JOB LOSS AND HEALTH IN THE U.S. LABOR MARKET*

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While U.S. unemployment rates remain low, rates of job loss are high and rising. Job loss is also becoming increasingly common in more advantaged, white-collar occupations. This article is concerned with how these patterns impact the health of U.S. workers. Drawing on recent data from the U.S. Panel Study of Income Dynamics, I find that job loss harms health, beyond sicker people being more likely to lose their jobs. Respondents who lost jobs but were reemployed at the survey faced an increased risk of developing new health conditions; they were not, however, more likely to describe their health in negative terms. This suggests that recent job "churning" within the United States (i.e., high rates of job loss but low unemployment) may impact certain health outcomes but not others. I find no evidence that the health consequences of job loss differ across white- and blue-collar occupations, although health-related selection out of jobs appears stronger within the blue-collar category.

Although the United States has managed to keep unemployment rates consistently low over the past several years (varying between 5% and 6% since 1997), rates of job loss are considerably higher and rising (from about 8.5% to 12% since 1997; Farber 2005; U.S. Department of Labor 2005). Recent decades have also witnessed an increase in white-collar job loss and unemployment. As professional and managerial jobs have become increasingly vulnerable to downsizing, higher socioeconomic groups are experiencing increased job instability, and the risk of job loss is becoming more equally distributed by socioeconomic status (Farber 2005). This article addresses how these labor market patterns impact the health of U.S. workers.

For decades, it has been documented that socioeconomic shocks, such as job loss, are associated with poor health (Beal and Nethercott 1987; Bjorklund 1985; Catalano and Dooley 1983; Catalano et al. 1993; Jahoda, Lazarsfeld, and Zeisel 1971 [1933]; Korpi 2001; Wadsworth, Montgomery, and Bartley 1999). And for decades, these results have remained controversial, raising questions about whether they reflect the health consequences of socioeconomic shocks, or the fact that sicker people are more likely to suffer a shock. It is important to revisit the potential health effects of job loss within a more recent socioeconomic/labor market context. Increases in U.S. income inequality have been well documented, and there is evidence that health disparities have increased in accordance with this (Deaton and Paxson 1998; Elo and Preston 1996; Pappas et al. 1993). Less well publicized, however, have been parallel increases in economic insecurity, which may also have implications for health. According to Hacker (2006), between 1970 and 2002, Americans' chances of losing 50% or more of their income in a given year increased from 7% to approximately 17%. This trend was driven by several different factors (such as health shocks, divorce, and bankruptcy), but primary among these factors was increasing job instability.

In this article, I use recent data from the U.S. Panel Study of Income Dynamics (PSID) to estimate the effects of job loss on health, reducing the risk of selection bias by first isolating job losses that resulted from establishment closures, and then focusing on specific

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health conditions that should be the most sensitive to a recent stressor like job loss. I conclude by considering whether job loss effects differ for white-collar (i.e., professional and managerial) and blue-collar (i.e., operative and labor) workers.

A "CHURNING" LABOR MARKET: IMPLICATIONS FOR HEALTH?

The U.S. labor market has been "churning" as of late; international trade and competition have continued to increase, and the number of mergers, restructurings, and downsizings has risen. In discussions of these trends, it is frequently reasoned that American companies need to be free to merge, restructure, and downsize so they can adapt to changing demands and remain competitive in a global market. In terms of employment, this suggests companies should be free to eliminate old, "dead-weight" jobs that are no longer functional in order to make way for new jobs that are more appropriate for a given economic climate. (For reviews of these issues, see Buchell, Ladipo, and Wilkinson 2002; Hacker 2006; and Uchitelle 2006.)

An economic strategy of high flexibility and little regulation seems to have mixed implications for workers. Allowing companies freedom to merge, restructure, and downsize obviously increases job instability, but giving companies flexibility to lay off workers also appears to make U.S. companies more likely to hire during periods of economic growth. Since the beginning of the 1980s, the United States has managed to keep unemployment rates quite low relative to, say, its European counterparts with more regulated labor markets (e.g., France and Italy; Ladipo and Wilkinson 2002). But, despite low unemployment, job loss rates have remained high and—in line with the logic of "job churn"—the gap between job loss and unemployment has increased. In the 1981–1983 period, the job loss rate was 3.5 percentage points higher than the unemployment rate; by the 2001–2003 period, that difference had grown to 7 percentage points (Farber 2005).

Drawing on data from 1999 through 2003, I examine the individual-level consequences of job loss as a means for investigating links between the labor market and population health. If job loss harms workers' health, independently of selection, it would appear that an unregulated labor market and high rates of displacement increase the disease burden among American workers. Beyond obvious implications for population health, this could also have economic consequences. While a lack of regulation may increase economic efficiency in various ways, if the job instability that goes along with this harms workers' health, there is also likely to be a counteractive effect that decreases productivity.

Job Loss and Health

There are several reasons to anticipate that job loss is hazardous to health. Job loss is a major social stressor that may simultaneously disrupt many dimensions of socioeconomic status (e.g., income, occupational standing, wealth, family life, and social connections). Following a job loss, annual earnings can decline between 25% and 45%, and families frequently have to spend down assets to smooth consumption (Jacobson, LaLonde, and Sullivan 1993; Ruhm 1991; Stephens 2003). It has long been recognized that occupations grant (or deny) social prestige (Weber 1999 [1922]), so job loss can further pose a major shock to one's social status. Additionally, job loss may disrupt social connections and communities as the families of displaced workers frequently relocate to find less expensive housing or better local job markets (Yeung and Hofferth 1998).

Several studies have shown that workers are in worse health after losing a job (see, e.g., Catalano et al. 1993; Dooley, Catalano, and Wilson 1994; Gallo et al. 2000; Weich and Lewis 1998). These analyses make a strong case; they all used sophisticated methods and incorporated relevant control variables. However, these studies could not overcome the possibility of upward bias because workers who were sicker or otherwise disadvantaged *in ways that remained unmeasured* were disproportionately losing their jobs. Underlying health problems that may go unmeasured in most social science data are likely to make someone a less productive worker (e.g., more absenteeism, slower work pace) and consequently increase one's risk of being fired or laid off.

Several authors have tried to isolate the effects of job loss with a quasi-experimental strategy that tracks the health of workers following a plant closure. In the case of a plant closure, virtually all the employees are let go, so there should be little health-related selection out of jobs (Gibbons and Katz 1991). Many plant closure studies show that workers' health declines following a job loss (Arnetz et al. 1991; Beal and Nethercott 1987; Gore 1978; Iversen, Sabroe, and Damsgaard 1989; Kasl and Cobb 1970; Kessler, House, and Turner 1987). This is strong evidence for a causal effect. However, it is not clear how far these results can be generalized. Studies of specific plant closures operate at the community level and are limited to particular cases of blue-collar production job loss. Estimates from these circumstances may not extend to the larger U.S. labor market. Most notably, they may not apply to the case of white-collar job loss, which, as discussed, has become more frequent. In the following analysis, I draw on the logic of plant closure studies by focusing on job losses that resulted from the closure of an entire worksite. But working with nationally representative data allows me to include more varied workplace closures involving a range of occupations.

White- and Blue-Collar Job Loss and Health

Although blue-collar workers continue to face a higher rate of job loss and unemployment, the gap between white- and blue-collar job loss/unemployment has shrunk since the beginning of the 1980s (Allegretto and Stettner 2004; Helwig 2004). If the health consequences of job loss differ across white- and blue-collar occupations, these trends may have implications for patterns of health and illness.

Focusing on the economic strain of job loss, we might expect blue-collar workers to suffer more than their white-collar counterparts. Since blue-collar workers will typically have lower incomes and fewer assets, they will generally have less of a buffer to protect against the economic strain of a job loss. There is evidence to suggest that less-educated displaced workers are in worse health than their more-educated counterparts following a job loss (Broman et al. 1995; Hamilton et al. 1990). However, these results are based on studies of plant closures, which focus primarily on blue-collar workers. This notably truncates the variation in economic resources and does not allow comparisons across occupational categories.

If we alternatively consider job quality, we might expect blue-collar workers to suffer *less* than their white-collar counterparts. Blue-collar jobs are generally worse than white-collar jobs (e.g., lower pay and status, less autonomy, less pleasant work conditions), so losing a blue-collar job may not pose as large a shock as losing a white-collar job. In addition, because blue-collar jobs themselves are more likely to pose health risks (e.g., injury, exposure to toxins), the negative effects of the socioeconomic shock of job loss may be partially offset by health benefits of freedom from occupational hazards (Iversen et al. 1989).

It is also possible that health-related selection into job loss differs across occupations. Blue-collar jobs tend to be more physically demanding than white-collar jobs, so poor health may pose a larger employment risk to blue-collar workers. Muurinen and Le Grand (1985), proposing an economic model of health inequalities, argued that, in the absence of human and asset capital, people will be forced to rely more heavily on their health capital, typically turning to manual occupations to earn a living. Case and Deaton (2003) provided empirical support for this, showing that health is a more important determinant of remaining employed if one is in a manual occupation rather than a nonmanual occupation. Such differential selection patterns could bias estimates of interactions between job loss and occupation in determining health. Focusing on particular types of job loss and health outcomes, the following analysis tries to address this concern.

DATA AND METHODS

Data are from the 1999, 2001, and 2003 waves of the U.S. Panel Study of Income Dynamics (PSID). The PSID is a nationally representative, longitudinal survey of American families. Interviews began in 1968 and were conducted annually until 1997, when the survey switched to a biennial design (Institute for Social Research, University of Michigan 2002). Since job separations are relatively rare, I pool data from all three waves to increase the number of events observed.

The PSID is well-suited to this analysis because it provides detailed employment information and regularly collects data on health.[1] Questions about employment history reveal whether a sample member experienced a recent job disruption. Queries into the reasons for a job disruption further allowed me to create distinct job loss/separation categories. Although the PSID has collected information on self-assessed health in each survey since 1984, questions about specific health conditions were begun only in 1999. I rely on the three most recent waves of the PSID (1999, 2001, and 2003) because they provide multiple different health measures. (For an analysis of the quality of the PSID health data, see Andreski et al. [2005].)

In order for the PSID to inquire about a person's last job, the person had to have been in that job in January of the year prior to the survey (e.g., for the 2001 survey, the person had to be in the job in January of 2000). This implies that the PSID detects job disruptions occurring only since January of the year before the survey. Consequently, this analysis can consider only relatively short-term consequences of job loss that emerge within a year or two following displacement. The structure of the employment questions also means that, for respondents to be included in the analysis, they must have been employed in January of the prior year. Working with the years 1999, 2001, and 2003 and limiting the sample to heads of households and wives/cohabiters who had valid data on all relevant variables for at least one wave, who were employed in January of the year prior to the survey, but who were not self-employed, yields a total sample of 8,125 individuals and 16,724 person-years. Isolating respondents who held blue-collar (i.e., operative or labor) or white-collar (i.e., professional or managerial) jobs in January of the year prior yields 3,359 white-collar respondents (5,954 person-years) and 1,851 blue-collar respondents (2,870 person-years). Respondents in other occupational groups that do not fit easily within blue- or white-collar categories (e.g., service or clerical occupations) are excluded from the occupational subgroup analysis (however, results were relatively robust to slightly different white- and blue-collar groupings).[2] Descriptive statistics are presented in Table 1.

Variables

Job losses/separations. Job disruptions are measured with four dichotomous variables. Using questions about why respondents left their last job, I create four categories:

(1) *No-fault job loss*: This includes cases in which people lost jobs because an entire workplace was shut down (e.g., when a company folded or relocated, a

1. Although the National Longitudinal Survey of Youth 1979 (NLSY79) and the Health and Retirement Study (HRS) both collect information on job loss and health, neither is appropriate for this analysis. While the NLSY79 regularly collects information on job loss, detailed health questions that were begun in 1998 are asked of each respondent once and not repeated for that respondent in the following wave. The HRS regularly collects both job loss and health information; however, the older age profile of the HRS means many sample members have relatively weak labor market attachment.

2. For instance, including clerical workers in the white-collar model and including crafts in the blue-collar model generated a similar pattern of estimates (although changes in cell sizes and slight changes in point estimates made some of the differences across models statistically insignificant).

plant closed, or an employer died). This category should be the least vulnerable to selection bias.[3]

(2) *Fired/laid off:* Since employers are likely to target sicker workers for firing/layoffs, the association between poor health and this category may be upwardly biased.

(3) *Voluntary job separation:* This is a diverse category for which the direction of bias is unclear. Respondents who leave jobs voluntarily in order to retire, go on disability, or take another less demanding job are likely to be in relatively poor health. However, respondents who are upwardly mobile and voluntarily leave one job for another, better job are likely to be quite healthy.[4]

(4) *Miscellaneous job separation:* This includes ambiguous and miscellaneous cases of job loss/separation (e.g., when respondents left jobs because seasonal/temporary work came to an end—which may or may not be voluntary—and cases that could not be classified in the PSID categories). Since the circumstances surrounding these job separations are ambiguous, results for this group cannot be clearly interpreted.

These four mutually exclusive dichotomous variables each indicate whether a respondent experienced the relevant type of job loss/separation since January of the year prior to the survey and are interpreted relative to the suppressed category of stably employed respondents.

The following analysis also examines the association between unemployment and health by dividing each of the job loss categories in two, depending on whether displaced respondents were reemployed at the time of the survey. This generates the following eight dichotomous variables: no-fault job loss–reemployed, no-fault job loss–out of work, fired/laid off–reemployed, fired/laid off–out of work, voluntary separation–reemployed, voluntary separation–out of work, miscellaneous-reemployed, and miscellaneous–out of work (stably employed respondents continue to be the reference category).

Distinguishing between these different job loss/separation categories and employment statuses is central to the logic of this analysis. However, these distinctions can also lead to small cell sizes. (Depending on the health outcome being used and whether the category is divided based on reemployment status, the number of cases in the no-fault group will range from about 100 to 300.) Given small cell sizes, I treat job loss/separation estimates with p values of less than 10% as statistically significant.

Health measures. Health is measured with three different variables: fair/poor health, likely health conditions, and unlikely health conditions. The first measure, fair/poor health, is a dichotomous variable in which 1 indicates that a respondent described his/her health at the time of the survey as fair or poor. This measure is based on a self-assessed scale in which respondents are asked to rank their overall current health as excellent, very good, good,

3. The no-fault category used in this analysis should improve on existing plant closure studies because a national sample should include more varied establishment closures occurring across a range of occupations. To confirm that the no-fault group is indeed more reflective of the occupational diversity at a national level, I can note that, of the approximately 300 instances of no-fault job loss, only about 20% involved displacement from a job as a machine operator (the typical focus of plant closure studies), about 30% involved displacement from a professional or managerial position, about 33% involved displacement from sales, clerical, or crafts jobs, and about 13% involved displacement from a service position.

4. The PSID asks respondents an open-ended question about why they left their last job, and their responses are then coded into categories. While it would be desirable to distinguish between firings and layoffs, and also to distinguish between these different types of voluntary separations, the PSID categories unfortunately do not allow for further refinement.

Table 1. Sample Means, by Occupational Groupings

Variable	Total Sample	White-Collar (professional/managerial)	Blue-Collar (operative/labor)
Fair/Poor Health in Previous Wave	0.079	0.045	0.127
	(0.270)	(0.207)	(0.333)
Fair/Poor Health	0.089	0.050	0.136
	(0.285)	(0.218)	(0.343)
Any Health Condition in Previous Wave[a]	0.349	0.322	0.351
	(0.477)	(0.467)	(0.477)
Likely Health Conditions[b]	0.074	0.064	0.086
	(0.261)	(0.245)	(0.280)
Unlikely Health Conditions[b]	0.008	0.006	0.008
	(0.089)	(0.075)	(0.091)
No-Fault Job Loss	0.017	0.012	0.025
	(0.128)	(0.114)	(0.158)
Fired/Laid Off	0.041	0.026	0.060
	(0.199)	(0.159)	(0.237)
Voluntary Job Separation	0.153	0.126	0.145
	(0.360)	(0.332)	(0.352)
Miscellaneous Job Separation	0.029	0.021	0.049
	(0.168)	(0.143)	(0.217)
No-Fault Job Loss–Reemployed	0.008	0.007	0.012
	(0.091)	(0.086)	(0.109)
No-Fault Job Loss–Out of Work	0.008	0.005	0.014
	(0.090)	(0.073)	(0.116)
Fired/Laid Off–Reemployed	0.019	0.014	0.024
	(0.138)	(0.117)	(0.153)
Fired/Laid Off–Out of Work	0.023	0.012	0.039
	(0.151)	(0.111)	(0.194)
Voluntary-Reemployed	0.100	0.088	0.088
	(0.300)	(0.284)	(0.283)
Voluntary–Out of Work	0.059	0.040	0.058
	(0.235)	(0.196)	(0.234)
Miscellaneous-Reemployed	0.012	0.009	0.022
	(0.111)	(0.097)	(0.146)
Miscellaneous–Out of Work	0.018	0.012	0.029
	(0.132)	(0.110)	(0.168)
Age (piecewise spline)			
Age 1 (min, 30)	29.424	29.595	29.432
	(1.644)	(1.282)	(1.656)
Age 2 (30, 40)	7.074	7.318	7.017
	(4.000)	(3.894)	(3.948)
Age 3 (40, 50)	3.804	4.146	3.497
	(4.246)	(4.310)	(4.142)
Age 4 (50, 60)	1.158	1.210	1.007
	(2.731)	(2.702)	(2.565)
Age 5 (60, max)	0.288	0.245	0.218
	(1.715)	(1.591)	(1.445)

(continued)

(Table 1, continued)

Variable	Total Sample	White-Collar (professional/managerial)	Blue-Collar (operative/labor)
Female	0.504 (0.500)	0.533 (0.499)	0.302 (0.459)
Black	0.258 (0.437)	0.169 (0.375)	0.337 (0.473)
Other	0.081 (0.273)	0.050 (0.219)	0.156 (0.363)
High School	0.333 (0.471)	0.175 (0.380)	0.472 (0.499)
Some College	0.267 (0.443)	0.262 (0.440)	0.178 (0.383)
College Graduate	0.257 (0.437)	0.527 (0.499)	0.038 (0.190)
Manager	0.132 (0.338)	0.370 (0.483)	—
Sales	0.064 (0.245)	—	—
Clerical	0.173 (0.378)	—	—
Crafts	0.093 (0.291)	—	—
Operative	0.137 (0.344)	—	0.800 (0.400)
Laborer	0.034 (0.182)	—	—
Service	0.142 (0.349)	—	—
Family Income (logged)	10.671 (1.114)	11.038 (0.983)	10.359 (1.044)
Continuous Insurance	0.861 (0.346)	0.943 (0.232)	0.759 (0.428)
Remains Single	0.212 (0.408)	0.172 (0.377)	0.220 (0.414)
Divorced/Separated Since Previous Wave	0.041 (0.199)	0.035 (0.183)	0.039 (0.194)
Widowed Since Previous Wave	0.004 (0.060)	0.003 (0.054)	0.003 (0.056)
Married Since Previous Wave	0.042 (0.200)	0.040 (0.197)	0.041 (0.199)
Remarried Since Previous Wave	0.004 (0.061)	0.002 (0.043)	0.006 (0.077)
Other Relationship Change	0.006 (0.074)	0.004 (0.059)	0.009 (0.095)
Moved to New Residence	0.267 (0.442)	0.255 (0.436)	0.254 (0.435)

(continued)

(Table 1, continued)

Variable	Total Sample	White-Collar (professional/managerial)	Blue-Collar (operative/labor)
2001	0.339 (0.474)	0.363 (0.481)	0.311 (0.463)
2003	0.344 (0.475)	0.308 (0.462)	0.379 (0.485)
Person-Years	16,724	5,954	2,870
Individuals	8,125	3,359	1,851

Note: Numbers in parentheses are standard deviations.

[a]Includes records from 2001 and 2003 waves (N = 11,425); health conditions questions were not asked in 1997, so the previous health conditions measure is not available in the 1999 wave.

[b]Excludes respondents with any preexisting health conditions in the prior January (N = 13,163).

fair, or poor.[5] This scale is used widely within the literature on health and stratification and is highly correlated with several more objective measures of health, such as mortality and physician diagnoses (Idler and Benyamini 1997; Liang 1986; Moosey and Shapiro 1982).

This outcome offers important strengths; most notably, it is an omnibus measure that can capture multiple different aspects of health (possibly more than could be queried on a single survey). However, casting such a wide net may also make this popular measure vulnerable to selection bias. If a person reports fair or poor health in this measure, we do not know what sorts of underlying conditions are responsible for the negative assessment, and we do not know whether these conditions are epidemiologically or physiologically reasonable responses to a recent event like job loss. Additionally, this measure informs us of a person's current health status but says nothing about the timing of the health events that may have determined that status. While the following analysis of self-assessed health adjusts for whether a person reported fair or poor health in the previous survey, the biennial design of the PSID means that the baseline health measure from the previous survey may precede the beginning of the job loss observation period by several months (see Figure 1). The categories in the self-assessed scale (e.g., excellent, very good, or good) are also rather broad. This is a useful characteristic when assessing overall well-being, but when self-assessed health is used as a baseline control, it may leave room for within-category variation that is associated with selection into job loss.

The other two health measures cast a narrower net and address some of these ambiguities. These measures are based on a set of questions in which respondents are asked whether a doctor has ever told them they have any of a long list of health conditions and, if so, for how long they have had the conditions(s). With these questions, we know what sorts of conditions a person suffers from and when the condition(s) began. This allows isolation of new health conditions that should be either more or less responsive to a recent event like job loss. I create two dichotomous variables—likely health conditions and unlikely health conditions—in which 1 indicates that a person developed one or more health conditions that are either likely or unlikely to be sensitive to job loss. When modeling these two outcomes, I use reports about the duration of health conditions to exclude respondents who had any preexisting health problems at the beginning of the job loss observation period (i.e., January of the previous year; see Figure 1).[6]

5. This scale is dichotomized because the association between job loss and fair/poor health is much stronger than the association between job loss and the other health categories. However, working with the full five-category scale and fitting an ordered logit model produced similar results.

6. The PSID asks how long a person has had a condition rather than asking specifically when they were diagnosed. In many cases (most notably, conditions that are not easily detected without a physician, such as

In the likely health conditions measure, I assume that stroke, hypertension, heart disease, heart attack, arthritis, diabetes, and emotional/psychiatric problems should all be sensitive to recent job losses. It is widely documented that stress can have negative consequences for the cardiovascular system, and hypertension, heart disease, heart attack, and stroke are all cardiovascular conditions (Gallo et al. 2004; Iversen et al. 1998; McEwen 1998; Sapolsky 2004). Long-term or repeated stressors have been shown to increase the production of proinflammatory plasmas (e.g., interluken 6) in the blood, which can lead to inflammation and rheumatoid arthritis (Kiecolt-Glaser et al. 2003; Kiecolt-Glaser et al. 2002; McEwen 1998; Sternberg et al. 1992). Diabetes should also be sensitive to job loss because stress can change eating and exercise habits, leading to weight gain, and can further affect insulin resistance by altering hormone levels (Sapolsky 2004). Finally, a large body of evidence has shown strong associations between job loss and emotional or psychological problems (Cohn 1978; Dooley et al. 1994; Kessler 1997; Kessler et al. 1987; Montgomery et al. 1999; Murphy and Athanasou 1999). All the above conditions are likely to be sensitive to a recent job loss. However, this does not imply that these conditions cannot also interfere with work and lead to a job loss. When interpreting results for the likely health conditions measure, I continue to distinguish between the different job loss categories.

In the unlikely health conditions measure, I assume that lung disease, cancer, and loss of memory or mental ability should *not* be sensitive to a recent event like job loss. With all of these conditions, it is difficult to imagine a causal pathway through which job loss could affect health. Lung disease is the result of long-term behaviors and exposures (e.g., smoking), so it is difficult to imagine a new case resulting from a job loss within the past year or two. Additionally, many cases of lung disease are a result of occupational exposures (e.g., asbestos), suggesting that it is frequently a matter of having a given type of job rather than losing a job (American Lung Association nd[a], nd[b]). Cancer is certainly socially determined via behaviors (e.g., smoking, diet) and exposures to environmental carcinogens. However, one needs to engage in these behaviors or experience these exposures for long periods before cancer develops. It seems unlikely that job loss could increase the risk of cancer within the relatively short time frame of this analysis (U.S. Department of Health and Human Services nd). Additionally, there is little evidence to suggest that stress has any effect on tumor progression, so it is unlikely that stress could speed the progression, and hence diagnosis, of cancer (Sapolsky 2004). Finally, clinically diagnosable loss of memory or mental ability generally emerges at older ages with the onset of dementia, and it is difficult to imagine any pathway through which job loss would increase the risk of dementia (Hendrie 1998).

Because these conditions do not provide reasonable causal pathways, an association between job loss and the unlikely health conditions measure probably reflects selection rather than an actual effect of job loss. I use this final health measure primarily as a test of the no-fault category. If people experiencing no-fault job loss develop these unlikely conditions with any significant frequency, it will point to selection within this group.[7]

Control variables. The following models adjust for basic sociodemographic characteristics measured at the time of the survey. These include age (which, in order to account for nonlinear associations between age and health, is fit with a piecewise linear spline with

hypertension), the duration of a condition will line up with the timing of diagnosis. However, in some other cases, respondents may report the duration of a condition according to when symptoms began. This option for respondents to report the beginning of symptoms may be a strength for this analysis because it may allow more accurate identification of when health conditions might have started to interfere with employment.

7. The PSID also asks respondents whether a doctor has ever diagnosed them with asthma. I do not include this health problem in either the likely or unlikely conditions variables because it is unclear whether it would be sensitive to recent job losses. I do, however, include asthma in the list of preexisting conditions that exclude respondents from the models predicting health conditions.

nodes at 30, 40, 50, and 60); gender (a dichotomous variable in which 1 indicates female); race (dichotomous variables indicating black and other; white is the reference category); and education (a series of dummy variables indicating degree status; having less than a high school diploma is the reference category). Dichotomous indicators for survey year are also included (1999 is the reference category).

The control for family income refers to the calendar year preceding the job loss observation period (e.g., in the 2001 wave, for which the job loss observation period begins in January 2000, income is measured from January 1999 to December 1999). This continuous measure of total family income is retrospectively reported during the interview and is logged in the following analysis to account for skewedness. The control for health insurance similarly refers to the calendar year preceding the job loss observation period. Respondents reported the number of months they were covered by health insurance during the previous two years. Based on these reports, I create a dichotomous indicator that is coded 1 for all respondents who had continuous health insurance in the year before the job loss observation period. The control for occupational category refers to the job that the respondent held at the beginning of the job loss observation period (i.e., in January of the prior year). This nine-category schema is based on U.S. census occupational groups, and professionals serve as the reference group.

It is important that all these variables are measured before or at the beginning of the job loss observation period. While each of these factors is a potential confounder, they could all theoretically act as mechanisms as well (e.g., while income and health insurance may alter the risk of job loss and poor health, shocks to income and health insurance may also be important pathways behind an effect of job loss on health). By measuring these variables before or at the beginning of the job loss observation period, I can be reasonably confident that they are adjusting for potential confounding rather than possible mechanisms.

Unfortunately, it is not as easy to pin down the timing for the two remaining control variables: changes in marital/relationship status and moving to a new residence. In each wave, respondents were asked whether they had experienced any changes in their marital/relationship status since the previous survey two years earlier. With a two-year window, this variable will adjust for relationship events that occurred both before and during the job loss observation period. Respondents were also asked whether they had moved to a new residence in the past year (i.e., since the previous spring). With a one-year window, this measure can adjust for moves that occurred during the job loss observation period. These measures do not allow us to sort out whether marital/relationship or residence changes occurred before or after a job loss, and it remains possible that these measures hold constant some variation in mechanisms as well as variation in potential confounders. Possibly adjusting for mechanisms with these controls implies that the following job loss estimates may err on the conservative side.

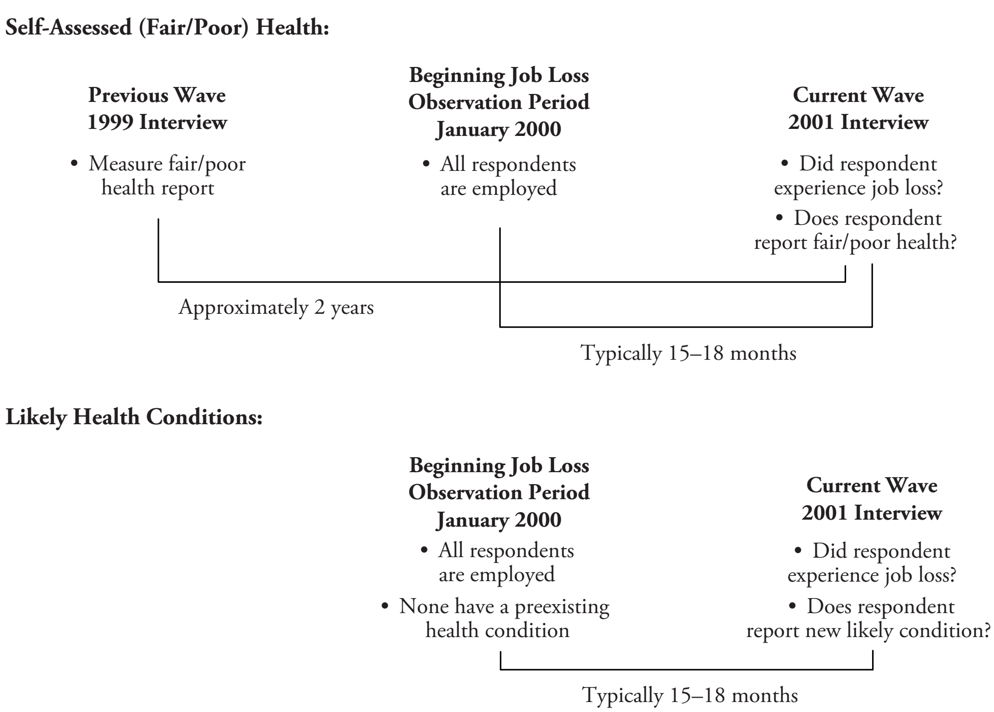
Changes in marital/relationship status are measured with a series of dummy variables indicating whether a respondent remained single, got divorced/separated, became widowed, married for the first time, got remarried, or experienced some other relationship change since the previous survey; remaining married is the reference category. Moving to a new residence is measured with a dichotomous indicator coded 1 if the respondent reported moving to a new residence in the preceding year.

Models and Analysis

The first set of models tests for health-related selection into establishment closures (i.e., the no-fault group). Such selection could work in various ways. Sicker workers may end up in firms or industries that are not doing well, and therefore face an elevated risk of having their workplaces close. Employers would also seem more likely to close less productive worksites, and productivity may be indicative of workers' average health at a given site. We can further anticipate that healthier employees with better prospects might leave for alternative jobs as the company begins to falter but before it actually closes. In all these cases, a concentration of sicker workers in the no-fault group could lead to upward bias. To test for such possibilities, I use health status in the previous wave (measured as both health conditions and self-assessed health) to predict the likelihood of reporting a given type of job loss, relative to no job loss, in the current wave. If these multinomial logistic regression models reveal any significant associations between earlier health and no-fault job loss, this will raise concerns about the assumptions of the job loss categories.

I next consider the central question of how job loss affects subsequent health. This involves logistic regression models in which retrospective reports of job loss predict different health outcomes. The timing of the job loss and health measures is depicted in Figure 1. Although a handful of interviews were conducted as early as January and as late as November, the vast majority of interviews were conducted in the spring (March through June). Therefore, while a minimum of 12 and a maximum of 23 months may pass between the beginning of the job loss observation period and the interview date at which health is measured, typically 15 through 18 months will elapse. Models predicting self-assessed health include all respondents who were employed at the beginning of the observation period, regardless of health status, and adjust for health in the previous wave, which, because of the biennial survey design, may precede the observation period by several months. Models predicting health conditions alternatively exclude respondents who had any health problems at the beginning of the job loss observation period and test whether job loss is associated with the onset of a new health condition.

Figure 1. Timing/Sequence of Job Loss/Separation and Health Measures: Example of 2001 Survey



In the next set of models, each of the job loss categories is divided in two depending on employment status at the time of the survey. This article is motivated by a concern with labor market "churning" (i.e., high rates of job loss, but not necessarily high unemployment). It is possible that job loss with quick reemployment is not particularly hazardous and that only unemployment following job loss has health consequences. This would not suggest that job loss, as a general category, is harmless because job loss is the event that leads to potentially harmful unemployment. Rather, it would suggest that only a subset of job losses (i.e., those followed by unemployment) is harmful. If this is the case, the churning of the U.S. labor market could have significantly fewer health consequences than an alternative scenario with higher rates of unemployment (and potentially lower rates of job loss).

While an analysis of reemployment is important for addressing this question, it also introduces a new selection issue because displaced workers in poor health should be more likely to remain out of work. This suggests that differences in health between the unemployed and reemployed are likely to reflect a combination of (un)employment effects and selection into (un)employment. Importantly, selection into (un)employment should be reduced in the case of establishment closures. Workers who were fired or laid off are likely to have a harder time finding new jobs relative to workers who were displaced because of an establishment closure. Being fired or laid off reflects a previous employer's discretion and therefore sends a negative signal to prospective future employers. In support of this, Gibbons and Katz (1991) found that workers who were laid off suffered larger wage losses and longer spells of unemployment relative to their counterparts who lost jobs because of an establishment closure. Greater barriers to reemployment among fired or laid-off workers imply fiercer competition for new jobs and likely stronger selection into (un)employment based on unobservable characteristics within the fired/laid off category relative to the no-fault category. While establishment closures certainly cannot eliminate bias from selection into reemployment, focusing the interpretation on the no-fault category should reduce unobserved differences between the reemployed and unemployed groups. It is also important to keep in mind that, since healthier workers should be selected into reemployment, any bias within the reemployed groups would be toward better health. Any evidence that displaced, reemployed workers were in poorer health than those who remained stably employed would be counter to this potential "good-health" selection bias.

The final set of models tests for differential effects of job loss on health by separating the sample into white-collar (i.e., professional and managerial) and blue-collar (i.e., labor and operative) categories.

Since all the models in this analysis pool data from multiple waves, all standard errors are adjusted for clustering within individuals. Because the PSID sampling weights are primarily a function of the explanatory variables, all the regression results are based on unweighted data (Winship and Radbill 1994).

RESULTS

The first step is to test the basic assumptions of the job loss categories. Table 2 presents relative risk ratios from multinomial logistic regressions. In these models, health status (i.e., having any health conditions or reporting fair or poor self-assessed health) in previous waves is used to predict the likelihood of reporting a given type of job loss/separation in the current wave. In both of these models, there is no significant association between earlier health reports and the likelihood of experiencing a no-fault job loss. However, poor health in the previous wave is positively associated with each of the other job loss/separation categories (i.e., being fired/laid off, leaving a job voluntarily, and leaving a job for miscellaneous reasons). These results support the basic assumptions of the job loss/separation categories and provide no evidence of health-related selection into the no-fault category.

Table 3 addresses the central question of how retrospectively reported job losses/separations impact health status at the time of the survey. According to odds ratios from

Model 1, job loss because of an establishment closure increases the odds of reporting fair or poor health by approximately 54%, adjusting for prior self-assessed health and several covariates. Predicted probabilities are shown at the bottom of Table 3. A member of the stably employed reference group has a .05 probability of reporting fair or poor health, while someone in the no-fault group has a probability of .075. Model 2 tests for associations between job loss/separation and the likely conditions outcome. The estimate for the no-fault group is robust to this more stringent outcome. Conditioning on respondents being healthy at baseline, losing a job because of an establishment closure increases the odds of a new likely health condition by 83%. As shown at the bottom of the table, a member of the stably employed group has a .059 probability of a new likely health condition, while a member of the no-fault group has a .103 probability. Focusing on the special case of establishment closures, which do not appear to be sensitive to earlier health in Table 2, there is considerable evidence for a causal effect of job loss on health.

According to Model 1 in Table 3, being fired or laid off increases the odds of reporting fair or poor health by 80%. Turning to Model 2, the fired/laid off estimate is less resilient to the stricter likely conditions outcome than the no-fault estimate. When focusing on health conditions for which there is a reasonable causal pathway and excluding respondents with preexisting health conditions, the estimate for the fired/laid off group is nearly halved—in Model 2, having been fired or laid off increases the odds of new health conditions by 43%. The sensitivity of the fired/laid off estimate to the different health outcomes fits with the assumptions of this analysis. If the fired/laid off group is indeed partially reflecting selection, it should have a weaker association with this more stringent health outcome because this more limited outcome should be less likely to detect a selection process.

Turning to voluntarily job separations, Model 1 suggests that leaving a job voluntarily increases the odds of a fair or poor health report about 50%. This positive association may reflect the poor health of respondents who leave jobs in order to select out of the labor market (e.g., retire, go on disability). Notably, the voluntary job separation estimate is much smaller in Model 2, reflecting only about a 20% increase in the risk of developing a new likely health condition.

In Model 1 predicting fair/poor health, the point estimate for the fired/laid off group appears to be larger than the equivalent estimate for the no-fault group, pointing to possible evidence of upward selection bias in the fired/laid off group. However, this difference between the point estimates is not statistically significant. In Model 2, predicting the stricter likely conditions outcome, the fired/laid off estimate appears somewhat smaller than the no-fault estimate, but again the difference is not statistically significant. Overall, differences between the no-fault and fired/laid off estimates cannot be clearly interpreted. In Model 1, the association between leaving a job voluntarily and reporting fair or poor health is statistically indistinguishable from the other job loss/separation point estimates. However, in Model 2 predicting the onset of new likely health conditions, the voluntary job separation point estimate is significantly smaller than the no-fault job loss estimate. This is probably because the likely conditions outcome is less sensitive to the selection of sicker workers out of the labor market within the voluntary separation group.

Model 3 summarizes the association between job loss/separation and the unlikely conditions outcome. Since there are no reasonable causal pathways for the unlikely conditions, this model is intended as a test for selection. It actually ends up being impossible to properly estimate a model predicting the unlikely outcome because no one in the no-fault group developed one of these conditions within the time frame of the analysis. The no-fault group drops out, and the estimate is missing from Model 3. The unlikely conditions do not appear to be a factor for the no-fault group. Leaving a job for other reasons, however, is positively associated with developing a new unlikely health condition. Being fired or laid off more than doubles the odds of developing an unlikely condition, while leaving a job voluntarily increases the odds by 72%. These significant associations point to health-related

Table 2. Association Between Earlier Health and Different Types of Job Losses/Separations: Relative Risk Ratios From Multinomial Logistic Regressions

	Model 1				Model 2			
Variable	No-Fault Job Loss	Fired/ Laid Off	Voluntary Separation	Miscellaneous Separation	No-Fault Job Loss	Fired/ Laid Off	Voluntary Separation	Miscellaneous Separation
Health Condition in Previous Wave	1.059	1.433**	1.326**	1.255†	—	—	—	—
	(0.174)	(0.144)	(0.083)	(0.152)				
Fair/Poor Health in Previous Wave	—	—	—	—	1.202	1.268†	1.258**	1.961**
					(0.246)	(0.172)	(0.106)	(0.292)
Age (piecewise spline)								
Age 1 (min, 30)	0.928	0.933*	0.933**	1.009	0.919†	0.927**	0.928**	0.988
	(0.049)	(0.027)	(0.018)	(0.044)	(0.042)	(0.023)	(0.014)	(0.036)
Age 2 (30, 40)	0.984	0.962*	0.960**	0.954*	0.993	0.970†	0.964**	0.970
	(0.031)	(0.018)	(0.011)	(0.023)	(0.027)	(0.016)	(0.009)	(0.020)
Age 3 (40, 50)	0.989	0.992	0.941**	1.044	1.012	0.992	0.942**	0.994
	(0.031)	(0.020)	(0.012)	(0.023)	(0.024)	(0.017)	(0.010)	(0.021)
Age 4 (50, 60)	1.069†	0.957	1.069**	1.024	1.040	0.968	1.081**	1.025
	(0.041)	(0.027)	(0.018)	(0.030)	(0.032)	(0.025)	(0.015)	(0.027)
Age 5 (60, max)	1.071†	1.030	1.032†	1.110**	1.066*	1.033	1.038*	1.101**
	(0.040)	(0.035)	(0.019)	(0.031)	(0.030)	(0.034)	(0.016)	(0.027)
Female	1.319	0.854	1.281**	1.599**	1.301†	0.907	1.361**	1.663**
	(0.225)	(0.093)	(0.081)	(0.208)	(0.184)	(0.085)	(0.073)	(0.196)
Black	1.111	1.493**	0.781**	0.799	0.992	1.402**	0.763**	0.830
	(0.198)	(0.160)	(0.056)	(0.119)	(0.148)	(0.132)	(0.046)	(0.108)
Other	1.125	0.912	0.822†	2.064**	1.024	0.864	0.731**	2.088**
	(0.318)	(0.176)	(0.092)	(0.305)	(0.240)	(0.145)	(0.069)	(0.281)
High School	1.063	0.607**	0.719**	0.614**	0.833	0.617**	0.701**	0.588**
	(0.247)	(0.080)	(0.065)	(0.101)	(0.157)	(0.071)	(0.052)	(0.086)
Some College	0.977	0.716*	0.840†	0.799	0.875	0.721*	0.838*	0.893
	(0.253)	(0.105)	(0.080)	(0.146)	(0.181)	(0.093)	(0.067)	(0.143)
College Graduate	0.749	0.755	0.825†	1.066	0.775	0.705*	0.791*	1.160
	(0.235)	(0.137)	(0.089)	(0.223)	(0.188)	(0.115)	(0.072)	(0.214)
Manager	3.318**	1.458*	1.129	0.640†	2.962**	1.297	1.163†	0.596*
	(1.058)	(0.280)	(0.119)	(0.153)	(0.722)	(0.225)	(0.100)	(0.129)

Sales	3.639**	2.248**	2.009**	1.094	3.283**	2.062**	2.101**	1.021
	(1.265)	(0.454)	(0.232)	(0.285)	(0.919)	(0.387)	(0.206)	(0.247)
Clerical	1.661	1.739**	1.176	1.116	1.530†	1.719**	1.205*	1.050
	(0.558)	(0.297)	(0.116)	(0.219)	(0.390)	(0.264)	(0.100)	(0.185)
Crafts	2.853**	1.715*	0.941	1.117	2.143*	1.458†	0.960	1.262
	(1.077)	(0.371)	(0.126)	(0.301)	(0.655)	(0.287)	(0.106)	(0.296)
Operative	3.949**	1.716**	1.132	1.040	3.232**	1.717*	1.137	1.139
	(1.309)	(0.325)	(0.129)	(0.244)	(0.866)	(0.295)	(0.109)	(0.238)
Laborer	2.757*	1.829*	1.579*	4.086**	1.699	1.569†	1.491**	3.882**
	(1.367)	(0.526)	(0.285)	(1.077)	(0.705)	(0.386)	(0.213)	(0.867)
Service	1.826†	0.977	1.588**	1.122	1.480	1.096	1.551**	1.080
	(0.610)	(0.186)	(0.160)	(0.248)	(0.398)	(0.184)	(0.131)	(0.214)
Family Income	0.873**	0.859**	0.898**	0.841**	0.849**	0.825**	0.873**	0.810**
	(0.035)	(0.025)	(0.023)	(0.023)	(0.032)	(0.021)	(0.020)	(0.021)
Continuous Insurance	0.499**	0.358**	0.534**	0.352**	0.481**	0.378**	0.488**	0.366**
	(0.094)	(0.041)	(0.041)	(0.048)	(0.077)	(0.038)	(0.032)	(0.044)
Never Married	1.120	1.278†	0.981	1.034	1.092	1.306*	0.971	1.104
	(0.244)	(0.168)	(0.085)	(0.176)	(0.209)	(0.146)	(0.069)	(0.159)
Divorced/Separated	1.456†	1.309†	0.952	0.905	1.302	1.309*	0.898	0.848
	(0.294)	(0.181)	(0.084)	(0.154)	(0.221)	(0.152)	(0.067)	(0.128)
Widowed	0.162	1.675	0.723	0.578	0.244†	1.464	0.598*	0.688
	(0.184)	(0.557)	(0.186)	(0.268)	(0.189)	(0.422)	(0.132)	(0.289)
Moved to New Residence	1.425*	1.316**	2.189**	1.605**	1.463**	1.345**	2.144**	1.503**
	(0.226)	(0.139)	(0.136)	(0.199)	(0.199)	(0.122)	(0.110)	(0.164)
2001	—	—	—	—	1.154	1.231*	1.024	1.349*
					(0.171)	(0.124)	(0.055)	(0.163)
2003	0.765†	1.279**	0.830**	1.037*	0.882	1.543**	0.851**	1.780**
	(0.113)	(0.121)	(0.045)	(0.141)	(0.138)	(0.156)	(0.048)	(0.209)
N		11,425				16,737		

Notes: Numbers in parentheses are robust standard errors. Stable employment (i.e., no job loss/separation) is the base outcome in both Models 1 and 2. Because the health conditions questions were first asked in 1999, Model 1 can employ only two waves of data: 1999 health conditions predict 2001 job losses, and 2001 health conditions predict 2003 job losses. However, because the self-assessed health measures go further back, Model 2 can use three waves of data: 1997 health predicts 1999 job losses, 1999 health predicts 2001 job losses, and 2001 health predicts 2003 job losses. Models 1 and 2 adjust for a respondent's marital status in the previous wave (i.e., at the time of the health measure), rather than adjusting for changes in marital status (as is done in the following logistic models). Because Model 1 uses only two waves of data, the number of cases in some of the less frequent relationship change categories (e.g., remarriage, other relationship change) became sparse and estimates for these variables were unstable. Point estimates for both health measures, however, were robust to several different specifications of marital status or marital status change.

† $p < .10$; * $p < .05$; ** $p < .01$

Table 3. Associations Between Job Loss/Separation Categories and Three Health Outcomes: Odds Ratios From Logistic Regression Models

Variable	Fair/Poor Health (1)	Likely Conditions (2)	Unlikely Conditions (3)
No-Fault Job Loss	1.540†	1.834**	—
	(0.353)	(0.406)	
Fired/Laid Off	1.802**	1.427*	2.774**
	(0.242)	(0.232)	(1.031)
Voluntary Job Separation	1.497**	1.197†	1.719†
	(0.129)	(0.122)	(0.481)
Miscellaneous Job Separation	1.591**	1.200	1.886
	(0.254)	(0.254)	(0.997)
Fair/Poor Health in Previous Wave	14.152**	—	—
	(1.136)		
Age (piecewise spline)			
Age 1 (min, 30)	1.072*	0.977	0.952
	(0.033)	(0.023)	(0.062)
Age 2 (30, 40)	1.038*	1.042**	1.014
	(0.015)	(0.016)	(0.045)
Age 3 (40, 50)	1.059**	1.063**	1.053
	(0.012)	(0.015)	(0.048)
Age 4 (50, 60)	0.998	1.055**	1.025
	(0.014)	(0.020)	(0.052)
Age 5 (60, max)	1.033†	0.975	1.067
	(0.018)	(0.024)	(0.053)
Female	1.111	0.876	1.339
	(0.079)	(0.073)	(0.340)
Black	1.504**	1.611**	0.728
	(0.113)	(0.142)	(0.215)
Other	1.304*	1.013	0.236*
	(0.139)	(0.141)	(0.138)
High School	0.589**	0.826	0.513*
	(0.051)	(0.100)	(0.161)
Some College	0.606**	0.980	0.605
	(0.058)	(0.126)	(0.208)
College Graduate	0.404**	0.832	0.720
	(0.049)	(0.120)	(0.279)
Manager	0.954	1.050	0.900
	(0.128)	(0.135)	(0.423)
Sales	1.183	0.999	2.376*
	(0.173)	(0.165)	(1.006)
Clerical	1.138	1.085	2.148*
	(0.131)	(0.137)	(0.769)
Crafts	1.329*	1.114	0.696
	(0.188)	(0.178)	(0.460)
Operative	1.321*	1.061	1.894
	(0.156)	(0.155)	(0.847)

(continued)

(Table 3, continued)

Variable	Fair/Poor Health (1)	Likely Conditions (2)	Unlikely Conditions (3)
Laborer	1.364†	0.889	3.330*
	(0.242)	(0.202)	(1.965)
Service	1.359**	1.150	2.251*
	(0.160)	(0.162)	(0.870)
Family Income (logged)	0.903**	0.934**	1.092
	(0.020)	(0.024)	(0.091)
Continuous Health Insurance	0.909	1.246†	1.215
	(0.081)	(0.151)	(0.394)
Remains Single	1.067	1.079	1.504
	(0.085)	(0.108)	(0.437)
Divorced/Separated Since Previous Wave	0.797	1.138	1.057
	(0.138)	(0.218)	(0.610)
Widowed Since Previous Wave	0.189**	2.358†	—
	(0.109)	(1.151)	
Married Since Previous Wave	0.624*	0.819	0.585
	(0.127)	(0.138)	(0.310)
Remarried Since Previous Wave	0.698	1.147	6.745**
	(0.343)	(0.605)	(4.046)
Other Relationship Change	0.438*	1.047	0.397
	(0.173)	(0.150)	(0.231)
Moved to New Residence	1.243**	0.935	1.065
	(0.097)	(0.083)	(0.241)
2001	1.001	1.205*	1.067
	(0.081)	(0.102)	(0.275)
2003	1.105	1.368**	1.070
	(0.083)	(0.117)	(0.280)
$\Pr(y = 1 \mid x)$ for			
Stably employed	.050	.059	.004
No-fault job loss	.075	.103	—
Fired/laid off	.086	.082	.012
Voluntary separation	.073	.070	.008
Miscellaneous separation	.077	.070	.008
N	16,724	13,163	12,917

Note: Numbers in parentheses are robust standard errors.

†$p < .10$; *$p < .05$; **$p < .01$

selection within these two job loss/separation categories. (However, when interpreting the sizes of these odds ratios, keep in mind that the onset of a new unlikely condition is a relatively rare event in all the job loss/separation groups—predicted probabilities vary between .004 and .012.)

In Table 4 the job loss/separation categories are split by reemployment status. Involuntarily displaced respondents (i.e., those in the no-fault and fired/laid off groups) who remained out of work at the time of the survey are in consistently worse health than the stably

Table 4. Association Between Job Loss/Separation, Reemployment, and Health: Odds Ratios From Logistic Regression Models

Variable	Fair/Poor Health (1)		Likely Conditions (2)	
No-Fault Job Loss–Reemployed	1.254		1.966*	
	(0.431)		(0.596)	
No-Fault Job Loss–Out of Work	1.832*		1.719†	
	(0.563)		(0.539)	
Fired/Laid Off–Reemployed	1.218	Sig. difference	1.307	
	(0.288)		(0.325)	
Fired/Laid Off–Out of Work	2.227**		1.532*	
	(0.358)		(0.309)	
Voluntary Separation–Reemployed	1.011	Sig. difference	0.955	Sig. difference
	(0.121)		(0.121)	
Voluntary Separation–Out of Work	2.313**		1.745**	
	(0.263)		(0.255)	
Miscellaneous Separation–Reemployed	1.195		0.862	
	(0.296)		(0.289)	
Miscellaneous Separation–Out of Work	1.895**		1.550	
	(0.383)		(0.414)	
Fair/Poor Health in Previous Wave	14.131**		—	
	(1.142)			
Age (piecewise spline)				
Age 1 (min, 30)	1.077*		0.979	
	(0.033)		(0.024)	
Age 2 (30, 40)	1.035*		1.041**	
	(0.015)		(0.016)	
Age 3 (40, 50)	1.058**		1.063**	
	(0.012)		(0.015)	
Age 4 (50, 60)	0.993		1.051**	
	(0.014)		(0.020)	
Age 5 (60, max)	1.028		0.970	
	(0.018)		(0.024)	
Female	1.077		0.863†	
	(0.077)		(0.072)	
Black	1.476**		1.597**	
	(0.111)		(0.141)	
Other	1.300*		1.010	
	(0.139)		(0.141)	
High School	0.595**		0.840	
	(0.052)		(0.102)	
Some College	0.612**		1.000	
	(0.060)		(0.129)	
College Graduate	0.410**		0.848	
	(0.050)		(0.122)	
Manager	0.948		1.048	
	(0.128)		(0.135)	
Sales	1.190		0.988	
	(0.173)		(0.163)	

(continued)

(Table 4, continued)

Variable	Fair/Poor Health (1)	Likely Conditions (2)
Clerical	1.142	1.082
	(0.132)	(0.137)
Crafts	1.320*	1.113
	(0.186)	(0.178)
Operative	1.299*	1.062
	(0.153)	(0.155)
Laborer	1.343†	0.887
	(0.240)	(0.202)
Service	1.353*	1.145
	(0.159)	(0.162)
Family Income (logged)	0.908**	0.938*
	(0.021)	(0.024)
Continuous Insurance	0.890	1.240†
	(0.079)	(0.151)
Remains Single	1.083	1.084
	(0.087)	(0.109)
Divorced/Separated Since Previous Wave	0.801	1.140
	(0.140)	(0.220)
Widowed Since Previous Wave	0.188**	2.293†
	(0.108)	(1.129)
Moved to New Residence	0.626*	0.813
	(0.127)	(0.137)
Remarried Since Previous Wave	0.698	1.116
	(0.356)	(0.584)
Other Relationship Change	0.472†	1.074
	(0.184)	(0.153)
Residence Change	1.242**	0.939
	(0.097)	(0.083)
2001	0.991	1.201*
	(0.080)	(0.102)
2003	1.103	1.371**
	(0.083)	(0.117)
$\Pr(y = 1 \mid x)$ for		
Stably employed	.050	.059
No-fault job loss–Reemployed	.062	.110
No-fault job loss–Out of work	.088	.098
Fired/laid off–Reemployed	.061	.076
Fired/laid off–Out of Work	.105	.088
Voluntary separation–Reemployed	.051	.057
Voluntary separation–Out of work	.109	.099
Miscellaneous separation–Reemployed	.060	.052
Miscellaneous separation–Out of work	.091	.089
N	16,724	13,163

Note: Numbers in parentheses are robust standard errors.

† $p < .10$; * $p < .05$; ** $p < .01$

employed reference group, regardless of the health outcome. A more important problem for the question of job churning, however, is whether displacement remains hazardous even after starting a new job. Upon examination of point estimates for the reemployed group, the answer appears to depend on how health is measured. Focusing on the no-fault category, which should be less vulnerable to bias from selection into reemployment, respondents who had found new jobs at the time of the survey face a 97% increase in the risk of developing a new likely health condition, but they are not at any increased risk of reporting fair or poor health relative to the stably employed reference group. This suggests that job churning may be hazardous to some health measures but not others.

Looking at workers who were fired or laid off, it does not appear that the reemployed respondents are in any worse health than the stably employed reference group; point estimates for the fired/laid off–reemployed group are statistically insignificant in both Models 1 and 2. As discussed, selection into reemployment and bias toward better health among the reemployed should be greater within the fired/laid off group. In support of this, differences between the health of reemployed and out-of-work respondents appear more notable within the fired/laid off category than in the no-fault category. Differences between the reemployed and out-of-work coefficients are never statistically significant within the no-fault group. However, in Model 1, respondents who were fired/laid off and remained out of work did assess their health in significantly worse terms than their reemployed counterparts. Notably, this difference across the reemployed and out-of-work members of the fired/laid off group is not replicated when the more stringent likely conditions outcome is examined in Model 2.

In both Models 1 and 2, respondents who left jobs voluntarily and remained out of work are in significantly worse health relative to both the stably employed reference group and the reemployed members of the voluntary separation group. Respondents who left jobs voluntarily and were reemployed by the survey are not in significantly worse health than the stably employed group. It is noteworthy that the association between voluntary separation–reemployed and new health conditions, while statistically insignificant, is actually negative. This probably reflects the good health of upwardly mobile respondents who leave a job on their own accord and quickly enter another job.

Models 1 and 2 in Table 5 present the association between job loss/separation and self-assessed health, by occupational category. Comparing across the job-loss coefficients in the models, we encounter no evidence that the effect of no-fault job loss on fair/poor health differs for white- and blue-collar workers. Within the fired/laid off and voluntary job separation categories, however, we do find significant differences across occupations. While being fired or laid off or leaving a job voluntarily more than doubles the odds of a fair or poor health report among blue-collar workers, such job displacements/separations have no significant association with the health reports of white-collar workers. Finding such occupational differences in the fired/laid off and voluntarily separated groups, but not in the no-fault group, suggests that both voluntary and involuntary health-related selection out of jobs is more pronounced within blue-collar occupations.

Considering the alternative likely conditions outcome, which should be less vulnerable to selection bias, confirms this interpretation. Estimates for the no-fault group again provide no evidence that the effect of job loss differs across occupation. The pattern within the fired/laid off and voluntary groups, however, changes notably with the new outcome. When working with this more limited health measure and excluding respondents with preexisting conditions, the fired/laid off and voluntary separation estimates for blue-collar workers drop to the point of statistical insignificance and can no longer be statistically distinguished from the equivalent white-collar estimate. Such sensitivity to this more stringent health outcome supports the interpretation that the stronger associations between being fired/laid off or leaving a job voluntarily and reporting fair/poor health within blue-collar occupations reflects stronger health-related selection out of jobs.

Table 5. Association Between Job Loss/Separation and Health, by Occupational Category: Odds Ratios From Logistic Regression Models

	Fair/Poor Health		Likely Conditions	
Variable	White-Collar[a] (1)	Blue-Collar (2)	White-Collar[a] (3)	Blue-Collar (4)
No-Fault Job Loss	2.010	1.689	1.508	1.491
	(0.970)	(0.673)	(0.779)	(0.636)
	ns		ns	
Fired/Laid Off	1.250	2.732**	1.262	0.859
	(0.483)	(0.633)	(0.460)	(0.330)
	sig.		ns	
Voluntary Job Separation	1.259	2.312**	1.120	1.304
	(0.256)	(0.410)	(0.219)	(0.313)
	sig.		ns	
Miscellaneous Job Separation	2.417**	1.461	1.437	0.925
	(0.709)	(0.413)	(0.546)	(0.405)
	ns		ns	
Fair/Poor Health in Previous Wave	20.494**	12.898**	—	—
	(3.583)	(2.040)		
Age (piecewise spine)				
Age 1 (min, 30)	1.073	1.085	1.076	0.843**
	(0.074)	(0.070)	(0.061)	(0.048)
Age 2 (30, 40)	1.034	1.048†	1.011	1.156**
	(0.031)	(0.028)	(0.028)	(0.046)
Age 3 (40, 50)	1.063**	1.009	1.069**	1.021
	(0.025)	(0.025)	(0.026)	(0.034)
Age 4 (50, 60)	1.005	1.075*	1.109**	1.063
	(0.028)	(0.035)	(0.030)	(0.047)
Age 5 (60, max)	1.002	0.933	0.978	0.878
	(0.029)	(0.043)	(0.034)	(0.100)
Female	1.100	1.281†	0.769*	1.080
	(0.151)	(0.173)	(0.099)	(0.207)
Black	1.483*	1.277†	1.386†	1.643*
	(0.247)	(0.189)	(0.235)	(0.322)
Other	0.861	1.258	1.227	1.166
	(0.288)	(0.234)	(0.332)	(0.301)
High School	0.678	0.511**	1.270	0.780
	(0.214)	(0.075)	(0.501)	(0.168)
Some College	0.710	0.394**	1.374	1.072
	(0.222)	(0.081)	(0.528)	(0.281)
College Graduate	0.468*	0.532*	0.972	1.356
	(0.147)	(0.169)	(0.370)	(0.518)
Manager	0.965	—	1.031	—
	(0.137)		(0.137)	
Laborer	—	0.965	—	0.832
		(0.157)		(0.187)
Family Income (logged)	0.901**	0.927	0.943	0.916
	(0.035)	(0.057)	(0.039)	(0.052)

(continued)

(Table 5, continued)

Variable	Fair/Poor Health: White-Collar[a] (1)	Fair/Poor Health: Blue-Collar (2)	Likely Conditions: White-Collar[a] (3)	Likely Conditions: Blue-Collar (4)
Continuous Insurance	0.875	0.862	1.059	1.341
	(0.243)	(0.145)	(0.300)	(0.331)
Remains Single	1.317[†]	0.928	1.277	0.848
	(0.212)	(0.159)	(0.223)	(0.203)
Divorced/Separated Since Previous Wave	1.013	1.000	0.908	1.896
	(0.359)	(0.381)	(0.358)	(0.752)
Married Since Previous Wave	0.645	0.514	0.992	0.754
	(0.288)	(0.236)	(0.298)	(0.293)
Other Relationship Change[b]	0.151**	0.326[†]	1.754*	0.638
	(0.109)	(0.204)	(0.478)	(0.206)
Moved to New Residence	1.461*	1.100	1.062	0.829
	(0.252)	(0.178)	(0.178)	(0.174)
2001	1.192	0.757	1.255	1.055
	(0.198)	(0.133)	(0.189)	(0.202)
2003	1.126	0.912	1.440*	1.071
	(0.190)	(0.138)	(0.218)	(0.212)
$\Pr(y = 1 \mid x)$ for				
Stably employed	.030	.071	.054	.060
No-fault	.058	.115	.079	.086
Fired/laid off	.037	.173	.067	.052
Voluntary separation	.037	.150	.060	.076
Miscellaneous separation	.069	.101	.075	.055
N	5,954	2,870	4,630	2,363

Note: Numbers in parentheses are robust standard errors.

[a]The white-collar models contain respondents who held professional or managerial jobs at the beginning of the job-loss observation period. The blue-collar models contain respondents who held manual or operative jobs. Respondents who held other types of jobs that are difficult to classify as blue/white collar (e.g., service, clerical) are excluded from this analysis.

[b]This variable combines respondents who were remarried, widowed, or experienced some other type of relationship change since the previous wave. Given the smaller number of cases in these models, it was necessary to collapse these less frequent types of relationship changes.

[†]$p < .10$; *$p < .05$; **$p < .01$

CONCLUSION

In this article, I investigated the association between job loss and subsequent health. Estimates based on the no-fault job loss category were significant and notable across both the fair/poor health measure and the likely conditions measure. Losing a job because of an establishment closure increased the odds of fair or poor health by 54%, and among respondents with no preexisting health conditions, it increased the odds of a new likely health condition by 83%. This suggests that there are true health costs to job loss, beyond sicker people being more likely to lose their jobs. These conclusions based on the no-fault category are supported by multinomial logistic regressions that documented no association between health status in the previous wave and the likelihood of no-fault job loss.

With regard to the question of whether the hazards of job loss remain even after workers become reemployed, the answer appears to depend on the health outcome. Respondents who lost jobs because of establishment closures but were reemployed by the survey do not appear to have assessed their health in worse terms than their stably employed counterparts; however, they do appear to have faced an increased risk of new likely health conditions. This implies that recent job "churning" within the United States—characterized by high rates of job loss but low unemployment—may affect certain aspects of health, but not necessarily others.

The results presented in this article support important linkages between labor markets and population health. Evidence from the above analysis suggests that increased job "churning" within the United States is likely to increase the number of health conditions suffered by American workers. Small cell sizes made it difficult to examine the effect of job loss on particular health conditions; however, the most common problems that emerged following no-fault job loss were cardiovascular conditions—primarily, hypertension and heart disease—and arthritis. These are serious conditions, which at the aggregate level could impact the broader U.S. economy. Encouraging U.S. companies to be flexible and to restructure may help increase U.S. economic efficiency in various ways. But if the associated job losses also increase chronic health conditions among workers, there may be a counteractive force that decreases productivity.

The above analysis provides no evidence that job loss effects differ for white- and blue-collar workers. This implies that the increased number of advantaged, white-collar workers within the population of displaced workers has probably not altered the average treatment effect of job loss. The analysis does suggest that there may be more voluntary and involuntary health-related selection out of blue-collar jobs. This supports Muurinen and Le Grand's (1985) assumption that people with less human and asset capital will rely more heavily on health in order to earn a living.

A few caveats should be noted. First, results are based on the 1999, 2001, and 2003 waves of the PSID. The health consequences of job loss may differ depending on local and national economic circumstances, so results based on these years may not extend to very different labor market circumstances. Second, because the analysis for the likely health conditions outcome was limited to respondents without preexisting health problems, these results are based on a healthy subsample and may not extend to other populations who are in poorer health at baseline. (However, estimates for the self-assessed health outcome include all respondents, regardless of prior health, and therefore should extend to populations with more mixed health statuses.) Third, attrition is a potential concern for this analysis. Respondents who experience the most dramatic consequences of job loss (e.g., getting particularly sick, having to relocate to new housing) may be the most likely to be lost between the beginning of the job loss observation period and the interview date. Losing such vulnerable respondents would limit the number of health events observed and, ultimately, could lead to an underestimate of the association between job loss and health. If this is the case, the estimates presented in this article may err on the conservative side.

Distinguishing between different reasons for job loss was crucial to this analysis. However, since job losses are relatively rare events, cell sizes for the job loss categories were somewhat small, particularly when reemployed and unemployed respondents were separated. It would be useful if the above results (particularly those regarding reemployment status and health) could be replicated in the future with alternative data containing a larger number of displaced workers. Focusing on no-fault job losses makes the implicit assumption that job losses from establishment closure are not unique and extend to other cases of involuntary job loss. If, for some reason, job loss because of an establishment closure is particularly hazardous, results based on this group could overstate the effects of job loss.

It is difficult to generate a compelling story for why being let go with all one's coworkers would be worse than being specifically chosen to be laid off during a downsizing; however, this assumption is inherent in the strategy of comparing across job loss categories, and it needs to be kept in mind.

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