JOB DISPLACEMENT AND MORTALITY: AN ANALYSIS USING ADMINISTRATIVE DATA*

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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 twenty 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 forty. We show that 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. This correlation remains when we examine predicted earnings declines based on losses in industry, firm, or firm-size wage premiums.

I. INTRODUCTION

A growing literature shows that displaced workers—individuals who lose their jobs 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.[1] In this

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1. See, for example, Ruhm (1991); Olson (1992); Jacobson, LaLonde, and Sullivan (1993); Gruber (1997); Stevens (1997); Chan and Stevens (2001); and Farber (2003). The Bureau of Labor Statistics defines displaced workers to be individuals who lose their main jobs 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]).

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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.[2] We then compare these displaced workers' subsequent mortality rates with 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 in 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 lasted 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 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.

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

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, which may substantially raise 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.[3] Our empirical findings are consistent with a reduction in such resources leading to reduced investments in health or chronic stress, which, 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.[4]

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%. Given the correlation of mean earnings with mortality, this 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

3. For example, a shift in the lifetime budget constraint would reduce health investments in a neoclassical 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). Although some of these factors are likely to operate in the short run as well, too many factors vary simultaneously for the effect of any single channel to be separately assessed.

4. 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 1992) but may have independent effects as well.

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.[5] However, our paper is one of the first studies to use U.S. 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.[6] Our study complements important recent studies based on European administrative data, which find mixed results on the effects of job loss on health.[7] 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, U.S. health care and labor market institutions differ substantially from those in Europe, where workers often have access to universal health insurance and where the

5. Typical estimates suggest a strong correlation between income and mortality (e.g., Deaton and Paxson [1999]). In addition, a growing literature in economics, sociology, and epidemiology has shown that unemployment and job loss correlate with the incidence of depression, low self-esteem, heart attack, and even suicide (see, e.g., Darity and Goldsmith [1996]; Burgard, Brand, and House [2005, 2007]; Gallo et al. [2006]). However, cross-sectional estimates may not represent causal effects of earnings on mortality because of reverse causality, omitted worker characteristics, and measurement error (e.g., Smith [1999]; Cutler, Deaton, and Lleras-Muney [2006]).

6. Most studies are not based on exogenous sources of variation in individuals' labor market conditions, objective measures of health and job loss, large sample sizes, a long follow-up period, or detailed pre–job loss career outcomes such as used in our empirical work. This has made it difficult to study the effect of labor market events on health free of measurement error, reverse causality, and omitted variable bias.

7. Rege, Telle, and Votruba (forthcoming) find that workers (men and women) losing their jobs in a plant downsizing during 1993–1998 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 that male workers losing their jobs in establishment closures in Sweden during 1987–1988 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. For example, Kuhn, Lalive, and Zweimueller (2007) find that job loss reduces the mental health of men in Austria, whereas Browning, Dano, and Heinesen (2006) find no such effects in Denmark.

earnings consequences of job loss typically are less severe than in the United States.[8] 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 male 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,[9] so our results likely give a good indication of the direction and at least the rough magnitude of the effects that can be expected for the typical displaced male high-tenure worker.

The next section discusses the properties of our data and introduces our econometric framework. Section III contains our main results; Section III.A presents the average effect of displacement on mortality; Section III.B 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 III.C discusses the implied reductions of life expectancy; and Section III.D summarizes our sensitivity analysis. Section IV discusses our assessment of potential mechanisms through which displacement raises mortality, and Section V concludes.

8. 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 2007). Earnings losses in Sweden have been found to be more persistent (Eliason and Storrie 2006). Income losses in Norway fall somewhere in between (Rege, Telle, and Votruba, forthcoming).

9. Earnings losses of duration and magnitude similar to those 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 and Placzek [forthcoming], and Kodrzycki [2007]), respectively, and for the entire United States during the early 1980s (von Wachter, Song, and Manchester 2009).

II. 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 SSA. It also explains how we identify displaced workers and contrasts their characteristics with those of 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 that of otherwise similar workers who are not displaced.

II.A. Data Construction and the Characteristics of Displaced Workers

Our data on workers' employment and earnings histories are derived from the UI records of the state of Pennsylvania (PA) over the period from 1974 to 1991. For a 5% sample of workers who held jobs covered by UI, we observe quarterly earnings from each PA employer, as well as the employer's industry.[10] Our data on mortality are derived from a database compiled by the SSA and cover deaths occurring 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.[11]

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

10. JLS (1993) used the same data for the period 1974 to 1986. For a detailed description of the data and their 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.

11. The SSA'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 that coverage for men is between 80% and 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 found similar results (in our empirical analysis, we also include deaths occurring in other states).

long-term jobs in the absence of mass layoffs. For these workers, displacement was likely to be unexpected and costly.[12]

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 period 1980–1986 and for whom their former firms' employment in the following year was 30% or more below its peak since 1974.[13] 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 the average, experience long-term earnings losses.[14]

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 for meaningful findings for women to be derived. Again following JLS, we restrict some of our analysis to workers born between 1930 and 1959, a group for whom retirement before the 1990s is unlikely. 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

12. Our sample is not meant to capture all job losers but maintains the focus on workers losing stable jobs that is common in the literature on job displacement (e.g., JLS [1993]; Schoeni and Dardia [2003]; Couch and Placzek [forthcoming]). 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, von Wachter, and Handwerker (2008) and von Wachter, Song, and Manchester (2009) show using administrative data that earnings losses from job displacements are substantial and long-lasting even at shorter tenure durations.

13. Hildreth, von Wachter, and Handwerker (2008) explore the issues that arise 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.

14. 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 upward. Thus, to err on the conservative side, we included them in our main sample as nondisplaced workers. We also show results based on the original JLS sample restriction.

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 nondisplaced workers would tend to bias simple estimates of the effect of displacement on mortality upward. To address this problem, following a suggestion from a referee, we drop from our samples workers who died during the years their firms suffered mass layoffs. 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 I show means for a number of worker characteristics for the full sample just described, as well as for displaced and nondisplaced workers separately. Both groups of workers were, on the average, in their late thirties, with earnings in the middle of the income distribution for the period. Displaced workers were about half a year younger, had earnings about 6% lower, and experienced slightly more quarters without earnings in the 1974–1979 base period than nondisplaced 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 industries. These patterns suggest that 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 1974–1979 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 UI. Such workers have zero reported earnings, but may, in fact, have income not covered in the PA UI system. To mitigate such concerns, the last three columns of Table I 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 nondisplaced workers in the period 1974–1979 are little affected by this restriction. However, the earnings differential for the period 1987–1991 is narrowed considerably, though it remains quite large.

Figure I 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 from 1980 to 1986

TABLE I
SAMPLE CHARACTERISTICS BY DISPLACEMENT STATUS

Work restriction in Pennsylvania labor market during 1980–1986	No work restriction			Work every year		
	All workers (1)	Displaced workers (2)	Nondisplaced workers (3)	All workers (4)	Displaced workers (5)	Nondisplaced workers (6)
Sample size	21,573	7,256	14,317	17,641	4,785	12,856
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 1974–1979)	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 1974–1979)	−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 1974–1979	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 nonemployment 1974–1979	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	8,556	10,483	7,579	8,087	9,065	7,723
	(13,944)	(16,287)	(12,479)	(13,267)	(15,018)	(12,534)
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 (nonsteel)	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)

TABLE I
(CONTINUED)

Work restriction in Pennsylvania labor market during 1980–1986	No work restriction			Work every year		
	All workers (1)	Displaced workers (2)	Nondisplaced workers (3)	All workers (4)	Displaced workers (5)	Nondisplaced workers (6)
Log(average quarterly earnings in 1987–1991)	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 1987–1991)	−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 nonemployment in 1987–1991	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 1,000 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 1,000 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 1,000 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 1,000 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 include only male workers born 1930–1959 in stable employment 1974–1979 at an employer of size fifty 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.

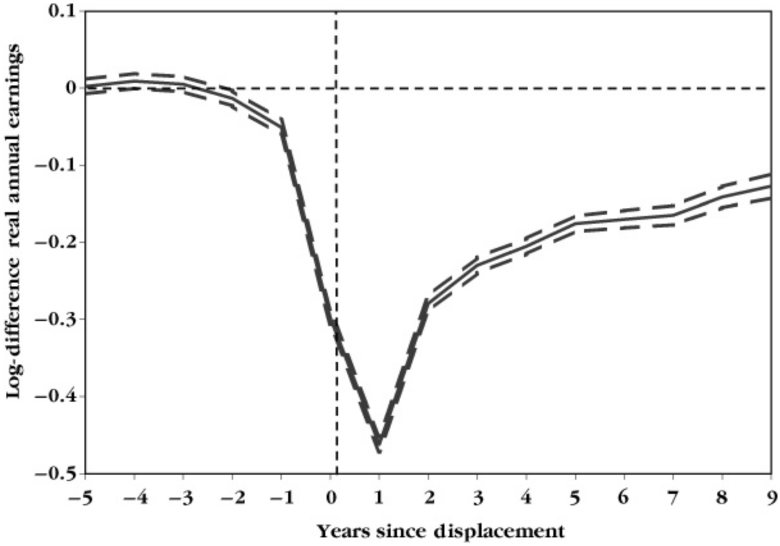

FIGURE I

Estimate of the Decline in Annual Earnings due to Job Displacement (Sample of Men in Stable Employment 1974–1979, Firm 1979 Employment ≥50, Born 1930–1959, Work in PA Labor Force Every Year 1980–1986)

Solid line represents 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.

controlling for year, age, and worker fixed effects. As in the last three columns of Table I, 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 eleven years after displacement they 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 nonemployment, industry mobility, or mobility across counties—is significant in the first two to four years after layoffs but not afterward.[15]

15. See Table 9 of our longer working paper for detailed results (Sullivan and von Wachter 2007).

The last several rows of Table I show mortality rates over a number of time periods (deaths can occur anywhere in the United States). As explained above, to avoid misclassifying dying nonseparators as displaced, we drop workers dying in the year of displacement. Not surprisingly, rates for all workers rise over time, from about four per thousand between 1987 and 1993 to more than ten per thousand 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 period 1987–1993, shortly after displaced workers lost their jobs. Indeed, during this period, displaced workers were more than 40% more likely to die as nondisplaced workers (5.151 per 1,000 versus 3.670 per 1,000). However, even twenty years later, during the period 2000–2006, 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 nondisplaced workers that are illustrated in the upper portions of the table.[16]

II.B. Main Estimation Strategy

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 $p_{it} \equiv \Pr\{\text{Death}_{it} = 1 | \text{Death}_{it-1} = 0\}$ 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.[17] 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

16. 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.

17. 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.

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.

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 SSA's death records. They also include a fourth-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 period 1974–1979 (x_i). In Section III.D, we summarize several additional robustness checks confirming that our results are not affected by selective job displacement.

III. 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 with time since displacement and other variables. Finally, we derive the implications of our estimates for life expectancy and summarize our sensitivity analysis.

III.A. Displacement and the Mortality Hazard

The first column of Table II 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 period 1980–2006. Controlling only for the mean and standard deviation of earnings during the period 1974–1979 as shown in row (1), we estimate that displacement is associated with about a 17% increase in the mortality hazard. The

TABLE II
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 1930–1959)

	No work restriction (1)	No work restriction (2)	Work at least three years (3)	Work every year (4)	Work every year, exclude non-MLF separators (JLS sample) (5)
Death follow-up period	1980–2006	1987–2006	1987–2006	1987–2006	1987–2006
(1) Baseline model with average and std. dev. of earnings in 1974–1979	0.170	0.147	0.148	0.088	0.104
	(0.036)	(0.037)	(0.038)	(0.044)	(0.046)
(2) Model in row (1) with one-digit industry fixed effects	0.170	0.137	0.139	0.077	0.098
	(0.037)	(0.038)	(0.039)	(0.045)	(0.047)
(3) Model in row (1) with one-digit industry effects and added career variables	0.163	0.129	0.128	0.069	0.088
	(0.038)	(0.039)	(0.040)	(0.047)	(0.048)
(4) Model in row (1) with industry effects and career variables*age interactions	0.169	0.136	0.138	0.077	0.098
	(0.037)	(0.038)	(0.039)	(0.045)	(0.047)
(5) Linear probability model (specification row (2))	0.0012	0.0011	0.0012	0.0006	0.0008
	(0.00026)	(0.00032)	(0.00031)	(0.00034)	(0.00034)
(6) Linear probability model (specification row (1)) with firm effects	0.0013	0.0008	0.0010	0.0006	0.0009
	(0.00038)	(0.00050)	(0.00048)	(0.00054)	(0.00051)

TABLE II
(CONTINUED)

	No work restriction (1)	No work restriction (2)	Work at least three years (3)	Work every year (4)	Work every yeasr, exclude non-MLF separators (JLS sample) (5)
Death follow-up period	1980–2006	1987–2006	1987–2006	1987–2006	1987–2006
(7) Percentage effect for linear probability model in row (5)	0.194	0.140	0.143	0.081	0.103
	(0.041)	(0.027)	(0.041)	(0.029)	(0.042)
(8) Percentage effect for linear probabiliy model in row (6)	0.200	0.104	0.120	0.075	0.117
	(0.059)	(0.042)	(0.063)	(0.046)	(0.063)
Observations	553,167	402,844	392,536	334,598	291,373

Notes. The 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 table are the coefficients 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 first column. In all models, the average of quarterly earnings 1974–1979 is entered in logs; the standard deviation is of the log quarterly earnings, also entered in logs. Industry dummies are for nonmanufacturing goods, nondurables manufacturing, other durables manufacturing, steel manufacturing, transportation–construction–public-utilities, trade, and services. The additional "career variables" in rows (3) to (6) are growth in quarterly earnings during 1974–1979 and the total time spent in non-employment in 1974–1979. Row (4) 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 (6) refers to the 1979 employer. The last row shows the number of person–year observations. Column (5) corresponds to the sample used in JLS (1993), which excludes nonmass layoff separators from the control group. Standard errors are in parentheses (for rows (7) and (8), these are calculated by the delta-method).

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 (2)–(4) has very little effect on the estimate.[18] Row (5) shows the estimate for a linear probability model version of row (2), whereas row (6) shows results from a linear probability model that includes firm fixed effects. When expressed as percentages of the baseline hazard, these latter two estimates are modestly higher than 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 that firms or sectors with high average layoff rates provide less healthy career environments or attract less healthy workers.[19]

The remaining columns of Table II 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 in this way lowers the estimates to the 10%–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%–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

18. If we include only year and age effects for our most general sample in column (1), we obtain a displacement effect of 0.227 (0.0354); if we include only log average earnings as an additional control variable, we obtain 0.2005 (0.0355).

19. 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.

11%.[20] Overall, we consider the estimates shown in Table II as indicating a reasonable degree of robustness to the set of additional control variables and the sample of workers included in the estimation.

Table III displays the other coefficients in the models of row (3) of Table II, which, as we discuss in Section IV, 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.[21] The elasticity of mortality with respect to the standard deviation of the logarithm of quarterly earnings is estimated to be around 0.17, indicating that higher earnings variability tends to increase mortality. Holding average earnings and earnings variability constant, an additional quarter of nonemployment due to sick leave or temporary layoffs in the base period reduces mortality by about 9%, an effect that is, perhaps, consistent with the findings of Ruhm (2000). Conditional on the other variables, the earnings growth trend from 1974 to 1979 has little effect on mortality.

III.B. Mortality Effects by Year since Layoff, Age, and Job Tenure

The results in Table II suggest that the immediate impact of displacement on mortality differs from the long-run effect. To explore this pattern further, column (1) in Table IV breaks up the effect of displacement on death by year since layoff for our most general sample (as in column (1) in Table II). Row (1) 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 nonmanufacturing 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

20. This suggests that non-mass-layoff separators experience mortality increases as well, which is 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 nontrivial earnings losses (see also von Wachter, Song, and Manchester [2009]).

21. 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 Sullivan and von Wachter (2009) and in Appendix 1 of our longer working paper.

TABLE III

COEFFICIENTS ON CAREER VARIABLES IN EXTENDED LOG-ODDS OF DEATH MODEL (VARIOUS SAMPLES, WORKERS IN STABLE EMPLOYMENT 1974–1979, FIRM 1979 EMPLOYMENT ≥50, BORN 1930–1959)

Work restriction in Pennsylvania labor market during 1980–1986	No work restriction (1)	No work restriction (2)	Work at least three years (3)	Work every year (4)	Work every year, exclude non-MLF separators (JLS sample) (5)
Death follow-up period	1980–2006	1987–2006	1987–2006	1987–2006	1987–2006
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 1974–1979)	−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 1974–1979)	0.172	0.163	0.170	0.174	0.174
	(0.027)	(0.028)	(0.028)	(0.032)	(0.032)
Number of quarters in nonemployment 1974–1979	−0.090	−0.090	−0.087	−0.095	−0.095
	(0.025)	(0.026)	(0.026)	(0.031)	(0.031)
Growth in quarterly earnings 1974–1979	−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 (3) of Table II. Please refer to notes to Table II for further explanations. Standard errors are in parentheses.

TABLE IV
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 six years			Tenure in 1979 at least three years		
Birth cohort	1930–1959 (1)	1920–1959 (2)	1920–1959 (3)	1930–1959 (4)	1920–1959 (5)	1920–1959 (6)
Displacement effect 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 than or equal to 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)

TABLE IV
(CONTINUED)

Restriction on job tenure	Tenure in 1979 at least six years			Tenure in 1979 at least three years		
Birth cohort	1930–1959 (1)	1920–1959 (2)	1920–1959 (3)	1930–1959 (4)	1920–1959 (5)	1920–1959 (6)
Displacement and current age above age 65			−0.003 (0.054)			−0.009 (0.050)
Displacement at age 60–69			−0.092 (0.041)			−0.099 (0.039)
Displaced from nonmanufacturing job	0.045 (0.084)	−0.065 (0.051)	−0.070 (0.051)	−0.040 (0.074)	−0.059 (0.047)	−0.066 (0.047)

Notes. Samples are workers born 1920–1959 in stable jobs from 1974–1979 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 1974–1979, and the log of the standard deviation of quarterly earnings in 1974–1979. 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.

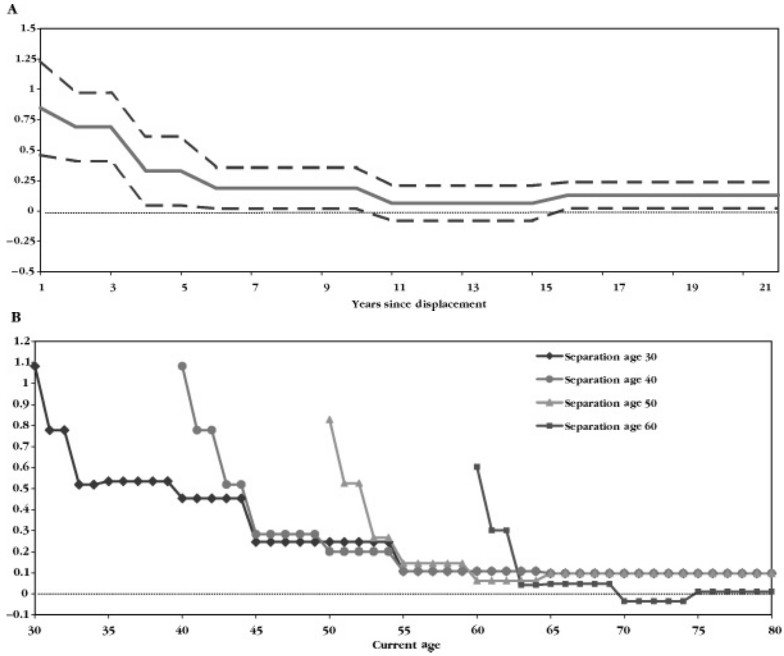

FIGURE II

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)

(A) Effect by years since displacement for workers born 1930–1959 (including two standard error bands). 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 IV. Dashed lines represent two-standard-errors bands. (B) Simulated effect of displacement by current age and age at displacement for workers born 1920–1959. 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 were added, as well as a dummy for whether age at displacement was sixty or greater. Coefficients are taken from column (3), Table IV. See text for details.

displacement on mortality at different years since displacement, the coefficients on the interactions have to be added to the main effect in row (1).[22] We see large percentage increases immediately after job displacement. The effect remains high for the first five years after job loss, 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 II, which plots the point

22. For example, for a displaced worker two to three years after layoff, the effect of displacement on mortality would be $0.131 + 0.559 = 0.69$.

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 II and III, column (1) of Table IV 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 lives (up to age 86).[23] 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 (ages 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 aged 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, because 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 [2008]; Black, Daniel, and Sanders [2002]). Younger and middle-aged workers do not have access to similar mechanisms to smooth long-term earnings losses. Moreover, as further discussed in Section IV, for these workers the reduction in lifetime earnings is larger because earnings losses accrue over a longer period.[24]

23. For men of the birth cohorts in our samples at ages forty to fifty, average life expectancy is about 70–75 (National Center for Health Statistics 2006, Table 11).

24. Another possibility is that more frail displaced workers die first, reducing the gap in mortality rates between displaced workers and nondisplaced workers. Such dynamic selectivity would lead us to understate the effect of displacement on mortality as workers age. However, because the number of deaths is small relative to the overall population at risk of death, the average underlying health of the

As workers age, the total effect of displacement on mortality is determined by the sum of the long-run mortality effect in row (1) 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 two to three 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 II. The entries in the figure are obtained by summing up the relevant coefficients in column (3) of Table IV.[25] 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 sixteen years after layoff for all age groups (the effect in row (1) of Table IV). Only for workers displaced after age sixty is the long-run effect close to zero.[26]

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 IV 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, because the lower tenure requirement tends to draw younger workers into the sample of displaced workers, and we find that 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 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.

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.

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

26. The figure also shows that for workers aged fifty 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, which 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.

III.C. The Reduction in Life Expectancy because of Job Loss

Because 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 V, we present a range of estimates of losses in life expectancies for alternative samples and ages.

Because 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 between laid-off workers and the control group past our observation window.[27] 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 remains so within the groups of our sample, this is a plausible assumption.[28] 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 1974–1979 working in nonmanufacturing industries in 1979.

Because there are large increases in mortality right after layoff, Table V is based on estimates of survival curves that take into account the dynamic response in mortality found in Table IV. Because the effect of displacement status also differs by age, we use our most general specification in Table IV to calculate life expectancy. The last column of Table V shows that losses in life expectancy are larger for workers losing their jobs in their thirties and forties. Life expectancy of workers displaced in their

27. 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 that our results are not driven by differences in extrapolated survival probabilities outside our sample period.

28. Because extrapolation of a quartic polynomial in age can be unstable, to calculate life expectancies we worked with a linear age specification for calculating differences in life expectancy. We find that 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 that it did not significantly alter our results.

TABLE V
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 1974–1979; no restrictions on earnings 1980–1986; 1920–1959 birth year; tenure in 1979 at least six years	Years since displacement categories	30	76.45	74.85	−1.59
	Current age categories	35	76.56	74.99	−1.56
	Displaced age $\geq$60	40	76.73	75.22	−1.51
	Nonmanufacturing	45	76.99	75.58	−1.41
		50	77.37	76.01	−1.36
		55	77.92	76.64	−1.29
(2) Stable job 1974–1979; no restrictions on earnings 1980–1986; 1920–1959 birth year; tenure in 1979 at least three years	Years since displacement categories	30	76.56	74.97	−1.59
	Current age categories	35	76.67	75.10	−1.57
	Displaced age $\geq$60	40	76.85	75.29	−1.56
	Nonmanufacturing	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, one-digit industry dummies, and a linear age effect. The rows labeled (1) and (2) correspond to models equivalent to columns (3) and (6) of Table IV, respectively. The numbers are based on a linear extrapolation in age for cohorts still alive. See text for further information.

fifties 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.[29] In our longer working paper (Sullivan and von Wachter 2007), we confirm similar orders of magnitude for samples restricted to workers who 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 4%. 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 one-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 the cost of job loss for displaced workers may be substantially underestimated by traditional summary measures such as the loss in lifetime earnings.[30]

III.D. Sensitivity Analysis: Pooling Displaced and Nondisplaced 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

29. Because 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 IV.

30. The loss in the present discounted value (PDV) of earnings is a sufficient statistic for the cost of job loss only if long-term health is exclusively an outcome of optimally chosen inputs given a lifetime budget constraint. In this case, the observed reduction of lifetime would represent the optimal response to the decline in resources following a job loss. 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 that are directly affected by job loss, such as social status. In that case, to obtain the total costs of job displacements, at least part of the monetary value of the direct effect of layoffs on mortality should be added to the PDV of earnings losses.

PA 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 either were 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 nondisplaced 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. Because 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.[31]

The results shown in Table VI 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 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.[32]

To compare the magnitudes of the estimates in Table VI to those in earlier tables, columns (2) and (4) show estimates divided by 0.3, which is approximately the effect of mass layoff on

31. 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).

32. Our results confirm that the timing of the shock at the firm level appears better captured by a sudden drop in employment. In the 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, because an individual's job separation always constitutes a distinct treatment.

TABLE VI
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 1920–1959, NO FURTHER PRESENCE RESTRICTION IN PA LABOR MARKET)

Mass layoff definition:	Year in which firm employment drops 30% relative to peak employment in 1974–1979		Year in which firm employment drops 30% relative to employment in year $t-2$	
	Coefficient estimate (1)	Rescaled for comparison (2)	Coefficient estimate (3)	Rescaled for comparison (4)
Panel A: Average effect of mass layoff at 1979 employer on hazard of death				
Average effect in 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, 1980–2006				
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) divide 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.

job mobility.[33] For either definition of firm-level mass layoff, the rescaled long-run effects of displacement on mortality shown in the bottom of Panel B in Table VI are of the same order of magnitude as the estimates shown in row (1) of Table IV.[34] Table VI also suggests a large effect of displacement on mortality in the year of layoff. This suggests that dropping separators who die in the year of layoff in our main analysis may lead us to underestimate the full effect of displacement on mortality shown in Table II. Correspondingly, row (1) of Table VI shows that the average effect of displacement on the mortality hazard, properly rescaled, is somewhat 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 the Appendix and further discussed 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.[35]

IV. 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 II roughly parallel the short- and long-run effects of displacement on earnings shown in Figure I and reported in JLS and elsewhere. In the short run, displacement is associated not only with a sharp drop in mean earnings, but also with increased unemployment, job, region, and

33. 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.

34. Note that the identification strategy differs for the two sets of estimates; in Table VI, the effect of the firm level event on separators and nonseparators gets *added*, whereas in the estimates in Table IV the estimates result from *subtracting* the effect on nonseparators from that of separators.

35. 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 predisplacement occupation, health status, and demographics does not affect their estimates of the impact of job loss during establishment closures during 1986–1987 on health in Sweden (see the second and third sets of estimates in their Table VI).

industry mobility, as well as high earnings instability.[36] 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. Our empirical findings are consistent with a reduction in resources and increased instability leading to reduced investments in health and chronic stress that lead to a smaller, but longer term increase in the mortality hazard.[37]

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 III. In our PA data, displacement reduces the mean of long-run earnings by 15%–20%. Taken at face value, the estimated −.5 correlation of average earnings with mortality would imply that we expect an increase in mortality of about 7.5%–10% for workers with high job 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 IV, about 0.1 to 0.15, could be explained by the observed declines in average earnings. Similarly, in the medium to long run, 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 III), this implies an increase in the probability of dying of about 3.2%. Although 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

36. An extensive discussion of these results is contained in our longer working paper version (Table 9, Sullivan and von Wachter [2007]).

37. As noted previously, there are other potentially relevant channels to which our data do not speak directly, such as the loss of health insurance or the role of the family environment. Some of these channels may be associated with earnings losses—for example, 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.

and face reduced exposure to workplace and driving accidents.[38] Although this channel may be present in our sample as well,[39] on balance, the health of job losers we study did not benefit from short-term employment reduction. This may be because in the short run, when employment reduction is more frequent, job loss also involves very large earnings losses and a stressful adjustment period involving multiple job changes, including changes in industry or location. In the long run, 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.

Overall, these considerations suggest that 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.[40] 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.

IV.A. 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

38. 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 television). They do not find that the unemployed spend more time on personal care (which includes health-related self-care) or sports (their Table 3). They confirm findings from the previous literature that the unemployed are more unhappy or sad on the average (their Table 4).

39. In fact, we do find a beneficial effect of lower employment in 1974–1979 on mortality thereafter (Table III). However, spending time in nonemployment after displacement has no statistically significant effect on mortality (Table VII). 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.

40. Reverse causation is less of a problem, because average earnings and earnings variability are measured in 1974–1979, whereas death is measured from 1987 to 2006 or from 1980 to 2006. 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. Were this result to apply to our sample of high-attachment workers (who are likely of better health than the sample of older workers studied by Lindahl), the predicted role of earnings in explaining the mass-layoff effects is reduced by about a third.

the size of earnings losses at job displacement to the long-term increase in the hazard of death. This is shown in Figure III, 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.[41] On average, those workers who have more substantial earnings losses also experience larger long-term increases in mortality hazard.

We also directly included the long-term earnings change for both displaced workers and the comparison group as a control variable in our main logistic model. The results suggest a strong correlation between the change in average earnings and the long-term mortality hazard (model (1), Panel A, Table VII). 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), Panel A, Table VII). 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 III. However, we do not interpret these results as necessarily indicating causal channels running from earnings losses or earnings variability to mortality, because there may be omitted variables driving both earnings losses and mortality increases, such as differential increases in depression in response to layoffs.

IV.B. 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

41. Specifically, the figure shows coefficients on dummies for deciles of changes in the log of average annual earnings from 1974–1979 to 1980–1986 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 1974–1979. To maximize sample sizes, the figure shows results based on the sample that includes older workers (see Table IV). The different lines correspond to different restrictions on presence in the Pennsylvania labor market during the period 1980–1986. All but the coefficients on the dummies [−0.2, −0.05], [0.05, 0.15], and [0.15, 0.3] are statistically significantly different from zero.

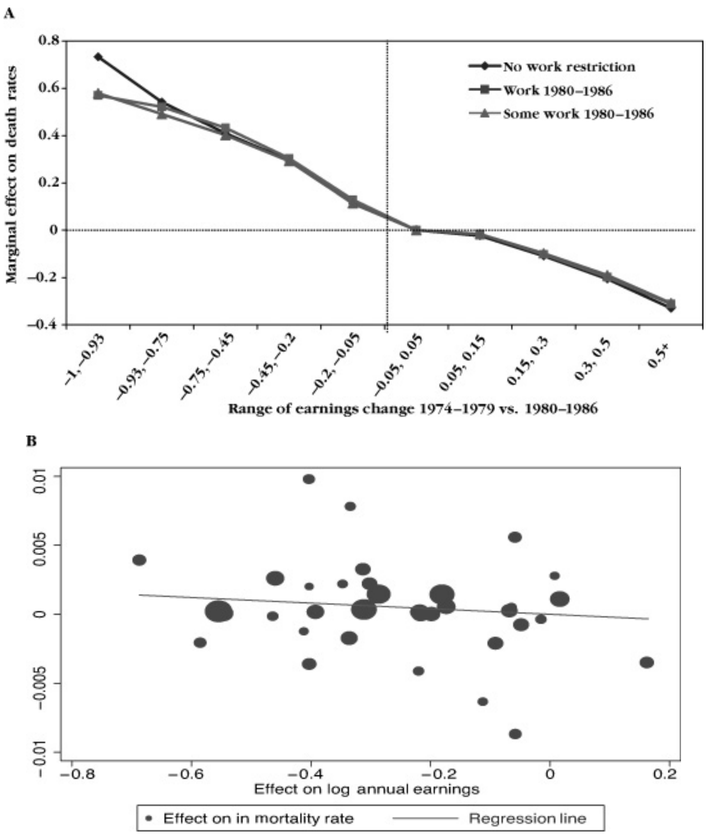

FIGURE III

Mortality Rate by Size of Earnings Change from 1974–79 to 1980–1986 at Individual Level and at Cell Level (Sample of Men in Stable Employment 1974–1979, Firm 1979 Employment $\geq$50)

(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 1980–1986, workers born 1920–1959. Coefficients on dummies for deciles of changes in the log of average annual earnings from 1974–1979 to 1980–1986 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 1974–1979. (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 1930–1959. Cells corresponding to model (7) in Table VII. 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 1974–1979 to 1980–1986 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.

TABLE VII
EFFECT OF JOB DISPLACEMENT ON DEATH CONTROLLING FOR CAREER OUTCOMES AFTER DISPLACEMENT (WORKERS IN STABLE EMPLOYMENT 1974–1979, FIRM 1979 EMPLOYMENT $\geq$50, BORN 1930–1959)

		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
	Panel A: Log-odds model of death as function of displacement and changes in earnings				
(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 nonemployment	0.012	−0.076	0.025	−0.054
		(0.047)	(0.088)	(0.048)	(0.090)

TABLE VII
(CONTINUED)

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

Cells used to predict change in average quarterly earnings	No work restriction during 1980–1986: Individual earnings change	No work restriction during 1980–1986: Predicted earnings change	Work every year in PA during 1980–1986: Individual earnings change	Work every year in PA during 1980–1986: Predicted earnings change
(3) Four age groups in 1979 times four groups of avg. earnings in 1974–1979	−0.0046	−0.0027	−0.0042	−0.0019
	(0.0003)	(0.0008)	(0.0006)	(0.0014)
(4) Four age groups in 1979 times seven industry groups in 1979	−0.0046	−0.0028	−0.0043	−0.0022
	(0.0003)	(0.0008)	(0.0006)	(0.0014)
(5) Ten groups of average 1974–1979 earnings of 1979 employer	−0.0047	−0.0024	−0.0043	−0.0022
	(0.0003)	(0.0009)	(0.0006)	(0.0005)
(6) Ten groups of average 1974–1979 employment of 1979 employer	−0.0046	−0.0026	−0.0042	−0.0018
	(0.0003)	(0.0003)	(0.0006)	(0.0004)
(7) Seven industry groups in 1979 times four 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 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 nonemployment, average quarterly growth in earnings, all measured from 1974 to 1979), as well as changes in career outcomes from 1974–1979 to 1987–1991 (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, and 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.

literature suggests that there are systematic differences in the effects of job loss on earnings,[42] 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]).[43] Because at least part of the loss in earnings for workers leaving high-paying industries 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. 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.[44]

The results suggest that even when we use cell-level variation, the effect of earnings losses on mortality is economically and statistically significant. Panel B of Table VII 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 effect of earnings losses on mortality at the cell level is 40%–50% of the effect at the individual level for the samples with no or a low work requirement. The cell-level effect is somewhat smaller but still statistically and economically significant for the high-attachment sample, yet standard errors increase as

42. See, for example, Kletzer (1989), JLS (1993), Neal (1995), and Farber (2003).

43. 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]).

44. 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).

well. Our findings are quite similar for the different definitions of cells shown in the table (models (3)–(7)).

Panel B in Figure III displays the corresponding cell-level averages and a linear regression line for the high-attachment sample for the results of model (7) in Table VII. The figure displays a clear negative and relatively precise association between earnings losses and mortality increases at the cell level, although there is an important degree of variation left. Overall, the results confirm that earnings losses after job displacement appear to be strongly correlated with increases in mortality rates.[45] Because 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.

IV.C. 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 in a mass layoff by age group (Sullivan and von Wachter, 2007).[46] We then compared losses in life expectancy taken from Table V with the corresponding PDV of earnings losses by age group. The correlation is strong, monotonic, and numerically large; as the percentage loss in the PDV of earnings doubles for thirty- to forty-year-olds relative to fifty- to sixty-year-olds, life expectancy declines by about 25%. Young workers have not only

45. The mean squared error of the group-level regressions is a test statistic for overidentification 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 overidentifying 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.

46. To do so, we allowed both the short- and long-term effects of job loss on earnings to vary by either ten-year or five-year age groups. Because 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 six and eleven after a job loss, and eventually stays fixed at zero. The resulting values are robust to alternative specifications. The percentage decreases in the present discounted value of earnings of our preferred specification for the age groups in Table V are, going from youngest to oldest, 12.3%, 10.8%, 9.2%, 7.4%, 5.6%, and 3.8%, respectively.

higher average mortality increases after a job loss (Table IV), but also the largest losses in remaining lifetime (Table V) and in lifetime earnings. Thus, it is not the oldest workers who are most affected, but those at prime working age who are exposed to the negative consequences of job loss over a longer period of time.

Overall, we interpret the results in Section IV as suggesting that at least for job losses involving earnings declines as dramatic as those in PA 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 include stress, adjustment costs, and other factors correlated with both long-term earnings declines and mortality.

V. Conclusion

This paper uses administrative data covering over fifteen years of quarterly earnings and employer records matched to information on date of death to study the effects of job displacement on mortality of high-seniority male workers losing their jobs in PA in the early to mid-1980s. 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 forces. 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 twenty 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 high 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 show that these effects can be large. A worker displaced in mid-career can expect to live about one and one-half years less than a nondisplaced 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 noneconomic factors such as stress, self-worth, and happiness. Yet they suggest that 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. Similarly, although the experience of displaced workers has been found to be similar in other states and time periods, it is important to replicate our study of male workers displaced in PA in the 1980s for other regions and time periods, and for women.

Finally, our results are not in conflict with recent work suggesting that mortality declines during recessions, possibly because of healthier lifestyles and a reduction in accidents related to work or commuting (Ruhm 2000). First, although 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.[47] Second, Ruhm (2000) focuses on fluctuations in mortality that are contemporaneous with cyclical fluctuations in economic activity, whereas 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

47. 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 by employed workers or to changes in employment rates of those with less strong job attachment.

income, without a corresponding reduction in work activity. Thus, the workers we study, although having fewer lifetime resources, did not enjoy the increases in leisure, healthier lifestyles, or reductions in accidents that may explain Ruhm's results.

APPENDIX

MORTALITY RATES IN DIFFERENT PERIODS BY DISPLACEMENT STATUS AND BY SIZE OF EMPLOYMENT DROP OF 1979 EMPLOYER

Range of years	All workers	Same firm 1974 to 1986	Non–mass layoff separators	Displaced 30%–60% below peak	Displaced 60%–90% below peak	Displaced more than 90% below peak
			Panel A: No work restriction in 1980–1986			
1987–2006	6.764	6.013	7.253	7.191	7.764	8.449
	(0.143)	(0.191)	(0.362)	(0.384)	(0.435)	(0.640)
1987–1991	4.167	3.446	4.334	5.012	5.132	5.518
	(0.181)	(0.234)	(0.451)	(0.516)	(0.569)	(0.830)
1992–1996	7.407	6.790	7.837	7.520	8.033	9.677
	(0.227)	(0.308)	(0.571)	(0.596)	(0.671)	(1.039)
1997–2006	10.815	9.639	12.163	11.129	12.847	11.892
	(0.427)	(0.570)	(1.108)	(1.130)	(1.324)	(1.803)
			Panel B: Work every year 1980–1986			
1987–2006	6.343	6.013	6.682	6.219	7.319	7.645
	(0.152)	(0.191)	(0.434)	(0.444)	(0.509)	(0.733)
1987–1991	3.745	3.446	3.762	4.134	4.798	4.202
	(0.189)	(0.234)	(0.526)	(0.583)	(0.664)	(0.875)
1992–1996	6.994	6.790	6.990	6.680	7.639	8.790
	(0.242)	(0.308)	(0.673)	(0.698)	(0.789)	(1.191)
1997–2006	10.347	9.639	12.211	9.581	12.010	12.351
	(0.458)	(0.570)	(1.383)	(1.298)	(1.541)	(2.205)

Notes. Deaths per thousand per year. Standard errors in parentheses. Panel A: Displaced workers left jobs in a year in which their former firms' employment was 30% or more below its 1974–1979 peak. Panel B: Displaced workers left jobs in a year in which their former firms' employment was 30% or more below its 1974–1979 peak; nondisplaced workers remained at their 1979 firms through 1986.

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