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Source: *Journal of Health and Social Behavior*, Vol. 28, No. 1, (Mar., 1987), pp. 51-59

Published by: American Sociological Association

Stable URL: <http://www.jstor.org/stable/2137140>

Accessed: 16/08/2008 13:01

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Unemployment and Health in a Community Sample*

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Journal of Health and Social Behavior 1987, Vol. 28 (March):51-59

This paper reports the first results from a survey of unemployment in Southeastern Michigan. The study includes samples of currently unemployed, previously unemployed and stably employed respondents. Special efforts were made to obtain information about the context surrounding each job loss reported, thus allowing the explicit analysis of selection bias in the estimation of unemployment effects. The analysis documents the substantial influences that unemployment has on self-reported physical health, somatization, anxiety, and depression that cannot be explained by selection bias.

This paper reports information about the health-damaging effects of unemployment from a cross-sectional survey administered in the Fall of 1984 in a high unemployment area of Southeastern Michigan. The survey sample was stratified into currently unemployed, previously unemployed and stably employed subsamples. This sampling scheme allowed us to study the health-damaging effects of losing a job as well as the health-promoting effects of reemployment.

Unlike previous surveys that have studied the effects of unemployment, special efforts were made in this survey to obtain information about the context of job loss. With this information we classified people who had lost their jobs into two categories: those who became unemployed

through no fault of their own, and those who might have played some part in causing the job loss. By studying these subsamples separately, it was possible to evaluate the possibility that selection bias accounts for the association between unemployment and poor health. As the analysis below shows, we found evidence of both selection bias and health-damaging unemployment effects that could not be explained by selection.

BACKGROUND

Many unemployment studies assume that unemployment leads to poor health. These studies try to understand why some unemployed people adjust better than others by examining health outcomes within a sample of unemployed people rather than comparing employed and unemployed samples (e.g., Feather and Davenport 1981; Jackson and Warr 1983).

Most studies that try to estimate the impact of unemployment, rather than assume the existence of such an impact, are based on cross-sectional survey data (e.g., Schlozman and Verba 1979; Kaufman 1982). These studies are vulnerable to selection bias because health problems by themselves may bring about job loss. The potential impact of selection bias is far from trivial (Thoits 1983; Kessler 1986), yet cross-sectional studies of job loss have characteristically ignored this problem.

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This research has been sponsored by Grants 5 P50 MH38330 (Richard Price, Principal Investigator), 5 T32 MH16806 (James S. House, Principal Investigator), 1 RO1 MH37706 (Ronald C. Kessler, Principal Investigator), and by a Research Scientist Development Award 1 KO2 MH00507 to the first author. We are indebted to Richard Price for his support throughout the research and to Linda Bronfman, Anita DeLongis, Janet Keller and David Williams for assistance in various phases of the work. We would also like to thank Evelyn Bromet, Phil Leaf, our colleagues in the Social Environment and Health Program at the Institute for Social Research, and the anonymous reviewers for useful comments on earlier drafts.

The literature on job loss has attempted to deal with selection bias through the use of more complex designs which, for the most part, have taken the form of panel studies (Cobb and Kasl 1977; Parnes and King 1977; Pearlin, Lieberman, Menaghan, and Mullen 1981; Grayson 1985; Linn, Sandifer, and Stein 1985) and aggregate time series analyses (Brenner 1976, 1983; Catalano and Dooley 1977, 1983; Dooley, Catalano, Jackson, and Brownell 1981). These designs, however, have other limitations.

Most panel studies that have examined the health-damaging effects of job loss are based on general population samples (e.g., Parnes and King 1977; Pearlin et al. 1981). The problem here is that only a small portion of the people surveyed lose their jobs, leaving the researchers with small sample sizes and low statistical power. Some studies have enriched the baseline sample by focusing on towns facing the impending shutdown of a major employer (e.g., Cobb and Kasl 1977). However, the baseline assessment of health in these studies is no longer valid because anticipation of the plant's closing has its own impact upon health (Kasl and Cobb 1979).

Dew, Bromet, and Schulberg (1986) have provided the most persuasive analysis of unemployment effects in a panel design. Their survey was administered in two towns in western Pennsylvania. After the completion of their baseline survey, the major employer in one of the towns announced a plant closing. The follow-up interviews with wave-one respondents, many of whom had either lost their jobs or were the spouses of persons who had, were able to provide powerful evidence for employment effects.

This kind of design is ideal; however, it is not one that the researcher can plan on following. By the time a plant closure is announced, it is too late to collect baseline data. It is only the unanticipated event, occurring between waves of a panel study being conducted for other purposes, that allows important work like that of Dew and her colleagues to be carried out.

Aggregate time series designs have also been used to estimate the health-damaging effects of unemployment in ways that remove selection bias. There are two problems with this design that limit its usefulness for this purpose. The first is that most time series studies have focused on the relationship between economic down-

turns and rates of health service utilization (e.g., Catalano, Dooley, and Jackson 1981, 1985; Stokes and Cochrane 1984). The results of these investigations are equivocal because economic downturns are known to affect patterns of helpseeking. In particular, financial constraints lead to the extrusion of chronically mentally ill people from their communities into psychiatric hospitals (Dooley and Catalano 1979; Dooley et al. 1981). This process confounds attempts to assess the impact of the economy on health with a simple time series design.

This first problem is avoided when one works with aggregate time series data on mortality. Studies regarding the influence of economic downturns on suicide rates are quite consistent in documenting an aggregate association of this sort (see Platt 1984 for a review). However, a second problem arises here in that this outcome is exceedingly rare. No more than two or three unemployed people in a thousand take their own lives. This raises the question of whether results involving this outcome have any implications for a broad understanding of unemployment effects.

Dooley and Catalano (1984) have conducted the most persuasive work using time series analyses to study the effects of unemployment. Their investigations use a pooled time series of separate cross-sectional surveys. By working with multilevel data on changes over time in unemployment rates, these researchers have been able to trace the effects of economic downturns on individual reports of health.

Neither the panel design—in the way used by Dew and her colleagues—nor the pooled time series of cross-sections employed by Dooley and Catalano was available to use when we planned our study. Our intention was to evaluate the impact of unemployment in Southeastern Michigan several years after the 1980 recession led to massive layoffs and firings. We wanted to estimate the extent of damage as a needs assessment activity and to study the process of adjustment to unemployment as a preliminary to an intervention program. To achieve these goals we needed to assess the effects of unemployment without benefit of a longitudinal data collection. The following pages describe our approach to this problem.

DATA AND METHODS

Sample

Respondents were selected from a multistage probability sample of households in 14 contiguous census tracts in southeastern Michigan.¹ People who had some unemployment experience in the five years prior to the interview were oversampled relative to those who had been stably employed during the same time period.²

In all, 492 interviews were completed from a total of 607 designated respondents, for an interview response rate of 81 percent. The numbers of completed interviews and the response rates for sample subgroups were: 146 interviews and 90 percent response rate for *currently unemployed* respondents; 162 interviews and 77 percent response rate for *previously unemployed* respondents; and 184 interviews and a 78 percent response rate for *stably employed* respondents. Combining the overall response rate with our 96 percent rate of successful screening yielded a combined screening/interview response rate of 78 percent ($.96 \times .81$). The obtained sample is 60 percent male, 20 percent black, 50 percent unmarried, and has a mean age of 35 years and a mean education of 12 years.³

To be classified as currently unemployed a person must have *involuntarily* lost a steady job where he or she had been working 30 or more hours a week and must have remained unemployed and seeking work (30 or more hours a week) for at least four weeks prior to the interview. Persons classified as previously unemployed had also experienced involuntary unemployment some time in the last five years, but had returned to work at a steady 30+ hours a week job before the interview. Persons classified as stably employed were working a steady 30+ hours a week job and had not involuntarily left a job in the last five years and been unemployed four weeks or more after such a job loss. Note that our sample design compares relatively stable full-time workers with other stable full-time workers who had lost their jobs involuntarily.

The five-year retrospective period was chosen because it defines the duration of the recession in the automobile industry in Michigan. The first mass layoffs of auto workers in this recession occurred almost exactly five years before our interviews began.

Available data suggest that the major difference between our sample and one based on the government definition of unemployment is that ours does not include persons entering the labor market for the first time and not finding work. This group may constitute 25%–40% of the unemployed as defined by the government (Schlozman and Verba 1979). The vast majority (80% or more) of people who lose jobs do so involuntarily, so our data can be considered generally reflective of the job loss experience.

Data Analysis and Measures

The analyses in this paper compare rates of several different health outcomes among respondents in the three employment groups described above. This is done by means of multiple regression equations in which age (in years), education (in years), sex, race (black vs. other) and marital status are controlled.

Eight health outcomes are considered in the analysis: (1) number of alcoholic drinks (beer, wine or hard liquor) consumed in the last month; (2) number of days in the last month in which tranquilizers were used; (3) a four-item index of self-evaluated physical ill health (reliability = .69); (4) number of days restricted to bed by health problems in the last month; (5) somatic symptoms of distress (reliability = .85); (6) symptoms of anxious mood (reliability = .80); (7) symptoms of depressed mood (reliability = .90); and (8) a dichotomous report of having considered committing suicide in the past five years (with over two-thirds of these reports referring to the past two years).⁴

To yield gross estimates of unemployment effects, relationships among the three employment categories and each of the outcomes, taking into consideration the control variables listed above, are initially examined. Subsequent analyses take into consideration selection into and out of unemployment.

RESULTS

The Gross Effects of Unemployment

The first analyses contrast mean levels of the health outcomes in each of the two unemployment groups, separately and in combination, with levels found among the stably employed.

The pooled analyses are computed on weighted data that take into consideration the different sampling fractions (see footnote 2) used to select respondents in the three unemployment groups.

Table 1 presents the metric regression coefficients for the effects of unemployment on each of the eight outcomes.⁵ Amount drunk is scaled in the metric of drinks per month. The next four outcomes (physical illness, somatization, anxiety, depression) are standardized to a mean of zero and a variance of one. Bed days is the number of days in the past month spent in bed due to sickness. Suicidal ideation is a dichotomy (yes coded 1 and no coded 0). The last outcome is a count of the number of days tranquilizers were used in the past month.

The results in panel (1) are clear in showing that the effects of current unemployment are consistently significant in statistical terms. Although not significant, the coefficients for the previously unemployed in panel (2) are consistently positive. Together, these results suggest that unemployment has a significant effect until the time of reemployment and a small residual effect thereafter. It is not legitimate to compare the effects of current and previous unemployment separately, however, if poor health decreases an unemployed person's chance of becoming reemployed. In a situation of this sort, the effect of current unemployment would be overestimated and the effect of previous employment underestimated. For this reason, we have most confidence in the analyses in which the two subsamples are combined. These results, reported in panel (3), show that the experience

of *having been* unemployed is consistently associated with poor health outcomes. These coefficients are not biased by selection out of unemployment.

The Effects of Selection Into Unemployment

Even these more conservative results are based on the assumption that there is no selection *into* unemployment on the basis of prior values of the outcome variables. We can evaluate the validity of this assumption by considering separately those respondents who might have played some part in bringing about their job loss and those who lost their jobs due to circumstances beyond their control.

Selection into unemployment was assessed by using responses to a series of questions about the circumstances under which respondents lost their jobs. These data tell us whether a respondent was forced to retire; was laid off because of a reduction in the number of employees at his workplace; was fired; or was the victim of a plant closing. We know how many others lost their jobs at the same time. We also obtained subjective reports about the extent to which each respondent felt that circumstances beyond his or her control accounted for the job loss, as about the part played by his own actions, unfair supervisor treatment, job performance, poor workplace management, discrimination, and several other possible factors.

We took a conservative stance on coding job loss as being outside the person's control. All

TABLE 1. Gross Effects of Unemployment^a

	Currently Unemployed (1)		Previously Unemployed (2)		Currently Previously/ Unemployed (3)	
	b	se	b	se	b	se
Amount Drunk	9.912*	4.900	2.219	4.621	5.399	4.077
Physical Illness	.309*	.113	.131	.107	.203*	.095
Somatization	.294*	.115	.284*	.109	.288**	.097
Anxiety	.490**	.110	.155	.103	.289**	.092
Depression	.499**	.108	.123	.102	.273**	.091
Bed Days	.628*	.304	.071	.287	.299	.250
Suicidal Ideation	.074*	.033	.055	.031	.062*	.028
Tranquilizers	1.134**	.398	.201	.376	.579	.328

^a Samples vary in size from 481 to 489. Listwise deletion of missing values is used to complete results.

* Indicates a significant univariate test at $\alpha = .05$.

** Indicates a significant test adjusting for multiple comparisons at $\alpha = .05$ (equivalent to a univariate test at $\alpha = .05/8 = .006$).

respondents who reported that their actions contributed in any way to their job loss or who said that circumstances beyond their control were less than totally responsible for their job loss were coded as having had some control over their unemployment. When we used this definition, we had 87 currently unemployed and 118 previously unemployed respondents classified as having lost their jobs through no fault of their own, and 59 currently unemployed and 44 previously unemployed respondents whose job losses were coded as under their control in some way. This dichotomous discrimination was used to examine the effects of job loss separately in these groups.

This coding scheme yields a subsample of respondents who we can be fairly sure lost their jobs through no fault of their own. The analyses in Table 1 were repeated for this subsample and the results are reported in Table 2. The data show that selection into unemployment plays no part in explaining the associations between unemployment and physical illness, somatization, anxiety and depression.⁶ For each of these outcomes, the regression coefficients in Table 2 are very close to their values in Table 1.

The data also show, however, that associations linking unemployment to the other outcomes are affected by taking fault into account. None of the five previously significant coefficients associated with these four outcomes (see Table 1) remain significant for the "not at fault" subsample. Furthermore, in separate analyses we found that the association between unemployment and poor health is significantly greater in the "at fault" subsample than in the "not at

fault" subsample in three of the five cases (involving suicidal ideation and tranquilizer use). This suggests that selection plays a large part in accounting for the association between unemployment and these outcomes.

On the basis of these results, we are unable to demonstrate that unemployment affects alcohol use, tranquilizer use, bed days or suicidal ideation. That is not to suggest that effects of this sort do not exist. It is clear from Table 2 that the coefficients for all of these outcomes remain positive even after adjustment for selection. It might well be that, in a larger replication, significant effects of unemployment on these outcomes could be found.

Estimates of Relative Risk

The analysis thus far has documented that unemployment affects a global measure of physical health and measures of somatization, depression and anxiety. These effects are all similar in magnitude. People in this sample who have lost their jobs are between 20% and 30% of a standard deviation higher on these outcomes than the stably employed.

The question arises, however, whether effects of this magnitude have any substantive significance. Perhaps the easiest way to answer this question is to express each outcome in a dichotomous form to represent a clinically significant cut-point on the severity continuum. In this way it is possible to determine whether unemployment leads to levels of poor health that are substantively significant.

TABLE 2. Effects of Unemployment on the "Not at Fault" Subsample

	Currently Unemployed (1)		Previously Unemployed (2)		Currently Previously/ Unemployed (3)	
	b	se	b	se	b	se
Amount Drank	7.960	5.688	.994	5.042	3.526	4.374
Physical Illness	.289*	.131	.151	.116	.199*	.102
Somatization	.246	.133	.286*	.119	.271*	.104
Anxiety	.453**	.127	.155	.113	.259*	.098
Depression	.481**	.125	.127	.111	.251*	.098
Bed Days	.628	.352	.047	.313	.259	.269
Suicidal Ideation	.039	.038	.039	.034	.038	.030
Tranquilizers	.496	.457	.255	.407	.319	.350

* Indicates a significant univariate test at $\alpha = .05$.

** Indicates a significant test adjusting for multiple comparisons at $\alpha = .05$ (equivalent to a univariate test at $\alpha = .05/8 = .006$).

There is no agreed-upon standard for selecting such cut-points in screening scales of the sort we have employed. However, in an effort to provide some rough feel for the magnitude of influences, we looked at three separate cut-points: the 80th, 90th and 95th percentiles of each outcome variable. For each of these cut-points, the pooled prediction equation was replicated using logistic regression analysis.

Using these cut-points for each of the four outcomes, we generated 12 logistic regression equations. The coefficients in these equations were transformed to yield estimates of relative risk—the percent of unemployed people who report extreme scores divided by the percent of stably employed people who report extreme scores.

These estimates are presented in Table 3.7 All of the estimates for the top 20th percentile of distress are significant (that is, the logistic regression coefficients associated with unemployment in these equations had standard errors which were less than half the magnitude of the coefficients) and the relative risk estimates are between 1.54 and 1.68. This means that people who experienced unemployment were between 54% and 68% more likely than the stably employed to report levels of distress in the top 20th percentile.

Comparable coefficients for distress at the top 10th percentile are significant for three of the four outcomes and somewhat larger than those in the first column. Risk of extreme anxiety, somatization and subjectively experienced ill health are all much more likely to be found among people with unemployment experience. These risks are twice as high as those of the employed for somatization and anxiety.

Distress levels in the top 5th percentile are also more likely to be found among those with unemployment experience. Although only significant in one of the four contrasts (anxiety), the magnitudes are uniform in showing higher relative risks than at the 20th percentile. In the case of anxiety, where the relative risk is so great that it is significant even with such a skewed outcome variable, people with unemployment experience are more than three times as likely to report extreme distress as are the stably employed.

We have no way of defining rigorously the level of distress that is clinically significant. However, these cut-points are in the same range as those considered clinically significant in validation studies of symptom screening scales (Manis, Brawer, Hunt, and Kercher 1963; Zung 1965; Derogatis 1977; Radloff 1977). We can say with confidence, then, that an experience of unemployment in the recent past is associated with distress levels severe enough to warrant professional intervention. The risk of these distress reactions is conservatively estimated to be at least 50% greater than the risk found among stably employed persons.

DISCUSSION

The analyses reported here represent an initial attempt to evaluate the effects of unemployment on worker health in a population that has been particularly affected by the economic recession of the early 1980s. Our results clearly document that unemployment has health-damaging effects in this population that can be considered clinically significant. This is true despite the fact

TABLE 3. Estimates of Risk and Relative Risk of Extreme Outcome Scores Among People With Unemployment Experience and the Stably Employed

	Top 20th Percentile of Distress			Top 10th Percentile of Distress			Top 5th Percentile of Distress		
	Risk _u	Risk _{se}	Relative Risk _(u/se)	Risk _u	Risk _{se}	Relative Risk _(u/se)	Risk _u	Risk _{se}	Relative Risk _(u/se)
<i>OUTCOME</i>									
		(1)		(2)			(3)		
Physical Illness	.274	.178	1.54*	.145	.083	1.75*	.071	.044	1.63
Somatization	.258	.154	1.68*	.157	.076	2.06*	.097	.038	2.54
Anxiety	.244	.153	1.59*	.125	.054	2.32*	.107	.031	3.45*
Depression	.256	.165	1.55*	.118	.079	1.50	.060	.027	2.22

* Indicates a significant univariate test at $\alpha = .05$. None of the coefficients are significant in a test adjusting for multiple comparison at $\alpha = .05$ (equivalent to a univariate test at $\alpha = .05/8 = .006$.)

that the health of our stably employed comparison group might well have been affected by the high rate of unemployment (Brenner and Mooney 1983).

We have been, in fact, consistently conservative in our estimations of the health effects of unemployment. For example, in the estimation of relative risk, we compared the pooled sample of currently *and* previously unemployed people with the stably employed group. We did this to avoid selection bias caused by differential selection into becoming reemployed on the bases of the outcome variables. However, to the extent that reemployment alleviates some of the health problems initially caused by job loss, we have underestimated the effects of unemployment on risk of clinically significant outcomes.

The association of unemployment with alcohol use, suicidal ideation and tranquilizer use were dismissed as selection effects. This represents a conservative stance for two reasons. First, we were careful to classify respondents as being possibly at fault for their job loss if there was any reason to suspect they may have played a part in bringing the situation on themselves. As a result, many individuals included in the "possibly at fault" subgroup may actually have been blameless.

Second, there is a possible substantive interpretation for the higher levels of health problems among people who reported being to some degree responsible for their job loss. We know that perceptions of fault affect adjustment to stress, and those who impute characterologic self-blame experience more extreme emotional reactions than those who do not see themselves at fault (Janoff-Bulman 1979). Therefore, fault for the job loss might be associated with health outcomes because the realization that one's joblessness was self-inflicted is an exacerbating condition of the health effect of unemployment. This is not a selection effect, but an important substantive specification.

There is no way for us to discriminate the selection and substantive interpretations with the data available to us here. Panel data would be required to make this assessment, in which we would use a pre-job loss measure of the health outcome to study selection effects and the information about attributions of blame to specify the effect estimates.

There is yet another way in which our estimates are conservative. In separate analyses,

we found a substantial subgroup of respondents who have been unemployed for a considerable period of time yet appear to experience no adverse health effects. When we looked at these individuals more closely, we discovered that they are largely single mothers of young children or women married to men who are the chief breadwinners in the families. Job loss appears to be a benign experience for these people; if they were excluded from the analyses, the estimates of the health effects of unemployment would be even higher.

Up to this point we have not investigated any specifications that might help provide targets for intervention efforts. We suspect that the impact of unemployment varies depending on the respondent's access to a variety of coping resources. The experience of unemployment is also affected, we believe, by characteristics of the lost job, including the ways in which this job fulfilled valued goals for the job incumbent. We anticipate that a complex array of specifying influences will be found in a systematic investigation of main fulfillments of work.

There is also likely to be considerable variability in the extent to which reemployment can reverse the health-damaging effects of unemployment. We know from preliminary analysis that many of our respondents succeeded in obtaining new jobs only by settling for lower salaries and worse job conditions. Underemployment or downscaling of job rewards may be pervasive features of the American occupational structure for the remainder of this century. It is therefore of considerable importance to investigate the implications of this situation for the health of workers.

NOTES

1. An attempt was made to choose the census tract with the highest rates of unemployment within a target area known to be experiencing high unemployment levels, while maintaining geographic continuity between the tracts. The unemployment rates in the 14 tracts ranged from 7.5 to 31.5 percent. The overall unemployment rate for the entire selected area was 13.3 percent.
2. The sample of predesignated housing units was divided into a series of random replicates before screening began. A disproportionate weighting scheme was assigned to the first of these replicates as a way of oversampling people with unemployment experience. Because subsequent replicates were randomly generated, it was possible to revise the weights during the course of the study and still arrive at a single overall weight for each stratum. This random replicate

- scheme allowed us to control sample selection so that the numbers of respondents in the three strata were roughly equal. The overall conditional selection probabilities were 1 (currently unemployed), .833 (previously unemployed), and .262 (stably employed).
- An equal probability sample of 2,333 occupied housing units was drawn from a total of over 14,000 in the sample frame. Screening for household composition was successfully completed in 96% ($n=2234$) of the sample households and 848 were found to contain no eligible respondent. Persons were ineligible if they were under age 18 or over age 69; if they were living in the area primarily for a household member to attend college; if they did not have and did not want a steady paying job of at least 30 hours a week; or if they had held no such job for at least 12 months during the preceding five years. These exclusionary criteria were established in order to focus on the impact of unemployment on relatively permanent and full-time adult workers who then became involuntarily unemployed.
 - The alcohol measure, constructed specifically for this study, derived from two questions. "About how often did you drink in the last month—how many days out of 30?" and "When you drink, about how many glasses of beer or wine or shots of hard liquor do you have in one day?" The "amount drunk" variable used in the analysis is the product of the answers to these two questions. The tranquilizer question was also developed for this study: "About how often did you take tranquilizers in the past month? How many days out of 30? The four-item index of self-evaluated physical health and the bed days question are modified versions of standard questions in surveys conducted by the National Center for Health Statistics (e.g., NCHS 1979). The somatization, anxiety and depression items were taken from the SCL-90 (Derogatis 1977). The suicidal ideation question—"Have you thought seriously about committing suicide in the past five years?"—was written specifically for this study.
 - The coefficients for the currently unemployed and previously unemployed are obtained from regression equations in which two separate dummy variables for these subgroups are entered as predictors along with controls for age, sex, race, education and marital status. The coefficients for the pooled analysis of currently/previously unemployed are obtained from a separate series of regression equations in which a single dummy variable is used to define either currently unemployed or previously unemployed. The control variables are the same ones used in the earlier equations. Complete results of these regression equations and all other results summarized in this paper are available from the authors.
 - It is useful to note that this is a much narrower view of selection than that in the classic selection-causation distinction made in research on the relationship between social class and mental illness (e.g., Kessler and Cleary 1980). It is likely, for example, that people in the lower social classes have a particularly high probability of holding jobs that are insecure and that this helps explain the high rates of mental illness

among lower class people. This kind of structural determination is not considered in our analysis of selection. We focus instead on selection at the individual level, in which the probability of an employed person's being fired varies as a function of his or her emotional functioning, holding social class constant.

- The logistic regression coefficients express the outcomes as nonlinear and nonadditive functions of the predictor variables. The relationship between a change in the probability of the outcome variable and a change in one of the predictors is

$$dP/dX_i = b_i P(1-P)$$

where b_i is the logistic regression parameter associated with the predictor X_i . The value of P where the derivative is being evaluated depends on the values of all predictor variables in the equation, which demonstrates the inherent interactive nature of the logistic formulation.

In our calculation of relative risk, we assume that all control variables in the prediction equation are at their means, so that we evaluate the influence of unemployment at the value of P equal to its observed mean value. In the case of an outcome variable defining extreme distress at the 95th percentile, for example, we estimate the change in probability of this outcome associated with unemployment to be $(b_u) \times (.05) \times (.95)$, where b_u is the logistic regression coefficient associated with the unemployment predictor in the equation. The product is then assumed to be the net effect of unemployment at the means of the controls.

This change in probability is the difference in the probability of extreme distress between those respondents who experienced unemployment ($n=308$) and those who were stably employed ($n=184$). That is,

$$\Delta P = P_u - P_{se}.$$

To solve for P_u and P_{se} , we must solve the equation

$$\begin{aligned} .05 &= (308 P_u + 184 P_{se}) / (308 + 184) \\ &= [308(P_{se} + \Delta P) + 184 P_{se}] / (308 + 184) \end{aligned}$$

Once we have solved for P_{se} we can estimate P_u by substitution. These two probabilities are then used to define relative risk of extreme distress among respondents with unemployment experience compared to those who are stably employed (P_u/P_{se}).

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