, and industry mobility, as well as high earnings instability.[41] Our results suggest these effects may lead to acute stress that substantially raises the mortality hazard. After this initially turbulent phase, in the medium to long run the majority of job losers settle into relatively stable employment at substantially lower mean earnings and modestly higher employment instability and earnings variability. Several economic models of health determination predict that losses in lifetime economic resources should reduce health (e.g., Deaton 2001). Our empirical findings are consistent with a reduction in resources and increased instability leading to chronic stress and reduced investments in health that lead to a smaller, but longer-term increase in the mortality hazard.[42]

To gain insight into the relative importance of some of the channels through which job loss could affect the long-run mortality hazard, we compare our estimates of the "reduced form" effect of displacement on mortality to what one would expect on the basis of displacement's long-run effect on the mean and variability of workers' earnings and the correlation of those factors with mortality shown in Table 3. In our Pennsylvania data, displacement reduces the mean of long-run

[41] An extensive discussion of these results is contained in our longer working paper version (Table 9, Sullivan and von Wachter, 2007). The variance of earnings, incidence of non-employment, mobility between jobs, industries, and counties all increase in the aftermath of a job displacement. However, the main effect lasting beyond four to six years after displacement is the strong loss in earnings documented in Figure 1 and some increases in the variability of earnings and probability to change jobs. As noted there, the results are based on an annual distributed lag model that controls for worker fixed effects, year effects, and a quadratic in age. Because in our Pennsylvania administrative data earnings may appear to be zero when workers have moved out of state, the numerical results quoted in the text are based on workers who had positive earnings in the Pennsylvania labor market for at least some years after job loss. However, very similar results are obtained for the unrestricted sample.

[42] As noted previously, there are other potentially relevant channels to which our data does not speak to directly, such as the loss in health insurance or the role of the family environment. Some of these channels may be associated with earnings losses – e.g., if lower paying jobs are less likely to provide health insurance – but may have independent effects. However, it is currently not possible to link information on health insurance, family status, or other worker or firm characteristics to our data.

earnings by 15-20%. Taken at face value, the estimated correlation of average earnings with mortality of -0.5 would imply that we expect an increase in mortality of about 7.5-10% for workers with high attachment (0.15 or 0.2 times 0.5). Thus about 50-75% of the long-run effect of mass layoff on mortality reported in Table 4, about 0.1 to 0.15, could be explained by the observed declines in average earnings. Though displacement does not have a significant long-run effect on employment rates, it does raise the variability of earnings somewhat. The standard deviation of log quarterly earnings increases on average by about 16% after a mass layoff (results not shown). At a coefficient of -0.2 (Table 3), this implies an increase in the probability of dying of about 3.2%. While the order of magnitude of this effect is much lower than the potential impact of earnings, it could still account for about 20% of the mass-layoff effect, at least in the short run.

In addition to these adverse effects, job loss may have beneficial effects on health. At least while not employed, displaced workers may be able to spend more time investing in their health and face reduced exposure to workplace and driving accidents.[43] This channel may be present in our sample as well.[44] However, our results indicate that, on balance, job losers' health did not benefit from short-term employment reductions. This may be because in the short run, in addition to very large earnings losses (Figure 1), job loss also involves a stressful adjustment period involving multiple job changes, including changes in industry or location. In the long run, the health of the majority of workers we study did not benefit from reduced employment but instead suffered from continued employment at significantly lower earnings with a higher degree of uncertainty.

[43] Exploiting time-use data, Krueger and Mueller (2008) find some evidence that unemployed workers sleep more, spend more time purchasing goods and services (which includes obtaining medical services), and spend more time in leisurely activities (although the majority of the difference is explained by watching TV). They do not find that the unemployed spend more time on personal care (which includes health related self-care) or sports (Table 3). They confirm findings from the previous literature that the unemployed are more unhappy or sad on average (Table 4).

[44] In fact, we do find a beneficial effect of lower employment in 1974-79 on mortality thereafter (Table 3). However, spending time in non-employment after displacement has no statistically significant effect on mortality (Table 7). Such potential beneficial effects of job loss on health are suggested by, among others, Ruhm (2000), who shows that mortality at the state level declines in recessions. We relate our results to Ruhm's (2000) findings in the conclusion.

Overall, these considerations suggest the impact of displacement on the mean and variability of earnings may explain a large fraction of the increase in the long-run mortality hazard that we estimate. Of course, if frail people have large earnings losses, higher earnings instability and higher death rates, the estimates underlying the above decomposition may not be causal parameters, and the predicted impact of each single mechanism is likely to be overstated.[45] Nevertheless, among the channels we can measure, these calculations suggest that long-term earnings losses are likely to play a dominant role in explaining our results.

4.1 Evidence from Individual-Level Models

To further explore the role of long-term earnings losses using the longitudinal data at our disposition, we also directly relate the size of earnings losses at job displacement to the long-term increase in the hazard of death. This is shown in Figure 3, Panel A. The figure shows the increase in mortality by deciles of long-term changes in mean earnings, controlling for age, year, and past average earnings.[46] On average, those workers that have more substantial earnings losses also experience larger long-term increases in the mortality hazard.

To further explore the role of earnings losses in explaining the long-term increases in the mortality hazard after job loss, we also directly included the long-term earnings change for both displaced workers and the comparison group as control variable in our main logistic model. The

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[45] Reverse causation is less of a problem, because average earnings and earnings variability are measured in 1974-79 while death is measured from 1987-2006 or 1980-2006 (with little difference in results between the two samples, see Table 3). In his study of Swedish lottery winners, Lindahl (2005, Appendix Table 2) shows that the effect of controlling for initial health conditions tends to reduce the correlation between mortality and earnings by about a third. (The magnitudes of the OLS effect that Lindahl estimates are somewhat smaller but in the same ballpark as ours. He does not report logit estimates, so the comparison is based on approximate percentage effects derived from the results of linear probability models.) Were this to be the case, the predicted role of earnings in explaining the mass-layoff effects is reduced by about a third, which still leaves a substantial effect. Note however that our high-attachment sample is likely to be of better health than the older sample used in Lindahl (2005).

[46] Specifically, the figure shows coefficients on dummies for deciles of changes in the log of average annual earnings from 1974-79 to 1980-86 in a logistic model of death. The omitted category is that for earnings changes in the range [-0.05, 0.05]. Other variables include year effects, a fourth order polynomial in age, and the average and standard deviation of earnings 1974-79. To maximize sample sizes, the figure shows results based on the sample that includes older workers (see Table 4). The different lines correspond to different restrictions on presence in the Pennsylvania labor market during the period 1980-86. All but the coefficients on the dummies [-0.2,-0.05], [0.05,0.15], and [0.15,0.3] are statistically significant from zero.

results, shown in the Panel A of Table 7 for samples with alternative degrees of presence in the Pennsylvania labor force, suggest a strong correlation between the change in average earnings and the long-term mortality hazard (Model 1). Once we condition on earnings changes, the effect of the mass- layoff dummy becomes numerically small and insignificant.

To directly assess the potential role of the increase in the variability of earnings at job loss, we also estimated models that included the change in the standard deviation of quarterly earnings from 1987 to 1991 as an additional control variable (Model 2 in Table 7). As expected, an increase in the standard deviation has a positive significant effect on death rates, and the impact of earnings changes on long-term mortality declines somewhat. The results are robust if we divide the sample by degree of labor force attachment or displacement status.

Estimates from the individual level models appear to confirm the results of our approximations based on the estimates in Table 3. However, we do not interpret these results as necessarily indicating causal channels running from earnings losses or earnings variability to mortality, since there may be omitted variables driving both earnings losses and mortality increases, such as differential increases in depression in response to layoffs.

4.2 Evidence from Group-Level Models

To mitigate the problem of omitted variable bias, we replicated our individual level analysis at the group level. A long literature suggests that there are systematic differences in the effects of job loss on earnings,[47] some of which arise from industry, firm, or job-match-specific rents workers receive on the job that are permanently lost as workers change employers (e.g., von Wachter and Bender, 2006).[48] Since at least part of the loss in earnings for workers leaving high-paying industries

[47] E.g., Kletzer (1989), JLS, Neal (1995), Farber (2003).
[48] Such rents could arise due to contractual premiums (Beaudry and DiNardo, 1991), job search (Topel and Ward, 1992), and firm, industry, and regional wage premiums (e.g., Krueger and Summers, 1988; Abowd, Creecy, and Kramarz, 2002).

or large employers is likely to be uncorrelated to health changes at the cell level, we expect the omitted variable bias of the cell-level estimates to be lower than that of individual-level estimates.

To implement the group-level analysis we use the detailed information available from our administrative data to divide workers into cells based on age at layoff, industry and local labor market conditions before layoff, and average employment size or average wage of the 1979 employer. We then run a regression of increases in the mortality hazard on losses in average earnings at the cell level. This is equivalent to using interactions between cell-level dummies and our displacement indicator as instruments for earnings losses in a linear probability model of mortality. The model controls for permanent differences in average health and earnings across groups through cell-level dummies. Similarly, it accounts for a common effect of displacement on all groups through a mass-layoff dummy.[49]

Panel B of Table 7 shows the coefficient estimates on actual earnings changes in a linear probability model as a benchmark, as well as the slope coefficient in the cell-level model. The results suggest that the effect of earnings losses on mortality at the cell level is 40-50% smaller than the effect at the individual level for the samples with no or low work requirement. The cell-level effect is still statistically and economically significant but somewhat smaller for the high-attachment sample, but standard errors increase as well.[50] As before, the coefficient on the mass-layoff dummy is small and insignificant, though now the estimates are less precise. Our findings are quite similar for the different definitions of cells shown in the table (Models 3-7).

[49] The cell-level model is implemented in two steps. First, we regressed individual earnings and mortality on characteristics such as baseline earnings, baseline standard deviation, a quartic in age, and year effects; in addition, we included cell-level dummies and interactions between cell-level dummies and the mass-layoff dummy. The coefficients on the interaction are used in the second step. Second, we regressed cell-level changes in mortality on cell-level earnings losses weighting by the inverse sampling variance of the cell-specific mortality increases. The resulting slope coefficient is equal to the two stage least squares estimate from using the interactions of group-dummies and mass-layoff dummy as an instrument for earnings. See Section 3 of our longer working paper for the relevant equations and further discussion (Sullivan and von Wachter, 2007).

[50] Note that the standard errors are from the group-level model and are thus corrected for group-level error components.

The bottom panel in Figure 3 displays the corresponding cell-level averages and a linear regression line for the high-attachment sample for the results of model 7 in Table 7. The figure displays a clear negative and relatively precise association between earnings losses and mortality increases at the cell level, albeit there is an important degree of variation left. Overall, the results confirm that earnings losses after job displacements appear to be strongly correlated with increases in mortality rates.[51] Since we cannot exclude that health and earnings responses to layoff may be correlated across cells as well, we do not interpret the resulting cell-level estimates as causal effects of earnings changes on the long-term mortality hazard.

4.3 Earnings Losses and Life Expectancy

The analysis thus far has concentrated on the impact of long-term declines in quarterly earnings, irrespective of their duration. However, standard models of health investment refer to lifetime resources as the relevant earnings concept. Thus, we calculated the present discounted value (PDV) of lifetime earnings losses following a job loss at a mass layoff by age group (Sullivan and von Wachter, 2007).[52] We then compared losses in life expectancy taken from Table 5 with the corresponding PDV of earnings losses by age group. The correlation is strong, monotonic, and numerically important; as the percentage loss in the PDV of earnings doubles for 30-40 year olds relative to 50-60 year olds, life expectancy declines by about 25%. Young workers not only have higher average mortality increases after a job loss (Table 4), but also the largest losses in remaining

[51] The mean-squared error of the group-level regressions is a test statistic for over-identification with a limiting Chi-Squared distribution with degrees of freedom equal to the number of cells minus the number of parameters (e.g., Angrist, 1991). In results not reported here, for none of our group-level models can we reject the over-identifying restrictions at any reasonable level of statistical significance. This supports an underlying model of a constant proportional effect of earnings on mortality, and gives no indication of endogeneity of cell-level earnings changes with respect to mortality that differs across cells. The fact that our cell-level estimates tend to be smaller than individual level estimates suggests that the latter indeed are affected by omitted variable bias common across groups.

[52] To do so, we allowed both the short- and long-term effects of job loss on earnings to vary by either 10-year or 5-year age groups. Since we do not have complete earnings histories after job loss for all workers, we assume that the earnings loss decays at the same speed of reversion observed between years 6 to 11 after a job loss, and eventually stays fixed at zero. The resulting values are robust to alternative specifications. The percentage loss in the present discounted value of earnings of our preferred specification for the age groups in Table 5 are, going from youngest to oldest, 12.3%, 10.8%, 9.2%, 7.4%, 5.6%, and 3.8%, respectively.

lifetime (Table 5) and in lifetime earnings. Thus, it is not the oldest workers that are most affected, but those in prime working age that are exposed to the negative consequences of job loss over a longer period of time.

Overall, we interpret the results in Section 4 as suggesting that at least for job losses involving earnings declines as dramatic as those in Pennsylvania in the early to mid-1980s, the sources of the increase in mortality are likely to be associated with long-term losses in average earnings and increases in the variability of earnings. This may include direct effects of reduced earnings and increased variability, but clearly also stress, adjustment costs, and other factors correlated with both long-term earnings declines and mortality.

5. Conclusion

This paper uses administrative data covering over 15 years of quarterly earnings and employer records matched to information on date of death to study the effects of job displacement on mortality. To measure an event plausibly exogenous to workers' own health outcomes, we analyze job losses occurring when employers experience mass layoffs affecting at least 30% of their work force. To further control for selection, we also control for workers' average earnings and a range of career outcomes in the period before job loss, and present selection-free estimates pooling movers and stayers. The results suggest a particularly pronounced increase in mortality during the period immediately following job loss and a long-run increase of 10-15% in the annual probability of dying persisting for at least the next 20 years. These effects, robust across alternative samples and specifications, are consistent with strong responses to both acute and chronic stress associated with worsened labor market opportunities.

To assess the channels underlying the mass-layoff effect, we analyze the correlation of long-run career outcomes with mortality. We show that the mean and standard deviation of earnings during a baseline period have large and significant correlations with mortality in a later follow-up period. Together with estimates of the effects of mass layoffs on long-run career outcomes, these results suggest that an important fraction of the effect of job loss on mortality can be attributed to persistent losses in earnings. This is confirmed by a direct analysis of differences in mortality responses by groups of workers with differential earnings losses at job displacement associated with industry or employer affiliation before displacement.

These results suggest that events in the labor market shaping workers' careers also have long-run effects on health outcomes. The losses in life expectancy implied by our estimates shows these effects can be large. A worker displaced in mid-career can expect to live about one and half years less than a non-displaced counterpart. The reduction in life expectancy is smaller for older workers who experience lower lifetime earnings losses and are exposed to increased mortality for a shorter period of time. Our results do not speak to the role of non-economic factors such as stress, self-worth, and happiness. Yet, they suggest an important avenue for future research would be to examine whether the negative health consequences of mass layoffs can be prevented by providing assistance that stabilizes the level and variance of earnings.

Finally, our results are not in conflict with recent work suggesting that mortality declines during recessions (Ruhm, 2000), possibly because of healthier life-styles and a reduction in accidents related to work or commuting. First, while recessions do increase the number of high-tenure displaced workers, whose mortality we find to be elevated, such workers are a small fraction of those affected by economic downturns.[53] Second, Ruhm (2000) focuses on fluctuations in mortality that are contemporaneous with cyclical fluctuations in economic activity, while the bulk of the effects we observe take place many years after displacement. Finally, from the perspective of the aggregate economy, a recession is a relatively minor event that only marginally reduces the present value of lifetime income for the representative worker-consumer and at the same time provides a modest increase in leisure. For an individual high-tenure worker, however, job loss is a major economic setback that significantly reduces lifetime income, without a corresponding reduction in work activity. Thus, the workers we study, while having fewer lifetime resources, did not enjoy the increases in leisure, healthier life-styles, or reductions in accidents that may explain Ruhm's results.

[53] See, for example, Aaronson and Sullivan (1998). The gains in health during recessions measured by Ruhm (2000) may be due to changes in hours worked of employed workers or due to changes in employment rates of those with less strong job attachment.

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Table 1: Sample Characteristics by Displacement Status (Workers in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, Born 1930-59)

Work Restriction in Pennsylvania Labor Market During 1980-1986	No Work Restriction			Work Every Year		
	All Workers	Displaced Workers	Non-Displaced Workers	All Workers	Displaced Workers	Non-Displaced Workers
	(1)	(2)	(3)	(4)	(5)	(6)
Sample Size	**21573**	**7256**	**14317**	**17641**	**4785**	**12856**
Age in 1979	**30.42**	**30.14**	**30.55**	**37.42**	**37.01**	**37.57**
	(7.124)	(7.422)	(6.964)	(7.031)	(7.295)	(6.925)
Log Average Quarterly Earnings in 1974-79	**8.74**	**8.70**	**8.76**	**8.75**	**8.70**	**8.76**
	(0.358)	(0.346)	(0.362)	(0.345)	(0.338)	(0.346)
Log Std. Dev. of Log Quarterly Earnings 1974-79	**-1.637**	**-1.483**	**-1.715**	**-1.680**	**-1.545**	**-1.731**
	(0.732)	(0.767)	(0.700)	(0.709)	(0.749)	(0.687)
Percent Change in Quarterly Earnings 1974-79	**0.513**	**0.677**	**0.430**	**0.459**	**0.582**	**0.413**
	(5.736)	(7.699)	(4.425)	(5.343)	(7.287)	(4.410)
Number of Quarters in Non-Employment 1974-79	**0.48**	**0.58**	**0.43**	**0.45**	**0.54**	**0.42**
	(0.977)	(1.100)	(0.904)	(0.919)	(1.029)	(0.873)
1979 Firm's Employment	**8556**	**10483**	**7579**	**8087**	**9065**	**7723**
	(13944)	(16287)	(12479)	(13267)	(15018)	(12534)
Fraction Steel Industries	**0.179**	**0.292**	**0.122**	**0.163**	**0.260**	**0.128**
	(0.384)	(0.455)	(0.328)	(0.370)	(0.438)	(0.334)
Fraction Other Durable Goods Manufacturing (Non Steel)	**0.297**	**0.349**	**0.271**	**0.300**	**0.365**	**0.275**
	(0.457)	(0.477)	(0.444)	(0.458)	(0.481)	(0.447)
Fraction Other Manufacturing	**0.191**	**0.164**	**0.204**	**0.200**	**0.183**	**0.206**
	(0.393)	(0.370)	(0.403)	(0.400)	(0.387)	(0.405)
Fraction Eastern PA	**0.562**	**0.475**	**0.606**	**0.581**	**0.521**	**0.603**
	(0.496)	(0.499)	(0.489)	(0.493)	(0.500)	(0.489)
Log Average Quarterly Earnings in 1987-91	**8.606**	**8.184**	**8.791**	**8.728**	**8.421**	**8.838**
	(1.069)	(1.310)	(0.883)	(0.891)	(1.064)	(0.792)
Log Std. Dev. of Log Quarterly Earnings in 1987-91	**-1.344**	**-1.119**	**-1.440**	**-1.393**	**-1.197**	**-1.462**
	(0.764)	(0.793)	(0.730)	(0.736)	(0.757)	(0.716)
Number of Quarters in Non-Employment in 1987-91	**4.31**	**6.66**	**3.11**	**2.20**	**3.32**	**1.79**
	(7.070)	(8.207)	(6.079)	(4.736)	(5.900)	(4.145)
Deaths per 1000 per Year 1987-2006	**6.764**	**7.639**	**6.325**	**6.343**	**6.913**	**6.132**
	(0.143)	(0.263)	(0.170)	(0.152)	(0.306)	(0.175)
Deaths per 1000 per Year 1987-1993	**4.167**	**5.151**	**3.670**	**3.745**	**4.400**	**3.502**
	(0.181)	(0.347)	(0.208)	(0.189)	(0.393)	(0.214)
Deaths per 1000 per Year 1994-1999	**7.407**	**8.114**	**7.053**	**6.994**	**7.451**	**6.826**
	(0.227)	(0.411)	(0.272)	(0.242)	(0.481)	(0.280)
Deaths per 1000 per Year 2000-2006	**10.815**	**11.909**	**10.270**	**10.347**	**11.033**	**10.094**
	(0.427)	(0.777)	(0.510)	(0.458)	(0.911)	(0.529)

Notes: Standard deviations in parentheses (with exception for death rates, which show standard errors). The samples only include male workers born 1030-59, in stable employment 1974-79 at an employer of size 50 in 1979. Displaced workers left jobs in firms whose employment the subsequent year was 30% or more below its post-1974 peak. Information pertaining to employment and earnings is from Pennsylvania. Deaths can occur anywhere in the United States.

Table 2: Effect of Job Displacement on Log-Odds of Death for Various Samples, Follow-Up Periods, and Specifications (Workers in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, Born 1930-59)

	Work Restriction in Pennsylvania Labor Market During 1980-1986	No Work Restriction	No Work Restriction	Work At Least 3 Years	Work Every Year	Work Every Year, Exclude Non-MLF Separators [JLS Sample]
	Death Follow-Up Period:	1980-06	1987-06	1987-06	1987-06	1987-06
		(1)	(2)	(3)	(4)	(5)
(A)	Baseline Model with Average and Std. Dev. of Earnings in 1974-79	**0.170** (0.036)	**0.147** (0.037)	**0.148** (0.038)	**0.088** (0.044)	**0.104** (0.046)
(B)	Model in Row (A) with 1-Digit Industry Fixed Effects	**0.170** (0.037)	**0.137** (0.038)	**0.139** (0.039)	**0.077** (0.045)	**0.098** (0.047)
(C)	Model in Row (A) with 1-Digit Industry Effects and Added Career Variables	**0.163** (0.038)	**0.129** (0.039)	**0.128** (0.040)	**0.069** (0.047)	**0.088** (0.048)
(D)	Model in Row (A) with Industry Effects, Career Variables*Age Interactions	**0.169** (0.037)	**0.136** (0.038)	**0.138** (0.039)	**0.077** (0.045)	**0.098** (0.047)
(E)	Linear Probability Model (Specification Row B)	**0.0012** (0.00026)	**0.0011** (0.00032)	**0.0012** (0.00031)	**0.0006** (0.00034)	**0.0008** (0.00034)
(F)	Linear Probability Model (Spedificattion Row A) with Firm Effects	**0.0013** (0.00038)	**0.0008** (0.00050)	**0.0010** (0.00048)	**0.0006** (0.00054)	**0.0009** (0.00051)
(G)	Percentage Effect For Linear Probability Model in Row (E)	**0.194** (0.041)	**0.140** (0.027)	**0.143** (0.041)	**0.081** (0.029)	**0.103** (0.042)
(H)	Percentage Effect For Linear Model in Row (F)	**0.200** (0.059)	**0.104** (0.042)	**0.120** (0.063)	**0.075** (0.046)	**0.117** (0.063)
	Observations	553,167	402,844	392,536	334,598	291,373

Notes: Dependent variable is the log odds of death in a year between 1980 or 1987 and 2006. Deaths can occur anywhere in the United States. The entries in the tables are the coefficient on a dummy for job loss during mass-layoff. See Sullivan and von Wachter (2007) for marginal effects. Columns represent different samples, rows represent different model specifications. All models include year effects and a quartic in age as well as the indicated variables in the column "Specification." In all models, the average of quarterly earnings 1974-79 is entered in logs; the standard deviation is of the log quarterly earnings, also entered in logs. Industry dummies are for non-manufacturing goods, non-durables manufacturing, other durables manufacturing, steel manufacturing, transportation-construction-public-utilities, trade, and services. The additional "Career Variables" in rows (C) to (F) are growth in quarterly earnings during 1974-79 and the total time spent in non-employment in 1974-79. Row (D) interacts the log of average earnings and the log of the standard deviation of log earnings with five dummies for age at layoff. The firm in row (F) refers to the 1979 employer. The last row shows the number of person-year observations. Column 5 corresponds to the sample used in Jacobson, Lalonde, and Sullivan (1993), which excludes non-mass layoff separators from the control group. Standard errors are in parentheses (for rows (G) and (H), these are calculated by the delta-method).

Table 3: Coefficients on Career Variables in Extended Log-Odds of Death Model (Various Samples, Workers in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, Born 1930-59)

Work Restriction in Pennsylvania Labor Market During 1980-1986	No Work Restriction	No Work Restriction	Work At Least 3 Years	Work Every Year	Work Every Year, Exclude Non-MLF Separators [JLS Sample]
Death Follow-Up Period:	**1980-06**	**1987-06**	**1987-06**	**1987-06**	**1987-06**
	(1)	**(2)**	**(3)**	**(4)**	**(5)**
Displacement-Dummy	**0.163**	**0.129**	**0.128**	**0.069**	**0.069**
	(0.038)	(0.040)	(0.040)	(0.047)	(0.047)
Log(Average Quarterly Earnings 1974-79)	**-0.504**	**-0.516**	**-0.499**	**-0.472**	**-0.472**
	(0.055)	(0.057)	(0.058)	(0.066)	(0.066)
Log(Std. Dev. of Log Quarterly Earnings 1974-79)	**0.172**	**0.163**	**0.170**	**0.174**	**0.174**
	(0.027)	(0.028)	(0.028)	(0.032)	(0.032)
Number of Quarters in Non-Employment 1974-79	**-0.090**	**-0.090**	**-0.087**	**-0.095**	**-0.095**
	(0.025)	(0.026)	(0.026)	(0.031)	(0.031)
Growth in Quarterly Earnings 1974-79	**-0.002**	**0.008**	**0.016**	**0.015**	**0.015**
	(0.052)	(0.054)	(0.055)	(0.062)	(0.062)
1-Digit Dummies for 1979 Industry	Yes	Yes	Yes	Yes	Yes
Observations	505,316	367,890	358,660	308,345	308,345

Notes: These are coefficients on covariates included in Model C of Table 2. Please refer to Notes of Table 2 for further explanations. Standard errors are in parentheses.

Table 4: Mortality Impact of Job Displacement by Time Since Displacement, Age-Group, Industry, and Tenure at Job Loss for Different Samples (Workers in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, No Further Presence Restriction in PA Labor Market)

Restriction on Job Tenure	Tenure in 1979 at Least 6 Years			Tenure in 1979 at Least 3 Years		
Birth Cohorts	1930-1959	1920-1959	1920-1959	1930-1959	1920-1959	1920-1959
	(1)	(2)	(3)	(4)	(5)	(6)
Displacement Effect at 16+ Years After Displacement	**0.131**	**0.108**	**0.133**	**0.161**	**0.123**	**0.152**
	(0.054)	(0.034)	(0.055)	(0.05)	(0.032)	(0.05)
Added Effect for 1 Year After Displacement Year	**0.716**	**0.619**	**0.582**	**0.782**	**0.606**	**0.585**
	(0.199)	(0.105)	(0.113)	(0.176)	(0.099)	(0.106)
Added Effect for 2-3 Years After Displacement Year	**0.559**	**0.307**	**0.279**	**0.525**	**0.318**	**0.303**
	(0.147)	(0.084)	(0.091)	(0.136)	(0.078)	(0.084)
Added Effect for 4-5 Years After Displacement Year	**0.198**	**0.040**	**0.020**	**0.204**	**0.033**	**0.024**
	(0.147)	(0.082)	(0.086)	(0.135)	(0.077)	(0.081)
Added Effect for 6-10 Years After Displacement Year	**0.057**	**0.045**	**0.036**	**0.027**	**0.053**	**0.051**
	(0.094)	(0.054)	(0.057)	(0.087)	(0.051)	(0.053)
Added Effect for 11-15 Years After Displacement Year	**-0.066**	**-0.045**	**-0.046**	**-0.053**	**-0.044**	**-0.042**
	(0.081)	(0.047)	(0.048)	(0.073)	(0.044)	(0.045)
Displacement and Current Age Less or Equal 45			**0.383**			**0.220**
			(0.131)			(0.116)
Displacement and Current Age Between 46 and 55			**0.136**			**0.117**
			(0.075)			(0.066)
Displacement and Current Age Above Age 65			**-0.003**			**-0.009**
			(0.054)			(0.050)
Displacement at Age 60-69			**-0.092**			**-0.099**
			(0.041)			(0.039)
Displaced from Non-Manufacturing Job	**0.045**	**-0.065**	**-0.070**	**-0.040**	**-0.059**	**-0.066**
	(0.084)	(0.051)	(0.051)	(0.074)	(0.047)	(0.047)

Notes: Samples are workers born 1920-59 in stable jobs from 1974-79 with an employer of over 50 workers. Dependent variable is the log odds of death. Death can occur anywhere in the United States. Entries are coefficient estimates from the logit-model. All models include year fixed effects, industry fixed effects, a quartic in age, the log of average quarterly earnings in 1974-79, and the log of the standard deviation of quarterly earnings in 1974-79. The coefficient in the first row is the main effect; the excluded categories are "16 or more years after displacement" and "displaced from manufacturing job;" in addition, in columns 2, 3, 5, and 6 the excluded group is "displacement and current age between 56 and 64". Standard errors are in parentheses.

Table 5: Impact of Job Displacement on Life Expectancy by Age at Separation and Job Tenure

	Sample	Displacement Interactions Included	Age at Separation	Life Expectancy given not Displaced	Life Expectancy given Displaced	Lost Years of Life due to Displacement
(1)	Stable job 1974-79 No restrictions on earnings 1980-86; 1920-59 birth years Tenure in 1979 At Least 6 Years	Years since displacement categories; Current age categories; Displaced age GE 60; Nonmanufacturing	30	76.45	74.85	*-1.59*
			35	76.56	74.99	*-1.56*
			40	76.73	75.22	*-1.51*
			45	76.99	75.58	*-1.41*
			50	77.37	76.01	*-1.36*
			55	77.92	76.64	*-1.29*
(2)	Stable job 1974-79 No restrictions on earnings 1980-86; 1920-59 birth years; Tenure in 1979 At Least 3 Years	Years since displacement categories; Current age categories; Displaced age GE 60; Nonmanufacturing	30	76.56	74.97	*-1.59*
			35	76.67	75.10	*-1.57*
			40	76.85	75.29	*-1.56*
			45	77.11	75.58	*-1.53*
			50	77.49	76.00	*-1.50*
			55	78.05	76.62	*-1.43*

Notes: All models include log of mean earnings, log of standard deviation of log quarterly earnings, 1-digit industry dummies, and a linear age effect. The numbers labeled (1) and (2) correspond to models equivalent to Column 3 and 6 of Table 4, respectively. The numbers are based on a linear extrapolation in age for cohorts still alive. See text for further information.

Table 6: Intent-To-Treat Estimates of the Effect of Mass-Layoff at Firm Level on Mortality Pooling Movers and Stayers (Workers in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, Born 1920-59, No Further Presence Restriction in PA Labor Market)

Mass Layoff Definition:	**Year in Which Firm Employment Drops 30% Relative to Peak Employment in 1974-79**		**Year in which Firm Employment Drops 30% Relative to Employment in Year t-2**	
	Coefficient Estimate	**Rescaled for Comparison**	**Coefficient Estimate**	**Rescaled for Comparison**
	(1)	**(2)**	**(3)**	**(4)**
Panel A: Average Effect of Mass-Layoff at 1979 Employer on Hazard of Death				
1980-2006	**0.066**	**0.221**	**0.042**	**0.139**
	(0.027)		(0.023)	
Panel B: Dynamic Effect of Mass-Layoff at 1979 Employer on Hazard of Death, 1980-2002				
Effect in Year of Mass Layoff	**0.252**	**0.841**	**0.275**	**0.916**
	(0.087)		(0.101)	
Effect 1-15 Years After Year of Mass-Layoff	**0.059**	**0.196**	**0.028**	**0.092**
	(0.036)		(0.032)	
Effect 16+ Years After Year of Mass-Layoff	**0.029**	**0.095**	**0.047**	**0.158**
	(0.035)		(0.029)	
Includes Mean Earnings and Employment of 1979 Employer	**Yes**		**Yes**	
Includes Effects for 1979 Industry	**Yes**		**Yes**	

Notes: Entries are coefficients on firm-level mass-layoff dummy (Panel A) and its interactions with year since layoff (Panel B) in a logit model of the event of dying in a given year. All models include a quartic in age, year effects, the log of average quarterly earnings in 1974-1979, the log of the standard deviation of log quarterly earnings in 1974-1979, and the average of the 1979 employer's employment and quarterly earnings from 1974 to 1979 as control variables. In addition, the models include six dummies for 1979 industry. Columns 2 and 4 devide the coefficients in columns 1 and 3 by 0.3, the effect of mass-layoff at the firm level on job separations. Standard errors clustered at the level of the 1979 employer are in parentheses.

Table 7: Effect of Job Displacement on Death Controlling for Career Outcomes After Displacement (Workers in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, Born 1930-59)

Panel A: Log-Odds Model of Death as Function of Displacement and Changes in Earnings

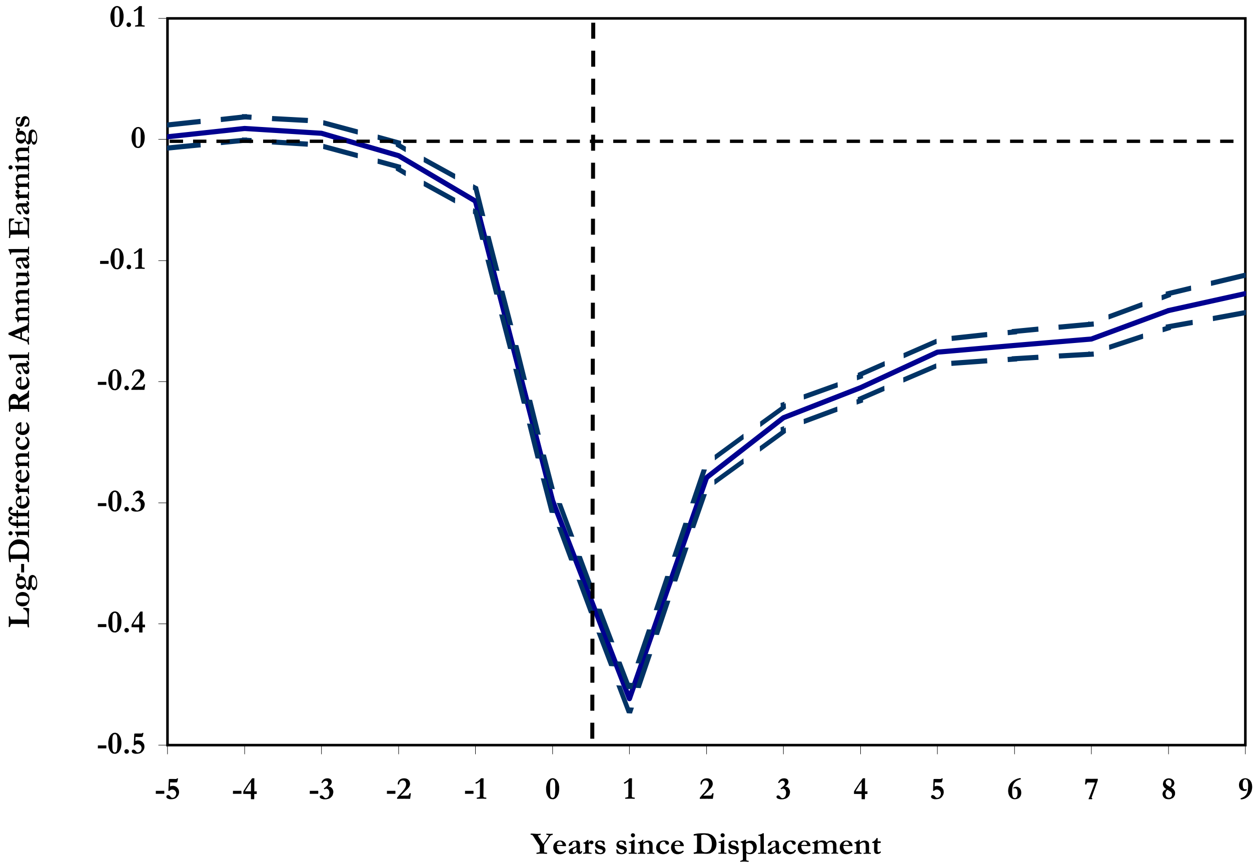
		No Work Restriction During 1980-1986		Work Every Year in PA During 1980-1986	
Model	**Coefficient in Logit Model:**	**All Workers**	**Only Displaced**	**All Workers**	**Only Displaced**
(1)	**Dummy for Job Loss During Mass-Layoff**	**-0.022**	**--**	**-0.011**	**--**
		(0.040)	--	(0.048)	--
	Percent Change in Long-Term Average Earnings	**-0.861**	**-0.672**	**-0.669**	**-0.502**
		(0.060)	(0.096)	(0.102)	(0.160)
(2)	**Dummy for Job Loss During Mass-Layoff**	**-0.041**	**--**	**-0.060**	**--**
		(0.048)	--	(0.051)	--
	Percent Change in Long-Term Average Earnings	**-0.391**	**-0.432**	**-0.524**	**-0.433**
		(0.089)	(0.150)	(0.110)	(0.186)
	Change in Log Standard Deviation of Log Earnings	**0.201**	**0.184**	**0.203**	**0.155**
		(0.026)	(0.046)	(0.028)	(0.054)
	At Least One Transition to Non-Employment	**0.012**	**-0.076**	**0.025**	**-0.054**
		(0.047)	(0.088)	(0.048)	(0.090)

Panel B: Linear Probability Models of Death on Individual Level Change in Average Earnings and Change in Average Earnings Predicted by Interaction of Displacement and Cell-Level Dummies

		No Work Restriction During 1980-1986		Work Every Year in PA During 1980-1986	
	Cells used to predict change in average quarterly earnings	**Individual Earnings Change**	**Predicted Earnings Change**	**Individual Earnings Change**	**Predicted Earnings Change**
(3)	**4 Groups of Age in 1979 Times 4 Groups of Avg. Earnings in 1974-79**	**-0.0046**	**-0.0027**	**-0.0042**	**-0.0019**
		(0.0003)	(0.0008)	(0.0006)	(0.0014)
(4)	**4 Groups of Age in 1979 Times 7 Groups of Industry in 1979**	**-0.0046**	**-0.0028**	**-0.0043**	**-0.0022**
		(0.0003)	(0.0008)	(0.0006)	(0.0014)
(5)	**10 Groups of Average 1974-79 Earnings of 1979 Employer**	**-0.0047**	**-0.0024**	**-0.0043**	**-0.0022**
		(0.0003)	(0.0009)	(0.0006)	(0.0005)
(6)	**10 Groups of Average 1974-79 Employment of 1979 Employer**	**-0.0046**	**-0.0026**	**-0.0042**	**-0.0018**
		(0.0003)	(0.0003)	(0.0006)	(0.0004)
(7)	**7 Groups of Industry in 1979 Times 4 Groups of County Unempl. in 1979**	**-0.0046**	**-0.0025**	**-0.0042**	**-0.0016**
		(0.0003)	(0.0005)	(0.0006)	(0.0009)

Notes: The models in Panel A of the table show coefficients for logit models of the annual hazard of death controlling for year and industry dummies, a quartic in age, pre-mass layoff career outcomes (log average quarterly earnings, log of standard deviation of log quarterly earnings, number of quarters in non-employment, average quarterly growth in earnings, all measured from 1974-79), as well as changes in career outcomes from 1974-79 to 1987-91 (percent change in the standard deviation of log quarterly earnings, change in the average growth rate of quarterly earnings). The models in Panel B report coefficients on linear probability models of the hazard of death on either the actual individual change in average quarterly earnings or the change in earnings predicted by interactions of cell-level dummies with mass-layoff dummy; all models control for cell-level dummies, year dummies, industry dummies, a quartic in age, pre-mass layoff career outcomes. The coefficient on the individual level earnings change varies across specifications because different cell-level dummies are included. Standard errors are in parentheses.

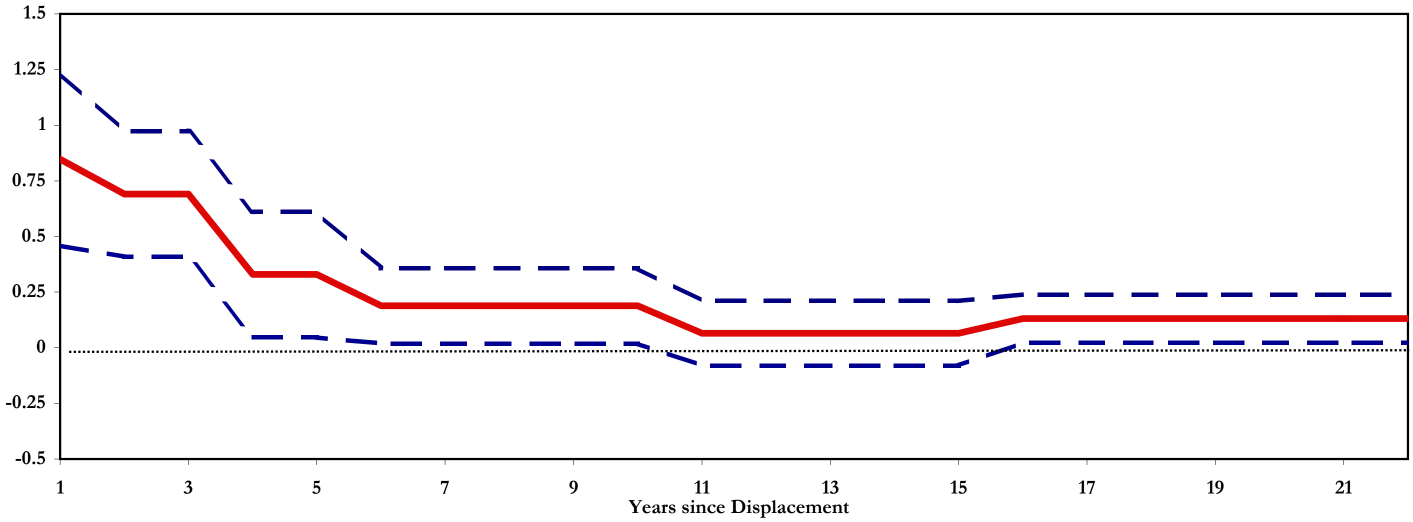
Figure 1: Estimate of the Decline in Annual Earnings Due to Job Displacement (Sample of Men in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, Born 1930-59, Work in PA Labor Force Every Year 1980-1986)



Notes: Solid line represent coefficient estimates of the interaction of year effects and displacement dummies in a regression model of log quarterly earnings including year fixed effects, person fixed effects, and a quartic for age. Two standard error bands are drawn around main effects.

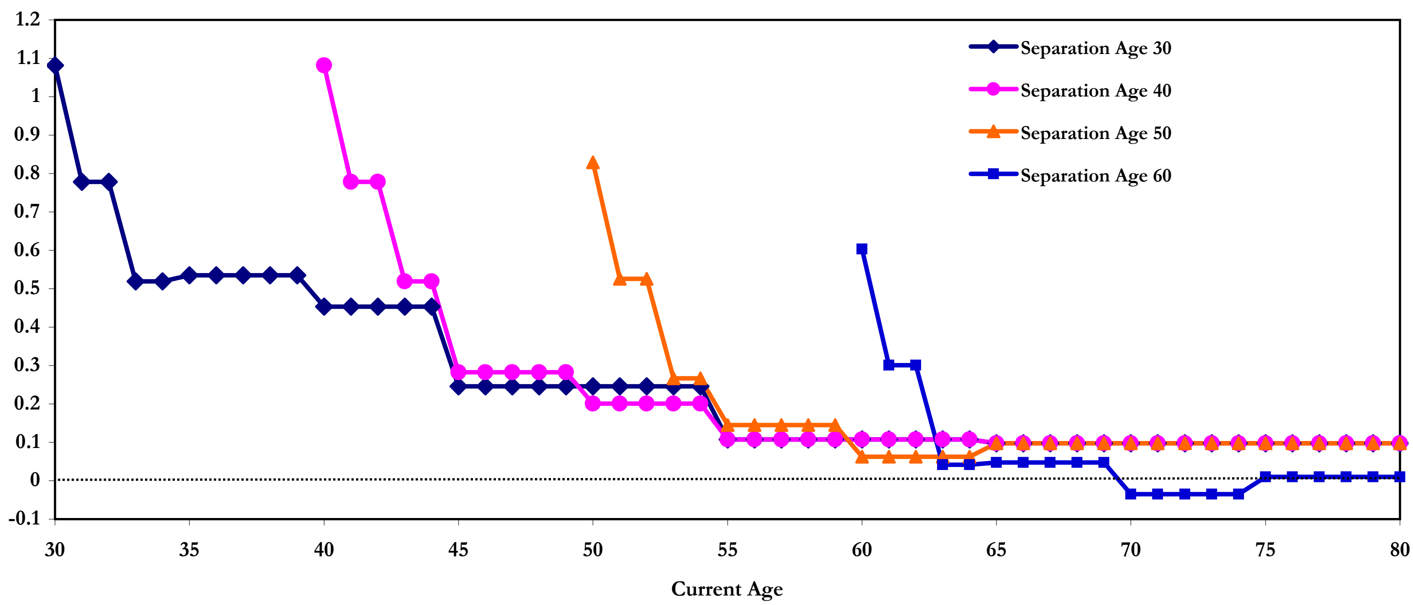
Figure 2: The Effect of Displacement on Log-Odds of Death by Years Since Displacement (Sample of Men in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50, No Further Presence Restriction in PA Labor Market)

Panel A: Effect by Years Since Displacement for Workers Born 1930-1959 (Including Two Standard Error Bands)



Notes: Solid line represents coefficients of log-odds model of mortality on years since displacement and basic other control variables. These are the main effects corresponding to Column 1, Table 4. Dashed lines represent two-standard errors bands.

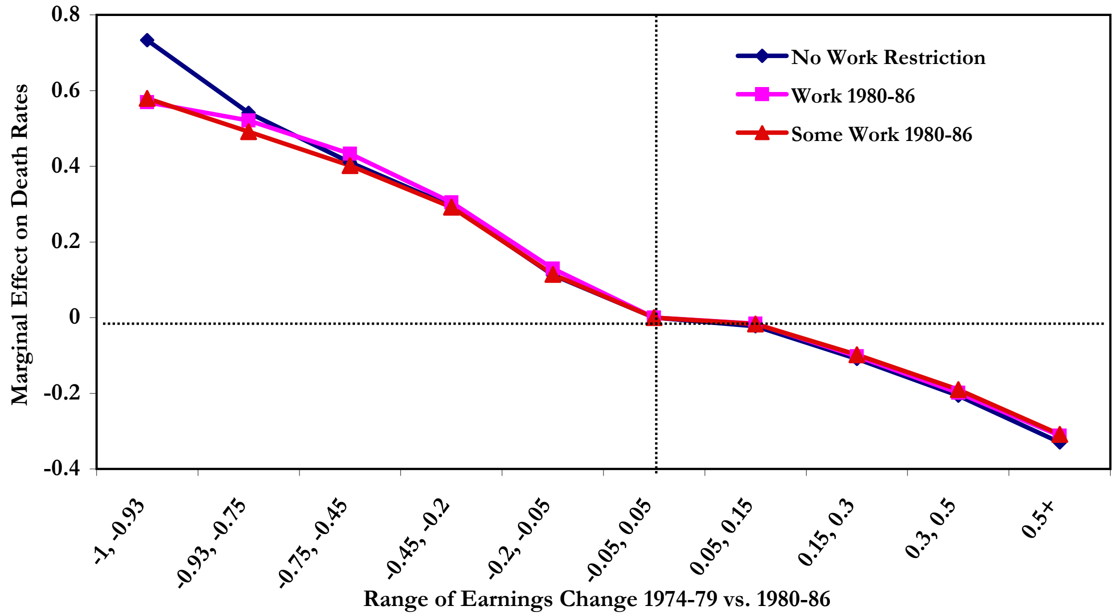
Panel B: Simulated Effect of Displacement by Current Age and Age at Displacement for Workers Born 1920-1959



Notes: The lines represent coefficients from a log-odds model of death on four dummies for current age interacted with displacement, to which dummies for years since displacement where added, as well as a dummy for whether age at displacement was 60 or greater. Coefficients are taken from Column 3, Table 4. See text for details.

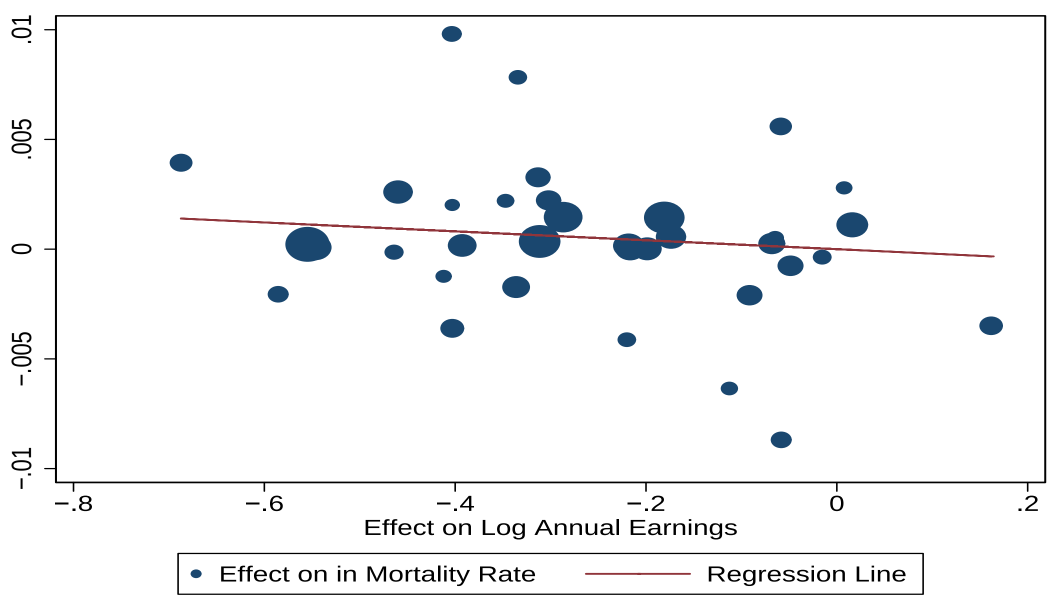
Figure 3: Mortality Rate by Size of Earnings Change from 1974-1974 to 1980-1986 at Individual Level and at Cell Level (Sample of Men in Stable Employment 1974-1979, Firm 1979 Employment ≥ 50)

Panel A: Differences in Mortality by Deciles of the Change in Average Earnings (Relative to Workers with No Change in Earnings), Alternative Degrees of Presence in PA Labor Force in 1980-86, Workers Born 1920-59



Notes: Coefficients on dummies for deciles of changes in the log of average annual earnings from 1974-79 to 1980-86 in a logit model of death. The omitted category are earnings-changes in the range [-0.05,0.05]. Other variables include year effects, a quartic in age, and the average and standard deviation of earnings 1974-79.

Panel B: Effect of Displacement on Mortality and Annual Earnings by Cells of Industry and Local Unemployment Rate in 1979 (28 Cells), Work Every Year in PA Labor Force 1980-1986, Workers Born 1930-59



Notes: Cells corresponding to model 7 in Table 7. The slope of the regression line in the figure corresponds to the coefficient in the last column of the table. The effect on annual earnings refers to the effect of displacement on changes in the log of average annual earnings from 1974-79 to 1980-86 by cell. The effect on mortality rate refers to the effect of displacement on mortality by cell. Both models include cell-level dummies. The regression line is from a regression of mortality effects on earnings effects by cell level weighted by cell size. See the text for further information.